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11 August 1966

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The Honorable Paul H. Nitze
Secretary of the Navy
Washington, D. C. 20350

Dear Mr. Secretary:

Enclosed is a final copy of my interview with you which incorporates all of your changes.

I appreciate very much your generosity in devoting time and effort to this interview.

Sincerely yours,

Alfred Goldberg

Alfred Goldberg

AG:mt

Enclosure: One carbon copy, 7 pages, final interview notes, June 15, 1966.

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INTERVIEW WITH SECRETARY OF THE NAVY NITZE (U)

by Alfred Goldberg

June 15, 1966

I became interested in strategic concepts when I was with the U.S. Strategic Bombing Survey (USSBS) in 1944. At that time it was not clear what questions we were supposed to answer. The first task then was to find the right questions and an appropriate methodology. General Fairchild had a forward looking concept of air strategy and of what the essential questions might be. Our discussions about the work of the survey also included talks with Brig. Gen. Fred Castle and with (Maj. Gen.) Orval Anderson in England. The particular point that emerged from discussions and from our study was the essentiality of establishing control by attacking other targets. The experience of the war demonstrated the essential requirement to destroy the German fighter force before the objectives of the air offensive against Germany could effectively be realized. This experience and the concepts derived therefrom go to the roots of the counterforce concept.

We tried to express some of these strategic ideas in the USSBS Report on the Pacific War that we prepared in 1946. (Maj. Gen.) J. B. Montgomery and Admirals Sherman and Radford helped work on that report. Even at that time it was clear that a military strategy should include the requirement to maintain control over the medium in which the military forces were operating--whether it be land, sea, or air. This idea comes through clearly in the Report.

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The competing concept was that one would not be able to maintain control of the air and that it was better to go immediately into the exploitation phase of operations.

Between 1946 and 1949 the dominant air strategy was to go immediately into the exploitation phase. This was because budget considerations did not permit the creation and maintenance of a force large enough to carry out a counterforce strategy. This was what the 70-group controversy was about. The Air Force needed more planes and weapons for a strategy that went beyond attacking some Russian cities and hoping for the best. The limited number of nuclear weapons was a major factor in shaping strategy.

The explosion of the Russian nuclear device in the fall of 1949 required a reexamination of the political and military foundations of U.S. strategy. Dean Acheson and I talked about it at the time. The issue was whether a U.S. strategic nuclear offensive against Russian cities in response to a Russian invasion of Europe would cause the Russian divisions to stop rolling into Europe. Our view was that it wouldn't. The Air Force differed. General Cabell told me, probably sometime in 1949 or 1950, that his view was that the destruction of the Soviet Union by our nuclear bombers would be such as to stop the Russian Armies. The issue at that time was not counterforce but the adequacy of our strategic bombing forces for our purposes. There was also the important question of whether we didn't need tactical nuclear weapons as well as strategic ones. As I recall, the inventory requirement for nuclear weapons projected by the Joint Chiefs in 1950 for



the 1960 time period was surprisingly low. My own calculations based on covering air, ground, and sea targets suggested a requirement many times the number projected by the JCS.

In 1949-1950 when we were working on NSC 68 there arose the question of what kind of defense capabilities we had against Soviet delivery capabilities. At that time the Russians didn't have much in the way of nuclear delivery systems, but one had to anticipate changes in the future that would create a serious threat to the United States. The problem of defense against a Soviet air attack would be a major one, but we couldn't get answers to key questions from the Pentagon. We made up our own figure of 15 per cent on the number of attacking Russian planes that could be shot down which was accepted by the Pentagon.

There were other important factors involved in an analysis of air defense, but the big question was what would be the effect on SAC of an enemy attack. There was a little doubt that an appreciable percentage of enemy bombers could penetrate our defenses successfully. This examination of air defense led, of course, to assessment of SAC's vulnerability to attack.

In 1954 I acted as a consultant to the Air Force Project Control. My view was that the concept as developed by Colonel Sleeper (the direct support of an active foreign policy by SAC) was not saleable, that the Pentagon, State and NSC would not buy it. A variation on it might have been saleable. We gave Sleeper a rewrite on it which we thought he might be able to sell, but I don't know what happened to it. My recollection is that it was a controlled

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phased counterforce strategy. It might be considered an early version of controlled general war.

In 1955 I was stimulated to write an article after listening to a debate between General Norstad and Admiral Buzzard at a Bilderburg meeting in Munich. The article, which appeared in Foreign Affairs, pointed to the importance of the West maintaining a position of nuclear attack-defense superiority versus the Soviet Union and its satellites.

At that time the general pattern of opinion was that ABM defense was not technically feasible. I speculated on the problem and suggested an approach to the problem to the Johns Hopkins Applied Physics Laboratory. They examined the problem and concluded that it might be possible to actually intercept a missile with another missile. Their computer studies showed that it was at least possible in principle. In 1956, therefore, I said that an ABM defense was not unthinkable.

About this time at a SAC briefing in Omaha, General LeMay disagreed with the concept of a controlled counterforce strategy. His view was that the tendency of my thinking was to undermine general acceptance of large, ground-burst weapons. My point was that if you stick solely to big, dirty weapons with high CEPs, it was unlikely the country would long rely on SAC for support of our foreign policy.

When I was working with the Gaither Committee in 1957, we were more concerned than in the past about the danger of Soviet counterforce attacks on SAC bases. We could see the future Soviet missile threat and the possibility of a lag

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in our own missile program. Wohlstetter and other RAND people had done a study on SAC vulnerability to attack. We had to look at the question of what could be done to avoid the worst effects on SAC of a sneak attack.

That brings us to 1960. In the speech I gave at Asilomar I was invited to give something way out in the wild blue yonder. This was worked up with Abe Lincoln being fully conscious of the controversial nature of the ideas.

My view of the missile gap was that the intelligence was uncertain. There was a substantial risk that the Russians would have a sufficient number of missiles to decimate our bomber force prior to the time we'd have an adequate missile force. It looked as if they'd be ahead of us to the extent of being possibly able to carry out a sufficiently effective nuclear attack to assure a win situation in military terms. Even though the probability of the Soviets being able to decimate SAC bombers might have been only 30 per cent, the risks involved were such as to require doing everything possible to reduce the danger. This, in essence, was the substance of the argument between Symington and Dulles.

I was aware of the Parrish-Kaufmann study on counterforce in 1960. My chief concern was over the target date-- 1963--when the counterforce capability was to be in existence. I wasn't sure that it could be done by that time, but I didn't quarrel with what they were trying to do on the premise that it could be important.

I was asked by President Kennedy in the fall of 1960, during the election campaign, to address myself to issues of national policy and defense policy for the new Administration. One of the problems was the degree to which it was possible to

achieve a true counterforce capability as opposed to the achievement merely of a secure second strike capability. It seemed that it was necessary to have a secure second strike capability but that this by itself was not necessarily enough. To win a war in politically meaningful terms it would be necessary to have a counterforce capability. Whether it was technically feasible to get such a capability was not clear. Therefore it was necessary to strive for some balance in forces that would provide more than a second strike capability even if a true counterforce capability could not be attained. We were hopeful that the development of Russian policy and the progress of our own technology would clarify the issues in time. This position was bought by President Kennedy, by Rusk, and by McNamara by the time they came into office. This approach was not in any formal NSC paper but no alternative ideas on the subject seemed practical, or if practical, adequate.

The further evolution of strategic ideas is pretty clearly spelled out in Bill Kaufmann's book. The policy advocated in my paper for the President also called for more limited war forces. There was no conflict over that. There was, of course, question about the feasibility of an effective and meaningful counterforce strategy. The conflict was over allocation of resources, and here there was a judgment factor.

When we came into office in 1961 we found that many people in DDR&E had already been working along the same lines we had been developing. Marvin Stern, for instance, had been hot and bothered for some time about the inadequacies of command and control. He didn't see Skybolt as an effective weapon. The DDR&E fellows were ahead of outside thinking in some areas of strategic thinking.

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The next important development was when McNamara went into the missile gap and came to the conclusion that our information was good enough to permit us to put an end to the idea. This was one of the first things McNamara went into. The evidence in 1961 was supported by much better reconnaissance.

I wasn't involved in the SIOF changes. McNamara and Gilpatric brought it about. The NESC and other studies were being done and used in this connection.

The point about the Athens speech was that in McNamara's view much of the problem in the NATO Council was that the other countries didn't really understand a great deal about strategic nuclear war and the considerations involved. Most of them were still living back in the massive retaliation deterrence period. McNamara thought that if he could get the defense ministers to appreciate something about the nuclear and military considerations involved in strategic planning, they'd come to recognize that small national nuclear forces or even a European nuclear force might be dangerous and largely non-contributory. Having given the talk at Athens, he subsequently thought that something should be said publicly to clarify his views and this he did at Ann Arbor. Looking at it from the standpoint of hindsight, he may later have concluded that he had gone too far and that the controversial issues he had raised had hurt rather than helped. He has never repeated these ideas in public statements.

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