Oak Ridge Technical Enterprises Corporation – ORTEC: how it all began

(As published in *The Oak Ridger's Historically Speaking* column on November 22, 2011)

As we continue looking at the history of ORTEC through the eyes of Harold W. (Hal) Schmitt, we will see the origin of the idea and how this first successful technology transfer experiment actually took place, step by step. Remember, this is from Hal's perspective and gives insight into the learning curve at the lab for spinning off new companies. It was like many other new things tried first at the Oak Ridge National Laboratory, some of the policies and rules had to be created as the experiment began.

Hal says, "This story begins in the late 1950's at the Oak Ridge National Laboratory. It tells about the events leading up to the formation of Oak Ridge Technical Enterprises Corporation (ORTEC), including the decision to form the company. It is a saga involving technical and administrative issues at Oak Ridge National Laboratory, and also perceived, as well as misperceived, ethical, cultural, and policy issues."

"Silicon surface barrier detectors had been developed in the ORNL Physics Division by John Walter and John Dabbs during the middle to late 1950's for their work on the emission of alpha particles and fission fragments from aligned nuclei. James Blankenship, in the research group of the Instrumentation and Controls Division at ORNL, was studying the detectors and techniques for their fabrication, from an instrumentation perspective.

"In 1959, I became interested in them for research on the mechanism of fission in my group in the Physics Division. The possibility of high-resolution measurements of the energies of fission fragments was exciting for this field and seemed to promise new and sharper insights into the mechanism of fission than we had at that time. John Neiler and I joined forces, and John Walter joined in with the detectors themselves. Preliminary experiments gave highly promising results and we began this work in earnest.

"We learned fairly early in our work that detectors of a similar nature were being developed at Chalk River Laboratories in Canada and at Bell Laboratories in New Jersey. However, both of those groups were developing diffused junction detectors, as opposed to our surface barrier detectors.

"Surface barrier detectors had a much thinner "dead layer," i.e. an inactive surface layer that would absorb some of the charged-particle energy before the particle would enter the pulse-producing region of the detector. Since our interest was in fission fragments, which are highly-charged heavy nuclei, whose energy would be strongly absorbed anyway, a minimum "dead layer" was essential, giving the surface barrier detectors a huge advantage for our work.

"As we began this work, we soon found that the detectors were subject to damage after a time, sometimes from extended radiation and sometimes from ordinary physical handling in the laboratory, and therefore had to be replaced from time to time. Dabbs and Walter, also Blankenship, had their own research interests and, after making the first few detectors for our preliminary experiments, did not want to be distracted by making detectors for other groups.

"My laboratory had some of the required equipment and we were able to procure the rest, so that we could set about making detectors, learning the subtleties and 'tricks of the trade' from experts Walter and Blankenship as we went along.

"Actually, the person who learned and developed detector fabrication in our laboratory was David Peach, the technician who worked in my group in the Physics Division. I had asked Dave to take this on as mostly his project, which he did in a superlative manner, becoming expert in detector fabrication himself.

"As the attributes and value of the new detectors became known, other researchers became interested and designed experiments for their own specific studies. Since we had the capability, we were glad to provide detectors to our colleagues. And, since the detectors required a special low-noise charge-sensitive preamplifier, we provided the circuit diagram for that as well. The preamplifier design originated with Ed Fairstein while he was at ORNL, in work with Dabbs and Walter.

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"Soon, through word of mouth, published papers, and participation in conferences and professional meetings, word of the detectors and what they could contribute to nuclear physics research spread pretty much throughout the U.S. and, subsequently, to laboratories in Europe and other parts of the world. As this occurred, we received an ever increasing number of requests for detectors, with varying size and geometry requirements, and almost always with requests for the preamplifier circuit diagram and instructions on the use and care of the detectors.

"The detectors were not easily made and we still had much to learn. For example, the quality (resolution) was variable for reasons we didn't initially understand, even though we kept the process as constant as we could. Each detector had to be tested, and we provided an alpha-particle test spectrum with every detector. We were indeed not a production facility but a laboratory facility, with the capability of producing one or very few at a time.

"The result of this proliferation of requests was that Dave Peach became occupied essentially full-time with detector fabrication, and had virtually no time to help, as he normally did, with the experiments that our group was doing. Since we relied on him for various aspects of those experiments, I asked for permission to hire another technician, so that we could have that participation as well as carry on detector fabrication for others.

"I regarded this as an opportunity for ORNL to be of service especially to other national laboratories, and also to the many accelerator laboratories at universities throughout the country - and world. But, in a nutshell, our division director Joe Fowler said no, the division budget could not accommodate another technician, and we should find another solution to the problem.

"Since we were determined that our research would go on, the immediate effect was that other groups would simply have to wait their turns for detectors. Even so, we tried to meet needs and to respond to requests as promptly as we reasonably could.

"Our next step was to try to engage the Instrumentation and Controls division at ORNL, where Cas Borkowski was division director. Roland Abel was head of the group in which many types of nuclear detectors and monitors (mostly proportional counters and scintillation detectors) were being fabricated for use within ORNL, mostly in the groups that operated the reactors and in those that were developing reactors at the time.

"In our discussions, and after looking at our little operation, Abel said that he would be willing to set up to fabricate silicon detectors in his group, with the view toward supplying detectors to us in the Physics division as well as to other laboratories. But Borkowski would have to approve.

"Abel took the matter to Borkowski. Borkowski, according to Abel, took the position that the new silicon detectors were 'just a flash in the pan,' and he would not approve any level of effort in his division to make such detectors. Abel of course reported this to us. Appeals were to no avail. So, we went back to the drawing board.

"Having exhausted the realistic possibilities for making detectors at ORNL, and given that the information concerning their properties, attributes, and fabrication had been published and was in the public domain, we naturally considered the possibility of having them made outside of ORNL. Inasmuch as their fabrication was complex, required great care, and each detector had to be tested, it seemed clear that those of us familiar with the process needed to be involved.

"That being the case, why not simply form a company ourselves and supervise the operation, so that we would know it was being done properly?

There would be two possible reasons not to proceed along this line: First, it would simply be too costly – perhaps more than a group of individuals could afford; and, second, and even more important, would be

that those of us involved were very much interested in our research and wanted to continue our research activities.

"Therefore the activity would have to be outside of our ORNL responsibilities (i.e. an 'extracurricular activity'). In those days, we professionally 'belonged' to the Atomic Energy Commission, and felt we would need the ORNL administration's approval to conduct such an extracurricular activity without leaving ORNL employment.

"Naturally, we were also sensitive to ORNL's concern about potential outside use of proprietary information and conflicts of interest.

"On the first point: I met with Jim Blankenship for several discussions, during and after which he provided the inputs for a budget for initial setup of a laboratory for detector fabrication. The upshot was that we would need $10,000 to $11,000 for equipment and some beginning labor expense to start the operation - $12,000 would allow a little contingency.

"We later increased this objective to $14,000 to provide a little more contingency, recognizing that there might well be additional unforeseen costs. When we actually capitalized a few months later in late summer of 1960, we raised $14,500 from 29 persons.

"On the second point: While John Neiler and I had been discussing this whole development along the way, we decided to bring it up to our close colleagues and friends, a group who usually ate lunch together, knew each other very well, and frequently worked together in various ways.

"John and I were both Ph.D. nuclear physicists; the rest of the group consisted of: Tom Emmer, electronics designer and Electrical Engineer; Jack Gibbons, Ph.D. nuclear physicist; Jim Johnson, mechanical designer and Mechanical Engineer; and Phil Miller, Ph.D. nuclear physicist. All of us were thirty-something in age and still in the early and very energetic stage of our careers.

"Everyone in that group of six was interested in the idea. They immediately understood the problems that prevented making the new detectors within ORNL, and they quickly comprehended the possible pitfalls that would likely occur as we went along. But we all also recognized a possible, though uncertain, financial potential, and this added excitement.

"Being supportive of the effort, they encouraged our going ahead with seeking the approval of Laboratory management. They also saw the real service that we would be providing to the nuclear physics community, ultimately worldwide. We discussed this enterprise enthusiastically as a significant contribution to research in its own right.

"Fowler and other ORNL administrators, probably excluding associate director Mansel Ramsey, never did buy into this view, as far as we could tell. We were now in the early months of 1960.

Do you feel the excitement growing? Can't you just see these energetic young scientists trying their business wings? Something they had not thought would be needed, I am sure, when they were taking their technical courses in technical colleges preparing for technical careers…but they did not let that or anything else stop them!

ORTEC was born in this atmosphere of scientific experimentation and the pressing need for specialized detection equipment, something they knew a lot about, were literally experts in the field…and only in their thirties.

We will continue to follow Hal's story in the next installment of *Historically Speaking*.

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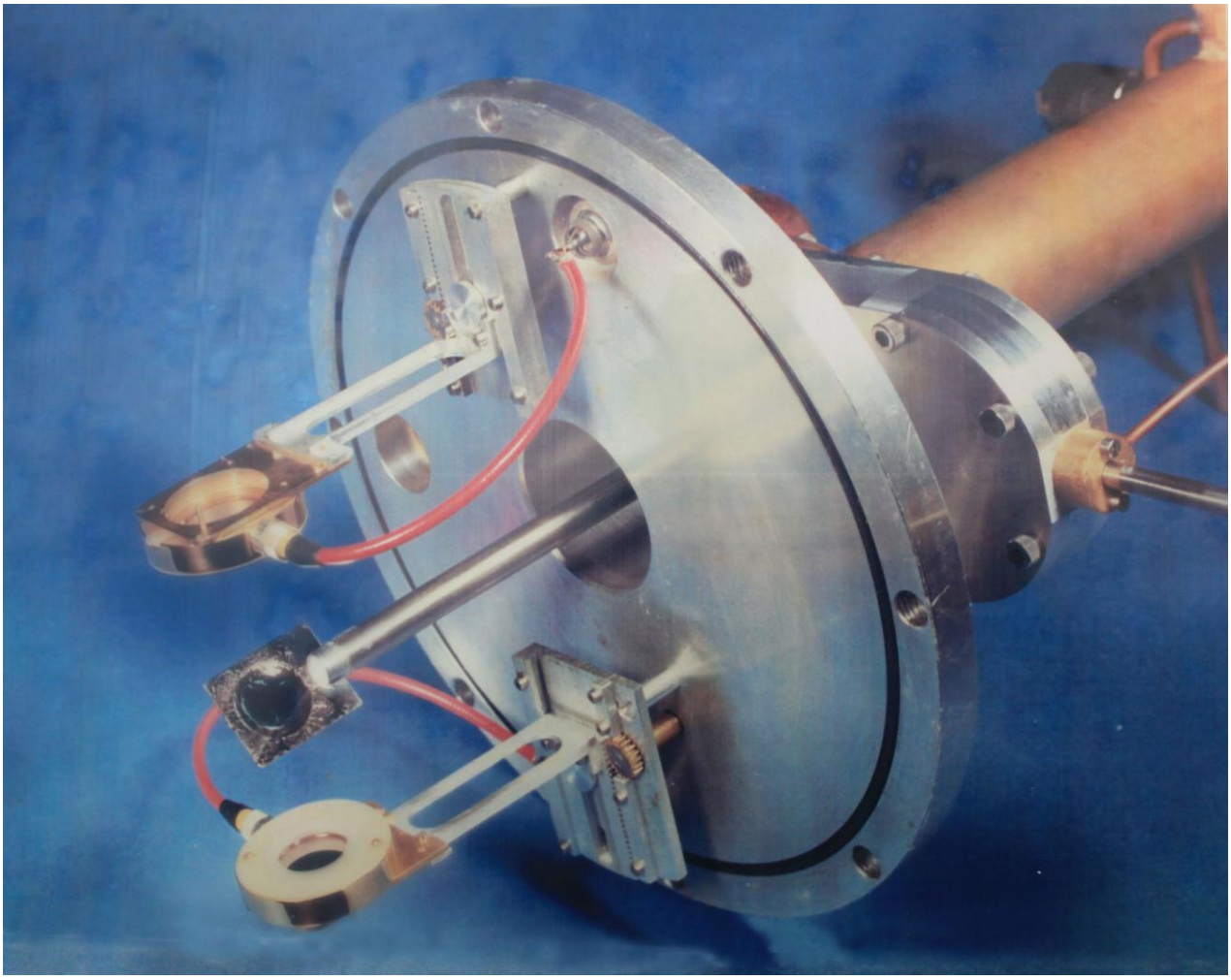

Two silicon surface barrier detectors, among the first developed by the company, shown with a thin-film target for nuclear reaction studies