New Light On the History of Hanford

By Sharon Ghamari-Tabrizi

EDITOR'S NOTE
This essay is based on an address delivered at the May 1998 annual meeting of the Washington State Historical Society as the second annual Curtiss Hill Lecture.

The following is a discussion of what I consider to be indispensable topics for an ideal public history of Hanford from the point of view of the history of Cold War science. I'd like to begin by addressing the problem of how outspoken public comment about events in recent history has become politically charged in the current climate.

One of the strangest phenomena since the end of the Cold War has been the double movement of opening archives and closing down public debate. On the one hand, we have seen the opening of the archives in Russia, the former Soviet republics and Warsaw Pact nations. Thousands of Atomic Energy Commission (AEC), Central Intelligence Agency, National Security Agency and other formerly top-secret documents have also been declassified and publicly released. At the same time as the scholarly world has been in a great ferment, with the current generation of graduate students combing through these newly available materials, we have seen a severe constriction in the public discussion of the events, personalities and contested meanings of the 50-year-long Cold War. In recent years we have experienced frequent lapses of civility in our public discourse. This has seriously degraded public debates about the Cold War and the enormous influence that scientific and technological developments have had on American society in the 20th century.

The series of disturbing events surrounding the cancellation of the *Enola Gay* exhibit at the National Air and Space Museum several years ago offers a case in point. Charges of "politically correct," "revisionist" curating dominated the angry and noisy dispute which resulted in the evisceration of the exhibit script, threatened retributions against the Smithsonian in the form of reduced funding, Congressional hearings, the forced resignation of museum director Martin Harwit, and the historical absurdity of displaying the restored front fuselage of the B-29 that dropped the atom bomb on Hiroshima with no accompanying photograph of ground zero.

There were other, less visible consequences of the *Enola Gay* debacle. Michael Heyman, the general secretary of the Smithsonian, issued what can only be called a "gag order," compelling curators not to speak to the press about the exhibit. Even more damagingly to the Smithsonian, Heyman authorized a new curatorial approach to future exhibits. For the indefinite future the National Air and Space Museum would exclusively focus on the material culture in its collection. Henceforth it would avoid so-called "interpretative" issues in its exhibit scripts under the mistaken historiographic principle that the facts could speak for themselves. Moreover, an exhibit on the Cold War that had been in preparation for a number of years lost its funding, and proposed exhibits on the Korean War and the Vietnam War were also canceled due to fear of provoking further public and congressional strife.

The public history of science has provoked the same charges of "political correctness" and the same attempts to silence critical discussion of America's scientific and technological legacy. There was another exhibit at the Smithsonian, following on the heels of the *Enola Gay* affair, that received far less press coverage but was almost as threatening to the autonomy of the National Museum of American History's curators. In the mid 1980s the American Chemical Society (ACS) decided to create a Chemistry Exhibit Center for its headquarters in Washington, D.C., in order to stimulate wider appreciation for the public benefits of chemistry. It was noted that a proposed exhibit, "Science in American Life," was currently being contemplated for the National Museum of American History. Recognizing the public relations value of attaching the prestige of the Smithsonian to the project, the ACS Board of Directors recommended that the chemical society underwrite the science exhibit. They committed $5.3 million of their exhibit fund to the museum, with the understanding that it would prominently feature chemistry in its displays and themes. However, the contract originally drawn up between the ACS and the Smithsonian specified that the final decisions regarding the overall content of the exhibit would remain subject to the curators' authority.

Given the ACS's later campaign to revise the exhibit in the spring and summer of 1995, it is important to note that in an interview timed with its opening in 1994, ACS president Ned Heindel endorsed the controversial features of the exhibit. He said, "The exhibit is consciously political, public and scientific.

It was also clear from the opinions of the exhibit's advisory board and the Smithsonian's own views that we weren't about to create a 'pep rally' for chemistry, but a picture of the effects of science in general in American society."

However, within a year of its opening, the ACS and the American Physical Society (APS) requested substantial modifications to the exhibit. Robert Park, public affairs director of the APS and professor of physics at the University of Maryland, attacked the exhibit for presenting "a century of correct, postmodern social constructivism." He took great exception to the tempering of the more flattering cavalcade of discovery, invention, and the salutary relief of human suffering with a social history perspective on the inequities, injustices and harms also resulting from high-tech science in the 20th century. The president of the APS sent a critical letter to the general secretary of the Smithsonian. He wrote, "We are concerned that the presentation is seriously misleading and will inhibit the American public's ability to make informed decisions on the future uses of science and technology." However, when many physicists prominent in their professional society actually visited the show, unlike the chemists, they didn't find much wrong with it.

So what did the historians of science have to say about the exhibit? As a counterbalance to the objections of the professional scientists, the executive secretary of the History of Science Society, University of Washington professor Keith Benson, wrote a letter to the Smithsonian's general secretary in support of the social historical approach to science in American life. He wrote, "Scientific practitioners operate within the cultural milieu of their times, . . . the Smithsonian fulfills its mission to educate American citizens by critically examining science and technology issues which we as citizens enjoy, oversee and fund."

I would add that deliberate attention paid to the unintended consequences of scientific practices is wholly consistent with the exhibit's theme of presenting science in its historical context. Clearly, the dismaying public relations messages of the social history approach to scientific research were at the heart of the chemists' objections to the exhibit.

When all of us were growing up we learned that scientists were intrepid explorers who discovered pre-existing phenomena that had been concealed in the obscurity of nature's mysteries. Newly discovered facts, so the popular account went, passively awaited discernment by the clever experimentalist. It made for exciting narratives of discovery, exploration and triumphant recognition—and it makes for entertaining television even today. However, academic research in the philosophy, sociology and history of science and technology disputes the fundamental thesis of this account. In particular, since the 1930s at least, scholars have proposed that the very data that record the movements of natural phenomena do not necessitate a single indisputable explanation. Rather, observations, research agendas, the design of experimental apparatus and measuring instrumentation all implicitly shape what appear to be raw, unprocessed data. Thus the first premise of much history of science concerns the inescapably interpretive acts that resolve the uncertainty presented by natural phenomena into coherent accounts.

Formerly, the history of science approached the narrative of scientific discovery as a kind of intellectual history. This is now called "internalist history"—that is to say, the account of advances in knowledge would be explained entirely within the leading terms of a discipline. Larger political, religious and cultural currents would be relegated to the unimportant historical background of scientific discovery. They were meaningful only when reasons were needed to explain the apparently scientific character of what would later come to be understood as pseudo-science or errors. While this mode of historiography is still practiced in some quarters, in the past 20 years or more what had once been a fairly rigid demarcation between the internal, technical details of a scientific problem and the external, social world has been largely dissolved. Science is now understood by many historians to be a thoroughly social, ideologically-invested, inescapably political enterprise. Thus I would like to present some of the elements of what I think is an adequate, publicly responsible account of the history of Hanford from the point of view of these recent studies.

Let's begin with a sample of what I consider to be an equally revisionist but this time "patriotically correct" version of the Hanford story. In March 1997 I was a member of a curation strategy workshop convened in order to decide on the best way of persuading the Department of Energy (DOE) to allocate money for curating the material culture at Hanford during its process of decommissioning the buildings at the site. In a bid to appeal to the DOE' s public relations interests, the first draft of the curation strategy document that was circulated to panel members for comment presented Hanford history in the following unqualified terms:

> *The panel believes that the DOE is uniquely positioned to capitalize on Hanford . . . to instill in our future generations an appreciation for the stunning scientific and technological achievements that are possible when the nation's intellectual resources and its industrial infrastructure are harnessed together to meet important objectives.*

Since the draft had not mentioned environmental contamination that resulted from Hanford's productive activities, I could not consent to the document in its draft form. In my opinion, a one-dimensional account of Hanford as a "stunning technological achievement" comes perilously close to propaganda. However, I am pleased to say that the final form of the curation strategy document, which was issued in December 1997, omitted the triumphant rhetoric of Hanford's achievements and substituted this far more acceptable and publicly responsible sentence: "The Site offers extraordinary potential for insight into how the nation and the industry dealt with conflicting pressures of plutonium production, worker safety, and environmental protection during World War II and the Cold War."

A closer look at current approaches to the history of science can guide us in telling a socially and politically informed story. In my ideal public history of Hanford, I can think of a number of equally important topics that should be addressed. For example, we should scrutinize the trade-offs between the political imperatives to acquire and stockpile atomic weapons against the hosts of unknowns in the new field of plutonium production, keeping in mind the steep learning curve of the 1940s and 1950s associated with handling radioactive materials on an industrial scale. We should examine the development of worker health and safety procedures as production runs increased over time, not neglecting to include the social history of labor at Hanford—focusing, for example, on how these procedures were implemented or disregarded in the day-to-day habits of the workers at the site. We would want to look at the emergence of the new professions of nuclear engineering and health physics, enumerating the leading questions of the disciplines, the sequence of technical problems they encountered, mastered or ignored over time.

We want especially to elucidate the histories of risk assessment, risk management, and risk communication at Hanford, including public relations efforts over the years concerning the harmless nature of stack wastes and effluent in the normal course of plutonium production, as well as communications concerning occasional accidental releases of contaminants. We want to look at risk communication to Hanford workers at the site, as well as to the citizens in the Tri-Cities area. Finally, we cannot overlook the management of internal criticism by contractor scientists, including the suppression of Hanford whistleblowers over the years. All of these topics must be included in the public account. They are relevant to the expanded notion of the history of Cold War science.

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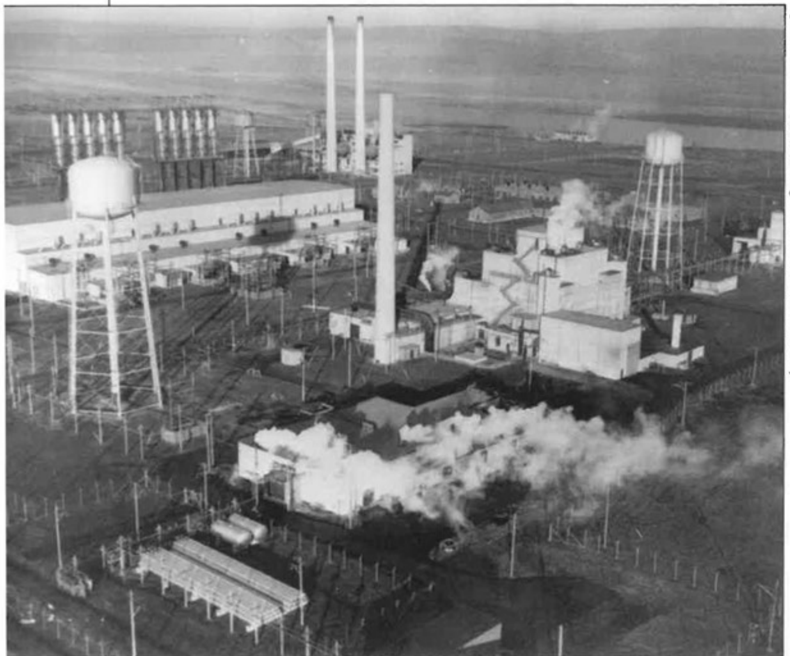
Flyer for the "atomic city" of Richland, owned and administered by the Atomic Energy Commission.

Let's start with the origins of Hanford. The organizational culture of Hanford arose from the odd mingling of academic, military and industrial communities during World War II. The mixture of these three radically different professional worlds was troubled by social awkwardness, personality conflicts, contradictory management styles and workplace conventions. One of the points that would be interesting to trace in my ideal Hanford history would be the collisions and compromises resulting from the intensely urgent collaboration of General Leslie Groves and the Army Corps of Engineers, the academic physicists, and the Du Pont chemical engineers and business administrators.

Many different kinds of stories can be told from this moment of origination. One need only consult S. L. Sanger's rich oral history of the construction and operations of Hanford during World War II in order to find dramatic instances of the friction between these various social actors. Most famously, the Du Pont engineers and the Chicago scientists struggled for dominance in deciding key components of the reactor design and operations. For example, the ultimate decision for a water-cooling design lay with the Du Pont engineers, which Eugene Wigner and some other University of Chicago scientists deeply resented.

As in large-scale industrial construction everywhere, problems that had not been addressed during the design phase arose during fabrication of the original B-Reactor. During the first start-up of the reactor, an unanticipated by-product of the chain reaction—xenon-135—essentially shut down the reactor. However, the engineers' design convention of building in margins of safety allowed the problem to be corrected by increasing the scale of the reaction. The organizational norms of Du Pont engineering design, which had prevailed over the academic physicists' design, enabled the eventual reactor start-up. In this case showing the conflict between the University of Chicago's metallurgical laboratory and Du Pont, it is illuminating to track the difficult transposition of an essentially experimental reactor in a university laboratory into an industrial-scale factory.

Another example of the institutional conflicts that had a lasting impact on Hanford was the origin of the government-owned/contractor-operated management structure of the laboratory. The wartime Office of Scientific Research and Development did not have the administrative expertise or the resources to transform Enrico Fermi's experimental reactor into a factory-sized production reactor. It was particularly important to be able to attract and retain a stable population of technical personnel to operate the plutonium factory over the long term. Hence, Hanford was contracted out to a private-sector company that could offer higher pay scales to engineers and corporate executives than the civil service could supply. More importantly, the construction encampment that built the reactor gave way to one of the most curious developments in American urban history—the construction of Richland, a secret "atomic city" like Los Alamos, owned and administered for more than a decade not by county or local government but by the Atomic Energy Commission.



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B-Reactor (right of center), the first full-scale nuclear reactor, went online at the Hanford site in 1944, the product of a collaborative effort among the academic, military and industrial communities.

Hanford was the biggest employer in southeastern Washington. In this regard, we can call upon "actor-network theory" in the social studies of science, which traces the organizational and monetary alliances between scientists, technologists, and their political advocates. Accordingly, we can align the Tri-Cities boosters—who fought to keep the reactors open and functioning in the 1960s as long as possible as a source of stable, high-paying jobs—with the expert scientific apologists for Hanford's environmentally safe operations in the same period.

Another long-standing feature of the origin of Hanford during the war was the prevailing military organization of the compound. The Hanford works was fenced in with armed guards stopping traffic and checking identification at the few entrances. There were rigid security restrictions, importantly involving the compartmentalization of knowledge and communication among the various sectors of the production complex. While the security apparatus of the Hanford Works makes sense as a national secret during World War II, the Korean War and perhaps the 1950s, the habits of secrecy made it institutionally difficult for Hanford managers to respond to citizens' demands for environmental accountability during the period of détente in the 1970s. Because the history of Hanford must include the production of waste, the original military organization of Hanford should be addressed as a social fact that impeded open discussion of the hazards associated with normal plant operations.

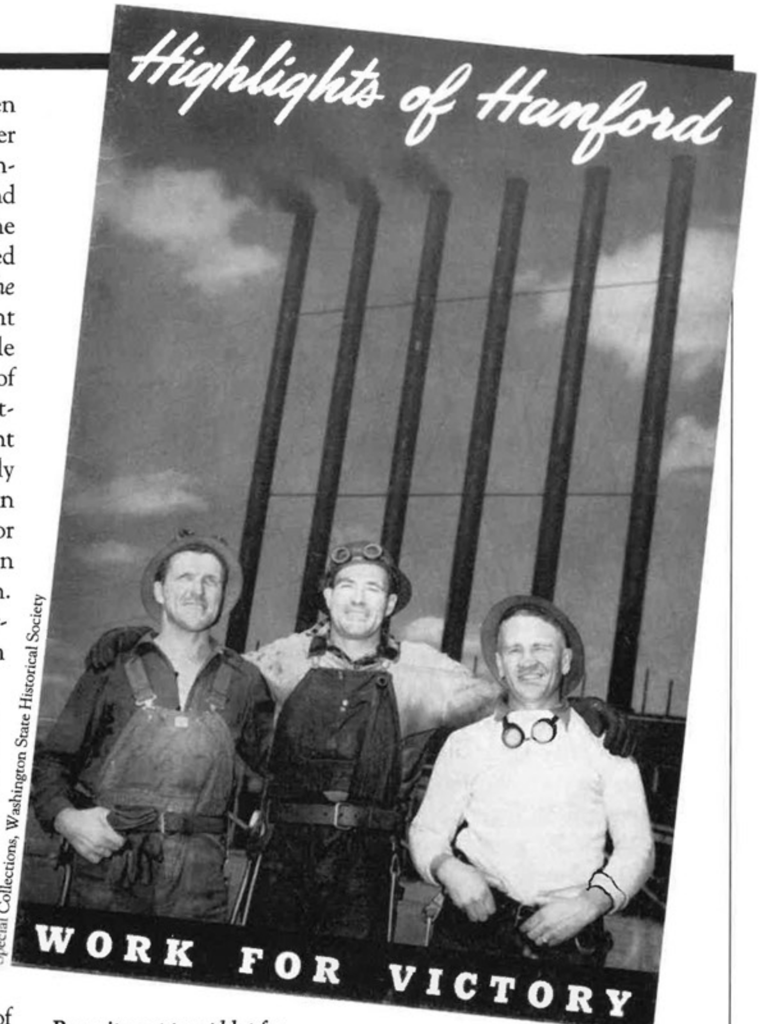
While most of Washington's citizens regard the production of radioactive and toxic waste as the legacy of contractor negligence, it is important to situate the production of these wastes within the historical context of the new post-war discipline of risk assessment in the emerging nuclear industry. This is surely the most contested topic in Hanford history.

One element in the history of Hanford waste concerns the extreme reliability of the water-cooled graphite reactor technology of the facility. In the opinion of historians John Findlay and Bruce Hevly, Hanford's graphite and water-moderated, water-cooled reactors "were recognized by the end of World War II as dead ends in terms of the development of nuclear reactor technology." Within the constraints of the "dead-end" graphite reactor technology, through tinkering and progressive adjustments, Hanford engineers were able to increase substantially the amount of irradiated product in each batch. Thus, the very reliability of the "low-tech" graphite reactors enabled the AEC and the General Electric engineers "to accept greater risks in terms of siting the piles or operating them above their rated powers in order to speed production."

Now we go from a stable production technology to the production of toxic and radioactive waste. The specific circumstances underlying the creation of these wastes at Hanford must be included in the public history of the reactor. This can be done by reviewing the events that impelled the breakneck production schedule for plutonium and the decisions regarding the risks implicit in speeding up and increasing the volume of irradiated product.

Some might find it surprising that Du Pont as a chemical company had a long tradition of concern for worker safety in handling toxics. From the beginning of operations at the Hanford Works, Du Pont engineers were consistently concerned with environmental safety and monitoring. Studies were conducted of radiation tolerance levels in drinking water, bathing in the river, and uptake by fish. Other studies examined the environmental and worker safety impacts of the varying reactor design problems such as shielding, water flow control, loading and unloading the piles, and cladding the uranium fuel. Similarly, Hanford workers were subject to what would have been for the period extremely rigorous radiological monitoring and workplace hazards procedures. At the same time, because of the specific character of the risk assessment ideas current at the time, the low-level release of radioactive effluent in the form of spills and incremental waste generated by the normal production process was considered to be well within the accepted tolerances for environmental exposure and was not regarded as hazardous to the surrounding area.

There were three successive phases of expansion at Hanford that were the result of such Cold War events as the eruption of the Korean War, the decision to develop the hydrogen bomb, and the desire on the part of the American military to increase and upgrade the nuclear weapons stockpile. The greater production runs resulted in a concomitant increase in the radioactive contamination of the air, the soil, and the waters of the Columbia River. Given the fact that the waste generated at the site was the consequence of decisions to shorten the cooling time of the irradiated slugs as well as to increase the amount of product in each batch, let us have a look at the risk assessment practiced by Hanford managers.



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Recruitment pamphlet for the wartime Hanford Works.

The historical evaluation of the risk assessment undertaken by Hanford managers and health physicists illustrates another conflict among the significant actors at the Hanford site. General Electric (Hanford's post-war contractor), the AEC, and health and safety officials pursued conflicting objectives, the consequences of which were production decisions that increased the possibility of a major catastrophe. In their book, *Supplying the Nuclear Arsenal*, Rodney Carlisle and Joan Zenzen document GE's difficulties in satisfying the AEC's production schedule with safety requirements. They note that in the early years of Hanford, ". . . there was no generally accepted means of estimating probabilities of reactor failure." The AEC's independent advisory committee responsible for reactor safety consistently criticized the commission's production division for pushing an ever-greater production schedule without sufficient regard for the increased risk of a major accident. Carlisle and Zenzen wrote, "As a contractor, GE found itself in a difficult position. At times, company managers sought to limit production in order to meet a safety objective, only to receive reprimands from . . . the Production Division."

GE and the AEC advisory group regarded the risk of a major accident differently; consequently, they could not agree on how to evaluate reactor safety. The Reactor Safeguard Committee concerned itself with the overall increased magnitude of risk as a general threat to human and environmental health in relation to increased production. GE opposed this risk philosophy with the argument that, insofar as the operating procedures were adjusted to allow for greater yields, neither the probability nor the magnitude of a disaster would increase.

The reactor safeguard committees that established oversight of GE's production division biased their risks assessment in favor of increased plutonium production over public safety in the following way. They conceptualized safety as designing the system to mitigate the hazards of a major accident or catastrophe. What is important about this concept is that it did not focus on the cumulative risks that would be incurred via contamination resulting from normal production releases. Moreover, since they had focused on the big risks, the major accidents, they did not consider smaller problems that could compound into a major catastrophe.

Carlisle and Zenzen tell the story of an incident that took place during the startup of a jumbo reactor. A leak in a process water tube seems to have occurred at the same time that one of the uranium slugs ruptured. There was at the same time another technical foul-up: the pressure gauge for measuring the water flow through the process tubes in the pile had been improperly calibrated. Neither of these problems had been detected during the setup procedures. What is important about this incident is that the shutdown stemmed from two unlikely events that compounded one another rather than from a catastrophe based on a structural flaw in the reactor design. After this incident Hanford managers began to address the fact that their risk assessment philosophy had a terrible blind spot regarding these compounding events.

If it was the case that the contractors employed the best industry standards for environmental safeguards and worker health and safety, then how can we understand the legacy of Hanford's radiological, toxic and thermal pollution? Hanford managers and the AEC practiced a historically contingent risk assessment—balancing the pressures for increased production with shortened cooling times and greater wattage against what they regarded as unlikely hazards of a major catastrophic accident. Moreover, they performed these calculations in the steep learning curve of the 1940s and 1950s. They were dealing with new substances and on an industrial scale with which no contractor had any prior experience.

Du Pont, as a chemical engineering corporation, had long experience in handling toxic materials and could transpose many of its design, operating and safety conventions into the new field. We should recall, however, that a political decision was made to scale up into industrial capacity what had been a university-based experimental reactor. These were brand-new industrial materials. Thus, we should examine the intersection of politics, rudimentary environmental and biomedical science, and emergent industrial practices. Successive political decisions were made by presidents Truman and Eisenhower, spurred on by Congress and the armed forces, to develop a weapons technology with materials about which there were a great many unknown factors. Political decisions were made to tolerate the many undecidables of radiation technology. Political and industrial decisions were made to allow risk assessments to go forward in the context of ongoing professional controversy about the proper balance between increased production and the proper standards for worker and environmental safety. Industrial and scientific decisions were made to allow the problems to remain unresolved, unattended and neglected for decades in the hope that the original estimates of the effects of contamination on the surrounding area and the local population would remain secure and defensible. Inevitably, there would be errors in reasoning and calculations, mistaken assumptions, blind spots and hosts of unknowns.

As a final example of what should be included in a public history of Hanford, consider the leakage of toxic sludges and salts from single-shelled tanks into the so-called "vadose zone" or soil directly above the water table in the Hanford Reach. Here is another case involving the combination of science, politics, and the local organizational culture of Hanford—all of which should be considered well within the domain of contemporary practices in the history of science. There are a number of interacting elements that make the waste tank leakage story a complex and troubling case for historical reconstruction. The vadose zone problem demonstrates the paradox of the kind of idealized extrapolations made in the laboratory that do not correspond to the specific constitution of the waste tank region's soil. Pacific Northwest National Laboratory (Hanford) director Bill Madia recently explained,

> *You can go into a laboratory in a rather stable environment and look at some of the major constituents . . . and draw conclusions . . . that you wouldn't see much [waste] migration. But . . . when you go into the real world and look into the geology below the tanks, . . . laboratories or test tube studies really aren't extrapolatable to what's happening in the real world. And so, generic scientific information would lead you to believe that the material shouldn't go very far, but in actuality when you get into the ground where you've got preferred pathways and unusual things happening, you really have open questions. . . .*

In addition to the scientific problem of why the migration of tank wastes is occurring so much more rapidly than expected, we cannot externalize the emergence and reception of this issue. A case in point is that of Casey Ruud, a quality assurance inspector for nuclear facilities at Hanford who was fired in 1986 after testifying to a congressional committee about production problems in the Plutonium Finishing Plant and the PUREX facility at Hanford.* Last year the Department of Energy undertook a review of the Hanford facility's management. It concluded that management at Hanford was either dismissive or menacing in response to the concerns of the contractor scientists. Tom Carpenter, the director of the Government Accountability Project in Seattle, has remarked that if a scientist's "concern doesn't go along with the paradigm of the day, what management wants to do, then [he or she is] just simply flushed out of the system." As I understand it, since then-Secretary of Energy Federico Peña met with the Hanford whistleblowers in August 1997, the DOE has prepared a corrective action plan that will allow for the airing of professional concerns from staff scientists and engineers.

If we look at the history of Hanford's public relations, we can see that for longer than the past decade, robust citizen pressure has repeatedly played a part in cracking open the long-standing managerial ethos of secrecy at the PNNL. Responding to the pressure of the Hanford Education Action League (HEAL) and other concerned citizens in the Tri-Cities area, Hanford Manager Michael Lawrence authorized the release in February 1986 of thousands of pages of documents concerning the early years of the Hanford site. These pages disclosed a pattern of customary release of contaminants, the deliberate release of the "Green Run" experiment, and accidents. Mr. Lawrence should be commended for responding to public pressure for disclosure, and Hanford should be praised for its openness in conferring with various stakeholders and involved community groups and citizens in its current curation strategy as well as in the new environmental laboratory effort.

Whereas the recent history of public consultation should not eclipse decades of secrecy, and it is our job as historians to document the co-production of plutonium and toxic waste at the site, still in light of this recent practice of openness to criticism we should all feel encouraged—even emboldened—to insist that the history of Hanford cannot be told by the pronouncements of an official, triumphant, post-Cold War history. To make reference once again to the "Science in American Life" exhibit, the lesson for us is that public history must not degrade into propaganda, no matter what corporation, professional society, chamber of commerce, party in power, or public fund is paying for it.

In the context of incivility in the current political climate, in response to the charges of "political correctness" in the public history of the Cold War, it is equally patriotic to remember that to soberly and respectfully allow outspoken debate is a fundamental tenet of democracy. The history of science can contribute to the relatively independent public space for the crafting of the nationally significant story of Hanford by offering an account in which the facts assuredly do not speak for themselves.

Sharon Ghamari-Tabrizi is a historian of Cold War science, specializing in the sciences and strategies for waging, surviving and reconstructing from atomic and thermonuclear war in the 1950s and 1960s. She is a research fellow at the Center for the Humanities at Wesleyan University, Middletown, Connecticut.

AUTHOR'S NOTE

While my name is affixed to this article, in terms of the substantive research about Hanford, the real authors are Rodney Carlisle and Joan Zenzen of Rutgers University and History Associates, Inc.; Bruce Hevly and John Findlay of the University of Washington, Seattle; and Joshua Silverman of Carnegie Mellon University. Their primary research in Hanford-related archives as well as their interpretations of this material serve as the basis for this article. All scholarly credit redounds to them; the political thrust of the piece is my own.

* *For more information on Ruud, see Michael D'Antonio,* Atomic Harvest: Hanford and the Lethal Toll of America's Nuclear Arsenal *(Crown Publishers: New York, 1993) and also the web page on Hanford whistleblowers (http://www.accessone.com/gap/www/Ruuddecpr.htm).*

COLUMBIA

The Magazine of Northwest History

A quarterly publication of the

WASHINGTON STATE HISTORICAL SOCIETY

VOLUME THIRTEEN, NUMBER ONE

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COLUMBIA (ISSN: 0892-3094) is published quarterly by the Washington State Historical Society.

All articles appearing in this journal are abstracted and indexed in ***Historical Abstracts*** and ***America: History and Life***. *EDITORIAL CONTRIBUTIONS:* Inquiries and submissions should be directed to **COLUMBIA**, WSHS Research Center, 315 N. Stadium Way, Tacoma, WA 98403; 253/798-5918. All unsolicited manuscripts and photographs submitted must include return postage (in stamps) and suitable packaging to ensure their safe return. Although reasonable care will be taken with materials received, no responsibility can be assumed for unsolicited materials, including photographs. *BOOK REVIEWS:* Address all review copies and related communications to Robert C. Carriker, Department of History, Gonzaga University, Spokane, WA 99258. *POSTMASTER:* Please send address changes to Washington State Historical Society, 1911 Pacific Avenue, Tacoma, WA 98402.

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COLUMBIA

THE MAGAZINE OF NORTHWEST HISTORY ■ SPRING 1999

***COVER:** Ten years before Lewis and Clark set off on their famed expedition to the Pacific, Alexander Mackenzie, a Scottish explorer and fur trader for the North West Company, had completed, with his voyageurs, a similar journey farther north. This documentary painting by contemporary artist Howard Sivertson depicts voyageurs making camp at a pleasant spot after a day on the move. (Courtesy of Howard Sivertson, Grand Marais, Minnesota)*