REMINISCENCES OF REACTOR DEVELOPMENT AT ARGONNE NATIONAL LABORATORY

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W.B. Lewis Lecture, Canadian Nuclear Society,
Saint John, New Brunswick , June 4, 2007

Some years ago I heard a baseball player being interviewed on radio. When he was asked what it meant to have won the game, he called it, and I quote, "a victory for the forces of light over the powers of darkness." Something of an over-statement, I felt at the time. I tell you the story to assure you that I am aware of the regrettable human tendency to overestimate the importance of one's own efforts.

The story I will tell you today is a story of reactor development. As you know, most of the principal reactor development was done fifty or sixty years ago. But this story is not a story of long ago. The time these events took place was in the last years of the twentieth century. The place was Argonne National Laboratory, the first of the great national laboratories created after World War Two, and in the earliest days the one charged with the principal role in civilian nuclear power development in the US.

This was no minor experimentation with test tubes or counters in small rooms at an obscure research laboratory. At its peak the program had almost two thousand scientists, engineers, technicians, and supporting staff at Argonne's two sites, the main lab in Illinois and the reactor test site in the desert in Idaho. They were working on the development of a new reactor system—a different type of reactor, certainly, but also different fuel reprocessing, different re-fabrication technology, and different waste forms—everything required for a complete system. Everything was to be developed at once; nothing would be left for later.

It was an adventure, a decade-long adventure. Its story is history now, but this history contains some technological possibilities perhaps not widely known and appreciated.

Of course, Canadian experience and Canadian procedures are different. But perhaps the fact that some of what I say will not be completely familiar to you may make it that much more interesting. It is the practical that interests me—what must be done in order to get things done. And it is that that I will deal with principally in what I have to say today.

I am delighted to be here, with all of you today. It's a homecoming of sorts for me. I worked in the Canadian program for a couple of years over forty years ago. I learned a very great deal in those years—a way of looking at things, a way of analyzing problems that identified the main thing and did not let the focus wander. And this served me well later.

I have retained a respect for the Canadian nuclear program through all the years since—for its basic common sense, its technical virtuosity, and, of course, for the quality its people. And perhaps most of all, I admire it for its determination to go its own way, a way suited to Canada's assets and Canada's needs. And I admire your brilliant success in its implementation.

None of this could have happened without the man whose memory we honor today—in this, the 2007 WB Lewis Memorial Lecture.

I am honored by the invitation to give this talk. It is particularly meaningful to me, for in the time I played my small part in Candu development, WB Lewis was at the height of his powers, and his very name meant something very special indeed. The time was 1961. I was a very green PhD, fresh out of school. I had been hired by Canadian General Electric to attend to the physics of NPD-2, and in particular, to its startup. In that role I came into occasional direct contact with WB Lewis. To suggest I knew him would stretch truth. But a cat may look at a king, and I had ample opportunity to observe him, listen to him, read his papers, and to marvel at his insight.

I also had more opportunity than I really wanted to present my work to him for his critical assessment. His knowledge was vastly deeper than mine, but as a rule, he satisfied himself with making his point, and then letting me off the hook with some graceful little comment.

WB Lewis was a great man. Great men do not seem to be much in fashion these days. But WB Lewis is a member of that very small group of absolutely first rate men, and there are just a few, who can honestly be credited with providing the world with civilian nuclear power.

My story is the story of the development of the Integral Fast Reactor.

For all of you who have never heard of it, I sympathize with your likely reaction. Such a name promises very heavy going indeed, dry as dust subject matter—and a talk that no one would willingly sit through.

But as Mark Twain once remarked about the music of Richard Wagner, "It's not as bad as it sounds."

Nor, I hope, is my subject. It's interesting in itself, I think, but interesting too as an illustration of the interactions between science, the media, contending advocates, and public policy in the US toward the end of the twentieth century.

The story is all quite personal, as I suppose most such stories are if they're presented honestly.

When I left the Canadian program in 1963, I moved along to Argonne, a Laboratory with many firsts in nuclear development. Some of the originals who worked with Fermi at Chicago were still active at the lab, and there was an entire new generation of men who had carried on from them. I was suitably impressed.

By the time I arrived at the Lab, in the early '60s, the feasibility of the most successful reactor concepts in the US—the PWR and the BWR—the ones now earmarked for implementation—had been established, in large measure by Argonne itself. The efforts of industry were focused on overcoming economic obstacles—bringing the price down to meet fossil fuel competition. When these efforts began to succeed, an impressive nuclear construction boom ensued. It seemed that a secure and valued future for the nuclear enterprise had been achieved, and it had been achieved remarkably quickly.

Further reactor development—the function of the Lab—was then focused almost completely on a long term reactor system. The Liquid Metal Fast Breeder Reactor, the LMFBR, would extend fuel resources by a very large factor, in fact, for practical purposes, indefinitely.

But by the late 1960s, the reactor development effort at Argonne was not what it had been. The programs were centered on LMFBR development, certainly. But an important development had changed things greatly for the lab, and not for the better—the responsibility for the central direction of the development program had been taken from the lab and assumed by the AEC in Washington. Development of the breeder was then spread over a number of labs and industrial institutions, not concentrated at one lab, at Argonne, as it had been. The AEC confidently assumed that a slow step by step development of the LMFBR would assemble a large industrial base, ready and able to build on a massive scale.

It took only a few years to prove these comfortable views of the future very wrong indeed. The boom in construction stopped abruptly in 1974. Forty or so GwE were ordered that year, but none at all the following year. By the late 1970s the early successes were overtaken by serious and rapidly hardening opposition to further nuclear expansion.

And so by the 1980s much had changed. A public environment increasingly hostile to nuclear had now made any further construction too risky to undertake. The very technology of nuclear plants was under attack. A variety of characteristics, which seemed well enough in hand in the 1950s, were now, some twenty-five years later, blocking, or more properly stated perhaps, were being used to block, further progress.

Safety, waste, proliferation, principally, were in the limelight, always cast as negatives.

It was in these unpromising circumstances that Argonne launched the development of an entirely new reactor system. Every element of the new system was to utilize new technologies, and all were to be developed at once.

Our intent was to see how far we could go in ameliorating the principal concerns through technology itself.

Argonne at this time still had a very broad reactor research and development capability. Divisions of a hundred or more people in Reactor Physics, Reactor Safety, Metallurgy, Engineering Components, Chemical Engineering gave a broad, mainly analytical, capability at the main site in Illinois. In Idaho, Argonne had its big reactor test facilities that had been painstakingly assembled over the years, which by now included all the facilities needed for complete reactor system development. At the center was the operating liquid metal cooled power reactor, EBR-2, but there were large adjunct facilities for physics, safety, fuel manufacture, as well as extensive remote handling facilities.

So Argonne had the capability to do what it proposed. And the proposal did not come out of the blue. It was based on a variety of experience at Argonne, accumulated over the years. Things had been discovered. These things had been written up. They gave new information on various elements of a reactor system. But they had not been pulled together to give a total picture of what should be possible.

There had been no purpose in doing so, really. A breeder demonstration plant of the technology of the previous decade was under construction at Clinch River in Tennessee.

Construction had gone very slowly due to the strength of the anti-nuclear opposition, in and out of government, that had been brought together by the Carter Administration of the late 70s. In fact, the Carter Administration had tried to cancel the project, but Congress had been keeping it alive, not healthy but still alive, year upon year. Its technology used oxide fuel, with the reactor in a loop configuration, as opposed to the Argonne pool configuration. Influenced by naval reactor technology choices, this had become the accepted direction of breeder development by the late 1960s. This was not the Argonne choice. But there was no room for two directions. The Argonne discoveries received little attention.

Then, in November of 1983 the Clinch River Breeder Reactor project, after years of debate in Congress, was cancelled. The direction that project represented was dead.

Suddenly the Argonne discoveries of the last decade did not seem so unimportant. Most were the results of experiments, some were due to the improving understanding of the relevant phenomena, made possible by improvements in analytical techniques, but assembled together, they seemed to suggest that some pretty revolutionary improvements might be possible.

The fundamental basis for the change lay in the fact that a radical change in prospects for metallic uranium fuel had taken place—metal fuel, not oxide—for use in a liquid-metal-cooled reactor. This had arisen from tinkering with how best to design fuel for EBR-II.

Uranium metal swells—greatly and often unpredictably—under irradiation. The swelling of metal fuel had always caused it to burst its clad after a short time in reactor. No clad was strong enough to contain it and no metallurgical treatment stopped it.

But Argonne had actually solved the swelling problem by the early seventies, and was using uranium metal fuel with very satisfactory burnups in EBR-2.

It was done was by taking advantage of the compatibility of liquid sodium with uranium metal. This was the fundamental basis. Sodium could be used to thermally bond a fuel pin to the clad. This meant that a tight fit between the fuel and the clad was no longer necessary. The pin could be sized to leave room for a fair amount of swelling, and still thermally bond the fuel to the clad with the liquid sodium. This simple change was really all it took to give a uranium metallic fuel with long burnup capability. The uranium did not go on swelling indefinitely—it ceased swelling after a while, as it became interlaced with fission gas bubbles, and it turned into exactly what is desirable mechanically—a relatively plastic fuel material contained in a strong clad.

To be useful in the Integral Fast Reactor concept though, the fuel could not be pure uranium, it would have to have a substantial amount of plutonium, always, and at least one other alloying constituent to raise its melting point. EBR-II used uranium only. The IFR would be based on a uranium-plutonium fuel cycle. If a suitable ternary with an alloying addition to plutonium and uranium behaved the same way, we would have our start.

The reactor would be a variant of the sodium cooled breeder, but its properties would be quite different. The safety characteristics would improve—the fuel-coolant combination would give a much better passive safety response; just how much better we did not yet know. The reprocessing could be based on a wholly different principle than solvent extraction. Electrorefining, common in the metals industries, could be adaptable to metallic fuel, and give a whole range of advantages, not the least of which was that it could be done cheaply. The waste product would be different; with less volume, and it would contain much less long-lived activity. And this would be intrinsic to the processing technology; it would come at no extra cost.

The goal was ambitious enough certainly—to develop and demonstrate a viable long term reactor system whose technology could be shown to eliminate or at least ameliorate all the negatives that were now being used to stop progress in nuclear.

But the basis for the development of a long-term reactor system in the US wasn't changed. The breeding characteristic is fundamental to the long term. The potential for complete fuel use through breeding and recycle would actually be improved in our system —substantially better breeding characteristics were inherent in what we proposed.

We knew what the fuel form would have to be, we knew principles that reprocessing could probably be based on, and we knew if these things worked out we'd be close to our goal. The question was, would they work out?

Calculation had told us all it could. An experimental program was needed, and if we were to get to our goal, it would eventually have to be a big one. Not at first, but soon. How to get started? Step by step was the only way possible—no big program could realistically be expected to be approved. But if we took it step by key step, and if each step succeeded, we felt we could gradually build the program that would be needed.

At this time I had been in charge of the Argonne Reactor Development program for some years. I knew the risks. This was certainly not an initiative that was welcomed by any of the constituencies normally necessary for success in such a project. Not by the sponsoring agency, who were not pleased that the Lab was pressing them to adopt new directions. Not by the nuclear industry, who had enough on their hands trying to inch forward with present systems, and who felt to some degree threatened by any large push for a long term system, particularly one with uncertain prospects for success. There were, of course, some very significant individual exceptions to this, men who wanted to see new technology emerge, particularly as the termination of the CRBR project seemed to clear the way for new long term reactor technology.

And of course, none of this was viewed with favor by the organized nuclear opposition. In the early years of the IFR though, they posed no real problem as they felt we had no chance of succeeding in any case.

I knew most of the players, technical and political, who could affect such a program. In fact, I knew personally most of our enemies—the anti-nukes—about as well as I knew our friends.

This dated back to the Jimmy Carter non-proliferation initiatives of the seventies. I had been part of the delegation in a series of State-Department-led international meetings. Our delegation included many of the most prominent antinukes. I traveled with them, I dined with them, I got to know them.

And I knew our Congressional delegations, both in Illinois, where the main Lab is, and Idaho, where our main reactor facilities were. Some were on the key committees of Congress dealing with energy matters. We were their constituents. I knew they felt some responsibility toward us, and might, under the right circumstances, be of real help to us.

And by now I had met many of the giants in the nuclear field. At Argonne all who were still interested in nuclear development came through the lab sooner or later.

And all this played a part.

The actual start arose from a meeting that had been arranged for me with the President's Science Advisor. The subject was to be this new reactor concept that we were proposing, and after hearing me out, he, in turn, arranged with DOE to supply Argonne an extra small amount of money to investigate feasibility of the IFR.

We had our start.

I needed a name for our proposal, and driving in to work the first morning I settled on the word Integral to suggest that everything needed for a complete reactor system was to be an integral part of the development, reactor, fuel cycle, and waste. If a better name showed up later, well and good. Instead, the Integral Fast Reactor it became.

The new fuel was to be metal, of course—an alloy of plutonium, uranium, and zirconium. It would have the very simple design. Zirconium would suitably elevate its melting point. There was concern that the substantial zirconium addition necessary would counteract the fission gas release characteristic of pure uranium fuel and re-introduce the swelling problem. Events proved the worry unfounded.

Further, with sodium as the thermal bond, the fuel would have a very small temperature profile in operation. This in turn would give a fuel with almost no reactivity to feed back in accident initiating situations. The reactor could realistically be expected to have much improved passive power reduction characteristics under accident conditions.

The simple design would allow remote fabrication of the fuel. The product from the new reprocessing technology would be highly radioactive—self protecting in the lexicon of non-proliferation analyst. Remote handling would be necessary.

Ten atom percent burnup would be our goal—one hundred thousand Mw-days per tonne.

Quite a load to place on a completely untried fuel, but everything depended on it. Success here would be a giant step toward feasibility.

Next was reprocessing: an electrorefining process needed to be defined and developed.. Argonne had world experts in this chemistry. And if it could be done at all, it could be done on small scale, and cheaply. Metal fuel was dense, the volumes of fuel loadings were small, compact processing was a good fit. Electrorefining would take all the higher isotopes at once, the chemistry suggested, and plutonium separation alone wouldn't be possible. The product would burn well in a fast spectrum. The waste product, being fission products alone, could be compact too. It all fitted together.

It took only a few months for a plutonium fuel fabrication facility to be constructed, fuel fabricated, and irradiations begun in our EBR-2 reactor.

The burnup tests were successful, extraordinarily so. Our 100,000 MwD.T goal was passed easily, the new fuel assemblies went all the way to 200,000 MwD/T burnup in its first trial—more than any oxide fuel at that time. Then the assemblies were taken out for final examination—they'd gone far enough. But the key thing was that none had failed.

It was a go.

Argonne was (and still is) part of the scientific and political mileu of the nation, and we knew pretty much the kind of things that were going to be necessary for success.

Sound science of course. Second, enthusiastic support from the scientific elite—that's vital to credibility. Congressional support would be necessary in a big way, for initially DOE would be neutral at best, and majority votes for funding of the IFR would be needed every year. Senators and congressmen need to be assured that their votes went to a project that had passed the critical eye of the most respected figures in the field.

And so I got on airplanes and one by one I visited many of the most respected names in our field, sometimes taking a full complement of specialists, sometimes one specialist, sometimes alone. In a few weeks we had assembled a review committee with unimpeachable credentials. It was made up of the most eminent nuclear statesmen—the Nobel Laureate, Hans Bethe, long known to the congressional committees for his expert testimony on all kinds of nuclear issues; Manson Benedict, the founding chairman of the MIT Nuclear Engineering Department; and others: Max Carbon, the ex-head of the nation's Advisory Committee on Reactor Safety; Lombard Squires, the ex-head of the huge reprocessing installation operated by DuPont at Savannah river; the ex-head of fuels research at another lab; and from time to time one or two more. But the core group stayed with us all the way through. This committee reviewed our program at the start and every year thereafter, and provided a written report on our progress. These men knew the rules of the game—their report in turn found its way to the key congressional staff.

The work expanded after that first year. You can move fast if all your resources are in a single lab. Lab scale refiners were built and put into operation. They worked in the lab, for uranium fuel, they worked less simply for plutonium-alloyed fuel, but they worked. The separations, it turns out, are crude, with two products: most of the fission products on one hand, to go as waste, and on the other an uninviting mixture in the fuel product of uranium, plutonium, and the higher isotopes.

The fuel product was not useful for thermal reactors, but, as we hoped, not really useful for weapons either—an attribute useful in making a non-proliferation case. It was admirable fuel, though, for a fast reactor: all the actinides fission well in a fast spectrum. It was highly radioactive, but if the fabrication is simple enough, remote fabrication, fitted to the simple fuel design, would be easy. Fuel slugs loosely inserted in steel clad, a pinch of sodium, a plenum weld of the top, and the pin is ready—this would be no stretch at all for remote techniques.

These two elements of the system—the fuel and the fuel cycle—were our principal focus. They were the necessary first steps. But by 1986, we had also prepared for many months for a series of demonstrations of the unusual safety characteristic made possible by the excellent heat-transfer characteristics of metallic fuel and the liquid sodium coolant. It made unaided shutdown of a properly designed reactor under accident conditions possible—power reduction and shutdown just from the interplay of the heat transfer characteristics of the new fuel with the sodium coolant. No operator action, no operation of safety systems, would be needed just to ride through the two major accident-initiating events: *Loss of Heat Sink*, as in the TMI-2 accident, and *Loss of Flow*, an accident possibility that at the time had not occurred in any power reactor, but which had long been studied.

In early April of that year, both accident cases were initiated in our test reactor EBR-2—both while at full power. In the morning, the reactor was suddenly isolated from the steam system, cutting off the heat sink. The reactor responded by smoothly shutting down. Then in the afternoon, after starting up again, the pumps were turned off; the flow coasted down, but, after an initial transient, so did the power —in lockstep with the flow coast-down. In both tests the reactor had quietly shut itself down. DOE duly issued a press release.

Nobody paid any attention.

Then the loss of flow accident happened. And it happened on the world stage, with riveting TV coverage, and the greatest possible concern—at Chernobyl.

An alert science reporter at the Wall Street Journal, Jerry Bishop, made the connection immediately. He remembered the press release and he made the connection himself. A reactor in Idaho had lost its coolant flow, and at full power, in this same month, and NOTHING WHATEVER had happened. He contrasted this with the tragedy unfolding at Chernobyl.

His article caused an immediate sensation—in the right congressional committees that year, and resulted in substantial increases in funding, sufficient for the first time to put people and facilities to work on every aspect of the IFR.

Every year is a fresh new start for the funding of a National Laboratory. Each year there was a battle that started with hearings by the appropriate committees of congress, where I sat, often side-by-side with my anti-nuclear opponents, giving prepared testimony on our progress, followed by detailed briefings of senators, congressmen, and—principally—congressional staff.

It was like that every year of the IFR program—for ten years—and always the votes were close. Each time I could only hope that what we had done was sufficient to carry us one more year. I came to feel like a character in the old silent movies, made to hang by his fingernails until the next episode starts, and only then does it turn out that he is indestructible.

Except that we were not indestructible, and we knew it.

For ten years the work went on all across the various elements of the new technology. By the early nineties we had had full reactor irradiations for the new fuel. We had refurbished and restarted the our hot-fuel remote-processing capability, part of the EBR-2 complex, and had begun processing the standard uranium-zirconium metallic fuel, an easier electro-refining task than the ternary alloy.

We had built the actual-scale refining equipment for the ternary alloy, and began readying it.

The actual waste forms by this time were far along in development too.

But as development continued, the anti-IFR volume increased. By 1994 the new Clinton administration had been in office for over a year, and my friends, the anti-nukes, were settled into the relevant departments in the new Administration.

And in February of 1994, what we had expected to happen did happen, though in a far more public manner than we had expected.

President Clinton, in his State of the Union address in February of 1994 to both houses of Congress, a speech always carried by all the TV networks, speaking of his plans for the year, said, "We will terminate unnecessary programs in advanced reactor development." We were in no doubt as who that was aimed at: we were the only program in advanced reactor development in the country.

This would be an entirely different year. We had had weak support, but support, up to now, but this Administration had gone from weak support to active opposition, and we felt it immediately. We had probably gone from the difficult to the impossible, I thought. But I also knew our congressional support was strong enough that we would not go without a fight. And so it proved.

I gave testimony to the various Congressional committees as usual, but now the anti-IFR Congressional staff people were not standing aside—they were active in lining up support for termination of our program. Both houses of Congress were held by the Democrats, and our support had been mostly along party lines, but enough Democrats had supported us to squeak through every year. But no longer—in a close vote the House narrowly supported the Administration's position.

In the Senate, it was different. Democrat Bennett Johnston, head of the Energy and Water appropriations subcommittee led the fight to maintain IFR funding.

The floor manager of the opposition, leading the push for termination, was a senator I had barely heard of, but who became better known later, to me and to the world, John Kerry. Kerry had been well prepared, and spoke, not accurately certainly, but articulately, in the fashion that became familiar later. After several hours of debate, Johnston summarized the IFR position briskly, and won the vote.

So there was brief hope, but in the House-Senate Conference, which only the Conferees have access to, the House position, as desired by the president, held. The IFR was gone.

And so in the end what did it all mean? We had had ten years—ten tumultuous years for those of us shepherding the program.

Ten years of accomplishment for the technical people in the program. We had accomplished a lot—things were now known that had not been known before. Some things were surprising, possibly with quite revolutionary implications. Some we had guessed at, and established as fact.

But we had established a number of quite important things. At the most general level we had shown that new technologies are still possible in nuclear power, technologies that can improve every part of a complete reactor system.

And we had established that:

1. Very-high-burnup uranium-plutonium fuel is possible. At least 200,000 MwD/Tonne was demonstrated, without a single pin failure.

2. This same fuel can be fabricated very simply, and this is easily done remotely.

3. The heat transfer characteristics of metallic fuel

and liquid-sodium metallic cooling are excellent, and in turn act very efficiently to lower the power in proportion to the need in the face of serious accident initiators, such as those at Chernobyl and Three Mile Island. These accidents would not have happened in an IFR.

4. This fuel can be reprocessed electrochemically. In fact, EBR-2 metallic uranium fuel is being reprocessed at industrial scale in this way today, as a waste management tool.

5. The processed fuel remains highly radioactive, requiring remote handling, and is self-protecting from a non-proliferation viewpoint. This too is intrinsic: nothing can be done to alter it.

6. Waste can be largely stripped of the transuranics without any additional steps or cost. The transuranics stay in the recycled fuel. Their presence in the waste is reduced two orders of magnitude.

7. The waste form can be simple, too. The waste itself is of two kinds—steel from the subassemblies and clad, which is simply recast, and the fission products, which are immobilized in ceramic. These forms are developed.

8. The size, the scale of these things, is on a human scale. Huge plants or installations are unnecessary.

But the termination of the program in 1994 stopped further plutonium work, before the key U-Pu process could be established at industrial scale. It worked at lab scale.

The process does take the fission products, or most of them, and leaves a fuel product ideal for a fast reactor, but for little else—most notably, for weapons.

However, I must stress that the Uranium-plutonium electro-refining process at real scale is the one key element in the system that has not been established. That’s important. They got us before we could get that done. And it may or may not be a difficult step.

So the development work isn’t complete.

And the team has now been scattered to the winds.

And I would like pay tribute to that team, in closing, to my colleagues, who as a collegial team developed the IFR technology and brought it so far along. I think a quote from George MacDonald Fraser, a long-time favorite of mine, is appropriate. He wrote these words in a far different context. But they summarize exactly my feelings about the IFR, and the team that worked on it.

“We did what we did, and it was worth doing, and no one could have done it better—or half as well.”

As to its ultimate importance, it can only be said, I suppose, that:

Time alone will tell.