After the Pentagon Papers: Talk with Kistiakowsky, Wiesner

The interview transcribed below grew out of the belief of the *Science* news department that the Pentagon Papers published by The *New York Times* in July deserved further study and discussion by the scientific community. Accordingly, *Science* asked Jerome Wiesner, now president of Massachusetts Institute of Technology, and George Kistiakowsky, professor emeritus of chemistry at Harvard University, to participate in an interview that would take the Pentagon Papers at least as a starting point. Wiesner and Kistiakowsky were science advisers to Presidents Kennedy and Eisenhower, respectively, and were among the leading spokesmen for the scientific community during the period covered by the Pentagon Papers. The interviewer was Elinor Langer, a former member of the *Science* news staff. In the introduction that follows, Miss Langer describes the ground rules under which the interview was conducted.

The interview took place in mid-August at Woods Hole, Massachusetts, in the offices of the National Academy of Sciences. Readers should know the conditions on which it was agreed that the interview would appear in print. Wiesner and Kistiakowsky retained the right to edit their remarks, a right both have exercised. The few places where the printed version truncates the actual discussion, or where amplification is required, are indicated in italics. A few topics have left no traces. It does not seem fair, in the case of an interview, for the interviewer to have the last word. Readers can judge for themselves to what extent the interview measured up to its purpose in candor and relevance.—E.L.

Langer. [Let me start by saying that] the assumption behind this interview is that the Pentagon Papers may have more serious implications than the level of public discussion so far has really indicated. It was conceived as a kind of fishing expedition to see what people like yourselves, who were in positions of some power—at least we were led to believe at the time—[and of] relative knowledgeability when some of the events were going on, made of the Papers. I have the impression that you're both very anxious to talk about Scientists and Engineers for Johnson, and your support for Johnson in 1964 [which is, of course, a very important part of it], but I hope our discussion can go beyond that.

Wiesner. I told you on the phone that I didn't think that anything has come out in the Pentagon Papers—though I haven't read all 47 volumes or even studied everything that's been printed—that would change my view of what Johnson did, or my attitude towards my participation in Scientists and Engineers for Johnson. I became badly disenchanted long before these papers came out, so they didn't really affect me in this respect. I don't know whether George has different views.

Kistiakowsky. I have really little to say about Scientists and Engineers for Johnson-Humphrey. The amount of my active, personal involvement was relatively small.

Wiesner. Well, it was never really for Johnson, it was against Goldwater. That was the central thing. I think both of us were reacting against what Goldwater stood for and, on balance, I think I am still not sure. I wouldn't work for Johnson knowing what I know, but I would not care for Goldwater either.

Kistiakowsky. I might add to that now. I vividly remember one specific issue when Dr. [Donald] McArthur, [former deputy director for research and technology, Office of the Director of Defense Research and Engineering (DDR&E)], who was the moving spirit behind the organization, sent me, as one of the members of the central committee, a draft of election-year pamphlets to be distributed by the million. When I read it, I telephoned him and said that I was flatly opposed to it because the whole theme of it was to extol Lyndon Johnson. I said that Scientists and Engineers were a nonpartisan group who were against what Goldwater stood for, and that a pamphlet saying so is the only kind of pamphlet that I would authorize over my signature. But I would not sign the pamphlet that was being proposed. Do you remember that?

Wiesner. I remember that, and I concurred, because my own personal experiences with Lyndon Johnson in the Kennedy administration did not leave me fond of him. But nonetheless, I must admit that I believed he was interested in de-escalating the war in Vietnam.

Langer. You were not familiar, at that time, with the plans and projections that were being made about the bombing campaign in North Vietnam?

Wiesner. I certainly knew nothing about it.

Kistiakowsky. Well, you were already out of office.

Wiesner. I had been out of Washington since March '64 and the war blew up after that. In addition, I haven't studied newly released papers sufficiently to be able to talk about what was just contingency planning that is necessary and what was, in effect, government policy. I think that one has to be very careful in making such interpretations.

Kistiakowsky. Yes, I must say for myself that by that time I was already, I think, sophisticated enough to sense the internal workings of the government: the fact that not everybody speaks the same language, not everybody has the same objectives. The Pentagon, for instance, is very different from the Arms Control and Disarmament Agency, and so on. But I was naive enough to think that when the President of the United States was running for election and made quite a flat, public statement that he was for peace as contrasted with his opponent—I was naive enough to believe him.

Wiesner. Well, I believed him for other reasons, too, in spite of the fact that, as I said earlier, I was not on good terms with the President. I had seen him push for disarmament initiatives inside the White House: press on the White House staff, including myself. I was not involved in Vietnam discussions because they were not a prominent part of the government business during the first months that he was President and I was still working there. There were other issues that were much more important. But in the contacts we had with him and with his senior staff during the campaign, he certainly gave us the impression of very different views about Vietnam than those that emerged later. And I suspect that nei-

ther of us would have worked for him to the degree that we did, or at all, if we had been able to anticipate the way the war was going to go.

Langer. Does it strike you in retrospect as surprising that in your position you didn't know very much? I'm struck by the way you both sound like ordinary citizens, like the "man on the street," when in fact you both were in and out of Washington, knew a lot of people, and presumably had some power.

Kistiakowsky. I think you exaggerate my position. By then I ceased to be—not that I was fired or anything—I ceased to be a member of the President's Science Advisory Committee [PSAC]. Jerry, you still were a member, weren't you?

Wiesner. Yes, I attended meetings, but even so, strategy issues were not discussed in PSAC. The point is, the thing that appalled us about Goldwater was his threats to use nuclear weapons and his very strong jingoism. Johnson never talked like that, and the contrast was attractive. We never asked the President about Vietnam specifically, so we can't say we were deceived in any literal sense. On the other hand, we were given the impression that we were supporting a peace platform: there is no question about that.

Langer. I have a question that might clarify what the role and limits of the science advisory apparatus was at that time. When you were science adviser, did you attend National Security Council [NSC] meetings?

Wiesner. Yes.

Langer. And did you when you were science adviser under Eisenhower?

Kistiakowsky. I had a standing invitation to attend all of them. Did you?

Wiesner. I occasionally did not go if the issue was far outside my field of competence and I was not interested in it. I wasn't concerned about gold flow, for example. And occasionally I would elect not to go if I was very busy on something vital. You should remember that the National Security Council mechanism was much less formal under Kennedy than under Eisenhower, and I believe that it remained that way under Johnson. Frequently Kennedy would call together small groups of particularly interested people to discuss an issue, the Cuba crisis, for example. Before an issue was finally resolved, the entire National Security Council would discuss it, but I believe that there were many more informal discussions in the Kennedy government than there were under Eisenhower.

Jerome Wiesner

Kistiakowsky. There were very few. The NSC met once a week unless the President was out of town, and most of the issues were usually discussed. There were a few *very* sensitive issues, such as U-2, which were not discussed in NSC, to some of which I nonetheless was privy. My impression is that in the Kennedy administration three men, namely the President, [Secretary of Defense Robert] McNamara, and [McGeorge] Bundy [special assistant to the President for national security affairs in the Kennedy and Johnson administrations], met frequently in private and made the major military decisions.

Wiesner. No major decision was made without a full NSC discussion, but it was frequently obvious that there had been a lot of private talk beforehand.

Kistiakowsky. Were plans for the Cuban invasion discussed in NSC?

Wiesner. The Cuban invasion was not. But I think that was the experience from which he learned that he should be more careful.

Langer. In preparing for meeting with you today, I reread a story I wrote for *Science* in 1967 based on telephone interviews with everybody who had been on Scientists and Engineers for Johnson. The range of disaffection then was certainly very great, but the article ended up by commenting on the feeling of powerlessness of all the people who had been on that committee, and I remembered wondering to myself, if those people felt powerless, who was feeling powerful at that time? Who was feeling in control?

Wiesner. I don't know.

Kistiakowsky. As a reward for my services during the election campaign, I was appointed a member of—as it turned out—a fictitious organization called something like the President's advisors or board of advisors on foreign policy. It met once or twice during the campaign for a briefing and never met afterwards. But I was never formally dismissed. Hence, I wrote a letter to the President urging him to de-escalate the war in the fall of '65, about Christmas of '65. Remember?

Wiesner. I remember that.

Kistiakowsky. You and I were working on it together. And that letter got no attention, shall we say?

Wiesner. And I later wrote another one with Roger Fisher [professor of law, Harvard University] which we sent in through the vice president, but the only effect was that the vice president caught a lot of hell for delivering it, according to him.

Kistiakowsky. That's right, and a third letter was written by [former Ambassador to India, John Kenneth] Galbraith, who also got only a telephone message in return asking him to desist.

Wiesner. I don't know what you mean by power, because I don't think we organized Scientists and Engineers to achieve power. What we were attempting to do was publicize the issues and avoid having Barry Goldwater for President.

Kistiakowsky. It was a strictly election-year organization.

Wiesner. Afterwards, many people tried to persuade us to keep it together, but we didn't believe this made any sense because it had been organized for a specific purpose and it would be wrong to try to use it for something else.

Kistiakowsky. I must say my first strong, explicit disappointment with President Johnson was in January or early February '65, when the so-called Gilpatric [Roswell Gilpatric, Deputy Secretary of Defense, 1961–1964] committee (appointed by the President) of which I was a member, orally recommended to him that he should place great emphasis on the issue of nonproliferation of nuclear weapons. The recommendation was immediately buried. There was hardly a "thank you" to the members of that committee.

Wiesner. Well, you remember what

happened to us on the ABM issue? In '67 we were invited by Secretary McNamara to come to the White House to discuss the ABM.

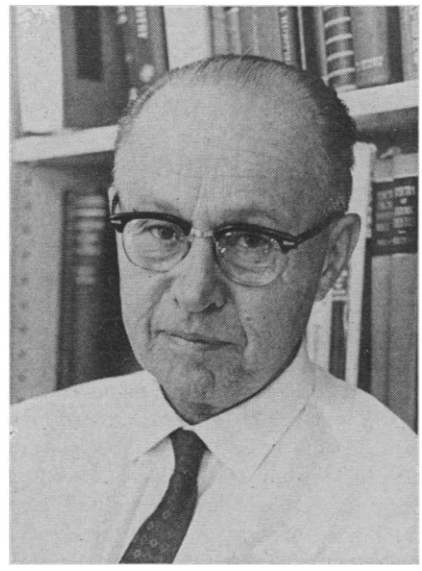
Langer. You two, or you two with a large group?

Wiesner. With a larger group.

Kistiakowsky. All the former science advisers.

Wiesner. All the science advisers and all the previous directors of DDR&E, including [Harold] York and Harold Brown, who was head of DDR&E at that time. To a man we were opposed to both the big system and, as far as I recall, the smaller system too, some of us more vigorously than others. And he not only made the decision to deploy the small system, but McNamara then sort of misused, I'd say, our position there in *support* of the small system. I think that we may have had many reasons to feel ill-used, but I didn't think the President ever owed me anything. I wasn't supporting him for his own sake or because of any personal trust. I did it for what I thought was the best interest of the country. You can question whether it was, of course, as things turned out. I think it was probably still better to have had Johnson than Goldwater, but that's a poor choice.

Kistiakowsky. And of course the issue of the use of nuclear weapons in Vietnam was very much in our minds. I might mention another episode. It was about the period of the Tet offensive, when one of our senior generals from Vietnam made a public statement saying that there was no need to worry about the fate of Khe Sanh, because if conventional weapons didn't work, we would use tactical nuclear weapons. When I read that I became very much exercised and I telephoned the two gentlemen, who were science advisers before I was, and chairmen of PSAC under Eisenhower, namely [James] Killian [chairman, M.I.T. Corporation] and [I. I.] Rabi [professor of physics, Columbia University]. They agreed with me that this was a very dangerous thing even to talk about. And we constructed a telegram to ex-President Eisenhower (who was then in Palm Springs) quoting the general and saying that this would be disastrous in its own right and also raised a danger of expanding the war. We sent that telegram to Eisenhower, and as luck would have it, President Johnson was visiting Eisenhower a day or so after he got the telegram. Apparently Eisenhower was



George Kistiakowsky

very much concerned and talked very vigorously to Johnson about it. I judge that that was the case because within less than 48 hours, each of us, Killian, Rabi, and myself, had a personal phone call from Secretary McNamara saying that he had been instructed by the President to state firmly that there was no planning, even contingency planning, to use nuclear weapons.

The "summer study" discussed below was a seminar of 47 scientists which was held under the auspices of the Jason Division of the Institute for Defense Analyses in the summer of 1966. Further discussion of it can be found in the Bantam edition of the Pentagon Papers, (pp. 483–485) and in Document No. 117 (p. 502). The New York Times, *citing the original Pentagon study, emphasizes that the scientists' work was a major influence in persuading then Secretary of Defense Robert McNamara that the bombing of North Vietnam was ineffective in curtailing North Vietnam's military activities in the south.*

Langer. What was the kind of thinking that led up eventually to the 1966 summer study which, at least according to the *Times*, turns out to have been so important?

Kistiakowsky. I'll try to summarize that briefly. Jerry and I probably were responsible for its start. We had prepared several letters to President Johnson in the winter of '65–'66. We got what amounted very much to a brush-off.

Wiesner. A pointed brush-off.

Langer. Did the study require some kind of presidential assent to set it up?

Wiesner. No, the study came about in another way. We had a small, self-initiated discussion group in Boston whose purpose was to see whether we couldn't find an acceptable way of stopping the war. We looked at a large variety of different ideas, and every time we thought we had one that looked acceptable to all sides, we would try to get the President to listen to it.

Kistiakowsky. There was no substantive consideration of our proposals. They were all just dismissed.

Wiesner. What actually happened on the study referred to in the *Times* was that we began to examine General [James] Gavin's enclave proposal seriously and we decided we didn't have enough facts about where the troops were, where the civilian population was, and a great number of other important questions. We called somebody, I believe it was John McNaughton [Assistant Secretary of Defense for international security affairs] and asked him for a briefing and he said he would send up [Adam] Yarmolinsky [Special Assistant to the Secretary of Defense] to give us the data we wanted. And Yarmolinsky came up with a lot of facts and data and . . . .

Kistiakowsky. Then he went back and there were some discussions in which I didn't take part. The result of them, however, was an offer to finance, out of the funds available to the Office of International Security Affairs, a study in the summer of '66 in which a considerable group, largely of physical scientists but also some social scientists, would be involved. I don't know that I'm at liberty to mention who they were, though Jerry and I were in the steering group. Out of that study came a number of recommendations to the Secretary.

Wiesner. And a number of observations, some of which have been in the Pentagon Papers. I think, as the paper said, the study probably played a very decisive role in convincing Secretary McNamara that, in certain respects, the intelligence information he was getting was wrong, that the estimates he was getting of where they were going were not very useful.

Langer. Just reading what's in the *New York Times* edition, [I think] the study seemed remarkable for the character of its thinking. It is a very agnostic document. It keeps stressing the things which can't be known—such as

the effects of the bombing—and asking how can you go on basing your assumptions on these unknowables. It seems as if the social scientists around the government, the Bundys, Rostows, [Walt W. Rostow, special assistant to President Johnson for national security affairs] et cetera, were always making recommendations which implied that a certain level of military action would have certain results. What seemed remarkable to me about this was its caution and conservatism and its insistence that the relationship between the bombing and Hanoi's "will to resist" could not be known.

Wiesner. Well, you see, we were trying to deal with known facts, with statements that were being made then.

Kistiakowsky. Let me put it a little differently. What we found was that the, so to speak, low-echelon findings—whether on the effect of bombing North Vietnam or on the amount of materiel and military supplies and manpower which were being brought into the south, or the number of the enemy killed—that extraordinary device, the body count . . . . On all these things we found that the numbers which were deduced at the lower echelons were uncritically and selectively used. By the time this enormous mass of detailed bits and pieces was synthesized into papers of manageable size to be read by the top echelon of the government, those people—for instance the Secretary of Defense—were in no position to find the time to read the detailed stacks.

Wiesner. You see, what is important to McNamara is how the information is distilled.

Kistiakowsky. He gets a short summary. The President gets a summary of a few pages and the Secretary gets 50 pages, but it is that kind of thing. By the time those papers were prepared, there was so much selection and value judgment and so-called agency position—in other words, you don't admit when you are wrong—that, in effect, the top-level papers were bearing very little resemblance to the basic background data.

Wiesner. And this is very important—they didn't show the true uncertainty. That was particularly striking. They simply appeared as if they had real validity. Of course, this problem isn't particularly unique to this situation. I remember that, after I became science adviser, I set out to see where some of the data we were given came from: information that was given as hard intelligence, like the number of Soviet missiles, or the number of tanks the Soviets had, or the number of divisions. And as I penetrated into the raw material I discovered that it often wasn't based on solid basic information. In fact, we caused the intelligence people to modify their estimates extensively by making them throw out the raw data they couldn't justify.

Kistiakowsky. That goes even farther back. I recall that in 1959 a well known columnist, Joseph Alsop, upon hearing that I was going to be the next science adviser, but while I was still a private person, invited me to have breakfast with him. He told me that he had hard intelligence information that the Soviets had deployed about 150 ICBM's. He had pretty good intelligence that they were going to attack the United States about July 1, 1959. It so happens that I was then in possession of relevant intelligence data because I was a member of the Ballistic Missile Advisory Committee to the Secretary of Defense. But of course, listening to Mr. Alsop, I had to act as if I was innocent. And Alsop didn't keep this information to himself. You will find articles that he wrote in which he made dire predictions. And this sort of thing happens incredibly frequently in Washington.

Langer. He's got quite a record for predictions, I think. Well, did you feel obligated for political reasons in the summer study to include something about a barrier, or was that McNamara's previous idea?

Kistiakowsky. No.

Langer. How did that proposal come about?

Kistiakowsky. In our agenda proposal, one of the topics we wanted to look into was a way of minimizing or reducing the flow of supplies and manpower from the north in a way that would lead to de-escalation of the war rather than escalation and bombing.

Wiesner. You see the avowed purpose of the bombing effort was trying to stop . . .

Langer. To stop infiltration.

Wiesner. To stop the flow of munitions. And we wanted to see whether there was an alternate way to achieve that end.

Langer. Was there no possibility of arguing about that objective?

Wiesner. We did that too, but first of all we looked at the question of whether the bombing was an effective way of achieving that objective, and it became clear that it was not. In fact, that's one of the things we said. But we also then asked whether the materiel coming in was significant. I think the general impression was that there was a significant flow of munitions. Isn't that correct? As I recall it, the amount of munitions coming along the trail could sustain the VC, [Vietcong] although a good deal of munitions were coming in other ways too. So then the question was whether there were any technical means that we could see that might do a better job and be much less destructive. We wanted a more benign way of achieving the same end.

Kistiakowsky. We were looking for ways of minimizing the casualties and minimizing . . .

Wiesner. The destruction.

Kistiakowsky. The idea was to lay the so-called barrier—which had nothing to do with a fence—through the uninhabited jungles through which the Ho Chi Minh trails are cut. We were very uncertain of the feasibility of this scheme. There was a very heated internal debate in August whether we should even present the plan to Mr. McNamara.

Langer. Because of its feasibility or because of its politics?

Wiesner. Because of its feasibility. I don't think we ever argued the politics.

Kistiakowsky. You have to be aware that we thought of ourselves as what might be referred to as His Majesty's Loyal Opposition. We were working through the channels, within the organization, as yet. In my case it was a bitter experience, and it led me outside the channels. Our recommendation to Mr. McNamara, made about Labor Day, 1966, was to confirm our ideas by a detailed, larger study of professionals to be organized within the Department of Defense. You remember that?

Wiesner. Yes.

Kistiakowsky. It was a very emphatic recommendation. We had even outlined a plan of what the study would involve in manpower and time and so on. Mr. McNamara's feeling was that time was of the essence. He wanted to develop details of the plan concurrently with the development of special devices, the so-called sensors, and so on, and also with plans for deployment. He felt if it were done in an orderly succession, it just would be much too slow. I think this is as far as I will go because I don't know how much of subsequent

history of the fence has been revealed in the papers.

Wiesner. I don't either.

Langer. Well, I was going to ask about that. The only thing I knew was that at a certain point you [Kistiakowsky] resigned from the committee. But I don't think it is generally known how far work proceeded.

Wiesner. I don't think we know or should talk about it.

Kistiakowsky. Let's put it this way: it's none of your business.

Langer. It's not a question of what's my business. It's a question of what is public business.

Wiesner. Well, it's a question of what it is our right to talk about.

Kistiakowsky. All I can say is that I was in one way or another connected on a part-time basis with this project. And at the end of '67, particularly after seeing that Mr. McNamara was essentially fired from his job, I reached the conclusion that it was completely futile to continue. At that point, I resigned, and resigned in what might be called a tactless way. In other words, I didn't claim illness or family business or fatigue. I just wrote that I vehemently opposed the present Vietnam policy and could not be even a minor party to it anymore. That's all I think I can say about it.

Langer. I'd like to say that I think the question of what is public business is sort of up for grabs at this point, and the more that is public, it seems to me, the healthier things can be.

Wiesner. I think that's right, and I believe that we ought to get rid of all secrecy on such matters. I feel strongly about this. But I think that, until there is a change in the security laws, violations of security represent a form of civil disobedience, and when one undertakes to do that, he should do it for a reason. I don't think that there's an issue on this point that justifies either of us doing it.

Langer. I didn't understand that the issue was breaking a law.

At this point, there ensued a brief discussion, largely excised by the interviewees, in which they explained the reasons for their reluctance to continue discussion of the barrier plan for publication. The conversation then turned to their subsequent discouragement and withdrawal from activity that was designed directly to affect the course of the Vietnam war. Wiesner, in particular, stressed that at that point his interest became focused primarily on the ABM.

Wiesner. The McNamara decision was announced in the fall of '67 and it was then that I began to oppose the ABM deployment publicly. I also stopped working within the government and started to work outside. After the so-called thin system decision, I gave up trying to convince anybody in the government to make sense on the ABM, for I regarded that as basically a political decision. There was no question in my mind that Mr. Johnson made the deployment decision for political reasons. He still expected to run for President, and he was protecting his flank by making that thin ABM decision. At least this is my view of what he did. There was no rationale to justify the ABM that I could see, and I decided to see if this waste for political reasons could be stopped.

Langer. How do you feel about the ABM battle? What do you make of it?

Wiesner. My feelings are complicated. I'm sorry we didn't win it. I think, nonetheless, it was a vital fight. It showed that you could make a good fight against a foolish decision. I believe that it exposed the military issues in a public way for the first time. I think that personally I spent far too much time on it. But I never really felt we lost it, because we kept it down, we helped Congress be responsible, we helped the public become informed. I think much of what has happened since, in the way of public debate on many things like the environment and the SST, grew out of the ABM experience.

Kistiakowsky. I very much agree with Jerry on that. In a personal sense, you might say, Jerry lost; I was a much more minor character in that one, though I lost also. So did York. But in a more fundamental sense we won, because we generated a completely new phenomenon.

Wiesner. I think in a real sense the nation won. Congress looks at everything seriously now. The public will not buy . . . new weapons without looking at their purposes. You can't scare them by telling them the Russians have three, as they used to do.

Kistiakowsky. The proposals of these, I might call them "hot-rod military" types, are not sacrosanct anymore. They are challenged, and the ABM debate was the first of these public debates.

Wiesner. The ABM has been held to a modest waste of money, you know, a couple of billion dollars instead of 40 or 50 billion. So even on that score I think there was substantial gain.

Kistiakowsky. You see, in the first years of the existence of the President's Science Advisory Committee, when we were really very involved in military technology, there were similar battles about proposals of the military, but they were held completely *in camera*, they were on a highly classified level between the White House office and the Pentagon. The new phase is the public debate.

Wiesner. When Johnson became President, he already had a history of differences with the scientists on issues that had nothing to do with Vietnam. We had differed on the space effort. Most of us were against the crash manned space program, and we had, of course, argued about that. I had been opposed to the Mach 3 SST and he was for it. There were a whole variety of issues that had caused tensions between the Science Advisory Committee and Johnson. So when [Donald F.] Hornig became science adviser, he had to carry the burden of Johnson's alienation from the scientists. The tension was greatest on the Vietnam issue. The result was that Johnson's Science Advisory Committee didn't have as much influence on military technology as it had under Eisenhower and Kennedy. Kennedy once told a reporter that the Science Advisory Committee and the science adviser kept the government from going all one way. He appreciated what it did and President Eisenhower appreciated it too. I don't think that Johnson felt a need for such help. Once that estrangement happened, it became necessary to take the battle elsewhere.

Kistiakowsky. Some of these quarrels that you are referring to, Jerry, took place long before he became President.

Wiesner. Right; when he was vice-president and chairman of the space council and space was the only real problem on which he had initiative.

Kistiakowsky. And I think he thought of the scientific community as being against him.

Wiesner. Because we were. We didn't really believe that the large manned space program made any sense scientifically and we kept saying so. In the end, we were willing to accept the

President's judgment that it was necessary politically, but we fought against it being started, on technical grounds.

Kistiakowsky. Of course you know Johnson pushed for it before he was vice-president, while he was in the Senate.

Wiesner. Then, in the case of the SST, I wanted the United States to join the British-French consortium and build the Mach 2 aircraft. There were many reasons why, in my office, we didn't believe a Mach 3 SST made sense, but it ultimately went that route because the vice-president wanted it.

Kistiakowsky. And I contributed earlier than that, in the Eisenhower administration, to rejecting an SST project that was pushed about '59 or '60.

Wiesner. So there were many issues of this kind that we disagreed on.

Langer. When you refer to "going outside the system," you mean going outside the administrative channels in government and going to congressional channels in government.

Wiesner. Congressional and public. I made my first attempt to do something about the ABM in a public speech for the Center [for the Study of Democratic Institutions] in Santa Barbara.

Langer. It's interesting because that distinction, which looms very large in your mind, doesn't necessarily loom so large to an outsider.

Wiesner. But it does to anyone who's worked in the government. It makes a tremendous difference for a variety of reasons. First of all, one is privy to large amounts of privileged or classified information, and so one has the problem of how to carry out an intelligent, useful discussion about something like the ABM without violating security. It was a serious problem. And, in fact, there were times when all of us were accused of violating security and threatened with prosecution for having done so.

Kistiakowsky. If you go back to, say the late 50's and early 60's, you will find that there was hardly a scientist who was privy to classified information because of his active part in government operations who ever made any public—either written or oral—statements on these matters involving security. The first change in that came about when the partial test ban treaty was signed and came up for Senate ratification. At that time, I was asked by the Administration to testify along with a number of others, like York, who had been in the government but were not in it full time anymore. I had vague qualms as to whether I should testify or not.

Wiesner. But of course, you were talking in support of the Administration.

Kistiakowsky. I was supporting the Administration and so I decided I would testify.

Wiesner. It was regarded as gauche.

Langer. I remember there was a lot of criticism of you [Wiesner] and your office at that time, for not testifying in public, being secretive about the people on your staff, and so on.

Wiesner. I remember an article to that effect in *The Reporter*, but I think —I thought at the time—it was a total misreading of the role of science adviser. When I became science adviser, it was to be an assistant to the President, not as the representative of the scientific community or anyone else. At least this was my view of the situation. So in that role, I obviously couldn't, and didn't intend to, oppose the President. But there are obviously many different roles. In some you have much less obligation, such as when you become a member of the President's Science Advisory Committee or a Defense Department advisory committee, but are not a full-time participant. Even for such people, it's still regarded as bad taste to engage in public debate. Some of the younger people do testify before Congress, and they've been criticized for doing so. Dick Garwin [adjunct professor of physics, Columbia University] was critized for his SST testimony because it opposed the Administration's position. There is a general view that, if you're going to be part of the Administration, you shouldn't simultaneously attack its posture. I believe that, if you join an Administration as a full-time employee, that is a reasonable position. If you become so disaffected with its programs that you want to fight, the proper thing to do is quit. But I don't think advisers should be throttled, that is, silenced on all issues, or the country is handicapped in making decisions. I've personally concluded—and I've thought about this since the Pentagon Papers were published—that the nation has paid a much higher price for its secrecy than it would have paid through a policy of complete openness. We've done many things on the basis of inadequate information, not only in the Vietnam war: I question whether the arms race would have taken the extreme form it did if the intelligence fellows had been forced to say what the bases of their estimates were and to defend them. If they had been exposed to serious questioning and hammered at by skeptics and asked, for example, "What makes you think the Russians are going to have a thousand bombers?" If they had been required to show their evidence, we would never have had that "bomber gap."

Kistiakowsky. And we would never have had a missile gap. And now another missile gap.

Wiesner. And there are many other examples. You mentioned, for instance, the U-2, and the extensive border penetration by U.S. electromagnetic intelligence in the 60's. When I first told Kennedy about it, he said, "My God, if the Russians did that to us, we'd go to war." And it was top secret, so secret that I, as science adviser, had a hard fight to learn about it. And after I finally had a briefing on it, I asked a colleague, "Who the hell are they keeping it from? The Russians know about it." And we concluded it was being kept from the American people so they would not know what was being done in their name. The Pentagon Papers show that there are many things of this kind. Not only should people who are government consultants not be in this embarrassing position, but the people of the nation should know what their government is doing in their name to a much greater extent than they do. I think democracy cannot function properly with so much secrecy.

The discussion ended with a brief exploration of Wiesner and Kistiakowsky's future political plans. Wiesner was asked to explain a comment he made when he assumed the presidency of M.I.T.—in effect, that he anticipated an end to his political involvements. He replied that he intended to avoid partisan politics, but not to withdraw from battle on substantive political issues when he felt them to be important Kistiakowsky added that he was concerned with restoring the dignity of science in the public mind. I indicated that, if I had had their experiences in Washington, I would feel particularly discouraged about continuing to believe, as they appeared to do, that individual men, and not the system, were responsible for the country's present difficulties. "What's the choice?" Wiesner asked. And the interview came to a stop.