# An Autobiographical History in PER

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This is an entry in an intended series of historical articles on Physics Education Research (PER). Please note that this essay is necessarily from an individual point of view. It is the hope for this project that we can gather recollections of these and other events from the multiple points of view of other participants to enable the reader to triangulate the events. We also wish that a record of the events that constitute the origins of physics education research be documented in a public setting. Described herein are events from about 1969 to 2009 mostly in connection with the American Association of Physics Teachers (AAPT).


## I. INTRODUCTION

The origins of my own involvement in PER come from two sources. One source was teaching. The other source was my enjoyment of research. In both of these the underlying motivation is a desire to understand the world around me. This means trying to function from an explanatory theory base which I am continually checking against experience.

Late in high school I had developed opinions about the schooling I had experienced, good and bad. Early in college I had mentioned that teaching might be an interest of mine. My parents responded with something like: Why not wait until you have finished grad school? [1]

I had had a first glimpse of research doing science fair projects. More exposure came as I worked part time in research labs, both chemistry and physics, freshman year to make sure I really wanted to be a physics major at Case Institute of Technology. Later as an undergraduate, my part time work led me deeper into research activities, as I continued to work part time in various labs during the school year at Case. Undergrad summer research opportunities at the University of Maryland and at Case contributed to my experience in the lab further. Right after receiving my B. S. in Physics, a summer job at the National Bureau of Standards (now National Institute for Standards and Testing) in Maryland gave me an even deeper taste of research as I solved a problem in metallurgical sample preparation for research in solid-state physics. Later, I was also involved in several research projects including my Ph. D. thesis during my graduate work at The University of Texas at Austin.

## II. TWO MOTIVATIONS INTO PER

### A. Teaching

I probably would have gone straight into grad school, but the Vietnam War interrupted these plans. Graduating in 1969 with a Selective Service lottery number of 69, no graduate program could keep me from being drafted immediately. But, I could still get a deferment for teaching. I took that as an opportunity to serve in a more constructive way. I knew that my parents would not object to my choice of teaching over being drafted and it would allow me to try out teaching.

I took a job teaching physics at a large inner city high school in Cleveland, OH. I had taken a couple of the required education courses before graduating and signed up for 18 credits in evening classes to earn the rest. The first semester I was a teacher for real and at the same time I was also earning credit for what was called "student teaching" and taking a general high school teaching methods course, plus six additional credits of electives I needed to satisfy requirements for a teaching certificate.

The school had PSSC materials, which had fallen in to disuse, but as my own high school course used these materials, I elected to dust them off and use them.[2] I also had a complete set of the Teacher's Guide for PSSC physics given to me by my high school physics teacher.

I can remember giving my students the first test on kinematics. These were multiple choice tests developed for the PSSC materials. As the students took the test I worked it myself. I had a startling realization. Most of the questions with numerical answers did not require calculations. If one knew the rules for propagating significant digits, the correct answer was also the only one with the proper number of significant digits. Had I only realized that when I was a student in the course! The other more significant realization came in the results on this test.

Earlier as I began the unit on kinematics, I had my first "teacher" thought. I knew that kinematics was a challenge to students. Much of this revolved around doing the "word" problems. Thinking about this, I decided that the challenge was probably because the students did not understand the meanings for the symbols in the equations. I figured this was in part because, when calculus is not in the picture, a lot of hand waving has to go on to develop the equations and this the high school students did not follow. So, I decided that some more pictorial method of developing the equations was called for. It hit me that straight-line graphs with related simple analytic geometry might do the trick. My students were taking a course called Advanced Mathematics at the same time, so my plan to use graphs and analytic geometry seemed appropriate.

I worked up a presentation developing the equations we use to solve problems involving uniform linear acceleration. I had everything color coded using different colors of chalk on the board. Since uniform linear acceleration involves things that are straight lines in the velocity graphs, I could use the equation for a straight line, slopes and areas under straight lines (simple geometric shapes, triangles, rectangles and trapezoids) to find everything needed. Indeed, I showed this to the students, patiently working my way through answering questions along the way and ended up with the whole thing on the board with no erasures. I personally found it spectacular. Why would anyone need to write whole chapters on kinematics? Everything one needed to know was in this picture on the board. I wished I could just take a picture of it. [3]

Folks who have been teaching for a while will probably be able to predict what the result of this lesson was. But, I was in my first semester of teaching and genuinely expected some great results. Sadly, my students averaged no better on this test than the national average. The national average was around 17 out of 40. I was crestfallen. Why did it not work?

As a brand new teacher it was natural to assume that these results were due to some inadequacy on my part. The assistant principal, Clayton Zeidler, assigned to shepherd the new teachers gave me many good pointers about teaching, but at that time I could not express clearly what my issue was. None of these useful ideas, either alone or together, really resolved my problem. As such there was something missing about teaching for me.

Three years later as a more experienced teacher in Maryland, teaching in the county where I had grown up, I was still searching for what was missing about teaching. I was the 9$^{th}$ grade physical science teacher and the 12$^{th}$ grade physics teacher. The physical science assignment was the vast majority of the job. During that year in Maryland, I had occasion to work with a physics professor at the University of Maryland to develop a weekend event for science teachers on lasers. This professor was John Layman. [4]

My students had taken 8$^{th}$ grade earth science the year before. They were quite positive about it. I went to my students' 8$^{th}$ grade teacher. He had been a teacher in the county for a while. I had not taken a course from him, but I knew him through my science activities when I was a student in the county. Our conversation went something like the following. I said, "It seems you have a pretty good thing going in your earth science course. The students this year seem to know some earth science and they speak fondly of their experience with you. How did you figure out what to do in your course?" His response was something like, "Well, I have been teaching for a while and tried a lot of things. I just kept the good stuff." I thanked him, but I was underwhelmed. I did not look forward to a future of this sort of shotgun approach.

Yet, I still could not put my finger on what the problem was for me. In hindsight now, I can see that the secret to my problem lay in the question I had asked him: "How did you figure out what to do in your course?" My colleague had heard this question as about a kind of generic, coming up with stuff that works. What was driving me, and still does, is the desire to *figure out* what will work. This means to operate from some notion about how the process of learning works and use it to, in essence, make predictions in the form of teaching strategies and student materials and then try out the predictions; in other words, see if the students changed their understanding of the phenomena we study. I can say now, this is what I liked about physics, but it was not, and sadly still is not, the approach used most in teaching, nor is it an approach taught in teacher preparation. At the time while still a school teacher, this clarity was not available to me.

The draft was terminated in the Spring of 1973. I had taught three years in the inner-city and one more in my home county in Maryland. I enjoyed teaching, but there was still something missing about it, so I elected to go to graduate school. My reasoning was that if I became a university professor, then I could teach and do research in physics. Maybe the combination would be fulfilling and my parents would be pleased.

### B. Graduate School & Research

I entered the graduate program in Physics at The University of Texas at Austin. It was a program somewhat different than typical in the U.S., but still many things seem to have been similar. Early on I received a teaching assistantship to work in a special course developed by Robert N. Little.[5] He had worked on a project to develop a junior high physical science curriculum for the state of Texas. In the course of this work, he realized that many of the people teaching the course in the schools had had no physical science beyond the course they themselves had taken in junior high. Clearly a college course was in order. He developed this course, which also became a general area studies course available to all non-science majors. The teaching of the course required that a person actually be the teacher for a couple of sections of 24 students. Because of this he was always on the lookout for physics and science education grad students with teaching experience to work in the course. My experience teaching high school gave me an automatic consideration for a teaching assistantship.

As I worked my way through the graduate program, I worked in several research labs in the summers in a couple of the groups. Ultimately, I settled back into solid state, or condensed matter as it was called in the department. I had developed a strong interest in solid state as an undergraduate, but as I went into graduate school I thought I might do plasma physics. In the end it turned out that there were people in the solid-state group who paid closer attention to their students at the time. I found an advisor, Bill McCormick, who was willing to work with me as much as I wanted to contribute to the efforts of his group.

I spent some long hours making and perfecting measurements. I was doing calorimetry. I remember my reaction when Bill first suggested it. I associated calorimetry with freshman and sophomore physics. As it turned out, this calorimetry was much more. The project involved elements of confirmation of basic properties of matter. Studying the heat capacity of a sample as a function of temperature reveals the presence of subtle phase changes by the presence of sudden large changes in heat capacity.

The project involved elements of physics and chemistry. I was working in collaboration with a group in the Chemistry Department. They were studying a family of compounds, at least some of which they claimed displayed solid-solid phase transitions.[6] They had built their case via Raman spectroscopy and optical polarization measurements, but these were not enough to convince "the powers-that-be" in the field that these transitions actually existed in these compounds. There were probably only two kinds of measurements that would resolve the issue. One was temperature resolved X-ray diffraction studies. The other was temperature resolved calorimetry.

Several challenges presented themselves. The temperature resolution required for either method to shed light on the question was beyond what had typically been done at the time. The thermal conductivity of the samples was low, so small samples had to be used and very slow temperature changes had to be used. Since solid-solid phase transitions were expected, passing through a transition temperature generally resulted in physical stresses, which converted a single crystal into multiple crystals. At best two passes through the transition temperature (down then up) rendered a sample useless for further study.

With my advisor I explored a method we called AC calorimetry. In the end the method enabled us to make heat capacity measurements on very small samples resolved to the nearest 0.1 K and required temperature control and stability to a few milliKelvins over periods of several hours. A reproducible spike or sudden shift in the heat capacity of the samples was a dead give-away for the existence of solid-solid phase transitions. As it turned out at about the same time as I was defending my thesis, a group in Germany was doing the temperature resolved X-ray study. Their work confirmed our calorimetric results and the structures predicted by our Chemist from Raman spectroscopy.

Two things about the work in the physics lab lit me up. One was the thrill of the chase: trying things, finding out what did not work, working out and testing things that would, and for periods of time knowing something no one else knew, yet. The other was finding things not expected and figuring out how to be convinced a particular explanation was appropriate. In short it was working from a theory base, not as a given, but as something that changes and evolves in the interplay between theory and experiment.

### C. Merging Teaching and Research

At about the time I was switching from a teaching assistantship to a research assistantship, I encountered an article in Physics Today. The article was entitled: "Can physics develop reasoning?" (Fuller et al., 1977) It was written by Robert Fuller, Robert Karplus, and Anton Lawson.[7,8] The authors were sharing implications of the Swiss genetic epistemologist Jean Piaget's theory for physics teaching and advertising a workshop that people could take at national meetings. This was one of the first AAPT workshops. When I read the article, in Piaget's theory of cognitive equilibration I saw suddenly how to express my unresolved issue with teaching and a way to resolve it. I realized that what I thought the purpose of teaching was to have students leave the experience having developed a new understanding of the phenomenon being studied. Piaget's ideas about how we come to understand not only made sense to me, but it suggested how, why and under what circumstances people's understanding changes. Not only that, but the interview method used by Piaget and his colleagues opened the door to examine students' understandings of the phenomena.

As a graduate student I had relatively little access to AAPT meetings, but Robert (Bob) Little was able to order the traveling version of the workshop so that we could do the workshop there in the department. The workshop was ordered in and we advertised a time and location, but the only people who showed up were Bob and I. I was very disappointed. But, shortly after that Bob found another opportunity.

At the time Addison Lee was head of the Science Education Center at UT-Austin and Director of the local site for the NSF-AAAS Chatauqua short-courses. Robert Fuller and Mel Thornton from the University of Nebraska-Lincoln were doing an extended version of the "Piaget" workshop that they had developed in conjunction with a project on their campus called the ADAPT Program. I was still a grad student and not technically eligible for these NSF funded courses, but since it was being offered in Austin and there would be no real expense to add me to the class, I was invited in.

The first installment to the course was a weekend in the Fall of 1977. We learned the basics of Piaget's theory, saw some application to the analysis of typical teaching materials and learned about an instructional strategy called the learning cycle. Since this course was for college faculty in any science or mathematics, examples were drawn from many fields.

We were given an assignment to carry out before we met again in the Spring. Part of the assignment was to administer some paper-and-pencil diagnostics of reasoning to a class of students and report back our results. The other part of the assignment was to design a learning cycle, use it with a class and report our experience and learning results.[9] I was no longer a teaching assistant, so I did not have a class to carry out the assignment. Luckily, Bob Little did and he needed someone to fill in for him during the first couple of weeks at the beginning of the semester in January. He also had a suggestion for a topic for the learning cycle: the work of Al Bartlett on the consequences of exponential growth and the problem of finite fossil fuel resources.[10] So, I developed a learning cycle based on these ideas and carried it out while substituting for Bob.

When the group met again for the final installment of the short-course, I was a thoroughly changed teacher. I had seen Piaget's theory in action, made and analyzed measurements of student reasoning, developed a learning cycle and seen how it worked. Bob Little, as a mentor throughout my time at Austin, watched all of this happen to me. I talked to him about my future plans off and on as a teaching assistant for him and during the course of the short course by Fuller and Thornton (Mel, that is).

I was coming to the decision that the physics lab was not the only way to satisfy my need to operate from a theory base. Piaget's theory could serve very well as a theory base from which to teach physics. This was what I had been missing about teaching. Such an approach was never presented to me in my training to teach or explicitly displayed by the colleagues around me in the schools or at the university. Sadly, this is still largely true today. I figured that with Piaget's theory I had a theory base from which to operate in the classroom. As such, I did not really need work in an experimental physics lab to make a contribution and satisfy my needs. In essence I was deciding to pursue physics education research. This decision changed my path as a university professor from the every beginning.

Bob Little had been following my development in our conversations. We discussed the pros and cons of my moving from the program at Austin to the SESAME program at Berkeley, which Bob Karplus had just established. For a number of reasons, I eventually decided against switching programs. With a sense of my decision about what I wanted to do as a university professor, Bob Little was on the lookout for opportunities that might fit my decision. As it turned out Oklahoma State had an opening, which they began advertising in the Fall of 1977 for what we call physics education, as opposed to PER today. If I pushed a little I could be available by the time they needed someone for Fall 1978, so Bob encouraged me to apply. I have relatives in the Oklahoma City area so I went to visit them at Christmas and stopped in for a visit in Stillwater at Oklahoma State. It seems this was a valuable move in that when my application was eventually submitted, I was invited for an interview. The colloquium I gave was not on AC calorimetry, but on my little project for the short course on Piaget and the Development of Reasoning with some material added from the small base of published work in the field. After the interview, I went back to making my calorimetry measurements.

I was in the lab one day when the call came in offering me the job at Oklahoma State. This raised the pressure to complete my degree, which I managed to do in time to be in Stillwater by Fall of 1978. As it turned out they had not really hired me for these early efforts, which we would associate with PER now, but that is another story on the sociology and politics of physics. At Oklahoma State I encountered another mentor, Lee Rutledge, who as it turns out had earlier been an advisor to John Layman when he was a grad student there.

In my pursuit of Piaget's theory as my theory base in the classroom, I realized that disequilibration was a key issue. Piaget's theory suggests that the preferred state is one of equilibrium between one's experiences with the world and one's explanations of the world. When one detects a mismatch, then one experiences disequilibration. There are two ways to respond. One is to avoid the 'offending' experience and hope it does not happen again. The other is to draw close to the new experience, examine it, and to construct and test adjustments until one's new explanation fits this new experience. In the end one has a new explanation that fits a wider range of experience better. The new understanding is the result of a process Piaget called self-regulation, initiated or triggered by disequilibration. Unless one experiences disequilibration, one has no need to make changes in one's understanding. The consequence is: If my goal was for students to be in possession of new understanding, I had to induce disequilibration in the classroom on a regular basis.

I had devised and used several learning cycles, some in the lab program at Oklahoma State.[11] But, I realized that there was one thing missing from the learning cycles I had seen and others I had developed. This one thing was the focus on trying to induce disequilibration. How does one get disequilibration to happen?

How does one get disequilibration to occur? The question was posed to anyone I thought might be able to answer the question. There were two kinds of responses available from people at that time. One was, "Huh?" The other was, "Well young man, that's a very good question."

My quest for an answer lasted until Arnold Arons introduced me to Jim Minstrell at an AAPT meeting in January 1981.[12] Jim was a high school physics teacher who had earned a doctorate in Science Education with Arnold as his advisor. They were at an AAPT meeting when I approached Arnold to say hello.[13] Arnold introduced me to Jim saying to me, "Here's a young man I think you should meet."

As it turns out Jim was in the throes of writing the "Explaining the 'at rest' condition" paper published in The Physics Teacher the next January. (Minstrell, 1982) As Jim explained to me what he was doing with his students on the nature of force, I realized he had the answer to my question about disequilibration. He was engaging students in testing their own explanations in settings where he was pretty sure their explanations would not work. In other words, Jim was creating situations in which students were likely to disequilibrate. Then, he would give them the chance to work out and test alternative explanations. In the end many of the students would have a new notion of the nature of force and its relationship to motion. This new explanation worked better than the original one. The process involved students reconstructing their own existing notions of force and their existing notion of acceleration.

I had been introduced to Jim early in the AAPT meeting that January. Jim had to leave the meeting early to go back to his classroom. When I returned home at the end of the meeting, I found a letter from Jim. Apparently on the airplane home, Jim had written an extensive set of notes about his approach with students on the nature of force and its relationship to motion on sheets of yellow paper and mailed them to me once the plane landed in Seattle. I tried them out that semester in my lecture classes in the algebra-trig level of introductory physics at Oklahoma State.

I came to understand the approach and students' understandings by using what Jim shared with me about the issue of the nature of force. Eventually I began developing the approach to other topics in physics. To be successful at this, two things were required. One is to have a sufficient working understanding of the students' understandings. The other is knowledge of many kinds of experiences with the phenomena that can be reproduced in lab. Knowing how the students think enables one to sort through the possible experiences to find those that probably do not match the students' expectations. If one can get the students to justify their predictions with explanations, then not only have the students elicited elements of their understanding, but also they have formed a commitment to their prediction. When the prediction is found not to fit the experience, the chances for disequilibration are heightened.

By 1989, I had been teaching at the university level for about 10 years. A Mathematician from Boise State, Dan Lamet, and another from Carnegie-Mellon University, Frank Boyle and I decided to go for a NSF grant. Eventually we were successful in our request for a grant.

The first semester of the project, Fall of 1989, called for me to spend time at Carnegie-Mellon working with Frank. Because Washington, DC was close, by Idaho standards at least, and my family lived about 50 miles north of Washington, I managed to get down to NSF to visit our Program Officer, Ray Hannapel, several times. The first time I made the trip, Ray suggested that my ideas sounded like those of Ernst von Glasersfeld and asked if I was familiar with his work. When I replied I was not, he handed me a chapter written by Ernst titled: "An Introduction to Radical Constructivism." (Glasersfeld, 1984) I read it as soon as I returned to Pittsburgh.

I had encountered the word, constructivism, several times previously. I could remember Rosalind Driver's group from Leeds using it in their publications, but I also noticed some difference between their constructivism and what made sense to me from my experience in the classroom and my studies of Piaget. I did not know what Ernst meant by radical constructivism, but I was interested to see if he and I were thinking in similar ways, since Ray had suggested it.

On the first read, Monday after returning from the DC area, much of the article was very appealing, but it seemed like in the end Ernst was talking about solipsism. The solipsism notion hung me up and I spent the next several days re-reading the article trying to figure out if indeed he was talking about solipsism. How could he be talking about solipsism, but otherwise make so much sense? This occupied my mind in the office, during the 20-minute walks between apartment and office, and in the evenings. Finally, walking back to the apartment on Thursday, I realized he was not talking about solipsism. He was not saying everything is in the mind and nothing is "out there," merely that we can never know that our mental constructions match an external reality, only that they fit within the experienced bounds of an external reality. Ernst von Glasersfeld put it this way:

"…as long as we remain, in our innermost belief, "metaphysical realists" and expect that knowledge (scientific, as well as the everyday) provide a "true" picture of a "real" world that is supposed to be independent of any knower, the skeptic cannot but seem a pessimist and spoilsport because his arguments perpetually draw attention to the fact that no such "true" knowledge is possible. The realist may, of course, remain a realist in spite of this and say that the skeptic's arguments can be disregarded simply because they contradict common sense. If, however, he takes the arguments seriously, the realist must retreat to some form of subjective realism, and this retreat inevitably leads to solipsism, that is to the belief that there exists no world at all apart from the conceiving mind of the subject.

"On the one hand, this situation seems inevitable because of the unimpeachable logic of the skeptical arguments; on the other hand, we are intuitively convinced and find constant experimental confirmation that the world is full of obstacles that we do not ourselves deliberately place in our way. To resolve the situation, then, we must find our way back to the very first steps of our theories of knowledge. Among these early steps there is, of course, the definition of the relationship between knowledge and reality, and this is precisely the point where radical constructivism steps out of the traditional scenario of epistemology. Once knowing is no longer understood as the search for an iconic representation of ontological reality, but, instead as a search for fitting ways of behaving and thinking, the traditional problem disappears. Knowledge can now be seen as something that the organism builds up in the attempt to order the as such amorphous flow of experience by establishing repeatable experiences and relatively reliable relations between them. The possibilities of constructing such an order are determined and perpetually constrained by the preceding steps in the construction. This means that the "real" world manifests itself exclusively there where our constructions break down. But since we can describe and explain these breakdowns only in the very concepts that we have used to build the failing structures, this process can never yield a picture of a world which we could hold responsible for their failure.

"Once this has been fully understood, it will be obvious that radical constructivism itself must not be interpreted as a picture or description of any absolute reality, but as a possible model of knowing and the acquisition of knowledge in cognitive organisms that are capable of constructing for themselves, on the basis of their own experience, a more or less reliable world." (Glasersfeld, 1982, p. 38 – 39)

This is the same claim about our mental constructions of the world as made by Piaget, physicists and historians of science, but coming from different perspectives:

"It is clear there is an undeniable role played by experience in cognitive development; however, the influence of experience has not resulted in a conception of knowledge as a simple copy of outside reality." (Piaget, 1972)

"Now there are two theorems that form together the cardinal hinge on which the whole structure of physical science turns. These theorems are: (1) there is a real outer world which exists independently of our act of knowing and (2) the real outer world is not directly knowable." (emphasis in the original) (Planck, 1981)

"If what we regard as real depends on our theory, how can we make reality the basis of our philosophy? ...But we cannot distinguish what is real about the universe without a theory...it makes no sense to ask if it corresponds to reality, because we do not know what reality is independent of a theory." (Hawking, 1994)

"As a result of modern research in physics, the ambition and hope, still cherished by most authorities of the last century, that physical science could offer a photographic picture and true image of reality had to be abandoned. Science, as understood today, has a more restricted objective: its two major assignments are the description of certain phenomena in the world of experience and the establishment of general principles for their prediction and what might be called their "explanation." (Jammer, 1999)

Once I understood this point that radical constructivism is not about solipsism, it was clear that Ernst's radical constructivism was a very good fit to how I was thinking. He had been working on it longer than I, so I figured he might be a very good mentor.

Because of the NSF grant I was able to visit the University of Massachusetts at Amherst where Ernst was retired in order to consult with John Clement and Jack Lochhead. I met Ernst von Glasersfeld for the first time then in the Fall of 1989. Since then I have been able to attend talks, visit every other year or so, communicate via email and study his writing. He has been a very valuable mentor to me. My pedagogical practice based in the constructivism of Piaget and Glasersfeld as it as matured demonstrates that this form of constructivism is worthwhile in the learning results that routinely occur as a result of using it. (Dykstra, 2005)

## III. AAPT AND PER

As an undergraduate at Case Institute of Technology in the later 1960's, Harvey Leff was my undergraduate advisor.[14] He shared an advertisement from AAPT that undergrads could join and receive journals at reduced rates. On the basis of that advertising, I joined in 1968. Between then and 1978, when I joined the Physics Department at Oklahoma State, I attended one national meeting in Albany with a fellow high school physics teacher from the Pittsburgh area. We had met in an NSF funded summer institute about teaching Project Physics at Knox College in Galesburg, IL. My membership enabled me to read the AAPT journals, which proved useful when I became a high school teacher.

During the first year at Oklahoma State I noticed an interesting session at the Summer 1979 AAPT meeting to be held in Las Cruces, NM. Bob Karplus organized a session in which Piaget's ideas, as applied to physics teaching, were featured. This was my cup of tea! The department-funded travel was a perk of being a university faculty member. I began going to AAPT meetings that Summer and have attended most since.

Karplus and others kept organizing sessions for meetings. I was hooked. At Oklahoma State, the Physics Department was very near the Library. I began to keep track of journals in which people published work using Piaget's ideas in science/physics teaching: Science Education, Journal of Research in Science Teaching, School Science & Mathematics, Physics Education, but rarely American Journal of Physics or The Physics Teacher.

After several AAPT meetings, Bob Fuller let me know that there was a move to establish a committee on Research in Physics Education (RiPE) and wanted to know if I was willing to be a member of the committee. I responded in the affirmative. I am pretty sure that Bob Karplus, Jim Gerhart, Arnold Arons, and Bob Fuller among others were involved in the deliberations.[15] After some behind the scenes deliberations, the formation of the committee was announced. Bob Bauman came to me to let me know that while my name had been discussed, I would not me one of the first members of the committee.[16] I was just glad to see the committee established.

### A. Differences of Opinion: Scheduling at National Meetings

As the PER community began to define itself as a discipline within physics, there were some who were not inclined to accept the new upstart. The primary manifestation was not to recognize PER as significantly different from what had always been called physics education. Several times the RiPE Committee voiced concerns over the inclusion of presentations that are not research in physics education in sessions labeled as physics education research.

The PER community was not objecting to these non-research presentations at AAPT meetings, but to their inclusion in sessions labeled physics education research. The RiPE committee had voiced its concerns several times when the issue reached a head again as Fred Goldberg was chair. It was fortuitous that at the same time Judy Franz was in the presidential chain in AAPT.[17] Fred and Judy knew each other well because they had both been at West Virginia University for a time. They had a good working relationship and their temperaments and inclinations were to find a reasonable working solution to the issue. They worked out a set of guidelines for the type of presentations that were appropriate for sessions labeled as PER and from the *AAPT Announcer* pointed to examples of presentations that should be included elsewhere in the meetings.

That the issue had come up more than once and was destined to come up again is in part due to processes established for the setting of meeting programs at AAPT. Since the beginnings of AAPT in the 1930's, the meetings were put together by the person who is at the beginning of the presidential chain. The presidential chain is a four-year commitment to which a new person is added by election each year. It consists of the following positions: vice-president, president-elect, president, and past president. In the first year of this process as vice-president the person assumes the chairmanship of the Programs committee. At the beginning of AAPT, anyone who attended meetings was familiar with essentially all the constituencies and interests within the organization. The meetings were not very long and there were many fewer parallel sessions, if any. By the 1980's the meetings were many times longer and had many parallel sessions involving differentiated, specialized information. Essentially no member of AAPT regardless of the amount of experience at the meetings has the capacity to be familiar with the interests and concerns of all the constituencies within the organization. But, the primary job of organizing the meetings was still in the hands each year of this one person at the beginning of the presidential chain, who only did the job one year and moved on. With the clerical assistance of seasoned office staff in the AAPT Executive Office, the vice-president did the organization of the whole meeting.

As a member of the PER community and at times member of the RiPE committee, I had been witness to this series of interactions over the issue of the PER sessions at AAPT meetings. We would think we had the matter resolved and then the issue would crop up again. In the early 1990's I was elected to the Executive Board. At the last meeting of my tenure on the committee, it was clear that the document worked out by Fred Goldberg and Judy Franz a few years earlier had been lost, forgotten or ignored. That meeting in Orlando contained multiple examples that concerned members of the PER community sufficient to be the object of discussions at the RiPE committee during the meeting. At the second Executive Board meeting at the end of each national meeting, it is the habit of the Board to make comments on how the meeting has gone. Because I considered the RiPE committee part of the constituency I represented, I contributed comments about the meeting reflecting the concerns of the PER community. An argument ensued.

I did not anticipate the argument. I had naively assumed that the response would be taken as a reminder to use the agreement worked out by Fred and Judy a few years earlier and that would be it. Instead I experienced an attack on PER from Reuben Alley who was at the time the Programs chairman.[18] He and Fred Stith, re-voiced the same defenses for the features of the organization of meetings that had concerned the PER community several times in the preceding decade.[19] I had not anticipated such a strident defense of these arguments. Unfortunately, when I raised the concerns, Judy Franz who had been in attendance at this Executive Board meeting had stepped out of the meeting for a while. When I attempted to remind the committee of the agreement worked out by Fred and Judy, she was not present to vouch for the existence of the agreement. It quickly became clear that Reuben Alley was a vigorous antagonist to PER who was not likely to change his mind in this setting, if at all. In retrospect, it seems entirely possible that either Fred and Reuben were unaware of the document, which is consistent with their reaction to my suggesting it existed, or they did not take it seriously, which is consistent with Reuben's later review of sample materials from the PIPS Project.[20,21]

The attacks on the request for a consistency between the use of the session label, research in physics education, and the content of sessions so labeled included some of the following. The PER community was trying to dictate which presentations could be included in AAPT meetings and which could not. The PER community was only willing to include "research" that was like that of a certain group.[22] Of course, neither of these claims was the case. Both claims were serious indictments in an organization that has the policy of accepting for somewhere in the program any contributed presentation by any member in good standing. The fact that the PER community was merely trying point out the inappropriateness of including presentations that were not research in sessions labeled research in physics education was not heard.

I figured I had made it clear that there were concerns from the PER community and that this was about all that could be accomplished at this point in the Executive Board meeting. A break was called. I dropped the argument at that point and ended up not returning to the meeting for other reasons.

After I returned home from the Orlando meeting, I tracked down the Goldberg-Franz document. With the document as a guide, I prepared a memo to the Executive Board pointing to examples in the Orlando meeting program where sessions did not follow the guidelines worked out by Fred and Judy. I transmitted the memo to the members of the Executive Board and other interested parties.

Later in the mid 1990's I was chair of the RiPE committee when this issue of the PER sessions came up again. I remembered the document that Fred Goldberg and Judy Franz had produced and the repeated frustrations of the PER community with this issue and attempted to initiate a constructive engagement over the issue once more. This time it came to a head at the Reno Winter meeting. The result was somewhat better and longer lasting change.

Several months before the last meeting of my chairmanship, I sent a memo to the Executive Board, reiterating PER concerns about the organization of sessions at the meetings. This evoked to my great satisfaction, a list 'in print' for all to see of the reasons for rejecting our requests about the composition of sessions labeled, research in physics education. With the list in this form, it was possible to systematically address each one, which I did in a response memo launching another round of the discussion. I believed that when there is the opportunity for a range of people to seriously consider the lines of reasoning enough would see through those weak aspects defense of the sessions the PER community considered offensive.

In the third round of this interchange, I figured that the responses to my memos had not changed sufficiently and the Reno meeting was coming up, so I escalated one more step. I also figured that if there are no reasonable options on the table, little change was likely to result. First, I pointed out that the continued refusal to clearly distinguish between research in physics education and physics education constituted a classic, inherently hegemonic political act to suppress a community in AAPT. It is an attempt by one group to control another by externally defining them in a way they do not use themselves. Then, I made three suggestions for change. One was to go back to using the Goldberg-Franz document to sort papers for sessions about research in physics education. The second was to establish some institutional memory that would preserve knowledge of the document, presenting it to each new Program Committee chair each year. The third was to establish a program sorting committee to assist the Program Committee chair. The committee would consist of AAPT members who live near College Park, who between them were familiar with most of the multiple constituencies in AAPT. Joe Redish was given as an example. He was in a position to represent PER, computers in physics teaching, the undergraduate and graduate levels and he lives in the College Park area.

It seems likely that the suggestion that political ideology was involved got people's attention. Physicists believe they act in strict accordance with objectivity and never ideologically. When one points out that they may be acting ideologically, they react strongly. Robert Hilborn contacted me asking if we could meet at the Reno meeting.[23] When we met, it was clear from the discussion that my comment about ideology had stung, but the reaction in this case was not to entrench, but to become more proactive about finding a solution satisfactory to all. He informed me that the Executive Board had already established a kind of handbook for the Program Committee chair and that an updated version of the Goldberg-Franz document could be included, if one was submitted. He also indicated a program sorting committee would be established along the lines that I had suggested. Joe Redish became one of the first members of this committee.

### B. Publication Issues

There is another front on which the hegemony attempted to control the upstart PER community and that is in publications. For several decades PER people rarely published in AAPT publications because the Editors of the journals resisted their manuscripts. The vast majority of PER publication was outside of AAPT journals. This has changed since about the turn of the current century. The story of this change is best told by some of those most directly involved such as Lillian McDermott (University of Washington), Joe Redish (University of Maryland) and Bob Beichner (North Carolina State University).

## IV. THOUGHTS OF THE FUTURE

Today, the expansion of activity in PER is such that in almost every block of time in the program of typical AAPT national meetings has multiple sessions involving various aspects of PER. One has to make choices and miss things of interest in PER because physical reality apparently limits how many places one can be at the same time. At this writing there is a revival of the original AAPT workshop: Physics Teaching and the Development of Reasoning which is to be offered at the AAPT Winter Meeting in Washington, DC, February, 2010. (Fuller, et al., 2009)

Meanwhile, a small number of Physics Departments have begun to support the establishment of PER groups and produce doctoral graduates whose research is PER. This is a positive sign, but it is still the case that a majority of Physics Departments will not have a PER group. Sadly, most physics teaching is still not significantly changed either in its practice or in the learning results achieved. Yet, PER has made available in print and in meetings evidence that with changed teaching practices quite spectacular learning results are possible for most students. While we should rejoice in our progress, we still have a very long way to go.

## ENDNOTES AND REFERENCES:

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1 After all, I was their first-born son and physics major at Case Institute of Technology. Graduate school was on their minds.
2 PSSC: Physical Science Study Committee, the first NSF funded national curriculum project. A history of NSF efforts along these lines can be found in Rudolph's book Scientists in the Classroom. (Rudolph, 2002)
3 Ah, the hubris of the young…
4 John Layman, AAPT President 1982 – '83
5 R. N. Little, AAPT President 1970 – '71
6 I made my measurements on potassium hexachlorostanate (IV).
7 Robert Fuller, AAPT President 1980 – '81
8 Robert Karplus, AAPT President 1977 – '78
9 A learning cycle was a particular strategy developed by Robert Karplus and colleagues for "lesson" design in the SCIS project both for student materials and teacher training. An early version of the learning cycle is described in an article by Karplus and Atkin. (Atkin & Karplus, 1962)
10 Albert Bartlett, AAPT President 1978 – '79
11 It became apparent that the real reason I had been hired at Oklahoma State was to run the introductory lab program in addition to the standard teaching load and other departmental chores. The supervision of the introductory lab program involved overseeing the work of about 30 TA's who instructed labs for about 3000 students each academic year on an equipment budget of about $0.75 per student.
12 Arnold Arons, AAPT President 1967 – '68
13 Bob Little had earlier introduced me to the work of Arnold Arons. Once I was on the faculty at Oklahoma State, Bob managed to get Arnold to come visit UTx at Austin and I piggybacked a visit up to Oklahoma State. In this way I had managed to spend a little time with Arnold driving 70 miles back and forth from the airport.
14 Harvey Leff, AAPT President 2007 – '08
15 James Gerhart, AAPT President 1979 – '80
16 Robert Bauman, AAPT President 1983 – '84
17 Judy Franz, AAPT President 1990 – '91
18 Reuben Alley, AAPT President 1993 – '94

19 James Stith, AAPT President 1992 – '93. In this interchange Fred Stith I believe was trying to negotiate a peaceful settlement of the argument, but he had walked into a proverbial lions den.

20 PIPS: Powerful Ideas in Physical Science, an NSF funded project of AAPT

21 That Reuben was a vigorous antagonist to PER became clear in a review he wrote several years later of materials developed in the AAPT sponsored Powerful Ideas in Physical Science (PIPS) Project, which coincidentally was about to have its first organizational meeting later on the same day as the argument in the Executive Board meeting in Orlando.

22 It may be that the problem is that "educational research" is thought to be a casual collection of some data from a semester's class, e.g., the effect of doing homework on grades in a college course. One often sees kind of thing in the pages of journals like the Journal of College Science Teaching. The PER community's meaning of research is decidedly not like this example. Instead it is about operating from a theory base and the principled collection and analysis of evidence concerning physics learning, essentially how we learned to think of research in physics.

23 Robert Hilborn, AAPT President 1996 – '97


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