

The Society for Historians of American Foreign Relations



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The Society for Historians of American Foreign Relations

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MEETINGS: The annual meeting of the Society is held in the summer. The Society also meets with the AHA in January.

PRIZES: The Society administers four awards that honor the late Stuart L. Bernath and two honoring the late Myrna L. Bernath; these are financed through the generosity of the late Dr. Gerald J. Bernath. Awards also honor Laura and Norman Graebner, the late W. Stull Holt, the late Warren Kuehl, the late Armin Rappaport, Robert Ferrell, Lawrence Gelfand, and Arthur Link. Details of each of these awards are to be found in the June and December *Newsletters*.

PUBLICATIONS: The Society sponsors a quarterly *Newsletter*; *Diplomatic History*, a journal; and the occasional *Membership Roster and List of Current Research Projects*.

IMPLEMENTING “ROLL-BACK”: NSC 158

by

Christian F. Ostermann

THE NATIONAL SECURITY ARCHIVE¹

Historians have for a long time wondered how serious the Eisenhower administration was in operationalizing and implementing the idea of “liberation” (or “roll-back”) in its policy towards Eastern Europe. Liberation and roll-back had proven to be powerful election campaign slogans in 1952, ideally suited to denounce the Truman administration’s seemingly “static” and “passive” and hence “immoral” policy of containment. Moreover, as the new evidence from the former Soviet bloc archives reveals, “liberation rhetoric” certainly created anxieties behind the Iron Curtain. Given its challenge to the hegemony the Soviets had gained in Eastern Europe in the aftermath of World War II, roll-back propaganda alarmed the post-Stalin Soviet leadership, and, at the same time, raised hopes for U.S. intervention among many East Europeans. But was “roll-back” more than a powerful slogan?²

The National Archives recently released in full NSC 158 “Interim United States Objectives and Actions to Exploit the Unrest in the Satellite States” which was adopted in the aftermath of the East German uprising of June 1953. Resulting from the contradictions between the harsh crash socialization and militarization program pursued in East Germany under the

¹I would like to thank Bill Burr, Malcolm Byrne, Jim Hershberg and Pete Voth for their support and advice.

²See Vadislav Zubok and Constantine Pleshakov, *Inside the Kremlin's Cold War*, Cambridge, MA: Harvard UP, 1996, 157.

regime of Stalinist Communist Party (SED) leader Walter Ulbricht and the post-Stalin Soviet leadership's "peace offensive," the strikes and riots which spread to more than 400 cities and villages in the German Democratic Republic marked the height of a wave of unrest pervading Eastern Europe in the wake of Stalin's death. Though eventually suppressed by Soviet tanks, the uprising became the first test case for the policy of liberation.³

Despite its efforts at destabilizing the Communist hold over Eastern Europe and Eastern Germany, the Eisenhower administration was surprised by the extent of the anti-Communist revolt in the GDR. Fearful of unleashing a military confrontation with the Soviets, the administration initially reacted with caution and moderation. Discussions in the NSC a day after the legendary June 17 riots were marked by a realization that the uprising, while a "sign of real promise," also "posed a very tough problem for the United States to know how to handle."⁴ Uncertain of his policy options, President Dwight D. Eisenhower asked the inter-

³Christian Ostermann, "Keeping the Pot Simmering: The United States and the East German Uprising of 1953," *German Studies Review* 19:1 (February 1996), 61-89. See also Valur Ingimundarson, "The Eisenhower Administration, the Adenauer Government and the Political Uses of the East German Uprising in 1953," *Diplomatic History* 20:3 (Summer 1996), 381-409.

⁴"Memorandum of Discussion at the 150th Meeting of the National Security Council," 18 June 1953, Department of States (ed.), *Foreign Relations of the United States 1952-1954*, VII, (Washington, DC: GPO, 1986), 1587.

agency Psychological Strategy Board to devise a short-term plan on how to deal with the East German riots.⁵

Led by Eisenhower's Special Assistant for Psychological Warfare, World War II psy war veteran and publisher, C. D. Jackson, the Psychological Strategy Board drew up an "Interim U.S. Psychological Strategy Plan for Exploitation of Unrest in Satellite Europe (PSB D-45)." A summary of the plan (printed below)⁶ was adopted by the National Security Council on 25 June as NSC directive 158 and approved by Eisenhower on June 26. As revised and approved by the Psychological Strategy Board on July 1, PSB D-45⁷ viewed the East German uprising in the context of existing signs of unrest in Czechoslovakia, Poland, Romania, and Albania. While resentment over excessive production quotas, food shortages, and low living standards had triggered the revolts, these grievances were, in the analysis of PSB D-45, "overshadowed by the clearly expressed political objectives of the German rebels." More than anything else, the uprising seemed to be "a kind of spontaneous direct-action plebiscite in which the East German masses voted with their fists for free elections, the reunification of Germany and the withdrawal of Soviet occupation forces." The PSB expected that local strikes, demonstrations, or other manifestations of continuing resistance could easily be renewed, and concluded that, with popular resentment of the Soviets "near the boiling

⁵Ibid., 1587-90.

⁶On file at The National Security Archive, Washington, D.C.

⁷Psychological Strategy Board (PSB), "Interim U.S. Psychological Strategy Plan for Exploitation of Unrest in Satellite Europe (PSB D-45)," 1 July 1953, National Archives (College Park, MD), Record Group 273, NSC 158 Series. - Curiously, large sections of the PSB plan remain classified.

point,” the uprising in the GDR created “*the greatest opportunity for initiating effective policies to help roll back Soviet power that has yet come to light.*”⁸

NSC 158 thus signaled the beginnings of a phase of aggressive roll-back policy. Though quickly conceptualized *in reaction* to the East German uprising, the measures suggested in NSC 158 reveal to what lengths Eisenhower and his advisers were willing to go in encouraging resistance to Soviet domination of Eastern Europe. Most importantly, the declassified summary of NSC 158 shows that the Eisenhower administration intended to rely heavily on subversive and covert operations (intensified defection programs, “black radio intruder operations”, support of “elimination of key puppet officials”) in pursuit of roll-back. Few strings were initially attached to exploiting and stimulating mass resistance against Communist authorities in an area perceived by Moscow to be crucial to Soviet security concerns.⁹

As far as one can make out from the declassified records, few of the measures authorized in NSC 158 were actually implemented. Sharp criticism of aggressive psychological warfare soon mounted within the administration which, in the course of its secret “Operation Solarium” policy reassessment in the summer of 1953, decided against an extreme roll-back

⁸Ibid. (Emphasis added).

⁹Eisenhower directed that “more emphasis be placed upon passive resistance in implementing paragraph 2-a.” Note by the Acting Executive Secretary to the National Security Council, June 29, 1953, copy on file at The National Security Archive, Washington, D.C.

policy.¹⁰ U.S. allies, moreover, were also vehemently opposed to some of the more extreme measures envisioned in NSC 158. The British and French governments, afraid of setting precedents for criticism of “internal affairs” of “colonial empires,” considered a propaganda offensive in the U.N an anathema. West Germany’s Federal Chancellor, Konrad Adenauer, facing elections in the fall, effectively blocked any immediate action on the Volunteer Freedom Corps which was to be based in Germany. Ironically, the most important actions therefore taken in response to the East German uprising by the Eisenhower administration were a proposal for a new foreign ministers conference on Germany and a large-scale food program — measures which temporarily put the Soviets and the East German regime on the defensive but hardly lived up to the expectations many East Germans and East Europeans had with regard to roll-back.¹¹

* * * * *

¹⁰John Lewis Gaddis, *Strategies of Containment. A Critical Appraisal of Postwar American National Security Policy* (New York: Oxford University Press, 1982), 145ff.

¹¹See Christian F. Ostermann, *The United States, the 1953 East German Uprising, and the Limits of ‘Rollback,’* Cold War International History Project Working Paper No. 1 (Washington, DC: Woodrow Wilson Center, 1994).

June 29, 1953

Top Secret Security Information

REPORT
by
THE NATIONAL SECURITY COUNCIL
on
INTERIM UNITED STATES OBJECTIVES AND ACTION
TO EXPLOIT THE UNREST IN THE SATELLITE STATES

1. Psychological Objectives

a. To nourish resistance to communist oppression throughout satellite Europe, short of mass rebellion in areas under Soviet military control, and without compromising its spontaneous nature.

b. To undermine satellite puppet authority.

c. To exploit satellite unrest as demonstrable proof that the Soviet Empire is beginning to crumble.

d. To convince the free world, particularly Western Europe, that love of liberty and hatred of alien oppression are stronger behind the Iron Curtain than it has been dared to believe and that resistance to totalitarianism is less hopeless than has been imagined.

2. Courses of Action - Phase I (Requiring less than 60 days to initiate)

a. In East Germany and other satellite areas, where feasible, covertly stimulate acts and attitudes of resistance short of mass rebellion aimed at putting pressure on Communist authority for specific reforms, discrediting such authority and provoking open Soviet intervention.

b. Establish, where feasible, secure resistance nuclei capable of further large-scale expansion.

c. Intensify defection programs, aimed at satellite police leaders and military personnel (especially pilots) and Soviet military personnel.

d. Stimulate free world governmental, religious, and trade union activities capable of psychological effect behind the Iron Curtain, such as:

(1) International campaign to honor martyrs of the East German revolt.

(2) Free trade union denunciation of Soviet repression and demand for investigation of basic economic and labor conditions.

e. Reemphasize U.S. support for German unity based on free elections followed by a Peace Treaty.

f. Implement NSC 143/2 (Volunteer Freedom Corps) completing discussions as soon as possible with Allied governments.

g. Consider bringing Soviet repression of East German revolt before the UN.

h. Launch black radio intruder operations to encourage defection.

i. Encourage elimination of key puppet officials.

3. Courses of Action — Phase II (Requiring lengthy preparation contingent upon developments)

a. Organize, train and equip underground organizations capable of launching large-scale raids or sustained warfare when directed.

b. Consider U.S. advocacy of (1) free elections in the satellites and association with the Western European community, with emphasis on economic cooperation and rehabilitation, and (2) subsequent withdrawal of all foreign troops from Germany, Austria and the satellites.

c. Consider new forms of covert organizations based on concepts of:

(1) Simulating¹² Soviet officer conspiracy to establish honorable peace with the West.

(2) Cooperation between satellite resistance elements and nationalists in non-Russian Soviet Republics.

(3) Cultural appeals to Soviet intellectuals.

d. Consider inclusion of USSR nationals in Phase II of Volunteer Freedom Corps project.

e. Consider large-scale systematic balloon propaganda operations to the satellites.

DOING RESEARCH IN NEW YORK AND TEXAS NEWSPAPERS

by

Nicholas Evan Sarantakes

UNIVERSITY OF SOUTHERN CALIFORNIA

Researchers planning trips to Austin, Texas might allot a day of their time to doing research in three newspaper clipping collections at the University of Texas and the state capitol. These collections are useful sources of information since most of the collected papers have no index, and are hard to use on microfilm, much less find. Another important point worth

¹²Handwritten addendum: "(sic)".

noting, a clipping file is a much quicker way of going through a newspaper than an index and microfilm.

The two major newspaper morgues at the University of Texas are from defunct New York city newspapers. The research files of the New York *Herald-Tribune* are at the Center for American History, which is located next to the Lyndon Baines Johnson Presidential Library on the east side of campus. From 1907 until the paper went of business in 1968, the *Herald-Tribune* was the rival of *The New York Times* and the voice of east-coast establishment Republicans. This collection became available for research in March of this year, and contains clippings, inter-office memos, research information, and government pamphlets. The files also contain clippings from other major New York and east coast dailies of the time, such as the *New York Sun*, the *New York Daily News*, the *New York World*, *New York Telegram*, and *The New York Times*. The clippings are arranged in biographical and subject files. The subject file has a card index. The staff will fetch requested material since the collection is stored off-site. Guidelines for use of this collection are still in development, but the staff requires some advanced notice for a fetch. The older, pre-World War I material is brittle and in bad shape. Photocopying is done by the staff at 25¢ a page. Plenty of parking is available in front of the Center. For more information, contact: Steve Stappenbeck, The Center for American History, Sid Richardson Hall 2.101, The University of Texas, Austin, TX 78712 Tel: (512) 495-4557; FAX: (512) 495-4542

On the other side of campus, at the Harry Ransom Humanities Research Center is the morgue of William Randolph Hearst's New York *Journal-American*. The *Journal-American* was the flagship of the Hearst communication empire. Hearst created the *Journal-American*, on June 24, 1937, merging two papers he owned that had lineages dating back to the 1890s. The paper went out of business in 1966. Like the personnel at the *Herald-Tribune*, the staff of the *Journal-American* collected clippings from many New York and east coast papers. The clipping folders in this morgue reach back to 1896, and are arranged into biographical, geographical, and subject files. The morgue also contains 2 million photos, and another million negatives. Items will be fetched and copied at 25¢ a page on request. Since the staff of the paper used an odd, unpredictable filing system, researchers should fully explain their topic and identify important individuals when making research requests. This morgue is also located in off-site storage, and the archives staff requires notice of at least twenty-four hours. Parking near the Center is expensive and limited. For more information, contact: Ken Craven, Head of the Reading Room, Harry Ransom Humanities Research Center, P.O. Drawer 7219, The University of Texas, Austin, TX 78713-7219. Tel: (512) 471-9119 FAX: (512) 471-2899

<http://rowan.lib.utexas.edu/Libs/HRC/HRC.NYJA.html>

Diplomatic historians might also find the clipping file on Texas political figures and politics in the Legislative Reference Library of some use. This collection of articles from major Texas and a select number of national dailies goes back to the start of the century. A focus on Texans and Texas politics might seem too narrow to be of use to members of SHAFR,

until one realizes the number of Texans involved in the making of U.S. foreign policy: Lyndon Baines Johnson, George Bush, James Baker, Tom Connally, John Tower, John Connally, Rob Mosbacher, Edward M. House, and Chester Nimitz are some of the better known figures who have held important positions affecting foreign policy. The clippings have been scanned into a computer, making a keyword search possible. The Legislative Reference Library is on the second floor of the state capitol building. Free parking is available on the north side of the capitol at the corner of 15th St. and Congress.

OPENINGS AT THE JIMMY CARTER LIBRARY

by

Martin I. Elzy

JIMMY CARTER LIBRARY

The Plains File consists of material moved from President Carter's office in the White House to his home in Plains, Georgia, at the end of his presidential administration. The material was then used by the President in writing *Keeping Faith: Memoirs of a President* (1982), before being transferred to the custody of the Carter Library. Most of the material was photocopied for the President's use from originals that are included in other Carter Library files from the White House. Within this collection of eighteen linear feet, two of the three series contain foreign policy material. About two feet of the material is in the series, President's

Personal Foreign Affairs File, arranged mostly by country and consisting of correspondence and notes. The larger Subject File of correspondence, notes, and briefing papers is not exclusively devoted to foreign policy but does include such compilations of types of documents as Brezhnev-Carter correspondence, daily Iran updates, National Security Council weekly reports, and State Department evening reports. Also included are the intriguingly titled folders "Mid East: Camp David Summit, President's Working Papers." The Plains File includes many document withdrawal sheets listing documents for which researchers may request mandatory declassification review.

The Carter Library also opened to review on request a folder title list for approximately twenty percent of the White House files of National Security Adviser Zbigniew Brzezinski and his staff. For the first time researchers get a peek into that 750 feet of material. However, only the folder title list is currently available and that only to a portion, twenty percent, of the file. Researchers may request the processing of folders listed. Carter Library staff will then open what can be opened from those folders and submit the security-classified documents for mandatory declassification review. When a folder is opened, a handwritten date of that opening will be placed next to the folder in the folder title list so that researchers will know which folders are available.

The National Security Adviser's file consists of two sub-collections, each of which includes sixteen series. The first sub-collection was called "Brzezinski Material" by the White House filing staff and the second was called "Staff Material." From the "Brzezinski Material" sub-collection, eight of the series have folder title lists and are subject to researcher request. From the "Staff Material" sub-collection, one series

has been entirely listed by folder title, and two have been partially listed.

For more information concerning this opening, please contact the Jimmy Carter Library, 441 Freedom Parkway, Atlanta, Georgia 30307; library@carter.nara.gov; 404-331-3942; fax 404-730-2215.

LETTERS

For whatever reasons, items published recently in the *Newsletter* have caused readers to respond vigorously to the editor or to one another. As editor, I am pleased to learn that the *Newsletter* is read by the membership and that it can serve as a venue for discussion. Several exchanges etc. follow:

— the editor

[The following is a comment on Shane Maddock's "LBJ, China and the Bomb: New Archival Evidence (*SHAFR Newsletter*, March 1996)]

The note by Mr. Maddock in the March 1996 *Newsletter* illustrates the problems with drawing larger inferences from a single government document uncovered in the files some thirty years after it was written. The document in question was a paper by George Rathjens of the Arms Control and Disarmament Agency summarizing parts of a paper I wrote in 1964 as a member of the State Department's Policy Planning Council on the subject of preemptive action against Chinese nuclear capabilities. Mr. Maddock draws the inference from the Rathjens' memo and his summary of my paper that, contrary to claims in McGeorge Bundy's book, *Danger and Survival*, the possibility of preemption was indeed taken very seriously in the U.S. Government.

I have no specific knowledge of the work of the Gilpatric Committee on non-proliferation for which Rathjens evidently prepared his paper, nor do I know whether his piece was ever discussed there. (I have seen a sanitized version of the Gilpatric report which makes no reference to preemption.) But I can speak to the background of my paper which provided the basis of much of the substance of the Rathjens piece.

In 1963-64 I directed a major interdepartmental study of the implications of China's prospective acquisition of nuclear weapons. Our interdepartmental committee concluded that the principal problems that that development would create were likely to be political rather than military, though we recognized that it could stimulate some further nuclear proliferation — development of an Indian capability was seen as the most likely possibility. Although the subject of preemptive military action against the Chinese capability was probably discussed in our committee, I am quite certain that it received relatively little attention in the reports we prepared. These reports were, incidentally, never formally cleared within the departments concerned.

However, in April, 1964, Walt Rostow, then the Chairman of the Policy Planning Council, asked me to prepare two papers on my own on the basis of the work our committee had done, but without formal communication with other agencies. The first was a summary of our overall study which mentioned in one sentence that preemptive military action would be undesirable except perhaps as part of a military response against mainland China in the event of major Chinese aggression. The second was a more thorough examination of the preemption issue. The former report has been declassified by the LBJ Library.¹³ The complete version of the latter, referred to by Rathjens, has not so far as I know been declassified.

¹³Memorandum for the President from W.W. Rostow, "The Implications of a Chinese Communist Nuclear Capability," April 17, 1964. (Country File, Asia and the Pacific, Cambodia, China, NSF, Box 237, LBJ Library.)

After these summary reports were prepared, Rostow, as was often his wont, sent both off to the White House with covering memos. Sending memos to the President and other high-level officials was one of Rostow's favorite ways of seeking attention for the products of the Council, which unhappily, like much foreign policy planning, were often ignored. Sending them to the President says nothing in itself about the attention such papers received. The general attitude of non-alarmism in the first of these reports did represent a consensus within the government — growing out of the work of our committee — which was later reflected in the official White House reaction to the first Chinese nuclear test in the fall of 1964.

As Rathjens says, the report on preemption was probably characterized as reflecting “the broad consensus” of State, CIA, DOD, and ACDA. Such a statement was designed to suggest, I believe, the fact that the representatives of those agencies on my committee accepted the case against preemption without necessarily concurring in all of the details of my argument. As I have said, I wrote these papers without clearing them with the agencies most concerned. Contrary to Rathjens' inference, however, my paper had no special standing, so far as I am aware, as providing “the basis of any subsequent consideration of the subject.”

I am also puzzled as to how Mr. Maddock can argue that the “possible use of nuclear weapons” was implied by the study I prepared. As Rathjens' comment on my report makes clear, use of nuclear weapons to take out the Chinese nuclear capability was not discussed in my paper. It is also not clear to me that the second numbered paragraph on the first page of the Rathjens' memo refers to a proxy attack as Maddock seems to assume in his commentary. The deleted sentence might have referred to any supposedly “clandestine” action as the reference to “discovered clandestine” action in the introductory sentence in par. 4 on the next page seems to suggest.

While I know nothing of what other consideration may have been given to preemptive action, I am quite certain that the paper I

prepared was not in itself a significant indicator that this was a subject that was more seriously examined within the government than has been claimed by Bundy. On the basis of my experience I also doubt very much that U.S. policy makers ever thought of military action against China as an “enticing” option as Maddock suggests. Not only practical, but also moral, considerations argued against it.

Robert H. Johnson (National Planning Association, Washington, D.C.)

* * * * *

February 28, 1996

To the Editor:

In April, 1993, I submitted a short communication, “Note on an article by Barton Bernstein, ‘Seizing the Contested Terrain of Early Nuclear History,’” which had earlier been published in *Diplomatic History*. In that note, which you subsequently published, I wrote as follows:

Barton Bernstein dismisses Harvey Bundy’s claim that the atomic bombs dropped on Hiroshima and Nagasaki were detonated high in the air to minimize radioactive poisoning. According to Bernstein the claim is “incorrect.” Bernstein bases this assessment on a study of minutes and memos between members of the so-called “Target Committee” as well as some of General Leslie R. Groves’s post war correspondence.

Unfortunately, in this instance, Bernstein was looking in the wrong place.

I then went on to cite and quote from a memo, dated 23 July 1945, written by J. Robert Oppenheimer, in which Oppenheimer states as one of the reasons for raising altitude of the detonation point of the atomic bomb that was to be dropped on Hiroshima that “with such

high firing heights it is not expected that radioactive contamination will reach the ground.”

Professor Bernstein is quite concerned that readers of SHAFR Newsletter know that in fact, he did have a copy of that memo in his files. I am writing to confirm the fact that Professor Bernstein has convinced me beyond any shadow of a doubt that he did have a copy of that Oppenheimer memo of 23 July 1945. The long delay in my writing this letter is my responsibility. For a long time I did not appreciate the fact that whether or not I believe Professor Bernstein looked in the wrong place, (which I do) it is extraordinarily important to Professor Bernstein that the implication not be that he had never seen the document in question. I take this to mean that Professor Bernstein does not believe that the document in question is relevant to the issue.

Sincerely yours,

Stanley Goldberg

(Transmitting Professor Goldberg's letter to Professor Bernstein for comment resulted in the following essay. - the editor)

DOING NUCLEAR HISTORY:
TREATING SCHOLARSHIP FAIRLY AND
INTERPRETING PRE-HIROSHIMA THINKING ABOUT
"RADIOACTIVE POISONING"

by
BARTON BERNSTEIN

So shall you hear
Of carnal, bloody, and unnatural acts,
Of accidental judgments, casual slaughters,
Of deaths put on by cunning and forc'd cause,
And in this upshot, purposes mistook
Fall'n on the inventors' heads.

Hamlet

...it had been assumed that the bomb would be dropped from a height that would minimize radioactive poisoning in order to avoid any contention that poison gases were being used.

Harvey Bundy, September 1946

A-bomb history, perhaps inevitably in our times, is fraught with controversy and often full of passion, because many of the questions are politically, and ethically, of great concern: Why did the United States drop bombs on Japan? Were the bombs necessary, or avoidable, and what do these terms mean? Were there anti-Soviet motives influencing the use of the bomb, and if so, did such motives dominate, or only confirm, the use of the bomb? Was the decision to use the bomb carefully considered, or did the bomb's use constitute the relatively comfortable implementation of an assumption? On these major issues, quarrels, even with name-calling, have sometimes erupted in publications and at meetings.

Despite interpretive differences, perhaps surprisingly, I have managed to maintain friendly relations with some historians with whom I substantially disagree (Gar Alperovitz, Kai Bird, and Robert Newman, among others), and, presumably not surprisingly, with

others (Martin Sherwin, Gregg Herken, and J. Samuel Walker, among others) where the disagreements in interpretation often seem much smaller. In dealing publicly with popular journalist Richard Rhodes, whose claims are often overblown, I have willingly risked going against the grain of published reviews, though acting in agreement with some A-bomb historians, in labeling Rhodes's two books as often derivative, seriously under-researched, and sometimes careless.

My aim, generally, in dealing with serious A-bomb history and responsible nuclear journalism, is to treat that work with respect: not to pick quarrels on minor matters, not to make unfair assumptions, not to ascribe invidious motives, and often to understate criticisms and differences. Above all, to try to be scrupulously fair in analyzing and reporting others' work, and to read the relevant documents carefully,

In seeking to interpret the major actors in the A-bomb past, and especially Secretary of War Henry L. Stimson, Secretary of State James F. Byrnes, President Harry S. Truman, General George C. Marshall, General Leslie Groves, physicist J. Robert Oppenheimer and his colleagues, and scientist-advisers Vannevar Bush and James Conant, among others, I have struggled to try to understand them, to recapture their sense of options and possibilities, and to explain fairly — though often disagreeing with — their thinking and their actions on crucial matters. Obviously, there is a dangerous tension in such a process, for understanding these men in their own terms can risk accepting their values, while criticizing their values can risk not understanding these men. It is a perilous tension, one that cannot be fully avoided, despite self-conscious efforts.

In a Winter 1993 *Diplomatic History* article on the roles of primarily Stimson and Conant in the construction of early postwar A-bomb history, I assessed as "incorrect," in a single sentence, Harvey Bundy's private 1946 claim about why the United States had detonated the A-bombs high in the air over Japan. Bundy had written in a private memorandum for Stimson that the air-detonation

was to “minimize radioactive poisoning...to avoid any contention that poison gases were being used” (my emphasis now added).¹⁴ I had sent a draft of this essay, with that statement to McGeorge Bundy, well before publication, and he had not quarreled, either as an A-bomb historian or as Harvey’s son, with this brief characterization of what was clearly a very minor issue in a lengthy essay.

The issue of why the U.S. chose a high air burst, and not a ground-burst or one very close to the ground, is an interesting historical problem. Up to 1992, in the preceding 17 years, I had intermittently spent a few months on that problem, but the subject was quite marginal to the purposes, and focus, of my 1993 published essay. The problem of the air-burst was a subject on which I had drafted a few pages in a 1985 and still-unpublished essay, one that I had retained in my files, because in dealing with some issues of radiation and fallout, I had not resolved problems satisfactorily, in my judgment. In 1985-86, a few scholars, journalists, and Manhattan Project alumni had read that essay, and at least two concluded that I had inadequately handled some interpretive issues.¹⁵ Interestingly, especially in terms of later controversy, a nine-line paragraph in that 27-page paper (“Japanese Deaths By Radiation — American Denials and Acknowledgments”) had drawn upon and quoted from a 23 July 1945 memorandum by Los Alamos director J. Robert Oppenheimer to Brig. Gen. T.P. Farrell and Captain W.S. Parsons on the subject of the air-burst for the A-bombs. That memorandum had been available to me from Los Alamos records and also from National Archives materials, and the different formatting of the two versions in no way altered the prose content of Oppenheimer’s report.

¹⁴Barton J. Bernstein, “Seizing the Contested Terrain of Early Nuclear History: Stimson, Conant, and Their Allies Explain the Decision To Use the Atomic Bomb,” *Diplomatic History* 17 (Winter 1993): 47.

¹⁵Barton Hacker to Bernstein, 20 September 1985; and Dr. Charles Rea to Larry [Suid], 13 September 1985, which Suid sent me in xerox.

Focusing, and even seeming to pounce, on my one-sentence statement about Harvey Bundy's claim in my 38-page, 1993 *DH* article, Stanley Goldberg rather soon wrote his own article-note.¹⁶ In it, he misrepresented my research, disregarded both publicly and privately available information, incorrectly summarized Bundy's and my position, also misquoted, and offered his own speculative interpretation of Oppenheimer's pre-Hiroshima *intentions* for having the U.S. explode the bomb well above the ground. Central to Goldberg's stated argument, as first expressed in his Spring 1993 correspondence with me and then in his September 1993 *SHAFR Newsletter* article-note, was the Oppenheimer memorandum of 23 July 1945, which Goldberg found in the Los Alamos archives and which he insisted, despite substantial contrary evidence, that I had failed to find, read, and use.

Goldberg claimed that I had failed to find this allegedly crucial Oppenheimer document because, in Goldberg's words, "Bernstein was looking in the wrong place." By that, he seemed to mean the wrong files. Actually, as I told him by letter in April 1993, when I sent him one of my own archival copies of that document for added evidence, "I had looked in the right place, have long had the memo at issue, and we do substantially disagree on its interpretation and possibly also on the meaning of Bundy's phrase about 'radioactive poisoning.' Certainly, my other publications have made clear, I think, that I have used a number of Los Alamos documents, especially from around the time of the Trinity test (see my *IS [International Security]* essay on tactical weapons), so why assume that I had used them, probably gone to Los Alamos, and not worked in the related papers involving expectations about radiation in the attacks on Japan? It seems to me that the more reasonable process for you would have been *not* to make any assumption about my allegedly inadequate research, or to ask me, or to make a more

¹⁶Stanley Goldberg, "Note on Barton Bernstein's 'Seizing the contested Terrain of Early Nuclear History,'" *Society for Historians of American Foreign Relations Newsletter* 24 (september 1993): 5-7.

favorable assumption based upon citations in my publications.”¹⁷ Put bluntly, be fair and accurate, not snide and inaccurate.

In that same April letter to Goldberg, I followed with another paragraph on the same subject: “Of course, the basic issue is what that July 23rd document means in terms of addressing motives pre-Hiroshima about avoiding ‘radioactive poisoning.’ I assume that you know that I’d been intermittently working on issues of ‘radioactive poisoning,’ which can include radiological warfare, since the early 1980s or before and actually published two little essays on that subject.¹⁸ Of course, you had no way of knowing that the subject has continued to be of interest to me and that I had carefully thought about Bundy’s comment at least since the late 1970s, about the time when I first drafted the essay that, after revisions, I decided to send to *DH* in 1991 [which was published in Winter 1993].”

Naively, I thought that my reply in late April, with a copy of the Oppenheimer document (in a different format from the copy that Goldberg had), would persuade him: that he would drop the contention about my allegedly defective research, and possibly even reconsider his own interpretation of that Oppenheimer document. The first (that he would drop his incorrect contention) was an expectation; the second (his reconsidering his interpretation) was a vague hope, not an expectation.

¹⁷Bernstein to Goldberg, 28 April 1993, with a xerox copy of my xerox copy of the National Archives copy of this 23 July 1945 document, on which (for Goldberg) I wrote these words, “Here’s a copy from my files. It seems to be a retyped copy by Washington of another version, which is the Los Alamos one.” The reference to the *International Security* article was to Bernstein, “Eclipsed by Hiroshima and Nagasaki: Early Thinking About Tactical Nuclear Warfare,” *International Security* 15 (Spring 1991), and see notes 9-11, 22-24, 33, and 38 for explicit citations to Los Alamos documents.

¹⁸At the time, I did not recall that, apparently at his 1987 request, I had sent him those two articles in 1987. Bernstein to Goldberg, 6 November 1987.

Though I did not realize it at the time, Goldberg simply brushed aside my April private statements about my research, my noting my published citations to Los Alamos archival documents, and my providing him with the copy of the 23 July Oppenheimer memorandum that was differently formatted from his copy. Such evidence, I think, would have persuaded reasonable scholars, since the documentation, including my earlier published work with Los Alamos citations, did seem to constitute substantial proof.

In his September article, Goldberg not only ignored all this evidence but he actually misrepresented my 1993 relevant footnote, in my published *DH* article at issue, in order to reach the wrong conclusion about my research. In my footnote, on page 47 of my Winter 1993 *DH* article, I had stated that I could find no evidence substantiating Bundy in the target committee minutes “or in *other documents*” (my emphasis now added). That brief phrase, “other documents,” had been carefully crafted because I did not want to burden readers, for what was a very minor point in my article, with a footnote reference to about 25 other archival collections, including various Los Alamos files, Stimson’s papers and diary, OSRD materials and especially the Bush-Conant files, the papers of Generals Henry Arnold and Carl Spaatz, JCS and Army OPD records, Oppenheimer’s papers, Army Air Forces records, the Harrison-Bundy files, Ernest Lawrence’s papers, Edward Teller materials and skimpy Harold Agnew files, that I consulted. But Goldberg somehow chose to state, uncharitably and incorrectly, that “other documents” could denote *only one* collection — in his words, “General Leslie R. Groves’s post war correspondence.” And that collection, Goldberg announced, was not the right collection, and only the Los Alamos file, he implied, was the right collection.

I still do not understand why Goldberg misrepresented my footnote statement, thus unduly narrowing my research. Nor do I understand why he disregarded my private correspondence, my providing the Oppenheimer document, and the published evidence that I had used Los Alamos materials. But Goldberg could not be weaned from his

misinterpretation and thus his misrepresentation, despite my efforts *some months before* he published his article in September 1993.

I had initially planned, upon discussions with the *Newsletter's* editor, to reply in the September issue to Goldberg's charges and strange ways. But I decided not to do so, substantially because an eye infection in the late Spring and early Summer made it difficult for me to read or write. And then I decided that I did not want to embarrass Goldberg by pointing out publicly, as I felt I should, if I engaged in public discussion, that he had misrepresented my research, ignored our private correspondence, and acted rather strangely. Despite my public silence, however, I did find his misinterpretation annoying and unfair, as I told him on more than a few occasions in 1993-96.

On a few occasions, upon my direct request, he stated that he would correct in print, by letter to the *Newsletter*, his statement about my research. But, somehow, that did not happen — and we would have the same discussion again, with the same indication of a letter by him. Finally, in February 1996, only partly because of his *Newsletter* article was cited by another scholar,¹⁹ I pushed Goldberg forcefully on this matter. The result was what seems a grudging and incomplete letter by him, one that curiously omits any reference to our correspondence and the evidence I had sent him well *before* the publication of his September 1993 article. His letter implies, mostly by omission, that he had not known of any of these problems until after he published — and then he still, strangely, insists that “Bernstein looked in the wrong place....” It seems “extraordinarily important” to Goldberg to insist on this.

Frankly, I am unsure of how anyone could budge Goldberg from his conclusion. Presumably, only pre-1993 publication of my 1985 draft-essay using that Oppenheimer document, or one of the half-dozen readers in 1985-86 turning up that essay in his files, or

¹⁹Robert J. Maddox, *Weapons for Victory: The Hiroshima Decision Fifty Years Later* (Columbia, Missouri: University of Missouri Press, 1995), 190 (note 17) and 196.

notarized evidence that I had read the Oppenheimer document would meet Goldberg's incredibly high — though always unstated — criteria for evidence on this matter. Proving that I had been in the relevant Los Alamos files would obviously be easy, but inadequate in view of his implicit standards.

In his February 1996 letter to the *Newsletter*, Goldberg — typically, I think — also misrepresented my position on the Oppenheimer document. I had never stated or implied that it was not “relevant to the issue” (Goldberg's words). Rather, I had strongly, and repeatedly, disagreed with *Goldberg's interpretation* of that document. I had contended that he greatly misconstrued it, and I also called upon him, at least a few times, to do the additional research, in the archives and in interviews with Los Alamos alumni, to put that single document in the proper historical context. Once or twice, I did point out to Goldberg that it seemed very strange that he would disregard substantial evidence about my research so that he might also imply a “new” discovery, and then that he would use very loose standards of evidence and interpretation to make too much — and to reach dubious conclusions — from this Oppenheimer document. I did tell him that he had a lurking double-standard, one that seemed rather self-serving and less than responsible. I often feared that Goldberg, as is frequently the situation for people in unpleasant matters, managed not to understand this criticism.

I do think that he substantially misinterpreted Oppenheimer's 23 July memorandum. In my view, Oppenheimer wanted a high-altitude burst *only* in order to maximize physical damage on the ground, and *therefore* he was concerned about establishing the proper height for the detonation over Japan's target cities. As Oppenheimer wrote in numbered paragraph 2 of that memorandum: “It is essential that the nature of the target be taken into account in determining these firing heights. The figures above [in Oppenheimer's numbered paragraph 1 specifying a height of 1850 feet] are appropriate for maximum demolition of light structures.

Should the target include important heavy structures, the fusing heights should be revised downward.”

These sentences are not the statements, contrary to Goldberg's general argument, of Oppenheimer being, in Goldberg's words, “quite concerned that the fireball not reach the ground.” Interestingly, in Goldberg's lengthy but abridged quotation from Oppenheimer's memorandum, Goldberg entirely omitted the first sentence (“It is essential...heights.”) that I quoted, and somehow also butchered the third sentence by leaving out some words and then failing to note this deletion. Thus, whether intentionally or not, Goldberg seemed to distort the relevant evidence of what Oppenheimer actually said in his memorandum and to omit important material that partly led me, and possibly could lead others, to reject Goldberg's interpretation.

In a comparatively minor error, but one that would undoubtedly have angered Oppenheimer, Goldberg somehow incorrectly transcribed one of Oppenheimer's other sentences, created a grammatical error by making the subject singular (“setting,” instead of “settings” in the document) but retaining the plural verb (“have”), and then Goldberg or the editor, or both men, inserted a “sic” in Oppenheimer's own prose. That was not a major Goldberg error by incorrectly transcribing the text, but it does seem unfortunately typical of his mode, at least in the particular venture at issue. By Goldberg's process, his own error was blamed on somebody else, and then that person (Oppenheimer in this case) was unfairly chided and held responsible for Goldberg's mistake.

Goldberg did correctly quote Oppenheimer, from his numbered paragraph 3, on what Goldberg seemed to assume was a key point, “With such firing heights it is not expected that radioactive contamination will reach the ground.” But the crucial interpretive problem, which Goldberg should have faced very directly, is whether this statement and the related additional words in Oppenheimer's memorandum clearly meant that Oppenheimer *sought* among other purposes to make sure that there was no such

contamination, or whether he was only noting that the absence of such contamination was the predictable result, because of the nature of fission bombs at that time. Put simply, was this one of Oppenheimer's *purposes*, or only his *acknowledgment* of a *predictable consequence*?

Nowhere in this memorandum did Oppenheimer cite, or seem to imply, that the avoidance of such contamination was among his *purposes*. So, why ascribe such a purpose to Oppenheimer? Somehow, Goldberg decided that Oppenheimer was "quite concerned that the fireball not reach the ground." Goldberg implied that such concern was evidence of *purpose*, and then Goldberg, in a speculative conclusion, ended his brief article, with this unsubstantiated sentence: "But it is *likely* that one of the problems Oppenheimer wanted to avoid was prevention of quick on-site inspection by American observers, immediately after the war, of the cities on which the atomic bomb had been dropped" (my added emphasis).

Why should we accept Goldberg's speculative interpretation for the "likely" reasons for Oppenheimer's alleged purpose when Goldberg has not even established that Oppenheimer had such a purpose? Somehow, Goldberg virtually dashed from Oppenheimer's alleged "concern" to ascribing purpose to Oppenheimer, and then offering a reason for such an alleged purpose. In that process, Goldberg seemed to demonstrate rather loose causal thinking, to strain the evidence, and to push well beyond the evidence he cited. Goldberg's argument seems hasty, requiring more systematic analysis, and demanding more evidence. Certainly, in trying to establish pieces of his argument, Goldberg should have worked in, and cited, the archival files well before Oppenheimer's 23 July memorandum to understand the historical context and, thus, the likely meaning of Oppenheimer's memorandum.

Goldberg's apparent procedure was to avoid explicit analysis and discussion of much of the relevant evidence and to think that he had established something quite important by noting that General

Groves, on about 12 August, had privately announced the plan to send observer teams to Hiroshima and Nagasaki soon after the then-imminently expected Japanese surrender. That plan did reveal that Groves was indeed concerned about whether the bombed areas were safe for U.S. troops, and also whether recent Japanese reports about lingering ground contamination producing deadly illness were correct. But there does not seem to be any pre-Hiroshima evidence that Groves, or Oppenheimer earlier, had conceived of the high air-burst over Japan's two cities *in order* to minimize deadly fallout. Groves had told General Marshall, however, that the high air-bursts would make it possible for U.S. troops to enter the bombed areas after about 30 minutes, thereby assuring Marshall that tactical nuclear warfare on Kyushu, in which Marshall was interested before the surrender, was possible in practice. Groves's 30 July 1945 memorandum on this subject, following his related mid-June memorandum to Marshall, neither stated nor implied that the high air-bursts had been chosen, even in part, to make such U.S. ground actions possible.²⁰ Groves's implications seemed to be that the high air-bursts had been selected much earlier, and that they would have the useful result of accommodating U.S. tactical nuclear warfare on Kyushu, in connection with the 1 November U.S. invasion ("Olympic") on that southernmost Japanese island.

When Goldberg drafted his article-note, there was already some published literature on secret American thinking in 1945, before Japan's surrender, about using tactical nuclear warfare against Japan.²¹ Strangely, Goldberg seemed to avoid that literature, the relevant materials that it cited, and the problems that it suggested for his line of argument. Possibly Goldberg did not deem that

²⁰Bernstein, "Eclipsed by Hiroshima," 160-161; Groves to Chief of Staff (Army), 30 July 1945, Top Secret Documents of Interest to Groves file, Manhattan Engineer District Records, Record Group 77, National Archives.

²¹Bernstein, "Eclipsed by Hiroshima"; and Marc Gallicchio, "After Nagasaki: General Marshall's Plan For the Use of Tactical Nuclear Weapons," *Prologue: Quarterly of the National Archives* 24 (Winter 1991): 396-404.

literature, basically two articles, relevant to his work. Or maybe he had other reasons for not dealing with them.

Goldberg, in doing archival research, should have carefully read the records, starting well before the 23 July memorandum, to determine whether there had been a longrun assumption, since at least about early 1944, that the bombs would be detonated high in the air, and if so, why? On this subject, a March 1944 report, by physicists Hans Bethe and Robert Christy ("Memorandum on the Immediate After Effects of the Gadget"), which I got from Los Alamos in 1984, is certainly relevant.²² It generally assumed a high air-burst, but also reported on the radioactive contamination on the ground from a low or ground detonation. That report did not seem concerned about such contamination, and was simply predicting such a result under specified conditions.

Six weeks later, in mid-May 1944, Captain W.S. Parsons wrote to tell General Groves that provisions were being made "for detonating the bomb well above ground, relying primarily on blast effect to do material damage. In this connection, the present thought is to use a height of detonation such that [even] with the minimum probable efficiency, there will be the maximum number of structures (dwellings and factories) damaged beyond repair."²³

A year later, in May 1945, Oppenheimer provided Groves with a brief summary of the likely radiological effects of the A-bomb. He did not expect dangerous fallout near the target area or elsewhere reaching the earth, unless there was rain or high humidity, in which case the bomb might cause rain. There might be need for some monitoring "within some weeks of the primary detonation [but] the

²²H.A. Bethe to Maj. Gen. L.R. Groves, 30 March 1944, with Bethe and Robert Christy, "Memorandum on the Immediate After Effects of the Gadget," n.d. [March 1944], Los Alamos Records, Los Alamos National Laboratory, Los Alamos, New Mexico.

²³Parsons to Groves, 19 May 1944, Top Secret Documents of Interest to Groves file.

probable results of monitoring will be that it is quite safe to enter.”²⁴ In this report, Oppenheimer was properly warning of possible dangers, and suggesting reasonable precautions, but he was not stating that the high air-burst was chosen to allow Americans to enter the areas safely sometime afterward. Rather, he was stating that they probably could do so.

It would be an analytical mistake, in interpreting this memorandum, which Goldberg may have entirely neglected to consider, to treat it as a statement, even implicitly, about the *purpose* of the air-burst. Oppenheimer was not addressing that matter. He was focusing upon the *consequences* of an air-burst high above the ground.

On 13 June 1945, in a report to General George C. Marshall, the army chief of staff, Groves obviously drew upon Oppenheimer’s May 1945 memorandum. Groves stated, “Testing will be required before entering the target area for an indefinite period after the detonation.”²⁵ But Groves, like Oppenheimer, did not even suggest that any effort would be made in order to avoid radiological contamination. Even when discussing the possibility of ground operations in the area after the detonation, and Groves may have meant to discuss tactical atomic warfare, he simply stated that the Manhattan Project had the “necessary instruments” to make sure that the area was safe for U.S. troops.

In a separate report to Marshall on the same mid-June 1945 date, General Groves explained in some detail the thinking underlying the decision to use high air-bursts for both the gun-type (uranium) and the implosion (plutonium) bombs. Groves wrote, “The detonation

²⁴Oppenheimer to Gen. Groves, 11 May 1945, Los Alamos Records. Also see Norman Ramsey to Oppenheimer, “Observational Equipment, 14 May 1945; Oppenheimer to Ramsey, “Observational Equipment,” 15 May 1945; and Ramsey to Oppenheimer, “Observational Equipment,” 9 June 1945, Los Alamos Records.

²⁵Groves to Marshall, “Radiological Effects From Atomic Fission Bomb,” 13 June 1945, Los Alamos Records.

height has been selected to cause serious above ground [blast] damage over the maximum area." To achieve "the maximum below ground damage," he explained, the height of the air-detonation would have to be substantially decreased. The Trinity test, scheduled for mid-July, Groves stated, would provide more precise data on the ideal heights for the explosion of the two A-bombs.²⁶

In late June, Los Alamos physicist Norman Ramsey wrote to Oppenheimer and a few colleagues about the detonation height and the need for later readjustments. He thought that Los Alamos had been wise to concentrate initially on the air-blast, "since it is probably the simplest of the effective means of use." But once the weapon was tested at Trinity or in combat over Japan, he recommended using lower detonation heights on some occasions in order to destroy very heavy fortifications and underground structures. In no way did he suggest that the likelihood of greater fallout on the ground would violate values or purposes that he and others had held to date. In apparently thinking about future American tactical nuclear war, he did add, however, that there would be need for "further study of the biological effects of the gadget [A-bomb] as affects close support operations [by U.S. troops]." ²⁷

On 18 July, two days after the Trinity test and five days before Oppenheimer's 23 July memorandum to Groves, the Los Alamos director discussed the meaning of the Trinity test in a memorandum to one of the working physicists at the Los Alamos laboratory. In his note to physicist Robert Brode, Oppenheimer wrote on 18 July, "the successful test of the Fat Man [the implosion weapon] has indicated that our estimates of efficiency are probably not seriously in error in the sense of being too optimistic. For this reason, it

²⁶Groves to Marhsall, "Atomic Fission Bombs," 13 June 1945, copy from George C. Marshall Library, Lexington, Virginia.

²⁷Ramsey to Oppenheimer, Parsons, and Norris Bradbury, "Unsatisfactory Features of Weapons Program," 23 June 1945, Los Alamos Records.

seems appropriate to raise the firing height for the Little Boy [the uranium bomb] to 1850 above terrain.”²⁸ Significantly, Oppenheimer’s explicit standard was efficiency, which presumably meant blast damage on the ground; he did not mention radiation or fallout, much less efforts to avoid their effects at ground level. Brode, as a physicist who understood the physics of the bomb, and the reasons for new calculations after Trinity, believed (according to his later testimony) that the only purpose of the high air-burst was to maximize ground damage from blast. Avoiding dangerous fallout on the ground near the target was a predictable consequence, not a purpose.²⁹

Whether or not Goldberg, before his 1993 article, had done this kind of research, carefully read these reports and other similar archival materials, and then reconsidered his interpretation, I do not know. Certainly he omitted such research, if done, from his brief 1993 article and, thereby, failed to *establish* the appropriate historical context for interpreting Oppenheimer’s 23 July memorandum.

In addition, as I told Goldberg privately, he had an intellectual obligation, as I had done, to check his interpretation by querying a number of Los Alamos alumni from the World War II period, despite the dangers of self-serving claims and the frailties of memory. In the 1970s and 80s, well before Goldberg offered his dubious interpretation, I had been interested in the issues of the air-burst, radioactive contamination, and radiation. Thus, I had questioned, usually in phone interviews, WWII Los Alamos

²⁸Oppenheimer to Brode, 18 July 1945, Los Alamos Records.

²⁹The first (phone) discussions with Brode in 1965-66 on this subject were at the behest of a Stanford student who raised questions about the high air-burst and fallout, at a time when I knew very little about the subject. In 1967 or 1968, I also received confirming judgments from chemist George Kistiakowsky, who had been at wartime Los Alamos, and from physicist I.I. Rabi, who had been a consultant to Los Alamos and present at the 16 July 1945 Alamogordo test. In neither discussion were my questions especially probing or deeply informed, so the evidential value of these interviews is slight.

physicists Luis Alvarez, Kenneth Bainbridge, Hans Bethe, Robert Brode, Richard Feynman, John Manley, Frank Oppenheimer, and Emilio Segré, among others. Not one believed that the high air-burst had been conceived, even in part, in order to avoid radioactive ground contamination. And after receiving Goldberg's 1993 article, I again questioned Bethe and also spoke, for the third time on this subject, with former Los Alamos physicist Robert Bacher. Bethe confirmed what he had told me years earlier, and Bacher independently again agreed, though Ramsey alone dissented.³⁰ I also tried, unsuccessfully, to gain responses on this subject from four other Los Alamos alumni.

On the basis of the evidence I had in the early 1990s from archives (including Oppenheimer's 23 July Memorandum) and from interviews, before submitting my *Diplomatic History* essay, it seems to me that I had substantial reason to conclude that Harvey Bundy's 1946 contention was "incorrect." Nothing that Goldberg has stated in print, nor in private correspondence with me, seems reason to revise my judgment.

Admittedly, the near-unanimity among more than nine Los Alamos alumni that the purpose of the high air-burst was maximization of blast damage, and not also avoidance of dangerous fallout, was rather surprising, because it could make scientists before Hiroshima seem insensitive to fallout. Such recollections, partly because they are not self-serving but contrary to self-interest, seem quite credible. In contrast, the opposite recollections, because they would be self-serving, could be regarded as possibly suspect.

It is certainly conceivable, though not very likely, that substantial, unsettling interview-evidence could become available on the subject of the reason for the high air-burst. Should four or five prominent Los Alamos alumni state, after 51 years, that the high detonation was planned partly with the purpose of minimizing fallout on the

³⁰Bethe to Bernstein, 28 July 1993; telephone interview with Bacher, 15 September 1993; and Ramsey to Bernstein, 2 July 1996.

ground, such claims would have to be taken seriously in a way that probably only a few such claims could be very easily doubted and simply tucked into a footnote or a single sentence on the subject. However, even four or five claims, in the face of more than nine on the opposite side, would not lead to the conclusion that this comparatively small minority is correct. A responsible historian would report such claims in the text, and seek to assess them for readers. That assessment would probably include going back to the relevant archival collections to re-examine the 1944-45 documents to determine whether they can be reasonably interpreted to support the claims of this (hypothetical) minority.

Ultimately, the resolution of the issue would depend very heavily, so I believed before Goldberg wrote and as I still believe, upon the *pre*-Hiroshima archival sources. Post-Hiroshima archival sources, because of the uneasiness among Groves and others about August 1945 reports of radiation illness in Japan,³¹ are likely to be unreliable, or not very reliable, if they contend that here had been a *pre*-Hiroshima effort to minimize radiological fallout on the ground. The post-Hiroshima uneasiness may well have created some need to rewrite the recent past, to ascribe purposes that had not existed, and to provide a psychological defense against unpleasant charges.

Perhaps substantial evidence — ideally from *pre*-Hiroshima documents³² — will cast new light on the questions of why the

³¹See, for example, Groves-Rea transcripts of phone conversations, 25 August 1945, Manhattan District Records. Also see Groves Diary, 8 August 1945, Record Group 200, National Archives; and George Kistiakowsky, "My Activities During Your Absence," 4 September 1945, Los Alamos Records.

³²N.A. (probably Stafford Warren or Louis Hempelman), "Toxic Effects of the Atomic Bomb," 12 August 1945, in Top Secret Documents of Interest to General Groves files, can be read with some strain to suggest a *pre*-Hiroshima purpose of avoiding radioactive contamination on the ground. The more reasonable and not strained interpretation, which I subscribed to in 1991 and also now, is that this post-Hiroshima document was expressing *pre*-Hiroshima expectations, not purposes.

high air-burst was selected and whether there was an intention to minimize, or avoid, ground contamination. Until that evidence, if it exists, is presented in a sustained, careful, and convincing way, there is no reason to accept Goldberg's interpretation (based on a strained reading of one document) and even less reason to accept what he seemed to regard as the significance of his brief article: that Harvey Bundy was not "incorrect," and that Bundy had accurately recalled a pre-Hiroshima *purpose* for selecting the high air-burst.

It is important to understand, as Goldberg presumably did not, that pre-Hiroshima evidence that Oppenheimer, or even others, desired to avoid radioactive contamination and therefore wanted a high air-burst would not adequately address Harvey Bundy's 1946 statement, which I termed "incorrect." In Bundy's claim, as I quoted him in 1993, the high air-burst had been chosen to "minimize radioactive poisoning...to *avoid any contention* that poison gases were being used" (my emphasis now added). Bundy's statement must be read carefully, not carelessly.

Somehow, Goldberg, in his brief article, and in the earlier draft that I received from him, entirely overlooked Bundy's statement of *ultimate* purpose. Thus, in summarizing my views in his own article, Goldberg omitted this important theme and contended, incorrectly, that the issue was *only* whether or not there had been efforts to minimize radioactive poisoning. That is not what I stated explicitly in the text of my essay, nor what I meant then or now. Nor is it what Bundy stated.

But it is correct, as I indicated in a footnote in my 1993 article, that the absence of evidence, after consulting the relevant archives, that there was a pre-Hiroshima effort to minimize radioactive poisoning, was sufficient for finding Bundy's claim "inaccurate." Hence, at the time, I dismissed Bundy's claim on such parsimonious evidential grounds. Only if one found documents from before Hiroshima *seeking* to minimize radioactive poisoning would there have been the

need to go carefully to the next issue: Why? Did they want to avoid “*any contention* that poison gases were being used”?

Bundy’s postwar claim of ultimate purpose (avoiding poison-gas contentions) is interesting for reasons that Goldberg seemed not to understand. After the war, for Groves, and some others too, there was a sustained effort to minimize evidence, and possibly to engage in self-denial, on the question of whether radiation had killed many Japanese. Groves seemed comfortable about accepting the fact of many Japanese deaths, but very anxious to minimize, or deny, the evidence of how lethal and injurious A-bomb radiation had been in producing these casualties. Why did radiation casualties after Hiroshima so distress Groves, while deaths by blast or heat did not upset him?³³ These are questions that Goldberg’s inaccurate summary of Bundy’s statement and my quotation of that statement managed to slide past.

Despite numerous defects in Goldberg’s short article, his focusing even briefly on pre-Hiroshima thinking about dangerous radiological fallout has the value of implicitly urging historians to seek to understand how scientists and top-level policymakers conceived of the bomb as a weapon for use on the Japanese enemy. Thus, there are important questions, little explored in the published literature, on what scientists and others before Hiroshima expected from nuclear radiation deployed against the enemy. In turn, such questions also raise another unsettling issue: In the aftermath of Hiroshima and Nagasaki, would Americans and the citizens of other nations have been as troubled by nuclear weapons if they had killed only by blast and heat, and not also by radiation? To many, that may seem a ghoulish question, but it is worth exploring to understand in part why the fire-bombings of Japanese cities by

³³ Bernstein, “An Analysis of ‘Two Cultures,’: Writing About the Making and the Using of the Atomic Bombs,” *Public Historian* 12 (Spring 1990): 103-104. This lengthy review-essay of Rhodes’s first book on nuclear history, a review that later upset Goldberg, was followed by my review of Rhodes’s second book, in “Two Historians Review Richard Rhodes’s *H-Bomb History*,” *Physics Today* 49 (January 1996): 61-64, which also upset Goldberg.

conventional weapons have received comparatively little scholarly attention and the atomic bombings far more consideration.

A-bomb history certainly deserves, because of its crucial issues, a commitment to seeking to understand why the bombs were dropped, why cities were chosen, whether there were reasonable alternatives to such targeting and to the use of the weapons, and what the bombings meant, in prospect and in retrospect, to key decisionmakers, to both elite and rank-and-file scientists on the secret project, and to other Americans also. In all that, radioactive contamination and radiation are part, but not the largest part, of the “story” that analysts seek to unravel and understand. Undoubtedly, that process of probing, understanding, and explaining will continue to result in hotly contested formulations and conclusions — but, ideally, with great efforts at fairness and close attention to the evidence and analyses by other scholars. Misrepresenting the published literature does not help us understand the contested past of A-bomb history.

* * * * *

To the editor:

This refers to Professor Frank Kofsky’s essay “Truman, Byrnes and the Atomic Bomb,” published in the June Newsletter.

— Robert J. Maddox (Penn. State)

Kofsky’s starting point is the familiar revisionist theme that President Harry S. Truman and Secretary of State James F. Byrnes knew that Japan was trying to surrender during the summer of 1945. The Japanese most likely would have done so if only they had been assured that they could retain their sacred emperor, according to this view, or if they had known the Soviet Union was about to join the war against them. Truman and Byrnes refused to extend an assurance about the emperor and sought to forestall a Soviet signal of its intentions because they *wanted* the war to

continue until they could use atomic bombs. The bombs were employed as diplomatic weapons to impress the Soviet Union, not as military weapons to defeat an already-defeated Japan.

Kofsky takes this thesis one step further. Truman and Byrnes did not merely want to awe the Soviets with the enormous destructive power of the new weapons, as some revisionists claim, they could have accomplished that by staging a demonstration. Their real motive was to show Joseph Stalin — who knew Japan was trying to surrender — that they were “sufficiently ruthless to rain atomic destruction on Japan *even when there was almost surely no military necessity for doing so.*” (His emphasis) He likens the conduct of Truman and Byrnes to the medical experiments carried out by Japan’s notorious Unit 731, and calls it “little short of premeditated murder. Butchering masses of Japanese to terrorize the Soviets was just the sort of tactic Stalin could appreciate.”³⁴

One might think that an author would have to have a great deal of evidence before levying such a monstrous charge. Kofsky has none. He explains this away with the standard conspiratorialist excuse: “cunning plotters such as Truman and Byrnes were hardly about to leave ‘smoking guns,’ much less fingerprints, so that later historians might sully their reputations.” He relies instead on “circumstantial evidence” which, as will be shown, utterly fails to substantiate his interpretation.

The basic fallacy in Kofsky’s essay is his claim that Truman and Byrnes knew the Japanese were trying to surrender during the summer of 1945, provided they could retain the emperor. The Japanese were doing nothing of the sort. Under the watchful eye of the military, civilians within the government were trying to get the Soviet Union to help arrange a negotiated peace that would have preserved the Japanese empire and political/military system intact. American officials on July 3 learned through an intercepted message

³⁴These and all other quotations attributed to Kofsky, as well as his citation of Bernstein, are from his essay, pp. 16-30.

that a Japanese spokesman had told the Soviet Ambassador that "Japan will increase her naval strength in the future, and that, together with the Russian army, would make a force unequalled in the world"³⁵ That is scarcely the talk of those trying to surrender.

The civilians wanted to send a delegation to Moscow to persuade Stalin to act in Tokyo's behalf, in exchange for which Japan would offer territorial and trade concessions. The Japanese foreign minister on July 17 spelled out the mission's purposes in a message the Americans also read: to solicit "Russia's good offices in concluding the war and also in improving the basis for negotiations with England and America." He added that "we are not asking the Russians' mediation in *anything like unconditional surrender.*" (Emphasis added)³⁶ Japan's own ambassador in Moscow sought to discourage such pipedreams. He bluntly informed Tokyo that Japan would have to accept unconditional surrender or something close to it, and repeatedly urged that an inquiry be directed to the United States as to what terms would be offered. No such inquiry was ever made.

The army controlled the situation in Japan, and its leaders gave no hint they were prepared to surrender. On the contrary, they were feverishly pouring reinforcements into the areas where they expected an American invasion to come ashore. Edward J. Drea, Sadao Asada, Lawrence Freedman and Saki Dockrill, among others, have shown beyond question that the militarists were unwilling to

³⁵MAGIC summary No. 1195, July 3, 1945, The MAGIC Documents, Summaries and Transcripts of the Top Secret Diplomatic Communications of Japan, 1938-1945, Washington, D.C.: University Publications of America, 1978.

³⁶Foreign Minister Shigenori Togo to Ambassador Naotaki Sato, MAGIC summary No. 1210, July 17, 1945.

consider surrender before Hiroshima and Nagasaki.³⁷ Even after both bombs had been dropped *and* the Soviet Union had declared war, they opposed an American offer to retain the emperor under the authority of the Allied Supreme Commander until Hirohito personally intervened against them. The claim that they would have accepted such a proposal before these cataclysmic events took place can not be taken seriously.

Getting to specifics, Kofsky cites Barton Bernstein's statement that no assurance was extended about the emperor before the bombs were used because "Byrnes feared a political backlash in America, where Hirohito was likened to Hitler and judged a war criminal, and because Truman and Byrnes feared that such modified surrender terms might also embolden the Japanese to fight on for better terms." "Professor Bernstein may be willing to assume that Brynes's [sic] and Truman's statements accurately reflected their thinking," Kofsky writes scornfully, "I am not."

If Byrnes feared in mid-July that an offer to retain the emperor would provoke a "political backlash," Kofsky asks, why was he "willing to accept just such a modification" one month later? "Nothing had occurred in the interim to lessen the likelihood of such a backlash," he points out, yet Byrnes himself "devised the arrangements" that left the emperor on the throne. "This fact alone," Kofsky concludes, "should excite our suspicion that Byrnes's 'fear' was the *pretext*, rather than the reason, for keeping the surrender terms unchanged." (His emphasis)

Kofsky apparently is unaware of the fact that even after the bombs were dropped Byrnes did argue against retention of the emperor

³⁷Edward J. Drea, *MacArthur's Ultra: Codebreaking and the War Against Japan* (Lawrence, University Press of Kansas, 1992; Sadao Asada, "The Shock of the Atomic Bomb and Japan's Decision to Surrender," unpublished manuscript in the author's possession; Saki Dockrill and Lawrence Freedman, "Hiroshima: A Strategy of Shock," in Dockrill (ed.), *From Pearl Harbor to Hiroshima* (London: Macmillan, 1994), pp. 191-212.

precisely on the ground of “political backlash.” He did so with great vigor during a meeting at the White House on the morning of August 10, which Truman called to decide whether to accept Japan’s offer to surrender provided there be no demand “which prejudices the prerogatives” of the emperor. “I cannot understand why now we should go further than we were willing to go at Potsdam,” he said, “when we had no atomic bomb, and Russia was not in the war.” He emphasized that a retreat from the unconditional surrender policy (announced by President Franklin D. Roosevelt in January, 1943) would invite charges of betraying a major war aim and result in Truman’s “crucifixion.” He drafted the conditional acceptance of Japan’s offer because Truman instructed him to do so, not because he agreed with it. He was furious at being overruled.³⁸

Kofsky similarly scoffs at the notion that Truman and Byrnes feared that an offer to retain the emperor would embolden the Japanese to fight harder, claiming “there was no *basis* for such a fear.” (His emphasis) He argues that such an assurance would “most likely result in the Japanese government redoubling its efforts to reach mutually agreeable surrender terms.” Here he commits two errors. He repeats the fallacy that the Japanese government was trying to surrender when actually it was trying to get a negotiated peace that would avoid the consequences of surrender. He also ignores the significance of the battle for Okinawa.

American troops invaded Okinawa on April 1. The ensuing struggle lasted far longer than the planners had anticipated. By the time organized resistance ended on June 21, US forces had sustained

³⁸Entry for August 10, 1945, Walter Brown’s Notes, Folder 602, James F. Byrnes Papers, Clemson University Library. Walter Brown was Byrnes’s aide and spoke with him shortly after the meeting. Admiral William D. Leahy, Truman’s chief of staff, was with the president when Byrnes arrived at the White House. Leahy already had prepared a draft reply to the Japanese that accepted their stipulation. Byrnes told Brown that Leahy “still thought he was Secretary (o)f State, just as he had been under Roosevelt, and he [Byrnes] had to show him differently.

more than 49,000 battle and 26,000 non-battle casualties. The first large-scale use of kamikazes resulted in 36 ships sunk and 368 damaged. This campaign, as Ronald Spector has written, had the “paradoxical effect of discouraging the Americans while inspiring the Japanese” with regard to the invasion of the Japanese home islands, the first phase of which was scheduled for November 1. Japanese naval strategists estimated that kamikazes could put from 30 to 50 percent of the invasion fleet out of action, and army planners thought the battle for Kyushu would be fought “under conditions incomparably more advantageous [than at Okinawa] to the Japanese.”³⁹

Japanese militarists had long since given up the pretense that Japan could win the war. They predicted that if the fighting continued long enough, however, and casualties ran high enough, a war-weary United States would abandon the demand for surrender in favor of a negotiated peace. Had Truman extended an assurance about the emperor during or after the battle for Okinawa, Japanese hardliners would have argued that the bloodbath there and at Iwo Jima earlier in the year *had* weakened the American will to go on fighting. All the more reason, therefore, to continue the war and to inflict even larger casualties when and if an invasion of the Japanese home islands — which they professed to welcome — took place. And to repeat, the militarists held effective power in Japan. They were quite prepared to arrest or assassinate those who opposed them, as they had done in the past.

The second example of what Kofsky presents as damning circumstantial evidence also proves under scrutiny to be something other than what he claims. Partway through the Potsdam Conference (July 17-August 2), Truman on July 26 issued what became known as the Potsdam Declaration. This was an ultimatum, issued jointly by the United States, Great Britain and China, calling upon Japan to surrender or face “prompt and utter destruction.”

³⁹Ronald H. Spector, *Eagle Against the Sun: The American War With Japan* (New York: Vintage Books, 1985), p. 543.

Having the Soviets sign the declaration, according to Kofsky, would have constituted the first step in making “it clear to Tokyo that its position was thoroughly hopeless and it could no longer avoid conceding defeat.” Failure to invite Soviet participation provides further evidence that Truman and Byrnes did not want Japan to surrender until atomic bombs could be used.

Kofsky correctly points out that the Soviets protested about not being consulted about the wording of the declaration, but wrongly presents this as a protest against being excluded as a co-sponsor “just as if Stalin had never bothered attending the Potsdam Conference in the first place.” but the Soviets never expressed any complaint about not being asked to sign the declaration because they had no wish to do so.

Truman failed to invite the Soviets to sign the ultimatum for the very good reason that they were not then at war with Japan. Stalin had told Truman and Byrnes on the first day of the conference that Soviet forces in Manchuria would be ready to move against the Japanese by August 15.⁴⁰

The notion that he would be willing to reveal his intentions to Japan three weeks before his troops were ready makes no sense. Equally senseless is the idea that he would want the war to end (which is what Kofsky claims might have happened had the Soviets signed the declaration) before formal Soviet entry activated the Yalta Far East Accord. This agreement between Roosevelt and Stalin provided for territorial and economic concessions in Manchuria and elsewhere in exchange for Soviet participation in the war.

Of course one can never prove that Truman and Byrnes did *not* act for the reasons Kofsky says they did. But his claim that their “excuses” were “such pathetically clumsy, transparently phony and

⁴⁰Charles Bohlen’s notes of meeting, July 17, 1945, *Foreign Relations of the United States: The Conference of Berlin (The Potsdam Conference), 1945*, 2 vols. Washington, D.C.: Government Printing Office, 1960, II, pp. 43-46.

implausible concoctions that their purpose had to be to conceal rather than reveal the innermost working of their authors' minds" is based on a hodgepodge of misrepresentations and factual inaccuracies.* The truth is that phrases such as "transparently phony and implausible concoctions" more accurately describe the examples *he* presents as circumstantial evidence."

*One of the many factual inaccuracies in Kofsky's essay is that at one point he refers to the "repeated appeals by Prince Fuminaro Konoye, Japan's special emissary to Moscow, seeking to have the Soviets intercede with the U.S. to arrange a surrender that would safeguard the person of the emperor." Elsewhere he refers to Konoye's "supplications." Konoye was not an emissary to Moscow of any kind, and neither he or anyone else ever made such an appeal.

* * * * *

April 8, 1996

Dear Editor:

You will no doubt agree with me that one of the historian's major tasks is to try to come to terms with the work previously done by his/her peers on the topic he/she is currently researching.

In addition to a book in French reviewed in the U.S. I happen to have published a number of pieces in *English* on Theodore Roosevelt and the second Venezuelan crisis that departed from and hopefully improved on previous treatments of the episode. For some reason my interpretation has not found its way into Professor

Nancy Mitchell's recent *Diplomatic History* article on the Venezuela blockade of 1902-3. Quite possibly it never appeared in the secondary sources to which Professor Mitchell had access, and none of *Diplomatic History's* reviewers was sufficiently well informed to point out that omission when assessing the proposed article. Incidentally, my "Theodore Roosevelt: Principles and Practice of a Foreign Policy," *Theodore Roosevelt Association Journal* 18.4 (1992): 2-6, contains all the useful bibliographical data.

I would hate to think that I was not mentioned on account of my being just another of those scholars futilely sucked into "convoluted, circumstantial arguments about the president's credibility" in his "secret ultimatum" claim. I also refuse to fall a prey to the suspicion that American historians of the United States tend to regard the European historiography in their field as unworthy even of a footnote?

Every historian has an inalienable right to dismiss every other historian's findings and conclusions as nonsense if he so regards them, but we all have a duty as serious scholars to do our very best to cite all the relevant publications in a given domain, if only to show we have done our homework thoroughly.

It seems to me that Venezuela has become of late the crowded scene of healthy disagreements.... I suggest SHAFR organize in 1997 a session on the Anglo-German intervention of 1901-1903 and invite Paul Holbo, Fred Marks, Edmund Morris, and others, as well as Nancy Mitchell and myself, so as to permit a form of scholarly exchange less frustrating and more fruitful than epistolary expostulation.

Yours faithfully,

Serge Ricard (University of Provence)

(On August 15, 1996, Professor Mitchell responded as follows.)

I am familiar with Professor Serge Ricard's work. I cite it in the comprehensive bibliography of my dissertation. In a scholarly article, however, the historian must be discriminating. One cannot, and should not, cite everything. I did not think that Professor Ricard's work made a significant contribution to the literature on the second Venezuela crisis, nor has it engendered scholarly debate (as Professor Ricard himself admits in his swipe at *Diplomatic History's* readers). Therefore I chose not to cite it in my article.

As to Professor Ricard's "refusal to fall prey to the suspicion that American historians...regard European historiography...unworthy of even a footnote," I believe that the footnotes of my article, which cite German, English, French, Italian and Spanish sources, speak for themselves.

Yours sincerely,

Nancy Mitchell (North Carolina State)

* * * * *

4 April 1996

To the Editor:

I generally don't bother to get worked up over Frederick Marks III fantasies about American diplomatic history. Perhaps I am in a particularly touchy mood today having an enormous stack of mid-terms to grade, but I couldn't let the numerous errors of omission and commission in his latest piece in the *Newsletter* ("Power and Peace in American Diplomatic History") slip by silently. His simplistic attempt to rationalize massive military spending as the way to peace relies on so much distortion and outright ignorance that I don't know where to start. I'll give it a try, nonetheless.

Marks claims that there is “practically a one to one correspondence between periods of military preparedness and periods of peace and diplomatic achievement.” Presidents who were intellectuals or advised by such pointy heads were more likely to go to war than toughened veteran soldiers.

This correspondence is so much fantasy. It is only possible if “peace and diplomatic achievement” are defined in the most distorted ways. Let’s take three of Marks’ examples. He lists President Grant as a notable peacemaker. Perhaps Marks has never heard of the Indian Wars of the post-Civil War period between the US and the Sioux, Apache, Kiowa, Comanche, Modoc, Cheyenne and so on? Apparently, these don’t rate in Marks’ scorekeeping. How about Marks’ hero Teddy Roosevelt? (The article is so full of bravado and cheers for aggressive stances that, partway through, I got the feeling Marks was channeling TR). Roosevelt, and his predecessor Mckinley, indeed benefitted from a massive military build-up dating back to the 1880s. But what did they do with it? McKinley started a war for conquest of former Spanish Territories.

Roosevelt supported and as President continued the Filipino-American War. Finally, I wonder which planet Marks was on during the Reagan era. As I recall the US participated in a number of brutal wars and armed conflicts from Nicaragua to Libya and many places in between. Bush, a veteran and someone who could in no way be accused of being an intellectual, may want Marks to remember his war with Iraq as well.

In the end I want to thank Marks and the editors for publishing his bit of nonsense and getting my mind off my grading.

Sincerely,

Victor Silverman (Pomona College)

SHAFR COUNCIL MINUTES

June 23, 1996
Boulder, Colorado

The meeting was called to order by Mark Gilderhus at 7:45 a.m. Members attending were P. Hahn, A. Spetter, H.W. Brands, R. Immerman, W. Walker, W. Kimball, M. Pereboom, D. Kunz, M. Leffler, E. Rosenberg, J. Utley, B. Unterberger, C. Pach, D. Anderson, R. Burns, E. Schulman. Also present were G. Burns and J. Serena.

Allan Spetter reported for the absent David Fogelson on the Holt Fellowship Committee; the committee awarded its forthcoming fellowship to Philip E. Catton.

Diane Kunz reported for the Program Committee. Attendance at this meeting was strong, in excess of 300 and equal to or greater than recent conferences held in the Washington area. The western experiment appears to have been a signal success. Kunz raised the problem of late papers and no-shows. Discussion followed regarding possible sanctions. No action was taken; the problem seems to be endemic to academic conferences and is exacerbated by the high number of graduate student papers on the programs. Student presentations are often dependent on tenuous travel support. The Council resolved to congratulate Kunz, Bob Schulzinger and the 1996 Program Committee for their fine work in making the 1996 conference a success.

Spetter reported on plans for the 1997 conference, to be held at Georgetown University. Maarten Pereboom will chair the program committee; Nancy Tucker and David Painter will handle local logistics. The 1998 conference is slated for the University of Maryland.

Mel Leffler proposed that the SHAFR presidential address be delivered at the summer conference. This received general assent,

although some question remained as to when the new schedule would take effect. The question was left unresolved.

Jonathan Utlely delivered the report of the Endowment Committee. Some \$450,000 is in the endowment fund, in a mix of treasury notes and stock. This figure represents a substantial increase over previous years — a rising stock market lifts all boats. The situation looks good for the near term; the only question regards the future of *Diplomatic History*. If it maintains its current subvention from Ohio State, all is well; if it moves and/or requires underwriting from SHAFR, then the endowment fund will have to provide the money. It was suggested that Warren Kimball or someone else attempt to divine *DH* editor Michael Hogan's plans.

In his report as executive secretary/treasurer, Allan Spetter was pleased to note the continuing growth of SHAFR to more than 1900 members — representing an increase of more than 100/year during the last several years. 300 members live outside the U.S. in some 40 countries. W. Kimball suggested possible liaisons with foreign organizations; Emily Rosenberg suggested that these groups be invited to organize sessions for the annual meetings. This latter suggestion was formally adopted by the council.

David Anderson reported that the SHAFR roster was nearly ready to go to the printer. It could appear within a month or six weeks.

E. Rosenberg and others initiated discussion of SHAFR liaisons with AHA and OAH. A central question was whether SHAFR ought to organize sessions for the annual meetings of AHA and OAH, via some formal arrangement with same, or whether the diplomatic history panels ought to go through the ordinary channels of the program committees of those organizations. M. Gilderhus advocated a formalization of liaison with AHA and OAH; Rosenberg thought this should be handled at the presidential level. Nothing was formally resolved; the matter was left in the hands of the president and president-elect.

Discussion turned to the forthcoming revision of the *Guide*. Richard Burns and ABC-Clio's Jeffery Serena presented options available to SHAFR, especially regarding publication formats (paper, CD-Rom, Online). Much talk of relative merits and costs followed. Also of the formation of an editorial committee. E. Rosenberg suggested that Chester Pack, R. Burns, A. Spetter and ABC-Clio work up estimates on the various options; this was agreed upon.

E. Rosenberg, speaking for Warren Kimball forwarded a request from the Historical Advisory Committee for SHAFR's support in opposing the creation of a special-access category of researchers in classified materials. Though the HAC resolution itself was not available, the sense of the Council was to support the HAC in opposing such preferential access. The feeling was: Open to one, open to all.

Betty Unterberger moved that SHAFR's records be transferred to Texas A&M, which has expressed willingness to accept them. After brief discussion, the motion was approved.

Richard Immerman raised a question regarding various SHAFR prizes, especially the Bernath book and article prizes. Submissions have been slim; should one or both of these be made biennial or otherwise altered? E. Rosenberg suggested that Immerman examine stipulations of the Bernath bequests to see what alterations are legal; the Council will proceed from there.

M. Gilderhus asked whether the annual meeting might be held at a hotel in a major city rather than always on a college campus. Minimal response seemed to suggest satisfaction with the status quo.

Gilderhus returned to an earlier topic: of additional efforts to get diplomatic history panels on AHA and OAH programs. He suggested that the president, president-elect and program chair put together high-profile panels for same. Discussion followed. Mel Leffler urged SHAFR officers to encourage members to submit

proposals to the AHA and OAH program committees. C. Pach agreed to put notices of submission deadlines on H-DIPLO.

President Gilderhus adjourned the meeting at 9:20 a.m.

ANNOUNCEMENTS

The program chair reminds the readers that proposals for the 1997 SHAFR summer conference are due *November 22*. The Twenty-third annual meeting will be held at Georgetown University, Washington DC, June 19-22, 1997. Proposals are to be sent to Maarten L. Pereboom, History, Salisbury State U., 1101 Camden Ave., Salisbury MD 21801. FAX: (410) 546-6006. e-mail: mlpereboom@ssu.edu

Call for Papers

The 1997 annual meeting of the Society for Military History, "New Interpretations in Military History," will be co-hosted by the Air War College, Auburn University, and the University of Alabama in Montgomery, Alabama, 10-14 April 1997. The intent of the conference is to encompass all fields of military history, both topical and chronological, regardless of era, nationality, culture or location. Send proposals by 1 November 1996 to: 1997 Society for Military conference, P.O. Box 4354, Montgomery AL 36103-4354

OHA Call for Papers

The Oral History Association Annual Meeting will be held in Philadelphia, October 10-13, 1996, at the Holiday Inn Select Center City. The theme will be "Oral History, Memory, and the Sense of Place." The program committee especially encourages proposals that examine relationships between science, religion, personal values, and debates over public policy. Contact: Alphine W. Jefferson, Dept. of History, College of Wooster, Wooster OH 44691; or Steven J. Novak, UCLA Oral History Program, UCLA 157511, Los Angeles CA 90095.

International Studies Association

Four panels on Diplomatic Studies are being organized for the International Studies Association annual conference to be held in Toronto, March 22-26, 1997. Themes include: historical approaches to the study of diplomacy, diplomatic theory, contemporary diplomatic practice, and "what is diplomacy?" There may be space on any or all of these panels for papers and discussants. If interested, please contact Paul Sharp, Political Science, University of Minnesota - Duluth, Duluth MN 55812 (E-mail: psharp@d.umn.edu) with a proposal and fifty word abstract.

GRENA/IRMA Call for Papers

The Groupe de Recherche et d'Etudes Nord-Américaines and the Institut de Recherche du Monde Anglophone will sponsor a meeting March 21-23, 1997, on the topic "Travels and Travellers." Papers will be presented in French or English. The deadline for submission of proposals is October 1, 1996. Contact:

Serge Ricard

Tour no. 10 ("La Biscaye")

92, allée Granados

13009 Marseille, France e-mail: ricard@newsup.univ-mrs.fr

SHA Call for Papers

The 62nd Annual Meeting of the Southern Historical Association will be held November 5-8, 1997, in Atlanta. The Program Committee invites proposals for single papers and entire sessions. Please send two copies of all proposals, including a brief summary of the proposed paper(s) and a curriculum vitae of each presenter to: Lacy K. Ford, Jr., Department of History, University of South Carolina, Columbia SC 29208. Tel: (803) 777-7774; Fax: 803 777-4494. The deadline for proposals is October 1, 1996.

World Wide Web

Two World Wide Web indexes of interest to diplomatic historians are now up and running. Both sites have links that will take a browser to the listed page. These indexes will grow and expand as new Web ones are created. The first site is an index of the personal web pages of diplomatic historians. These sites often include vitae, phone numbers and addresses, and current projects. The URL address of this site is:

<http://www-scf.usc.edu/~sarantak/historians.html>

The other site is an index of resources of use to historians of U.S. foreign policy. Material indexed includes: web sites promoting individual books; the WWW pages of archives and presidential libraries, which contain finding aides; the official sites of government agencies' individual books; the WWW pages of archives and presidential libraries, which contain finding aides; the official sites of government agencies with some involvement in foreign policy; the pages of many foreign ministries; sites for think tanks, institutes, and centers that often announce conferences and upcoming speakers; home pages of academic organizations, such as the American Historical Association, and the Organization of American Historians; and the WWW pages of several academic journals. The URL address of this site is: <http://www-scf.usc.edu/~sarantak/stuff.html>

For more information contact: Nick Sarantakes, History, USC, Los Angeles CA 90089-0024. Tel: (213) 740-1657; e-mail: sarantak@aludra.usc.edu

Commission of History of International Relations

The Commissione di Storia delle Relazioni Internazionali which numbers some 300 historians from around the world is headquartered at the University of Milan. The president, Professor Brunello Vigizzi, sends information that the organization is sponsoring the following conferences over the next several years.

Moscow — October 15-16, 1996 — The Origins of the World Wars of the XX Century: Comparative Analysis

Rome — September 27-28, 1996 — The Historical Archives of the Great International Organizations: Conditions, Problems and Perspectives

Univ. of Cluj-Napoca (Romania) — September 25-27, 1997 — The Lessons of Yalta

Argentina — September, 1997 — Integration Processes and Regional Blocs in the History of International Economic, Politico-strategic, and Cultural Relations

Madrid — April, 1998 — Fin-de-siecle: anciens at nouveaux empires coloniaux

Newark, NJ — September 17-20, 1998 — A New Dialogue between Historians and Theorists of International Relations

Japan — 1998 — Political Interactions between Asia and Europe in the Twentieth Century

Paris — 1999 — Le poids de l'image des peuples dans les relations internationales

Milan — 1999 — The History of International Relations and its Periodization

The Commission has access to information about research conducted by historians of international relations in Europe and Asia.

For membership in the organization contact: the Commissione di Storia della Relazioni Internazionale, Via Festa del Perdono 7, I-20122 Milano, Italy.

PERSONALS

Lloyd Ambrosius (Nebraska) has been awarded a Fulbright in Germany for 1995-96.

Philip E. Catton (Ohio University) has been awarded the Holt Fellowship for research on his dissertation, "Uncertain Ground: The Strategic Hamlet Program in South Vietnam, 1961-1963."

David Foglesong (Rutgers) recently promoted to Associate Professor with Tenure has been awarded a citation for scholarly excellence and a \$2,000 expense account to continue his research.

Burton I. Kaufman (Virginia Tech) has been appointed Director of a new Center for Interdisciplinary Studies. Kaufman served previously for seven years as head of the History Department.

Dennis Kux (former foreign service officer) has received appointment as a Woodrow Wilson Center Fellow (1996-97) to research the History of relations between the U.S. and Pakistan.

Melvyn Leffler (Stettinius Professor of American History at Virginia), has been named the first Archives II Fellow. This semester Leffler is teaching a seminar at the University of Maryland which meets at Archives II allowing the students to use the NARA resources for their research projects. (The History Department at the University of Maryland is soliciting applications for the second Archives II fellowship. The Award includes a stipend of \$25,000 for a scholar with a distinguished publication record and strong teaching credentials whose proposed research project focuses extensively on the collections at Archives II.

T. Christopher Jespersen (Clark Atlanta) has received a Mellon Resident Research Fellowship from the American Philosophical Society Library.

Melvin Small (Wayne State) has been awarded a NATO Research Fellowship.

Reginald C. Stuart (Mount Saint Vincent, Halifax) has completed his stint as Dean of Arts & Sciences and will continue as Professor of History and Political Science.

The Truman Library has awarded research grants to Patricia Knol (Northern Illinois) and J. Samuel Walker (U.S. Nuclear Regulatory Commission).

The Eisenhower Library has awarded grants to Kenton Clyner (Texas-El Paso), Bruce Karhoff (Ohio State), Bruce Khula (Ohio State), Michael Ruhl (Ohio U), and Tracy Uebelhor (Indiana).

Bryan A. Young (Ohio State) has received a grant from the Eisenhower World Affairs Institute for work on the relationship between domestic politics, culture, and U.S. propaganda abroad in the late 1940s and 50s.

CALENDAR

1996	
November 1	Deadline, materials for December <i>Newsletter</i> .
November 1-15	Annual election for SHAFR officers.
November 1	Applications for Bernath dissertation fund awards are due.
November 15	Deadline for Myrna F. Bernath research fellowship proposals.
November 22	Deadline for SHAFR summer conference proposals.

1997

- January 1 Membership fees in all categories are due, payable at Blackwell Publishers, 238 Main St., Cambridge, MA 02142.
- January 2-5 The 111th annual meeting of the AHA will take place in New York.
- January 15 Deadline for the 1996 Bernath article award.
- February 1 Submissions due for Warren Kuehl Award.
- February 1 Deadline for the 1996 Bernath book award.
- February 1 Deadline, materials for March *Newsletter*.
- February 1 Deadline for Ferrell Book Prize.
- February 15 Deadline for the 1996 Bernath lecture prize.
- March 1 Deadline for Graebner Prize nominations.
- April 1 Applications for the W. Stull Holt dissertation fellowship are due.
- April 17-20 The 90th meeting of the OAH will take place at the San Francisco Hilton.
- May 1 Deadline, materials for the June *Newsletter*.
- June 19-22 SHAFR's 22nd annual conference will meet at Georgetown University.
Program chair - Maarten Pereboom, local arrangements chair - David Painter & Nancy B. Tucker.
- August 1 Deadline, materials for the September *Newsletter*.

Future OAH meetings will be in Indianapolis (Westin Hotel and Indiana Convention Center), April 2-5, 1998; Toronto (Sheraton Centre) April 22-25, 1999; St. Louis (Adam's Mark) March 30-April 2, 2000.

Future AHA meeting will be in Seattle, Jan. 8-11, 1998; Washington, D.C., Jan. 7-10, 1999; and Chicago, Jan. 6-9, 2000.

Future OAH meetings will be in Indianapolis (Westin Hotel and Indiana Convention Center), April 2-5, 1998; Toronto (Sheraton Center) April 22-25, 1999; St. Louis (Adam's Mark) March 30-April 2, 2000.

Future AHA meeting will be in Seattle, Jan. 8-11, 1998; Washington, D.C., Jan. 7-10, 1999; and Chicago, Jan. 6-9, 2000.

PUBLICATIONS

William S. Borgiasz (Falls Church, VA), *The Strategic Air Command: Evolution and Consolidation of Nuclear Forces, 1945-1955*. Praeger, 1996. ISBN 0-275-94861-7, \$49.95.

Lester H. Brune ed., (Bradley), *The Korean War: Handbook of the Literature and Research*. Greenwood, 1996. ISBN 0-313-28969-7, \$79.50.

Paolo E. Coletta (Annapolis - emeritus) trans., Raimondo Luraghi, *History of the Confederate Navy*. Naval Institute, 1996. ISBN 1-55750-527-6, \$39.95.

Saki Dockrill (King's College, London), *Eisenhower's New Look National Security Policy, 1953-1961*. St. Martin's, 1996. ISBN 0-312-15880-7, \$59.95.

John E. Findling (Indiana - Southeast) and Kimberly D. Pelle, eds., *Historical Dictionary of the Modern Olympic Movement*. Westport, 1996. ISBN 0-313-28477-6, \$79.50.

J. E. Findling and Frank W. Thackeray, eds., *Events that Changed America in the Twentieth Century*. Westport, 1996. ISBN 0-313-29080-6, \$39.95.

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AWARDS, PRIZES, AND FUNDS

Complete details regarding SHAFR awards, prizes, and funds are found in the June and December issues of the *Newsletter*, abbreviated information in the March and September issues.

THE STUART L. BERNATH MEMORIAL PRIZES

The Stuart L. Bernath Memorial Lectureship, the Memorial Book Competition, and the Memorial Lecture Prize were established in 1976, 1972, and 1976, respectively, through the generosity of Dr. Gerald J. and Myrna F. Bernath, in memory of their son, and are administered by special committees of SHAFR.

The Stuart L. Bernath Book Prize

This is a competition for a book dealing with any aspect of the history of American foreign relations. The purpose of the award is to recognize and encourage distinguished research and writing by scholars of American foreign relations. Five (5) copies of each book must be submitted with the nomination and should be sent to: Frank Costigliola, Department of History, U. of Rhode Island, Kingston, RI 02881. Books (five copies of each) may be sent at any time during 1996, but should not arrive later than February 1, 1997.

The Stuart L. Bernath Lecture Prize

The Bernath Lecture Prize seeks to recognize and encourage excellence in teaching and research in the field of foreign relations by younger scholars. Prize-winners deliver a lecture, comparable in style and scope to the SHAFR presidential address, at the SHAFR meeting during the annual OAH conference. Nomination is open to any person under forty-one years of age whose scholarly achievements represent excellence in teaching and research. Send nominating letter and *curriculum vita* no later than 15 February 1997 to: Arnold Offner, Department of History, Lafayette College, Easton PA 18042.

The Stuart L. Bernath Scholarly Article Prize

The purpose of the prize is to recognize and to encourage distinguished research and writing by young scholars in the field of diplomatic relations. Chairperson of the committee: Ralph Levering, Department of History, Davidson College, Davidson NC 28036.

The Stuart L. Bernath Dissertation Grant

This grant has been established to help doctoral students who are members of SHAFR defray some of the expenses encountered in the writing of their dissertations. Applications should be sent to: William Miscamble, Department of History, University of Notre Dame, Notre Dame IN 46556. The deadline for application is November 1, 1996.

The Myrna F. Bernath Book Prize

A prize award of \$2,500 to be offered every two years (apply in odd-numbered years) for the best book by a woman in the areas of United States foreign relations, transnational history, international history, peace studies, cultural interchange, and defense or strategic studies. The next prize will be awarded to a book published in 1996-1997. Contact: Carolyn Eisenberg, Department of History, Hofstra University, Hempstead NY 11550. Submission deadline is November 15, 1997.

The Myrna F. Bernath Research Fellowships

The society announces two Myrna F. Bernath Research Fellowships, \$2,500 each, (apply in even-numbered years), to research the study of foreign relations among women scholars. The grants are intended for women at U.S. universities as well as for women abroad who wish to do research in the United States. Preference will be given to graduate students and newly finished Ph.D's. The subject-matter *should be historically based* and concern American foreign relations or aspects of international history, as broadly conceived. Work on purely domestic topics will not be considered. Applications should include a letter of intent and three copies of a detailed research proposal of no more than 2000 words. Send applications to: Carolyn Eisenberg, Department of History, Hofstra University, Hempstead NY 11550. Deadline for applications is 15 November 1996.

THE W. STULL HOLT DISSERTATION FELLOWSHIP

This fellowship is intended to help defray costs of travel, preferably foreign travel, necessary to the pursuit of research on a significant dissertation project. Contact: Roger Dingman, History Dept., University of Southern California, Los Angeles CA 90089-0034.

Most recent winner: Philip E. Catton (Ohio U)

THE NORMAN AND LAURA GRAEBNER AWARD

The Graebner Award is to be awarded every other year at SHAFR's summer conference to a senior historian of United States foreign relations whose achievements have contributed most significantly to the fuller understanding of American diplomatic history. Contact: Chester Pach, History Department, Ohio University, Athens OH 45701. The deadline for nominations is March 1, 1997.

THE WARREN F. KUEHL AWARD

The Society will award the Warren F. Kuehl Prize to the author or authors of an outstanding book dealing with the history of internationalism and/or the history of peace movements. The subject may include biographies of prominent internationalists or peace leaders. Also eligible are works on American foreign relations that examine United States diplomacy from a world perspective and which are in accord with Kuehl's 1985 presidential address to SHAFR. That address voiced an "appeal for scholarly breadth, for a wider perspective on how foreign relations of the United States fits into the global picture." Contact: Thomas Knock, Department of History, Southern Methodist, Dallas, TX 77275 (Southern Methodist).

ARTHUR LINK PRIZE FOR DOCUMENTARY EDITING

The prize will recognize and encourage analytical scholarly editing of documents, in appropriate published form, relevant to the history of American foreign relations, policy, and diplomacy. By "analytical" is meant the inclusion (in headnotes, footnotes, essays, etc.) of both appropriate historical background needed to establish the context of the documents, and interpretive historical commentaries based on scholarly research. The competition is open to the editor/author(s) of any collection of documents published after 1984 that is devoted primarily to sources relating to the history of American foreign relations, policy, and/or diplomacy; and that incorporates sufficient historical analysis and interpretation of those documents to constitute a contribution to knowledge and scholarship. Contact: George Herring, Department of History, University of Kentucky, Lexington KY 40506.

THE LAWRENCE GELFAND - ARMIN RAPPAPORT FUND

The Society for Historians of American Foreign Relations established this fund in 1990 to honor Armin Rappaport, the founding editor of the Society's journal,

Diplomatic History. The fund will support the professional work of the journal's editorial office. Contact Allan Spetter, Department of History, Wright State University, Dayton, OH 45435.

ROBERT H. FERRELL BOOK PRIZE

This is competition for a book, published in 1995, which is a history of American Foreign Relations, broadly defined, and includes biographies of statesmen and diplomats. General surveys, autobiographies, or editions of essays and documents are not eligible. The prize is to be awarded as a senior book award; that is, any book beyond the first monograph by the author. Contact: James E. Miller, 132 13th St. SE., Washington DC 20003.