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Sidney Drell and National Security

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It has been my great pleasure to have been involved with Sid Drell in several of the activities through which he has performed such outstanding service toward U.S. national security and the security of the world.

I believe that Sid's first involvement in this regard began in 1960 when he studied for the newly created JASON group of consultants to the U.S. Government the question of the influence of a high-altitude nuclear explosion on the ability of an infrared satellite to see the heat emitted by a strategic ballistic missile in boost phase. In 1961 Sid became a member of the Strategic Military Panel of the 18-person President's Science Advisory Committee (PSAC), on which he joined such legendary physicists as Hans Bethe and Pief Panofsky. Although a Science Advisory Committee had existed previously in the Office of Defense Mobilization (part of the Executive Office of the President), it was only when Dwight D. Eisenhower entered the White House in 1953 that there was truly presidential involvement. Up to that time, the PSAC chairman-- I.I. Rabi of Columbia University-- was a part-time presence in Washington, as were the members, and Rabi decided that he wanted to remain a part-time advisor. So James R. Killian became the first full-time President's Science Advisor and chairman of PSAC. PSAC members were for the most part academic physicists. George B. Kistiakowsky who was to be Eisenhower's Science Advisor for the last year and a half of his term, was a chemist who had led the high-explosive activities at Los Alamos from April 1943 until August 1945, and whose work was crucially important to the implosion weapon. The contribution of scientists to the war effort-- radar for air defense and the naval battle, nuclear weapons, antisubmarine warfare, and the like-- gave scientists great status. The roster of PSAC members included several of the greatest physicists. But the problems and opportunities facing the United States--even those with scientific content-- could not be addressed responsibly by PSAC membership alone. Besides, there was too much to do. And while there was no difficulty in finding outstanding scientists who had been tried and contributed vitally in the war effort, 10 and 15 years later the problems and the technologies had changed, and it was necessary to reach out to those who had not gone through the fire together.

A typical PSAC panel had two or three PSAC members and was almost always chaired by a PSAC member, together with the best experts in the country for the problem at hand. In addition, PSAC members and others who had been associated with the Committee were asked to propose young people who might contribute to the panel study at hand and who might be PSAC material.

My own involvement with national security matters had begun ten years before, when in 1950 I began the first of many summers at Los Alamos, working on nuclear weapons. In my case, the bridge from New Mexico to Washington was a year's half-time work at MIT on Project LAMP LIGHT 1953-1954-- a study led by Jerome B. Wiesner and Jerrold Zacharias concerned with extending the air defense of the U.S. and Canada (against Soviet nuclear-armed bombers) to the sea lines of approach to the United States.

This led to intense involvement in Washington, with a two-year study, part-time, led by William O. Baker, chemist, PSAC member, and head of Bell Telephone Laboratories. The Baker Panel was largely concerned with intelligence regarding the Soviet Union-- especially cryptanalysis. And the Baker Panel led to the PSAC Strategic Military Panel chaired at the time, I believe, by Pief Panofsky. Hans Bethe was also a member of the Strategic Military Panel, as were Richard Latter and Dan Fink-- experts in nuclear weapons and radar. We had also outstanding practitioners of the art of ballistic missiles, and our problem was nuclear offense and also defense against the Soviet nuclear forces that might be used against the United States.

The topic of air defense against Soviet bombers was largely handled by the PSAC Military Aircraft Panel which I was to chair for many years, so the Strategic Military Panel concentrated very largely on our strategic deterrent force-- its capability, survivability under attack, and "penetrativity" (the ability to penetrate potential Soviet defenses). The Strategic Military Panel was much concerned with the status and the future of Soviet air defense and its defense against ballistic missiles, but the Soviets revealed nothing of their capabilities; we had to rely on Intelligence, supplemented by our own intelligence-- analyses, judgment, and projection. The offense-defense problem is still with us, although the information on the opposing system is far more available today, as a result of improved U.S. tools for gathering the data on which Intelligence is based. In the evolution of these tools, Sid Drell played an important role, as we shall see.

The problems facing the Strategic Military Panel, on behalf of the President of the United States, were difficult:

- Technical problems in the face of uncertainty.
- Limited resources.
- The ever-present possibility (even certainty) that information was being withheld from us.
- The prospects for non-military solutions, e.g., arms control.
- The likelihood that our analysis would not be read and our advice ignored.

Information was withheld by some in order to protect "sources and methods"-- for instance, we were provided with data from the dozens of overflights of the Soviet Union by the ultra-secret U-2 aircraft that began operation over the Soviet Union July 4, 1956, even though most of us did not know of the existence of the U-2. Information was withheld by others in order to avoid decisions on U.S. defense programs that would alter the normal course of development and expenditure. And there is always the problem of focus-- the tendency of any group such as the Strategic Military Panel to regard its particular threats and opportunities as the most important in the nation and to the world.

Sid served on the Strategic Military Panel until 1973 and was its Chairman 1968 to 1970.

This exposure was later to serve Sid Drell in good stead as he tackled the problems of a sensible basing for the ten-warhead MX missile in the late 1970s and early 1980s. PSAC made major contributions to U.S. security, as did Sid during his service on the panels and the Committee, and in his later activities. PSAC largely set its own agenda,

keeping the President informed, and, of course, being not only willing but delighted to receive a problem or a study topic from the President.

PSAC members served a four-year term and occasionally a non-contiguous second term. But membership on a PSAC panel could be a commitment of very long standing. There were ad hoc panels such as one on insecticides and pesticides, chaired by John W. Tukey, mathematician of Princeton University, and standing panels such as the Strategic Military Panel, Military Aircraft Panel, Antisubmarine Warfare Panel (later to become the Naval Warfare Panel), and the like.

The key to the effectiveness of PSAC and its panel activity and to the (usually) high quality of the panel studies and reports, lay in the discipline enforced by having PSAC members chair and be involved in the panel activity. The study topic was worked out with PSAC, and there was typically a mid-term briefing to PSAC, as well as one or two briefings at the time the study was almost complete. Typically, at the two-day monthly PSAC meetings, there would be a rundown of status and problems of the panel activities.

PSAC met typically for two days a month, for the most part in a vast room in the Old Executive Office Building just west of the White House. Room 206-208 held an enormous table at which 30 people could be comfortably accommodated, with an additional single row of chairs lining the room. Each of the panels typically met for two days every month, too, so that a person who served on PSAC and on two panels was in Washington six days a month-- quite a burden for the West Coast participants.

In these activities, both process and substance are important. On the process side we soon learned that the military and the contractors who would brief us interminably in Room 206-208 would move at an agonizingly slow pace; we eased that problem by having them deliver enough "hard copies" of the briefing foils or slides the day before so that each panel member would have his (or very occasionally her) own. With the briefings now more of a discussion, it turned out that some of the briefers knew little more than the words of their presentation. It was at that point that General Glenn Kent (an Air Force officer skilled in strategic analysis involved with ballistic missile defense and offense) revealed "You don't understand, the purpose of a military briefing is not to convey information but to fill time."

A PSAC Military Panel was staffed by an Executive Secretary who was a full-time employee of the Office of Science and Technology (OST), and we had typically also a military liaison person, who was on active duty at the Pentagon. My liaison for the Military Aircraft Panel was Navy Captain Noel Gayler, later to head the National Security Agency and then to be Vice Director of the Joint Strategic Target Planning staff at Omaha. For the Naval Warfare Panel I had Navy Captain Elmo Zumwalt, who was then selected to head the "brown water navy" in Vietnam, and from there was selected over many more senior officers to be Chief of Naval Operations. The panel activities were an education not only for the panelists but also for the military, civilian Pentagon personnel, and the contractors who were involved. The educational aspect was enhanced by our practice of inviting those who were to present later in the day to attend the presentation of the earlier groups, and those who had presented to stay on. Of course, occasionally the security classifications were such that we made an exception to this practice.

In several cases the friendship and mutual respect that grew up between a panel chair and the military liaison officer were later to serve the nation in good stead as these officers rose to the highest ranks. Sid Drell still works with Admiral Elmo Zumwalt (now long retired from the job of Chief of Naval Operations) on the President's Foreign Intelligence Advisory Board.

Relatively early in his PSAC involvement, Sid was asked to look at the the MIDAS program-- a satellite with the goal of detecting the launch of intercontinental ballistic missiles (ICBMs). As Ted Postol has explained, a large missile once it gets above the atmosphere radiates on the order of a megawatt of infrared power, and if one chooses a detector and filter combination such that the water in the atmosphere prevents heat sources on the Earth from radiating to space (and if one looks in the mid-range infrared rather than the near infrared so that there is not much solar radiation to be reflected from clouds) in principle missile launches can be detected day or night. Field trips to contractors were always informative and sometimes a revelation. MIDAS was judged not to be technically ready; in fact, it would have been built without adequate knowledge of the background noise against which the launch of a ballistic missile anywhere on Earth must be detected-- namely sunlight reflecting from cloud tops. The MIDAS work eventually resulted in the Defense Support Program (DSP) satellites that have since the late 1960s watched every portion of the world for missile launches and have provided much important information and assurance.

SAMOS, like MIDAS was an Air Force satellite program, but the goal of SAMOS was to radio pictures back to Earth for intelligence purposes. In 1956 the U-2 aircraft had begun what were planned to be secretive flights over the Soviet Union. This sail plane with a jet engine had been created in eight months by a small team led by Kelly Johnson at the Lockheed Skunk Works, to carry a camera for strategic reconnaissance from an altitude above 70,000 feet. In fact, it was tracked by Soviet air defense radar from its first flight; and the Soviets knew about it, but very few in the United States did until on May 1, 1960 Gary Powers' U-2 was shot down by a Soviet SA-2 missile and Powers (alive) and the equipment (damaged) put on display in Moscow. The Paris Summit between Eisenhower and Khrushchev was cancelled by Khrushchev and Eisenhower was heartsick. With first-hand experience of the horrors of World War II and with a good understanding of the annihilation that would be brought by nuclear war (an understanding provided to the President by PSAC and the 1954 Technological Capabilities Panel on which many PSAC members served) Eisenhower would later call the failure to achieve a ban on nuclear testing, "the greatest disappointment of any administration -- of any decade -- of any time and any party...." Over the decades, and especially in 1995 and 1996 (a third of a century later) Sid Drell would play a leading role in finally bringing a CTBT into being.

In the early 1950s Project 3 of the Technological Capabilities Panel-- including John Tukey, Edwin Land (inventor of polarizing film and instant photography), Ed Purcell of Harvard University, and James G. Baker (a genius at optical design) played a definitive role in defining and selecting the concept that was later to be the U-2. Management was given to Richard Bissell, economist and operations manager at the Central Intelligence Agency, who with Col. Osmund J. Ritland as Air Force Project Manager administered the program at Lockheed. The U-2 development had more in common with the design and operation of a physics experiment than with a military contract.

Although neither Sid nor I knew it at the time, in 1958 Richard Bissell was again put in charge of another reconnaissance system-- this time for strategic reconnaissance via film photography from satellites in low Earth orbit, and again with Brigadier General Ozzie Ritland as partner. The system (code named CORONA) was first launched in February 1959. The first 12 launches were failures, and the 13th, on August 12, 1960, was the first to return a reentry vehicle from orbit. The CORONA Program returned the first man-made object from orbit, the first pictures (CORONA 14 on August 18, 1960) from orbit, and by the time the program was superseded in 1972 had made more than 100 successful flights and returned 800,000 pictures-- each 70 mm wide by almost a meter long. Each strip covered a region about 16 km along the track of the vehicle and 200 km cross track, providing stereo photography of a strip 200 km wide as the vehicle in its near-polar orbit traversed the Soviet Union or other region of interest. Ultimately CORONA was to provide a resolution of two meters on the ground. Its existence and its performance were secret until the program was declassified in February 1995 and revealed in detail at a symposium in May 1995. Bissell recalled that he had chosen the code name, CORONA, by glancing around the office in 1958 and noticing his Corona-brand typewriter.

Here are some sketches of the CORONA system, as well as reproduction of some images like those of interest to the Strategic Military Panel. **P. 695, 696, cover, 151, 221**

When our colleague Albert D. (Bud) Wheelon became the first Deputy Director for Science and Technology at the CIA in 1962, he was in charge of the CORONA Program as well as several other interesting major projects such as the totally "black" SR-71 strategic reconnaissance aircraft program-- the Mach-3 successor to the U-2. But CORONA needed help; pictures were being returned with the important photographs obscured by beautiful but concealing patterns that looked like flames or brushes or flowers. Bud Wheelon called on Sid Drell to investigate and solve this problem. To do so Sid brought in Mal Ruderman and Luis Alvarez, who worked with the contractors (Kodak for the film, Itek for the camera, Lockheed for the spacecraft, and General Electric for the reentry vehicle) to establish that the fogging was (ironically) caused by corona-- an electrostatic discharge that was taking place in the vacuum of space as the film was being unwound from the supply spool, moved over the guide rollers and wound into the take up spools in the reentry vehicle.

CORONA had had another problem in July of 1962, when a U.S. nuclear explosion in space (1.4 megatons at an altitude of 400 km) filled the Van Allen Belt with trapped electrons from the fission products of the nuclear explosion. These electrons trapped in the Earth's magnetic field penetrated the light construction of the CORONA vehicle and fogged the film-- a problem that we readily helped solve by adding a little shielding.

Following his work on CORONA-Corona and his initiation into the ultra-secret world of satellite reconnaissance, in January 1964 Sid became a member of the Land Panel, headed by Edwin H. Land, which advised the President's Science Advisor on strategic reconnaissance. Its members included Ed Purcell, Jim Baker, myself, and Al Donovan of the Aerospace Corporation. The Science Advisor, the Director of the CIA, and the Deputy Secretary of Defense constituted an Executive Committee for the Strategic Reconnaissance Program, which by then was administered by the National Reconnaissance Office (NRO)-- an organization so closed that its existence was a secret until about 1992. The Land Panel influenced significantly the evolution of film photography from space in which images of enormous scope and of exquisite resolution

came back to Earth in tremendous rolls of film within reentry vehicles (buckets) that following the CORONA approach were slowed by parachute after reentry and snatched in mid-air by special recovery aircraft equipped with a trapeze. SAMOS, with its proposed on-board development and radio link, could not have provided a tiny fraction of the intelligence obtained from these film-return systems.

The principal agents of the NRO were the CIA and the Air Force, which vied for the role of developer of individual new systems. During the 1960s, the competition was fierce and sometimes vicious. I recall a two-day meeting of the Land Panel during which we visited a couple of the contractors for a candidate system for which Air Force and CIA were in competition. At the last moment, the presentation at this particular contractor for the camera had to be given by CIA personnel-- Leslie C. Dirks and Jackson Maxey-- because the contractor had been leaned on by the Air Force not to present the concept that it had developed for CIA.

But the film-return reconnaissance system did not provide instant photography-- or even images every orbit-- because one needed to wait until enough film had been accumulated in a reentry vehicle to make it worthwhile to send down. And the vehicles carried a finite number of film-return buckets. So the Land Panel again considered electronic transmission of images-- to capture the optical image via some kind of transducer, and then to convert that image into an electrical signal that could be sent by radio to the ground. Human ingenuity provided half a dozen candidate approaches, some of them using an intermediate image receptor that might or might not be recycled but that could then be scanned by an electron beam (or a laser). The Land Panel's staff, the late Dr. Donald H. Steininger, commented to me that he was astounded at the speed with which the Land Panel could review and grasp the various approaches and home in on an electro-optical system that could be made to work. The vision of Edwin Land was essential in this regard, as were the contributions and collegiality of the Panel's members.

The Land Panel did not take its responsibility lightly. It had several meetings on the electro-optical system and sought confirmation of its views in simulation and aircraft trials. Its final recommendation was made in 1968 that the country should go with the electro-optical system.

President Richard M. Nixon took office in January 1969, with Henry A. Kissinger as his National Security Advisor. PSAC member Paul Doty-- a Harvard biochemist-- knew Kissinger well and managed to persuade him that an informal technical advisory committee to the President's National Security Advisor would be useful. This was duly constituted, and consisted of Doty, Drell, Garwin, Pief, Jack Ruina-- electrical engineer from MIT who had been in the Defense Department, and George Rathjens-- chemical physicist who had been in the Advanced Research Projects Agency. PSAC had just completed a study on the targeting of the U.S. nuclear forces and presented this to Kissinger. The Doty group was to work also on arms control policy-- especially the Strategic Arms Limitation Treaty (SALT I), which consisted of the ABM Treaty of 1972 and the Interim Agreement on Strategic Offensive Arms. We probably had some influence on the ABM Treaty, but we failed to persuade Kissinger that SALT I should ban MIRVs-- the multiple, independently targetable reentry vehicles. The significance of MIRVs was two-fold-- first, they enabled the existing large payload capability (tons of payload thrown by a ballistic missile to intercontinental range) to be divided among many warheads-- perhaps 3 to 20-- somewhat increasing the area destroyed to any level of overpressure, five psi (1/3 of an atmosphere) being enough to destroy normal

buildings. The silos that protected our ICBMs were eventually upgraded to withstand 1000 psi overpressure. The volume of air that can be brought to 1000 psi overpressure is about 1/1000 as large as that which can be brought to 5 psi by the point deposition of energy that constitutes a nuclear explosion. The accuracy required it destroy a 1000-psi point target is thus about ten times better (the radius of the corresponding sphere being smaller by the cube root of 1000).

More importantly, if a nation chooses to MIRV its own land-based ICBM forces, it inevitably creates for itself the fear of destruction before launch-- a fraction of an equal number of enemy warheads might destroy these missiles and eliminate the deterrence provided by the threat of eventual nuclear response.

MIRVs thus bring the spectre of an unstable equilibrium of land-based forces, which leads to a demand for warning of attack and a launch of missiles before they are destroyed-- which leads to the hazard of accidental total nuclear war in which a billion people are likely to die. In reality, the unstable MIRV-MIRV confrontation of land-based forces is stabilized by the existence of survivable sea-based forces; but the position adopted by the Office of the Secretary of Defense, in an after-the-fact justification of the excessive size of the strategic offensive force, was that each of the components-- bombers, land-based ICBMs, submarine launched ballistic missiles-- was supposed to provide deterrence by assured destruction on its own. Although the United States has already tested MIRVs, the Soviets could verify our lack of deployment, because of our relatively open society, while the U.S. could verify through its national technical means (intelligence) that the Soviets had not tested MIRVs. Sid had, of course, an active role in these studies as did Pief. But Sid and I were the only members of Kissinger's informal advisory committee who were on the Land Panel and who had clearances for the nuts and bolts of the acquisition of such intelligence.

In the Government we encountered bureaucratic competition to the electro-optical system that the Land Panel had proposed for the evolution of the U.S. reconnaissance capability. The Air Force proposal was to have an "interim" system while the technology for electro-optical imaging matured. The champion of the interim approach was Eugene G. Fubini, who had been Principal Deputy Director of Defense Research and Engineering in the Pentagon. Sid and I learned that the President's National Security Advisor did not have a good understanding of the performance of the proposed electro-optical system or of the confidence one could have in the program to achieve it. Fubini had organized an ad hoc technical group of the Defense Science Board, which we discovered had not been given the technical basis for judging the relative risks and performance of the two system in question. It was arranged that I attend a session of the Fubini group as acting chairman of the Land Panel for this purpose, and on the spot Fubini and I worked out a Memorandum of Understanding, giving the joint views of the Land Panel and of the Fubini Panel.

So Sid Drell and I carefully drafted a TOP SECRET code-word hand-written letter that was personally delivered to Henry Kissinger (together with this Memorandum of Understanding) at what seems to have been the last possible moment to obtain a favorable decision for the electro-optical system that was indeed built and that has served, with improvements, for many years.

Sid Drell's involvement in national security matters has been so broad and so deep that even the parts of it with which I have been associated could fill this whole symposium.

He has been not only a technical contributor, but a leader in the JASON group as well as in Washington circles. For instance, in the late 1970s and early 1980s, the desire of the Air Force to have a missile to replace the Minuteman led to a configuration of a highly efficient, highly accurate three-stage so-called MX missile (i.e., "Missile Experimental") with ten independently targeted warheads. It was a missile as large as allowable under the 1972 Interim Offensive Agreement. The Air Force had really expected to substitute the MX for the Minuteman, but was stymied by a certain logic that had been popularized by supporters of the MX. In order to make this transition compelling, they raised the spectre of "Minuteman vulnerability" to a first strike by accurate Soviet missiles. Paul Nitze was among the foremost proponents of this vulnerability, which was later to result in the inability to deploy any significant number of MX missiles. The problem, of course, was that the deployment of the MX in the Minuteman silos would simply replace Minuteman vulnerability by MX vulnerability-- and this was something that even the greatest skill at obfuscation could not conceal from everyone at all times. A great hunt began and Air Force was proud of saying that they had reviewed and evaluated more than 30 alternate basing schemes-- all of which were given more consideration than they deserved. Sid and I were lucky to form a kind of odd alliance with Representative John F. Seiberling and testified many times over the inadequacy of the basing schemes that had been proposed for the MX missile.

Finally, Sid led a contingent of JASONs to study for the Department of Defense-- specifically from Bill Perry, who was at that time (in the Administration of President Carter 1977-1981) Director of Defense Research and Engineering-- what seemed to us to be about the only technologically satisfactory way to deploy the MX missile to achieve the goals of survivability. We eventually ended with a proposal to base the MX missile on capsules attached to small diesel-powered submarines. A submarine of about 1700 tons (compared with the 8000-ton Poseidon submarine or the 18,000-ton Trident submarine) would in this way carry two to four MX missiles horizontally in capsules alongside the submarine. We published an unclassified version of this in Technology Review of May 1981 as "Basing the MX Missile: A Better Idea". Sid had had the foresight to anticipate the argument that would be used against this system-- that of vulnerability to anti-submarine warfare. ASW, itself, has traditionally been a field in which there was always something beyond the edge of the curtain that was revealed to the ordinary practitioner, so Sid obtained a memorandum from Bill Perry that stated specifically that Drell and Garwin had been granted access to the ASW field which was adequate to form their own sound opinion as to vulnerability of a submarine-based system to Soviet ASW. Technically, our Small Undersea Mobile system was a winner, but the Air Force was not in the least interested in basing its new missile on tiny submarines. And the Navy saw a threat to the future of its large strategic submarine systems. Nevertheless, our proposal was received favorably in some circles.

Insert New Yorker cartoon here.

Sid did yeoman service not only in analyzing MX missile basing and in testifying in Congress and in providing more advice than the White House was willing to accept, but in also taking this to the people-- the governors of Utah and Nevada (the states where land-based MX would have been deployed) and to university campuses in that area. The Air Force had a policy that they would not appear on the same platform, but would come either before or after Sid or I would appear, in order to counter whatever it was that we had to say. The over-sophisticated logic-torturing analyses that they provided in this campaign were a wonder to behold, and were sometimes quite amusing to counter. The depth and breadth and duration of Sid's involvements of national security

and arms control is illustrated by the MX missile basing question. In addition to his activity as an individual, he was key to staffing an Office of Technology Assessment study on MX missile basing which reported in 1981. Three principal participants in the OTA study were Ashton B. Carter, whom Sid had met at Oxford, Ted Postol of the Argonne Laboratory, and Robin Staffin, who had been a student of Sid's at Stanford and a Post-Doc with me at Harvard-- all of whom have been much involved since then in national security matters.

I told you that Sid was eventually to play a key role in permitting the Comprehensive Test Ban Treaty to move forward, and that was in large part as a result of his leadership of various JASON studies over the last five years on the utility of nuclear testing in general and so-called hydronuclear tests in particular-- those in which a nuclear weapon would be so modified that a full-scale implosion would result not in a yield of 10 or 20 or 50 kilotons of high explosive equivalent, but in a yield not exceeding two kilograms of high explosive-- a factor ten million times less. A JASON Report of July 1996, deftly led by Sid, was immediately briefed by him to the chairman of the Joint Chiefs of Staff, the National Security Advisor, and played a key role in the decision of President Clinton to move forward with a CTBT that did not permit any nuclear yield at all from any test. Technical analysis, persistence, and persuasion has finally led to an arms control agreement that will help to reduce the danger from nuclear weapons, if we value improved security over the momentum of bureaucratic programs. As for the question, "National Security, is there still hope?" there is-- so long as people of integrity and intelligence like Sid Drell involve themselves in the battle.

For all of these contributions, for his role as leader and exemplar-- both technical and human-- and for the pleasure it has given us to work with him over the decades, the national security community of the United States owes Sid Drell a great debt of gratitude and I am delighted to have had the opportunity to sketch a bit of what he has done.

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