

Chapter 6

The "Work Smart" Standards Era, 1997-1998

I call 1997-1998 the "Work Smart" standards era because Work Smart was DOE's Next Big Thing over this period. As this occurred, DOE's loosey-goosey regulatory stance became more pronounced than ever, with DOE seeming to defer to the contractor's judgment even in areas clearly within DOE's purview. As a result, the O&R segments of ORNL (and perhaps of other sites as well) appeared to feel empowered to make many safety decisions with only a perfunctory nod to DOE regulatory authority and with a concomitant pushing back at the internal safety organization as intrusive or unnecessary.

Bulk Shielding Reactor (BSR) Fuel Removal Project

The Bulk Shielding Reactor (BSR) was an oldie-but-goodie pool reactor that was used for important shielding experiments in its day. It was a principal facility for training of reactor operators and engineers; many now-retired reactor folks got their training at this reactor as part of the old AEC "reactor school". BSR was in standby when I came in 1989, but when DOE decided to close down all ORNL reactors but HFIR for financial reasons, the old stalwart BSR was sentenced to eventual decommissioning.

It was on Death Row for years before that. The fuel was left in the pool and personnel overseen by the Research Reactors Division (RRD) kept the pool circulation pumps, ventilation fans, and other equipment running. It was a concern of the Reactor Operations Review Committee (RORC) that the fuel was sitting in the pool all that time; RRD claimed that with the water circulating, corrosion of the aluminum cladding was a non-issue, but RORC was not so sure. Eventually the fuel was inspected and the cladding on some assemblies was found to be pitted. Thus the fuel had to be removed once and for all.

From June 1996 on, I attended BSR fuel removal project meetings, as the AEG rep to the project (and as the AEG rep to all RRD work) or as a member of the RORC BSR subcommittee. The BSR had never had a safety analysis report (SAR) done to modern standards, since it was in permanent shutdown. But the existing SAR was inadequate to govern the decommissioning work. So DOE had invoked a provision in its safety analysis Order that allowed a sort of temporary operation-specific safety analysis to be written. This was called a Basis of Interim Operations document, or BIO, and would be reviewed by RORC. In addition to the BIO, there would be a work plan, ALARA Plan, etc. On 3 January 1997, I attended a joint RORC-RRD meeting to go over the proposed RRD-written draft BIO, including RORC's comments.

Roger Stover was the lead RRD person on the BSR fuel removal project. Stover was not my favorite O&R manager to deal with because of what I regarded as his dismissive attitude toward contamination concerns. But he seemed to be a competent manager and I had thought that in this special project, he would not fail to "dot the i's and cross the t's" in safety matters. Nevertheless, he surprised me with his increasingly obstructionist attitude toward RORC comments. I had about 40 comments, large and small; he refused to consider most of them, on the grounds that they "did not add value". He similarly dismissed other RORC members' comments (although proportionally not as many). These dismissals without response were done unilaterally -- he simply declared the subject comments void without any explanation. This was a challenge to the authority of RORC, a gauntlet thrown down. Effectively, RORC approval was needed for the BIO to be adopted and the project to go forward, so he was risking offending us and delaying approval until he addressed all our comments. But the new RORC chairman and second member of the BSR subcommittee, R. Mike Harrington, did not confront Stover about this at all, nor did the third member, Rob McKeehan, of the Office of Nuclear Safety (ONS). They accepted Stover's declaration without question, even though my comments were mostly in my specialty area, rad protection and dose assessment, and even though Stover, although a nuclear engineer, had no special rad protection expertise other than his reactor experience.

Regarding the placement of the area dose rate monitors -- called "monitrons" at ORNL -- Stover and his minions said they "didn't understand" one of my comments about the area dose rate monitors. I thought that if they truly understood the principles by which it operated -- as they should have done if they were relying on its use during the operation -- then they should have understood my comment. But trying to be helpful about this, I provided additional written comments on the BSR BIO to an RRD analyst. The reader must understand that a division like RRD, which has many radiation-related calculations to be done, will usually have some people who can run dose rate and shielding codes, even the more complex ones. Although these people will usually be engineers of one sort or another (I think this analyst was a nuclear engineer), they usually have only a limited knowledge of formal rad protection, and what they know of it they don't have to apply often enough to be said to be qualified practitioners of it, e.g., as health physicists. So while the analyst did a good job of calculating such things as how much of each isotope there was in each fuel assembly, he did not understand my concerns about the area monitors. (Or at least he professed not to -- I had a hard time believing that this smart guy had a blind spot in this particular technical area.) So after our talk I left him to mull it over further.

In January 1997, I was also meeting with the BSR task leader, Bob Childs, his crew, and the rad techs. The techs were all people with whom I had had a favorable past history. A notable event occurred when Childs, his helpers, and two rad techs began one of the tasks preliminary to the fuel removal: the removal of some mildly tritiated deuterium oxide (D_2O) stored in a small tank in the pool. I and others were watching the operation through the reactor bay window on the second floor, while the operation took place on the first floor, i.e., at the bottom of the two-story-high bay. Suddenly there was a big thumping bang, so loud at my position that it must have been almost deafening down at the work area. Work was stopped immediately. It turned out that a line got plugged while the pump continued to operate; this led to a sharp decrease in pressure in the closed drum into which the D_2O was being pumped and then to a sudden implosion of the drum. Fortunately, the drum did not rupture and the extracted D_2O did not escape to the pool, the air, or the pool area surfaces.

Childs et al. had planned this operation very carefully and I did not feel that the implosion was something that could be anticipated, even though with pumping out of and into closed containers one always has to consider pressure-related problems. Further, drum rupture was considered very unlikely, everybody was wearing protective clothing, the air monitors were operating, etc., because of the possibility of unplanned disconnections and like contingencies. The radioactivity level in the tank was low. For all these reasons, I thought that this was a reportable occurrence (and in fact it was reported), but not something worthy, either in potential or actual hazard, of a big investigation. What happened after the bang and the stopping of the job was that the event was analyzed over the next day or two, the problem was diagnosed, and a solution satisfactory to everybody was proposed. The job was then done successfully. In the future, some of my critics would say I "demanded perfection", etc., but I think that my participation in the planning of this task and my reaction to the adverse event showed that I was not an extremist about such incidents. What I minded was people's failing to plan the job carefully, brushing off deficiencies and adverse events, and moving on before the lesson had been learned, which Childs certainly did not do.

In early February, I met with the RRD analyst again about my comments on the BIO; again my concern was particularly with the monitron response. In the BSR reactor bay, there were three monitrons, two opposite each other on the long walls and one at the side of the pool. All three were far from the most exposed worker position, which was on the movable bridge that ran across the pool, and all three had the same setpoints. The lay reader should understand that a setpoint is a dose rate trigger level that works like a setting on a thermostat: when the setpoint is reached, the monitron alarms, a sound that can be heard by anyone in the room. This is called the local alarm. The monitrons are also wired together in such a way that when the setpoint is reached for two of them simultaneously, an evacuation alarm is triggered. While a person does not necessarily need to evacuate on a local alarm (this would depend on the operation), he

is supposed to evacuate if the evacuation alarm sounds. A setpoint(s) is supposed to be chosen to correspond to a conservative expected accident or incident situation particular to the facility or area or type of work. Thus the practice at ORNL of having virtually every monitor, in whatever facility and wherever located with respect to the work area and for whatever operation, have the same setpoint was counter not just to good radiological practice but to common sense.

I pointed out to the analyst that since the monitors were so far away from the work area, the dose rate at the worker location on the bridge could be many times higher than the dose rate at any of the monitors. Also, the dose rate at the body position would include a beta contribution, whereas the monitor detected only gammas. Thus the monitor might not alarm even if the dose rate at the body position was quite high. I put my money where my mouth was, so to speak: I had done a calculation to illustrate my point about demonstrating the adequacy of the monitor position and response, and I went over this calculation with him. We spent perhaps half an hour on this, but he claimed that he still didn't get what I was saying about monitor response. Again, I had trouble believing that. In the first place, he did not appear to have read my comments carefully and second, he did not seem to be listening to my explanation and he did not counter any of the points I made. He said he "had a lot of other work to do", so I thought that he simply did not want to spend any more time on the BIO. I reached another conclusion later, as I'll explain below.

In May the RORC BSR subcommittee met again with RRD, with Stover still refusing to consider most comments. Soon after, I spoke at length with Harrington and McKeehan. Harrington finally asked me to reduce my comments to the minimum, which I did. I ended up with eight comments, all substantive. But when we met in July, RRD addressed perhaps four and still refused to address the rest.

My most significant concerns of the eight left over involved ventilation and the positioning of the monitors and air monitors. I will discuss this below, but first I will cite an example of the other concerns that Stover and his minions regarded as trivial but that to a professional rad protection person would seem significant. In the text and tables, the hazards, one dose route was given as "ingestion" but it seemed that they meant "inhalation". Stover and his people tried to tell me that "ingestion" included inhalation. I pointed out that in both rad protection usage and the dictionary, "ingestion" meant taking in by mouth and nose into the digestive system, while "inhalation" meant taking in through the nose and mouth into the pulmonary system (with intake across the lung membranes). I noted that they could mean ingestion, i.e., if they meant the worker would fall into the pool and swallow some water. But they said they didn't mean that (and hadn't considered it at all). Eventually they changed "ingestion" to "inhalation" in some places, but in the final BIO, there were still multiple appearances of "ingestion" where inhalation was meant. Stover's attitude seemed to be that doing a global search and replace to correct this did not add value.

Later that year, I heard that my comments were moot: DOE had blessed the BIO, with a few changes. RRD, without resolving all the RORC comments (including my comment on the monitors), had gone ahead and submitted the draft BIO to DOE for review. A DOE safety reviewer had decided that the relevant monitors for indicating a release were not the monitors (i.e., the monitors of external dose rate) but the air monitors. I expressed concern about this to Harrington. Somebody forwarded my memo to Jim Mincey, the criticality safety section head in ONS. Mincey did not speak with me directly, but sent a memo to, I believe, Harrington, saying that he too recognized the inadequacy of the BIO statements about the air monitors (which was not necessarily to say that he agreed with me, of course). But he contended that there was no use contradicting DOE -- if DOE said something was adequate for a use, then ORNL people, no matter how expert, could not contradict them.

I will try to explain the issue here well enough to satisfy health physics readers but not to confuse lay readers. A release during the fuel removal operation would come (if it did at all, which was quite unlikely) as a result of an inadvertent criticality during movement of a fuel element. The criticality would produce a lot of heat, which would cause the gases inside the element to be heated and pressurized. Since

the fuel remained well below the surface at all times, the direct gamma and neutron dose rate from the criticality at the worker's body, perhaps four feet above the water, could be significant but would not be life- or health-threatening. However, should the fuel rupture (due to compromised cladding integrity), then the gases and some particulates could be released. The particulates could normally be expected to be "scrubbed", i.e., taken out on contact with the water. But if they were small and were contained in a hot gas bubble, they could be borne rapidly upward with the bubble and enter the air space over the pool as the bubble broke the surface. Not only would these particles be a respiratory hazard (the workers would not be wearing respirators), but they would be a direct-dose hazard as well. The gas bubble itself would also produce some dose. The dose rate would be mostly due to beta particles, but the bubble would pass very close to the unshielded worker's body as it rose and thus the dose rate could be appreciable.

As I noted above, the monitrons would not detect the betas. So they had to be able to detect the gammas, which, looking at all the isotopes that were left after so many years, would nearly all be from the main gas isotope. This, by my calculations, the monitrons could not do with their current setpoints. But the air monitors were not suitable either, even though they could detect betas. This was because they were relatively far from the work area (the center of the bridge) and it was not demonstrated that the ventilation flow path would be from the work area to the air monitors. There was the additional consideration that the gas bubble and particles would be very hot (thermally) and would heat the air they came into contact with; the gas, air, and particles would tend to rise at the same time they were being carried by the ventilation flow. Their path would thus very likely go over and miss the air monitor intakes because the air monitors sat on the floor. All of this made detection by the air monitors quite unlikely. I believe that the DOE safety reviewer failed to appreciate the significance of all this because of the information he was given by RRD. Also, he did not appear to have visited the BSR. I did not know him at the time but came to know him later. He was a very sharp guy and had an excellent understanding of safety issues, so I think he might well have agreed with me if he had been told my concerns about the monitors.

I usually did not communicate with my ORP management about RORC matters, but I thought that RRD's non-cooperation and especially its accepting DOE's statement about the air monitors was serious enough to report. I sent an E-mail memo to Sims, Mlekodaj, and Mei about the difficulties in RORC interactions with the RRD with respect to the BSR project and the problem with the monitors that RRD was refusing to address. I stressed that the monitor problem was a technical issue that ORP should be concerned about. They did not reply. So two weeks later, I sent another E-mail memo to Sims and Mlekodaj (copy to Mei). I explained that the issue was that the monitors were said in the BSR BIO to be relied on in ways that were not valid. I pointed out again that RRD regarded this issue as a non-problem since DOE had not identified it as such and had given a mistaken statement about it. I noted too that many RORC comments were not addressed (even after some others were withdrawn). Mei was the only one who got back to me, although I went to Mlekodaj and explained it all in person. They understood, but could do nothing.

Since RRD had successfully blown off my concerns about the monitor, I tried to think of something that ORP could do to ensure that any release was promptly detected. I thought that electronic personnel dosimeters (EPDs) could perhaps be used: they are little personal monitrons worn on the chest, with dose and dose rate setpoint and alarm options. I wrote a technical basis for their use in this case. I met with an expert in ORNL's Controls & Instrumentation Division and I spoke with Siemens, the EPD vendor, about the response times of EPDs. The result was disappointing: the response time for betas was too slow to detect a bubble rising at a realistic rate. Thus detection via EPDs, monitrons, and air monitors for the assumed release would not be possible in order to avoid the calculated accident dose. But I asked the BSR rad techs to use EPDs anyway and of course the monitors would still all be operating. Mei and I agreed that that was the best that I could do on this.

Some time later, I talked with two RORC members who were not on the RORC BSR subcommittee, one an instrumentation specialist in the Controls and Instrumentation Division and one a reactor reliability and

operations person in a non-RRD division. They said that they would have supported me on the airing of the issue had I brought it up before the full RORC. I was chagrined that I hadn't done so -- which was because Harrington controlled the RORC agenda and I thought there was no point in bringing it up.

Molten Salt Reactor Experiment (MSRE Project)

At some point in 1997, DOE gave a contract for all environmental remediation activities to Bechtel Jacobs, including the MSRE project. Chem Tech (the Chemical Technology Division) continued to plan and carry out the remediation operations, but now it was under contract to Bechtel Jacobs. This was all part of DOE's "spread the wealth" multi-contractor plan, but it made a complicated project even more complicated in terms of the number of spoons stirring the pot.

As I noted earlier, I often went to a regularly scheduled MSRE meeting only to find it cancelled without notice to anyone without an office out at MSRE. I would protest to Richard Faulkner, the facility manager, about the waste of my time, but nothing was done. Once I attended an MSRE portable criticality detector meeting, one of many meetings that I should logically should have been invited to by MSRE management but wasn't. (I was invited by a person in another division who was working on the project.) I feared offending MSRE management by showing up, although because of the way the detector worked and other radiological aspects of the detector placement, this was clearly an issue of interest and relevance to an AEG rep. MSRE management would even hold ES&H-specific meetings and not tell me. The most outrageous example was an ES&H issues review meeting that MSRE project management knew that I expected to attend, but to which, when they finally held it, I was not invited. I protested to Faulkner and Mei, but nothing was done. Mei never seemed to want to speak to MSRE management about any of this.

There were two main MSRE meetings I attended, the weekly planning one and the technical one. More and more often, I would find out at the weekly planning meeting about some new thing with radiological implications that the project was planning to do, or had already done, without telling me. Technical meetings were usually for a specific task or operation and again, as time passed there seemed to be rad-related decision after decision made that I was not told about. There was no requirement that they do so (although the readers should note that at most other sites there would be). But for each operation and each design or modification they potentially faced a rad review by me, the ALARA/rad engineering rep. So keeping me in the loop was advantageous for them as well as for me. As I was given to understand, MSRE project management (Chem Tech) had deadlines and milestones and Chem Tech needed to keep the support the project brought in to them. They thus had a reason to exclude anybody who might hold up the planning and especially the execution of the work and to present any reviewer with virtually a done deal, by delaying providing any information until practically just before a task or operation was to start.

There was little opportunity for me to discuss things in these meetings. They would spend 20 minutes talking about bolts, which most of the people there had no responsibility for or interest in, but they begrudged a few minutes spent on rad protection, which supposedly affected everybody's work. Some of them, especially Faulkner, acted as if they themselves had taken care of the important rad protection aspects and the rad techs would take care of the minor items. When I asked the rad techs what they knew about a task or operation, I often found that they had been told little or nothing. They were more comfortable than I with the "I'll tell you when my plans are firm" attitude from MSRE management. But this again reflected the rad tech approach to work, in that they looked at the work as planned and then selected the controls, while the rad engineer's approach was to participate in planning the work, in which one factor to be considered was the practicality and effectiveness of a potential control. That said, I note that MSRE rad tech John Allred was very questioning and did try to be involved early in the planning.

It often occurred to me in my MSRE work that I got along well with what my "peer engineers", i.e., engineers and other technical specialists who weren't part of management although they might be in charge of individual aspects of a project. For example, I met with Joe Devore of Engineering about some

MSRE calcs of his that I had checked. As I noted earlier, Devore and I had worked on the Advanced Neutron Source and another project. I had found that he did not resent my comments but considered them carefully and spoke up if he disagreed. The give and take, the challenging question and the logically argued answer, that are characteristic of engineering work were part of what might be called the engineering team style. I think it is also characteristic of the research scientist style, where if one scientist says to another, in effect, "Prove it", the second one not only doesn't resent the challenge, he expects it.

This was in contrast to the "push-back" style of many ORNL managers, who practiced what I call the "How dare you doubt me?" technique: they reacted to all but the most minor questions, however politely or factually put, with irritation and indignation. A refinement of this is the "How dare you doubt me, you insignificant little worm?" technique: here the idea is that the questioner owes it to the questionee not to rock the boat because of the importance or high position of the questionee and the low rank of the questioner. In this thinking, a statement of a manager or "customer" trumps a statement of a peon or "service provider" regardless of the qualifications or experience of the latter; the former feels entitled to be catered and deferred to. The peon is in the position of having to figure out a way to persuade the manager or customer that it is in the latter's best interest (especially best financial interest) to do it the peon's way. Or the peon can become pals with the manager/customer and thus gain acceptance of advice on the basis of friendship. Either way, there is always an imbalance so that the peon -- the subject matter expert -- is in a defensive position. How this works in practice is shown by an example from MSRE.

MSRE had a safety and health officer, who I think was "rented" from the Health Sciences Research Division (HSRD). I believe that he tried to involve me in things when he could. In late 1997, he advised me to talk with an MSRE safety analyst (I think also a "rented" person and also cooperative) about gamma spec data recently taken by Ian Gross of Chem Tech. As I said earlier, Mei, Utrera, and I were leery of the way Gross had previously used data he took and the way he selected his measurement points. On this occasion, the gamma spec results were from the uranium plug in the Aux Charcoal (Filter) Bed (ACB). I had to ask Faulkner for this information in order to get to see it, even though as the rad engineering rep I should have been given it as a matter of course.

A few days later, I attended an MSRE meeting to go over results of radon measurements taken by Robert (Bobby) Coleman of HSRD, which was very experienced in taking this type of measurement. Coleman was forthcoming regarding his measured data and what he further proposed to do to study the radon problem in the ACB. He seemed to think that my interest in his data was natural and appropriate, given my role on the project. In fact, from about this time on, Coleman always kept me informed as to what was going on with his MSRE work.

But he too seemed to have some problems with communicating with MSRE management even though one would have thought that as an expert doing actual measurements, he would have been provided with all relevant information. In his data, he saw an unusually high amount of Cs-137 (cesium-137), a fission product. This could indicate that the uranium on the bed had gone critical in the past, or that the spontaneous neutron emitters on the bed were present in greater amounts than thought (producing extra Cs-137 by activation), or that the Cs-137 had migrated there as the uranium had. I did some SCALE (computer) calculations to see if the Cs-137 could be accounted for by spontaneous fissions and alpha-neutron-produced fissions occurring in the bed, but found that it could not -- the two processes were occurring at too low a rate, if we believed MSRE management's estimate of how much uranium was on the bed. Coleman agreed that there seemed to be too much Cs-137 for that. (Neither did we think there had been a criticality on the bed.) But some time later, I heard for the first time at an MSRE meeting that indeed there was already some Cs-137 on the bed and that MSRE management had known this for a long time. I called Coleman to tell him this and he said he had found it out very recently himself from the MSRE people. I don't believe that not telling us earlier was an oversight since the puzzling "extra" Cs-137 had been pointed out to them by Coleman in an earlier meeting and they had said nothing to him then. I

believe that for reasons of their own -- probably so they wouldn't have to contradict something they said to DOE before they had a new story line made up -- they withheld this important information from someone they had hired to study the bed. Because of this, I wondered if there was a release or migration that had previously been unacknowledged or whose extent had been minimized up to then.

In early December 1997, I attended an MSRE meeting on a planned mass spectrometer disassembly operation to be led by Gross. This mass spec, an old instrument with various internal parts that had gotten contaminated in use, had sat unused for many years. The type and extent of the contamination were not known, so it fell under RPP-310's provision covering unusual operations and unknown conditions. Gross was supposed either to write an ALARA plan or to demonstrate by smear samples that the total radioactive content was minimal and the form not unduly hazardous. He also needed an RPP-310 review unless he had good evidence that the hazards were minimal. He strung me along for some time, telling me that he had to plan the tools to use, to do the training, etc., etc. Then an MSRE person told me the job was postponed, so I heard no more about it. But in early April 1998, I found out that the job had been done. I wrote Faulkner, stating that the project failed to have this job reviewed as per RPP-310 and as agreed on previously by MSRE personnel and Gross. Mei spoke with Faulkner too, but I think he put her off with a weak excuse. I must reiterate that Gross had a history of not following rad rules at other Chem Tech facilities; it was of course he of whom Sims complained that he got an "attaboy" award from Chem Tech for work in the course of which four or five Radiological Event Reports were written against him. In that work as in the mass spec job, he got the job done, in effect, by "cheating" with regard to safety rules.

In late 1997, I attended a combination worker training and procedure comment session for a milestone task. At one point, someone referred to it as "the prejob briefing", but fortunately, a project person joined with me to correct that. As I noted earlier, it is not appropriate for the prejob briefing to be combined with any type of meeting during which training or planning is done, because all that should have been completed before the prejob briefing. Also, the Radiation Work Permit (RWP) had not been written yet. The purpose of this session, I was told, was to be mainly for worker training on the procedure, for which there was a signoff sheet. But it was also for obtaining the workers' comments on the procedure, so a followup official training on the final form of the procedures was clearly necessary, although it this was not stated. Finally, there was a considerable amount of technical and other discussion among MSRE management and task leader people that seemed not to have anything to do with the workers' scope of interest -- it was thus another technical discussion meeting as well. The work plan and procedure were obviously still in a state of flux. So this was a catchall meeting in which project management hoped to accomplish several goals, but the goals seemed to some extent to be mutually exclusive.

In January 1998, I met with Elvira Hodges of Chem Tech regarding the RPP-128 design review of the MSRE denaturing operation. Hodges, an engineer who ran some isotopes work in a Chem Tech facility, was also doing some MSRE work. As I said earlier, she always cooperated with us and seemed to "see the point of it all". As we went through the RPP-128 design review checklist, we found that neither of us knew the answers to some of the items. We consulted with the MSRE ventilation engineer and another engineer on several issues. We agreed that we had learned some significant information about the project and about the denaturing operation from doing the review (I as reviewer, Hodges as project responder). Some ORNL people seemed to think that design reviews by rad protection people other than the rad techs were unnecessary, but from examples like this, I felt confirmed in my belief that they were of use. Nothing adverse turned up in this review, but in other instances it had (i.e., not every review will or should result in findings, as Mlekodaj so wisely used to point out).

In early March 1998, I met with Linda Gilpin of the Office of Nuclear Safety (ONS) regarding MSRE drain tank computer modeling. Gilpin was rented from ONS to do shielding and other calculations because she was already doing criticality documents and calculations. She said she was glad to consult with me about the shielding calculations since she felt it best to have such calculations checked even

when the preparer was very adept at such calculations. Once or twice I did find an error in Gilpin's input data, and later she found at least one error in a run I had done. This was quite normal since shielding input is often extensive and complicated. Gilpin did not resent my finding an error since that was the point of my check. She and I were resource persons for each other on MCNP and SCALE calculations. But one point I must emphasize: although she did shielding and dose rate calculations for MSRE, she properly refused to draw any ALARA conclusions or make any rad protection recommendations regarding them because she felt she was not qualified to do so. I respected this, particularly since so many non-ORP people on MSRE and other projects seemed to go in for do-it-yourself rad protection.

Gilpin had replaced Don Mueller of ONS as the criticality safety rep to MSRE. Unbeknownst to me at the time but as he told me years later, Mueller had been effectively removed from the MSRE project. It seems that MSRE managers wanted to do a certain job immediately. Under their interpretation of a approved criticality document he had written, they could do the job without changing the document, but under his interpretation, the document would have to be revised first. MSRE management did not want to take the time for revision. The peer reviewer on the document was consulted and he agreed with Mueller's interpretation; Mueller's section head, Mincey, also backed Mueller. So MSRE management went to the ONS division director, Mark Kohring, who in effect agreed to allow MSRE to interpret the document the way they wished. It was then clear that to Mueller that he could not continue on MSRE-- they would not want him because they viewed him as uncooperative and he did not want to work on a project that exerted pressure on his management to overrule him in his area of expertise (Kohring was not a criticality specialist). So Gilpin was assigned to the project in his stead. Mueller also told me that he had on an earlier occasion received a call from a concerned person at MSRE, who told him that an operation was going to be performed that was in violation of the criticality documentation. Mueller went out to MSRE and verified that it was so, then spoke with MSRE management and his management. I believe that on that occasion MSRE management backed down, but obviously they were not inclined to have Mueller continue to hold things up, as they viewed it.

Sometime in early 1998, I met with some Robotics Division people who were working on a "ball port" for use with the tanks in which the uranium salts were stored. This was basically a ball that had a hole ("port") in it through which a tool could be put; the ball was set in an opening in the floor over the tanks. Thus access could be gained to the top of a tank with a tool that could swivel, using a small port instead of a big floor opening up through which radiation could stream. (Think of a straw stuck through an apple, with the straw as the tool and the apple as the "ball".) I pointed out some ergonomic and radiological difficulties and some desirable characteristics of such a port. I also noted that a dose calculation needed to be done for the design of the shield and the port diameter. This ball port was obviously a design measure intended to reduce dose and an engineered feature. As such, it was reviewable under RPP-128 (design review). Operations using the ball port were also reviewable under RPP-310. I never was asked to meet with Robotics again and to my knowledge the design was never reviewed by AEG.

In June 1998, the MSRE safety and health officer, Steve Burman, called to ask how the consequences of a potential release of gas to the environment might be calculated. I explained how to approach it. Trying to be helpful, I said that while I could do it by hand, it could be done faster and more accurately by Frank O'Donnell of the Office of Environmental Protection (OEP), who I knew did release calculations all the time using appropriate computer programs. I pointed out that even if I did the calculation, MSRE would still have to satisfy OEP, so having O'Donnell do it would be most efficient. I told Burman I was leaving the decision up to him. A day later, Faulkner called Mei to complain that I had "refused" to do the calculation and had failed to support the project. I was flabbergasted. I didn't think that I had left Burman with that impression at all. I assured Mei that I had been willing to do the calculation. With Mei's concurrence, I sent Faulkner a memo relating my conversation with Burman and assuring him of my willingness to support the project. Faulkner never replied to this memo and did not refer to the matter the next time he saw me. Later, I spoke with Burman, who sounded embarrassed and regretful about the

whole thing. When I asked him if he had thought I was blowing him off, he was evasive and did not say that he thought that I had refused to do the calculation. I didn't know if Faulkner had misunderstood Burman or what, but since Burman was normally a nice and straightforward guy, I concluded that it must have been Faulkner who blew this matter all out of proportion due to a misunderstanding.

In late August, I went to an MSRE meeting at which gamma spec data from the Aux Charcoal Bed (ACB) was tabulated on a handout. I trusted the data because it was taken by the Controls & Instrumentation Division's Mark Buckner, a capable professional with a good reputation. The results of some calculations by Faulkner, using the data, also appeared on the handout. I was surprised that Faulkner had done the calculations himself, due to his heavy administrative duties, but I thought that his calculations would have been checked and would prove to be correct. However, examining the data, I found that while Buckner's data looked okay, there was an error in Faulkner's calculations, specifically the interpolations from which he determined the amount of uranium deposited on each segment of the bed. This was a crucial point, since the contaminated portion of the charcoal was to be removed from the bed later and it was essential to determine how much had to be removed. It was also crucial because MSRE management seemed to want very much to find that there was not enough U-233 on the bed ever to have formed a critical mass.

I wrote Faulkner a memo (copy to Mei) pointing out the error. I tried to be professional but courteous, but of course there was no getting around the fact that I was telling him that he had made a mistake. Subsequently, I met with him at his request. I showed him a corrected table and graph, with the proper weighting based on the linear length of the bed segment. My calculations showed that significant deposition of radionuclides went farther down the bed than his tabulation showed, i.e., my version was bad news in terms of how much charcoal had to be taken out and in terms of how much uranium might potentially be on the bed. Faulkner did not seem to take offense as I explained my work -- he seemed calm and matter of fact. However, as we finished he segued to another topic without any transition. He stated that he liked for people to "take ownership" of things they saw that were wrong or were going undone on the project; to take care of these things themselves; and then to report the resolution or completion of them to project management, without bothering management with them in the meantime. He made no specific reference to me or any part of my work. So his statement seemed to come out of the blue, to be a non sequitur, and I wondered why he had said it -- was he trying to tell me something or was he just talking? I also thought it did not make sense with respect to the calculation error issue: since I was not in charge of calculations of, e.g., how much uranium was on the bed, since I hadn't been consulted about how they were to be done, and since I hadn't even known that MSRE management had had Buckner taking the measurements until they were almost done, I couldn't have "fixed the problem and presented management with the solution". In fact, I thought I had done all I could along those lines. I told Mei all this, but she must not have thought it meant anything either because the only advice she gave was to keep trying to do my best.

In mid-October 1998, I pointed out to Rob Szozda, an MSRE task leader, that he had not told me about a meeting to review more mass spec data taken by Buckner and evaluated by Dr. Larry Miller, a University of Tennessee professor. Again, these data were to be used to determine the ACB radionuclide content and distribution. I believed that I had been excluded to keep me from poking around and contradicting the project's official line; I did not say so to Szozda, but did express concern to Mei and Mlekodaj.

At this time I discovered that the project had not only designed its own storage cask to hold the contaminated charcoal when it was removed from the ACB, but had sent the cask specification out for bids. I found out only because an engineer sent me the bid spec for information. (I think he did so in innocence, thinking that I had a need to know, and so he did not think to ask MSRE management if he should.) I sent memos to Faulkner and Szozda giving comments on the spec and expressing my concerns regarding this home-designed cask. I noted that although MSRE management stated that the reason for designing and using its own cask was cost, no cost-benefit analysis or dose calculation was done for this

rad-protection-affecting measure as was required by RPP-129 (optimization) and 10 CFR 835. Gilpin had done the dose rate calculations, but she had not chosen the final thickness -- the project people seemed to have selected it themselves arbitrarily or based on some (as far as I could find out) unwritten criteria. I noted that this was clearly a dose-related decision and should have been reviewed by me under RPP-128 (design review) before being sent for bid.

Examples of the problems with the cask as they had designed it were the penetrations, the specification of the concrete density, and the placement of the cask. First, the penetrations (holes to allow tubing, etc., to run in and out of the cask) could be a streaming path for gammas and especially neutrons, but they were not analyzed to see what the dose rates might be. Second, the bid spec did not include a minimum density for the concrete. This was important because while, e.g., steel and aluminum densities do not vary very much, even ordinary concrete density can be quite variable. Since in doing the shielding calculations Gilpin had assumed a certain density, it was important that this density be specified as the minimum acceptable density in the finished product, if the calculated shielding efficiency was to be achieved.

Third, the cask was supposed to sit for some lengthy, indefinite period in the MSRE bay when filled with the radioactive ACB charcoal. But this could create a large "hot spot" in the bay that would make for significant background dose rates everywhere. "Significant" would at worst mean that the people working in the bay a lot would receive a significant additional dose. I tried to talk with the project people about this, noting that a greater thickness of the cask might be necessary. Their answer was that was that the floor might not be able to support more weight and there "wasn't room for a bigger cask". I asked about supplemental shielding to be used if the dose rates did turn out to be high. Faulkner's impatient and careless answer was that they could use some of the large concrete shielding blocks that were a legacy of the operating years. I pointed out that these were so big that they were even more of a floor loading problem than the cask and more of a space problem than another few inches of diameter on the cask would be. I also noted that they couldn't be stacked in an efficient manner (e.g., you couldn't use half a block or a fourth of a block, but had to use the whole thing). Faulkner's answers did not make sense, especially as nobody brought out any data or sketches to support his contentions.

Even in the best case, however, it appeared that the dose rates in the current low-background areas would probably rise significantly. Rad tech Allred and I were concerned about clearing materials in the bay (being able to release them as uncontaminated): if the background were too high, fixed contamination could not be assessed and materials would have to be moved to the clean area outside the bay to be surveyed. Also, the hand-and-foot monitors just outside the door of the bay might have to be moved significantly further away from the bay if the background count rate at their location went up to an intolerable level. A lot of work had to go on in the bay after the charcoal removal, so it seemed inefficient to store the cask there. There were security considerations related to the U-233 content, but there seemed to be other options that could be considered, e.g., putting the cask in its own little outbuilding on the MSRE site. I tried to query the project as to other options, as was my job as an ALARA person (i.e., to find the optimal solution you have to know what all the practical solutions are). But I was brushed off.

At the end of October 1998, I attended a meeting at which Buckner and Miller presented the final gamma spec results and analysis. I had been acquainted with Miller for some years; I hadn't known Buckner, but we had heard of each other. Both of them were cordial and professional at this meeting when I asked questions, which they appeared to regard as germane. However, Dr. Jim Rushton, the MSRE project manager, seemed impatient with my questions and irritated that I was there. He prided himself on his rad expertise; as I stated earlier, he had shouted at me when I offered a different opinion at an MSRE meeting two years before. So I did not push at this gamma spec meeting to have all my questions answered, but desisted when Rushton began to show impatience.

In early 1998, I had produced a technical basis document and procedural basis for the use of a new MSRE hood for recycling uranium traps. I went over them with Roger Aigner, a savvy engineering aide, who had been rented to Chem Tech by his own division. He then wrote a procedure referencing my documents and used it to do work. So, the reader should note, the technical basis and procedural basis that I found acceptable were adopted by the MSRE project as the basis. But in November 1998, I found that MSRE had arranged with Building 3019, a Chem Tech facility at which U-233 was stored, to use a 3019 hood for recycling traps. (I do not know why they chose not to continue to use the hood at MSRE; they were evasive when I asked.) At 3019, they were using a hood procedure essentially the same as the one for the MSRE hood -- except that a limit for one radionuclide had been raised significantly. Since this limit was set in my technical basis as a result of calculations done there and since my technical basis was still referenced in the new procedure, I naturally wondered why and on what basis the limit had been changed. I sent a memo to MSRE task leader Bob Kite (copy to Faulkner and Mei), expressing my concerns about the new procedure and pointing out that although my technical basis was in the 3019 procedure's reference list, the 3019 procedure was not covered by the technical basis because the increased limit was not consistent with the basis document.

I forwarded this memo to 3019 rad tech complex leader Nancy Sweat and her group leader Bryce Power, stating in a cover note that by ORNL rad protection procedures every hood was required to have posted activity limits. I asked if the 3019 hood's limits allowed for the work described in the new procedure. Later I spoke to Sweat by telephone. She seemed unaware of the requirement to have hood limits. She told me that she didn't know what the 3019 hood's existing limits were and so did not check them when she reviewed the new procedure. I observed that it seemed likely that the new procedure contradicted the technical basis and further, that it was not clear what the MSRE people had told the 3019 people about the use of the hood. Sweat became angry and said that the 3019 facility people "must know what they're doing". I stated that it was not clear that 3019 people themselves would be doing the work, that they seemed to be allowing MSRE people to come into their facility and "borrow" the hood for MSRE work. But the responsibility for any incidents would fall on the 3019 people. Sweat became really hostile and so I tried to placate her by being conciliatory. She would have none of it. Later I spoke with Power (her supervisor) in person about the seemingly out-of-proportion response by Sweat. He appeared to understand (from past experience with her) exactly what I was talking about: he sighed and stated "Well, that's Nancy [Sweat]". He did not dispute any of my points about the limits issue, but said only that if the 3019 and MSRE people were in agreement about using the hood, he would not go against them or look into the limits question. He seemed resigned about this rather than defensive.

In early November 1998, I asked Kite by E-mail about the shielding cask. I found his answers to be vague and evasive and I informed Mei. A week later, I spoke with Faulkner by phone about my concerns. He did not seem to think my questions were unreasonable; in fact, he asked me to go ahead and do the penetration streaming calculation. He told me that there was a milestone associated with the job the cask was to be used for, but he didn't emphasize this in any way. However, the next day he sent me a querulous E-mail message in which he expressed resentment at having to address "again" my previous written questions (to Kite) at 7:00 in the evening -- implying that my questions were keeping him from going home and that I should somehow have known this. I was taken aback, since his comments seemed to come out of left field: if he had a problem, why hadn't he told me when we talked by phone the previous day? But he did direct Kite to put a minimum concrete density in the bid spec (as an add-on, since the spec had already gone out). I started the penetration analysis, which was extensive since both gammas and neutrons had to be done (separate runs in the MCNP computer program). I also sent an E-mail message to Mei and Mlekodaj on my talk with Faulkner and on his cranky E-mail memo. I explained why I asked the questions I did and defended them as being technically necessary.

Then Kite sent me a memo replying to the issues I had raised regarding the hood and the cask. Kite, who in our many interactions over the course of the project was formerly very affable and willing to provide

information to me, took an unaccustomed "push-back" tone in this memo. I forwarded it to Mei and Mlekodaj, with a cover note about problems with the cask. On 19 November 1998, I again wrote Kite (copy to Faulkner and Mei) about the 3019 hood procedure. After that, I gave up on the hood procedure, based on Mei's and Mlekodaj's unwillingness to insist on verification of the basis for its use and thus to find out whether the rad protection procedures were being violated. The workers did indeed turn out to be MSRE people, not 3019 people, so this "loaner" situation presumably provided a source of income to 3019. I was never able to find out who had authorized the higher limits and why, but it must have been high up in Chem Tech (section head or project manager) for both facilities to have gotten on board. The MSRE people made a slight change to the procedure: they removed my technical basis as a reference. The bottom line, however, was that there was no check by ORP as to whether the 3019 hood's new use was consistent with its previous authorization or with any technical basis for its new use. (The reader will recall that after a certain point, the rad protection procedures were changed so as to imply that once a hood had been authorized to be used, only the division rad control officer needed to review any changes in the use of the hood, not ORP.) Due to his previous interactions with Chem Tech, Geber commiserated with me about the discouraging lack of response and support from our management regarding the hood concerns and how MSRE management thus was able to stonewall us so successfully in this matter.

On Friday afternoon, 4 December 1998, I attended a demonstration at a building run by the Robotics Division, who were doing some contract MSRE work. Bechtel Jacobs reps, DOE people, etc., were there also. The operators (Robotics personnel) showed how they would use a robot-operated drill to penetrate a line connected to the ACB. The demo went slowly: the preparation and practice had not proceeded far enough for all the bugs to have been worked out. This was surprising since the operation was supposed to be performed the next Monday or Tuesday. It was found, e.g., that the drill was slipping in the robot arm, which should have shown up the first time they tested it (pre-demo). A commitment was made to fix the problem before the actual operation, but then the project people said (I assume after a side consultation with DOE) that another demo needed to be done. So the operation was put off a few days.

On 8 December 1998, I talked with Allred, the lead rad tech for this Robotics uranium removal operation. We talked over some innovative contamination control measures he had come up with and agreed that we had reservations about the plan for the drilling operation. Allred said he would do everything he could to ensure containment of contamination and easy cleanup if a release did occur. At a removal operation meeting some weeks earlier, I had asked about the possibility of a burst of material out the hole as the drill penetrated into the closed filter air space. I was assured by the project management that this was not a possibility: the contents would not be under pressure -- if anything, there would be air flow down (in) through the hole. So the project people were reluctant to consider any possibility of a release.

On about this day I also talked with Szozda. I had some questions related to the RPP-310 review and had had a hard time getting Szozda to return my calls (he was famous for not returning calls from anybody). We finally met, but it was a meeting of the "I can give you ten minutes" variety on his part. We did cover one important area: contamination control. Szozda assured me that they were going to adopt Allred's suggested measures. I stated that the penetrations into the shell building over the ACB pit needed to be sealed, just in case. This was very important because if they were not sealed and there was a release, contamination might get outside. That would be considered a release to the environment, which DOE always took seriously. Szozda said that they would seal the penetrations, or would at least "do the best we can", and I indicated that I took that to mean the penetrations would each be entirely or mostly sealed.

On 10 December 1998, a Thursday, Mei informed me in person that Faulkner and Rushton wanted me off the project, effective immediately. She also said that Brad Patton of REDC wanted me off all REDC work. I was stunned and asked why. Mei told me that she didn't know exactly but she had been ordered to take me off; she said something vague about their finding me hard to work with. A little later I spoke with both Mlekodaj and Mei, who said that apparently Rushton and Faulkner spoke with Gene McNeese, head

of Chem Tech, perhaps with Patton present, and the decision was made that they wanted me to be taken off all my Chem Tech work, including REDC. McNeese apparently then spoke with Sims. Sims did not consult with me or Mei or (I understood Mlekodaj to say) even Mlekodaj before agreeing. Mei and Mlekodaj told me explicitly that they themselves did not want to do it, but Sims ordered them to. Mei admitted, when I asked her, that Faulkner and Rushton had not asked to meet with me or Mei, or both of us, about any problems. Mei stated that in fact, she thought I had been doing a good job. (Of course, I had kept her informed all the time and had copied her on all project memos, so she knew everything that had been going on.) She also said that "for now", she would be the AEG rep to MSRE, although as she said she had warned the project, she would not be able to devote much time to it because of her other duties.

On the very next day, 11 December 1998, a Friday, the drilling operation in the ACB began in the afternoon, I think after 2:00 p.m. I believe that I am on firm ground in saying that there are not many operational groups in the DOE complex who would begin a major operation so late on a Friday, with one of the operators' wives expecting a baby any time, and with rain expected all evening. But the MSRE folks forged ahead -- they had a milestone to meet. At first, the operation proceeded more or less as planned: the robot advanced to the hole in the floor of the shell building, put the drill into place, and began to drill. The operators who controlled the robot watched the action remotely on a television screen. Suddenly on the screen they could see a big puff of white "smoke". The operation was stopped.

After a while, during which I believe they checked various readings and talked over what to do, Allred cracked open the door to the shell building and carefully took some contamination smears. He told me later that he had had to dry them out before he could put them into the smear counter; they had gotten wet from the rain as he took them back out through the door of the shell building. Some time during this evening, I think before the release, the operator with the pregnant wife got a call: she had gone into labor. So he had had to leave. People talking about this later seemed not to think this was a big deal, since there were two other operators, but it surely did not help the situation. Allred told me that he did not get to leave MSRE until about 1:00 a.m. -- and then had to get up at 6:00 a.m. for a family commitment.

It was clear that there had been a release of a uranium-fluorine compound -- the white "smoke" suggested that even before the smears confirmed it. There was widespread contamination all over the floor of the shell building and part of the way up the wall. Smears taken the next day inside the penetrations -- by another one of the rad techs up a ladder against the outside of the shell building -- indicated some contamination, but this was ascribed to natural "radon daughters". (I thought the explanation of that decision was lacking in detail since it seemed odd that the daughters would somehow have collected in that spot.) The rad tech told me that the penetrations were not sealed. I do not know at what level this incident was reported, but it did not seem to occasion any stir at all at (local) DOE-ORO.

On 14 December 1998, I sent Mei and Mlekodaj a memo about this MSRE filter-drilling job incident. I emphasized that Allred and I had had concerns about just such a release (although we hadn't specified a release mechanism, of course). I pointed out that the penetrations that Szozda had said would be sealed as much as possible were not sealed at all. Later that day, I got more information and sent Mei and Mlekodaj an update. They were thus well aware that one day after I was removed from the project, an incident of a type I was concerned about occurred. I felt vindicated. But I was not restored to working on the project.

I attended one more MSRE meeting, on 15 December 1998 with Mei, to hand off to her and say goodbye to others on the project. People said nice things to me, notably including the rad techs (who had already heard about my removal) and Gilpin (who had not). Complex leader Jo Ellen Francis expressed sympathy and said that her supervisor, group leader Jerry Gray, was angry on my behalf. (Thanks, guys!)

On 16 December 1998, I sent Sims a memo with the details of the drilling job. I urged that Allred be kept on the project due to his intelligence and conscientiousness and due to MSRE's being a safety-averse

project. I noted that since Allred was actually in HSRD, not in ORP, he was more vulnerable to layoff than ORP techs. I felt that the last thing I could do to protect MSRE workers was to try to persuade my management to keep as the lead tech on this critical project someone with his eyes and ears open.

Shortly after my removal, I put a note in an extra name plate slot on my door: it read "Recovering Messenger". This was of course a reference to the saying "Don't shoot the messenger" that is frequently quoted in safety circles. I felt that I had been "shot", but I was trying to be optimistic that I would eventually get over it. Mei, whose office was across the hall from mine, and Mlekodaj, who had to pass by my office to get to the bathroom, saw the sign but did not tell me to remove it. I believe it was because they knew my removal was unjust and political and I needed to protest it in a low-key way.

Because the rad techs all knew right away that I had been taken off the MSRE project at the behest of MSRE management, I assumed that everybody else on the project knew it as well. So I was puzzled that for some time MSRE project announcements were still sent to me. Finally, I wrote a note to the engineer who was the administrative hub to remind her that I was no longer on the project. She replied that she would take me off the distribution list. She added a nice comment about it being a shame I was no longer working on MSRE. But it was worded in a way that implied that she thought that I had been taken off for financial reasons -- that they couldn't afford an AEG rep any more. I wrote back, asking if she really thought that and did not realize that I had been taken off at the request of her project management. She replied that she had indeed been told that it was for financial reasons. I realized -- and presumably she too realized at that point -- that her management had lied to her about the reason for my removal. I think that others on the project were also given the false explanation. Months later, I had occasion to speak to another person on the project on another subject. He alluded to my removal and for the first time I heard anger and exasperation in this mild-mannered person's voice. It was not directed at me; rather, he gave me to understand that he thought the way I had been treated was unfair to me and adverse to safety.

I think that my tenure on the MSRE project and my subsequent removal should be taken as a lesson by safety people everywhere. I do not have space here to discuss all the different MSRE jobs and operations I worked on -- if I did, it would illuminate the deficiencies even more. However, it should be noted that at the beginning of the project, task leaders and others in charge of operations generally cooperated with the rad reviews, but by the end they were seeking to avoid them; milestones did not seem to loom too large at the beginning of my tenure, but by the end, corners were being cut in various ways in order to meet them at all costs. Also, I was the third ES&H person removed in about as many years (the others were a rad tech and a criticality safety specialist, as I noted earlier). It was clear that all the rented folks, even more than the "institutional" folks like us three ES&H people, would have to please their customer, Chem Tech, or Chem Tech would get them out. ES&H management would go along, as Kohring's and Sims' knuckling under in the face of clear-cut technical and compliance issues showed. Their pusillanimity was demoralizing and troubling, but it was also revealing of their priorities. It was clear that ES&H management would not support its people if they tried to insist that the ES&H rules, as embodied in the procedures, be followed. It was also clear that many technical issues would be swept under the rug -- my comments on the cask, for example, were simply dropped after I was removed from the project and the required reviews were never done. Thus there would be no professional health physics review of the various MSRE operations, except for what little Mei was allowed to provide, even though most of these operations were novel and the conditions were often uncertain.

I truly believe that I was removed because I was pointing out that decisions were being made and conclusions reached because they were convenient, not because they were consistent with good engineering practice (and especially good rad engineering practice) and not because they were true. This is particularly so with respect to my questioning of the amount of uranium on the ACB and my efforts to have required reviews done (as for the cask) and agreed-on protocols followed (as for the hood). Chem Tech's stated objections to me personally were just a smokescreen. I think that my comments might have

been coming to the attention of the Bechtel Jacobs people (under contract to whom Chem Tech ran these operations) or even DOE, or perhaps Chem Tech feared that they would; Bechtel Jacobs oversight people came to project meetings from time to time and once in a great while the DOE rep was at a meeting. If either Bechtel Jacobs or DOE started an inquiry into these matters, it might have slowed down the project and caused a milestone to be missed; it might even have resulted in those responsible being penalized. Even worse, if the official project story of the day about, e.g., how much uranium there was on the ACB or about how great the risk was of this or that operation was contradicted by my findings or comments, Bechtel Jacobs or DOE might think that Chem Tech project management was not as on top of things as they professed to be. I think that this supposition matches the evidence much more than the allegation that I was "difficult to work with" since MSRE management did not try to resolve the alleged problems with Mei and me, but went immediately and directly above our heads. My argument is bolstered by my simultaneous removal from REDC work, for which no reasonable explanation was ever given.

However, I will note here two things that happened years later. First, I had heard while I worked on the project that there were strong indications even in the 1980's that conditions were not stable at the supposedly stable MSRE. This is not something that I can state here as being certain, although it would account for some dubious statements by project management about the time at which the valve failure occurred and deposits began to accumulate. While some activists' claims of unbridled criticality and life-threatening doses are not credible, I believe that the true story of MSRE would indeed reveal that a lot of things were glossed over and covered up. Because of the many statements over the years (and especially before the migration was detected) by a variety of people about how ORNL wanted to clean MSRE up but DOE refused to fund it, I believe that DOE is mostly to blame for the MSRE situation.

Second, at some point in 1998, Bechtel Jacobs decided to save money by running MSRE themselves. So they dismissed Chem Tech, except for some consulting and special task work. An experienced non-ORNL rad protection professional I know became the rad engineer at MSRE and worked on various aspects of the operations, including the use of the ball port valve. I found this out in 2001, long after I was laid off, when he gave a talk about his MSRE work to our local chapter of the Health Physics Society. Of four or five of the various operations or tasks he spoke of, I exclaimed to myself, "That's just what I said they'd need to do!" That is, as he discussed the planning and/or results of various operations, I saw that what he said were the control and mitigation measures that he had recommended and that were provided when Bechtel Jacobs ran MSRE were just what I had told the MSRE project they would need back when Chem Tech ran it. I felt vindicated, but also frustrated because the vindication wasn't public. I also felt frustrated because the way this rad engineer described his work with the operations people was "the way it's supposed to be" for a rad engineer: he admitted that he'd "had to persuade them to do" some things, but they did talk with him routinely and they did respect his expertise. I think that the principal difference between my situation and his was that Chem Tech wanted to control rad protection decision making and thus viewed my comments as those of a troublemaking interloper, while Bechtel Jacobs acknowledged the role and the expertise of their rad engineer. (The reader can find a photo of MSRE people using a long-handled tool down what I assume is a ball port in The Oak Ridger (newspaper), 8 April 2003.)

Of interest too at this chapter meeting was the matter-of-fact comment by a Bechtel Jacobs audience member regarding how much uranium had been found on the charcoal removed from the ACB. Although ORNL and in particular Chem Tech had contended for years that there was not enough U-233 to have constituted a critical mass, this person said that there was and he stated the amount. Of course there is more to going critical than just having the minimum amount of uranium, but this significant point did confirm my feeling that Chem Tech had been trying to control the "spin" when they were running MSRE.

HFIR

In January-February 1997, I did three RRP-310 reviews for HFIR, the last ones under the old RPP-310. One HFIR project that would require an ALARA Plan and an RPP-310 review was the "hydro" in-service

inspection (ISI) test. Joe Inger, the HFIR engineer who was my contact, was cooperative. We had both been through this several years before and although some changes had been made, the basic process would be the same. The test consisted of pressurizing the reactor and its coolant system for a short time and checking for leaks by direct visual inspection or remote visual inspection. The direct inspection would involve a large corps of people and could result in a potentially significant collective dose.

On 5 March 1998, I observed the HFIR ISI dry run. This was a time-limited operation in which the corps of people moved rapidly into designated piping areas and checked the welds visually for leaks. A few minutes before the operation started, I noticed that most of the people who were to enter the main pipe tunnel had their dosimeters (TLDs) strapped to their thighs, not on their chests as usual. The HFIR rad tech complex leader, Roger Davis, told me that "we" decided that the thigh would receive the most dose since the people would be up on ladders with the sides of their thighs next to the pipes. (By "we", I assumed that he meant himself and the task leaders and probably Dale Perkins, the HFIR-REDC rad tech group leader.) However, I saw during the dry run that the thigh was not the high-dose area for at least some people. This included most notably HFIR's Don Abercrombie, who actually hugged a large pipe to the side of his chest as he strained to observe a weld. I reported this to Davis, but he declined to change the TLD location for the actual run, even though not having the TLD at the demonstrated probable high-dose location meant that it would register a dose lower than was actually received by the body. I had no standing to insist, because by procedure the rad techs dictated the choice of TLD position and I knew I would not be supported by my management if I raised a fuss. I was trying to be a good soldier and not make trouble with the rad tech section, RSS, because we were trying to mend fences after our loss in the RPP-310 battle. I did tell Mei about this, of course.

On 28 April 1998, Mei called me into her office and told me that she and Mlekodaj had met with Perkins and Larry Merryman. She said that Merryman had "requested" that Rich Utrera be assigned instead of me as the rad engineer supporting the upcoming major beryllium outage at HFIR instead. I was shocked and asked why. Obviously casting about for tactful reasons she could give me as to why RRD wanted Utrera, Mei stated that "they know him over there".

When I pressed her further, she said "maybe they think" that I couldn't physically handle the job. This was not just an implication -- she was explicit about this. The reader should understand that I had polio as a tot and I have a visible limp. Besides that, I am very small, 4'-9" and about 85 pounds at that time. I can't run fast and I can't lug heavy things long distances. Because when sitting I can't move my leg very fast to the brake, I use a hand control on my car. I had a permit to take my car into the ORNL site (where private cars are prohibited except by permit, for security reasons), but that was mostly because I couldn't drive the company vehicles, which lacked hand controls, and as my supervisors agreed, it wasn't fair for me to have to trek out to a distant parking lot to my own car every time I had to tote a box of training materials or attend a meeting. But on the other hand I did not have a Tennessee handicapped permit because I did not think I needed one. I climbed two flights of stairs to my office every time I came to my building at ORNL and in 1996 I had completed a trek on foot through a Costa Rican rain forest with my child's classmates and their families. Back when I worked in the power plant world, there were several occasions when I visited our client power plants and had to climb stairs and walk all over work areas; once I had to scale a ladder to the overhead area of a turbine. So clearly I could go take measurements of dose rates and distances, go eyeball work situations, and even go up and down ladders when necessary for my work.

There had not been any occasion so far when I hadn't been able to get to an area I needed to, but then since we rad engineers were so shut out of operations by RSS, we didn't get to access many problematic work areas anyway. Besides, as any lawyer could have told my supervision, it was not legally correct for my management and others to make any assumptions about what I could do without consulting with me. So when Mei said that in the meeting with Merryman and Perkins, someone had indicated that physical limitations were a reason I might have trouble doing the job, I replied that I knew what operations were to

be done from my RORC work and that there did not seem to be any anticipated outage job that I could not physically do. I asked who, exactly, had said this. Mei avoided answering that.

She then told me that this work would likely be outsourced by RRD if Utrera were not assigned as RRD desired. She noted that the support was to be charged out at 50% of a person, a significant level of support for AEG. She said that Geber was "too busy" to take it on and so was she, and in any case RRD wanted Utrera, i.e., had asked for him by name. I protested the assignment of Utrera: it was unfair because I had not been told of any deficiencies in my performance, either by RRD or by Mei, and it set a bad precedent regarding O&R division pressure on the safety organizations. I asked again why RRD didn't want me to be the support person. Mei told me that "they" (RRD) did not want me, but she could not or would not say which RRD person in particular objected to me or why, other than to suggest that I was regarded as "too thorough" or "too picky" and "might take too long" to do reviews. I pointed out to her my record of quick response to RRD and suggested that it was my RORC work, specifically on the BSR fuel removal review, that was the real issue. She did not comment on that statement at all. She told me, however, that I would still be doing the RPP-310 and RPP-128 reviews for HFIR (as the independent reviewer), that I would review the ALARA Plan, and that I would be the "backup" support person on the outage. She stressed that Utrera was to keep me apprised of what was going on so that I would be prepared to do the reviews.

I sent a protest memo to Sims, copy to Mlekodaj and Mei. I observed that this proposal had not been discussed with me in advance even though I had been the ALARA rep to HFIR since I came to ORNL in 1989. I pointed out that this agreement was a professional slap to me since it cut me out of participating in the planning of a radiologically challenging project and embarrassed me within my own division. I pointed out that the reasons given by RRD for excluding me were specious and I asserted forcefully that this was a cave-in to O&R pressure that set a bad precedent for safety people. To the best of my recollection, Sims never replied to this memo. In response, Mei tried to comfort me by again assuring me that I would still be doing the RPP-310 and RPP-128 reviews, etc., and Utrera would keep me informed.

Note that Utrera had no reactor experience prior to this assignment, except for helping set up the electronic personnel dosimeter (EPD) system -- that was how "they" knew him at HFIR. But the EPD work did not require any real knowledge of the operations being done at HFIR, of the HFIR operating areas, etc. The "they" who knew him were not the operational people, but the rad techs who issued the EPDs. Perkins was in fact a pal of Utrera's and as will be seen in the "REDC" section below, had tried some months earlier (prior to my removal from MSRE and REDC work) to get Utrera assigned to do an RPP-310 review for REDC, a facility of which Utrera also had no knowledge. From things Mei and Mlekodaj said, it was clear that Perkins was instrumental in getting Utrera assigned to HFIR.

To smooth things out and to "introduce" me as the backup to Utrera, Mei arranged for her and me to meet with RRD's Merryman on 4 May 1998. I asked Merryman why he felt that Utrera would be a better support person than I -- had he worked with Utrera before? He stated that he did not know Utrera and had never worked with him. He agreed that he had never worked directly with me either, although we had seen each other at meetings of the canceled Advanced Neutron Source project. He said that "others" at HFIR had recommended Utrera and had told Merryman that I might be "difficult" and my work would be too detailed for the busy beryllium outage. In answer to my direct question, he admitted that one of the "others" was Roger Stover (see the Bulk Shielding Reactor section above). I asked Merryman if, during the Mei-Mlekodaj-Merryman-Perkins meeting, there had been any mention of physical limitations on my part; he denied it. I then asked him if there was any place a rad engineer would have to get to or any task he would have to do that Merryman thought I could not physically get to or do. He couldn't think of any.

On 6 May 1998, I talked about the Utrera HFIR assignment with Mlekodaj. I asserted that it was not fair and would set a bad precedent. I related what Merryman had said, noting particularly Merryman's acknowledgment of Stover's involvement, and I reminded Mlekodaj of how Stover had handled the BSR

issues. I emphasized to Mlekodaj that Merryman could not think of any anticipated job that I could not physically do. Then I asked Mlekodaj explicitly if the question of physical limitations had been raised at the Mledodaj-Mei-Perkins-Merryman meeting. He replied that it "came up". I asked if it was just mentioned or if it was actually discussed. After thinking momentarily, he replied that "someone" mentioned it and they all contemplated that momentarily, then moved on. No one, apparently, disputed that there might be physical limitations. I asked him why RRD did not want me. Mlekodaj said that they thought I would be unnecessarily thorough in my work at a time when speed was important. I replied that I certainly understood, from RRD briefings of RORC, why the outage needed to proceed without undue delay. But I disputed that RRD had any reason to think I would not be prompt in doing my work. I noted that based on past evidence, their real concern was that I might point out something they should be doing but didn't want to do. I reiterated that it was likely my RORC work, specifically the BSR fuel removal review, that was the real issue. Mlekodaj said the decision was final, implying that it was made higher up than he (i.e., by Sims). He emphasized repeatedly the importance of the financial aspect, stating as Mei had that the work might be outsourced if not given to Utrera as RRD requested.

The next day I consulted Leila Sutherland, ORP's human resources person, about the physical limitation issue, explaining to her that both Mei and Mlekodaj had said that the question came up in the course of discussing assigning another person to work that I would normally be doing. I stated that I did not want to be ruled out of work by others' perceptions of what I could or could not do; I suggested that for them to rule me out of the rad engineer role on this basis could be considered discriminatory. I told her that I wanted ORP to tell RRD in writing that in the future I should be consulted regarding any questioned tasks and I would say if I was able to do them or not. I did not ask her to have me put in and Utrera taken out of the job; I dealt solely with the physical limitation aspect and future work.

Sutherland spoke with Mei and Mlekodaj about this. Each of them then sent a message to Sims and to me in which they denied that there had been any mention of physical limitations in their meeting with Perkins and Merryman. I was surprised and angry at Mei's and Mlekodaj's denials -- after all, they had each said so to me clearly and explicitly. I couldn't recall, in all the years I had known and worked with Mei and Mlekodaj, ever catching them squarely in a lie like this. Mei could be evasive if she felt she couldn't or shouldn't tell us all that she knew about something; Mlekodaj too might sometimes tell the truth but not the whole truth. But this? I realized that there must be much more going on here than I knew for these two usually straight shooters to act this way. So I dropped the matter, out of respect for Mei and Mlekodaj. I could keep stirring the pot, but that would just bring trouble down on them, without changing anything.

I then asked Sutherland to request that Mlekodaj tell RRD that I was to be asked in the future if I could physically do a job or not. She told me later that she had done so. After some weeks, during which, Sutherland said, she prodded Mlekodaj to do it as I requested, I asked her one last time whether he had done it. She told me that Mlekodaj had finally called Merryman and had discussed this issue by telephone, but Mlekodaj had refused to put it into writing. I considered putting the statement in writing myself (i.e., in a memo to RRD) but didn't do so -- again, what good would it do?

As I noted earlier, Perkins had implied to me that he thought I was not physically up to rad engineering work. So I think it was probably Perkins who brought up the idea of my physical limitations, as a convenient behind-the-scenes reason that they could give other people for asking for Utrera. When I pointed out, from my knowledge of HFIR work and what was planned for the outage, that there didn't seem to be any rad support task I couldn't do, that reason was revealed as being unviable. When I went to Sutherland, management must have felt that the specter of the Americans with Disabilities Act was being raised. The point that I had made to Sutherland -- that this was like reverse discrimination, i.e., penalizing people for being perceived as not being able to do something they actually could do, instead of penalizing people for not doing something it was perceived they ought to be able to do -- must have "hit a resonance".

Merryman was clearly persuaded by Perkins and Stover to ask for Utrera instead of me despite Utrera's complete lack of experience with reactor work and the importance of this outage. That indicated to me -- and to Mei, by her obvious embarrassment when I pointed it out to her -- that the decision was political, not technical, and that Utrera was attractive to RRD because he was ignorant of the work and would not ask too many questions. I believe that succeeding events abundantly demonstrated that that was true. Neither Utrera nor RRD kept me apprised of what was going on in the outage planning, despite Mei's promise that Utrera would do so. I had to take the initiative to call Utrera and Merryman to find out what was going on and to get planning documents. The usual reply from Merryman was that I should "look at the procedures on the Web", but those were often a revision behind or hadn't been posted yet.

On about 11 June 1998, we RORC members were told by RRD that the same question I had about the BSR monitors was being asked by DOE about the HFIR bay monitors. That is, DOE asked if the setpoints and positions of the monitors were appropriate for detecting the accidents they were supposed to detect. I told my ORP management about this. Eventually RRD did a calculation to demonstrate monitor adequacy for the one accident they said they counted on the monitors to detect. I am sure they did do such a calculation -- years after it should have been done -- but as I never got to see a copy of the calculation, I was not sure of all the assumptions that went into it and hence cannot say if it was adequate or not. I believe, however, that no rad protection specialist -- certainly nobody from ORP -- reviewed the calculation.

The HFIR Resin Campaign

In August 1998, the main part of a multi-stage operation that I will call the HFIR resin campaign began. Resins are a material used to filter out radionuclides and other contaminants from the reactor coolant water. They were very hot in terms of specific activity (a technical term for how much radioactivity there is per unit mass). Although HFIR used to regenerate (clean) resins for reuse, they had changed to the one-use mode of operation, so that when the resins were full, they were removed and disposed of. This was done by "sluicing out" the resins from their vessels, i.e., water was used to "backwash" the resins out of the tank. The water-plus-resin combination, called a slurry, traveled through piping to a HIC (high-integrity container), which is like a giant thick-walled polyethylene bottle. The resins were then allowed to sit and settle to the bottom like coffee grounds. Finally, the water over the top of the resin layer was pumped out -- this was called "dewatering" the HIC. Thus the end product was a HIC whose contents were dry enough to be designated as "solid waste". The HIC was taken to a "temporary" storage site elsewhere at ORNL to sit for some years until it could be shipped to its permanent site.

However, Bechtel Jacobs, which was now in charge of the temporary storage site, insisted that HFIR use a concrete-and-steel box to put the resins in. (This was the reason RRD gave for using the box instead of the HIC.) This box was approximately 6 ft by 4 ft by 4 ft high; the long dimension was horizontal, unlike the HIC, whose long dimension was vertical. The box lining was concrete, with 15" on the sides, but only a few inches on the top. Also, the top had voids to allow for the piping to run through it. Considering that most of the dose was incurred when a worker was on top of the box (disconnecting the piping and other connections), this meant that from a shielding point of view, the HIC geometry was more favorable than the box geometry. (Readers to whom my explanation is unclear should note that the RRD operations people and the HFIR rad techs agreed with this statement. In fact, RRD initially tried to get out of using the box partly for this reason -- to no avail, the RRD people told me.)

So here was a job involving significant dose that was part of a campaign involving significant dose. Although the operation itself had been done before, there was a dose-increasing change in the way it was being done. Obviously, on both these counts, this campaign should have been reviewed by me as per RPP-310 (since I was still the AEG representative for non-outage HFIR work). Furthermore, the design of the box, since it hadn't been used for these HFIR resins before, should have been reviewed as per RPP-

128. But neither of these reviews was done. I was not told about the campaign as the AEG rep and I heard only vague things as an RORC member -- RRD spoke of it as though it were a routine operation and never mentioned the box issues. Thus I might never have found out about the problematic aspects of this operation at all, were it not for a major planning error in this operation. However, this occurred only after the first stage was almost complete, as I will relate below.

The earliest dose-producing step in the campaign had actually been performed weeks earlier. This was the sampling of the two cation resin vessels and two anion resin vessels. The sampling was done mainly to make sure that the resin would meet the radwaste acceptance criteria applicable to its eventual disposal site. In sampling, the vessel water level was lowered to below the level of the vessel manway (access hole); they couldn't lower it too much because the water and the vessel wall were the only shielding. The sampling worker, who was from the Plant & Equipment Division rather than RRD, would lean in through the manway and take the sample. That the resin in one of the vessels was much hotter than expected appeared to have been realized only after the workers' dosimeters were read. The rad techs were said to have noticed that the dose rate at the manway was high -- 10 R/hr at the first cation tank manway -- but the only operational adjustment made was to drain the second cation tank less. The fact that these workers were getting higher doses than expected was not conveyed to the ALARA rep, me.

Similarly, although calculations were done of the potential dose rate from a box, the results (250 mR/hr at the side) were not shared with me, or even the fact that calculations needed to be done at all. The calculations were done for radwaste acceptance purposes and not personnel protection per se, so the people doing the calculations looked only at the side dose. If RRD had told me about this, I would have pointed out that the more critical dose rate was the one through the top hole (for the disconnection step). Actually, the people doing the calculations should have known this: one was a former rad tech who now, as a member of the Radwaste division, was helping RRD, and one was a current HFIR rad tech. Thus although these people both had rad tech training and experience, they failed to consider properly the most crucial dose-imparting area of the box and the most dose-imparting step in the operation. As I have pointed out before, when a rad engineer skilled at doing shielding and dose rate calculations is used, the rad protection implications of the operation are much more likely to be considered completely.

It was decided to fill the anion box first because the cation resins were hotter -- the latter had the cobalt-60. It was also decided to set a collective limit of 300 mrem and a whole-body dose rate limit of 5 R/hr in the Radiation Work Permit (RWP). Besides modeling the box via calculation, as I mentioned above, there was another opportunity for it to be determined before the disconnection what the dose to the disconnection worker would be: prior to the start of the task, the rad tech was to survey the top of the filled box. Thus he would know the actual dose rates at different points on top of the box, including the dose rate out the unshielded top hole and connection points. If the dose rate was at all significant (not just in this but in any operation), the rad tech was supposed to do a dose estimate before work started. This was to aid in planning, including briefing the worker, and also to determine what level of review under RPP-310 was required. But the rad tech, although he did measure the dose rates, did not do a dose estimate. Rad protection professionals will appreciate the significance of this from the fact that the dose rate at the hand position was 7 R/hr (yes, R/hr, not mR/hr) on the day before the disconnection. On the day of the disconnection, the dose rate was 9 R/hr at the hand position and 2 R/hr at the body position (the resins had further settled and probably been dewatered some more). The tech updated the RWP, and thus his complex leader Davis and the workers did see the dose rates. But Davis and the disconnection worker (a reactor operator supervisor) did not know or think to have a dose estimate done either. As I pointed out to Davis and RRD later (and they agreed), had I or another rad engineer been involved, the dose estimate would have been done and the RWP violation I am about to relate would not have occurred.

The RWP had actually been written for several tasks in the resin campaign (another bad practice due to the variation in the conditions of the tasks), so almost 200 mrem had already been accumulated against

the 300-mrem RWP limit. The disconnection task itself proceeded without incident. However, when the self-reading dosimeters were read at the end of the task, it was seen that the disconnection worker had received enough dose that the total (collective) dose was potentially over the RWP limit. The hole in the top through which the connections passed was supposed to be grouted after the disconnection; even though it was clear that the RWP limit had likely been exceeded, the workers went ahead and did the grouting because the grout had already been mixed and they did not want to waste it. They quickly poured in the whole bucket, but it was too much: one worker and one of the rad techs had to scoop the excess out by hand (presumably using a scoop). All of the workers' official dosimeters (TLDs) were pulled and sent off to be read out. The results confirmed the violation. It should be noted that although EPDs or chirpers were supposed to be used for high-dose-rate or high-dose jobs like this, they were not.

The first I heard of it was when Perkins called me to ask if I would "investigate" on an informal basis. I was surprised and somewhat suspicious because of course Perkins was AEG's principal nemesis in excluding us from work planning. But as usual, I put on my cooperative face with my fellow ORP person and went over to HFIR. It turned out that the RRD Divisional Rad Control Officer (DRCO), Wilbur Harris, was also investigating. We decided to do our interviews together, which would be more efficient.

We found the following. The time the disconnection would take was known in advance because the disconnection worker had practiced beforehand on an empty box. The rad tech was apologetic and seemed to be truly embarrassed by his error. He said that he hadn't done a dose estimate because "they" were "in a hurry"; he clearly felt cowed by the operational people and their schedule pressures. His supervisor, Davis, admitted that the campaign had been broken down into separate tasks, contrary to RPP-310's requirement to consider the campaign as a whole; that an RWP had been written for each task with a limit of 300 mrem; and that this was done for the purpose of keeping below the 300-mrem RPP-310 review limit that would have required that I (and also DRCO Harris) be involved and that I sign the RWP. A Radiological Event Report was written for the RWP violation -- but not for the RPP-310 violations. I think an occurrence report might have been written too, but I never saw a copy of it; I am sure that it would have referred only to the RWP violation and not to the RPP-310 violation. (As I noted earlier, at ORNL the rad techs assigned to a facility decided when to write an RER and what to put into it; the management of the facility in which the violation occurs was in charge of writing an occurrence report. Thus the failure to have a dose estimate done and comply with review regulations would be documented by these groups and not by, say, an independent review or reporting group within ORNL.)

Harris and I wrote a joint investigation report that was sent to Sims, Hunt (as rad tech section head), Mlekodaj, Mei, and various RRD operations people. Subsequently, I signed one RWP for the next step of the operation. However, the rad techs did not tell me when any of the rest of the operation was to be performed nor did they tell me of any more RWPs (all of which I should have signed). Note that an RPP-310 review of the campaign was never done because while I was waiting for RRD to digest the investigative report (for which they never provided any official reply to me), they finished the campaign.

The RWP violation was not the end of their troubles with the resin job. The second cation box was filled successfully with only about 10% more dose incurred than the first one, even though it read 18 R/hr at the hand position and 4 R/hr contact, because the first disconnection worker (a reactor supervisor) carefully coached the second disconnection worker (a fellow supervisor) prior to the latter's performing the task. (Interestingly, the multiple badges showed that the second disconnecter's hand got 1 rem and the right thigh got 435 mrem, versus 194 mrem for the left thigh -- this worker knelt on his right knee most of the time, thus exposing the front of the thigh to the beam, while the other badge on the horizontal left thigh faced the ceiling.) But RRD ended up using five boxes total instead of four because one box did not fill properly. This was probably, or so RRD said, because the slurry was made too thick and mounded up inside one box up to the top hole instead of leveling off. Also, prior to filling the second cation box, the DRCO proposed using some little flexible shields around the connecting piping (such as the flexible

beanbag-type lead shot shields I suggested), but the union raised objections: by their contract only union people could place lead shielding. I persuaded the DRCO and I believe that he persuaded the union rep that these little shields would be moveable by one person, even with one hand, and were not in the category covered by the contract, i.e., large heavy shields. But RRD did not use them, presumably because they would have taken too much time to make up.

I think Perkins asked me to investigate the RWP violation because he realized that the principal fault lay with the rad tech and Davis and that they and Perkins looked bad. Like many a politician, he enlisted the aid of one known to be standup in such matters -- me -- to look into the affair. Then -- again like many a politician -- he completely ignored my findings. After this, he continued to exclude me from HFIR work.

On 11 October 1998, I remonstrated with Davis by memo (copy to Hunt, Mei, Mlekodaj, and Perkins) about yet another last-minute notification to review a HFIR job, when there was not an emergency occasioning the late notification, just a failure to plan. This was a prime area of customer service disagreement between the rad engineers and the rad techs: while the rad techs blamed line management for notifying the techs at the last minute and thus for the techs' notifying the rad engineering reviewer at the last minute, they also asserted (as though they were representing RRD) that HFIR management needed to be able to count on virtually instant service and not have their jobs delayed by even few hours, much less a day or two, once they decided to do the job. Mei talked with HFIR section head J. Ed Lee about this, but the situation did not change. We had thought that the RWP violation would get RRD to be more cooperative, at least for a while, but this was obviously a vain hope.

On 24 November 1998, a post-job critique of the main part of the resin campaign was held. The RRD task leader, Don Abercrombie (the pipehugger of the ISI project), stated that one box of resin had excessive levels of C-14 and could not be sent to the temporary storage area. This had been discovered after the box had been filled, when a radiochemist noticed that there was a high gross beta count on the sample that had been taken at the time the box was filled. I asked Abercrombie if the similar box sent to the storage area earlier had also contained excessive levels. He was evasive to a blatantly obvious degree, saying he didn't recall, he'd have to "dig out" the files (from waaaaay back in the file cabinet), etc. I urged him to check on it and communicate with the Radwaste people if the already sent box had high levels (because it would be outside the acceptance criteria and they might not be able to store it, legally). But he said the Radwaste people were "okay with" the first box -- which made me think that the box did exceed the criteria and that either the Radwaste people had been persuaded to accept it anyway or they hadn't been told about it. We moved on to other subjects because the HFIR operators present were getting impatient with this discussion. I reported this to Mei and Mlekodaj. Nothing appears to have been done about the first box.

REDC Matters

Around 6 December 1996, an industrial hygienist in the industrial hygiene/industrial safety office (OSHP) told me that a craft supervisor at REDC had told one of his people that a power tool could not be used because it "wasn't ALARA". The industrial hygienist thought this was odd. I agreed because the reason given, waste minimization, did not seem to be a significant driver (compared to reducing dose and effort), so not using the tool would be un-ALARA. I spoke with two craftsmen at REDC. The issue was whether one of the craftsman, who had been injured in an off-duty accident, should have to work a stiff crank on a "confinement box". The craftman asserted (and his colleague agreed less emphatically), that the crank was quite stiff and required a significant physical exertion. This was hard on the craftsman's injured ribs and increased the time needed to turn it (versus the time for a more easily turned crank); there was a close body approach to the box during the cranking, which also increased dose. The craftsman thus proposed that the box mechanism be redesigned to use a motorized rather than a manual crank. After speaking with them, I met with J. Keith Waggoner, REDC rad tech, and then with the craftsmen's supervisor. The supervisor said that the craftsman had injured himself carelessly on his own time; besides that, he wasn't hurt badly and was likely malingering to get out of work or to jerk his management's chain. The

supervisor's antipathy to the craftsman's complaint was very pronounced and I deduced that he and the craftsman "had a history". I found out later that the craftsman was indeed vocal about little things. But his colleague had the reputation of being patient and relatively uncomplaining. Also, rad tech Waggoner took me up and showed me the box. Although it was locked so that we could not crank it, I could see how strenuously a person would have to exert himself to operate it. I was given an idea of the dose involved in the overall operation, but it was hard to pin down what fraction was due to the box task alone.

The cognizant REDC engineer said that the box would have to be redesigned to allow for a motorized crank. Since the box operation was infrequent and wouldn't take place again for a while, he would get to it when they were planning to use it again. But he refused to tell me when that might be. I checked back with him six months later, but nothing had been done. He said that they hadn't needed the box, wouldn't need it for a while, etc. I don't think anything ever was done, because clearly REDC did not think it was an issue and they strung me along until it became clear that they would do nothing. In any case, I did not have any authority to push for resolution of this issue, nor did my supervisor Mei, and there was no way of tracking this issue formally. While AEG supposedly had authority to look into this, subsequent events showed that AEG had no authority to impose corrective actions, make a division respond in writing, or even get a timely response from line management. So in such cases the de facto ALARA arbiter of an O&R division's activities was the division itself. The only way to have resolved the issue of a motorized crank versus a manual one would have been for the union to raise this as a safety issue.

We were well aware that AEG was being kept out of REDC radiological work. In mid-November 1997, I wrote to REDC complex leader Kelly about REDC's failure to have a job reviewed by AEG as per RPP-310. He never replied. Mei told Mlekodaj about it, but nothing was ever done. In my memo, I also referred to a prior case in which Kelly claimed that the reasons for not using shielding were "ALARA"; that is, he meant that the shield assembly and disassembly dose would exceed the dose saved by using the shielding. But he did not appear to have done any calculation at all, so I had expressed doubt about that, as I reiterated in my memo. The RPPs gave the rad tech organization complete control over temporary shielding, so I had no standing to insist on being involved in shielding decisions even though I was supposedly the AEG rep to REDC and I was arguably "the" shielding specialist in ORP.

On about 8 December 1997, Mei was informed by Kelly that there was an REDC job that needed an RPP-310 review by AEG. But prompted by Perkins, he asked Mei to have Utrera do the review instead of me, the assigned AEG rep to REDC. Mei refused, mainly because I was the assigned rep but also because Utrera knew nothing about REDC operations. Perkins then visited Mei personally to ask for Utrera, but Mei again refused. It was outrageous of Kelly to ask such a thing in the first place and even more outrageous of Perkins to push the matter, but such was Perkins' operating style.

So on 9 December 1997, I attended a meeting at REDC regarding the job, the proposed changeout of the hopcalite filters. The hopcalite filters preceded the HEPA (particulate filters) and other filters in the exhaust ventilation ducting to protect those later filters from the acidic fumes that were given off during some of the REDC processing. As the first filters in line, they of course had some radioactivity on them. This was not the first meeting for this job -- I had not been invited to the first one -- and as I recall, Kelly had told me to come to this meeting not directly but through someone else.

I was thus coming into a meeting where most of those present knew much more about the job than I did. I tried to support Kelly, who seemed to be advocating some sort of advanced rad controls, albeit vaguely. However, he gave some dose numbers that were obviously taken from thin air to win his point. I had a moment to decide whether to keep my mouth shut and let everybody think that these were the real expected doses or to speak and thus fail to support my fellow ORP member. Because of my long exclusion from REDC, I thought this was likely be my one and only opportunity to get everybody thinking on the right dose track. So I stated, as mildly as I could, that with the assumed dose rate and

such-and-such assumptions regarding time, the doses would likely be significantly different from what Kelly had just said. But, I added, Kelly was right that we did need to nail them down better. I knew I was not endearing myself to Kelly, but I did emphasize the need to do a real dose calculation. This supported the principle, though not the data, of Kelly's contention, I thought. I hoped that he would view it that way.

A central problem was that one of the only two engineers who was qualified to make the entry and supervise the job had reached almost 1000 mrem for 1997. But the other had bad knees and couldn't get down to the filter pit (a small space reached via a ladder and a deep step). It was obvious that the knees had not deteriorated overnight any more than the filters had: REDC had not planned the year's doses with this operation in mind even though supposedly they knew all year that they would have to do the job in 1997. If the good-knees engineer got 1200 mrem, that would be okay as long as REDC got permission in advance from the ALARA Steering Committee, which of course they would. However, 1500 mrem was our administrative limit as per DOE Order N 441.1 ("Radiological Protection for DOE Facilities", a supplement to 10 CFR 835), so he would need permission from the ORNL director if he were to approach that. Thus the job had to be planned to ensure that he would not go over 500 mrem.

After this meeting, Waggoner took me on a tour of the area over the filter, the area from which the operation would be staged. They had already erected a plastic tent. As usual, I wondered about the ratty-tatty tents that were erected at ORNL -- the plastic was always nailed to wooden studs and over time, tears appeared at the nail sites. Here, the plastic was taped, but there were already two or three holes. Other sites seemed to do better, often using prefab structures, as I observed when I visited them.

There was a second meeting a few days later, by which time I had found out a lot more about the job. But in this meeting I felt that some of the REDC people were impatient with me from the moment I walked into the room. I thought that they might be afraid that I would throw a monkey wrench into their getting the job done before the Christmas holidays. After this meeting, having learned all I could, I wrote my RPP-310-required review memo. I signed the RWP at the prejob briefing the next day. The rad techs promised me at the briefing that they would call me after the job was over and tell me how the doses came out. Of course they never did. (I finally called one of them, but got only vague information.)

The reader will note that this was the only REDC job that I was ever called on to review under RPP-310. This was despite the fact that as I noted previously, REDC got 30-40% of the total ORNL dose (more than any other facility or project), it had arguably the most complicated set of rad hazards (gamma + neutron + alpha), and it had the least ALARA or professional health physicist scrutiny of its operations. An REDC rad tech told me some time later that he thought that Kelly had asked for the RPP-310 review in this instance (i.e., instead of avoiding it as usual) because Kelly wanted AEG to back him up against Chem Tech on one issue. His purposes would thus have been better served by Utrera, whom Perkins seemed to think he could count on to go along with what Kelly proposed.

There were other issues that turned up as a result of my involvement in this operation. I did an RPP-310 review of only the hopcalite filter change. However, in the course of finding out about this job, I discovered that it was part of a much larger and dose-heavy project. As I noted about the HFIR resin job, RPP-310 said that a campaign (or its project equivalent) "shall" not be broken down into smaller units in order to avoid or lower the level of an RPP-310 review. Chem Tech did exactly that in this job. Furthermore, as part of the project, they had made some new shields, tools, and even a new cart -- the purpose of all of which included saving dose. The design of these things thus fell under RPP-128 (the design and modification review procedure), but they were already fabricated and installed and some were in use when I found out about them. Although I pointed out to Kelly that these should have been reviewed and I told Mei and Mlekodaj about them, I had no way of getting Chem Tech to have us review them or of having Chem Tech reprimanded for not having had a required review done. I believe that Mei and Mlekodaj did not take this up with Perkins, Hunt, or REDC management.

In mid-December 1997, Brad Patton, REDC facility manager and Chem Tech section head, asked me to send him some information on optimization, without telling me what he wanted it for. I did so. In March 1998, I was given a Chem Tech waste handling study that purported to include an optimization (cost-benefit) analysis such as is required, where appropriate, by RPP-129 and 10 CFR 835. But it was deficient because there was no real dose analysis; clearly RPP-129 had not been followed. I reported this to Patton by memo. A few days later, Patton thanked me by memo for my comments, but did not address any of the points I raised. I never heard anything further. I believe that they just filed it away as "done" because they had created a piece of paper.

The All-AEG Meeting of 3 February 1997

Mei told us "Don't worry about pink slips" even though Chem Tech was laying off another 20 people and RSS was in danger of losing some rad techs too. She said that there was some talk about having rad techs in RSS do source control surveys and recordkeeping instead of AEG's Source Control techs, but Hunt would have to square it with Mlekodaj. She had reminded Mlekodaj that there was more to Source Control than just leak-testing sources.

The AEG Source Control people were supposedly underfunded, yet here was a proposal to have source control functions "shared" by the rad techs. I reminded Mei several times that source control functions were specialized, especially the data entry and tracking functions, and that it didn't make sense to train dozens of people to do, on an occasional basis, what our two and a half Source Control people could do more efficiently as a near full-time activity. There were also proposals to have the X-ray machine surveys done by rad techs, supposedly several specially trained ones, instead of our one rad tech who had a degree and had previously worked for an (NRC) Agreement State inspecting X-ray machines. Again, from an efficiency point of view, this did not make sense. Having Source Control do the work meant that their work was overseen by Mei or Geber, so that there was consistency and continuity of coverage. This would not have been the case if the work had been distributed to RSS, whose supervisors did not know the ins and outs of source control under the newer DOE requirements. The advantage of having a rad tech do the work, from an O&R division's point of view, would be that if they were already paying for 100% of a rad tech's time, the source survey would be "free", i.e., included in his work. But this would be true only if the rad techs were underemployed enough to have time for the source control training, surveys, and data entry. This did not seem likely in view of all the rad tech overtime that was being worked. So this would not have been a bargain. Thus the intent of the proposals to have rad techs do the work seemed to be to siphon off work from AEG to support RSS, not to improve efficiency or save money.

Mei also told us explicitly that with more pressure regarding the budget, RSS might try to take over AEG-type work. In consequence, she warned, AEG definitely needed to learn more skills with regard to pleasing a customer, in order to "compete". Mei added that doing a professional job was not enough nowadays; thinking the customer should be satisfied with the great job you did was not the same as "satisfying" the customer. She intended to use a customer survey form to "see their [customers'] degree of satisfaction with our services". The drumbeat of "customer service" heightened from about this time on, in that we heard it at virtually every staff and safety meeting. Mei was anxious and concerned about our "survival", as she called it, although she kept telling us not to worry. She seemed to have bought into the idea that we safety people were providing "services" that we had to "sell" and that if we didn't "please our customer" we could lose our jobs. I kept telling her, Mlekodaj, and everybody else -- as I keep telling you, the reader -- that it is nuts for safety people to think like that and thus to abdicate our responsibility for identifying safety deficiencies and making sure they are corrected before work can be allowed to start.

Mei also told us to put our actual hours worked in the timekeeping system even though for salaried employees there was no credit for hours worked over 40. The "recording more than 40 hours even when we weren't paid for more than 40 hours" refers to the Lockheed Martin Energy Research (LMER)

company policy that people paid on a monthly basis would not be paid for the first 4 hours above 40 hours worked a week and would be paid only on a one-hour-paid-for-two-hours-worked basis after that (and then only if approved). Thus, for example, if Mei worked 48 hours in one week, she would be paid for 42 at most. We could take the "extra" time (e.g., Mei's two paid hours) as compensatory time, with permission. The point of putting down all the hours worked on our time sheets, even though we weren't paid for it all, was supposedly to show the higher-ups how hard we worked. However, this did result in one distortion of reality. Since the rad techs and rad engineers charged out the same per hour of time spent and since rad techs were paid for overtime while rad engineers generally were not (at least not on a one-for-one basis), extra time worked by rad techs brought more revenue into ORP than extra time worked by rad engineers. I kept my own log of hours worked and time taken off. Every year I worked "extra" hours that added up to weeks by the end of the year, but I never put in for overtime or comp time. It was a matter of principle, in that professionals supposedly do not put in for time they worked above 40 hours unless it is a really significant amount of time.

Control and Instrumentation Division Source Feud

The Control and Instrumentation Division (C&ID) source feud is an example of how a lack of clear authority can result in a situation remaining unresolved even though everyone agrees that it needs to be resolved. It is also an example of how ORP allowed its personnel to be harassed and criticized even when they were doing a conscientious job. Some people will disagree with me as to my characterization of this situation and I admit I don't know all of the details of the feud. But I think this incident was a blot on the reputation of an otherwise mostly admirable division, C&ID.

In early February 1997, I spoke with rad tech Jimmie Kilby, who was assigned to C&ID facilities, and separately with his complex leader, C. Lynn Sowder. They said that C&ID had two feuding sections: one used strong neutron sources in its research and the other did not. I'll call them the user section and the non-user section. The non-user section was fearful of the "second-hand" dose from the sources, i.e., the dose through the walls and floors to people not involved in that research, and they had expressed concern to the user section and to the rad techs. The user section agreed that they would use their sources only after hours, even though that was a hardship for them and for the rad techs (since a rad tech would have to be available to cover the work). They also now stored many sources in the basement and were trying to figure out how to store others there. Note that these sources were mostly in large drums lined with thick paraffin or similar and that they had to be moved on a type of hand cart. Although the user section had thus cooperated, the non-user section was still unhappy. One woman in the non-user section had suffered a miscarriage and another was trying to get pregnant; the non-user section, and particularly these women themselves, wondered whether their problems were due to dose from the neutron sources. The non-user section also had people who did not follow dosimeter rules. E.g., one C&ID person put his dosimeter on top of a file cabinet overnight, contrary to company policy, to see what the dose would be from a source used in the room overhead. The dosimeter would thus register dose that he himself hadn't gotten.

Kilby, a nice guy, was anxious and frustrated because of the conflict. He told me he had been personally blamed by some in C&ID, even reviled and accused of lying about doses and dose rates by couple of them. This occurred even though, by his and Sowder's information, he had been more helpful even than most rad techs. For example, he found and ordered a cart for the sources so that they could be moved more readily and without damaging the floor. He also identified a room in the basement that C&ID could turn into a shielded, secure storage area. In early March 1997, I met with Kilby, Sowder, and I believe also Mei regarding the source problem. In this meeting and in my earlier talks with him, I thought that Sowder should have been more supportive of his guy Kilby and taken the lead in resolving the problem, instead of delegating so much to Kilby, but at least Sowder seemed to be honest about the problems.

Later in March I arrived for a planned meeting with C&ID regarding the neutron source problem, only to find that it had been cancelled without notice. In mid-April, Mei, the AEG pregnancy survey tech, and I

met with some C&ID user section people regarding the problem. They stated candidly that they thought the non-user section was jealous because the user section had ample funds (from the source-associated work) and the non-users were scratching for funds. There was likely some truth to that, but still it could not be the whole story behind the non-users' concerns. By the end of the meeting, it seemed clear that C&ID did not really want anything to be done about the sources; the two sections seemed to be scoring points off one another and consulting ORP to show concern, but they didn't really want any advice. I think that the feud eventually lost steam and petered out.

Fetal Protection

I was always the person in ORP who tried most to keep up with fetal protection -- what DOE said, what the international authorities said, what the latest research showed about the effects, etc. So as I noted earlier, as of 1997 I was the technical lead on fetal protection, while our AEG pregnancy surveyor continued to do the surveys and keep the survey and contact records. In February 1997, I organized a meeting attended by three people besides me who could serve as fetal protection counselors on various aspects (external dosimetry, internal dosimetry, etc.) and also by the surveyor and Mei. I later compiled and sent out fetal protection information to these people. Mei agreed that we needed communications and issues training. So in October 1997, I organized for this group a talk by Dr. Ronald Goans, then of REAC/TS (the DOE emergency training and response facility). Dr. Goans was both a medical doctor (formerly an obstetrician) and certified health physicist who had counseled pregnant women about radiation concerns. His talk counted as training for us, so this was the second type of training I organized (the first being the ALARA course for engineers).

AEG-RE Meeting of 10 February 1997

Geber reported that he had sent an extremity exposure evaluation to an HSRD person for comments. He said that both the subject with the high extremity dose and the HSRD person were cooperative, but not the DRCO of the Chemical and Analytical Sciences Division (CASD), Dr. Fred Smith. Smith kept promising to get together with Geber about this but somehow never did -- while telling Geber to keep reminding him about it. As usual, AEG, lacking authority, could be stonewalled by O&R division people like Smith.

Mei also said that AEG had a heavy workload in 1997 compared to 1996. She had had no word from Mlekodaj on re-engineering. Sims and Mlekodaj would have to go back before the Re-Engineering Committee; the committee didn't question AEG functions, she said, since Sims had presented them so well, but they did ask about the chargeback process. The direct chargeback was tough for AEG since unlike rad techs, AEG-RE couldn't be shuffled from lean projects to fat ones. Mei added that per Sims, "low-key" was the watchword in spending for the year: e.g., one should do one's prep work on weekends or while one was on a plane, etc. She stated that there were 16 "monthlies" (salaried workers) in RSS, but fewer in AEG's section, RCS. Even our section head Mlekodaj was not authorized to say who could go on travel -- Sims himself had to authorize all trips, contrary to past practice.

Regarding the technical basis for beta reduction factors for clothing that had been put together by complex leader Pedro Gonzalez and Greg Franklin, Mei and I had provided comments that were mostly not incorporated. An external dosimetrist in the dosimetry and records section (a certified health physicist with a master's degree) had commented too, but her comments also were largely ignored. The two authors supposedly checked cases in real situations in response to our comments, but Geber observed that there was a suspiciously big gap in their mg/cm² numbers. We rad engineers had a sad little conversation about this, opining that our comments on this document and Gonzalez' earlier waste characterization document were beyond the ken of the two authors (the idea being that if they had understood the implications, they would not have kept their documents as they were). But, we noted, we -- the rad engineers and shielding specialists -- had no approval power and were powerless to get the documents corrected.

Thus in the Gonzalez-Franklin technical basis effort, we again saw the rad tech organization doing work that at other sites would be done by rad engineers or senior professional health physicists, not, e.g., by a complex leader who spent more than 90% of his time supervising rad techs. The fact that three or four senior health physicists found holes in their technical basis was, at ORNL, not sufficient reason to have the authority for preparing or approving the technical basis shifted to others. Gonzalez and Franklin made a sincere effort to turn out a usable technical basis, but it was an amateur sort of effort, as was demonstrated by the technical comments that more experienced and broadly trained people made.

Some Notes on the Re-Engineering Effort

The ORNL Re-Engineering Committee seemed like a good idea at the time: to eliminate "fat", reorganize organizations for more efficiency, etc. However, some ideas advanced as part of the Re-Engineering effort appeared to be more of an "O&R groups empowerment" push and they foreshadowed the later utter dominance of the customer service model at ORNL. This was made clear in a May 1996 presentation (which for its relevance I discuss here although it occurred before the other events related in this chapter). The Re-Eng honcho who gave the presentation quoted Peter Drucker as claiming that "knowledge employees" could not be supervised because as specialists, they had to know more than anyone else about their area or they would be useless. This idea seemed ironic to me, seeing that we in AEG were put down so much as no-nothings, even when the question was clearly about ALARA.

The presenter stated that the Re-Eng effort proposed to replace associate directors and division directors with "competency directors", to improve opportunities for teaming, and to redirect management time into research. The number of people in each division would be limited (arbitrarily?) to about 40. The competency proposal included the establishment of "competency centers" for support functions and the matrixing of the support functions as "shared services", "to allow R&D areas to concentrate on R&D and be provided the services to facilitate that essential function". The large central organizations would (somehow) shrink and become smaller but still central; many more services would (somehow) be matrixed to customers; there was to be accountability to customers (which allegedly there previously was not); and there would be formal customer satisfaction measures (which ditto). These last bullets seemed dubious because they set up the "old way" as a straw man of ineffectualness. The presenter went on in that vein: "you" (the customer) needed shared services because the old organizational processes were "slow and sequential" instead of "fast and parallel". Also, in the old system, functional groups were "more concerned with protecting their turf" than with serving the customer, placed greater priority on meeting their own functional goals than on contributing to overall organizational achievements, and "regard each other with suspicion, blame each other for problems, and operate as though the enemy is within the organization". No examples were given as to how these phenomena had manifested themselves or as to how they would be avoided in the new system. One slide, headed "Organization: Characteristics of Shared Services", asserted that shared services were not the same as "Big Central"; it claimed that shared services was a process, not a structure. In addition to reiterating the slow versus parallel and other dichotomies given above, it stated that "processing" (of requests?) was "value-added" and "collaborative"; that control was not by function (i.e., the function-performing people) but by the "users" (i.e., the customers); that power and influence were dispersed instead of being concentrated at the top; that service agreements countered the "enemy within" syndrome; and that the new system would be "pull" to the old system's "push". The slide concluded triumphantly that "The user is the chooser".

But Slide #13 and following were the giveaway. As I noted earlier, there had been a huge push by Lockheed Martin to centralize services on the reservation, with the aim being to entwine ORNL and Y-12 and to some extent K-25 so tightly that the contracts could not be bid out separately. So while the "long term view, 1/1/98" was that all safety & health and environmental compliance functions should be distributed, not shared (presumably because of the very different hazards at the three sites), a reservation-wide directorate was to be established to manage all facilities and site management activities under Lockheed Martin, including ES&H. With all the improvements in all areas, it was hoped that within three

years, some \$18-30 million/year could be saved. The ES&H improvements were slated for FY 1997, i.e., October 1996 through September 1997. An "Internal Customer Advisory Board" -- a group of "influential Lab managers" -- would act as advisors supporting the various re-engineering effort teams.

The Re-Engineering Committee had a lot of power, it seemed; Sims, Mlekodaj, and the other ORP managers appeared to be fearful of what conclusions they might reach. So far from being the ORNL authorities on what functions rad protection people should be performing, etc., Sims et al. seemed to be at the mercy of what the Re-Engineering Committee perceived to be their proper functions. So as Mei had told us earlier, there was a heavy emphasis in ORP on keeping a low spending and perk profile, to the point where Sims was telling us, e.g., to do prep work on what was supposed to be our own time.

RPP-310 (Operational Radiological Reviews)

Also at the 10 February 1997 AEG meeting, Mei said that regarding the presentation of the proposed revision of RPP-310 to the Directives Review Committee (DRC), Mlekodaj had resolved comments with all but one reviewer. Wilbur Harris, RRD's DRCO, had commented that a DRCO could handle the Level 3 review and call AEG only if needed, but he ended up agreeing with Mei's version (i.e., with AEG's doing the Level 3 review). Mei told him that a division could always add requirements (e.g., including the DRCO in a Level 3 review), but RPP-310 represented the minimum. However, at the next DRC meeting, Hal Glovier, RRD director and DRC member, said that either the DRCO or the AEG (presumably at the O&R division's discretion) should be able to do the review. The committee backed him. So, Mei said, Sims would now have to go to the next DRC meeting to defend RPP-310.

Note that the new proposed Level 2 RPP-310 review would be done by the complex leader and the Level 3 (the old Level 2) review by AEG. However, as noted above, RRD was trying to have the new Level 3 review performed by the O&R DRCO, not by AEG. As I stated in a previous chapter, DRCOs were usually not people with professional rad protection training; they were appointed by the O&R division directors and were usually of the division. Often they had only rad worker training (until John Alexander put together a course for them). Thus they were usually not qualified to do a review that was predicated on rad protection rather than operational expertise and they were arguably not independent of operations.

Shockingly, the outcome of Sims' "defense" of Mei's proposed RPP-310 revision was as follows. At the next DRC meeting, Glovier proposed that the Level 3 review include both AEG and the DRCO. Sims agreed to this proposal on the spot, without consulting with his people, to preclude further delay in approval of RPP-310. This was not kosher, for three reasons. First, some DRCOs polled by Mei before this meeting indicated that they did not want to be included in Level 3 reviews as a routine matter; they were comfortable having AEG do the review. RRD thus dragged these other divisions' DRCOs in unwillingly in order to have RRD's preferred requirement cover everybody. Second, the other divisions were not allowed to review and comment on this substantive change in the procedure before it was approved -- i.e., the ORNL-wide comment period was over and all comments were supposedly resolved before this committee meeting, so a significant change at the committee meeting meant that divisions not represented on the committee had no chance to comment on the change. Third, it was not clear that this machination was allowed by the rules of the committee, that is, that Glovier could hold the procedure hostage and Sims could bless the change on the fly. This whole episode was thus a concession by Sims to RRD because Glovier, as a prominent committee member, might have held the procedure up indefinitely.

In mid-March 1997, Mei told us that the revised RPP-310 would go into effect on 31 March. A few days later, responding to an inquiry from Chem Tech director Dr. Gene McNeese, Mei sent McNeese a memo (copy to Sims, Mlekodaj, etc.) about "independent" rad protection review. She stated that there was not an explicit requirement for this in 10 CFR 835, but the 835 ALARA Program implementation guide indicated that the requirements of 835 did imply such a requirement. She quoted the relevant passages for both operational and design reviews; in both, the first or main reviewer type was "rad engineer" and the

need for, in effect, a review by two members of the rad protection staff was emphasized for design cases. Mei got most of her information for this from me. I never heard that Chem Tech disputed these quotations, but neither did there ever seem to be any explicit acceptance of this by Sims or McNeese.

ALARA Suggestions and Awards

In September 1996, I had met twice with consultant Hal Butler to discuss an ALARA suggestion by REDC complex leader Kelly. I noted that there were some documentation deficiencies that made it impossible to tell if the suggestion would work or not. In February 1997, I told Butler about the failure of Bob Wham, REDC facility manager, to respond in writing to Kelly's suggestion, as procedure called for.

In February 1998, I had occasion to remind Chem Tech DRCO Richard Shoun that he was to provide me with information regarding two ALARA awards, one proposed and one already approved. I asked if there were writeups for them. I don't recall what I got in return, but it was either nothing or something minimal. We in AEG were supposed to check a suggestion before the ALARA Working Committee (AWC) had voted on and approved it, but that hadn't happened in one of these cases. In fact, when it was brought up at an AWC meeting, I urged the members not to vote on it until AEG had verified that it was a suggestion that would at least potentially be useful, but several members opined that they should reward the person simply for taking the trouble to make the suggestion, whether or not the suggestion had any real value or not. We in AEG disapproved of this approach, but Mlekodaj acquiesced to the will of (some of) the committee. We were disappointed that he, as AWC chairman, would not provide leadership on this.

On 6 November 1998, Kelly and I met in Kelly's REDC office to discuss three ALARA award nominations made for the "VOG" filter changeout. I asked him for documentation related to the VOG work, such as calculations for a special shield that he said were performed by a rad tech (not Waggoner). He was unable to produce any documentation, such as the calculations of dose for one award ("I didn't put it in the file") and the shielding calculations. I noted that the design features of this project were not reviewed as per RPP-128 and the operation itself was not reviewed as per RPP-310. Kelly replied that in his interpretation of RPP-310, it had not needed to be reviewed. He said that the only part of RPP-310 he ever looked at was Table 310-1, i.e., he ignored the text that required review of novel or infrequent operations or those with uncertain conditions. He did not comment on RPP-128 at all, I believe because he was unfamiliar with it. He told me flatly that he avoided having me review REDC work because he wanted to save his customer money. I pointed out that my reviews were free for ORNL entities and were covered by overhead (as was true until 2000). Kelly was surprised to hear this, but remarked that he was still going to protect his customer from, e.g., delays that might be occasioned by such reviews.

I realized that AEG and I were only going to get into trouble if I pressed Kelly further on these issues, whereas if I let them go, Kelly might be better inclined toward involving AEG. So I wrote a memo to Mlekodaj, copy to Mei, that I was passing these three ALARA awards. But I also told them of my misgivings about Kelly's suspicious failure to document the work (which documentation, the reader may recall, was required by 835). I explained how difficult it had been to get information from Kelly. Later I met with Mei and Mlekodaj and told them more about my meeting with Kelly, including his admission of the avoided RPP-310 reviews and the undone RPP-128 reviews. I explained again that I did not press Kelly further for information about the work covered by the awards so as not to offend him, in the hope he would in the future call me as required by procedure to review jobs such as the VOG filter changeout (which I felt was more important than giving a potentially undeserved award). But a few weeks later, I told Mei and Mlekodaj that I regretted blessing those awards: it hadn't made Kelly any more cooperative than before. Then in December 1998, as I described in the MSRE section above, Chem Tech kicked me off MSRE and REDC work. In the case of the REDC work, I felt it was particularly undeserved, because I had performed only that one hopcalite filter RPP-310 review in all the years I was the REDC rep.

I hope that it is clear to every reader that an ALARA award bestowed at ORNL was not necessarily recognition of an achievement in reducing dose: a person or group could be nominated by a division just for participating in an operation declared successful by that same division, or even just for submitting a suggestion that might never be adopted. In effect, one could get a plaque for just showing up.

RORC

In February 1997, RORC met to review the proposed new RORC charter. The changes were alarming to some of us RORC members because they lessened RORC's scope and authority from what they had been traditionally. There was also a proposal to include a member of RRD on this supposedly independent review committee -- on the grounds that the RRD person could "present" RRD documents for approval and "explain" them to the committee. Chairman Harrington and quasi-secretary McKeehan -- nuclear safety specialists who could have been supposed to be in favor of rigorous independence and amplitude of scope -- were the most in favor of approving the new charter. Tom Wilson (of C&ID) and I were the most against it, with Bill Kohn (of an engineering research and consulting division) weighing in with us. Wilson's boss, a former RORC chairman, also spoke against the charter. It was clear that this RRD-friendly charter was being pushed by RRD and by persons high up at ORNL who favored reducing review scope. But as a result of our firm and indignant protest, the charter revision was put on hold.

On 30 May 1997, I attended an RRD briefing of RORC on the pony motor problem at HFIR. This was not the first time this had been discussed; RRD had known there were problems for several years, but due to funding constraints was not able to address the issue. DOE seemed uninterested and so after a time RRD did not stress the importance of the issue to DOE any more. RORC kept expressing concern, but did not indicate it as an urgent priority, based mainly on reassuring information from RRD. But finally, when megger tests showed a marked deterioration in a couple of the motors, the issue could no longer be swept under the rug. It was irritating that about a year later, some people in RRD were saying that the fault for not emphasizing this issue should be shared by RORC -- because RORC believed RRD's protestations that the situation was stable and didn't red-flag it. This was the old "How could you let me do that?" ploy. I thought that was pretty arrogant of RRD.

In June 1997, Harold Denton, formerly of the Nuclear Regulatory Commission and now a private consultant, met with RORC. He had been asked to look over certain RRD operations and oversight of RRD, including RORC and its functions. He told us that it was of concern that RRD unilaterally postponed addressing so many of RORC's recommendations (by implication, that ORNL management allowed RRD to postpone this). He advised RORC, in effect, to put its foot down and insist on early and substantive responses to its recommendations. He also recommended maintenance of independence of RORC. In light of the proposed charter revision, I think the powers-that-be who hired Denton hoped he'd say that the changes in the revision were okay. But in fact, he said the opposite, although perhaps not specifically with respect to the charter. (That is, he may have said them specifically with respect to the charter to higher-ups, although not to RORC as a whole.) Denton struck me as a person who did not cherish a lot of illusions about what motivated people to comply with regulations and good practices; his message seemed to be that RORC should be a benevolent but strong overseer of the RRD scene, granting some "wiggle room" but not too much, lest people be tempted to wiggle all the time. (That was his take as best I captured it -- I hope that I am not misrepresenting him in saying that.)

Some time in this period, I commented on some RRD document submitted to RORC. I don't seem to have a copy of my comments now, but an RRD compliance person -- who shall be nameless here -- provided the RRD response. In a long, passionate, extravagant, and completely unprofessional put-down, he provided what he undoubtedly regarded as a rebuttal of my points. The trouble was that any substance there might have been was lost among all the insults. One particular phrase that he repeated with nearly every point was "gross conceptual error", even abbreviating it "GCE" because he used it so frequently. After reading less than half of his response, I could read no more. Another RORC member, seeing the

RRD response, agreed with me that it was just indigestible as it was. I told to Harrington that until the RRD commenter redid the comments, I could not evaluate his responses, and Harrington apparently passed this message on. The RRD commenter defended himself, saying that "gross conceptual error" was not meant to be insulting, was just a phrase everybody used, etc. I don't know what the outcome of all this was; I think my comments stood as they were, but RRD never provided a further response as far as I can recall. However, I felt acutely that if I had ever written anything like that and sent it to someone in an O&R division, the outcry would have been heard in Outer Mongolia and Timbuktu.

In mid-December 1997, RORC was finally reorganized. Tom Wilson and I were demoted from full members to ad hoc members. This meant that instead of being included in every all-member meeting and in all RORC decisions, we were included only "as needed", i.e., only in meetings and decisions that the chairman chose. There were other changes, including one that made the Office of Nuclear Safety member, McKeehan, the gatekeeper for documents coming to RORC for review.

I sent a memo to Mark Kohring, head of the Office of Nuclear Safety, to protest the way in which RORC was reorganized. Specifically, I noted that the manner in which it was done made it appear that the more vocal and inquisitive members (Wilson and I) were being sidelined and that ONS was controlling the committee. I also pointed out to him that an RRD compliance person had sent a response to some comments of mine that was professionally insulting (the "gross conceptual error" stuff). Wilson, who had challenged RRD on things within his area of expertise even more frequently than I had, resigned from RORC in protest of the reorganization. I almost did, but ever optimistic, I thought that I should stay and try to "make a difference". I thought that it was important to see what happened and, if necessary, to call out any problems or deficiencies. So I continued as an ad hoc member of the committee. I told myself that I could always resign the next year if things worsened.

We RORC members had heard some time before from people in RRD and working for RRD that RRD was conducting a "push-back" campaign directed at DOE, RORC, and the safety organizations. With respect to DOE, this campaign seemed to have backfired. We were told that DOE became irate at RRD and ORNL and insisted that the RRD director, Glovier, be removed. He was in fact removed as head of RRD, although he stayed for another year in an advisory capacity. I liked thinking that DOE, the 98-pound weakling, had finally stood up for itself in the safety area, but even so, I thought that getting rid of a person I regarded as an effective manager was overkill and thus counterproductive. I disapproved of the push-back campaign, but I thought overall Glovier had done a lot for RRD. I sent Glovier a sympathy E-mail message, but he never replied. Years later, someone told me that actually Glovier had had a drinking problem and had come to be viewed by DOE as a liability to ORNL. Thus DOE was supposedly not reacting to the push-back campaign but to potential embarrassment. Today I don't know what to believe about why Glovier was removed, but either way it doesn't speak well for DOE's judgment.

The push-back campaign might have failed with DOE, but it seemed to score one big success, i.e., the reorganization of RORC and the limiting of its scope. A less visible but even more significant success was that the safety organizations were also more limited and more controlled by line management. We saw that reviews were being more openly avoided in RRD and other divisions, not just in Chem Tech.

In January 1998, I sent two memos to Sims, Mlekodaj, and Mei. In the first, I pointed out my disagreement with the reassignment as an ad hoc member and I noted some deficiencies in RRD documents regarding rad protection. In the second, I reported that I had overheard one RRD person say to another that RRD had told DOE that RRD had a written evaluation or analysis in the case of an incident, but then found that they hadn't. The RRD person said that they had been "too busy" to do it. (This occurred while I was at HFIR for an RORC meeting and I had the feeling, given that the person who said it spoke right out there in the hall, with me in plain sight, that I was meant to hear it.) I got no response as to this from Sims et al.

In July 1998, I participated in the annual review of RRD reactors. The annual review, which used to take about two weeks, now was essentially shortened to one week and its scope was more limited. This was true of later reviews as well and they tended to be every 18 months instead of annually. Two reasons were given: everybody in RORC was so busy and our review interfered with RRD work.

Work Smart Standards

In about October 1997, some us RORC members were aghast to find that RORC was not included among the groups and individuals asked to review the HFIR "Work Smart" ("Necessary and Sufficient") standards set. Since these were reactor safety standards -- which would become part of the plant's safety basis -- and we were the internal reactor safety review committee, this review was clearly in our scope. McKeehan, of ONS and RORC, knew about the non-referral to RORC, but in his new function as "gatekeeper" for RORC review he had agreed with RRD that RORC did not need to review it. When he was put on the spot by being asked for copies by some of us RORC members, however, he provided them. I thus did review the HFIR Work Smart set, albeit only unofficially.

In this document, I saw a list of the members of the HFIR Work Smart standards identification committee. OSHP (industrial safety and hygiene) and ONS (nuclear safety) were represented -- but not ORP. Nor was there an ORP member on the ORNL-wide "other facilities" committee, although OECD (environmental) and OSHP were represented and there were two people from the Robotics division. I asked HFIR-REDC group leader Perkins and HFIR complex leader Davis if they had seen the HFIR Work Smart set and reviewed it. Perkins told me that he had had nothing to do with development of the set and had not seen it. Davis was vague about it; he said he might have been sent a copy and might have leafed through it, but hadn't reviewed it. I also spoke with the manager of NAAF, the HFIR neutron activation analysis facility that was run by the chemistry division; although he was a facility manager and thus responsible for applying the standards to NAAF operation, he was not involved or consulted at all and in fact was surprised to hear that a HFIR standards set applicable to his facility had been produced.

As part of my review of the HFIR set, I looked at Work Smart pilot projects done for DOE and at other sites' Work Smart sets and their documentation of how they had arrived at them. I was taken aback at how slipshod and superficial the RRD process and product were, compared to those of other sites. I was also upset to see how inept the hazards statement was: there were various incorrect or misleading rad statements and there was confusion about what was cause and what was effect for other types of hazards as well. I reported this to the RORC chairman, Harrington, and other RORC members. Harrington asked me to report this to Kohring (ONS division director) also, which I did on 6 January 1998. In my memo I noted that since RRD had not addressed the hazard evaluation in a methodical manner, as other sites had per their documentation on the Web, the rad protection part of the set might be inadequate.

On 7 January 1998, Sims told me by E-mail and later by phone that Kohring had called him to say that my comments in my memo of 6 January would be taken seriously. However, I expressed concern to Sims regarding this, pointing out that I did not answer to Sims in my capacity as an RORC member but to the RORC chairman, and the chairman in turn to an associate director of ORNL. (As I noted earlier, formerly the RORC chairman formally reported to the ORNL director, but the report level was lowered, apparently because the director was deemed to be too busy to concern himself with this stuff.) Kohring sent a copy of my RORC comments to Sims, which I thought was a breach of RORC confidentiality since normally RORC internal communications were not shared with those outside RORC and ONS. Because Kohring had done this, I sent a copy to Mei for information (not that it contained anything that I hadn't told her already as safety standard information, not as RORC business).

In sending my comments to Sims, Kohring's motive might have been to alert Sims to the possibility of RRD's not having adequate rad protection N&S standards. However, because Sims and Kohring did not

appear to have tried to follow up on this aspect -- and in fact, this issue was never addressed formally by either RORC or ORP -- I concluded that Kohring actually wanted Sims to be aware of my RORC activities and possibly to rein me in on the Work Smart standards issue if necessary to placate RRD. I think that Kohring's telling Sims he would take my comments seriously, instead of telling me himself or instead of telling me with copy to Sims, showed that his true message was directed at Sims and not at me.

HFIR's Work Smart standards set was approved by the powers that be (which didn't include RORC) pretty much as was. This set omitted parts of the DOE Order on conduct of operations. I believe that the general ORNL set also omitted these parts. In 2002, after I was laid off, I heard that DOE made them put those parts back in, under pressure from the Defense Nuclear Facilities Safety Board.

AEG-RE Meeting of 14 March 1997

Mei said that Sims had been afraid that the ORNL Re-Engineering people would cut the AEG (overhead) budget to a third or less, but they just said that AEG rad engineers "should charge out more". However, she said that in RCS there were only 8 FTEs funded. I think this was supposed to cover the 4 people in Vince Bishop's computer and procedures group, the 4 rad engineers and 2.5 Source Control people in Mei's group, the 7(?) people in Halliburton's instrument calibration group, the 1 secretary, and Don Gregory and Steve Hamley, for a total of about 20 people (but not Mlekodaj himself). It did not seem as if we rad engineers could charge out more than 10-15%, since our RPP-310 reviews were "free", our preparation or reviews of procedures and other documents were on overhead, etc.

The DOE Trailer Dose Problem

On 20 March 1997, I spoke with Kim McMahan (ORP external dosimetrist) regarding dose problems at Building 7964B, which was actually a trailer that stood between the HFIR reactor and REDC, where neutron-emitting isotopes were processed. It seemed that some DOE people in offices in 7964B were showing neutron doses on their dosimeters (TLDs). While these doses were not high, they were not near-zero as they usually had been. So there was a possibility that something was going on, especially since 7964B faced the REDC temporary waste storage area.

This obviously was an ALARA and rad controls problem and was squarely in AEG's scope. So with Mei's approval, I began to try to establish where the dose was coming from and whether it was a problem. One wrinkle was that, as McMahan told me, the DOE people's TLDs were read at Y-12 instead of at ORNL -- apparently all the DOE TLDs for all the Oak Ridge sites were read at Y-12, perhaps for convenience. But with neutron dosimeters it is important to determine a correction factor suitable for the neutron spectrum present. The Y-12 people had been given no information about an appropriate neutron correction factor for the 7964B area and so were using a default one. This might tend to give a higher dose than it should. Of course, that explanation would not cover the question of why the neutron dose had not shown up on their TLDs before (e.g., did no one notice it before?).

I tried to nail down the facts of the matter. I spoke with Mark Robinson, the DOE-ORO rad protection rep, and with REDC complex leader Kelly and HFIR complex leader Davis. The first conversation I had with Robinson was cordial; he seemed to welcome my help in finding out things. But Kelly intimated to me that I should butt out because the rad techs were handling the matter. He made it clear that he felt that I had no official standing to investigate and prescribe a resolution because there was no procedure that said so, even though there was an obvious ALARA question. I thought that my authority proceeded from the ALARA Program Manager, Mlekodaj, through Mei; Mlekodaj had of course been briefed on this by Mei. McMahan could do nothing but advise regarding the neutron correction factors; she too had no official standing to inject herself into the matter since it was the complex leaders who by procedure generally determined dosimeter placement and use. Davis tried to measure the dose rates with area TLDs with the aid of McMahan. However, he did not feel empowered to be the voice of rad authority on the matter. Davis was a nice guy and a capable complex leader, but he clearly was not comfortable with

breaking new ground or figuring out complicated matters such as this. He seemed to regard my involvement as natural at first, but soon he too was telling me that they were handling things and didn't need my help.

The person evidently making the decisions on all this seemed to be, from direct statements by Davis and from implied statements by Kelly, their mutual group leader, Perkins. Perkins, as I have noted before, was the principal exponent of the "AEG does not add any value" school of thought. I guessed that when he found out I was getting involved, he gave his henchmen strict orders to keep me out. I was never able to find out the area TLD results; Davis was evasive when I asked, saying that the numbers were not to hand at the moment, he'd have to ask someone else and get back to me (but didn't), etc. I tried to discuss this with Robinson later, but he too had become dismissive of any help from me, saying that he would handle the matter on behalf of the DOE exposees. While he wanted to hear any information I might find out, he did not wish to give me any information or refer the matter to me as an official ORNL finder of fact. He had been talking to Perkins et al. and I think that he was persuaded that it was best to deal with them alone. This discouraged me, because it meant that REDC could sweep under the rug any problem or procedural failure that there might have been -- and DOE would have allowed them to do it.

What I did manage to find out -- from Davis, in an early unguarded moment -- was that the dose was real and had likely been produced by waste sitting at the REDC temporary waste storage area for much longer than the few days or a week that was supposed to be the limit. I say "real" because there was an actual reading on the dosimeters (i.e., the TLD reader hadn't malfunctioned), but the correction factors were still under consideration. So the DOE people's doses still might need to be adjusted for the true spectrum, which might mean that the doses would finally be determined to be in the "negligible" range again. The problem, if it existed, could be solved by having firm limits on waste storage time before removal (or by providing the extra shielding that Kelly said they didn't have room for) and by considering the true spectrum in evaluating the TLDs. It would also be important, e.g., to compare the TLDs from people in 7964B nearest the waste area to the TLDs from those in 7964B farthest away to check differences in scatter and spectrum.

The resolution seems to have been that the doses were reassigned based on a different neutron correction factor, REDC did shield its waste better, and eventually the TLDs of DOE people based at ORNL were read at ORNL. Despite the fact that this incident showed that there was no formal resolution process for situations like this, no protocol was set up as a result -- such situations would be handled in the same ad hoc and disorganized manner in the future. Although some changes were made, the fact that they had to be made at all showed the lack of previous formal and holistic evaluation of the situation. I thought that a rad engineer assigned to the REDC-HFIR could have pulled this all together and made the changes before a problem was created, but alas, RSS claimed all the relevant turf. In this case, it would have fallen to the RSS person in charge of the two areas to have kept track of the waste presence, made sure the dosimetrist knew the TLD issues, etc. But Perkins, who was that RSS person, did not seem to be doing it.

AEG-RE Meeting of 14 April 1997

Geber announced the completion of his dose-estimating EXCEL template. This was meant to be a tool used by rad engineers and rad techs to do dose estimates and document them. It was similar to one that Brookhaven's ALARA Center had created, but Geber's version used the easy-to-learn EXCEL and was ORNL-specific. I believe that RSS rejected it, because it was NIHIRSS ("Not Invented Here In RSS").

Utrera had in March 1997 told us of difficulties on the Bethel Valley Evaporator Storage Tanks (BVEST) Project in the direction of less safety, including various changes of mind on the part of DOE; now he said of the project that for the pump and valve vault part, management was pulling back on engineering measures and putting more emphasis on administrative measures. There was also some "temporary-permanent shielding" (in Scott Taylor's immortal phrase). This project was no longer an "ITO", but an

award fee project -- this made the company handling the project, AEA, a prime contractor, so we could anticipate a repeat of the MKF-as-prime problems (i.e., lack of clear oversight authority, different standards, etc.). LMER did not want to accept this because of the experience in interface problems, but DOE insisted. Note that the Rad Con Manual, 10 CFR 835, etc., said that engineering measures were to be preferred to administrative measures, which were to be resorted to only when engineering measures were impractical. Thus the Bethel Valley Tank project management may not only have been renegeing on representations it made to DOE and to reviewers during the bid proposal and planning stages, it would be in violation of 835 if the changes were made by management fiat to save money and not as the result of a considered and reviewed process of controls selection (or reselection). We could do nothing about this, of course, because of the corollary problem: if AEA was a prime contractor, i.e., a contractor to DOE itself, rather than a subcontractor to LMER, LMER had no authority over them. From what Utrera and Mei said, LMER had actually "grown a backbone" and protested this, but obviously to no avail. So as with MK-Ferguson, ORNL would be interfacing with them and seeing them do things that DOE would never let us get away with, but we would be powerless to do anything about it.

The ALARA Course and Other Training

In June 1997, I reviewed the DOE version of the ALARA course for design and operations engineers at the request of the DOE person who was getting the course produced and distributed among the DOE sites. A substantial portion of this course was drawn from the course that I developed for ORNL. In March 1998, I taught a module called "Facility Design and ALARA" at ORISE (the partly DOE-sponsored training and documentation contractor). This course was for people from the nuclear power plant world (an enthusiastic bunch) and I was asked to teach the module because the regular instructor was absent. I was flattered to see that his text was essentially my work, obtained (legally and appropriately) when the ORNL ALARA course for engineers was transferred to DOE's TRADE organization some years before. I updated the text for ORISE before giving the module. I did this teaching as a professional courtesy (i.e., ORNL was not reimbursed for my time, as Mei and I agreed was acceptable).

The Surface Impoundments Decision by DOE: Politics Trumps Technical Truth

In mid-July 1997, Robert Dean of Engineering visited me to ask my assistance regarding a decision that DOE was about to make. We in AEG liked Dean because of his intelligence and his good safety attitude. I felt an extra rapport with him because he was an engineer. In presenting his case to me, he made the argument that the decision that he feared DOE was about to make was not cost-effective from either a money or a dose point of view. This was obviously an ALARA type of question and that is why he asked for help from us.

The question was of choosing one of several options for remediating some surface impoundments (holding ponds). Only two options are of concern to us here. One was remediating promptly (within a year) so that the radioisotopes would be completely removed in short order. The other was drying and sealing over the ponds and then letting them sit for, say, 30 years; this would allow the radioactivity to decay significantly and dramatically reduce the eventual dose to the cleanup workers. Because protective measures for the earlier and much higher dose rates would be more expensive, waiting would be cheaper even with the sealing up and the "babysitting" for the waiting period.

The dose estimates were 36-39 man-rem for the no-wait option and 13 man-rem total for the wait option. Mei told us that it was not Sims' call; it was Tim Myrick (who I believe was in the Radwaste division at the time) who had to make a recommendation to DOE about this. Mei said that in fact Sims protested the choice of the no-wait option and pointed out the dose consequences to Myrick, but this was "not his [Myrick's] thing" (to be interested in). Mei added that DOE might have talked with Frank Kornegay (ORNL ES&H head) and Myrick, but DOE certainly didn't talk to Sims -- i.e., not to the head of ORP, who was presumably the voice of rad protection authority and expertise at ORNL.

I think that Dean and his associates had made this case to DOE also, but the State of Tennessee was pressing DOE for an immediate remediation. Mei said that the local Site-Specific Advisory Board (SSAB), composed of local-area residents and including many technical people, had accepted the no-wait option without comment or argument. I must point out that I am mostly cheering the State on in its efforts to get DOE to live up to its commitments, particularly those included in the Federal Facilities Agreement that DOE entered into with the State. And I respect the SSAB, which historically does not seem to me to be a rubber-stamp committee of "tame" citizens DOE can count on, nor on the other hand a bunch of wild-eyed anti-nuclear types. However, in this case I agreed with Dean that the State and the SSAB were dead wrong. There was to be a public hearing at which people could express their preferences for one or another of the options. I told Dean that I would speak to the issue there.

I got the dose rate and isotope characterization data and dose estimates from Utrera and the engineering descriptions from Dean. I did some calculations with regard to doses and so forth and then prepared a short statement on why the seal-and-wait option was preferable. This all fell squarely in the realm of radiological optimization, i.e., the determination of the optimal choice from among various choices. The reader should note that the State and even DOE may not have had a health physicist or rad engineer give more than a cursory look at the optimization question.

On 15 July 1997, I attended the evening surface impoundments public meeting and read my technically based statement about the inadvisability of the DOE-favored option. I tried to read it so that the listeners - - hearing the numbers rather than seeing them -- could follow my arguments. For comparison, I pointed out that the additional dose from the no-wait alternative amounted to a near-doubling of the annual ORNL dose (which was then about 45 man-rem). Further, over the one year of the remediation, the estimated 36 man-rem would amount to 2 rem to each of 18 people, or 1 rem to each of 36 people. This would be a high individual dose by ORNL and DOE standards, considering that only 1-2 ORNL people per year (and sometimes none) got 1 rem and that in each of the years 1993-1994 (the latest data at the time) 40-90 people in the entire country got 1-2 rem annually in DOE work and very few if any got 2-3 rem. Thus giving 36 people 1 rem each would not be typical of ORNL or of DOE work and giving 18 people 2 rem each would be far outside the modern DOE experience for a single project.

I also pointed out, semi-seriously, that if they had to spend money now, they would do better to seal the ponds, put the money in escrow in the money market or a certificate of deposit for the waiting period, and then at the end of the period remediate, with probably money left over. (\$20 million invested at 8% for 30 years would give \$200 million.) After all, it was not likely that ORNL would close in that period. I concluded that the wait alternative was thus better for both workers and the public. But the DOE people present said their hands were tied because the State had them over a barrel: the State had veto power over the decision under the Federal Facilities Agreement. The State rep(s) present did not contradict this.

So despite the technical demonstration of the optimal choice, both before the meeting (by Dean et al.) and at the meeting (by me and others), DOE picked near-term remediation. Politics won out over economics, ALARA, and even common sense here. But I believe it would not have happened if DOE had done the right thing -- if DOE had gotten its ducks in a row early on and persuaded the State and the SSAB on sound technical grounds, as they could have if they had cared to and if they had been capable of appreciating the technical aspects themselves. I certainly think this case says something to remediation workers and to taxpayers about how DOE handles matters that affect their interests.

AEG Meeting of 14 July 1997

Utrera said that he had "fallen by the wayside" regarding the BVEST project, i.e., he was not being kept informed by AEA. He heard that AEA was saying it intended to do this project without using ORNL's rad protection procedures. An Engineering manager seemed to think AEA would use MK-F's rad protection procedures -- but MK-F was using ORNL's procedures at the time. Utrera also said that ORNL was to

supply oversight, but it was unclear whether ORNL or MK-F (which supposedly was overseeing the subcontract) would do the rad reviews. Utrera asked a DOE-ORO rad protection person about this, but she said she would have to ask a DOE-ORO project manager about it. Utrera said that everybody was confused also because ORNL was supposed to cover one part of the project, MK-F another. Since the project was supposed to start in several weeks, I urged Mei and Utrera to speak with Sims about this and to get him to light a fire under the Engineering manager to pursue a resolution. Also, Mei said that the privatization "request for proposal" for the important Melton Valley TRU (transuranic) waste processing project was to be reviewed at a public meeting. This RFP was supposed to have been sent to ORNL ES&H for review before it was issued, but it was not.

Again, the sort of "who's on first" confusion seen here was a fairly frequent occurrence at ORNL once DOE started to have multiple prime contractors and to "privatize" work. This lack of understanding of who was providing what, only several weeks before the project was due to start, was not only very inefficient but also evidenced deficiencies on the part of DOE and ORNL management, i.e., DOE's inattention to delineating the contractor-contractor interfaces (such as designating the governing documents) and ORNL's reluctance to take a stand and refuse to provide any oversight unless the interfaces were clear. And again, there was the almost casual bypassing of ES&H review in the case of contract-related documents.

Illumination from Gregory

In July 1997, Don Gregory, after attending the recent Health Physics Society annual meeting, circulated his notes on the talks given there. I sent him a message asking for clarification of some of the notes. First, he had quoted a noted nuclear industry defense lawyer as stating that a company doing rad work should dispense with "ALARA rules" and just have an "ALARA program". In clarification, he further quoted the lawyer as stating that once a company sets "ALARA thresholds", it has in effect created an ALARA rule, has set a limit that a jury could take to be what the company has accepted as a danger level. Thus if a person receives a dose over the threshold, the company "has automatically harmed them by its own admission". This was to be contrasted to an ALARA program, where every job was planned so as to minimize dose, but no specific limits were established. Gregory said that in his opinion, once a company set goals that they could not exceed without written approval, then it had crossed over from an ALARA program to a set of rules. This could make ALARA the "standard of care" (duty of care), legally speaking.

Second, Gregory had used the term "perceived value" when quoting another speaker about pleasing the customer. I asked whether he meant that ironically. He replied that he did not. Apparently paraphrasing the speaker with approval, he told me that the point of the talk was that if you did the minimum required of you on whatever time scale you chose, you could be easily replaced "and probably will be these days through outsourcing". An outside firm could provide better service for less money, or could claim to. But if your customer thought you were providing excellent service for a good price, outsourcing was less likely to be proposed, and your customer would fight to retain your services if it was. So ideally, "we [ORP] should" provide excellent service and "see that our customer realizes that they are getting excellent service". One way to do that, the speaker suggested, was by asking what the customer wanted, then providing it, "then asking what MORE you can do for them"; "customers stay very happy that way". Rule No. 1 was that "Perception is Reality": "if the customer thinks he is happy, then he is happy".

I doubted that establishment of goals and internal limits would make ALARA the de facto standard of care. Of course, it is the great fear of all companies doing rad work that some judge or appeals court will rule that ALARA is the standard of care, but it hasn't happened yet, to my knowledge. That is because the NRC and DOE laws, regulations, and guidance covering ALARA are carefully worded so as to preclude this interpretation. I personally back the "ALARA is not a standard of care" interpretation and hope that NRC and DOE will always be willing to testify as to the intent of their respective directives. Although I see that any company could fear being held to its internal ALARA limits as the standard of care, instead

of the regulatory limits established by NRC and DOE, I think that eliminating ALARA goals and internal limits is irresponsible from the point of view of consistent control of dose and execution of procedures.

With regard to the please-your-customer talk, this was one of the earliest times that I had heard some of these statements, but it was by no means the last. The real message, it seemed to me, was that while intellectually you want to provide good service -- and truly protect the worker -- keeping your job is of utmost importance. So you need to make the customer think you are doing a good job and, especially, like you. You shouldn't leave it to him to call you when he needs you; you should suggest that he might need more help; you should be solicitous, even kiss up if you have to. The phrase "Perception is Reality" was being widely used at the time by high management types, while the peon-level scientific and technical folks widely mocked it. There was a grain of truth to the concept, but as one astute person pointed out, was what a schizophrenic person perceived also "reality"? I thought that if the customer was inclined to favor giving his business to a bunch of schmoozers or gladhanders, rather than to a group with known technical chops and a good track record in rad protection, then he wasn't really interested in protecting his workers but in maintaining a shiny happy low-stress workplace for himself; if he was not inclined to favor schmoozers, then the schmoozer approach would just backfire. Not only that, to make your professional behavior conform to the schmoozer model would be to make the customer's perception of reality your reality and would be to deny your professional knowledge and ethics. I believe that this actually happened at ORNL once the customer service model took hold.

The Safety and Health Evaluation and Support Team (SHEST)

As I noted earlier, SHEST was a team of people assembled to specialize in working on one area: construction activities. The rationale for having a special group was said to be that fast and dedicated safety response was essential for construction work. There was one ORP member -- a rad tech -- and there was at least one member from each of the other relevant safety disciplines. The SHEST people were isolated as a team: they were off by themselves in a trailer, away from their supervisors and their peers, and seemed to have relatively little interaction with their discipline groups. It seemed to me that this would make it easier to evade requirements and shortcut procedural safeguards, if and when project pressure was applied. It also seemed to me that since construction was a very engineering-oriented activity and the only ORP member was a rad tech, frequent contact with AEG would be essential.

But AEG was excluded by SHEST. At one AEG meeting in 1996, Geber unhappily noted that the SHEST leader, who was not qualified as a rad protection specialist, wrote the rad protection requirements for subcontractors on one SHEST project. In 1997, the SHEST leader had signed off on some item that Mei was supposed to sign as AEG head. She remonstrated with him and he promised not to do it again. The reason he gave for signing himself, Mei told us, was that "they" (the operations people) wanted him to complete the documentation right away. It seemed improbable to us that SHEST called AEG each time a review was needed; more likely, here was yet another area of widespread RPP-310 review avoidance.

This was underscored in July 1998 when draft Procedure ORNL-ENG-005, "ORNL Construction ES&H Requirements Identification and Oversight", appeared on the Web for comment. Its purpose was to "state the process for identifying ES&H requirements" for construction activities and the level of oversight needed to verify compliance with Work Smart Standards and contract requirements. The statement was made that the bases for health and safety compliance and oversight on construction projects would consist of the Work Smart standards, subcontractor health and safety plans, and like documents. But no mention was made of the ORNL rad protection procedures or the 835 rad protection program plan. The statement was also made that the SHEST representative, ORP, and other entities "shall" identify ES&H work requirements. Finally, the statement was made that the project manager, the construction field representative, the SHEST representative, and the facility manager "shall determine the extent of ES&H oversight and how that oversight will be accomplished". I believe that the final version of the procedure was essentially identical to the draft, including the statements above. This procedure thus appeared to

bypass the rad protection procedures, in that they were not cited as bases for decisions about oversight and compliance; rather, there appeared to be an ad hoc determination by people who were not rad protection professionals.

AEG-RE Meeting of 21 October 1997; A Note from Sims

In the internal assessment of the Engineering Technology Division that Utrera had recently participated in, it was found that one rad tech (who was still working in the area after the audit) did not have a good grasp of the rad protection procedures, e.g., he did not know about the requirement to post survey maps at the front of the entrance to rad areas and had posted some on the back of the door or not at all. Also, although one worker's rad worker training had expired two months earlier, the worker was still doing rad work. RSS and line management were supposed to be responsible for checking whether workers were up to date in their rad work training before they did rad work. The ORP data base for keeping this and other information was up and running, but it was hard to keep it completely current. The poor ORP person who was responsible for maintaining it took a lot of flak -- unjustly, in my opinion -- because status checks weren't always available at a moment's notice. Thus some rad techs and DRCOs who called in the morning with a long list of workers whose training status, bioassay status, etc., needed to be checked because they weren't on the data base would get impatient if they had to wait until the afternoon or the next day for the answers: the job "had" to start that very morning. The workers' supervisors supposedly knew when they had sent their workers for training or bioassays; the workers got a dated card when they finished training; and every division had a training officer who could keep additional records of all training and a DRCO who could have kept bioassay records. But somehow for years it was ORP's responsibility alone to keep up with this. I did think it was better to have ORP maintain the official central registry for training, bioassay, etc., but it was unreasonable for other divisions to expect instant response from ORP when they themselves had failed to allow sufficient time for the checks to be done.

Mei also reported on the problem of PNADs (neutron accident dose TLDs that are hung in areas rather than on people). The PNADs were supposed to be changed annually by the rad techs, but it was not being done, so external dosimetrist McMahan was working on correcting this. Note that the use of PNADs was a DOE requirement for certain areas. The failure of multiple rad tech complex and group leaders to have these changed annually demonstrated that they were focussed on tech subjects, such as covering rad work and the details of local surveys, and not on the larger accident aspects of running facilities. This would have been a good task to transfer to Source Control, because they got around to the entire Lab and because as a small but close-coordinated group they could do a large-scale collection efficiently. Alternatively, McMahan or a fellow external dosimetrist could have gone around periodically with one of the shift rad techs and made the changes. But RSS never gave up any work, particularly if it was viewed as a complex-owned responsibility, however inefficiently it might be done and however illogical it was for them to have it. I believe that no occurrence report was ever issued for this requirement violation.

I believe that Mei also discussed the memo that Sims had sent all of ORP about two weeks earlier. In it he stated that "it is expected that we follow our procedures, both site-level and our own SOPs". If an ORP person believed that a procedure was incorrect, couldn't be followed, or wasn't being followed, he was to bring it to the attention of his immediate supervisor or "an appropriate manager" and to provide enough details that the matter could be cleared up or the procedure changed. But he then went on to say "Let's remember that our primary customers are those folks for whom we provide radiological protection services". (He did not specify whether he meant the workers or the management of the division or project.) He thanked us for "your continued hard work during this time of much uncertainty and change".

AEG Seminar on Optimization

In November 1997, I gave a talk on optimization as part of the AEG seminar series, to which all of ORP was invited. Mei's intent in this series was not only to enhance the technical knowledge of AEG people (by inviting people from outside AEG to speak on, e.g., dosimetry), but to show other ORP people what

AEG's knowledge base was (by having AEG members give talks on special areas). Rad tech complex and group leaders were especially invited to attend and bring their people. We usually had a good attendance from the records and dosimetry section and even from outside ORP, but few RSS people ever attended.

The Frog Story and the Forged E-Mail Message

In the early 90's, a rad tech surveying drains on an ORNL street noticed that his detector was giving a above-background reading near something lying in the street. It was a frog that had been flattened by a truck. Several other "hot" dead frogs were found. A Radwaste manager issued a serious advisory notice, stating that if a dead frog was found in your area, you should have ORP survey it and if it was found to be radioactive, you should "manage the frog as radwaste". This tickled everyone's funnybone. Chris Scott, a Radwaste division manager, aided and abetted by his people, put out a hilarious mock menu for "Scotty's Roadkill Cafe" (named after him), featuring such delicacies as "Fricassee of Frog".

The frogs were hot because they ate bugs that had hatched over at the radioactive settling ponds. Over the years, hot mud daubers' (wasps') nests, hot geese, and hot deer were found, the latter during the annual Oak Ridge Reservation hunt. In the fall of 1997, a dead frog was found in some metal that had been shipped from ORNL to Savannah River, another DOE site. The receiver company apparently did not have any rad protection service (the metal was supposed to be clean) and they worried that this frog might be hot. So ORNL sent an RRD person with a detector to check the frog. It was clean. (Or at least so we assume -- RRD didn't send a rad tech, who could officially "clear" material, but an RRD engineer. He probably used the detector correctly but he didn't have official authority to clear material per DOE rules.)

The whole sequence of events from the first frog to the last one inspired me to write a story called "The Life and Times of Dermot T. Frog". This was supposed to be a recounting of ORNL events from a frog's point of view and it included a version of the Scotty's Roadkill menu, by permission. I submitted the story to the ORP newsletter, The Gamma Gazette, and they published it. I didn't expect everyone to like it, but I certainly didn't expect what happened next.

On 26 November 1997, I got a "Returned as Undeliverable" response to an E-mail message that I had sent. But -- I hadn't sent any such message! One could see from the text that it was purportedly a message from me to a HFIR rad tech, ridiculing him for denigrating my frog story. I reported this forged message to the ORNL central computer system organization (which was in charge of the ORNL network) and to our computer group leader, Bishop (who was in charge of the ORP network). Soon Bishop told me that a HFIR rad tech had forged my name on the message and sent it to another rad tech. But the culprit had mistyped the E-mail address of the recipient and thus the message had bounced back to me.

Bishop explained to me how the forgery could have been done. I was amazed at how easy it was; I pointed out that anybody at ORNL could forge anybody else's name on any message and send it out to everybody in the world. This would include, e.g., the name of Dr. Alvin Trivelpiece, director of ORNL. Bishop agreed that it was possible, but he shrugged it off. I couldn't understand his "oh, well" attitude -- it seemed awfully tolerant for him. Bishop did speak either to the offender or to someone in the RSS chain, because the offender called me to apologize. I'm sure it was at his management's behest and not his own idea. He sounded more rueful than repentant, but I decided to take the high road and be gracious. (I am not giving his name here even though he deserves to have his mother find out what he did.)

I asked both Bishop and Walter Dykas of ORNL Computer Security to (1) correct the security loophole that allowed this forgery (and potentially worse ones) to occur and (2) have an ORNL computer security person speak at an ORP safety meeting about computer security and the problems with behavior like prank forgeries. Dykas told me he would like to come and speak and he seemed to take this matter very seriously. I told Mei, Mlekodaj, and Sims that "Dykas was willing". But nothing came of the suggestions: the forgery and even the general topic of computer security were never mentioned in any all-ORP memo

or in any of our safety meetings (where administrative matters were usually discussed). I never heard that the rad tech was punished or reprimanded in any way. As in the case of the Dilbert cartoons of the previous year and the repeated barrage of aggressive and hostile comments in the Suggestion Box and elsewhere, harassment of AEG people by RSS seemed to be tolerated and even condoned.

AEG-RE Meeting of 13 January 1998

Utrera talked about a project in which a hose was to be laid in a trough to convey radioactive liquid, with a float valve to detect leakage. He called the hose a "containment". Because "containment" meant something very specific in the DOE world and there were requirements associated with containments or the lack thereof, I asked Utrera what basis the project people had for calling the hose a containment. He said he didn't know; he didn't even know if the hose was rigid or not. I noted that over several weeks of ultraviolet light (from the sun), temperature changes, etc., the hose could deteriorate and, e.g., blown-down branches could damage it. Geber added that snow or ice build-up could also result in deterioration. Utrera did not know how sound the hose was or what integrity requirements it needed to fulfill.

Utrera was always the least intellectually engaged of us four rad engineers. As the years went by, Geber and Mei and yes, even I seemed to learn a lot, to "grow" in our knowledge, but Utrera seemed content to get to a certain point and then quit. This meeting demonstrated Utrera's almost unquestioning acceptance of what the project people told him, while Geber and I were alert to potential defects in the plan. Mei, as usual, did not comment much if at all on these technical engineering-type questions although she, as group leader, was supposed to adjudicate technical differences of opinion. Still, she did listen carefully to these discussions and encouraged us to dig out the answers to technical concerns. I don't know if Utrera ever took this particular concern to the project or checked out their responses for credibility.

The Visit by the Russians

In March 1998, I talked with Dr. John Baum of BNL's ALARA Center. He was hosting a visit to the US of two Russian nuclear navy officers, two Scandinavian nuclear people, and a translator -- they would be in Oak Ridge in May and he wanted them to visit ORNL for an afternoon. He asked if I would act as host and show them around. Mei and Mlekodaj had no objection, but Sims gave only reluctant permission. I arranged tours and visits for this group and also arranged for the Russians to speak (about, e.g., the reactors on their nuclear subs) that evening at a special meeting of the local chapter of the Health Physics Society. Mindful of Sims' reservations, I took as little company time as possible. All our ORNL people treated the visitors well, except for REDC. I got a nice thank-you from Dr. Baum for this and a kind note from the excellent translator, while the Russian and Scandinavian folks seemed to have had a good time.

Evangelizing for Sound Safety Management

In early April 1998, I sent a memo to Mei, copy to Mlekodaj, stating that ORP was what I called the "institutional rad protection organization". I pointed out that AEG performed its reviews by procedural requirement and that thus line management (i.e., the O&R divisions) could not simply hire a subcontractor to do these reviews. I explained that having an independent institutional safety organization is based on the well-established principle that the safety organization should be independent of the operational groups (e.g., the safety organization answers to the Lab director or his designate, not to the operating groups directly). I also enunciated the idea that ORP's first "customer", from a safety point of view, was not line management, but (although they don't always realize it) the workers and the public being protected. The second customer in line would be DOE (for compliance reasons); then ORNL as a whole (i.e., as represented by Lab management, for the best interests of the Lab, legally and otherwise); and finally line management. This was not the first time I had said this to Mei and Mlekodaj, but it was the first time I put it in writing. I told them this again and again over the next two-and-a-half years.

AEG-RE Meeting of 13 April 1998

Mei told us that Mike Harrington was now the Spallation Neutron Source (Accelerator) ES&H contact. She noted that Steve Hamley of RCS (of our same section but not our same group) was on the general ORNL accelerator review team, and also John Alexander of ONS. She said that a "design manual" had been produced for the accelerator but it had no statement as to when radiological reviews would be done, such as at the conceptual design stage. I pointed out that RPP-128 required a review of modifications that have radiological impacts, such as the existing accelerator should have had. Mei had no answer to that.

We were surprised at the appointment of Harrington as ES&H coordinator. Harrington's background was in reactor design and particularly reactor accident scenarios and he did not appear to have any background in the sort of operational and task-oriented areas that ORP and OSHP covered. He was a conscientious person, but I thought that perhaps the job was given to him to support him (i.e., as a basis for him to charge out). But I was not surprised at the failure of the design manual to deal with rad reviews. As I stated earlier, the HRIBF accelerator modifications had never had an RPP-128 review, even after we had pointed out the requirement to the HRIBF people. When Mei took over as the AEG rep to HRIBF, she did not push review issues. She told me that "the facility people [line management] decided they didn't need a review". Mlekodaj commented to me that 10 CFR 835 did not apply to accelerators, but I pointed out that the review requirement in RPP-128 was not limited to just 835-applicable facilities. He did not deny it.

In this meeting too, Mei stated that Utrera should train Geber and me to issue electronic personnel dosimeters (EPDs) from the AEG EPD workstation. Utrera had been chosen as the EPD point person in ORP (why, I don't know). Since for several years he was on a "four-by-twelve" schedule (working 12 hours per day, 4 days per week and having the other three days off), he was out every Friday and a backup was indeed necessary. But despite reiterations by Mei several times a year over the next two years or so, Utrera never trained Geber and me to issue the EPDs. I don't know if the motivation on Utrera's part was that he wanted to keep this operation "his" and not share it, or if he just didn't want to bother. In any case, it was a link between him and the rad techs since they were the ones who asked for and picked up the prepped EPDs. Mei's motivation in letting him beg off month after month seemed to be that she wanted to avoid giving him a direct order, to avoid having to "make" him do it. This unwillingness to confront a subordinate, even when his behavior was clearly counter to the best interests of the group or the customers, was a sometimes frustrating and sometimes endearing characteristic of Mei.

AEG-RE Meeting of 6 May 1998

At an earlier staff meeting, Mei had said she would poll our "customers" on our performance. The criteria were to be (in this order) customer service, professional approach, technical results, and feedback. Now she asked Geber, Utrera, and me to send her a list of our contacts (i.e., project and facility people we had worked with) so that she could do the poll. She also said that the upcoming ORP budget cut might be 12% and that ORNL management wanted ORP to increase the chargeout-to-overhead ratio. This meeting was eight days after Utrera was assigned to provide rad support to the HFIR outage -- with the stated reason being that RRD would outsource the work if he were not assigned. Clearly, the significant proposed budget cut and the consequent pressure to charge out were a club that the O&R divisions could now use.

AEG-RE Meeting of 11 June 1998

Mei told us that AEG was running right at budget (in expenditures), so there was no "fat" to pay for the proposed MCNP (computer code) course at \$1000-1050 per person. (But Utrera asked if he could still take a "Seven Habits of Highly Effective People" course.) In view of the position that MCNP held in the shielding world, its flexibility, and its relative ease of use, we thought that learning how to use it would be advantageous. Mei had found out that if AEG hosted the special MCNP course for health physicists and got enough people to attend, the trainers from Los Alamos National Laboratory would come to us to give it. This would keep costs down, but it would still be expensive, so it would be our one trip/course/extracurricular activity for the year. Mei did succeed in getting enough other students to sign up for the

course, which was held in September 1998. Mei, Geber, Utrera, and I all took it, but I seem to have been the only one to use it after that, with the possible exception of Mei. It is a truism of any computer program that you need to practice using it for at least a while after you have taken the course, otherwise you forget what you learned. Hence taking the course may have been of value only to me. Similarly, complex leader Bryce Powers and REDC rad tech Keith Waggoner also took it with us, but only Waggoner seemed to have used the code subsequently (for REDC calculations that also formed part of his master's thesis).

Mei also said that in the ALARA Steering Committee meeting, Division Directors Dr. Marvin Poutsma of Chemistry, Hal Glovier of RRD, and someone else stated that the budget committee they were on had concluded that it was more effective to have a central organization (i.e., ORP) do rad reviews, etc., rather than having "their own people" do it. This conclusion was probably based on financial and practical considerations; I do not think the division directors were referring to the central organization's greater independence as part of the advantage. Still, it was nice to get this statement on the record.

The ORNL Ethics Survey Focus Group

In May 1998, Lockheed Martin Energy Research (i.e., the contractor that managed ORNL) did an employee survey emphasizing ethics. This survey showed a surprisingly high percentage of ORNL folks who did not think highly of LMER as an ethical company. So in June, employees were selected, supposedly at random, to participate in focus groups to discuss the results of the survey. There was one group for the hourly workers, one for the weekly workers, and one for the monthly workers. I was selected to attend the monthlies' meeting.

Some observations in this meeting were that "Some leaders are not modeling desired behaviors"; that there was "more of a company policy (statement)" about ethics than there were actual ethical standards; that "Leaders must be condoning the lack of commitment to ethical standards since employees are not seeing the commitment"; that those who had "behaved inappropriately" might still get rewarded or promoted; and that there was "no [apparent] discipline for bad or inappropriate behavior". It was also stated that the application of "situational ethics" seemed to be trending upward and that the idea of the "public hanging" had merit in showing other employees that misconduct would not be tolerated. I told the group about the forging of my name on an E-mail message and about how, as far as I could tell, the forger was not punished or even reprimanded and nothing was done to correct the computer system weakness that allowed messages to be forged. The other people at the meeting seemed shocked by the incident but were not surprised that nothing had been done. I said that I didn't want revenge, but to know that misbehavior was not condoned. The other people said something like "Hear, hear!"

At this meeting, a researcher got in some digs at safety people. He made a statement that has stuck in my memory: "We [researchers] know how to work safely". He implied that they thus didn't need as much safety coverage and safety rules as were in force. I did not challenge him but said something conciliatory about the safety people wanting to support the work and not get in the way. But his statement was a crystallization of the resentment that many of the O&R people had toward the safety people; they believed that they knew as much about their own situations as the safety people did, that they had an adequate commitment to safety, and that the safety people thus had little to contribute. Many of the O&R people looked down on safety people as somewhat inferior intellectually (or at least as just bystanders on the field of work) and as leeches, sucking off funds without tangible return. Sometimes I thought I should have a tee shirt made that said "Another Safety Parasite".

Westbrook-Mlekodaj Talk of 9 July 1998

On this occasion, as many others over 1996-2000, I spoke with Mlekodaj about rad engineering and AEG problems. I emphasize to the reader that Mlekodaj was well aware of the compliance problems that AEG and I personally were having with line management. Mei and Geber also spoke with him about this on multiple occasions. An intelligent man, he always realized very quickly the implications of this or that

action. But his response to problems, as he expressed it to us, was usually that his hands were tied and that he could do nothing. This was not comforting, but at least we knew where we stood.

AEG-RE Meeting of 5 August 1998

Mei noted that AEG's budget didn't cover our salaries and other base expenses, therefore we needed to charge out 10-20% (compared to 15% the previous year). My notes say that there ensued "a mind-boggling pseudo-discussion regarding this best of all possible AEG worlds", i.e., Mei was presenting the Pollyanna view of bad news by trying in unrealistic ways to say that management's decisions were for the best and that everybody just had to accept quietly that this was the way things were. Mei then gave a new reason why monthly employees should put into the ORNL timekeeping system their extra time worked even though they weren't paid for it: ORP would get some sort of credit for this. For example, Mei said, if 45 hours were put in instead of 40 hours, the extra 5 hours gave credit to a common pot. However, she said, AEG got only 1% or 2% back. We asked why any entity (e.g., ORP) got credit if the employee was paid for only 40 hours; Mei was unsure and evasive, saying that the timekeeping system "has problems".

Earlier I mentioned the "half credit for overtime hours over 4 hours" rule. Then we found that there was also a rule that if a monthly person routinely worked, say, 45 hours, that was his baseline time to start from to count overtime, rather than 40 hours. Now we were hearing about some pot into which we donated but from which we seemed to get very little. I pointed out to Mei that if the employee put in extra hours every week into the system (say the 45 hours), then he was establishing his baseline as being above 40 hours, thus being forced, by his own "testimony", to accept a higher baseline for comp time determination purposes. Under these circumstances, I asked, why should anyone want to document any time more than 40 hours? She did not know. Mei seemed to have a lot of trouble explaining all the timekeeping ins and outs. I don't think this was her fault -- the story the supervisors heard seemed to change almost every week and they thus gave us conflicting accounts of what to do. Mei told us that we should charge as little time off as possible and should make up time where we could instead of taking personal time or limited sick time (e.g., as visits to the doctor). I had always been doing this anyway, but, disgusted at the confusing company rules, I made extra-sure that unless I was sick most or all of a day or had an all-day personal commitment, I made up every single minute of time I missed.

AEG-RE Meeting of 14 September 1998

Mei told us that Source Control must soon charge out entirely, even to ORP groups such as RSS and the instrument calibration group in our own section. AEG needed to absorb the fetal protection program expense next year (i.e., by charging out rather than doing this on overhead); a small portion could be charged out, e.g., to subcontractors on the ORNL site, but most couldn't be. (E.g., Bechtel Jacobs was not being charged because they were already paying ORP some overhead for rad protection support -- which AEG didn't seem to get any of.) She told us again that everybody in ORP charged about \$46/hour, regardless of actual per-hour rate paid. She implied that one Source Control tech's AEG home page work was not in the Source Control subbudget, but came from the ALARA engineering subbudget. Mei said that "when it comes time to defend our program(s)", AEG should be prepared to do so.

As the reader can see, Mei was always having to try to find ways for Source Control, whose 2.5 full-time equivalents were covered only in small part by overhead, to charge elsewhere in AEG and ORP. As noted earlier, we had the pregnancy surveys only because the tech who conducted them was a Source Control tech. Overhead that supported the pregnancy surveys helped support her (and thus eased the Source Control financial problem); when that overhead was withdrawn, all such surveys had to be charged to somebody. Mei kept trying to publicize the AEG contributions to the ALARA program, e.g., issuing "ALARA Alerts", but nobody seemed to consider issuing alerts to be much of a contribution to safety.

AEG-RE Meeting of 12 October 1998

Geber remarked that the ALARA Working Committee (AWC) was not as active as it was conceived to be (e.g., as per the RCM): they did not check into important jobs, did not really discuss ALARA issues, etc., and attendance was declining. I agreed with Geber and pointed out that contrary to Mei's statement that the AWC chairman was a "facilitator", the chairman was supposed to be the "leader". I termed the idea that AWC meetings should be made more "interesting" so that members would be more apt to attend "entertaining the members". Mei said she was talking only about what AEG-RE was able to do (we could not influence Mlekodaj to do things differently); we AEG members should "just do our job" (even though it might seem pointless). The do-nothing-ness of the AWC had been coming on for years, but had really taken hold when the RCM was made a nonmandatory standard and having an ALARA committee was no longer required. There were active ALARA committees at other sites, but only the ALARA Steering Committee, which met too infrequently to review work in any detail, did anything much at ORNL.

Mei said that there was \$325,000 in the previous year for the ALARA Program, with 10-20% more(?) from chargeouts. AEG's travel in the last year was just Mei's two on-the-cheap trips that totaled about \$300 (she stayed with people she knew instead of in hotels). Source Control had been cut back to 2 FTEs (two full-time positions' worth of hours), so AEG needed to charge out 1 FTE. Mei said that two of the techs "can do stuff for you [rad engineers]", such as home page work that AEG-RE could assign to a Source Control tech to "save you time and give her some [chargeable] time". Such efforts would buy time to "find new jobs for the future". She claimed that if Source Control did not charge out, "everybody would have to cut more" and that "If we help them out now, maybe they'll help us out some day". Thus she was proposed charge-shifting within AEG itself.

Mei also said that according to the original DOE plan, Bechtel Jacobs was to receive legacy waste but the characterization was to be done by ORNL. Then ORNL lost even this work and thenceforth the ORNL Radwaste people would be dealing only with the small quantities of newly generated wastes. AEG might get some of the characterization work, but we would have to "compete" for this with others, including RSS. The "double overhead" problem was rearing its ugly head again, as a Radwaste person had pointed out (i.e., Bechtel Jacobs would add its management overhead onto the total charge by ORNL, which already included ORNL's doing-the-work overhead). Mei said that besides that, Thein (the dosimetry and records section group leader who presided over the external dosimetry work) was not allowed to bid on outside work because of some preclusion from use of government facilities (which, however, didn't seem to apply to other ORNL entities). Mei thought that this exclusion was true of most of the national labs, with the exception of PNNL (run by Battelle), which seemed to have some special agreement with DOE. Mei stated again that the current ORP chargeout rate was \$45/hr – this seemed low, but, she surmised, it was perhaps subsidized by overhead to some degree. As at every meeting, the perilous financial state of AEG and how much in jeopardy our jobs were were emphasized and discussed at length. However, Mei now said in so many words that we were competing with RSS to sell our services. An additional wrinkle was that DOE was telling different contractors different things and contractors with "money to hand out" were playing coy with their plans for it. So there was an element of ORNL selling itself too.

Finally, Mei told me that she needed my performance plan that week -- the same plan that I had tried to give her earlier, when she had told me to hold onto it for some unspecified period. There seemed to be strange things going on with my performance plan, in that I was not allowed to hand mine in for some time even though it was overdue according to the performance plan schedule. Mei would not tell me why. I believe that Geber's and perhaps Utrera's plans were delayed also, but not as long as mine.

ORNL Management Statement Regarding Safety

In late October 1998, an ORNL Safety Bulletin was issued by the Office of Safety and Health Protection that summarized six "near-miss" incidents at ORNL, including two MK-Ferguson skin contamination incidents. It stated that "A widely accepted theory by safety professionals is that many occurrences

representing serious injury potential can eventually lead to a workplace fatality. ORNL has not developed or operated on a posture that the success of our safety and health program is dependent on luck or fortunate circumstances. Each employee has the individual and collective responsibility to follow established procedures governing the application of necessary safety and health requirements in the conduct of work. The need to adhere to these requirements is paramount; any less effort is considered inconsistent with management's expectations of ORNL being considered successful....". (underlining mine). I found this statement to be ironic.

AEG-RE Meeting of 9 December 1998

Utrera said that he was "out of the loop" as far as the HFIR spent fuel transfer operation was concerned. He said that some of his suggestions might be adopted, but that was not certain since the former rad tech who had helped RRD with the resin box calculations and was now helping in the planning of this operation was "a loose cannon". Mei stated that on the Spallation Neutron Source, ORNL was responsible for the final review of the linac (linear accelerator) and that Don Gregory was involved as the instrument person and the radiological accident scenario person. I was not very surprised that even the go-along-to-get-along Utrera was having problems being taken seriously; the former rad tech was a person who seemed to talk a good game, but not necessarily to play it, and he was obviously telling RRD what it wanted to hear. But I was surprised that Gregory was the accident scenario person. He had been a physics researcher for years before coming to ORP and he was the chairman of the Health Physics Instrument Committee, so he was an understandable choice for the instrument specialist. But it was not clear what expertise he might have in the area of accident analysis.

AEG Meeting of 11 December 1998

Mei handed out a sheet titled "ALARA Engineering" that listed ORNL ALARA Program elements, rad engineering functions, and Source Control "services". She said that "they [ORP management] want support, not overhead" (i.e., they wanted chargeout by AEG). We were supposed to comment and suggest ways to present our services.

AEG-RE Meeting of 14 December 1998

Mei stated that if project and facility staffs could do things themselves, they would not call AEG. Mlekodaj had told her that AEG should "Do what you need to do to get the job", but gave her no real directions as to how to do it. Mei's statement seemed to me to be a non sequitur because it implied an element of choice. It was true that the O&R divisions could do some things themselves, but these had to be things they were permitted to do -- not, e.g., RPP-310 reviews that were procedurally assigned to AEG (and that 835 implementation guides said should be done by rad protection professionals). But I was sympathetic to her frustration with Mlekodaj, who kept telling her to increase AEG chargeout without telling her how. In O&R divisions, section heads often were also business development people, beating the bushes for work for their people; Mei was always trying to do that for us, but Mlekodaj seldom did.

Mei questioned rhetorically how REDC could continue to ask for ALARA (dose) goal increases with so little of the professional ALARA staff's involvement. This was the "\$64,000 Question". Here was the top dose-getting facility at ORNL, both individually and collectively, yet it had the least rad engineering and ALARA staff involvement. I would go Mei one better: how could ORNL management be serious about safety and dose reduction and yet permit such a facility to be so exclusionary of safety people?

Mei said that John Alexander was to retire in March 1999, so he and Geber (his glovebox understudy) needed to revise the glovebox procedure before that. Geber was "too busy" to oversee the gamma spec work, so for "professional supervision" of the work and of EPD work, Mei said that AEG could hire a subcontractor "two days a week". Mei noted that rad tech complex leaders Gonzalez and Willie Hayes were to supervise the RSS gamma spec work -- but Hayes had yet to be trained in gamma spec'ing. Once again, I was amazed that ORP management would allow complex leaders to supervise the use of a gamma

spec instrument for stuff destined for free release, if they themselves had little or no experience in the use of such an instrument and its limitations. But it was even loopier to hire someone from outside to "supervise" the work. For technical consistency, gamma spec work would be better done out of one AEG center, rather than multiple rad tech complexes. I suggested that Source Control or a part-time loaner from RSS be used as the gamma spec tech(s), but under AEG supervision. Since higher ORP management seemed bent on having rad techs do the actual scans, I thought that having dedicated, trained techs -- like the pregnancy survey techs -- would be the way to go. Such an assignment could represent a plum, a step up for a worthy rad tech or two (and a good resume builder as well), given their limited promotion potential. But ad hoc "point and shoot" training seemed to be all that was contemplated.

AEG-RE Meeting of Late 1998

Mei handed out a new draft list of our assignments. She was now the lead rad engineer on MSRE, the Spallation Neutron Source, and HRIBF, in addition to managing AEG; Utrera had the HFIR beryllium outage, the gamma spec work, EPDs, and one geographical ORNL area; Geber had another geographical ORNL area, gloveboxes, and REDC (see the next chapter for why he never actually got to take over the REDC work); and I had the rest of the HFIR work, unspecified "reactor projects", the ALARA course, "project review", and validation and verification of the computer codes we used.

As the list above shows, most of my significant project and facility work had been taken away from me. Much of my work now was deferrable, noncritical work done on overhead money. It can be seen too that the work on what might be termed the continuing projects and facilities was not distributed evenly among the group -- Mei and Utrera had nearly all of it except for REDC. Besides that, Utrera was now supposed to be Mei's backup on MSRE. However, he was off on all Friday afternoons (having finally given in to pressure to adjust his schedule to work on Friday mornings because of an important weekly HFIR outage meeting) and Mei was also off most Friday afternoons for family reasons. Thus the assignment of Utrera to a major HFIR project and to MSRE as a backup was inappropriate, especially regarding responsiveness to customers. To ensure 50% personnel coverage as per higher management directive, Geber, Mei, and I constantly had to coordinate our schedules before we could take off a Friday -- because Utrera, in effect, had dibs on all Fridays. Mei always said she was willing to be there on Friday afternoon if Geber and I both wanted or had to take it off, and she said she "would speak to" Utrera about staying if needed. But as best I can tell she never even asked that Utrera -- the most junior one -- give up the day so that one of us senior three could take it off.