September 13, 1968

Dr. Matthew Meselson
Department of Biology
Harvard University
Cambridge, Massachusetts

Dear Matt:

Senator Mondale would be a very good champion for following through on some of our mutual concerns about work on biological warfare. I did send him some material of my own in response to his letter, but I pointed to you as having much more authoritative information on the history of BW control proposals and the general atmosphere that pervades the administration on this subject. I promised him that I would communicate with you directly and try to provoke you to contact him at your own initiative with further material in response to his request.

I don't know just how many of these columns have been appearing in the Boston Globe, so I enclose them anyhow for your own information.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

MESELSON

Sent 3/30/68

+ May to Aug 1968

THE UNIVERSITY OF VERMONT
COLLEGE OF AGRICULTURE AND HOME ECONOMICS
BURLINGTON, VERMONT 05401

DEPARTMENT OF BOTANY
MARSH LIFE SCIENCE BUILDING

9 December 1969

Professor Joshua Lederberg
Department of Genetics
Stanford University College
of Medicine
Palo Alto, California.

Dear Dr. Lederberg,

I have read that the report of your committee to the World Health Organization on the genetic effects of defoliant use has been published.

I would appreciate greatly a copy of this report or information on how and where I can obtain it.

Thank you for your consideration.

Sincerely,

Richard M. Klein
Professor

→ M. Meselson

October 12, 1986
Joshua Lederberg

A subcommittee of the NAS/CISAC Committee devoted to BW met in Moscow October 8-10, 1986. A list of the Soviet and U.S. participants is appended.

This is a preliminary transcript of my impressions of the meeting.

There will be a detailed transcript of the notes taken by Lynn Rusten and others of our delegation. This is a preliminary account of first impressions. I stress here matter that pertains particularly to problems of verification. In fact with the exception of Ustinov the Soviet scientists present at the meeting had very little experience or background in BW problems. I asked Ustinov about that and he said that he and Antonov had given that same group a briefing approximately a week earlier. It does happen then that this meeting has become a way of introducing a significant group of Soviet scientists from the civilian sector into some thought about BW arms control problems.

It is important to note that Dr M. Meselson of Harvard had been invited by the Ministry of Public Health to visit Moscow at the end of August, and was briefed for 3 days about the 1979 anthrax epidemic at Sverdlovsk. His respondents were Burgasov (Deputy Minister of Health), Bezdenezhnykh, (an epidemiologist from the RFSR health ministry), Nikiforov and Yampolskaya (both physicians from Moscow. Meselson talked to our group for about two hours in New York on September 22. The 5-year BWC review conference was held in Moscow at the end of September, and I was briefed about that by Robert Mikulak at ACDA.

Bochkov did not attend: "away on business." Sagdeev attended briefly at the end of the second day and at one of the luncheons. In addition I had an extended conversation with him at his home for dinner on Friday evening.

The meeting was held at the Institute for Bioorganic Chemistry, still under construction. Its director, Yuri Ovchinnikov, was away on business, in Europe. He has been rumored to be in poor health, but is evidently asymptomatic according to Dr. Rich, who saw him recently.

Although Ivanov is the deputy director of the Institute Sverdlov acted as co-chairman on the Soviet side. In a brief discussion of the agenda he was quite agreeable to devoting the first day to questions about BW arms control and the second day to cooperative research programs. It was evident from the outset that the latter was of the greatest interest and incentive to the group. I asked him whether it would be possible to invite Nikiforov to the meetings and he said that that had already been arranged and he would be available for the Thursday morning. Burgasov it turns out has retired only very recently (to the great delight of one of the Russians who said that he had been extremely restrictive, for example forbidding the publication of any information about AIDS in the Soviet Union). Bezdenezhnykh has died of a heart condition during the last few weeks. Nikiforov said that he was already in very bad health at the time that they had talked to Meselson.

My opening statement is attached. There was little concrete response

about my remarks on means of verification. By common agreement the conference of experts to be held at Geneva in early April would be the place to negotiate detailed proposals. There was, however, general agreement about the need for confidence building and a very strong affirmation that medical scientists did have an important responsibility to be sure that BW was indeed controlled and that the Treaty be strengthened in a way to assure mutual confidence. There was a repeated echoing of the thought that extensive scientific cooperation between the U.S. and the USSR in fields related to BW would contribute greatly to providing that mutual reassurance. There were really no dissonant notes of any kind: the only approximation to it was from Schvedkov a political scientist from the Institute for the US and Canada. He said the Soviet public has reason to worry about what the US military is doing. He quoted press reports about happenings in the Pentagon. He also quoted a story in Graham Allison's book on formation of foreign policy "that Nixon's proposals for BW disarmament had been resisted by the US Army. Also, Gorbachev had made the furthest proposals on verification in his speech on September 9th that would allow for an international network for verification of the nuclear test ban. Indeed a supra national network that might be viewed as a prototype for what could be done in the BW area. The Third country problems have to be looked at in a broad international context. Terrorism is generated by international conflicts and is the recourse of the weak against the strong. As to the BWC, suspicions are not supported by the fact and the convention is working. "

Nevertheless everyone else including Ustinov seemed to resonate with the conclusions of the BWC review conference in Geneva that called for strenthening of the BWC and even Schvedkov's remarks were made in a much milder tone than what usually comes forth from his institute. I had the impression that he had a perfunctory message that he was obliged to deliver and that nobody was paying very much attention to it.

My own remarks about the importance of affirmative cooperative verification, that each side had in his own interest the promulgation of openness and reassurance were responded to positively by every other member of the group to whom I talked. (Lisov from the Ministry of Defense did not utter one word during the entire meeting. He had a rather dour expression throughout, which may or may not have any significance.) The entire proceedings were videotaped. Lynn Rusten's request to get a copy was at first rebuffed bureaucratically "that it had not been arranged beforehand. Videotapes are stringently controlled." Sagdeev reassured her however that it would be done.

During the discussion I brought forward the Wall Street Journal article on BW defense budgets of the Pentagon. This was not new information but I think that some of our colleagues were a little startled to see the depth and candor with which this kind of discussion was published in the United States. At the very end of the meeting I also deposited the DoD Report to Congress on the same subject as an illustration of the openness that prevailed on our side. The scientists were hardly in a position to make promises about future Soviet behavior in this

direction but I'm sure this was useful and appropriate information for them to have.

Nikiforov arrived at the very start of the Thursday session but he had left the slides back at his institute (!). We agreed that it was important that they be included as part of his presentation and so he agreed to wait and to sit in in our discussion for the hour or so that it took for them to arrive. Nikiforov heads the department of infectious diseases at the Central Institute for Postgraduate Medicine in Moscow. This has a hospital of 360 beds; there are 110 departments which give courses ranging from a few weeks to many months to a total of 28,000 physicians every year (There are 1.2 million physicians in the Soviet Union). His department provides teaching for about 280 postgraduate physicians in the field of infectious diseases. Yampolskaya is an assistant in that department, one or two layers removed from Nikiforov. It is not unusual at all for them to be consulted on medical problems that arise throughout the Soviet Union. In this case they were called by Professor Kortev from the Medical Institute in Sverdlovsk about the two cases of disease with very strange onset. (In this precis, I will omit most detail that duplicates what was recorded by Meselson. As stated before, a full report will be provided later.)

Acute and severe abdominal pain and high fever suggested to them some form of intoxication. It was only after the post-mortem that they were able to confirm a diagnosis of intestinal anthrax. Throughout his career Nikiforov has seen many sporadic cases: perhaps 100 to 120 altogether, which had much the same picture. He said that previously they were all fatal so that he felt gratified that they were able to save even the small proportion that did survive in this case. There was no precedent for an outbreak of intestinal anthrax of this dimension in history. He did not think the clinical course of the disease was unusual for that particular diagnosis. They are not particularly research oriented and he does not believe the strains were saved. When tested contemporaneously they were sensitive to all the usual antibiotics including penicillin. The bacilli did have a very thick capsule which is closely associated with very high virulence in anthrax.

We had not as yet received Meselson's notes of his August trip. We therefore had only a few limited points of testimony to use as the basis of more detailed questioning.

At my request, Nikiforov met with us again on Friday, and asked Olga Yampolskaya to join us, together with Dr. Sverdlov. Her English is reasonably fluent, and this facilitated the discussion. When we asked them questions about epidemiology they stressed that they had no first hand information of those studies since their task was the clinical care of the individual patients in the hospital. Besides Yampolskaya there were five other assistants who came up at various times from Moscow and they had 22 local M.D.'s also helping in the management of the outbreak. Yampolskaya in fact had no detailed knowledge at all about the epidemiology until she heard Professor Bezdenezhnykh at the

meeting with Matt Meselson. That was the first time that she had any inkling that there were political overtones to the epidemic. Neither of them had the detailed case records at their own disposal. Yampolskaya thinks she was selected to help in the briefing because she had kept certain personal notes on a few of the individual cases. We did not think it fruitful to pursue very strongly the epidemiological side in Bezdenezhnykh's absence. We strongly urged, and Sverdlov echoed this, that it would be of great scientific (not to mention political) importance for a detailed account of the epidemic to be published. Nikiforov said that he had come to that conclusion himself. It was urged on him that he get a young epidemiologist to assist him in the compilation of the detailed records.

On the clinical course, Nikiforov gave very dramatic account of the development of the syndrome. When it reaches a stage of toxic shock it does include cyanosis and dyspnea but this is only fairly late in the development of the disease. As to the family distribution Nikiforov thought he remembered one family with as many as three victims. Perhaps ten of the families of the total had more than one case. He has no explanation for why a rather small proportion of those who presumably ate the infected meat actually came down with the disease. As the cases began to accumulate they became very concerned about trying to collect them at the earliest possible point so besides the public health measures with which he was not directly connected (the circulation of notices warning about meat contamination; destruction of sources of infection) he encouraged the hospitalization of essentially every case who presented with fever or other promonitory symptoms. He said that in total perhaps as many as 500 individuals with even mild fever were hospitalized for a time in order to enhance the opportunity of catching any new cases as early as possible. He concluded that penicillin was as effective as any of the other antibiotics but they had tried a variety of broad spectrum antibiotics not to leave out any possibilities. They used steroids in massive doses to attempt to mitigate the shock syndrome. (There is a mistranslation in our copy of the 1980 paper: prophylactic antibiotics were given to not by family members of the cases.)

Some of the gross pathology that he desccribed was quite impressive. One set of pictures showed multiple lesions on the tongue and stomach which he believed were primary sites of infection by anthrax organisms.

At one point near the end of the discussion on Friday morning Nikiforov left an opening with a remark about the political colorations so we pressed him a bit more closely. He was unaware of any military involvement of any kind. There were no military or police in the hospital. He thought that conceivably they might have played some role in the public health management for example in arranging for the burning of some sheds in which infected animals had been kept. He said that most of these would have been in the suburbs in the south probably about two weeks after he arrived in the city. We showed him The New York Times article (dated '80) giving the emigres' report of the outbreak. He only seen it the day before, namely the copy I had given

to Sverdlov. He never heard of Kashino, reported there as a suburb of Sverdlovsk which was in the path of the airborne plume. As to the rumors, he was not aware of any at the time. He said he had had some hint of some fuss later on but had paid no particular attention to it. He put what he read in The New York Times article as the "Mark Twain Syndrome", recalling that he had once been involved in a cholera epidemic. He heard over the radio that the entire medical team including himself had been killed by the disease. "Reports of his own death were grossly exaggerated". After Chernobyl, families had all kinds of rumors about what was happening to them based on their fear of radiation.

Yampolskaya said that there were lectures from time to time in Sverdlosk at the Medical Institute and in Moscow, where the outbreak was used as teaching material and there must be dozens if not hundreds of medical residents who have heard about it.

I was left in very little doubt that they had been describing their personal experience in the management of an epidemic of intestinal anthrax. For sources of corroboration it will be necessary to go into the epidemiological aspects of the disease and this information is now in the hands of the Ministry of Public Health. It is certainly an obstacle that Bezdenezhnykh has died and whether Burgasov's successor will be more or less amenable to the distribution of those records remains to be seen. Without yet having seen the internal report I did not have any grounds to proceed very much further in my own questions and I made no effort to get in touch with the Ministry. I'm sure that Ustinov would be very cooperative in responding to any requests to try to reach that channel. Official diplomatic sources should request documentary material from the Ministry of Public Health.

My own private thoughts are that the whole question of the anthrax epidemic is a secondary issue. The story told so far appears to be internally consistent and not in sharp violence with the other available data, although these can be interpreted in different ways. It would be very easy for other observers to panic about the nature of the epidemic particularly if they had reason to believe that there is indeed a military BW facility near Sverdlovsk. That question is after all not touched by any of the medical questions that we have addressed here so far. If there were also military personnel who had succumbed to the infection, whatever the source, they probably would not have been treated at the civilian hospital; so there is not a necessary contradiction with Nikiforov's statements.

The primary question, verification of the nature and functions of the military facility will have to be addressed at other levels. But it would be an interesting test of just how far the Russians are willing to go in "openness" to ask whether there is any form of inspection of the suspected facility to which they would be agreeable.

Meanwhile, again as a personal view, I believe that it would extremely imprudent on the part of USG to continue to refer to the epidemic as evidence of violation of the BWC. Separate questions are a) the

channels through which the Soviet Union is responding to its obligations for consultation under the BWC and b) the primary allegation, which may have nothing to do with the epidemic, of continued BW production activities at Sverdlovsk in violation of the BWC. It must cause them much embarassment that a) anthrax remains endemic in Russia, and b) that public health safeguards had broken down, especially in the management of the bonemeal plant!

If we continue to refer to the epidemic per se in the face of the evidence they have presented, we face a) discredit from third parties, and b) [if it was indeed foodborne] the consolidation of Soviet views that we are not serious about our concerns for verification, but propaganda-motivated. It may be difficult to reach a standard of evidence that finally settles the matter and would require an affirmative retraction on the U.S. part; but there may be no need to reach that if we simply withhold further adverse comment about the epidemic. There remains every reason to demand satisfaction about facilities suspected of being BW-oriented; and the current Soviet mood may bend them to some accomodation by way of some form of inspection

If the epidemic itself is to be pursued, diplomatic channels might explore other kinds of corroboration: a visit to Sverdlovsk, if done at all, could focus on further conversations with the medical personnel resident there who were involved in treating patients. One might also be able to interview some of the few survivors and perhaps some family members of fatal victims to try to get some more detail about the clinical course of the disease.

Emigre sources might be interrogated more closely on the factual evidence of pulmonary vs abdominal involvement; also how they knew that the first casualties were military.

I did ask Nikiforov if he had seen inhalation anthrax himself. He said yes some years ago in Albania he had encountered a few cases of it. The distinctive pathological feature is involvement of the lung parenchyma which he said he did not see in the autopsies at Sverdlovsk. Fibrinolysis and plural hemorrhage as well as hemorrhage in every other organ system including the brain are characteristic of the systemic form of the disease whether of intestinal or other origin. He had graphic autopsy pictures of these features.

Dr. Woodward and Dr. Bennett could add with the benefit of their personal experience in pathology their impressions of what was being said.

Nikiforov and Yampolskaya have, I think, said all that they know and remember and they do not have more by way of their own records. Further investigation would have to stress the epidemiological aspects which would involve separate negotiations with the Ministry of Public Health, through governmental channels. We should not be too optimistic that they have the more detailed records that would conform to our expectations.

Our further discussions with the Russian academicians concerned areas that would be promising for scientific research cooperation. Dr. Sverdlov's resume is a fair account. It would further both medical science and cooperative verification of the BWC if some of these proposals could be implemented.

I am checking: Meselson had evidently phoned the Soviet Ministry of Health about our impending visit and this was undoubtedly how it came about that Nikiforov was primed to talk to us.

M. M. also recalled that the Russians had mentioned radio broadcasts warning of contaminated meat.

Date NYT, WSJ articles

DATE: February 28, 1977

TO : Matt Meselson

FROM : Joshua Lederberg

SUBJECT: Soviet Interpretations of Recombinant DNA ("U.S. Military 'R&D' through Soviet Eyes" by Michael Milstein and Lev Semejko, Bull. Atomic Scientists 33:32-38, 1977).

I had not understood the fuss about "recombinant DNA" at the CCD, and had taken for granted the substance of editor's note, (p.36) - until I had the opportunity to read this article.

But in fact, there is a loophole in the 1972 BW convention! Article I refers to "Microbial or other biological agents, or toxins...."

A synthetic DNA might validly be claimed to be neither a "biological agent" nor a toxin. Yet one could imagine the development of a synthetic DNA - modelled upon but not identical to a virus - which could be used for hostile purposes.

A similar loophole applies to new synthetic polypeptide sequences that might mimic natural toxins.

The problem arose, in part, from the introduction of the "toxins" question after the main problems in drafting the treaty had been overtaken - and no one was willing to risk reopening the text for "technicalities".

"Recombinant DNA" has little to do with this story. But synthetic DNA - a la Khorana - is right on the mark. And it is not covered by disclaimers at the last session of CCD.

If there is any possibility of any CW treaty, that may be the best place to plug these loopholes. If not, a formal assertion that the U.S. regards any use for hostile purposes of nucleic acids (whether biological, semi-synthetic, or synthetic) to be forbidden within the spirit of the BW convention, and asking other powers to do the same, would be a positive step (1) in answer to the Milstein-Semejko perspective, and (2) to quiet a certain amount of domestic paranoia.

(a propos the workshop you are running at the NAS next week.)

Josh

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STANFORD UNIVERSITY MEDICAL CENTER

DEPARTMENT OF GENETICS

July 29, 1977

Mr. Robert Mikulak
Arms Control and Disarmament Agency
State Department
2201 C, NW
Washington, D.C. 20451

Dear Mr. Mikulak:

I have been rather tardy about following up our conversations of early May concerning possible restatements of the U.S. definition of biological agents for the purposes of the BW treaty.

In the interval, as you know, there has been considerable motion with respect to regulatory legislation in this field, but the outcome is still uncertain. In that light, I suspect that it would be only adding to the confusion to attempt to make further statements about the BW connections of recombinant DNA at this time. I would rather suggest that we would wait until the dust settles in the arena of the legislative controversy. When that legislation has been enacted, I will write you further to suggest clarifying wording of definitions that would be consistent with the law as it is ultimately written.

Yours sincerely,

Joshua Lederberg
Professor of Genetics

cc: Dr. Matthew Meselson

JL:ek-f

COPY

Memo from
JOSHUA LEDERBERG

To: Bob Mikulak
ACDA.

JUL 29 1977

"BIOLOGICAL WARFARE

Excerpts From the Addr

Special to The New York Times 77

CHARLESTON, S. C., July 21—*Following are excerpts from the text of President Carter's address here today to the Southern Legislative Conference:*

The whole history of Soviet-American relations teaches us that we will be misled if we base our long-range policies on the mood of the moment, whether that mood is euphoric or grim. All of us can remember times when relations seemed especially dangerous and times when they seemed bright. We have crossed those peaks and valleys before. And we can see that, on balance, the trend in the last third of a century has been positive.

The profound differences in what our two Governments believe about freedom and power and the inner lives of human beings are likely to remain, and so are other elements of competition between the United States and the Soviet Union. That competition is real and deeply rooted in the history and values of our respective societies. But it is also true that our two countries share many important overlapping interests. Our job is to explore those interests and use them to enlarge the areas of cooperation between us, on a basis of equality and mutual respect.

Growing Disillusionment Noted

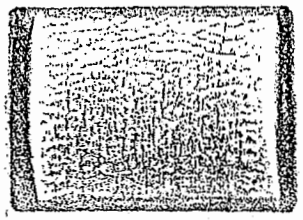
I felt that it was time for honest discussions about international issues with the American people. I felt it was urgent to restore the moral bearings of American foreign policy. And I felt that it was important to put the U.S.-Soviet relationship, in particular, on a more reciprocal, realistic and ultimately more productive basis for both nations. It is not a question of a "hard" policy or a "soft" policy, but of a clear-eyed recognition of how most effectively to protect our security and to create the kind of international order I have just described. This is our goal.

We have looked at the problems in Soviet-American relations freshly and have sought to deal with them boldly and constructively with proposals intended to produce concrete results:

¶In the talks on strategic arms limitations we advanced a comprehensive proposal for genuine reductions, limitations and a freeze on new technology which would maintain balanced strategic strength.

¶We have urged a complete end to all nuclear tests and these negotiations are now under way. Agreement here could be a milestone in U.S.-Soviet relations.

¶We are working together toward a ban on chemical and biological warfare and the elimination of inventories of these destructive materials.



Is that reference in the present tense an oversight? Or is there still some agenda for Bw.

It may get an unintended rise from the other side if it is an oversight but not explained as such!

[X] (2)

PROFESSOR JOSHUA LEDERBERG
Department of Genetics
School of Medicine
Stanford University
Stanford, California 94305

Memo/Reply From
JOSHUA LEDERBERG

TO:

What I have particularly in mind is to trace the reports on German BW potential (referred to, e.g., in the Merck Report) that led to the establishment of our own massive program at Ft. Detrick.

I have seen only oblique references to these (note enclosure); I have not had the opportunity to see the full monograph on CW Research in the US. The archivist complained that it was over 1000 pages, and I would need more specific identification of the relevant pages -- if you could provide just those, it might help me trace further. Or any other secondary sources -- I do not know if RC Cochrane is still professionally active (or living).

Thank you for any help you can offer.

Sincerely,

Please reply to:

J. Lederberg Box 115
1230 York Avenue
New York, NY 10021

Joshua Lederberg
President
The Rockefeller University
New York, N.Y. 10021
(212) 360-1234

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☐ COPY RETAINED

—REPLY FORM—

Memo/Reply From
JOSHUA LEDERBERG

TO: Dr Brooks E Kleber

NOV 9 1980

World War 2— intelligence re German CBW

As a member of the Defense Science Board, I have been actively involved in a reassessment of our CW posture, and in a related sphere in a study of the implications of the 'Sverdlovsk incident'. It is hard to say what would be the prtinence of WW-2 history to the current scene. However, for many years --and now more than ever -- I have been eager to get more information on the historical foundations of our own responses during and since WW-2, in re both CW and BW.

My particular request is for your help in tracing the details of the intelligence we received during WW-2 concerning German activities, intelligence that proved later to be seriously defective both in failing to detect the development of nerve gas, and in exaggerating their investment in BW.

I would have no trouble with respect to classification --if that still lingered on these topics-- but do not have the time for a personal search at CIA and Army archives without some prior leads as to the relevant documents. By chance, I noticed your name in the recent Guide..., and the footnote that connected your name with authorship of Chemicals in Combat, and this raised the hope that I might have found the expert on this facet of military history.

Can you help me?

Joshua Lederberg
President
The Rockefeller University
New York, N.Y. 10021
(212) 360-1234

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—REPLY FORM—

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Geneva, 8 April 1981

Dr Joshua Lederberg
President
The Rockefeller University
1230 York Avenue
New York
N.Y. 10021
USA

Dear Josh,

I am enclosing the report of our 8th Workshop on Chemical Warfare. It was a good meeting with excellent participants, and I believe we made some progress on the question of on-site inspection. Unfortunately, Bob Mikulak had to return to Washington unexpectedly, but with Col. Sanches and Matt Meselson the USA side was well represented.

Matt and I had opportunities to talk privately with Acad. Oleg Reutov and USSR Ambassador (to the CD) Issraelyan about the Sverdlovsk affair. They stick firmly to their explanation. They did not exclude, however, the possibility of a private meeting of scientists from the USA, USSR and perhaps other countries to discuss Sverdlovsk amongst other public health and medical questions, provided Sverdlovsk was not the exclusive subject. Let's see.

Best regards to you and Marguerite.

Yours,

M. M. Kaplan

Matt has doubtless briefed you.

P.S. The pre-1979 Soviet literature on anthrax that Jonathan Hoffman sent me is amazing in distinguishing inhalation from cutaneous anthrax but the former is very rare. [Who would recognize it?] J.

W1 OP (Lederberg)

DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138

November 18, 1981

Joshua Lederberg, President
Rockefeller University
1230 York Avenue
New York, NY 10021

Dear Josh,

Here are some items of interest in connection with the "yellow rain."

I think you would find it interesting to talk with Sorenson by telephone (304-599-7516). He administered T-2 solution to rats by tracheal instillation. At lethal doses he found massive lymphatic damage but little or no local damage in the lungs and no gross hemorrhage anywhere in the body. His interpretation is that the mycotoxin is transported out of the respiratory system too rapidly for detectable damage to have resulted. Of course the alveoli may respond differently and a solid aerosol may give different results, but these things remain to be demonstrated.

In certain forms of malnutrition, hemorrhaging from mucus membranes is not uncommon. Is it possible that the target populations include malnourished individuals with a resultant sensitivity to trichothecenes considerably greater than that expected on the basis of the existing literature?

As I testified at the Senate hearing last week, I think we have preliminary evidence for the use of mycotoxins. Nevertheless, considering the historic importance of the matters involved and the high value that must be placed on the credibility of the US government, I believe that the case is very seriously incomplete. One concern is the possible natural systemic occurrence of the toxins in certain higher plants. These materials are powerful insecticides and phytotoxins and may play important roles in the natural ecology of various regions. The feeling of some experts that systemic occurrence in higher plants is unlikely must be discounted because of the fact that, with one exception, all the species hitherto examined are crop plants. Species given to systemic accumulation of mycotoxins would not have been chosen by man as crop plants. The only non-crop plant that has been analyzed, so far as I know, does contain trichothecenes, 200-300 ppm of the macrocyclic baccharinoids. The trichothecenes seem to be derived from a soil fungus, absorbed through the roots and stored in the plant. Jarvis finds that this effectively protects the plant against certain leaf sucking

insects. This biological perspective underscores what is obvious: we need analyses from the attack areas and from comparable unattacked sites nearby, both in reasonable numbers, and using plant material of the same species growing under similar conditions. There is only one plant sample analyzed from an attack area, so far as I know. Sharon Watson said last week that there are also two control samples of the same species. As I understand it, the positive sample weighed about 0.4 grams and consisted only of leaf and stem. Species identification, especially in the tropics, usually requires inspection of flowers and/or fruits. Without knowing just how the species identification was accomplished, I am skeptical of its accuracy.

I was puzzled by Richard Burt's statement last week that the person bringing back water containing 66 ppm deoxynivalenol became "gravely ill" after spilling some of it on himself. Vesonder at Peoria has come in contact with stronger solutions with no ill effect. Amongst the obvious possibilities, if the story is not a gross exaggeration, is that some as yet unidentified substance in the water was responsible.

In trying to think of chemical tests that would shed light on the origin of the trichothecenes, I have wondered what high resolution mass spectrometry could tell us. Two things might be looked into. First, carbon isotope ratios can be used to distinguish the major photosynthetic pathways used by higher plants. This might allow some deductions regarding the substrate on which the fungus grew. Second, hydrogen or oxygen isotope ratios might provide information regarding temperatures of synthesis.

Warm regards.

Sincerely yours,

Matthew Meselson

P.S. I also enclose the sanitized version of my review of the DSB CW Pand Report.

bcc: Dr. Robert Mikulak

Enclosures:

- Burmeister, H. R. "T-2 Toxin Production by Fusarium tricinctum on Solid Substrate." Applied Microbiology 21:739-742, 1971.
- Rukmini, C., Prasad, J. S. and Rao, K. "Effects of Feeding T-2 Toxin to Rats and Monkeys." Food and Cosmetics Toxicology 18:267-269, 1980.
- Materials from AOAC Meeting, 21 October 1981.
- Abstract of "Toxicity of Inhaled Mycotoxins," by W. Sorenson, from Toxicology Research Projects Directory, v.4, no.1, January 1979, page 1-43.
- Jarvis, Bruce B., Midiwo, Jacob O. and Tuthill, David. "Interaction Between the Antibiotic Trichothecenes and the Higher Plant Baccharis megapotamica." Science 214:460-462, 1981.
- Meselson, M. "Comments on the Defense Science Board (DSB) Chemical Warfare Panel Report." 8 January 1981.

W1 OP (Lederberg)

DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138

November 18, 1981

Joshua Lederberg, President
Rockefeller University
1230 York Avenue
New York, NY 10021

Dear Josh,

Here are some items of interest in connection with the "yellow rain."

I think you would find it interesting to talk with Sorenson by telephone (304-599-7516). He administered T-2 solution to rats by tracheal instillation. At lethal doses he found massive lymphatic damage but little or no local damage in the lungs and no gross hemorrhage anywhere in the body. His interpretation is that the mycotoxin is transported out of the respiratory system too rapidly for detectable damage to have resulted. Of course the alveoli may respond differently and a solid aerosol may give different results, but these things remain to be demonstrated.

In certain forms of malnutrition, hemorrhaging from mucus membranes is not uncommon. Is it possible that the target populations include malnourished individuals with a resultant sensitivity to trichothecenes considerably greater than that expected on the basis of the existing literature?

As I testified at the Senate hearing last week, I think we have preliminary evidence for the use of mycotoxins. Nevertheless, considering the historic importance of the matters involved and the high value that must be placed on the credibility of the US government, I believe that the case is very seriously incomplete. One concern is the possible natural systemic occurrence of the toxins in certain higher plants. These materials are powerful insecticides and phytotoxins and may play important roles in the natural ecology of various regions. The feeling of some experts that systemic occurrence in higher plants is unlikely must be discounted because of the fact that, with one exception, all the species hitherto examined are crop plants. Species given to systemic accumulation of mycotoxins would not have been chosen by man as crop plants. The only non-crop plant that has been analyzed, so far as I know, does contain trichothecenes, 200-300 ppm of the macrocyclic baccharinoids. The trichothecenes seem to be derived from a soil fungus, absorbed through the roots and stored in the plant. Jarvis finds that this effectively protects the plant against certain leaf sucking

insects. This biological perspective underscores what is obvious: we need analyses from the attack areas and from comparable unattacked sites nearby, both in reasonable numbers, and using plant material of the same species growing under similar conditions. There is only one plant sample analyzed from an attack area, so far as I know. Sharon Watson said last week that there are also two control samples of the same species. As I understand it, the positive sample weighed about 0.4 grams and consisted only of leaf and stem. Species identification, especially in the tropics, usually requires inspection of flowers and/or fruits. Without knowing just how the species identification was accomplished, I am skeptical of its accuracy.

I was puzzled by Richard Burt's statement last week that the person bringing back water containing 66 ppm deoxynivalenol became "gravely ill" after spilling some of it on himself. Vesonder at Peoria has come in contact with stronger solutions with no ill effect. Amongst the obvious possibilities, if the story is not a gross exaggeration, is that some as yet unidentified substance in the water was responsible.

In trying to think of chemical tests that would shed light on the origin of the trichothecenes, I have wondered what high resolution mass spectrometry could tell us. Two things might be looked into. First, carbon isotope ratios can be used to distinguish the major photosynthetic pathways used by higher plants. This might allow some deductions regarding the substrate on which the fungus grew. Second, hydrogen or oxygen isotope ratios might provide information regarding temperatures of synthesis.

Warm regards.

Sincerely yours,

Matthew Meselson

P.S. I also enclose the sanitized version of my review of the DSB CW Pand Report.

bcc: Dr. Robert Mikulak

Enclosures:

- Burmeister, H. R. "T-2 Toxin Production by Fusarium tricinctum on Solid Substrate." Applied Microbiology 21:739-742, 1971.
- Rukmini, C., Prasad, J. S. and Rao, K. "Effects of Feeding T-2 Toxin to Rats and Monkeys." Food and Cosmetics Toxicology 18:267-269, 1980.
- Materials from AOAC Meeting, 21 October 1981.
- Abstract of "Toxicity of Inhaled Mycotoxins," by W. Sorenson, from Toxicology Research Projects Directory, v.4, no.1, January 1979, page 1-43.
- Jarvis, Bruce B., Midiwo, Jacob O. and Tuthill, David. "Interaction Between the Antibiotic Trichothecenes and the Higher Plant Baccharis megapotamica." Science 214:460-462, 1981.
- Meselson, M. "Comments on the Defense Science Board (DSB) Chemical Warfare Panel Report." 8 January 1981.

Lederberg

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DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138

February 17, 1982

Joshua Lederberg, President
Rockefeller University
1230 York Avenue
New York, New York 10021

Dear Josh:

I enclose three items for your possible interest. One is a "sanitized" version of the review of the DSB Chemical Warfare Panel report which I wrote as a consultant for the Defense Department last January. I also enclose a copy of my Senate testimony on Yellow Rain. What I have learned since giving it would lead me to place much less emphasis on the question of natural occurrences.

The third is a copy of a letter to Chester Mirocha which is self-explanatory.

If you have any plans that would bring you to the Boston area with a little time to spare, I would love to have a long chat with you.

As Ever,

Matthew Meselson

MM:mp
enclosures

CBW

Matt Meselson

Personal Memo from
JOSHUA LEDERBERG

Dear Matt; 4/6/82

Your March 22 testimony was very well stated. Thank you for sending it.

I think there is much to be said in behalf of bombs in preference to shells in view of the deployment/release procedures of CW.

My reflexes are for very great restraint; I have to steel them up against the possibility that we will be caught pants-down against an adversary who is tempted to show no restraint whatsoever. How "idealistic" will our "restraint" appear from that retrospective?

However I certainly will be armed with your critical questions in my own further queries.

What analysis do you have of collateral damage beyond your Sci Am paper?

Personal Memo from
JOSHUA LEDERBERG

More urgently than ever, I feel the need is to find some way to reestablish mutual trust with the USSR on A/C verification issues. The charade of the "UN investigation" of 'yellow rain' was a serious step backward.

Can you suggest any positive steps? (I don't believe giving them the benefit of every conceivable doubt is in that category. It just reinforces their stonewalling paradigm. -- Our own Secy. isn't always that helpful either!)

Is it really to the USSR's advantage to see us escalate CBW? (Or do they believe that they will always have the freedom of initiative for first use in a deliberate confrontation?)

No part of what's happening is giving joy to either of us!

Yours,

J. Lederberg

Please return to J. Lederberg

Version discussed with US colleagues – not at Moscow!

5/25/85

Draft position paper for CISAC, Moscow, June 1985

General considerations of biological warfare, arms control and proliferation.

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1. BW – define, distinguish from CW, toxin weapons.

Biological Warfare (BW) is defined as the use of a living microorganism or virus for military purposes. It is distinguished from chemical warfare (CW) which uses the toxicity of specific chemical (but nonliving) chemical substances. CW and BW are often confused, especially in the light of toxin weapons which are chemical substances produced as a by-product of biological fermentation. (Because the production facilities for either would be quite similar to an outside view, biological agents and toxins are treated in common fashion by the BW disarmament treaty of 1972.)

What sets BW apart is the infectivity of the agent: in principle one particle of a virus or a bacterium entering the human body could be sufficient to infect that individual by virtue of the exponential replication of the invading particle. This confers a very high potency in lethal doses per gram on biological agents. It also opens up the likelihood of spread from one infected individual to another, namely the initiation of an epidemic.

Most of this discussion will be centered around the use of BW against human targets. Similar principles would apply to attacks against crop animals and plants: in fact there may be fewer hindrances to such attack because of the specificity of the targets and the expectation that retroactive or collateral spread could be contained.

Fortunately there has been no historical experience of the use of BW on a significant scale in modern times; for that reason there is no empirically founded doctrine for the military use of BW, a fact which alone may be the main hindrance to its proliferation. By the same token this makes BW arms control a matter of the most urgent concern.

2. Treaty status: disarmament re actual weapons. # Agent stockpiles difficult to define or verify. # R&D of any kind not effectively covered. # Potentiality for quick breakout.

BW and CW are both governed by the Geneva protocol of 1925. De facto, the Geneva protocol is a no first use agreement, binding on the signatories with respect to one another. It does not prohibit the development, stockpiling, or transfer of CW or BW agents. It has therefore been enforced primarily by mutual deterrence based on the prospect of retaliation against a first use. Starting with the use of "poison gases" like chlorine and

phosgene in World War I, followed by lewisite and mustard gas, CW does have an actual history in military application and doctrine which has impelled most countries to demand a very high standard of verification as a precondition to further arms control in that arena. To the extent that BW was not tested in battle, the signatories to the BW convention of 1972 were willing to commit to disarmament of these weapons. Despite many uncertainties with regard to the verifiability of that disarmament they relied on shared motivations for cooperative verification as the principal method of enforcement.

The BW disarmament treaty prohibits the production, stockpiling, and deployment of biological weapons, defining these as living agents of kinds and amounts that had no peaceful application. Under this doctrine, research and development is not regulated since the testing of offensive agents on a small scale would be a necessary component of defensive developments. This circumstance leaves open the possibility of a technology race within the legal limits of the BW treaty. Such a race in BW weapons R&D has now generated much concern because of the potentiality of rapid breakout after R&D had been consummated.

3. Military utility of existing (known) entities: hardly for # tactical use: large collateral damage; danger of spread and # retroaction; unpredictability of damage effects.

To return now to military considerations in the use of BW, in contrast to CW or other weapons: since disease depends on extensive biological multiplication of a single particle, there will always be a latent period of days to weeks after exposure, before the target is impaired (possibly excepting massive doses of the noxious organisms). Compared to physical and chemical agents, biological agents depend for their efficacy on a complex range of interacting factors. Therefore, their actual performance applied to large population targets is poorly predictable. A given infection may have no consequence or it may initiate a large epidemic, spreading to many other individuals. New technologies might result in the development of organisms whose latent period is accelerated. Nevertheless, they are not likely to fundamentally alter the limited utility of BW for tactical purposes.

4. Role as strategic or doomsday weapon. # Similarity to nuclear winter; Alternative resort to counter SDI.

On the other hand BW could be used with great effectiveness in attacking large populations, in undermining the economic functioning of an adversary, in the chronic depletion of fighting capability of units in confined areas. BW would be especially devastating in combination with physical weapons that damaged the infrastructure, nutrition, sanitary and health

systems of a target country.

For the foreseeable future, the dangers of spread and retroaction from a strategic BW attack would give this the characteristics of a doomsday weapon, i.e. a last resort of threatened mutual suicide. This may not be fully understood by an attacking power, so the doomsday effect may be inadvertent; the attacking power might be misled into believing that retroaction could be controlled on the basis of half-way and hard-to-test technological innovations. On the other hand, prior to a demonstration of the controlled damage that a BW attack might deliver, this might be unrealistically under-estimated by the party under threat. Hence BW has many characteristics that will hinder a rational calculus between adversaries under stress, mostly in a direction that is full of risks of unintended consequences and crisis-instability.

With respect to retroaction, BW resembles the nuclear winter scenario, the threat of which has not noticeably affected the doctrine for nuclear arms buildup or ~~their~~ military use, ~~doctrines~~. This leaves modest optimism that the fear of retroaction will automatically restrain investment in and plans for the deployment of BW, were the treaty restraints to weaken.

Another gloomy prospect is that the successful development of defense systems against ballistic missiles, if shown technologically feasible, will add further motivation to the superpowers to develop alternative weapons that will enhance the destructive power of warheads delivered by other platforms.

5. Hazard from proliferation. Low cost of entry.

Nuclear powers armed with multiple delivery systems can deter attack without taking the risks of recourse to BW. The real hazard is proliferation, as the cost of entry into BW-capability is relatively low, so that less industrially advanced states, even small terrorist organizations or individuals could inflict great damage on a modern state. BW would probably find its greatest applicability as a clandestine sabotage weapon in the hands of desperate and irresponsible groups who felt they had nothing to lose.

Countries with poorly developed public health infrastructure are generally even more vulnerable, as they are to natural epidemics. (On the other hand some populations with poor sanitation may have developed immunity to some biological agents that would be more devastating to an economically more advanced,, country.)

Almost anyone with a fragmentary medical education would have learned enough to design and produce biological weapons of great destructiveness. After all, much of medicine is learning the characteristics of naturally occurring lethally infective

microorganisms. The technology of growing these 'germs' is also quite simple, apart from safety precautions that might be neglected, or obviated by immunization. Some individuals can always be found who have a solid immunity by having survived a prior natural infection with a given pathogen. Terrorist groups are also well habituated to clandestine modes of delivery.

It is a mistake to believe that new and sophisticated technologies are needed to practise BW. In 1346 the Tartars attacking the Venetian outpost at Kaffa (Feodosia) on the Black Sea concluded their siege by catapulting corpses of their own troops into the fort. The result was to bring plague into the Venetian defending force, which they carried back with them to Italy, introducing the great Black Death into Europe.

The recent use of CW in the Iran-Iraq war, in contravention of the Geneva Protocol, is a serious contemporary threat to the treaty restraints on proliferation that may also open the door to BW uses in warfare.

International cooperation to limit proliferation will be be very difficult, however, in a climate of mutual suspicion fanned by the continuation of clandestine R&D in military biotechnology, and in the absence of procedures for consulation and for confidence building.

6. Civil defense: low key essential

As almost always happens with hypothetical threats, the prospect of BW terrorist attack has not yet motivated ~~serious~~ uniform civil defense measures, e.g. the protection of municipal water supplies -- which in the U.S. has been left mainly to local officials, and is therefore quite variable from one city to another. One difficulty is to arouse considered responses, without generating undesirable publicity: none of us wishes to be responsible for inspiring the use of BW by a terrorist group. There is a formidable and useful challenge to technology to design more effective means of monitoring water supplies, and of ensuring that they are safe with respect to both naturally occurring and maliciously injected pathogens. Similar steps must also be considered for the food chain, and for atmospheric transfer.

~~# 7. Role of new technology: R&D congruent with biomedical research. # (secrecy as hallmark)~~

The added input of biotechnology is fairly small compared to the ~~revolution in modern politics and warfare that would follow the~~ introduction (even serious testing) of BW, whether with existing ~~or advanced agents.~~

~~In the near future the main application of new technology in~~ this sphere will be the production of more reliable prophylaxis,

protecting the attacker from retroaction to his own people, primarily by immunization. This is a mission parallel to that of peaceful biomedical research! This difficulty of ascription poses a most serious problem to the design of measures needed to build mutual confidence and to regulate R&D to guide it to peaceful purposes. The only hallmark of more fundamental research as peaceful is open publication:: even this is complicated by the intervention of commercial exploitation of biotechnology, which may be associated with trade secrets for limited periods of time.

It would not be fruitful to disseminate details of speculations about how BW might be made more militarily effective. Some that have been mentioned in past years include:

- Non-lethal but incapacitating infections (reducing total risks from retroaction)
- Shortening latent period after infection
- Agents better adapted to aerosol or other transmission
- Agents difficult to diagnose
- Agents difficult to treat with existing medications
- Agents designed to defeat physical protection or other countermeasures
- Agents expected to be specific against populations of defined genetic characteristics, or with prior exposure to specific dietary or other antigens.

Unhappily, past experience suggests that military users of new weapons will not always be sensitive to the long-range ecological implications of their use. An attacker may be optimistic about self-protection, but end up starting the evolution of an epidemic disease which becomes a disaster for all concerned -- the inadvertent doomsday mentioned above. It is hard to imagine any weapon more difficult to test properly than a BW intended to be controlled by immunization of the attackers.

8. US-USSR steps?

Threats to humanity from the further development and proliferation of BW are related to two closely intertwined issues, neither of which is sufficiently addressed by existing treaties:

a. The sparking of a bipolar technology race in biological weapons development, accompanied by deep-seated anxieties and suspicions within each country. These anxieties in fact

contaminate the entire atmosphere with respect to arms control.

b. Threats from the proliferation of BW capability and use to other parties.

It will be most difficult to deal with either issue if the other is not addressed.

To deal with a), we should find means to restore a cooperative framework of verification, to deal (for example) with the U.S. questions about the 1979 Sverdlovsk incident, and with USSR concerns about US biotechnology development. This should include a strengthening and exercise of the provisions for mutual consultation mentioned in the 1972 BW treaty. The forthcoming quinquennial review of the BW treaty would be an especially propitious occasion.

US-USSR cooperation in international medical research in infectious diseases would be an important way to bring the humanitarian scientific communities of the two countries in closer convergence, and improve mutual understanding of the objectives of microbiological research programs in each country.

For b), much more exploration and imagination is necessary; however nuclear non-proliferation is already a matter of shared interest on the part of the US and USSR, and may be a model for cooperative measures. Technology for civil defense may be an area of specific joint interest; this technology is substantially what is needed to improve public health in regard to old and new (cf. Legionella, AIDS) diseases of natural origin as well.

9. US-USSR CISAC role

For many reasons, including the need for specialized expertise, I acknowledge that this committee is not the place for detailed questionning about the substance of quarrels like those over the Sverdlovsk incident. I would suggest that Soviet members of CISAC use their own means of inquiry to inform themselves about that incident, so they can reach their own conclusions about the most practical forum for quieting the concerns that have arisen in the U.S. There has been a great deal of comment in the U.S. about these, and about allegations of secret military biotechnology R&D in the Soviet Union. It might be valuable for you to become informed about the temper of these concerns, again for you to reach your own conclusions as to their underlying validity, and about the best measures from your side to quiet them. I will express a personal opinion that the published explanations of 'Sverdlovsk' were not a scientifically satisfying report; and I have brought a copy of that for you to reach your own conclusions.

After you have had time to crystallize your own information and standing on these matters, I hope we can discuss them further. Meanwhile, I stand ready to furnish all available information to help enlarge your personal information and perspective about the concerns we have on this matter. I hope I have been able to convey to you that regardless of the underlying substance, the inability to get an open discussion of BW problems has been a serious source of erosion in public confidence about arms control in the U.S. Obviously we have a reciprocal responsibility to understand and discuss where your corresponding concerns are lodged.

Observations from Lederberg during his presence at the joint US/USSR CISAC meeting in Moscow in June 1985

Dated June 12, 1985

Attached is the text of the remarks I presented at the joint US-USSR CISAC meeting in Moscow, Thursday 6 June 1985. At their request, I also gave copies to Sergei Kapitsa and Nicolai Pavlovich Bochkov, and to the latter as well some of the backup material, including US news stories about Sverdlovsk etc.

At the meeting, Arbatov's response was evidently signalled by the article by Marshal Akhromeyev that appeared in Pravda June 4: there was no point discussing these technicalities when the US had not yet reached a political decision to accept equal security (parity) for the US / USSR. The emblem was SDI, which signified a US effort to achieve unilateral strategic superiority. (Previously, Sagdeev had presented an analysis of the dynamics of development of the SDI, the most likely stopping point of which would be a substantial but not comprehensive SD. At that point, he warned, the power with SD could not feel invulnerable against a first strike, but could feel safe in making a first strike, the SD providing a shield against the attentuated retaliatory force. He warned of the instability and escalation of fear this would eventuate.) Indeed, this was the main preoccupation of the meeting.

Arbatov made a point quite strongly that has concerned me, but which I preferred not to be the first to voice. If SDI is perceived as a step to superiority, and if it appears in any way credible, other powers [USSR] plainly will have to escalate their other conventional and unconventional warfare capabilities, including CW and BW.

After Arbatov, however, Batenin, Baev and Bochkov offered sympathetic and measured responses to my remarks, echoing my concern that BW would be a doomsday weapon, and expressing appreciation for my pointing out the hazards of horizontal proliferation. Baev asked why I was accusing the USSR of a treaty violation, when Sverdlovsk was a public health problem. I responded I had not voiced that accusation, but that the failure to provide satisfactory reassurances had left a very difficult evaluation on the US part; that we badly needed better forums for discourse. Baev and Markov responded that 1) I could get whatever information I needed from the public health authorities, and 2) that the US was primarily responsible for shutting off scientific discourse. They did not volunteer how to contact the public health people. Bochkov echoed my (and David Hamburg's) public health concerns about BW development.

In private conversation during and after the meeting (including at Bochkov's home), Bochkov stated that Soviet public health reports were generally as unsubstantive as what I showed him about the publication on Sverdlovsk, and I should not attribute too much malice to that one case. He offered to look for anything further that might have been published on the subject. I urged him to pursue his own information about Sverdlovsk, and he agreed he would do that. He urged me to stay in touch with him about this and other matters.

In some of these conversations, and at Bochkov's home, Kapitsa joined in. He is of course educated at Cambridge, has lived much of his life in his father's shadow, is interested in the culture of science in a depth that reminds me of Jacob Bronowski. He arranged, at short

notice, the taping of a TV interview (Kapitsa, Bochkov, Lederberg) that will be broadcast this fall as part of his educational series. This was an utterly serious discussion of the realities of human and medical genetics, that found Bochkov and myself in total agreement on matters like the genetics of IQ (but they don't test in the USSR - they know the schools differ vastly in performance), the prospects of genetic diagnosis for prenatal disease, the needs for protecting germ plasm against environmental chameicals, the general nature-nurture problem and so on. Bochkov's own national responsibility includes the organization of and training for genetic counselling; in many other ways it is congruent to my own professional role at Wisconsin (where he had visited for a semester in mid-60s) and at Stanford. So we were the most natural counterparts. He is the only M.D. on their CISAC - having been deeply involved in "Scientists/Doctors for the Prevention of Nuclear War", analogous to the US "Physicians for Social Responsibility". Bochkov is a member of the Academy of Medical Sciences, directs the Institute for Medical Genetics.

Scriabin, listed as a member of the delegation, is also a microbiologist but he was hospitalized for a heart attack.

At the meeting, Baev urged me not to take seriously the undocumented allegations from dissidents. I said that I took them only as allegations, but that in the vacuum of response it was impossible to discount them. Later, Kapitsa said he understood that.

I visited Ovchinnikov at his new institute a large part of Friday. It is the larger part of biotechnology in the USSR - a $300,000,000 (dollars!) construction budget, 85000 sq. meters; 100,000,000 roubles annual budget, superbly equipped. It was entirely open, with a number of students from Moscow University, no remarkable security barriers. I did not however see the P2 - P3 building myself-- I should have thought to press for that. He told me there were other facilities at Pushkino about 40 km. NE of Moscow, which had their animal facilities - for monoclonal antibody work etc. Baev also does much of his work there, and invited me to visit at a future visit.

Ovch. told me he had gotten Brezhnev's personal backing to modernize Soviet biology through molecular genetics, [fairly explicitly to get over the Lysenko blight] for its indispensable values for medicine and agriculture. He was meeting that morning with agri-specialists to be setting up programs for biotech programs in that field: they were going to focus on the molecular genetics of photosynthesis - I got a garbled reference to a particular stage. Their effort is limited by people, and the difficulty of getting the best of them to work in Siberia and other remote areas where they were the most needed. They have extensive programs of first rate molecular genetics, widely published, and of which they are very proud. I see no reason to rate them 'behind' the US or anybody else: we did compare notes on our views about technological competition from the Japanese -- we agreed they were superb in the integration of their teams. Ovch. agreed that one of their problems was correlation with (and in his view quality of) the application specialists in medicine and in agriculture. The Sov. Academy is making a major push at the domestic production of reagents and scientific equipment. As he said, about half of what I saw in the labs was Soviet (or eastern bloc); but there was lots of LKB, Mettler, Beckman instrumentation. In addition, he is going to set up a foreign equipment demonstration facility (he mentioned DuPont as a cooperator) at the institute. They are proud of having manufactured interferons, and that their products are in

clinical trial for cancer, and topically for herpes and for respiratory viruses. Likewise, biosynthetic insulin.

Ovch. and his colleagues made repeated references to scientific competition with western scientists in several areas, were proud of coming out ahead in several. They were a little apprehensive that Gobind Khorana (MIT) was going to scoop them in some membrane problem because he hadn't been talking much at international meetings lately.

The word I had received was that we should 'talk strictly science' but at lunch one of his colleages asked me what CISAC was for; and we had a lively discussion about arms control, SDI, etc. They did not seem well-informed at all; evidently have taken no part along the lines of Velikhov and Sagdeev. I summarized my presentation about BW proliferation. Several of them were incredulous that, say, an attack with typhoid would be as easy as I put out. I reminded that Leningrad water supply already had a problem with Giardia; and Ovch. said he agreed that home-brew typhoid would be easy. The group has no M.D.'s that I could see -- I don't recall, however, who is doing the hepatitis A vaccine work.

Ovch. spoke very articulately about the hazard of BW proliferation, that we should take every step to prevent microbes from being used as weapons, agreed with my demand for more discussion, said he would do everything on his part -- but again that most of the obstacles came from the US side. His colleagues knew nothing of Sverdlovsk, as an arms control issue. Ovch. said this had not been published in the USSR: [Even Science Magazine is censored; a few senior academicians, presumably including Ovch. have the privilege of uncensored copies]. Ovch. did not respond explicitly when I urged him to get authentic information himself; but he urged me to work with him to open up better scientific communication of US with USSR.

I mentioned to Ovch. (and to Baev - who had asked me where were the major US centers for DNA cloning vectors) that Goldfarb's detention, and "human rights" generally, were among the gnawing obstacles. He told me (as I had heard from other Soviet scientists) that he expected Goldfarb would soon be allowed to leave -- there were bureaucratic fowlups to which Goldfarb had contributed himself, and Goldfarb had evidently gone out of his way to antagonize Aleksandrov; but he [Ovch.] was going to straighten it out as a special case. But he thought we were misjudging the "human rights" issues. Scharansky, as far as he could tell had committed 'serious crimes', but Ovch. did not know what they were. In general, the USSR could not allow free exit, else everyone assigned an unpleasant duty would bargain to emigrate. When I asked about persecution of scientists, he said that there needed to be laws, and people who persisted in violating Soviet law were asking for punishment. His colleagues were fairly silent during this exposition, in ways that hinted they were not too content with his explanation; but only one asked the mildest of questions (about some tortured anecdote about disciplining a driver for nonfeasance). I persisted that I hoped the USSR could find a better way to deal with these problems - that they were serious obstacles to scientific communication. Baev's response to a similar remark was stony silence. Ovch. expressed a desire to continue to talk about it.

My bottom line is fairly gloomy. The new technologies that may reopen BW for the strategic conflict will come primarily from MEDICAL, not military research. During the next decade, it is US researchers who will be uncovering the biology of virulence and of host-specificity; and that publically available work will be capable of fairly prompt breakout by any side that has that intention. My 'demand' for better regulation of BW R&D matches theirs in the SDI field: we don't know feasible ways to monitor R&D. Secretive work certainly does speak to hostile (or defensive) intentions; and we would be better off were we able somehow to get to more open communication about work in the microbiological area. So we are back to CBM's; BW is an area where some good may be possible (and at least we should try to avert aggravations as we have seen in the past.) I believe this visit did communicate that message, but whether it will reach or influence real decision makers is problematical. SDI is much closer to their central concerns; as long as this remains such a burr under their saddle, they will give only marginal attention to cooperative alleviation of anxiety in the BW field.

Meanwhile the more urgent threats are from terrorism, state-sponsored or not. We did not carry conversations very far on how the US and USSR could cooperate in that field: we should be brainstorming some ideas on that.

For the US' own actions to counter terrorist BW, the best I can offer in the short run is a) as always, intelligence, and b) civil defense, which is badly neglected.

------ On a different subject, Gorbachev has just introduced restrictions on alcohol, which are taken quite seriously: they had a real impact on our official functions!

There was NOTHING in the mood of the scientists to suggest that they have the least anxiety that the USSR is collapsing economically. Their own budgets seem to be rising. Several did express the hope that Gorbachev would bring a modern outlook to the domestic economy, and seemed fairly optimistic about the future -- pace only their concerns about the deterioration of US - USSR relationships.

lederberg

DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138

July 23, 1985

Joshua Lederberg
President
Rockefeller University
President's Office
1230 York Avenue
New York, New York 10021

Dear Josh,

Maybe this is relevant to some of the Hmong accounts of illness and death. Are there any tropical medicine specialists at Rockefeller who might like to read it?

Sincerely,

Matt

Matthew Meselson

MM:lal
Enclosure

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE NEW YORK, NY 10021

JOSHUA LEDERBERG
PRESIDENT

SEP 16 1985

Dear Victor:

Our [CISAC] meeting in Moscow achieved every reasonable expectation (not to set that too high!) about reopening discussions with Soviet scientists aimed at bolstering the Biological Weapons Disarmament Treaty. As they have now expressed some receptivity to further discussion, we obviously have the obligation to firm up our objectives. The overarching aim of reducing the odds of biological warfare is easily stated. Translating that into concrete and feasible steps is much more difficult.

The most proximate threats of BW come from its availability to small states and terrorist groups -- the virtual "poor man's nuclear weapon" -- for clandestine attack on populations. How to cope with terrorism is already one of the most vexing contemporary problems: the potential aggravation from BW that much the worse.

This is compounded by the anxieties that stem from the limitations of the treaty, which do not deal effectively with military R&D, nor with the prospects of rapid breakout of civil biotechnology, which is advancing so rapidly today. Concern about continued Soviet investment in BW R&D, heightened by matters like the Sverdlovsk incident (1979), has been sharpened by the Soviets' categorical refusal to discuss the area. This stance makes difficult concerted measures on horizontal proliferation, and in due course may motivate a reentry by the US into military BW R&D.

My own imagination has been quite limited about what might be done; at the very least we might explore what measures could restore some degree of confidence about the bilateral problem. The Soviets' official position about Sverdlovsk has been that it was an anthrax epidemic of limited scope, originating in contaminated black-market meat. The biologists I have talked to have had no reason to doubt that version; nor have they had access to any detailed information themselves. They may (or may not) be able to help a process of more open discussion of that incident, which would be a confidence-building measure in its own right (even if Sverdlovsk was a seat of BW R&D -- whose proscription under the treaty is only the vaguest.)

We have such a complex blend of biotechnical and political issues that it would be indispensable to have a more extensive discussion before we continue the discussions with the Soviets (at CISAC or some derivative group). Would it be possible to convene an informal, mixed group to meet in Washington to help take stock before we (CISAC) proceeds further?

If you do call such a meeting, it would be fine with me were you to distribute any of the background that was given out to CISAC before the Moscow trip, including the "tutorial".

Yours sincerely,

Joshua Lederberg

PRIVILEGED Revised: 12/9/85

Summary Minutes
Ad Hoc Meeting on Biological Weapons
October 18, 1985
National Academy of Sciences
Washington, D.C.

An ad hoc meeting on biological weapons, sponsored by the Committee on International Security and Arms Control of the National Academy of Sciences, met in Washington, D.C. on October 18, 1985 from 10:00 a.m. to 4:00 p.m. Those present at the meeting, chaired by Joshua Lederberg, were: William Augerson; Albert Balows; Ivan Bennett; Philip Brachman; Paul Doty; Paul Marks; Mathew Meselson; Patricia Minard; Joseph Nye; Victor Rabinowitch; Thomas Reutershan; Walter Rosenblith; John Steinbruner; and Lynn Rusten (see attachment #1).

Lederberg began the meeting with introductory remarks about the Committee on International Security and Arms Control (CISAC) and its past activities, including biannual meetings with a delegation of the Soviet Academy of Sciences on problems of international security and arms control. Lederberg explained that the topic of biological weapons was an agenda item at the last meeting of the joint CISAC committees in June in Moscow. These discussions focussed on proliferation and confidence building.

Lederberg said the Soviets were receptive to continuing a dialogue on BW and to establishing groups of experts meeting separately from the regular joint CISAC meetings. Lederberg said CISAC would be drafting a specific proposal to send to the Soviets, but wanted to first hold this meeting to help define objectives and means of further communication with the Soviets.

Turning to the substance, Lederberg said the main bilateral issues with the Soviets were confidence building, Soviet compliance with the 1972 BW Convention, and the biotechnology threat to future arms control agreements, particularly with regard to research and development. As for terrorism, Lederberg said his main concerns were the danger of clandestine attack and civilian vulnerability to attack which could be very destructive using even a low level of technology in the BW field.

Lederberg posed several questions he hoped would be addressed at the present meeting: What can usefully be accomplished in U.S. - Soviet discussions with respect to the terrorist problem? How can we maintain and strengthen compliance with the BW Convention? Lederberg noted that the BW Convention does not cover research and development.

Lederberg said that one inhibition on the use of biological weapons is the difficulty of preventing their spread to the attacker. However, if one side had the prevention technology, it might be more inclined to use these weapons.

Lederberg said the issues of mutual confidence are very grave and asked for ideas on how to translate this concern into effective measures that both

sides would accept. Lederberg said he wasn't sure there was a solution to that problem. But, he said that opening up communication between the biomedical communities on each side so there is continual communication and greater confidence could help.

Turning to the BW Convention (attachment #2), Lederberg read Article I aloud, noting that developing defenses against bugs is considered a legitimate activity for peaceful purposes. Article V lays out the procedure for cooperation and consultation. Lederberg said that there will be a review conference in 1987, and that the Sverdlovsk incident and allegations concerning the use of toxins may come up in relation to this provision on consultation and cooperation.

Doty recalled that some of the evidence on the Sverdlovsk incident came to the attention of the U.S. delegation during a BW Convention Review Conference, so the U.S. felt it had to be raised. The Soviets started then to build a public case against the charges.

Steinbruner said it was important to note that the U.S. presented the Soviets with a public case without first presenting it privately, thereby making it a public confrontation. He said it is hard to know what the response would have been if the discussion had gone through private channels. Steinbruner suggested that it might be useful, as a confidence building measure, to try to explain to the Soviets why the U.S. reacted the way it did and went public.

Lederberg said the main Soviet violation was unwillingness to explain the event. Even admitting that spores were released from a military plant doesn't necessarily mean a violation occurred.

Marks suggested dropping the issue of Sverdlovsk on the grounds that we won't get anywhere with the Soviets if we keep trying to deal with the issue head on. Instead he suggested that a groundwork for better cooperation and communication in the future be established. He said Sverdlovsk was now a political issue in the Soviet Union, not a technical one, so he recommended dropping it.

Lederberg disagreed, saying it was important. He asked how it would be possible to lay the groundwork for confidence without resolving this.

Steinbruner suggested it could be possible to lay the groundwork by seeing what's going on at Sverdlovsk now. He said we might see that there is not huge stockpiling there now. Steinbruner noted that Gorbachev is using candor as a political tool. Steinbruner suggested presenting Gorbachev with a way in which candor on the Sverdlovsk incident would be least embarassing.

Doty remarked that any inspection of Sverdlovsk now wouldn't be informative about the incident in 1979.

Nye said that leading with Sverdlovsk would be a big mistake. He suggested instead starting with proliferation. He said the goal should be to set up an institutional framework to deal with proliferation and other issues, along the lines of the Standing Consultative Commission. Then we could look at why we failed to communicate clearly with each other on the Sverdlovsk incident. Nye said that trying to get the Soviets to publicly retract their story on Sverdlovsk is a loser.

Augerson asked what were the objectives of the National Academy of Sciences in meeting with the Soviets on security issues. He asked how these discussions would stay clear of official negotiations and discussions.

Rabinowitch explained that it is the policy of CISAC and its counterpart Soviet committee to stay clear of issues being directly negotiated, though he acknowledged that there is some occassional deviation from this policy. He added that CISAC is routinely and properly briefed by the government, and that CISAC briefs the government on its activities, particularly on its discussions with the Soviet group. Rabinowitch said it is an important outside private channel of communication of influential scientists on both sides. CISAC aims to understand, to educate itself, and to educate a wider audience - namely, the officials who make policy. CISAC does not make public statements; and its sessions are considered private.

Steinbruner added that the two governments are not dealing with the BW issue now, but that it will be an important issue for them in the future. It is important to lay the groundwork now for professional communication.

Augerson said some countries still are not signatories to the BW Convention, so this group should consider holding discussions with scientists from non-signatory states, like China. Augerson noted the many Soviet charges of U.S. activities in violation of the Convention. He recommended compiling a list of these Soviet charges in advance of a meeting with the Soviets.

Steinbruner said there are real advantages in starting with proliferation. However, one disadvantage is that bilateral issues and methods are more important to the Soviets. Therefore we should start a bilateral discussion with a bilateral definition of the issues, such as regulating U.S. and Soviet activities.

Nye said it was possible to deal with the proliferation issue in a bilateral forum. He said the issue of nuclear proliferation and suppliers was a good example of this. Nye suggested three goals: 1) increasing adherence to the Convention; 2) ideological detente; 3) quiet approach to other countries. Nye said this was a highly bilateral agenda that deals with proliferation.

Lederberg said to remember that our forum is scientific and medical, not the Politburo. He said discussions should take place at the technical level.

Steinbruner suggested telling the Soviets we want to develop mechanisms for scientific cooperation in this field.

Marks noted that the high technology of this field is not limited to the Soviet Union and the United States. He said other states could easily be brought in.

Steinbruner said the most powerful motive for the Soviets is the potential to have deep and longstanding scientific cooperation.

Marks concurred, but noted that there could be a problem with the private sector in the U.S. He indicated the private sector here might be wary of cooperating with a country that could become a world market competitor in biotechnology.

Meselson said there were arguments for avoiding a linkage between proliferation and cooperation, such as public perception. He said if you talk up the linkage, it can make cooperation sound like more of a threat. He cautioned against saying that the purpose of cooperation is to prevent the use and spread of biological weapons. He suggested instead promoting cooperation for its own sake.

Lederberg noted that all we have to offer are private discussions. He said he had received a benign "go-ahead" from people in the government to do this.

Meselson asked whether the Soviet Academy was the right group to go through for these discussions. He said he has had better discussions with non-Academy Soviets.

Lederberg said Bochkov was interested, and that he stands next to Chazov in the Soviet Physicians for the Prevention of Nuclear War.

Rabinowitch predicted that Ovchinnikov (Vice President of the Soviet Academy) will get involved once this gets going. (He is in charge of all biological facilities in the Soviet Union).

Nye said that when the Soviets want to get serious with CISAC, they'll bring in experts from outside the Academy as needed.

Steinbruner agreed and said if outside experts start appearing, this would be an indication that we were getting somewhere.

Meselson said it was important to find the right Soviet individuals and invite them directly.

Lederberg said we don't have access to the Soviet decisionmakers. One problem is that there is no internal Soviet constituency for BW arms control. Lederberg said he wanted to help create this constituency.

Nye agreed with this goal and noted that the pattern has been for things to become part of the agenda for Soviet elites after they have become important issues in the U.S.

Lederberg said our natural allies would be people involved in biomedical research in the Soviet Union who are working against these agents.

Steinbruner predicted that the BW tutorial Lederberg gave in Moscow last June would be taken seriously in the Soviet Academy and government. He said it may be a year or so before we learn what they propose to do. Steinbruner said we were on track to see if it's time to get a serious response.

Lederberg agreed with earlier comments that we shouldn't let Sverdlovsk ruin the talks. He suggested finding a different example, an alternative to Sverdlovsk, for confidence building. Lederberg said we could offer the Soviets access to what we are doing in biological research. We could find out what they want to know and whom they'd like to see to learn more. Lederberg said we needed to build a bigger constituency for BW arms control here too. He said it could be useful to discuss with the Soviets how R & D, production and compliance are monitored in this country too.

Doty said if there were an arms control agreement, Sverdlovsk could be an impediment to U.S. ratification. Something needs to be done about Sverdlovsk before the next treaty is made.

Meselson said a group like this one could show the Soviets how they could satisfy the U.S. on Sverdlovsk.

Augerson recommended caution in the desire to keep the channels of communication open. He said that appearing to participate in working this out could be sticky for this group.

Lederberg said we wouldn't go beyond any official posture.

Augerson said he was concerned about the radical difference in the openness of the two sides and how Sverdlovsk could even be resolved.

Meselson said the U.S. got out of biological weapons production unilaterally; maybe we could teach the Soviets it's in their interest too.

Brachman said he assumes the Soviets know we know what happened in Sverdlovsk. On a private level, the committee should let the Soviets know how it feels about the incident and then say that it won't discuss the issue anymore.

Meselson said he still thinks it's worth discussing with the Soviets alternative explanations for the incident. He said it could have been a batch of bad vaccines or something equally embarassing.

Balows said there is the example of Gruinard island which was used by the British to conduct experiments with Bacillus anthracis. It is still off-limits after many years because of anthrax spores in the soil. He suggested that we could try to get samples of spores from soils around Sverdlovsk.

Lederberg noted that this would require an extreme of cooperation.

Nye said it would be a mistake to raise Sverdlovsk. He said it was like Krasnoyarsk, on which, unofficially, you get less silly, but still contradictory, responses. Nye said the U.S. signed the BW Convention because we were already out of BW production and figured if the Convention constrained the Soviets only 10% it would still be worthwhile. Nye said this turned out to be faulty reasoning. It has hurt the arms control process. He said we need mechanisms to make Article V work. We need to look back at the past with the aim of finding ways to improve the consultation procedure.

Steinbruner noted that Lederberg has already raised the issue effectively with the Soviets. It's on the record and the Soviets have the opportunity to respond. He said this takes care of the public opinion aspect. The written record is clear. We don't need to harp on it any more.

Lederberg said we've let them know we won't forget about Sverdlovsk. He said it is the only example to date of Soviet behavior in response to a questioned incident and it is an unacceptable precedent. Research and development programs are the next challenge. He asked how we could regulate research and development in any sphere, especially in mixed use technologies such as this. He asked what we could propose.

Steinbruner said there was an analogy in the fusion program. He said both governments allowed an unusual degree of cooperation. He suggested we try to establish an area of research cooperation that plays an analogous role, one not of direct military importance.

Marks noted that this research is potentially interesting commercially. He said the private sector in the U.S. might not like this cooperation, because the U.S. and the USSR might be competing in the world market.

Lederberg said the Soviet Union needs an exchange of its people to work in our labs. He noted that Japan is a major competitor of the U.S., yet there are 1500 Japanese scientists at the National Institute of Health. He suggested that one hundred Soviets here won't make that much difference.

Marks said we might not be able to get the cooperation of some people in the private sector because they think they can't deal with the Soviets, unlike the Japanese.

Steinbruner said it works both ways, meaning that the Soviet Union represents a potentially big market for U.S. companies.

Balows noted that we can learn from the Soviets too. He said they have prepared some superior vaccines, and they have successfully aerosolized vaccines.

Meselson noted that Soviet hoof and mouth vaccine is exported. Marks said the vaccine was not yet perfected, so this could be a good area of cooperation.

Brachman remarked that the Soviets excel in forecasting the spread of disease.

Meselson asked whether any countries that buy vaccines from the Soviet Union have inspectors in Soviet vaccine plants. The answer was yes.

Bennett said we should follow up on this vaccine business. He suggested it would be useful to explain that we can gain something from the Soviets, such as their advances in vaccine technology. However, Bennett noted that the Department of Defense was about to clamp down on biotechnology transfer.

Lederberg raised the issue of who would be liable for the Soviet vaccine if it were imported. Balows responded that this entire issue of liability was currently working its way through Congress.

Lederberg suggested that while cooperation in vaccine development is possible, buying vaccines from the Soviet Union is less so.

Augerson said there is much to be learned from cooperating with the Soviets. For instance, what are the agents that terrorists might use? This could lead to cooperation on vaccines against these agents.

Lederberg raised the possibility of agricultural technology cooperation. He said he was talking about fundamental research, not applied, so that competition was not so much of an issue.

Nye asked whether we could regret opening up cooperation if in ten years it looks like the Soviets have a program for biological warfare.

Lederberg said there are probably people in the military who would say yes. But he wouldn't. Lederberg said the destructiveness of the current technology is already great, and it won't increase that much in ten years.

Meselson said the key thing is intent. That is what we have to affect, unlike for other weapons. He said it is not a problem of technological breakthrough, it is a change in the attitude of mankind, that we need to prevent.

Nye asked whether there was a military argument in favor of BW. He asked if there was a doctrine for a biological weapon that was debilitating but not lethal and for which there would be a vaccine for one's own troops.

Lederberg remarked that the thing to look out for is controllability, not greater lethality. He said there was still the problem of testing the performance of weapons and their vaccines.

Steinbruner said the Soviets are doing research in BW, but they haven't developed it as a serious offensive capability. He said we want to prevent these kinds of missions from developing.

Nye said he was trying to understand the rationale for the Department of Defense putting biotechnology on the COCOM list. He asked if it was designed to hold back Soviet economic development or if it was a real national security concern.

Lederberg noted that twenty years ago there was a doctrine for the utility of BW.

Augerson said the ability to make decisions in crisis could be impaired if decisionmakers all got sick at the same time.

Nye said if he were a military planner, he would go not for greater lethality, but for fast acting viruses as an augmentation to conventional forces.

Lederberg said there are agents today that come close. But, for the superpowers, as long as they have nuclear weapons, there are only marginal advantages to adding currently available BW agents. He said it would be more advantageous for the non-nuclear powers.

After a short break for lunch, Lederberg asked Rosenblith to report on the status of the Academy negotiations with the Soviet Academy on the exchange program. Rosenblith reviewed the history of the program, its partial suspension in protest over the treatment of Sakharov, and the decision to negotiate a new agreement. Rosenblith reported on the status and details of the negotiations, which are not yet complete.

Lederberg turned the meeting back to the subject of BW, asking what would be a useful agenda for discussion with the Soviets.

Meselson suggested we not say we want to stop biological weapons production. Rather, he said we should emphasize humane concerns. He said we should not cause the Soviets to want to make the weapons. He suggested starting with broad idealistic intentions, such as not using the life sciences for hostile purposes. In the area of cooperation, Meselson suggested visits to high containment labs and notification of disease outbreaks. Meselson said the Soviets currently report outbreaks of influenza. Brachman said they report other diseases too. Meselson said a U.S. National Academy of Sciences exchange person was in Sverdlovsk a few days after the outbreak is said to have started. Since there's this precedent, Meselson suggested trying to send someone there again.

Lederberg then introduced Thomas Reutershan, Emergency Coordinator of the U.S. Public Health Service, and Albert Balows, Assistant Director for Laboratory Science at the Center for Infectious Diseases, who were invited to give a briefing on the threat of terrorist attack with biological weapons.

Reutershan introduced Balows, who would talk about the vulnerability of the U.S. to terrorist attack. Reutershan said that afterward he would talk about how the U.S. would respond to such an attack and about a new system - the National Disaster Medical System - that is being put into place.

Balows said that one mission of the Centers for Disease Control (CDC) is to investigate outbreaks of infectious diseases through epidemiologic and laboratory work leading to the control and prevention of epidemics. In 1982, the CDC was asked to be able to respond to possible incidents that might occur at the recent World Fairs in Knoxville and New Orleans and at the Olympics in Los Angeles. He said the slide show and talk he would give were an outgrowth of that assignment.

Balows said that in trying to determine what agents a terrorist might use, the CDC group looked at variables such as the stability of the agent, preutilization tests, the ability to produce or procure the agent, criteria of agent selection from the point of view of the terrorist, and the desired objective of the terrorist. The group considered different types of agents, their applicability to the criteria and methods for their delivery, and then prepared to investigate and identify the agents that most likely would be used in a given incident once the field investigations had been done.

Balows said the group created different scenarios and conducted mock exercises for dealing with different types of crises. Precautionary actions were taken at the Olympics, including regular checks of outpatient clinics for indications of increases from the norm of infectious diseases. In response to a question, Balows said the threat level in terms of numbers of people affected was open-ended.

Meselson said he wished to emphasize the uncertainties and difficulties of executing a successful attack. But he agreed that the psychological effects, such as panic, could easily be achieved. He said it was important to maintain the idea that biological weapons attack is not dependable, that it could fail.

Augerson said Meselson puts too much weight on the idea that the military or terrorists make decisions on the basis of the reliability of weapons.

Lederberg stressed the importance of discussing these issues quietly so as not to educate potential users. He said water supplies are very vulnerable.

Nye, returning to the issue of motives, said terrorists were more interested in publicity. He suggested that BW is still viewed as illegitimate, and therefore might not gain sympathy for the terrorist.

Lederberg said it was important to distinguish between the technical threat and motives. He said he thinks the technical threat is great. It is relatively easy for someone to do this.

Meselson said he thought it was conceivable but did not see it as a great threat.

Doty raised the issue that we are considering collaborating on this problem with a country that supposedly trains terrorist groups.

Steinbruner asked if this were so, why have there been no successful missions against the U.S.

Lederberg, referring to a Rand report by Brian Jenkins, said he thought BW could be very effective even for theatrics.

Marks said AIDs was a good example of a disease that has elicited an out-of-control emotional response, with small impact on public policy. He agreed that a public threat of the use of biological weapons could throw a community into panic.

Steinbruner said terrorists have an incentive to restrain what governments will do to go after them. He suggested a deterrent would be to demonstrate that the government would go all out after someone that used biological weapons. Steinbruner said the thing to worry about is people to whom it is a form of warfare, who want it to be destructive. Steinbruner said this posits a very sophisticated operation that might show some traces. If the U.S. and the Soviet Union were cooperating against it, an organization like this would have a hard time. Steinbruner said this sort of cooperation would be useful to discuss with the Soviets.

Nye said the Soviets are generally more worried about interstate terrorism, while the U.S. is more worried about terrorists. However, there is more symmetry in the case of BW because of its transportability and the chance that it could spill back into Soviet society.

Responding to Doty's earlier comment, Steinbruner said the evidence indicates that the Soviets give only standard military training to terrorist groups. They get their terrorist training from somewhere else.

Augerson said the concern is not Soviet sponsored terrorists, but someone like Iranians, whose intent is to do damage. Signatures from these activities might not be easy to discern.

Minard said there is growing attention to the importance of the second tier of suppliers . She said it was important to focus on the fundamentalist terrorist threat.

Nye delineated serveral types of terrorists: the classical terrorist, who wants attention; and kamikazes of two types - "disorganized" and state-supported. He said the greatest threat is that of the "free-lance" kamikaze. But the greatest threat in terms of numbers of people to be killed is the state-supported kamikaze. Nye said the Soviets have an interest in both kamikaze types because they are both too uncontrollable.

Steinbruner added that this all presupposes a discussion with the Soviets that assures us that they share our interests.

Lederberg then turned the discussion back to Reutershan, who discussed how the U.S. would respond to natural disasters and possibly to such an attack on civilian populations. Reutershan said the Centers for Disease Control is the lead agency to assess the nature of the occurrence, to work with local health officials on how to control and prevent spread of disease and how to treat people, and to supply needed anti-toxins, etc.

Reutershan said he was the director of the National Disaster Medical System, a new joint venture of the Department of Defense, the Federal Emergency Management Agency, the Veterans Administration and the U.S. Public Health Service. He said it is a plan to have available a national network of 100,000 hospital beds, civilian and medical disaster teams, and patient evacuation by air in event of a medical disaster. It is a system that relies on existing resources.

Reutershan said he was concerned about the threat of a terrorist incident and welcomed remarks or letters from this group on the new system.

Lederberg asked if managers of municipal water systems were being educated about the possibility of intentional contamination of water supplies.

Reutershan responded that there are classified discussions going on to assess and detect threats to the water supply. He said the government was very concerned about these potential situations. He added that the Army Corps of Engineers has responsibility for the safety of water supplies.

Meselson said we should suggest to the Soviets sharing information on terrorists that may be developing biological weapons.

Steinbruner suggested putting cooperation up front so the Soviets know we're not trying to pressure them with a technological advantage, as they think we are doing with the Strategic Defense Initiative.

Lederberg said a real danger was a state or regime that was declining (a Qaddafi or Castro), which could result in a breakdown of disincentives and the ability to do a lot of damage.

Augerson mentioned that Castro has charged the U.S. with using biological warfare against his regime. Castro has blamed the U.S. for some crop failures and outbreaks of disease.

Nye asked to what extent export controls could slow down biological warfare capabilities.

Meselson referred to a relevant bill in Congress - H.R. 187. He said the technology of BW was getting smaller, and therefore harder to control.

Minard said the suppliers groups on Iran and Iraq were concerned with this legislation.

Meselson, raising the issue of liability, wondered if a company that knowingly supplied even unrestricted equipment for biological weapons purposes could be fined or penalized.

Minard said this leads us back to the problem of verifying research and development.

Meselson said it should be illegal for him to go to Libya and make a biological weapon but he doesn't think it is.

Returning to the main task of the meeting, Lederberg asked each participant to give his or her views on what should be discussed with the Soviets.

Brachman said Sverdlovsk is the natural take-off point, from the perspective of prevention in the future.

Balows suggested the reestablishment of exchange scientists at all levels with the Soviets.

Minard recommended focussing on the third world. She expressed mixed feelings about scientific exchange, because she was concerned about restricted U.S. access to Soviet labs.

Steinbruner suggested the topic of the policy implications of modern biological weapons.

Nye said the goals should be to develop a constituency in the Soviet Union for biological weapons arms control, to discuss our common interests in this common threat, and to persuade the Soviets that a regular format for discussion is necessary.

Meselson said that just talking was a useful and realistic goal for now. He said we should not give up on Sverdlovsk, and that U.S. public opinion won't let it fall into history. But, he thought it should be dealt with privately. Meselson also liked the idea of scientific exchanges and said he would be interested in seeing more of their techniques in high containment work.

Doty suggested that the American group devise alternative versions of the BW Convention, setting out, for example, what an SCC-like mechanism would do. Then it could consider bringing forth the end product as an agenda item with the Soviets.

Auguerson listed as topics to discuss with the Soviets: proliferation, how to protect publics, and means of cooperation in evaluating ambiguous or apparent BW events elsewhere in the world.

Marks recommended confidence building measures and preventing proliferation to third parties.

Augerson said the Soviets have interesting knowledge to contribute in the areas of aerosol vaccines and epidemiological prediction.

Brachman agreed that it would be wise to approach the Soviets through their strengths.

Lederberg said he was sorry that Hilary Koprowski and Robert Channock could not attend the meeting. He asked everyone present to send him names for a roster of people that would be good resources on this material and perhaps who have had contacts with the Soviets. Lederberg said he would also welcome names of appropriate Soviets for these discussions.

Marks mentioned a standing committee on international affairs of the Institute of Medicine as a possible resource.

Doty suggested getting the views of recent Soviet emigres in this field.

Augerson asked if Lederberg was considering the formation of "Biologists Against Biological Weapons." Lederberg said he was thinking about it. Meselson suggested making it Biologists <u>for</u> rather than against something.

Minard wondered whether the Soviets believe we stopped making biological weapons at Detrick and how we could demonstrate this to them and vice versa.

Steinbruner said we should not preclude getting an arrangement of that sort.

Lederberg mentioned some of the sensitivities in communicating with Ovchinnikov, but stressed the importance of gaining his support for this bilateral dialogue on BW. He said the next step was to draft a cable to the Soviets proposing a bilateral meeting.

The meeting adjourned at 4:00 p.m.

7/16/86

PRIVILEGED

Summary Minutes
Planning Meeting on Biological Weapons
June 23, 1986
Rockefeller University, New York

A meeting to plan for future discussions of a subgroup of the Committee on International Security and Arms Control of the National Academy of Sciences with Soviet scientists on biological weapons took place at Rockefeller University on June 23, 1986, from 10:00 a.m. to 2:30 p.m. Present at the meeting, chaired by Joshua Lederberg, were: Ivan Bennett; Paul Marks; Alexander Rich; Theodore Woodward; Benjamin Tua and Lynn Rusten.

Lederberg began the meeting with a short review of the Committee on International Security and Arms Control's discussions with its counterpart Soviet group, and the history of the introduction of BW issues into those discussions. Lederberg explained that by stressing the problems of the potential for an all-out technology race in this area and of proliferation and third party use, he had elicited a more productive Soviet response than by focussing on the issue of Soviet non-cooperation in connection with the BW convention. He said the Soviets have become progressively more interested in the subject, leading to their agreement to hold separate BW discussions with a group of American scientists.

A discussion of probable dates for the Moscow meeting resulted in agreement that September 29 and 30 would be possible for everyone present, and that the staff should communicate this to the Soviets and confirm the dates as soon as possible. Information on visa application and travel arrangements should also be sent to the participants as soon as possible. It was tentatively agreed that everyone would arrive in Frankfurt (or maybe Geneva) on Saturday, September 27 in order to meet, and then fly together to Moscow on Sunday.

Lederberg said the purpose of this meeting was to plan for the Moscow meeting and decide what could be accomplished with the Soviets. He said he felt gloomy about our ability to have an impact on the course of events and that even with good will, it would be difficult.

Bennett asked whether the CISAC delegation should go into the meetings with the Soviets with the assumption that their programs were civilian, even though Bennett said he was reasonably certain this was not the case. He asked how they should handle the issue of Sverdlovsk.

Lederberg said that the Soviets had an out with Sverdlovsk because it could be explained as an accident that occurred in a facility engaged in activity permitted by the BW convention; but that

the Soviets had failed to offer a satisfactory explanation of the event. Lederberg said that the objective of discussing BW with the Soviets was to get Soviet scientists like Bochkov more sensitized to what was going on in this field and to create a constituency for a BW arms control regime.

Lederberg said he thought the Soviets had signed the BW Convention in part because the U.S. had renewed relations with the PRC and the Soviets did not want to feel isolated. He said the Soviets may have some anxiety about U.S. activities and that this group could suggest a more open system that would alleviate anxieties on both sides. Lederberg said that improved communication about what actually was going on was the goal, and that the carrot to accompany the confidence building measures was better access to biotechnology. He added that human rights, particularly the case of David Goldfarb, could not be ignored.

Woodward said that he had reviewed the minutes of the October 18 meeting on BW and agreed with Meselson's statement in those minutes that it would be a mistake to open the discussion with the Soviets with the Sverdlovsk issue. Woodward raised the question of whether there were existing U.S.-Soviet cooperative programs in biomedicine.

Tua responded that he did not know. He said he would say a few words about his background and then brief the group on the State Department approach to the upcoming BWC Review Conference. Tua said he was a Foreign Service Officer and had served in Moscow for two years. He said he has been working on BW for about a year. He emphasized that his remarks were not to be taken as official policy, but as personal views of someone who was involved in the policy aspects of BW issues.

Tua said he found the minutes of the October 18 meeting interesting, and noted that since then there had been a change in the environment due to the November summit. Tua recalled that the summit statement included two sentences on CW cooperation, and noted that CW and BW are linked in the common mind. Tua said the communique called for intensifying bilateral efforts to control chemical weapons and for initiating a dialogue on CW proliferation. He said there had been two rounds of U.S.-Soviet discussions on CW, and that there would be another round in July which both he and Bob Mikulak from ACDA would attend. Tua said the atmosphere at these meetings was good, and that the Soviets seemed interested in progress. He said Israelyan headed the Soviet delegation.

Lederberg asked whether the Soviet interest in non-proliferation was related to a ban on chemical weapons in Europe. He said it was logical that the Soviet position would be that part of a non-proliferation program would include pulling the chemical weapons out of Europe.

Tua noted that NATO presently had unitaries in Germany which would be taken out and replaced by the binaries. He said a speech by Israelyan on April 22 at the CD did address on-site inspection and verification in connection with the destruction and elimination of stocks and with production facilities. Tua said the big question was that of challenge inspections, which the U.S. wanted and the Soviets opposed. Tua said the Soviets may accept a compromise British or non-aligned position. Tua said there had been one meeting on non-proliferation last March in Bern and that the U.S. was keeping these talks separate from the CW control talks, emphasizing that by holding them in separate cities.

Tua said that last January the Soviets had imposed export controls on some chemical precursors. He said the U.S. is doing that with regard to Iran, Iraq and Syria. He said the Warsaw Pact countries have imposed their own controls too. In response to a question about the activities of the West European countries, Tua said the U.S. has been trying to sensitize its allies to the problem of exporting these chemicals. Tua said there would be another round of U.S.-Soviet talks on non-proliferation in September in Bern. He noted that the Soviets were careful to get credit in the CD for the things they were doing in these bilateral talks. He said the Soviets were talking much more about verification and that Gorbachev's tenure may bring profound changes in Soviet policy and society.

Turning to the BW Convention Review Conference scheduled for September 8-26, 1986, in Geneva, Tua said there had been a preparatory committee meeting held in Geneva at the end of April. He said the Soviets took the position that an East European representative should chair both the preparatory meeting and the review conference.

Rich requested that Rusten prepare a short briefing book on the BW Review Conference for the CISAC delegation to study at their meeting in Frankfurt just before entering Moscow. Tua said Rusten could probably be briefed on the conference by Bob Mikulak at ACDA and David Jones in Tua's office. There was some discussion of whether the CISAC group should stop over in Geneva instead of Frankfurt in order to be briefed by U.S. participants in the BW Review Conference.

Marks asked what would be the main issues at the Review Conference. Tua responded that the U.S. posture would be critical but constructive. He said the U.S. was concerned about Soviet activities in Southeast Asia, the Sverdlovsk incident, and Soviet facilities that have high security and from which comes no published research. Tua said the U.S. has concerns that are covered by the Convention and which the Soviet Union has not allayed. He said the U.S. will not propose verification amendments to the Convention because it takes the position that the Convention does not have verification provisions and cannot be made meaningfully verifiable. However, he said it represented an important international norm that

Bennett said the point was to build a structure where people would be able to discuss these problems after a broader dialogue has been established. Rich referred to the NRDC-Soviet Academy agreement on seismic monitoring and said that the Soviet biologists might want a similar CBM in their field.

Lederberg said the Soviets were most intent on biotechnology, and that this must be coupled with work on infectious disease and control. He said we would then have a framework with a dual purpose.

Marks said we should think through a limited number of areas where we could learn from the Soviets, and then define the programs in terms that could be implemented. Lederberg said we could propose a workshop on infectious disease and public health.

Rich said that if this group is successful in creating confidence building measures, this could make people breathe easier about biotechnology transfer. He said cooperation in epidemiology is a possibility because the Soviets keep good records and access to these would be useful. Lederberg said he was not so sure and gave as an example the Soviet report on Sverdlovsk that appeared in the Journal of Microbiology.

Marks said the Soviets keep better data on cancer now, but there are indications that they have even better data than they are publishing. He said that epidemiological data from the USSR is bound to be interesting comparatively.

Rich noted a bureaucratic problem posed by the structures of the Academy of Sciences and the Academy of Medicine of the USSR, which are not easily mixed. He said the idea was to make clear to the Soviets that progress in CBMs would lead to cooperation in areas they are interested in, and that the function of this group was to work on CBMs.

Marks said the CBMs must be related to the science, such as in epidemiology. He said that through this dialogue, we might be able to identify good projects. Lederberg said this group could talk about further steps and then pass the suggestions off to other committees to work out. Rich gave as an example the possible role the Academy may play in studying Soviet populations affected by Chernobyl.

After a lunch break, Tua asked whether advances in biotechnology were radically changing the situation. Rich said it would take decades before we could do radically new things as a result of biotechnology advances. Lederberg said that one should not take confort from this because existing organisms were dangerous enough. Lederberg said the main danger was the more efficient production of vaccines for self protection against existing organisms. Bennett said there was also the danger of military use of toxins, which can be produced more quickly.

Lederberg said he worried about a USSR-PRC competition in BW, and said he thought that was why the USSR was staying in the BW game. Tua noted that the PRC signed the BW Convention sometime after 1980.

Lederberg asked whether this group should bring up Chernobyl with the Soviets, and everyone agreed to wait for the Soviets to raise it if they wished. Rich said a cooperative Chernobyl study would be a confidence building measure. Marks said this study would be very complicated scientifically. He said it would be hard in the Soviet Union to put in place the skills and resources necessary for a properly done study. He noted how hard this had been for Three Mile Island. Marks said they should start thinking about what they are interested in in terms of BW.

Lederberg said there were structural problems arising from the advance of microbiological science, which was that research and development was subject to potential breakout on short notice. He asked what could be done about that.

Bennett said this raised the problem of the potential for terrorism and small countries exploiting this technology. Rich said that, when talking with the Soviets, one gets a freer conversation by projecting ten or so years down the road. Rich said it would be good to tell the Soviets that their current stress on verification is good, and to get them to think about how this would be applied to BW, perhaps by using international on-site inspections. Lederberg recalled that Velikhov had introduced him to Dobrynin and they had a brief discussion about BW in May 1984. Rich said they should explore with Sagdeev the possibility of meeting with Dobrynin when in Moscow this September.

Lederberg said it was essential to discuss human rights when discussing confidence building with the Soviets. He said he would raise the issue of David Goldfarb, especially since the Soviet Academy position is to allow him to emigrate. Rich agreed it was important to reiterate to the Soviets that not letting him go was an irritant. Lederberg asked Rusten to find out whether there were other human rights cases involving Soviet biomedical people.

Woodward asked what would happen if the dialogue leads to a concenus to develop a joint biomedical program. He asked whether they should develop ideas for areas of cooperation, such as anthrax and AIDS. Lederberg said they could also discuss modalities of testing and dissemination, including aerosolization. Lederberg said it was also time the U.S. and the USSR cooperated in a third world oriented program, such as to develop a vaccine program for the third world.

Lederberg asked for a final word from everyone present. Rich said that, to summarize, the focus of the discussion with the Soviets should be on confidence building measures. He said the agenda should be to discuss what things we should have in place to prevent us from getting into trouble when these advances in biotechnology could cause trouble, things that would be of value ten years from now. Rich said

we should flesh out our menu of CBMs which could include verification and on-site inspection, joint epidemiological research studies, and more generalized CBMs, such as in the human rights area.

Marks said it was important for this group to do its homework and gather all the information it has requested. He said it was most important to enter the discussions with a flexible idea of what we will get out of it, and that we shouldn't expect a product from two days of talks. He asked whether Lederberg could communicate to the Soviets the flexibility of this American delegation's approach to the talks.

Bennett said he agreed and that it was a long term endeavor. He said just socializing and getting acquainted served a useful purpose.

Woodward quoted from Lederberg's BW tutorial: "U.S.-USSR cooperation in international medical research in infectious diseases would be an important way to bring the humanitarian scientific communities of the two countries in closer convergence, and improve mutual understanding of the objectives of microbiological research programs in each country." Woodward said he agreed with this and said it should be a long term dialogue and scientific program, and that they should also talk about proliferation.

Rusten agreed that the September meeting should be seen as the beginning of an on-going dialogue along the lines of the regular CISAC meetings on arms control. She noted that the arms control dialogue has been successful in part because of the longevity of the activity and the relationships that have developed over six years, and that this could be the beginning of a similar process in the BW/biotechnology field.

Tua noted that the timing of the September meeting was interesting, coming at the end of the BWC Review Conference. He said the prospect of a second summit could also influence the atmosphere.

Lederberg said he wanted to discuss the potential for BW proliferation and terrorist use with the Soviets. Bennett agreed that this should be mentioned as something the U.S. and the USSR need to be prepared to cope with. Rich said there was a precedent for consultation in the recent U.S.-Soviet agreement to consult on terrorist use or access to nuclear weapons technology.

The meeting adjourned at 2:30 p.m.

Lynn Rusten

Action Items

1) Rusten: Communicate with Soviets on dates (Sept. 29 & 30) and confirm with participants ASAP.

2) Rusten: Send visa application forms and travel information to participants ASAP, including arrangements for a European stop and meeting. Determine whether stop should be in Frankfurt or Geneva.

3) Rusten, Rich, Bennett, others: Research Existing U.S.-Soviet cooperative programs in biomedicine, including NAS, NIH and U.S.-USSR Joint Commission on Scientific Technology.

4) Rusten: Prepare a short briefing book on the BW Review Conference to bring to Frankfurt based on news clips and information gathered from Bob Mikulak at ACDA and David Jones in Tua's office. Also, arrange for briefing on this from U.S. embassy in Moscow on Monday, September 29.

5) Rusten: Gather information in the open literature on suspicious Soviet facilities, including Shultz-Haig report from the early 1980's, ACDA publication on Soviet Compliance, and Soviet Military Power.

6) Rusten: Learn status of Academy involvement in Chernobyl population study.

7) Rusten: Check with human rights committee to see if the Academy is aware of human rights problems involving Soviet biomedical people in addition to David Goldfarb.

Olga.
Burgasov.
Bezdezhnykh.

September 12, 1986

MEMORANDUM FOR THE RECORD

SUBJECT: Discussion with Dr. Matthew Messelsen, Harvard University

Dr. Messelsen had recently (about 23 August 1986) returned from a visit to Moscow, Ministry of Health (3 days) where he met with four doctors all of whom were involved in treating patients during an outbreak of anthrax in Sverdlovsk in April 1979. He was invited by and his trip was fully supported by the USSR Ministry of Health.

Dr. Messelsen did not visit Sverdlovsk, he was briefed only in Moscow. He opened his discussion with the statement that anthrax is endemic in Sverdlosk (attached papers Nos 1 & 4). He further stated that over 2 million people are innoculated against anthrax each year (mostly butchers) with a live encapsulated strain. Immunity develops in 12 to 14 days and lasts for 1 year but does not preclude cutaneous anthrox.

With regard to Sverdlosk, he stated that a large outbreak occurred in the spring of 1979 with 96 cases and 64 deaths. The first case occurred on April 4 and the last on May 18. Eleven cases were cutaneous, six were cutaneous leading to systemic and all 17 survived. Of the remaining 79, all were gastric anthrax cases and 64 died for 81 percent fatalities.

He was told that 1979 was an unusually wet year and fodder for animal feed was short. To increase meat production, fodder was supplemented with meat bone flesh powder (assumed to be bone meal by U.S.). He explained that anthrax cases have been identified as caused by bone meal. In this case there are about 11 producers of bone meal. One facility 16 km south of Sverdlosk produced one 29 ton lot which was responsible for this outbreak. The lot was put on sale on 25 March and all sold by 29 March to private animal owners. Anthrax was isolated from this lot of bone meal powders and victims and by capsule morphology and virulance in white mice was ascertained to be the same microorganism. Sheep and some cows were infected and he estimated that hundreds to thousands of people ate the contaminated meat. The animal sites were destroyed by open air burning of the buildings.

In addition 300 street dogs were sacrificed with two dogs infected. Chloramine was used extensively as a decontaminant. Leaflets, dated 14 April (attachment No. 6) were distributed warning of risk of contracting anthrax from consumption of uninspected meat.

According to Dr. Messelsen all patients from this outbreak were treated at one hospital, City Hospital No 40. The incidence of admittance to the hospital was as follows beginning on 4 April and ending on 18 May: 5/5/5/7/7/6/10/9/7/5/4/4/1/0/0/1. In addition 67 percent of the cases were male, 70 percent lived in the southern half of Sverdlosk, and most cases (32) were between the ages of 40 to 49. No cases occurred in the same household, no cases involved restaurants, and there was no military involvement. Thirty percent of cases involved workers from a ceramics factory which employed about 3000 workers. Between April 8 and 15 cattle carcasses from private sources were received and offered for sale at the factory to the workers. Only one carcass was infected with anthrax. He further stated that the incidence of disease was higher in truck and auto drivers who were thought to be more mobile and thus able to buy meat first.

As regards symptomology, Dr. Messelson reported he was told blockage of the intestine was the first sympton follwed by chills, high fever (39°-41°), dypnesia, rapid pulse, cynosis, and death. In some cases the temperature dropped rapidly immediately before death. All victims reported some stomach pain. The first diagnosis was made on April 7 and the first laboratory test on April 10. Extensive treatment was reported, mostly with a wide variety of broad spectrum antibiotics which included penicillin and serum fractions. All 96 cases were thought to be from animals who ate contaminated bone meal. Although he had no autopsy information he had been provided one photograph of a victim which he had turned over to government officials. He was told autopsies indicated hemmorage in the intestine, swollen intestine with severe hemmorage all over, lymph nodes hemmoraged, and lungs free of hemmorage. Some victims died at 48 hours after onset of illness; some lived up to 4 days. The causative organism was reported to be a thick capsule strain of anthrax. Survivors all felt fine two years after illness. No information was reported on burial or treatment of bodies of victims.

He reports a list of all foreign visitors is available and provided a trip report (attachment 7) by Prof Donald Ellis (which has been available in the U.S. for several years). Professor Ellis reports no unusual activity in Sverdlosk even though he reportedly arrived on 6 or 7 April, stayed about one month, visited Novosibirsk for one month, then returned to Sverdlosk for another month. Dr. Messelson also was provided in Moscow a phamphlet which was supposedly distributed in Sverdlosk at this time (attachment 6).

Dr. Messelson is planning three actions: a follow on visit of specialists from the U.S. to Moscow; the four doctors he had discussions with will be brought here to lecture; and that a scientific paper they are preparing will be translated and he will arrange for publication in an American journal.

Lederberg

Personal Memo from
JOSHUA LEDERBERG

Matt Meselson

SEP 25 1986

Sverdlovsk

Thanks again for the special effort you took to come down on Monday!

I've briefed Bill Casey that he has to take your account seriously; and I'll see Ken Adelman next week. I'll also seek what I think will be positive encouragements about the visit to US we talked about.

The press says that the Russians offered to talk about Sverdlovsk at the BWC review conference. There seems to be some problem about deciding on the most appropriate forum.

Of course I want to stay in touch.

Joe.

THE ROCKEFELLER UNIVERSITY NEW YORK 10021

Lederberg, J.

Matt Meselson

SEP 25 1986

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JL

(2)

OCT 23 1986

Memo From JOSHUA LEDERBERG

Subject: Anthrax Epidemic in Sverdlovsk 1979, and Soviet Compliance with the BW Disarmament Convention | CISAC-Moscow Oct. 8 '86

The Biological Weapons (BW) Disarmament Convention went into force in 1972. Since that time a number of incidents have raised questions about Soviet compliance. One of the principal incidents that has hindered confidence in this respect was an outbreak of anthrax which occurred in March 1979 in Sverdlovsk, USSR. According to emigre reports the outbreak stemmed from an accident in a military facility doing illegal work on BW. By their account an airborne plume of anthrax spores caused a widespread epidemic of pulmonary anthrax among military and civilian personnel in Sverdlovsk. Until recently, despite numerous demarches by the United States Government, the Soviets have refused to discuss this incident in any detail, claiming that it was a public health matter of purely internal concern and therefore out of the purview of the obligations for consultation under the BW Convention. The Sverdlovsk incident, and especially the Soviet refusal to consult about it, has remained high on the list of alleged treaty violations on the part of the Soviet Union and has figured in several policy statements by senior US officials.

The same subject has been on the agenda of the National Academy of Sciences Committee on International Security and Arms Control (CISAC) for a number of its recent sessions. This committee meets jointly with peers from the Soviet Academy of Sciences. During the past 18 months there has been an apparent increase in flexibility on the Soviet part and they had agreed sometime ago to convene a meeting in Moscow. This was held October 8th through 10th 1986; by design experts were available for further discussion.

The Sverdlovsk epidemic also was briefed by Dr. Antonov at the BW Convention Review Conference held in Geneva the latter part of September. Finally the Soviets invited Dr. Matthew Meselson of Harvard University, to visit Moscow for three days at the end of August, and provide him a very detailed briefing on their version of the 1979 epidemic. These moves seem to be related to 1) a general amelioration of Soviet policy on the release of information about catastrophic events: a policy that was tested and in due course complied with after the Chernobyl tragedy. 2) the looming review conference on BW and concurrent talks on CW arms control.

Attached herewith are some documents pertaining to the 1979 Sverdlovsk event as portrayed by Soviet sources, and my personal comment on them based on our CISAC visit to Moscow in October. They now detail that epidemic as intestinal anthrax, attributed to the distribution of contaminated beef. Many cattle were fed a bone meal supplement that had been improperly sterilized and had been produced from naturally infected carcasses. Anthrax is known to be endemic (or rather enzootic) in the northern Soviet Union and has been a low level but recurrent problem among cattle and other livestock. Sporadic cases of cutaneous and intestinal anthrax have been reported over many years from the Soviet Union. The 1979 outbreak is by far the largest and most vicious epidemic

so far recorded. The scope of the epidemic is attributed to the fact that there was a primary focus of many infected cattle and the illegal distribution of beef from these carcasses without proper veterinary supervision.

My personal conclusion from that evidence, taking account of other evidence from US sources, is that the present Soviet account of the epidemic is plausible on its face and internally consistent. The contrary testimony of the emigres is for the most part hearsay. While it may be given in good faith it can hardly be said to be grounded on a professional understanding of the different forms of anthrax. Wild rumors do spread around every epidemic; and this one conjoined with the widely held belief that the military contonment No. 19 is a BW development or production facility. Remaining evidence that would link the epidemic to an accident in the facility is quite thin and subject to arguable interpretations and may have other explanations. The current Soviet account is very likely to be true; and at the very least the evidence that they have presented gravely impairs the credibility of a portrayal of the epidemic as directly related to illegal activity in Sverdlovsk.

The latter remains the central question: US concerns that the USSR continues to do secret military work on BW are not alleviated in any way by removing the epidemic per se from the list of allegations. I believe that it would be in the interest of the credibility of our arms control initiatives for the US to continue to press for satisfaction on the primary issue, but no longer to insist that the epidemic is important evidence for such violations.

It would be possible to pursue the veracity of their account of the epidemic by raising further questions with their Ministry of Public Health. It is unlikely however that any amount of evidence they are likely to provide will induce a public reversal on the part of people here who have taken strong positions. Hence, it may or may not be worthwhile to spend much capital in that pursuit. This discussion of the epidemic does distract from the central issue as to the nature of the secret facilities that we have questioned.

One other topic of discussion of the CISAC group was a proposal to strengthen US-USSR scientific cooperation and exchanges, in the biomedical areas most germane to BW and the enforcement of the BW Convention. I would urge the most sympathetic consideration be given to these initiatives. They provide the most likely avenue for both sides to have an accurate and uninflamed estimate of the continued work in biomedical research in each country. I have no doubt that the overwhelming majority of biomedical scientists in the USSR, as in the US, abhor the very concept of biological warfare. If given any opportunity and encouragement they would weigh in favor of compliance with, and strengthening of, the BW Disarmament Convention.

Preliminary statement by Joshua Lederberg
for the Committee on International Security and Arms Control
Moscow, 8 October 1986.

This group does not need to be reminded of urgent reasons for strengthening controls on Biological Weapons. (I have brought a copy of a prior statement to this committee that reviews this issue for the benefit of those of you who did not participate in earlier meetings.) The recently concluded 5-year review conference on the BW Convention also stressed the importance of strengthening the treaty. I believe that this concurrence is an important step forward, and that our own discussion here will be very much in the spirit of implementing the strengthening measures advocated at Geneva.

Even with the best of good will and mutual confidence, the control of BW poses serious difficulties, and it may not be possible to solve all of them as long as there remain unresolved sources of interstate conflict. Even while we seek progress toward broader aims of harmony, prevalent suspicions, fears and doubts about BW remain a serious obstacle to those goals. Confidence-building measures therefore remain the most important step we can take, both for BW arms control and for broader aims.

Certain progress has also been made at the CD and in bilateral discussions towards advancing non-proliferation and disarmament in the CW field. My own discussion will center entirely on BW with infectious agents to the exclusion of toxins and of CW, acknowledging that progress in each arena contributes to the others. I am therefore more optimistic than has been possible for several years.

The central difficulties in BW arms-control are a) verification b) definition, c) the rapid advance of biotechnology, and d) the potential for rapid breakout.

a) The limitations of BWC verification by NTM have been well understood; several states were reluctant to sign a treaty that seemed to depend entirely on cooperative verification. Cooperative verification is tightly intertwined with mutual confidence: each depends on the other. It should be in the interest of each state to do all possible to reassure the others. I am pleased that a reaffirmation of this principle, and hopefully a fresh start in its practice, were signalled at Geneva. CW arms control may also show how mutually satisfactory regimes of inspection may be crafted that could later be applicable to BW as well (or even sooner, since the BWC already mandates BW-disarmament). I will return to CBM's later.

b) R&D related to BW is difficult to define, so much so that definition may be a graver problem than verification. The scale of facilities needed for production (forbidden under BWC) is fairly small, and difficult to separate from the scale for R&D (allowed). Defensive work, e.g. the production of vaccines, or the testing of potential threat agents in order to refine countermeasures, is difficult to separate from work with offensive goals. The BWC is somewhat vague about the level of production that would clearly mark an effort as offensive and illegal. At the same time, biomedical research, our common war against natural enemies, requires almost

identical tools, training, and knowledge as those which would have potential military application. (Conversely, work in military laboratories has played an important part in the history of the conquest of communicable diseases.)

c) The growth of biotechnology will eventually enable the production of BW agents of greater precision of targetability and control, attributes that are far more important than lethality to make them more usable for military purposes. The future prospects of such military uses heighten the anxiety about the intentions of work that is kept secret. At the same time, industrial biotechnology has already greatly expanded overall investment in large scale microbiological facilities which might have dual potential (i.e. to produce BW agents.) There is also a certain international competition for economic purposes, and industrial proprietary secrecy also may complicate the effort to build confidence by the freer exchange of information.

d) There is, and should be, grave concern about breakout. However effective an arms-control and confidence-building regime we may build tomorrow, ether side's accumulated knowledge, technical knowhow and industrial facilities could be rapidly converted from civilian to military purposes.

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Medical scientists in any country therefore have a complicated burden of conscience: on the one hand, to sustain their own country's security with realistic advice about vulnerability to attack with BW; on the other to do all possible to assure that biological weapons are never used, never produced, insofar as possible never developed, by anyone. My advice to my government has always been, unequivocally, to avoid BW as a military utility; and I believe any informed medical scientist will speak with the same voice to his government. Openness may therefore have a twofold benefit: to provide reassurance building confidence as between countries; and to give medical scientists everywhere the best opportunity to advise their own governments about the wisest policies for their own national as well as global interests.

Medical scientists, besides their unique ethical situation, also are uniquely qualified to work out the most feasible framework of cooperative verification, to understand its possibilies and its limits, and to take an active role in its implementation. We have a difficult task in thinking of measures that can meet the constraints of verification, definition, rapid technology and breakout well enough to promote confidence and enhance mutual security. We cannot expect perfect solutions overnight, and pragmatic advances will need the most thoughtful participation of scientists from all sides. It is therefore especially gratifying that we can have succeeded in arranging for this meeting, and its particular membership.

CBMs.

The Geneva BWCRC suggested a number of measures, above all mutual

consultation in a variety of forums and with the participation of experts. (The U.S. government has acknowledged the value of informal exchanges, and encourages them; it also insists that formal consultation within the terms of the treaty not be evaded.) A meeting is agreed to be held in Geneva, April 1987 to work out modalities of exchange. Other steps include the registration of high-hazard facilities, and the publication of research related to BW. The overall framework of scientific cooperation in biotechnology and other biomedical research should be bolstered. We should discuss all of these, and other possibilities at this meeting.

I would not be candid if I overlooked what has been a major impediment in mutual confidence from a US perspective. We are also here to learn what the USSR's concerns may be. But I am glad to acknowledge a major positive step on the USSR's part in opening up discussion about the anthrax outbreak in Sverdlovsk in 1979. I was delighted to learn from Dr. Matthew Meselson about his visit in August this year with Moscow public health officials who were directly involved in managing that outbreak. He has briefed our delegation about what he learned. I have also received notes of Dr. Antonov's report to the BWCRC on the same subject. These reports have provided detail that was not hitherto available; and above all the opening of clear channels for further discussion with the relevant public health authorities is a very large an positive step that we all commend. The epidemic is a subject of considerable scientific interest, and I hope we will have time for some informal discussion with the principals to learn more from that perspective, as well as to advance the publication of detail in a way that can overcome the accumulated speculation of the past six or seven years.

A more difficult problem, because it must touch on the policies of controlled disclosure that are the privilege of each country, is wider exchange of information about facilities that work on BW-related matters. The US already publishes some information on these subjects. I am not authorized to speak on behalf of the US government but I am confident that many still larger steps could be agreed to on a reciprocal basis. Without broader disclosure, many biotechnology-related facilities in the USSR rumored to be BW-related are candidates for anxiety, and motivate initiatives to match them in the US: a tacit BW-technology race within the latitude of the treaty. If these anxieties are groundless, it is not in the USSR's interest that they be sustained by a refusal to discuss them; and needless to say, vice versa.

Third party and terrorist use of BW should be a matter of equal concern to the US and the USSR. Similar concerns about CW have been discussed bilaterally at Berne. If we can achieve higher mutual confidence about BW, we will be better able to advance our mutual stance about BW proliferation and terrorism.

An important objective, as well as instrumentality, of CBMs is enhanced scientific cooperation. It is unrealistic to expect striking

progress in cooperation so long as fear about the other side's technology is the dominant emotion in the relationship. The US can benefit from USSR experience and skills in many aspects of epidemic disease; the converse is true, I believe, for industrial and pharmaceutical biotechnology. Most important, perhaps, the third world is legitimately demanding that both superpowers mitigate the bilateral problems, and devote attention and resources to its needs.

PRIVILEGED

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Moscow, October 8-9, 1986

SUMMARY

Delegations from the U.S. National Academy of Sciences (Committee on International Security and Arms Control subgroup on Biological Weapons) and the Academy of Sciences of the U.S.S.R. met on October 8-9, 1986, at the Shemyakin Institute of Bioorganic Chemistry in Moscow. Dr. Joshua Lederberg, President of Rockefeller University, chaired the American delegation. Dr. Evgeniy Sverdlov, of the Institute of Bioorganic Chemistry, chaired the Soviet delegation.

The agenda for the meeting was a synthesis of items suggested by both sides and included discussion of a) the problem of biological weapons and control of their proliferation; b) problems in U.S.-U.S.S.R. confidence in areas related to biological warfare and measures to build confidence in these areas; and c) possible areas of scientific cooperation to increase contacts and enhance confidence between American and Soviet biomedical scientists.

Problems of Biological Weapons and Their Control

Both delegations came to rapid agreement that biological weapons were extremely dangerous, had no rational military utility for a superpower, and that their development should be prevented in accordance with the 1972 Biological Weapons Convention. They agreed that neither the U.S. nor the U.S.S.R. had used BW in recent history, and that both countries shared an interest in preventing the proliferation and use of BW by third parties.

They agreed that the primary task now was one of trying to prevent BW development at an early stage. The American side emphasized the difficulties involved in drawing the line between permitted and unpermitted research under the Biological Weapons Convention, and addressed the problem of the dual nature of fundamental research whereby it is applicable to both the civilian and military spheres. Inherent in this situation is the unavoidable possibility and danger of rapid breakout from the Convention. The American side emphasized that the control of the development of BW, unlike nuclear arms control, would depend not on the limitation of technical developments but on limitations on the transfer of technology emanating from the medical community to military organizations. The American side emphasized that this clearly posed monumental challenges in definition and verification which would be much more difficult to solve than analogous challenges in the nuclear realm.

The Soviet side was eager to report the results of the recently concluded Biological Weapons Convention Review Conference in Geneva. Ustinov, a Ministry of Foreign Affairs representative who had been on the Soviet delegation at the Review Conference, offered an upbeat report of the Conference and emphasized Soviet initiatives made in the area of measures to strengthen verification. He expressed Soviet surprise at the negative response of the U.S. at the Conference to Soviet proposals for legally binding measures to strengthen the verification of the Convention. Ustinov catalogued Soviet offers to declare hazardous facilities and the basic thrust of their research, as well as to expand publication of research from those facilities. He was optimistic that these measures could be elaborated at the April 1987 experts meeting. In a similar spirit, several members of the Soviet delegation made efforts to describe the research being

undertaken in their own labs and extended open invitations to the Americans to visit their labs and talk directly with researchers in them.

Problems in U.S. - U.S.S.R. Confidence in Areas Related to BW

Because of the inherent possibility for dual application of fundamental biological research to the civilian and military spheres, both sides recognized that full exchange of information about their scientific research was the best way to promote confidence. However, they acknowledged and discussed the existence of barriers to this openness including national security considerations, industrial proprietary secrecy, and differences in the scientific cultures of each country.

In one of the few polemical statements made at the meeting, Schvedkov called attention to recent American press reports about increased Department of Defense spending on BW development and possible testing. The American side took advantage of this remark to stress that open information on and debate about the U.S. program was a positive development, and that the lack of information from the Soviet side was a source of tension and anxiety in the U.S. about Soviet activities in this area. The American side was responsive to Soviet concerns about reports of increased U.S. activity, and took the opportunity to clarify what the U.S. was and was not doing. The American side emphasized that the asymmetry in available information contributed to an atmosphere of distrust and even a technology race within the limits of the BW Convention.

In prior discussions, the American side had (as has the U.S. government over many years) raised Sverdlovsk as an issue corrosive of confidence because of Soviet reticence in supplying comprehensive

information on the epidemic as called for under the BW Convention. Dr. Nikiforov, the physician from the Ministry of Health called in to treat the victims of the 1979 Sverdlovsk anthrax epidemic, gave both delegations a two-hour lecture on the epidemic, showed autopsy slides of the victims, and responded to questions. The Soviet delegation as well as the Americans were quite interested in the presentation. Some Soviets indicated they had been familiarized with the Sverdlovsk incident only in preparation for this meeting. The American side explained why this has been such a serious issue in the U.S., commended the recent Soviet efforts to be more forthcoming with information about this event and in general, and encouraged the publication of the details of this epidemic for a broader audience.

The Americans requested and were given an additional two hour question and answer session with Nikiforov and his assistant Yampolskaya to probe further into the matter (see attached appendix). The Soviet doctors were forthcoming in the session, providing essentially the same information they had provided to another American scientist in August 1986. Two new pieces of information they provided were that 1) they had lectured extensively over the last five years on the Sverdlovsk epidemic to many Soviet doctors, particularly in the Sverdlovsk region; and 2) there were incidences of more than one anthrax case in some families.

Possible Areas of Scientific Cooperation to Increase Contacts and Enhance Confidence Between Scientists

Both sides agreed that scientific cooperation in the biomedical area could contribute over the long term to enhanced confidence through personal contacts and the opening up of a window on the activities of the other side. The Soviets, not surprisingly, were eager for scientific cooperation and contacts. Mirzabekov noted that the current U.S. policy seemed to be one of limiting Soviet access to

biotechnology and genetic engineering, and said that a first step toward enhancing confidence would be to gain a relaxation of the current restrictions.

Sverdlov firmly pushed for the establishment of an Academy to Academy institutional mechanism for scientific cooperation directly related to confidence-building in the BW area. The American side said the essential criteria for cooperative programs were that they be: 1) of humanitarian significance and great medical benefit; and 2) that they lend themselves to true scientific reciprocity and symmetry of input. The Soviets stressed the criteria of 1) humanitarian significance and 2) prestige and ability to attract first-rate scientists on both sides. Both sides agreed the chosen topics should hold great promise for scientific success. The American side stressed that human rights issues remain a possible barrier to cooperation because many American scientists oppose cooperation with Soviets until certain human rights cases are resolved.

The specific areas for possible collaboration raised in this meeting were: 1) structure of the human genome; 2) development and cell differentiation in cancer; 3) vaccine development; 4) genetic engineering of plants (a Soviet proposal aimed at helping and including the Third World); 5) mutational genetic load of man in the biosphere.

General Observations

The atmosphere of this meeting, coming after the BWC Review Conference and before Reykjavik, was very good. There was rapid agreement on the necessity of discouraging BW development, and interesting discussions on possible areas for scientific collaboration and on tangential scientific topics. The Soviet delegation indicated privately that they had been brought together as

a delegation for the first time for this meeting, and had been briefed both on the Sverdlovsk incident and on the BWC Review Conference also for the first time in preparation for this meeting. The discussion of the Sverdlovsk incident was extremely interesting.

There were only two or three interjections that could be characterized as polemical. Schvedkov's lengthy statement endorsing every Soviet nuclear arms control proposal and calling attention to American press reports about U.S. BW activities was one of them. Most members of the Soviet delegation were serious and prominent scientists, rather than political types.

Sagdeev came in at the end, primarily to give strong Academy endorsement to the continuation of this dialogue and the initiation of a cooperative scientific program. He did make a rambling statement discussing the differences between the BW problem and the nuclear arms problem, including an uncharacteristically caustic remark, two days before the Reykjavik meeting, about those who would defer substantial reductions in nuclear arms now for a perhaps unobtainable hope of protecting entire populations from the nuclear threat sometime in the future.

The meeting concluded with an understanding that each side would take the resulting ideas back to their respective Academies for further discussion, and that perhaps there would be another meeting of this group in Washington in May or June 1987.

PRIVILEGED

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Moscow, October 8-9, 1986

The first session of a two-day meeting of delegations of the U.S. National Academy of Sciences (a subgroup of the Committee on International Security and Arms Control) and the Academy of Sciences of the U.S.S.R convened at 11:30 a.m. on October 8, 1986, at the Shemyakin Institute of Bioorganic Chemistry in Moscow.

The members of the U.S. delegation were: Joshua Lederberg, chairman; Ivan Bennett; Paul Marks; Alexander Rich; John Steinbruner; Theodore Woodward, and Lynn Rusten (See attachment #1).

The members of the Soviet delegation were: Academicians R.Z. Sagdeev, N.P. Dubinin, and R.V. Petrov; Corresponding Members V.T. Ivanov, A.D. Mirzabekov, and E.D. Sverdlov; Academician S.G. Drozdov; Dr. Y.A. Schvedkov; Dr. V.I. Ustinov; Dr. O.M. Lisov; and Dr. Y.K. Shiyan (See attachment #2). N. Belousov and Mr. Chesnokov from the Foreign Relations Department of the Academy of Sciences of the U.S.S.R. also sat in on portions of the meeting, as did two unidentified individuals said to be experts on the subject sitting in on behalf of Academician Sagdeev who was absent most of the meeting.

Ivanov opened the meeting by welcoming everyone to the Shemyakin Institute. He expressed Academician Ovchinnikov's regret that he was unable to attend this meeting because he was out of the country. Ivanov noted the propitious timing of this meeting, coming just a few days before the Reagan-Gorbachev Reykjavik meeting.

Sverdlov said he had been asked by Ovchinnikov to co-chair the meeting with Lederberg. He suggested they begin with introductions. He said the Soviet delegation consisted largely of people from the Academy of Sciences of the USSR and the Academy of Medical Sciences and that they were well-known people in the Soviet Union involved in biology and medicine. He introduced each person and gave their affiliations, as indicated on the attached delegation list. He added that Petrov was chairman of the Immunologist Society and a member of both the Academy of Sciences and the Academy of Medical Sciences.

Lederberg thanked Sverdlov. He recalled his last visit to Moscow in June 1985, when there had been a good opening discussion on these issues in the regular CISAC meeting. He expressed regret that Ovchinnikov could not attend, but thanked his institute for its hospitality and noted the symbolic importance of holding the meeting at an institution where exciting advances were taking place. Lederberg introduced his delegation as follows: Ivan Bennett, a Professor of Medicine and former Dean of the New York University School of Medicine, and long a student of the problems of BW; Paul Marks, President of the Memorial-Sloan Cancer Center and known for his leadership in scientific research in cell biology and cancer; Alexander Rich, professor of biology at MIT who has done important work, including discovering new forms of DNA, and who had worked closely with Mirzabekov; Theodore Woodward, a professor of infectious diseases at the University of Maryland and chairman of the Armed Forces Epidemiology Board, which provides scientific guidance to U.S. Army Programs; John Steinbruner, a political scientist, Director of the Foreign Policy Studies Program at Brookings, and a member of the regular CISAC committee; and Lynn Rusten, staff to the CISAC

committee of the National Academy of Sciences. Lederberg said he was President of Rockefeller University, and had spent many years at Stanford teaching molecular biology.

Sverdlov said he and Lederberg had discussed the agenda (see attachment #3) and that they had agreed to discuss today the problem of biological weapons and the recent Biological Weapons Convention Review Conference. He said they would work in an atmosphere of candor and openness, with everyone free to make comments at any time. He said they could spend the entire day on the first point, and then go on to discussions of possible areas of cooperation the second day. He said Lederberg would start on the first point with a position paper he had prepared.

Lederberg said he appreciated this opportunity for discussion. He said he had been involved for 16 years in efforts to control biological weapons, that he had played an active role in the U.S. in efforts resulting in President Nixon's unilateral moratorium on BW. He said he advised the U.S. Arms Control and Disarmament Agency during the negotiation of the Biological Weapons Convention, and that long prior to that, he had a deep concern that the fruits of biological research be used for the benefit of mankind, not for military purposes.

Lederberg said this group did not need to be reminded of the urgent reasons for strengthening controls on biological weapons, however he brought a copy of a prior statement reviewing the issue for the benefit of those present who had not participated in the previous meetings. Lederberg said the recently concluded five-year review conference on the BW Convention demonstrated the importance of review conferences and of efforts to strengthen the treaty. He said this concurrence was an important step forward, and that their discussion here would be very much in the spirit of implementing the strengthening measures advocated at Geneva.

Lederberg said that even with the best of good will and mutual confidence, the control of BW posed serious difficulties, and it might not be possible to solve all of them as long as there remained unresolved sources of interstate conflict. He said that even while we sought progress toward broader aims of harmony, prevalent suspicions, fears and doubts about BW remained a serious obstacle to those goals. Confidence-building measures therefore remained the most important step we could take, both for BW arms control and for broader aims.

Lederberg said certain progress had also been made at the Conference on Disarmament and in bilateral discussions towards advancing non-proliferation and disarmament in the chemical weapons field. He said his own discussion would center entirely on BW with infectious agents to the exclusion of toxins and of CW, while acknowledging that progress in each arena contributed to the others. He said he was therefore more optimistic than had been possible for several years.

Lederberg said he would be compact in his outline, but even so, his talk would take an hour. He welcomed questions at any time. He identified the central difficulties in BW arms-control as a) definition; b) verification; c) the rapid advance of biotechnology; and d) the potential for rapid breakout.

Lederberg said research and development related to BW was difficult to define, so much so that definition might be a graver problem than verification. He said the scale of facilities needed for production forbidden under the BWC was fairly small and difficult to separate from the scale for research and development which was allowed under the BWC. He said defensive work, such as the production of vaccines or the testing of potential threat agents in order to refine countermeasures, was difficult to separate from work with offensive goals. Lederberg said the BWC was somewhat vague about the level of production that would clearly mark an effort as offensive and illegal. At the same time, biomedical research, their

common war against nature's enemies, required almost identical tools, training, and knowledge as those which would have potential military application. He added that, conversely, work in military laboratories had played an important part in the history of the conquest of communicable diseases.

Lederberg said the limitations of BWC verification by National Technical Means (NTM) have been well understood; several states were reluctant to sign a treaty that seemed to depend entirely on cooperative verification. He said cooperative verification was tightly intertwined with mutual confidence: each depended on the other. He said it should be in the interest of each state to do everything possible to reassure the others. He said he was pleased that a reaffirmation of this principle, and hopefully a fresh start in its practice, were signalled at Geneva. Lederberg said CW arms control may also show how mutually satisfactory regimes of inspection may be crafted that could later be applicable to BW as well. He said he would say more about confidence-building measures later.

Lederberg said the growth of biotechnology posed other problems. It would eventually enable the production of BW agents of greater precision of targetability and control, attributes that were far more important than lethality to make them more usable for military purposes. He said the future prospects of such military uses heightened the anxiety about the intentions of work that was kept secret. At the same time, industrial biotechnology had already greatly expanded overall investment in large scale microbiological facilities which might have dual potential (i.e. to produce BW agents). He said there was also a certain international competition for economic purposes, and industrial proprietary secrecy also may complicate the effort to build confidence by the freer exchange of information.

Lederberg said there was, and should be, grave concern about breakout because however effective an arms-control and

confidence-building regime we might build tomorrow, either side's accumulated knowledge, technical knowhow and industrial facilities could be rapidly converted from civilian to military purposes.

Lederberg said that medical scientists in any country therefore had a complicated burden of conscience: on the one hand, to sustain their own country's security with realistic advice about vulnerability to attack with BW; on the other to do all possible to assure that biological weapons were never used, never produced, and insofar as possible never developed by anyone. Lederberg said his advice to his government had always been, unequivocally, to avoid BW as a military weapon; and he believed any informed medical scientist would speak with the same voice to his government. He said openness may therefore have a twofold benefit: to provide reassurance building confidence as between countries; and to give medical scientists everywhere the best opportunity to advise their own governments about the wisest policies for their own national as well as global interests. He said he feared development of BW by governments who lacked good advice from scientists who knew its danger and uncontrollability.

Lederberg said medical scientists, besides their unique ethical situation, also were uniquely qualified to work out the most feasible framework of cooperative verification, to understand its possibilities and its limits, and to take an active role in its implementation. He said they had a difficult task in thinking of measures that could meet the constraints of verification, definition, rapid technology and breakout well enough to promote confidence and enhance mutual security. He said they could not expect perfect solutions overnight, and pragmatic advances would need the most thoughtful participation of scientists from all sides. Lederberg said it was therefore especially gratifying that they had succeeded in arranging for this meeting, and its particular membership.

Turning to some remarks about confidence-building, Lederberg said the Geneva BWC Review Conference suggested a number of measures, above all mutual consultation in a variety of forums and with the participation of experts. He said the U.S. government had acknowledged the value of informal exchanges, and encouraged them; it also insisted that formal consultation within the terms of the treaty not be evaded. A meeting was agreed to be held in Geneva in April 1987 to work out the modalities of exchange. He said today's discussion could be useful in outlining certain measures. Other steps included the registration of high-hazard facilities, and the publication of research related to BW. He said the overall framework of scientific cooperation in biotechnology and other biomedical research should be bolstered, and they should discuss all of these, and other possibilities, at this meeting.

Lederberg said he would not be candid if he overlooked what has been a major impediment in mutual confidence from a U.S. perspective, and that his delegation was also here to learn what the U.S.S.R.'s concerns might be. He said he was glad to acknowledge a major positive step on the U.S.S.R.'s part in opening up discussion about the anthrax outbreak in Sverdlovsk in 1979. He said this was a great step. He wanted to explain what a serious issue this had been in the U.S. He said there had been some propaganda surrounding the issue, but that also there had been at the highest levels of government a sincere adoption of a malignant interpretation of that event. Lederberg said he was glad there had been a chance to ventilate it, and that he had been delighted to learn from Dr. Matthew Meselson about his visit in August this year with Moscow public health officials who were directly involved in managing that outbreak. Lederberg said Meselson had briefed this delegation about what he learned. Lederberg also received notes of Dr. Antonov's report to the BWC Review Conference on the same subject. He said these reports provided detail that was not hitherto available and opened up clear channels for further discussion with the relevant public health

authorities, all a very large and positive step that he commended. Lederberg said he was glad that Dr. Nikiforov would participate in this meeting so they could discuss the issue again more fully. He said he hoped this could erase a needless point of controversy between their two countries. He said the epidemic was a subject of considerable scientific interest, and hoped they would have time for some informal discussion with the principals to learn more from that perspective, as well as to advance the publication of detail in a way that might overcome the accumulated speculation of the past six or seven years.

Lederberg said a more difficult problem, because it must touch on the policies of controlled disclosure that were the privilege of each country, was wider exchange of information about facilities that work on BW-related matters. The U.S. already published some information on these subjects. Lederberg said he was not authorized to speak on behalf of the U.S. government, but was confident that many still larger steps could be agreed to on a reciprocal basis. He said without broader disclosure, many biotechnology-related facilities in the U.S.S.R. rumored to be BW-related caused anxiety, and motivated initiatives to match them in the U.S., resulting in a tacit BW technology race within the latitude of the treaty. Lederberg said if these anxieties were groundless, it was not in the U.S.S.R.'s interest that they be sustained by a refusal to discuss them; and needless to say, vice versa.

Lederberg said third party and terrorist use of BW should be a matter of equal concern to the U.S. and the U.S.S.R. Similar concerns about CW have been discussed bilaterally at Berne. He said if they could achieve higher mutual confidence about BW, they would be better able to advance their mutual stance about BW proliferation and terrorism.

Lederberg said an important objective, as well as instrumentality, of confidence-building measures was enhanced scientific cooperation. It was unrealistic to expect striking

progress in cooperation so long as fear about the other side's technology was the dominant emotion in the relationship. He said the U.S. could benefit from Soviet experience and skills in many aspects of epidemic disease; and the converse was true for industrial and pharmaceutical biotechnology. He said that most important, perhaps, was that the third world was legitimately demanding that both superpowers mitigate the bilateral problems, and devote attention and resources to its needs.

Lederberg said he had as an appendix excerpts from Articles V and X of the agreed conference report from the 1986 Geneva BWC Review Conference, but in the interest of time he would just table them rather than read them aloud. He concluded his statement, which was met with applause.

Sverdlov said Lederberg's remarks were of overriding importance, and that he would add a few words of his own. He quoted a Soviet scientist who said: "Science lies in the palm of the state and warms itself on the heat of that palm." Sverdlov said science was becoming increasingly hot, that the role of science was increasing with the greater development of society. He said the scientific community was a presence in today's arena that could not be ignored, and that its role should be positive. He said science had been used both to harm and to benefit mankind. Madam Curie did not realize her discoveries would result in the bomb. Sverdlov said he was a specialist in the chemistry of radioactive isotopes produced by neutron absorption. He said he was struck by some of the things written in a book by Ralph Lapp called The New Force: Atoms and Men. Lapp participated in The Manhattan Project. In this book he wrote about the myth of radioactivity, saying that Hiroshima proved that a city could be lived in after bombing, that radioactivity was not as dangerous as it was once thought to be. Sverdlov said that Lapp did not foresee the long-term consequences. Sverdlov said today presented a situation like that of the 1940's and 1950's, when lots of gaps in our

knowledge existed. He said today we did not foresee the long-term consequences of developments in our labs. However, he said the biological sciences were in a better stage than nuclear research because there was a ban on biological weapons calling for the destruction of all stockpiles. Sverdlov said this was a first step toward the elimination of all weapons of mass destruction. He said at the recent Review Conference they declared that all biological weapons were disposed of and the signatories could continue building on confidence-building measures.

Sverdlov said he was preoccupied also with the differences between biological research and atomic bomb research. He said nuclear research was controlled by the state, because it required so much capital, etc. But biological research was undertaken privately in some countries, so it was more difficult to control by the governments. He said they had to think about the dangers of this research and possibility of circumvention of the rules and regulations governing it. He said some of the issues were being oversimplified. He said the Nobelist Wilbur had said that any recombination was less dangerous than natural occurrences. Another Nobelist believed that in the labs there was nothing new or worse than occurred in nature, like recombination, mutants, etc., and that what had not been created in nature would be in the future. Sverdlov, however, said that what happened in nature happened on an individual scale, but in the lab they created populations and favorable conditions for their survival, and they did not know what would happen if they escaped from the lab.

Sverdlov said there were two camps: the prophets of doom and the optimists. He said they should be more attentive to the prophets of doom, learning a lesson from the negative results of atomic research. He said this was the viewpoint of the staff of his institute, and that they had discussed it with their director, Ovchinnikov. He said they designed their lab as a P-III level

containment lab and it was becoming almost a P-IV level containment lab. He said he was offering this information about his lab as had been suggested at the Review Conference. Sverdlov said they were developing a vaccine against leukosis of cattle. He said they spent $5 million for the lab on top of ruble expeditures, and that they were willing to do this to protect personnel and the environment. He said the lab would be completely open and he invited everyone present to visit it when it came on line and said they would be free to ask questions of the workers, etc. Sverdlov said this should be an important discussion point of this seminar.

Sverdlov said his second point was that the problem of confidence-building measures was a most crucial issue. He said if there were mutual confidence, then issues such as propaganda about certain events became less important. He said for instance that if he and Rich were in frequent contact, and if there were allegations that Rich was involved in developing BW, that Sverdlov would be able to discount those allegations. But if he did not know Rich and his work, he would not be able to evaluate those charges.

Sverdlov said it was important to collaborate on the most humane biological problems. He said in this meeting they could formulate areas of collaboration to present to their Academy leaderships. He noted the existing record of scientific collaboration, recalling a time when they tried to organize a permanent seminar on molecular biology. He said the first meeting, attended by David Baltimore, occurred in 1975 in Kiev, but that regrettably was the first and last meeting. Sverdlov said they had bilateral symposiums with other Western countries, but regrettably they did not know as much about scientists in the U.S. He said this went to the level of friendship as well as to professional relationships. Sverdlov asked that these points he raised be added to Lederberg's list of issues to discuss.

Petrov said Lederberg had mentioned the ethical responsibility of scientists. Petrov said this was well understood and that it was his understanding that this responsibility rested heavily on scientists

who had made major breakthroughs. He said they were familiar with Lederberg's accomplishments. He said these techniques were now available to "the man in the street," and that maintaining the responsibility of scientists was easier to achieve than resolving the responsibility of "the man in the street." Petrov said this ethical dimension became very important because who knew what were the ethics of the man in the street. He said it was difficult to predict the areas in which a major breakthrough would be disseminated to lesser minds.

Petrov said he was an immunologist, trying to remove immunity by creating tissue compatibility. He said if this occurred by a new method, then BW would not be needed. Any germ in the environment would become a danger, the body would be open to assault by hostile germs. Petrov said he said this to make the point that someone needed to make a list of the most potent hazardous lines of biological research and determine whether the research was necessary. He said it was necessary to monitor and verify these research facilities, and asked where the dividing line was to be drawn. Petrov said genetic engineering was on the hands-off list. He asked whether suppression of immunity for transplants needed to be closely monitored. He reiterated the necessity to list these potentially dangerous areas and to make humankind aware and alerted to the dangers.

Sverdlov agreed Petrov's point should also be discussed. The meeting broke for lunch.

After lunch, Rich said he wanted to make some statements reinforcing some of the comments made earlier. He said that BW were weapons of mass destruction, and that both the U.S. and the U.S.S.R. already had weapons of mass destruction. He said the fact that other nations might develop BW posed a great risk, and it was in both countries' self-interest to set up a system of adherence to the BWC and to make it impossible for others to develop BW. Rich said their

countries' interests were congruent in this area, and they had the opportunity here to exercise ingenuity. He asked whether they could invent political and social mechanisms which would build confidence and strengthen the treaty. He asked whether they could do things to ensure that BW development was not being considered. Rich said he could think of many approaches, and the most obvious had to do with openness. He said they in the U.S. were interested in and encouraged by the Soviet policy of "glasnost." He said this principle could be applied in the field of BW. Rich said he wanted this kind of activity to be discouraged in the world, that he did not want BW falling into the hands of terrorists, and so they were left with the challenge of inventing mechanisms that build confidence.

Schvedkov said it was his privilege to address this meeting in a broader framework. He said this reflected not only how he felt about it, but also what his profession as a political scientist motivated him to do. Referring to the hazards and dangers of BW proliferation, Schvedkov said he had been asked at lunch whether the Soviets were worried about the development of BW in the U.S. He said the U.S. press had given them reason to worry. He said it was one thing when they wrote about developments in Southeast Asia, but another when they wrote about Department of Defense activities. He said the Washington Post and Wall Street Journal reported on U.S. programs to test BW. Schvedkov said they were concerned about these developments. However, despite this, he said he wanted to make clear they regarded the BWC Review Conference as a first step toward eliminating weapons of mass destruction and changing the way of thinking in this century. He said in the late 1960's, some people thought BW could serve a rational purpose as weapons of mass destruction or for terrorists, but this realization did not come that easily to the U.S. Schvedkov referred to a book by Graham Allison on U.S. foreign policy which demonstrated that the rational argument to give up BW was resisted by DoD. Schvedkov said proof of this existed

in recent reports in the U.S. press. He said from a broad political perspective, it was not possible to examine compliance with the BWC outside of confidence-building between the U.S. and the U.S.S.R.

Schvedkov said the Soviets did want to progress in advancing new innovative concepts. He referred to the Soviet proposal to eliminate nuclear weapons and the Warsaw Pact proposal to make deep cuts in weapons from the Atlantic to the Urals, both butressed by their moratorium on nuclear testing. He said these proposals signified progress in the thought of the Soviet people and leadership, and the desire of the leadership to deal with those concerns. He said they did not believe the American Generals anymore then American Generals believed them, so they too wanted far-reaching verification. He quoted a Gorbachev interview of September 9, 1986, in which he said there could be a supranational network of CTB verification. Schvedkov said that to him personally, this was a serious phrase with far-reaching implications. He said the Soviet government recognized the feasibility of international and supra-national forms of verification. He said the Warsaw Pact in its proposals was calling for verification and on-site inspection, and that in the Chemical Weapons talks they were discussing far-reaching methods of verification. The Stockholm accord represented early steps toward confidence-building measures.

Schvedkov apologized for talking about things so remote from BW, but said they were related. He said BW could be expected to be used in a war of complete destruction. He said confidence-building measures were related to measures to increase security between their two countries. But the third-country problem was reflected by this upsurge of terrorism, which was generated by countries feeling insecure. He said they had to do more than limit BW, they must ensure confidence between their two countries, and then maybe the entire international environment would be more stable. He said they could provide an additional impetus toward better and more

international security, and scientists could do much to promote international negotiations. Returning to the subject of BW, Schvedkov said they should go on record to say that the BWC was a working convention. He said he did not think it was being violated anywhere, and biological scientists would have to do their utmost to enhance the convention.

Lederberg said he was interested that Schvedkov raised questions about publicly available information in the U.S. on BW. Referring to a Wall Street Journal article of September 17, 1986, Lederberg said he would stipulate that the numbers were approximately correct and showed an increase in research in this area, with spending at $40 million in 1986. Lederberg repeated that this was publicly available information, and that the nature of the facilities at Dugway was under intense debate, with close scrutiny by Congress. He said these activities were legal and within the bounds of the Treaty, yet they must make the Soviets very uneasy about the long-range intentions of the U.S. Lederberg said maybe they thought this was just the tip of the iceberg. Yet, he asked them to try to look at the problem from the American point of view, where there was not this kind of open information about Soviet activities. He said in the absence of public information, there was only speculation with a tendency toward worst case scenarios, and this drove the process. Lederberg said he was worried about a technology race within the bounds of the BWC. He said an important step would be reciprocity with regard to providing information. He said more, not less, discussion on both sides would produce realistic appraisals of each others' activities. He said he was alarmed because it was natural that these newspaper articles would alarm the Soviet Union and spark Soviet activities, thereby feeding into a cycle of technology race. He said openness must be encouraged by an understanding of the possible disastrous final consequences. He said there was agreement on this point, that it was reflected in the Review Conference in Geneva.

Lederberg said he had with him public information about U.S. programs which was fairly comprehensive. He said questions must be addressed not just through the press, but through proper channels. He said he agreed generally on the relationship between BW and arms control. The prospect of nuclear annihilation was the main source of anxiety, but he worried that if nuclear weapons were controlled, there would be easy recourse to BW as weapons of mass destruction, and they would be technologically more readily available to other countries. He said he hoped the Convention was working, but it was not enough that each side knew it was complying; each must know that the other was complying. He said they had to discover more active means of assuring each other, through what he termed "affirmative cooperative verification." He said progress was being made in that direction.

Bennett made some additional comments on what had appeared in the press. He said the numbers were quite accurate and showed an increase in spending. But, he pointed out that a lot of that money was for vaccines, and full scale production of vaccines was costly, so just looking at the numbers could be misleading. He said concern about research in the U.S. was also related to concern about the environmental hazards of this research. He explained this had ended up in the U.S. courts on that basis, and unfortunately not on the basis of whether these activities were in compliance with the BWC. Bennett said this discussion pointed out that the asymmetry in available information was a source of tension. He said he favored this idea of affirmative exchange of information so we could know what each other was doing. He said they needed to talk about what they as scientists could do to build confidence in the BWC, and he hoped they would come up with concrete suggestions at this meeting.

Turning to a new subject, Dubinin said that 50% of zygotes persisted in the natural environment. He said this was a biological point, that environmental mutagens were little else than a way to affect human inheritance. He said environmental mutagens were

related to nuclear weapons by a steady, gradual, slow process. He said they could be incorporated into the environment and could be large enough to affect mankind. He said in that area, one needed to have methods of analysis into the mutation of man, but these methods were not practical because they required high investments. He said six methods were being developed for DNA mutagenesis; none were realistic, but they were in progress and could become an area of joint Soviet-American research. Should this research be achieved, he said it would build confidence. Ten point five percent of newborns were born with genetic defects. He said they could affect human development in a common effort to make new peaceful developments.

Lederberg said he shared an interest in this problem of chemical factors in the environment, some natural and some from pollution, and now the problem of toxins for military uses. He said this research should be encouraged and would produce positive results. He said the relationship of this to confidence-building was that openness should operate internationally and intranationally. He reiterated that the Wall Street Journal article was a matter of public debate, and they could learn by talking to American scientists about these programs. He agreed there was deep interest in this area in the U.S. and that direct measures of mutational changes within the U.S. population would be achievable at lower cost in ten years.

Steinbruner called attention to the peculiar character of this problem as an arms control problem as different from other arms control problems. He said the good news was that the problem was almost entirely in the future, if at all. The BWC was in place, there had been no recent modern day use of BW, and there was only one past incident that needed to be cleared up and that had begun. He said they were preventing something, rather than having to roll something back. He said BW arms control also had a different character in that the research and technological dynamic was coming not out of the military community, but out of the medical community,

which was conducting the research for good reasons. Technology to help society could be used to hurt it. He said this was mixed up with constructive work in a way that weapons were not. He said they could not try to limit technology development as they did in the ABM Treaty. Fundamental techniques would be created for medical purposes. He said they had to get at intentions, instead of capability, and this posed a very big challenge. He said openness and cooperation were necessary, but very abstract. They faced the problem of bringing definition to these principles that would give them meaning. Steinbruner said that if the technology were developed outside of military organizations, an important key to control would be prevention of the transfer of technology to military organizations. To do so in a credible, verifiable way, they would have to depend on rules of how military organizations conduct themselves, and that would put them into the different area of how one observes military operations to be sure their character reflected what they had agreed was limited. He said they would have to think about how to control the transfer of technology from the civilian to the military sector.

Lederberg said one criterion to define the dividing line was secrecy. He said large scale conversion of civilian technology to the military would be done in secrecy. It was not impossible to imagine an agreement that the militaries would not conduct BW prevention research, but that would require enforcement.

Sverdlov said he had a few words of commentary on the issue of openness. He said the problem was vaster than one of openness, that confidence was the criterion of confidence and that was a vicious cycle. He said the people at this meeting were organizers of research. He said an agreement required an organizational and institutional mechanism to ensure confidence. He said they all subscribed to the ideas expressed so far, but the big issue was to take action.

Merzabekov said that governments sometimes made decisions without consulting scientists. He said scientists thought that if anything was interesting, it should be studied, regardless of possible long-term hazards. Politics did have an impact on scientists. He said President Reagan regarded biotechnology and genetic engineering as areas where the Soviets should have limited access. Merzabekov said the first confidence-building measure should be to increase the sheer numbers of people in scientific exchanges. Governments sought advice from scientists. He said if American scientists harmonized with Soviet scientists, the U.S. government would listen. Merzabekov said there was always the danger one side could duplicate what the other was doing in its labs, and this fact could lead to restraint. He expressed his hope that at Reykjavik the politicians could make a step forward in confidence-building measures as a step toward further openness.

Marks offered some personal reflections on the comments made so far. He agreed with Sverdlov that communication must be freer and said the challenge was how to accomplish that. He said they had to understand the differences in the cultures in which they worked. He said the Americans could provide the Soviets with more information about Department of Defense support of microbiological research. He expressed the view that the Soviets had nothing to worry about so long as it was in the open realm and subject to public discussion. He said the amount of communication between scientists in the U.S. was more intimate and rapid than within the U.S.S.R. If this was not true, he asked to be informed and educated. He said these steps required commitment to a long-term process. Neither side had a record of resorting to biological warfare. But, he said the technology was evolving rapidly and they had a window in which to move expeditiously toward full public disclosure and a process of science fully in the public eye. He said this group must move slowly in the area of scientific seminars to explore advances. Marks said

that one could not understate the problems of differences of perception due to differences in the scientific cultures of their two countries. He recommended they take a long-term view of achieving these goals through the establishment of coordinating committees in the two Academies to oversee joint projects. He suggested certain areas for collaboration including vaccines and cancer research.

Ustinov offered some remarks on the translation of confidence building measures into specific steps. He said actions should be taken at the junction of science and politics. Ustinov said the Soviet side made some steps at the Review Conference, including a proposal to have a group of scientific experts discuss breakthroughs in technologies relevant to the BWC. Other suggestions they made included exchange of data on research centers undertaking biological research including location of facilities, and volume and basic thrust of work; and on epidemic breakouts. He said the Geneva forum accepted many of these suggestions and they were translated into a decision to convene in April a conference of scientific and technical experts to work out these measures. Ustinov said the Soviets were open to the ideas of other parties, including the U.S. proposal to intensify the publication of research relevant to the BWC. He said Sverdlov's call to complement the exchange of ideas with an exchange of information would build confidence. Ustinov said the Soviets preempted the U.S. side at the Review Conference by proposing to formulate a protocol to the Convention to include legally binding measures to improve compliance with the BWC. He said the Americans were the first to oppose this proposal and it surprised them because usually the Americans were vigorous in looking for stronger verification measures. He said the Americans were not prepared to accept this idea, even though it was supported by Ireland, Pakistan and the socialist countries. He said if they were preoccupied with the BWC, then they might think of it also in these terms: That promoting it and ensuring its effectiveness was a matter of goodwill

and of what the U.S., the U.S.S.R. and the U.K. would do -- this would determine world perception of the BWC. Ustinov said it now had 100 or 103 signatories, but now there were hesitations to join due to internal reasons, such as some African countries not being ready to address the BWC. He said there was some relation between the BWC and the negotiations to ban chemical weapons. In 1969, they decided to ban BW and chemical weapons in separate treaties. He said the chemical ban may soon be ripe for signature, but there was an attitude to wait until the end of the chemical weapons negotiations so that some of those verification methods could be used to verify the BWC.

Rich said there was strong sentiment that the BWC had been effective, and it might be unwise to open it up and change it. He said the Americans may have felt that opening the treaty to add verification measures might also open it up to measures that could weaken it. He said it would be possible to develop measures to strengthen it without opening it up to discussion and risking weakening it.

Drozhdov said the problems being discussed here had been discussed from different angles. He said he was a virologist, and this created for him the image of using viruses as weapons to cause outbreaks of epidemics. He was glad to learn of the existence of the Convention and its effectiveness. He asked how it could be made workable, how the world public could be given guarantees against possible violations. Drozhdov said that researchers were responsible for the outcome of their research and its possible misuse. He said research was two-sided. If something was ripe to be examined, scientists examined it. He said gene-engineering could be both a great benefit and a great detriment to mankind. He said BW was not realistically applicable today, but it was self-reproductive and self-propagating, and unless it was controlled, it could destroy mankind. He referred to a Jack London story, "Scarlet Fever," which

was about the destruction of society except for two men. He said one idea heard here for averting such a catastrophe was to develop protection from BW through vaccines. He said products to counter BW would contribute to confidence building, and if they could work for defense, instead of attack, this would modify the mindset of the people involved in it.

Drozhdov said he was from the Institute of Polioviruses, which was open to foreigners and was itself an outgrowth of U.S.-Soviet cooperation in developing the vaccine against polio. He said his institute was open to all guests and it was necessary that each side go to the other side and question what it was doing, rather than having to resort to press reports. He said each side should visit the other side's labs and ask questions of the researchers. That kind of cooperation would be useful. He said what was happening now was that scientists were harnessing powerful forces that were hard to control, and their task was to give humanity safeguards that these forces would be used for good, peaceful developments, and not to the detriment of mankind.

Lederberg suggested they look ahead. He said they were just starting to look at problems at the boundary of science and politics. He said they could take some encouragement from progress at the BWC Review Conference, including the ad hoc meeting of experts in Geneva in April which might cover some of the issues they have been concerned about. He said looking ahead, there might be a special role to be played by this group. He suggested an extension of these discussions after the experts' meeting in April. Lederberg invited the Soviet delegation to continue this discussion in Washington at a mutually convenient time, perhaps in May or June.

Woodward said the most important product of this meeting had been the openness of the discussion and confidence-building between these two groups. He said if this process stopped here, it would stop on shallow ground, so he hoped there would be a continuation of the dialogue.

Woodward said he would follow up on some of the comments made earlier. He said he was interested in infectious diseases, and noted that $40 million was spent by the U.S. military on infectious diseases research. He said the problem was that diseases like malaria did not interest their civilian researchers, that their purpose was to protect military people in areas where those diseases persisted.

Woodward said this conference reminded him and Bennett of a meeting twenty-five years ago where they evolved a joint U.S.-Japan medical cooperative program. He said he could think of one area where U.S. and Soviet scientists could cooperate to build confidence: in smallpox, which had been eradicated except from primates in Africa. Woodward said the U.S. had stopped immunization against smallpox. He said they could agree to do away entirely with the smallpox vaccination, and this was an example of how to take a little step toward progress. He said he did know that military research had helped the civilian sector, so they were talking about a two-way street as far as military research was concerned.

Sverdlov said he would offer some concluding remarks for the first day. He said their discussion had been open, candid and friendly. He said the plan was to talk about joint research programs the following day, but they had started on that today. He suggested that the next day they start thinking in institutional terms about confidence building measures. He said Marks had made a suggestion similar to one of his own, and which he had discussed with Ovchinnikov. Sverdlov said there was a very good starting point in launching U.S.-Soviet joint committees to organize cooperative projects, seminars, and committees. He said the discussions had been useful and they had heard a series of interesting specific suggestions. He thanked everyone for a useful day of discussions.

Lederberg agreed, saying it had been an exemplary discussion. He said it was an historical event to get well-known scientists to discuss these issues at the border of science and politics. He said this day alone was an outcome that justified the effort involved.

Sverdlov thanked the interpreters for their good work. The meeting adjourned for the day and resumed at 10:30 a.m. on Thursday, October 9.

Sverdlov opened the Thursday session, saying that Dr. Nikiforov had been invited by Lederberg and Marks to address the group. Nikiforov was the immediate physician at Sverdlovsk during the anthrax outbreak. Sverdlov said Nikiforov did not bring his slides, but they could be brought here. He asked Lederberg what he preferred to do. Lederberg said it would be better if Nikiforov brought his slides. Sverdlov said it was agreed then that Nikiforov's slides would be brought here and while they waited, they would start on the discussion of specific measures of confidence-building. He asked Lederberg if he wished to make any remarks.

Lederberg thanked Sverdlov. He said confidence-building measures fell into two categories. The first was steps specifically related to BW programs. He said they also fully understood the importance of more general measures of scientific cooperation to improve the quality and effectiveness of medical research. Lederberg said they probably wanted to spend most of their time today discussing the second category. He said it was unfortunate that they lived in a world where they had to discuss this, because science ideally should have no national bounds. He said there was, of course, individual competitiveness, but that was not a serious problem. He said the intermingling of the pursuit of science with national competition and rivalry was the source of the problem. In regard to military applications of science, Lederberg said it would be desirable to reverse the trends of the last 10-15 years. He said they must do this in a step-wise fashion and start with those things that would have the least resistance and the widest appeal due to their humanitarian significance. He recommended focusing on subjects with these features: a) programs stressing medical problems of broad significance; b) programs with true reciprocity, where both sides

would bring something of equal significance and magnitude. Lederberg reiterated that if they proceeded in this manner, they would be least likely to counter resistance in the U.S., and he said he thought there were probably similar sentiments in the U.S.S.R. Lederberg said they should of course work on things of interest and importance in which there was eagerness to participate on both sides. He said his statement was obvious, but served as a good guideline for their discussion.

Sverdlov said he was struck by the degree of coincidence of philosophy of himself and Lederberg. He said Lederberg's thoughts were remarkably similar to the ideas he had written down in preparation for today's discussions. He told a story of a Russian general practitioner of the last century who always turned away from a cemetery when he rode past it, explaining that he was ashamed because many in that cemetery had been his patients. Sverdlov said their shame as medics could be reduced by cooperating on medical problems. He said Lederberg had ably expressed that concentrated scientific programs could help achieve medical, biological and human goals, as well as build confidence.

Sverdlov said he would read his prepared notes, which were interesting in that they were so close to Lederberg's remarks. Sverdlov said the first requirement of cooperation was that there be just a small number of well thought-out programs. Second, they must be relevant to the times and humanistic, the opposite of BW in substance, operating under the slogan "biology for the benefit of mankind." Third, they must be prestigious, attracting serious scientists, and they must be successful projects. He said the programs must be well financed, and there should be bonuses and incentives to attract the best scholars. Those involved must realize that they were to advance both science and politics, and it was hard to say which was more important. He said scientists' personal ambitions must be in harmony with the project and they must realize

that they were part of a scientific and political experiment which could lay the foundation for cooperation. There must be a political result as well as a scientific result.

Sverdlov recalled that Marks the previous day said that management of the program should be by a joint committee which should provide a broader context such as information sharing through seminars, workshops, etc. Sverdlov said one possible program could be in research on the human genome and diagnosis and treatment of hereditary diseases. He said this was an important problem on which there was already scientific cooperation, as in Huntington's disease, muscular dystrophy, etc. These involved methods of pre-natal diagnoses, but could involve later gene therapy. He suggested they formulate a specific program on structure of the human genome. He said this was a costly program which perhaps could be done on the international level. Sverdlov said a second area was development and cell differentiation in cancer, with the long term goal being to cure cancer. He suggested looking at the role of the individual gene in carcinogenisis.

Petrov said it appeared they had reached some agreement already, with everyone agreeing there should be uniform guidelines. He said the main idea was to oppose BW and the intervention of BW into human organisms. He said there were certain scientific fields that were very hazardous for the production of BW, and that they should keep promising scholars in those fields in mind and set exclusively peaceful objectives before them and not let them get into the hands of the military. Continuing the ideas expressed by Drozhdov the previous day, Petrov said they should discuss new approaches to designing new vaccines. This was important because it involved some of the same fields and scientists as could be involved in BW activities. He said Pasteur's principles for designing vaccines were not working for new infections, and no other types of vaccines were capable of killing some types of infections. He said they must try to create artificial vaccines, recombinated vaccines, on the basis of

genetic engineering synthesis. He said work in this direction was taking place, they were trying to integrate and complex natural and artificial material, and they were on the threshold of success in this field. He said working on this were Professor Lerner in the U.S., a large group of scientists in Israel, Professor Ladnor in France, Svenson in Sweden, and a large group of Soviet scientists doing work on this in the Institute of Virology and Immunology. Petrov said he was putting forward another program idea dealing with genetic vaccines, involving the fields and scientists discussed here.

Lederberg expressed interest in Petrov's proposal. He said in the U.S. they had reached a tragic impasse in technical and legal developments regarding vaccine development. He said every vaccine would have side effects, even though it would save many lives. He spoke of the so-called "swine flu fiasco." That was a good vaccine, but it became enmeshed in politics. Lederberg said legal liability for vaccine side-effects had become a big problem. Juries tended to focus on individual distress, not the larger humanitarian and medical context. Lawyers, eager to earn large fees from large damages, were pushing this in a vigorous way. He said the net result was that it was almost impossible for pharmaceutical firms to stay in the vaccine business, and the ones that stayed in charged a great amount for vaccines, with 95% of the cost to cover insurance. He said it would be natural for this to be succeeded by a nationalization of the process, removing the profit motive. Lederberg said this area should be socialized, but it was taking a long time in the U.S. Technical possibilities were far ahead of the legal possibilities. He said AIDS represented serious technical problems with the possibility of vaccines, but the technical problems were small compared with the operational problems of getting into testing and development of vaccines. Lederberg asked what was happening in the Soviet Union in this regard. He asked whether they had analogous problems, or whether they were able to test and develop vaccines as easily as twenty years ago. Lederberg said it had been said that the polio

vaccine could never have been tested in the U.S. today. He said it would be ten to fifteen years before this situation changed in the U.S., and asked what was the situation in the U.S.S.R.

Sverdlov said Petrov and Drozhdov could provide exhaustive answers to this question. Petrov said he had only a small remark, which was that it seemed the U.S. now faced a situation where there were fewer legal barriers to the creation of BW than to new vaccines.

Drozhdov said Lederberg's question about operational differences with vaccines was interesting and complicated because it illuminated a range of problems. He said doctors were concerned about improving human health, but the final testing must be done on people. He said that according to Soviet medical tradition, at a certain stage they begin "field tests," which were required before introducing medicines. He said they were familiar with the problems in the U.S. of legal actions taken against vaccine producers and improvers. The World Health Organization tried to work out an international position on that problem. He said he attended this meeting, which resulted in a document outlining new principles to be used in the development and production of vaccines. Drozhdov said this document could be a good basis for a solution, to control the relationships between the designer, producer, state and recipients of the products. He said he was not familiar with the legal basis in the Soviet Union, but offered to explain their guarantee system that guaranteed safety to the participants. He said vaccines went through testing before they were adopted for distribution, and this was approved by the Ministry of Health. The test results were discussed by the Committee on Vaccines of the Ministry of Health, which was authorized to carry out independent recommendations which were binding on the Ministry of Health. Drozhdov said they were aware of possible side-effects, so they considered public opinion and the opinion of all relevant organizations. He said they had a different system, and also a different system for compensation for health damages. He said

international cooperation could help to solve this problem on the basis of recommendations made by Petrov. He said in the U.S. there were tests of vaccines which were the basis of the polio vaccine, and wide testing in the U.S. and the U.S.S.R. speeded up its development. He said widespread testing could yield information on epidemiology and side-effects, and they could use cooperation in some way to develop new vaccines.

Sverdlov called for a coffee break and said Nikiforov's slides would arrive shortly. After the break, Drozhdov reiterated that these cooperative programs must have a high measure of publicity so that everyone's reputation would gain and good researchers would be attracted.

Rich endorsed the comments made by Petrov and Drozhdov concerning the field of vaccine development. He said it was an area of increasing importance for a variety of reasons. He said the important point about doing joint research in this area was that it was at the heart of mutual confidence because it would involve the same people as those involved in BW research. Rich said it would be effective in letting people feel they had a window on the activities of both sides. He said it was a very useful area deserving a lot of attention for confidence building in the BW area.

Lederberg said he was glad to have been reminded of the history of cooperation in this field. He recalled a moving article by Sabin about the development of the polio vaccine. He said that prior example lent credibility to this as an area for cooperation. He said what should happen next was that each side should refer this and the other proposals raised back to their respective Academies. He said he would also bring back these ideas to the NIH Director, who would be coming to the U.S.S.R. in the next month primarily to discuss cancer research. Lederberg noted that Marks and Rich were both on the advisory committee to the NAS on international activities, so they would have a strong voice in Academy deliberations.

Lederberg asked whether they should talk more about specifics. He said whooping cough represented a problematic situation in the U.S. There was a vaccine, but it caused side-effects because it was toxic. He said there had been a public reaction against it, particularly in Great Britain. The quality of the vaccine may be among the worst that is produced. He said this was a complicated problem that deserved attention, and the appropriate route for vaccination was uncertain. He said there was lively interest in this in the U.S. and the U.S.S.R. Lederberg said diarrheal disease was underestimated as a cause of morbidity in the world, and it would be desirable to have more effective approaches to deal with it. He said the World Health Organization sponsored efforts in other areas such as leprosy and TB. He said the BCG vaccine for TB was now believed to have limited value and this would be another important area of inquiry. He said it would be interesting to have some discussion on this.

Sverdlov observed that this problem of vaccines had stirred a lot of resonance. Woodward expressed his agreement with Lederberg on the importance of diarrheal disease. He said another possible vaccine candidate was encephalitis. He said this was an important area of bilateral pursuit that would help the world at large.

Bennett said it was useful to discuss candidates, but the final choice should be to work on vaccines for specific diseases. He said this should be looked at from the point of view of scientific opportunities. He suggested a mechanism modeled after the program with Japan, which also bore on rewards to the scientists involved. In the program with Japan, they had a panel on viral diseases which would work on two or three diseases at a time. But, they held annual symposia which would be addressed by the most prominent virologists speaking about their work. Their techniques could be applied more broadly. Bennett said the choice of topics should be made by experts who could evaluate the scientific opportunities to succeed.

Lederberg said he was reminded of an Institute of Medicine report on priorities for vaccine development, which he said he would be sure to share with his Soviet colleagues. Bennett said that report was based on both priorities of public health and scientific opportunities.

Marks endorsed what Bennett had said about the importance of opportunities for progress. Marks said research on cell differentiation and the human genome were both areas that provided opportunities for broad collaboration in areas of basic science. He strongly endorsed these areas and said they would be well-advised to focus much more specifically within these areas. For cell differentiation, Marks said an important concept was that of the reversibility of malignancy. He said this was opening an important conceptual approach to treatment, and some labs in the Soviet Union were also involved in this. He said it was so complex and broad that it would require a significant commitment at the clinical level. Sverdlov agreed this was an interesting field from the scientific point of view.

Merzabekov said they had been participating today in a seminar organized as a consequence of expanded cooperation of scientists. He said they should think about cooperation in basic research. In researching the human genome, Merzabekov said there were opportunities to make physical maps of the human genome, to make sequences of the original genome structure, and to understand the operational process of chromosomes and genomes. He said they could begin long-term research in this field; they were at the initial stages of this research, and it was important to begin collaboration at the beginning.

Merzabekov said they had started new research in DNA operation in terms of the human genome in certain tissues. He said it was recently reported that this could be useful in treating AIDS. The inhibition of replication of certain viruses could be of fundamental importance. He observed that one danger of these bilateral meetings

was that some countries might suspect a "superpower condominium," so it would be good if they could do something about hunger by engineering the development of plants and vegetation. He suggested they could include Third World people in these discussions.

Sverdlov said the slides had arrived and they could now hear the presentation by Nikiforov.

Nikiforov said he would like to present material on a special form of Siberian anthrax. He said he was a general practitioner who had devoted almost all of his career to its study. He said it was a source of great trouble in Russia and its danger was still significant today. He said it was endemic in Sverdlovsk, which happened to be the interest of certain researchers, as well as of political interest. Nikiforov said that since 1938, there had been over one hundred fifty recorded cases of animal diseases, and anthrax had been reported in 30 administrative areas in the Sverdlovsk region. He said that against this background, an outbreak in this region could have passed unnoticed, except for the fact that in the Soviet Union, 98% of anthrax in man occurs in a dermal form. However, in Sverdlovsk in 1979, they were faced with a large outbreak of intestinal anthrax. He said they had been unable to totally explain the pathogenesis of this. He said in a previous outbreak in Smolensk [sic] the cause of the outbreak was contaminated sausages, and twenty seven out of thirty seven cases died. Nikiforov said one or two anthrax cases annually was usual for the Soviet Union.

Nikiforov said what was extraordinary about Sverdlovsk was the intestinal form. The outbreak had been preceded by morbidity among domestic animals. He explained that people with private livestock circulated meat and bone flour that proved to be infected with the agent that caused anthrax. It took four to five days to market the flour, followed closely by disease outbreaks among animals. He said some of the sick animals were killed and their meat was sold on the black market, bypassing proper inspection. He said this occurred

mostly in the southwest portion of Sverdlovsk. The first human case was on April 5. It was very serious, with incredibly fast lethal outcomes. He said the disease set on violently, acutely, with unbearable cutting pains in the abdomen which they were unable to control. He said the victims had swollen bellies, bloody diarrhea and vomit, and clinical symptoms of toxic infectious shock including labored breathing (47 breaths per minute), cyanosis, tachycardia, and an unstoppable fall of arterial pressure. He said there were major disturbances in coagulation, fibronolysis, increased urea, and the body temperature rose to 41° centigrade and then fell to 35-33° centigrade. Nikiforov said many patients developed subjective improvement with the fall of fever, but then died within five to seven hours. He said this summed up the general clinical picture of the symptoms of the patients.

Nikiforov said he was flown to Sverdlovsk on April 6. The number of cases kept increasing and so did their lethal outcomes. He said on some days they had to autopsy five or six corpses. In one month, 96 people got the disease, representing the largest single outbreak of this disease in Soviet and Russian history. He said 17 were identified as having the pure form of dermal anthrax, of which six cases had this form complicated by generalization of the process. He said 79 had the pure intestinal form, and out of those, 64 died. Fifteen patients with the intestinal form survived. Nikiforov said that, apparently, this was an extraordinary thing to achieve, since they knew of no other survivors in the world of this form of intestinal anthrax.

Nikiforov explained what they did. First, they took strict hygienic steps to withdraw the infected meat, broadly communicated the danger and warned people not to eat it. Second, they installed promptly a treatment clinic next to a hospital, converted it into an anthrax treatment center and took there all people with fast rising temperature, bad feelings and changes on their skin. He said they

intentionally erred toward hospitalizing more people than probably had anthrax. He said they used the latest available antibiotics in 1979, and used them in maximum allowable doses. They diagnosed a total of 96 anthrax cases, even though they hospitalized and examined five times as many to detect all anthrax cases. He said they were very fast in diagnosing anthrax. On April 10, they obtained bacterial evidence to confirm the diagnosis of anthrax, and the strains proved identical from animals and people. He said the strain was virulent, with sharply defined capsula. It was sensitive to all the antibiotics they had at their disposal, including penicillin. He said they paid maximum attention to bringing patients out of toxic shock, but the most active treatment failed to produce much result. Nikiforov said it was impossible to stop the clinical development of toxic shock and the patients died within twenty-four hours after the shock developed. He said the life of the infected persons was only 24-48 hours, and it took intense efforts to extend that period even a few hours.

Nikiforov said there was edema of the brain and of different tissues. There was acidosis indicated by an extremely low pH of 6.8. Early introduction of active treatment allowed them to save 15 patients. He said they were unable to bring patients out of toxic and infectious shock. Nikiforov said he would show some slides, and he apologized for the poor quality of some of them, explaining that sometimes he had a shortage of color film, and that the situation was such that taking pictures was not the most important thing. He then showed a series of slides showing skin lesions and autopsy slides showing severe damage including extensive hemmorhage to the intestines, spleen, lungs and brain of several anthrax victims.

Sverdlov thanked Nikiforov and said it was time for the lunch break. After the break, Sverdlov said Nikiforov was available for questions now, and that the Americans would meet with him further the following day.

Lederberg said he would have more detailed questions for him the following day, but did have one question now. He said the epidemic must have been very difficult to treat, because it had many unique qualities. Lederberg asked whether lab studies had been done on the strains they isolated in the epidemic to ascertain whether they were extraordinarily virulent.

Nikiforov said they conducted research to determine if it was anthrax or some other infection, and to learn its sensitivity to antibiotics and discover regular features of the strain.

Woodward said he and Nikiforov had a good discussion during the lunch break and summarized for the group the results of that discussion. He said each patient that had the cutaneous form had contact with animals. The man with the swollen arms shown in the slides had been given steroids which had no effect. He said there was a relationship between earlier treatment and earlier recovery. Woodward noted parallel features with hemorrhagic fever, where once shock appeared, steroids also had no effect.

Nikiforov said that all cases of skin form of anthrax in the uncomplicated form survived, and that penicillin was quite successful. He said the treatment of toxic shock included treatment with intravenous medication and large doses (up to 10 grams) of steroids. He emphasized the necessity of administering large doses because the sensitivity of tissues to these steroids had changed. He said they used colloids and blocking "ferments." They administered large quantities of intravenous fluids plus calcium chloride and insulin when they recorded DVC (diffuse vascular coagulation) syndrome. Nikiforov said they administered antibiotics, using a wide range in the case of toxic shock. However, they could not find any that were clinically effective in diagnosed cases, including tetracycline and penicillin. They also used cardiac stimulants. Nikiforov said finally the toxic shock was complicated by kidney failure and insufficiency. He said the difference between

hemorrhagic fever and anthrax was that with anthrax, the kidneys were the first to be damaged. He said that with hemorrhagic fever in Korea, the kidney problem was secondary, occurring after toxic shock. He noted that kidney insufficiency was now a common problem in the Soviet Union, characterized by strong hemorrhaging around the kidneys.

Lederberg thanked Nikiforov for his comments and said he looked foward to discussing more of the details the following day. (See attached appendix.)

Sverdlov said they could now further discuss the proposals on the table.

Lederberg said they had discussed earlier in the morning work on the human genome. He said one subset of that issue was the suggestion for a crash program to sequence the entire human genome. He said the idea of achieving total understanding of the entire genome was an important metaphor, but he had problems with doing this to the exclusion of other scientific research. He said captivating the image of this one highly mechanized program could replace thought with brute force. He said there was a place for some investigation, for instance to decide to map one X and one Y chromosome. Still, there was the problem of deciding whose X and whose Y chromosome to map. There is no average genome, but perhaps a consensus genome. He said it could be as interesting to focus on the differences as well as to get the totality of it. He said this was an oversimplified statement of the objectives - the problem of focus on certain loci of genetic disease, half a dozen loci with genetic polymorphic diversification. He said the concept "the human genome" became faulted when you looked more closely. For example, the mechanism of antibody formation was based on somatic genetic diversification. Differentiation in other systems may be comparable, i.e. in the neurosystem. There are also examples in the development of invertebrates - gene amplification. Lederberg said it would be

better therefore to state proximate objectives and landmarks; he said he thought this inevitably would happen. He said he did not know if there was widespread agreement with this idea.

Lederberg asked what were the high priority problems. He said fixed costs were paid in medical care, so we already got information about sickle-cell anemia, for instance. He said they were encouraged to do those investigations because there was a medical reason. He said the structure of a protein could be altered by the change of one amino acid. DNA changes could be correlated with the protein outcome. He said there were many polymorphisms. He said we came back to the fact that polymorphism had a relation to medically significant syndromes.

Lederberg said they had more findings in new methodology for tagging chromosomes, allowing mapping from parental to the F1 and F2 generations. He called for further study of genes that have to do with mental traits like schizophrenia. He said they have had positive experience in learning about chromosomes relating to cholesterol. Receptor defects are involved with hypercholesterolemia. He said people were looking for polymorphisms at these loci. These contributed to most basic issues. Lederberg asked what were the priorities? He said he thought there was a unique opportunity in psychiatric disorders, that they have had no good way to trace genetic factors. He said this was a favorite topic of his, but required populations willing to disclose information about psychological disorders.

Rich said he would make a few comments, since he had been involved in the U.S. in discussions of the human genome. He said the issue had a technical and a political dimension. The political dimension related to the larger issue of the funding of science in the U.S. He said there were a number of groups within the government and outside who were interested in this project. He said a meeting at the NAS brought these groups together to discuss how to proceed. He said there was also a scientific problem. We could now sequence

small segments of DNA. The process was labor intensive, moderately expensive and slow. Some people would like to do the whole job - not a crash program, but a long-term, expensive one. Rich said the decision was made that sequencing would require machinery that would automate sequencing, which would not be available for five to six years. In the interim, the plan was to use large pieces of DNA, to isolate individual chromosomes and break them up further into an ordered set of overlapping cosmid clones. He said that with 50,000 base pairs, it would take 60,000 cosmid clones to organize three billion nucleotides of the human genome. Rich said this ordering of the human genome could be accomplished with modest cost using well developed technologies. In the end, one would have fragments of DNA in an ordered array on filtered paper equaling one chromosome in a series of 1000 dots. He said the point was that you could take a chromosome and identify where in that chromosome a gene is found with resolution 10^2 times better than what we now have. Rich said this was important for mapping. It was not yet sequencing, but it was making ordered large groups. He said this would facilitate investigations of genetic diseases. He said the advanced machines were likely to be available in five to six years. Japan has been developing a machine since 1981 which was being made by three companies in association with people from the University of Tokyo. The plan was to automate the existing technology with robots, which would allow a computer printout of the sequence. Rich said it would be erroneous to automate sequencing now with the primitive technology. He said he was not keen to divert research money into a project of this type. A consortium of U.S. government agencies was in the process of forming to act as a clearing house for international information. He said this was a collective activity. It was not a crash program, but a program with some planning. He said it would transform their ability to understand some diseases. He said they had the methodology and would have the information and

would have to learn the meaning of this sequence. Rich said it should be an international effort.

Lederberg acknowledged the arrival of Roald Sagdeev.

Bennett asked Rich whether there would be any international members of the consortium. Rich said the problem right now was one of too many voices in the U.S., which they were trying to meld into one voice before inviting foreign participation.

Sverdlov noted that discussion of sequencing the human genome was going on intensively, noting one West German who was doing this. He said he agreed with Rich that the process was still a concept. He said it must go on and that they should think about how to coordinate the effort so that individual efforts did not overlap.

Mirzabekov said the use of equipment and machines for this was not a scientific, but a technical problem. He recommended concentrating their efforts on learning about differences between different loci, and after that they could deal with sequencing of the entire human genome.

Marks asked if sequencing the human genome was a high priority for the Soviet Academy of Sciences. Sverdlov responded that they had no program for this, that they were still discussing it. He said they agreed with Rich that complete sequencing was most unrealistic today, and that one runs into difficult technical problems. He said they were working on selected points in his lab and in Ovchinnikov's. He said they had determined sequencing of one of the human genes - the protein responsible for transportation of potassium. Sverdlov said each researcher was engaged in the field that interested him most, but there was no uniform plan to sequence the entire human genome. He said Lederberg had talked about the fascinating problem of the genetic/psychological disorder relationship. He said they were working on programs to compare different human genomes. In his institute, three researchers were comparing a human with a chimpanzee genome to find the principal

differences between humans and apes. He said they found some sequences they believed to be typical of a human. They had not done it with the ape yet, but this was the work they were engaged in now.

Sverdlov suggested they now sum up their discussions. He noted that Sagdeev had joined them, so they had a representative from the Committee of Soviet Scientists for Peace, Against the Nuclear Threat. Sverdlov said they had discussed candidly the possibility of cooperation and ideas for topics. He asked Lederberg to sum up.

Lederberg said the atmosphere of their discussion reflected a fresh start on this issue. He said this had been reflected also in the BW Convention Review Conference and the report they had heard of that. He said they were dealing here with the prevention at an early stage of a problem that could become uncontrollable in the future. He said they had propitious ground to strengthen the Biological Weapons Convention. He said the atmosphere of openness was to be commended. He said they had much further to go, but this was start. Lederberg said he would leave behind information including a listing of all research programs funded by NIH and the Defense Department Annual Report on the Chemical Warfare and Biological Defense Research Program. He said the latter provided details on the U.S. research program, and that even though some of the attributions were vague, having this in the public record permitted questions for more details and debate. He urged open publication of similar information in the Soviet Union.

Lederberg expressed appreciation for the poignant talk by Nikiforov on his experience dealing with the anthrax epidemic in Sverdlovsk. He said in the last few months there had been more information on this. He said things would not change overnight, but the mood was right and this group's involvement in these issues should have a positive effect.

Lederberg said they had earlier discussed areas of scientific cooperation. He said the most effective programs would be those that had medical benefit for all and a symmetry of input from both sides.

He said there were still difficulties over human rights, explaining that for many individuals in the U.S. this remained an important issue that impeded willingness to undertake scientific collaboration. However, he said that programs of great medical benefit and potential for reciprocity in input could overcome some resistance and that his delegation would support those.

Lederberg said he found this meeting personally, professionally and technically informative. He suggested waiting until after the BWC April meeting of experts to decide the next steps of this group. He noted he could discuss this with Sagdeev when he is in Washington for the April CISAC meeting. He said they would want to see the formation of specialized expert groups on whichever programs proved most appropriate. He thanked Sverdlov and the entire Soviet delegation for their participation.

Sverdlov expressed for his entire delegation gratitude for this friendly and informative dialogue. He said they had found many common points of understanding, that it was obvious that none of them wanted biological weapons to exist and that they all wanted confidence-building measures and collaboration that would further these objectives. He said he would stress the basic points from their discussions which they would each have to report to their respective Academies: 1) Sverdlov and Marks suggested a committee in each of the Academies be constituted to coordinate their activity; 2) It was important to determine the most humanistic and important subjects for collaboration, and they should be prestigious; 3) Specific suggestions included; a) structure of the human genome; b) problems of the development of cell differentiation in cancer; c) vaccines; d) genetic engineering of plants. Sverdlov said they could select specific subjects within these topics. He said the projects should be supported by the two Academies and be continued until positive results were achieved so as to generate and expand confidence. He said he hoped this summary could form a basis of their research, and if so he would like to have it typed and distributed.

Lederberg said he thought it would be wise not to have a joint communique or declaration, stressing that it is the policy of CISAC not to do so.

Sagdeev thanked everyone for the constructive and fruitful atmosphere of the meeting, and for making this first meeting a success. He said the monopoly of those in physics, mathematics and political science in meeting with CISAC was lost, but not regretted. He said that nothing was as useful as prevention.

Sagdeev recalled that CISAC had been at work for five years, and that their ninth meeting took place last week. He said those meetings were always candid and businesslike, and that they always began with a stocktaking of the current balance of forces and examination of trends in the key technologies and their possible effects on strategic stability. He said for many years these matters had been confined to the balance of offensive forces - accuracy of terminal guidance as a trend which could inhibit retaliation, cruise missiles, etc. Sagdeev recalled that at Geneva, their two leaders agreed that the nuclear threat had to be averted. He said that unfortunately, there were two views. One side called for liquidation of the nuclear threat, a course that was difficult but necessary. The other side said it would be nice to stick with half measures until we could find a way to make ourselves safe against nuclear weapons in the future, meaning SDI. Sagdeev said the comparison of these two viewpoints was central to the international debate and the upcoming Reykjavik meeting. He said the Soviet side believed in the relationship between offense and defense, and that unless we abandoned the idea of superiority, it would be difficult to accede to liquidation. He said that so far the ABM Treaty had deterred both sides from this race. He said they believed there were advances so that today at the government level they could say that high levels of verification had to be adopted. He said this had been done at Semipalatinsk. Gorbachev made the open labs proposal to constrain

technological competition. Sagdeev said this group's confidence-building measures were steps toward this.

Sagdeev said the Soviet Union was abiding by two unilateral moratoriums: on anti-satellite testing and nuclear testing. He said the CISAC discussion must have had a more technical/military/strategic character than the BW discussion because the nuclear disease was more advanced. He said they had agreed that cooperation was needed. He said he would repeat an analogy he made at the CISAC meeting: The U.S. administration said the ABM Treaty allowed development and testing. He said the Soviet outlook was that they should not leave the limits of fundamental research, which he said was analogous to "harmless flirtation" under a marriage contract, while the U.S. position was a more serious indiscretion.

Sagdeev said the Soviet Academy would like to see the BW meeting continued. He said their Academy would gladly accept the invitation to continue this discussion next April.

Sverdlov clarified that his earlier statement was meant to be typed up and distributed as an informal "memory jogger" for both sides for discussion with their Academies, but that it was not meant as a formal joint statement. He said he would add to the list Dubinin's suggestion for joint research on the role of mutagens in the environment. Sverdlov reiterated several times how useful this summary memo would be. Lederberg said he thought it would be okay so long as it was not an official communique. Sverdlov said that was good, and that he would be sure to get this informal paper, a memory jogger for forthcoming discussions, to Lederberg before his departure (see attachment #4).

Sverdlov distributed gift books to the group, and suggested they have tea before touring the institute.

The meeting adjourned at 5:00 p.m.

Lynn Rusten

PRIVILEGED

Appendix: Special Session with Dr. V.N. Nikiforov and Dr. Olga Yampolskaya

A special informal session was held with Dr. V.N. Nikiforov and Dr. Olga Yampolskaya from 10:00 a.m. - 12:00 on Friday, October 10, 1986, at the Shemyakin Institute of Bioorganic Chemistry. Present at the session were: Dr. Sverdlov, Dr. Lederberg, Dr. Bennett, Dr. Marks, Dr. Woodward, Lynn Rusten and an interpreter from the Academy of Sciences of the U.S.S.R. Dr. Nikiforov and Dr. Yampolskaya from the Ministry of Health treated the victims of the 1979 Sverdlovsk outbreak. This appendix summarizes the information they shared about the Sverdlovsk epidemic.

Dr. V.N. Nikiforov said he was Chairman of the Department of Infectious Diseases, Central Institute for Post-Graduate Training, Moscow. His Department had 360 beds, and 280 M.D.'s were trained per year in infectious disease. Twenty-eight thousand physicians were trained per year in the Post-Graduate Institute which had 110 departments. Training periods were for 1 to 4 months. He said there were 1,200,000 M.D.'s in the U.S.S.R. Olga Yampolskaya said she was an assistant in Nikiforov's Department. She spent half of each day caring for patients and half of each day providing practical instruction for M.D.'s. She said her research was mostly involved in clinical observations on virus hepatitis, and the Institute of Virology performed the laboratory studies. Nikiforov's Department had two docents, six assistants (of which Yampolskaya was one) and twenty-two other M.D.'s.

Nikiforov said he was generally called to consult on all outbreaks of infectious disease in the U.S.S.R. In the Sverdlovsk epidemic of 1979, Nikiforov was called by Professor Kortev, Chairman of Infectious Disease in the Sverdlovsk Medical Institute. Nikiforov said he was called two to three days after the first two cases of acute severe illness were seen. When Nikiforov arrived in Sverdlovsk his first impression was that these patients had a very serious disease which was strange. He said his first impression was that it was due to an intoxication -- but the high fever made him consider infection. After being present at the first autopsy (on April 6 or 7), he thought it might be intestinal anthrax. Nikiforov said that Dr. Peter Burgasov, Deputy Minister of Health for the U.S.S.R, was involved in providing overall direction for dealing with the epidemic. Burgasov had retired one week prior to this meeting. Dr. Ivan Bezdenejnich, Chief Epidemiologist of the Russian Federation Department of Health, was the epidemiologist called to Sverdlovsk and responsible for accummulating the epidemiological data during the Sverdlovsk anthrax epidemic. Nikiforov said Bezdenejnich died during the month prior to this meeting. Yampolskaya said she learned about the epidemiologic data when she heard Bezdenejnich's presentation to Professor Matthew Meselson in August 1986.

In response to a question, Nikiforov indicated there were some instances of multiple cases in a single family, but did not have the exact data. These data were obtained by Bezdenejnich. Nikiforov said he had seen about 100 to 120 cases of gastrointestinal anthrax prior to the Sverdlovsk outbreak, and all had died. Thus, he said that fifteen of seventy-nine in the Sverdlovsk outbreak were saved was unusual. He believed more individuals ate contaminated meat than became ill and died. He believed illness was determined by dosage of anthrax organism and state of health of the host. He said variation in preparation of food might also be a determinant. Nikiforov said the Sverdlovsk epidemic took on a political coloring for him only when Meselson arrived in August 1986.

Nikiforov said that only one child - a six year old girl - fell ill. Yampolskaya gave the following age distribution of the victims:

Age	Number of Victims
under 20 (female child, age 6)	1
20 - 29	11
30 - 39	10
40 - 49	32
50 - 59	10
over 59	12

Nikiforov said there were 17 dermal cases, and out of those 6 became generalized. He said the usual percentage of dermal cases that became generalized was 5%. He said that in the Soviet Union, dermal cases which were not serious did not regularly go to the hospital.

Of the lethal cases, none of the victims had skin lesions. Nikiforov said there was nothing unusual about this particular strain of anthrax. He said there were many degrees of virulence of strains in the Soviet Union, and confirmed that there was a correlation between the thickness of encapsulation and the virulence of the strain.

When asked if any examples of this strain had been preserved, Nikiforov said perhaps, but he did not know for sure. He did not preserve the strain. He explained there was a special institute that dealt with dangerous strains and that perhaps it had kept samples.

Nikiforov said he did not know how air samples had been taken in the hospital rooms of the victims, but said he thought the surfaces and dust had been analyzed. He said cultures of air samples were negative.

When asked if there had been other cases of anthrax in Sverdlovsk since the 1979 incident, Nikiforov said there had been no cutaneous cases in the city of Sverdlovsk. However, he said that in Sverdlovsk

oblast there were some cases every year, and that there had been three cases of cutaneous anthrax and cases of gastrointestinal and generalized form of the disease.

When asked if he had ever seen a case of pulmonary anthrax, Nikiforov responded that he had seen such cases in Albania, where he had worked for three years. When asked what the lungs looked like in those cases, he said the main symptom was hemorrhagic edema of the lungs, and that they did not resemble the brain shown in his autopsy slides the previous day. When asked if he was surprised that the patient with the severely affected brain did not have damaged lungs, Nikiforov responded that in that case the lungs were affected partly, that there was some hemorrhaging.

Woodward asked whether a vaccine program was begun. Nikiforov explained that the Soviets vaccinate two million people every year against anthrax. He said that thirty districts in the Sverdlovsk region were regularly vaccinated in the spring, but that if there were an outbreak, then the plan called for immediate vaccination regardless of the time of year. Nikiforov said, then, that in this case the vaccination time was changed to take place right after this outbreak, but he emphasized that this was not a special program of vaccination, but an additional vaccine ditribution at that time, as called for in their plan. (There was some confusion in Nikiforov's answer to the questions on this topic.) When asked whether the vaccine against anthrax was effective in people, Nikiforov said it was his personal opinion that it was not effective.

Asked which antibiotics were used for prevention, Nikiforov said that for the families of the victims it was his personal opinion that antibiotics in the tetracycline family were most effective, though he had no statistics to validate this. He said there were some people who received antibiotics but fell ill. In fact, he said those people had very severe symptoms and did not survive. In response to another question, Nikiforov said there were variations in the time of onset when there was more than one case in a family. As best he could

recollect, Nikiforov said there were perhaps ten families which had more than one case. He said he could look up the exact number later. When asked by Marks if he had lectured about this outbreak, Nikiforov responded that he had lectured many times in Sverdlovsk about this epidemic and that he gave this lecture and the slides he had shown this group to many Soviet doctors.

Lederberg urged Nikiforov to publish this information in detail because it was so important and scientifically interesting. Lederberg then spent a few minutes explaining to Nikiforov the news sources (New York Times article of July 16, 1980, was given to Nikiforov) of the rumors about the Sverdlovsk outbreak resulting from an accidental release of anthrax spores from a closed BW facility. Lederberg asked if they had heard these kinds of rumors when they were in Sverdlovsk. Yampolskaya responded that she did not hear these rumors in Sverdlovsk, but did hear them afterwards in Moscow.

Lederberg asked them how these rumors might have started. Sverdlov interjected with an anecdote of how he had taken in Soviet evacuees, whom he did not know, from Kiev to stay with him after the Chernobyl accident. One of these women was very concerned that her hair would fall out, and she asked for daily blood tests. She refused to believe that the results were negative, and insisted on independent confirmation. She also told Sverdlov that she was sure her friend in Kiev had lost her hair. Sverdlov reported that the friend later came to visit, and had a full head of hair. He told this story as an example of how rumors spread. Nikiforov agreed that the New York Times article about the Sverdlovsk outbreak which Lederberg showed him reflected similar kinds of rumors. Nikiforov related a story about a cholera epidemic, during which he heard reports of his own death over the radio.

In the New York Times article, a map of the Sverdlovsk region was reproduced showing Sverdlovsk and a town, Kashino, 18 miles SE, which was cited as a locale of additional cases of anthrax. Nikiforov said he did not know of such a town. Yampolskaya also indicated she had

not heard of any of the three towns indicated on the map reproduced in the New York Times -- Polevskoi, Sysert or Kashino.

When asked about the sanitation measures that were taken, Nikiforov said he did not know in great detail. He said they did burn old buildings where the contaminated animals had been kept. He said these were mostly in the suburbs, where people kept their private animals. He said the diseased animals were also burned, not buried. He said only one or two sheds within the city were burned, but many in the suburbs were burned. Yampolskaya recalled that they were in the south part of the city.

Lederberg said there were rumors that the hospital was taken over by military personnel. Nikiforov adamantly said there was not one military person there, not even one policeman. He said he thought the epidemic station did the burning, not the military. In response to a question, Nikiforov said these sheds were burned in the middle and end of April.

Sverdlov noted that it was not unusual for the military to be requested to help in some cases. He referred to a fire that took place in Zagorsk just a few days ago, at which he said military men were used to help fight it. Sverdlov said that was a natural thing and did not mean that the burned structures were military properties.

Woodward asked how dermal anthrax was treated. Nikiforov indicated he treated dermal lesions with penicillin -- 30 million units every 24 hours, until the fever disappeared and the dermal lesions regressed, usually in seven to ten days. Nikiforov said he believed patients with dermal lesions did not gain immunity to future infections.

Lederberg expressed his gratitude to Nikiforov and Yampolskaya for their time and willingness to discuss this outbreak in such great detail. The session adjourned at 12:00 p.m.

Lynn Rusten

October 12, 1986
Joshua Lederberg

A subcommittee of the NAS/CISAC Committee devoted to BW met in Moscow October 8-10, 1986. A list of the Soviet and U.S. participants is appended.

This is a preliminary transcript of my impressions of the meeting.

There will be a detailed transcript of the notes taken by Lynn Rusten and others of our delegation. This is a preliminary account of first impressions. I stress here matter that pertains particularly to problems of verification. In fact with the exception of Ustinov the Soviet scientists present at the meeting had very little experience or background in BW problems. I asked Ustinov about that and he said that he and Antonov had given that same group a briefing approximately a week earlier. It does happen then that this meeting has become a way of introducing a significant group of Soviet scientists from the civilian sector into some thought about BW arms control problems.

It is important to note that Dr M. Meselson of Harvard had been invited by the Ministry of Public Health to visit Moscow at the end of August, and was briefed for 3 days about the 1979 anthrax epidemic at Sverdlovsk. His respondents were Burgasov (Deputy Minister of Health), Bezdenezhnykh, (an epidemiologist from the RFSR health ministry), Nikiforov and Yampolskaya (both physicians from Moscow. Meselson talked to our group for about two hours in New York on September 22. The 5-year BWC review conference was held in Moscow at the end of September, and I was briefed about that by Robert Mikulak at ACDA.

Bochkov did not attend: "away on business." Sagdeev attended briefly at the end of the second day and at one of the luncheons. In addition I had an extended conversation with him at his home for dinner on Friday evening.

The meeting was held at the Institute for Bioorganic Chemistry, still under construction. Its director, Yuri Ovchinnikov, was away on business, in Europe. He has been rumored to be in poor health, but is evidently asymptomatic according to Dr. Rich, who saw him recently.

Although Ivanov is the deputy director of the Institute Sverdlov acted as co-chairman on the Soviet side. In a brief discussion of the agenda he was quite agreeable to devoting the first day to questions about BW arms control and the second day to cooperative research programs. It was evident from the outset that the latter was of the greatest interest and incentive to the group. I asked him whether it would be possible to invite Nikiforov to the meetings and he said that that had already been arranged and he would be available for the Thursday morning. Burgasov it turns out has retired only very recently (to the great delight of one of the Russians who said that he had been extremely restrictive, for example forbidding the publication of any information about AIDS in the Soviet Union). Bezdenezhnykh has died of a heart condition during the last few weeks. Nikiforov said that he was already in very bad health at the time that they had talked to Meselson.

My opening statement is attached. There was little concrete response

about my remarks on means of verification. By common agreement the conference of experts to be held at Geneva in early April would be the place to negotiate detailed proposals. There was, however, general agreement about the need for confidence building and a very strong affirmation that medical scientists did have an important responsibility to be sure that BW was indeed controlled and that the Treaty be strengthened in a way to assure mutual confidence. There was a repeated echoing of the thought that extensive scientific cooperation between the U.S. and the USSR in fields related to BW would contribute greatly to providing that mutual reassurance. There were really no dissonant notes of any kind: the only approximation to it was from Schvedkov a political scientist from the Institute for the US and Canada. He said the Soviet public has reason to worry about what the US military is doing. He quoted press reports about happenings in the Pentagon. He also quoted a story in Graham Allison's book on formation of foreign policy "that Nixon's proposals for BW disarmament had been resisted by the US Army. Also, Gorbachev had made the furthest proposals on verification in his speech on September 9th that would allow for an international network for verification of the nuclear test ban. Indeed a supra national network that might be viewed as a prototype for what could be done in the BW area. The Third country problems have to be looked at in a broad international context. Terrorism is generated by international conflicts and is the recourse of the weak against the strong. As to the BWC, suspicions are not supported by the fact and the convention is working. "

Nevertheless everyone else including Ustinov seemed to resonate with the conclusions of the BWC review conference in Geneva that called for strenthening of the BWC and even Schvedkov's remarks were made in a much milder tone than what usually comes forth from his institute. I had the impression that he had a perfunctory message that he was obliged to deliver and that nobody was paying very much attention to it.

My own remarks about the importance of affirmative cooperative verification, that each side had in his own interest the promulgation of openness and reassurance were responded to positively by every other member of the group to whom I talked. (Lisov from the Ministry of Defense did not utter one word during the entire meeting. He had a rather dour expression throughout, which may or may not have any significance.) The entire proceedings were videotaped. Lynn Rusten's request to get a copy was at first rebuffed bureaucratically "that it had not been arranged beforehand. Videotapes are stringently controlled." Sagdeev reassured her however that it would be done.

During the discussion I brought forward the Wall Street Journal article on BW defense budgets of the Pentagon. This was not new information but I think that some of our colleagues were a little startled to see the depth and candor with which this kind of discussion was published in the United States. At the very end of the meeting I also deposited the DoD Report to Congress on the same subject as an illustration of the openness that prevailed on our side. The scientists were hardly in a position to make promises about future Soviet behavior in this

direction but I'm sure this was useful and appropriate information for them to have.

Nikiforov arrived at the very start of the Thursday session but he had left the slides back at his institute (!). We agreed that it was important that they be included as part of his presentation and so he agreed to wait and to sit in in our discussion for the hour or so that it took for them to arrive. Nikiforov heads the department of infectious diseases at the Central Institute for Postgraduate Medicine in Moscow. This has a hospital of 360 beds; there are 110 departments which give courses ranging from a few weeks to many months to a total of 28,000 physicians every year (There are 1.2 million physicians in the Soviet Union). His department provides teaching for about 280 postgraduate physicians in the field of infectious diseases. Yampolskaya is an assistant in that department, one or two layers removed from Nikiforov. It is not unusual at all for them to be consulted on medical problems that arise throughout the Soviet Union. In this case they were called by Professor Kortev from the Medical Institute in Sverdlovsk about the two cases of disease with very strange onset. (In this precis, I will omit most detail that duplicates what was recorded by Meselson. As stated before, a full report will be provided later.)

Acute and severe abdominal pain and high fever suggested to them some form of intoxication. It was only after the post-mortem that they were able to confirm a diagnosis of intestinal anthrax. Throughout his career Nikiforov has seen many sporadic cases: perhaps 100 to 120 altogether, which had much the same picture. He said that previously they were all fatal so that he felt gratified that they were able to save even the small proportion that did survive in this case. There was no precedent for an outbreak of intestinal anthrax of this dimension in history. He did not think the clinical course of the disease was unusual for that particular diagnosis. They are not particularly research oriented and he does not believe the strains were saved. When tested contemporaneously they were sensitive to all the usual antibiotics including penicillin. The bacilli did have a very thick capsule which is closely associated with very high virulence in anthrax.

We had not as yet received Meselson's notes of his August trip. We therefore had only a few limited points of testimony to use as the basis of more detailed questioning.

At my request, Nikiforov met with us again on Friday, and asked Olga Yampolskaya to join us, together with Dr. Sverdlov. Her English is reasonably fluent, and this facilitated the discussion. When we asked them questions about epidemiology they stressed that they had no first hand information of those studies since their task was the clinical care of the individual patients in the hospital. Besides Yampolskaya there were five other assistants who came up at various times from Moscow and they had 22 local M.D.'s also helping in the management of the outbreak. Yampolskaya in fact had no detailed knowledge at all about the epidemiology until she heard Professor Bezdenezhnykh at the

meeting with Matt Meselson. That was the first time that she had any inkling that there were political overtones to the epidemic. Neither of them had the detailed case records at their own disposal. Yampolskaya thinks she was selected to help in the briefing because she had kept certain personal notes on a few of the individual cases. We did not think it fruitful to pursue very strongly the epidemiological side in Bezdenezhnykh's absence. We strongly urged, and Sverdlov echoed this, that it would be of great scientific (not to mention political) importance for a detailed account of the epidemic to be published. Nikiforov said that he had come to that conclusion himself. It was urged on him that he get a young epidemiologist to assist him in the compilation of the detailed records.

On the clinical course, Nikiforov gave very dramatic account of the development of the syndrome. When it reaches a stage of toxic shock it does include cyanosis and dyspnea but this is only fairly late in the development of the disease. As to the family distribution Nikiforov thought he remembered one family with as many as three victims. Perhaps ten of the families of the total had more than one case. He has no explanation for why a rather small proportion of those who presumably ate the infected meat actually came down with the disease. As the cases began to accumulate they became very concerned about trying to collect them at the earliest possible point so besides the public health measures with which he was not directly connected (the circulation of notices warning about meat contamination; destruction of sources of infection) he encouraged the hospitalization of essentially every case who presented with fever or other promonitory symptoms. He said that in total perhaps as many as 500 individuals with even mild fever were hospitalized for a time in order to enhance the opportunity of catching any new cases as early as possible. He concluded that penicillin was as effective as any of the other antibiotics but they had tried a variety of broad spectrum antibiotics not to leave out any possibilities. They used steroids in massive doses to attempt to mitigate the shock syndrome. (There is a mistranslation in our copy of the 1980 paper: prophylactic antibiotics were given to not by family members of the cases.)

Some of the gross pathology that he desccribed was quite impressive. One set of pictures showed multiple lesions on the tongue and stomach which he believed were primary sites of infection by anthrax organisms.

At one point near the end of the discussion on Friday morning Nikiforov left an opening with a remark about the political colorations so we pressed him a bit more closely. He was unaware of any military involvement of any kind. There were no military or police in the hospital. He thought that conceivably they might have played some role in the public health management for example in arranging for the burning of some sheds in which infected animals had been kept. He said that most of these would have been in the suburbs in the south probably about two weeks after he arrived in the city. We showed him The New York Times article (dated '80) giving the emigres' report of the outbreak. He only seen it the day before, namely the copy I had given

to Sverdlov. He never heard of Kashino, reported there as a suburb of Sverdlovsk which was in the path of the airborne plume. As to the rumors, he was not aware of any at the time. He said he had had some hint of some fuss later on but had paid no particular attention to it. He put what he read in The New York Times article as the "Mark Twain Syndrome", recalling that he had once been involved in a cholera epidemic. He heard over the radio that the entire medical team including himself had been killed by the disease. "Reports of his own death were grossly exaggerated". After Chernobyl, families had all kinds of rumors about what was happening to them based on their fear of radiation.

Yampolskaya said that there were lectures from time to time in Sverdlosk at the Medical Institute and in Moscow, where the outbreak was used as teaching material and there must be dozens if not hundreds of medical residents who have heard about it.

I was left in very little doubt that they had been describing their personal experience in the management of an epidemic of intestinal anthrax. For sources of corroboration it will be necessary to go into the epidemiological aspects of the disease and this information is now in the hands of the Ministry of Public Health. It is certainly an obstacle that Bezdenezhnykh has died and whether Burgasov's successor will be more or less amenable to the distribution of those records remains to be seen. Without yet having seen the internal report I did not have any grounds to proceed very much further in my own questions and I made no effort to get in touch with the Ministry. I'm sure that Ustinov would be very cooperative in responding to any requests to try to reach that channel. Official diplomatic sources should request documentary material from the Ministry of Public Health.

My own private thoughts are that the whole question of the anthrax epidemic is a secondary issue. The story told so far appears to be internally consistent and not in sharp violence with the other available data, although these can be interpreted in different ways. It would be very easy for other observers to panic about the nature of the epidemic particularly if they had reason to believe that there is indeed a military BW facility near Sverdlovsk. That question is after all not touched by any of the medical questions that we have addressed here so far. If there were also military personnel who had succumbed to the infection, whatever the source, they probably would not have been treated at the civilian hospital; so there is not a necessary contradiction with Nikiforov's statements.

The primary question, verification of the nature and functions of the military facility will have to be addressed at other levels. But it would be an interesting test of just how far the Russians are willing to go in "openness" to ask whether there is any form of inspection of the suspected facility to which they would be agreeable.

Meanwhile, again as a personal view, I believe that it would extremely imprudent on the part of USG to continue to refer to the epidemic as evidence of violation of the BWC. Separate questions are a) the

channels through which the Soviet Union is responding to its obligations for consultation under the BWC and b) the primary allegation, which may have nothing to do with the epidemic, of continued BW production activities at Sverdlovsk in violation of the BWC. It must cause them much embarassment that a) anthrax remains endemic in Russia, and b) that public health safeguards had broken down, especially in the management of the bonemeal plant!

If we continue to refer to the epidemic per se in the face of the evidence they have presented, we face a) discredit from third parties, and b) [if it was indeed foodborne] the consolidation of Soviet views that we are not serious about our concerns for verification, but propaganda-motivated. It may be difficult to reach a standard of evidence that finally settles the matter and would require an affirmative retraction on the U.S. part; but there may be no need to reach that if we simply withhold further adverse comment about the epidemic. There remains every reason to demand satisfaction about facilities suspected of being BW-oriented; and the current Soviet mood may bend them to some accomodation by way of some form of inspection

If the epidemic itself is to be pursued, diplomatic channels might explore other kinds of corroboration: a visit to Sverdlovsk, if done at all, could focus on further conversations with the medical personnel resident there who were involved in treating patients. One might also be able to interview some of the few survivors and perhaps some family members of fatal victims to try to get some more detail about the clinical course of the disease.

Emigre sources might be interrogated more closely on the factual evidence of pulmonary vs abdominal involvement; also how they knew that the first casualties were military.

I did ask Nikiforov if he had seen inhalation anthrax himself. He said yes some years ago in Albania he had encountered a few cases of it. The distinctive pathological feature is involvement of the lung parenchyma which he said he did not see in the autopsies at Sverdlovsk. Fibrinolysis and plural hemorrhage as well as hemorrhage in every other organ system including the brain are characteristic of the systemic form of the disease whether of intestinal or other origin. He had graphic autopsy pictures of these features.

Dr. Woodward and Dr. Bennett could add with the benefit of their personal experience in pathology their impressions of what was being said.

Nikiforov and Yampolskaya have, I think, said all that they know and remember and they do not have more by way of their own records. Further investigation would have to stress the epidemiological aspects which would involve separate negotiations with the Ministry of Public Health, through governmental channels. We should not be too optimistic that they have the more detailed records that would conform to our expectations.

Our further discussions with the Russian academicians concerned areas that would be promising for scientific research cooperation. Dr. Sverdlov's resume is a fair account. It would further both medical science and cooperative verification of the BWC if some of these proposals could be implemented.

I am checking: Meselson had evidently phoned the Soviet Ministry of Health about our impending visit and this was undoubtedly how it came about that Nikiforov was primed to talk to us.

M.M. also recalled that the Russians had mentioned radio broadcasts warning of contaminated meat.

Date NYT, WSJ articles

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE NEW YORK, NY 10021

JOSHUA LEDERBERG
PRESIDENT

October 23, 1986

Dr. Ivan Bennett
Professor of Medicine
and Dean
New York University Medical Center
550 First Avenue
New York, New York 10016

Dear Ivan:

Thank you for sending me the dissertation: I will certainly read it with great interest.

Lynn Rusten is moving very quickly towards a draft transcript of our meeting in Moscow and you should be getting a copy of that pretty soon for your own comments and corrections.

Meanwhile Paul Marks and I have been trying to put the question of scientific exchanges on vaccines etc. up through channels, so far the NAS and NIH. At some point we are going to have to counter the resistance that will almost certainly rise on technology export restriction policy considerations; and your input at that time would be particularly helpful.

I've also been briefing Bob Mikulak and agency people about my impressions of the meeting and of the Soviet story on Sverdlovsk 1979. I think it would be a good idea if we simply stop talking about the epidemic and go to the more primary questions in BW verification, namely what's going on in the facility and so forth. I tend to believe the account that Nikiforov gave; but I doubt if it will ever be possible to collect enough evidence to be totally compelling. Even more than that might be needed in order to get some of the people to budge off the limb they have crawled out on during these past many years. So my line is not that you have to believe their story; but that they have provided enough information to give it some face credibility. It is therefore no longer profitable to continue to press the airborne escape hypothesis whatever one's private belief may be.

The Soviets of course deserve great criticism for their unwillingness until now to talk about it; but I think we ought to offer them some reinforcements for their change of posture and hope that they continue to be forthcoming in more important areas. I'm sure you will be having your own private conversations on the same subject but of course none of us wants to be involved in any public embroil.

Matt Meselson has been holding to a similar line although I believe he is now organizing another trip to Moscow with more experts; and I suspect that the outcome of that will be some fairly strong public statements. I am not sure that sharp confrontation is all that constructive but there may not be that much that we can do about it.

Without going to any public media, I wonder if it would not be worthwhile to spend a few minutes briefing the DSB on our visit. I've offered that to Bert Fowler and if that comes off would you play whatever part you would care to in the discussion?

Yours sincerely,

Joshua Lederberg

Lederberg

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE NEW YORK, NY 10021

JOSHUA LEDERBERG
PRESIDENT

October 23, 1986

Dr. Ivan Bennett
Professor of Medicine
and Dean
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Yours sincerely,

Joshua Lederberg

(76)

x OSTP 11/6/86 mg

OCT 27 1986

NATIONAL ACADEMY OF SCIENCES

Committee on International Security and Arms Control

October 24,1986

Dr. Lederberg,

Three qmick points:1)Feel free to revamp the summary as you wish. 2) I note that in Meselson's report, Bezdenejnich said there were no cases in the same household, which is inconsistent with what we heard; 3) The reference to an earlier Smolensk incident which you and I discussed does not jibe with the Yaroslavl incident either, because in all the incidents on the sheet Meselson gave us there was a 100% mortality rate. Nikiforov said 27 out of 37 people died in the incident he was talking about, so I decided to let his words stand for themselves rather than add to the confusion with a footnote. Okay with you?

In addition to your comments on the draft, I would appreciate your suggestions for people in government, NIH, etc who should be on the distribution list for the final report, since it should probably be sent to a slightly broader group than we send the CISAC reports.

Sincerely,

Lynn

Tom Welch + Tommy Dashiell + DoD

Hexell - Ft Detrick

+ ask Woodward

Sam Thier IOM

J Wyngaarden NIH

W Graham OSTP

R Mikulak ACDA

+ copies for DoS

he is your routine contact at { CIA } : (Corn)

THE ROCKEFELLER UNIVERSITY
1230 YORK AVENUE NEW YORK, NY 10021

November 20, 1986

JOSHUA LEDERBERG
PRESIDENT

Major General Philip Russell
Commander, U.S. Army Medical
Research and Development Command
Fort Detrick, Maryland 21701

Dear General Russell:

I am very glad you called, and look forward to finding a time that we can sit down together in Washington or Fort Detrick.

Enclosed is some material outlining what I learned in Moscow last month: their account of the 1979 anthrax epidemic at Sverdlovsk. Ted Woodward was in our group and I am sure you would find it useful to get his own views on what we heard together.

The material gives my own personal impressions and recommendations about suitable courses of action. While I believe that we should put less emphasis on the 1979 epidemic as evidence, there are ample other reasons to believe that the Soviets continue to sustain a substantial BW development effort. Despite the 1972 convention, the verification and enforcement of BW arms control remains a vexing problem. Even absent flagrant violations of the convention, a residue of overhanging problems are not so easy to address. I have attempted to outline some of these in the enclosed material.

Also included are materials that I have received from Dr. Matthew Meselson of Harvard University. In late August he was briefed even more extensively than we were; and he has been an efficient conduit for other documents that the Soviets appear to be willing to release at this time. Dr. Meselson has authorized me to distribute the segment that is identified as his own notes only on the understanding that it will not be published in the open press without his consent.

I hope this will be helpful to you and that some of it at least will be important background for our own discussion when we can schedule that.

Yours sincerely,

Joshua Lederberg

Encls.

X... ots

Personal Memo from
JOSHUA LEDERBERG

Matt Meselson

MAR - 5 1987

Sverdlovsk: How many cases per household?

I saw Tommy Dashiell last week.

He says they said "only 1" at* Geneva too.

(* on Tower)

He insists that "release the internal report" is the sine qua non of credibility. I agree that if they won't do that they won't do much else for verification.

PF

Privileged

BW Planning Meeting
September 23, 1987
Rockefeller University

Summary Minutes

A meeting to discuss the future activities of CISAC's biological weapons subgroup was held at Rockefeller University on September 23, 1987. Present were: Joshua Lederberg; Paul Doty; Wolfgang Panofsky; Malvin Ruderman; John Steinbruner; Victor Rabinowitch and Lynn Rusten.

Lederberg opened the meeting by saying that he agreed with what he believed was a consensus that the dialogue on biological weapons with our Soviet counterparts should be continued and that the purpose of this meeting was to discuss agenda for the next bilateral meeting.

Rusten then reviewed what Bob Mikulak in ACDA told her about the results of the April 1987 Experts Conference. The BWC signatories agreed to an exchange of data through the UN on October 15, 1987, and thereafter the data exchange would occur annually on April 15. The data exchange will cover: 1) declaration of location and activities of all PIV labs and identification of all biological defense labs (there are five in the U.S.); 2) information on unusual disease outbreaks; 3) promotion of scientific contacts and dissemination of information about the above mentioned labs and; 4) prior announcement of all conferences on BW defense.

Mikulak told Rusten that the next BWC Review Conference will take place no later than 1991 and will focus on whether to add verification measures to the Convention. The plan is to see what verification measures are agreed to in the Chemical Weapons Treaty now under negotiation and then see if those can be applied to the BWC.

Mikulak told Rusten that he thought the CISAC dialogue was useful and could lay some important ground work for the next review conference. He thought it would be useful for both sides to exchange

views on what aspects of new technology they view as most dangerous. He expressed his personal opinion that advances in biotechnology facilitating large scale production was most dangerous. Mikulak thought that rather than start from scratch, both sides could react to the official papers on this topic submitted to the 1986 Review Conference. Mikulak also thought that promoting visits to Soviet facilities and other cooperative measures would be very useful. He indicated the time was ripe for this given recent Western visits to Krasnoyarsk, a military installation in Minsk, and his upcoming visit with a CDE delegation to a Soviet chemical weapons storage site. This led to a brief discussion of the status of the CW negotiations and verification provisions.

Doty mentioned he had heard that Ovchinnikov had died recently. Rabinowitch later looked into this and learned that it was not true.

Panofsky said it might be useful to ask the Soviets to explain to us how they classify the levels of caution at their facilities. Lederberg agreed that a discussion of how pathogenic agents are handled and what are the inspection procedures and signatures of these facilities would be a good thing. He said it would be useful to have someone on the U.S. delegation familiar with pharmaceutical production. He said he would try to think of an appropriate individual, perhaps a quality control person.

Doty said there was someone at Merck he could ask. Lederberg said the Waxman Institute played a bridging role between research and development. He said Arnie Demain at MIT might be a good person to ask. He said there might be someone appropriate at Fort Detrick. Lederberg said he would think more about this and asked Doty to do so as well.

Ruderman asked about the possibility of technological developments that could change the scale of production. Rabinowitch said that scale changes were more likely to come on the delivery end than on the production end.

Lederberg then reported on the US government reaction to the Soviet explanation of the Sverdlovsk incident given to the CISAC

sub-group last October in Moscow. He said the National Intelligence Officer (NIO) for BW does not think the story is credible. He evidently believes the Soviets first tried out the story on Meselson, then cleaned it up and gave it to the CISAC group. Lederberg said the issue revolves around the raw intelligence data and that it is hard at this point to trace back and interpret the raw data. Lederberg said Colonol Huxsoll, at Fort Detrick, is not far from the view of the NIO.

Lederberg said he thought other people in the government were more open-minded and wanted more information, including a copy of the official internal Soviet report written at the time of the incident; the names of the doctors involved and detailed case histories of the patients; and permission to go back and interview the surviving victims. Lederberg expressed his view that Sverdlovsk would not be removed from the official list of accusations of Soviet violations, nor did he think the issue was important enough to take it to higher levels of government.

Steinbruner said one consideration was whether this issue would just fade away. He posed the question of what would be the value of discrediting everyone involved in the U.S. side or of getting a Soviet case that would stand up to scrutiny. He suggested letting it ride through 1989 and seeing what happens then.

Doty referred to a new book by Joseph Douglass that argues that the U.S. should withdraw from the Convention. Everyone present agreed that was not a mainstream view.

Steinbruner said it would be hard to visit the Soviet facility at Sverdlovsk without mentioning the incident. Lederberg said the facility was a different issue from the Sverdlovsk incident. He said he thought the Sverdlovsk facility was a BW defense facility on a large scale. Steinbruner asked how we could go there and then on return say we were agnostic on the subject of the Sverdlovsk incident. Rabinowitch agreed with Steinbruner, saying we would have to be prepared to discuss the incident.

Steinbruner outlined two alternative approaches: a) say we want to go to the facility and encourage them to build a definitive case about the incident by letting us talk to people in Sverdlovsk and having Nikiforov go on a lecture circuit in the U.S., explaining that without further evidence we could not take a definitive stand; or b) separate current use and the Sverdlovsk facility from the past incident and focus on what is taking place in that facility now.

Ruderman expressed concern about having a short visit to Sverdlovsk with a set agenda, which would make it hard to make a definitive judgement. Lederberg said we should explain to the Soviets that we cannot press this issue further in the government, and that Soviet unwillingness to go the last mile is keeping the case from being resolved. He said we should ask them what is the barrier and urge them to finish the process they began. Regarding bringing Nikiforov here, Lederberg said this was not so important as having a scientifically thorough and credible publication. He added that Nikiforov, who is not an epidemiologist, is not the best witness. Steinbruner conceded that a Nikiforov speaking tour might not be desirable.

Rabinowitch suggested that when we are negotiating the agenda, we can explain why we think it is important for them to follow through with a credible publication explaining the incident. Doty said it could be explained to the Soviets that this incident might be raised in the ratification hearings on the INF treaty; others questioned whether this linkage should be raised.

Lederberg suggested they move on to discuss a possible agenda for the next meeting. He noted that one important item not on the list (See attachment #1, meeting agenda) was the issue of how to deal with third party capability to develop BW. He suggested they first discuss the problem that the fundamental R&D is dual capable. Doty said that if the research is medical, then openness and access should be total. Panofsky asked to what extent is openness a standard now in U.S. medical research and development, given proprietary concerns.

Lederberg responded that in practice no one lags more than a year in publishing their results, and that it is well known who is working for whom. He said the Board of Directors of a pharmaceutical company can make inquiries about what is being done, and so could act like an audit committee to comply with a Treaty regime.

Doty noted that the leading trade group, Industrial Biotechnology Association, supports the idea of openness and is supporting a pending bill that would apply the BWC to nongovernment activities.

Rabinowitch expressed the concern that our calls for greater openness might sound hollow given that the U.S. government keeps vetoing U.S.-Soviet cooperation in biotechnology. Lederberg said, however, that openness was generally complied with on the academic level. Doty said U.S. government labs and pharmaceutical companies were not so open.

Lederberg noted that the Soviets are eager for our industry to be more open, but that there are proprietary interests and concerns on the part of U.S. industry. He said he thought the U.S. could declassify information on its government facilities and activities if the Soviets would do the same. For instance, there could be systematic reporting on all PII and PIII facilities conducting medical research related to the military. He said openness about people was an outstanding difference between the two countries. He also said the Soviets could much improve their public health reporting.

Lederberg said registration of personnel would go beyond current levels of openness. Promotion of international exchange programs to get people in each other's labs would also be positive.

Panofsky said then that it would be useful to discuss with the Soviets standards of openness in fundamental R&D and reporting procedures about one's activities.

Doty suggested the problem was when one went beyond R&D to large scale production. Steinbruner asked whether one could define scale of production criteria. Lederberg said it would be very difficult

because the scale is modest. However, the high level of protection has a signature and this would focus attention on a limited number of sites. He said the packaging was telling.

Panofsky asked whether there was any hope for defining R&D quantities vs production quantities. Lederberg said you could try to quantify the treaty and say that amounts above x must be declared and registered. Steinbruner asked if one could define storage facilities? Lederberg said yes, that this could be defined as over 100 liters of pathogens. However, the said that recombinant DNA is defined as a pathogen, so this complicates the matter. Lederberg said in order to bolster the verification aspects of the treaty, one had to focus on quantity as the criterion rather than on types. Steinbruner asked then if large scale quantities could be considered offensive. Lederberg said yes, unless it is part of a research program for vaccines. Steinbruner said if you register something and there's a question about it, there should be a way to sample and investigate. Lederberg said this would pose proprietary problems, and also gets into the technology transfer issue, which is something the Soviets want to promote. He said you'd have to evaluate loss vs gain. He said one side could get a lot of the technical capability by sampling the strain, and that maintaining security by using dead samples would be much easier. Steinbruner said there could be a mutually run test facility with safeguards. Doty said he did not go along with this. Doty noted the spread of the practice of putting in a sequence that has a unique signature that can be traced back to the inventor. Panofsky said this issue of quantitative delineation would be a good issue to discuss with the Soviets. Lederberg agreed, saying it would force them to bring a delegation who could competently discuss the issue.

Rabinowitch suggested we proceed by producing an outline and discussion paper which would hint to the Soviets the type of expertise they should have on their delegation. It was agreed that Steinbruner, Rabinowitch and Rusten would carry this paper to Moscow in October and arrange in advance a meeting with the Soviet BW

delegation head and Sagdeev to discuss in detail our agenda ideas. Rabinowitch will then try to get a response when he is back in the USSR in December.

There was then a discussion of Soviet charges concerning ethnic weapons. Lederberg said the danger was in developing something lethal for which you have a defense for your own people through large scale production and distribution of a vaccine. He said he did not want to discuss this issue with the Soviets early on. He noted Meselson had once proposed that each side declare its vaccination programs and provide samples to the other.

Lederberg said reviewing the magnitude and character of each side's program should stay on the agenda. He said we could review the annual DOD reports to Congress on the US BW program. Doty said there should be some visits to US facilities. Steinbruner noted a Wall Street Journal article discussing the increase in US spending on BW defense, saying that we could explain where that extra money is going. The idea is over time to get the Soviets to share similar information with us. Someone said that Ustinov last time expressed concern about the purpose of US activities at Dugway.

Regarding cooperative programs, Rabinowitch said he did not have the details on the inter-academy cooperation on vaccines, but that there would be planning workshop for this in Moscow next spring. Therefore we can say we've implemented that recommendation from the first meeting and are moving ahead on it.

On the topic of AIDs, Lederberg pointed out that Soviet scientists have now criticized the Soviet media for charging that AIDs was a DOD-created lethal weapon.

Lederberg said we could discuss the possibility of cooperation in AIDs research. Rabinowitch said that by next spring, the IOM will have made some progress on this with the Soviet Academy of Medical Sciences. Steinbruner said we could then give an update on the state of cooperation on AIDs. It was agreed not to put AIDs explicitly on the agenda but to let it come up under the issues of cooperation and confidence building.

On the third party issue, Lederberg said there could be mutual assessment of the risk and sharing of information with each other about this, and perhaps discussion of civil defense measures. He suggested Rusten ask Mikulak whether there is any official bilateral exchange of information on third party BW activities. Steinbruner said we could discuss precedents for this in other areas and why it is useful. He said we could also discuss the possibilities for tracking end use if you transfer biotechnology capability. Rabinowitch said this was very hard to track and that it would again bring you to the problem of intent.

Panofsky said all the measures we discussed needed to be made multilateral, and that the multilateral aspect should be brought up under each agenda item.

A discussion of the probable American delegation resulted in the following list: The original delegation: Joshua Lederberg, Ivan Bennett; Theodore Woodward, Paul Marks, Alex Rich and John Steinbruner; and additionally: IOM President Sam Thier; Spurgeon Keeny; Paul Doty; Robert Chanock (NIH); and a person with experience in engineering and production.

It was agreed we would try to arrange a visit of both delegations to Fort Detrick, and that we would facilitate other scientific visits in the hope of setting up a reciprocal exchange of visits.

The meeting then adjourned and Lederberg, Steinbruner, Panofsky and Rusten worked on an agenda outline and accompanying prose to be carried to the Soviets in October. (For result see attachment #2).

Lynn Rusten

PRIVILEGED

BW Panel Planning Meeting
Rockefeller University
March 16, 1988

Draft Summary Minutes

The BW panel of the National Academy of Sciences' Committee on International Security and Arms Control met on March 16, 1988, at Rockefeller University. Present were: Dr. Joshua Lederberg, chairman; Robert Chanock, Paul Marks, Matt Meselson, John Steinbruner; Frank Vandiver; Theodore Woodward; and Lynn Rusten.

The purpose of the meeting was to plan for the upcoming US/Soviet bilateral meeting on BW to be hosted by the NAS group May 9-11 in Washington. Everyone introduced themselves. Rusten then reviewed the general schedule for the May meeting. She suggested that the first day, Monday May 9, be devoted to the thorough discussion of the agreed upon agenda items and that there be a dinner/reception that evening at the NAS to which outside individuals and government officials be invited. On Tuesday May 9, the plan is for both delegations to spend the day at Fort Detrick. A cultural event (jazz, or Kennedy Center event) will be planned for Tuesday evening. Wednesday a.m. will allow a few hours of discussion in the morning to follow up on the Fort Detrick visit and discuss future steps for the bilateral delegations. Rusten suggested the formal meeting adjourn before lunch on Wednesday.

At Lederberg's request, Woodward reviewed the state of plans to take both delegations to Fort Detrick. He reviewed steps taken to gain approval for the visit and expressed optimism that the visit would go off as planned. Woodward suggested that the groups would plan to arrive around 10:00 a.m. on May 10. He suggested that he, Lederberg, and some invited members of the Army Science Board would make some introductory remarks. Then General Russell and Colonel Huxsoll would make some remarks providing an overview of the research program at Fort Detrick. This would be followed by a tour of the facility. Following a lunch break, the groups would reconvene for a scientific seminar on infectious diseases. Woodward suggested that we ask the Soviets to come prepared to make two 30-minute presentations on designated topics. The session would conclude with a general discussion of the entire day at Fort Detrick, allowing for informal discussion and questions. The groups would then return to Washington.

Lederberg emphasized that on the third day of the meeting it would be appropriate to stress the importance of reciprocity to lay the groundwork for a visit to an analogous Soviet facility. He also suggested we invite Col. Huxsoll to be a guest observer at our discussions on Monday and Wednesday.

Chanock suggested that Alex Shelokov might be a good person to add to the American delegation. He is a virologist in charge

of the Swiftwater Army vaccine facility, and is also a native Russian language speaker. Lederberg agreed to consider this recommendation.

Lederberg then turned to the substantive items to be discussed with the Soviets, starting with the nature of the U.S. BW defense research program. Lederberg observed that, reading the previously distributed DoD Annual Report on Biological Defense Research Program Obligations, he did get the impression that we had entered into a BW arms race. He emphasized that the research was legal under the Biological Weapons Convention, but it nevertheless raised the question of how to cope with the situation. He observed that the acceleration of the US program had in part been provoked by Soviet obtuseness. Lederberg reiterated that the problem was the maintenance of a program that could be broken out of for offensive purposes, and raised for discussion the question of what could be done about that. He said the only idea he had was for greater openness about each side's program, and even that was only a partial solution.

Vandiver reiterated the importance of confidence-building measures regarding each side's program, which he noted was a major theme of the first bilateral CISAC BW meeting. Steinbruner agreed, noting that confidence between individuals and between the Academies was part of the purpose of this activity. He added that the assumption underlying these discussions was that neither side as yet has a massive militarily organized offensive capability.

Woodward said the relationship with the Soviets had to be developed gradually and in stages. First, there would be greater openness of discussion, then perhaps programs of scientific cooperation, and finally, confidence building steps such as stopping the practice of vaccinating US and Soviet military troops against smallpox.

Chanock agreed that the fact that the US and Soviet armies are still vaccinating against smallpox shows a lack of confidence. He said the only existing stocks of smallpox are in the USSR and the CDC in the U.S. However, he said that agreeing to eradicate all smallpox vaccination was not a good idea because it was useful as a vehicle for enhancing the effectiveness of other kinds of vaccines. Lederberg said we could use smallpox as an example of a problem of confidence that has a technical aspect. Chanock noted that smallpox would not be a useful BW agent. Lederberg said that was universally agreed, although there was same question about its effect in aerosol form. Steinbruner suggested that this kind of issue would be useful to discuss with the Soviets, without necessarily coming to a concensus. He said it would be useful to discuss what is the utility and feasibility of stopping the vaccination of armies.

Lederberg said it would be useful to have some prepared discussion papers for the May meeting. He asked Chanock to prepare something on the vaccination issue, and Chanock later agreed to do so.

Lederberg then raised another item for discussion, which was the impact of the changing political climate in the Soviet Union. He noted the Soviet acceptance of more intrusive verification measures in the INF Treaty. Steinbruner predicted that glasnost would affect the BW area too, and that there may indeed be substantial opening of these facilities. Steinbruner said it was his impression that the Soviets had done a lot of research in this area but that they had not widely developed a military capability. He observed that scientific collaboration was an overriding Soviet objective, and he predicted that we would be surprised by Soviet opening up in this area too. Lederberg said he would try to impress upon Sagdeev the importance of greater openness in this area as they have opened up in other areas.

Lederberg said these groups could usefully provide a technical basis for arriving at cooperative measures. Steinbruner agreed we should focus on the most constructive suggestions for first steps.

Chanock said he did not find the DoD report that threatening because it was obvious that the US program was aimed at producing vaccines against public health threats. Vandiver said he thought the report could appear threatening from the Soviets' perspective.

Meselson then spent some time discussing the visit of three Soviet scientists whom he is hosting in April. Two are doctors who were involved in treating and studying the victims of the 1979 anthrax outbreak in Sverdlovsk. Meselson reviewed the history of his acquaintance with these individuals and the questions surrounding the Sverdlovsk incident. This was followed by a lengthy discussion of arrangements for their visit and of anthrax in general.

Chanock noted that one point to make was that the Soviets should routinely publish data about this sort of event, and that medical surveillance in the USSR is not very good. He said they have nothing analogous to the CDC's weekly morbidity and mortality report. He said it would be excellent to send Soviet epidemiologists to the CDC for several months or a year to see how it's done here.

Lederberg returned the discussion to the structure and content of the May meeting. He asked Chanock if he would talk about the issue of smallpox vaccination. Woodward suggested there be a workshop on recombinant vaccines sometime in the future.

Steinbruner said it would be useful to introduce the subject at a detailed level so the Soviets could think about it and respond at a future meeting. Lederberg suggested the topic be enlarged to include the disclosure and exchange of samples for major vaccine programs. Chanock noted that the USAMRIID openly discusses vaccines under development. Lederberg said there were two issues to raise: more open publication and stringent reporting of vaccine development. Steinbruner agreed we should raise the question of whether both sides should undertake more stringent reporting.

Chanock agreed to be the lead discussant on major efforts on defensive vaccines at USAMRIID. Lederberg said the point was to discuss the foundations for mutual disclosure of vaccines to be produced in some quantity. Chanock agreed and said he could supply the Soviets with a FDA list of all vaccines in use in the US and could then ask for the same information from the Soviets. Woodward said Dr. Jordan, whom he would invite to the seminar at Fort Detrick, could have a packet of this information prepared.

Lederberg concluded that a discussion of monitoring vaccination programs could take about a ½ hour on the agenda. He said a subset of that could be the question of why are both sides' militaries vaccinating against smallpox? He said we could raise the possibility of stopping comprehensive smallpox vaccination, while allowing the use of the vaccine as a vehicle for other vaccines and requiring both sides to disclose when they are so doing. Lederberg stressed the importance of having on the agenda detailed issues with technical content such as this one.

Meselson cautioned against making it sound that it was very important to stop smallpox vaccination. Lederberg observed that the fact that it wasn't that important meant that the risks of cheating were relatively small.

Lederberg said that on the final day, Wed. May 11, it would be appropriate to discuss the structure of the next meeting in the USSR.

Meselson listed other suggestions he had for bilateral discussion: 1) vaccines; 2) medical surveillance and reporting domestically and to international organizations; 3) epizootics, because animals are important indicators of diseases such as anthrax. Lederberg suggested for the second item the wording "structural frameworks for national epidemiology," which he agreed would be a good agenda item whereby each side could explain its epidemiology/surveillance network.

Woodward said it would be important to stress the importance of continuity of membership on each delegation. Lederberg noted that Sagdeev was very conscious of this in the CISAC interaction.

Meselson mentioned the importance of reciprocal visits to sites. He also expressed his interest in getting younger scientists thinking about these issues.

Returning to the agenda, Lederberg asked Vandiver if he would be willing to make a 20 minute opening presentation on the US BW defense program. Vandiver agreed. Lederberg reiterated that the closing discussion Wednesday a.m. would be devoted to discussing reciprocity of visits and future activities. Steinbruner suggested that reciprocity be broached in terms of general openness, as opposed to demanding strict reciprocity.

Lederberg suggested we keep on the agenda the issue of the danger of proliferation and use by third parties.

Meselson noted that the House of Representatives was currently considering legislation to make illegal the creation of biological weapon agents by private individuals. This

apparently would fulfil an obligation of the Biological Weapons Convention. He said the Soviets already had similar legislation and that we could asked to see theirs.

Lederberg asked how to approach discussion of the information exchanged on PIV facilities in conjunction with the biological weapons convention experts conference. Meselson suggested the staff write a short comparison/analysis of the US and Soviet submissions. Rusten agreed to try to enlist the help of someone at the Institute of Medicine to prepare the paper.

Lederberg noted the suggestion of Robert Mikulak in the Arms Control and Disarmament Agency that the Koltsovo facility in Novosibirsk or the facility no. 19 in Sverdlovsk would be the sites most analogous to Fort Detrick for a reciprocal visit.

Vandiver offered to host some of the Soviets at Texas A&M following the meeting. He agreed to inform Rusten what kind of scientific program he could arrange so she can convey the invitation to the Soviets.

Returning to the subject of evening entertainment, it was agreed that there would be a dinner/reception at the NAS Monday night, and a cultural activity (jazz concert) Tuesday night. Rusten collected everyone's suggestions for outside individuals to invite to the dinner Monday night - additional names should be conveyed to Rusten.

The meeting adjourned at 3:00 p.m.

Lynn Rusten

PRIVILEGED

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R on Biological Weapons
Washington, May 9-11, 1988

Summary Report

The second meeting of delegations of the U.S. National Academy of Sciences (Committee on International Security and Arms Control subgroup on biological weapons) and the Academy of Sciences of the U.S.S.R. took place on May 9-11, 1988, in Washington, D.C.

The members of the US delegation were: Dr. Joshua Lederberg, chairman; Dr. Robert Chanock; Dr. Paul Marks; Dr. Alexander Rich; Dr. Alexis Shelokov; Dr. John Steinbruner; Dr. Samuel Thier; Dr. Frank Vandiver; Dr. Theodore Woodward; Dr. Victor Rabinowitch; and Ms. Lynn Rusten. Dr. Alexander Langmuir, Dr. Matthew Meselson and Dr. Glenn Schweitzer were present as observers (see attachment #1).

The members of the Soviet delegation were: Academician V. Ivanov, chairman; Dr. E. Sverdlov; Dr. A. Yablokov; Academician S. Prozorovskiy; Mr. N. Smidovich; Mr. O. Lisov, and Mr. Valery Nemchinov (see attachment #2).

The agenda contained the following items (see attachments #3 and #4):

1. Review of Current US and USSR Programs Related to BW
2. Confidence Building: Positive Measures and Impediments
3. Scientific Cooperation and Confidence Building Measures
    a. Structural Frameworks for National Epidemiology
    b. Information Sharing and Monitoring of Vaccination Programs
    c. Cooperative Programs in Biomedical Research
4. Arms Control
    a. Definition of Legitimate and Prohibited Activities

b. Principles of Control

c. Compliance: Verification and Enforcement

d. Discouraging the proliferation and use of BW agents by third parties

5. Next steps

Review of Current US and USSR Programs Related to BW

Discussing the US program, Lederberg called attention to the existing open publications in the US including the October 1987 submission to the UN, the annual DOD Report to Congress on the BW defense research program, and open scientific publications on the research conducted at defense labs, particularly at the US Army Medical Research Institute of Infectious Diseases (USAMRIID). Some of these publications were made available to the Soviets during the meeting.

Vandiver made a presentation on the facility and activities at the Dugway Proving Ground, particularly in its Life Sciences Division. He provided information about the Division's mission, size, personnel and research. The Soviets expressed interest in and appreciation for Vandiver's presentation. Ivanov said he hoped that before long the Soviet government would be able to provide similar information and slides on its BW defense program.

Smidovich discussed the October 1987 and April 1988 USSR submissions to the UN. He explained how the Soviet Council of Ministers organized internally the collection of information across agencies. He stressed that the USSR had provided information that went beyond the letter of the reporting requirements, including the declaration of all BL-3 facilities, listing of authors and publications emanating from the military research labs, and provision of information about related scientific conferences. Smidovich said his government was disappointed by the low participation rate of BWC signatories in the data exchange. He said the USSR thought governments should declare related government activities taking place outside of

the national territory. He noted that the US had been late in submitting its April 1988 report, and expressed disappointment in the US October 1987 report for not reporting on activities at the margin, especially work done under contract, e.g. at universities.

Confidence Building: Positive Measures and Impediments

Both sides understood the need for greater confidence about each other's activities in the BW area, and agreed that greater openness and increased contact among scientists involved in related fields was the most practical method of promoting confidence. The open publications and data exchanges discussed earlier were recognized as significant contributions to the understanding of each others' activities.

The 1979 Sverdlovsk anthrax incident was cited by the Americans as an example of an event which was poorly handled. The Soviets refused to offer detailed information; the incident then became part of political attacks on the Soviet record of compliance with arms control agreements. The Americans expressed appreciation that more information about the incident was now forthcoming from the Soviets, but noted that the details had still not been formally published.

Lederberg raised the issue of the remark made by Valentin Falin which had been quoted in a New York Times column by Flora Lewis. Lederberg said Falin's threat that the Soviets could use BW as a response to SDI was an example of the ease with which fears could be created and with which treaty commitments could be broken. Smidovich asserted that Falin's remarks were misunderstood or taken out of context, and that the incident was merely a misunderstanding. Thier made reference to US displeasure with accusations in the Soviet press that AIDS was created by DOD as a biological agent. The Soviets immediately said that scientists knew this was not true. They also noted that the Soviet people were very afraid of AIDS.

Scientific Cooperation and Confidence Building Measures

Much of this discussion stemmed from the recognition that scientific cooperation and personal contacts could enhance knowledge about research being conducted by individuals and labs and thus do much to create confidence.

Thier, President of the Institute of Medicine, Rabinowitch, Director of the NAS Office of International Affairs, and Schweitzer, Director of the NAS Committee on the USSR and Eastern Europe, summarized the current state of cooperative programs with the Soviet Academy of Sciences and the Soviet Academy of Medical Sciences. Much of the ensuing discussion focused on the value of conducting seminars, workshops and joint research in related fields. Enthusiasm was also expressed on both sides for the idea of placing American and Soviet post-docs in each other's institutes for periods of a year or more. Some of these suggestions will be implemented through the existing inter-Academy mechanisms.

Chanock gave a detailed presentation on the open process by which vaccines for civilian and military use are developed, evaluated, licensed and monitored in the US. He indicated where and how this information could be obtained by the public, and emphasized that even vaccines developed by the military had to be licensed by the civilian Food and Drug Administration.

The Soviets expressed much interest in Chanock's presentation. Prozorovskiy said there was not an equally open system of vaccine development in the USSR. But he did say that all vaccines developed by the Soviet military had to go through the civil system of approval before they could be used. He suggested there should be a mutual obligation to announce the creation of vaccines against dangerous and toxic diseases, perhaps even giving the other side the right to examine samples under appropriate safeguards. He also recommended joint research on such vaccines to enhance confidence and reduce the possibilities for unilateral advantage.

Langmuir made a detailed presentation on the system of epidemiological surveillance in the US. He stressed the importance of first-rate surveillance and the capability to

disseminate the information broadly from a centralized source. The Soviets expressed much interest in, and respect for, Langmuir's presentation and work in this field. Prozorovskiy said the Soviet epidemiological system currently was not centralized, and furthermore statistics in the past had been suppressed and distorted. He said that due to restructuring, and with the help of computers, they were starting to improve their collection of epidemiological data. The Americans stressed the importance of having good data and disseminating it in a timely fashion to aid science and confidence building.

This discussion led to the suggestion that there be an exchange of post-docs who could spend one year at the CDC and the Soviet equivalent to learn and share information about the science of epidemiology and its conduct in each country. The discussion of vaccines led to a similar suggestion to put post-docs in large vaccine production facilities, perhaps even the ones producing vaccines for the military.

Arms Control

Discussion of this item was more procedural than substantive. Smidovich from the Ministry of Foreign Affairs and Lisov from the Ministry of Defense were apparently instructed or at least authorized to propose that a bilateral scientific forum be established to consider how to devise meaningful measures to enhance the verification of compliance with the BWC. The Soviets appear to be trying to come up with some ideas in advance of the third BWC Review Conference, scheduled to occur before 1991. Smidovich and Lisov were very open as to what the structure of such a scientific forum might be.

The Americans responded with interest to this suggestion, with the proviso that the interaction continue to operate as an unofficial dialogue among scientists conducted under the auspices of the two Academies of Science. The Americans noted the asymmetries between the two Academies and emphasized the tradition of the NAS and the parent Committee on International Security and Arms Control to both maintain its independence from

the government and avoid interfering in the affairs of government. Therefore, this dialogue would have to be understood as scientific, informal, and unofficial.

Both sides agreed that such an effort would demand a greater commitment of time, energy and expertise than had been contributed so far; both sides indicated a willingness to make such an effort.

Lederberg flagged some of the fundamental issues deserving deeper thought: How could one define and verify the permitted level of BW research and development work? What would be needed to verify compliance and how would it be done through an on-site inspection? What activities at a research laboratory would be cause for alarm? The Soviets listed similar issues for examination. It was agreed that the chairmen and staff would communicate in the near future to hammer out a detailed agenda, work plan, and mechanism for regular and more frequent meetings.

USAMRIID Visit

On the second day of the three day meeting, both delegations attended a scientific seminar in which three individuals from USAMRIID presented research on hemorrhagic fevers and vaccine development.

Following the seminar, the delegations took a brief tour of a part of USAMRIID, where they saw a high containment lab and an isolation unit. This unprecedented visit provided an opportunity to ask questions about the USAMRIID program and facility.

The following day, the two committees had a frank discussion about the tour. It was acknowledged that the tour had been rather brief, and that it had been difficult to arrange. The Soviets were nevertheless appreciative that it took place. Ivanov said he recognized its implication for a reciprocal Soviet site visit, which he indicated he would try to arrange. The Soviets asked why the security had been so heavy, and this issue was discussed. The Americans made the point that the lab

did harbor dangerous infectious agents and so one could not roam freely without risking accidental exposure. The Soviets understood the significance of the fact that the visit took place at all and that it had set an important precedent, despite its limitations. Furthermore, the visit stimulated a discussion of what one could hope to learn from an on-site inspection, no matter how extensive. This question is to be examined in future discussions. It also resulted in the suggestion that it might be good to work toward, in the future, the possibility of putting a Soviet post-doc at USAMRIID and an American post-doc at a comparable Soviet military facility. This was recognized to be a long-range goal.

Next Steps and General Observations

The atmosphere of the meeting was good. The Soviets, due largely to the participation of Smidovich and Lisov, were much more focused on the arms control issues than they had been at the October 1986 meeting. The scientists on the Soviet delegation appeared to be just beginning their exposure to these issues.

The next steps will be to implement the recommendations for scientific exchange and cooperation through the existing inter-Academy mechanisms, and to work out a modus operandi for deeper consideration of verification and confidence building measures regarding each side's activities in BW defense.

Lynn Rusten

Attachment #1

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Washington, D.C.
May 9-11, 1988

US Participants

Dr. Joshua Lederberg, chairman
President
Rockefeller University

Dr. Robert Chanock
Laboratory of Infectious Diseases
National Institute of Allergy and Infectious Diseases
National Institutes of Health

Dr. Paul Marks
President
Memorial Sloan-Kettering Cancer Center

Dr. Alexander Rich
Professor of Biophysics
Department of Biology
Massachusetts Institute of Technology

Dr. Alexis Shelokov
Director of Vaccine Research
Government Services Division
The Salk Institute

Dr. John D. Steinbruner
Director
Foreign Policy Studies Program
Brookings Institution

Dr. Samuel Thier
President
Institute of Medicine

Dr. Frank E. Vandiver
President
Texas A&M University

Dr. Theodore Woodward
Professor of Medicine Emeritus
University of Maryland Hospital

Dr. Victor Rabinowitch
Director
Office of International Affairs
National Academy of Sciences

Lynn Rusten
Director
Committee on International Security
and Arms Control
National Academy of Sciences

Observers

Dr. Alexander D. Langmuir
Former Chief Epidemiologist
Centers for Disease Control
US Public Health Service

Dr. Matthew Meselson
Department of Biochemistry and Molecular Biology
Harvard University

Dr. Glenn Schweitzer
Director
Committee on the USSR and Eastern Europe
National Academy of Sciences

Attachment #2

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Washington, D.C.
May 9-11, 1988

USSR Participants

Academician V. Ivanov
Deputy Director
Institute of Bioorganic Chemistry

Dr. E. Sverdlov
Corresponding Member of AS USSR
Institute of Bioorganic Chemistry

Dr. A. Yablokov
Corresponding Member of AS USSR
Koltsov Institute of Developmental Biology

Academician S. Prozorovskiy
Director
Gamaleya Institute of Epidemiology and Virology

Mr. N. Smidovich
Ministry of Foreign Affairs

Mr. O. Lisov
Military Expert
Ministry of Defense

Mr. Valery Nemchinov
Delegation Secretary
AS USSR

Attachment #3

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Washington, D.C.
May 9-11, 1988

Draft Agenda

1. Review of Current US and USSR Programs Related to BW

2. Confidence Building: Positive Measures and Impediments

3. Scientific Cooperation and Confidence Building Measures
   a. Structural Frameworks for National Epidemiology
   b. Information Sharing and Monitoring of Vaccination Programs
   c. Cooperative Programs in Biomedical Research

4. Arms Control
   a. Definition of Legitimate and Prohibited Activities
   b. Principles of Control
   c. Compliance: Verification and Enforcement
   d. Discouraging the proliferation and use of BW agents by third parties

5. Next steps

Attachment #4

Meeting of the Delegations of the U.S. National Academy of Sciences and the Academy of Sciences of the U.S.S.R. on Biological Weapons

Washington, D.C.
May 9-11, 1988

Highlights:

1. Their scientists disavowed Falin's threats and the stories of AIDS being invented in USA.

2. They are eager for much more bilateral scientific exchange, including at BW Defense Research facilities. As here, that will take further dealings with their defense authorities. They welcome US post-docs at the Institutes in Moscow.

3. They took the initiative that scientific groups continue the dialogue on problems of verification and definition at the R&D level, and what proposals to make on OSI.

4. They welcomed the opportunity to visit USAMRIID and saw that as a precedent for reciprocity on their part.

5. They welcome other cooperative activities, including workshops on vaccines, arborviral diseases, epidemic surveillance.

6. A group of their biological scientists is gradually developing familiarity with BW arms control issues and, hopefully, the USSR's own programs as disclosed in the submissions to U.N.

The most important point: what do we really want to see (and are willing to allow reciprocally) that we can verify on site? They appear now very willing but we haven't made up our own minds on the standards.

J.

(CBW) Matt M.

Personal memo from
JOSHUA LEDERBERG

Dear Matt

MAY 27 1988

The Scientist

did take out the most damaging reference to classified info. (The editor was quite upset that I called you directly!)

He regards that as an abuse of his confidence.

So once again I was in the middle.

J.

Personal memo from
JOSHUA LEDERBERG

Frank Press
Sam Thier

Press interview with Matt M.

They did take out 1 (most inappropriate) sentence.

Jo.

Personal Memo from
JOSHUA LEDERBERG

Matt Meselson

MAR 23 1989

Defining a "toxin"

A discussion paper for our meeting in London.

I enjoyed seeing you beyond Istria.

Yours,

J.

THE ROCKEFELLER UNIVERSITY NEW YORK 10021

3/3/89

STATUS OF TOXINS UNDER THE BWC

Lederberg

The BWC prohibits "Microbial or other biological agents, or toxins whatever their origin or mode of production" [if for other than peaceful purposes.] There is no definition of toxins either in the treaty, nor so far as I am aware in the negotiating history of the Convention -- they were thrown in as an afterthought. The historical context does identify biological agents with those that proliferate in the course of doing harm. Review conference discussions have fairly certainly included infectious nucleic acids and recombinants among forbidden biological agents. "Toxins" are generally understood to be poisonous substances generated as byproducts of biological growth -- examples are botulinum toxin or mycotoxins (like trichothecenes). They generally have complex chemical structures, but not always. New methods of chemical synthesis leave open the possibility that any toxin could be produced by chemical methods as an alternative to biological but the "whatever mode of production clause" would prohibit such products as well.

Toxins (as well as microbial agents) are clearly also included under the provisions of the Geneva Protocol. Our discussion may be moot if a general Chemical Disarmament treaty is concluded. But until that eventuates, there is a zone of definitional ambiguity about just which chemical substances are "toxins" under the BWC. So far, this is purely hypothetical: we are not aware of any allegation about "development, production, stockpiling, acquisition or retention" of substances in the gray zone, nor has any country asserted that its possession of a toxin-related chemical was permissible under the treaty.

The difficulty arises from the existence of toxic chemicals which resemble, in structure or in pathological effect, the toxins of biological origin which are clearly forbidden. For example, a synthetic polypeptide may well be identified which comprises the active site of the botulinum toxin. Indeed, it is often discussed that such a substance, especially if built along with skin penetration aids, might be far more potent than nerve gas, and as such would be an attractive target for chemical weapons development (a dangerous vertical proliferation). Further developments in the understanding of molecular structure may allow non-polypeptide structures to be designed which bear no direct analogy to botulinum toxin, but which are conceptually derived from insights into how this toxin works. Mycotoxins and zootoxins likewise could have synthetic molecular variants that are conceptually but not structurally related to biological prototypes.

As the BWC is silent or vague, there has been a certain amount of discussion about more precise definitions to clarify the existing uncertainties. At the Quinquennial Review, it was agreed that synthetically produced analogues are covered; but this begs the question of what is an analogue. Three lines of further progress can be envisaged:

1) Within the negotiating framework of the CW disarmament discussion, interim declarations that disavow any novel chemical agents other than those now in admitted stockpiles or closely related to them. This would leave mustards and organophosphates as a class under the same heading as existing chemical weapons, but would label all novelties (including synthetic peptides) as already forbidden by the BWC. Such entities would be encumbered with the same verification problems, no better, no worse, as biological agents and toxins.

Get to formal declaration of stocks ASAP.

* I wouldn't want to waste much energy on refining definitions. But as the matter has come up from time to time (2) is scientifically the most economical; (1) ought to be done anyhow.

or, as a specific and emphatic subset of the class of novelties:

2) Defining as subsumed by toxins, under the BWC, any chemical substances targetted against specific cellular receptors other than those (cholinesterase) associated with nerve gas.

or

3) More specific designations of oligopeptides and other chemical categories. This would not be foolproof, but would promptly cover the most likely, immediate prospects. Non-polypeptide myco- and zoo-toxins generally offer no dramatic advantage in lethality compared to nerve gas; hence there is less motivation to invest in synthetic chemicals that mimic their activity.

While CW-disarmament must be concurred with multilaterally, the high technology associated with toxin extensions would lend great value to interim declarations initiated on a bilateral basis. These might be revocable in the unlikely event that third parties were found to be proceeding along these denied paths. Since we are dealing with still hypothetical innovations, there should be far less reluctance to accept these restrictions than would apply to well established chemical weapons.

The broadening of the toxin provisions lends nothing to the verification dilemmas, but would be a confidence building measure especially if it is associated with free scientific discussion of permitted R&D on toxic activities and their receptors.

Joshua Lederberg
3 March, 1989

Memo/Reply From
JOSHUA LEDERBERG

TO: Matt Meselson

APR 4 1989

APR - 4 1989

Re: Lebedinskiy.

890128 Izvestiya publishes the following statement by Lt-Gen V A Lebedinskiy, a full member of the USSR Academy of Medical Sciences "and a leading expert on protection from biological weapons": "The Soviet Union has always been in favour of a ban on biological weapons, regarding them as an inadmissible means of warfare. Our country has never developed and never produced such weapons and has never possessed them". The statement was in rebuttal of an article in the 19 Jan issue of Washington Times which said that, according to "a secret Reagan administration report...prepared a year ago by US intelligence agencies for the National Security Council", the Soviet Union had been producing lethal biological weapons at several clandestine facilities at least as recently as 1987.{Izv 28 Jan 89 via FBIS}

In a subsequent version of the rebuttal statement issued by Novosti on the authority of the "competent Soviet bodies", it is said: "The USSR strictly abides by the 1972 Convention, has no bacteriological (biological) weapons and toxin agents, related equipment and transportation means, which are prohibited by the Convention, and does not carry out research to develop weapons of this type". {Soviet News 8 Feb 89}

Thank you, Matt.

There's a lot of semantics here: what's a weapon. Apart from the toxin issue I don't know of any allegation that the Russians have been manufacturing weaponized bugs.

Joshua Lederberg
President
The Rockefeller University
New York, N.Y. 10021
~~(212) 360-1234~~

☐ ORIG. RETAINED
☐ COPY

—REPLY FORM—

Memo/Reply From

JOSHUA LEDERBERG

TO:

There's not much point in asking for more statements from the USSR that they're in compliance (though the statement about never in the past is interesting — how can it be true?) We are approaching more open-ness about what they are doing and that is important.

Entre nous (do not quote). The Russians at London (I'm just back) were not keen on any redefinition of toxins, or any interim declaration about CW. Perhaps they feel that would just get in the way of completing the CW negotiations.

But the overall spirit was superb.

Yours

Joshua Lederberg
President
The Rockefeller University
New York, N.Y. 10021
~~(212) 360-1234~~

☐ ORIG. RETAINED
☐ COPY

—REPLY FORM—

STATUS OF TOXINS UNDER THE BWC

Lederberg

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3) More specific designations of oligopeptides and other chemical categories. This would not be foolproof, but would promptly cover the most likely, immediate prospects. Non-polypeptide myco- and zoo-toxins generally offer no dramatic advantage in lethality compared to nerve gas; hence there is less motivation to invest in synthetic chemicals that mimic their activity.

While CW-disarmament must be concurred with multilaterally, the high technology associated with toxin extensions would lend great value to interim declarations initiated on a bilateral basis. These might be revocable in the unlikely event that third parties were found to be proceeding along these denied paths. Since we are dealing with still hypothetical innovations, there should be far less reluctance to accept these restrictions than would apply to well established chemical weapons.

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Joshua Lederberg
3 March, 1989

Lederberg

NATIONAL ACADEMY OF SCIENCES
COMMITTEE ON INTERNATIONAL SECURITY AND ARMS CONTROL
2101 Constitution Avenue Washington, D.C. 20418

July 26, 1989

Dr. Matthew Meselson
Fairchild Biochemistry Building
Harvard University
7 Divinity Avenue
Cambridge, Massachusetts 02138

Dear Matt:

Dr. Lederberg suggested I write to inform you of the status of the CISAC working group on biological weapons control. You may recall that at the May 1988 bilateral meeting with the Soviets at the Academy in which you participated, it was suggested that each side appoint a small working group of individuals who would work more intensively on these issues and that the working groups might meet together several times a year.

Both sides have since formed active working groups. The members of the American group are Dr. Lederberg, Robert Chanock, Thomas Monath, Alexis Shelokov and John Steinbruner. They met several times over the fall and winter to prepare for the first meeting of the Soviet and American working groups which took place last April. Both groups have been working hard to develop concrete suggestions for improving confidence and enhancing the verification measures of the Biological Weapons Convention. They have also been addressing the challenge of bilateral approaches to the prevention of BW proliferation. The two working groups will meet again in October to continue their work.

For the time being, we plan to continue the activity through these smaller working groups. On behalf of Dr. Lederberg and the Academy, I would like to thank you for your participation in the initial development of this activity. I hope we may call on you again in the future should the opportunity arise.

Sincerely,

Lynn

Lynn Rusten
Director
Committee on International
Security and Arms Control

COPY

Lederberg -

October 20, 1989

Dear Josh,

Here is an outline of the presentation I gave to Dick Kerr and Gordon Ohler. "Presentation" isn't quite the right word, since the dislcussion was informal and low-keyed. Kerr said he would consider the possibility of a review and I got the impression that he was amenable to doing so. With your permission, I volunteered you as a possible scientific advisor.

I haven't yet had time to type up a proper memo on the discussion, but I will send it when I do.

As ever,

THE SALK INSTITUTE
GOVERNMENT SERVICES DIVISION

March 27, 1992

Dr. Joshua Lederberg
University Professor
The Rockefeller University
New York, 10021-6399

Dear Josh:

Thank you for the memo of March 17th regarding the CISAC-BW subcommittee. I reread the IZVESTYA item -- I saw it as part of Matt Meselson's effort in pursuing the Sverdlovsk-1979 affair. Having read your memo, I should like to record my immediate response -- at least for now.

Yes, it makes sense for the proliferation issue to be central, and I share your concern about civil defense. Both issues are related to the threat of terrorism, whether rogue-state-sponsored or by indigenous fringe groups. Surveillance and a means of detection are needed. The military may have the means that are in place, but what is the status of the civilian preparedness?

You mentioned export controls -- a complex issue as related to realistic control policies that would be acceptable to the nation's own commercial interests.

I will write again as I give thought to your provocative memo. In the meantime, kindest regards to you and Marguerite.

Sincerely,

Alexis Shelokov, M.D.
Director of Medical Affairs

4438 Centerview Drive, Suite 200 • San Antonio, TX 78228-1407
Telephone (512) 732-4164 • FAX 512-732-7544

DRAFT — cc list John Steinbruner

THE ROCKEFELLER UNIVERSITY

pro bono humani generis

1230 YORK AVENUE · NEW YORK, NEW YORK 10021-6399

Joshua Lederberg
UNIVERSITY PROFESSOR

June 14, 1992

Dr. Vadim Ivanov
Shemyakin Institute
Moscow

Dear Vadim:

This message is sent after consultation with Pief Panofsky about the utility of reopening discussions with respect to BW arms control. The political context has, of course, changed dramatically since our last meeting; and we can look forward to the rapid dissipation of any adversarial tinge. Nevertheless, in light of the perils of proliferation, it is no less important that we reinforce our mutual understanding. The initiative to which we agreed in December, 1990, strongly condemning any use of use of biological weapons, and responding with alarm to the threats made on behalf of Saddam Hussein, is very much in the spirit of what I hope might be advanced by further discussions.

In addition, American scientists would like to find all possible avenues of support for biomedical research in the CIS: for example, I am on the advisory board of the new American-Russian Biomedical Research Foundation, and we would welcome your input on the most efficient ways in which such funds might be allocated.

We have also to bring to fruition our discussions about the smallpox sequencing project, and about standards of regulation and disclosure that would be appropriate for the stronger enforcement of the BWC; and above all how these measures might be applicable to the threats of 3-d country prliferation. To that end, our NAS group has been reinstated, and I hope to meet with them very soon with the aim of establishing an agenda, and specific proposals for a meeting with our counterpart Russian Academy of Sciences group. We would welcome your informal advice about the participation of other CIS republics' scientific organizations.

There is however a cloud on the credibility of all of our academies' activities, highlighted by Mr. Yeltsin's revelations concerning the Sverdlovsk (1979) anthrax epidemic. We are all very much encouraged by the candor with which he is addressing these past offenses, and look forward to the completion of his own investigations and their disclosure to the world.

Unfortunately, our own group will be widely pictured as the vehicle for the transmission of a false account of that incident, particularly on the part of Dr. Burgasov during his visit to the United States. I single him out as he was responsible for the epidemiological inquiry. When we questioned Dr. Nikiforov about that, he disclaimed any personal knowledge; and there is perhaps room for controversy about the interpretation of gross pathology that he presented directly to us in Moscow. It does not take much imagination to foresee that this will become

a matter of domestic concern in the Russian press, as grave as it also is in the arena of US-Russian scientific relations. I am well aware that you and your colleagues who met with us made no claims, and that you stated you had essentially no knowledge of the entire affair prior to our having placed it on the agenda of our discussions. In any event, it would be most helpful if you could suggest ways in which your academy could 1) further the complete ventilation of that lamentable history, and 2) clearly establish its distance from it, so as to dispel those clouds, and provide a confident basis for candid discourse.

You will recall that my own stand was to reserve judgment until the detailed account provided by Burgasov and Nikiforov had been widely published within the Soviet Union, so it could be given critical examination by your own colleagues and compatriots. The latter part of this expectation is being fulfilled for the first time right now, and plainly is the most reliable approach to establishing the truth.

Please let me hear frankly from you about any steps that I should recommend to our academy that would be of assistance to clearing away these clouds.

Yours sincerely,
— DRAFT —
Joshua Lederberg

Dear Brief—
Would this fill the bill?
Josh

June 15,1992

Dr. Joshua Lederberg
President Emeritus, Professor
Rockefeller University
1230 York Ave.
New York, N.Y. 10021

Dear Josh:

After sending you the revised version of my memo earlier today, I have received indirectly some additional information. According to Jeff Smith of the Washington Post, Yeltsin said on May 27 that the KGB had admitted to him, apparently at the time of the Sverdlovsk incident, that the anthrax outbreak had originated at the BW research installation. Jeff gave me no further details but from his questions it is apparent he is writing an article that will indict Burgasov, Nikiforov et al for deliberate misrepresentation and will probably treat all the Americans involved in their visit here somewhat harshly as well.

I suggested that the details of what Yeltsin had said would matter in particular whether the KGB had specifically said at the time that the incident involved airborne propagation. I said that would be an important element in judging whether the account given here during the 1988 visit was accurate but incomplete or whether it involved direct misrepresentation.

I assume that Jeff's article will appear tomorrow since he said he was working on deadline.

Sincerely,

John Steinbruner

To: Joshua Lederberg

From: John Steinbruner

Date: June 15, 1992

Re: Regenerating the BW Discussions

According to my records the last communication with our Soviet and presumably now Russian counterparts occurred nearly a year ago. On June 27, 1991 I received a fax letter from Vorob'ev (his transliteration) saying the he and Rayevsky had reviewed the paper Tom Monath and I had sent them and that they had some ideas of their own they wanted to send to us. He said that they expected to have a draft paper of their own in September and hoped to come here sometime thereafter to discuss both papers. The coup attempt then intervened and I have not heard from him since.

It will undoubtedly require a substantial initiative on our part to regenerate our discussions. Judging from experience with other relevant exchanges, we can expect to find our Russian colleagues intensely interested and liberated from some of the restraints they previously perceived but also very distracted and unable to finance their normal share of the working group's activities. As a practical matter we will probably have to assume primary responsibility for organizing and financing any meetings and related activities. In compensation for assuming a greater burden, however, we can also have higher aspirations. The political situation and the succeeding governments may not be entirely transformed, but there now is much greater chance for the openness and candid cooperation that has all along been our principal objective. The opportunity may be perishable if it is not developed at what is unquestionably a formative moment. There is some urgency.

I believe we should redesign the agenda of the meetings and the proposals we make to reflect the new situation. That in turn will require some discussion among ourselves. With that first step in mind, let me try to summarize the major substantive and procedural issues:

1. Who do we meet with?

As best I can guess, all of the counterpart group will now belong to the Russian Federation. I believe we must and should start with them, but I also assume that we will need to consider supplementary discussion with representatives from at least some of the other successor states. We will have to make a judgment as to whether to integrate these discussions or try to have them separately.

2. Do we have a consensus message to convey?

In our previous discussions we have concentrated on the categorization of agents in terms of risk and the definition of plausible thresholds for determining the amounts of these agents that would be inconsistent with the BWC. I do not think we should repudiate or ignore that line of argument but I do think we should now give greater priority to the underlying purpose of inducing a pattern of transparency and systematic international monitoring of BW related activities. I believe that organized transparency and active monitoring focussed on apparent violations will have to be the primary means of controlling weapons proliferation generally. I also believe that proliferation controls will rapidly become the central concern of security policy. If so, then BW arrangements might come to have a major formative role in redirecting overall security policy. This is plausible enough to imagine strong interest at high political levels and some dramatic developments in institutional cooperation.

3. Should we attempt to formulate an agreed proposal?

In the past we resisted such an objective as being neither feasible nor appropriate. I believe we should now reconsider. If we can reach consensus among ourselves, I would argue that we should attempt to enlist Russian/CIS colleagues as well and should attempt to issue a common paper.

4. Should we revisit the Sverdlovsk incident?

A series of articles in the Russian press dating back to December have suggested that an accident at the BW facility did initiate the anthrax outbreak in 1979 and that various military and state security officials consciously perpetrated a deception to conceal the installation's culpability. The published material derives from recent interviews with officials who said they were involved. It does not provide a detailed account of what happened but some of the assertions are difficult to reconcile with the account that we were given. In particular the articles cite statements by individuals who said they performed autopsies on victims and saw evidence of pulmonary origins of the infection.

There is a tendency in the United States to assume that the most nefarious explanation of this event is the correct one, and therefore these sketchy and not yet authoritative accounts will probably be believed. Certainly the explanation we were given will not be believed unless the new allegations are refuted or somehow made consistent with what we were told.

There are several possible constructions of this situation with very different implications for our activities: a)

The Burgasov/Nikiforov account is both accurate and substantially complete. Contrary accounts of an accident at the research facility either involve a separate incident at a different time or simply reflect unfounded fears of responsibility arising at the facility and generating a great deal of protective activity that was externally observed as cleanup and coverup. b) Burgasov and Nikiforov did give us an accurate account of their experience, which was nonetheless only a part of the actual episode. That would require that the case sequence they were directly involved with did extend over two months and did exclusively involve intestinal infections. The story as to the origins of the infection in contaminated meat might have been given to them by local officials and might have seemed plausible to them or at least not something they could question as a practical matter. If there were pulmonary cases they would have been handled by other people at another facility. c) Burgasov and Nikiforov gave us an account which they knew to be inaccurate and misleading. That would entail at least the suppression of some relevant evidence and at the extreme the fabrication of nearly everything they said.

I am of course reluctant to believe that the actual situation falls in category c, but if it does then we have to ask whether we can usefully proceed with our original counterparts. They and we would be seriously compromised by deliberate deception, and arguably both committees might have to be replaced in order to transcend the episode. Clearly we need to make some judgment about this within our own group even if we chose not to initiate any additional direct discussion with our Russian colleagues.

I suggest that we organize a meeting of our own group to review these issues and to work out a new agenda we might use in suggesting another round of the exchange. If you wish I could circulate the draft concept paper I sent to you earlier as a outline of the general security issues that might provide the context for BW issues. That paper has been exposed to Russian officials who have been interested in it.

NATIONAL ACADEMY OF SCIENCES
COMMITTEE ON INTERNATIONAL SECURITY AND ARMS CONTROL
2101 Constitution Avenue Washington, D.C. 20418

June 18, 1992

TO: Josh Lederberg, Pief Panofsky, and John Steinbruner
FROM: Jo Husbands

SUBJECT: Sverdlovsk Chronology

Since the dates for various events related to the explanation the Soviets gave CISAC and the NAS about Sverdlovsk are confused (the Post story is wrong, for example), I went back over the meeting/trip reports and put together the attached chronology. Also included is the Wall Street Journal article and Frank Press' letter after the Meselson meeting at the NAS. Hope you find these useful.

Please let me know if you need any additional information. (Pief, copies of the meeting reports will go out to you FedEx this afternoon.) Good luck.

6/18/92

CHRONOLOGY

CISAC AND THE SVERDLOVSK STORY

August 1986. Matthew Meselson visits Moscow and receives extensive briefings from 4 physicians, all affiliated with Russian government of Soviet Academy of Medicine. As far as we know, this is the first time a private U.S. citizen has been given this much detail about what supposedly happened in Sverdlovsk. (We have a copy of his trip report.)

October 8-9, 1986 The first CISAC BW meeting is held in Moscow. Josh Lederberg and Evgeniy Sverdlov are the chairs. CISAC receives a briefing from Nikiforov similar to that given Meselson during the meeting and also has an informal question and answer session with Nikiforov and a colleague. CISAC is apparently the second American group to hear the briefing. Both Sagdeev and Ivanov are present at the meeting, which is sponsored by the ASUSSR, along with other ASUSSR scientists.

April 29-May 1, 1987 Regular CISAC meeting in Washington, with BW on the agenda to report on October meeting. Plate reports for Soviet side, although he had not attended. According to meeting report, he says that "between the information they had shared with the NAS delegation, with Matt Meselson, and at the BWC Review Conference in Geneva last fall, they hoped that the Sverdlovsk incident had been discussed sufficiently so there would be no need to return to it. Osipyan emphasized the same point in his closing remarks." ... "He [Panofsky] said the point of the committee had always been that procedurally the USSR had been in violation of the BWC by failing to provide adequate information concerning the 1979 Sverdlovsk outbreak. Panofsky said he was pleased that procedurally this situation was very much improved in this and other forums."

April 11, 1988 Public meeting held at NAS to hear presentation from three Soviet physicians and public health officials (Nikiforov, Burgasov, Sergiev). This meeting is at request of Matt Meselson. No formal NAS or IOM sponsorship, although IOM hosts a dinner for them after the meeting. About 75 people attend the meeting, including press and their is subsequent coverage. A letter from Frank Press to the Wall Street Journal in response to an editorial in November 1990 (after Gorbachev wins the Nobel Prize) makes this clear and says that NAS does not endorse explanation.

May 9-11, 1988

Second CISAC BW meeting is held in Washington. Sverdlovsk is not on the agenda, but Meselson attends as an observer and reports on the April meeting. According to the meeting report, Sverdlov said "he had heard about the Sverdlovsk case for the first time at the meeting of these two committees in Moscow in 1986. He said he had been convinced by Nikiforov's presentation. But Sverdlov acknowledged that questions remained, partly because the Americans did not know Nikiforov and they did not know how much they could trust him."

NATIONAL ACADEMY OF SCIENCES
COMMITTEE ON INTERNATIONAL SECURITY AND ARMS CONTROL
2101 Constitution Avenue Washington, D.C. 20418

July 7, 1992

Dr. Vadim Tikhonovich Ivanov
Shemyakin Institute of Bioorganic Chemistry
Russian Academy of Sciences
Ul. Miklukho-Maklaya, 16/10
117871 GSP-7 Moscow V-437
Russia

Dear Vadim,

This message is sent after consultation with Pief Panofsky about the utility of reopening discussions with respect to BW arms control. The political context has, of course, changed dramatically since our last meeting; and we can look forward to the rapid dissipation of any adversarial tinge in our binational relationships. Nevertheless, in light of the perils of proliferation, it is no less important that we reinforce our mutual understanding. The initiative to which we agreed in December, 1990, strongly condemning any use of biological weapons, and responding with alarm to the threats made on behalf of Saddam Hussein, is very much in the spirit of what I hope might be advanced by further discussions.

In addition, American scientists would like to find all possible avenues of support for biomedical research in the CIS; for example, I am on the advisory board of the new American-Russian Biomedical Research Foundation, and we would welcome your input on the most efficient ways in which such funds might be allocated.

We have also to bring to fruition our discussions about the smallpox sequencing project, and about standards of regulation and disclosure that would be appropriate for the stronger enforcement of the BWC; and above all how these measures might be applicable to the threats of third country proliferation. To that end, our NAS group has been reinstated, and I hope to meet with them very soon with the aim of establishing an agenda, and specific proposals for a meeting with our counterpart Russian Academy of Sciences group. We would welcome your informal advice about the participation of the other CIS republics' scientific organizations.

There is however a cloud on the credibility of all of our academies' activities, highlighted by President Yelstin's revelations concerning the Sverdlovsk (1979) anthrax epidemic. We are all very much encouraged by the candor with which he is addressing these past offenses, and look forward to the completion of his own investigations and their disclosure to the world.

Unfortunately, our own group will be widely pictured as the vehicle for the transmission of a false account of that incident, particularly in the course of Dr. Burgasov and Nikiforov's visit to the United States. It does not take much imagination to foresee that this will become a matter of domestic concern in the Russian press, as grave as it also is in the arena of U.S.-Russian scientific relations. I am well aware that you and your

colleagues who met with us made no claims, and that you stated you had essentially no knowledge of the entire affair prior to our having placed it on the agenda of our discussions. In any event, it would be most helpful if you could suggest ways in which your academy could (1) further the complete investigation and documentation of that lamentable history, and (2) clearly establish its distance from it, so as to dispel those clouds, and provide a confident basis for candid discourse. As further testimony unfolds, there may be a good opportunity for both of our academies to help be the vehicles for the dissemination of the factual evidence.

You will recall that my own stand was to reserve judgment until the detailed accounts had been widely published within the Soviet Union, as well as in the U.S., so they could be given critical examination by your own colleagues and compatriots. We earnestly hope that this expectation might be fulfilled in the near future, and plainly this is the most reliable approach to establishing the truth.

Please let me hear frankly from you about steps that I can report to our academy that would be of assistance to clearing away these clouds.

Yours sincerely,

Joshua Lederberg /gk

Joshua Lederberg
Chair
Working Group on Biological Weapons Control

Lederberg

July 15, 1992

Dr. Joshua Lederberg
Rockefeller University
1230 York Avenue
New York, NY 10021

Dear Dr. Lederberg:

I am writing to seek your review of the enclosed manuscript "Preliminary Report on the Pathology of Fatal Anthrax in the Sverdlovsk Outbreak of 1979", by F.A. Abramova et al., which I wish to submit to the Proceedings of the U.S. National Academy of Sciences.

This article represents the first scientific report resulting from a study trip to Ekaterinburg (formerly Sverdlovsk) conducted by Dr. Walker, myself and four others last month. A second article will deal with epidemiologic aspects.

Since the Sverdlovsk anthrax outbreak has been a matter of unusual importance and controversy, we are anxious to exercise the greatest care and rigor in reaching conclusions regarding its nature and, as will be considered in the subsequent article, its epidemiologic aspects and possible source. I would therefore appreciate your most careful review of both the substance and the organization of the present manuscript.

Sincerely,

Matthew Meselson
Professor of Biochemistry
and Molecular Biology

May 16, 1993

To: jhusband%NAS.BITNET@cunyvm.cuny.edu
Fcc: NAS
Subject: backlog, re end May mtg
Reply-to: (J. Lederberg) lederberg@rockvax.rockefeller.edu

I did get your faxes while I was travelling in Japan last week, but it was not convenient for me to respond before now.

Time is short, but both of the two alternates you msg'd me about would be OK.

I should have raised this question sooner; but I'm wondering about our standing in having invited our own nominees to attend. How does this jibe with the definition of CISAC as an inter-academy consultation? If not that, how then are we defined? Could you ask Dr. Wyngaarden or Dr. Press about that? I'll be glad to call them for any further discussion. Do we just assume their academies are in such disarray that we're ad-hoccing?

Matt Meselson was not very happy about having been "left out"; though he probably couldn't make to it to Wye anyhow. I did promise him we'd send him minutes. I just told him we were trying to get OFF the question of "What happened at Sverdlovsk?", we were not an investigative or intelligence body.

He calculates as little as 10 mg. spores could have accounted for the epidemic!

Please acknowledge you've received this.

Josh

MAY 16 1993

SUMMARY OF CONCLUSIONS AND DECISIONS

CISAC Working Group on Biological Weapons Control
Planning Meeting
October 5, 1993

BW Conversion

1. If the proposed NAS program for the Department of Defense on conversion assistance goes forward, and includes BW, the Working Group is interested in helping to at least launch the effort.

   a. For this purpose (and for the Working Group's other activities), it seems likely that a trip to Moscow in early 1994 will be needed.

   b. In advance of such a trip, considerable research would be needed to find out about, and then make contact with, individuals and institutions who might be of interest for conversion projects or other activities.

   c. The Russian Academy is probably not the channel for conversion projects like the ones the Group has been discussing (see #2 below). Since the NAS does not feel bound to work through the Russian Academy for its much larger program, if Working Group projects are part of that effort, it may be relatively easy to disengage from Petrov & co. for this activity.

2. Tom Monath's idea for cooperative work on dangerous diseases remains the best potential project. To get things moving, the initial effort should be between the U.S. and Russia, and should begin with laboratories like Sandakchiev's. Involving the purely military labs will require greater openness than is currently the case, and a more broadly international effort will take more time to develop. Both of the latter goals are important, however, and should be what the project works toward.

3. Encouraging greater transparency from the Russian military remains extremely important, and military-to-military contacts appear the best vehicle for that. The Russian Academy does not appear a profitable channel for such efforts. Josh Lederberg and John Steinbruner will take advantage of having General Kuntsevitch here for the CISAC meeting in late October to assess whether he could be helpful in this regard. More broadly, this will be an opportunity to decide how much we would want to work with him in future projects.

Smallpox

1. At the moment there is nothing that the Working Group needs to do, since it seems likely that the December deadline for destruction will be postponed to ensure a more thorough international discussion.

2. As an individual, Josh Lederberg will try to find outlets to publish the results of the discussions at the Glasgow conference, as one means of encouraging international attention.

Anthrax/Sverdlovsk

1. Further disclosures about what happened in Sverdlovsk will require a political decision at a relatively high level (not necessarily Yeltsin, but close to his level). Recent events might actually enhance the chances for openness, but that is the view of only some analysts.

2. There are a number of rather modest efforts that could help advance knowledge about the remaining questions regarding the Sverdlovsk incident. Most of these would be indirect, however, and would deal with more general issues of anthrax pathogenesis.

a. A small conference is Sverdlovsk on some aspect of anthrax might make productive connections. There are Americans and credible Russians who are interested and could make contributions.

b. One topic might be the anthrax vaccine that the Russians have said they developed, about which little is known in the West.

c. It might be possible to develop small joint research efforts. A conference devoted to discussing the research challenges posed by anthrax could be a first step toward such projects.

3. Once Matt Meselson's findings are published they could provide the basis for further discussion of Sverdlovsk, and the Working Group might be one vehicle for dissemination. At present, there is still no one on the Russian side prepared to or capable of playing a role comparable to Meselson's in promoting further disclosures.

Bilateral Cooperation

1. Verification.

a. The Working Group will keep in contact with Elisa Harris as the Administration develops its strategies to strengthen the BWC through greater "transparency." A meeting with her may on this topic may be arranged later.

b. On these issues, bilateral contacts and discussions appear to remain useful.

c. With the idea that any regime of disclosure must be simple and easy for nations to comply with, John Steinbruner and Tom Monath agreed to revise their classification scheme to produce a simplified list and a short discussion paper on how a disclosure regime might operate. They agreed to a 3 week deadline (about October 26th) for a first draft.

2. A Grassroots Movement by Physicians against BW.

a. After further discussion, especially comments from Matt Meselson, the group decided that this sort of effort would not be worthwhile, and could even have negative consequences.

3. Global Surveillance.

a. The recommendations of the report on Emerging Infections have been adopted by the Centers for Disease Control. At present, the main issue is how to get funds for the new projects into the budget cycle.

b. As these projects develop, there may be a basis for bilateral cooperation.

4. Intelligence Sharing.

a. The group decided that these activities are best left to develop through the military-to-military channels that it hopes will be developed.

Next Steps: Josh Lederberg and John Steinbruner will meet with Rem Petrov and General Kuntsevitch on October 27th during the main CISAC meeting with its Russian counterpart group. A schedule and potential topics for a next meeting, presumably in Russia, will be discussed.

THE SALK INSTITUTE

GOVERNMENT SERVICES DIVISION

San Antonio, Texas

DATE: January 7, 1994

MEMO TO: Jo Husbands

FROM: Alexis Shelokov AS (by ams)

SUBJECT: Agenda Item for Meeting on January 14

In your memo of December 13, you mentioned developing a list of possible Russian participants to be invited by the Russian colleagues. Here are my suggestions:

- Drs. F. A. Abramova and L. M. Grinberg, the two Ekaterinberg pathologists -- key players in unraveling the story.

- Dr. Olga Yampolskaya -- Professor Nikiforov's assistant in 1979 and when Matt Meselson later visited Moscow; also, member of Matt's 1992 Ekaterinberg team; she is currently in Galveston, but she expects to return to Moscow on or about April 12. (She may have to be invited while she is still in the United States.)

- Dr. Benjamin (Ven'yamin ??) Cherkasskiy -- my contacts tell me that he is a senior Moscow veterinarian, who for the past 10 or 15 years has headed the WHO Collaborative Center for Zoonoses; he is very knowledgeable about anthrax.

- Professor A. V. Yablokov -- State Counselor to President Yeltsin; I believe that he made it possible for Matt Meselson's team to travel to Ekaterinberg in 1992; he has been otherwise much involved in the Sverdlovsk affair.

- Several civilian medical personages originally involved in in the Sverdlovsk affair; we met them, but subsequent scheduled visits with us were cancelled by political edicts; we can discuss this on the 14th. My tentative list would include; Dr. Il'yenko (Chief Physician, Hospital #20), Dr. Ponomarenko (Chief Oblast Epidemiologist in 1979), and Dr. Bolshakov (chief medical officer on the scene both in 1979 and 1992 -- who obviously had a lot more to tell us than he was allowed). (I would have included Drs. Babich and Yaroslavtsev, two of the really key Sverdlovsk players, but both died since our 1992 visit -- joining in death the two key Moscow experts, Professors Bezdenezhnykh and Nikiforov; 20 years ago I'd be very suspicious -- but even today should Dr. Ilyenko die in 1994, I'd get suspicious again.)

4606 Centerview Drive, Suite 157 • San Antonio, TX 78228-1203
Telephone (210) 732-4164 • FAX (210) 732-7544

Should we suggest inviting Generals Yevstigneyev and Konsevich, and some of the military medical officers formerly working on anthrax at Compound #19 (e.g., General Kharechko)?

- There is reason to believe that there were anthrax research experts, including pathogenesis, not only at Compound #19, but also at the Koltsovo/Novosibirsk laboratories, now known as NPO "VECTOR"; the director, Prof. Lev S. Sandakhchiev, joined us at the meetings last May.

- According to my information, the Gamaleya Institute, directed by Prof. Prozorovskyi, has several major laboratories devoted to research on bacteria and bacterial diseases; I recollect that several years ago his Institute sponsored an anthrax symposium (which I did not attend, but I was able to arrange an invitation for S. H. Leppla of Fort Detrick, who did attend).

- Finally, I urge our own committee to invite two additional members of Matt's Sverdlovsk team -- Martin Hugh-Jones of LSU/Baton Rouge and David Walker of UTMB/Galveston. Both, besides their other relevant qualifications, have recently reviewed in depth the world literature on anthrax.

P.S. I dictated the above before your Fed Ex package arrived today (January 7th).

A.

Copy to: J. Lederberg (FAX)
M. Meselson (FAX)

From jsl@rockvax.ROCKEFELLER.EDU Mon Jan 17 16:37:46 1994
Received: from rocky8.rockefeller.edu by rockvax.ROCKEFELLER.EDU (5.65/1.34) id AA28949; Mon, 17 Jan 94 16:36:31 -0500
Received: from jl.rockefeller.edu by rocky8.ROCKEFELLER.EDU (5.65/1.34) id AA02670; Mon, 17 Jan 94 16:36:30 -0500
Message-Id: <9401172136.AA02670@rocky8.ROCKEFELLER.EDU>
To: msm@isr (Matt Meselson)
Subject: Your ms. on Sverdlovsk
Date: Mon, 17 Jan 94 16:38:54 EST
From: Joshua Lederberg <jsl@rockvax.ROCKEFELLER.EDU>
Status: R

Thank you for letting me see that. It is a meticulous work! - beyond what I really had time to dig into, in the time now available.

You might want to plot the hypothetical plume with isochors (is that the right word?) for given exposure densities according to your model.

I'm not sure how much of the commentary about Yeltsin etc. belongs in the paper. It could go in the commentary that I'm sure Maddox will invite.

You refer in the abstract and on p. 2 how "US Agencies" contradicted the Soviet account. Didn't it all start from German emigre publications, and then the Wall St. Journal, before the "agencies" took any position? But this stuff is commentary too: obviously you need some bare minimum to explain why a paper is only possible now.

Yours,

Josh

MAR-25-94 FRI 12:38 ALEXIS SHELOKOV FAX NO. 12107327754 P. 01

Post-it™ brand fax transmittal memo 7671 | # of pages ▸ 1

To J. LEDERBERG	From ALEXIS SHELOK
Co.	Co.
Dept.	Phone #
Fax # (212) 327-8651	Fax # (210) 733-7544

MEMORANDUM

DATE: 24 MARCH 1994

MAR 25 1994

TO: JOSHUA LEDERBERG

FROM: ALEXIS SHELOKOV

SUBJECT: ANTHRAX OUTBREAK IN SVERDLOVSK

A Russian virologist, now residing in the U.S., aware of the Meselson team visit to Sverdlovsk, asked me about our findings. He also related the following.

His mother, an epidemiologist and infectious disease specialist in Moscow (particularly interested in airborne transmission) told her son sometime in the early '80s (she died in 1984), that the Sverdlovsk outbreak was not due to ingestion of meat or bone meal contamination, as announced by the Deputy Minister of Health, Academician Burgasov. Rather it was an accidental release from a military laboratory during lyophilization of a large quantity of anthrax bacilli. Somehow, the lyophilizer came apart (blew up?); the spores were only partially dried, which, according to the informant's mother, was unfortunate (could it be that the fully dried lyophilized "plug" would have been more difficult to disperse?). Owing to poor laboratory practices, the filter in the exhaust mechanism was not in place, and the rest is history.

For those of you with more aerobiologic background than mine -- what atmospheric conditions would have favored the spread of partially dried spores that winter April morning? Below-freezing temperatures? With low humidity? A cloudless sky? Would a snowfall have hindered the spread??

Alexis

cc: Robert Chanock
Jo Husbands
Matthew Meselson
Thomas Monath
John Steinbruner

Confirmation Copy

Kaplan

SEP 01 1995

To: Josh Lederberg, Matt Meselson and Julian Perry Robinson
From: Martin Kaplan 25 August 1995

On 22 August I had an extended meeting with Anthony Piel, Director, Cabinet of the Director-General at WHO. We discussed potentialities of WHO with regard to decisions to be taken at the BW Review Conference in 1996. In particular, as I have mentioned in our meetings on CBW in the Netherlands, it is highly doubtful that appreciable financial allocations within the UNO would be made to support any new effort analogous to IAEA to monitor compliance with the BWC, such as full-time standing inspection teams for routine activities or allegations of use. I believe WHO is the proper organization to be given responsibility in these matters. This would mean a large saving in costs for implementing the strengthening of verification aspects of the BWC.

Piel strongly agreed with this view and outlined WHO's resources for that function: WHO's scientific knowledge and epidemiological interest as well as administrative capability and existing geographical presence in six regional offices and 110 countries of 190 Member States. I will mention this in a short working paper I will prepare for the meeting. Piel endorsed the decision of Dr. Ralph Henderson. Assistant Director-General to whom I had spoken the previous week, to send Jim Le Duc (seconded to WHO by the CDC) and Francois Meslin Chief VPH in their personal capacities to attend our meeting. Apart from the BW aspect, WHO is greatly interested in and is expanding its program regarding new and emerging infections.

Piel emphasized, however, that WHO has not played an "enforcement" role; the UN's and WHO Member States consensus would be needed. He pointed out that there is an absolute necessity for additional funds to be committed to cover fully such purpose in view of the current budgetary and financial constraints. He also stressed that support by the USA for these matters is essential, particularly in Washington. I call this to your attention now so that it can be thought about and perhaps discussed privately apart from our meeting itself, and then with Piel himself.

Piel also passed on to me a disturbing piece of news that he said I could share with you, although it has not yet been revealed publicly. Despite a strong written agreement between WHO and Russia to retain stocks of the smallpox virus in Moscow accessible to inspectors from WHO until agreement is reached on their destruction, WHO has recently been informed by Russia (without prior consultation) that these stocks have been moved to a military laboratory, presumably in Siberia, for "security" purposes.

cc. Francesco Calogero, Rome

Matt,
I saw you for 30 seconds
the other day on CBS news reporter
in [illegible] re Anthrax
Love to the family
Martin

To: Professor Rotblat and Claudia, London office
From: Martin Kaplan MK
25 August 1995

1. Please discard the version of my paper for the London meeting sent yesterday. I have made minor but important modifications in the revised version attached.

2. You also received yesterday two minor modfications on pages 3 three and four of my book review that were suggested.They are line 12,of p. 3 deletion of (now termed executive committee), and p.4 third line of par. 3 should read myself included, held a closed private session (which was not a Pugwash meeting) to hear etc. Please let me know if this satisfies the suggestions made.

3. Claudia, with regard to your latest update of the BW meeting in December, the response to Barbara Rosenberg should be that we are inviting leading members of the ad hoc group to a cocktail reception on Saturday evening some of whom will be participants in the meeting itself, she can refer to the list you are sending. Mahy's abstract is O.K. As Francesco will realize from Batsanov's letter he has a place to stay in Geneva and that would save the hotel expenses that would ordinarily be involved. We have just received the contribution of 10,000 francs from the Federal Government in Bern which is in addition to the 10,000 francs also contributed by the Canton of Geneva. I can probably use a small part of these funds to help with travel of important participants such as Batsanov if necessary. I leave this to FC to suggest. Pearson's fax is O.K. Concerning Geissler's two papers, if they are not too long I should like to see them because he has sung the same song several times in other meetings. Perhaps he can be persuaded to shorten them or merely refer to his other publications on the subject. I am going to write Lederberg, Matt and Julian on another subject, with copy to you, and state that I have answered your item 2 and the Geissler query. Perhaps Julian will go to the Wilton Park meeting which he knows about.

CC Meselson & Julian

(111)

NATIONAL ACADEMY OF SCIENCES
COMMITTEE ON INTERNATIONAL SECURITY AND ARMS CONTROL
2101 Constitution Avenue Washington, D.C. 20418

/ BW

TELEFAX

Cover Sheet

DATE: May 30, 1996

TO: Joshua Lederberg, Rockefeller University

Robert Chanock, Laboratory of Infectious Diseases, National Institute of Allergy and Infectious Diseases

Matthew Meselson, Harvard University

Thomas Monath, OraVax

Alexis Shelokov, The Salk Institute

John Steinbruner, Foreign Policy Studies, The Brookings Institution

FROM: Jo Husbands and La'Faye Lewis-Oliver
National Academy of Sciences
Telephone: 202/334-2811
FAX: 202/334-1730

NUMBER OF PAGES: 4
(including cover sheet)

MESSAGE: The news for our project is generally very good. Thanks to John Steinbruner's heroic efforts with Harold Smith, several roadblocks that had come up have been cleared and we are back on track for our contract negotiations. The optimistic assessment is that we will have our money by the end of the month, or certainly by mid-summer.

Matt Meselson and I had a good meeting with Lev Sandakhchiev in Moscow; the notes from that meeting are included with this fax.

There are a number of important issues that we need to discuss in anticipation of our new work. **According to the calendars you sent in a few weeks ago, everyone could take part in a conference call on Friday, June 7th. We will call each of you later today to confirm that and to choose a time, ~~or to find~~ an alternative date and time.** We would probably need about two hours to cover everything we should discuss, and would send out an agenda and materials for the call early next week.

Cheers.

NATIONAL ACADEMY OF SCIENCES
COMMITTEE ON INTERNATIONAL SECURITY AND ARMS CONTROL
2101 Constitution Avenue Washington, D.C. 20418

May 29, 1996

MEMORANDUM OF CONVERSATION
with Lev Sandakhchiev, NPO Vector
May 13, 1996

Matthew Meselson and I met with Lev Sandakhchiev and Giorgy Scherbakov at the Danilovsky Hotel.[1] Scherbakov is the Chief of the Department of Science and Technology for the Biopreparat institutes. Sandakhchiev had come to Moscow to join him for a meeting with officials from NASA about 17 joint projects at NPO Vector involving experiments under microgravity conditions. They had just completed their first year of work and were to present the results and discuss plans for new and continuing efforts.[2]

Sandakhchiev said that he had begun working on his paelovirology with victims of natural smallpox found in Siberia, which would keep him very engaged during the summer. We told him that we had seen the long report he had sent to Josh Lederberg. He also told us that Vector had submitted a proposal to ISTC for paleovirology research on smallpox; we need to doublecheck on its status. He also mentioned that Lyme disease is a serious problem for them, on which they would be interested in joint work.

Matt and I explained our new project, emphasizing that, even though the money is not in hand we expect that it will arrive soon and that we did not want to wait until then to talk to him about our plans and to get his advice. We explained that there would be two components: (1) the report to DOD from the National Academy with a plan for a major cooperative program; and (2) the initial grants to enable the Russian researchers and their

---

[1] At the last minute, Alexis Shelokov was not able to come to Moscow. Matt and I were concerned about language problems, but it turned out we were able to get help with translation from Igor Kharitonenkov, a virologist from Moscow State University who had come to the hotel to collect some materials that one of my Academy colleagues had brought him from the U.S. It also turned out that he does research on influenza, knows Rob Webster, and was a great help, especially since Scherbakov did not speak much English. Because of his presence, however, we did not raise the question of involving Russian researchers from other than the former BW institutes in the project.

[2] The project leader at NASA is Robert Rhome and Laura Holgate has also advised us to talk to him, as there are apparently some lessons and cautions for our work from their experience.

Western partners to make concrete plans and even undertake some preliminary work. We said that our initial thinking was to work with NPO Vector and with Obolensk for this first year, and then hope to expand the program in the future.

Scherbakov commented that, as part of the trilateral agreement in 1992, Vector and the other institutes had prepared a program for conversion with many projects that were approved by the MOD. The proposed program had been presented to the U.S. and U.K. delegates, James Goodby and Terrence Taylor, but nothing had ever happened. Nonetheless, he was happy to see that there was now the prospect of new actions. We replied that knew there had been too many promises and too little action, which was why we had waited until we were confident we had something to offer. Sandakhchiev commented that, like us, the Russians had had no success in getting assistance for BW conversion on the Gore-Chernomyrdin agenda from their side.

Scherbakov also commented that he wished it were possible to have the kind of lab-to-lab cooperation between Russian and American scientists BW issues that was proving so successful in the nuclear and CW fields. He would welcome the opportunity to collaborate with scientists at Ft. Dietrick.

Scherbakov added that a year ago, ISTC had held a seminar in Novosibirsk to give directions for conversion projects. He expressed his great appreciation for the U.S. assistance and said that we should be careful not to duplicate those efforts. We told him that we were working closely with ISTC and saw our work as complementing what ISTC is doing. With the help of our Russian colleagues, we also hope to be able to offer a broader vision and expanded scope for such cooperation.

Sandakhchiev suggested that we might want to organize a Russian and American working group for the project. He volunteered to organize the delegation from the Russian side, which would include people from the Ministry of Science, the Ministry of Health, and the Russian Academy. He suggested that our first formal contacts should go through Saltykov, the Minister of Science. Academician Kerpechnikov is in charge of the relevant department in the ministry, he told us.

Sandakhchiev asked about the funding mechanism and suggested that we consider either the ISTC or the Russian Fund for Basic Research, headed by Academician Fortov. Both of these have already survived the process of getting a favorable and workable tax status and this would save us considerable effort. We agreed and said that we had been exploring such a method for transferring the funds to our Russian colleagues.

We gave Sandakhchiev the letter from Rob Webster, and here he was interested but less encouraging. They have had good success in acquiring data, and hence victims, of outbreaks of cholera, smallpox, and "red measles" from the 18th and 19th century. They are also currently working on a 5,000 year-old mummy found in the permafrost. But because the influenza outbreak occurred during the civil war that followed the Russian Revolution, he

is not confident that they can locate adequate records and, hence, find the bodies of relevant victims. He suggested that Alaska or Canada might be better places to investigate. When Matt pressed him on his general interest, Sandakhchiev repeated that NPO Vector would be interested in participating in the research effort on influenza, including sequencing efforts. He also noted that, once a body has been located, it is possible to test for a range of disease agents.

SUMMARY: Sandakhchiev was very cordial and interested in participating in our work. He had a number of helpful suggestions to make and seems prepared to be actively engaged. Whether the specific research project on influenza will work out is less clear; he welcomed the proposal, but raised some important practical questions. Perhaps the next, best step is to put him directly in touch with Rob Webster.

Jo L. Husbands

Lederberg

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To DR MATT MESELSON	From DR A. SHELOKOV
Co.	Co.
Dept.	Phone #
Fax # (617) 495-8308	Fax # (210) 732-7544

MEMORANDUM

DATE: 24 MARCH 1994

TO: JOSHUA LEDERBERG

FROM: ALEXIS SHELOKOV

SUBJECT: ANTHRAX OUTBREAK IN SVERDLOVSK

A Russian virologist, now residing in the U.S., aware of the Meselson team visit to Sverdlovsk, asked me about our findings. He also related the following.

His mother, an epidemiologist and infectious disease specialist in Moscow (particularly interested in airborne transmission) told her son sometime in the early '80s (she died in 1984), that the Sverdlovsk outbreak was not due to ingestion of meat or bone meal contamination, as announced by the Deputy Minister of Health, Academician Burgasov. Rather it was an accidental release from a military laboratory during lyophilization of a large quantity of anthrax bacilli. Somehow, the lyophilizer came apart (blew up?); the spores were only partially dried, which, according to the informant's mother, was unfortunate (could it be that the fully dried lyophilized "plug" would have been more difficult to disperse?). Owing to poor laboratory practices, the filter in the exhaust mechanism was not in place, and the rest is history.

For those of you with more aerobiologic background than mine -- what atmospheric conditions would have favored the spread of partially dried spores that winter April morning? Below-freezing temperatures? With low humidity? A cloudless sky? Would a snowfall have hindered the spread??

Alexis

cc: Robert Chanock
Jo Husbands
Matthew Meselson
Thomas Monath
John Steinbruner

Date: Thu, 24 Apr 1997 12:27:01 +0200 (MET DST)
From: Pugwash <pugwash@hei.unige.ch>
To: Charlene_Breedlove@ama-assn.org
Cc: Joshua Lederberg <jsl@rockvax.rockefeller.edu>,
Matthew Meselson <msm@wjh.harvard.edu>,
Julian Perry Robinson <j.p.p.robinson@sussex.ac.uk>

Dear Ms Breedlove, I have your letter of April 17 and the enclosed comments on my article by reviewers A and B. I disagree with practically every comment made by reviewer A, and would accept partially only the last paragraph of reviewer B. In any event I withdraw my submission to JAMA for publication as I do not consider it worthwhile to spend the time and effort to fulfill the requirements for consideration and possible publication in your journal. Thank you for the time and trouble you have taken. Please note that I am sending a copy of this message to the individuals I have mentioned in appreciation for their help in preparing the manuscript. Yours, Martin Kaplan

Date: Thu, 24 Apr 1997 10:31:57 +0200 (MET DST)
From: Pugwash <pugwash@hei.unige.ch>
To: Joshua Lederberg <jsl@rockvax.rockefeller.edu>
Cc: Matthew Meselson <msm@wjh.harvard.edu>,
    Julian Perry Robinson <j.p.p.robinson@sussex.ac.uk>
Subject: Re: Pugwash 1959

Josh, I am arranging to send you a copy of the proceedings of the 1959 Pugwash CBW meeting that you requested as well as a copy of the New Scientist article by Heden on the 1970 meeting in Mexico City. Also, I have received today a letter from Charlene Breedlove, Associate Editor of JAMA with copies of comments from two reviewers which recommend extensive cuts and some factual clarifications. I do not mind doing the latter, but I will not undertake the time and effort required to meet their requests as I do not believe such an undertaking to be worthwhile or, in many respects, justified. I will
notify JAMA accordingly. In fairness to the three of you, and again thanks for your help, I will send you copies of the JAMA letter and the comments of the reviewer. Martin. On
Wed, 23 Apr 1997, Joshua
Lederberg wrote:

>
> Thank you, Martin
>
> This will be helpful also for my general introduction.
>
> Josh
>

Lederberg

DEPARTMENT OF MOLECULAR AND CELLULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138

24 April 1997

Dr. Joshua Lederberg
Rockefeller University
1230 York Avenue
New York, NY 10021

Dear Josh,

Here are three items I thought you might like to have in connection with your overview article for JAMA. The first "A Proposal to Inhibit the Development of Biological Weapons" was written for the Fourteenth Pugwash Conference held, I believe, in Venice, Italy. The other two are private papers I wrote for Henry Kissinger in 1969 and 1970. Henry told me at the time that President Nixon had read both of them, and I know that the President had the paper on toxins with him and took it into consideration when he made his decision regarding toxins at Key Biscayne.

I am sending "The United States and the Geneva Protocol of 1925" and "What Policy for Toxins?" to Charlene Breedlove at JAMA in case there is any interest in publishing either one of them.

Tomorrow I am off to Moscow along with Alexis, John and Jo.

Sincerely,

Matt

Date: Fri, 25 Apr 1997 16:12:02 +0200 (MET DST)
From: Pugwash <pugwash@hei.unige.ch>
To: Charlene Breedlove <Charlene_Breedlove@ama-assn.org>
Cc: Joshua Lederberg <jsl@rockvax.rockefeller.edu>,
    Matthew Meselson <msm@wjh.harvard.edu>,
    Julian Perry Robinson <j.p.p.robinson@sussex.ac.uk>
Subject: Re: your mail

Dear Ms Breedlove, Thank you for your message of the 24th. I would like to make it clear that on my part correcting any factual differences was not the problem, although it may be that of the reviewers. My major objections concerned the
political and ad personem innuendos by the reviewers, and the time and effort involved in trying to take them into account and to rewrite a personal memoir on what I
thought were interesting aspects of post World WarII history of biological and chemical weapons not generally known. As to the length of the article, I had thought that a previous answer from JAMA stating that my submission might better fit the JAMA "In Retrospect" series would be followed by a suggested abbreviated version which I would have been glad to consider, but that seems to have been forgotten. I hope this clarifies the matter for you as well as for my colleagues who have expressed dismay.On Thu, 24 Apr 1997, Charlene Breedlove wrote:

> I'm terribly sorry to learn that you are withdrawing your paper and of
> your complete disagreement with the reviewers' comments, though I can
> well understand that some differences of historical interpretation may be
> irreconcilable. Perhaps these events are best reported in a book. Thank
> you for the opportunity to consider your work. I shall return the
> manuscript.
>
>

Date: Fri, 25 Apr 1997 10:15:31 +0200 (MET DST)
From: Pugwash <pugwash@hei.unige.ch>
To: Joshua Lederberg <jsl@rockvax.rockefeller.edu>
Cc: Matthew Meselson <msm@wjh.harvard.edu>,
Julian Perry Robinson <j.p.p.robinson@sussex.ac.uk>
Subject: Re: Pugwash 1959

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[Your display is set for the "US-ASCII" character set]
[Some characters may be displayed incorrectly]

Dear Josh, Thanks for your message about the JAMA article. Yesterday I airmailed to the three of you the comments of the reviewers, but I didn't include the onerous requirements and work involved in answering their points. I would have been willing to correct any factual errors, and to shorten the paper as I thought JAMA might take in hand themselves after they notified me that it would better fit their "In retrospect" series. But with other obligations I am facing during the next several months I could not agree to undertake the hassle that would be involved in answering the reviewers point by point. In any event as soon as some time and leisure permit I'll return to the article and prepare it for archival purposes, and include any factual errors I have made. In the latter connection I solicit the help of all of you.
Josh-- yesterday I also airmailed
to you copies of the Bull. Atomic Scientists articles in 1960 which dealt in some detail with our 1959 meeting, and a copy of the minutes of each of the sessions. I am preparing for you a copy of the entire proceedings (some 150 pages)which include the working papers. I also included in yesterday's dispatch the four page
article of Hedén in the New Scientist describing the meeting in Mexico City in 1970 of the International Microbiological Congress's resolutions against BW.

Anyhow, cheers for the ratification of the CWC.

On Thu, 24 Apr 1997, Joshua Lederberg wrote:

> Dear Martin
> I will try to intervene with JAMA. But whatever you do, please
> complete your article -- with factual clarification -- to your
> own standards. It will be an important archival document, and sure
> to be published elsewhere if not in the JAMA.
>
> Josh
>

Date: Mon, 28 Apr 1997 11:40:29 +0200 (MET DST)
From: Pugwash <pugwash@hei.unige.ch>
To: Julian Robinson <J.P.P.Robinson@sussex.ac.uk>
Cc: Joshua Lederberg <jsl@rockvax.rockefeller.edu>,
    Matthew Meselson <msm@wjh.harvard.edu>
Subject: Re: Pugwash 1959

Dear Julian, Thanks for your kind offer to help in any revision of the JAMA article. You will have seen by now my last reply to Breedlove, and perhaps you have received by now a copy of the reviewers' comments. I doubt that she will accept my suggestion abou JAMA preparing a shortened article for inclusion in their Retrospect series which they brought up in their earlier message. During the next few days we will send all of you the excerpt in the Bull. Atomic Scientists on our '59 meeting, a set of the minutes and a complete copy of the report (including the working documents) of that meeting which all of you requested. I was glad about the arrangement for Jeanne to carry the tab for Pugwash at the First Session of the SPOPCW next month. Martin.

On Fri, 25 Apr 1997, Julian Robinson wrote:

> Dear Martin,
>
> Thank you for this message. I was very much looking forward to
> reading, and being able to cite, your impending JAMA article, so
> I do hope a way can be found around the current impasse.
>
> I haven't yet seen the reviewer's comments, so I don't know what
> would be involved in amending the text to cope with them, but if
> you think I could help with the amendment process and maybe take
> some of the load off you, I would be very happy to do so. Please
> let me know and I will make the necessary time.
>
> If, however, you definitely decide to go down the unshrunk
> manuscript-for-the-archives route on which you solicit our help,
> remember that I am still in the process of completing my paper on
> Pugwash and CBW for Alex Keynan's project. What you had from
> me back in January is as far as I have got. I have still some
> detailed research to do on which meetings happened when and who
> participated, and such like matters. Not all of this will be
> presented in my paper but might very well go into yours. In
> fact, knowing that it might could mean that I can be briefer than
> I had intended in the project paper, and therefore able to finish
> it more quickly. Alex is breathing down my neck.
>
> Best,
> Julian
>
>
> Dear Josh, Thanks for your message about the JAMA article.
> Yesterday I
> airmailed to the three of you the comments of the reviewers, but
> I didn't
> include the onerous requirements and work involved in answering
> their
> points. I would have been willing to correct any factual errors,
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> But with other obligations I am facing during the next several

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> (some 150 pages)which include the working papers. I also included
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> yesterday's dispatch the four page
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> Mexico
> City in 1970 of the International Microbiological Congress's
> resolutions
> against BW.
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> On Thu, 24 Apr 1997, Joshua Lederberg wrote:
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> please
> > complete your article -- with factual clarification -- to your
> > own standards. It will be an important archival document, and
> sure
> > to be published elsewhere if not in the JAMA.
> >=20
> >   Josh
> >=20
>
>
>
>
>

Date: Thu, 23 Oct 1997 18:41:19 -0400 (EDT)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: "Dr. Paul JACKSON" <jackson@telomere.lanl.gov>
Cc: Professor Joshua LEDERBERG <jsl@rockvax.rockefeller.edu>
Subject: Your MS

Dear Paul,

Thank you for sending your MS to me for one last look. Here are some comments you might consider.

1) Abstract, line 8: In order to make your unambiguous proof that the infection was definitely anthrax stand out more clearly than it does, you could say something like "Tissue samples from 11 persons who died during the epidemic were examined. The results demonstrated that the entire complement of toxin and capsular antigen genes of B. anthracis required for pathogenicity were present in tissues from each of these 11 victims. Tissue from a vaccination site of one of the victims contained primarily..."

2) Abstract, line 13: Since you do not rule out cross contamination, it would be more accurate to add the word "samples", giving "...region were present in the tissue samples."

3) Page 3, para 1, line 6: "...narrow high-risk zone within which most of the victims..."

4) Page 4, para 2, line 1: You could identify the two authors: "...two of the authors (FAA and LMG) and used..."

5) Page 7, para 1, line 10: In view of your experience of cross-contamination from even carefully cleaned blades and the virtual certainty that new blades and similar precautions were not taken at the time of sampling and storage, I agree with Joshua that for emphasis you should restore the sentence "However, the handling history of the human samples prior to their arrival in our laboratory is unknown, and it is unlikely that such precautions were taken during the initial sampling and histological analysis." (Underlined words added).

6) Page 8, para 2, lines 3-5: The sentence is a tautology. Also, you have data on 11 victims, not on "all" victims. So you might say something like "These results clearly demonstrate that B. anthracis was responsible for the 1979 epidemic." Considering that bacilli were reported by the Russian pathologists to be visable in tissues from all 11 victims, the failure to culture live B. anthracis from victims 21 and 31 is less important than your definitive demonstration that B. anthracis was indeed present in all victims you tested. If you agree, you might omit the words "...regardless of whether microbial analysis of the tissues detected this organism." since the point is clearly stated on page 11 in para 2.

7) Page 9, para 2, line 4: Did you test one or more than one primate? Also, better to say "non-human primate".

8) State explicitly whether nested primers used to analyse the infected non-human primate tissue.

9) Page 11, para 2, line 6: "Exceptions" seems the wrong word. Abramova and Grinberg do report visable bacilli in samples 21 and 31. The fact that they report negative cultures for these samples is not really an exception. You could omit the words "with two exceptions" and start the next sentence with "Although Abramova et al. report..." and remove the period before "PCR results..."

10) Page 12, para 1, lines17-18. I would omit the last sentence here. It doesn't add anything to your report and I doubt the approach would be reliable.

11)Page 13, para 3, pages 4-6: The argument here is tautological. It assumes that the distribution in the tissues is like that in the

world. I suggest you delete it.

Page 17, legend for Fig. 4, line 9. It would help to say "...on the left side of the gel..."

Aside from these few comments, most of them minor, I think the MS is excellent and adds one more piece to the Sverdlovsk puzzle, an important piece -- it really was anthrax!

Jeanne and I have not forgotten Los Alamos. We both send our warm regards.

Sincerely,

Matt.

^Z

Date: Mon, 13 Oct 1997 18:21:57 EDT
From: Joshua Lederberg <jsl@rockvax.rockefeller.edu>
To: Paul Jackson <jackson@telomere.lanl.gov>
Cc: Matt Meselson <msm@wjh.harvard.edu>
Subject: Manuscript describing the Sverdlovsk anthrax samples.

I have to be deeply apologetic about not following through on this over the summer. You and Matt have been in direct conversation; and I am utterly confused whether you and he have reached a satisfactory resolution about the comment in "Results" about the overhang of possible contamination.

It is a feeble compensation that it is more in your interest (and the public's) to get this just right than it is to expedite publication.

At end of "Results":
For my part, I would urge you to leave in the statement seen in a prior draft that "the prior handling history is unknown, and it is unlikely .. precautions .. during histological analysis"

"and ... Does not alter the results." is certainly too sweeping;
Matt's suggested " does not alter the conclusion that multiple strains were present in the samples regarded collectively is better.

With such a low attack rate, it opens up new questions that such a high proportion of individuals who come down with anthrax at all, would appear to show multiple strains. That is, a high correlation between the presence of one strain with another. That could speak to heterogeneity of the target population (genetic or environmental) or a very lumpy dissemination of the spores.

The *important* thing to recognize is that molecular analysis has corroborated anthrax as the culprit, whatever the final disposition of the multiple srains. I hope one day we can be as decisive about the 14th C. "black death".

You mention difficulties engendered by cross-linking by formaldehyde. Has anyone studied the use of displacing agents -- I think of hydrazines -- to break the Schiff bases; though perhaps they make for still greater problems. Formaldehyde hydrazone is itself an alkylating agent, but it would be generated in trace concentrations and could be competed for.

So I will take the occasion to copy this to Matt to get his "last word", I hope with little delay, before sending this on to the journal. I will have to include your remark about shift in authorship, so the editors are not blindsided about that issue. Feel free to restate that (email will do) if you wish.

Again, apologetically

Reply-to: lederberg@mail.rockefeller.edu

==========
Matt - please do try to get back to me on this soon.

Kaplan

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

Nobel Peace Prize 1995

Cambridge Office	*Geneva Office*	*London Office*	*Rome Office*
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136 Irving Street	1202 GENEVA, Switzerland	63A Great Russell Street	via della Lungara, 10
Cambridge, MA 02138, USA	Phone:**41-22-906 1651	LONDON WC1B 3BJ, England	I-00165 ROME, Italy
Tel. (++1-617) 576-5022	Fax:**41-22-731 0194	Phone:**44-171-4056661	Phone:**39-6-6872606
Fax: (++1-617) 576-5050	Email: pugwash@hei.unige.ch	Fax: **44-171-8315651	Fax: **39-6-6878376
Email: pugwash@amacad.org		E-mail:pugwash@qmw.ac.uk	E-mail: pugwash@iol.it

Dr. David Hamburg, President
Carnegie Corporation of New York
437 Madison Avenue
New York, New York 10022

By FAX (212) 754 4073

Geneva, 29 October 1997

Dear Dave,

Josh Lederberg and Alex Keynan were in Geneva a few days ago attending an ACHR meeting at WHO. Lenna and I had dinner with Josh and had a pleasant evening catching up on various matters. I brought to Josh's attention the following problem which troubles me greatly.

It concerns Julian Perry Robinson, a world recognized authority on chemical and biological weapons (CBW), formerly with SIPRI and now with the University of Sussex who, along with Matthew Meselson, a distinguished biochemist and geneticist at Harvard, have been our mainstays since the 1960s in Pugwash's CBW Study Group. Meselson and Robinson head a Harvard-Sussex (H-S) project on CBW which carries out joint activities with the Pugwash Study Group. Amongst other activities, the H-S project publishes a quarterly journal which occupies a unique and indispensable reference source on the implementation and monitoring of the biological and chemical weapons conventions of 1972 and 1993, respectively.

At the end of this year Robinson's association with the University of Sussex may be terminated since the University has no funds to support their part of the CBW program, and has depended on Meselson and Robinson to obtain grants for that purpose, which they have done, thanks largely to the MacArthur Foundation for several years, and which is also drawing to a close. Robinson does not have a tenured or academic staff position at the University which requires an endowed chair (some £1 million). At his present age (around 50) and without a pension he must look elsewhere for work or other support. His loss to CBW work would have serious adverse consequences to efforts in that field. I am therefore asking you, at Josh's suggestion, whether you could advise me where and to whom an application by the H-S group could be made which might remedy this situation. Would the Carnegie Corporation be a possibility? If not, are there other sources including private donors you could suggest? I would then pass on to Meselson and Robinson such information for their consideration.

With warm personal wishes,

Martin Kaplan

cc: J. Lederberg

Prof. M. Meselson - FAX 617-496-2444
Mr. J. Robinson - FAX 1273-685-865

Date: Sat, 8 Nov 1997 14:15:45 -0500 (EST)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: "Dr. Paul JACKSON" <jackson@telomere.lanl.gov>
Cc: Professor Joshua LEDERBERG <jsl@rockvax.rockefeller.edu>
Subject: Jackson ms. (fwd)

Dear Paul,
I am happy to be able to transmit this news from Josh.

If you would like to know in what issue of the PNAS your article will appear you could telephone the editorial office at (202) 625-4725. Do let me know of any further sequencing results you obtain from the Sverdlovsk samples.

Should you wish to pursue the question of possible contamination during processing or storage in Sverdlovsk, there may be two possible sources of material providing negative controls.

Lev Grinberg told me of three persons autopsied at hospital #40 during the anthrax outbreak who were judged both at autopsy and on the basis of tissue samples examined later at Galvaston not to have had anthrax. They are:

Buchelnikov #37
Chapayeva
Lyzlov #33

The numbers are those on the "official list" of those who died and correspond to the numbers in our Science paper. There may still be tissues from them at Galvaston.

In addition, there was another person, Spirina (#56 on the official list and #24 on the Abramova/Grinberg list given in the PNAS article), who was judged negative or doubtful for anthrax on the basis of examination of tissue done at Galvaston.

I seem to remember that the Galvaston examinations included immunological tests.

Finally, the anthrax victims autopsied during the epidemic at hospital #40 were only a subset of nearly 100 persons autopsied in the same series. I have a list provided by Lev of all the names and diagnoses, many of which have nothing to do with infectious disease. It may well be that Lev has retained tissues from some or all of them. If it becomes important to know for sure whether cross-contamination is a problem, I expect that Lev would be glad to help in providing samples.

With best wishes,

Matt.

---------- Forwarded message ----------
Date: Fri, 07 Nov 1997 18:25:29 EST
From: Joshua Lederberg <jsl@rockvax.rockefeller.edu>
To: Matt Meselson <msm@wjh.harvard.edu>
Subject: Jackson ms.

Dear Matt

The Jackson et al ms is in the mail to PNAS with our collective approval. If you have his email, would you copy this to him as well.

Thank you
Josh

> From: Matthew S . Meselson <msm@wjh.harvard.edu>
> To : Professor Joshua LEDERBERG <jsl@rockvax.rockefeller.edu>
> Cc : Dr. John STEINBRUNER <JSteinbruner@brook.edu>; Dr. Peter B. JAHRLING
<pbj@FTDETRCK-CCMAIL.ARMY.MIL>; Dr. Jo HUSBANDS <jhusband@nas.edu>;
calisher@usa.healthnet .org; grobertson@erols.com; Julian Perry ROBINSON <J. P.P . Robinson@sussex.ac.uk>
> Subject: PRO/AH> Biological weapons, TV program - USA (fwd)
> Date : Wednesday, February 25, 1998 8 : 58 AM

> This show is expected to feature Lev Grinberg in Ekaterinburg (honest and
> pretty reliable if not edited by a sensationalist ABC producer) and
> Kanatjan Alibekov, who has been saying two things I know for certain are
> wrong: that all the people under the Sverdlovsk anthrax " footprint" have
> now died and that the accident took place on a Friday night . So he is a
> man who asserts as fact what he in fact cannot know. The NYT has a
> somewhat toned down piece about him this morning .

> (If Alibekov were correct the approximately 40 widows, widowers, sons, and daughters, and neighbors still living in the footprint who were
> interviewed at length by Prof. Jeanne Guillemin, Dr. Olga Yampolsaya and
> two Yeaterinburg university women who worked with us must all have died
> rather suddenly -- since 1992- 1993 when they were interviewed in their
> homes. See Science for November 18, 1994. Also for the date of the
> accident: Monday April 2, 1979, probably around mid- day.)

> Matthew Meselson
> Department of Molecular and Cellular Biology
> Harvard University
> 7 Divinity Avenue
> Cambridge, MA 02138 USA
>
> email: <msm@wjh.harvard.edu>
> telephone : (617) 495- 2264
> telefax: (617) 496- 2444

>---------- Forwarded message-------- --

> BI OLOGICAL WEAPONS, TV PROGRAM - USA
> *******k**********
> A ProMED-mail post
>
> Date : Mon, 23 Feb 1998 19:38:25 -0500
> From: George A. Robertson <grobertson@erols.com>
>
>
> ABC News has announced an edition of their program "Prime Time Live", which will focus on biological weaponry. The program, scheduled for Wednesday,
> Feb. 25, is hosted by Diane Sawyer. Included is a discussion with the
> former Deputy Director of the Civilians Biological Warfare Program for the Soviet Union and an interview with Larry Wayne Harris, one of the men
> arrested by the FBI last week for alleged possession of anthrax; that
> interview had been conducted in New York prior to Harris' arrest.
>
> It is to be expected that the program will be somewhat sensationalistic but it does cover areas of interest to ProMED-mail subscribers .
>
>
> Charles H. Calisher, Ph.D.
> Moderator, ProMED-mail
> e - mail: calisher@usa.healthnet.org

nal memo from
UA LEDERBERG

Matt

12/23/99

I thought you might not mind seeing this.

Happy '00.

Josh.

JAN 7 2000

I of Meselson

THE ROCKEFELLER UNIVERSITY NEW YORK 10021-6399

Lederberg, J.

STANFORD UNIVERSITY MEDICAL CENTER
STANFORD, CALIFORNIA 94305 • (415) 321-1200

STANFORD UNIVERSITY SCHOOL OF MEDICINE
Department of Genetics

December 11, 1969

Mr. A. Schou
Nobel Peace Institute
Oslo, Norway

Dear Mr. Schou,

This is to follow up our conversation in Stockholm in which I advocated a broadening of the mandate of the Nobel Peace Prize to include those scientific studies which might have a direct bearing on the rational ordering of human affairs towards the peaceful solution of group conflict.

May I take the occasion now to recommend a concrete step that would at least facilitate the consideration of such proposals without imposing a binding commitment on you. I suggest merely that you enlarge the field of qualified nominators for the peace prize to include previous recipients of the other Nobel Prizes in the sciences, medicine, and literature.

I would be happy to think that you would be burdened by a large number of additional nominations for if this were the case it would bespeak further intellectual attention to the problems of peace. My expectations are more modest but in that same direction.

Having requested such a self-serving gratuity it probably is already incumbent upon me to illustrate the point with some of my own suggestions.

For their important clarification of the role of instinct in aggressive behavior of animals and of men I would suggest Nikolas Tinbergen and Konrad Lorenz. Their findings should be no means be confused with the pernicious simplification that has been advertised, for example, by Ardrey.

In another sphere, namely the specific contributions of scientists towards initiatives for peace I would happily nominate Jerry Wiesner for his remarkable role in the development of the non-proliferation treaty. I am sure that you are already far more acquainted with this than I could be.

Finally, I would call to your attention a very able scientist whose dedication to an extremely important peace-related activity has just been crowned with a very significant concrete action, namely President Nixon's recent statement concerning U.S. policy on chemical and biological warfare. In my opinion this step has an importance that transcends even the crucial issues of biological weaponry as it may be a major avenue for international consensus on problems on global concern. My candidate is Professor Matthew Meselson about whom you may not have heard so much but whose contribution to this field has been acurately summarized in a recent profile in The New York Times, a copy of which is enclosed.

I have already intruded myself too far but in the event that there is indeed an avenue where people like myself can make explicit nominations for the Peace Prize I would be happy to undertake the effort of a more complete statement of justification.

Please accept my very best personal regards for Madame Schou as well as yourself.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

1 Enclosure
JL/rr

Date: Wed, 13 Jun 2001 22:58:06 -0400 (EDT)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Joshua Lederberg <jsl@jl10.rockefeller.edu>
Cc: Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: Bill Broad

DearJosh,

Yes, Bill seems to want to write some kind of thriller, hardly what I thought he as a science journalist would be inclined to do. I too was irked at his setting us up that way and at much else besides. Attached in Word Perfect 8 format, here is what I faxed to him today. I'd value your comments on it. Where I thought he was going beyond reasonable limits, I minced no words.

Josh, if you know of any talented young person who would be interested in what we are doing with bdelloid rotifers, I am looking for an outstanding postdoctoral student/colleague.

Best regards,

Matt.

Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
7 Divinity Avenue
Cambridge, MA 02138 USA
email: <msm@wjh.harvard.edu>
telephone: (617) 495-2264
telefax: (617) 496-2444

On Wed, 13 Jun 2001, Joshua Lederberg wrote:

> Date: Wed, 13 Jun 2001 20:59:29 -0400
> From: Joshua Lederberg <jsl@jl10.rockefeller.edu>
> To: Matthew S. Meselson <msm@wjh.harvard.edu>
> Cc: jsl@mail.rockefeller.edu
> Subject: Bill Broad
>
>
> <<<
> You are probably reading some of the same pages of Broad's draft as I am
> and finding quite a few errors and exaggerations that one can help him
> to avoid.
> >>>
>
> Agreed. I am particularly irked that he seemed to be setting us up as
> competitors. I told him "brothers in arms" was closer to the mark.
>
> Josh
>

Date: Wed, 2 Jan 2002 00:46:46 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Dr. Richard GARWIN <RLG2@watson.ibm.com>
Cc: Joshua LEDERBERG <jsl@mail.rockefeller.edu>,
Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: dose response

Dear Dick and Josh,

I know of no experimental or theoretical reason to believe that there is a threshold number of spores (other than one) below which inhalation anthrax cannot be initiated. For a variety of microbial infections the evidence and arguments for no threshold are substantial.

1) For some pathogens the measured inhalation ID50 in human volunteers is very low, in the range of 10 organisms. And even fewer organisms still give rise to infection, at lower frequency than 50%. Examples are tularemia, Q-fever and some viruses.

2) Experimental infection with aerosols or innocula containing two distinguishable strains of the same pathogen, when done at low response rates, give animals in whose blood one usually finds only one or the other infecting strain, with equal frequency. Although one could argue that more than one microorganism is needed to initiate infection, it would then be necessary to belive that one or the other almost invariably wins out during subsequent proliferation in the host. At higher dose one does find mixed infection but with a frequency that agrees with independent action. Specifically, mixed aerosol experiments done with B. anthracis in mice, if I recall correctly, are consistent with no threshold.

3) The same total number of administered organisms delivered all at once or over a period of hours or even many days gave the same percentage of infected animals. If there is a threshold, the "counting mechanism" would need to have essentially perfect memory in such experiments.

4) Single-spore aerosols of B. anthracis are about as infective in laboratory animals on a per spore basis as aerosols the particles of which include a few spores. Thus there is no evidence for a non-linear effect operating at the site of deposition, the individual alveolar sac.

5) Independent action models fit dose-response data for a variety of infective organisms better than do log normal models (which in effect assume a log-normal distribution of individual thresholds within the exposed population). The best fits are said to be obtained under the assumption of independent action but with heterogeniety in the exposed population with respect to the probability of infection by a single organism.

Some references: Druett, Nature 170: 288-, 1952; Druett et al. J. Hyg. 51: 359-371, 1953; Meynell, J. Gen Microbiol 16: 396-404, 1957; Moxon and MUrphy PNAS 75: 1534-1536, 1978; Rubin and Moxon J. Infect Dis. 149: 278, 1984; Rubin Revs. Infect. Dis. 9: 488-493, 1987; Goldberg et al. J. Infect. Dis. 94: 9-21, 1954; Haas, Rose and Gerba, "Quantitative Microbial Risk Assessment", John Wiley and Sons, 1999.

Below I have pasted:

(1) Excerpts from an email I sent to Martin Furmanski in November.

(2) A note from the current issue of The ASA Newsletter.

Best regards,

Matt.

****

1) I know of no sound theoretical or experimental reason to exclude the possibility of independent spore action in the initiation of inhalation anthrax.

2) The most simple independent action model is that in which no allowance is made for heterogeneity in individual susceptibility. (Druett. H.A. Nature 170: 288, 1952). In that oversimplified case the attack rate is related to the ID50 by:

1-exp[-0.69(dose/ID50)].

For a mean dose of one spore and an ID50, for example, of 8,000 spores, the attack rate would be 0.000086 or roughly 0.01 percent.

3) So far as I know the basis of the old US Army estimate of the human ID50 as 8,000-10,000 inhaled spores in particles of respirable size has not been published in the open literature. It may have no meaning for any real human population.

4) The largest experiment (with 1,236 cynomolgus monkeys) was done in the "8-ball) at Fort Detrick in the 1950's under contract with the Ralph Parsons Company. So far as I know, the detailed data are not available and may have been lost. A brief summary (H.N. Glassman, Bact. Rev. 30: 657, 1966) reported fitting the data to a log-normal function and obtained an ID50 of 4100 spores and a probit slope of 0.7. No actual data was given but very low attack rates were not studied.

5) Other experiments, with fewer monkeys but carefully done with well characterized aerosols have given monkey ID50 values ranghing from 2,500 to 45,000 spores. In view of the great number of variables involved (strain, conditions of growth, sporulation, storage, aerosolation, mouth versus nose breathing, etc.) this is not unexpected. Also, it is known that various additives can greatly lower the ID50 for inhalation anthrax.

6) Almost certainly there is at least one factor causing substantial heterogeneity in human populations. That is age. Among the 66 well-documented fatal cases and the 11 non-fatal cases of inhalation anthrax cited in our article in Science (Meselson et al. volume 266: 1202-1208, 1994), none were younger than 24 of age. During our second (1993) field trip to Sverdlovsk Guillemin and I made a special effort to determine whether there might have been some unusual deficiency of children and young adults along the zone of exposure. We found no evidence for any gross deficiency. In addition to several factories, there were schools and many private residences in the exposed zone.

The much lower susceptibility of young persons to inhalation (but not cutaneous) anthrax is supported by a search of the older literature and, most recently, by the age distribution of the recent US cases of deliberate infection. As an aside, Legionellosis seems to show a similar trend.

7) In view of the above, there is unlikely to be any one meaningful ID50 value for human populations. As the independent spore action model is a reasonable one and also as there is certainly heterogeneity in human susceptibility, we cannot reject the possibility that even a single spore has a significant albeit very low probability of initiating infection, higher in some people than in others.

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The "Note Regarding Source Strength" reproduced below is the same as that published in the ASA Newsletter of June 8, 1995, except for the correction of a typographical error (the omission of "pi") in the equation for dose in Table 1.

Note that source strength is defined as "the number of viable spores released at the source that travel in the atmosphere as particles small enough to initiate inhalation anthrax". Using this definition, the source strength estimates in Table IV are given in milligrams, taking the number of spores per milligram, as stated, as 109. The question of whether the aerosol released at Sverdlovsk consisted only of viable spores or also contained inviable spores and other material is obviously not addressed in the present estimates. These estimates should be regarded only as what they are: estimates of source strength, as defined in the note, that follow from the stated assumptions regarding atmospheric dispersion and regarding dose-response relations for the infectious aerosol and the human population exposed to it.

Although the present estimates follow from the assumptions made, the most relevant dose-response data available are for non-human primates, not for any human population, and none of it is for the low attack rates observed in the Sverdlovsk outbreak. Neither do we know if the virulence of anthrax spores in the aerosol released at Sverdlovsk was like that in aerosols employed in published experiments with monkeys. And even the well done experiments at Fort Detrick and Porton with monkeys gave ID50 values covering a more than twenty-fold range -- from 2,000 to 45,000 respirable spores. These uncertainties are only imperfectly addressed by considering a number of different dose-response relations, as is done in Table IV.

It may be of interest that the estimates presented here have an antecedent in an April 1980 memorandum I wrote for the US Interagency Sverdlovsk Working Group. We had no reliable information about the geographic distribution of attack rates. So we could only estimate what dose would be inhaled at a given downwind and crosswind distance from an aerosol release of a given number of spores under given atmospheric conditions. My estimates of dose as a function of source strength, based on a Gaussian plume model for a moderately stable atmosphere, were, as to be expected, in good agreement with those in the present Table II. Some members of the Working Group, apparently unfamiliar with atmospheric dispersion models, thought my estimates of source strength were far too low. The following month we received a memorandum from Dugway Proving Ground with estimates in essential agreement with those in my memorandum and in the present Table II. By then, however, a very much higher estimate had already been briefed to President Carter by Admiral Stansfield Turner, then Director of Central Intelligence.

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Date: Sat, 13 Oct 2001 12:53:28 -0400
From: "Leduc, James W." <jwl3@cdc.gov>
To: 'Matthew S. Meselson' <msm@wjh.harvard.edu>
Cc: "Ashford, David" <dba4@cdc.gov>, "Leduc, James W." <jwl3@cdc.gov>
Subject: RE: Stevens' anthrax

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Matt, thanks for your valuable thoughts. I spoke very briefly with Dave Ashford about this-literally a short hallway chat as we each were running t o
different meetings-and he thought that this was unlikely. In addition, emerging results appear to more clearly support the alternative hypothesis.

best regards, Jim

-----Original Message-----
From: Matthew S. Meselson [mailto:msm@wjh.harvard.edu]
Sent: Thursday, October 11, 2001 8:25 PM
To: James LEDUC
Cc: Joshua LEDERBERG; Dr. Richard GARWIN; Richard FALKENRATH; Don MAHLEY; Dr. Robert MIKULAK; Matthew S. Meselson
Subject: Stevens' anthrax

Dear Jim,

Further to my earlier emails, here are some more specific thoughts:

Could all of the the nasal and environmental isolates have come from anthrax cells shed by Stevens? If nasal or tracheal exudates contain B. anthracis, a few nasal positives among fellow employees could arise from inhalation of droplets or aerosol from sneezes or coughs or from finger contact with contaminated surfaces and subsequent nose-finger contact. Environmental positives could arise by direct deposition from Stevens' sneezes, coughs or his exudate-contaminated fingers.

So far as I know, nasal and tracheal exudates of inhalation anthrax patients have never been examined for the presence of B. anthracis. Do we know if there were pneumonia-like lesions anywhere in the air spaces of Steven's respiratory system? A good many from Sverdlovsk did. Not finding lesions at autopsy would not exclude their presence. If there were such lesions, one would expect vegetative cells to be present in nasal and tracheal exudates.

The fact that anthrax is not contagious person-to-person is not inconsistent with the possibility of nasal and tracheal sheding of vegetative cells or spores:

First, vegetative cells may be quite susceptible to killing by macrophages and other bodily defenses and may therefore be essentially non-infectious. I may have a reference bearing on this.

Second, vegetative cells deposited with exudate material would be impossible to aerosolize and therefore not likely to cause infection. The same would apply to spores that may form on exposure to oxygen. Does anyone know if vegetative cells can sporulate when in exudate exposed to air?).

A finding of anthrax in Stevens' home would strongly suggest that Stevens was indeed shedding B. anthracis. Otherwise we would have to believe that the workplace and the Stevens residence were independently exposed. That that would seem extremely unlikely. It is therefore important to search for B. anthracis in the Stevens home and any other place he may have frequented while ill. Air filters, fan blade edges, bedding, unwashed handkerchiefs, etc, should be thoroughly examined.

If Stevens was sheding B. anthracis, his exposure was not necessarily at his workplace but could have been anywhere else he had been during the days or perhaps few weeks before he fell ill.

This would not rule out either foul play or accidental exposure but it would broaden the possibilities for where Stevens' exposure took place. If accidental, it could have been from a natural source or from a facility working with virulent anthrax somewhere near where Stevens had been in Florida or on his trip. If there is such a facility, DNA sequence information could be decisive.

All good wishes,

Matt.

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