Perspective

The Realism Project: It's time to get real

BY THEODORE ROCKWELL

Do we already have analysis and data to show that the ultimate casualty—a meltdown with damaged containment—cannot cause a significant number of deaths? If not, what more would it take?

THE NUCLEAR COMMUNITY agonizes over its inability to communicate its message to the public. We hire public relations experts, pollsters, and communication consultants to polish up our messages of reassurance. But PR expertise cannot overcome a basic problem: Our credibility is continually undermined by ostensibly authoritative statements that no amount of radiation is small enough to be harmless and that a nuclear casualty could kill as many as hundreds of thousands of people. *That* message we *have* communicated, and therefore the public and the media are not wholly to blame for the resulting public fear of radiation and all things nuclear. We cannot expect people to believe our assurances of safety so long as we acquiesce in terrifying messages to the contrary.

Framing the problem

In the spring 2003 issue of *Science & Global Security*, Robert Alvarez et al. published a report claiming that if a spent fuel pool were to lose water, by accident or terrorist action, the consequences "could be significantly worse than those from Chernobyl," killing tens of thousands of people. They presented their conclusions to members of the U.S. Congress, which then asked the National Research Council of the National Academies to form a committee to evaluate this study and to report back to both houses, which it did in June 2004. An unclassified summary of its findings is expected by the end of the year.

On February 12, 2004, the committee held its first meeting, which was open to the public and webcast. The authors of the Alvarez report summarized their conclusions and the basis for them. Farouk Eltawila, of the Nuclear Regulatory Commission, reported the agency's conclusion that Alvarez et al. had based their predictions on unrealistic premises, and therefore the predicted deaths are unrealistic—i.e., that they cannot occur in the real world. The authors replied that they had not invented any new methods of analysis for the report but had merely applied premises and methods similar to decades of reports by NRC contractors, citing one in 1997 that "predicted" as many as 143 000 extra cancer deaths from a spent fuel fire.

I testified that the authors were correct that the issue is broader than a single antinuclear report. These government-sponsored studies have claimed consequences that if true, should have long ago led to the shutdown of nuclear power. To its credit, the NRC has now addressed this issue, distinguishing between the simplistic scoping calculations and realistic assessments of actual potential consequences.

How did we get here?

From the beginning, nuclear safety has been implicitly defined as preventing fuel melting. This, of course, is a proper goal for designers and operators, but for casualty analysis, it raises problems. It requires that a literally endless number of potential scenarios be analyzed. One can always think of one more. As suggested scenarios have become increasingly extreme, the industry has chosen not to challenge the premise, but to respond, "We can meet that." This has led to many obvious (but still unchallenged) absurdities, each met with additional guards, barriers, and rules.

Meeting every challenge, regardless of price, implies that the consequence of failure is unprecedented catastrophe. That is the argument for Price-Anderson insurance: that a catastrophic casualty is acceptably improbable for the public (but not for the industry). No other hazard claims such protection.

We must face the ultimate casualty: meltdown of the core and compromise of containment. Page-one headlines tell us that we can kill 518 000 people that way, with just one scenario, and they promise there are worse ones. Deciding we don't want to talk about that, the industry mumbles that, well, that's a very low-probability scenario. This is a *bad answer!* It's not enough to say that sort of thing doesn't happen very often. We have to demonstrate that certain consequences are simply impossible. Zero probability. The probability of a nuclear plant's getting hit by a squad of flying pigs is zero. And the same laws of nature that allow us to say with assurance that pigs can't fly tell us that dangerous quantities of fission products can't travel 500 miles in respirable form, waiting to be ingested. The probability of that is not "small." It's zero. And we have data to support that.

Moreover, when we consider a core with melting fuel and a containment blown open, we're beyond probability. That's the worst realistic casualty. We can estimate how bad it is. And we find it is tolerable—less severe than accidents already experienced with coal, oil, gas, and other nuclear competitors.

We've spent a billion dollars over 30 years gathering data on this and analyzing the implications. If we have some gaps in the data, then we should fill those gaps. But there is no mystery about it. This work and its real-world implications were reported in many talks by Chauncy Starr and Milton Levenson, and in a well-documented paper (Levenson and Rahn 1981).

We find that doomsday scenarios are based on three types of simplistic premises, involving (1) release and dispersion of fission products from molten fuel; (2) health effects of low-dose radiation; and (3) arbitrarily extreme assumptions as to heat transfer, personnel actions, etc.

The Three Mile Island-2 (TMI-2) accident in 1979, which resulted in the meltdown of much of the reactor core, released only trivial amounts of significant radioactivity into the environment, despite compromise of containment through the auxiliary building (Kemeny Report 1979). This was in contradiction to the computerized scenarios used to "predict" consequences of casualties, but it was consistent with the experimental and analytical data.

The Chernobyl accident in 1986 was an event that is physically impossible in a commercial light-water reactor power plant. The reactor core was blown through the roof and burned for days, releasing a million times more radioactivity than TMI-2, with an extended delay in evacuating the surrounding population. Even so, the detailed and heavily documented report from the United Na-

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tions Scientific Committee on the Effects of Atomic Radiation (UNSCEAR 2000) on the radioactivity sources and the consequences concluded that no one outside the plant boundaries was killed or had serious radiation injuries.

These conclusions were substantially reconsidered, and confirmed, in a June 2001 conference in Kiev, Ukraine. There were 1800 reported cases of thyroid cancer, primarily in persons who were children at the time of the accident, with few if any fatalities, but these do not correlate well with the radiation doses and were less frequent than the *natural* occurrence of such cancers in some other countries (e.g., Finland). Cancers normally associated with high radiation doses, such as leukemias, were not yet found to be elevated. The radiation level of the evacuated areas is lower than many high natural radiation background areas of the earth where people have lived healthily for generations, and are much lower than some of the rooms in the granite-walled U.S. Capitol building or New York's Grand Central Station.

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The actual consequences of Chernobyl, TMI-2, and more than a dozen other meltdowns also confirmed what realistic analyses have shown: Even with compromised containment, cold steel and concrete structures and large quantities of water in pressurized water and boiling water reactors are tremendously effective in reducing the quantity of respirable radioactivity released into the air, even when containment is severely compromised as in the TMI-2 accident (see particularly Morewitz 1981).

Health effects of low-dose radiation

Authors of nuclear studies showing high fatality rates from radiation exposure generally concede that no individual would be expected to receive a harmful dose, "But nuclear terrorism could result in large numbers of people being subject to these very small risks. That's why it may represent a significant public health concern," noted David Brenner, commenting on the *Science* paper that concluded few, if any, fatalities could result from even the worst realistic casualty to a PWR or BWR or its fuel (Chapin et al. 2002). This is the other aspect of realism, and its resolution is long overdue.

The linear no-threshold hypothesis (LNT) claims that no amount of radiation is harmless. That is simply untrue. A single gamma ray can kill you only in the sense that a single flu germ can kill you. A healthy body is always loaded with radiation, as it is with germs. But the number of cells damaged by radiation is millions of times lower than the number routinely damaged by normal metabolism. Radiation may break two strands of the same DNA molecule, which is harder to repair than a single-strand break. Even making a generous allowance for the fact that this occurs more often from radiation damage than from metabolic damage, the number of misrepaired cells surviving is still a trivial factor that is more than offset by the enhancement of the repair process brought about by the radiation's stimulatory effect. These processes have been measured in the laboratory and well-demonstrated in the literature. Even at lethal radiation doses, it is not the damage to the cell's DNA that causes harm, but the overpowering of the body's defenses. So worrying that someone might get 4 mrem per year from Yucca Mountain (in a natural background of hundreds) is like worrying that you will pick up germs on a doorknob. You will. The body competently deals with both situations. Thus, the LNT hypothesis does not properly describe the consequence of radiation doses of the magnitude we generally encounter.

Let's be clear that realists are not advocating the overthrow of some basic Law of Nature. The LNT hypothesis is merely the observation that at high radiation levels, the health effects seem to be proportional to dose. That is true when the dose exceeds the body's ability to respond defensively. No one argues that. All we claim is that at lower levels, there is no evidence that organisms are harmed, and there is a great deal of evidence (largely ignored in the policy-setting documents) that there is no harm, that the body's defenses are stimulated by the radiation, and that the net effect is generally beneficial. That phenomenon—hormesis—is the process that works for vaccination, exercise, nutritional supplements, sunshine, and virtually every assault on a living organism. To paraphrase Paracelsus, a 16th-century Swiss physician, "Nothing is poison but the dose makes it so."

Setting a threshold

So how should we apply this knowledge to our current situation? How do we set a permissible threshold dose? Don't all our official advisory and regulatory documents tell us to assume LNT-to-zero? Well, let's look carefully at what they say.

Start with the basic U.S. regulatory guidebook on the LNT hypothesis, NCRP-136, p. 6:

> It is important to note that the rates of cancer in most populations exposed to low-level radiation have not been found to be detectably increased, and that in most cases the rates have appeared to be decreased.

And there are many more such statements. Let's look to Lauriston S. Taylor (Taylor 1980), the "father of radiation protection":

> No one has been identifiably injured by radiation while working within the first numerical standards set by the National Council on Radiation Protection and Measurements and the International Commission on Radiological Protection in 1934. The theories about people being injured have still not led to the demonstration of injury and, if considered as facts by some, must only be looked upon as figments of the imagination.

The 1934 standard set by Taylor and his colleagues was 0.1 rad/day (NCRP) and 0.2 rad/day (ICRP). For a 250-working-day year, that's 25 rad/yr and 50 rad/yr, respectively. These numbers were later reduced to 15 rem/yr, which lasted for many years, then dropped to 5 rem/yr. The American Nuclear Society and the Health Physics Society recently issued position statements, agreeing that "Below 10 rem . . . risks of health effects are either too small to be observed or are non-existent," and suggesting 5 rem/yr as a threshold. A massive body of data shows that a threshold of 25 to over 100 may be reasonable (e.g., T. D. Luckey 1997).

So pick a number. I'd pick 25 rem/yr, and might settle for 10. And I'd say in blunt and simple terms: "Doses below 25 rem/year are known to be harmless and are generally beneficial. Therefore, ALARA [As Low As Reasonably Achievable] below that number is not beneficial and is generally counterproductive. There is no justification to regulate or decrease doses below that number. And there should be no regulatory action in response to occasional exposures above that number if long-term averages do not significantly exceed that number."

Collective dose

The idea of adding up a large number of small individual radiation doses to predict health effects is universally discredited. Populations can't get cancer; only individuals can. See, for example, Roger Clarke, ICRP chairman (Clarke 1998):

Continued

If the risk of harm to the health of the most exposed individual is trivial, then the total risk is trivial—irrespective of how many people are exposed.

Or the official LNT guidebook, NCRP-136 (p. 58):

The summation of trivial average risks over very large populations or time periods . . . has produced a distorted image of risk, completely out of perspective with risks accepted every day, both voluntarily and involuntarily.

And again (p. 62):

. . . it is recommended that regulatory limits not be set in terms of collective dose. . . . When the uncertainty in the number of individuals . . . is large . . . collective dose should not be used as a surrogate for risk, even at relatively high levels of individual radiation dose.

The French Academy of Medicine stated in a December 4, 2001, press release:

[The Academy] associates with many international institutions to denounce improper utilization of the concept of the collective dose to this end. These procedures are without any scientific validity, even if they appear to be convenient to administrative ends.

On September 28, 2004, an extensively documented report was unanimously approved by both the French Academy of Medicine and the French Academy of Sciences, reaffirming and strengthening this position. It refuted in specific detail statements by Brenner and others supporting the use of the LNT hypothesis at low doses.

And the Health Physics Society, in a March 1993 position statement, emphasized in bold-faced type:

We strongly recommend that dose limits be applied *only* to individual members of the public, *not* to the collective dose to population groups.

Therefore, I would ban the use of collective dose for regulation or for analysis. Using collective dose as a measure of merit of a facility's radiation protection program creates a harmful incentive to decrease the amount of important inspection and testing in radiation zones. "Predictions" of health effects based on collective dose should be characterized as invalid, and all other practices based on collective dose should be eliminated. For example, the common toxicological practice of diluting a solution to bring it within permissible levels should be equally applicable to radioactivity (but, of course, is equally vulnerable to abuse, which should be proscribed). Instead of collective dose tables, one could use histograms showing, for example, number of persons receiving 0–5 rem, 5–10, 20–25, etc.

How do these reports justify recommending LNT-to-zero after conceding that science says otherwise? Simply by claiming it is "prudent" and "safe" in view of the "lack of data at low levels." This is wrong on both counts: The evidence appears to be meager only because authors ignore the mass of credible evidence refuting their claim. And the harm done by this distortion of reality belies any claim of prudence. Even the studies used to support their position (A-bomb survivors, nuclear workers, tissue studies) have been distorted by improper data manipulation. They exemplify the saying, "Scientific data are like captured spies; if you torture them long enough they will say whatever you want to hear."

Other realism reforms needed

A realistic "quality factor" (rad-to-rem conversion) should be specified for alpha particles. The consensus for low-dose chronic exposures seems to be 2 or 3, rather than the current 20. This would largely eliminate the radon "problem," and also ameliorate the wholly inappropriate attempt to regulate natural radium and uranium in water, which have never been shown to be a health problem. It would also permit more realistic evaluation of the risk from plutonium, americium, neptunium, and other alpha emitters sometimes encountered in nuclear work.

Realistic premises for exposure should be used in place of some of the current extreme premises (e.g., after a terrorist attack on a shipping cask, the Department of Energy presumes that no one will move, and no cleanup will be accomplished, *for one year* [USDOE 2002]). Realistic premises should also be mandated for fluid flow and heat transfer during casualties.

Realistic premises should be used as to fuel cooling and fission product release and dispersion, including size distributions for radioactive particles for inhalation fractions, etc. These should be based on actual test data and realistic analyses developed for that purpose over the past 30 years.

In all analyses used for calculating doses and health effects, the best realistic numbers should be used throughout, with any safety factor added at the end and so identified. Nils Diaz, NRC chairman, recommended this in his remarks to the Japanese Atomic Industrial Forum in Tokyo in April 2004: "When engineering margins are applied to input parameters, they can distort our understanding of what is truly important. Safety margins are better discerned when they are applied at the decision-making stage, rather than at the analysis stage."

The distinction between routine application and a one-time emergency should be strengthened. First responders to a radiological incident should not hesitate to rescue an injured person because of a radiation field defined as unacceptable for routine operation of a waste disposal facility.

Action needed

Although the case is persuasive that the worst realistic nuclear casualty is less harmful than that of nuclear power's serious competitors, the evidence has not yet been assembled into an overall documented statement and evaluation. Many, perhaps most, nuclear advocates are not familiar with the data, and so we are not yet ready to mount an assault on public opinion.

The action urgently needed now is to prepare the case, and then discuss it within our own ranks. This will require some money, but, more importantly, some aggressive leadership from at least a few prominent nuclear statesmen. Until that happens, the status quo will prevail.

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