Pief's Contributions to Arms Control and Nuclear Disarmament

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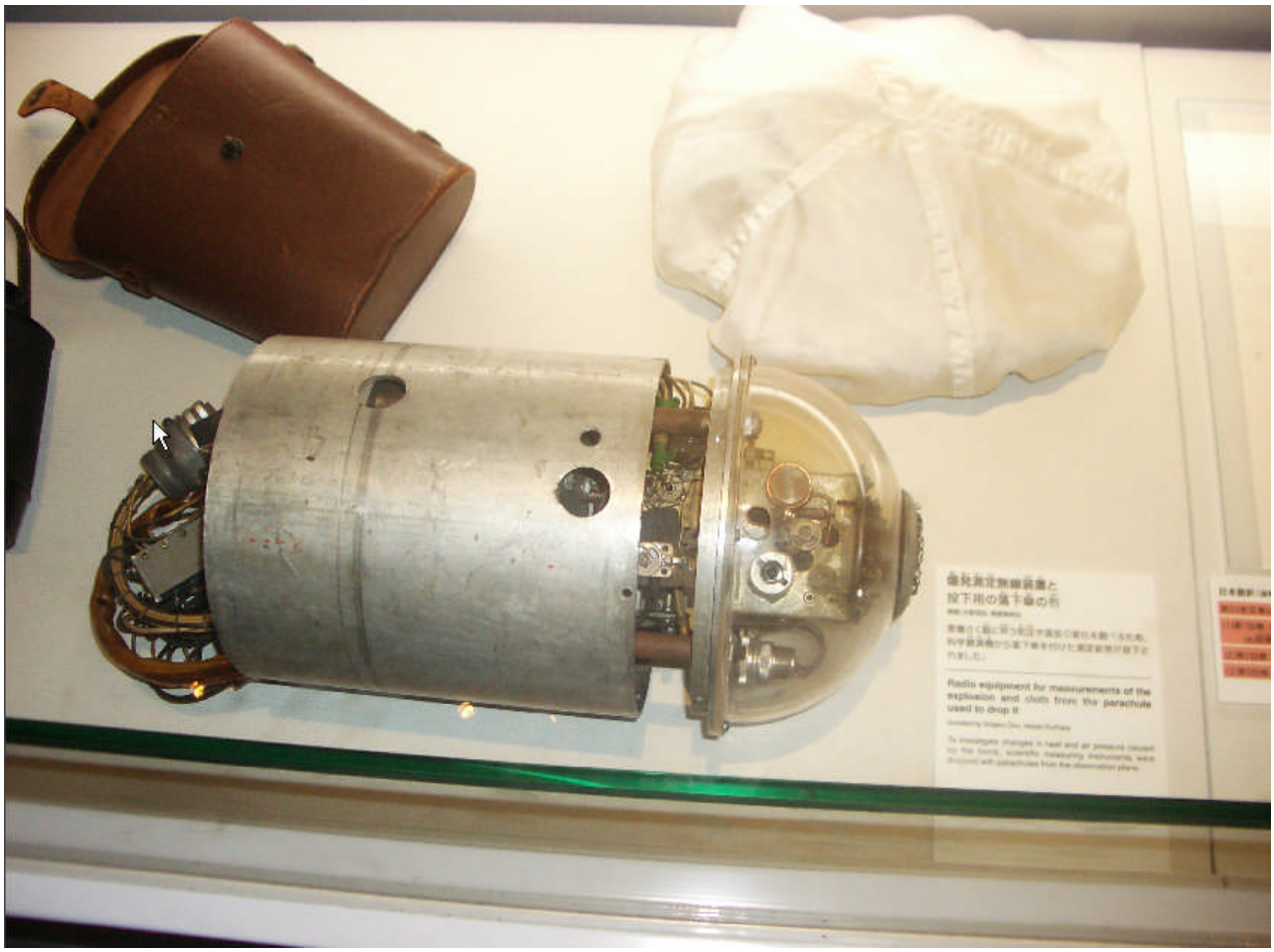
W.K.H. Panofsky (Pief) was a great man in his field of high-energy physics, in his creation and operation of accelerators and a laboratory that led to 3 Nobel prizes for work he promoted as Director, in his teaching and service to the physics profession, and in his contribution to national and international security. In this presentation I shall review Pief's work in national and international security, especially as I saw it when we were both members of the President's Science Advisory Committee (PSAC), its Strategic Military Panel, an advisory panel to Henry Kissinger-- National Security Assistant to President Nixon, and the Committee on International Security and Arms Control of the National Academy of Sciences (CISAC). This covers the period from the late 1950s through 2007.

We are fortunate to have Pief's own views of these efforts in his 2007 autobiography, "Pief Remembers: Panofsky on physics, politics, and peace." It may be useful to see these from a different perspective. Pief's experience in the technical aspects of security in the Nuclear Age began at Los Alamos during the war. There he worked on a parachute-borne device to determine the yield of the nuclear weapons under development by responding to the pressure pulse and transmitting the signal by radio to a receiver on a nearby aircraft. This drew on his prior work on a "firing error indicator"--FEI-- for anti-aircraft gunners in training-- work done with Alex E.S. Green under Jesse DuMond, whose daughter, Adele, Pief would later wed.



Radio equipment for measurements of the explosion and cloth from the parachute used to drop it. Hiroshima Peace Museum. (Photo courtesy of Ben Rusek, NAS CISAC)

Recruited to Berkeley by his Los Alamos colleague Luis Alvarez after the war, Pief worked with him on the proton linear accelerator and the MTA ("Materials Testing Accelerator). This was to compensate for a supposed uranium shortage by accelerating deuterons against a uranium target in order to breed plutonium for nuclear weapons. Pief's broader national security work probably began with the "Screwdriver Report," his response with Robert Hofstadter to the task of detecting "one cubic inch" of weapon material-- plutonium or highly enriched uranium-- HEU-- that might be smuggled into a United States port, concealed in a packing case. Despite the mild radioactivity of the uranium and the enormous rate of emission of alpha particles from 20 curies of plutonium, the "Screwdriver Report" nevertheless judged quantitatively that detection sensitivity and shielding was such that the only sure way of fulfilling the task was to disassemble the packing crate with a screwdriver. In a 1946 hearing of a committee of the U.S. Senate, Robert Oppenheimer responded to a Senator's question as to how one might detect a smuggled nuclear weapon, "... my most important tool would be a screwdriver to open the crates and look." In 1955 Pief was involved in a much broader task for the Air

Force Scientific Advisory Board-- to explore means for countering the delivery of nuclear weapons against the United States-- a task that was to occupy him the rest of his life.

In 1958 the first opportunity presented itself for actual negotiations with the Soviet Union to explore the possibility of verifying a ban on nuclear explosive tests, and a Conference of Experts convened in Geneva to explore the matter. On the basis of its report, negotiations began in the Fall of 1958 on a ban of all nuclear weapon tests. Spurgeon Keeny will discuss this further, but I just note here that the first Conference of Experts was regarded by pro-testing scientists and officials in the United States as too optimistic on detection of clandestine nuclear explosions, and a second Conference of Experts was convened in 1959 to explore this further. From 1961-64, Pief was a member of the President's Science Advisory Committee-- PSAC-- attending two-day plenary meetings in Washington each month and in a typical month several two-day sessions of a subcommittee or PSAC panel. He notes,

> "Because I had to teach freshman physics on Wednesday mornings, my wife would pick me up from my return flight to San Francisco on Tuesday evenings, drive to the Stanford lecture hall and work with me to prepare the demonstrations needed for the next day's classes. We then went home and early on Wednesday mornings I gave the lectures and accompanying demonstrations, usually to three classes in succession."

Pief is remembered for his effectiveness in teaching and for his dedication to that profession, as indicated by this example.

In June of 1961, Pief chaired a PSAC panel to evaluate technical factors on the need for nuclear testing and also to assess whether the Soviet Union had or had not conducted any secret nuclear tests during the moratorium on nuclear testing which was then in place. There was much public interest in this report, and properly so. It concluded that "It was feasible for the Soviet Union to have conducted secret tests, that there was no evidence that it had done so (or had not done so), and that there was no urgent technical need for immediate resumption by the United States." This conclusion was not universally shared, and was especially criticized by the Department of Defense. Thus, after the demise of PSAC in 1973 the technical question of the necessity for or the technical benefits of nuclear testing and the possibility that it might be done covertly continue to recur, and were addressed by Pief, particularly via the instrument of the National Academy of Sciences' Committee on International Security and Arms Control (CISAC) which played a key role in such later analyses.

At a memorial for Pief at SLAC on September 28, 2007, a musical interlude was accompanied by photo collage prepared by Brookes Collins, which I will now show to accompany my spoken text.

In the modern world, after the signing of the Comprehensive Test Ban Treaty (CTBT) in 1996, it was submitted to the U.S. Senate for ratification September 25, 1997, but it was only brought up for a vote on October 13, 1999, where it failed of ratification. President

Clinton set up a special advisor, former chairman of the Joint Chiefs of Staff, General John Shalikashvili, who in April 2000 requested the National Academy of Sciences to conduct a study on technical issues regarding the CTBT. Although that study was complete and approved by the authors December 2000, discussions over the classification of the Report, together with the required approval process within the Academy delayed publication of the Report until July 2002. This study, chaired by John Holdren, in which Pief played a leading role, compared three possible future worlds-- one without any restriction on nuclear testing; a world where a CTBT is obeyed by everyone; and a world where a CTBT is in existence but evaded to the extent possible without detection by the worldwide system established for monitoring the CTBT. This technical and military analysis concluded that U.S. national security is served better with a CTBT than without one, even if extreme evasion efforts continued.

Now I turn to Pief's work in the Strategic Military Panel (SMP) of the President's Science Advisory Committee and his activities in the group led by Paul Doty that advised Henry Kissinger as President Nixon's national Security Advisor and, to a lesser extent, when Kissinger was both Secretary of State and National Security Advisor Pief's work was characterized by a dedication to scientific correctness and thoroughness, compatible with producing a coherent report in time to do some good. In this he exemplified the best of technical advisors, not falling into the policy-only trap or into the technical-only trap, without proper concern for timeliness or understandability.

For years the SMP had the task of assessing the state-of-the-art and deployed capability of ballistic missile offense and defense, on both the U.S. and the Soviet sides. This was very helpful in bringing realism to the two sides. It was all too easy to postulate effective defenses on the U.S. side, as unfortunately the U.S. Army often did, in fulfillment of its obligation to provide plans and programs for such defenses and to implement them if judged desirable by the President of the United States. But because the SMP had the job of assessing Soviet missile defenses and of U.S. capability to penetrate them, it was familiar with the techniques that offensive nuclear weapons could use to confuse, deceive, or overwhelm the defense until it was too late for the defense to destroy them.

Each year, therefore, the SMP prepared a Top Secret assessment for the President. In the 1960s this meant advising on a series of attempts to develop a credible anti-ballistic missile defense system. First to be examined by SMP was the 1961 proposal of NIKE-ZEUS-- an evolution of the Army-developed and deployed air defense systems (NIKE-AJAX and NIKE-HERCULES). However, it suffered from the problems associated with mechanically steered radars of inadequate agility to cover a large threat and also inadequate traffic-handling capability, even if there were only pure warheads and no decoys.

The Army then proposed (in 1965) the NIKE-X system of ballistic missile defense, which would have multiple phased-array radars and many interceptors to protect the entire country. Its fatal flaw was the ability of an adversary to focus the attack on a small region of the country and thus to exhaust the local supply of interceptors.

The next proposed was the SENTINEL system in 1967; it used NIKE-X technology and aimed to provide a "light area defense" of the United States against a small number of Chinese intercontinental ballistic missiles. Furthermore, the planned Sentinel system was to have two layers. One a 5-megaton exo-atmospheric interceptors (the Spartan) and, second, a short-range interceptor that would engage the nuclear-armed reentry vehicles in the upper atmosphere-- the Sprint interceptor missiles. The SMP's final assessment of the Johnson Administration effort was that the Sentinel system would not work for reasons that were both technical and strategic, particularly the ability of the offense to deploy many light-weight decoys that could not be discriminated by the ground-based radar and would thus require more interceptors than could be deployed. By the time the discrimination would be effected by the slowdown of decoys in the atmosphere, it would be too late to launch the nuclear-armed interceptors to destroy the warheads.

But there was a deeper reason for the failure of such a system. It was intended to defend the entire U.S. population against nuclear-armed ICBM attack by the Chinese. It thus required more anti-missiles than could be afforded. Consequently, the offense could choose the regions that were not defended by Sprint missiles rather than the regions that were defended, still destroying as many millions of people as if there had been no defense at all.[1]

In July 1965, Pief wrote the members of the SMP a memo, "Changes in the AICBM Picture" that is, changes in the status of understanding of anti (intercontinental) ballistic missiles in the world and particularly in the United States. Pief argued that the technical situation had changed since 1961, with the recognition of a new longer-range kill mechanism from interceptors with exoatmospheric burst of their nuclear warheads, and with better discrimination of decoys from warheads within the atmosphere. Pief noted the vulnerability of the 1961 system (and potentially the 1965 system) to a "decoy-only" attack with multiple decoys launched by small missiles to exhaust the interceptors protecting a portion of the country-- incidentally a proposal I had made in 1953 while I was working on Project Lamp Light, on the air defense of the United States and Canada.

Here are some further examples of the work of the PSAC Strategic Military Panel. On September 25, 1967, Marvin Goldberger, at the time chairman of the SMP, wrote on

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[1] The SMP had the most experienced and best-qualified technical people to be found, without attention to their partisan political or philosophical bent, including over the years Hans Bethe, Lewis M. Branscomb, Sidney D. Drell, Dan Fink, myself, Marvin Goldberger, Richard Latter, Pief, Jack P. Ruina, Kenneth M. Watson, Albert D. Wheelon, and Jerome B. Wiesner. Some of the Panel members were experts in radar, some in nuclear weapons. Others specialized in intelligence as applied to the Soviet threat, and others in system analysis and computing. Still others were experts in the interaction of rockets and nuclear explosions with the atmosphere, important in determining the detectability of missiles, the disruption of radar capability by the effects of nuclear explosions on the atmosphere, and the like. In the earliest days of the SMP there was much attention to "wake effects" and discrimination.

behalf of the SMP that the Panel did not find Secretary of Defense Robert S. McNamara's arguments for the Sentinel system convincing and that such an ABM system should not be deployed "(except for domestic political considerations beyond our competence)".

In May 1967, Drell and Panofsky, as members of the SMP, had written Spurgeon Keeny, staff of the Office of Science and Technology with chief responsibility for strategic military matters, a memo "Bilateral Strategic Weapons Freeze", copied to Marvin Goldberger as chair of the SMP and to Donald F. Hornig as President Johnson's Science Advisor. Keeny was at the same time and had long been a senior staff member of the National Security Council, providing that body a unique competence in technical matters and an invaluable link to PSAC.

When President Nixon took office January 20, 1969, one of the first decisions of his administration was to refocus the light ABM deployment of the Johnson Administration, in view of the quite unexpected popular opposition to the proposal to defend only 12 or so localities in the United States. It had been expected that the public would clamor to have the defense extended to their locality, but instead the prospect of the certain deployment of nuclear-armed interceptors in their neighborhood brought strong opposition. Although the Nixon Administration was philosophically favorable to the deployment of an effective and heavy defense against Soviet nuclear-armed missiles, it was politically infeasible to move in that direction and so the argument for the full-scale development of the technology and its initial deployment shifted in favor of a limited defense of one wing of Minuteman ICBM silos-- that is, 150 of the total of 1000 silos-- a task for which the Sentinel System was ill-suited, despite the change of name to Safeguard. Note that the modified deployment did nothing to directly protect the population.

President Nixon had chosen as his National Security Advisor Prof. Henry Kissinger of Harvard University, who although capable, confident, and energetic, knew little about science or technology, but nevertheless rather than taking advantage of the powerful mechanism of the President's Science Advisory Committee and the President's Science Advisor chose to cut PSAC out of direct contact with the president. For instance, the President's Science Advisor at that time, Lee DuBridge forwarded to the president a report of the Strategic Military Panel signed by its chair, Sid Drell, and a copy of that report, now declassified and available in image form, bears Kissinger's marginal note, "We must get PSAC out of strategy." Fortunately, a good personal friend and Harvard colleague of Kissinger's was Prof. Paul Doty, a noted biochemist and long-time member of the President's Science Advisory Committee. Doty suggested, and Kissinger agreed, that Doty should lead a group of technical colleagues, most of them members of the PSAC, to provide informal advice on request to Kissinger as National Security Advisor. The group initially was constituted by Doty, Drell, myself, George B. Kistiakowsky, Pief, George W. Rathjens, and Ruina.

We would meet with Kissinger in the White House Situation Room (with maps of crisis areas on the wall, and behind a curtain the military staff involved in crisis communications) nominally at about 6 pm, but it was often 7 pm or later before the

National Security Advisor could make room for us in his schedule. We would discuss briefly and leave with him a highly classified paper that we had prepared for him over the previous month, and we would of course respond to any current questions he had. We would meet with him the next morning for breakfast at about 7:30 am to go over issues he wanted us to study for the next month's meeting and for further discussions of the paper of the previous evening.

The group had expertise not only in nuclear weapons and radar and military systems such as air defense and missile defense and strategic submarines and antisubmarine warfare, but also in space, intelligence, biology and biochemistry and biological warfare, and several other academic and security-oriented fields. Kistiakowsky, who had succeeded James R. Killian as President Eisenhower's Science Advisor, soon left the group because of philosophical differences with Kissinger over Vietnam, and Rathjens soon resigned because of his opposition to the bombing of Cambodia. The rest of us stuck it out, feeling that the influence that we could have was worth the likely rejection of our advice.

An important topic on which we spent considerable time in analysis and discussion was that of multiple independently targeted reentry vehicles for strategic ballistic missiles (MIRVs) and the question of whether they should be banned in a Strategic Arms Limitation Treaty. At that time the United States had tested but not deployed MIRVs, and the Soviet Union had not tested. U.S. unilateral intelligence resources ("National Technical Means") could verify with high confidence the absence of MIRV testing, and one option for a SALT agreement would include a ban on testing or deployment of MIRVs. Kissinger decided not to include this because he felt that he had enough difficulties overriding military preferences by severely limiting ballistic missile defense, and he did not want to jeopardize that achievement by proposing to limit MIRVs as well. At one point in 1974 Kissinger is quoted as saying, "If I'd realized what a MIRVed world meant, I would have been more serious about obtaining a MIRV ban."

A flavor of our activity, for instance, is in a paper of February 1971, on "Collateral Constraints on Surface-to-Air Missiles as Anti-Ballistic Missiles, and Implications of Hard-Site Defense." (Hard-site defense (HSD) is the use of specialized interceptors or even guns or small rockets to destroy reentry vehicles containing nuclear warheads before they can approach the silo or other hardened target to within lethal range-- a distance at which an explosion could destroy or disable a target.) Far from rejecting ABM, the Doty Group concluded that if there were to be a Strategic Arms Limitation Treaty (SALT) that limited ABM systems, United States' interests would be better served if the treaty banned hard-site defense, but that in the non-SALT context of a continuing arms race, HSD might become valuable as a way to preserve the nuclear deterrent and that R&D work for the design and advanced technology of HSD should continue to be supported at that time. But the Group viewed with concern the political risk of introducing a demand to permit hard-site defense at that stage in the negotiation.

Of current note, in view of the UK program to begin the construction of a replacement for its Trident submarines and the beginning of consideration in the United States of a similar program, is a report prepared April 1971 by the Doty group to evaluate the Undersea

Long-Range Missile System (ULMS), that was eventually funded and became the Trident submarine with the Trident-II missile. The group reported that "the smaller Polaris submarines have an indefinite life or can be assumed to operate for at least another 20 years" and that the more urgent the feeling that a greater capability was needed, the more one should modify the Polaris boats to handle an enhanced Poseidon missile that could be built for the ULMS role. The following paragraph has been added for the conference record: Of current note, in view of the UK program to begin the construction of a replacement for its Trident submarines and the beginning of consideration in the United States of a similar program, is a report prepared April 1971 by Doty, Drell, Garwin, Ruina, and Panofsky, "An Evaluation of the Undersea Long-Range Missile System (ULMS)." This was a technical paper for Kissinger that reviewed the proposal for ULMS (that was eventually funded and became the Trident submarine with the Trident-II missile). It did not conclude that ULMS was necessary or even desirable and although it provided a full but concise evaluation of ULMS and of a converted Poseidon to carry the ULMS missile, as competitors, it argued that if there were urgency in providing the ULMS system, "ULMS should not be thought of as a replacement for worn out Polaris boats. Polaris boats have an indefinite life or can be assumed to operate for at least another 20 years." "If an urgent need for ULMS developed, the 8-yr lag before first delivery could be shortened by redesigning the Poseidon conversion to accommodate the missile designed for ULMS ... In this way some boats with ULMS missile capability could be on station in less than half the time required to produce ULMS boats. All the advantages (of ULMS) apply equally to redesigned Poseidon."

Political arguments over the program were later to overwhelm the technical arguments that would have brought increased and more flexible capability sooner via the converted Poseidon submarines than via the Trident route. The work of the SMP as well as of PSAC was terminated by President Nixon in 1973 and for nearly a decade the White House had no continuing independent scientific advice. However, in the Carter Administration this was partially reversed. An interesting example is the technical analysis of a possible nuclear test, possibly from a ship or barge in the "South Atlantic."

THE "SOUTH ATLANTIC EVENT"

On September 22, 1979, one of the VELA satellites in 100,000-km radius circular Earth orbit detected with its two whole-Earth light sensors a double-peaked flash. Although differing in detail, the signal resembled that from some of the atmospheric tests that the 12 VELA satellites had detected over the years. Agencies of the U.S. government responsible for analyzing and interpreting such detections attempted in the immediate aftermath to determine whether this was indeed an atmospheric nuclear explosion or an artifact of the system. It was reported to the President as a likely nuclear test.

A few days after the event, President Carter's Science Advisor, Frank Press, at the suggestion of Spurgeon Keeny, convened a panel chaired by Jack Ruina of MIT that included myself, Pief, and Luis Alvarez, among others, to review the information that had by then accumulated. Keeny had reviewed the data with Secretary of Defense Harold Brown before it was released to the intelligence community. We began our work and,

with these people experienced in treating large amounts of data from particle physics, requested much information from the VELA system that had NOT originated with nuclear explosions. We found many such "zoo events." In this activity, Pief showed his insistence on reviewing all available information, keeping an open mind toward all hypotheses, and searching for ways of analyzing the data best suited to determining whether the records were indeed the double-humped light output of a nuclear explosion. The report of the Ruina Panel is publicly available on my website at http://tinyurl.com/yww4vf. The key to the determination was provided by Panel member F. Williams Sarles, who plotted the data in the "phase plane" of one light sensor vs. the other, differing nominally only in sensitivity. However, the two sensors, having the same view of the Earth, did behave differently, thus indicating that they could not have been viewing the same event on or near the Earth; in this regard the signal differed from any of the detections of a true nuclear test.

The panel concluded, "Although we cannot rule out the possibility that the signal was of nuclear origin, the Panel considers it more likely that the signal was one of the zoo events, possibly a consequence of the impact of a small meteoroid on the satellite." Apparently, the light source in question was close to the satellite and thus viewed differently by the two light-detectors. One of the many micro meteors that strike satellites could have liberated a small piece of reflective insulation that traversed the field of view of the VELA satellite, the double-hump in time occurring because the flake of insulation was spinning as it passed the field.

Pief followed the VELA detection puzzle through the years, and received occasional updates that in fact provided no new information to confirm that VELA had detected a nuclear explosion. Nevertheless, the so-called "detection" continues to be accepted in some U.S. government circles as an attempt to hide a test of a low-yield nuclear weapon.

THE STANFORD ARMS CONTROL PROGRAM.

In the last 1960s, Stanford University was a center and victim of student turmoil, especially protests against the Vietnam War. As a concerned senior faculty member, Pief was much involved in general and in particular with one case of a professor who had been brought up for discipline by the university.

Beyond suffering in silence and working in general to calm the unrest, Pief and his SLAC colleague Sid Drell -- also Professor of Physics and former member of the President's Science Advisory Committee-- wanted to show students that there were other more important and more general topics of concern, to the solution of which they might contribute if they turned their interest and their talent in that direction. Pief and Drell organized and co-taught a famous arms control course beginning in the very early 1970s, and Pief was very active in creating the Stanford arms control program, which eventually became in the late 1980s the Stanford Center for International Security and Arms Control.

Two such charismatic teachers attracted many students to this course, among them James B. Timbie, who has been an important senior official in arms control and national security in the Department of State.

JASON WORK

The JASON group was formed in 1960, initially attached to the Institute for Defense Analyses, a federally funded research contractor. There was a perception by senior scientists advising the U.S. government that the expertise accumulated during the Second World War was a waning resource, and that scientists involved in real technical work for the government would be needed to replace the aging coterie of those who had worked on technical problems during World War II. This was dubbed the JASON group, which met in the summertime for six weeks or so, and typically had a couple of field trips and a three-day meeting in Washington, DC Spring and Fall. Pief contributed to many Jason reports. Most had to do with his areas of expertise-- charged particle accelerators as weapons or for the production of tritium, or monitoring of proliferation. We note a 2003 JASON report which was a return to his roots-- the Screwdriver report-- as an analysis of prospects for detecting a concealed nuclear weapon. Technology had evolved since the study more than 50 years earlier, so that by the use of conventional high-energy x-rays from a portable electron accelerator, together with modern particle detectors, it is indeed technically feasible to scan the 7,000,000 cargo containers entering the United States each year to detect an intact smuggled nuclear weapon, or to identify a small fraction of containers that would need further scanning or even unpacking to ensure the absence of a weapon. But it would be necessary to build the extensive system to perform the scanning, preferably in the ports of shipment rather than on arrival, and to communicate, store, and interpret the information from the scan and other data relating to the container.

Pief also contributed to several other reports dealing with a nuclear test ban or of programs of stockpile stewardship-- maintaining nuclear weapons safe and reliable without nuclear explosion tests. The Jason mode is such that it is difficult to discern the contribution of individual authors, but Pief's participation was always much esteemed.

THE NATIONAL ACADEMY CISAC AND THE SOVIET UNION

The year 1980 was a difficult and perilous time during the Cold War. The Soviet Union had invaded Afghanistan in December 1979, and President Jimmy Carter cancelled U.S. participation in the 1980 Moscow Olympics. In 1979 the United States possessed some 25,000 nuclear warheads and the Soviet Union several thousand more. These were loaded on bombers and missiles and aimed mostly at targets in the opposing nation. Ronald Reagan was about to be elected President of the United States and Leonid Brezhnev was the longtime General Secretary of the Soviet Union, to be succeeded on his death in 1982 by Yuri Andropov and then by Konstantin Chernenko. Mikhail Sergeyevich Gorbachev assumed that position in March 1985.

Formal negotiations initially planned in the administration of President Lyndon B. Johnson, resulted in Richard M. Nixon's administration in the 1972 ABM Treaty and the

Limited Offensive Agreement, but the building and deployment of nuclear weapons continued. Non-official contacts between scientists in Soviet Union and the United States continued discussions of nuclear hazards and of means of controlling the nuclear threat. Perhaps the most important of these was SADS (Soviet-American Disarmament Studies) led by Paul Doty beginning in 1964 and ending in 1975. These contacts were for the most part under the sponsorship of the American Academy of Arts and Sciences (Boston), supported by the Ford Foundation. In 1980 the US National Academy of Sciences began to explore a more formal but still unofficial relationship with the Soviet Academy of Sciences, and such meetings began in 1981 at a pace of two per year in the Soviet Union or in Washington.

The US CISAC thus created was chaired initially by Marvin L. Goldberger, then president of CalTech. The Soviet counterpart was chaired for two years by Nicolai Inozemtsev, Director of the Institute of World Economy and International Relations, and after his death for a long time by Evgenij P. Velikhov. It had as members, among others, Roald Z. Sagdeev, Georgi A. (Yuri) Arbatov, and Evgenii Primakov. Initially, Igor Tamm was a member and after his release from internal exile Andreii D. Sakharov. Our early sessions discussed some details of nuclear forces, of crisis and arms-race stability, and the relationship between defenses and offensive forces. Pief was an active participant in all of these and later succeeded Goldberger as chairman.

In 1982 and 1983 there was much talk in the Western press about directed energy space weapons-- DEWS: lasers and particle beams as a new means of defense against nuclear-armed missiles. These were seriously discussed between our groups. In early 1983 there was such a bilateral discussion at a meeting in Washington, with very detailed analysis of the effectiveness and vulnerability of space-based DEW that led to the judgment that they would not be militarily effective. One week later, on March 23, 1983, President Reagan gave his television speech announcing the Strategic Defense Initiative-- SDI-- calling for the scientists "who gave us the nuclear weapons to give us the means to render them impotent and obsolete" by intercepting them before they could reach their targets. It was clear that the defense was to be non-nuclear and largely dependent on DEW, including a nuclear-explosion-pumped x-ray laser weapon! The SDI announcement provided further focus to our bilateral discussions.

When Mikhail Gorbachev assumed the Soviet leadership in March 1985, he felt the need to hear from capable, honest people outside the power structure of the military, the administration, or the Party and worked closely for a year or more with Arbatov, Primakov, Sagdeev, and Velikhov, so we were sure that our bilateral analyses were brought to the attention of the Soviet leadership by individuals capable of interpreting them. Probably to the disappointment of many in science and engineering in the Soviet Union, Gorbachev did not follow the United States into a major SDI defensive program but decided that he could defeat SDI with means that were asymmetric, and at some 1% of the cost to mount an SDI system. That was probably the most exciting aspect of the CISAC bilateral with Soviet counterparts, and Pief played a big role.

Pief with CISAC and Soviet counterpart at STRATCOM HQ
(E.P. Velikhov, et al. July 1991)

The major reports of CISAC bear Pief's stamp of thoroughness, clarity, and integrity. For the Conference record, I have added a brief excerpt or summary of each report:

o Monitoring Nuclear Weapons and Nuclear-Explosive Materials: An Assessment of Methods and Capabilities (2005)

"1. Present and foreseeable technological capabilities exist to support verification at declared sites, based on transparency and monitoring, for declared stocks of all categories of nuclear weapons-strategic and nonstrategic, deployed and nondeployed-as well as for the nuclear-explosive components and materials that are their essential ingredients. Many of these capabilities could be applied under existing bilateral and

international arrangements without the need for additional agreements beyond those currently in force."

o Technical Issues Related to the Comprehensive Nuclear Test Ban Treaty (2002)

"The worst-case scenario under a no-CTBT regime poses far bigger threats to U.S. security-sophisticated nuclear weapons in the hands of many more adversaries- than the worst-case scenario of clandestine testing in a CTBT regime, within the constraints posed by the monitoring system."

o The Future of U.S. Nuclear Weapons Policy (1997)

"In any case, the regime of progressive constraints constituting the committee's proposed near- to midterm program makes good sense in its own right -as a prescription for reducing nuclear dangers without adverse impact on other U.S. security interests- regardless of one's view of the desirability and feasibility of ultimately moving to prohibition."

o Management and Disposition of Excess Weapons Plutonium: Reactor-Related Options (1995)

Separated weapon-usable material-highly enriched uranium or plutonium of any composition aside from almost pure Pu-238-- should be provided security comparable to that provided nuclear weapons in storage-the "stored nuclear weapons standard." The initial goal of disposition of excess weapons plutonium should be to degrade it to a condition in which its security needs are comparable with those of spent reactor fuel itself-the "spent-fuel standard."

o Management and Disposition of Excess Weapons Plutonium (1994)

o The Future of the U.S.-Soviet Nuclear Relationship (1991)

"Instead, we seek an appropriate balance between the positive and adverse effects of nuclear weapons in the face of many uncertainties. We recommend, in furtherance of a new nuclear policy, that:

"(1) In the agreements that follow the Strategic Arms Reduction Treaty (START), the United States and the Soviet Union should reduce the number of nuclear warheads in their strategic forces to 3,000-4,000 actual warheads, a reduction of as much as a factor of 3 below anticipated START levels. The remaining strategic forces of both sides should be made more survivable, both by eliminating the most vulnerable forces (in particular MIRVed ICBMs) and by reducing the vulnerability of retained forces. "

These studies, available to read and download at www.nap.edu constitute a tangible and enduring part of Pief's legacy.

PIEF AND THE AMALDI CONFERENCES

Pief's involvement with the Amaldi Conferences was intense and better known to some here than to me. But it would be good to record his role in creating the Amaldi Conferences. By 1986, it was clear that the principal purpose of CISAC was being achieved-- that the interaction with our Soviet counterpart group had paid off in better informed scientists on both sides who, especially on the Soviet side after the accession of Mikhail Gorbachev, were having a substantial impact on Soviet policy. CISAC then asked whether it would be possible to influence other national academies, particularly in Europe, to play such a role with their governments. To explore this further, CISAC prepared and hosted a "European meeting" in Washington June 28-30, 1986. Ten CISAC members and eleven European scientists took part in this meeting, including Klaus Gottstein, Francesco Calogero, and Carlo Schaerf. On their return to Italy, Calogero and Schaerf reported to Edoardo Amaldi, then Vice President of the Accademia Nazionale dei Lincei. Amaldi was very much in favor of this initiative and set up a Working Group on International Security and Arms Control (SICA). The first informal meeting was held at Rome at the Lincei June 1988, and then an international conference, "International Security and Disarmament: The Role of the Scientific Academies" was held in Rome in June 1989.

There was good interaction at the CISAC "European meeting," and I believe that following the meeting the Royal Society did step up its activities with the government of the United Kingdom, as did the French Academy of Sciences with that of France. As indicated, Edoardo Amaldi was particularly inspired by the proposal and hastened to create not simply an interaction between the Accademia dei Lincei with the Italian government, but on a grander scale hoped to have a continuing involvement among the academies for contributions to their national security. When Amaldi died unexpectedly in December 1989 Prof. Giorgio Salvini was elected to succeed him as President of the Academy and of the SICA group as well, and the international meetings were named henceforth, "Amaldi Conferences."

PIEF AND CHINA

Pief first went to China in 1976 with his wife, Adele, for a two-week visit to which, characteristically, he gave his all-- touristically, diplomatically and to collaboration in High Energy Physics. The result was his intense involvement in advancing China's participation in High Energy Physics. He was proud of his unique status. Chinese leaders proposed to Pief that he become an advisor to the Chinese Academy of Sciences, but Pief did not think it appropriate to be a formal advisor to a foreign country, and instead a section in the annual agreement between the US Department of Energy and the Chinese Academy of Sciences provided for his services as an unpaid consultant in the field of High Energy Physics. SLAC thus played a leading role with Chinese scientists resident at SLAC in the design and construction of the Beijing Electron-Positron Collider.

Pief's excellent relations with scientists in China and with high government officials there, encouraged him to suggest to the Chinese that they create a mechanism to work

with the NAS CISAC to better understand the threats nuclear weapons posed to their security and how to bring them under control. Since Pief then chaired CISAC, it was natural that he should propose such an interaction with the Chinese. Rather than the Chinese Academy of Sciences, which does not have a role in nuclear weapons, the Chinese Academy of Engineering Physics was the counterpart of the US NAS, and so a productive interaction began with the Chinese Scientists Group for Arms Control, under the leadership of Hu Side, head of the Institute of Applied Physics and Computational Mathematics--IAPCM, the design branch of the Chinese nuclear weapons program. The CSGAC-CISAC bilateral continues to this day and will have its 20th anniversary this year.

The first bilateral talks between Chinese scientists and the CISAC delegation led by Pief. 20 years ago.

Pief was welcomed by the leaders of COSTIND in China.

Pief with CISAC and CSGAC in Beijing, ~ 2003

Evidently it is valued by both sides. It permitted frank discussions of the prospective Comprehensive Test Ban Treaty and the drawing of a balance between the security benefits and the costs to the nuclear weapons program and the potential hazards of a collapse of the CTBT regime.

CODA

While this brief review covers only part of Pief's role, it makes clear that he was one of the most important founders of the tradition of American science advising in national security matters. He had a unique combination of breadth of interest, focus, energy, and talent that led to his becoming one of the great scientific advisors in the first half-century of the nuclear age. He made full use also of his energy and intellect in trying to make the world's decision makers better informed in the national self interest and in the interest of the world's inhabitants.

In this approach he was my personal hero, for his dedication, his good spirit, his ability, his insistence on integrity, and his readiness to take pencil in hand to commit ideas to paper as informative and persuasive prose. I am honored to have had the opportunity to present at this Amaldi Conference the first Panofsky Lecture, which cannot possibly do justice to such a great man.