

SIPRI
INTERNATIONAL INSTITUTE FOR PEACE
AND CONFLICT RESEARCH

Sveavägen 166 Stockholm Va Telephone: 34 96 00 Cable: peacere search, Stockholm

JUN 17 1968

JPPR

STOCKHOLM 13th June, 1968

Professor Matthew Meselson
The Biological Laboratories, Harvard University
16 Divinity Avenue
CAMBRIDGE
Massachusetts 02138
U. S. A.

Dear Matt,

It was very good news hearing that you will be free during the first week in August to attend our CW symposium. Here are some details.

Enclosed is an outline which sets out those aspects of CW on which we aim to concentrate at the meeting. As you will see, we are looking for fairly specific information, rather than a general survey of the whole subject. Nonetheless, the specific topics mentioned are intended more as guides than an actual agenda, and we are not necessarily planning to restrict ourselves to them. We have the following participation:

Subsection 11: I am at present preparing a background paper on this topic from my own sources and from material supplied by Dr Tammelin of the chemical section of the Swedish Research Institute for National Defence (FOA 1). This will be checked through and revised as necessary by FOA 1

Mr Alan Pittaway of Booz-Allen Applied Research Inc, who, as you know, has done a lot of CW-related work under contract both with ACDA and Edgewood Arsenal, is preparing a paper on topics 111 and 112. Although much of the work done by Booz-Allen is closely related to topic 113, it is doubtful whether Pittaway would be able to obtain clearance for a paper covering it too. The firm has a number of contracts for work on mathematical techniques for predicting weapons-system effectiveness and for comparing different classes of weapons-systems - biological, chemical, fragmentation, etc., and most of their reports still have a security classification.

Besides Mr Pittaway, we are also hoping for papers within this subsection from Dr Moravec, a Czechoslovak chemical defence worker, and from a French chemical defence worker to be found for us by

SIPRI

Professor Matthew Meselson

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M. Gandilhon. The position over the latter is rather uncertain at the moment: Gandilhon, who is now President of the Compagnie Générale de Systèmes et de Projets Avancés, has been locked out of his office by strikers. It is likely also that we shall get the participation of at least one FOA 1 representative.

Subsection 12: We are searching for a German military historian to cover topic 121. John Cohen, the Professor of Psychology at Manchester University (UK) is doing topic 122, and will be covering some of the broader psycho-social issues of CW. Colonel David Smiley, a retired British military man with many years experience in the Middle East, is to speak on Yemen CW.

Section 2: I enclose a background paper written for this section. Mr Pittaway is preparing a paper covering topics 213, 214, and 215. We have approached the WEU Armaments Control Agency about topic 22.

Section 3: The International Committee of the Red Cross is sending a representative to speak on topics 31 and 32. Martin Kaplan, as I mentioned, has prepared a brief paper on topic 32, and I enclose a copy of this herewith.

Section 4: This aspect of the subject is being dealt with by consultants, and we have not yet made any arrangements to cover it at the meeting itself.

In addition to these participants, we have a number of others lined up. Next week, Academician Dubinin and Dr Chapurnov from the Soviet Union will be visiting us for a few days discussions. We are forced into this compromise by their inability to come at any other time. A report of our talks will be agreed upon before they leave, and will be circulated before the main meeting.

Professor Koelle from the University of Pennsylvania has been invited to speak on possible future CW agents. As yet we have had no reply from him.

The nascent Finnish Pugwash group is supposed to be finding us a suitable chemist.

We hope that a representative of the British Foreign Office's ACDRU will be able to participate; Deryck Viney will probably be coming.

Professor R. B. Fisher of the University of Edinburgh Medical School has been recommended to us by the British Ministry of Defence. As he holds at least one prominent advisory post in the British military set-up (he is a scientific adviser to the CDEE at Porton), a useful weight can be attached to whatever he may say.

We have not yet given up hope of getting a US DoD man: it would be

Professor Meselson

13th June, 1968

splendid if you could indeed do something for us in this connection. Ideally we would like him to cover topic 125 (for which our alternative would be a French military attaché from Viet Nam).

The meeting will be a cross between a symposium and a study group. On the one hand, we want to do something more than merely give people a chance to air their views or to listen to those of others, yet, on the other hand, we don't want to constrain the discussions by demanding some sort of agreed text: we are after as much information as we can get. For this reason, we hope that everyone who does participate will prepare a paper on an agreed topic, and we are offering a small commission to those who can: \$250 for 2,500 words or so.

If you can find the time to prepare us a paper, I would think that the most valuable one from our point of view would come within section 3: how best to deal with allegations of use of C-weapons, complementing Kaplan's paper (which he very obligingly prepared at short notice in case the meeting were indeed held in June).

Provisionally, the meeting is planned for the week beginning 5th August, lasting 3 - 4 days. SIPRI will pay travelling expenses, hotel bills and a small per diem allowance.

Please let me know if there is anything more you need to know. In the meantime, we look forward to having your views on the meeting.

Yours,



Julian Perry Robinson.

Encls: Outline of 1st April
Section 2 background paper
Dr Kaplan's paper

November 20, 1968

Mr. Julian Perry Robinson
SIPRI
International Institute for Peace
and Conflict Research
Svaevagen 166
Stockholm, Sweden

Dear Julian:

It seems that events at the General Assembly and at the ENDC are going to force a clarification of U.S. policies for CBW. This could take the form either of forward progress or a step backward with respect to arms control. The outcome will depend considerably on the ability of arms control advocates to propose precise policy alternatives that both serve the interests of arms control and can gain acceptance or at least not arouse opposition within the U.S. government. I am writing to ask if you have written anything, however, informal, that I might have setting out your view of what might be the alternative agreements or resolutions to come out of Geneva and New York. If you have anything on this subject or are prepared to produce something, I would greatly value it and would find it most helpful in the coming weeks.

Julian, it may be that my filing system has gone awry, but I cannot find the report on the Idaho sheep deaths that you were going to send to me. If you still have it, would you please send it along.

With best wishes,

Sincerely yours,

Matthew Meselson

Give my best regards to Mary.

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Director: Mr. Robert Neild
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Professor Matthew Meselson
The Biological Laboratories,
Harvard University,
16 Divinity Avenue
Cambridge
Mass. 02138, USA

7th December, 1968

DEC 10 1968

Dear Matt,

I'm afraid I haven't anything at all considered or thought out to send you in reply to your letter of 20th November; and it is unlikely that the CBW project here will produce anything until the new year. Still, here are some thoughts:

Nabrit's speech in the General Assembly on 5th December 1966, just before the US voted in favour of the Hungarian resolution, contained the following: "What is essential today is to obtain from States a formal public expression of intent to observe strictly the objectives and principles of the Geneva Protocol. And this draft resolution before us does" (A/PV.1484).

Bunn drew attention to this "formal expression of intent" in the ENDC on 13th August 1968. As far as the UN goes, these seem to be the existing pressures for a clarification of US policy on CBW; if/as further discussion of CBW in the ENDC gets under way, more pressure will build up (hence, possibly, the British Ministry of Defence's strong opposition to Mulley's initiative).

It is presumably impossible now for any declared US CBW policy to be anything worse (from an arms control point of view) than no first use. But no first use of what? Are there any chances for a declared interpretation of "the objectives and principles" of the Protocol being an extensive interpretation (ie, one which proscribes nonlethal toxics)? The only ray of

hope here seems to be from the State Department's evasiveness on the Yemen issue, and the possibility that a contributing factor was a despair of convincing the world of a difference between upper respiratory irritants and lung irritants or vesicants.

Once the USA is committed to an extensive interpretation, the importance of modernizing the Protocol is presumably reduced. Continuing demands by the Russians for actual accession to the Protocol will then seem less reasonable, and will no longer serve as their contribution to Geneva or New York CBW discussions. Their own attitudes to CBW may be forced nearer the surface.

The Swedes made an interesting proposal in the ENDC on 25th August:

"some joint collective statement in the General Assembly or elsewhere might be useful which, without regard to the various positions and practices of the past as to the extent of the existing ban, would enable States to register adherence to a ban on all B and C means of warfare, comprehensively interpreted" (ENDC/PV.391).

If someone were to put forward a UNGA resolution along these lines, perhaps one which declared that the principles and objectives of the Protocol necessitated an extensive interpretation, the USA would be given an opportunity to alter and declare its policy in a relatively unexposed manner.

The US attitude at Geneva towards Mulley's working paper was that the drafting of a convention banning possession of B-weapons should be delayed until an ENDC working group or subcommittee had reported on the possibilities of verification (ENDC/PV.389). This could be interpreted in several ways, including (1) procrastination, to give everyone a chance to forget the whole CBW question; or

(2) a way of obtaining implicit acceptance of the idea of treating BW and CW separately from one another.

The proposed study by the Secretary-General of CB weapons effects, if it comes off, should defeat (1). As to (2), my own feeling is that so long as there is the remotest possibility of the USA accepting an extensive ban on first use, BW and CW should be treated together, so far as the drafting of an international convention is concerned. The argument that they should be treated separately because offensive BW hardware has not yet been developed is wrong and in any case invalid, as you presumably know better

than we.

My own hope is that the ENDC, if it takes up the CBW question again, will consider the possibilities of bans on possession of CB weapons for which the parent agreement does not include a verification mechanism: safeguards which are a lot less than perfect may nevertheless be adequate. One of my background papers for the SIPRI CW symposium discussed this in a cursory sort of way; and I see that the Swedes have already brought it up at Geneva (ENDC/PV.391): they suggested that the safeguards would emerge firstly by "control through public shame", then by a scheme for "verification by challenge".

I am sorry to have delayed replying for so long, and to have done so in such a disordered way: things are rather hectic here, with our present plan of getting something out on CBW before U Thant's study gets under way. I am just off to London for Christmas, so that added to the rush.

With best wishes,

Yours ever,



Julian Perry Robinson

PS. The sleep paper is enclosed: sorry not to have sent it on before.
T.

HARVARD UNIVERSITY
THE BIOLOGICAL LABORATORIES
16 DIVINITY AVENUE
CAMBRIDGE, MASSACHUSETTS 02138

December 17, 1968

Mr. Julian Perry Robinson
SIPRI
Sveavagen 166
113 46 Stockholm
Sweden

Dear Julian:

Thank you for your letter of 7 December. I am in complete agreement with you regarding the desirability of an extensive interpretation of the Geneva Protocol. No nation has taken up the 25 August proposal made by Sweden made at the ENDC. Neither has Sweden pressed the issue. However, this would seem appropriate at this time in view of the fact that the Secretary General's study is about to begin. Once the study has been completed the time might be ripe for pressing the Swedish proposal or for a UNGA resolution such as you suggest. The study might facilitate matters by discussing the manner in which non-lethal gases may be used in conjunction with lethal high explosives and other lethal weapons to increase the effectiveness of the latter.

One of the problems facing us now is to find an acceptable formula under which the United States might subscribe to an extensive interpretation of the Protocol, assuming that the United States wishes to move in that direction. I have written a memorandum, a copy of which is enclosed, to be used as a "talking paper" in discussing this matter with persons here. My intention is to prevent the discussion of tear gas and similar gases from placing primary emphasis on moral or even legal considerations at this time. Clearly, if the matter is viewed from a moral or legal point of view, the United States would have no choice but to defend its present practices.

However, if the question can be seen instead as a technical one it might be possible to broaden the discussion enough to include an extensive interpretation of the Geneva Protocol. The conclusion might then be reached that although the legal status of tear gas and similar gases has been ambiguous since the inception of the Protocol itself, it would seem that the most workable and generally acceptable standard is "no gas of any kind". In other words, the United States might allow itself to be convinced of the desirability of an extensive interpretation, not because of legal considerations and certainly not because of moral considerations, but rather because of technical ones. I should greatly appreciate your reaction to my memorandum and your further thoughts on this subject in general.

I enclose a copy of a British command paper mentioned by Brownlie at the London Conference. I have not yet attempted to learn the outcome of the proposal contained in point no. 5. If you have any information about this, I would appreciate receiving it.

I trust you have noticed the publication of "Chemical Warfare, a Study in Restraints" by Frederick J. Brown, published by Princeton University Press this year. Although Brown's underlying philosophy seems to me rather immature, he has uncovered a mountain of new material.

With best regards as ever,

Matthew Meselson

MSM/psl

August 14, 1969

Mr. Julian Perry-Robinson
c/o Kaldor
320 Chelsea Cloisters
Sloane Avenue
London, S.W.3
Great Britain

Dear Julian,

Can you send, rather quickly, the following items?

- 1) A brief academic and professional biography also including particulars regarding birth and citizenship.
- 2) A "project statement" describing in one or two pages what you plan to do and what your approach will be. Basically this would be research and preparation of a book. Give some idea of the nature of the projected book. Also include an estimate of the time required. One or two years would be a reasonable estimate.
- 3) A letter of general recommendation from Robert Neild.

The above items should be sent to Professor Thomas Schelling, Harvard Center for International Affairs, 6 Divinity Avenue, Cambridge, Massachusetts 02138. I have spoken with him and he expects to receive the above items from you. After the wheels have turned awhile, a definite offer will be sent to you for your consideration.

I cabled Stubbs my permission to publish our paper in New Scientist. If they had not already gone ahead, I would have suggested some changes. For example, it is probably premature to raise the issue of carcinogenesis by CS, except in a purely technical article.

August 14, 1969

2

Could you send copies of anything from the British press related to our discussions in London? Also, could I have a copy of the Parliamentary discussion regarding the locations in which CS has been used by British colonial police? Has it been used only by police or also by soldiers?

With best regards,

As ever,

Matthew Meselson

MM:las

CB

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Professor M. Meselson
Biological Laboratories
Harvard University
16, Divinity Avenue
CAMBRIDGE, Massachusetts 02138,
United States of America.

27th August 1969.

Dear Matt,

Thank you for your letter of 14th August. I left England on the Thursday after your departure, and since then have been immersed in a series of conferences here - first Russians, then Czechs, then Schutzenberger and Fisher taking the WHO model a stage further. I am now beginning to surface. Reading the New Scientist article again, I see a number of passages that need improving: it is a pity it had to be so rushed. Let us exchange annotated copies.

I see that some doctors in Londonderry (who have been treating Irish CS casualties) are calling attention to the absence of data on the long-term effects of CS.

I enclose some bits and pieces of interest. British colonial police forces may either be natives or imported UK police: I do not know what they were in British Guiana.

As for the results of our agitating in London, I left before anything appeared in the press, and I did not watch television. Have you heard anything from Nick? (Incidentally, do you have a spare copy of his translation of the Der Spiegel article that I could have?).

Thank you for liaising with Professor Schelling: I have now written to him along the lines you suggested (see the enclosed copy of my letter to him). Robert Neild is off to a conference, so his letter of recommendation may be a bit delayed (when I asked him for it, he was not exactly enthusiastic, and proceeded to make a gratifying series of alluring counter offers: I hope he will not write Schelling too off-putting a letter).

I will write again at more leisure in the next few days. I have some additions for your tear gas bibliography.

Yours ever,

Julian

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January 21, 1970

JPPR/kb

Professor Matthew Meselson
Harvard University
The Biological Laboratories
16, Divinity Avenue
CAMBRIDGE, Mass. 02138
USA

Dear Matt,

I am afraid I have been a very bad correspondent over the past month, particularly on the matter of the U.S. Administration's new attitude towards CBW. This is all immensely encouraging, and let me add my own words of praise to your part in bringing it about. Two years ago one would never have dreamt that an effective counter-lobby could ever have been deployed against the Chemical Corps.

You have presumably heard the nasty rumours about the British Government's vacillations on the tear gas issue. It is those damned Irish again. We are doing what we can to stop the imminent slide, but the danger is considerable.

Did you know that the Swedish police used tear gas in 1968?

Portugese methods of warfare in Africa are obscure, as Robert Neild remarks in a letter on the Geneva Protocol he has just written to The Times. I enclose a photocopy of a page from our draft CBW study which summarizes all we know about recent Portugese chemical warfare. The literature reference is to the 21st August 1968 issue of Dagens Nyheter, one of the two principal Swedish newspapers.

The first two volumes of our CBW study will be circulating (in a mimeographed provisional edition, alas) by the end of February. Each will be about 100,000 words long. One part will be my history of CBW usage, CBW technology and CBW constraints; the other part will be Goldblat's history of League of Nations and United Nation's CBW activities. An abbreviated version of our verification study may also be ready in time, and possibly also our review of international law relating to CBW. I will send you copies as soon as they are ready. We shall use the interval between the provisional and the printed editions for expert consultations (this will mean you, Fisher, etc.)

I have some queries and requests to put to you.

1) Robert Smith's article in the New York Times of 29th September, 1969, cites a private U.S. Army assessment of the manner in which CS is being used in Vietnam. I take it that the article is an accurate reflection of the "private report", but can you confirm this? Is it so that the report has been presented as evidence to one of the Congressional Committees that have been holding CBW hearings? (Incidentally, are any of these hearings available in printed record yet?)

2) Referring to the latest edition of your CS bibliography - the one you just sent Milton - have you copies of CWLR 2365 and CWLTM 24-50, listed in the addendum, that I could have? And also items nos. 12 and 13?

You might care to add these AD numbers to the bibliography:

- (15) EATR 4072 = AD 808731
- (16) EATR 4071 = AD 808732
- (19) EASP 600-1 = AD 661319
- (21) The IDA study = AD 661041

I enclose a listing of a few more literature references: it was intended to supplement one of your earlier lists, but I see you have now incorporated several of its entries.

With best wishes,

Yours ever,



Julian Perry Robinson

Encl.

January 22, 1970

Mr. Julian Perry-Robinson
SIPRI
Sveavagen 166
113 46 Stockholm
Sweden

Dear Julian,

This is to introduce you to Mr. Richard Brooks, a friend of mine and a distinguished film director and producer. Brooks is making a film in which toxic warfare plays a major role. He needs advice and since he will be in Stockholm soon, I have taken the liberty of giving him your name. If he should call on you, I hope you will be able to find a little time to help him.

In order to dispel undoubtedly false rumor that you have defected to direct United Arab Republic chemical warfare effort, please advise me regarding your estimated time of arrival here.

As ever,

Matthew Meselson

MM:ls
Encl.

P.S. If Robert Neild is not too terribly pressed when Brooks comes through, would you please introduce him?

January 29, 1970

Mr. Julian Perry-Robinson
SIPRI
Veavagen 166
113 46 Stockholm
Sweden

Dear Julian,

My information is that the Protocol will not go to the Senate until mid February or later. The Foreign Relations Committee will probably conduct its hearings a few weeks after the treaty is submitted to the Senate. It would be a great pity if the SIPRI CBW Study became available only after the drama is all played out. When you finally send my copy of the provisional edition, be sure to let me know whether or not I am permitted to circulate it among those most concerned with the ratification debate.

The Zablocki hearings have not yet been printed. Some of the more interesting testimony is enclosed. So far as I know, the report cited by Robert Smith in the New York Times of September 29 was not presented to any Congressional committee. The report is still classified secret even though it appears that Smith somehow obtained a copy.

I do not have copies of items 2 and 4 in my "additional" reference list. Can you supply me with a copy of additional reference 3, Porton Technical Paper 757, or, indeed, any material on the skin sensitizing effects of CS. I enclose a copy of one of the items you requested. I do not have the other.

There is an almost typographical error in the WHO report that you should be aware of. Somehow we let stand a statement that 0.5-5 mgm per square meter of VX on the ground would constitute a serious hazard to unprotected personnel. The SIPRI report says 0.5-5 gm. I think that 0.1gm per square meter might be enough but the WHO value is clearly a mistake.

January 29, 1970

2

United States policy for irritant gas and anti-plant chemicals is still not irrevocably set. The President himself had said nothing about the subject. Even the background briefing provided by Henry Kissinger on November 25 left room for the evolution of policy, although this was not well communicated by the press. In this situation, when our policy can still evolve in a favorable direction, it would be tragic if the process were cut short by a British decision to reverse their traditional policy on tear gas.

With warm regards,

As ever,

Matthew Meselson

MM:ls
Encl.

P.S. The House Committee hearings on the Protocol will be available within a week. I shall send you a copy.

CBW

THE FORD FOUNDATION

320 EAST 43RD STREET
NEW YORK, NEW YORK 10017

INTERNATIONAL DIVISION
EUROPEAN AND INTERNATIONAL AFFAIRS

March 19, 1970

Professor Matthew S. Meselson
Biological Laboratories
Harvard University
Cambridge, Massachusetts 02138

Dear Matt:

I attach for your background use a copy of the most recent letter from Perry Robinson. What do you think about the Sussex connection? I'm not sure we can handle this but I would like to have your advice.

Hope to see you soon.

Sincerely,



William B. Bader

Attachment

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William B. Bader, Esq
The Ford Foundation
New York

6th March 1970

Dear Mr Bader,

Thank you for your letter of 10th February. I have at last finished a major chunk of work for the SIPRI CBW study, so I hope you will find me a more efficient correspondent.

Parts I, III, and IV of the SIPRI study have just been published in a small provisional edition, and I am sending you a set under separate cover. Parts II, V and VI have yet to be finished.

The reason why I am havoring about going to Harvard is that I understand that only brief periods of absence from the Center would be tolerated during the academic year. Part of my project requires at least three months of research work in the UK; I had hoped that I could have fitted this in during the Summer of 1970, but as you will see from the timetable I outline below, this is no longer feasible. In addition, a variety of family complications have recently arisen for me which, after my two-year absence from home, make me rather reluctant to commit myself to spending more than three or four months at a stretch in the States.

If I go to Sussex, I intend to pursue the same project as that which I outlined to you in my letter of 21st October 1969. The Director of the Science Policy Research Unit there, after talking with the Director of this Institute, wrote to say that his unit would be pleased to have me, provided I had means of support, and that Mr A. D. McKnight (who used to be Inspector-General at the Vienna IAEA) would be glad to work with me.

The attraction of the Sussex unit is that, unlike the Harvard Center, I do not anticipate any difficulty over obtaining leaves of absence. This means that I could spend as long as I liked in the USA, family permitting, without being under any actual commitment to do so.

If you feel that the Sussex unit is unsuitable, I am sure that I could find other UK universities at which to base myself. I understand that the Ford

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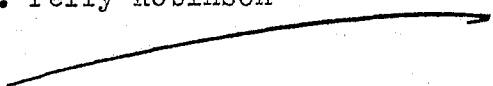
Foundation is already sponsoring one of the Unit's projects.

The timetable for my project has been confused by the way the SIPRI CBW study is dragging, so that my original plan has slipped by a good three months. At the moment it does not look as though I shall be clear of it until the end of June or so, unless I abandon it in midstream, which I don't in the least want to do. Thereafter my chief commitment for remaining in Europe is a consultancy for a CW disarmament verification exercise that SIPRI is organising: this will involve a conference in Helsinki and probably also visits to the USSR and the FRG in July. I can therefore probably not begin work on my project until the end of July. Ideally I would hope to spend a month or so in the States in the early Autumn to discuss the project and get some of my ideas straight. (I shall be attending the Microbiology Congress in Mexico City in August, and the Pugwash Conference at Lake Geneva in September). I should then like to return to the UK in the winter to do my UK case-study, returning to the States in the Spring to do my US case-study. This will mean spending a minimum of three months in the USA; I would hope to be able to spend much longer.

Yours sincerely,

Julian Perry Robinson

J. P. Perry Robinson



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19th May, 1970

JPPR/kb

Dr. Matthew Meselson
Harvard University
The Biological Laboratories
16, Divinity Avenue
CAMBRIDGE, Massachusetts 02138
USA

Dear Matt,

I have just got back from a 3-week trip, so I can set about writing you a letter instead of ~~adju~~ agitated cables (I am sorry that I should continually have to begin my letters with apologies for general dilatoriness).

Let us make it a date, then, that you should come here for July 10th to 14th, subject to possible slightpostponement. Would you like us to send you an air ticket, or would you rather be reimbursed at our end? I will be sending you a new chunk of the SIPRI CBW-study in the next couple of weeks or so.

Copies of parts I, II and IV duly went off to the people you suggested. I had in fact sent a set to William Bader at the beginning of March, together with a fairly lengthy letter; am I to infer from your letter of 13th March that my letter never reached him? If not, then he is almost as bad a correspondent as I; I have been delaying writing to Finkelstein again until I had had Bader's reply.

Thank you for Baxter and Buergenthal's paper (which arrived without its page 22); I have passed it on to the person writing the legal part of our study. Has your Carnegie study yet produced anything further? And when is the Senate debate expected to get underway?

It very much looks as though the new British interpretation of the Geneva Protocol is here to stay. For what it is worth, the British Pugwash group has organised a large meeting about it, but I am afraid it will be a very ~~und~~ muddled affair - as the enclosed programme suggests. I have done a background paper for it, a copy of which I also enclose, which I should be glad to have your comments on.

Yours ever,



Julian Perry Robinson

Encls.

CSW

June 5, 1970

Mr. Julian Perry Robinson
Sveavägen 166
S-113 46 Stockholm
Sweden

Dear Julian;

I am afraid that the only time I can come to Sweden during the ~~summer~~ is July 10-14. I must be in Washington July 15. I will need the rest of July to prepare our itinerary in Viet Nam, where we shall be throughout the month of August. If I can be of any use between July 10 and 14, I would like to come. It would be ~~very~~ helpful if you could send an airticket. I would be prepared to leave Boston July 10 and could return via Washington, where I must be all day on the 15th.

The Baxter and Buerghenthal paper is now being printed by Carnegie. The remaining three papers are still in preparation. I shall send them to you when they become available. There is now some expectation that the Geneva Protocol may be sent to the Senate in July.

With warm regards, as ever,

Matthew Meselson

MM/ss

JPR/gf

June 24, 1970

Professor Matthew Meselson,
Harvard University,
The Biological Laboratories,
16 Divinity Avenue,
Cambridge, Mass. 02138

U.S.A.

Dear Matt,

Thank you for your letter of 5th June. We look forward very much to seeing you on July 10; hotel reservations have been made and we have asked SAS to have their Boston office send you a ticket.

By separate cover, I am sending you Robert Neild's latest draft for an expanded version of Part IV of the provisional edition of our CBW study. This will be a rather crucial part of study. It is very much a first draft: a good deal of it is bare bones, with the main substance yet to go in. In view of its unfinished state, Robert asks me to ask you to keep it confidential.

If you have anything more on CS that I have not seen, could you bring it with you?

Yours ever,

Julian Perry Robinson

SIPRI
STOCKHOLM INTERNATIONAL PEACE
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JUL 24 1970

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Professor Matthew Meselson
Harvard University
The Biological Laboratories
16, Divinity Avenue
Cambridge, Mass. 02138

July 22, 1970

U.S.A.

Dear Matt,

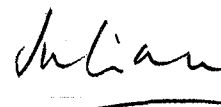
I hope you got back safely and in time: we were all very sorry to see you leave and wished you could have stayed longer. I think your visit did us a lot of good.

If you can possibly find the time, do please sound out Bowman Cutter about the possibility of my discussing Part I of our CBW-study with him. It really is most frustrating knowing that people have comments on the thing while being prevented from passing them on. What would be especially convenient from my point of view would be a meeting in Geneva (if indeed he is, or will be, there with the U.S. Delegation, as he was earlier this year). Failing that, I could probably persuade Robert to send me to Washington. One way or the other I ought to try do something quickly.

Could you also give me the reference to Congressional Record in which the statutory DOD statement on its CBW activities appears?

With best wishes.

Yours ever,



Julian Perry Robinson

HARVARD UNIVERSITY
THE BIOLOGICAL LABORATORIES
16 DIVINITY AVENUE
CAMBRIDGE, MASSACHUSETTS 02138

July 24, 1970.

Mr. Julian Perry Robinson
Stockholm International Peace
Research Institute
Sveavagen 166
S-113 46 Stockholm
Sweden

Dear Julian,

I asked Bowman Cutter if he would be willing to go over the CBW with you. He replied that he would be pleased to do so. So far as I know he is not going to Geneva but you should contact him directly.

The reference you request is, at least approximately, as follows: House Armed Services Committee, Sub Committee Number Three, Hearings on Military Posture and Legislation to Authorize Appropriations for Fiscal Year 1971, part 2, pages 6861, 8306-8307, 8346, and Appendix pages I-IX. There may be additional references in the same hearings.

I enclose an annotated copy of the Year Book draft by Josef Goldblat. Would you please give it to him, with my very best regards. I am sorry there is not time for me to write to him directly. I also enclose an annotated copy of Anders' paper and would appreciate your transmitting it to him.

It was lovely visiting S.I.P.R.I. in mid-summer. Best regards to everyone.

As ever,

Matthew Meselson

MM:ag
Encl.

SIPRI
STOCKHOLM INTERNATIONAL PEACE
RESEARCH INSTITUTE

SEP 11 1970

Chairman: Prof. Gunnar Myrdal
Director: Mr. Robert Neild

Sveavägen 166
113 46 Stockholm
SWEDEN
Telephone: 34 96 00
Cables: Peaceresearch

September 8, 1970.

Professor Matthew Meselson
Harvard University
The Biological Laboratories
16 Divinity Avenue
Cambridge 38, Mass
USA

Dear Matt,

Your letter of 27th July has just reached me. I do not have items 1,3,6,24,26 or 32 of your list, so I cannot send you copies of them. I have items 15 and 30, the Army Manuals, and item 25, the IDA report, but as they are rather long I am reluctant to photocopy them in their entirety. Please let me know if you really do want them in toto, or whether you could make do with those parts of them that relate ^{to} irritant agents.

I am sending you photocopies of items 2,4,12,14,16,21,28 and 31 under separate cover. The only reimbursement I ask for is that you send me copies of items 5 and 10. Please do this if you can as I am particularly anxious to see these two references.

With best wishes,

Yours ever,

Julian

Julian Perry Robinson

JPR/CP

*PS. We are all longing to learn how
your Vietnam trip went off.*

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2. ✓ Porton Technical Paper, No. 672 (1959). "Toxicity of CS." JUL 3 1970
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4. ✓ Penn. W.E., Ordnance, No. 272, 192-194 (September-October, 1965). "Riot control chemical." ←
CS gas
5. Special Summary Report Directorate of Medical Research, Edgewood Arsenal Chemical Research and Development Laboratories, Edgewood Arsenal, Maryland (September, 1965). "The toxicity of CN, CS and DM." ←
6. * Edgewood Arsenal, Maryland. Research Laboratories Human Estimates Committee Minutes. (August 25, 1966).

7. ✓ Hellreich, A., Goldman, R.H., Bottiglieri, N.G. and Weimer, J.T., Edgewood Arsenal Technical Report EATR 4075 (January 1967) (AD 809-485) "The effects of thermally-generated CS aerosols on human skin."
8. ✓ Striker, G.E., Streett, C.S., Ford, D.F., Herman, L.H., and Helland, D.R., Edgewood Arsenal Technical Report EATR 4072 (January 1967). "A clinicopathologic study of the effects of riot control agents on monkeys. V. Low concentrations of diphenylaminochloroarsine (DM) or o-chlorobenzylidene malonitrile (CS) for extended periods." (AD 808731). ←
9. ✓ Striker, G.E., Streett, C.S., Ford, D.F., Herman, L.H. and Helland, D.R., Edgewood Arsenal Technical Report EATR 4071 (January 1967) (AD 808732) "A clinicopathologic study of the effects of riot control agents on monkeys. IV. o-chlorobenzylidene malonitrile (CS) grenade."
10. ✓ Cohan, Leon, Jr. (Capt.) Marine Corps Gazette, pp. 33-35 (April 1967). "Vulnerable (How the use of CW could affect U.S. Forces in Vietnam)."
11. ✓ Sigmeth, et al, Am. J. Hosp. Pharmacy 26, 41-42 (January, 1969). "A note on lachrymator weapons."
12. ✓ Army Research and Development Newsmagazine, p. 10 (January 1969). "Two chemical products play strategic role in Vietnam combat."
13. ✓ Army Training Circular 3-16 (April 1969). "Employment of riot control agents, flame, smoke, antiplant agents, and personnel detectors in counter guerrilla operations."
14. ✓ Washington Post, p.A6 (July 24, 1969).. "CS gas purchase for Vietnam increased 16-fold since '64."

- ✓17. Mitchell, Alexander, The Sunday Times, p. 6 (August 24, 1969). "Taking CS gas to the Viet Cong."
- ✓18. Leach, Gerald, The Observer, pp. 1 & 2 (August 24, 1969). "60 Bogside babies 'suffering from effects of riot gas'."
- ✓19. Reid, Raymond T., Colonel, GS, Office, Chief of Legislative Liaison, in a letter to Congressman Richard D. McCarthy published in The Ultimate Folly, pp. 54-55 (1969). Knopf, New York, 1969. U.S. Army Procurement of CS for S.E. Asia.
- ✓20. Smith, Robert M., The New York Times, p. 11 (September 29, 1969). "U.S. Command in Saigon rejects Pentagon view that use of tear gas reduces civilian casualties."
(We do not have this one)
- ✓21. Applegate, Col. Rex., Ordnance No. 293, 180 (September - October 1969). "Riot control 1969."
- ✓22. Applegate, Lt. Col. Rex, Ordnance, No. 282, 604-609 (May-June 1967). "Weapons for riot control."
- ✓23. Edgewood Arsenal Special Publication, EASP 600-1, 4 pp. (October 1967). "Characteristics of riot control agent CS." (661319).
24. ✗ Mennonite Central Committee News Service, Akron, Pa. (November 10, 1967). "Tear gas found lethal for Vietnam refugee."
25. ✓ Coates, Joseph F., Institute for Defense Analyses, Science and Technology Division Study S-271, IDA Log No. HG 67-5851, p. 125 (November 1967) (Virginia). "Nonlethal weapons for use by law enforcement officers." (661041)
26. ✗ Coates, Joseph F., Proceedings of the First National Symposium on Law Enforcement Science and Technology, pp. 787-795 (1967) (Washington, D.C., Thompson Book Co. and Academic Press). "Nonlethal weapons for domestic law enforcement officers."
- ✓27. Huddle, Franklin P. Science Policy Research Division, The Library of Congress Legislative Reference Service, Washington, D.C. (March 5, 1968). "Tear gas for riot control."
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- ✓29. Army Digest, pp. 6-11 (November, 1968). "Silent weapons."
30. ✓ Department of the Army Technical Manual, 80285 (1968) (G.P.O., Wash.) "Treatment of chemical casualties."
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32. ✗ Product Information Publication, (1968) Federal Laboratories Inc., Saltsburg, Pa. "Decontamination instructions."

December 10, 1970

Mr. Julian Robinson
Stockholm International Peace
Research Institute
Sveavagen 166
113 46 Stockholm, Sweden

Dear Julian:

In reply to your letter of November 15 I have the following comments:

Page 1, footnote 1: Have you run across the statement that Admiral Leahy, President Roosevelt's military advisory once criticized a proposal for chemical anticrop as " . . . violating every Christian ethic that I know."

Page 3, second paragraph, line 19: At the start of 1964 the Ranchhand Base was at TonSon Nhut. In mid-1964 an additional base was established at Da Nang.

Line 24: The C123 pilot is instructed to fly at 130 knots, not 150.

Page 4, paragraph one, last two sentences: C123 aircraft were used for crop destruction and for defoliation. They were U. S. aircraft with interchangeable insignia. For defoliation U. S. insignia were used. For crop destruction these were replaced with Viet insignia.

Page 5, paragraph three, first sentence: Your references to cutbacks should be carefully checked. Your figures on page 10 do not reflect a 35% cutback for 1969. Unfortunately, I do not have any good references on this point, however, the New York Times article earlier this year was filled with errors exaggerating the "cutback."

Page 6, paragraph 1: I do not understand how you arrived at the figure 56,000 tons.

Page 13, last sentences: My own impression when speaking to military personnel in Vietnam is that many of them are not sure of the effectiveness of herbicides.

Mr. Julian Robinson
Page Two
December 10, 1970

It is important to distinguish three roles in which herbicides are used. First, along lines of communication and defensive parameters. One high official expressed the opinion that for this purpose ~~that~~ herbicides are largely a failure. The reason is that the remaining branches, stems, and trunks substantially limit visibility. Indeed, visibility is often extended to approximately the accurate range of enemy small arms, a very undesirable result. On lines of communication and parameters, herbicides have been almost entirely replaced by plowing, diesel oil applications, ~~or~~ hand clearing. ^{with fire.} The second use for herbicides is for crop destruction. There is extreme skepticism regarding the military utility of this program among many high government officials in Vietnam. Furthermore, there is no doubt that it imposes very serious costs of its own. My own opinion is that it continues under its own momentum, independent of any rational analysis. Finally, herbicides are used to clear large forest areas. I do not have any analytical information regarding the effectiveness of this tactic. Its advocates claim that it helps reveal enemy fortifications depots and supply lines. Its critics argue that it is too slow to act and too quick to be undone by regrowth to have more than marginal effectiveness, given the ability of the enemy to keep shifting his operations. *After all, war zones C+D, although repeatedly sprayed and rather dense Saigon, still belong to the enemy.*

Page 14: Here and elsewhere you quote from the Carnegie Symposium. It is important that you quote only from the final, edited version. I will send you copies of all the papers within a week. All but Brown are enclosed. The Blumenfeld and Meselson paper should not be referred to as ARPA-Carnegie as you do on pages 22, 24, 25, 27, 28, 31, 36, and possibly elsewhere.

Pages 18 and 19: So far as I know your history of the strategy of crop destruction is based on inference. Although the movement of populations may well have been an objective early on, Robert Komer writes in a comment on the Carnegie Symposium that it was not an objective. I presume this at least covers the period of his ambassadorship in Vietnam.

Page 19, paragraph three and four: I know of no published reference for your statement that the VC-NVA grow one percent of their food in-country. However, one calculation that can be made with published data is the total number of man-years of food that have been destroyed by the crop destruction program. Making reasonable estimates for the yield of sprayed crop land and the daily Vietnamese diet one concludes that on the order of more than a million man-years of food have been destroyed under the program. Obviously the main impact has been on civilians since even if say ten percent (a very high value) of VC-NVA is grown in-country expressly for their consumption there are simply not enough enemy soldiers in Vietnam to account for anything but a small proportion of the total food destroyed. The most badly affected civilians are

Mr. Julian Robinson
Page Three
December 10, 1970

almost certainly Montagnard tribesmen. This may be concluded from the fact that crop destruction has been largely confined to the northern three tactical corps zones and generally kept away from coastal regions. *Thus it takes place where the Montagnards, estimated at up to one million in numbers*
Page 25, paragraph one, line 3: Bien Hoa. *line*

Page 27, paragraph two: I have never before heard of agent CD-1.

Page 31, paragraph two: Not having your reference list, I cannot tell whether reference 598 establishes the existence of an ARPA study.

Table 2.7: I do not understand how the weight of active ingredients per gallon of orange can be greater for D than for T. Perhaps you are referring to acid equivalent.

As you probably already know, it has been reported that Portugese aircraft have recently been spraying herbicides on Cassava crops in nationalist-held regions of Angola. See the New York Times for December 9. I have looked into this and am essentially certain that it is true.

With warm regards.

As ever,

Matthew Meselson

MM:mcm
Enclosures



SCIENCE POLICY RESEARCH UNIT

NUFFIELD BUILDING FALMER BRIGHTON SUSSEX BN1 9RF

Chairman: Professor ASA BRIGGS
Director: Professor CHRISTOPHER FREEMAN

Telephone:
BRIGHTON (0273) 686758

Dr M. S. Meselson
The Biological Laboratories,
Harvard University
16 Divinity Avenue
CAMBRIDGE, Mass. 02138

JUL 16 1971

12 July 1971

Dear Matt,

I've been meaning to write for a long time now, and not only to regret not having been able to visit the States in the spring. This bloody SIPRI CBW-study is still mucking everything up, taking far longer and causing far more ill-feeling than even I imagined it could. I walked out of it at the beginning of the year in disgust at the way Volume 5 was being handled, but have got sucked back into it again, nominally as an overseas consultant, but in fact as the only remaining author to finish it off. So here I am at Sussex, working full time on Volume 2; fortunately there is a lot of overlap between it and my Ford/Harvard project.

I enclose a couple of recent drafts you may find time to look at - one on the way WEU handles its CB weapons controls, and the other on Soviet CBW policy, organisation and capabilities. I would very much welcome any comments or additional material you may have, particularly on the Soviet one. (Incidentally, missing entry no.90 in the bibliography refers to you - a conversation we had in July 1969; I would be enormously grateful if you could give me a quotable reference.) Actually, it's probably too late to make any substantial changes to the WEU draft; it was whisked off to the printers as fast as I could write it.

Have you heard from the Sunday Times? I gave them (or rather their reporter Peter Pringle) your name as being a suitable expert to consult about mounting an expedition to Angola to look into the herbicide allegations. In case Peter didn't send you the latest MPLA handout, I enclose a copy herewith. I also enclose a copy of a rather odd story in the Daily Telegraph, which I should be very interested to learn more about.

Did you hear that Frank Barnaby is to be the new Director of SIPRI? He goes out there next month.

Yours ever,

Julian Perry Robinson

PS. Have you a spare copy of the Foreign Relations Committee hearings on the Geneva Protocol? I'm a bit out of touch with things here.

16 July 1971

Mr. Julian Perry-Robinson
Science Policy Research Unit
Nuffield Building
University of Sussex
Falmer, Brighton, Sussex

Dear Julian,

Just got your welcome package. Better delete reference 90. I do not believe I said that nor do I believe what I take the statement on page 12 to mean. Be assured that the data on the three sheets enclosed have been supplied by DoD to the Congress, and not on a classified basis either. Nevertheless, please do not connect my name with the references.

Thank you for putting me in touch with Pringle. I believe I can help him.

Cheers.

MM:mm

enclosures: 3

26 October 1971

Mr. Julian Perry-Robinson
Science Policy Research Unit
University of Sussex
Nuffield Building
Falmer, Brighton, Sussex BN19RF
ENGLAND

Dear Julian,

Here are two references on crop destruction in Malaya:
Pelzer, Karl J., "Land Utilization in the Humid Tropics" in
Proceedings of the Ninth Pacific Science Congress of
the Pacific Science Association, Volume 20 (1958), pp.
124-143, Bangkok.

Pelzer, Karl J., "Man's role in Changing the Landscape of
Southeast Asia", The Journal of Asian Studies, Volume
XXVII, Number 2 (February 1968), pp. 269-279.

Pelzer, in the second reference, mentions "poison and defoliants"
and Brigadier Henniker (Red Shadow over Malaya, Edinburgh and London, 1955, p. 180),
after writing that the spraying of an unnamed herbicide was ineffective, says: "Later
the R.A.F. adopted a more effective technique, but by then we had moved to Selangor
where there were no gardens worth mentioning." Have you any idea what the more
effective technique was?

Enclosed is also a bibliography for your possible interest.

We look forward to seeing you soon. Love to Mary.

as ever,

MM:mm
encl: 4

Robinson
Geneva, 21 April 1972.

MAR 23 1973

Dear Matt,

A note written in great haste (I am attending a rather hectic ICRC conference on behalf of Vtro). Sorry about my hand writing.

I was in the London Foreign Office a week ago, where I spoke with the man concerned with CW disarmament questions. I delivered your warning that some sort of enquiry was likely to be made shortly from Washington about the current state of British policy on the CS matter. I was asked to urge you to do what you could to discourage such an enquiry. It was explained to me that whereas the Foreign Office was anxious to see the 1970 decision reversed, they anticipated fierce, and almost certainly successful, opposition from the Ministry of Defence, should the matter come up for ~~review~~ formal discussion at a Governmental level.

The MOD is apparently practically paranoid about all matters to do with Northern Ireland; the Foreign Office has explained to them several times, but to no avail, that the Geneva Protocol has no bearing on their use of CS there (or, for that matter, of CR). Bondi, in his capacity as Defence Science Adviser, is likely to side with MOD, should an American enquiry precipitate a re-examination of the CS policy review; and even if he sides against MOD, his influence is unlikely to prevail. The Foreign Office ^{are} apparently confident that reason will eventually succeed; they feel that time is on their side, but that for the ~~present~~ next few months, sleeping dogs should be left well alone.

Yours ever,

Julian.

WB ROBINSON

UNIVERSITY OF OXFORD

DEPARTMENT OF FORESTRY

TELEPHONE

57891 : DEPARTMENT
55757 : PROFESSOR OF FOREST SCIENCE
J. L. HARLEY, M.A., D.PHIL., F.R.S.

COMMONWEALTH FORESTRY INSTITUTE

SOUTH PARKS ROAD
OXFORD
OX1 3RB

27th July 1973

Dr. J.P. Perry Robinson,
Science Policy Research Unit,
The University of Sussex,
Nuffield Building,
Falmer, Brighton

Dear Robinson,

I am sorry I have taken a little time to answer your query, but I had to go back to Plant Protection about Trioxone (not Trioxene), since this is only made in relatively small quantities and has been very largely sold abroad. I have not been able to obtain specific information about the exact formulation of Trioxone at the time of the Communist troubles in Malaysia, but all the evidence is that the formulation has not changed over the last decade or more. Trioxone has been used for sales abroad of an ester formulation of 2,4,5-T, so that it is essentially very similar in its characteristics to the 2,4,5,-T esters used in Vietnam, but does not contain 2,4-D.

Yours sincerely,



G.E. Blackman

AUG 09 1973

WB ROBINSON

UNIVERSITY OF OXFORD

DEPARTMENT OF FORESTRY

TELEPHONE

57891 : DEPARTMENT
55757 : PROFESSOR OF FOREST SCIENCE
J. L. HARLEY, M.A., D.PHIL., F.R.S.

COMMONWEALTH FORESTRY INSTITUTE

SOUTH PARKS ROAD
OXFORD
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27th July 1973

Dr. J.P. Perry Robinson,
Science Policy Research Unit,
The University of Sussex,
Nuffield Building,
Falmer, Brighton

Dear Robinson,

I am sorry I have taken a little time to answer your query, but I had to go back to Plant Protection about Trioxone (not Trioxene), since this is only made in relatively small quantities and has been very largely sold abroad. I have not been able to obtain specific information about the exact formulation of Trioxone at the time of the Communist troubles in Malaysia, but all the evidence is that the formulation has not changed over the last decade or more. Trioxone has been used for sales abroad of an ester formulation of 2,4,5-T, so that it is essentially very similar in its characteristics to the 2,4,5,-T esters used in Vietnam, but does not contain 2,4-D.

Yours sincerely,



G.E. Blackman

AUG 09 1973



American Academy of Arts and Sciences

165 Allandale Street

Jamaica Plain Station, Boston, Massachusetts 02130

Telephone 617 522-2400

21 October 1976

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton, Sussex BN1 9RF
ENGLAND

Dear Mr. Robinson,

On behalf of the Organizing Committee, I wish to express our pleasure at learning that you plan to attend the Conference on Policies for Chemical Weapons and Chemical Arms Control to be held at the American Academy of Arts and Sciences in Boston on January 21-22, 1977.

Following our recent telephone conversation, I am writing to confirm our understanding that you will contribute a paper for distribution to the participants in advance of the Conference. We would particularly value a paper that provides an analytical framework for considering the major U.S. policy options for lethal and incapacitating chemical weapons, especially in the context of European defense. The actual choice of topics and treatment will of course rest with you. However, you may wish to consider some or all of the following questions:

- . What military purposes are served by the lethal/incapacitating chemical retaliatory capability available to NATO?
- . To what extent would these purposes be served by other already existing or attainable military means if chemical weapons were not available to NATO?
- . What estimates can be made regarding Soviet chemical warfare capability and doctrine?
- . What are the trade-offs for NATO relinquishing its retaliatory chemical capability, in terms of alternative utilization of resources, simplified command and control, and other factors?

- . What are the attitudes of the European NATO governments toward the role and value of chemical weapons?
- . What benefits would be achieved by an international arms control agreement prohibiting the production and possession of lethal and incapacitating chemical weapons of war and allowing only defensive and protective equipment and preparations?
- . What risks are inherent in such an agreement?

The paper should be long enough to allow a clear presentation of the subject to readers generally familiar with defense and arms control matters but knowing little about chemical weapons and U.S. policy regarding chemical warfare. Approximately ten thousand words would not be too long, but you should be the judge. We hope to receive a copy by December 15. This would allow time for preliminary review and possible limited revision and for duplication and distribution before the Conference. The American Academy staff will retype the manuscript if necessary and distribute it to participants. The Conference is prepared to reimburse you for necessary secretarial costs and to provide an honorarium of \$500.

For your information I enclose a list of acceptances received to date from other participants, including those who have also agreed to provide a paper.

Sincerely yours,

Matthew Meselson

encl.

Ms Patricia Maherty
American Academy of Arts & Sciences
165 Allandale Street
BOSTON
Mass 02130

20 Dec 76

Dear Ms Maherty,

Matthew Meselson told me that I should send directly to you, by collect air freight, my paper for the Jan 21/22 Chemical Weapons conference, so here it is. I am very sorry to have been behind schedule, and then so verbose.

You may think my typescript is legible enough not to warrant retyping. I would certainly have no objection if you were to circulate Xeroxes of it. But if you do want to retype, maybe you should think about sending Xeroxes of the original straightaway to the two participants who are supposed to be commenting on my paper, viz Canby and Makins.

I am today airmailing a copy of this letter with a Xerox of the paper to Meselson in California. I am also airmailing a Xerox to you. I enclose a copy of my letter to Emery Air Freight.

Sorry again for burdening you like this just before Christmas.

Yours sincerely,

②

J P Perry Robinson

PS I've just found that I've forgotten to do the bibliography. I'll set to straightaway in the hopes of getting it done before the Emery Air Freight man arrives. If not, I'll airmail it out today.

cc X Meselson



THE UNIVERSITY OF SUSSEX

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Mantell Building Palmer Brighton Sussex BN1 9RF

Telephone:
BRIGHTON (0273) 686758

Professor Matthew Meselson
4160 Los Encantos Way
LOS ANGELES
California 90027

20 Dec 76

Dear Matt,

Here is my paper at last. I'm afraid my flu not only delayed things but made me excessively verbose. You are welcome to cut if you want to, but the structure is quite tight.

Until January; with best wishes to you all

As ever,

Julian Perry Robinson



THE UNIVERSITY OF SUSSEX
SCIENCE POLICY RESEARCH UNIT

MANTELL BUILDING FALMER BRIGHTON SUSSEX BN1 9RF U.K.

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Mass 02138

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3 May 1977

Dear Matt,

You've done a really excellent job on that CW transcript. I hope the general won't be having too many second thoughts.

Enclosed are copies of pages III-5, III-6 and III-20, on which I've entered revisions of things I said.

On page II-14 there is Lennon quoting a figure of 117,000 for the establishment of the Soviet chemical corps. After one of the sessions I asked him how that number had been reached, and it turned out that it was the upper end of a broad range, the whole estimate being very speculative. Or at least that's what he told me. It would be nice if wording could go into the transcript indicating that the 117,000 was not a confirmed figure.

As to my paper, you remember that I left you with a revised copy to which further revisions would be needed if you decided against publishing the two appendices. I've since had more comments, including some rather cross ones from Deryck Viney, which tempt me to make further revisions (primarily clarifications). Please let me know quite soon what you plan to do, particularly on whether I should try to make my own publishing arrangements for the appendices.

With best wishes,

As ever,

Julian Perry Robinson

motivated by military objectives and are, to various degrees, insulated from the broader social and ethical concerns of the general society. Decoupling certain kinds of research from military programs could be a major long-term benefit of chemical and biological arms control.

ROBINSON: As I indicated in my paper, neither of the two military types of threat mitigation policy -- weapons emphasis or protection emphasis --

provides a counter to the threat of wider assimilation of CW. In my view this is the main argument for placing emphasis on arms control. One can define a threshold below which the degree of assurance provided by an agreement of not being menaced by CW weapons is less than the confidence generated by

possessing them. If one keeps ^(this threshold) ~~that concept~~ in mind, one can proceed to

consider how one should ideally adjust the two main negotiating variables

^(But here one has to recognize) of verification and scope. ^(even counterproductive) ~~I suggest~~ that it may be awkward ~~to specify~~

^(precisely how it should be verified) ~~verification measures~~ in an agreement. One would perhaps be better advised

^(think) to ~~talk~~ in terms of the verifiability of possible agreements rather than the efficacy of ^{agreed} verification measures. Those are two very different concepts and

it is important to keep them separate. One might judge that, in view of the

~~relatively~~ marginal significance of chemical weapons in a European confron-

^(to seek to enhance the verifiability of an agreement by writing into it) ~~tation, it would not be necessary~~ ~~for~~ special verification provisions ~~to be~~

^(It is true that the possibility of hidden) ~~written into an agreement.~~ The question of stocks ~~undoubtedly~~ presents a

^(not so much stocks of filled munitions, which deteriorate rather fast, but of bulk agent. Yet) difficult problem, ~~but~~ the process of putting the stocks into munitions is relatively ^{(and one might justifiably rely on national technical means detecting the breaking} ~~conspicuous)~~ ^{(and therefore could be detected with normal verification of over-} ~~devices~~

However, if one broadens the perspective to ^{embrace} ~~include~~ the objective of ^{including proliferation,} constraining the assimilation process, I think that one does need special

verification provisions written into the treaty. Clearly, a variety of

factors contribute to assimilation. Research and development is a major

one, and there is a need for a greater stress on some form of constraint or

even control of research and development, ^(especially if possession of CW protective equipments is) ~~chemical weapons are indeed allowed~~

^(to remain legitimate. Here) ~~to exist in any forthcoming treaty~~ One might perhaps think in terms of quite

simple verification measures, perhaps merely information exchanges on protective activities mediated by a consulting committee of the parties. One would also want to concentrate very carefully on the scope of the chemicals to be covered in the treaty, ^{particularly as regards the problems presented by riot-control incapacitants.} here, a registry of domestically-used chemical agents could prove useful.

^{As to} ~~With regard to the question of proliferation,~~ ^{itself} it is clear that an agreement

that relies or places emphasis on national technical means of various types would offer little in the way of proliferation constraints. Under such circumstances the only constraints one can ^{envisage} ~~envisage~~ would arise from ^{the consequences of} public disclosure, ^{But such} ~~and that~~ disclosure ^{would often be inimical to} ~~is difficult to assure~~ with national technical

~~(Instead one should look to such devices as means. Perhaps the proposed phosphorus accountability scheme might serve that purpose.~~

SCOVILLE: Could you explain why you think national technical means or intelligence does not have the capability to verify nonproliferation?

ROBINSON: I assume that indeed it would have some capacity for ascertaining any proliferation taking place. My point was that national technical means might well detect this activity but ^{without} ~~it will not prevent~~ ^{ing} ~~it, from~~ ~~happening~~. I was looking to the treaty to provide a constraint on the proliferation process.

For example, a country might decide that it would be advantageous for its immediate security to buy some nerve gas. This country makes the calculation that national technical means will very likely pick up the activities associated with ^{acquiring} ~~developing~~ a nerve gas capability. However, it might conclude that it might be able to rely upon a particular political

situation to insure that its activities are not impeded. ^{For example, it} ~~It~~ was quite evident that chemical weapons were being used in ^{the} ~~Yemen,~~ ^{but beyond a token wringing of hands} ~~yet no Western power undertook~~ by the British Government no serious effort was made by outside powers to obstruct that use. ~~to condemn this use of CW as a reprehensible act.~~

MIKULAK: From a technical standpoint, national technical means might be used to pick up proliferation activities, but as a practical matter the United States is not likely to spend much of its time looking for activity in areas which are not of direct concern to its security. If one wants to

*Didn't
Prime Ministers
Wilson
condemn
it?
M.M.*

LEONARD: There is a no-transfer clause in the EW treaty that I assume could be used as a model for an agreement on chemicals.

GUHIN: There is an indication in the discussion that the proliferation problem is not a serious one which seems to be reflected in a lack of concern about the conversion of industrial agents. While this evaluation may be true with respect to the U.S. and the Soviet Union, I think that the possibility of diversion from the civilian sector may be greater in other countries.

DOTY: Perhaps it would be useful to ask the three participants who prepared papers and today's discussants for final comments.

LENNON: I think that it would be desirable to solicit comments on this document from various sectors. Such an exchange of views would be most helpful to those who are going to be involved in either processing the institutional views or national consultation. In my view this effort can be either a U.S.-Soviet venture after consultation with their allies or a NATO-Warsaw Pact venture. I believe that consultation on technology and techniques of disposal would be a particularly appealing item for the Soviets. We have learned a great deal in the last few years and are presently engaged in considerable research in this area to enable us to accomplish the detoxification more efficiently and more rapidly. This information might be most helpful to the Soviet Union.

ROBINSON: I am pleased that the concept of assimilation seems to have been considered ~~seen as a useful, one, to consider in connection with an analysis of policy options.~~ ^{after these two days} The overall impression that I am left with/is that while one can spend a great deal of time discussing the variety of possible options and future progress, in fact the situation is one in which ~~one's~~ flexibility is very severely restricted by the realities -- the lifetime of weapons stocks, diplomatic problems of prepositioning stocks, etc. ^{My only reservation about the} ~~the~~ ^{is that it side-steps the issue of research and development which}

(10273) 686758

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10 May 1977

Mr. Julian Perry Robinson
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer, Brighton, Sussex BN 19 RF
ENGLAND

Dear Julian,

We estimate that your two appendices would occupy about 13% of the published book. The Carnegie people feel this is rather high and I agree. Do you think that the appendices could be condensed by ~~any~~ ~~about~~ like 20-30% without unacceptably reducing their value? Alternatively, do you see any merit in present only appendix B, a suggestion from Halsted?

If you can condense the appendices by 20-30%, we will include them, otherwise we will not. In any case, I gather from your letter that there may be revisions to your main piece. It is not too late to incorporate them, but please let me have them very soon, in any event before 31 May, so that I can take them away with me on vacation.

I plan to attend a meeting near Aviemore, Scotland 20-25 June and to stay in England ~~until~~ ~~July~~. ~~Are~~ ~~you~~ likely to be in London any time between 27 June and 3 July? I plan to spend those days partly in Cambridge and partly in London and would try to see you and Mary if you should be in the city.

With best wishes,

as ever,

Matthew Meselson



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Professor M S Meselson
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22 May 1977

Dear Matt,

Here are the two appendices, reduced from 32 to 23 pages in all.
I'm glad that you are going to be able to keep them in.

I also enclose a small schedule of further amendments to the main
body of the paper. They are responses to comments from Deryck Viney.
I should have done more if I wanted to keep him completely happy.

I certainly hope we can meet when you are over here at the end of
next month. It's just possible that I shall have to be in Switzerland
at about that time, but in any event I'll be in touch with Harrison
at Trinity.

As ever,

Julian Perry Robinson

encs.

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20 June 1978

Dr. Julian Perry Robinson
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer, Brighton, Sussex BN 19 9RF
ENGLAND

Dear Julian,

Gerard Piel has asked me to write an article ~~about~~ chemical warfare for Scientific American. Would you like to join me in writing it? We are off to France for a vacation until August 1st. I hear you may be in the States sometime after that. If so, we could perhaps find some time then to work together. Let me know.

Best wishes to you and Mayy.

As ever,

Matt 



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Telephone:

BRIGHTON (0273) 686758

18 July 79

Dear Matt,

Hope my revised MS for the Scitech piece
has arrived safely. Here is the first
of the revised tables/figures. It would
be nice to have Read data for 1962-70,
and both Read or Insurance data prior
to 1963.

Mary presented a baby boy late on 16th
July: He's a bit big very well.

As ever,

Robin.



THE UNIVERSITY OF SUSSEX

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Telephone:
BRIGHTON (0273) 686758

24 July 79

Dear Pat -

Here is a typed-up version, in duplicate,
of the manuscript-annotated draft of
15 July.

Also, a revamped version of the
missions-expenditure table, making use
of those manuals you sent me, and
your suggestions. [Also a detailed account of how
I did it: you may want to
amend or correct.]

Am now finishing the journal-based
diagram.

From 7th August to 4 Sept with a view
to the following address:

40 Kador
18 rue St Joseph
La Garde Freinet,
Var, France.

As ever,
Julian.

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17 September 1979

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
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Falmer, Brighton, Sussex BN 19 RF
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Dear Julian,

Don't you think that these old manuals make chemical warfare seem much nicer than the new ones do? I haven't sent the manuscript to Piel. Michael, by some miracle, has arrived to help me out of all the accumulated administrative matters and, after a five year grant request is submitted on 1 October, I will make some limited revisions, mainly in the arguments pro and con, and telephone you for agreement before mailing a copy to Piel.

Best wishes to Mary, Joshua, and Oliver James,

as ever,

HARVARD UNIVERSITY
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16 DIVINITY AVENUE
CAMBRIDGE, MASSACHUSETTS 02138

18 October 1979

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Julian,

Here are six copies of the 15 October draft sent to Piel plus cover letter and notes relating to illustrations. I have also sent copies to Col. Bay (Bug Dayway) and to several persons in Washington. Piel says all revisions should reach Scientific American by 15 November.

You will note increased figures for spending in FY71 and 72. This comes from a DoD statement of 6 December 1971 and may account for most of the \$16M for protective clothing spent before FY77 but not attributed to ~~particular ways~~ on our earlier tabulation. I will try to verify all spending figures we present.

Some miscellaneous questions:

--Is the evidence on delayed effects of nerve gas you cite that of Duffy and Burchffel at the Harvard Medical School?

--What does NMAD weigh?

--Should the rationale given for decontamination be amended to include allowing the relaxation of protection or the avoidance of spread to clean areas?

--What is the basis for taking ECT40 as 12 mg-min/m³? Since your dosage-distance plot reaches this value only 50 km downwind, would it be more accurate at the top of page 15 to delete the words "...at least twice that percentage of all..."? What is the meaning of "seriously endanger"?

--What caused the revision of your earlier estimate of 50 mg-min/m³ at 90 km to about 19 mg-min/m³ for the same strength, wind speed, and Pasquill category?

--I have misplaced the references to the Salt Lake Tribune(?) with the photos we want. Please let me have them again.

I Feel there are still some weaknesses in the article.

(i) No reason is suggested for Soviet incentives favoring chemical disarmament. (ii) The discussion of verification measures is thin. (iii) The most recent result we cite from the bilateral talks occurred in 1978. Coming in 1980, the article can give the impression that the talks are stalled--which they are not. (iv) We fail to make the point that retaliation is not in itself an objective and that arguments about retaliation in kind tend to obscure the larger issues of deterring war itself and, failing that, denying success to the Soviets. Chemicals are irrelevant to the first and of dubious, possibly negative value for the second.

as ever,

HARVARD UNIVERSITY
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16 DIVINITY AVENUE
CAMBRIDGE, MASSACHUSETTS 02138
9 November 1979

Mr. Juliana Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Juliana,

Writing you is my only pleasant task today, a day of grant proposals, repropoals, and friends' funerals.

I sent our opus to Harold Brown and he is having it reviewed and will provide comments plus an opportunity to talk with the reviewers. I suspect that that means Tom Dashiell but we will see. Amy Hoerber sends regards to you and will provide comments on the manuscript shortly. Jim McCullough sends the enclosed comments on which I have made some notes to you. I presume you have seen the statement of the Chinese government urging international agreement on chemical disarmament made at the U.N. in May. One of the officials personally stressed the importance of the statement to a number of U.N. delegates in private conversation.

I incorporated your changes and rearranged and slightly enlarged our section on arms limitation. So here is one more draft for your collection.

Dennis Flanagan said on the phone today that they plan to publish our article in the April or May issue. Because of their normal two-month work cycle and shortage of personnel, he would find it difficult to ~~have the~~ manuscript through preliminary editing now. He says they have read the piece and do like it.

Incidentally, is it known ~~for~~ certain that the TMS-65 was designed principally for chemical decontamination? I understand that it has been ~~used~~ in use for melting ice on roads.

as ever,

Encls.



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19 Nov 79

Dear Matt,

For your interest, here are ~~a~~ copies of a couple of replies I've had to requests for comments on the Sci Am draft. The one from Ron Mason, at least, you should keep to yourself. It's too late now to make any changes to the draft--which, anyway, neither of these letters seem to necessitate.

Some press cuttings are also enclosed.

As ever,

J P Perry Robinson



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27 Nov 79

Dear Matt,

This is in response to your letter of 9th November which enclosed rev 2 of the 15th October Sci Am draft and Jim McCullough's comments; to Michael's letter of 20th Nov, enclosing Chuck Bay's comments; and to your telephone call yesterday. *and paper*

Your letter of 9th Nov

- 1) I had indeed seen the Chinese statement at the UN Disarmament Commission (if that's the one you mean). Were the officials you mentioned stressing the importance of the statement as a whole--it had to do with all aspects of disarmament--or just the rather brief bit in it on CW ?
- 2) Chassis-mounted aero-engine devices for various different purposes have been described in the Soviet literature over the years. Krasnaya Zvezda for 5 June 1965, page 6, describes a tracked-chassis device ('MAG') for wide-area dissemination of insecticides. Tekhnika-molodezhi, 1968(5):11, describes the 'TRU-100' fire-fighting equipment as an obsolete jet aero-engine mounted on a ZIL-157 truck chassis, the engine being used to aerosolize water. But I haven't come across a reference to ice-melting applications.
- 3) Heaven knows what was behind that Now! story; am inquiring. A copy is enclosed.

The 15 Oct (rev 2) draft

- 4) Regarding page 5 line 18, will you be asking at the Pentagon when BZ was abandoned ?
- 5) On page 8, 9 lines from bottom, the "woven" should be "nonwoven" -- a typo that has survived several drafts.
- 6) According to published figures I've only just come across, NAIAD is about an order of magnitude more sensitive than the M8 to nerve gases.
- 7) On page 10 line 18 you now have the expression oxime "derivative". Could we have "compound" ?
- 8) I like your rewrite of the middle of page 19. But, regarding lines 17-18, the joint statement from which you are quoting was in fact read to the CD on 31 July; and the 10th round of the bilaterals lasted from July 16 to August 31. Didn't you gather also that there had been further progress after the joint

statement had been issued ? Maybe the sentence should open "By July 1979, at the time of the tenth round of the talks, agreement ..." ?

9) On page 20, lines 6-5 from bottom, the "elimination" may be misleadingly ambiguous, for it could subsume conversion of dedicated plant to peaceful purposes as well as demolition. Only the latter, presumably, could be adequately verified by national technical means.

Jim McCullough's comments

10) Regarding the first of his page 2 comments, your change of "can be" to "is" at line 18 looks fine. As to his other comment, we don't want to suggest that the Congress is not part of the U.S.

11) How quickly a lethal dose of VX is absorbed through the skin depends, I would suppose, on the amount of skin surface that is contaminated; but it will always be slower than uptake via the lungs.

12) I'm not worried by our usage of 'gas mask' and 'respirator' (which anyway can mean something else). And though 'nerve gas' may be loose, 'nerve agent' is spuriously precise.

13) On page 6 towards the bottom we could replace "in both European and Asian forward areas" by "both in Europe and near the Sino-Soviet frontier".

14) On page 8 line 13 we could replace "indefinitely" by "for ~~as~~ long periods".

15) A sentence such as the following could be added to page 21 line 19: "In the meanwhile the danger will continue to grow that additional countries around the world may move to acquire chemical weapons of their own" or some such.

Bay's comments

16) As he says, most of his comments are gut reactions: they don't bear very strongly, it seems to me, on the logic of the article and therefore don't require much in the way of amendment. But, as tips of icebergs heading at us, they need considering rather carefully. So, largely as an aide-memoire to myself but also with regard to your day at the Pentagon, I will set out my reactions to each comment in turn:

17) 1/10: Here he wants us to endorse, at the outset of the article, the idea of like-with-like CW deterrence. As I see it, one of the purposes of our article is to show that it is an idea whose validity can reasonably be questioned. The language you have used in the intro is neutral on this issue, and shouldn't be tampered with.

18) 2/4: What is meant by "substantial" of course emerges later in the draft. Evidently Bay feels that on important criteria the stockpile is not substantial. It would be instructive to know what those criteria are--positioning ? munitions mix ? age? His own article seems to suggest that the US has no retaliatory capability worth talking about. But this may just be an over-selling of binaries on his part.

19) 2/14: It would indeed be interesting to note this. But, as academics, we are hardly in a position to know what the military talk about among themselves.

20) 3/4-6: This comment is no doubt inspired by a belief that the abstention from CW during WW2 is evidence for like-with-like CW deterrence. Maybe it is. But

other explanations for the abstention are also ~~px~~ tenable. Non-assimilation has always seemed to me the best. As for the Laotian allegations---and there doesn't seem to be anywhere in the draft where a reference to them would fit--- is he suggesting that the Vietnamese/PL used CW because they weren't deterred? Presumably not.

21) 3/26: You raised this yourself. No less to the point would be reference to evidence that delayed disorders do not develop.

22) 3/31: But, more important, the prophylactics---P2S pills or the new pyridostigmine ones---apparently are not significantly debilitating.

23) 7/19-21: There are at least two possible explanations for the apparently continuing Soviet CW buildup apart from the one Bay is hinting at: (i) it is much easier to start a programme than stop it; (ii) appearances may be wrong. But we don't really need to go into this. Note that if explanation (i) has any validity, it undercuts the 'bargaining chip' argument which Bay advances in his paper for buying binaries. Or does he really believe that US weapons programmes can be switched on and off at will? In this regard it would be interesting to know more about the political setting in Arkansas for the binary production facility.

24) 7/26-29 & 8/11-15: In these two comments Bay is reacting against the structure of our argument. What we say is this: (i) protection against CW is possible without preventing most normal activities (ii) NATO has enough of this protection to negate the mass-destructiveness of CW (iii) the price of that negating capacity is impediment upon normal activity (iv) which, in the sense of reduced combat efficiency, could be a price worth an adversary's while to exact. Bay seems to want us to start out by emphasising performance degradation. If we were to do that, we would be degrading one of the policy options that follows logically from (iv): that of improving protective equipments and drills so as to cut the price, thereby making chemicals less attractive than ~~xxx~~ competing weapons in Soviet eyes (which is the present British declared-policy stance).

Is the burden of Bay's comments here, then, that this policy option is not a realistic one? If so, his reasons should be probed. Perhaps the criticism which NATO CW people used to make of the US CW posture is still valid: that the US Army was unwilling to take the trouble of inculcating antigas doctrine, procedures, &c, and therefore took to a weapons-emphasis CW policy as an easy way out---despite the likelihood of two-sided chemical warfare immobilizing any future European war into WW1-type meatgrinding.

Maybe our draft doesn't stress the performance-degradation considerations sufficiently. But I don't see what else we can say.

(General Falter's equivalent in our MOD remarked the other day that the bearskins and heavy leather in which British soldiers fought at, say, Waterloo must have been at least as encumbering as present-day NBC protection.)

25) 8/21-22: What has USAEUR done with the 200,000 Mk3 NBC suits it has procured in Britain if it hasn't issued its forces with them?

26) 9/1-5: This passage may now be out of date. I am told that the Soviets have now actually introduced an air-permeable NBC suit (called 'OZKZ'), as well as a new, more comfortable respirator of the canister-attached type. But I'm not sure how reliable that information is.

27) 10/32: An earlier draft had the "fuller explanation" Bay wants, but we cut it out to preserve overall balance. Is he getting at anything in particular?

28) 11/1st para: If we do want to address "the tremendous logistical burden", we could do so at page 12 line 1 by adding a couple of sentences. But is it not self-evident?

- 29) 11/24: I'd be happy to replace "surging" by "moving".
- 30) 12/1st para: Has Bay any figures which would describe the Soviet horizontal NBC manning to compare with our implicit ca 100,000 for US forces ?
- 31) 13/29: Is Bay deliberately missing our point here (that the availability of competitive weapons will act to reduce both Soviet incentives to resort to CW and the military sense of retaliating in kind)?
- 32) 14/8-11: A comment we can rightly ignore.
- 33) 14-15/linking para: Maybe we should stress our point still more. Environmental transport and persistence of CW agents inevitably means that collateral-damage probability from CW will inevitably exceed that of conventional weapons, other things being equal. We could say that this puts CW into the same category as TNW.
- The underlying assumptions are those of the downwind-hazard illustration which, as far as I'm concerned, Bay is welcome to see. It would be interesting to know what his underlying assumptions are in thinking that what we say "appears very unrealistic".
- 34) 16/1: It's interesting the way Bay distances himself here from the draftsmen of FM 100-5.
- 35) 17/1st para: In his eagerness to attack our counterarguments, Bay has evidently not read this paragraph very carefully. Contrary to what his comment suggests, the arguments presented are not in support of NATO having no retaliatory capability at all. Indeed, the sentence spanning pages 16 and 17 explicitly admits possible war-fighting value for a NATO CW retaliatory capability. Rather, the arguments are directed against like-with-like CW deterrence. The reasoning is that if the existence of a retaliatory capability is to deter CW, the retaliation which it threatens must (because of deployed, alerted protection) be escalatory, in which case the capability may either be otiose or provocative. These are not arguments for unilateral CW disarmament: they are arguments against expanding what we have.
- I'm not sure that's the way we planned it originally, but I'm happy with it. If we wanted to make a case for unilateral CW disarmament, we would have to move onto a different plane of analysis: one in which longer-term considerations such as assimilation and proliferation could be taken into account as well as the shorter-term military-cum-strategic considerations. But our article isn't the best place for this.
- 36) 18/3: The basis for the "billions of dollars" is the SRI study and the estimate given by (?)Herrington during the American Academy meeting. Costs of demilitarization included.
- 37) 18/6-9: Is this comment inspired by a genuine concern that, without binary production, obsolescence and deterioration will impose de facto unilateral disarmament ? If so, and if his concern is justified, we ought to be responsive. I have tended to assume that Army spokesmen have exaggerated the deficiencies of the stocks in Germany. Is there any way in which we can probe this without getting into classified domains ? There were GAO studies a couple of years back which apparently went into it all: are they still secret ? The unclassified abstract that was released certainly suggested that the Army had been exaggerating. And what of all the money now being spent on rehabilitating unserviceable chemical munitions ?
- 38) 18/last para: By taking exception to "greatly improved", Bay is suggesting that the current billion-dollar CW defence upgrade programme will not provide a great improvement over the pre-1974 posture. Is he then suggesting that no

amount of money and effort can significantly improve protection ?

I wouldn't object to dropping the "sharply".

39) 19/5-11: Bay seems to be saying here that unless NATO has a capability for putting the Russians into rubber suits, its overall defence strategy will surely fail. No wonder the Cml C is unpopular.

40) 20/23-26: we ought to find out why Bay is so disparaging of the efficacy of antichemical defence. And why, moreover, there appears to be such a difference between US and British military opinion on this (or is it that I only get to talk with the wrong people?).

41) 21/1st para: What Bay has in his own paper on precedent-setting was about the only part of it I could agree with. But do we really owe a discussion of it to our readers ? As to his second point here, an agreement would surely be acceptable only if the associated risks were acceptable. Is he trying to put us out onto a limb ?

42) Downwind hazard illustration: As I say, he's welcome to a copy of my workings for this (as amended). If he is suggesting that sarin bombs have not yet been qualified for Phantoms, why are MC-1s listed in the F-4C flight manual ? Was DTC Test 69-14--which were MC-1/F4 trials, noted in the CB Obligations Report to Congress for 2HPY73--a failure ?

43) I started to write up a detailed critique of Bay's paper but it took too much time. Would there be any point, do you think ?

44) It's a bit much, I must say, for him to profess in his letter to you a sensitivity to hyperbole and the use of 'scare' words while at the same time launching that piece of ~~defamatory~~ defamatory invective (pp.26-28) against the likes of me. What did I do to upset him so ? We got on very well together at first.

45) The only bit of his paper I feel might benefit our article is the first sentence of the 68th footnote (p.49), on nonproduction verification: "In the opinion of experts, there are ways of assuring that single-purpose chemical agents are not being manufactured". I take it he means something other than tamper-proof seals: presumably, in view of the context, some sort of national technical means. Do you know what he is referring to ? And would it really be applicable to nonsupertoxic single-purpose chemicals, such as the binary reactants DF and QL ?

Your telephone call

46) The basis for my sentence about near-zero degradation of aircraft-turnaround performance is the enclosed article by "John Marriott" (pseud.) in NATO's Fifteen Nations (22(3):51- , 1977) at page 64, as amplified in an off-the-record conversation with a well-informed official. As you will see, this is an optimistic sort of article as regards the feasibility of antichemical protection. Maybe Bay would say that its author was selling a British bill of goods.

47) As for other material on the efficacy of protection, I haven't much. Most of my own impressions come from private conversations with CDE Porton people, who very rarely venture into print on such sensitive matters. One pertinent exception is enclosed: a short history of CDE put out to mark its 60th anniversary: note the last paragraph. Also enclosed is another article by Dodd. Plus a recent press item and the original copy from which New Scientist took it.

48) A copy of the last Pugwash CW Workshop report is enclosed. The next Workshop is scheduled for East Germany next Spring, to include a site-visit to a pesticide factory. The East Germans suddenly called it all off in August, but have now changed their minds again.

49) As to questions you might ask on December 10th, several possibilities arise out of some of the things I've written above. Others:

- a. How do Europeans feel now about taking in binaries ?
- b. What has come out of the LEACHE (long-term effects of antiChEs) project at Edgewood, referred to in an Obligations Report a few years ago ?
- c. Is CAMDS on stream yet at Tooele ?
- d. What does the TAB-followon antidote consist of, and how good is it against soman ?
- e. Is West German policy on CW still as set out in the 1970 White Paper ?
- f. What of the French chemical-weapons capability ?
- g. How confident are they in the new binary 155 round, now standardized without full testing ?
- h. Why do they think the Soviets have soman ? (Tabun poisoning is also refractory to pralidoxime; pinacolyl alcohol is not easy to make; &c)
- i. Is EA 5365--the principal candidate for the Intermediate Volatility Agent binary munition now in development--really a member of a new class of OP antiChE ?
- j. What is the logic behind the Highly Persistent Agent programme referred to in the latest Arms Control Impact Statement on CW ?

I expect I'll think of some more.

50) Meanwhile I await Geo Berdes' comments with some trepidation.

As ever,

Julian

J P Perry Robinson

encs.

HARVARD UNIVERSITY
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16 DIVINITY AVENUE
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4 January 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
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ENGLAND

Dear Julian,

After receiving comments at DoD and another re-reading, here are some more suggested changes. Reject them or re-write as you see fit. The following notes include some related questions and explanations.

Page-Paragraph

- 1 - 2 The last sentence may suggest more of a deadlock than exists.
- 3 - 1 Mustn't we somewhere take note of WWI chemical warfare?
- 6 - 1 In view of uncertainty regarding stocks in Germany, we should use a less specific description. I was recently told that the total chemical stock in Germany is 47,000 tons of munitions. I will try to check this.
- 6 - 2 It seems unsuited to Scientific American to give Ruhles' estimate without knowing how it was obtained.
- 7 - 1 I understand that there is no good evidence for any continuing buildup of Soviet chemical weapons. Do you have any information on this?
- 7 - 1 In view of Dashiell's comment, shall we remove the implication that the U.S. seeks a successor to the 115-mm rocket system? See enclosed conversation notes.
- 10 - 2 Can a word be added to "oximes" to include pyridostigmine and physostigmine by implication?
- 10 - 2 See enclosed notes regarding therapy for GD.
- 12 - 3, 13 - 1 If you think that in spite of Dashiell's disagreement we should keep the substance of this, we should try to remove the implication that qualitative improvements were needed only to encourage assimilation and not for their own sake

Page-Paragraph

- 15 - 1 The 50 km figure given in the text may seem at variance with the suggestion given in our illustration that fatalities might occur out to no more than 20 km. The term "serious incapacitation" is rather vague but see figure legend revision. I am concerned that Fisher at page 83 of the WHO report finds $Ct = 100 \text{ mg-min/m}^3$ at only 1.5 km of his finite 4 ton GB source. Our 5.7 ton point source gives this dosage at about 12 km. I question whether Fisher's use of Pasquill's curve relating D to h is valid for the vertically extended source he assumes.
- 15 - 2 Has Britain renounced the use even of U.S. chemicals?
- 15 - 2 Should the substance of this paragraph be transferred to the last paragraph of page 17?
- 16 - 2, As you point out in your last letter, we are presenting the
17 - 1 argument against like-for-like deterrence, not that for NATO having no chemicals at all.
- 17 - 2 Is there evidence that "The leading NATO allies have made it clear..."
- 18 - 1 Lennon's figure of \$300-400 million to weaponize the bulk stocks would presumably add about 50,000 tons of modern chemical munitions to the stockpile. I guess the same weight of binaries might cost twice as much. Even with inflation and overruns it seems our figure is high--or else Lennon's was low. But restore the passage if you think it desirable.
- 21 - 2 Now that the CTB talks are badly snarled and the CW talks are looking up, the reverse of our statement may be nearer the truth. Larger issues now seem to eclipse the influence of CTB.
- 21 - 3 Should we delete this paragraph?

The bases for a few other changes ^{I have not made but which may be desirable} are contained in the enclosed conversation notes. Please do not cite Dashiell or me. Scientific American will send us their comments on 15 October Rev2 in about ten days. We can then combine all the changes. Incidentally, did SA reimburse you for the second (California) trip? Please let me know. At a minimum, Afghanistan will certainly sour round eleven. Damn.

as ever,

Mat

Encls.

CONVERSATION NOTES

Of Dashiell on his penciled comments to our 15 October draft

Page

- 5 BZ was never standardized, should not use words "approved fill." Munitions were filled, however, and still exist pending disposal decisions. BZ is obsolete and T.D. will provide the date.
- 7 "Modern delivery systems" for which our chemical weapons are not well suited are extended range artillery, tactical missiles and forthcoming aircraft such as F15 and F16. The MCI is compatible with existing aircraft such as A7, F4 and A10. The artillery projectiles can, of course, be used with the ER tubes, but only at ranges below those of which the tubes are capable. Multiple rocket launchers are not thought very useful for chemicals and replacement for the 115-mm Bolt system is not being sought. It is considered that such launchers are suitable for area attack using non-super-toxic agents but that they are not needed for nerve agent. It is thought that the Soviet weapons of this type are useful for agents like HCN. For air delivery, Bigeye would be much superior to Weteye.
- 8 All U.S. Army forces in Europe do not have the MK3. There have been two large purchases of the U.S. suit since then and there are now more of the U.S. suit in Europe than there are of the MK3. The U.S. suit is only water-repellant, not flameproof and waterproof. The outer layer is woven nylon-cotton. Unlike the MK3, the U.S. jacket can be opened in front. It has greater lifetime in toxic atmospheres and can be worn in contact with skin without irritation problems. Saturation of charcoal by hydrocarbon air pollution near motor pools, etc. is not a problem. (F. Frank had said it might be.) The U.S. gloves as well as the overboots are of butyl-rubber.
- 12 This refers to obidoxime being effective against GD.
- 13 The U.S. did not (and does not) have enough of such equipment, including masks. The 1983 date cannot be met. The XM29 mask is still in development due to problems with permeability of the face piece material. A new designation, XM30, is used for the concept now being pursued--two pieces of different transparent material molded together. Can't give a date for first production. Still, the basic design of the XM29 is thought to be excellent and will be kept in the XM30. For the time being, more of the M17A1 are being produced.

Page

- 13 Thinks last two sentences misleading in that the U.S./NATO antichemical protection goals are well beyond, not just "up to", those of existing NATO.
- 14 Agrees that casualty production of HE and CW cannot simply be added together. Instead, chemicals exert their own special effects, such as slowing mission performance. These effects are added to the effects of HE.
- 20 Thinks at least ten years is more accurate.

OTHER COMMENTS

- 10 Considers that available studies show TAB to be about as effective against GD as against GB. "Several median lethal doses" is accurate for both. There is no approved plan to field pyridostigmine. It and physostigmine are being studied. One attraction is that they are already approved and well studied for medical use.

Will provide illustrations for our article.

Cannot talk about our specific stockpile estimates.

Notes on meeting with Thomas Dashiell and persons from the offices of the Undersecretary for Policy and the Assistant Secretary for Program Analysis at DoD. 12 December 1979.

We overrate effectiveness of protective posture. But the government lacks good data on this themselves.

We overstate progress on bilateral. Must distinguish bilaterals from CD, which must become involved before true treaty negotiation can begin. Tempo did pick up at round ten and makes round eleven look promising.

Some increased willingness to discuss CW exists in European military circuits. The shift is not massive. We are probably right that FRG will register distaste for more forward deployment. But they ought not to object to replacement with binaries.

Bilaterals lack time-table for progress. Only CD imposes the timing pressure.

A model, not yet reviewed, indicates there are situations in which Soviets win the war if we cannot retaliate with chemicals. But this may assume only the then present protective posture.

Protective equipment procurement does not faithfully follow for dollar outlays because much of the money goes to create a new production base.

The CW munitions renovation program involves exterior cleanup, painting, testing the agent. There is no problem of nerve agent stability. Contrary impression is "figment of the imagination."

The Soviet buildup started in the late 1960's. There is no present surge, just maintaining. Big Soviet organization is primarily defensive.

U.S. stockpile is 30% bulk. Much is not usable, firable, employable. The stockpile is much less than we imply. (But I am not sure that the distinction was understood between weight of munitions and weight of agent.)

Notes on conversation with Dr. Ralph Goldman, U.S. Army Institute of Environmental Medicine, Natick, Massachusetts. 4 January 1980.

Goldman has made a very detailed study of heat balance in protective clothing under various conditions. He feels that it is incorrect to state that "...it interferes little more than ordinary clothing..." The main reason for this is that the protective suit would ordinarily be worn over existing clothing rather than only over underwear--thus an additional layer. Although our words are therefore more or less correct, two layers of ordinary clothing are effectively what a man in the protective suit is wearing. In addition, due to its close weave and water-repellant treatment, the suit has reduced permeability. He prefers to use dry bulb temperatures because evaporative cooling in the suit plays a relatively small role. He suggests that 75° would be somewhat more accurate than 80° in our sentence: "But above X° F periods of heavy exertion must be limited..." He would agree with the rest of the sentence including the reference to removing the gloves and unzipping. The British suit is thinner and therefore slightly cooler--he estimates about 2° F difference in the temperature causing equivalent heat stress to wearers of the different suits. The British suit, however, has less charcoal, would last less long in a toxic atmosphere and is somewhat less resistant to wear. He will send copies of some quantitative studies.

Notes on Talks with Chemical Officers and Others

New FM 21-40 is being drafted for internal comments. It has new MOPP definitions. Temperatures are wet bulb, hence lower than ordinary temperature. Cool is wet bulb 50-70°; warm 70-85°; hot 85-100°. MOPP one is overgarment, unzipped in front.

two adds overboots

three adds mask and hood

four adds gloves and zippers up, also has body armor.

At MOPP four, warm, heavy work may be conducted nine hours if broken into 30 min work-30 min rest cycles. Light work done at MOPP four in warm weather can go on for 15 hours with 10 min rest per hour. The mask and hood have more effect on perceived fatigue and on calculated heat loss than does the suit. If work rate is low, it is reduced 5% by MOPP 3 or 4. if weather is cool, 10% if warm and 20% if hot. For heavy work rate, the reductions are 25%, 35% and 50%. It is okay to say, as we do, that "At temperatures usually encountered in central Europe, the degradation of combat performance in full protective gear due to heat stress is minimal. But above 75-80°F periods of heavy exertion must be limited to about an hour-- or else protection must be..."

A study by Dr. Ralph Goldman gives a predictive model of performance degradation under various conditions. The motivation of men in actual combat may make performance better than Goldman concludes.

Air Force has serious vision problems with existing mask. USAF has purchased 350 Honeywell IDS gas ionization 27lb nerve agent detectors at about \$8,500 each. They detect 0.2-0.3 µg/liter. A lighter, cheaper Army version is being developed to be ready in about one year.

U.K. chloroprene gloves leak after about 2 hours, failing NATO STANAG of 6 hours, eventually 24 hours. Butyl-rubber less strong but does not leak. New butyl, chloroprene double dip on cotton is better. U.S. suits cost about \$32, boots \$5-6, gloves \$10. Visual acuity is poor in U.K. mask. Probably best fielded mask is Canadian.

Our estimates of total nerve agent in U.S. munitions is accurate.



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21 Jan 80

Dear Matt,

This is by way of being a postscript to my letter of 18th January: things I forgot about in the rush of trying to catch the post.

(1) In your notes on your 12 Dec 79 session at the Pentagon, you quote DoD people as saying that it is a "figment of the imagination" to suppose there is a problem of nerve-agent stability. Yet there is congressional testimony from 1975 to the effect that VX at Tooele has been showing signs of "accelerating deterioration" (I quote this on page 115 of your Carnegie volume); and there is a Berdes/Frank annotation of our 15 Oct (rev 2) draft about "allegations to the effect that the stabilizer in the bulk agents are rapidly deteriorating". I take it it was the latter that you had been asking about ?

(2) The same notes record the Soviet buildup--you meant of CW weapons, I take it--as starting in the late 1960s. Was this ascertained (your wording suggests it is 'confirmed' intelligence) only in the wake of the post-Yom Kippur upgrade of CW intelligence effort ? Presumably, to judge from European military-press reporting, there was some sort of intelligence on this prior to Yom Kippur, though maybe only of an inconclusive sort ?

(3) Still on those same notes: "US stockpile is 30% bulk": am I right in supposing (a) that this meant both mustard and nerve gases, and (b) that 'stockpile' subsumed all types of weaponized agent: usable, unserviceable, scheduled for destruction but not actually destroyed, inside and outside the United States ?

Note this though: when testifying on the FY 1970 defence budget, the then Chairman of the JCS (Gen Wheeler) spoke early in 1969 as follows:

"We actually have not too large a supply of what we classify as lethal chemical agents, mustard gas and nerve gas. The total comes to about (deleted). About (deleted) half of this is mustard gas, (deleted). The immediately usable portion of the stockpile, which is the portion that is in the filled munitions, comes to about 50 percent of the total supply".

So far as I have been able to make out from the public record--particulars following under separate cover--chemdemil operations since Wheeler spoke have consumed the following quantities (tonnes) of mustard and nerve gases:

Agent	Bulk	Weaponized	TOTAL
Nerves	1900 (20)	2000 (250)	3900 (270)
Mustards	2800 (40)	0 (300)	2800 (340)
TOTAL:	4700 (60)	2000 (550)	6700 (610)

(the tonnages given in parentheses referring to agent currently--no, at the time of the March 1977 Environmental Impact Statement--scheduled for destruction at CAMDS Tooele). If the current stockpile is indeed 30% bulk, and if the Wheeler statement of 50% bulk is comparable with what you were told, the figures in the above table mean that the total supply of bulk and weaponized agent in the present

stockpile, T tonnes, is given by: $0.3T + 4700 = 0.7T + 2000$; i.e. $T = 6750$ tonnes. In which case our estimate of 7500 tons for present stocks of weaponized nerve gas can hardly be "phenomenally accurate". Something doesn't square somewhere. Have you any means of checking my chemdemil figures ?

(4) Do you know what the June 1979 Detroit meeting of the Military Operations Research Society was and why Berdes/Frank should have supposed that Falter and Watson were expecting the Europeans to go for binaries there ?

(5) Enclosed is ACSFCR congressional testimony of March 1974 claiming quantitative adequacy of USAREUR protective-equipment supply, conflicting with what Dashiell comments. Maybe the criteria of adequacy have changed since then.

(6) On CW downwind-hazard/collateral-damage, have you heard any more about the S.D.Fair study for ACDA ?

As ever,



J P Perry Robinson.

enc: US chemdemil operations since 1969' (following under separate cover)
Almquist testimony in SASC FY75 9:4929.

OBSERVATIONS ON THE US CHEMDEMIL DATA

(1) When the currently scheduled operations at CAMDS Tooele are completed, the total number of M55 rockets to have been demilitarized over the years will be at least 118,000. Further supplies are known to exist on Johnston Island, where M55s alone occupy 33 igloos (19), and presumably also in West Germany.

(2) The four ocean-dumping operations of 1967-70 (CHASE VIII, X, XI and XII) appear to have been planned originally as parts of a series of seven, the projected CHASE XIII, XIV and XV chemical dumps being abandoned during the 1969 controversy over the M34s (3). The weight of agent dumped in CHASE X (60 tonnes of GB) was about one percent of the total weight of all the chemical agents which would have been included in the sea-dump planned for 1969 (20). Observe that the total quantity of agent disposed of during CHASE X and both phases of Project EAGLE is 4737 tonnes. Since there was no intention in 1969 to declare surplus the non-M34 GB materiel at Rocky Mountain, this suggests that at least some of more than 1000 tonnes (100 x 60 = 4737) of agent identified for disposal in 1969 has since been reclassified for retention.

Additional evidence on this: Apart from 21,108 M34 cluster-bombs (which were later destroyed in EAGLE-II) and the 418 coffins of M55 rockets (which later constituted the CHASE X cargo), the original 1969 ocean-dump plans included a total of 5311 1-ton containers of mustard located at Rocky Mountain, Edgewood and Anniston (2). This mustard comprised 2572 short tons of HD and 2214 short tons of H (21). A further 7332 1-ton containers of mustard, located at Pine Bluff and Tooele, had been identified for future disposal operations (2). The mustard destroyed during EAGLE-I constituted the entire Rocky Mountain stocks, namely 2214 short tons of H and 857 short tons of HD (7). Therefore, there currently remain undestroyed, despite their inclusion in the 1969 plans, some 1715 short tons (2572 - 857) of HD, constituting the formerly condemned stocks at Edgewood and Anniston: 2975 short tons of filled containers at Edgewood and 165 short tons at Anniston (24)—i.e. 1474 tonnes of actual HD at Edgewood and 82 tonnes HD at Anniston. Of the additional supplies of mustard identified for post-1969 ocean dumping, nothing is known from the open literature about those at Pine Bluff (which was a WW2 production site for H but not for HD (22)); but, with regard to those at Tooele, it seems that no code-H supplies of mustard in 1-ton containers are on hand for proof of CAMDS (16); instead items "which are currently in the serviceable stockpile" are to be used, and these are filled with HD not H (16). (Note: "storage condition code H applies to materiel which have been declared unserviceable or obsolete and identified for disposal" (16)).

So: were there subsequently rescinded plans to declare mustard obsolete ?

(3) The demil data enable estimates to be made about current US CW stockpile content when collated with other data.

In May 1974 DoD informed the Zablocki Subcommittee that its "research confirmed the almost 75 percent reduction in worldwide stockpiles since World War II when the worldwide inventory exceeded 100,000 tons" (24). On 9 May 1974 General Lewis of the OJCS had told the subcommittee that: "The stockpile has gone down. We now have about one-fourth of the agent we had at the end of World War II" (25). A volume of the official US Army WW2 history (22) records WW2 procurement of CW agents as approximately 146,000 short tons. The implication, then, is that the total US agent stockpile as of May 1974 somewhat exceeded 36,500 short tons (25% of 146,000). It seems reasonable to assume that Lewis/DoD were including in their 75% reduction figure those stocks that were, as of May 1974, scheduled for destruction but not actually destroyed. The 36,500 short-ton estimate would then be as valid for 1980 as it was for 1974, almost.

Early in 1969 the then Chairman of the JCS, Gen Wheeler, spoke as follows to the Senate Defense Appropriations Subcommittee, the gist of his remarks being repeated by several other DoD witnesses during subsequent testimony on the FY70 defence budget:

"We actually have not too large a supply of what we classify as lethal chemical agents, mustard gas and nerve gas. The total comes to about (deleted). About (deleted) half of this is mustard gas, (deleted). The immediately usable portion of the stockpile, which is the portion that is in the filled munitions, comes to about 50 percent of the total supply." (18)

During the period intervening between the Wheeler and the Lewis statements, it appears that the total quantity of CW agent destroyed or scheduled for destruction --namely the agent destroyed during demil operations (3)-(6) above--amounted to 7316 short tons. From this it may be supposed that the total CW agent stocks at the time of the Wheeler statement amounted to not less than 43,800 short tons (36,500 + 7316).

Late in 1973 the JCS provided the following statement for the record of the House Armed Services Committee:

"...during the period 1968 ("2½ wars") to 1973 ("1½ wars") the United States nerve agent stockpile was reduced 17.5% (this includes M-34 clusters located at Rocky Mountain previously approved for destruction but does not include the stocks at Rocky Mountain Arsenal whose destruction was approved by the National Security Council on 3 October 1973." (17)

On the assumption that the 17.5% reduction was constituted by chemdemil operations (1), (2), (3) and (5), in which at least 2623 short tons of nerve gas were destroyed, the nerve-gas stocks prior to 1968 would have constituted at least 15,000 short tons. On the alternative assumption that the 17.5% reduction referred only to the agent of chemdemil operations (3) and (5), namely 2152 short tons, the pre-1968 stockpile would have been about 13,000 short tons. Note, however, that this range of from 13,000 to greater than 15,000 short tons seems too low to jibe with the estimate of total GB/VX production derived by Robinson (26) from DoD congressional testimony and the MRI/ACDA history of US nerve-gas facilities, namely 22,500 - 28,400 short tons. Possible ways of reconciling these divergent estimates include assuming (a) that some 10,000 tons of the original GB/VX production were consumed in field trials or undisclosed demilitarizations, or (b) that the 1973 JCS statement referred only to nerve-gas stocks located within the United States, or (c) that the reduction referred to included not only the nerve-gas stocks consumed during chemdemil operations (1 or 3)-(5) but also further, undisclosed, supplies that had been categorized out of the War Reserve for reasons of munition-obsolescence, unserviceability, &c.

For several months from late 1977, two US newspapers were embarked upon investigative journalism into the security of US chemical and nuclear weapons storage locations. In one of the many resultant articles there is this passage:

"...congressional sources have said the Army has more than 3 million nerve gas bombs and artillery shells as well as about 4000 tons of nerve gas in one-ton bulk containers. In addition, the Army has a very large inventory of mustard gas." (27)

No additional information seems to be available with which to judge the reliability of this report or, in particular, the likely accuracy of the 4000 ton figure for bulk nerve gas. However, during July-August 1973 newsmen ~~had been~~ ^{had been} given access to the Tooele depot: their subsequent published accounts and photographs (28) can be interpreted (29) to suggest that there could have been upto as much as 6000 tons of bulk GB at Tooele. The 4000 ton figure therefore seems plausible.

So, if the 4000 ton figure is inserted into the appropriate slot of the following matrix, and if the 36,500 ton figure derived from the Lewis statement



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24 Jan 80

Dear Matt,

Herewith the notes about US chemdemil operations I mentioned in my letter of 21 Jan.

Preparing them led to me to look again at the bases for the US stockpile tonnage estimates we give in the Scientific American draft, so notes on these are included as well.

As you will see, the estimates are really very tenuous.

As ever,

J P Perry Robinson



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28 Jan 80

Dear Matt,

On the matters outstanding after our mammoth telephone conversations on 25-26 Jan: a check-list of things you are going to do or consider:

p.14: Date of obsoleting of BZ. Note: I imagine that the process of declaring a standardized item obsolete requires much paper- and committee-work, so that by the time an item is formally obsolete it may in practice long since have become nonoperational. For example, according to the Brophy, Miles & Cochrane (1959, pp68-69) volume of the official US Army in WWII history of the CWS, lewisite was not obsoleted until 1955 even though the decision to close down the lewisite plants had been taken in 1943. It could be, then, that BZ has not yet been declared obsolete (though Bob M implied to me, back in 1976, that it had been) even though all stocks may have been identified for disposal.

p.19: Are we to go on suggesting, as we now do in the last line, that the GSRS MRL system will not have CW warheads available? (You had had the impression that the whole GSRS project, a collaborative US-NATO one, was on the skids, not just the binary GSRS warhead).

p.20: Whether to retain lines 4-8 or to delete them (i.e. is binary Lance-warhead R & D still alive?) The reason why I am quite keen on retention is that these lines are about the only place in the article where we indicate that the USA has a programme of CW weapons R & D. There is quite a lot of press comment that seems predicated on a belief that weapons R&D stopped when production stopped, back in 1969. As it happens, development of the E-27 CW warhead for Lance, which had begun around 1962, was indeed curtailed in 1969 (according to congressional testimony); but my understanding is that, at that time, a decision was taken to abandon all non-binary munition R & D. The reference in the last ACIS to flight testing of a binary Lance warhead suggests that the E-27 project was simply revamped as one of dozens of binary projects.

p.43: Has Canada, like Britain, renounced CW weapons only for the time being? If so, has she also renounced the option of seeking access to US (or French) supplies?

p.47 & 50-51: Do the anti arguments need expanding? The pro arguments (pp44-7) concern "NATO maintaining/having a chemical retaliatory capability". But the anti arguments (pp47-50) address only "the deterrent value and the de-escalating/escalation-damping potential claimed for NATO's chemical weapons". Only in the sentence beginning three lines from the bottom of page 47 do we comment on the war-fighting value to NATO of having CW weapons, and then all we say is that they "might or might not" have value.

For the reason outlined in para 35 of my letter to you of 27 Nov 79, I don't think we can usefully add to the anti arguments at this part of the article. But we could, as you suggested, towards the bottom of page 50: by inserting a sentence or two which alludes back to page 49 and forwards to page 53 (lines 4-6), making explicit the two main European worries about CW--decoupling and imposition of WWI-meatgrinding--previously left implicit.

On the assumption that we do indeed replace "sign of a diminished resolve to use nuclear weapons" (bottom of page 49) by "sign that even in the event of massive Soviet conventional and chemical attack, NATO might refrain from using its nuclear weapons", the following could go in after "military capabilities." at the end of the first para on page 51:

'At a more fundamental level there is fear that, if an expanded chemical retaliatory capability were ever used, the consequence would be immobilization of battlefields and reversion to the meatgrinding tactics of World War I: a horror that is deeply embedded in European consciousness, the recurrence of which nuclear deterrence is relied upon to avert. US pressure for expansion of NATO chemical forces on grounds of preserving the nuclear threshold stands to be misinterpreted as a weakening of the foundation of that deterrence, namely commitment of US nuclear forces.'

The words "and concern for the hazard to civilian populations" could then be deleted from page 50, lines 6-5 from bottom.

Actually, rereading this, I'm not sure it's much of an improvement. Maybe you can tighten it up.

In haste to catch the post,

As ever,



J P Perry Robinson

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1 February 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Julian,

Here is a copy of the text and figure legends we sent to Scientific American today. I believe there will still be a chance to make limited changes in proof, so let me know of any revisions you want.

I was told yesterday that still another function for the TMS-65 is the production of smoke by introducing petroleum into the air-intake.

~~an essay~~'s letter to Scientific American I submitted a request for reimbursement to you for your Laker flight to L.A. (\$409) and a telephone call you made to me in January (\$39).

~~And~~as ever,

Encls.

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6 February 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
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Dear Julian,

These photographs, recently taken at the U.S. base in Ramstein, belong to NBC News. I thought you might like to see them before they are returned. Please send them back within a week.

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6 February 1980

Mr. Julian Perry Robinson
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Dear Julian,

Scientific American thinks that copy ~~66r~~ final corrections will be available for one day during the week of 18 February.

If you want to say something about extended range projectiles, it might fit after "spray tanks" on line 12 of page 5.

After making inquiries, Binan informed me that Canada has renounced chemical weapons of its own and also has renounced chemical weapons from any other sources ~~as~~ well. Should we remove the words "from the time being" from page 15?

Scientific American may substitute a photograph of the U.S. suit for the present Figure 6.

In the sixth line of the legend for Figure 8 I am tempted to insert the words "...representing a few percent of the existing stockpile..."

I'll be away 10-16 February, returning the night of the 16th. I can be reached by telephone at home on Sunday morning, the 17th.

as ever,

encls.

110011501
= (also Seagrave)

WORDSMITHS

STERLING SEAGRAVE
4922 Van Masdag Court
Annandale, Virginia/ USA-22003

7 February 1980

Dear Julian,

Please excuse my long delay in writing. When I returned from England, I realized that the chemical warfare book would require undivided attention, so I decided to complete my other book first -- the book on mercenary pilots for TIME-LIFE Books. I became submerged.

Fortunately, that has gone well and quickly. I finished the book last week, with such excellent results that the editors are contracting for another one to be written in the summer, on bush pilots. How bizarre that after all these years of groping for encouragement from publishers, I should end up writing three books in one 12-month period. I feel as if I am setting up permanent residence on the end of a lit

There has been considerable progress with the toxin investigation, which I would like to report to you. I am no more certain about Palytoxin now than I was when we talked. It remains a candidate. The problem with Palytoxin is that it causes radical bleeding in dogs, but not in primates, and no examples can be unearthed of what it does to humans. I find that Dr. Paul Scheuer at U. of Hawaii chemistry dept thinks it very unlikely that Palytoxin (which he has analysed) could be responsible. I wish I could find records of how death came to Hawaiian divers who contacted the coral. So far no luck.

But, a totally different toxin has emerged as the leading contender: T₂ Toxin, or Trichlorothesene, which is a fungal metabolite that grows on rice and other grains. (Remember the Medieval rye bread that, when the rye went bad, the bread would turn-on whole towns?) A Russian town in 1947 was killed off, to the last comrade, by T₂ toxin, after which the USSR assigned a team of scientists in some remote lab to study it. So much is known historically. It now appears that a Cuban worker, handling material in a CW depot in Cuba recently died a horrible death in which he drowned in his own blood. His autopsy has been obtained, and all indicators point to death from T₂. This being the case, the corollary to Laos and Afghanistan blood deaths is obvious. The outward manifestations are the same -- vomiting blood, blood from all orifices, massive internal haemorrhaging. And in the case of the Cuban there was no vesicant present to cause tissue perforation and speed the bleeding.

I wonder if you can get a record of the autopsy of the "Third Bulgarian" (Vladimir Simeonov) who was with BGC in London on 2 Oct 78 when he "fell down his stairs" and "died as a result of inhaling blood from his nasopharynx." Then we could compare the Simeonov autopsy with the Cuban worker's autopsy, and see whether Simeonov was aerosoled with T₂. (This came right after the ricin doses were given to Markov in London and Kostov in Paris. Extensive autopsy narratives exist of those two over here, but not of Simeonov, presumably because no pellet or ricin were found in Simeonov, so his death -- albeit oddly coinciding with the other two assassinations -- was presumed to be from breaking his neck in a fall.)

Other Cubans apparently have died handling CW in Cuba, but I do not have details yet.

2.

I presume that such an autopsy of Simeonov would be available from Dr. Rufus Crompton at Wandsworth Public Mortuary, or from the British Anti-Terrorist Squad.

I have gathered most of the superficial data on the bleeding cases in Yemen, and only await receipt of particulars on one case that reached a field hospital. I hope to compare the autopsy there (if any) with that of the Cuban and Simeonov.

I now understand that reports of gas attacks in Laos continue to reach Thailand as recently as November-December. (I have a good friend at the USEmbassy/Bangkok, who is watching it closely.) The hunt is on for charcoal from campfires in villages that were said to have been attacked with chemicals. It has proved to be so difficult to obtain tissue samples because of the remoteness of the attack sites, that charcoal now seems an interesting tangent (it will be easier to get Hmong youths to go get it and carry it out in plastic bags).

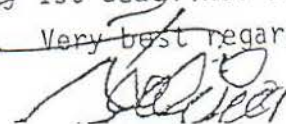
Meanwhile, I understand that the gas attacks by the Vietnamese on the Peoples Liberation Army during the Spring 1978 offensive may have resulted in the deaths of a whole battalion. I am trying to ascertain whether this is true, and what medical symptoms and signs were evident in the casualties (far fetched, to think the Chinese would part with this data, but I might as well try).

And, last week I learned that a platoon-sized unit of US Marines, in 1969-70 or thereabouts, all died, drowning in their own blood if you will, during an operation in Vietnam. I am trying to locate the details, and will then try to obtain their autopsies from the Surgeon General's office.

My preliminary conclusion is that the clearly identifiable death of the Cuban worker establishes beyond further doubt that a "blood agent" (presumably T₂ toxin or another toxin of the same general type) is in use; exists in CW depots in Cuba where Soviet CW provisions are kept; that this may be the cause of similar deaths from massive bleeding in Laos and possibly elsewhere.

A last note: Got a call from a reporter at the Portland Oregonian newspaper, who reports that three Hmong and one Mian male have died in the Portland area (out of a population of 2,000 Hmong) since April of inexplicable convulsions occurring in their sleep. Another five have since been reported from St. Paul-Minneapolis by the AP. Vang Pao claims there is no incidence of this among Hmong historically (as there is among some Filipino males, where it is called Nightmare Death). My friends in Thailand who have spent lifetimes with the hilltribes, agree. Apparently by Friday evening the count was up to 20+ deaths of exactly the same type in various parts of the U.S., and the survey was only beginning. The conjecture, of course, is that they are caused by delayed action of organo-phosphorous compounds used in Laos, but I gather there is no history of such delayed action from nerve agents or any other anticholinesterases. Might be a delayed action of a toxin, however.

I'll be writing daily on the CW book from now to May 1st deadline. If you can respond to this, I'd like to hear from you. Very best regards,





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29 Feb 80

Dear Matt,

Thanks for your note of 22 Feb with its attached copy of our SciAmer piece in its composited form. I'm glad, and very impressed, that you managed to stand firm against so many of the editorial quirks.

I think we can be pleased with the article: its structure and language has given a good groundswell to the component descriptions and arguments which carries the piece firmly in the direction we wanted--an achievement which is very much yours: I tend to get distracted by detail. Mind you, it is probably as well that we stopped adjusting the text when we did, as I think we were getting into falling returns. It's a pity, for example, that the pressure of time and space forced out any reference to British policy on CW weapons, and I'm not sure the 'meatgrinding' insertion ended up quite the way we had meant it to. The London Sunday Times--which is probably our best newspaper--is thinking of running a long piece on CW soon; should we give them an advance copy or is that properly left to Scientific American to handle ?

Enclosed is the draft of a piece on the recent allegations which I am keen to finalise and get to its intended audience--mainly British journalists and members of parliament--as soon as possible, hopefully by the end of next week. Any comments, amplifications, &c would be gratefully received.

I have decided, after all, not to write the piece for Nature (for which the original request had come from a news editor there, keen to build up Nature's defence/arms-control coverage, to which my outfit here has already contributed). The reason is perhaps foolishly petty. What do you think ? John Maddox, who has just returned as Editor after a spell as Director of the Nuffield Foundation, had as good as promised me, in his latter capacity, a two-year grant to write up the arms-control research we have been doing here at SPRU over the past several years. At the very last moment the Foundation decided against the grant, thereby leaving me high and dry and unsalaried. I strongly suspect that the reason for this was that our research here was felt to be insufficiently detached from concerns not thought proper for academics, which is a perennial hazard for anyone attempting policy-orientated research within British universities, especially so in the arms-control field where crucial data are hard to come by. But our focus on topicalities and current political alignments &c--which is what I think gives our work practical value, biased though I may be--makes us much in demand by journalists. We usually encourage this even though we end up being grossly exploited, for what good is policy research if its findings don't get into policy-shaping circles--another perennial problem in Britain, with its hermetic modes of government. So here is Maddox in one hat apparently denigrating our work and in another seeking to profit from it. Now that I've written all this down, I see that I see that I really

am being petty and probably maligning Maddox grossly. But the whole thing sticks in my gullet nonetheless, and anyway I should be putting all my efforts into finding a job. I suppose you wouldn't want to write the piece yourself? They want a one-page article (1000-2000 words) on what's been happening in CW for the 'News' section. I don't imagine they'll pay anything. Such an article ought certainly to be written. If you don't, maybe I'll stick to my old allegiance and do one for New Scientist.

What is to be made of the enclosed press cuttings? Are they reporting a move by DoD to reverse the OMB decision against funding binary procurement? Or are they simply reporting Congressional testimony in support of the binary funding that OMB did approve--which I gathered from you was for continued R&D plus planning work on a binary production facility? If any documentation has come your way on this, do please send me a copy. Likewise a copy of Bill Brown's article when it comes out in NYT Magazine.

Mary and I were watching the television a few days ago and were delighted to hear your voice coming from it, as background to some Granada TV World in Action footage on Agent Orange. I'm sorry to say that you were completely unattributed.

As ever,



J P Perry Robinson

ehcs

HARVARD UNIVERSITY
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8 March 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Julian,

Your draft on recent allegations, etc. is very impressive and, of course, urgently needed. I can make but a few possibly useful minor comments. When it's ready for distribution, may I have a copy soonest? Also, I hope you will send copies to Mikulak and to Mr. L. Craig Johnstone, Embassy of the USA, Paris, France. If for any reason you don't send both of them copies and have no objection to my doing so, please tell me. Craig is now or soon to be political/military officer at the US embassy in Paris and has some interest in these matters. I hope you will also provide others with your piece: Congressman Leach, Berdes, Forrest Frank, various journalists, etc.

My comments:

P. 4. I thought the foot/mouth development was connected with Poulenc/Murieux France, not a Swiss firm or is it?

P. 7. You might indicate why Marburg, Lassa and Ebola are very unlikely on technical grounds.

P. 8. Similarly, you might cite one of the "remarkable errors" in Moss' article.

Pp. 12, 13, 14. In Newsweek, March 10, page 59 the new Col. Watson, a likeable man named Col. Bobby Robinson, is quoted as saying the Soviets have capability for >30 days of extensive chemical war. It is the first number to come out of the DoD in quite a while, assuming, of course, that Newsweek got it right. Thirty days' worth is the lowest estimate ever to come out of DoD, so far as I recall.

Pp. 12-14. Notice Newsweek on March 10 says 42,000T USS and 350,000T Soviets but this time it's "weapons", not agent. Their previous claim that these numbers are agents is indeed what is in Eifried (which we sent to you two days ago). The 350,000 tons figure should be given the lie, especially since it is published both ~~by Army~~ and by Newsweek: 3.5×10^6 tons of chemical weapons. Enough for 5-10 years of extensive chemical war... Eifried makes another imbecilic numerical

claim: that 3,000 Soviet tubes in one volley would, by implication, crack allied defense. Even so enormous a barrage over the 30 x 10 km area that would be involved would cause few casualties. If defenders had masked and very few even if they all fall instead into the "carrying but not wearing" category.

P. 20. Although taken "secretly" the discussion must have been evident to the Soviets, at least by the time of the DoD letter to Kastenmeier.

Pp. 24-25. Aren't holes in leaves and diarrhea also cited?

P. 28. 105-MM is a US but not a Soviet caliber.

P. 31. I thought the last sentence of paragraph 2 a bit gratuitous since the evidence for nerve agent is so flimsy. The sentence seems to give it undeserved credence.

P. 33. Safire named the Soviet officer: is that worth addressing?

P. 37. I thought the end a bit flippant for so thorough and penetrating a piece.

Next week I hope to send you Scientific American tear sheets. By all means give it to the Sunday Times or anyone you like. Wisnowsky says they welcome such advance notice.

I hope you write for Nature anyway. Their D.C. office says that Maddox hasn't taken over quite yet as editor. I'm badly over-committed, having accepted Bill Brown's invitation to join him in writing a piece for the NYT Magazine. I hope he and I can agree on its content.

In spite of the misinformation and hysteria, the possible damage may be averted if those making the decisions are given more accurate assessments. I will send the Scientific American piece out widely, accompanied with letters or conversations. I don't know many European officials and hope you can get to an appropriate group of them.

as ever,

Robinson

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13 March 1980

Colonel R. Robinson
HQ, Department of the Army
3c549
ATTN: DMMO-NCC
Washington, D.C. 20310

Dear Colonel Robinson,

This is the article by Julian Robinson and me that will come out in next month's issue of Scientific American. I know it would have benefited if I could have met and talked with you before writing it. I hope the opportunity arises to discuss some of these matters with you in the future.

Sincerely yours,

Matthew Meselson
Professor of Biochemistry

Encl.



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28 March 1980

Dear Matt,

Thanks very much for your letter of 8th March about my paper on allegations &c. You should by now have received a finalized version, which takes a few of your comments (which arrived too late) into account. I plan a second edition, to cover the early March Afghan allegations (are they any more firmly grounded than the January ones?) and of course the Sverdlovsk affair. It's a pity I had missed the Bild Zeitung pieces; do you by any chance have a copy I could have of the FBIS translation of them?

Your phone call had the effect of propelling me to Geneva for the last few days of the BWC review conference, which, after all, didn't end too badly. I enclose a copy of a piece I did for Nature on it all at their request: very truncated, I expect, it will be coming out in the 3rd April issue. You will see that I have overcome my qualms about Nature. I've also done them the enclosed piece on chemicals. This will appear, slightly shortened and unsigned, as a Nature editorial. It's topicality is rather heightened over here just now since Now! magazine has just come out with a piece saying that Britain is likely to buy binary munitions from the USA. BBC-TV Panorama, one of our better tele-documentary-current-affairs things, is working on a programme about CW, filming in April or May: I have been working quite closely with them. They will be coming over to the States: I have impressed upon them the need to talk with you, not only qua expert but also as a lead in to people in Washington they should interview. Survival-- the IISS journal-- has a piece in the offing, unfortunately by Brian Beckett, who was responsible for the Now! item I've just mentioned and is not very good. Stern magazine in Germany is working on a piece about the US stocks. And there seems to be a lot else besides. I've been advertising Scientific American widely. I'm just off for ten days in the South of France; I'll let you know further what's been happening when I return.

Is it at all likely that your West Point person--? Mike Brown--will let me have a sight of his CW paper you were telling me about? I have been working up some calculations of my own on the use of soman or HCN in breakthrough operations.

As you recommended, I have sent copies of my allegations paper to Leach, Berdes, Frank and Craig Johnstone, and gave one to Mikulak in Geneva. Otherwise the only other American that has it is Jim McCullough.

Why do you imply a figure of about 2000 tons per day per side for consumption of chemical munitions in a European war? Not that it looks unreasonable. I gather there are satellite photos of streets in Sverdlovsk being bulldozed.

As ever

Robin

J P Perry Robinson

encs



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17 Apr 80

Dear Matt,

I'm now back from France, and very glad to find so many things from you.

I am intrigued to know how you came upon the Posev piece; maybe the fact of your having done so should increase its authenticity in my mind. My understanding is that Posev is a Springer cum Radio Liberty organ which does occasionally get copy from the dissident run, as is purportedly the case with the Sverdlovsk piece. ~~But~~ All the Soviet-studies people I have talked to here view Posev with the deepest suspicion. Would you say that the Robert Moss article (London Daily Telegraph) of 11 Feb and the Bild-Zeitung one of 13 Feb both draw from it, either directly or, in one or the other ~~piece~~ case or both, via the enclosed Economist Foreign Report article of 23 Jan? Or are there quite different parentages involved, ones which perhaps also produced the Ma'ariv piece of 21 March? Since US officials now seem, to judge from the press, to be taking an even stronger line on Sverdlovsk, one ought to suppose that there is something a great deal more solid in the background. Why, incidentally, does William Beecher now write (Globe 29 March) that Omutninsk has, after all, been discounted as a BW production site? (The Merieux plant was at Zagorsk, wasn't it?) And why, for that matter, does Charles Corddry write (Baltimore Sun 29 March) that there are thought to be nine Soviet BW production/storage sites, in contrast to Beecher's original six?

My first thought, when looking over your Anthrax-USSR bibliogs was to find out from Zhores Medvedev who the very diligent research assistants were who had been helping him go through the Russian scientific literature in connection with his book on the Kyshtym radiological accident: maybe they would take an interest in the Sverdlovsk affair. But maybe you are following-up on the bibliog already. And one would certainly hope that others, with governmental resources at their disposal, would by now have beaten out a similar track. If I were doing it--and it's the translation problem that really puts me off--I guess I would start from your numbers 8, 40, 50 and 44 (taking in 30, to relieve a horrid curiosity). No 42 surely has to be BW-related?

I think I told you in my last letter that Survival (the IISS journal) was about to carry a piece on CW. Apparently I had this wrong, and IISS have now asked me to do something for them, as you will see from the enclosed letter. But although this is just the opportunity you and I were talking about a while back, I am reluctant to accept the assignment, for time reasons, unless no one else can be persuaded. Should I suggest Mike Brown (?) to them? Or maybe he and I could collaborate?

I've also been asked by the nascent London version of the New York Review of Books to do 2000-4000 words for them on the Afghan allegations, &c. This I find very

tempting, though again I don't really think I have the time. How is your NYT Mag piece coming along ?

I see that the US press has given some coverage to our Defence Secretary's recent statement on the possible need for a British chemical-weapons capability; enclosed are some pieces from the British press. I am reliably informed that all this is not to be taken too seriously: that Pym was making an essentially political gesture. Studies are certainly in hand, but at a low level of effort: completion dates, and hence any decisions, are way into the future.

Also enclosed is another Robert Moss story, *etc*

As ever,



J P Perry Robinson

encs

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11 July 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
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Dear Julian,

I have talked with Garry Piel and the German Scientific American (*Spektrum der Wissenschaft*) editors are definitely not going to run the article. The foreign editions always leave out one of the articles in the American edition and ours is the one they are leaving out this time. We are free to have it published in German elsewhere. The German Scientific American editors hope, however, that we will not submit it to *Bild und Wissenschaft*, their detested competition.

What I need to know is whether you will take the ball from here or whether I should. We could consider *Die Zeit*, the *Frankfurter Allgemeine Zeitung*, or, in Switzerland, the *Neue Zürcher Zeitung*. All of these, however, and publications like them would probably require us to rewrite, which I would rather not take time to do. Let me know whether you want to try to find a publisher or whether you want me to try from here. Naturally, I would prefer you to do it.

Do you know anything other than Glassman's data at the end of Brachman's article regarding probit slopes for aerosol infection of primates by anthrax?

as ever,



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18 July 68

Dear Matt,

Since your 'phone call last night, I've
called no Christie but got no response.
So I've written him a letter asking
him to check through my memorandum
of our May telephone conversations
from that it may surface in public.

Enclosed are some clippings about the
current CW scene in Britain. I'll
send more as they turn up.

As ever,

Julian.



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25 July 80

Dear Matt,

My best contact with the German publishing scene is on holiday so my efforts to find a German outlet for our SciAmer article are not going too well. (Who has the copyright, incidentally? If it's SciAmer shouldn't their agents be looking for the outlet?) What I have found is the enclosed piece in Stern for 17 July: which may undercut us. But I'll go on looking. I'd be prepared to rewrite as necessary.

Enclosed is a letter I've just had from Christie, which is self-explanatory.

Also a piece I wrote in a great hurry for The Guardian.

I'm going to be away on holiday in France from about 2 August (c/o Kaldor, 18 rue St Joseph, La Garde-Freinet, Var, tel (094)436089), with a brief sojourn in Frankfurt around 7 August--for this CBS programme on CW, to be interviewed by Mike Wallace-- and then to the Pugwash conf beginning 20 August in Holland. Back end of August.

Very anxious to hear how your sojourn in Washington went.

As ever,

J P Perry Robinson

encs.

Rob.

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28 August 1980

Mr. Julian Perry Robinson
Science Policy Research Unit
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Dear Julian,

I don't seem to have a copy of the final version of your 1979 paper, "Chemical Warfare Capabilities of the Warsaw and Atlantic Treaty Organizations: An Overview from Open Sources ...". "Could you send one?"

As EVER,

Matthew Meselson

R361

2 September 1980

Julian Perry Robinson
Science Policy Research Unit
Mantell Building
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ENGLAND

Dear Julian,

Sulzberger is right. The real issue is not whether or not to make binaries but whether the Army needs more chemical munitions. If the enclosed is anywhere near right, and the objective is only to place the WPO into protective gear, we have plenty. I would value your rapid comment on the estimate. Do you know how the Germans estimated their WWII requirement of 30,000 agent tons or how the US Army estimated its similar requirement in the post war period? If 2/3rds of it is artillery munitions, this is enough for about 100 U.S. WWII divisions firing 10% chemical rounds for a year. Maybe that's the logic, but why 10% Simple imitation of WWI plus a bit extra -- or something more elaborate? Another question, the Army may be using 3-5 gm/m² for VX. Have you run across such high estimates anywhere? Have you any comment on the value? WHO cites 0.5-5 mg/m², we cite 300 mg/m². Is the WHO value of 6 mg for the percutaneous LD₅₀ valid?

It would be extremely helpful, I believe to both of us, if you could come across before too long. Any chance?

As ever,



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cf Set 60.

Dear Nat,

I'm just back from France,
and am in the process of
writing to you. But I
thought you might like to
see them ASAP.

As ever,

Julian.



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8 Sept 80

Dear Matt,

I'm now back in my office after vacation and the Pugwash Conf. Thank you for the many things you've sent, not least that declassified Dorothy Clark paper. The final version of that 'Chemical Warfare Capabilities...' paper of mine which you asked for is in SIPRI, Chemical Weapons: Destruction and Conversion (London, 1980), pp9-56. SIPRI should have sent you a copy of this, but in case not I am sending one under separate cover.

Various matters:

1) At Pugwash I was very glad to have from Hiatt and Martin Kaplan an update of your news on the anthrax business. The principal purpose of my paper--a copy of which I sent you the other day--was to open the subject for a discussion that, hopefully, would lead towards a conference commendation of that specialist level meeting "possibly under Pugwash auspices". Enclosed is a copy of the Pugwash Council's version--i.e. the abbreviated one that gets public distribution--of the Conference final document. The rather feeble bit on page 4 reflects much compromise. The full final document--in its report from Working Group 1--has slightly more, including a suggestion of Pugwash assistance in gaining "more medical information". Bernie Feld will have a copy of the retyped final version: I don't yet.

The first Soviet comments on Sverdlovsk came in a special CBW working group, from Reutov. I don't think he knew much about the business anyway, and we had to spend a lot of time persuading him that it should not be dismissed as a propaganda stunt and that clarification really was important. In this we succeeded to the extent of him agreeing that "more medical information would be desirable"; but he preferred to defer further discussion to Working Group 1. When the matter was raised there it was Pavlichenko who took over from Reutov--the Academy giving way, presumably, to the KGB. His objective was to have Pugwash publicly dismiss the stories not merely as unfounded but also as untrue. In this he made much of Zhores Medvedev's New Scientist piece, which Martin had shown to Reutov. When he encountered opposition he became very heated, denouncing the stories as "crude propaganda" etc etc. On no other subject during the Conference, not even Afghanistan, did the Russians get so intemperate in their language. Which being the case there was clearly no hope for an agreed final statement that went very far (it was surprising, in fact, that the final statement went as far as it did). Some people did, nonetheless, continue to push for the meeting idea: Murph Goldberger of CalTech especially.

One quite widely shared view among the participants was that the correct stance of Pugwash towards the allegations should be aloof scepticism: a stance

ridiculing the idea of any state doing anything as absurd as making anthrax weapons in secret: to rush in with offers of assistance in clarifying the allegations would risk lending them credence

2) On CW at Pugwash, we were told of the final and definite decision of the DDR Pugwash not to host the next workshop. The decision has apparently been taken at the highest level of government. Both Leibnitz (overlord of DDR Pugwash) and Lohs were very apologetic about this, genuinely so. However, the Russians went out of their way to say, on several occasions, that they wanted the workshop series to continue, approving of an agenda on which the main item was the circes under which onsite inspection might be called for. They advocated Yugoslavia as the place for the next meeting with, interestingly, Czechoslovakia for the one after that, a suggestion which the Czechs present endorsed.

We did not spend much time on the CW-use allegations. Rightly or wrongly I reckoned it more important to focus efforts on Sverdlovsk. Perhaps we should have done more to advance the idea of an international scientific inquiry. But with that disgraceful State Dept compilation just published, I don't think we would have got anywhere.

3) During two different conversations Leibnitz seemed to be hinting that military factories in the DDR were manufacturing CW agents for shipment to the USSR. But he could have been referring to explosives or some such.

4) Had you heard that, as part of the recent Brezhnev troop withdrawals from the DDR, the bulk of a brigade of Chemical Troops (a Front-level formation) had been withdrawn ?

5) Can you get me a copy of the 'Freedom House' publication referred to in the enclosed Baltimore Sun (12 July 80) clipping ? Presumably it's a translation of the RM piece of 3rd July ? Also, are there any full translations around of the various Posev bits ?

6) At Pugwash was Dr Igor Mann who, from his decades of FAO/WHO/UNEP experience in Kenya and other parts of Africa, knows a great deal about intestinal anthrax. I gather you phoned him ~~back~~ in May. He is willing to turn out his own records, and the various official files to which he has access, for whatever clinical or epidemiological information about the disease he may be asked for, provided the request comes to him on reputable letter heading (the above would do). You have a copy of his letter to Martin of 29 May: is there anything more he might be specifically asked for ? He told me that he was shortly going to give a series of lectures in the USSR where he knows personally a number of the people who would presumably have been involved, at the medical level, in the Sverdlovsk outbreak.

7) I talked with Gottstein (overlord of the FRG Pugwash) about possible West German outlets for our Sci Amer piece. He was genuinely concerned, I think, and has promised to make inquiries. He is a good ally to have. I told him I would be prepared to do any revisions, updatings, etc that might be warranted, in consultation with you.

8) Have you a copy yet of that other DIA manual thing (on offensive capabilities) ?

Meanwhile we are waiting anxiously over here for news of the Congress' final action on binaries.

As ever,

Philian

J. D. Brown, Baltimore



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11 Sept 80

Dear Matt,

Thanks for your letter plus encls of 2nd Sept. Oddly enough I too had been working up a similar table of minimum requirements, primarily for my projected piece for the IISS's Survival (in regard to which you recall my question about your Captain ?Brown--possible collaborant with or substitute for me, and sight of that paper of his ?): but your approach is better than mine. Will you mind some plagiarism ? One reservation: might critics not argue that retaliation in kind limited only to targets within artillery reach--and may not your one-third estimate be over-generous--be insufficient for/overall degradation thereby imposed to be significant in redressing asymmetries, having regard especially to (a) Soviet successive-wave tactics and (b) tac air turnaround ? The premises on which your table is based could then admit a rationale for Bigeye procurement, when taken with the expressed unwillingness of USAF tac air to fly spray missions. (Somewhat in which connexion, have you heard anything to suggest that the recent London Times report of the projected UK-based cruise missiles being equipped with 30% nerve gas warheads is correct "?)

I take it that your ca 10 ha assumption for the area over which a WFO company might be expected to deploy is carefully considered--notwithstanding the company R_m of 250 metres (equiv to about 20 ha) used in the GMT described in FM 6-20 of 1977 ? In regard to which, incidentally, I have come across a piece which states that Soviet artillery doctrine takes a US platoon strongpoint as covering about 4 ha; the platoon R_m in the GMT is 150-200 m (7-13 ha).

I am afraid I have nothing on the manner in which anyone's CW requirements have been calculated in the past. Enclosed are some documents setting out the British, French and American (note the far-seeing eye of MI 2c) CW artillery requirements as of the late 1950s, plus intelligence appraisals of German, Soviet, Japanese & ditto. You will see that these are all expressed as percentages of total artillery capacity--which suggests to me that the dominant considerations in formulating the requirements, at that time at least, were more to do with prevailing user artillery practice than with effects to be imposed on enemies. Just as you are suggesting, in other words.

For your interest I also enclose a recently released Cabinet paper (CID 1465-B of July 1938) which indicates the main considerations that influenced EMC last time it thought seriously about CW rearmament. I have this courtesy of a PhD student--the author of the enclosed RUSIJ article--who has been burrowing in the Archives.

On VX required contamination densities, the WFO figure was a misprint: mg for gm. So far as I can recall, we based the 0.5-5 figures on a hint from you plus a

calculation involving the 6 mg LD50 (which came from a Swedish defence source on 'F gas') and a figure for the surface area of a man. David Fisher, I remember, remained tactfully silent. US Army figures are---I presume---set out in the old FM 3-10B: though classified this was nonetheless used, apparently, by a French official for the enclosed tables; in which observe the 15 mg percutaneous LD50 figure in Table 2 and the VX munitions expenditure data in Table 6. It was from the latter that I derived the 300 mg/m² figure for our SciAm piece.*

If one were ^{using} ~~using~~ VX firing tables ^{based} on a figure as high as 3-5 g/m², would one want to seek complete target-area coverage, as opposed to sufficient coverage to create, in the minds of the attacked, a high doubt about noncontamination---say 10% coverage ?

I would certainly love to be in America before too long, but it's none too easy getting away. Mary---to whom I am entrusting this for speedy delivery to you---may have some views ~~on this~~ which you could talk about with her.

As ever,

Julian

J P Ferry Robinson

Encs: As stated (viz WO 32 3663 07021; CAB 29 159 06107; RUSIJ 125(2):56-62, ~~and~~ 1980; plus CAB 4 28 05573; plus FAF 24: 449-475, 1969 (Ganas), extracts only)

And: Fin Times 10 June 80; New Statesman 22 Aug 80 pages 8-9.

*55 M23 mines would spread 262 kg of VX over the 1 km² surface coverte; 45 M55 rockets, 208 kg over the 0.7 km². Which works out at just short of 300 mg/km².



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Dr. M S Meselson
 The Biological Laboratories
 Harvard University

making
14 mg/m²
DM

Telephone:
 BRIGHTON (0273) 686758

9 Jan 81

Dear Matt,

Following your phone call on 7th January, here and enclosed is some information from my files which might possibly be of some use to you.

EA 3148 is an OP antiChE of some sort.

Maybe it would be worthwhile seeing if Hormats is still around. So far as I recall. I got to him originally through Jeremy Stone: Hormats having approached the FAS early 74 or late 73 to see if he could contribute to any CW arms-control endeavours. He knows all the US CW weapons development programmes during his period of tenure as director inside out (and much of the French too).

Edgewood Arsenal reckons that "systemically, VX is about twice as toxic as GB" in man.* So, with VX on his skin, a man will be at the brink of death by the time about half a milligram has penetrated. Craig et al (1977)--copy enclosed-- indicate degrees to which the rate of VX skin penetration in man may vary with environmental temperature and site of application: within a given time, the dose of VX absorbed from a cutaneous dosage may vary over three orders of magnitude for the temperature range X -18 to +46 deg C and for the cheek as compared with the forearm. After 3 hours at 18 deg C about 0.4 percent of dosages of VX applied to forearms (having "intermediate permeability to VX") had penetrated, as compared with about 3 percent at 46 degC. So, to get 0.5 mg through the skin of the forearm within a 3-hr period, some 125 mg would have to be applied at the lower temperature, or 17 mg at the higher. For the cheek (which has "high permeability to VX") the corresponding dosages would be around 3.3 and 1.6 mg respectively (7 mg at 2 degC, 14 mg at -18 degC).

The surface area of a man is about 1.8 m². At 18 degC against a target of prone naked men, the permeability of whose skin averaged out to that of the forearm, VX would have to be deposited at a density of at least 140 mg/m² to kill the men, assuming they did not try to decontaminate themselves before 3 hrs had elapsed. This must come quite close to being a specification of a minimum effective deposition density for VX.

No. This is only if we want LD50 by 3 hrs.

For t = ∞
4 1/2 g
dose is
7000 mg
For 0.5 mg
taken in,
need 12.5 mg
on skin.
or 14 mg/m²
in above
example.

I have some more relevant stuff somewhere which I will try to find.

As ever,

J.P. Perry

J P Perry Robinson

*Chem Abstr 81: 34040a: abstract of AD 770434/9GA, B P McNamara et al, 'Proposed limits for human exposure to VX vapor in nonmilitary operations'.

encs



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20 Jan 81

Dear Matt,

On the matter of Soviet masking doctrine, I have a long-standing date next week with someone whose job it is to know such things, and will probe--though without much expectation of getting very far. In the meantime I have been going through my files and have found only one Soviet paper which speaks of masking whenever there is incoming artillery fire, prior to specific gas alarms. A copy is enclosed. It is a CIA (I think) translation of an article from the Soviet General Staff journal Voyennaya Mysl': 1968(6): 67-74, Colonel A Markynov, 'Comprehensive protection of troops from weapons of mass destruction'. You will see, however, that the article advocates pre-alert masking, thus implying that it was not, at the time of writing, current doctrine. And that it may not have become doctrine even ten years later is suggested by the second Soviet paper I enclose: a UK translation of a question-and-answer piece from the Red Army journal Voyenny Vestnik 1978(10):113. This item seems to indicate that there is no army-wide standard operating procedure on gas alerting and masking.

Some recent press cuttings are enclosed. The Washington Post story you mentioned on the phone is, I think, off beam: British interest is waning. Observe the Times lead letter from the Conservative MP.

Also enclosed is a letter from Gottstein about possible German outlets for our Scientific American piece (now much in need of updating). I saw him in Paris last week and he is very willing to help with whatever we decide.

My hopes of being in the USA by the end of this month are being obstructed by teaching commitments, but I may yet get out from under. Planning for a longer stay later in the year is advancing, but is currently much complicated by some political ambitions which Mary is developing. I hear Enid Schoettle is now a vice-president of the Ford Foundation.

As ever,

J P Perry Robinson

encs



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26 Jan 81

Dear Matt,

Further to my letter of 20 Jan, I must tell you that my source on Soviet CW affairs professed scepticism about the Russians being taught to mask-up whenever there is incoming artillery fire. He cited, as I did to you, that VV piece. But he would look around more for me. He did remark that current NATO-wide practice is to mask not only on incoming artillery fire but also on arrival of low-flying aircraft. I wasn't clear whether this was done always or only under conditions of state Amber alert. Do you know ?

I enclose a copy of a piece which this person has written on CB defence for the next edition of Soviet Army Review Annual, due out in the summer sometime. Sorry about the scribbles .

He is convinced that the Russians value CW very highly. But whether this is a result of careful analysis or, instead, a gut reaction stemming from the nature of his working environment I'm not sure. The principal reason he gave for the Russian evaluation was the antitank defence suppression argument. From Soviet military writings, defector reports, &c he has the impression that the Soviets are, just now, enormously concerned about NATO antitank capacities which they see as the chief threat to Soviet battlefield dominance; ergo, ways of countering that threat will be highly valued.

As ever,

J P Perry Robinson

encs



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Dr M S Meselson
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Harvard University

20 March 81

Dear Matt,

It was good to see you. Gather you got back safely. Bob is stopping by this weekend. Enclosed are those of the papers I have to hand from the RRG/Dutch/Yugoslav coordinated work on new antidotes (there's a lot more somewhere); also one from Porton; plus some press cuttings &c. I am still working on a list for you of British officials known to take an interest in CW.

Would you care to lend me two things you spoke of; (a) Amy Hoeber's book and (b) the tapes of your CW session with Deutsch at the CSIA? If you did, maybe you could bring them with you to Geneva.

And have you any news of that AD doc with the new HCN estimates--AD A028 501/5GA?

As ever,

J P Perry Robinson

erics

Robinson
= (also Seagrave)

WORDSMITHS

4/19/81

Dear Julian,

The Bulgarians offered to help investigate the Markov murder, also. The presumption expressed at the time was that Sofia got such bad press after discovery of the ricin pellet that they tried to save face by disclaiming responsibility, and offering to "help find the real culprit." The suspected other motive, of course, was that Sofia would have the opportunity, during the joint investigation, to see what the British (or the Austrians) had on file about the exiles/emigres. I speculate that the assassinations are carried out by a special branch, so that most Bulgarian officials would have no knowledge, and would be justifiably indignant at such an assertion of guilt. However, before, during and after the Markov murder, the Yard's investigation went into the emigre community all over the Continent, with interesting results, I'm sure. First chance I get to catch my breath, I'll xerox my files on the Bulgars, and on T₂ (mostly material passed to me from Ft. Detrick, where a great deal of effort is going into the mycotoxins).

I really do not view the Simeonov death as other than a shot in the dark. It remains unsolved. The conclusion of the coroner's court was that he died because his heart stopped beating, basically. What I want to see from the medical examiner, is what burst loose to cause all the blood in his breathing tubes, in which he choked to death. That he choked to death aside, it would be quite remarkable for him to have so much blood in his bronchial tubes just from a nose bleed caused when he fell down stairs (there is no real reason to believe that he fell down stairs, in any case). Lots of curious little details involved, such as interesting marks on the body that I can't make heads or tails of.

Regarding Laos, and your earlier letter, I've just had another long chat with Matt Meselson about his theory of a "Super-Adamsite." There are many appealing justifications. The biggest drawback, it seems to me, is the lack of any signs of arsenical residues in any report over the past five years from any part of Laos, and certainly none on the leaves examined and about which testimony was given at the same hearing where Dr. Lewis spoke. Nothing on the leaves but the surfactant, lauryl sulfonate -- and no earthly reason why a surfactant should be present on leaves from a remote mountain redoubt in a place where nobody ever bathes, and certainly never uses soap. Tantalizing, all of it. But let's assume that it was a brownish micropulverized organo-arsine, an order of magnitude stronger than anything previously known. There were two types of lethal attacks, separate and simultaneous. One was brownish-yellow powder and did not necessarily cause remarkable bleeding, although there was often blood of the amount you'd expect with any violent chemical agent, sometimes more than at others. The other -- in the early days in Laos before all kinds of colors began to be used -- was a red powder or mist, and its most remarkable effect was spewing blood. The brownish

agent and the red agent were used together at times, the village being hit first by one, then the other. When this happened, one set of signs was visible from the onset of the brownish agent, another (primarily spewing blood) became apparent as soon as the red agent was delivered.

Interesting.

I will be very happy to put all this aside and go back to narrative writing. This long preoccupation in one subject is fascinating but exhausting. I have only one chapter remaining to write, plus a preface; the publisher seems delighted with all the others. Thank god.

By the way, T₂ apparently is quite easy to muck about with in the lab, making it possible, theoretically, to practically redesign it -- made to order, as it were. And there are all kinds of related toxins within this group, with varying effects on humans, and they extraordinarily synergize.

I'm chasing the Dakto platoon; thanks for the NYT date.

I approached the coroner's court through TIME-LIFE's bureau in London, I'm afraid, so the coroner ruled that it did not seem to be a serious need when it was for a popular magazine. I've now written directly to him, explaining the real purpose of my inquiry, and hope I will receive a copy of the inquest in the next week or so. I'll send copies to you.

Hope all goes well for you, and your family, and that this is a good spring in Sussex.

Best,



Sterling Seagrave

PS--About two weeks ago, 1st of April, I began to pick up strong reports that a "major announcement" will be made in May about Sverdlovsk. As you may know, the case had been reluctantly put to rest last year, leaving a number of nagging points unsatisfied. Around end of December, for various reasons, the case was re-opened for a fresh re-examination of all the data in the light of some new things that had come up. Now, people at four different agencies have told me "in a month" there will be a major disclosure or announcement or some such. They uniformly refused to reveal anything further, which in itself is oddly consistent. So, dust off your Sverdlovsk files, here we go again. So many of the people who have been involved in the Sverdlovsk brouhaha are such bunglers, dimwits, or people of perverse motivation, that the whole business can now only become yet more Gothic.

MATT KNEW ABOUT IT. HE HAS
A SECRET CLEARANCE NOW.

M.M. Kaplan

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

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21st July, 1981

Dear Matt and Julian,

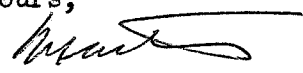
Bob Mikulak submitted the attached as a suggested agenda for the next CW workshop. He feels we should accept the Czech invitation to hold the meeting in Prague, and schedule it around the time of the CD expert group on chemicals meeting in Geneva.

Lundin and I feel that some sort of symbolic visit to a chemical plant in Czechoslovakia is essential, and we suggest stipulating that such a visit should be in connection with national verification procedures.

Franek and Lohs pushed for a meeting in Prague on "Consequences of a Chemical War", but I believe the revision would be acceptable to them.

I must take this up with the Pugwash Council in Banff, and would appreciate your comments.

Yours,



M.M. Kaplan

ENC

P.S. Enclosed is an excerpt from Haig's speech to the Foreign Policy Association on 14 July, supplied by Mikulak.

Professor Matthew Meselson ✓
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The Biological Laboratories
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Cambridge, Mass. 02138

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
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Falmer
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NINTH PUGWASH WORKSHOP ON CHEMICAL WARFARE, 4 - 7 March 1982
Prague, Czechoslovakia

A G E N D A

Discussion topics

- 1) Technical procedures for monitoring the destruction of declared CW stocks
- 2) Technical procedures for conduction on-site visits for fact-finding purposes in the event questions regarding compliance arise
- 3) Identification of commercial chemicals, including precursors, which are of particular significance from the standpoint of a CW prohibition.

Note: Discussions of topics 1) and 2) would deal with technical matters and not with the issue of which authorities (e.g. national or international) would be responsible for verification.

Visits

- 1) To a toxicology laboratory to learn about facilities and procedures for determinations of acute lethal toxicity (subcutaneous and inhalation routes).
- 2) To a chemical plant to determine procedures for verification by national authorities of non-production of chemical weapons.

Time: 4 - 7 March 1982.

Draft outline for next Pugwash meeting on CW

(Militar)

discussion topics

- 1) technical procedures for monitoring the destruction of declared CW stocks
- 2) technical procedures for conducting on-site visits for fact-finding purposes in the event questions regarding compliance arise
- 3) identification of commercial chemicals, including precursors, which are of particular significance from the standpoint of a CW prohibition

(Discussions of topics 1) and 2) would deal with technical matters and not with the issue of which authorities would be responsible for verification)

visit

to a toxicology laboratory to learn about facilities and procedures for determinations of acute lethal toxicity (subcutaneous and inhalation routes)

timing

first week of March 1982

HARVARD UNIVERSITY
THE BIOLOGICAL LABORATORIES
16 DIVINITY AVENUE
CAMBRIDGE, MASSACHUSETTS 02138

24 July 1981

Mr. Julian Perry Robinson
Science Policy Research Unit
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University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Julian,

My comments on the enclosed Issue Brief were written in response to an earlier version, not the published one which I am sending you.

As ever,

COMMENTS ON MAY 1981 "CHEMICAL WARFARE: BACKGROUND AND ISSUES"

Page 1:

- The statement that "binary nerve agent weapons would be significantly safer to store and use ..." is disputable. The hazard associated with unitary munitions has been exaggerated. Unitary artillery projectiles have not been demonstrated by experience or by risk assessment studies to constitute a serious hazard. Indeed, in some respects, such as the problem of liquid trailout and unexploded duds, binary rounds may be less safe.
- It should be stressed that the analysis of possible US chemical needs is in a state of rapid flux. It is important to realize that there are not any mission-based and validated requirements for specific chemical munitions. It should also be pointed out that there have been no congressional hearings that have explored the essential questions raised by proposals for resumed chemical weapons production.

Page 2:

- US policy on chemical warfare has not been consistent in calling for no first-use. In the late 1950's and early 60's, the US position became ambiguous. For example, in 1960, the Departments of Defense and State opposed a resolution proposed by Congressman Kastenmeier which called for reaffirmation of a no first-use policy (Chemical-Biological-Radiological (CBR) Warfare and Its Disarmament Aspects. Subcommittee on Disarmament of the Committee on Foreign Relations. US Senate, 86th Congress, 2nd session. Committee print. 29 August 1960). At about that time, US national policy was changed from "no-first-use" to "presidential option".
- It may be misleading to state "production of biological agents was continued only for defensive purposes." It would be more accurate to say research on biological agents was continued only for prophylactic and protective purposes.

Page 3:

- It is not correct to say that the "technical advantages of binary munitions seem to be accepted by both the supporters and most of the opponents ...". Binary munitions have a number of potential technical disadvantages, as compared with unitary ones. These include ballistic uncertainties and variability, problems of front-line assembly, distinctive sound signature, greater temperature dependence, and hazards associated with duds and liquid trailout, among others.

Page 4:

- "... significant civilian casualties would result ..." fails to convey the results of relevant studies which indicate that millions of civilian deaths could occur in a european chemical war. The word "significant" should be replaced with a more informative adjective, such as "millions of".

Page 7:

- Point #1 should be split in two: 1) there is no definitive evidence as to whether the Soviet stockpile is larger or smaller than the US stockpile; and 2) it is not known whether the Soviets have or have not appreciably increased their chemical weapons stockpile since the US stopped production.
- An important point that follows from these uncertainties is that proposals for enlarged US chemical weapons programs may stimulate a chemical arms race, to our long-term disadvantage.
- It is not clear that "the Soviets have been continuously developing their chemical capabilities ...".

Page 8:

- A significant factor that must have affected Soviet calculations was the major US build-up of chemical munitions during the period 1950-68, when we were one of the few major nations that still had not ratified the 1925 Geneva Protocol. This should be cited.
- The statement about forward deployment exaggerates the known situation.
- References to stockpile ratios need qualification in view of the uncertainty even as to whether Soviet stocks are greater or lesser than those of the US.

Page 9:

- It is debatable whether Soviet chemical capabilities are "the most extensive ...".

Page 10-11:

- The argument regarding adoption of rubberized garments by the Soviets makes little sense technically. It should be omitted.

Page 11-12:

- The discussion should reflect more closely the analysis given in the June 1980 DIA report on Soviet defensive capabilities. Their existing tanks are said not to have chemical protection. It is expected that their newer ones will, although this remains to be seen.
- Soviet chemical defense troops are estimated in the DIA report at approximately 50,000, not 80,000-100,000.
- I know of no reliable evidence that the Soviets routinely use diluted, lethal chemical agents in troop training exercises.

Page 13:

- While there is undoubtedly some emotion on both sides, I question your implication that it is the arguments against chemical weapons that depend mainly on emotion. More important, the argument for chemical weapons has not been subjected to the military analysis customary for other weapons for which procurement requirements are issued. Indeed, within the Defense Department, the arguments for chemical weapons have generally not been based on detailed military analysis at all.

Page 14:

- Contrary to the sentence at the top of the page, the US did develop a strong offensive chemical capability after WW II.
- The last paragraph fails to reflect that the stockpile requirement reportedly favored by the Joint Chiefs is likely to represent the upper limit of the available crude estimates for such requirements. The Joint Chiefs generally release requirements far in excess of eventual agreed requirements in many fields, such as conventional ammunition, number of aircraft wings, etc. I believe that our stockpile of chemical artillery munitions is quantitatively adequate, and that statements poor-mouthing it create a false impression and reduce its deterrent effectiveness.

Page 15:

- It is correct that US forces in Europe could not make use of 105 mm artillery projectiles. However, it is important to add that some European NATO allies do retain 105 mm tubes in considerable number and these are compatible with our 105 mm chemical rounds.
- You do not mention 105 mm, 155 mm, and 4.2 inch mustard munitions. It is wrong to neglect mustard; in overall effectiveness, it can be competitive with nerve agents.

Page 16:

- In real measurable terms, just what are the safety problems associated with storage and transportation of unitary artillery projectiles? Do you know of any sound evidence that there is any substantial hazard? As for disposal, new batch incineration methods now being considered could make the safe disposal of unitary weapons much cheaper.
- It is implied that binary weapons are superior to unitary ones because of "greater variety, increased safety, larger stocks, simpler handling procedures, and greater variety of deployment options ...". I do not consider that any of these points is well-established in regard to binary weapons of current design. What is the evidence?

Page 17:

- Deploying only part of the binary components does not address european concern regarding civilian casualties. What evidence is there that this "technological fix" addresses any real problem?

Page 18:

- The importance of the Yom Kippur war findings is exaggerated. The former president of Israel, Katchalski, says that all they found that was new to them were bigger blowers in the personnel carrier ventilation systems, and the antidote TAB. The tanks did not have chemical filtration, and we now know that our reaction to TAB was incorrect. The antidote is defective and may have been purchased by the Egyptians commercially rather than having been provided by the Soviets.

Page 21:

- I know of no reliable evidence that Soviet troops use actual lethal agents (such as nerve or mustard) in their training exercises.

Page 23:

- The words "distinct possibility of significant civilian casualties" understate the problem. "Millions of civilian casualties" would be more accurate.

Page 24:

- You should add that binaries are much easier to produce than unitaries because elaborate precautions are not necessary to protect the workers. Ease of production is an important advantage for others, but not for the US. It is against our interest to make chemical weapons production easier for others.

Page 25:

- The statement attributed to General Myers should be quoted exactly and referenced. Some analyses I have seen suggest that retaliation with chemicals instead of competitive weapons might contribute to losing rather than to winning.

Page 26:

- May I suggest the following wording for paragraph 2: "...his expertise on the subject of chemical warfare emphasizes that the wearer of chemical warfare protective equipment is much more vulnerable to conventional explosive weapons than to chemical weapons."
- The use of chemicals in WW I included heavy use of mustard against the allies during the last 14 months of the war. This provides some real world basis for evaluating the consequences of chemical war. When it is taken into account that mustard is quite an effective agent, that more than 3 million rounds were fired by the Germans at the western allies, that WW I protective equipment was greatly inferior to present equipment, and that conventional competitor weapons (improved conventional munitions etc.) are much more effective than WW I weapons -- the experience of WW I strongly suggests that battlefield chemical warfare would be of low military effectiveness in a European-type war of the future. Taking the trade-offs into account, it might be, in many cases, counter-effective.

Page 28:

- I do not agree that symptoms reported from Afghanistan suggest nerve agents. I doubt that any knowledgeable person would now consider nerve agents probable.
- I think the simplest explanation for the many effects, including numerous deaths, reported by Lao tribespeople, is that they were exposed to agent DM or a related compound, and also drank water in which the agent had hydrolyzed.

Page 29:

- Although the talks began in 1976, serious negotiations did not begin until the summer of 1977 when the US first presented a negotiating position.

Page 30:

- Whether or not US chemical weapons production affects the possibility of reaching a treaty, there is a different point worth adding, namely that such production may stimulate a chemical arms race counterproductive to us.



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20 Sept 81

Dear Matt,

Here is a bunch of things, including some items on mycotoxins. One, as promised, is a copy of that Bamburg et al (1969) review article I showed you when you stopped over in London last Spring. Then there is a couple of other mycotoxin papers I happened to have in my files and a reading list I've just done of more recent items to update the files. The only one of its entries I've yet managed to retrieve is the Austwick review, copy enclosed.

I had a call, on 17th September, from Sohrab Kheradi in New York. As you may recall he is the UN staffer for the CW Expert Group investigation. He said he would be in Europe for a couple of weeks, starting yesterday, and would I be free for a chat-- 'nothing specific', he said. Have you any idea where the UN group is at now, or what he might want? I've written a sort of feeler to Marcovich (copy enclosed) in the hopes that he may tell me something.

I must get that Dutch-Meselson tape back to you: sorry to have hung on to it for so long. You come over really excellently (as always). One thing that surprised me was your reference to six different USAF nerve-gas-munition requirements having been both stated and met during 1953-69. Would it be indiscreet of you to identify them for me? I know only of the following air munitions having been developed for nerve-gas fills:

- 750-lb GB bomb, MC-1
- 1000-lb GB cluster bomb, M34/M34A1
- 160-gal VX spraytank, TMU-28/B
- GB bomblet dispenser munition, CBU-15/A
- 500-lb GB bomb, Mk 94
- 500-lb GB splash bomb, Mk 116 Weteye
- 90-gal multipurpose spraytank, Aero 14B.

But only the first four of these were USAF munitions, and of these I had thought that only the first three had gone into quantity production.

Have you been able to think any further about the matters I put to you in my letter of 19th June?

As ever,

Philan

Dr Herbert Marcovich
Institut Pasteur
28 rue du Dr Roux
75724 PARIS Cedex 15
France

20 Sept 81

Dear Dr Marcovich,

It occurred to me that you might find some small use for the enclosed, which is a reading list I've just got together for myself on Fusarium mycotoxins. There's a good deal more in the literature besides, but the items listed seem to offer an introduction good enough for the likes of me--i.e. someone basically ignorant of the subject. Maybe there'll also be an item or two of interest to you as well.

Personally, I was astounded by the US State Department's announcement. If what it alleged is true, the implications seem so great that one feels obliged to assume that the State Department was very sure indeed of its ground before making the announcement. There would not only be the implication of violation of the 1972 BW Convention, as well as violation of the conventional and customary proscription of use of CW/BW weapons, but also the implication that manufacturers of CW-weapons were already exploiting biotechnologies for agent production--i.e. that the shift out of the domain of petrochemicals had already started. Maybe I attach more significance to the latter than it warrants; but it is certainly not irrelevant to the matter of CW nonproduction verification, to say nothing of the broader consequences of opening up the biotechnologies to weapon applications.

Even so, I find myself deeply sceptical of the State Department's claim. A year or so ago I had heard that a senior US Army Medical Corps officer who was working with the Defense Intelligence Agency on the reports of CW in Laos was speculating that a poison such as T2 toxin was involved. (Hence my allusion to trichothecene mycotoxins in that paper of mine for the Pugwash CW meeting you were at.) He was doing so because T2-intoxication seemed to him to be about the only way of explaining the haemorrhaging that some of the H'Kong refugees seemed to have been describing. Clearly he was attaching high evidential value to the output of that necessarily difficult and uncertain interrogation process. I am told that he had been struck, also by apparently similar allusions to haemorrhaging in certain of the intelligence reports that had come in from Afghanistan (I have no idea what the provenance of these reports was) and also in an old intelligence report he had found in the files relating to the Yemen CW episodes of the 1960s. But because it seemed so improbable that anyone who had decided to resort to poison weapons would have opted for such a difficultly accessible and, in quantitative dosage terms, such a relatively weak poison as T2 mycotoxin, this aetiology appeared ridiculous. (Unless, I suppose, the idea was to camouflage the CW attacks as natural outbreaks of mycotoxicosis).

So, like many other people, I am very curious to learn more about the analytical procedures which the Americans applied to their sample from Kampuchea, including their use of controls and precautions taken to exclude Fusarium contamination. Are such

particulars in the public domain yet, do you know? If they are, have you a copy I might have?

With best wishes,

Yours sincerely,

D

J P Perry Robinson

PS Since mycotoxins fall within the scope of the 1972 BW Convention, reports of their use might perhaps be held to fall outside the UN Expert Group's terms of reference, which are limited to chemical weapons. No doubt this thought has already occurred to the Group.

enc

Robinson

Institut Pasteur

28, RUE DU DR ROUX. 75724. PARIS CEDEX 15

TEL: 541-52-66

Paris, October 2nd 1981

Dr J. PERRY-ROBINSON
The University of Sussex
Science Policy Research Unit
Mantell Building Falmer
Brighton
Sussex BN1 9RF
U.K

Dear Dr Perry-Robinson,

Thank you for your very thoughtful letter and for the references enclosed.

The news about the Group of Experts are the following ; The choice was made on political basis only. The four experts are EZZ, whom you know, a specialist in tropical medicine from Peru, a children's orthopedist from Kenia and a colonel from Philippines specialized in explosives.

After the second session in July a report was issued which concluded that, in many cases, harrassants might have been used ; in some instances the symptomatology described was consistent with some kind of neuro-toxic and blistering agents.

As regards the State Department's allegations of use of mycotoxins I am, as you are, staggered. The official documents released contain little substantial information. There is a need for a clear description of the collected material, of the way in which the samples were selected and the time elapsed between the alleged attack and the sampling.

.../...

The finding of a mixture of three trichothecens toxins must be explained. T2-toxin and nivalenols are not produced by the same organism. In natural conditions, association of different fungies is frequent ; as a consequence, the mixture of toxins described by the State Dept. could occur in natural intoxications, To my knowledge, no search for organisms has been done in the analysed samples.

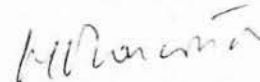
It is not easy to understand why, for a chemical weapon, such an association is advantageous, unless there is a synergistic effect between T2-toxin and Nivalenols. In addition, one of the arguments on which the assumption is based is that the reported symptomatology does not fit that which is described for conventional chemical weapons. However, in many cases, the symptoms do not fit either with what is induced by trichothecens. The acute intoxication with these substances is not known in man. Extrapolation made from data obtained from animals is very aleatory. Basic information on the sanitary condition of the region is needed. The argument that the observed symptoms did not fit with any known disease in the area, cannot be taken for granted without further inquiries, specially with consultation with experts from FAO and WHO. Immediate bloody diarrhea have never been observed with humans following trichothecens intoxication.

The use of mycotoxins has more to do with sabotage than with military operations. The information that I have in this respect is that, indeed, assailants no matter who they can be, aimed at inducing the inhabitants of the concerned regions to evacuate the country and they seemed to have succeeded.

I think that the mycotoxin issue can be considered to fall within the competence of the Group, since we can accept a definition for chemical agents in its broadest sense irrespective of their origin.

I do hope to meet you soon.

Best regards.



H. MARCOVICH

. Copy to Matt.Meselson

Dr Julian Perry Robinson
The University of Sussex
Science Policy Research Unit
Mantell Building Falmer
Brighton, Sussex BN1 9RF
U.K.

Dear Julian,

I thank you very much for your letter of October 30, 1981, with your request for comments by me on the accompanying paper. I am sorry being so late in answering you but as usual pressing business and also the flow of events have prevented me to do so earlier.

With regards to my comments I would like you to understand that it is of course impossible for me to take any stand to the allegations as such. However, since the purpose with your paper seems to be to question the role of the trichothecenes it is of course possible to comment on some of your arguments from a theoretical point of view. The most important issues raised by you in this respect seem to be the following:

p. 6: You are raising the question of naturally occurring levels of mycotoxins arguing that they can be quite high in naturally occurring substrates. One comment might be that there is a certain difference between a leaf and wheat or barley as a substrate for Fusaria. Grains are probably far more "perfect" substrates, and higher toxin levels would be expected.

p. 9: Referring to your argument of what toxins that occur in samples of naturally produced toxins - and of course Fusaria, even toxin producing Fusaria occur in Southeast Asia - one should recall that trichothecene production at "higher" temperatures is usually slow. Furthermore T-2 is usually found together with HT-2 when the Fusaria have grown at temperatures above 20°C.

p. 8, note 23 and p. 9: According to the French investigation referred to in note 23 it lists also zearalenone to occur together with the other toxins. That substance was not found in the alleged Cambodian sample. However, whether many or only a few toxins can be found in a sample from an alleged CW agent seems to be irrelevant assuming in that case that the agent was intentionally produced. Under such conditions it seems quite plausible to get a composition being the result of the production technique rather than corresponding to those occurring naturally. This of course is also an argument against claiming a particular composition being an evidence for intentional production of an agent.

p. 9: Translocation of trichothecenes is obviously theoretically possible, e.g. cfr Jarvis, Midiwo and Tuthill, Science 214(1981) 460-1, October 23, 1981. However, this seems so far to be a unique report. Further, it would not be applicable to talk about translocation on samples having been collected from rock, and other surfaces. Also blanks would show the same conditions if it occurred.

p. 12 - 13: With regard to your discussion on LD50s for the trichothecenes it seems to me that nothing is really known about them in man. Cf the differences of LD50 of CH_2FCOONa for e.g. dog ($0.1 \text{ mg}\cdot\text{kg}^{-1}$) and man ($2 - 10 \text{ mg}\cdot\text{kg}^{-1}$). Further there is a complete lack of data on inhalation toxicity of trichothecenes. It is not impossible that the onset of effects might be more rapid if they are administered that way. When comparing with the ATA poisonings there might also be a difference between being subjected to many small doses when eating daily contaminated wheat and barley and getting a lethal doses from the beginning.

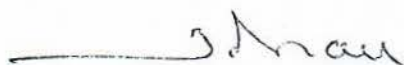
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I would also like to ask you to have your comments on the alleged new evidence brought forward by Richard Burt recently - if you feel like giving them to me.

I hope that my comments - which of course origin from discussions with colleagues more versed in the subject than I am - are of some help to you. I certainly take your remarks on the confidentiality of this communication very seriously. I am sure that you agree with me that it is impossible for anyone to give any definite opinion on this matter without concrete proof. Lacking that I do not want to be quoted in any connection as having expressed any particular views.

I wish you best luck with the further work on this matter and send you my best regards.

Sincerely yours



Dr Julian Perry Robinson
The University of Sussex
Science Policy Research Unit
Mantell Building Falmer
Brighton, Sussex BN1 9RF
U.K.

Dear Julian,

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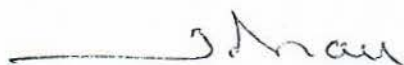
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I hope that my comments - which of course origin from discussions with colleagues more versed in the subject than I am - are of some help to you. I certainly take your remarks on the confidentiality of this communication very seriously. I am sure that you agree with me that it is impossible for anyone to give any definite opinion on this matter without concrete proof. Lacking that I do not want to be quoted in any connection as having expressed any particular views.

I wish you best luck with the further work on this matter and send you my best regards.

Sincerely yours



DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY
7 Divinity Avenue
Cambridge, Massachusetts 02138

August 9, 1982

Mr. Julian Perry Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton, BN1 9RF
ENGLAND

Dear Julian:

According to my notes from a conversation with Peter vonHippel the persons at the Open University who were studying European population distribution are Philip Steadman and Milton Keynes. I believe they are at the Center for Configurational Studies of the Faculty of Technology at the Open University. The telephone number of Keynes is 653-55

Sincerely yours,

Keynes

Matthew Meselson

MM/db

Grant Evans
Dept of Sociology
La Trobe Univ
BUNDOORA
Victoria 3083
Australia

6 Dec 82

Dear Grant,

Since my letter of 4th December I've read your MS again for the detailed comments set out below. Somehow a publisher must be found. Speed of publication is maybe no longer quite of the essence I thought it was when we started corresponding, though it would be nice. There's obviously some editing to be done, if only to correct the typos &c, but it seems to me the MS is now very near what could go to the printers (though of course I hope you will take due note of my detailed points). It would, however, make a lot of sense if you were to take in the new UN report and the new technical data on mycotoxin (including aflatoxin) findings, especially as set out in the Mirocha & Watson papers of September 1982 (in case you do not have these, I am sending copies under separate cover).

Apart from Penguin, Sphere and those left-wing houses, have you considered any of the more academic publishers? If you wanted to make some money from the book they certainly wouldn't be your best bet, but at least they would take your bibliographical apparatus in their stride, which is most important. I think particularly of Frances Pinter here in Britain. Say the word and I will write her an enthusiastic letter.

Have you sent Meselson a copy of the redraft? If not, you must. His comments would be much more valuable than mine.

There are eight of your cited sources, mainly newspaper articles, that I'd very much like copies of for my files; could you oblige? They are:

Ch 2, fn 36 Bangkok Post 24 Oct 76
fn 41 do 28 Nov 77
fn 42 do 7 Dec 77
fn 43 do 1 Dec 77
fn 46 Soviet Analyst 7 Oct 81
Ch 8, fn 4 AMPO 3 Mch 76, 52-57
Ch 9, fn 16 Age 26 Mch 82
Ch 6 &c National Geographic Jan 1974.

Here are my comments: (page no / line no)

2/11 Better: "untrustworthy Soviets. On the eve of the second special session on disarmament of the UN General Assembly".

2/bottom: The original sponsor of the UNGA resolution calling for the UN investigation was New Zealand—but of course pushed vigorously by the USA.

- 4/11-12 "fusarium strains of" isn't quite right; why not just have "trichothecene mycotoxins".
- 16/3 up "Lytek" here, but "ly tek" on page 12.
- 32/3-4 Is it really known that "massive" American stockpiles of riot gases were left behind in Indochina? Can reliable sources be cited?
- 35/10-12 It is true that the 1979 interview data provide the most substantial body of such evidence yet published. But can you say that it is the most substantial body collected by the US Govt? During the March 1982 HEAC hearing, Sharon Watson spoke of 'thousands' of interview reports.
- 29/17 It is true that particulars of corroborative military and other intelligence have not been made public. But in the Report the US Secretary of State has asserted that such corroboration does exist.
- 41/last sentence Is there some documentation you can cite in support of the information here that the US Govt has actually sought gassing reports in the northern refugee camps and failed to find it?
- 44-45/bridging para The para starts with an account of plans for Ban Vinai at its establishment; the last sentence of the para—"It was not..."—implies that the plans were what actually happened. If it were possible to expand and document this, you might then be able to open up—if not for actual discussion in the book, then at least for the questions to be posed—the matter of how significant a factor the militant Hmong refugees are in Thai politics, as, for example, yet another element of armed force (for COIN or anything else) in the polity of Thailand. If the Hmong presence does indeed have any such significance, one can understand how different Thai factions might be interested in promoting (or suppressing) Hmong gassing stories. That would then lead straight into the persistent stories—whether they be fact or fiction—of certain Thai groups having physical evidence (actual gas munitions, autopsy samples &c) denied to the rest of the world.
- 50/19-20 A specific aspect of a general point: throughout the NS there are references to various organisations and publications which have figured prominently in public knowledge of the gasings; it would often be useful to know a bit more about them. I personally would like to know more about the International Rescue Committee than that it is "conservative" (who or what does it rescue?)—not least for the light it might shed on Amos Townsend. I would also like to know about the political hue of, for example, the Bangkok Post.
- 88/last line Care needed here. Against hydrogen cyanide (a nonpersistent quick-acting lethal agent) poisoning, a standard Soviet (and Western) antidote is amyl nitrite, inhaled after breaking open a small ampoule of it.
- 90/1 Isn't the "Only" here a bit strong? We cannot discount the possibility of the interviews conducted with refugees since 1979 if having been much improved methodologically, and hence having yielded information conforming to higher standards of evidence. If that is actually the case (maybe it isn't), the question then to ask is why these more recent interview records have not been published.
- Also, there is one other category of evidence which, having only just heard about, I find myself rather impressed by: the epidemiological statistics produced by doctors at Ban Vinai (particularly Milton Amayun, to whom you refer rather dismissively later in the NS) that indicate an apparently significant incidence of chronic lung disorder that cannot be explained by tuberculosis or that lung-fluke disease got from eating freshwater crabs among subjects who say they have been gassed. Again, the question is: Has such evidence entered into the US Govt's assessment process? And if it has, have analogous epidemiological survey results from the Northern refugee camps also been taken into account—plus any survey data showing a nonincidence of that particular disorder among gas-reporting subjects.

- 91/2nd para The 1979 Hearing itself concluded nothing, of course. Better to refer here to the Army Surgeon-General report/ (Lewis's) disclosed at the hearing.
- 92/15-16 "Only". It is true that no physical evidence has been adduced in support of the conclusion of nerve-gas employment. But there are stories--which may be true or false--that such evidence exists (e.g. findings of depressed cholinesterase levels in tissue/blood samples).
- 94/23 I wasn't aware that any of the sceptics had asserted that "mycotoxins could in fact be found in Southeast Asia". May be better to replace these words with the following: "...there was no obvious reason why, if anybody had actually looked for the mycotoxins there previously, they would not have found them. In climatically similar places where they have been sought, they have been found."
- 95/10 Suggest "could" be inserted after "mycotoxins".
- 98/bottom If you are going to quote some of the testimony from the March 1982 HFAC hearing (which was actually a joint hearing before the subcommittees on Asian & Pacific Affairs and International & Scientific Affairs), you ought to quote, or at least cite, all the rest that was new and relevant: some of the Mirocha and Watson testimonies particularly, in this chapter, and the Haruff and Amayun testimony in chapter 3. Maybe you haven't seen the published version yet (it only came out last month); I've asked for more copies--I'll send you one if I get them.
- 99-100/bridging para You might want to recast the argument here a bit. The point about the absence of data on inhalation effects of trichothecenes is that, for all anyone can yet tell, the trichoths may be significantly more toxic by the inhalation route than by the parenteral routes on which the current human LD50 estimates (my own included) are based, and, if so, could make a more powerful weapon than the sceptics have argued. There is no particular technical problem in designing a weapon to disseminate mycotoxins in inhalable form: a simple aircraft spraytank can do that.
- ix/12 And several other places too (e.g. p160): "phosphorus" not "phosphorous"
100 (which means something else).
- 100/18 "diphenylcyanocarsine".
- 103/11-15 I haven't read the Soviet Critique all that closely yet, but if it really does say that it is impossible to form mycotoxins into an inhalable aerosol it is wrong. (NB: 'aerosol' is a technical term, misused popularly to mean 'aerosol dispenser'. It means a suspension of solid particles or liquid droplets in air, the properties of which retard settling-out of the particles/droplets; the key property is particle/droplet size. A fog or a mist or a smoke is an aerosol.) Suggest the last sentence be deleted.
- 105/6-10 It is on this very point that the Haruff/Amayun HFAC testimony bears.
- 107/15-16 And earlier. Thai investigators are rumoured to have acquired gassed corpses, but to have denied the Americans access to them. Sounds improbable, though; but see the comment above on 44-45.
- Ch 4 fn 16 Judith Nottingham, not "Julie".
- 154/14 "tremendous impact". I can see that gassing stories might indeed have had such an impact on local westerners. But is it in fact possible to document the actual occurrence of such impacts? E.g. an aid-worker filing an aghast report of gassing stories with head office? Is it at this point in the text that reference to the Haney testimony should first be made?

- 155/middle Did Cambodia actually identify "the Americans" in their complaint to the UNSC in 1964 about deadly yellow powder? My recollection is that no one was directly accused.
- 156 The US NAS study did not commence until the early 1970s. But unofficial studies by other ~~XXX~~ US scientific bodies were in progress at the time you say—e.g. Meselson's, for the LAAS/HAC.
- 180 Line missing at top?
- 197-8/bridging para It's a pity you have to use Spiegel as your source for this Congressional exchange. I don't think Spiegel quoted it quite correctly. Somewhere in my files I think I have the pages from the relevant committee hearings but can't for the moment find them; I'll keep looking.
- 205/18-19 The 1972 BW Convention does not prohibit the use of toxins (or of BW) in war; it was a contentious negotiating point at the time. So, instead of "...use of toxins in war were...", better to have "possession of toxins for weapons purposes was", or some such.
- 208/5 We would also need to ask whether it was in the interests of anyone else, apart from the USA, to make it look as though the USSR was violating the treaties. China, for example.
- 209/7ff So far as I know (and I have asked a knowledgeable East German too) there is no such thing as a "1977" manual of that title. Earlier dates, yes; but those earlier editions do not contain the passage quoted in the Report. I am waiting for an answer on this point from the State Dept.

With best wishes,

J F Perry Robinson

Jones

W. ALTON JONES FOUNDATION, INC.
TWO TOWN HALL SQUARE
CHARLOTTESVILLE, VIRGINIA 22901

~~WJF~~-295-2134
804

May 4, 1983

Dr. J. P. Perry Robinson
Visiting Scholar
Department of Biochemistry &
Molecular Biology
Harvard University
7 Divinity Avenue
Cambridge, Massachusetts 02138

Dear Dr. Robinson:

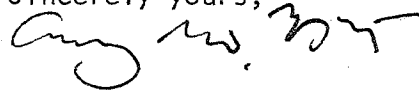
Adele Simmons called me on your behalf just as your letter of April 26 was on my desk for review. As she must have told you, I have the utmost respect for Professor Meselson and, as the saying goes, any friend of his is a friend of mine as well!

I was extremely interested in your review of the yellow rain matter and Adele's interpretation of your preliminary findings. It is important that you find the resources necessary to prevent this issue from being distorted in international circles.

Nevertheless, the W. Alton Jones Foundation is not the place to find that support. Our longstanding program in academic research was discontinued late in 1982, and we will devote our resources to topical areas for the next several years at least.

I am sorry to have to be so discouraging, but it is better for you to have the bad news now rather than later.

Sincerely yours,



Charles H. W. Foster
President

Thurs

CHWF/cr
cc: Adele Simmons

by phone 17 June 83
Suggests Rockefeller Bros Fund
NYC
Wm. Dietel
212-397-4800
Rockefeller Fdn.



Perry Robinson

SCIENCE POLICY RESEARCH UNIT
University of Sussex

Mantell Building
Falmer, Brighton
East Sussex BN1 9RF
UK

Tel: (0273) 686758

Dr. M. S. Meselson
Fairchild Biochemistry Building
7 Divinity Avenue
Cambridge
Mass 02138
U.S.A.

17th October 1983.

Dear Matt,

I've just had a letter from CDE Porton saying that "the case for bees is becoming increasingly credible". Presumably this was not meant for public consumption. A while back I had sent them a revamped version (copy enclosed) of your matrix analysis. They were responding with some notes on it by Robin Black. A copy of these Notes are enclosed. What is the answer to point (6) ? Am using the matrix to collate additional YR data.

I've been meaning to write you a long letter, but am hideously oppressed by deadlines. Hope to goodness we hear from Ford soon.

As ever,

Julian

enc: Item 166
Black Notes of 6 Oct 83

Geneva, 29 December 1983

Dr Robert Mikulak
215 Highland St.
Arlington, VA. 22201 (USA)

Dr Charles Bay
8500 Shadeway Place
Springfield, Va. 22153 (USA)

Dear Bob and Chuck,


I was more surprised with your letter of 14 December than you were with my memo of 25 November. On a matter of principle: my capacity as Secretary General does not exclude my voicing personal opinions on substantive issues at any time, e.g. my article in the Bulletin of the Atomic Scientists. Perhaps you don't read my annual reports given at our Conferences. The second part of these reports are highly personal, although they also try to reflect what I believe many of our members think and feel on certain subjects.

I have never stated or even implied that all members of our CW workshops agree on conclusions concerning subjects discussed. That would be plain stupid in view of USA and USSR participants (as individuals) in our workshops, not to speak of other nationalities represented. And all our workshop members know this, including yourselves I thought. Also, they are expert enough to come to their own conclusions when presented with all the facts. Nevertheless I will do as you request (about a personal judgment) in my next memo to the group to preserve, in your terms, Pugwash's utility as an objective forum for discussions.

You have received notice by now of the postponement of the next CW workshop. A complete working group will be devoted to the subject at our annual Conference to be held in Sweden 9-15 July. I am trying to arrange for key members of the CW workshop to attend including Lohs, Dymaev, Meselson, Robinson, Goms, Lundin, Mietinen, and one of yourselves. Enclosed is a copy of the agenda for the Conference in Sweden. Let me know how you feel.

Here's for a non-Orwellian 1984.

Yours,


Martin M. Kaplan

M.M.K., Wasth's letter for review then by ACDA?

cc: (not on original)
Meselson)
Robinson) with copy of their letter

London office (ditto)

Footnote (4)

Robinson

MemCon: Ricardo FRAILE, visiting SPRU, 7 Feb 84

And stayed overnight with us.

- 1) He is commissioned by UNIDIR to write them a backgrounder on the CW issue.
- 2) He had lots of refs to the use of CBW in blackmail attempts: a Newsweek item on FMV in Australia; something that happened in the USA in November; a successful BW blackmail in the FRG in 1969; an episode in Lisbon involving dioxin.
- 3) He said that Ricaud had approved of my estimate that the French CW stocks were similar in size to those of the USA in the FRG.
- 4) There had been someone from Strasbourg Univ in Sverdlovsk at the time of the anthrax. He would send me his name.

Travel -
Ghent

Heyndrickx

Professor A Heyndrickx
Laboratorium voor Toxicologie
Rijksuniversiteit-Gent
Hospitaalstraat 13
9000 GENT
BELGIUM

18 June 1984


Dear Professor Heyndrickx,

I am asked by Professor Meselson to forward to you the enclosed paper. He is at present travelling in southern Europe with his family, and has thought it best to submit only a summary of his presentation at the First World Congress. He has had no opportunity yet to prepare a full text, but wants to avoid imposing upon you any delay in your publication of the Proceedings to which people have subscribed.

Speaking for myself, I would imagine that I am not alone among the participants in wishing to be able to read as soon as possible the important papers by yourself and others on, especially, the Gulf War episodes.

Please accept my thanks and expression of admiration for the great efforts you and your colleagues put into making the Congress the highly instructive occasion that it was.

Yours sincerely,


J P Perry Robinson,
Senior Fellow

enc.

→ cc Dr M S Meselson

YELLOW RAIN: CHEMICAL WARFARE OR NATURAL PHENOMENON?

Meselson, M (Department of Biochemistry and Molecular Biology,
Harvard University, Cambridge, Massachusetts, USA)

We do not rule out the theory that mycotoxin warfare has occurred in Southeast Asia, but the evidence for it is unconvincing and becomes more doubtful with continued investigation and analysis. Our letter published in Science last October ⁽¹⁾ pointed out that there were no adequate controls to test the possibility that the reported mycotoxins were produced naturally by toxic fungi in foods and in the environment. This shortcoming in the evidence remains, but is partly superceded by newer evidence casting doubt on whether the reported mycotoxins were even present, at least at the time the samples were collected.

The numerous samples of alleged chemical warfare agent from Laos which we and others have examined, including the ABC News sample reported to contain T2 and other mycotoxins, are almost certainly the natural feces of Southeast Asian honey bees ⁽¹⁾⁽²⁾⁽³⁾. We know of no sample claimed to be a chemical warfare agent from a site of alleged attack in Southeast Asia which has been shown to be anything other than bee feces.

Out of 18 groups of Hmong refugees from Laos to whom we showed bee feces on leaves during March 1984 in Thailand, only one individual in one group identified the material as insect droppings. Several of the Hmong said that the fecal spots were 'chemie', their term for the purported chemical warfare agent. We know that massive showers of bee feces do occur, having been caught under one near Chaing Mai. Although standing about 200 meters from a large tree with many bee nests, we heard no bees and saw none overhead.

We know of no reason to believe that bee feces pose a toxic hazard to people. How then are we to interpret refugee claims that showers of this material cause illness? It is possible that the Hmong, Kampuchians and Thais who have made such claims, not knowing the correct explanation of the shower of yellow spots and being aware of stories of poison from the sky recurrent in Southeast Asia for more than 20 years, have mistaken bee defecation for chemical warfare and have associated it with real or rumored illness. Almost all interviews have been done with persons self-selected in advance for believing themselves to be chemical warfare victims, so that such stories would be over-represented in the available reports.

A precedent for widespread but almost certainly mistaken attribution of illness and death to unusual materials from the sky exists in a U.S. National Academy of Sciences study of the effects of herbicides in Vietnam done for the Department of Defense published in 1974⁽⁴⁾. The Academy reported widespread claims by Vietnamese highlanders of diarrhea, abdominal pain, vomiting, skin rash, fever, dizziness, coughing blood and massive deaths attributed to herbicide spraying. However, the 10 villages studied by the Academy were not all exposed to the same herbicide and a parallel Academy study of lowland Vietnamese did not find major illness or deaths. None of the herbicides used would be expected to cause such serious effects. It therefore seems likely that the illness and death which the Vietnamese highlanders attributed to herbicide spraying was partly of natural origin and partly exaggeration and rumor. We have spoken with Hmong leaders in the U.S. who expressed similar reservations about the yellow rain stories.

- (1) Ashton, P.S., Meselson, M., Perry Robinson, J.P., and Seeley, T.D. Science 222:366 & 368 (1983).
- (2) Nowicke, J.W., and Meselson, M. Nature 309:205-6 (1984).
- (3) Meselson, M., and Seeley, T.D. Press release dated 28 March 1984, as reported in Science 224:138-9 (1984) and Nature 308:485 (1984).
- (4) United States National Research Council. The Effects of Herbicides in South Vietnam (Washington, D.C.: National Academy of Sciences, 1974).

Travel -
Ghent

Heyndrickx

Professor A Heyndrickx
Laboratorium voor Toxicologie
Rijksuniversiteit-Gent
Hospitaalstraat 13
9000 GENT
BELGIUM

18 June 1984


Dear Professor Heyndrickx,

I am asked by Professor Meselson to forward to you the enclosed paper. He is at present travelling in southern Europe with his family, and has thought it best to submit only a summary of his presentation at the First World Congress. He has had no opportunity yet to prepare a full text, but wants to avoid imposing upon you any delay in your publication of the Proceedings to which people have subscribed.

Speaking for myself, I would imagine that I am not alone among the participants in wishing to be able to read as soon as possible the important papers by yourself and others on, especially, the Gulf War episodes.

Please accept my thanks and expression of admiration for the great efforts you and your colleagues put into making the Congress the highly instructive occasion that it was.

Yours sincerely,


J P Perry Robinson,
Senior Fellow

enc.

→ cc Dr M S Meselson

YELLOW RAIN: CHEMICAL WARFARE OR NATURAL PHENOMENON?

Meselson, M (Department of Biochemistry and Molecular Biology,
Harvard University, Cambridge, Massachusetts, USA)

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EXECUTIVE OFFICE

CENTRAL OFFICE

Please reply
to →

11A, Avenue de la Paix
1202 Geneva
Switzerland

Telephone: (022) 33 11 80
Telex: Peace 28 167 CH
Telegraph: Pugwash, Geneva

9 Great Russell Mansions,
60 Great Russell Street,
London WC1B 3BE
England

Telephone: 01-405 6661
Telegraph: Pugwash, London

Geneva, 28 September 1984

To : Karlheinz Lohs
Matthew Meselson
Julian Perry Robinson

From : M.M. Kaplan, Secretary-General

In Björkliden the Council established the above individuals and myself as a subcommittee to prepare a statement on behalf of Pugwash for the review conference of the Biological Weapons Convention which presumably will take place in 1985, although the final date has not as yet been fixed. I have been asked to report on this matter to the Pugwash Executive Committee which will meet in Geneva on 15 and 16 December.

I would therefore welcome any suggestions you have, and I hope that Julian will be good enough to prepare a first draft.

My own view is that despite some shortcomings the BW Convention has been very valuable and any tampering with it should be done with great hesitation lest an unravelling occurs. I believe that the USA will focus on shortcomings which they believe exist with respect to lack of verification provisions and the potential of DNA recombinant work. We should try to address these two possible objections as well as any others.

Matt,

I don't recall if I sent you a copy of my editorial coming out soon in the October issue of the Newsletter, immortalizing your talk in Björkliden. Matt

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

EXECUTIVE OFFICE

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Geneva, 25 October 1984

Prof. Dr Karlheinz Lohs
Forschungsstelle für chemische
Toxikologie der Akademie der
Wissenschaften der DDR
Permoserstr. 15
DDR-7050 Leipzig

Dear Karlheinz,

Many thanks for your helpful comments of 16th October concerning the BW Convention. I hope to receive from Julian a first draft of a possible Pugwash statement which I will take up with our Executive Committee in December.

I was in Bonn this past weekend, attending the 25th Anniversary of the FRG Pugwash Group. I heard there that your meeting with the SDP people on a chemical weapons free Germany came off well. I should welcome any details of that meeting.

With cordial regards,



Martin M. Kaplan

cc: Julian Perry Robinson
Matthew Meselson

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

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CENTRAL OFFICE

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REPLY TO- 9 Great Russell Mansions,
60 Great Russell Street.
London WC1B 3BE
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Telephone: 01-405 6661
Telegraph: Pugwash, London

Geneva, 8 November 1984

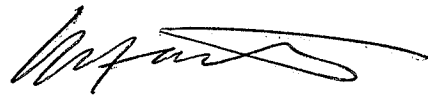
Professor Matthew Meselson
Fairchild Biochemistry Building
Harvard University
7 Divinity Avenue
Cambridge, Massachusetts 02138
U.S.A.

Dear Matt,

The enclosed correspondence is in reference to a round-table discussion on CW that SIPRI is arranging in Stockholm with some 15 ambassadors attending the Stockholm conference on disarmament. Sverre Lodgaard of SIPRI is in charge. I suggested that Dioumayev be invited if Bob Mikulak attends if they want to "balance" the participants. I told Lodgaard that if Dioumayev will attend, you ought to be there. He agreed, but said it was entirely a question of lack of funds. My answer to that was funds would somehow be found. He will keep me informed. ...

Would you come if Dioumayev would be there? It is expensive but I believe it would be worth it. I'll arrange somehow to cover half the ticket (possibly through the U.S. Group) if SIPRI takes on the other half.

As ever,



Martin M. Kaplan ✓

Enclosure

cc: Mr Julian P. Perry-Robinson

SFRUSCIENCE POLICY RESEARCH UNIT
University of SussexMantell Building
Falmer, Brighton
East Sussex BN1 9RF
UK

Tel: (0273) 686758

JAN 7 1985

Maybe I should come over for a few days
in mid-January? I want to get
the peripatetic research-assistant scheme
fully A&P, if you think it a worthwhile
idea.

Julian R.

With Compliments

19 Dudy.

19 December 1984

J M Furman
Executive Vice President
John D and Catherine T MacArthur Foundation
Suite 700
140 South Dearborn St
CHICAGO
Illinois 60603
USA

By Swiftair

Dear Mr Furman

GRANT NO. 84-342

Thank you for your two letters of 13th December. Needless to say, I was delighted by their news that the Foundation had decided to support, as part of its new International Security Program, the joint project on chemical and biological weapons which we here at Sussex are engaged upon in collaboration with Harvard University. It will now become possible for us to do some really concerted, innovative and forward-looking research within this project, with some real expectation of improving the foundation for prudent public policy-making in the dangerous area of CBW. I hope that our work will indeed live up to the fine expression of confidence which the Foundation has made by its decision to support the work so generously.

I enclose herewith as requested, the copy of the first of your letters of 13th December, duly counter-signed in the place indicated by the Administrative Secretary of the Science Policy Research Unit on behalf of the University of Sussex. We have, of course, closely studied the terms and conditions under which the grant is to be made. We do have one query about them, set out below, on which we would have preferred prior consultation with you, but we have been mindful of the time-urgency expressed in the letter.

One query arises primarily from the circumstance that, between now and the end of 1984, we shall not have time to consult adequately with Harvard about how exactly the funds made available under the grant should be allocated.

Specifically, we can envisage some of the particular research activities on which we might decide to embark, especially some of the archival research, not being completed within the three-year period of the grant. Decisions on whether to embark upon those particular activities in the first place

must be conditioned, in some degree, by the attitude which the Foundation is likely to take on the question of whether the grant funds and any income thereon remaining unexpended during the three-year term might be available to support the completion of sub-tasks remaining uncompleted at the end of that term.

We would very much appreciate your opinion on this point as soon as it may be convenient for you to consider it.

Yours sincerely

J P Perry Robinson
Senior Fellow

Enc

cc Ms Jackie Fuller, SPRU
Dr M S Messelson, Harvard University

JAN 7 1985

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Senior Fellow

Enc

cc Ms Jackie Fuller, SPRU
Dr M S Meselson, Harvard University

MemCon: Phone call from ANON, 1 May 1985

11:36 AM

Call was to Meselson on (617)495-2264; myself listening in, at MSM's request, on his secretary's extension, Anon consenting (I don't recall whether MSM identified me to him by name—but think he did).

Part-way through this long call, Colonel Hugh Stringer arrived in the secretary's office and sat there by the desk waiting for me to ring off and start our lunch date.

Caller was male. No marked accent: sounded home-grown American, a person perhaps in his thirties. No obvious nervousness. Fluent even in response to probing questions.

or other intel. agency.

1) How he opened the conversation, I do not know. From what I gathered from MSM later, he indicated (a) that he was a CIA employee, and (b) that he was calling from a pay-phone. /NOTE: although the call lasted the best part of half an hour, I heard no interruptions from an operator./ During the middle of the conversation, ANON said that his job (~~at the CIA~~) involved a reading of incoming telexes and cables—from which he had learnt about what he was relating. His section (~~in the CIA~~) had the task of monitoring the press on the Yellow Rain issue.

2) He spoke at length of a CIA disinformation campaign involving YR. The operation that had been publicized, during Spring 1982, by Scott Barnes had been for real. It had been but one part of the overall "diso" campaign. The whole thing had been the largest and the best diso campaign the CIA had ever run.

MSS(s) taken to NE Thailand May 81.

Captured late May (?)

or Bobby connected to "The Activity": Ctr Thomas Task Force.

3) He gave more details of the operation BARNES had described: In May 1981, BO GRITZ had taken delivery of two M55 nerve-gas rockets from Johnston Island. GRITZ had been tasked to get this nerve gas taken into Laos and planted there as evidence of toxic warfare; in addition, 500 doses of atropine, provided by Task Force 60, were to be delivered to VANG PAO supporters. *Some MSSs "missing" from Johnson.* GRITZ met with Vice Adm Inman but was kept in the dark about nature of mission (?). **GB**

4) The operation, to plant the nerve gas ran during June/July 1981. But two of the people on the team got captured. Their identities have not been revealed. GRITZ was then taken off the operation, and a Special Forces rescue mission instituted, BARNES being part of the team. The team's orders were to kill the two prisoners if they could not be rescued. This operation ran during November 1981.

Robyn Schwab

5) Meselson asked about THOMAS STEARNS. Anon said he was "a kid who picked up bones and got paid for MIAs"; this he had been doing in southern Laos. Other than this, Anon knew nothing of STEARNS.

6) Several people were identified by Anon as part of the overall diso campaign. There was JANE HAMILTON-MERRIT, who was set up to write about the CW plight of the Hmong. There was JAMES COYNE, described as a "disinformation officer", who was running a drug operation (with the involvement of BOB MOBY of FDA) to finance his doings; paid by CIA to write a "diso article". There was ALAN DAWSON, described as a "diso agent" who, in his regular journalism, wrote much to discredit JERRY DANIELS—stories about the latter's homosexuality, &c. *Soldier of Fortune*

for the Agency

Drug Enforcement Agency

Congress would not pay for

7) Regarding DANIELS, who had been a close personal friend of VANG PAO and his case officer, Anon said he had been killed in a carefully planned homicide, probably with GB nerve-gas, because he had discovered the disco operation. The killing had been ordered by Colonel Mike EILAND of the Bangkok US embassy. Autopsy had been denied. Body had been sent back to USA in a sealed casket.

Cables said body found by Thai Insell agent.

8) Anon said that word was out to keep MESELSON, PFEIFFER and WATSON under "extreme observation": "the Company doesn't like you". Home phones certainly tapped.

9) When asked why, nonetheless, he felt free to telephone Meselson, Anon said that the CIA would not tap a Harvard-University phone.

10) In responding to the questions which Meselson then put to him, Anon stated:

- a. BOBBY INMAN had been deeply involved in the whole disco campaign. He had met with GRITZ.
- b. GRITZ had, however, been kept largely in the dark, and made a fool of.
- c. A CIA warlord on the GRITZ operation was JOHN STEIN, a no.3-level CIA officer who knows about the operation.
- d. Rep ROBERT DORNAN had been exploited totally by the CIA. He was "an absolute flake".
- e. HUGHES AIRCRAFT had been involved.
- f. The DCI had called the Chairman of ABC regarding their YR film. The latter conceded on everything that the DCI wanted.
- g. Colonel PAUL MATHER of DIA had been "used quite a bit" by CIA.
- h. The Special Forces members of the rescue mission had been very carefully selected. A top polygraph man in California had been used: Dr CHRIS DUGAS, who does a lot of polygraph screening for CIA, NSA and FBI. He was a possible corroborant.
- i. Anon had no information to impart as to whether JULIAN HOPTMAN at CIA knew about the disco campaign. Anon said that JOHN McMANN there would have known about it.
- j. AMOS TOWNSEND, who had been a Phoenix adviser under Colby, had wittingly been assisting the disco campaign.
- k. Anon did not know the name DENNISON LANE, and Meselson's saying LANE had been working under MATHER prompted no recollection. Anon suggested LANE might have figured in the cable traffic under a code name. Anon had nothing to say, either, about CHARLES WHITNEY.

possible other homicides
George Weise in Md.
Paisley in Lake Mich.

Mar ABC retraced story on World News to Bishop. Baldwin

Did not seem to know Kit Green. That him a Canadian.

Michael Eiland was dangerous. Mentioned Paul White, Deputy Utah.

Anon had reservations re death of Pop Buch.

11) As another possible corroborant, Anon spoke of DAVID TAYLOR, a BEC correspondent in Washington with inside lines into CIA, though much disliked by the Agency. TAYLOR had investigated CRITZ, and had recently discovered a good story about a fronted CIA operation out of Hawaii.

12) As for other routes to corroboration, Anon said that it was most important to locate: INMAN, STEIN, BARNES and EILAND.

13) What Anon wanted, he said, was a serious Congressional investigation

- 14) Anon had nothing to say about trichothecenes. He would inquire into this aspect, especially re WATSON.
 - 15) He would call back again in about a month.
-

16) The following names are in my MS notes taken during the phone call. I can't now recall why.

POP BUELL

BUTH (?)

DUAKOWSKY (?)

17) Some thoughts:

- a. An anonymous phone call alleging disinformation could very well itself be disinformation, perhaps seeking to discredit Meselson, and me too, maybe.
- b. The impression I got from the caller's general tone was either that he was a counterintelligence person within CIA, or that that was what he wanted to be thought.

18) Some relevant documentation:

- a. Scott Barnes' original disclosure: the press release dated 7 April 1982 put out by Covert Action Information Bulletin, as developed further in issue no 17 of the Bulletin, dated Summer 1982.
- b. Harper's, September 1984, p 43. This reports the remarks of RALPH McGEHEE, ex CIA, during a 'forum' held at the Russell Senate Office Building at which, in response to a remark by Sen MOYNIHAN, he spoke of the YR charges as having been fabricated by the CIA. That was something McGEHEE had also said (so I was told by a participant) during a session of the 34th Annual Meeting of the Association for Asian Studies in Chicago, 2-4 April 1982.

19) During a telephone conversation I had with STERLING SEAGRAVE on 1 November 1981, SEAGRAVE told me that there was now some physical evidence of tabun having been used in Laos.; but this might not be released for fear of compromising a source in place. And during the 'seminar' SEAGRAVE gave at the US Embassy in London on 17 November 1981, he said that there was now positive clinical evidence of tabun having been used in Cambodia

Barnes

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- 13) What Anon wanted, he said, was a serious Congressional investigation, using powers of subpoena.

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17) Some thoughts:

- a. An anonymous phone call alleging disinformation could very well itself be disinformation, perhaps seeking to discredit Meselson, and me too, maybe.
- b. The impression I got from the caller's general tone was either that he was a counterintelligence person within CIA, or that that was what he wanted to be thought.

18) Some relevant documentation:

- a. Scott Barnes' original disclosure: the press release dated 7 April 1982 put out by Covert Action Information Bulletin, as developed further in issue no 17 of the Bulletin, dated Summer 1982.
- b. Harper's, September 1984, p 43. This reports the remarks of RALPH McGEHEE, ex CIA, during a 'forum' held at the Russell Senate Office Building at which, in response to a remark by Sen MOYNIHAN, he spoke of the YR charges as having been fabricated by the CIA. That was something McGEHEE had also said (so I was told by a participant) during a session of the 34th Annual Meeting of the Association for Asian Studies in Chicago, 2-4 April 1982.

19) During a telephone conversation I had with STERLING SEAGRAVE on 1 November 1981, SEAGRAVE told me that there was now some physical evidence of tabun having been used in Laos.; but this might not be released for fear of compromising a source in place. And during the 'seminar' SEAGRAVE gave at the US Embassy in London on 17 November 1981, he said that there was now positive clinical evidence of tabun having been used in Cambodia.

Name list April 21, 1986

ARMITAGE, MICHAEL
ARNOLD, DANIEL
BARNES, SCOTT
BATH, J.D.
BROOKS, GEORGE
BUELL, DR. POP
BURNS, DAVID
CHUMO, NORMAN
COYNE, JAMES
DANIELS, JERROLD E., JR.
DANIELS, JERROLD BARKER (PERHAPS AKA MICHAEL BALDWIN, GERRY HALL)
DAWSON, ALAN
DORNAN, CONG. ROBERT
EILAND, COL. MIKE
GREEN, DR. CHRISTOPHER C.
GRITZ, COL. BO
GUGGIS(?), DR. CHRISTOPHER
HAMILTON-MERRITT, JANE
LANE, COL. CHARLES DENNISON
MATHER, COL. PAUL
MOBERG, ROBERT
MOBEY, BILL
~~NAMES FROM ANON, BARNES, ETC~~
NUNC, COL. PAUL
O'ROURKE, Mrs.
OAKLEY, ROBERT
FAISLEY
REWALD, RONALD, R.
ROBINSON, COL. BOBBIE
ROENSLATT, LIONEL
SEAGRAVE, STERLING
SHWAB, ROBERT (AKA THOMAS STEARNS AND JACK SCHRAMM)
SICARD, DR. MARIE-NOEL AND DR. DIDIER
STEIN, JOHN
THOMPSON, MAC ALLEN JR.
TOWNSEND, DR. AMOS
WATSON, DR. SHARON
WEISZ, GEORGE



SCIENCE POLICY RESEARCH UNIT
University of Sussex

Mantell Building
Falmer, Brighton
East Sussex BN1 9RF
UK

Tel: (0273) 686758

16 May 1985

Dr M S Meselson
6 Divinity Avenue
Cambridge
Mass. 02138
USA

Dear Matt

We are all looking forward eagerly to seeing you next month. Elisa, too - who warns me against entrusting too much to the mails. A few items are enclosed, however.

A favour: in one of the heaps (top left of the further heaps, as I recall, at the top of the heap) is an offprint of a long article by Paul G Cassell in Stanford Law Review, 'Establishing violations of international law...', or something like that. Would you lend it to me?

The university here has in mind transmitting to Harvard an initial sum of £5,000 for the Sussex/Harvard MacArthur scheme. Does that sound a sensible sort of sum? More could be sent later, gathering interest in the meanwhile. What do you think? As soon as I hear from you, I will have a letter sent to Dr Thomas Bazone.

As ever.

Julian.

J P Perry Robinson

Encs

P.S. After all, I would like a copy of the Ochsner study for the Civil C. My own copy is barely legible in places.

DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138



May 21, 1985

Mr. Julian P. Robinson
Science Policy Research Unit
Mantell Building
University of Sussex
Brighton BN1 9RF
ENGLAND

Dear Julian:

Since the bank has misspelled Mary's name I am enclosing only one checkbook. However, if you want the other seven, I will bring them to England with me when I come.

Sincerely,

Matthew Meselson

Encs: Checkbook
Les Ruses de Guerre Biologiques
dans L'Antiquité
Issues Regarding Production of New
Chemical Weapons

DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138



May 29, 1985

Mr. J. P. Perry Robinson
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer, Brighton
East Sussex BN1 9RF
ENGLAND

Dear Julian:

If they can be found in the heaps, I will bring Cassell and Ochsner to London.

Sussex should send no more than the £5,000 you suggest, since the money will not gather interest here. If the only certain expenses over the short term are a possible trip to England or something of that nature, even less than £5,000 would be enough.

As ever,

A handwritten signature in dark ink, appearing to read 'Matt', with a long, sweeping horizontal stroke extending to the right.

Matthew Meselson

MM:bne



Robinson
JUL 22 1985

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University of Sussex

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Falmer, Brighton
East Sussex BN1 9RF
UK

Tel: (0273) 686758

Dr. M. S. Meselson
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Fairchild Biochemistry Building
7 Divinity Avenue
Cambridge
Mass. 02138
U.S.A.

15th July 1985.

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Among the enclosures you will find copies of two of my MemCons on chats with you. You will see that they are full of lapses of memory. It would be nice if you could annotate them with corrections &c and send them back sometime. I have in mind taking all my YR material with me to France in August with a view to making detailed suggestions to you, when I get back, on how your Scientific American piece might be expanded into a SIPRI occasional paper such as the one enclosed.

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- JCS Pub.1 (1 June 79), 'Department of Defense: Dictionary of Military and Associated Terms'.
- *AOR 310-25 (15 Sept 75), 'Military publications: Dictionary of United States Army terms'.

If there are costs, maybe they could be charged to the MacArthur account, which should by now have some money from Sussex in it.

As ever,

J. P. Perry Robinson

Encs.

MemCon: Matthew F. SELSON, 27-29 April 1985

At Harvard.

1) About a week before, he had met with Dennison LANE in Washington and had had long chat about YR. Some points:

- a. Lane's recollection was that the original leaf-and-stem sample had been collected by McWilliam and Wilde, the latter a medical officer at the US Embassy in Bangkok.
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- d. MSM asked Lane who had been the most sceptical people in USG re YR. Lane said that the most sceptical had been Jerry DANIELS, followed by Mac-somebody, the USAID person who had discovered Daniels' body.
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2) Bill GARETT (sp ?) of National Geographic had told MSM that Seagrave had had an assignment from NG to write vignettes of life in Thailand. One of NG's best photographers had been assigned, but Seagrave didn't show. It was at that time that the Army Surgeon General's team had been in Thailand. When Garrett read Yellow Rain, his reaction was that Seagrave couldn't have written it.

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5) Charles WHITNEY had had the YR sample that went to ABC news in his fridge for a year and a half previously. (Did MSM have this from LANE ?) /NOTE: Esmat Ezz had contact with Whitney in Thailand; check with him ?/

6) MSM gave me a copy of the new Sharon WATSON document dated 18 June 1984, its unclassified excerpts only. The person who had given it to him would get really screwed if that became known. MSM had also shown it to Peter PRINGLE, who is now trying to get the complete thing through FOIA.

7) Phil HILTS is a Niemann Fellow until June 1985. He is working on a YR book, and is inquiring closely into the early days of YR.

In London, while comparing his and my copies of the YR-sample tables.

Note: what is recorded here comes solely from the annotations I made during the conversation(s) onto my copy of the tables

- 1) During a conversation which MSM had with Gary CROCKER in Geneva on 9 November 1983, CROCKER said the following:
 - a. It was Kit Green who had collected the original leaf-and-stem sample ('FS698A') in Kampuchea. Many leaves were gathered then. No one has reported on the delivery means supposedly used in the attack.
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- 4) Re sample 'US/UN-2': Gary CROCKER and Kit GREEN had been together in March 1982 when a group of 70 refugees came into Ban Vinai.
- 5) The Kabul gas-mask ('Shultz-10'): the source for this having been purchased was Gary CROCKER talking to Phil HILTS. Hilts has assured MSM that Crocker's statement is recorded in his interview notes.
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- 7) Or was it 'Shultz-10' (the Kabul gas-mask) on which CROCKER had said he would not place great emphasis ?
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Robinson
JUL 22 1985

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Robinson



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Lunch with MSM and me at a Wheelers near the Royal Society.

- 1) Pleasant, straightforward concerned sort of person. Has teenage children in USA. Doing some sort of degree in London. Started out rather on the defensive. Hong Kong background. Arrived Thailand Jan 1980.
- 2) Asked us if we had followed the latest attacks, those of Dec84/Jan 85 in Cambodia.
- 3) Not a committed true believer on Yellow Rain; but believes that something untoward had been happening.
- 4) Interested in the Daniels affair. Suggested the Thai boy as a follow up source. DANIELS had worked in JVA; 'can do many things', people said. Had been doing interviews of certain special kinds. About battles, &c. With the Hmong.
- 5) ~~The~~ The Medecins sans Frontieres people had a very rapid turnover. Include Belgians as well as French. No one with them long enough to do detailed studies.
- 6) Was aware of Israeli and FRG embassies having taken an interest—but they didn't seem to have had much in the way of resources, e.g. for shipping samples back home.
- 7) Knew of Col MATHER as someone who collected bones: MIA etc. Harry WILDE was known as an 'eccentric'; married to a Thai. Jane HAMILTON-MERRIT 'gives her ulcers': an intolerant, impatient sort of person. Lionel RESENBLATT was married to a nurse. Charles WHITNEY—if it was 'Chuckie' Whitney, lots of people wanted to kill him: owed money, unfulfilled promises, &c.
- 8) Samples: Thai/Lao ones are suspect, since they are always offered for sale. Hmong ones are not. Doug HULCHER has a Customs list of samples.
- 9) She had done her interviewing outside Ban Vinai, not inside; had interviewed only 10-12 people, not the 40-odd stated in that report. Speaks some Hmong. Keeps careful notes.
- 10) Had worked for WVI at some point.
- 11) Some of the relief agencies have a practice of burning all their case notes at year end; deplored this.
- 12) Had handled 12-40 samples. Most were yellow. Some were black.
- 13) Very few physicians were interested in studies of YR; could not suggest any names.
- 14) Oz re sample.

MSM has more recollections.

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

EXECUTIVE OFFICE

PLEASE REPLY

TO ----- : 11A, Avenue de la Paix
1202 Geneva
Switzerland

Telephone: (022) 33 11 80
Telex: Peace 28 167 CH
Telegraph: Pugwash, Geneva

CENTRAL OFFICE

Flat A
63A Great Russell Street
London WC1B 3BG
England

Telephone: 01-405 6661
Telegraph: Pugwash, London

Geneva, May 30, 1986.

Prof. Matthew Meselson
Fairchild Biochemistry Building
Harvard University
7 Divinity Road
CAMBRIDGE
MA 02138
U.S.A.

Dr. Julian P. Perry Robinson
SPRU
University of Sussex
Falmer
BRIGHTON BN1 9RF
Sussex
United Kingdom

Dear Matt and Julian,

Enclosed is the answer from Fokin. Pavlichenko, who is in Geneva for the USA/USSR negotiations, told me two days ago that he had delivered my letter to Dioumayev and Fokin, and that they had conferred. I told Pavlichenko that the brush off is a bad mistake. He suggested that I discuss the matter with General Nicolai Chervov (in charge of disarmament negotiations for the Army) and Vadim Zagladin, Member of the Central Committee CPSU, who will be here in Geneva for our meeting on nuclear forces, 14 and 15 June. I'll probably speak to both of you on the phone before then.

Yours,



Martin M. Kaplan.

Encl. : 1.

P.S. Matt - of course we would be delighted to have you and the girls stay with us towards the end of June/early July.

CBW

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

EXECUTIVE OFFICE

CENTRAL OFFICE

PLEASE REPLY
TO _____ :

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1202 Geneva
Switzerland

Telephone: (022) 33 11 80
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United Kingdom

SEP 24 1986

Dear Matt and Julian,

I enclose the more interesting official statements made at the BW Review Conference thus far, but apparently a nasty dispute broke out in the plenary session yesterday afternoon which unfortunately I missed.

I was told that Lowitz made a savage attack on the USSR about not answering satisfactorily allegations about Sverdlovsk and CW, bringing with him for distribution all the official U.S. statements on the subject since 1980 (Haig, Shultz, etc.). He cited numerous instances of unanswered allegations. Issraelyan then took the floor mentioning that recent terrorist activity, and now another bomb had been dropped in their midst. He said that if a constructive review conference document was not desired, so be it.

Andrzej Karkoszka reported this to me this morning. He saw Bob Mikulak afterwards who said they were following instructions, but that everything would turn out well in the end.

I had a long talk with Dickson of Science yesterday explaining our views on the overblown attention being given to genetic engineering and declaration of P4's as the best way of handling the matter.

Let's await the next round.

Yours,



Martin M. Kaplan.

6 February 1987

Dr G S Pearson
Director
CDE Porton Down
SALISBURY, Wilts
SP4 OJQ

Dear Graham

I wonder if you would be able to help me with a monograph I am writing on the 'Yellow Rain' affair? It will most probably be published in SIPRI Chemical & Biological Warfare Studies, of which I am the editor, assuming the new director of SIPRI decides to continue with it. The series is meant for a specialist audience and aims to provide careful commentary and analysis of kinds that may assist CBW arms control: a field in which the Yellow Rain bulks large.

The monograph will do two things. It will draw such lessons as may be drawn from the affair about the verification problem. And it will assemble a detailed and documented record of the manner in which the affair developed, paying particular attention to the attempts made by different bodies--governmental, intergovernmental and nongovernmental--to ascertain the true nature of Yellow Rain by applications of scientific method. It is in this second area that I am hoping you will be able to help, especially as regards the work done at CDE.

The only published print I have seen which describes, or purports to describe actual findings from the CDE YR investigations is the MoD written reply of 19 May 86 to the Abse PQ, plus items in Defense Attache (1986 no 4, p 55), Science (4 July 86, p 18), Chemistry in Britain (July 86, p 615), Chemical and Engineering News (9 Jun 86, p 23; 9 Jan 84, p 33), Washington Post (29 May 86), Nature (24 Apr 86, p 669), Observer (24 Mar 85) and Guardian (3 Nov 83). (I had heard tell, incidentally, that the New Scientist for 15 Aug 86 at p 16 had also contained some sort of disclosure, but I find that what little the article actually reported about the CDE work had already been published elsewhere.) It cannot be said that this literature amounts to very much, and there is a good bit of inconsistency in the press-reporting part of it. It seems to me important that the monograph should rely as little as possible on hearsay, and that it should put the CDE work into its proper perspective. But these are mutually contradictory requirements the way things stand at present.

Presumably there is no question yet of any of the CDE's own reports on its findings being released. So I am writing to ask if you would care to consider whether I might be given--on an attributable basis--answers to some or all of the following sets of questions:

.../...

Page 2
Dr G S Pearson
6 February 1987

- (1) When did CDE begin to analyse YR or YR-associated samples? When did it do so specifically for trichothecenes using GC/MS? When did it stop doing so?
- (2) Is there to be publication of the details of the techniques used in the analyses or of any of the background reviews and studies? If so, when and where, and might advance copies be given to me? (CDE TN 519 and 553 I have had already, from Rex Watson.)
- (3) How many environmental samples from alleged CBW attack sites were analysed for trichothecenes? How many of them were yellowish in colour or spotted with yellowish material? How many were examined for pollen? And how many were found rich in pollen?
- (4) How many biomedical samples from supposed CBW victims (i.e. bloods, urines, &c, from how many individuals) were analysed for trichothecenes? What sorts of control samples were also analysed?
- (5) What is the nature of the 'epidemiological evidence' referred to in the 19 May Written Answer, and in what material respects does it differ from the interview-derived data published by the UN and by the Canadian and US Governments?

I hope very much that I may hear from you on this in due course.

With best wishes,

Yours sincerely

J P Perry Robinson
Senior Fellow

WJ Pearson

From: Dr G S Pearson, Director

Procurement Executive Ministry of Defence
CHEMICAL DEFENCE ESTABLISHMENT

Porton Down, Salisbury, Wiltshire. SP4 OJQ
Telephone: Idmiston (0980) 610211 Ext 208



Ptn IF 4900
Ptn TG 1091/200/87

J P Perry Robinson
Senior Fellow
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer
Brighton
East Sussex
BN1 9RC

16 February 1987

Dear *Julian,*

1. Thank you for your letter of 6 February 1987 which will take some time to answer but I will do my best to encourage the system to provide attributable answers for you.
- ... 2. In the meantime I enclose as promised a list of the CDE papers on trichothecene mycotoxins which have either been published or are in the course of publication.
- ... 3. I also enclose a copy of the paper on the generation of aerosols by accidents which may occur during plant-scale production of microorganisms that we mentioned in the context of discussion of Sverdlovsk. I will be writing to you shortly on the Sverdlovsk material you left with me.

With best wishes

Yours sincerely

Graham

Published papers

1. Analysis of some trichothecene mycotoxins by liquid chromatography.
R Maycock and D Utley
J Chromatogr (1985) 347 429-433
2. Acute toxicity of T-2 mycotoxin to the guinea-pig by inhalation and subcutaneous routes.
T C Marrs, J A G Edginton, P N Price and D G Upshall
Br J exp Path (1986) 67 259-268
3. The in-vitro penetration and distribution of T-2 toxin through human skin
S A Maxwell, R F R Brown and D G Upshall
Toxicology (1986) 40 59-74
4. Survey of Fusarium species for plasmid-like DNA and some evidence for its occurrence in a strain of F. merismoides
T Rubidge
Trans Br mycol Soc. (1986) 87 No 3 463.466
5. Rhodotorula rubra bioassay for T-2 toxin: increased sensitivity and wider application
P E Stone, T Rubidge and K D Macdonald
J. Microbiol Methods (1986) 5 59-63

Papers in the course of publication

6. The effect of T-2 toxin on cell-mediated immune status
E D Williamson
(Toxicology Letters)
7. Time course of development of mycotoxicosis after T-2 toxin given by gavage to rats
T C Marrs, S Fairhurst and J Scawin
(Br. J. exp Path)
- 8. The detection of trace levels of trichothecenes mycotoxins in human urine by chromatography-mass spectrometry
R Black, A J Clark and R W Read
(J Chromatogr)
- 9. The detection of trace levels of trichothecenes in human blood using capillary gas chromatography electron captive-negative ions chemical immunisation mass spectrometry.
Foulgar, Beezly, Jeffnis, R Black and Read
(J Chromatogr)
10. Comparative toxicity of trichothecene mycotoxins
S Fairhurst, T C Marrs, D C Parkes, J Scarwin and D W Swanston
(Toxicology)
- ↙ 11. Detection of trace levels of trichothecene mycotoxins in environmental residues and foodstuffs using gas chromatography with mass spectroscopy or electron-captive detection.
R Black, A J Clarke and R W Read
(J Chromatogr)

12. The inter-conversion of T-2 toxin and neosolaniol
D W Anderson, R Black and G Williams
(Tetrahedron Letters)
13. Skin effects of trichothecenes and their amelioration by
decontamination.
S Fairhurst, S A Maxwell, J Scawin and D W Swanston
(Toxicology)

22 June 1987

Dr G S Pearson
Chemical Defence Establishment
Porton Down
Salisbury
WILTSHIRE SP4 0JQ

Your refs: Ptn IF 4900
 Ptn TG 1091/200/87
 Ptn TG 1091/247/87

Dear Graham

I am very much dismayed to find that I failed to respond to your letters of 16 and 25 February, the former on the matter of the Yellow Rain, the latter on the Sverdlovsk anthrax.

In fact I have nothing in particular to convey to you yet on the second matters, other than my thanks for the trouble you took to set out those questions. The contacts are still alive, however, although not at the moment developing as rapidly as we had been given to understand they would.

As for the first matter, I am of course continuing to look forward to a response to the questions I had put in my letter of 6 February; but no doubt the mill is grinding slowly as usual. It was good to have the list of published and forthcoming CDE papers on mycotoxins &c: thank you. The project I mentioned to you is advancing. Enclosed, for your possible interest, is a bit of spinoff from it: the draft for an article recently submitted to an American journal. I am sure there will be a number of revision before it sees the light of day. I hope you will not feel that I have misrepresented the British end of things; if I have, I do hope you will tell me.

With best wishes,

Yours sincerely

J P Perry Robinson
Senior Fellow

22 June 1987

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J P Perry Robinson
Senior Fellow

From: Dr G S Pearson, Director



Procurement Executive Ministry of Defence
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Porton Down, Salisbury, Wiltshire. SP4 OJQ
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W1 c.12
Pearson

Ptn/IF 4900/821/87

Mr Julian Perry-Robinson
Senior Fellow
University of Sussex
Mantell Building
Falmer
Brighton
East Sussex
BL1 9RF

29 June 1987

Dear *Julian,*

YELLOW RAIN

1. Thank you for your letter of 6 February 1987 seeking our help in providing some information for you on the work done by CDE and ARE Holton Heath on the analysis of yellow rain samples. As you point out there is distorted reporting in the press reports that you refer to in your letter and the only authoritative statement is that given in the written reply in Hansard of 19 May 1986.

2. The answers to your questions are as follows:

a. When did CDE analyse YR samples All YR samples were analysed specifically for trichothecenes and using GC/MS techniques. The CDE analyses of environmental samples were carried out between March 1982 and September 1984. Blood samples and a single urine sample were analysed jointly by CDE and ARE (Holton Heath) between November 1983 and April 1984.

b. Technique publications The GS-MS methodology developed by CDE and ARE for the detection of trichothecenes in blood, urine and environmental residues have been published (copies enclosed)

i. Begley P, Black R, Foulger B, Jeffery P and Read R. Detection of trace levels of trichothecenes in human blood using capillary gas chromatography-electron capture negative ion chemical ionisation mass spectrometry. *J Chromatography* 367 87-101 (1986).

ii. Black R, Clarke R and Read R. Detection of trichothecene mycotoxins in human urine by gas chromatography-mass spectrometry. *J Chromatography* 367 103-115 (1986)

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The urine method was not used for the single urine sample mentioned above; this was analysed using the method for blood. The published methods for the analysis of environmental samples were applied to all samples analysed after January 1984 (this represented over 50% of the environmental samples analysed). The earlier samples were analysed using a GC-MS methodology based on that used by the MAFF Food Science Laboratory, Norwich.

c. Numbers of environmental samples About 50 environmental samples were analysed for trichothecenes; all solid samples were examined by microscopy (optical and electron where appropriate). About 70% of the samples were yellow or yellow-brown powders or deposits; all of these consisted predominantly of pollens.

d. Numbers of biomedical samples About 20 biomedical samples were analysed for trichothecenes. Stringent laboratory controls were included in the analyses of these samples. Each analysis was preceded by a glassware blank to ensure freedom from laboratory contamination. Blood and urine samples taken from volunteers were analysed to check for interference from coeluting substances. Spiked samples were used as positive controls.

e. Epidemiological evidence This appraisal comprised a detailed medical review of all available information from the area; this included case histories and eye witness accounts supplied by UK and US enquiries, the Canadian surveys, a UN survey and various reports and case histories supplied by resident medical staff based with the World Vision and International Rescue organisations.

3. The above provides some additional background to the authoritative statement given in Hansard for 19 May 1986.

Yours sincerely



From: Dr G S Pearson, Director

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From: Dr G S Pearson, Director

W1 Pearson



Procurement Executive Ministry of Defence
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Porton Down, Salisbury, Wiltshire. SP4 OJQ
Telephone: Idmiston (0980) 610211 Ext 208

Ptn/IF 4900/940/87

J P Perry Robinson
Senior Fellow
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer
Brighton
East Sussex
BN1 9RF

22 July 1987

Dear *Julian,*

"YELLOW RAIN" AND TREATY COMPLIANCE

1. Thank you for your letter of 22 June 1987 enclosing a copy of your draft article for eventual publication in an American journal. You invited us to let you have any comments we might have on the draft.
2. Our overall feeling is that it is a reasonable appraisal of the situation although in the interests of Anglo-US relations I would not wish such a view to be attributed to CDE. You will by now have had my letter giving you answers to the questions you raised in your letter of 6 February 1987 and this should enable you to firm up some aspects.
3. We note that your paper concentrates on the environmental samples and does not address in depth the reporting of trichothecenes in biomedical samples although subsequent work has shown that the reported trichothecenes would be rapidly metabolized.
4. We have a few comments of detail which are given at the Annex.
5. I would like to say that I would welcome a continuing dialogue with you on the various technical facets of the CW scene as I believe this would be mutually beneficial and of overall benefit to the UK.

With best wishes

Yours sincerely

John

DETAILED COMMENTS

1. Page 5. Last two lines. This particular sample was very dirty, mouldy and generally heterogeneous. It would be more accurate to describe it as

"a sample of yellow rain contained an unusual abundance of pollen".

2. Page 9. Line 6. You refer to lethality and ease of distribution as examples of the aggressive characteristics of the trichothecenes. We would point out that it is their ability to cause skin necrosis and vomiting, accompanied by general malaise, which could be of potential military value rather than their lethal toxicity - even though it is claimed that "yellow rain" caused many deaths. I also believe that there is little that has been done to show whether trichothecenes are easily distributed or not. I suggest your paper might benefit from some rephrasing, perhaps along the lines of

"For example, if lethality and ease of distribution are considered then the characteristics of the trichothecenes appeared inferior to those of ...".

3. Page 15. Line 3. It would be more appropriate to say that

"... they speculated that low level contamination ..."

as we could not really reach such a conclusion from the analysis of only five samples. A better alternative phraseology might be "this indicated" instead of "they concluded" or "they speculated".

4. Page 15. Lines 5 to 12. You are right to point out that these findings have not yet been described rigorously in the scientific literature as until they are, it is hard to be sure what significance should be ascribed to the finding of 2% positives and whether this is statistically significant.

From: Dr G S Pearson, Director

W1 Pearson



Procurement Executive Ministry of Defence
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Yours sincerely

Lyndon

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W1 Pearson

19 August 1987

Your ref: Ptn/IF4900/940/87

Dr G S Pearson
Director
Procurement Executive
Ministry of Defense
Chemical Defense Establishment
Porton Down
Salisbury
Wiltshire SP4 0JQ

Dear Graham,

This is simply a note of very belated thanks for your letter of 22 July, which was greatly appreciated. I have some travels to get through this coming month, but when I am back I would like to pursue one or two points with you.

A much truncated version of that draft will be coming out in early September in the Fall 87 issue of Foreign Policy.

With best wishes,

Yours sincerely,

J P Perry Robinson,
Senior Fellow

W1 Pearson

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With best wishes,

Yours sincerely,

J P Perry Robinson,
Senior Fellow

28 September 1987

Dr Ruth Adams
Director, International Security Program
John D and Catherine T MacArthur Foundation
140 South Dearborn Street
Suite 700
Chicago
Illinois 60603
USA

Dear Ruth

I am now back in harness here and able to write to thank you for the kindness and delicacy with which, at Gmunden, you asked after my MacArthur-funded work and whether I would be looking to you for further support.

As you recommended, I arranged (from Switzerland) for copies of those June/July reports and correspondence from our end to be mailed to you. But I am not sure whether only the University-to-Foundation papers were copied to you or the Robinson-to-Adams correspondence as well. In case only the former, I enclose copies of the latter herewith.

You will see that I have been hoping from word from you on the possibility of the Foundation continuing to support the Harvard-Sussex CBW programme over the next three years, this time at a level of £67,000 per year. If it is now too late to consider this, might there be a chance of a small bridging grant to prevent things from coming to an abrupt end on 1 January 1988? My failure to approach other foundations for support in the meantime has now made all this rather urgent: hence the importunate stickers on the envelope.

With best wishes,

J P Perry Robinson

Encs

cc Matthew Meselson, Harvard

SCIENCE

Last word on yellow rain?

TOXICOLOGISTS at the Research Institute for Animal Disease (BALITVET) in Bogor, Indonesia, have evidence that fungi were the source of the toxins in "yellow rain". Their findings are another blow to those who believe that yellow rain was some sort of chemical weapon.

In 1981, the US State Department claimed that low-flying planes—presumably from the Soviet Union—had discharged a powder containing mycotoxins over areas of Laos, Kampuchea and Afghanistan. Scientists investigating the claim analysed samples of yellow spots on leaves and rocks and identified three mycotoxins: deoxynivalenol, nivalenol and T2, which all belong to a group of toxins called trichothecenes. Many species of the fungus *Fusarium* produce these toxins naturally. *Fusarium* grows on cereal crops in cool climates—but apparently not in warm and tropical parts of the world. An extensive search of the scientific literature failed to turn up any reports of natural mycotoxins in Southeast Asia.

Symptoms of poisoning by trichothecenes include vomiting, haemorrhaging and sudden death. The high death rate following the reported sprayings of yellow powder by Soviet planes and helicopters prompted the initial examination of the substance. Reports from refugees provided circumstantial evidence that someone was

Sandra Ackerman

waging some sort of chemical warfare in the region.

In 1984 and 1985, the scientific consensus was that the main component of yellow rain was digested pollen in the faeces of honeybees. The faeces rained from the sky as thousands of bees flew from their hives on "cleansing flights", a well known phenomenon. This might explain the yellow colour but no one could explain the presence of the trichothecenes.

Last year Raphaella Widiastuti, an analyst at BALITVET, and Douglas Stoltz, a toxicologist working on an Australian Aid project in Indonesia, reported that they had found trichothecenes on corn growing in Central Java. Of almost 300 corn cobs picked at random, 61 per cent showed the reddish-purple discoloration characteristic of *Fusarium*. Analysis by thin layer chromatography (TLC) revealed significant levels of the mycotoxin zearalenone, ranging from 2.5 to 40 parts per million. Zearalenone is not one of the poisons in yellow rain but its presence demonstrated that mycotoxins do occur naturally in Southeast Asia.

Meanwhile, scientists working for the Canadian Ministry of External Affairs detected trichothecenes in the blood of five people in Thailand who had never been to areas supposedly sprayed with yellow rain.

A key piece of evidence has come to light in the past year. The team at BALITVET has now identified deoxynivalenol, nivalenol and T2 in samples of corn growing in Central Java. After analysing their samples by TLC in Indonesia, the researchers sent them to the Queensland Department of Primary Industries in Australia where Barry Blaney and Christopher Moore confirmed the identities of the toxins with gas chromatography and mass spectroscopy.

Trichothecenes are known to be toxic to both humans and livestock—at levels of about 50 parts per million. But there have never been any cases of disease in Indonesia that could be traced to the mycotoxins.

Much of the corn Widiastuti and Stoltz sampled showed the reddish-purple *Fusarium* only at one end of a cob. However, when they broke cobs in half and analysed the apparently unaffected parts, they found that they also contained significant levels of mycotoxins.

So although the fungus is not always immediately obvious and reports so far are restricted to high altitude areas of Central Java, natural infection of grains with *Fusarium* could account for the presence of trichothecenes in the area. There is no good reason to raise the spectre of chemical warfare to explain what happened in Southeast Asia. □

British children hold their own in the IQ stakes

BBRITISH children are brighter than they were. A study of more than a thousand 9- to 11-year olds in England produced an average IQ of 112 points compared with an average of 100 in a similar study half a century ago (*Nature*, vol 328, p 797). But IQ testing and interpretation is fraught with difficulties, and anyway IQs in other developed nations seem to have increased by at least as much.

The new findings come from Richard Lynn and his colleagues at the University of Ulster, Coleraine. They first noticed a trend of increasing IQ among Japanese children in 1982. Japanese children seem to have outstripped their American counterparts by 11 points. However, after making allowances for differences in testing methods, much of the lead disappeared. Lynn and his colleagues planned their latest study to see if Britain had been left behind in the IQ stakes.

The IQ test, a non-verbal test devised in 1935 by Raymond Cattell, is thought to be "culture fair". In other words, it does not put children of different racial or cultural backgrounds at a disadvantage. The test involves pictures and diagrams rather than words and language. Psychologists believe that it tests innate intelligence rather than upbringing and educational attainment.

However, its value in comparing generations, nations and cultural or social groups is still controversial.

The results of the study suggest that the IQs of children in developed countries have



Sally and Richard Greenhill

The answers should come easier these days

increased in the past few decades by between 2 and 12 points a decade. On that basis, the English performance is not outstanding. In 50 years, IQ has increased by only 2.48 points a decade, although the rate of increase has speeded up to 3.19 points a decade since 1949.

The poorer performance in the years of the Second World War may be a clue to one underlying reason for the improvement—nutrition. Lynn suggests that imp-

roved health and a better diet played a part in the improvements in Japan. Another factor might be changing social attitudes. As long ago as 1977, Lynn noticed that what he calls "extroverted tendencies"—recklessness and "non-conservative" social attitudes—tend to result in higher scores in this form of IQ test. Perhaps improvements in IQ have led to increasing crime rates, dissent and self-expression rather than to academic success. □

→ MS M

**Evidence shows US administration
used intelligence for political ends**

**CIA director
pledges to
end doctoring
of reports**

From Michael White
in Washington

Persistent allegations that the Reagan administration has systematically manipulated the advice of US intelligence agencies to support its own political agenda have stung the CIA's new director into a public pledge that future reports will be fully independent.

The move comes as renewed evidence has emerged that in 1981 the administration consciously overrode the doubts of its scientific analysts about "Yellow Rain" in south-east Asia and launched a worldwide propaganda campaign against the supposed Soviet chemical warfare policy, designed in part to persuade European public opinion that cruise and Pershing missile installation was necessary.

Judge William Webster, the new broom who brought a wholesome reputation with him when he moved from the FBI to sort out the Irangate legacy left by the late William Casey to the CIA, has told senior colleagues he is determined to end doubts about the reliability of his analysts' conclusions about the raw data their colleagues amass.

And in a statement at the weekend which infuriated some critics as mere playing to the gallery, he pointedly declared: "Important estimates are now fully addressed at senior levels throughout the intelligence community and divergent points of view are disclosed in the finished product. The reports are furnished without any regard to whether they support a particular policy line and they may not be altered for that purpose."

That is a not-very-coded way of attacking his deceased predecessor's widely-suspected habit of feeding the Oval Office the kind of news the President wanted to hear about support for the Nicaraguan contras.

In one effort, Iran was presented as reducing its support

Iran-contra hearings fuelled long-standing concern.

Such charges are rarely clear-cut and the agency's defenders legitimately point out that it is attacked from both left and right for producing analyses which displease. Conservatives like Senator Jesse Helms have long complained that the CIA is soft on Soviet military capability and intentions and that during the Vietnam war its assessments were doggedly pessimistic.

Nor is Mr Casey the only policy-maker so charged, merely the most energetic in recent years and the most attracted to "Reagan Doctrine" covert operations.

The "Yellow Rain" controversy arises from CIA accounts that Cambodian and Laotian refugees reported toxic yellow powders falling from the sky which resulted in death and illness.

The critical decision to go public in a major campaign is attributed to the then Secretary of State, Mr Alexander Haig, and his Assistant on Political and Military Affairs (now ambassador to Bonn), Mr Richard Burt. Notwithstanding the warnings of the Pentagon toxicologist who first analysed a mouldy yellow leaf sample that a premature campaign based on one sample was "ill-advised", they spoke out in 1981.

Subsequent analysis, in the US and at Porton Down in England, have cast grave doubt on the chemical warfare theory — though it still has its adherents. The Washington Post reported on Sunday that President Reagan continued to speak out even as his own government accumulated evidence to the contrary.

The whole story, a Harvard biochemist, Mr Matthew Meselson, concludes in the new edition of Foreign Policy magazine, shows how the intelligence community "departed from established procedures for verifying

Russia's yellow rain was bee faeces, pundits say

From the Reagan administration's frequent charges of Soviet arms violations — that they supplied a toxin nicknamed "yellow rain" to the Khmer Rouge operating in Laos and Cambodia — has been severely discredited by official documents released under the Freedom of Information Act. The documents, obtained by Jeanne Garbus, a sociology professor at Boston College, and also by the *Washington Post* that while US officials, including President Reagan, were making the charges against the Russians, US government researchers in South-East Asia were gathering important information that supported the accusations. The charges were first made in Sep-

tember 1981 by then Secretary of State Alexander Haig, who said the US had evidence the Russians had devised a mysterious new weapon containing toxins called trichothecenes, made by a fungus.

The weapon appeared to be part chemical — the toxin — and part biological — the fungus. The US formally charged the Russians with violating the 1925 Geneva Protocol banning the use of chemical and biological weapons, and also the 1972 Biological Weapons Convention, which bans their manufacture and stockpiling.

Refugees from battlegrounds in Laos and Cambodia interviewed by US officials had described the new weapon as "yellow rain" that left a yellow deposit on vegetation. A team of independent scien-

From Peter Pringle
in Washington

tists led by a Harvard biochemistry professor, Matthew Meselson, examined samples of the yellow spots, and revealed they were bee faeces.

Embarrassed US government officials sought to ridicule the Meselson revelations, but government scientists soon admitted the spots were bee faeces. The rest of the scientific evidence, including extensive analyses from Britain's chemical warfare laboratories at Porton Down, did not support the US charges. Still, US officials continued to state "yellow rain" was a new, illegal Russian weapon, on the

grounds of refugee evidence that people had become severely sick or died from it.

However, the recently released documents show that when US officials returned to South-East Asia in 1983-5 to re-interview the original refugees who had told of the "yellow rain" they got a markedly different story. In an article in the American quarterly, *Foreign Policy*, published today, Meselson, Guillemin and Britain's leading independent chemical warfare expert, Julian Perry Robinson, say documents show the officials discovered "serious problems with the reliability of the previous interviews."

The documents tell of one refugee who had seen chemical attacks "with his own eyes and witnessed six deaths", but when

re-interviewed said he had not seen the attacks at first hand but had only heard about them. A Laotian resistance commander, who had operated in an area where refugees had spoken of 200 chemical attacks, said he had never experienced any. He suggested the stories might have been made up by peasants.

In Washington none of these doubts was ever mentioned by US officials who continued to charge the Russians with using "yellow rain". The authors of the article conclude that "in retrospect, the US intelligence community departed from established procedures for verifying information and instead supported a conclusion that should have been regarded only as a hypothesis."

p6

Monday 31 August 1987

THE INDEPENDENT

Cambodge: *Foto CW* la guerre chimique

par Richard Sola

Au-delà de l'aspect politique ou diplomatique, l'occupation du Cambodge est aussi un théâtre d'opérations militaires. Opérations particulières qui visent à tester certaines armes chimiques, dont certaines sont désormais connues du grand public puisqu'on les retrouve en Afghanistan.

Les substances toxiques

Si l'on en croit les informations fournies par certains soldats du régime cambodgien de Heng Samrin, qui ont déserté pour se réfugier en Thaïlande, les forces d'occupation vietnamiennes utilisent deux types principaux de produits toxiques: une poudre brune et une poudre rose. Ces produits ont été fournis aux Vietnamiens par les autorités soviétiques pour être testés au Cambodge. Pour l'heure, ils ne semblent pas avoir été utilisés au Laos. Lorsque les troupes régulières cambodgiennes ont recours à ces produits chimiques, cela se déroule toujours sous la supervision d'officiers vietnamiens. Il est à noter que si certains obus chimiques d'origine américaine, datant des années soixante-dix, sont quelquefois employés, leurs effets ne sont pas mortels.

Les opérations de bombardement chimique avec des armes soviétiques sont souvent effectuées avec des mortiers K-61, K-82 et K-205. Les personnes contaminées par ces produits chimiques ont les symptômes suivants: suffocation, dessè-

chement de la gorge, maux d'estomac, halètement, brûlure de la gorge, troubles visuels et déjections fécales de couleur noire. Après un traitement d'un mois à un mois et demi, les blessés se rétablissent mais on observe des rechutes graves. Durant la convalescence, le patient est très faible, a un comportement similaire à celui d'un homme ivre et est atteint de pertes de mémoire.

Les armes chimiques

Quatre types d'obus sont fréquemment utilisés au Cambodge lors des combats qui opposent les petites unités de la résistance khmère rouge aux troupes vietnamiennes:

— obus N° 52. Il s'agit probablement d'une munition d'origine chinoise ou nord-coréenne bien qu'il soit difficile d'identifier les marques qui y sont inscrites. Lorsqu'il explose, l'obus dégage une fumée de couleur blanche qui provoque la suffocation.

— obus N° 141. Matériel de fabrication soviétique (quelquefois cubaine) contenant une substance chimique N° 94 = 7462. Les obus sont tirés par des

5

mortiers de 38 mm. La substance provoque une mort instantanée.

— obus K-60 MM. Tiré par un mortier de 61 mm, cet obus dégage en explosant au sol une odeur très puissante qui entraîne un évanouissement.

— obus et grenade chimiques. Leur utilisation remonte au mois de décembre 1986. En explosant en l'air, l'obus ou la grenade répand une poudre jaune qui se dépose en particulier sur la végétation. Lorsqu'un individu vient à toucher ou à respirer cette poudre, il est atteint d'évanouissements et de vomissements. Chose surprenante, la poudre provoque immédiatement des hémorragies cutanées à l'endroit où la peau a été en contact avec la substance.

Des exemplaires des obus des trois premiers types et des échantillons de substance chimique ont été obtenus par les soldats khmers rouges appartenant au bataillon N° 340 commandé par M. Wong, opérant non loin du district de Ban Guad. Certaines autres armes de lutte antipersonnelle ont été introduites par les Soviétiques. Parmi celles-ci on trouve une mine de type OZM-3 dont le détonateur est incorporé et une autre mine MYB-Z, qui se présente sous la forme d'une boîte en bois contenant du TNT et des pointes de métal qui sont autant de projectiles qui déchiquettent tout lors de l'explosion.

Top secret

Moscou

Selon la *Pravda* une bande de contre-révolutionnaires afghans se serait infiltrée le 9 avril 1987 dans le territoire de *Tadjikistan* soviétique en tuant deux gardes frontaliers. La *Pravda* n'a pas précisé s'il y avait eu des morts ou des blessés dans les rangs des attaquants. Mais elle a lancé un avertissement selon lequel l'Union soviétique prendrait les mesures nécessaires pour assurer la sécurité de son territoire. La *Pravda* avait rapporté quelques jours plus tôt que les résistants afghans avaient attaqué la ville de Pianj (*Tadjikistan*) avec des roquettes à travers l'*Amou Daria* qui forme la frontière entre l'Afghanistan et l'Union soviétique.

D'autre part, un résistant afghan Sayed Abdur Rauf, actuellement sous traitement à Washington, a révélé qu'il avait personnellement posé des mines en territoire soviétique dans la région du *Turkménistan* limitrophe de l'Afghanistan, où les deux pays ont une frontière commune.

Le Congrès mondial juif en faveur de Gorbatchev

Les prises de position en faveur de Gorbatchev d'Edgar Bronfman, président du Conseil mondial juif, et de Morris Abram, président de la conférence nationale des organisations juives aux USA, ont entraîné un durcissement de la part de la puissante fraction antisémite de l'appareil soviétique qui dément les chiffres articulés quant aux visas accordés aux « Refuzniks » ainsi que la reprise imminente des relations diplomatiques entre Israël et l'URSS.

De plus, les propos d'Israël Singer au *Jewish Times* (13/26 mars), journal américano-canadien, selon lesquels le monde juif devait soutenir Gorbatchev qui n'a pas encore consolidé son pouvoir, apportent des éléments à la propagande développée en URSS contre le premier secrétaire du Parti. Ces propos iront grossir le dossier, dont la cassette contre Raïssa Gorbatcheva n'est qu'un élément mineur.

LIVRES NOUVEAUX

Au-delà de la verticale

par Georges Livanos

A 34 ans, Georges Livanos en comptait déjà 20 d'escalades, soit 5000 heures pour 150.000 mélos du IV^e au VI^e supérieur; 18.000 pitons dont 12.000 plantés de sa propre main; 700 rappels; 300 « premières »; 38 bivouacs.

Il fait ses premières armes dans les Calanques, confirme son art dans les Alpes occidentales, et se voit consacré dans les Dolomites un des meilleurs grimpeurs de sa génération.

Spécialiste d'escalade artificielle, aucune paroi, fût-elle la plus surplombante, la plus « au-delà de la verticale », ne résiste à ses assauts et à sa technique.

Ce récit, d'un style alerte où l'humour méridional ne perd pas ses droits, l'a consacré depuis un quart de siècle comme un des grands conteurs de l'escalade et de l'alpinisme.

Editions Flammarion, Paris.

Le Centre d'Etude et de Prospective International (C.E.P.I.) suit pour vous l'évolution économique et financière de 150 pays répartis à travers le monde.

Il publie toutes les semaines une lettre télex, qui vous permet de vous tenir au courant de l'essentiel de l'actualité en matière de « RISQUE ».

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13 JAN 1988



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Dr. J. Perry Robinson
Science Policy Research Unit
University of Sussex
Mantell Building
Falmer Brighton
EAST SUSSEX BN1 9RF
UNITED KINGDOM

Date
7 January 1988

Our ref.
88 CR 27

Subject
Yellow rain

Your letter

Dear Dr. Perry Robinson,

With interest I read your publication in Foreign Policy, number 68, 1987, about 'yellow rain'.

On page 110 you mention 'a written statement to the House of Commons' of May 1986 by the British government. I would be very grateful to receive a reference or a reprint giving the text of this statement.

On the same page you write: 'Nor had any such samples yielded positive results in the French or Swedish defense laboratories'.

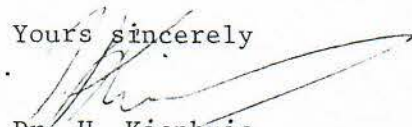
I would highly appreciate to receive references to the results of these analyses, or reprints if available to you.

On page 111 you mention 'no positives out of more than 100 such samples examined in government laboratories'. I would also appreciate very much to have one or more references to these results.

I apologize to burden you with so many questions but it is essential for us to base our conclusions if possible on information published by the original sources.

I take the opportunity to wish you a very good 1988.

Yours sincerely



Dr. H. Kienhuis
Deputy Director
Chemical Research

Enclosures

W2 Pearson

From: Dr G S Pearson, Director



Procurement Executive Ministry of Defence
CHEMICAL DEFENCE ESTABLISHMENT

Porton Down, Salisbury, Wiltshire. SP4 OJQ
Telephone: Idmiston (0980) 610211 Ext 208

IF 4900/114/88

J Perry Robinson
Senior Research Fellow
University of Sussex
Mantel Building
Falmer
Brighton
East Sussex
BN1 9RF

3 February 1988

Dear *Julian,*

YELLOW RAIN

1. I am grateful to you for the copies you sent me of some of the information you obtained from the US under the Freedom of Information Act.
2. I would like to enquire whether you were able through that Act to obtain a copy of a letter report entitled "Yellow Rain - Separating Fact from Fiction" by Dr Sharon A Watson of AFMIC and dated 18 June 1984. If you did, I would be very grateful if you could send me a photocopy.

With best wishes

Yours sincerely

Graham

W3. Robinson

Dr H Kienhuis
TNO Prins Maurits Laboratorium
PO Box 45
2280 AA Rijswijk ZH
The Netherlands

Your ref: 88 CR 27

February 9, 1988

Dear Dr Kienhuis,

Further to my letter of 13th January, I am now able again to take up yours of 7th January. You were asking for references on four particular items of information conveyed in the Foreign Policy paper, as follows.

- (1) The Swedish defence laboratories have had no positive findings for trichothecenes in the 'yellow rain' samples which they examined by GC/MS.
- (2) The French defence laboratories likewise.
- (3) More than 100 environmental 'yellow rain' samples were examined for trichothecenes by GC/MS in government laboratories.
- (4) None of those examinations yielded positive results.

In all cases we were relying on information provided by the investigators concerned but not yet published by them. Corroboration, however, is not entirely absent from the published literature. There is mention of both (1) and (2), and at least allusions to (3) and (4), in Science 234: 528 (remembering that, on (2), our reference was to findings from GC/MS, not from such techniques as TLC).

Item (4) is of course the one on which we were on least reliable ground, for GC/MS analyses of environmental samples might conceivably have been conducted in government laboratories in countries where we had neglected or been unable to make the appropriate inquiries. But we judged this to be only a slight risk, and nothing has transpired since publication of the article to make us regret having taken it.

For your private information, I enclose copies of letters dated 29 June and 22 July 1987 addressed to me by Dr Graham Pearson, the director of CDE Porton Down. This is in regard, especially, to the arithmetic underlying (3); Science 221: 242 tells us that Edgewood has done GC/MS on at least 60 environmental samples.

Yours sincerely,

J P Perry Robinson
Senior Fellow

encs

W3. Robinson

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J P Perry Robinson
Senior Fellow

encs

WZ Pearson

Dr G S Pearson
Director
CDE Porton Down
Salisbury, Wilts

SP4 OJQ

February 9, 1988

Your ref: IF 4900/114/88

Dear Graham,

Thanks very much for your letter of 3rd February. Documents continue to come through to us in response to our original FoIA requests, but not yet the letter-report by Sharon Watson to which you referred.

I am able, however, to send you a copy of its 'unclassified' pages. It is enclosed, together with a copy of a somewhat similar document which did come through FoIA: a memorandum dated some two weeks later by Carolyn Stettner of USACDA.

The Swedish Government is, I gather, on the verge of releasing the final FOA report on YR, which includes some interesting interview data as well as accounts of sample analyses, but which has been politically bothersome to the Swedes because of its mention of positive findings for CW agent (in samples of munition fragments, I understand), namely CS2.

It was good to see you so prominently depicted in the pages of Soviet Military Review (the December 1987 special supplement on Shikhany)!

With best wishes,

Yours sincerely,

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With best wishes,

Yours sincerely,

J P Perry Robinson

encs

Kaplan

Dr Martin Kaplan
Pugwash Executive Office
11A, Avenue de la Paix
1202 Geneva

Switzerland

17 October 1990

By telefax to Switzerland 7337313

Dear Martin,

This is in response to your fax of 3rd October in which you sent for comment the draft WHO briefing dated 29 September, 'Biological Warfare Threat in Middle East'. My comments are as follows:

Page 1, para 1. What Les Aspin actually said, according to the Washington Post for 29 September (which was presumably what Radio France was quoting), was: "Saddam Hussein...is expected to have a militarily significant biological [weapons] program by the end of this year or early next year. This will be a new dimension to the problem. It is a more important and more serious element than the chemical threat. It is a harder thing to deal with."

Page 1, para 3. The suggestion here is that the WHO has sufficient information about Iraqi activities to permit this strikingly clear ranking of the possible BW threats, a ranking which leaves out many other possibilities.

Page 2, 1979. The US Sverdlovsk allegation was first voiced publicly, not in the UN Disarmament Commission, but during the first review conference (Geneva, 1980) of the 1972 Biological Weapons Convention.

Page 2, 1979. The word 'admitted' implies guilt, but there was nothing necessarily illegal in there being a military research facility, even one working with anthrax, at Sverdlovsk.

Page 2, 1981. In comparison with the various other items of anthrax-weapon history that might have been mentioned, this one scarcely merits inclusion, surely?

Page 4, bottom. 'They deliver more destruction per kg than any other biological or toxin weapon' is a most questionable statement. One reason

why anthrax spores never became an authorized fill for USAF BW weapons was that the human inhalatory ID50 was reckoned to be at least 8000 spores and very possibly an order of magnitude more. In contrast, the bacterial agents which were authorized had estimated ID50s a thousand times smaller and with narrower margins of uncertainty.

Page 4, later. '50 kg' of what? In terms of the particular slurry with which the anthrax bombs would have been filled (an aqueous slurry comprising about 4 percent by weight of actual spores), the munitions expenditure envisaged for the raids on Aachen etc noted on page 2 corresponded to about 1050 kg of slurry per square kilometre of target area. The expenditure was calculated for a 50 percent casualty rate.

Page 8, top Maybe the figures given for the relative toxicity of botulin should be checked? The implication that the LD50 in man of ricin is 30,000 times smaller than that of tabun looks odd to me.

Page 8, top It was for aerosol weapons, at least as much as a water contaminant, that botulin was studied in past BW programmes.

Page 8, bottom The manuscript emendations here convert the 1969/70 WHO estimates of the typescript into something altogether more fanciful. Has this been done advisedly? Recall that the munitions expenditures prescribed for the original botulin aerosol weapons in WW2 corresponded to an application density of about 3500 kg of botulin preparation per square kilometre; and recall too that, in the late 1960s, a fill of sarin nerve gas for the warhead of the Lance ballistic missile was estimated to be more lethal than a fill of the then-current botulin formulation.

With best wishes,

Yours sincerely,



J P Perry Robinson
Senior Fellow

cc Dr M S Meselson, Harvard University

Robinson F4



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FAX TRANSMISSION FORM

DATE: 19 Mar 91

TO: M S Meselson

DEPARTMENT: Dept of Biochem + Mol Biol, Harvard C.

ORGANISATION:

FAX NO: 010 1 617 495 8308

FROM:

Julian Robinson

Total Number of Pages Transmitted (Including Cover Page): 6

Please contact ~~Janie Scroggie~~ on the above telephone or fax number if there are any problems concerning this transmission.

→ Lisa Donovan

MESSAGE:

Here are the Swarthmore (YR) bits from Graham P.'s paper for the conference? They be at tomorrow. I'll ^{mail} ~~bring~~ the whole thing. ~~with~~

See you on December 7th.

Best - Julian



30 October 1991

BIOLOGICAL WEAPONS: THEIR NATURE AND ARMS CONTROL

by

DR GRAHAM S PEARSON CB

Director General, Chemical and Biological Defence Establishment
Porton Down, Salisbury SP4 OJQ, United Kingdom

INTRODUCTION

1. The term Biological Weapons is little understood and little appreciated by the public at large. Whilst there is some comprehension of chemical weapons largely arising from the use of such weapons in World War I and the lasting images of incapacitated troops in that war together with the use by Iraq of chemical weapons against Iran and against its own Kurdish population in the mid 1980s, there is no such general appreciation of biological weapons or biological warfare. All too often, biological warfare conjures up images of uncontrollable epidemics attacking both aggressor and attacked. Not so, as although transmissible BW agents could be selected by an aggressor, there are many BW agents that are non-transmissible and will only affect the target population as the aggressor either takes care to deliver the BW agent in such a way that his forces are not exposed or they are protected either through immunisation of the body by vaccination or by physical protection using respirators. Additionally, by selection of the BW agent used, the outcome for the attacked population can be incapacitation or death.

2. The revulsion against the use of chemical weapons in World War I led to the Geneva Protocol of 1925 which prohibited the use of chemical weapons and was extended to include a ban on the use of biological (bacteriological) weapons:

"That the High Contracting Parties, so far as they are not already Parties to Treaties prohibiting such use, accept this prohibition, agree to extend this prohibition to the use of bacterial methods of warfare and agree to be bound as between themselves according to the terms of this declaration."

3. Although the 1925 Geneva Protocol has now been signed by over 125 States Parties, some 30 of the major nations which signed the Protocol have done so with a reservation. Thus the United Kingdom signed with the following reservation:

HISTORICAL PERSPECTIVES

7. Allegations of biological warfare go back to ancient times. Table 1 includes some of the conflicts in which BW is alleged to have been used.

8. Historically, 190 BC saw Hannibal winning a naval victory by firing earthen vessels full of venomous snakes into the ships of the enemy. In the 18th Century, the Russians threw bodies of plague victims into Swedish cities, the British used smallpox-contaminated blankets (Fort Pitt, Ohio) and Napoleon in his Italian campaign attempted to infect the inhabitants of the besieged city of Mantua with swamp fever. The next significant mention of biological warfare comes in World War II with the activities of Unit 731 in Japan. Although there is a paucity of definitive information, it appears certain that Japan used prisoners of war to evaluate the effects of various candidate BW agents. There are also allegations of use of biological warfare by Japan against China in the 1930's.

9. Another landmark was the use of Gruinard Island in 1942-43 to carry out trials to determine whether anthrax spores could be disseminated from bombs and whether such spores borne downwind would infect a flock of sheep. The experiments showed that this could be done and Gruinard remained a prohibited place until decontaminated in 1986 and returned to its owner in 1990. It is, however, important to recognise that the actual hazard from anthrax spores on Gruinard Island during the intervening years were slight. In the early years, the extent of the contamination was unknown. In reality, the risk of sufficient anthrax spores being re-aerosolised to present a hazard was minimal. As the island was to be returned to its original owners, the MOD considered that all traces of anthrax should be destroyed which led to the island being decontaminated in 1986 using treatment with a 5% solution of formaldehyde in sea water leading to it being declared safe for man and beast and returned in 1990 to its original owners (ref 1).

10. The agreement of the Biological and Toxin Weapons Convention in 1972 prohibits the development, production and stockpiling of biological and toxin weapons represented the next milestone. This Convention is discussed at some length later in this paper.

11. The next milestone in the story of biological warfare comes with the outbreak of anthrax in the Soviet city of Sverdlovsk. Early in 1980, articles appeared in various Western newspapers claiming that in April 1979 there had been an explosion at "Military Village 19" in the Sverdlovsk area in which anthrax bacteria had been released and resulted in a number of deaths reported as ranging from 300 to 1000. The US State Department asked the USSR for an explanation in March 1980 which produced a reply that an outbreak of Siberian fever (ie anthrax) had indeed occurred in March/April 1979 in the Sverdlovsk area but that it had been caused by the illegal sale of infected meat. The US State Department issued a statement later in March indicating that it did not consider the Soviet response plausible and remained concerned about the incident as the US had ruled out natural causes as a likely explanation.

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LIST OF REFERENCES

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16. *US Information Service*, 16 July 1990. *Jane's Defence Weekly*, 14 July 1990, p 51.
17. Statement on the Defence Estimates, Britain's Defence for the 90s, Cmd 1559, London, HMSO, July 1991

W1 Pearson



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FAX TRANSMISSION FORM

DATE: 19 Mar 91

TO: M S Meselson

DEPARTMENT: Dept of Biochem + Mol Biol, Harvard C.

ORGANISATION:
FAX NO: 010 1 617 495 8308

FROM: Julian Robinson

Total Number of Pages Transmitted (Including Cover Page): 6

Please contact Jane Scroggie on the above telephone or fax number if there are any problems concerning this transmission.

→ Lisa Donovan

MESSAGE:

Here are the Starbuck/72 bits from Graham P.'s paper for the conference? They be at tomorrow. I'll ^{mail} ~~bring~~ the whole thing. ~~with~~

See you on December 7th.

Best - Julian



30 October 1991

BIOLOGICAL WEAPONS: THEIR NATURE AND ARMS CONTROL

by

DR GRAHAM S PEARSON CB

Director General, Chemical and Biological Defence Establishment
Porton Down, Salisbury SP4 OJQ, United Kingdom

INTRODUCTION

1. The term Biological Weapons is little understood and little appreciated by the public at large. Whilst there is some comprehension of chemical weapons largely arising from the use of such weapons in World War I and the lasting images of incapacitated troops in that war together with the use by Iraq of chemical weapons against Iran and against its own Kurdish population in the mid 1980s, there is no such general appreciation of biological weapons or biological warfare. All too often, biological warfare conjures up images of uncontrollable epidemics attacking both aggressor and attacked. Not so, as although transmissible BW agents could be selected by an aggressor, there are many BW agents that are non-transmissible and will only affect the target population as the aggressor either takes care to deliver the BW agent in such a way that his forces are not exposed or they are protected either through immunisation of the body by vaccination or by physical protection using respirators. Additionally, by selection of the BW agent used, the outcome for the attacked population can be incapacitation or death.

2. The revulsion against the use of chemical weapons in World War I led to the Geneva Protocol of 1925 which prohibited the use of chemical weapons and was extended to include a ban on the use of biological (bacteriological) weapons:

"That the High Contracting Parties, so far as they are not already Parties to Treaties prohibiting such use, accept this prohibition, agree to extend this prohibition to the use of bacterial methods of warfare and agree to be bound as between themselves according to the terms of this declaration."

3. Although the 1925 Geneva Protocol has now been signed by over 125 States Parties, some 30 of the major nations which signed the Protocol have done so with a reservation. Thus the United Kingdom signed with the following reservation:

HISTORICAL PERSPECTIVES

7. Allegations of biological warfare go back to ancient times. Table 1 includes some of the conflicts in which BW is alleged to have been used.

8. Historically, 190 BC saw Hannibal winning a naval victory by firing earthen vessels full of venomous snakes into the ships of the enemy. In the 18th Century, the Russians threw bodies of plague victims into Swedish cities, the British used smallpox-contaminated blankets (Fort Pitt, Ohio) and Napoleon in his Italian campaign attempted to infect the inhabitants of the besieged city of Mantua with swamp fever. The next significant mention of biological warfare comes in World War II with the activities of Unit 731 in Japan. Although there is a paucity of definitive information, it appears certain that Japan used prisoners of war to evaluate the effects of various candidate BW agents. There are also allegations of use of biological warfare by Japan against China in the 1930's.

9. Another landmark was the use of Gruinard Island in 1942-43 to carry out trials to determine whether anthrax spores could be disseminated from bombs and whether such spores borne downwind would infect a flock of sheep. The experiments showed that this could be done and Gruinard remained a prohibited place until decontaminated in 1986 and returned to its owner in 1990. It is, however, important to recognise that the actual hazard from anthrax spores on Gruinard Island during the intervening years were slight. In the early years, the extent of the contamination was unknown. In reality, the risk of sufficient anthrax spores being reaerosolised to present a hazard was minimal. As the island was to be returned to its original owners, the MOD considered that all traces of anthrax should be destroyed which led to the island being decontaminated in 1986 using treatment with a 5% solution of formaldehyde in sea water leading to it being declared safe for man and beast and returned in 1990 to its original owners (ref 1).

10. The agreement of the Biological and Toxin Weapons Convention in 1972 prohibits the development, production and stockpiling of biological and toxin weapons represented the next milestone. This Convention is discussed at some length later in this paper.

11. The next milestone in the story of biological warfare comes with the outbreak of anthrax in the Soviet city of Sverdlovsk. Early in 1980, articles appeared in various Western newspapers claiming that in April 1979 there had been an explosion at "Military Village 19" in the Sverdlovsk area in which anthrax bacteria had been released and resulted in a number of deaths reported as ranging from 300 to 1000. The US State Department asked the USSR for an explanation in March 1980 which produced a reply that an outbreak of Siberian fever (ie anthrax) had indeed occurred in March/April 1979 in the Sverdlovsk area but that it had been caused by the illegal sale of infected meat. The US State Department issued a statement later in March indicating that it did not consider the Soviet response plausible and remained concerned about the incident as the US had ruled out natural causes as a likely explanation.

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MIKULAK

MemCon: Bob MIKULAK on telephone, 9 December 1991

Woods Hole. Three-way conversation MSM/JPR.

- 1) He expects the US capability proposal will be tabled at the CD very soon.
- 2) Encouraging about the Pakistan meeting. March would be good. Participants he advocated were OOMS, GEE, Mike WALLS (from CMA: "knowledgable and persuasive"), GIZOWSKI. Less ringing: Graham COOPER ("certainly knowledgable"). Opposed: Tamar POSNER (representing industry interests exclusively; not sure what she would say; may come on too strong), Ron NELSON.
- 3) Two suggestions on guest writers for CWCB: (i) VON WAGNER, maybe on finishing the Convention. (ii) Alexei SEMEIKO, who was on the USSR CD delegation this past summer, in his early thirties, and is now Political Director of the Russian Foreign Ministry. Might write on destruction of CW stocks. Contact him through Nikita SMIDOVICH.
- 4) Advice on whether we should pay SMIDOVICH's fare to the January Pugwash: he is now "pretty much decoupled from the CW negotiations"; better to pay for a Russian from Moscow, such as SEMEIKO.
- 5) He himself won't be in Geneva at the time of the Pugwash. We should invite Sherry MANNIX, and Ron NELSON too.
- 6) Tear gas: the US keeps trying to push it aside.
- 7) Challenge inspection: lots of discussion in Geneva this past 1-2 weeks.
- 8) US objective is to get the Convention done in May 1992.
- 9) He will be in Geneva during 12-20 December.

Carpenter

January 22, 1992

Dr. Will D. Carpenter
Vice President, Technology
Monsanto Agricultural Company
800 N. Lindberg Blvd.
St. Louis, MO 63167

Dear Will:

We are delighted to hear that you will be awarded the AAAS Hilliard Roderick Prize in Science, Arms Control, and International Security. It helps express the gratitude of many of us for your wise counsel and leadership toward preventing the exploitation of biomedical technologies for hostile purposes.

Warm regards,

Matthew Meselson

Julian Perry Robinson



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Kama

Ambassador Kamal
Permanent Mission of Pakistan, Geneva
56 rue de Moillebeau
1211 GENEVA 19
Switzerland

18 February 1992

By fax to 01041-22-734.8085

We very much appreciate, Matthew Meselson and I, your efforts for the projected CWC seminar in Islamabad. Over this past week, since you and he were last in touch, we have been consulting about alternative dates with potential participants. What we have been finding is that the periods immediately after Ramadan that are compatible with our academic calendars, in May and June, present problems; so much so that we have come to wonder whether it might not be preferable to postpone the seminar until further into the future. Maybe, for example, we might hold it after the next UN General Assembly?

Your advice on such a course of action would be greatly welcomed. A possible benefit of delay is that it could allow Meselson and me to make a visit to Pakistan for purposes, with your help, of exploratory discussion. We would anyway enjoy a chance of talking with interested people, especially people in academia, government, the military, the news media and industry.

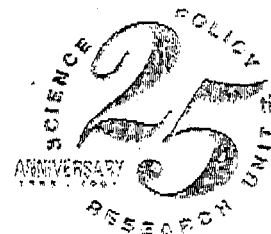
I am just now on my way to the USA where, during 25-29 February, I shall be in Harvard. My hope is that, during those days, Meselson and I will be able to take the planning for the seminar a stage further with you. We look forward keenly to hearing from you.

With best wishes,

J P Perry Robinson
Senior Fellow

cc Dr M S Meselson (by fax to 0101-617-495-8308)

FEB 18 1992



Monsanto

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3/13/92

MAR 17 1992

Matthew Meselson
Dept. of Biochemistry
7 Divinity Avenue
Harvard University
Cambridge, MA 02138

Dear Matt:

Thank you for sending the document on disabling chemicals to me for comment.

To the extent the treaty language can remove ambiguities in the area of riot control, we should try to do it. The risks are, or I see them, as follows:

1. What will be the delays, if any, in reaching agreement on the treaty - weighed against the need for clarification and reinforcement on this issue.
2. The definition of toxic chemicals, when translated to the action of the national authority, and regulatory impact in the industry, could end up a bureaucratic nightmare. Under the definitions, aspirin would qualify. Couple that with the national authority wanting a listing of all "toxic chemicals," and you can see the potential problem.

I don't see the issue of modifying the treaty from its current position as justification for not signing the treaty. I think we (society) would be better off with a treaty that is, in this issue, flawed, than no treaty at all.

On the issue of valid verification, as opposed to this issue, we simply must have a procedure that gives an assurance that those who would do evil will not do so because of the risk of detection.

Matthew Messelson

Page 2

You've probably thought of it already, but would a small, very low-key discussion ($\frac{1}{2}$ day) with a few people from ACAD, DOD, DOS, etc., be of value?

Sincerely,

A handwritten signature in cursive script, appearing to read 'M. Messelson', written in dark ink.

/cs

DeBisschop

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FROM: H.C. DeBisschop

TO: Mr Julian FERRY ROBINSON

SPRU University of Sussex

Fax: 0044 273 685865

NUMBER OF PAGES (THIS PAGE INCLUDED): 4

DATE: april 30, 1992

MESSAGE:

Dear Julian, Dear Matthew,

(in volume)

My comments go hereby! It is less than I estimated, but legal problems seem to be at least as difficult -if not more difficult- than technical ones.

Sincerely,
Herbert

Riot Control Agents - Comments

Some Background information.

In any major conflict where chemical warfare was employed, "tear gas" was also used. In fact the use of tear gas was generally introductory to the use of other chemical weapons. The Vietnam conflict is -to my knowledge- the only major war where the escalation to other chemical agents did not take place.

A possible definition of such compounds is given by Hanslian:

"Reizstoffe in militärischen Sinne sind chemische Kampfmittel, bei denen eine nachhaltige oder gar todbringende Schädigung nur deshalb nicht in Erscheinung tritt, weil sie durch ihre sofort oder sehr bald auftretende reizwirkung den Gegner zwingen sich ihnen zu entziehen. Ist der Gegner dazu nicht in der Lage, so können sie auch tödlich wirken."¹

The use of such chemicals for domestic law enforcement and riot control purposes seems to be generally accepted. In Belgium, such agents are normally used by collectively acting police forces (usually specially trained units); it is accepted that such actions occur under a hierarchical and political control.

The problem caused by this dual nature of tear gas is also present in armed conflict of a non-international nature. So, even if the international common law presumes an absolute interdiction on the use of chemical weapons, its extension to domestic armed conflicts is not trivial, as far as "tear gas" is concerned:

"Dans toute analyse sur l'interdiction coutumière des armes chimiques, la question de la portée de cette interdiction, quant aux types d'armes concernées, se pose. Il semble en effet qu'il ne soit pas encore possible d'étendre l'interdiction coutumière des armes chimiques aux agents destinés à combattre les émeutes, soit, essentiellement, les agents lacrymogènes (riot control agents), ..."²

This may be another "grey area" in addition to those foreseen by the US Executive Order 11850.

¹ R. Hanslian, Der Chemische Krieg. I. Militärischer Teil, E.S. Mittler, Berlin, 1937, p.43.

² D. Plattner, Règles du droit international humanitaire relatives à la conduite des hostilités dans des conflits armés non internationaux. Revue internationale de la Croix-Rouge. 72.

The actual problem in the negotiations.

Under the currently negotiated Convention the use of toxic chemicals for riot control and domestic law enforcement purposes is not prohibited ("purposes not prohibited by the Convention, as long as types and quantities are consistent with such purposes"). It therefore seems not to be necessary to have a complete exemption for "riot control agents" (RCA). If this is the case, some verification is necessary; such verification shall be related to the type(s) of agents (chemical structure, weaponized forms, ...) as well as to quantities. Notwithstanding the fact that such verification may be "unacceptable" to some states, definitions and criteria for RCA are necessary - a comprehensive definition is needed anyhow, even if exemption is complete. In this respect it seems impossible to allow the use of any non-lethal chemical as RCA; according to some military classification (not to the convention!) e.g. mustard belongs to the non-lethal (mortality 2%) incapacitating agents. The US seem to have a clear tendency towards the introduction of another terminology: disablement, as distinct from temporary incapacitation.

A definition of RCA (which does not yet exist under the current negotiation) should include CN and CS, but special operations, such as anti-gang or hostage liberation, may need other chemicals. The use of such chemicals in an international context would -in my view- be difficult to accept for most negotiating delegations.

RCA, if completely exempted, may still be covered by the 1925 Geneva Protocol. Only four nations spoke in favour of exempting RCA from the Protocol, among them was Belgium: In 1968, according to a Belgian position, "tear gases and other gases which are in police arsenals" did not fall under the Geneva Protocol of 1925. At the First Commission³, the Belgian delegate stated:

" [La Belgique] se prononce donc en faveur de la recommandation tendant à faire préparer par un groupe d'experts une étude sur les effets de l'utilisation éventuelle de ces moyens de guerre, tout en attirant l'attention sur l'existence de certains gaz qui ne sont ni asphyxiants, ni toxiques, utilisés parfois comme moyens de répression intérieure. Il serait utile que l'étude envisagée établisse une distinction claire entre les armes visées par le Protocole de Genève et les gaz lacrymogènes et autres qui font partie de l'arsenal policier⁴". Presently, this position is reconsidered.

³ During the 1968 debate on the UN resolution, introduced by Hungary to condemn the use of CS and herbicides by the US forces in Vietnam.

⁴ Nations Unies, Assemblée Générale, Première Commission

Even if RCA are not completely exempted, one may ask what the level of control under the Convention will be. With the present criteria RCA do not seem to fit completely in any of the existing schedules. Since it is a dual use chemical, schedule 3 may be indicated. Producers will have to declare their production only if a threshold of minimum 30 tonnes per year (probably the agreed figure will be higher) is reached. Will there be a country that has to declare its production? What will be the confidence level for such chemicals?

If the non-use on a battlefield is not forbidden under any convention, but is only dependent on a politico-military decision some countries may be tempted to keep a minimal level of protection -the gasmask- in the basic equipment of the soldier. Such a protective equipment ~~in~~ ^{increase} the threshold for the use of toxic chemicals, such as phosgene and hydrogen cyanide, that are (theoretically) available to sufficiently developed countries. If protection is completely absent any large-scale use of those -and similar- chemicals may render a first-use militarily efficient - in analogy with the situation in 1915.

H.C. DeBisschop



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6 April 1992

Messrs M Meselson and J P P Robinson
Harvard Sussex Program on CBW
Armament and Arms Limitation
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Cambridge MA 02138
USA

Dear Messrs Meselson and Robinson,

I am sorry that it has taken me so long to respond to your note of 28 February concerning the issue of riot control agents, and obligations to be assumed under the Chemicals Weapons Convention. My major preoccupations for the last few weeks have concerned a certain Senator Evans and his initiative to accelerate the CWC negotiations! If it is not too late, I offer the following personal observations.

I can only agree with the central point in your paper, that a blanket exemption in the CWC for non-lethal chemicals could leave a loophole for the development, production, stockpiling and use of new generation incapacitating CW for employment in both riot control and battlefield environments.

CD/1143 of course does contain an express broad exemption for RCA in para 2 of its Definitions Article—and this clearly does not meet your concerns. It is just as clear however that if the development of third generation incapacitating CW based on non-lethal chemicals is to be prohibited in the CWC, while at the same time retaining the option for "legitimate" RCA use, then the question of the scope of the use of such non-lethal chemicals will need to be addressed in the Convention. Taking the language of CD/1143 as a basis, the scope of use of non-lethal chemicals could be constrained by inserting the word "exclusively" before "used" in the first line of the definition. Alternatively, using the language taken to deal with herbicides, the words "as long as they are used as such" could be added at the end.

Both of these possibilities however would require the U.S. to modify their current policy for RCS use in war. The only other way I can identify to restrict development of new CW without restricting the scope of its use would be to change CD/1143's current illustrative listing of RCA types into a restricted definitive list. However, existing RCA have already proven to be useful battlefields weapons and development efforts could easily switch from chemical types to the development of munitions and delivery modes for existing RCA.

In short, I agree with your view that if all chemicals are to be banished for all time from the battlefield this will need to be spelt out in the Convention. CD/1143, as currently structured, does not do this, but it could be adjusted slightly to do so, if all of our negotiating partners were to agree (not least the United States).

Yours sincerely,

A handwritten signature in cursive script, appearing to read 'R S Morris', written in dark ink on a white background.

R S Morris

Krepon

Michael Krepon
The Henry L Stimson Center
Suite 304
1350 Connecticut Avenue NW
Washington, DC, 20036

U. S. A.

12 May 1992

Dear Michael,

Thank you for copying to me your letter of 6 May to Matthew Meselson. Not having actually heard or read your testimony, I'm really in no position yet to get into the substance of what you say -- except to express the general hope that anyone who advocates a loosening of constraints on the use of toxic weapons will not have done so lightly. One thing I can clarify, however, is that an 'editorial' is something written by the editors of the journal in which it appears. Signing such things is otiose. Those two editorials you mention went through a whole succession of drafts before Matt and I were both happy with what they said. I am sorry you found them unclear.

With best wishes, and hopes of seeing you again soon,

Yours sincerely,



J P Perry Robinson
Senior Fellow

cc Elisa Harris
David Hafemeister
Matthew Meselson

MAY 18 1992

The Hague

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Dr M S Meselson
Dept of Biochemistry & Molecular Biology
Harvard University
7 Divinity Avenue
Cambridge
MA 02138

U. S. A.

By fax to 0101.617.495.8308
(5 pages in all)

20 May 1993

Dear Matt,

Hope you got back OK and not too exhausted. For me it was a great week.

Here is a draft, not of the Pugwash workshop report, but of a scheme Peter Herby and I have been cooking up, stimulated by your and my conversation with him and what Batsanov and Mashhadi said at the workshop. Peter would be the "additional staff" person under budget item 3 on page 4.

What do you think? It could all fit in rather well, it seems to me, with our various other HSP and Pugwash plans. If we do proceed along such lines, we would really need to get started right away, initially by soliciting informal comments from the PTS on the draft (which may need some revision before doing so) and then approaching the foundations.

To that end, you and/or I might send the draft to Batsanov with a covering letter (a) asking for PTS views, (b) saying that Herby would be available to work with us on the project from the coming autumn, and (c) saying that one of us plus Herby would be happy to travel to The Hague to talk with PTS folk about it all. (Peter has his children coming over on 12th June, so, if (c) were taken up, the visit to the PTS would best be on 10 or 11 June: I would be free then if I give up the Stockholm annual Pugwash conference, which I shall probably have to anyway, given the CWCB 20 schedule.)

Perhaps you and I could have a telephone conversation about all this sometime between now and, say, monday? I'll give you a ring.

As ever,

A handwritten signature in cursive script, appearing to read "J P Perry Robinson".

J P Perry Robinson

MAY 20 1993

enc Proposal, draft 2.

DRAFT 2

DRAFT 2

Proposal for an Information Program
on
Implementation of the Chemical Weapons Convention

Background

A major task of the Provisional Technical Secretariat (PTS) of the Organization for the Prohibition of Chemical Weapons (OPCW) and National Authorities established under the Chemical Weapons Convention (CWC) is to ensure that, upon entry into force, state parties and their relevant ministries, industries and other entities are fully prepared to fulfill their obligations under the Convention. If significant numbers of states parties are unable to discharge their responsibilities when the treaty enters into force the chemical disarmament regime could easily falter. Entry into force is expected during or soon after January 1995.

A key element in ensuring effective implementation will be a clear understanding on the part of ministries, national CWC authorities and chemicals industries of their general obligations under the Convention and of the specific requirements elaborated during 1993-94 by the Preparatory Commission for the OPCW. These obligations will include extensive reporting of industrial chemical production data, reporting of past or existing chemical weapons programs, the preparation and passage of domestic legislation to bring the CWC into force and the development of procedures to carry out routine and challenge inspections on national territory.

While a number of national programs exist or are planned for the training of inspectors for the OPCW much less attention has been given to the education of national bureaucracies and industries about their CWC rights and obligations. This is especially notable as regards the many developing states which did not play an active role in the Geneva CWC negotiations and are unable to participate actively in the work of the PrepCom or, in particular, in the work of its experts' groups. The difficulty of such states in anticipating their obligations is increased by the fact that many of the detailed legal and operational guidelines and reporting requirements will only be available from the PrepCom in 1994. A well prepared and clearly focused information program during 1994 will be a prerequisite for effective implementation of the CWC in 1995.

Proposal

The current proposal is for a cooperative information program on implementation of the CWC to be carried out jointly by the Harvard Sussex Program on CBW Armament and Arms Limitation and the Provisional Technical Secretariat of the OPCW. The Harvard Sussex Program (HSP) would bring to the project the best available expertise from the academic and industry circles which for many years were active supporters of the Geneva CW negotiations. It would also provide significant resources and help to ensure that an ambitious information program is carried out in a cost effective manner. Further, the HSP could, through

DRAFT 2

DRAFT 2

dissemination of program information and other means, extend the reach of the program into countries that are still considering whether to sign the CWC. The Provisional Technical Secretariat's involvement as a full partner in the project would ensure that the information provided is fully up-to-date, accurately reflects the emerging implementation requirements being produced by the PrepCom, and is effectively coordinated with work within the Secretariat and PrepCom. Both parties will need to agree on issues of content and participation in the activities undertaken.

The program would consist of the following elements:

1. Development and Production of information materials on implementation issues.

(booklets, audio-visual materials etc. ...to be expanded)

2. Planning, coordination and running of 5 regional seminars on CWC obligations for officials from national ministries with CWC implementation responsibilities, 'national authorities' designated under the CWC and from relevant chemical industries.

Each of these seminars would last 2-3 days and include the following components:

- * The history and significance of the CWC
- * Rights and responsibilities of states parties under the CWC
- * Reporting requirements for - industrial activities
- chemical weapon programs
- * The planning and management of inspections - routine
- challenge
- * Models for designated 'national authorities'
- * CWC implementation legislation
- * A guide to the OPCW (other items?)

Seminar presentations would be standardized to ensure that the same information is presented on each occasion. Presenters would be recruited from among PTS staff, national experts, academic specialists on the CWC and industry experts on CWC implementation. Various components of the seminar could be expanded in response to the needs of particular regions and of the participants. Seminar locations would be selected in response to needs identified by the OPCW and invitations from signatory states to host the event.

This program will make use, where appropriate, of materials and presentations on the CWC and implementation issues already prepared by national governments,

DRAFT 2

DRAFT 2

particularly the training programs of CWC signatory states. It will also draw on Australian experience with Asia-Pacific regional CW seminars and on the experience of the Quaker United Nations Office with Middle East CWC conferences.

Funding

The projected funding period for the program is September 1993 through February 1995. This includes (1) a planning period in autumn 1993, (2) an operational phase throughout 1994 and (3) a wrap-up and reporting phase in early 1995. The life time of the project roughly coincides with the PrepCom's operations prior to entry-into-force of the CWC.

Upon completion of a joint program proposal and budget with the PTS, the Harvard Sussex Program will seek funding from major foundations for the program. A target date for finalizing funding applications is 5 July 1993 in anticipation of funding decisions in late summer and early autumn.

Staffing

The program will be managed jointly by the Harvard Sussex Program and the External Relations Division??? of the PTS. The PTS will provide one full time staff person to coordinate the publications and seminars involved. The Harvard-Sussex program will devote ??% of the staff time of one of its directors to work on the program and will seek funding for an intern/administrative assistant for the program, to be located in PTS offices in the Hague.



SUSSEX
AT BRIGHTON

Mr S Batsanov
External Relations Division
OPCW Preparatory Commission
Laan van Meerdervoort 51 A
2517 AE Den Haag

The Netherlands

By fax to 01031.70.360.0944

25 May 1993

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Dear Sergei,

It was good to see you and so many of your PTS colleagues at the Pugwash CBW workshop earlier this month. We all very much appreciated the way Ian Kenyon spoke on what was happening in the PrepCom and its associated works.

Matthew Meselson and I have since been thinking over what Ian, you and others said about the work which lies ahead for the PrepCom in which outsiders might possibly help. The educational side of your operations looks to us like one area where the Harvard Sussex Program might contribute. As you know, HSP exists expressly in order to promote *international* understanding of CBW issues (such as implementation of the CWC), and we have gained a fair bit of experience over the years, even goodwill in some quarters.

In the planning which we are now engaged upon for future HSP activities and fundraising, we have in mind a project for the preparation of briefing papers about CWC-implementation and related topics, and for the international dissemination of such information through publication and regional seminars. We are encouraged in this by the news that Peter Herby -- who, as you know, has a lot of relevant experience -- might become available this autumn to work with HSP on such a project. However, we would certainly not want to go ahead on any substantial scale without close consultation with the PTS. In fact, after what we heard at the Pugwash workshop, we now think it could make a lot of sense for the project to proceed in actual coöperation with the PTS. Indeed, the project might well be counterproductive without such coördination of our respective endeavours.

If you should have some time to spare, we would like to visit you in The Hague to learn your reactions to these thoughts and take them a stage further. If you can indeed give us an appointment, we will send on a paper ahead of time that will lay out a more concrete proposal, albeit still a tentative and provisional one. Over the next couple of weeks we shall be heavily engaged in getting out the June issue of *Chemical Weapons Convention Bulletin* (now being published here, at least temporarily); maybe after that, say on 10 or 11 June, we might visit you? Probably Meselson would not be able to participate, but Peter Herby would be free to come with me on either of those dates (or, at a pinch, earlier, though not later).

I hope I may hear from you on this in due course. With best wishes,

Yours sincerely,

J P Perry Robinson
Senior Fellow

fc M Meselson and P Herby

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By fax to 01031.70.360.0944
(5 pages in all)

4 June 1993

Dear Sergei,

Thank you for your fax yesterday, with its news that you will indeed be able to see Peter Herby and me next week.

What we would like to do is to visit you on Friday 11 June, possibly in the morning if that were convenient for you. It would be nice to see Ian, too, though I have not yet told him of my plan to be in The Hague. Peter and I would travel over on the thursday, he probably from Paris and me from Brighton, putting up somewhere in the Hague on thursday night; so we could be at your office virtually any time you wanted on the friday. Maybe you could fax me a note to say what time we should best come?

I said I would send you a paper beforehand about what we have in mind. Attached is a rough outline which Matthew Meselson and I feel might serve as a basis for discussion. As you will see, it is very crude and has lots of gaps. Essentially, it is a preliminary draft for a funding proposal to grant-giving foundations.

I look forward very much to seeing you again next week. All the best,

Yours sincerely,

A handwritten signature in cursive script, appearing to read 'J P Perry Robinson'.

J P Perry Robinson
Senior Fellow

Attachment

fc M Meselson and P Herby

DRAFT, 4 June 1993

Proposal for an Information Program on Implementation of the Chemical Weapons Convention

Background

A major task facing the Provisional Technical Secretariat (PTS) of the Organization for the Prohibition of Chemical Weapons (OPCW) and the National Authorities established under the Chemical Weapons Convention (CWC) is to ensure that, upon entry into force, states parties and their relevant ministries, industries and other entities are fully prepared to fulfil their obligations under the Convention. If significant numbers of states parties are unable to discharge their responsibilities when the treaty enters into force, the chemical disarmament regime could easily falter. Entry into force is expected during or soon after January 1995.

A key element in ensuring effective implementation will be a clear understanding on the part of ministries, national CWC authorities and chemicals industries of their general obligations under the Convention and of the specific requirements elaborated during 1993-94 by the Preparatory Commission for the OPCW. These obligations will include extensive reporting of industrial chemical production data, reporting of past or existing chemical weapons programmes, the preparation and passage of domestic legislation to bring the CWC into force, and the development of procedures to carry out routine and challenge inspections on national territory.

While a number of national programmes exist or are planned for the training of inspectors for the OPCW, much less attention has been given to the education of national bureaucracies and industries about their CWC rights and obligations. This is especially notable as regards the many developing states which did not play an active role in the Geneva CWC negotiations and are unable to participate actively in the work of the PrepCom or, in particular, in the work of its Experts' Groups. The difficulty of such states in anticipating their obligations is increased by the fact that many of the detailed legal and operational guidelines and reporting requirements will only be available from the PrepCom in 1994. A well prepared and clearly focused information program during 1994 will be a prerequisite for effective implementation of the CWC in 1995.

Proposal

The proposal here is for a co-operative information programme on implementation of the CWC to be carried out jointly by the Harvard Sussex Program on CBW Armament and Arms Limitation and the Provisional Technical Secretariat of the OPCW. The Harvard Sussex Program (HSP) would bring to the project the best available expertise from those academic and industry circles which for so many years were active in promoting the Geneva CW negotiations. HSP would also provide significant resources and help to ensure that an

ambitious information programme were carried out in a cost-effective manner. Further, HSP could, through dissemination of programme information and other means, extend the reach of the programme into countries that are still considering whether to sign the CWC or to remain outside it. The involvement of the PTS as a full partner in the project would ensure that the information provided is fully up-to-date, accurately reflects the emerging implementation requirements being produced by the PrepCom and is effectively co-ordinated with work within the Secretariat and PrepCom. Both parties will need to agree on issues of content and participation in the activities undertaken.

The programme would consist of the following elements:

- 1 Development and production of information materials on implementation issues [booklets, audio-visual materials, etc.... to be expanded]
- 2 Planning, co-ordination and running of 5 regional seminars on CWC obligations for officials from national ministries with CWC implementation responsibilities, 'national authorities' designated under the CWC, and from relevant chemical industries.

Each of these seminars would last 2-3 days and include the following components:

- * The history and significance of the CWC
- * Rights and responsibilities of states parties under the CWC
- * Reporting requirements for (a) industrial activities and (b) chemical weapon programs
- * The planning and management of inspections, both routine and challenge
- * Models for designated 'national authorities'
- * CWC implementation legislation
- * A guide to the OPCW

Seminar presentations would be standardised to ensure that the same information is presented on each occasion. Presenters would be recruited from among PTS staff, national experts, academic specialists on the CWC and industry experts on CWC implementation. Various components of the seminar could be expanded in response to the needs of particular regions and of the participants. Seminar locations would be selected in response to needs identified by the OPCW and to invitations from signatory states to host the event.

This programme will make use, where appropriate, of materials and presentations on the CWC and implementation issues already prepared by national governments, particularly the training programs of CWC signatory states. It will also draw on Australian experience with Asia-Pacific regional CW seminars and on the experience of the Quaker United Nations Office with Middle East CWC conferences. HSP, whose raison d'être lies in its efforts to promote international understanding of CBW issues, also has practical experience to draw upon.

Funding

The projected initial funding period for the program is September 1993 through February 1995. This includes (1) a planning period in autumn 1993, (2) an operational phase throughout 1994, and (3) a provisional wrap-up and reporting phase in early 1995. The lifetime of the project would thus roughly coincide with the PrepCom's currently projected operations prior to entry-into-force of the CWC.

Upon completion of a programme proposal and budget drawn up jointly with the PTS, the Harvard Sussex Program will seek funding from major foundations for the programme. A target date for finalising funding applications is 5 July 1993 in anticipation of funding decisions in late summer and early autumn.

Staffing

The programme will be managed jointly by the HSP and the External Relations Division of the PTS. The PTS will provide one staff person to co-ordinate the publications and the seminars. HSP will devote ?? percent of the staff time of one of its directors to work on the programme plus such other staff time as may be necessary, and will seek funding for an intern/administrative assistant for the programme, to be located in PTS offices in The Hague.

Budget

A program budget is to be developed jointly by HSP and the PTS. It is anticipated that funds raised by HSP would provide roughly 1/3 of the staffing costs and 2/3 of the operational costs of the program. PTS participation would involve roughly 2/3 of the staffing costs and 1/3 of the operational costs.

Preliminary estimates are as follows (all figures in US \$):

	<u>HSP</u>	<u>PTS</u>	<u>Total</u>
1. Information resources	75,000	25,000	100,000
- preparation and production costs			
- (distribution costs met by PTS?)			
2. Regional seminars			
- airfares for non-regional participants			
- accommodation and facility costs			
- travel subsidies to regional partic.			
5 seminars at \$60,000@.....	250,000	50,000	300,000
3. Harvard Sussex Program staff costs			
- director's participation ???			
- additional staff ???			
- intern/admin asst in NL 35,000			
subtotal	????		
4. PTS staff costs			
Totals	????	???	????
TOTAL sought from grant sources	????		

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FAX TRANSMISSION FORM

DATE: 21/9/93
 TO: M J MEJELSON
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 FROM: Julian Robinson

Total Number of Pages Transmitted (Including Cover Page):
 Please contact Robyn Forman on the above telephone or fax number or direct line Brighton
 [*44 273] 678177 if there are any problems concerning this transmission.

MESSAGE:

Here's copy for the Hague quarterly review. I'm
 also sending it to Peter Harty — though there's not
 much of his text left.

If you have comments — as I'm sure you will — please
 let me have them — well before Thursday

Progress in The Hague

Quarterly Review no 3

Building the Organisation for the Prohibition of Chemical Weapons

The OPCW looks like having a headquarters staff of 500-800 people to oversee the Chemical Weapons Convention. That is not small; and, because of the potential impact of the OPCW on chemical industry and trade worldwide, the organization-building now under way in The Hague is a delicate as well as a large task. How it should be undertaken was agreed in principle by the 101 states represented at the first and second plenary sessions of the OPCW Preparatory Commission, in February and April. During the period under review here -- June through August -- the Provisional Technical Secretariat (PTS) and the other structures of the PrepCom advanced steadily along the agreed track, stimulated by an otherwise largely uneventful third plenary at the beginning of July. Though their workload became progressively heavier, they registered solid progress. Thanks to a PrepCom information policy more open than once it looked like being, the main accomplishments through to the end of the third plenary are clear for all to see. What exactly was achieved during July and August will soon be on display in the documentation of the fourth plenary.

Overall, a notable trend has been the recruitment by the PTS of yet more of the CW specialists previously available to the national delegations (and also to UNSCOM). The PTS is the international civil service rather than the governmental part of the PrepCom, and the community of CW specialists worldwide is not large. One consequence is that the pool of people able and willing to take on the crucial jobs of chairing the Experts' Groups is shrinking. An effect has been to increase the responsibilities falling to the PTS, for, by providing a secretariat for each Group, the PTS necessarily plays a key role in the drafting and therefore the reporting processes.

Not far from the surface now is this question: what happens if the Chemical Weapons Convention does not after all come into force in January 1995? Some signatory states are finding that the legislative and other burdens of preparing for domestic implementation may preclude their early ratification. For entry into force on schedule, 65 states must ratify by mid-July 1994. This, in some peoples' minds, is no longer the certainty it once seemed. So the ramp-up of the PTS to staffing levels commensurate with the duties that will fall to the Technical Secretariat within a month of the treaty coming into force may need to be delayed. But the time for decision on the 1994 PrepCom work-programme and budget is fast approaching.

The possibility of postponement is disturbing, but actually it might not be an unmitigated setback to the prospects for global CW disarmament. If properly managed, delayed entry into force might allow the geographical spread of original states-parties to broaden in a manner important for the well-being of the fledgling OPCW. The recent breakthrough in the Middle East peace process gives this consideration an added salience.

Actions by the PrepCom The Preparatory Commission in plenary session is the ultimate source of authority for the organization-building. The third plenary took place during 28 June to 2 July and was attended by 80 signatory states. This was nine down from the previous session and only seven short of inquoracy. Although no major decisions were taken -- which was perhaps one of the reasons for the reduced attendance -- important purposes were nevertheless served. In particular, the 50 or so participating states that had not been represented in the Experts' Groups must surely have become more aware of the scope of the work before the Commission and therefore also of the nature of their obligations under the CWC.

The formal business of the plenary session included receiving and acting upon the reports of the Commission's two principal subsidiary bodies, Working Groups A and B. So the Commission duly took note of the projected scheduling of meetings of Experts' Groups during the two following intersessional periods, including the detailed task assignments, and approved the changed dates for the fourth session, namely 27 September through 1 October. It decided to adopt the specification for the OPCW's ambitious computerized Information Management System which had come up from the Group of Experts on Data Systems, even though, because the relevant papers had not been distributed in all official languages in time, including French, a recommendation to that effect had not been put forward by Working Group A. On the report of Working Group B, the Commission approved the draft introductory sections of the projected OPCW Safety and Health Policy Document, even though, for those same "technical reasons", it had not been recommended to do so by the Working Group. The report on Inspection Team Composition was not adopted, a number of delegations raising questions which the Commission then referred back to Working Group B; a particular difficulty was the report's mention of "NBC" expertise as being a possibly desirable attribute of the inspectorate. The Commission approved the recommendation that the PTS should convene a meeting at Experts' Group level with industry representatives during 4-8 October.

Other formal business included the election of Ambassador Sirous Nasserri of Iran as Chairman of the Preparatory Commission for the next six-month period, and, as Vice-Chairmen, the representatives of Chile, Japan, Romania, Tunisia and the United States.

The Commission reverted to certain unresolved issues which had exercised previous plenary sessions. One was the lack of full interpretation facilities for the Groups of Experts, an issue on which France, Cameroon and Latin American delegations expressed themselves strongly, and which, as just noted, had obstructed the Working Groups (though not the Commission itself). Those delegations insisted on the principle of universal access, while the delegations of major financial contributors such as the United States, Japan and Germany stressed the need for efficiency and fiscal stringency. A study commissioned by the PTS and reported to the plenary indicated that an additional \$1.45 million per year would be required for full language interpretation at all meetings of Experts' Groups. By way of interim compromise, the Executive Secretary told the plenary that, during the remainder of 1993, the PTS would experiment with provisional solutions, responding on a case-by-case basis to particular needs. The PTS would also be making proposals for a longer-term solution in connection with the 1994 budget.

Differences also resurfaced over the implementation by the Netherlands of its original undertakings on hosting the OPCW and the PrepCom. The OPCW Foundation, which is the Netherlands authority charged with this task, had put forward a narrow interpretation of the accepted hosting bid, an interpretation which would require the PrepCom to invest \$140,000 in improving its conference facilities. This was challenged by several delegations. Difficulties have also arisen because the permanent building to house the OPCW -- the Peace Tower, to be built in the centre of The Hague -- cannot be available for three years, whereas the OPCW, on the current working assumption, must be fully operational in 1995. Alternative sites in The Hague are currently being investigated.

Transparency of the PrepCom's work was a less prominent issue. The Australian delegation informally circulated a draft decision providing for public sessions of plenary meetings. It was too soon for full consensus to be achieved on the proposal, but that may well emerge at the fourth plenary. Hitherto all PrepCom meetings, other than the ceremonial first few minutes of the inaugural plenary, have been held in private.

Sensitivities about geographical balance in appointments to the PTS continued in evidence. An intervention by Mexico deplored the fact that more than half of the 25 senior

posts in the PTS had gone to just one of the five politico-regional groups. Mexico went on to state that "the drafting of the staff rules...should unequivocally reflect the necessity of rigorously applying the principle of equitable geographical distribution among the five regional groups". At least one delegation even insisted that the principle should be applied to inspection teams of the future OPCW.

Actions by Member States By the end of the period under review, 148 states had signed the Chemical Weapons Convention, thereby becoming entitled to join the PrepCom. Three of them -- Laos, Latvia and Rwanda -- had signed before the third plenary, though none then participated. Three others signed after the third plenary: Dominica, Liechtenstein and Panama.

Of the member states that attended the second plenary, eleven did not attend the third: Albania, Bangladesh, Belarus, the Holy See, Mali, Mauritius, Namibia, Nepal, Papua New Guinea, Yemen and Zambia. Two member states that had attended only the first session participated in the third: Senegal and Slovenia.

By mid-June, 45 of the then 142 member states had paid, in full or in part, the contributions required of them to cover PrepCom expenditure during 1993. These payments corresponded to 45 percent of the \$8.84 million budget for the year. Ninety-seven states, representing 22.5 percent of the budget, had made no payment. The organisation remained solvent nevertheless, having spent only a quarter of its 1993 budget by the end of May.

Sweden on 17 June became the fourth state to deposit its instrument of ratification with the United Nations. Swedish law, like that of some but by no means all other signatory states, permits ratification prior to the passage of national implementing legislation. Another three states-signatories have since ratified as well, bringing the total to seven: Norway, Saudi Arabia and the United Arab Emirates. So, if the treaty is to enter into force in January 1995 (i.e. after the prescribed minimum interval of two years from opening for signature), 58 more ratifications will be needed between now and mid-July 1994.

One ratification in particular, that of the Russian Federation, is bound to exert a stimulatory effect on others -- or, if it fails to materialize, an inhibitory effect. Whether the Russian parliament comes to look favourably on the Convention will almost certainly be contingent on its approving the detailed plans for destruction of the 40,000 agent-tonnes of former Soviet chemical weapons held within the Federation. The various draft destruction plans have been challenged on safety, environmental and financial grounds. Their future seems dependent on the degree of assistance which other countries are able to provide for their development and implementation.

Progress in the Provisional Technical Secretariat Evident during the third plenary session was the fact that the PTS had now become an organization capable of functioning efficiently and coping with a growing workload. A number of delegates commented privately on the high quality and commitment of the staff which the PTS has been able to attract.

Appointment of the head of the Division on Technical Cooperation and Assistance was again postponed because of absence of agreement in the Group of African States over the proposed candidate. Possibly a decision on this one remaining top-level position will be reached during the fourth plenary. Having been asked to nominate a candidate, the Executive Secretary had duly considered the possibilities and then conducted interviews, for which purpose he had visited Algeria, Morocco, Nigeria and Zimbabwe. As to the next level

down, he informed the third plenary that all P5 posts were now either filled or under offer to chosen candidates. The latter included: Pavel Castulik of the Czech Republic, to head the Inspector Training branch in the Verification Division; Raul C Fernandez of Argentina, as Industry Relations Officer in the External Relations Division; Johan Santesson of Sweden, as Technical Cooperation Officer in the Technical Cooperation and Assistance Division; Chamuta D Banda of Cameroon, as Personnel Officer in the Administration Division; and H V D Becker of Germany, as Budget & Finance Officer in the Administration Division. Ron Manley of the UK, currently serving on UNSCOM, has been offered the Chemical Weapons branch in the Verification Division.

Tentative planning has been proceeding within the PTS for staffing levels for 1994 and beyond. It appears likely that two staffing patterns will be considered for the next budget year. One will cover an expanded core staff to continue and broaden the preparatory work for implementing the CWC. A major second wave of hiring of implementation staff, such as inspectors and additional support personnel, would occur 6-8 months before anticipated entry-into-force of the CWC. On the current working assumption, the decision to authorize this second, ramp-up, phase of 1994 staffing would be made by the PrepCom in mid-1994. During the period under review, the Executive Secretary was preparing detailed proposals on PTS development for consideration by the Experts' Group on 1994 Program of Work and Budget. This Group will presumably be submitting at least an interim report in time for the fourth plenary at the end of September.

The PTS has been expanding its contacts with non-governmental bodies. Some of these contacts it has reported in its newsletter *OPCW Synthesis*, for example with Pugwash [News Chronology, 1 June], CEFIC, UNIDO and UNIDIR; and the Monterey Institute internship at the PTS continues. As a further aid to transparency, the PTS now has a policy of making available on request to research institutions and other non-governmental organizations documents it has published "in accordance with the Rules of Procedure". The Executive Secretary reported his intention of adopting this policy to the PrepCom at its third plenary, and met with no objection. In practice, the policy has come to mean the public availability of all PrepCom documents formally distributed at plenaries. During the third plenary, moreover, the PTS External Relations Division embarked upon the experiment of daily briefings for the press and other people formally excluded from the conference chamber.

Progress in the other PrepCom structures Although the PTS is clearly growing in influence, it is the Experts' Groups, made up of members of the national delegations and reporting to the two Working Groups, which are the powerhouse of the PrepCom. The more active delegations in The Hague either already include specialist members or bring specialists in to serve on the Experts' Groups. Less active ones simply assign local diplomats to the Groups, sometimes more in a listening than a contributory role. The inactive delegations -- around two-thirds of the PrepCom membership -- are unrepresented.

During the period under review, there were 17 Groups of Experts at work, four of them under Working Group A, 12 under Working Group B, and one reporting to both. Under the prevailing publication policy, the results of their work remain undisclosed to the outside world until after formal reporting to the responsible Working Groups, by which time the reports will have been sent out for consideration in national capitals. Membership of the Experts' Groups, other than the chairs, is not disclosed at all -- though this seems to be policy by default, not design. The reports thus far from the Experts' Group are typically a couple of pages long, briefly noting whatever written submissions the Group may have been received, identifying the topics discussed, and putting forward recommendations. In most cases a lengthy "Chairman's Paper" is annexed, setting out particulars. Insofar as it has yet been published, the work of each Experts' Group is noted briefly in turn below. The first

four Groups are the ones reporting to Working Group A.

Financial and Staff Regulations (chair: Sarvajit Chakravarti of India, succeeding Ralf Trapp, now with the PTS). Having completed the Draft Staff Regulations in early May, this Group was tasked during the third plenary to examine the consistency with them of the Executive Secretary's Staff Rules. The Group was also tasked to finalize its Draft Provisional Financial Regulations. It reconvened for a one-week session in August.

Privileges and Immunities (chair: Natalino Ronzitti of Italy). Having partly completed, in May, the draft for an agreement with the host government on privileges and immunities for diplomats accredited to the PrepCom and OPCW and for senior Secretariat staff, the Group was tasked during the third plenary to review the outcome of further negotiations between the Executive Secretary and the Netherlands government, and then to finalize the draft in time for the fourth plenary. It reconvened at the end of August.

Data Systems (chair: Tibor Tóth of Hungary). The PrepCom at its third plenary approved the detailed set of requirements which this Group had elaborated during April and May for a comprehensive computerized Information Management System suitable for the OPCW. The PrepCom then tasked the Group to consider how the IMS might be developed further, including the contributions that member states might offer for the system. The Group reconvened for a week in early August.

OPCW Building (chair: Radoslav Deyanov of Bulgaria). Having produced a second interim report in May detailing the sort of building the OPCW would need, this Group proceeded to establish a Task Force which included both its own specialists and ones from the OPCW Foundation. The Task Force began meeting in June, among other things to wrestle with the awkward fact that the accepted Netherlands bid to host the Organisation had expressly estimated a 3-5 year life span for the PrepCom whereas the PrepCom's mandate is to prepare for full OPCW operations from as early as February 1995; and the new building offered by the Netherlands cannot be ready before 1996. The Group and its Task Force were mandated during the third plenary to continue their efforts.

Analytical Chemists (chair: Marjatta Rautio of Finland). After a two-week session in June, this Group recommended that the OPCW should establish a centrally controlled Quality-Assurance/Quality-Control programme which built on existing international initiatives in this field (such as the Western European Laboratory Accreditation Co-operation); that the OPCW should perform regular proficiency-testing of its projected network of approved laboratories and field activities; and that it should assign to the projected OPCW laboratory, a properly accredited one, the role of co-ordinating analytical activities under the Convention, rather than actually undertaking all the analyses itself. The Group further recommended that the PTS should ask Member States to provide spectra and other analytical data on relevant chemicals in order that construction of the requisite OPCW data-base could begin early. Its report also set out areas in which the Group believed it needed to do more work.

Technical Co-operation and Assistance (chair: Sarvajit Chakravarti of India). Having produced, during its initial session in May, an expanded formulation of the tasks and issues that had been referred to it, this Group was mandated during the third plenary to continue along the lines it had developed. It duly reconvened for a second one-week session, in July. Its May report had recorded the Group's view that: "The multifaceted terms 'technical cooperation' and 'assistance' among States Parties and between States Parties and the Organisation apply in their general meanings to many provisions of the Convention; technical co-operation and assistance run throughout the Convention as common threads and serve as a lubricant for the smooth meshing of its gears; the terms should be interpreted in the broad sense of process -- permitting the Convention to operate successfully -- rather than in the restricted sense of giver and receiver; all parts of the Convention are inter-related and are

to be implemented as a whole."

Industrial Declarations (chair: Graham Cooper of the UK). During a two-week session in June, this Group discussed the formats of declarations to be required from the chemicals industry about production and use of chemicals for purposes not prohibited by the Convention. This also meant that the Group had to consider such questions as what to do about 'captive use' of scheduled chemicals (e.g. phosgene made at the same location as its use for making urethanes) and the guidelines for 'low concentrations' (scheduled chemicals present as impurities in other products). The Group developed a set of 31 charts to serve as the basis for forms to be used for transmitting in a standardized and computer-assimilable fashion 31 different categories of information declarable under CWC Article VI. (Certain interpretations of the Convention are embedded in these charts: for example, on the declaration by states parties of aggregate national data on the quantity of each Schedule-2 chemical produced, the aggregate is to be the sum of production only in sites declared for each such chemical, not the country as a whole.) The report of the Group, which contained a dissenting footnote by Iran, noted areas where further work would eventually be needed, including work by the Experts' Group on Industrial Facilities. On the 'captive use' issue, the report stated that a facility producing for captive consumption a chemical listed on Schedule 2 or 3 should be declared and verified in the same manner as any other Schedule 2/3 facility.

Equipment (chair: Henk Boter of the Netherlands). After a one-week session in mid-June, this Group recommended that the OPCW should have its own laboratory, but that it should be small and limited in its functions to the following: (a) providing the OPCW Director-General with technical assistance and advice on the certification of Designated Laboratories (DLs); (b) handling verification-related samples prior to analysis by DLs; (c) preparing control samples and distributing them to the DLs concerned; (d) quality-auditing and proficiency-testing of the DLs; (e) organizing, updating and certifying the OPCW analytical databases; (f) certifying methods, reference compounds and inspection equipment; (g) supporting maintenance and storage of inspection equipment; and (h) supporting training activities. It was mandated during the third plenary to start work on inspection equipment and other matters, and duly reconvened in August for a further one-week session.

Training (chair: Raul Fernandez of Argentina). This Group convened for a one-week session in June during which it discussed the training necessary to enable the Inspectorate and Verification Division of the OPCW Technical Secretariat to discharge their inspection duties when the Convention enters into force. The Group also discussed training requirements for other PTS/TS personnel and for personnel of the National Authorities of states parties. It developed some preliminary guidance on these matters and outlined concepts and a curriculum for a General Training Scheme which the PTS might establish in the near future. Its report, without mentioning the training programmes which several countries are already setting up and advertising, urged Working Groups A and B speedily to resolve the issue of whether training for inspectors should be undertaken before or after employment by the PTS/TS. The Group was mandated during the third plenary to develop the detailed contents of training courses and guidance for their implementation. It reconvened in August for a further one-week session.

Chemical Weapons Storage Facilities (chair: Jim Knapp of Canada). This Group was mandated during the third plenary to work on the formats of the declarations to be made by states parties of their holdings of chemical weapons and of the depots where the weapons are stored. The mandate also included: guidelines for stockpile and depot inspection procedures; suitability for CWC purposes of bilateral verification procedures; inspection-report formats; model facility agreement for storage sites; and identification of possible types of inspection equipment. The Group convened for a one-week session early in July.

Chemical Weapons Destruction Facilities (chair: Ron Manley of the UK). This Group was mandated during the third plenary to work on the formats of the declarations to be made by states parties of their plans for destroying such chemical weapons as they have declared, and also to establish the deadlines for submitting detailed chemdemil-facility data. The mandate included several associated matters as well, such as guidelines for inspection procedures, suitability of the bilateral verification procedures, inspection-report formats, a model facility agreement, and identification of possible types of inspection equipment. The Group convened for a one-week session early in July.

Single Small Scale Facility/Schedule 1 Facilities (chair: Bernhard Oderheimer of Germany). This Group was mandated during the third plenary to work on declaration-formats, guidelines for inspection procedures, and related matters. It convened for a one-week session during mid-July.

Inspectorate Planning (chair: Johan Santesson of Sweden). This Group was mandated during the third plenary to review staffing options for the Inspectorate, such as 'on-call' vs fulltime inspectors, and to consider the size of the Inspectorate in relation to the 1994 Staff Plan. The Group convened for a one-week session during mid-July.

Chemical Industry Facilities (chair: Adam Noble of the UK). This Group was mandated during the third plenary to extend the work of the Cooper Group on industrial declarations (see above) and to develop guidelines for inspection procedures in the civil chemical industry. The mandate also included several associated matters, such as inspection-report formats, a model facility agreement, and identification of possible types of inspection equipment. The Group was the largest yet, and worked intensively -- both morning and afternoon sessions -- during the middle of July.

Chemical Weapons Production Facilities and Their Destruction or Conversion (chair: Jack Ooms of the Netherlands). This Group was mandated during the third plenary to develop declaration-formats for former chemical-weapons factories, to specify allowed maintenance activities at such facilities, and to develop a destruction schedule. The mandate also included several associated matters, such as guidelines for inspection procedures, suitability of the bilateral verification arrangements, inspection-report formats, model facility agreements, and identification of possible types of inspection equipment. After a somewhat difficult start, the Group worked intensively during the middle of August.

Challenge Inspections (chair: Andrea Perugini of Italy). This Group was mandated during the third plenary to develop guidelines for the conduct of challenge inspections and to consider a range of associated matters. The Group convened for a one-week session in the latter part of August.

1994 Programme of Work and Budget (chair: Jorge Morales Pedraza of Cuba). This Group began to meet at the very end of the period under review here. It will be reporting jointly to Working Groups A and B.

As for the two Working Groups themselves, the custom has now become established that they meet formally only during the plenary sessions. During the third plenary, Group A had the work of five Expert's Groups to consider, while Group B had eight, including three which, because they had completed reports before the period under review here, are not listed above:

Safety Procedures (chair: Raymond Fatz of the USA), which in May had produced a draft for the initial sections of the OPCW Safety and Health Policy Document;

Chemical Engineers (chair: Bernhard Odemheimer of Germany), which in May had compiled inspection requirements and inspection activities for four different categories of declarable facility, and whose report had put forward the principle that it should be the PTS, not the Experts' Groups, that should draft the actual operational inspection procedures for the inspection manual, the draft being based on the results of Experts'-Group discussions; and

Inspection Team Composition (chair: Johan Santesson of Sweden), which in May had elaborated illustrative examples for ten different categories of inspection, the sizes of the teams envisaged ranging from a low of 4 inspectors, for a Single Small Scale Facility, to a high of 50 inspectors, for a military depot challenged as a possible undeclared CW storage facility.

Of these three, the Santesson Group had produced a final report (containing, however, a footnote reservation by Iran); the Fatz and Odemheimer Groups, producing interim reports, were mandated to continue their work. Of the other ten experts' reports that went up to the Working Groups, four were distributed less than the requisite 21 days before the relevant Working Group met and in English versions only (those from the Rautio, Cooper, Boter and Fernandez Groups), while certain others were not made available in all official languages within that period. Working-Group recommendations to plenary on some but not all of these late-distributed reports were accordingly blocked as part of the language-service dispute mentioned earlier, in particular those of the Tóth and Fatz Groups.

Looking ahead to the fourth intersessional period (4 October to 10 December), the current projection is that one further Experts' Group will be established under Working Group B: on *Old and Abandoned Chemical Weapons*.

This review was written by Peter Herby of the Quaker United Nations Office, Geneva, and Julian Perry Robinson.

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Please reply to the Director General

Your reference

Our reference

Date **26** October 1993

Dear Julian

Very many thanks for the project document and for the opportunity to comment on it prior to your meeting in The Hague. I am indeed very sorry not to be able to be with you in The Hague due to a prior commitment; commenting on your document at this time is the least I can do to help support your project and I would certainly appreciate being kept informed and, hopefully, involved in the future.

Reading through the document last evening, I must first say that it is very impressive. Clearly a lot of thought has been given to the project, it is well considered, soundly based and very clearly expressed; it is also very realistic - the costings illustrate the realism that has gone into the definition of the project. Altogether a very sound, well prepared document and consequently at this stage my comments will necessarily be relatively few - the better a document the fewer the comments required!

Background

Regarding the background section, I agree and fully endorse all that is said - in fact it would be a good idea if these remarks, appropriately edited, could be released into the public domain in a way which enables them to reach the right people. Some of these points individually have been made before, and the relevant concerns expressed, but it is the first time that I have seen them collected together and the impact they collectively make is thus much greater. The only additional point I would add is that for the first 120 days, and especially for the first 30 days, the OPCW will be subjected to a veritable deluge of paper as the declarations come in. All these will need to be scrutinised and decisions made on each one, as well as putting the information into the OPCW database. For many declarations this will be relatively easy, but I fear that the sheer mass of paper and information may swamp the OPCW's limited resources for the first, very critical, few weeks after the Treaty comes into force. The OPCW will at that time be under enormous pressure and despite having some very good people, as you say, it may be necessary to supplement the permanent OPCW headquarters staff with a number of short-term, but knowledgeable employees to help sift through and analyse the declarations according to guidelines laid down by the PTS. Is this something to be included in the project or does it follow-on from the project? Certainly the personnel involved in the project would be uniquely experienced to assist the OPCW for this initial, critical period in this way.



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Proposal

One of the strengths of the HSP is its independence from vested interests. Partnership with the PTS on this project may give the impression to some people that this independence has to some degree been eroded thus possibly lessening the impact of the project documents to be produced. However, to be effective, the project will require considerable support, by way of staff-time, information etc, from the PTS; the PTS and the OPCW eventually will also derive considerable benefit from the project - so a very close working relationship is of great benefit to both HSP and PTS. Provided that this close working relationship is clearly defined and publicly understood, I think the concern I have expressed above may prove groundless, but perhaps another word than 'partner' could be found to describe the close working and mutually supporting relationship between the HSP and PTS.

Concerning the categories of people to be targeted, two additional categories should be added to the list on page 3 - viz industrialists at managing/works director level or higher and the technical scientific community which may be expected to devise improvements to the scientific/technical support utilised by the OPCW. The relationship of this technical/scientific support, and the technical/scientific community, to the Science Advisory Board envisaged in the Treaty, could also be explored.

The list of topics given on page 3 is really very comprehensive - so comprehensive and well considered that in a way it represents a challenge to find one or two additions! One such is the topic of inspections and control of those private (and for some countries State-owned) chemical industries which may potentially present a threat to the Treaty because of the nature of the chemicals used/consumed in the course of their quite legitimate, non-prohibited activities. This I believe is still a difficult area (indeed it is referred to at the bottom of page 1) and needs to be added to the list, separate from and in addition to those items on page 3 which already refer to industry. Destruction of old/abandoned chemical weapons is also worth separate inclusion since these will generate some special problems. I would suggest that you keep clear of the very difficult question of old (and not so old) chemical weapons dumped at sea eg in the Baltic. The potentially contentious nature of such issues could distract effort and time away from the real purposes and aims of the project.

Regarding the presentation of the output of the project, the written word will be essential, particularly if it is intended to reach people whose native tongue is not English. However, I believe videos could have a useful role to play and one in particular may be useful - the conduct of inspections based on the practice inspections which several countries have already carried out. Combining and editing the records made by the countries of these practice inspections could do much to allay existing concerns about the possibly excessively intrusive nature of inspections.

Staffing and Budget

These items seem to be well-based and realistic - they should convince most people that this is not a 'kite-flying' exercise, but a well-based, soundly thought-out, carefully prepared and realistic proposal designed to meet achievable, worthwhile and realistic objectives. My one concern here is the question of translation - the question of languages is referred to, briefly, under item 4(iii) of the provisional agenda. Bearing in mind the global nature of the Treaty and therefore the need to reach a wide audience, I believe that translation facilities will be

required and these will need to be included in staffing and budget. They could be quite expensive but their probable costs can only be estimated when it has been decided what shall be the languages in which the output of the project will be issued. In order to keep costs down, I would suggest, for consideration, English, French, Spanish, Arabic and a Far Eastern language as the minimum, but it may be more defensible to argue for issues in the official UN languages. This will be an issue to be debated and decided at the first project meeting next month - until these decisions are taken the overall project costs cannot be finalised.

I have no further comments at this stage. I would just like to close by wishing you personally, and your colleagues at the first project conference, a very successful meeting which I hope will progress the project sufficiently rapidly to enable it to make a positive contribution to the effective and timely implementation of the CWC. I look forward to hearing about the outcome of the first meeting.

With best wishes

Yours sincerely

Bryan.

Dr B C BARRASS

PS. One final thought concerns the extremely intense pressure under which the present PTS staff are working. They really are working under a very high level of pressure in order to achieve all their very varied tasks within the short timescale allotted. I believe that the overall aim of the project must be to assist and support the PTS to achieve its tasks in a timely and effective manner and not to add significantly to their existing high workload - although I do recognise that some input from the PTS personnel will be essential if the project is to achieve its joint aims.

BARRASS1-C/WP/009/93/PJ

PREPARATORY COMMISSION FOR THE ORGANISATION
FOR THE PROHIBITION OF CHEMICAL WEAPONS
PROVISIONAL TECHNICAL SECRETARIAT
External Relations Division

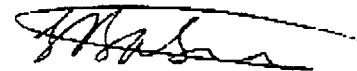
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From: S. Batsanov
Date: November 11 , 1993
Number of pages: (including fax leader)4

Dear Prof Meselson & Mr Robinson ,

I am enclosing the text of a letter which I will be writing to you officially, which has been drafted on the basis of the text prepared by you, when you were at The Hague . Do let me know by fax at your earliest whether you agree with the contents.

Yours Sincerely,



S.Batsanov

(Director for External Relations)

PREPARATORY COMMISSION FOR THE ORGANISATION
FOR THE PROHIBITION OF CHEMICAL WEAPONS
PROVISIONAL TECHNICAL SECRETARIAT
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November 10, 1993
Ref:

Prof. Matthew Meselson ,
Department of Biochemistry & Molecular Biology,
Harvard University,
7, Divinity Avenue,
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USA

Mr Julian Perry Robinson,
Science Policy Research Unit ,
University of Sussex,
Brighton, BN1 9 RF, Falmer,
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England

Dear Messrs Meselson and Robinson,

Following the letter of 16 August 1993 (Ref. 492/ 93) from our Executive secretary Mr Ian R. Kenyon, endorsing the proposal of the Harvard - Sussex Programme for a PTS- HSP International Information Project on implementation of the Chemical Weapons convention and the subsequent conceptualisation Conference of 6-7 November of members of the PTS and the Harvard - Sussex programme , we now wish to acknowledge in more specific terms the nature of the proposed Project.

II. In outline, we request that HSP, in collaboration with PTS undertake to produce for the PTS information materials on specific topics that may be used separately and in appropriate groupings to meet the needs of government officials and others in developing national policies regarding the CWC and programs for its implementation. Topics on which information materials are to be prepared by HSP / PTS are as follows :

List I:

- Outline Summary of the Chemical Weapons Convention (PTS)
- Rights and responsibilities of States Parties (PTS)
- Reporting requirements: Industrial activities (PTS)
- Guidelines for establishing/ designating National Authorities (PTS)
- Challenge Inspections (PTS)

- Guide to the Institutions under the Chemical Weapons Convention (PTS)
- Benefits of the Chemical Weapons Convention to States Parties (HSP)
- The Convention and economic and technological development (HSP)
- Assistance available under the Chemical Weapons Convention (HSP)
- Consequences of remaining outside the Chemical Weapons Convention (HSP)
- Demonstrating Compliance with the Chemical Weapons Convention (HSP)
- Preparations for receiving inspectors (HSP)
- Guidelines for National Implementing legislation (HSP)

List II:

- Confidentiality and the Chemical Weapons Convention (PTS)
- The Convention from the perspective of the industry (HSP)
- Financial contributions of States Parties (HSP)
- History and nature of Chemical Weapons (HSP)
- Safe destruction of Chemical Weapons (HSP)
- Reporting requirements : Past Chemical Weapons activities (HSP)
- Purposes not prohibited under the Chemical Weapons Convention (HSP)
- Impact of the Chemical Weapons Convention on military organisations (HSP)

III. It is understood that HSP shall , in consultation with PTS :

- (1) Design an overall scheme to guide the scope, organisation, levels of detail, and style of the required materials .
- (2) Commission authors highly knowledgeable in the subject matter and skilled in clear exposition to prepare draft materials dealing with the various topics. In some cases, such materials would be contributed by the directors , associates or staff of HSP. Issues of attribution will be decided by PTS.
- (3) Guide the preparation, coordination, and editing of the separate materials, presenting them to the PTS for approval or possible revision, as deemed appropriate by PTS.
- (4) Participate in dissemination and presentation of the various materials.
- (5) Assist in the organising and planning of regional meetings, including the support of attendance by individuals without access to travel funds from their national governments or other sources.
- (6) Develop and produce from the various information materials and experience gained in their preparation an integrated Guide to Implementation of the Chemical Weapons Convention.
- (7) Prepare appropriate slides and background film clips for use during regional educational Seminars organised by the PTS with assistance from HSP.
- (8) Provide access to its data base for the use of PTS.

IV. The Guide could eventually be made available in computer accessible form, organised in a hierarchy of directories and files with cross- references to relevant provisions in the Convention.

V. The decision of PTS to collaborate with HSP in the Project stems from the magnitude and urgency of the educational task confronting the Preparatory Commission if the Convention, now signed by 154 States, is to enter into force and be effectively implemented at an early date. Our decision is also conditioned by the long experience and high standards of scholarship of HSP and by its extensive data base and international resources and contacts with specialists worldwide.

VI. As the PTS contribution to the Project, we will :

- (1) Prepare information materials on certain topics (mentioned in Para II above) to be included in the guide.
- (2) Review, within 14 working days exclusive of Plenary meetings , materials submitted to PTS for approval or possible request for their revision.
- (3) Accept an HSP intern to work under PTS direction and to act as liaison between HSP and PTS. Each such intern is to work at the PTS for a period of not less than 4 months and to be supported by funds not provided by PTS.
- (4) Provide to HSP appropriate access to its data base, in order to ensure up to date awareness of procedures being developed by the preparatory commission for the CWC regime with due regard to confidentiality requirements of the Chemical Weapons Convention and the PTS.
- (5) Integrate HSP directors, associates or staff members into regional seminars and other PTS educational activities, as appropriate.
- (6) Subject to a final decision by the Preparatory Commission on the budget for FY 1994, to be taken in December, 1993, contribute up to US\$ 75,000 for five regional educational Seminars over the next year.

VII. I look forward to an early reply.

Yours Sincerely,

S. Batsanov

(Director for External Relations)

Canonne

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23 October 1993

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By fax to 01033.1.49.12.30.97

Dear Pierre,

I am so sorry that your letter of 6th September, which arrived while I was away, should have remained unanswered for so long. It was very good to hear from you. I do hope the first session is going well. I find myself peripherally involved -- as one of the lecturers -- in the UK course and I am looking forward to seeing how it develops.

A copy of the latest *Chemical Weapons Convention Bulletin* is enclosed. I will make sure CEFFIAC is on the mailing list for future issues.

We would be delighted if you would contribute to the *Bulletin*. An article on training would be of widespread interest, the more so if you were to draw from the experience of the first CEFFIAC session. If you could get something to Matthew Meselson (who is in charge of guest articles) by the last week of November, we might well be able to run it in the December issue.

With best wishes,

J P Perry Robinson
Senior Fellow

cc Dr M S Meselson



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REPUBLIQUE FRANÇAISE
MINISTÈRE DE LA DÉFENSE

Paris, le 06 septembre 1993

N°81DEF/CEFFIAC/PC

Dr. Julian PERRY ROBINSON
University of Sussex

Dear Julian,

As you know, France has set up a training center for the prohibition of chemical weapons.

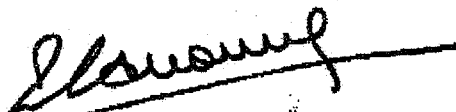
The first session will start on September the 13 th. Twenty trainees from seventeen member States (China, Pakistan, Romania, Greece, Bulgaria, Russia, Poland, Italy, Germany, Ireland, Algeria, Argentina, Mexico, Cameroun, Ivory Coast, Marocco and of course, France) will participate until December the 17 th to numerous, various activities in all fields related to the CWC.

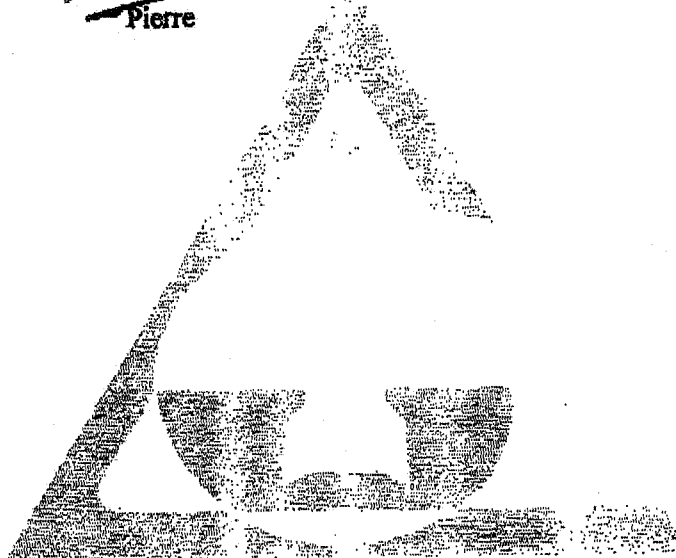
The CEFFIAC will be very happy receiving regularly a copy of the CW Convention Bulletin.

Moreover, if you are interested in, I could write some paper on training, for the next issue of the Bulletin.

Thank you for your help.

Best regards,


Pierre



UNIVERSITY OF

SUSSEX
AT BRIGHTON

SPRU

Science Policy
Research UnitMantell Building
University of Sussex
Falmer, Brighton BN1 9RF
Telephone: (0273) 686758
Direct Line: (0273)
Fax: (0273) 685865Mr Hassan Mashhadi
OPCW Preparatory Commission
Laan van Meerdervoort 51 A
2517 AE Den Haag

The Netherlands

28 June 1994

By fax to 01031.70.360.0944
(this page only)

→ Copy by fax to Dr M S Meselson, 0101.617.496.2444

Dear Hassan,

It was good seeing you again at the Pugwash workshop (which seems to have had some effect at least, in Washington). I hear the Brno meeting went well – hard to imagine, given the absence of certain people!

This is to tell you three things. First, two boxes of *Chemical Weapons Convention Bulletin* issue no 24 are now on their way to you via TNT carriers. They should arrive late wednesday – in which case they may just have got to you in time for Seventh-Plenary participants. The lead article is not quite as inflammatory as the last issue's; in fact it is the paper that Pierre Cannone gave at the Pugwash workshop.

Second, the HSP Intern, Justin Smith, is also just about to arrive at the PTS. I had hoped he would get to you before the *Bulletins* (so he could help distribute them) but his ticketing situation means he will not arrive in The Hague until 3 July. Maybe you could alert people to expect him at the PTS on Monday 4 July?

Third, HSP has finally started to have some success on the fundraising front, not as much yet as we need, but at least something for the HSP/PTS International Information Project. Do we put it into the writing side of the project or into the regional seminar side? My own preference is for the latter: for helping to bring regional participants and HSP people to the seminars. We'll need to talk about this quite soon, maybe once the current plenary is behind you.

All the best,

Yours sincerely,

A handwritten signature in black ink, appearing to be 'JP' with a flourish.

J P Perry Robinson
Senior Fellow

Hudson

PROFESSOR MATTHEW MESELSON
DEPARTMENT OF BIOCHEMISTRY AND MOLECULAR BIOLOGY
HARVARD UNIVERSITY
7 Divinity Avenue
Cambridge, Massachusetts 02138

Phone: (617) 495-2264
Fax : (617) 496-2444

Fax Transmission

Date: 08/03/94

Julian Perry Robinson

To: _____

Barbara Ring

From: _____

2

Number of pages including this sheet: _____

Julian -

Matt's answer to the attached letter is "yes."
However, he wants the journal and other materials sent
to you instead of him.

Best wishes,

Barbara



The Binding Triad System for
Global Decision-Making

May 6, 1994

Center for War/Peace Studies

218 East 18 Street
New York, N.Y. 10003
Tel: (212) 475-1077
Fax: (212) 260-6384

Matthew Meselson
Julian Perry Robinson
Committee for National Security
1601 Connecticut Avenue, NW, #600
Washington, DC 20009

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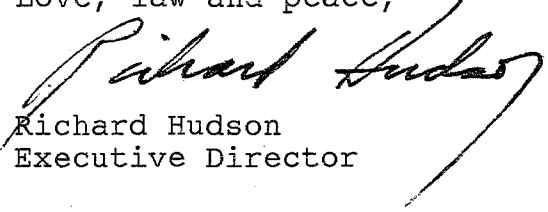
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Myron Kronisch
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Miles Pennybacker
Charles C. Price
Joan Warburg
Hannah Wassermann

Dear Matthew Meselson and Julian Perry Robinson:

We would be much interested in receiving the Chemical Weapons Convention Bulletin, and propose we exchange publications. We enclose two copies of the current issue of Global Report and the latest brochure on the Binding Triad system for global decision-making.

If this is agreeable to you, please send us your best addresses for our mailing list, and also any other names and addresses of people that you think would be interested in receiving Global Report and our other materials.

Love, law and peace,


Richard Hudson
Executive Director

/bh
Enclosures

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 reported
in Geneva re Anthrax
Love to the family
Martin

To: Professor Rotblat and Claudia, London office
From: Martin Kaplan *ML*
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 modifications 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 Musselman & Julian

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CC Musselman & Julian

AUG 25 1995

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



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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

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To: Charlene Breedlove <Charlene_Breedlove@ama-assn.org>
Cc: Joshua Lederberg <jsl@rockvaX.rockefeller.edu>,
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Julian Perry Robinson <j.p.p.robinson@sussex.ac.uk>
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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:

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From: Pugwash <pugwash@hei.unige.ch>
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Cc: Matthew Meselson <msm@wjh.harvard.edu>,
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[Your display is set for the "US-ASCII" character set]
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Anyhow, cheers for the ratification of the CWC.

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From: Pugwash <pugwash@hei.unige.ch>
To: Julian Robinson <J.P.P.Robinson@sussex.ac.uk>
Cc: Joshua Lederberg <jsl@rockvax.rockefeller.edu>,
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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 about 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,
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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 about 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,
> 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 Hed=E9n 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.

> >=20

> > Josh

> >=20

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>

Date: Tue, 27 May 1997 14:47:05 BST
From: Julian Robinson <J.P.P.Robinson@sussex.ac.uk>
To: Matthew Meselson <msm@wjh.harvard.edu>
Subject: Harvard consultants at Sussex

Dear Matt,

I keep forgetting to send you the particulars of Kevin Matthews, who I want to take on for the three-month period from 12 May to do Lynn-type work in SHIB. Following our last e-mail exchange about him, I have offered him \$500 a month, which he is glad to accept.

If you are indeed able to do this, could you fax him (care of me at SPRU, 44.1273.685865) a letter saying so. The one you did for Daniel Feakes would do fine, with the following changes:

- (1) Date it 12 May 1997.
- (2) Address it to: K M Matthews, Flat 10, 30 Buckingham Place, Brighton, BN1 3PJ, England.
- (3) Replace the second, third, fourth and fifth sentences with:
The work would be undertaken in England in connection with the maintenance of the Sussex Harvard Information Bank and production of the CHEMICAL WEAPONS CONVENTION BULLETIN. Your consultancy would be effective immediately and would run through to 8 August 1997. We would be able to pay you a total of \$1,500. The payment schedule would be on a monthly basis (\$500 per month for three months).

Kevin is a British subject, until 11 May employed by the University of Sussex under its Graduate Intern Programme. I will snail-mail on his c.v.

Meanwhile, Daniel Feakes is thinking seriously about taking up our offer of becoming the next HSP Hague Researcher. I have told him to make up his mind before the middle of next week, when you and I will be setting off for The Hague again. I expect he will want more details of the job.

As ever,
Julian

From Dr Graham S Pearson CB

Fieldway
Winterbourne Monkton
SWINDON
Wiltshire SN4 9NW
U K

Telephone No 01672-539382
Facsimile No 01672-539582

18 September 1997 7

Professor Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
7 Divinity Avenue
CAMBRIDGE
Massachusetts 02138
USA

Facsimile to: 001-617-496-2444

Dear *Matt,*

YELLOW RAIN REFERENCES

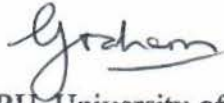
1. At Woods Hole, I undertook to obtain from Porton Down the references of their published work on trichothecenes. These are as follows:

- a. P Begley, B E Foulger, P D Jeffery, R M Black and R W Read, Detection of trace levels of trichothecenes in human blood using gas chromatography - electron capture negative ion chemical ionization mass spectrometry, *J. Chromatography*, **367**, 87-101 (1986)
- b. R M Black, R J Clarke and R W Read, Detection of trace levels of trichothecene mycotoxins in environmental residues and foodstuffs by gas chromatography - mass spectrometry, *J. Chromatography*, **367**, 103-115 (1986)
- c. R M Black, R J Clarke and R W Read, Detection of trace levels of trichothecene mycotoxins in environmental residues and foodstuffs using gas chromatography with mass spectrometric or electron capture detection, *J. Chromatography*, **368**, 365-378 (1987)
- d. R M Black and D G Upshall, Assessing the danger, *Chemistry in Britain*, July 1988, 659-664
- e. S A Maxwell, R F R Brown and D G Upshall, The in-vitro penetration and distribution of T-2 toxin through human skin, *Toxicology*, **40**, 59-74 (1986)
- f. T C Marrs, J A G Edginton, P N Price and D G Upshall, Acute toxicology of T-2 mycotoxin to the guinea pig by inhalation and subcutaneous routes, *Brit J Exp Pathology*, **67**, 259-268 (1986).

2. I have not got a copy of these references so I will copy this letter to Julian and ask him if he can obtain copies for both the SHIB (if they are not already in there) and for me so that I can refer to them as you develop the planned paper on yellow rain 10 or 15 years on!

With best wishes

Yours sincerely

A handwritten signature in cursive script that reads "Graham". The signature is written in black ink and is positioned centrally below the typed text.

Copy to: Julian P P Robinson, SPRU, University of Sussex

From Dr Graham S Pearson CB

Fieldway
Winterbourne Monkton
SWINDON
Wiltshire SN4 9NW
U K

Telephone No 01672-539382
Facsimile No 01672-539582

18 September 1997 7

Professor Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
7 Divinity Avenue
CAMBRIDGE
Massachusetts 02138
USA

Facsimile to: 001-617-496-2444

Dear *Matt,*

YELLOW RAIN REFERENCES

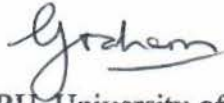
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2. I have not got a copy of these references so I will copy this letter to Julian and ask him if he can obtain copies for both the SHIB (if they are not already in there) and for me so that I can refer to them as you develop the planned paper on yellow rain 10 or 15 years on!

With best wishes

Yours sincerely

A handwritten signature in cursive script that reads "Graham". The signature is written in black ink and is positioned centrally below the typed text.

Copy to: Julian P P Robinson, SPRU, University of Sussex

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

Nobel Peace Prize 1995

Cambridge Office American Academy of Arts and Sciences 136 Irving Street Cambridge, MA 02138, USA Tel. (+1-617) 576-5022 Fax (+1-617) 576-5050 Email: pugwash@amascad.org	Geneva Office 69, rue de Lausanne 1202 GENEVA, Switzerland Phone: +41-22-906 1651 Fax: +41-22-731 0194 Email: pugwash@hel.unige.ch	London Office Flat A Museum Mansions 63A Great Russell Street LONDON WC1B 3EJ, England Phone: +44-171-4056661 Fax: +44-171-4315651 E-mail: pugwash@qmw.ac.uk	Rome Office Acc. Naz. dei Lincei via della Lungara, 10 I-00165 ROME, Italy Phone: +39-6-6572606 Fax: +39-6-6878376 E-mail: pugwash@ici.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

PUGWASH CONFERENCES ON SCIENCE AND WORLD AFFAIRS

Nobel Peace Prize 1995

Cambridge Office	Geneva Office	London Office	Rome Office
American Academy of Arts and Sciences 136 Irving Street Cambridge, MA 02138, USA Tel. (+1-617) 576-5022 Fax (+1-617) 576-5050 Email: pugwash@amascad.org	69, rue de Lausanne 1202 GENEVA, Switzerland Phone: +41-22-906 1651 Fax: +41-22-731 0194 Email: pugwash@hel.unige.ch	Flat A Museum Mansions 63A Great Russell Street LONDON WC1B 3EJ, England Phone: +44-171-4056661 Fax: +44-171-4315651 E-mail: pugwash@qmw.ac.uk	Acc. Naz. dei Lincei via della Lungara, 10 I-00165 ROME, Italy Phone: +39-6-6572606 Fax: +39-6-6878376 E-mail: pugwash@ici.it

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By FAX (212) 754 4073

Geneva, 29 October 1997

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With warm personal wishes,


Martin Kaplan

cc: J. Lederberg

Prof. M. Meselson - FAX 617-496-2444

Mr. J. Robinson - FAX 1273-685-865

DEPARTMENT OF MOLECULAR AND CELLULAR BIOLOGY
HARVARD UNIVERSITY

7 Divinity Avenue
Cambridge, Massachusetts 02138



Mr. José M. Bustani
Director-General
Organization for the Prohibition of Chemical Weapons
Laan Van Meerdervoort 51
2517 AE The Hague
The Netherlands

February 2, 1998

By Fax to 31-70-360-0944

Dear Mr. Bustani,

We refer to our letter of 19 July 1997 about the Harvard Sussex Program researcher based in The Hague. Mr. Daniel Feakes has served in that capacity since August 1997 and is now up for contract renewal with HSP. We were most glad to learn from your letter of 23 July 1997 (DG/IC/0240/97) that you looked forward to maintaining and continuing the relationship between HSP and the Secretariat.

Trusting that you have found value in the work of Mr. Feakes both for the Secretariat and as a point of access to HSP information resources and publications, we have agreed with Mr. Feakes that he should continue in his present position until the end of January 1999.

Among Mr. Feakes's assignments from HSP during this period is the organization of the next workshop, in Noordwijk, of the Pugwash Study Group on Implementation of the Chemical and Biological Weapons Conventions. The theme of the workshop, scheduled for 15-17 May, will be *CWC Implementation: Balancing Transparency and Confidentiality*. It is our hope that you yourself will be able to join us. Previous workshops in this series have benefited greatly from the participation of individuals from the Secretariat. Pugwash will be sending out formal invitations near the end of February.

Meanwhile please accept our very best wishes. We look forward to continued cooperation between HSP and the OPCW during the year ahead.

Sincerely yours,

Matthew Matthew
Harvard HSP Director

J P Perry Robinson
Sussex HSP Director

W1 Wilde

H7/FS

dept.

Date: Tue, 10 Feb 1998 20:51:29 -0500 (EST)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: An old story: Trichothecenes (fwd)

FYI

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 -----
Date: Wed, 11 Feb 1998 07:21:28 +0700 (BKK)
From: Henry Wilde <fmedhwl@chulkn.car.chula.ac.th>
To: mathew meselson <msm@wjh.harvard.edu>
Subject: An old story: Trichothecenes

You may recall me in my former life as a US Embassy officer here. I am now in Bangkok 8 months as a faculty member at Chulalongkorn University and summers I become a fisherman in Juneau, Alaska. To make it brief, I was asked to give a seminar on biological weapons and reviewed the new book: Textbook of Military Medicine PART I, Medical aspects of chemical and biological weapons published by the Army. It is an dcellent source of history and data BUT: Chapter 34 by Drs. Wannemacher and Wiener present the "yellow rain" issue as if it was for real. However, they are unable to cite any physical evidence data in Asia. The chapter is well written but gives, in my opinion, a completely false impression to the reader, particularly a historian. As you know well, I interviewed many people who came over the river or Cambodian border, spent 3 days in a KR field hospital looking at patients suspected as having been "yellow rained" and found nothing. I almost lost my feathers with my Thai academic colleagues working in this political arena and am gld that the lady that wrote the paper in Chemical News had mercy with me and did not cut me up in small pieces as she did with the other players in this field. Read the chapter and tell me what you think.

Best wishes,
Henry Wilde, MD
Professor of Medicine
Queen Saovabha Memorial Institute
1871 Rama IV Road, Bangkok 10330, Thailand

Date: Sat, 24 Oct 1998 19:43:53 -0400 (EDT)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: "Professor James R. CRAWFORD" <jrc1000@hermes.cam.ac.uk>, JRC3000@aol.com, Professor John DUGARD <J.Dugard@Law.Leidenuniv.nl>, JFVRJD@law.LeidenUniv.nl, Professor Philip HEYMANN <heyman@law.harvard.edu>, Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>, Raj DE <del@law.harvard.edu>
Subject: Three questions

Dear Friends,

Raj De and I need your advice on the three questions below in order to complete a draft update of the Commentary to the Draft Convention:

1) Considering the passage from Bassiuni and Wise given below, do you agree that Article V.2 of the Draft Convention provides jurisdiction to the custodial State party over any citizen of another State party even if there is no other nexus and WITHOUT THE NEED FOR A REQUEST TO EXTRADITE?

Bassiouni and Wise in their book "Aut Dedere aut Judicare" (1995) pages 16-18, draw a distinction between "regional conventions" (such as the 1971 OAS Terrorism Convention) and the 1970 Hague Hijacking Convention (and the other model conventions we have used). Certainly the wording in these two types of convention is quite different as regards the point at issue.

"In these regional conventions, the obligation to submit for prosecution comes into play only when a request for extradition has been refused, while the Hague Convention imposes a duty to prosecute whenever the fugitive is not extradited; It makes no difference that extradition has not been requested by the state where the offence took place (or by a state with another more specific jurisdictional interest)."

--Bassiouni and Wise

2) If your answer to question (1) is affirmative, could we delete V.1(f)? A reason for doing so is to make the Draft Convention neater and to remove the possibility of unintentionally weakening its applicability to development, production, transfer, etc.

3) If you agree that V.2 provides jurisdiction to the custodial state so long as the offender is a national of a State Party and even with no other nexus and no need for a request to extradite, would you further agree that if and when the Draft Convention is ratified by a large number of states a State party wishing to do so could reasonably claim universal jurisdiction even over a national of a non-party simply on the basis of custody, with no other nexus and no need for an extradition request?

If the answer to all three questions is yes, completing the commentary will be straightforward.

With very best regards,

Matthew

Date: Sun, 25 Oct 1998 01:08:32 EDT

From: JRC3000@aol.com

To: msm@wjh.harvard.edu, Dugard@law.LeidenUniv.nl, JFVRJD@law.LeidenUniv.nl, heyman@law.harvard.edu, J.P.P.Robinson@sussex.ac.uk, del@law.harvard.edu

Subject: Re: Three off the cuff answers

Dear Matthew,

I am in Stockholm away from my books, so I can only answer these questions off the cuff. Others may be able to be more definitive.

1) The passage from Bassiouni and Wise supports the broader interpretation of provisions such as Draft Article V.2. However that interpretation is not necessarily correct, and the position is unsettled. The origin of the provision lay in the need to reconcile the prohibition of extradition of nationals, and other constraints on extradition under treaties and national law, with the need for prosecution; the solution was to give a requested third state jurisdiction in the event of non-extradition even if it would not otherwise have had jurisdiction. Precisely because these conventions do not take the simpler step of conferring jurisdiction irrespective, I suspect that the better interpretation of Art V.2 may be the narrower one. However as far as I know the point has never been decided, and the broader interpretation is a possible one.

Yes. But language is #

2) If your answer to question (1) is affirmative, could we delete V.1(f)? That is a matter of judgement. Should you take a significant step away from the standard model in the direction of universality? That is a question of balancing the need for acceptability, especially at the threshold stage, with the need for universality. I have no strong view.

3) I do not see how universal jurisdiction can be asserted on the basis merely of a concession of jurisdiction over nationals of states parties. That jurisdiction is consensual. It would be easier to do so on the basis of an assertion of jurisdiction over crimes committed by anyone at all on the territory of states parties, because that has potentially erga omnes effects.

Best regards

James

The third state

A

offender

(A prohibits extradition even if non nationals)

B

C Has Jurisdiction

If ~~the~~ offender is national of A, no problem
if not but for some other reason extradition is constituting, country C can request that state A extradite.

Robinson

Date: Sun, 10 Dec 2000 13:19:03 +0000
From: Julian Robinson <J.P.P.Robinson@sussex.ac.uk>
To: Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re:

Dear Matt,

Your comments on Jonathan's YR piece are exactly right. It was a pedestrian, corner-cutting and sanctimonious piece of work, with a very nasty undertone to it -- but unfortunately quite in keeping with Monterey's standards on CBW. Let us hope that Parachini's influence will ultimately be felt there.

That said, I do notice one or two items in Jonathan's literature citations that I don't think I have seen -- the 1982 SNIE, for example, or the DIA report in n15, or the Edgewood one in n39, or the NRL one in n78. Do you have these?

As ever,
Julian

Date: Sun, 10 Dec 2000 01:32:42 -0500 (EST)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: "Jonathan B. TUCKER" <jtucker@miis.edu>
cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>,
"Matthew S. Meselson" <msm@wjh.harvard.edu>
Message-ID: <Pine.SOL.4.30.0012100112200.24470-100000@wjh1.wjh.harvard.edu>
MIME-Version: 1.0
Content-Type: TEXT/PLAIN; charset=US-ASCII
Status: RO
X-Status:
X-Keywords:
X-UID: 183

10 December 2000

Dear Jonathan,

Thank you for asking me to look over your draft article with a view to accurately representing my studies and conclusions about the Yellow Rain. I checked over the 13 places in the text and the 5 footnotes where my name appears and a few places where it is implied and did find a few errors, the most important of which are as follows:

1) page 2, para 3: I have never asserted that the "Reagan administration was cynically using false allegations of Soviet toxin warfare to justify a slowdown in arms control negotiations and the modernization of the U.S. chemical arsenal." Indeed, I have been criticized by some for not believing there was such a connection. I would be surprised if your reference (number 6) says otherwise. It is mistaken if it does.

2) page 19, para 1. It was not me but rather a British scientist named Green at Porton Down who in January 1992 discovered the abundant presence of pollen in yellow rain samples. I was told of this and discussed it with Green when I visited Porton in 1984. The Porton pollen finding was soon passed on to the USG and scientists at Edgewood then examined their collection of yellowish samples, confirming Porton's discovery. This was first made public in the November 29, 1982 State Department press briefing cited in your footnote number 54. On pages 19-20 of that briefing, Dr. Emory Sarver of Edgewood says "Most of the samples that are of yellow rain are fairly dry and they have a high level of pollen grains in them..."

The briefing continued on November 30 and on pages 11-12 of the transcript Gary Crocker is quoted as saying "It contains pollen, and not windborne pollen, but

pollen that would be commercially collected or is collected, if you will, by insects, the type of thing you would get in a honey bee would take from flowers."

So it was not me but rather Porton and then the USG that discovered Yellow Rain samples to have a high level of pollen and it was the USG that first came up with the idea that bees might somehow be involved. This is summarized on pages 223-224 of the chapter by Robinson, Guillemin and me that is your reference number 67. In case you do not have the full texts, I am sending you copies of the November 29-30 State Department briefings.

It was only after reading the November 29-30 press briefings that Joan Nowicke and I began to examine Yellow Rain samples ourselves, finding abundant pollen of the same plant families as gathered by southeast Asian *Apis dorsata* in the Joe Rosen/ABC sample and in all the samples provided to us by US, Canadian, and Australian officials.

In order to try to come up with more plausible hypotheses for what the yellow samples might be, I organized and Carl Kaysen chaired a two-day conference here in Cambridge. Sarver, Crocker, Rosen, Nowicke, Ashton, and Guillemin were among those who participated. I also invited Mirocha but on his day of departure he telephoned to say he was grounded by a snowstorm and could not come. At the end of the conference, Ashton and I telephoned Seeley at Yale and described to him the pollen-laden yellow spots. Seeley then immediately said it sounded like bee feces. The detailed pollen studies of Nowicke at the Smithsonian published in *Nature*, our staining and other tests done at Harvard, and the field observations of bee feces showers in Thailand by Seeley, Akwatanakul and me proved, published in *Scientific American*, and confirmatory work at Edgewood and Porton proved that the yellow spots and powders, including samples of those in which Mirocha and Rosen had reported trichothecenes, were bee feces.

In sum, it was Porton and the USG who discovered that the alleged CW agent was rich in pollen and suggested that there might be some connection with bees. What we did was to realize what the connection might be and then to demonstrate that the pollen-laden material was bee feces.

3) page 19, para 2: It was Dr. Joan Nowicke, not Prof. Thomas Seeley, who performed electron microscope analyses of the TY samples. This work is described in Nowicke and Meselson, *Nature* 309: 205-206, 1984 -- your footnote number 66.

4) page 21, para 3 and top of page 22: The reference (number 74) you give for this passage about "false positives" is from a secondary source (The WSJ) and neither it nor your description accurately reflect what our concerns were about the reports of trichothecenes in samples. That concern is stated in the letter on page 10 of *Scientific American* for January, 1986 by Thomas D. Seeley, Joan W.

Nowicke, Matthew Meselson, and Jeanne Guillemin. Please add a reference to this letter, which constitutes a concise statement of our overall view of the matter at that time. Also, please modify your account of our views about the reliability of the sample analyses more accurately to reflect the relevant passage, to be found at the top of the last column of our letter to Scientific American, a copy of which I will mail to you on Monday.

5) page 22, para 1: Mirocha reported to the USG his results for trichothecenes in more than 250 control samples that were submitted to him by the USG. His reports for the great majority of these control samples have never been made public. Because of this, it cannot accurately be said that "...neither Mirocha nor Rosen ever reported a false positive in control samples..." One might instead say something like "Among the results made public, neither Mirocha nor Rosen reported a false positive in control samples." [You may know that a document from within the USG tabulates many samples that were reported by Mirocha as positive but which were actually controls. This matter has yet to be resolved.]

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7) page 34, para 1: It is not correct that "Meselson and his colleagues reply that...the most rigorous standards of scientific proof and forensic procedure were appropriate." We never called for such an unusual degree of rigor, nor does the reference you give (number 107) support such a conclusion. Instead, we pointed out there that the established procedures ordinarily used by the USG were in this case initially bypassed and that subsequent USG studies and investigations discredited the Yellow Rain allegations but were not made public. As we wrote in the reference you cite, "In large measure, it was the systematic efforts of American government investigators that undermined the administrations case." ... "The U.S. intelligence community departed from established procedures for verifying laboratory AND field information and instead supported a conclusion that should have been regarded as only a hypothesis." I would be grateful if you would make these central conclusions of ours clear to your readers. [PLEASE ADD TO YOUR QUOTATION THE OMITTED WORD "AND" AS INDICATED BY CAPS ABOVE.]

If time had allowed, I would have gone beyond simply checking the passages and references where my name appears. I think that Jeanne will be in touch with you about the refugee reports, which we obtained and analyzed in detail. It is sad that you have not had time to review declassified cable traffic and other documents available from the USG that are available under FOI. Perhaps you will

someday find yourself writing yet one more article about the Yellow Rain, so as to describe the important dimension of the story that is largely missing from your present article: That what the USG was saying to the public was contradicted by USG studies and investigations that were kept from the public. When you are next in the neighborhood, let us try to get together.

With 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

Date: Sun, 10 Dec 2000 01:32:42 -0500 (EST)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: "Jonathan B. TUCKER" <jtucker@miis.edu>
cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>,
"Matthew S. Meselson" <msm@wjh.harvard.edu>
Message-ID: <Pine.SOL.4.30.0012100112200.24470-100000@wjh1.wjh.harvard.edu>
MIME-Version: 1.0
Content-Type: TEXT/PLAIN; charset=US-ASCII
Status: RO
X-Status:
X-Keywords:
X-UID: 183

10 December 2000

Dear Jonathan,

Thank you for asking me to look over your draft article with a view to accurately representing my studies and conclusions about the Yellow Rain. I checked over the 13 places in the text and the 5 footnotes where my name appears and a few places where it is implied and did find a few errors, the most important of which are as follows:

1) page 2, para 3: I have never asserted that the "Reagan administration was cynically using false allegations of Soviet toxin warfare to justify a slowdown in arms control negotiations and the modernization of the U.S. chemical arsenal." Indeed, I have been criticized by some for not believing there was such a connection. I would be surprised if your reference (number 6) says otherwise. It is mistaken if it does.

2) page 19, para 1. It was not me but rather a British scientist named Green at Porton Down who in January 1992 discovered the abundant presence of pollen in yellow rain samples. I was told of this and discussed it with Green when I visited Porton in 1984. The Porton pollen finding was soon passed on to the USG and scientists at Edgewood then examined their collection of yellowish samples, confirming Porton's discovery. This was first made public in the November 29, 1982 State Department press briefing cited in your footnote number 54. On pages 19-20 of that briefing, Dr. Emory Sarver of Edgewood says "Most of the samples that are of yellow rain are fairly dry and they have a high level of pollen grains in them..."

The briefing continued on November 30 and on pages 11-12 of the transcript Gary Crocker is quoted as saying "It contains pollen, and not windborne pollen, but

pollen that would be commercially collected or is collected, if you will, by insects, the type of thing you would get in a honey bee would take from flowers."

So it was not me but rather Porton and then the USG that discovered Yellow Rain samples to have a high level of pollen and it was the USG that first came up with the idea that bees might somehow be involved. This is summarized on pages 223-224 of the chapter by Robinson, Guillemin and me that is your reference number 67. In case you do not have the full texts, I am sending you copies of the November 29-30 State Department briefings.

It was only after reading the November 29-30 press briefings that Joan Nowicke and I began to examine Yellow Rain samples ourselves, finding abundant pollen of the same plant families as gathered by southeast Asian *Apis dorsata* in the Joe Rosen/ABC sample and in all the samples provided to us by US, Canadian, and Australian officials.

In order to try to come up with more plausible hypotheses for what the yellow samples might be, I organized and Carl Kaysen chaired a two-day conference here in Cambridge. Sarver, Crocker, Rosen, Nowicke, Ashton, and Guillemin were among those who participated. I also invited Mirocha but on his day of departure he telephoned to say he was grounded by a snowstorm and could not come. At the end of the conference, Ashton and I telephoned Seeley at Yale and described to him the pollen-laden yellow spots. Seeley then immediately said it sounded like bee feces. The detailed pollen studies of Nowicke at the Smithsonian published in *Nature*, our staining and other tests done at Harvard, and the field observations of bee feces showers in Thailand by Seeley, Akwatanakul and me proved, published in *Scientific American*, and confirmatory work at Edgewood and Porton proved that the yellow spots and powders, including samples of those in which Mirocha and Rosen had reported trichothecenes, were bee feces.

In sum, it was Porton and the USG who discovered that the alleged CW agent was rich in pollen and suggested that there might be some connection with bees. What we did was to realize what the connection might be and then to demonstrate that the pollen-laden material was bee feces.

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With 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

From msm@wjh.harvard.edu Wed Dec 13 14:14:05 2000 -0500
Date: Wed, 13 Dec 2000 14:14:01 -0500 (EST)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
To: Jonathan Tucker <jtucker@miis.edu>
cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>,
"Matthew S. Meselson" <msm@wjh.harvard.edu>
Subject: Re: Follow-up questions on Yellow Rain
In-Reply-To: <3A37836E.AD87237A@miis.edu>
Message-ID: <Pine.SOL.4.30.0012131215120.14950-100000@wjh1.wjh.harvard.edu>
MIME-Version: 1.0
Content-Type: TEXT/PLAIN; charset=US-ASCII
Status: RO

X-Status:
X-Keywords:
X-UID: 214

Dear Jonathan,

We have just faxed to you the Scientific American letter you requested and tonight will send by Fed Ex a package of various relevant materials, as listed below.

Dr Akratanakul was at the time of our field study with him in March 1984 Associate Professor of Entomology at Kasetsart University, Kamphaeng Saen Campus, Thailand. He holds a PhD in entomology from Cornell University and was consultant in apiculture for the FAO. Seeley described him as the main Thai bee expert.

In May 1984, two months after our field trip, Prof. Akratanakul accompanied the US Defense-State yellow rain team, at their request, on a similar field trip in order to confirm our earlier findings. An account of their observations, entirely consistent with our own, is contained in the unclassified telegram from the team to the State Department I have included in the Fed Ex package.

I imagine that dense flights of *Apis dorsata* if not to high and with favorable light conditions could be seen from the ground. Still, I do not recall seeing them during the showers that fell on us at Khao Yai and Chiang Mai. We did see groups of bees just leaving their big nests moments before a shower at Chiang Mai. The telegram at (4) says that the USG Defence-State team could see the bees "only with difficulty" during the shower in which they were caught. Obviously, it depends.

The amazing thing is that the bee expert Akratanakul cited at (B) in the telegram "never noticed bee feces deposits in all his years of work" until the yellow rain controversy arose. Yet the Temple Abbot says at (5) that the showers of bee feces "regularly take place". Neither of our faculty member who have studied bees, Ed Wilson and Carroll Williams, when shown bee feces on leaves from around Harvard had any idea what they were. Makes one think...

Incidentally, I think you give a diameter of the spots larger than what we actually describe in Nature or Scientific American. Also note that the Defence-State telegram states in its last paragraph that the bee feces spots it examined had the same size, color and consistency as spots of material described (in other telegrams) as the yellow rain.

You ask why the reports of allegations tailed off by 1984. That would be expected on either view -- if the allegations were true and the attacks halted

or, instead, if the allegations were erroneous but followed the sort of pattern that might be expected under the circumstances of the refugees and their initially uncritical interrogators. The interviewers of the Defence-State team concluded that the Hmong were not "accurate reporters of reality" as quoted in note 35 of our article in the 1990 MIT Press book and as reported in numerous other telegrams from the team. At least two reports from the USG team state that coercion from activist Hmong caused respondents to make allegations that they subsequently denied. To this add the evidence and reasoning in the middle column of our 1986 letter to Scientific American, stated again in the MIT book, and one has a heavy burden of solid, rather than anecdotal, evidence for not accepting the allegations at face value.

Jeanne has communicated with you about her detailed analysis of reported symptoms and the lack of the pattern claimed by some. To that I would add the fact that eye damage is almost never mentioned even though that should be an especially conspicuous effect if even very small amounts of trichothecenes contact the eyes.

Finally, a very important point. While T2 and other trichothecenes have definitely been found at low levels in mouldy grain from Thailand (as published by Porton) and at low levels in some control bloods (by a Canadian government lab), there is, as our 1986 letter to Scientific American states, "strong evidence against their common occurrence".

What our letter to Scientific American goes on to say is that the positive reports from Mirocha and Rosen are unconfirmed by any independent analysis done elsewhere, are without fully revealed control results, and are of uncertain integrity. Please do not overlook our letter and make the mistake of saying that we considered natural contamination to have caused any illness or to have been responsible for the positive results reported. As neither the US Army, Porton, Swedish MoD, or French MoD, analysing a very large number of samples and using methods and instruments far less prone to false positives than those used at Minnesota never found a single positive, even in portions of samples in which Minnesota had reported trichothecenes, the most plausible explanation is that the positives announced by the USG were false positives.

The so-called controls are not evidentiary because the results of only a very small proportion of the more than 250 control samples analysed in Minnesota were ever made public. Such highly selective disclosure is in itself suggestive of false positives and is certainly contrary to proper scientific practice.

Best wishes for the New Year.

Matt.

Robinson

Date: Thu, 07 Jun 2001 10:04:15 +0100
From: Julian Robinson <J.P.P.Robinson@sussex.ac.uk>
To: Prof M S Meselson <msm@wjh.harvard.edu>
Subject: Fwd: Fw: Further article

Dear Matt,

Thanks for the copy of your recent exchange with Khripunov about the Petrov/Supotnitski article in NG. I had just heard about the article from Derek Averre up at Birmingham, who also told me about a second article by those two. Here is his last message, and I have also pasted in his summary of the NG article. I have asked him for a copy of the Russian original of the NVO one.

>Envelope-to: jppr@hspsxsq.u-net.com
>Date: Thu, 7 Jun 2001 09:45:31 +0100 (GMT Daylight Time)
>From: lafa9@sussex.ac.uk (Carolyn Sansbury)
>To: j.p.p.robinson@sussex.ac.uk (Julian Perry Robinson)
>Subject: Fw: Further article
>X-Mailer: Siren Mail (Windows Version 4.0.2 (Windows 95/NT))
>X-Sender: lafa9@mailhost.central.sussex.ac.uk

>
>

>-- Begin original message --

>
>From: "DL Averre" <D.L.Averre@bham.ac.uk>
>Date: Wed, 6 Jun 2001 09:41:14 GMT
>Subject: Further article
>To: lafa9@sussex.ac.uk
>Message-ID: <47EFA0E4757@bss1.bham.ac.uk>
>Return-path: <D.L.Averre@bham.ac.uk>
>Received: from mailer3.bham.ac.uk ([147.188.128.54])
> by rinka.central.susx.ac.uk with esmtp (Exim 3.20 #10)
> id 157Yt1-0003ZY-00
> for lafa9@sussex.ac.uk; Wed, 06 Jun 2001 09:41:55 +0100
>MIME-Version: 1.0
>Content-type: text/plain; charset=US-ASCII
>Content-transfer-encoding: 7BIT
>Return-receipt-to: "DL Averre" <D.L.Averre@bham.ac.uk>

>
>Carolyn,
>Hoping that it's not too late can you pass on one more item for the
>next CBWCB, please.
>The source is Nezavisimoe voennoe obozrenie, no. 18, 2001 and it is
>by Mikhail Supotnitskii and Stanislav Petrov and entitled 'Sverdlovsk
>plague'. In it they more or less repeat the argument they made in
>their article which I abstracted and sent yesterday, but the
>conclusions are more explicit and link the furore which erupted in
>1980 with recent attempts to put political pessure on Russia. It
>finishes:

>
>'An analysis of key publications during the last propaganda outbreak
>which took place early in 1998 reveals that all materials used by
>[those 'exposing' the incident] were taken from open Russian
>scientific journals and then shamelessly distorted. The anthrax
>epidemic in Sverdlovsk in 1979 may have been only part of a
>large-scale subversive operation carried out against the USSR and its
>allies in the late 70s and early 80s with the use of BW. This
>operation continues against Russia today, only via information
>warfare.'

>
>Please confirm receipt.

>Best wishes
>Derek
>
>
>--- End original message ---

7. S. Petrov, M. Supotnitskii, 'Biological diversion in the Urals', NG-NAUKA, no. 5, 2001, p. 5. This lengthy article, co-authored by the former head of the Russian Radiation, Chemical and Biological Troops Stanislav Petrov, argues inter alia that the US State Department acted out of political motivations in spreading the story that the anthrax outbreak in Sverdlovsk in April 1979 was caused by leakage from a Soviet BW facility. It was first widely described in the Western press in January 1980, immediately after the Soviet invasion of Afghanistan. The article goes on, basing its arguments on open sources, that the epidemiological evidence, including the pattern of spread and length of the outbreak, and the meteorological conditions at the time cast doubt on the assertion that it was the result of leakage from a BW facility; in particular, it attacks the reliability of Professor Matthew Meselson's assessment of the incident and the attempts by K. Alibek ('recruited by the CIA') to falsify proof of the allegations. It makes an implicit connection between the unexplained fact that the anthrax strains detected were of North American and South African origin and the outbreaks of anthrax in Zimbabwe in 1979-80 in zones controlled by pro-Soviet partisans and of Dengue fever in Cuba in 1981, suggesting a 'biological diversion' which was used to justify the resumption of the US BW programme at a time when genetic engineering techniques for improving BW agents had just appeared and in order to depict the USSR as a 'rogue state' on the international arena.

Date: Sat, 09 Jun 2001 00:07:26 +0100
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
To: Prof M S Meselson <msm@wjh.harvard.edu>
Subject: Fwd: Re: Recent articles

FYI

>Envelope-to: jppr@hspsxsq.u-net.com
>From: "DL Averre" <D.L.Averre@bham.ac.uk>
>Organization: The University of Birmingham
>To: J.P.P.Robinson@sussex.ac.uk (Julian Robinson)
>Date: Fri, 8 Jun 2001 08:28:30 GMT
>Subject: Re: Recent articles
>X-Confirm-Reading-To: "DL Averre" <D.L.Averre@bham.ac.uk>
>X-pmrc: 1
>Priority: normal
>X-mailer: Pegasus Mail for Windows (v2.42a)

>
>Julian,
>The NG-Nauka - as you pointed out the science supplement to the
>respected (at least until recently, but the general feeling is that
>its quality has diminished in recent times) Nezavisimaya gazeta -
>article is 23 May, and the other one (NVO, translated as Independent
>Military Review, started as a supplement to NG but is now published
>separately though within the NG stable; much of its content reflects
>views in the military/security establishment though it is often
>critical of government policy, especially on military reform) is
>25-31 May. Do you want me to send a copy of the NVO article? As I
>said, it more or less repeats the NG-Nauka one but with the more explicit
>conclusions which I translated.
>I met Kalyadin a few months ago; he's knowledgable about the politics
>of chemdemil but without (at least so it seemed) being close to the
>government agencies carrying it out. I'd appreciate it if you let me
>know if he says anything new or interesting.
>Best wishes
>Derek

>
>
>Date: Wed, 06 Jun 2001 18:44:12 +0100
>To: DL Averre <D.L.Averre@bham.ac.uk>
>Subject: Recent articles
>From: J.P.P.Robinson@sussex.ac.uk (Julian Robinson)

>
>Dear Derek,

>
>Excellent to get your e-mails via Carolyn.

>
>Do you have dates of publication for the two Petrov/Supotnitskii articles?
>And is NG-NAUKA an adequate citation for our purposes -- what is it: a
>science monthly of some sort? 2001 no 5 implies May 2001, which would put
>at least one of the two articles just outside the catchment period of the
>Bulletin we are now finalizing. For a weekly, 2001 no 18 implies
>publication somewhere in the period 30 Apr - 6 May -- which could be just
>in the catchment. It sounds like this is, in its context, rather an
>important publication, so maybe I shouldn't rush into recording it in our
>Bulletin until Meselson and I have seen the whole thing. Might you be abl
e

>to copy us the Russian originals? I've no Russian but Meselson has a bit
>and also plenty of colleagues/students who could produce complete
>translations. Maybe FBIS or BBC-SWB already have, and I haven't yet notic
ed.

>
>Anyway, very many thanks for sending those items and also the others. Big
>changes, it looks like.

>
>Do you know Alexander Kaliadin of IMEMO? He is down as a
>fellow-participant in a workshop I'm going to in a couple of weeks time.

I
>met him several years ago, but have retained no very clear impression.

>
>Best,
>Julian

>
>
>=====

>
>J. P. Perry Robinson
>Sussex Director, Harvard Sussex Program
>SPRU -- Science and Technology Policy Research
>University of Sussex
>Brighton BN1 9RF
>United Kingdom

>
>Tel: **44 1273 678172 or 686758
>Fax: **44 1273 685865
>www.sussex.ac.uk/spru/hsp/
>

Matthew S. Meselson, 17:53 10/01/01 , Re: Christie

To: "Matthew S. Meselson" <msm@wjh.harvard.edu>
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
Subject: Re: Christie
Cc:
Bcc: qsff6@central.susx.ac.uk
Attached:

Dear Matt,

I have at last found the file containing my correspondence with Dr A Barnett Christie during the period May 1980 to March 1981. I am copying it all to you by snail-mail.

The undated letter of his that you mention I have dated to around 20 October 1980. He was referring to an article by Nicholas Wade in *Science* [vol 209, pp 1501-2, 26 Sep 80, "Death at Sverdlovsk: a critical diagnosis"] in which he had been quoted, a copy of which I had sent him by -- as you will see -- my letter of 15 October. I think you will remember that article well, if you haven't done so already.

I am very much looking forward to learning more about your exchanges with Zelicoff.

As ever,
Julian



.At 04:41 08/11/00 , you wrote:

>
>
>
>

>Dear Julian,

>

>Do you remember where it was that Dr. Benett Christie (spelling?) the anthrax expert was quoted as saying something to the effect that he doubted that clinical examination could distinguish anthrax infection caused by inhalation from that caused by ingestion? I do have the third edition of his textbook in which such a statement appears. Was there another relevant statement by Christie published somewhere else?

>

>Later, Christie wrote to you saying that he took no exception to how his views had been represented. I have a copy of his (undated) letter to you but not of your earlier letter to him.

>

>Could you let me have a copy of your letter and a copy or a reference to the publication to which Christie refers?

>

>

>Best,

>

>Matt.

>

>

>

>

>

>_____
>_____
>Matthew Meselson

>Department of Molecular and Cellular Biology
>Harvard University

Memorandum from telephone conversations with Dr A B CHRISTIE, 31 May 1980

NOTE: Dr Christie, a leading British clinician, is now in retirement (a very active one). Formerly head of the Dept of Infectious Disease at the University of Liverpool and a Senior Consultant Physician at Fazakerley Hospital, Liverpool, he is the author of the standard British work in this field, namely Infectious Diseases: Epidemiology and Clinical Practice (1st ed 1969, 2nd ed 1974, 3rd ed due out shortly). His clinical experience of anthrax in man is probably unsurpassed in Britain, for he has seen more than half of the cases that have occurred in the country over the past thirty years.

His address is 2 Beach Lawn, Waterloo, Liverpool, L22 8QA; telephone (051) 928-8645.

(1) He stated that, if he were confronted with a case of what was in fact intestinal anthrax but in which the symptoms did not include gastro-intestinal pain, he probably would not diagnose the disease correctly, even though the possibility of anthrax would, because of his past experience, be more likely to occur to him than it would to most clinicians. He qualified this by remarking that if the case displayed pustules on, say, the chin, that observation might possibly set him thinking about intestinal anthrax. In his book he had remarked that intestinal anthrax "is unlikely to be diagnosed clinically unless it is known that infected meat has been eaten".

(2) He thought it unlikely, though not inconceivable, that gastrointestinal pain would in fact be absent from intestinal-anthrax symptomatology. He remarks in his book that the intestinal lesions that have been reported on autopsy in cases of intestinal anthrax closely resemble the sores of cutaneous anthrax--which, despite their often hideous appearance, are accompanied by little, if any, pain. But he stated that anthrax sores in the stomach or the intestines would, in his opinion, be likely to give rise to an irritation manifest as gastrointestinal pain. This opinion was, he stressed, pure conjecture on his part--since he had never seen an actual case of intestinal anthrax--and could well be wrong (see para 3 below). He observed, moreover, that abdominal pain is uncommon in cases of typhoid and paratyphoid fevers, even though the most characteristic lesion produced in these diseases is an often extensive ulceration of the intestinal mucosa: there was usually pain only when the ulceration culminated in perforation of the intestinal wall. This, too, he has noted in his book.

(3) He stated that it was conceivable that gastrointestinal symptoms might not be apparent in cases of intestinal anthrax, especially fatal cases; the dominant feature might instead be a state of severe shock. Supporting this possibility, which he has noted in his book, is the relative commonness of apoplectic death in mortally anthracose cattle; this appears to be due to the 'lethal factor' (anthrax toxin) acting at some central site and dominating the course of the disease.

(4) Several times he reiterated the point that, because of the rarity of the intestinal form of anthrax, there was very little that could be reliably stated about the characteristics and course of the disease. Anybody who might venture an opinion on such matters could do so only in the most speculative of terms. There could be no solid empirical evidence to support that opinion, only a small body of anecdote.

(5) Though he doubted whether anyone else in Britain could add much to what he had said about intestinal anthrax, one person that might be worth contacting is Dr Joan R Davies, who is a Deputy Director of the Public Health Laboratory Services and whom he knows. She is the PHLS's listed anthrax person. Address: PHLS, Colindale Avenue, London NW9; telephone (01) 200-6868.

JPPR

167-80

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

18 July 80

Dear Dr Christie,

You may remember our telephone conversations towards the end of May on the subject of gastric anthrax. I think I explained to you that my interest in this question stemmed from the efforts currently under way in Washington to ensure that the Government's appraisal of the allegations that the Russians have a secret germ-warfare factory at Sverdlovsk is solidly founded. Enclosed are copies of recent articles from New Scientist (10 July) and the International Herald Tribune (30 June) which describe something of the background.

Also enclosed is an aide-memoire I wrote for myself after our telephone conversations. May I trouble you, first, to look it over to see whether it reflects your views accurately, and, second, to consider whether you would mind my showing it (amended as necessary) to people in Washington? If I were to do the latter, there is a slight chance that extracts from the memorandum might find their way into a Congressional publication.

I have taken the liberty of giving your address to an American friend of mine who is deeply involved in all this, namely Matthew Meselson, the geneticist, a professor of biology at Harvard. He has been in close touch with Dr Brachman (and several other US authorities inside and outside government, and he has some points that he wishes to take up with you.

I must apologise for putting you to this bother. I do hope it will not deter you from assisting in resolving this murky business, in which there has so far been far too much muddled thinking and jumping to unjustified conclusions. I should perhaps add that our own Government, despite being one of the three co-depositaries (with the USA and the USSR) of the 1972 Biological Weapons Disarmament Convention, has chosen to leave investigation of the affair to the Americans. I hope very much that I shall hear from you soon.

Yours sincerely,

J P Perry Robinson
Senior Fellow

22/7/80

Dr A. Barnett Christie

Telephone:
051 - 928 8645

2 Beach Lawn
Waterloo
Liverpool L22 8QA

22 . 7 .80

Dear Dr Robinson,

Thank you for your letter of 18th July, 1980,
and for the enclosed articles which I find most interesting.

The side-memoire of our telephone conversations
is very accurate and I have no objection to your showing it to
people in Washington.

I am reviewing the newest, and much the heaviest,
book on infectious disease from America. The section on anthrax
is not very full. There is one paragraph on intestinal anthrax:-
'Symptoms of gastrointestinal anthrax appear 2-5 days after the
ingestion of raw or inadequately cooked food. Symptoms are
initially quite non-specific with abdominal pain, nausea,
vomiting and anorexia. This is soon followed by bloody
diarrhoea, often with toxæmia and shock. Commonly the
diagnosis is made too late unless an adequate epidemiologic history
is obtained'.

I am not sure whether this reflects personal
experience of the gastrointestinal disease or not. I suspect the
latter. The author is Dr Donald M. Poretz, Fairfax, Virginia.
There are 173 authors in this book and except in Dr Poretz's
case there is a note of their appointments etc. So Dr Poretz may
be retired. The title of the book is Principles and Practice

of Infectious Diseases. Editors Mandell, Douglas and Bennett;
John Wiley & Sons, New York etc. ¹⁹⁷⁹ It might be worthwhile
contacting Dr Forsetz. I would be interested in anything he might
add if he has personal experience of intestinal anthrax.

I will be happy to answer, if I can, anything
your friend, Matthew Meselson, may wish to discuss.

Yours sincerely,

Barnett Christie
A.B. Christie

Dr J P Perry Robinson,
Senior Fellow,
Science Policy Research Unit,
University of Sussex.

25/7:80

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

25 July 80

Dear Dr Christie,

Thank you very much for your letter of 22 July. It's
most good of you to have taken such trouble.

I'll keep you informed.

Yours sincerely,

J P Ferry Robinson

1/8:00

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

1 Aug 80

Dear Dr Christie,

May I bother you with another tricky point to do with the Sverdlovsk anthrax outbreak ?

On autopsy in cases of gastric, as opposed to pulmonary, anthrax would you expect the mediastinal lymph nodes to appear haemorrhagic ? Might that be a rare or a common finding, do you think ?

Sorry to go on like this.

Yours sincerely,

J P Perry Robinson

6/8/80

Dr A. Barnett Christie

Telephone:
051 - 928 8645

2 Beach Lawn
Waterloo
Liverpool L22 8QA

6th August 1980.

Dr. J.P. Perry Robinson,
University of Sussex,
Science Policy Research Unit.

Dear Dr. Robinson,

Thank you for your letter of 1st August re anthrax.

I would not expect to find haemorrhagic mediastinal lymph nodes in a case of intestinal anthrax: the mesenteric nodes would be affected. It is, I suppose, possible that while masticating infected meat one might inhale some anthrax bacilli and so get both pulmonary and intestinal anthrax but I would expect that to be very rare and I don't know of any such reports. If it were suggested as a common occurrence I would simply not believe it.

I have discovered that there was a large outbreak of intestinal anthrax in Haiti in 1943. It was reported by Steele (mentioned in Van der Hoeden's Zoonoses, Elsevier Press). The only reference given to Steele is Steele J D and Helvig R J Public Health Reports Washington, 1953, 68,616. I don't know if this deals with the Haiti outbreak or if it is the same Steele. The librarian should be able to find Public Health Reports for you-- public health departments in this country used to take it.

I go abroad for a short tour next week and am very busy clearing up: otherwise I would have gone to the library myself to look things up.

Yours sincerely,

Barnett Christie

A.B. Christie.

26/8:fo

Dr. A. Barnett Christie
2 Beach Lawn
Waterloo
Liverpool L80 2BA.

26th August 1980.

Dear Dr. Christie,

Thank you very much for your letter of 25th August. Julian Perry Robinson is currently away but I will draw your letter to his attention when he returns at the beginning of September.

Yours sincerely

M. A. Holly (Miss)
Secretary to J. P. Perry Robinson

15/ro:fo

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

15 Oct 80

Dear Dr Christie,

I enclose a copy of a recent article in Science about the Sverdlovsk anthrax affair, in case you haven't seen it. That you are mentioned is, indirectly, my fault, I'm afraid; but I hope it won't have disturbed you.

Also enclosed is a copy, plus English translation, of the Bezdenezhnykh & Nikiforov article referred to in the Science piece.

With best wishes,

Yours sincerely,

J P Perry Robinson

encs: Science 209: 1501-2 (1980)
Zh Mikrobiol Epidemiol Immunobiol (1980), 5, 111.

ca 20/10:80

Dr A. Barnett Christie

Telephone:
051 - 928 8645

2 Beach Lawn
Waterloo
Liverpool L22 8QA

Dear Dr Perry Robinson,

Thank you for sending me the articles on the Sverdlovsk affair. I find it all very interesting, and not a little puzzling. The Russian article is difficult to assess. If this was a matter of scattered cases always related to the slaughter of an animal why all the testing of air samples, hospital wards, walls, dishes etc, emergency antibiotic prophylaxis and immunization of the population? There was no 'outbreak of cases' but all this complicated investigation and preventative measures! It is very strange.

I am not at all disturbed at the way my name is mentioned although I usually try to keep out of controversy.

Dr J P Perry Robinson,
University of Sussex.

Yours sincerely,

A.B.Christie

Barnett Christie

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

11/3: F1
11 March 81

Dear Dr Christie,

May I once again ask for your help with regard to the Sverdlovsk anthrax affair ?

A surprising new twist has caused the American authorities concerned to embark upon a complete reassessment of the affair. I understand that the question of differential diagnosis as between the gastric and inhalation forms of the disease remains crucial. One of the American officials primarily involved in the reassessment, namely Dr Robert Mikulak of the US Arms Control & Disarmament Agency, has just telephoned me to say that he would very much appreciate an opportunity of meeting with you to discuss the affair. He has other duties which require him to be in Geneva during 23rd March to 3rd April, and is wondering whether by any good chance you might be in London at one or the other end of that period and be willing to meet him (and me) then. From his standpoint the best time would be the morning of 23rd March; but I imagine that this would hardly be convenient for you.

Mikulak, whom I know well, is a chemist by training, but I think you would find him at least as competent as any chemist to comprehend and discuss the minutiae involved. He has already spent many months seeking out expert American opinion on the points at issue; I know he has had long discussions with Dr Brachman. I have the strong impression that, if the American reassessment is to arrive at well balanced and well founded conclusions, it is essential that Mikulak be as well informed as possible. So I do hope very much that you and he will be able to get together. He will be telephoning me again around 16th March to learn whether anything can be arranged.

Under separate cover I am sending you various papers on anthrax that have recently come my way in case there may be some among them that you have not seen, for example those in translation from the Soviet epidemiological and military-medical literature.

With best wishes,

Yours sincerely,

J P Perry Robinson
Senior Fellow

19/3/81

Dr A B Christie
2 Beach Lawn
Waterloo
LIVERPOOL
L22 8QA

19 March 81

Dear Dr Christie,

Here at last are the papers on anthrax I promised in my letter of 11th March. I am sorry it has taken so long.

I spoke with Dr Mikulak the day after you very kindly telephoned me, telling him of your readiness to meet with him. We left it that he would telephone you from Washington to make a definite date once he had checked the possibilities of travelling to Geneva from Manchester. I haven't heard from him since, but expect to do so this evening.

With best wishes,

Yours sincerely,

J F Perry Robinson

encs

- Cherkasckiy, JMEI 1979(F):3-6
- Dallwitz + Beall, Arch Pathol 83: 154-161 (1967)
- Perl + Dooley in Binford + Connor (eds) Pathol. of Hospital & Extrahosp. Disease (1976): 11f-12j
- Kefedzhiev, Mikhov, Nacherama, Epidem. Microbiol. Infect. 65 (1975) 16(1): 86-89, 1975
- Cherkasckiy, Savinykh, Markov, JMEI 1978(11): 130-134
- Etkin (ed) Atlas in Epitomi (R. 1961): 491-7
- Man 5th Edn 9 Feb 80 + NYT 26 Sep 80
- Marchuk + Boyatzyrov, Feldsher. Akush 40(2): 10-15, 1975
- Zhurav JMEI 1977(11): 1-5

Domaradskij
=

Date: Tue, 09 Oct 2001 20:46:03 +0100
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
To: Wendy Orent <orentw@mindspring.com>
Subject: Re: Igor Domaradskij's Memoir

Dear Wendy,

Thank you for this and also for trusting me with a copy of your new and (much) improved version of the text. I am sorry that four months should have gone by without word from me.

Of the two feelers I put out, as I promised I would, into CEUP's behaviour, one has produced results. I am still waiting on the other. Recent events, chiefly in the form of an interview earlier today, have made up my mind to tell you what I have learned thus far, without waiting for the corroboration I had been anticipating from my second feeler.

You will appreciate that confidences are involved here. I simply cannot identify to you my source. All I can say is that my source was a major figure in the formation and direction of CEUP. That is now in the past, but my source's contacts with CEUP remain good enough for me to have been shown, but not allowed to copy, the hostile third review.

I described that review, to my source, as a work of art. Not many words, but its gist was that the text was worthless because it said nothing new. The review did not in fact substantiate this assertion in any way, but stated it with such assurance that the slightest predisposition on the part of a reader to take the reviewer seriously would have caused the review to be read as a profound indictment of the text. For my part, including my rather substantial knowledge of a few of the things that Professor Domaradskij was writing about, I can say without a shadow of doubt that the reviewer's assertion was false, and therefore that the review was a conscious fabrication. I do not know who the reviewer was, but his status was evidently sufficient for CEUP to decide to reject the MS. We know well that the Russian Defence Ministry has long been blocking attempts by outsiders to understand more of its BW-related work, both past and present. I take seriously, therefore, your contact's remark about the KGB having long hands. It is those hands, or hands closely related to them, that I see in the third review.

Today I find myself accused of having been a KGB spy -- of having taken money in return for information or influence (I am not sure which). The allegation is entirely preposterous, but it is always difficult to demonstrate a negative. None of this is in any way public, at least not for the time being, and I tell it to you (and to anyone else who may be reading this message) in confidence, but I am clearly set to suffer in ways that have yet to become apparent. Knowing, however, that the allegation is false, I of course ask myself who I have upset recently. In my business, like most others, petty enmities and rivalries exist, but one would not expect those whom I might conceivably have driven to secret denunciation to know how actually to do it and also, in doing it, to be taken seriously. So I wonder if I, too, as an enthusiast for publication of Domaradskij's memoir, may not have become a victim of Russian disinformation. My two feelers may not have gone unnoticed.

Even the possibility of this explanation being correct has thrown me into such a rage as to make me yet more determined to help the Domaradskij/Orent project see the light of day. Problem is I can see no practical way to help. Maybe you can suggest something.

It would be instructive to know is whether the second reviewer, who you

indicated in your original e-mail of 9 July was also an enthusiast for publication, has also been on the receiving end of possible KGB ministrations. Or maybe he is too grand or senior a figure to be vulnerable.

Please feel free to share this message of mine with Ben. Under the circumstances, I would not want to burden him with it directly, but I have no doubt at all that he could offer you wise guidance on the implications of what I say.

Best,
Julian

At 16:30 10/07/01 , you wrote:

>Dear Julian,

>

>I don't know whether I have managed to reach you before you set out
>on your travels - I wrote you immediately yesterday but couldn't send
>the email because heavy winds sent a large hickory limb crashing down
>on our powerlines outside, causing an immediate blackout! So whatever
>I wrote you has vanished into oblivion.

>

>We did not think your comments were trivial - they were thoughtful
>(as Ben Garrett had said they would be), and they helped us a great
>deal. I had taken too much for granted in the earlier edition - that
>people would know who Lysenko is, for instance. Your notes brought me
>up short - I tried to annotate everything I could find. Whether I
>have succeeded is another matter - things get by one so easily. So
>any comments you care to offer us would be gladly received.

>

>If you can find out anything about CEUP we would of course be very
>interested. I spoke to one ex-BW scientist who said "the KGB has long
>hands (sic) and if they wanted to stop the book, especially over
>there in Hungary, they would." Domaradskij, for his part, is
>skeptical.

>

>I don't know about smallpox in Iraq. Michael Osterholm has said
>publicly that King Hussein told him on his deathbed (the king's, I
>mean) that they do. That probably means about as much to you as it
>does to me. North Korea, unfortunately, is an entirely different
>matter. The evidence is much stronger than testimonials. I can't say
>what it is, as I have promised not to. But a good account appears in
>one of Judy Miller's NYT articles. (She had not been given the same
>warning I had, or she would not have disregarded it - it involves
>someone's personal safety.)

>

>All the best, and thanks again for your help.

>

>Wendy

>

>P.S. I've sent the text in RTF and in Microsoft Word formats. I hope
>you can open them - please let me know if you cannot. I am sending
>one in a separate email.

>

>

>

>>Dear Wendy,

>>

>>Thank you for your message earlier today. I am very upset to learn that
>>the CEUP has decided against publishing the Domaradskij memoir: it is a
>>fine work, from which many will benefit. I shall inquire into CEUP's

>>behaviour through such channels as are open to me. I take it to be a
>>further sign of the quality and value of the work.
>>
>>You are far too generous about my mostly trivial comments on the MS. Of
>>course I would like to see the revision, and would respect the confidence
>>in which you and Professor Domaradskij shared it with me. Tomorrow I head
>>off on travels but I shall be reading my e-mail, so an attachment would be
>>fine. Whether I shall be able to come up with any practical suggestions is
>>another matter. My own publisher at present is Palgrave's (formerly

>>Macmillan).

>>
>>My colleague Matthew Meselson of Harvard University recently met with
>>Professor Domaradskij and speaks most warmly of him. I do hope this
>>enterprise finally turns out well..

>>
>>Best,
>>Julian

>>
>>PS Are we really to believe that smallpox virus is held in Iraq and North
>>Korea?

>>
>>
>>=====

>>
>>Professor J. P. Perry Robinson
>>SPRU -- Science and Technology Policy Research
>>University of Sussex
>>Brighton BN1 9RF
>>United Kingdom

>>
>>Tel: **44 1273 678172 or 686758
>>Fax: **44 1273 685865
>><http://www.sussex.ac.uk/spru/hsp/>

>
>
>--
>Wendy Orent
>4562 Club Circle
>Atlanta, Georgia 30319
>(404) 841-9727
>

Date: Sun, 25 May 2003 18:19:43 -0400
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
To: Prof M S Meselson <msm@wjh.harvard.edu>
Subject: HSP agenda

[Part 1, Text/PLAIN 154 lines.]
[Unable to print this part.]

[The following text is in the "iso-8859-1" character set.]
[Your display is set for the "US-ASCII" character set.]
[Some characters may be displayed incorrectly.]

Dear Matt,

I'm now safely back after far too short a visit. Among the many pleasures was working once more in lovely Wood's Hole and, on the plane back, starting in on the Holmes book about Meselson-Stahl. The book is wonderful, to me, in both its conception and in its execution (though admittedly I still have a way to go) and one day you must tell me whether it is also accurate -- or at least as accurate as one might reasonably hope such a thing to be. I have never before read a biography of an experiment. When I am next visiting I shall beg a second copy (if you really do have a 'box full') to give to Brian Balmer: more than most, he would appreciate its unique character, and, as a professional observer of science, he would much benefit from it, I think.

You and I managed to pack in a good bit of HSP business during those few days, but there were also some things left undone, so, given my increasingly feeble memory, I shall try to recall them all here. Later, on specific items, I shall send more detailed messages. First, though, please tell me again your dates for Moscow and say whether there really might be a chance of my intercepting you in London either on your way out or on your way back.

Here is my recollection of our HSP agenda:

[1] WHO CBW study. On the List of Contributors to the Second Edition, we agreed on a proper rendering of the Russian names, which I will now transmit to Ottorino/Karen.

[2] Projected meeting/seminars at WHO in November. You would be willing to stay an extra day in Geneva after the 8-9 November Pugwash workshop in order to participate in discussions at WHO of post-Second-Edition work on CBW, but only if we can also visit the ICRC on that same day (Monday 10 November) to discuss criminalization with ICRC lawyers. Peter Herby has told us that he will fix something for us at ICRC that day. Should I now tell Ottorino (a) that we cannot do the seminars he is proposing, but that we could (b) participate in a half-day CBW-related meeting? If (b), should the meeting be in the morning or afternoon, and whichever it isn't should you or I propose to Peter for the ICRC session?

[3] Agenda for the November Pugwash workshop in Geneva. Dear Graham, in his e-mail to us dated 10 May, has proposed an agenda under the general title "The BWC Intersessional Process Towards the Sixth Review conference". If we think this too narrow/boring, we need to come up with an alternative quite soon otherwise Martin may rush forth. Richard Burgess has offered to contribute a paper, if invited, on the US DHHS and DA biosecurity regulations. We did not talk about this.

[4] Bulletin 60. By asking Fiona to cover the 33rd Session (24-27 June) of the OPCW Executive Council in PITH, we in effect postponed the copy date for the June issue from 13 June by 2-3 weeks -- indeed, until July, I hope no later than its first week. That gives us more time to (a) produce an Editorial about the outcome of the First CWC Review Conference and the importance of a process now being set in motion to solve the 'law enforcement' problem, and (b) to decide whether we want to accept dear Graham's offer, in his e-mail of 10 May, to provide an Invited Article on 'Security and Oversight of Pathogenic Microorganisms and Toxins'. Given that we drew a blank on Ledogar, I would be happy to go with Graham's

suggestion (which fits squarely into this year's agenda for the New Process) provided we can indeed do the Editorial.

[5] Fundraising. We wrote emergency project-funding letters to Christine Wing at Ford and Kinette Benedict at MacArthur and also a good core-funding proposal, which we sent, as a draft for comment, to MacArthur, warning Ford that we would be sending it to them also, in due course. Next step is to send the core-funding proposal to Sloan and, after that, to others in the light of the foundation data you downloaded from the internet and of the advice received from Phil Heymann (about approaching the Bill Gates Foundation, Ted Turner Foundation, Soros via Neier, Johnson Foundation, &c). Also, Margaret Hamburg at NTI may be expecting something from us following your visit to NTI on 6 May. In Europe, I shall not hear until the middle of next month at the earliest about the three project-funding proposals Sussex has put in to European Commission and UK Research Council organs; all are for a 2004 start. The University of Sussex will have to take its decision on renewal of Dragffy's contract by the end of June at the latest. Daniel and I will be working on proposals to Wellcome and to Rowntree (the latter in the hope that Dando and the Bradford cuckoos haven't entirely ejected us from the Rowntree nest), but any success will be too late to save Dragffy.

[6] The Tregonning succession. You will have seen Scott Spence's favourable response to our offer. We now need to ensure a reply from DG Pfirter to the letter from us that will possibly reach him the day after tomorrow. Since we have asked Fiona not to visit Sussex in June, shall we instead ask Scott to do so before he takes over from her? That could make a fair bit of sense.

[7] The Luis Moreno Ocampo proposal. His suggestion, when we met him in Phil Heymann's office on 20 May, was that we should think of aligning our projected HSP workshop at the Hague Academy of International Law with the public hearings that he is contemplating holding in The Hague, prior to the end of September, on the subject of a possible ICC Prosecutor's Plan regarding War Crimes relating to CBW -- a plan that would enunciate a message for business corporations contemplating the CBW-weapons market. I think he is now expecting to receive from us, rather soon, a 1-2 page paper with suggestions for such public hearings, including thoughts about industry sponsorship (Carpenter/Walls?) and associated networking (I had mentioned BWPP).

[8] The Commentary on the HSP draft criminalization convention. The outstanding task is for the (mixed) contributions of the three students to become properly integrated into the original draft. You reckoned that input from Nick Dragffy would, at this stage, be premature. I have not yet seen anything beyond the original text.

[9] Sandy's book-purchases for SHIB. I was very glad to be able to take away the Sidel book. I am also hoping for a sight of Thomas Graham, Disarmament Sketches: Three Decades of Arms control and International Law (2002), which Sandy says you still have.

[10] HSPOP-1. Even though I am hoping to get a complete MS to Palgrave in August, we did not talk much about this -- other than about the possibility of your contributing to the US case-studies section a text drawing from the Howard assessment of CS operations during the Vietnam War. That would be really great, if you can find the time. For my part, I am starting work on integrating Sandy's new Eisenhower/Kennedy/Johnson documents into the chronology on policy, employment and technical developments concerning disabling chemicals. When you are in Moscow, will you keep your eyes open for a possible author of a case-study on the Moscow theatre siege?

[11] My paper on the 'law enforcement' chemicals problem. Under the label 'Item 421Rev2' you have in your computer the latest (10 May) version of the paper I presented at the Oegstgeest Pugwash workshop in April. Should you have a chance to look at it, do please tell me of any improvements that need making to it. I am currently expanding the paper in the light of new data and revising it in the light of comments received. My plan then is to disaggregate it into contributions to

HSPOP-1 and possibly also to submit it for publication in some journal, possibly the London International Affairs.

[12] Richard Guthrie. I told you that he has now been offered my old job at SIPRI. I think he will accept the offer, in which case he will leave HSP come the end of August. We have been paying him so little that his departure will not actually save us all that much money, but the gap will need to be filled. My plan is to assign Bulletin DTP duties to an existing HSP Sussex person, most probably to Caitriona McLeish, and to buy (the very expensive) Quark Express DTP software so that she is not compelled to use Richard's DOS Ventura, which is excellent but difficult and does not run under the Windows to which which most people are nowadays more accustomed than DOS. She'll need to go on a training course, I think, but her partner should be able to advise. Done expeditiously, such an arrangement should mean that we would not be dependent on Richard for production of Bulletin 61 in September. I would be quite keen on his remaining involved, while at SIPRI, in further development of the HSP data-bases, especially the development backwards in time of the CBW Events Data-Base. As for the website, I propose putting Carolyn in charge, assisted by the University Computer service (which offers website guidance) and Caitriona. Their first task will be to redesign the HSP websites for mounting solely (if you agree) on a Sussex server and then to post full texts of the latest HSP papers, as well as the Bulletin, &c.

I know I have probably forgotten some things, but that seems quite enough for the moment.

As ever,
Julian

Garwin

Date: Mon, 14 Jul 2003 18:42:58 -0400 (EDT)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Re: 07/09/03 letter R.L. Garwin to M.S. Meselson. (fwd)

Dear Julian,

For you only.

M.

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 -----
Date: Mon, 14 Jul 2003 17:47:10 -0400 (EDT)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Jean Hernandez <jahern@us.ibm.com>
Cc: Dr. Richard GARWIN <RLG2@watson.ibm.com>,
Graham ALLISON <graham_allison@harvard.edu>,
Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: 07/09/03 letter R.L. Garwin to M.S. Meselson.

Dear Dick,

If you are in Woods Hole on the afternoon of Friday August 8, Jeanne and I hope you can come, with Lois and Laura if they will be in WH, to our annual summertime party, 4:00 to 6:00 PM, at our house at 341 Woods Hole Road (508-540-0086).

I have been thinking more about your invitation to contribute materials to the Non-lethal Weapon Task Force Panel and my offer to do so. Not without misgivings, have decided that I should qualify whatever help I can give. Let me explain:

On the list of panel members you sent, at least 16 of the total of 27 are active duty or retired USMC officers, employees of companies with a business interest in the subject, and/or members of the Joint Non-lethal Weapons Project, of which the Executive Officer is an active duty USMC general. I admire the USMC, but the Corps is not charged with responsibility for long-term policy and overall security planning in the CBW disarmament area.

I do not see the names of anyone I know to believe that the fire-break provided by the present US guidelines is more important to long-term US security than is the possible utility of departing from those guidelines.

My concern is that important parts of the Administration are failing to appreciate the menace to the long-term safety of American citizens, and to US security generally, inherent in relaxing the present guidelines. I am therefore reluctant to have my name associated with the panel report, lest some of its audience conclude that the basis for such concern was adequately weighed in the deliberations of the panel.

So, despite my great affection and admiration for you, I ask that I not be cited anywhere in the panel report as having been consulted or having contributed in any manner. If that is acceptable, I would of course be delighted to provide written materials and to spend as long as you wish

discussing the subject in Woods Hole on August 8 or any other convenient time and place.

All good wishes,

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

Date: Tue, 3 Feb 2004 19:14:38 EST
From: Martinfuranski@aol.com
To: msm@wjh.harvard.edu
Cc: J.P.P.Robinson@sussex.ac.uk
Subject: Re: Ricin paper

Dear Matthew & Julian:

I apologize for the delay on a reply about an article for the CWC bulletin. I have been swamped with moving and had a bad bout with that nasty post-Christmas flu that was circulating. And the Harvard U Press/SIPRI Korea Allegations chapter has been taking up most of my available academic time and resources (only a few boxes of my archives were accessible for several weeks during the moving process).

Apologies aside, it seems that Ricin is now big news. I attach a paper I wrote a year ago, but for reasons addressed in the introduction, I only circulated to an individual well known to us who works for the Justice Department (or is it Homeland Security now?) who has a special interest in CW. I had obtained some more complete archival material on my last trip to NARA in November, and had hoped to revise it, but perhaps I had best circulate it again as it was in Feb 2003. I do not think the central conclusions would be changed by the new data.

I do not know if it is suitable for the CWC bulletin, but you two probably ought to have a look at it, if only because you will be interviewed on Ricin by the media, and some history is always useful. If you think it would have a place in the published realm, I would appreciate any suggestions.

One item that I did not have a chance to put in the paper is the fact that can find NO instance of a HUMAN ever dying, or even becoming ill from INHALED Ricin. The talk of micrograms of Ricin being lethal by inhalation are extrapolations from animal studies, and as the paper addresses, these laboratory exposures are unlikely to be duplicated in a real weapon. Human deaths of ingestion are quite rare (usually homicide). Castor beans were used in some traditional Chinese remedies, and in looking over my Chinese Medical History, there are a few reports of Ricin poisoning from this source. Injected, it is also also deadly in small amounts, as Bulgarians with umbrellas well knew. But I can find NO mention of illnesses or deaths at Castor Oil Industrial factories, where raw Ricin containing materials were handled in industrial quantities, nor in the CBW Ricin preparation laboratories, which produced kilogram quantities of highly enriched (even crystalline) Ricin and milled dry concentrates to 6 microns. Such human illnesses would almost certainly have been mentioned in my archival sources, as HUMAN susceptibility was always a question, and the BW/CW program was not bashful about using laboratory infections and industrial accident exposures to calibrate their agents.

I will be in touch, but will be out of email contact from 15 Feb to 10 March 2004, being out of the country.

I have moved. My email remains the same, but otherwise my contacts have changed:

Martin Furmanski
1333 Beachmont Street
Ventura, CA 93001 USA
805-654-1694 voice
805-654-1872 fax
Martinfuranski@aol.com email

I have received Jeanne's MS, and am looking forward to reading it tonight. I will reply with comments soon.

Best personal regards,

Martin

[Part 2.1, Application/APPLEFILE 640bytes.]
[Unable to print this part.]

Date: Sat, 6 Nov 2004 17:43:02 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: Re: Yellow Rain, etc. (fwd)

Dear Julian,

The correspondance below is for you only. Recall that Wilkinson was political officer in the US Bangkok embassay during the early 1980s. I may already have sent you the email from Kit Green, Hope to see him before too long.

As ever,
 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

----- Forwarded message -----
 Date: Tue, 2 Nov 2004 16:13:22 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Jim Wilkinson <jimwilkinson@earthlink.net>
 Cc: Matthew S. Meselson <msm@wjh.harvard.edu>
 Subject: Re: Yellow Rain, etc.

Dear Jim,

Many thanks for this and for the later email with coordinates for Don Stader. Wouldn't it be best to find a way to bring all of us together, perhaps in DC -- you, Mort Abramowitz, Don Stader, me, and perhaps also Kit Green, from whom I recently received the nice email copied below. I could probably find some money for everyone's air fare. With all of us together, we might produce something of archival value on the YR issue. Do let me know what you think. If you think it a good thing to do, would you be willing to invite Don Stader?

Regarding your hypothesis about the yellow materials cited and handed in by Hmong, here are some points that would need to be accomodated:

- 1) Reference to a yellow material began quite early. Most respondants in even the earliest interviews tabulated on the summary sheet that I sent to you cite a yellow material.
- 2) I suspect that the State Department summary of colors is not accurate. Unless the State Department characterization of the colors is based on interviews that I do not have, it is based on misreading their own data.
- 3) Samples of bee feces were handed in quite early. The two samples handed in during the State Department interviews of 22 Hmong at Ban Vinai in June 1979 were yellow spots on vegetation. And bee feces on bark was handed in during the 38 US Army interviews of October 1979. I received a portion of that sample for investigation. It was definitely bee feces.
- 4) Bee feces showers are not rare in the parts of Thailand where we travelled. We recorded a shower in Khao Yai national park and another shower in Cheng Mai. Yet, very few Thai and no Hmong who we interviewed them for what they were. The State-Defense team experienced a shower in a Temple Garden in Cheng Mai, described in a telegram from the Embassy to DC.
- 5) Despite the fact that bee feces showers are not rare, very few Thai or

Hmong who we interviewed could identify bee feces (on leaves) we showed them and one Hmong group said they were fallout from Soviet nuclear tests.

6) Chinese peasants in Jiangsu are reported to have thought that bee feces showers of 1976 were "poison from the sky", yet had no incentive to fabricate. This makes it plausible that the Hmong did similarly.

7) So far as I know, nothing but bee feces (and occasionally some common industrial chemicals) was ever handed in by Hmong. So if there was an agent, it was something that could not be sampled, surely not a solid or a non-volatile liquid. That leaves only a gas or highly volatile liquid and this does not tally with most Hmong accounts, even accounts not accompanied by samples.

8) The great majority of Hmong accounts of yellow sticky materials and of yellow spots were not accompanied by samples.

It seems to me that, like the Chinese of Jiangsu, many Hmong respondents really did believe that bee feces were or could be "poison from the sky". As one of the State-Defense telegrams to DC said, the Hmong are not very good observers of reality. Incidentally, neither are distinguished Harvard Professors of Entomology! When I showed bee feces on leaves to Ed Wilson and Carol Williams one evening, neither had any idea what the stuff was...

It's 4:15 PM EST now and we just came back from voting the old-fashioned way. Ink pen, numbered paper ballot, smiling neighborly attendants, and a bake sale, most delicious, in the back of the room.

As ever,

Matthew

Matt!

I just heard that you have recently (I hope, recently...and not some time ago...indicating I am not in the know!) won the 2004 Lasker...and soon the Nobel. Congratulations!

I good deal has gone over the bridge, under the dam, or some other mixed metaphor since the last time we spoke...and I hope things are going very well with you. I will be in Boston certainly sometime in the near-term...and it is long overdue for us to meet face-to-face and tell war stories.

After spending 20 years post CIA (I resigned in 1985) with General Motors, I ended up there as Chief Technology Officer, and lived in Asia for 10 years...last in Shanghai and Singapore. About 15 years ago I started a training program in MRI, and three years ago returned to the full-time practice of medicine. I do a huge amount of consulting with DOD on various terrorism issues, and am the Chair of the Joint Independent Science Panel for the Undersecretary for Operations Research, and on several Academy Commissions as well...so we may be able to catch up one day in D.C..

But, the purpose of this note is to offer the warmest congratulations and best wishes to you...hope to see you soon.

Kit

Date: Sat, 6 Nov 2004 17:43:02 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: Re: Yellow Rain, etc. (fwd)

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Kit

Date: Fri, 26 Aug 2005 11:15:11 -0400 (EDT)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: missing commentary found;Katz

Dear Julian,

Develops that the 29 March draft13M with my handwritten notes from April was all the while of your visit lurking in a heap just to the left of your elbow.

When in Cambridge later this week I will photocopy and send it to you and Nick. As my writing may not all be decipherable to you, I will also type the relevant notations into the text and send that electronically.

Fresh from Milton and attached to the email immediately following is the Katz dissertation. I have not yet looked at it. Odd that I was asked by her supervisor Burton Singer to be on her dissertation committee and accepted, even travelling to an early meeting with Singer, Katz and her DIA mentor, later sending her comment on her first prospectus...but in the event never consulted further. Academic misbehavior on the part of Singer, as it deprived his student of the possibility of a very possible useful source of information on the topic of her work.

Don't know much about Singer, do you?

We miss you here in WH.

As ever,

M.

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

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M.

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Harvard University
7 Divinity Avenue
Cambridge, MA 02138 USA
email: <msm@wjh.harvard.edu>
telephone: (617) 495-2264
telefax: (617) 496-2444

Date: Sat, 28 Jan 2006 15:30:32 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Martin FURMANSKI <Martinfurmanski@aol.com>
Cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>,
Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: Evaluating yellow rain hypotheses (fwd)

Dear Martin,

In order to evaluate the validity of the hypothesis that mycotoxins or any other chemical weapons were responsible for claims attributed to Hmong refugees it is useful to list the specific propositions that must be accepted in order to validate the particular hypothesis.

For example, in order to accept the hypothesis that trichothecene mycotoxins were used, one must accept that:

- 1) Trichothecenes were present in the blood and urine of alleged victims at detectable concentrations -- even though such substances were never detected in the analyses of ca 20 such samples done at CDE Porton using a methodology with greater discrimination than the methodology employed at the University of Minnesota. The analyses of biomedical and environmental samples done at the University of Minnesota for AFMIC are those on which USG claims of trichothecene use have been based.
2. Trichothecenes were present in blood and urine of alleged victims -- even though many days elapsed between alleged exposures in Laos and taking of samples in Thailand and even though experiments done at Porton showed that the half-lives of the relevant trichothecenes in blood are much shorter than the times between alleged exposure and sampling.
- 3) Trichothecenes were used -- despite the fact that trichothecenes were not found in any of the more than 200 environmental samples from alleged attacks analysed by high resolution GC/MS at the US Army laboratory at Edgewood or the ca 50 environmental samples from alleged attacks analysed at Porton. Among the samples analysed at both of these laboratories was an aliquot of the sample reported by the Minnesota lab to have the highest concentration of trichothecenes (a yellow powder). Neither were trichothecenes detected in environmental samples from alleged attacks done by high-resolution GC/MS at the Swedish MoD lab at Umea in Sweden or at the French MoD lab at Le Bouchet.
- 4) The results from the Minnesota laboratory are free of false positives -- despite the fact that only a minority of the analyses actually done at Minnesota were publicly reported and among those not made public there were numerous false positives, according to an internal unpublished AFMIC document which concludes that the entire body of reports from the Minnesota laboratory is therefore non-evidentiary.

Similarly, in order to accept the hypothesis of CS use which you mention and which may indeed have happened on a few occasions (I myself was exposed in 1970 at a US firebase in Quang Nai Province, South Vietnam when VietCong/NVA fired a probably US made CS mortar round at us) one must believe that, despite CS exposure, most of the recorded claims attributed to refugees neglected to cite the principal expected effects of exposure to CS, including eye irritation.

Jeanne and I would be delighted to help you to survey the entirety of the holdings we have, including transcripts and records of refugee reports, internal USG telegrams regarding Hmong interviews, other relevant USG telegrams and documents, personal memos, technical literature, etc.

Very best regards and hoping to see you before long,

Matthew

Date: Sat, 28 Jan 2006 22:18:44 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Matthew S. Meselson <msm@wjh.harvard.edu>
Cc: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Yellow rain

Dear Julian, would you like any changes in this draft email to Graham?

Dear Graham,

I spoke briefly with you in Geneva when we were last together about an article Julian and I are preparing on the yellow rain allegations, with emphasis on lessons learned regarding the value and importance of high quality science in helping to evaluate allegations or indications of chemical (including toxin) attack. I recall that you mentioned studies conducted at CDE Porton on the stability of trichothecene mycotoxins in blood and perhaps in live experimental animals. Was it Black or Upshaw who did some of this work? I would be much indebted to you for any detailed information on this topic, both published and unpublished, as well as any information regarding physical examination of the various yellow materials designated as coming from alleged attack sites.

With all best wishes, as ever,

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

Date: Sun, 29 Jan 2006 14:48:26 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Graham PEARSON <100703.162@CompuServe.COM>
Cc: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Trichothecenes, etc.

Dear Graham,

Recall that we spoke briefly in Geneva when we were last together about an article Julian and I are preparing on the yellow rain allegations, with emphasis on lessons learned regarding the value and importance of high quality science in helping to evaluate allegations or indications of chemical (including toxin) attack.

If it is not too great an imposition, I would ask for your help in obtaining information on three topics:

1) Trichothecene stability. I have what I believe to be all of the published papers from CDE Porton describing the stability of trichothecenes in blood (listed in your article in issue 20 of our Bulletin) but would very much appreciate learning any additional details and, if such exists, information on stability in live experimental animals, not just in blood in silico. In case I have missed any publications or otherwise available internal reports, I should appreciate having citations or copies of these.

2) Pollen in yellow materials. I would like to know the results of physical examination of the various yellow materials designated as coming from alleged attack sites -- both as to the proportion of such samples found to contain substantial amounts of pollen and the approximate number of such samples examined.

3) Calculations done at Porton showing that under field conditions trichothecene mycotoxins would not be toxic enough to have caused the effects attributed to them, as you mentioned to a committee of the House of Commons in April 2000.

All good wishes, as ever

Matt

Matthew Meselson
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W.L. ROBINSON

Date: Wed, 8 Feb 2006 18:28:41 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Robinson <jppr@hspsxsq.u-net.com>
Subject: Re: YR chapter

Dear Julian,

Does this mean that Tom is willing to join you in writing for the Lavoy meeting and the ensuing book?

Which of the following should I say when I tell Lavoy that I am unable to take it on:

- 1) nothing more, or
- 2) that he should ask you, or
- 3) that I understand that you would be willing to do it and he should ask you do so, leaving it to you to invite Tom to join you?

I would favor option 3.

Instruct me soon, so that I may relieve Lavoy's anxiety.

Doesn't this mean that you and Tom will give us the pleasure of your company during the time that you will be looking through the YR archive I have at Harvard????

As ever,

M.

Matthew Meselson
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On Wed, 8 Feb 2006, Julian Robinson wrote:

Date: Wed, 08 Feb 2006 20:30:06 +0000
From: Julian Robinson <jppr@hspsxsq.u-net.com>
To: Prof M S Meselson <msm@wjh.harvard.edu>
Subject: YR

Dear Matt,

Tom is game for involvement. He says he has some deep scars from his old battles. What shall I send him to read, do you think?

As ever,
Julian

=====

J. P. Perry Robinson
Sussex Director, Harvard Sussex Program
SPRU -- Science and Technology Policy Research
University of Sussex, Freeman Centre
Brighton BN1 9QE
United Kingdom

Date: Wed, 8 Feb 2006 07:34:20 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: Re: Visits to So Cal (fwd)

First is a series of which you may already have one or two...

Matthew Meselson
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----- Forwarded message -----
 Date: Thu, 26 Jan 2006 14:45:38 EST
 From: Martinfuranski@aol.com
 To: guilleje@bc.edu, Martinfuranski@aol.com
 Cc: guillemin@mit.edu, msm@wjh.harvard.edu
 Subject: Re: Visits to So Cal

Jeanne:

I will send my thoughts about the YR documents when I get through them. Rebecca Katz finished her PhD dissertation on YR at Princeton last year. I got an electronic copy from Martin Hugh-Jones, and can send you a CD-ROM of it if you want. It is about 6 megabites, and more than I can practically upload through AOL. Rebecca Katz has also put it on Amazon.com as a downloadable pdf book for \$55. Martin H-J had said she was in negotiations with U of La Press to publish it formally: I don't know if her amazon.com release adversely affects a more academic publication, or implies U of La passed on publishing it.

Dr Katz's dissertation is very long (over 350 pages) and not terribly accessible. She relies upon a system of judging the "veritas" of the raw YR information based upon her quantitation of its "dubiousness" and "fallacy" and a determination of its appropriate "weight." Such a system of evaluating evidence is totally unfamiliar to me. It certainly is not the usual methods used in the biological sciences or epidemiology. She says in her dissertation that she developed this system herself for the evaluation of the data, but I wonder if it is not based upon some more fundamental system familiar to social scientists. Your impression of this would be useful.

Dr Katz's dissertation will require some additional detailed scrutiny on my part. In her "implications" chapter she states "An unsuccessful investigation would be one where the findings were inconclusive. By this definition the Yellow Rain investigation was not successful. Investigators were unable to prove CBW use in a court, to the United Nations, or to the general public, and the debate over Yellow Rain continues until this day." And in her "introduction" chapter she states: "My findings support the contention that weapons attacks occurred in he late 1970s and early 1980s in Southeast Asia and Afghanistan, but the strength of this determination is limited by gaps in the available data and by problems with the investigative and laboratory methods at the time."

However, using this same data, she concludes in her abstract and conclusion that: "I find that there appears to be sufficient information from a variety of sources to make a confident assessment that a chemical or toxin agent was used against the Hmong, Khmer Rouge and Mujahidin." She stops short of saying the agent was a mycotoxin. She speculates on an "unknown mixture" or

the use of a mycotoxin-based herbicide. How the data progressed from "inconclusive" to allow a "confident assessment" is a little mysterious to me at this point. However, I must note that she was a fellow at the military intelligence college, and I believe one of her dissertation advisors was an ex-military officer who had been a principle in the YR investigations.

Elements of Dr Katz's dissertation certainly demonstrate a definite political point of view. For instance, in her "implications" chapter she states: "Reliance on treaties for arms control is naive as well as perilous, as States can revoke their affiliations, evade, or just ignore an international treaty with limited consequence." She also dismisses what she calls "the bee theory." At times this dismissal becomes more than a little pointed, as in a footnote (on page 70) which states:

"Both Meselson and Evans are purported to have had ulterior motives that shaped their view of yellow Rain evidence. Meselson helped create the Biological Weapons Convention and was committed to proving that chemical and biological weapons were not being used by the Soviet Union (or any other nation), as that would have meant a violation and failure of the treaty. He also suggested the Bee Theory, which was inconsistent with the refugee testimony. Grant Evans was a professor in Australia and ardent member of the Communist Party of Australia. He was given a tour of Laos, traveling with Pathet Lao officials. It is possible that some of his claims are derived from his sympathies with the Pathet Lao. This note is not intended to discredit either man, but rather to inform the reader of possible biases."

On the theme of disclosing "ulterior motives" of YR investigators, she is sometimes less vigorous in her research and/or disclosure. For instance, she characterizes the initial YR field team as including Drs Fred Sidel, who is described as "from Aberdeen Proving Grounds" and Dr (MajGen) William Tigertt, who is described as "a retired general and pathologist." No mention that Dr Sidel was the physician in charge of the human experimentation program at Edgewood from the late 1950s to its end in 1975, and that William Tigertt was his corresponding physician in charge of the human BW experimentation program at Detrick, again from the late 1950s to its end in 1975. Both men were famous in these positions, were long-term "advocates" of CBW weapons, and both had lost their careers in the 1969-1975 rollback of CBW research and development. If there ever were two men where there might have been "an ulterior motive" to find the Soviets were using both BW(toxin) or CW weapons, and to justify a re-establishment of the US BW and expansion of the US CW programs, it was these two old CBW weaponeers. Clearly they had a lot of experience examining humans affected by CBW weapons, but they couldn't identify a single CBW agent to explain the symptom reports they collected. Sidel took liberties with Ockham's Razor and proposed YR was Sarin + Mustard + an unidentified agent. Neither Nerve nor blister agents were identified chemically on specimens.

If you're going to share a podium with Dr Katz this summer, maybe you'd better read her dissertation. Let me know where I can mail the CD-ROM if you want me to send you one.

Best regards,

Martin

In a message dated 1/25/06 5:20:35 PM, guilleje@bc.edu writes:

> Dear Martin,

> Many thanks for your message. We will try to connect when we are next in LA.
> Anything of note you can find in those 8000 pages would be of great interest
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> Best regards,
> Jeanne Guillemin
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Date: Wed, 8 Feb 2006 07:34:20 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: Re: Visits to So Cal (fwd)

First is a series of which you may already have one or two...

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 -----
 Date: Thu, 26 Jan 2006 14:45:38 EST
 From: Martinfuranski@aol.com
 To: guilleje@bc.edu, Martinfuranski@aol.com
 Cc: guillemin@mit.edu, msm@wjh.harvard.edu
 Subject: Re: Visits to So Cal

Jeanne:

I will send my thoughts about the YR documents when I get through them. Rebecca Katz finished her PhD dissertation on YR at Princeton last year. I got an electronic copy from Martin Hugh-Jones, and can send you a CD-ROM of it if you want. It is about 6 megabites, and more than I can practically upload through AOL. Rebecca Katz has also put it on Amazon.com as a downloadable pdf book for \$55. Martin H-J had said she was in negotiations with U of La Press to publish it formally: I don't know if her amazon.com release adversely affects a more academic publication, or implies U of La passed on publishing it.

Dr Katz's dissertation is very long (over 350 pages) and not terribly accessible. She relies upon a system of judging the "veritas" of the raw YR information based upon her quantitation of its "dubiousness" and "fallacy" and a determination of its appropriate "weight." Such a system of evaluating evidence is totally unfamiliar to me. It certainly is not the usual methods used in the biological sciences or epidemiology. She says in her dissertation that she developed this system herself for the evaluation of the data, but I wonder if it is not based upon some more fundamental system familiar to social scientists. Your impression of this would be useful.

Dr Katz's dissertation will require some additional detailed scrutiny on my part. In her "implications" chapter she states "An unsuccessful investigation would be one where the findings were inconclusive. By this definition the Yellow Rain investigation was not successful. Investigators were unable to prove CBW use in a court, to the United Nations, or to the general public, and the debate over Yellow Rain continues until this day." And in her "introduction" chapter she states: "My findings support the contention that weapons attacks occurred in he late 1970s and early 1980s in Southeast Asia and Afghanistan, but the strength of this determination is limited by gaps in the available data and by problems with the investigative and laboratory methods at the time."

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>

Date: Wed, 8 Feb 2006 07:34:59 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: second in series

[The following text is in the "X-UNKNOWN" character set.]
 [Your display is set for the "US-ASCII" character set.]
 [Some characters may be displayed incorrectly.]

second in series

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 -----
 Date: Thu, 26 Jan 2006 17:00:30 EST
 From: Martinfurmanski@aol.com
 To: msm@wjh.harvard.edu, Martinfurmanski@aol.com
 Cc: guilleje@bc.edu, guillemin@mit.edu
 Subject: Katz dissertation

Matthew:

I just finished a long email on the subject to Jeanne, which should have been cc'ed to you as well. Since you are going to Princeton shortly, I will make a few more, perhaps rash and premature comments. I would have held off for a few more reads of Katz and the documents, but you may need a precis before going down to PU.

To put it bluntly, I think Katz's dissertation leans heavily toward the military's view of the YR events. I wonder if there was any influence. As I mentioned in Jeanne's email, the body of the dissertation seems more equivocal regarding the evidence than does the official, visible "conclusions" in the abstract and conclusions chapter.

One thing I noticed was a co-incidence in the official US military interpretation of the YR event and Dr Katz's conclusions. I received her dissertation from Martin H-J in conjunction with a review of "misinterpretations" of BW treats that I was completing for a volume on Agro-BW terrorism that Martin H-J is editing for the OIE. In my research for that paper I had gone over the "official" statements regarding YR that appear in US military technical manuals. The US military continued to state specifically and unequivocally that the use of mycotoxins had occurred in SE Asia and Afghanistan in the late 1970s and early 1980s. (the latest such statements were in 1996 editions, superseded in 2003). However, in March of 2003, the wording changed to "chemical attacks" in Laos and Cambodia in 1970s and Afghanistan in the 1980s (mention of mycotoxins or biotoxins was dropped). Knowing the lead time for official approval of such changes, and then proofing and printing, I must conclude that the US military made this decision to change their position on mycotoxins per se no later than late 2002. Dr Katz comes to exactly this conclusion (confirming CW attacks but not confirming mycotoxin agents). She began her dissertation research in Fall of 2002: the declassified materials she used to reach these conclusions were not released through FOIA until Sept-December 2003, and at that time she states that she had personally been involved in their declassification (!) and that their physical state indicated they had not been examined for years

if not decades. I can't help but wonder how much the official military position influenced the dissertation's findings. It appears from the chronology that the military policy change occurred in advance of the dissertation research, and did not rely upon the re-examination of the declassified Katz documents.

One of Katz's dissertation committee members was Col Michael Meese (prof at West point, Nat'l War College and Princeton U) who had been involved in the YR investigation as a West Point cadet during a summer internship. Katz had a fellowship with the Joint Military Intelligence College during the dissertation research.

I must say I personally have not had to defend a dissertation, but I would imagine that one does take into consideration the dissertation committee member's positions.

A couple of thoughts of my own on YR. These are just my preliminary ideas so please don't share them just yet, though I would appreciate your opinion:

There just isn't good scientific proof of "lethal" CBW attacks in the YR events: even Katz admits this. Despite Katz's use of a lot of quantification of "veritas" it all boils down to whether you believe the accounts that "CW" attacks occurred, and if you believe the deaths occurred and were related to CBW agents. There is a broad and a narrow view of this. I think the bee feces element is well established, and explains many of the reports, and that the intentional generation of erroneous/fabricated accounts is also well established and accounts for many others. Katz dismisses the "bee theory" because it does not explain deaths, and only grudgingly acknowledges the obvious fabricated elements of the YR allegations, and chooses to give "added weight" to early accounts before there was obvious secondary gain to fabricate CW stories to obtain refugee status. However, she seems to retain the conviction that the figure of thousands of deaths must be true. To my view that detracts from her credibility as a disinterested academic evaluating the controversy. She should at least "derate" the death numbers to account for deliberate exaggeration.

However, I think a more focused investigation may show some interesting "meat" in the YR allegations. I will admit that there could well be a "nidus" of true "CBW" attacks buried in a larger body of erroneous/fabricated/exaggerated reports. I spent a lot of time chasing such isolated events in the Korean War allegations, and though they did not prove to be BW attacks, they proved important in characterizing the scientific/epidemiological underpinnings of the entire event, beyond the additional and confusing overlay of deliberate propaganda fabrications. At his point I am looking into the likelihood that there is a "nidus" of "CW" attacks that are due to "non-lethal" CW agents, specifically RCAs (CN, CS) and perhaps herbicides. Many of the accounts of victims are quite typical of high-dose CN and CS effects, and CN and CS were identified in the materials from SE Asia (perhaps the most important data in the Katz papers!). And CN can be lethal, particularly when in a liquid vehicle, such as is commonly used, and might be used in a YR aircraft spray tank or chemical rocket warhead or artillery shell. And I would suggest that RCAs (or the accompanying munition explosions) might well cause bees to vacate their hives and defecate en masse. (would your bee expert friends know about this, or be able to do an ad hoc experiment?) (I had a beehive taken down from my house, and the bees left behind were really mad and came in the house, landed on the ceiling, and defecated on my desk!) Yellow rain from enraged bees might have been an effect of an RCA attack (or conventional weapon attack) and not just a natural occurrence. So an {air strike-gas attack-yellow rain} event is an understandable constellation of events. Katz even theorizes a herbicide containing

mycotoxins as a candidate agent (with unintended human toxicity) to explain the trace mycotoxin results. The early reports of US teams unequivocally identify RCA attacks as well. So there is good data to account for RCA air strikes or RCA artillery strikes with CW effects. It is just that the US couldn't very well scream and yell about Vietnamese/Soviet use of CS in SE Asia! Or herbicides for that matter. And of course, using RCAs and herbicides to attack indigenous "Insurgent" peoples without CW protective gear, who were screened by jungle, or to drive them out of safe havens and deny them territory was one of the uses the US had put RCAs and herbicides to in the Vietnam War. Perhaps the take-home lesson is that YR is part of the "Great Chemical Warfare Taboo Breach" that the US made in the Vietnamese War. A lesson perhaps not too welcome among the advocates of CW and BW that did the investigations.

Let me know if you have any thoughts.

I will take you up on the offer of looking over your YR documents, once I get to a point where I have enough command to properly benefit from them.

I will attempt to upload the Katz dissertation on an email to follow. It may fail, as AOL doesn't do large uploads very well. If it does fail, give me an email with your preferred address and I will US mail a CD-ROM to you.

Best regards,

Martin

In a message dated 1/26/06 7:55:59 AM, msm@wjh.harvard.edu writes:

> Dear Martin,
>
> I will be visiting Princeton on February 8 and expect to discuss the
> thesis of Rebecca Katz with people there. Could you let me know at least
> your preliminary appraisal of the yellow rain material and the questions
> it may raise at your early convenience? We have a large archive of
> USG telegrams, Hmong interviews and other documents regarding the yellow
> rain which you would be most welcome to peruse. Besides, it would be fine
> to see you again.
>
> As ever,
>
> Matthew
>
>

Date: Wed, 8 Feb 2006 07:35:41 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Third in series

third in series

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 -----
Date: Sat, 28 Jan 2006 15:30:32 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Martin FURMANSKI <Martinfurmanski@aol.com>
Cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>,
Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: Evaluating yellow rain hypotheses (fwd)

Dear Martin,

In order to evaluate the validity of the hypothesis that mycotoxins or any other chemical weapons were responsible for claims attributed to Hmong refugees it is useful to list the specific propositions that must be accepted in order to validate the particular hypothesis.

For example, in order to accept the hypothesis that trichothecene mycotoxins were used, one must accept that:

- 1) Trichothecenes were present in the blood and urine of alleged victims at detectable concentrations -- even though such substances were never detected in the analyses of ca 20 such samples done at CDE Porton using a methodology with greater discrimination than the methodology employed at the University of Minnesota. The analyses of biomedical and environmental samples done at the University of Minnesota for AFMIC are those on which USG claims of trichothecene use have been based.
2. Trichothecenes were present in blood and urine of alleged victims -- even though many days elapsed between alleged exposures in Laos and taking of samples in Thailand and even though experiments done at Porton showed that the half-lives of the relevant trichothecenes in blood are much shorter than the times between alleged exposure and sampling.
- 3) Trichothecenes were used -- despite the fact that trichothecenes were not found in any of the more than 200 environmental samples from alleged attacks analysed by high resolution GC/MS at the US Army laboratory at Edgewood or the ca 50 environmental samples from alleged attacks analysed at Porton. Among the samples analysed at both of these laboratories was an aliquot of the sample reported by the Minnesota lab to have the highest concentration of trichothecenes (a yellow powder). Neither were trichothecenes detected in environmental samples from alleged attacks done by high-resolution GC/MS at the Swedish MoD lab at Umea in Sweden or at the French MoD lab at Le Bouchet.
- 4) The results from the Minnesota laboratory are free of false positives -- despite the fact that only a minority of the analyses actually done at Minnesota were publicly reported and among those not made public there were numerous false positives, according to an internal unpublished AFMIC document which concludes that the entire body of reports from the Minnesota laboratory is therefore non-evidentiary.

Similarly, in order to accept the hypothesis of CS use which you mention and which may indeed have happened on a few occasions (I myself was exposed in 1970 at a US firebase in Quang Nai Province, South Vietnam when VietCong/NVA fired a probably US made CS mortar round at us) one must believe that, despite CS exposure, most of the recorded claims attributed to refugees neglected to cite the principal expected effects of exposure to CS, including eye irritation.

Jeanne and I would be delighted to help you to survey the entirety of the holdings we have, including transcripts and records of refugee reports, internal USG telegrams regarding Hmong interviews, other relevant USG telegrams and documents, personal memos, technical literature, etc.

Very best regards and hoping to see you before long,

Matthew

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telefax: (617) 496-2444

Date: Wed, 8 Feb 2006 07:36:12 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Fourth in series

fourth in series

Matthew Meselson
Department of Molecular and Cellular Biology
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email: <msm@wjh.harvard.edu>
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----- Forwarded message -----

Date: Mon, 6 Feb 2006 13:48:25 EST
From: Martinfurmanski@aol.com
To: msm@wjh.harvard.edu
Subject: Re: Katz dissertation

Matthew:

I received the Katz documents from Rebecca Katz with the understanding that I would await the publication of her thesis or obtain her permission before publishing material based on her declassified documents. Since she has "self-published" her thesis on amazon.com, I feel the letter of our agreement has

been fulfilled, but I still do not want to disaffect her unnecessarily as she has been cooperative in sharing these documents. Conferring with you is of course not a "publication" and is perfectly appropriate, but please exercise some prudence in using my name or this material at Princeton. I would bet I am the only "outside source" regarding these declassified documents. But you of course may already have had some of these from other sources.

This must be a very preliminary assessment of the Katz YR documents. It consists of about 500 electronic files, each of may contain from 1 to 156 pages of declassified documents. The files are arranged in a series of random short runs of differing dates of origin. I have so far examined perhaps 20-25% of the files, and presumably a similar percentage of the documents. So while this is probably a representative sample, there is the possibility that new elements will emerge as I wade through this further.

The documents appear to be primarily of US Army origin, and so far fall appear to be files of the US Army Medical Intelligence Organizations. Their dates of origin run from 1979 to 1986. The documents themselves run from finished formal assessments of the YR events offered to other organizations, to interdepartmental memos regarding assessments of the YR data at different points in time, to a wealth of "primary data" of YR events, including the telegraphic reports of witness/victim interviews, specimen identification information, laboratory summaries of results, and copies of GC-MS scans of individual specimens that were investigated. It appears that eventually the data will provide a rather complete data trail for most of the important specimens, though it

requires piecing together a lot of documents and requires decoding the "chain of custody" (such as it was) which typically uses a series of different specimen identifiers. One aspect of the documents is that they have been redacted to obliterate nearly every personal name, which makes attributing the policy statements or identifying specimens to specific victims/witnesses very difficult and reduces the reliability of such attributions.

As for new information in the Katz declassified material, I am at a little bit of a disadvantage because I have not previously investigated YR (Mark W did the YR portion of the allegations chapter in the HUP Deadly Cultures book) and so I am not sure what is new in the Katz material and what was previously available from other sources. So forgive me if I include "old history" in

the following summary.

ORIGINS OF APPRAISALS:

A review of the official "appraisals" and internal memoirs in the Katz documents reveals what is pretty clear from the open source material: there are two distinct eras represented, an early period 1979-c1983 when "enthusiasts" for CW and specifically mycotoxin use (mainly Sharon Watson at USMIIA, but including others, apparently at CIA) held sway, and a 1983-1986 period when "skeptics" (represented by the Joint State-Army CBW team) held sway. One element in these appraisals that will prove interesting is to evaluate drafts of appraisals versus the officially released versions. One discrepancy that I have found relates to the late 1979 field work of Lewis/Sidel/Tigertt. Lewis in his "trip report" of 23 Oct 1979 (Katz Doc [KD] 153071) states in his "Comments":

"3. Many of the effects described by refugees are not characteristic of any particular agent. On the other hand we know little of the effects of large, saturation doses of many agents on this population. I have seen one report (attached) indicating that the effects of agents thought to be rather innocuous [sic] (CS) might be more severe than we expect. It seems that there should be better and more complete reports on the effects of these agents on a similar population as a result of our recent experience in SEA. If so we should be aware of them and it is vital to the mission of BML that these effects be known so that prophylactic/therapeutic measure can be investigated."

As I recall, Lewis later testified in Congressional hearings with a great deal more certainty that CW attacks had occurred, and that he ascribed the symptoms to the effects of a nerve agent combined with other, unspecified agents. The potentially lethal effects of CS (clearly not a subject the USG was about to raise in an international CW allegation) was never raised.

Another interesting internal assessment was one from the Army Surgeon General's Office that with great prescience describes the difficulties of doing an appropriate scientific investigation of the SEA CW allegations, and makes concrete proposals for what that would take (setting up permanent team and GC-MS tox capability at the Army Med Research Unit in Bangkok). It also correctly predicts that this ambitious program would not be approved or funded, and that ultimately the investigation would be flawed and inconclusive. This program was of course not adopted, but the evaluation program adopted was always characterized as highly competent in the public appraisals.

TECHNICAL ANALYSIS OF MYCOTOXIN ASSAYS

There is a wealth of material regarding the specific mycotoxin assays throughout the history of the YR investigation. It will take a lot of careful reconstruction to make the formal case, but I have a pretty clear picture of what happened. Much of this will come as no surprise to you, but perhaps the high degree of detail and documentation will be a new contribution.

Broadly, the technical evaluation follows the "editorial" progression of enthusiasm to skepticism. Early positive results from Mirchoa's lab were accepted eagerly. These were scattered among a series of specimens that had originated overwhelmingly from "exposed" persons, with few unexposed "control" human samples. Later (in the 1982-83 timeframe) more unexposed samples were submitted, and a contracting lab (almost certainly Mirchoa's) reported some of them as positive. Clearly unwelcome results, as it implied either false positives, or a significant "background" mycotoxin exposure. At the same time

a large backlog of unassayed specimens had accumulated, and several additional labs were contracted/established to deal with the backlog.

In 1984 (as part of the realignment of the program that included establishing the State-army team in SEA) a comprehensive Quality Control Program was established with a battery of known, quantified positives and known negatives distributed blind to all participating mycotoxin laboratories (names of labs are redacted, so their identity must be appraised by indirect evidence). I

should mention that this procedure was standard procedure in medical clinical labs at the time: I participated in such programs regularly and such participation was required for accreditation. The results showed that all mycotoxin labs produced false negatives, and some produced false positives, and others mis-identified toxins. In the July 1985 assessment of this QC program, the statement appears: "An honest two-word assessment of the data that we have on hand [for mycotoxin assays] for FY 83 and 84, in my opinion, is "inconclusive junk." (see attachment to following email).

A laboratory that was particularly deficient, and produced many false positives (almost certainly the CRDCArmy lab) was reexamined by the best "reference" laboratory (almost certainly Mirchoa's). A wide variety of deficiencies was revealed, from inexperienced operators, to "a desire to find positive results" to strictly methodological deficiencies such as inaccuracies in determining retention time in GC and lack of rigor in demanding multiple mass fragment

matches in the MS detection. Considerable interference in the GC-MS was recognized from uncharacterized material present in blood and urine specimens. A feeling that this interference might occasionally produce false positives was recognized.

Recognizing the likelihood of sporadic false positive mycotoxin assays in biological specimens, new criteria were adopted. For a person to be deemed positive for mycotoxin exposure, positive results in two separately drawn blood specimens and a urine specimen was required. I need to review all the older material, but I feel it likely that very few if any of the previously described "positive" results from humans would meet this criteria, developed after many years of experience with the samples and labs involved.

KATZ, MYCOTOXINS AND REPRODUCIBILITY

I must say that Katz does give the mycotoxin assays her minimum level of veracity, though she defends them in her text, and states in some paragraphs that they indicate mycotoxins were used as CW weapons. One portion of Katz's dissertation that I find difficult to pass unchallenged is her statement on page 257 that:

"It is important to remember that while replicability of analytic findings is helpful, it is not normally required for scientific work. Peer-reviewed articles are published in the most prestigious journals without requiring results to be replicated in an outside laboratory."

Clearly, replicability of analytic findings is the GOLD STANDARD of science. Publications may be accepted without independent confirmation, but when the data proves unreproducible in subsequent evaluations or by other laboratories, it must be retracted or the inconsistencies resolved.

BEE STUDIES

Following your visit to Thailand, the Joint State-Army unit did some extensive work on bee feces, including several field trips documenting and quantifying bee feces "yellow rain" events. It turns out bees move their hives in response to seasonal and food availability, so fixed sampling sites show greatly variable deposition rates over time. In one instance, the team was unable to capture any bee feces on their cardboard targets, and returned to their vehicles only to find them covered with "yellow rain!" Of interest, the J State-Army team had examples of bee feces that they carried, and victims/witnesses to YR attacks routinely identified the bee feces as looking identical to the YR material of the alleged CW event.

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So far, those are the major points. Much material remains to be examined, and much cross-correlation needs to be made.

I hope this will be of some use,

Best regards,

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>

Date: Wed, 8 Feb 2006 07:36:12 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: Fourth in series

fourth in series

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 -----

Date: Mon, 6 Feb 2006 13:48:25 EST
 From: Martinfurmanski@aol.com
 To: msm@wjh.harvard.edu
 Subject: Re: Katz dissertation

Matthew:

I received the Katz documents from Rebecca Katz with the understanding that I would await the publication of her thesis or obtain her permission before publishing material based on her declassified documents. Since she has "self-published" her thesis on amazon.com, I feel the letter of our agreement has

been fulfilled, but I still do not want to disaffect her unnecessarily as she has been cooperative in sharing these documents. Conferring with you is of course not a "publication" and is perfectly appropriate, but please exercise some prudence in using my name or this material at Princeton. I would bet I am the only "outside source" regarding these declassified documents. But you of course may already have had some of these from other sources.

This must be a very preliminary assessment of the Katz YR documents. It consists of about 500 electronic files, each of may contain from 1 to 156 pages of declassified documents. The files are arranged in a series of random short runs of differing dates of origin. I have so far examined perhaps 20-25% of the files, and presumably a similar percentage of the documents. So while this is probably a representative sample, there is the possibility that new elements will emerge as I wade through this further.

The documents appear to be primarily of US Army origin, and so far fall appear to be files of the US Army Medical Intelligence Organizations. Their dates of origin run from 1979 to 1986. The documents themselves run from finished formal assessments of the YR events offered to other organizations, to interdepartmental memos regarding assessments of the YR data at different points in time, to a wealth of "primary data" of YR events, including the telegraphic reports of witness/victim interviews, specimen identification information, laboratory summaries of results, and copies of GC-MS scans of individual specimens that were investigated. It appears that eventually the data will provide a rather complete data trail for most of the important specimens, though it

requires piecing together a lot of documents and requires decoding the "chain of custody" (such as it was) which typically uses a series of different specimen identifiers. One aspect of the documents is that they have been redacted to obliterate nearly every personal name, which makes attributing the policy statements or identifying specimens to specific victims/witnesses very difficult and reduces the reliability of such attributions.

As for new information in the Katz declassified material, I am at a little bit of a disadvantage because I have not previously investigated YR (Mark W did the YR portion of the allegations chapter in the HUP Deadly Cultures book) and so I am not sure what is new in the Katz material and what was previously available from other sources. So forgive me if I include "old history" in

the following summary.

ORIGINS OF APPRAISALS:

A review of the official "appraisals" and internal memoirs in the Katz documents reveals what is pretty clear from the open source material: there are two distinct eras represented, an early period 1979-c1983 when "enthusiasts" for CW and specifically mycotoxin use (mainly Sharon Watson at USMIIA, but including others, apparently at CIA) held sway, and a 1983-1986 period when "skeptics" (represented by the Joint State-Army CBW team) held sway. One element in these appraisals that will prove interesting is to evaluate drafts of appraisals versus the officially released versions. One discrepancy that I have found relates to the late 1979 field work of Lewis/Sidel/Tigertt. Lewis in his "trip report" of 23 Oct 1979 (Katz Doc [KD] 153071) states in his "Comments":

"3. Many of the effects described by refugees are not characteristic of any particular agent. On the other hand we know little of the effects of large, saturation doses of many agents on this population. I have seen one report (attached) indicating that the effects of agents thought to be rather innocuous [sic] (CS) might be more severe than we expect. It seems that there should be better and more complete reports on the effects of these agents on a similar population as a result of our recent experience in SEA. If so we should be aware of them and it is vital to the mission of BML that these effects be known so that prophylactic/therapeutic measure can be investigated."

As I recall, Lewis later testified in Congressional hearings with a great deal more certainty that CW attacks had occurred, and that he ascribed the symptoms to the effects of a nerve agent combined with other, unspecified agents. The potentially lethal effects of CS (clearly not a subject the USG was about to raise in an international CW allegation) was never raised.

Another interesting internal assessment was one from the Army Surgeon General's Office that with great prescience describes the difficulties of doing an appropriate scientific investigation of the SEA CW allegations, and makes concrete proposals for what that would take (setting up permanent team and GC-MS tox capability at the Army Med Research Unit in Bangkok). It also correctly predicts that this ambitious program would not be approved or funded, and that ultimately the investigation would be flawed and inconclusive. This program was of course not adopted, but the evaluation program adopted was always characterized as highly competent in the public appraisals.

TECHNICAL ANALYSIS OF MYCOTOXIN ASSAYS

There is a wealth of material regarding the specific mycotoxin assays throughout the history of the YR investigation. It will take a lot of careful reconstruction to make the formal case, but I have a pretty clear picture of what happened. Much of this will come as no surprise to you, but perhaps the high degree of detail and documentation will be a new contribution.

Broadly, the technical evaluation follows the "editorial" progression of enthusiasm to skepticism. Early positive results from Mirchoa's lab were accepted eagerly. These were scattered among a series of specimens that had originated overwhelmingly from "exposed" persons, with few unexposed "control" human samples. Later (in the 1982-83 timeframe) more unexposed samples were submitted, and a contracting lab (almost certainly Mirchoa's) reported some of them as positive. Clearly unwelcome results, as it implied either false positives, or a significant "background" mycotoxin exposure. At the same time

a large backlog of unassayed specimens had accumulated, and several additional labs were contracted/established to deal with the backlog.

In 1984 (as part of the realignment of the program that included establishing the State-army team in SEA) a comprehensive Quality Control Program was established with a battery of known, quantified positives and known negatives distributed blind to all participating mycotoxin laboratories (names of labs are redacted, so their identity must be appraised by indirect evidence). I

should mention that this procedure was standard procedure in medical clinical labs at the time: I participated in such programs regularly and such participation was required for accreditation. The results showed that all mycotoxin labs produced false negatives, and some produced false positives, and others mis-identified toxins. In the July 1985 assessment of this QC program, the statement appears: "An honest two-word assessment of the data that we have on hand [for mycotoxin assays] for FY 83 and 84, in my opinion, is "inconclusive junk." (see attachment to following email).

A laboratory that was particularly deficient, and produced many false positives (almost certainly the CRDCArmy lab) was reexamined by the best "reference" laboratory (almost certainly Mirchoa's). A wide variety of deficiencies was revealed, from inexperienced operators, to "a desire to find positive results" to strictly methodological deficiencies such as inaccuracies in determining retention time in GC and lack of rigor in demanding multiple mass fragment

matches in the MS detection. Considerable interference in the GC-MS was recognized from uncharacterized material present in blood and urine specimens.

A feeling that this interference might occasionally produce false positives was recognized.

Recognizing the likelihood of sporadic false positive mycotoxin assays in biological specimens, new criteria were adopted. For a person to be deemed positive for mycotoxin exposure, positive results in two separately drawn blood specimens and a urine specimen was required. I need to review all the older material, but I feel it likely that very few if any of the previously described "positive" results from humans would meet this criteria, developed after many years of experience with the samples and labs involved.

KATZ, MYCOTOXINS AND REPRODUCIBILITY

I must say that Katz does give the mycotoxin assays her minimum level of veracity, though she defends them in her text, and states in some paragraphs that they indicate mycotoxins were used as CW weapons. One portion of Katz's dissertation that I find difficult to pass unchallenged is her statement on page 257 that:

"It is important to remember that while replicability of analytic findings is helpful, it is not normally required for scientific work. Peer-reviewed articles are published in the most prestigious journals without requiring results to be replicated in an outside laboratory."

Clearly, replicability of analytic findings is the GOLD STANDARD of science. Publications may be accepted without independent confirmation, but when the data proves unreproducible in subsequent evaluations or by other laboratories, it must be retracted or the inconsistencies resolved.

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From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: fifth in series

fifth in series

Matthew Meselson
Department of Molecular and Cellular Biology
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----- Forwarded message -----
Date: Mon, 6 Feb 2006 14:00:38 EST
From: Martin.furmanski@aol.com
To: msm@wjh.harvard.edu
Subject: Inconclusive Junk

Matthew:
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Note although the summary says "lab A" (which is undoubtedly Mirchoa's lab) had no false positives in the QC set, it does state that:

"In the controls [of field specimens] one individual was reported as positive in multiple specimens by laboratory A. A re-examination of the data had of the laboratory resulted in his changing his graduate student's assessment to negative because of the retention time did not exactly match. In an additional two controls both laboratory A and B reported a positive."

Hence the feeling that false positives were to be expected.

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[Part 2, "" Application/PDF 986KB.]
[Unable to print this part.]

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From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: sixth

sixth

Matthew Meselson
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----- Forwarded message -----
Date: Mon, 06 Feb 2006 21:39:04 -0500
From: Matthew Meselson <msm@wjh.harvard.edu>
To: Martinfurmanski@aol.com, msm@wjh.harvard.edu
Subject: Re: Inconclusive Junk

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Less important but still relevant, would a case be designated positive if one specimen is reported positive for T2 but not HT2 while another specimen from the same individual is reported positive for HT2 but not T2, if the specimens are either both blood or both urine?

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Please keep up your interest in YR. A fresh eye is needed. When you are ready, let's get together with others with diverse views who have ploughed these fields and spend a few days thinking together.

By the way, why do you have to go all the way to Washington State to read the ca 8,529 pages?

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Harvard University
7 Divinity Avenue
Cambridge MA 02138
(617) 495-2264 (tel)
(617) 496-2444 (fax)

Date: Wed, 8 Feb 2006 07:37:45 -0500 (EST)
 From: Matthew S. Meselson <msm@wjh.harvard.edu>
 To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
 Subject: seventh

[The following text is in the "X-UNKNOWN" character set.]
 [Your display is set for the "US-ASCII" character set.]
 [Some characters may be displayed incorrectly.]

seventh

Matthew Meselson
 Department of Molecular and Cellular Biology
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----- Forwarded message -----
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 From: Martinfurmanski@aol.com
 To: msm@wjh.harvard.edu
 Subject: Re: Inconclusive Junk

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I think the biggest problem with Katz's thesis is her acceptance of a "unitary" concept of the YR events. Her method of ranking the usefulness of data groups against simple mutually exclusive hypotheses implies this unitary concept.

I would contend that there is good reason to consider that the YR events were made up of a constellation of events which had variable degrees of interrelationship. It is highly similar to the Korean War BW allegations. Both had formal propaganda elements, bona fide medical occurrences, misinterpreted natural phenomena, and manipulation by opportunists. Both also had highly polarized camps of advocates and detractors.

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From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>
Subject: YR/Wilde

Dear Julian,

Confidential for your eyes only. Good old Wilde.

Matthew Meselson
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Date: Wed, 8 Feb 2006 04:29:27 -0800 (PST)
From: Prof Henry Wilde <docwilde@loxinfo.co.th>
To: Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: From Matthew Meselson (fwd)

Dear Mathew:

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"Matthew S. Meselson" <msm@wjh.harvard.edu> wrote:

Dear Henry,

This is a second transmission as I am not sure the earlier one went through in good order.

Matthew

Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
7 Divinity Avenue
Cambridge, MA 02138 USA
email:
telephone: (617) 495-2264
telefax: (617) 496-2444

----- Forwarded message -----
Date: Tue, 7 Feb 2006 23:42:30 -0500 (EST)
From: Matthew S. Meselson
To: henry wilde
Subject: Re: From Matthew Meselson

Dear Henry,

Very good to hear from you.

Attached in .pdf format is the Princeton doctoral dissertation of Rebecca Katz, also now available from Amazon.com. where you may want to contribute an online comment. In a few days, I'll write up some comments of my own and send them to you.

I was initially asked to be an outside member of her dissertation examining committee but was never actually called on to do so. Pretty obvious that Princeton failed its mentoring and oversight responsibilities.

Any chance of you being on either US coast within the foreseeable future?

All good wishes,

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On Wed, 8 Feb 2006, henry wilde wrote:

Date: Wed, 8 Feb 2006 10:12:08 +0700
From: henry wilde
To: Matthew S. Meselson
Subject: Re: From Matgthew Meselson

You can use either wildehenry@yahoo.com or docwilde@loxinfo.co.th. I am still here in Bangkok plugging along. Would be nice if we could meet some place some time. It is not easy to be an overseas American these days with a religious war going on inflamed by the worst government that the US has ever had. Best, Henry

----- Original Message -----

From: "Matthew S. Meselson"
To: "WILDE, DR. Henry" ;
Sent: Wednesday, February 08, 2006 9:33 AM
Subject: From Matgthew Meselson

Date: Wed, 8 Feb 2006 07:32:26 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Cc: Jeanne GUILLEMIN <GUILLEJE@bc.edu>
Subject: YR/Wilde

Dear Julian,

Confidential for your eyes only. Good old Wilde.

Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
7 Divinity Avenue
Cambridge, MA 02138 USA
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telephone: (617) 495-2264
telefax: (617) 496-2444

----- Forwarded message -----
Date: Wed, 8 Feb 2006 04:29:27 -0800 (PST)
From: Prof Henry Wilde <docwilde@loxinfo.co.th>
To: Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: From Matthew Meselson (fwd)

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To: "WILDE, DR. Henry" ;
Sent: Wednesday, February 08, 2006 9:33 AM
Subject: From Matgthew Meselson

Date: Thu, 09 Feb 2006 11:36:41 +0000
 From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
 To: Matthew S. Meselson <msm@wjh.harvard.edu>
 Subject: Re: YR chapter

[The following text is in the "iso-8859-1" character set.]
 [Your display is set for the "US-ASCII" character set.]
 [Some characters may be displayed incorrectly.]

Dear Matt,

No, I have mentioned neither the Lavoy attribution book-project nor its planned July meeting in London to Tom -- only that the YR issue seems to be coming alive again in America and that you are thinking of (as you put it to Furmanski) getting together with others with diverse views who have ploughed these fields to spend a few days thinking together. I find it increasingly odd that, despite prompts both from Jeanne and from the KCL CBW person (Susan Martin), Lavoy has not yet contacted me.

Still, if you are now going to go for 3) -- and I would not object if you did, though the London dates could prove impossible if there were to be a Symi Symposium this year -- I should tell Tom about the opportunity ASAP, perhaps at the same time warning him about the possibility of ambush.

Pasted in at the end of this message is a copy of my original e-correspondence with Susan Martin, in which you'll find her e-mail address.

As ever,
 Julian

At 18:28 08/02/2006 -0500, you wrote:
 Dear Julian,

Does this mean that Tom is willing to join you in writing for the Lavoy meeting and the ensuing book?

Which of the following should I say when I tell Lavoy that I am unable to take it on:

- 1) nothing more, or
- 2) that he should ask you, or
- 3) that I understand that you would be willing to do it and he should ask you do so, leaving it to you to invite Tom to join you?

I would favor option 3.

Instruct me soon, so that I may relieve Lavoy's anxiety.

Doesn't this mean that you and Tom will give us the pleasure of your company during the time that you will be looking through the YR archive I have at Harvard????

As ever,

M.

Matthew Meselson
 Department of Molecular and Cellular Biology

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Cambridge, MA 02138 USA
email: <msm@wjh.harvard.edu>
telephone: (617) 495-2264
telefax: (617) 496-2444

On Wed, 8 Feb 2006, Julian Robinson wrote:

Date: Wed, 08 Feb 2006 20:30:06 +0000
From: Julian Robinson <jppr@hspsxsq.u-net.com>
To: Prof M S Meselson <msm@wjh.harvard.edu>
Subject: YR

Dear Matt,

Tom is game for involvement. He says he has some deep scars from his old battles. What shall I send him to read, do you think?

As ever,
Julian

Envelope-to: jppr@hspsxsq.u-net.com
X-Sender: zday4257@pop.kcl.ac.uk
X-Mailer: QUALCOMM Windows Eudora Pro Version 4.0.1
Date: Wed, 16 Nov 2005 10:39:35 +0000
To: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
From: Susan Martin <susan.b.martin@kcl.ac.uk>
Subject: Re: Bio attribution
Cc: plavoy@aol.com
X-OriginalArrivalTime: 16 Nov 2005 10:41:59.0673 (UTC)
FILETIME=[5F6C6690:01C5EA9A]
X-flag: true
X-Bogosity: Ham, spamicity=0.000 (bogofilter 0.11.1.8, fisher)

Dear Julian,

Good to hear from you--I've been meaning to email but have been swamped at work.

Thanks for your comments on the cases, which I think are very helpful. I've attached the rest of the information that I've had from Peter, cutting and pasting it from a power point presentation that he gave. As you'll see, he has been thinking about the Korean case, though as far as I know not the other ones that you have mentioned.

My guess about the Taiwan foot and mouth disease is the outbreak that occurred in 1997. As far as I can tell from a quick internet search, there may have been some allegations that it was deliberate, but the outbreak now seems to be blamed on pigs smuggled from China. But I'm not sure that's what he has in mind.

As for the counterfactual analysis, my understanding is that the case studies have two objectives: 1. to determine the facts and set the historical record straight as much as possible, and 2. to examine whether different investigative methods, different technologies, etc could have allowed earlier detection of the incident; resolution of whether it was deliberate, natural, accidental or faked; and in the case of deliberate attacks, attribution to the attacker. It is in this second set of questions that the 'counterfactual analysis' comes in, trying to 'replay events' with different methods, etc to see if that would have mattered. The point of this, as I understand it, is to try to use past events to help us to determine how we can best approach the challenge

of identifying and attributing deliberate outbreaks today.

I will cc Peter on this so he can add to and correct all of this!

As I think I mentioned in my pervious email, we are looking for case study authors--people who may have been involved in investigating suspected cases in the past or those who have a good knowledge of the record and would be interested in delving into all the available documentation, etc. I don't know if you or any one else at Harvard Sussex might be interested, or if you could recommend other people. And of course, any further thoughts that you have on the project will be most appreciated!

Thanks much,
Susan

At 17:47 15/11/2005 +0000, you wrote:

Dear Susan,

It was good talking with you during the ESRC seminar at UCL. As I said, Daniel had shared with me the paper "lavoy 2 page attribution proposal.doc" that you copied to him with your e-mail of 7 November, and I promised to follow it up with you; as I now do.

"Counterfactual analysis" still baffles me, but my main comment is to ask whether the list of possible case studies set out in the paper would support a sufficiently rich inquiry. Certainly there is no shortage of historical instances in which use of BW weapons has been alleged or suspected. Many of these are either too far-fetched to be worth studying or else too loose to provide any worthwhile insight into the complicated problem of attribution. Not all of them, though; and it seems to me that certain of the allegations that have in the past been directed against the USA or its close allies could be worth including in the list. True, the sign, so to say, of the attribution problem would be reversed, but it seems to me that the most pertinent political and technical considerations would not be entirely dependent on who exactly was being suspected or accused. The anti-US/UN germ-warfare allegations of the Korean War are perhaps the best example. Maybe also the 1996 anti-US Cuban allegations of deliberate thrips infestation. Of the various other episodes, one thinks particularly of the USSR-Germany tularaemia accusation (on which HSP has an historical note in its latest CBW Conventions Bulletin), the reports of deliberate typhoid and cholera infections during 1947/48 in Palestine, even the Azerbaijani allegations of toxin warfare that the UN Secretary-General investigated in 1992. I would love to read some Yellow-Rain-type scrutiny of any of those episodes.

Do you know what Lavoy was referring to in his mention of "suspected use of Foot and Mouth disease by the Chinese in Taiwan"? I thought I followed the literature on this sort of thing very closely, but the reference here has completely escaped me.

Best,
Julian

W1 Robinson

Date: Fri, 10 Feb 2006 17:40:03 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Re: From Wheelis

FYI

Matthew Meselson
Department of Molecular and Cellular Biology
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telephone: (617) 495-2264
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----- Forwarded message -----
Date: Fri, 10 Feb 2006 14:17:03 -0800
From: Mark Wheelis <mlwheelis@ucdavis.edu>
To: Martinfurmanski@aol.com
Cc: msm@wjh.harvard.edu
Subject: Re: torian report

Hi Martin

I am sending today the stuff from Matt on yellow rain. It consists of the following:

1. the Torian report
2. Matt's letter to Deutch about the report. My notes of my telephone conversation with Matt indicate that Deutch was upset to hear of the results reported in the document, and assigned Gordon Eiler to look into the matter. However, Deutch left the CIA shortly thereafter and the investigation ws never completed.
3. A table Matt compiled of results of lab analysis of yellow rain samples. Note that since the Torian results are included in this table (Report 1987), the restrictions on citing, copying, and circulating would apply here too.
4. A copy of a 1982 USIA briefing on yellow rain.

If you have any questions about this material, check with Matt. I have copied him on this in case he has anything he wishes to add, or any additional concerns about use of this material.

best,

mark

>Mark:
> I would be grateful to see the report from MSM, and will accept
>the restrictions. I am leaving WA tomorrow morning and will be back
>in Ventura by Sunday, so send it to the Ventura address.
> I received the Canadian YR report, and find some interesting
>things in it. I will copy it when I get to Ventura and return the
>original to you.
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>gaining a nuts-and-bolts appreciation of how it ran and what grist
>ran through it.
> Also I am working on a rather exciting finding that I hope may
>be important, but I need to get back to Ventura and have access to
>my full archive to check it out.
> Thanks for thinking of me and my current YR activities.
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>In a message dated 2/9/06 12:04:52 PM, mlwheelis@ucdavis.edu writes:
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--

Mark Wheelis, PhD
Section of Microbiology
University of California
1 Shields Ave
Davis, CA 95616

530-752-0562
530-752-9014 (fax)

<http://microbiology.ucdavis.edu/faculty/mwheelis/>

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 To: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
 Subject: Re: YR-RK

dear julian,
 poor web connectivity here for the moment so i will be brief.

after a little while i will transmit to lavoy the splendid news that we will co-author the chapter. until then, i will forward all communications from him and others connected with the project.

i do not want to give out more copies of torian just now. better first to contact him to learn about its circulation, reception, etc.

let us not expand the authorsip beyond you, me, perhaps jeanne (the original trio) and i very much hope, tom.

am i to understand that tom is willing to join us? it would make a world of difference. do let me know.

if you see graham, please let him know that you and i are co-authoring a piece on yr and would value whatever help he can provide, including what i have already requested and whatever else he thinks would be relevant.

our telephone number here is 242 333 2091.

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Good to see that you've exposed Martin and Mark to the mysteries of Torian. What about Milt, too? It might well trigger something useful from him. I don't recall if you showed it to Elisa, with whom Milt is presumably in close contact.

Milt is trenchant on the subject of Tucker's pathetic YR study: "As for Tucker, what happened is that he spoke to Crocker, and swallowed Crocker's story on the still-classified communication intercepts lock-stock-and-barrel. I talked to him a bit about it after he published that paper. What that does, if the person (Tucker) buys the secret

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He also put me this question: "Yesterday evening, I went back and looked at what RK had to say in her evaluation of anthrax data and at the very bottom of page 108 in the "E1" evidence block it says: "There are pictures of roadblocks and activity around Compound 19 at the time of the alleged incident." I don't believe that I have ever seen such photographs in public? Have you? Do you know if Matt has copies of any such photos? Please let me know." I told him I knew of no such imagery, and neither was it mentioned in the long Gelb NYT Mag piece, which made several mentions of satellite stuff.

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>

>Masterful response to Patricia Moore Nicholas .

>

>M .

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>about authorship including James Wilkinson, Henry Wilde, Tom, You, Jeanne
>and me?

>

>Masterful response to Patricia Moore Nicholas .

>

>M .

>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

Date: Wed, 22 Feb 2006 08:04:45 -0500 (EST)
From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: peter lavoy <plavoy@aol.com>, "Stone, Liz USA" <elstone@nps.edu>
Cc: Matthew S. Meselson <msm@wjh.harvard.edu>,
Jeanne GUILLEMIN <GUILLEJE@bc.edu>,
Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: UPDATE: BW attribution conference

Dear Peter,

I have invited my colleague Professor Julian Robinson of the University of Sussex in the UK to join me in co-authoring a chapter on "Yellow Rain: lessons learned" for the conference and book you are organizing and he has accepted my invitation. Both of us will plan to attend the July authors meeting in London.

Please send Robinson an invitation to which he will respond by sending you the requested one page CV. His email address is:

Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>

Best wishes,

Matthew

Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
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Cambridge, MA 02138 USA
email: <msm@wjh.harvard.edu>
telephone: (617) 495-2264
telefax: (617) 496-2444

Date: Wed, 22 Feb 2006 05:13:11 -0800 (PST)
From: peter lavoy <plavoy@aol.com>
To: Matthew S. Meselson <msm@wjh.harvard.edu>
Cc: "Stone, Liz USA" <elstone@nps.edu>, Jeanne GUILLEMIN <GUILLEJE@bc.edu>
Subject: Re: UPDATE: BW attribution conference

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we've been trying to determine how to fit julian in, but frankly we ran out of case slots. this will be perfect.

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From: Matthew S. Meselson <msm@wjh.harvard.edu>
To: Julian Perry ROBINSON <J.P.P.Robinson@sussex.ac.uk>
Subject: Re: UPDATE: BW attribution conference (fwd)

The "kick the television set" approach seems to have worked. Now you will want to send your vita.

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Date: Wed, 22 Feb 2006 12:05:03 +0000
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To: Matthew S. Meselson <msm@wjh.harvard.edu>
Subject: Re: UPDATE: BW attribution conference (fwd)

Dear Matt,

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But there is no way I can send in the "one page biography", as I have no standing with the organizers, who have to my mind conspicuously excluded me from the project. I still don't have dates for the July Symi Symposium, which I have a nasty feeling are going to conflict with the KCL dates.

Tom seems to be away and I have had no word back from him yet in response to my e-mail forwarding him the project proposal and the State Department "case study". I shall try him again over the phone tomorrow.

Good idea to think of a longer later version of the chapter, especially if we can indeed get together the group you have in mind.

As ever,
Julian

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Date: Wed, 22 Mar 2006 12:48:12 +0000
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
To: Prof M S Meselson <msm@wjh.harvard.edu>, Thomas Inch <Incht@btinternet.com>
Subject: Fwd: Attribution conference

[The following text is in the "iso-8859-1" character set.]
[Your display is set for the "US-ASCII" character set.]
[Some characters may be displayed incorrectly.]

FYI

Date: Wed, 22 Mar 2006 12:43:07 +0000
To: rblack@dstl.gov.uk
From: Julian Robinson <j.p.p.robinson@sussex.ac.uk>
Subject: Attribution conference
Bcc: Julian Robinson

Dear Robin,

It was very good talking with you over the phone yesterday.
In the attachments to this message you will find the items I
promised to send:

[1] The Concept Paper by Peter Lavoy and Anne Clunan of the
US Naval Postgraduate School for the project on
Identification, Characterization, and Attribution of
Biological Weapons Use sponsored by the US Defense Threat
Reduction Agency that is to result in a multiple-authored
book whose contributors are to meet at an authors' conference
in London this coming July. You will notice that Chapter 12,
"UK Attribution Policies" has no assigned author, but
presumably the organizers have been looking for one. You
will also notice that there are to be two chapters on the
Yellow Rain. Matthew Meselson and I will be collaborating on
Chapter 6. The projected author of Chapter 7, Rebecca Katz,
is a young US State Department official to whom Princeton
recently awarded a PhD on the strength of a 337-page May 2005
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for the Investigation of Chemical and Biological Weapons
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Defense Intelligence Agency, where she was once an intern,
released 8529 pages of documents on YR to her for her
dissertation work.

[2] An October 2005 paper posted on its website by the US
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[3] The abstract that Meselson and I have prepared and
distributed to indicate what we plan to write in Chapter 6.

Meselson and I currently see our eventual Chapter 6 as the
point of departure for a more detailed study on YR to be
published in the academic international-security literature.
To this end we are convening a working party of people who
had been professionally involved in YR elucidation. Most are
now retired, though of course not all, and we expect this to
ease their participation. For the Americans at least, this
is indeed proving to be the case, and from the correspondence
thus far we already have the makings of something pretty
good.

After you have had a chance to study the attachments, maybe
you could tell me if there is anything more about the project
that you would like to know. I could send you an electronic
version of the Katz dissertation. Meselson will be over here
in May and again in July, keen to meet with possible
working-party members, of course including Tom. I am copying
them both in on an attachment-free version of this message.

Best,

Julian

=====
J. P. Perry Robinson
Professorial Fellow
SPRU -- Science and Technology Policy Research
University of Sussex, Freeman Centre
Brighton BN1 9QE
United Kingdom

Tel: **44 (0) 1273 678172 or 873627
Fax: **44 (0) 1273 685865
<http://www.sussex.ac.uk/spru/hsp/>

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<http://www.sussex.ac.uk/spru/hsp/>

Subject: Re: Your visit Jemski and Larson

From: "Reid Kirby" <rkirby@eximdyne.com>

Date: Sat, 24 Jun 2006 23:26:50 +0000

To: "Matthew S. Meselson" <msm@wjh.harvard.edu>, "Reid Kirby" <rkirby@eximdyne.com>

CC: "Julian Robinson" <j.p.p.robinson@sussex.ac.uk>

Matthew,

Many thanks on the independent action dose-response relationship lead. I found an excellent source on the subject that applies an exponential model to US Army figures, and now feel I have a good grasp on the subject.

Once I get the notes back from Bud and Joe, I will send HSP a copy of the notes and documents shared. Tom Collins' report sort of insulted Joe by stating repeatedly that his methods were not sound, but the author evidently had the benefit of regression analysis using MS Excel. I'm old enough to remember the days of using specialized graph paper or log tables and a ruler through a cloud of data points when one did not want to go through a ream of paper calculating least squares. I found an oral history from MORS that interviewed the first statistician to work for Fort Detrick. He didn't have an office and had to work from a desk next to the eight-ball for Ralph M Parsons company, and confirmed what I suspected on how they made their analysis. Also recall a munition expenditure calculation for the E61 that stopped at 500 bomblets per a cluster since their computer couldn't handle the anything more.

Funny to hear Bill Patrick "hooted" at your Sverdlovsk source strength estimate. If he was using "tables" for atmospheric diffusion analysis, then the debate ended before it started. I too have been skeptical that the release was only a few mg, but honestly had not made any attempts to validate your work until today. Atmospheric diffusion is an area that I have spent some time studying, and after reading your ASA Newsletter article I made my own estimate (attached) changing the assumptions slightly, and now regret having any doubts.

I read a book review of War of Nerves by a former chemical soldier and as expected there was a considerable discourse on the Skull Valley incident. My personal belief is that a contemporary investigation is neededoin this for the health of the Chemical Corps. Anyways, what made the book review entertaining was an illustration by this chemical soldier showing a two-step sheared trapazoid leaving DPG towards the sheep with the notion that it was way to far to have made an impact. What caught my attention was the reviewers ignorance on the adjustment of source strength when a spray line is no longer perpendicular to the wind as he was illustrating.

I have a few more interviews to make, and a need to answer why anthrax was in the stockpile in 1969 even though it was not standardized and not accepted. Once that is complete, I have a full featured story on St. Jo with many historical lessons.

Sincerely,

Reid Kirby
2208 Autumn Trace Parkway
Wentzville, Missouri 63385
United Stated

Telephone: 01-314-324-0997

<ex>-----Original Message-----

<ex>From: Matthew S. Meselson [mailto:msm@wjh.harvard.edu]

<ex>Sent: Friday, June 23, 2006 09:43 AM

<ex>To: 'Reid Kirby'

<ex>Cc: 'Julian Robinson'

<ex>Subject: Re: Your visit Jemski and Larson

<ex>

<ex>

<ex>Dear Reid,

<ex>

<ex>Very good that you met with Joe and Bud. I would have liked to have been a
<ex>fly on the wall.

<ex>

<ex>Could you send me a copy of the Collins' paper you mention below.

<ex>

<ex>Do you have a copy of my 2001 note on source strength calculations wrt the
<ex>Sverdlovsk outbreak published in the ASA Newsletter volume 48 and the
<ex>recent paper by Dean Wilkening PNAS 103:7589-7594?

<ex>

<ex>Although probit models are often used and are appropriate in chemical
<ex>toxicology, I believe that for a homogenous host population the
<ex>independent action model is more appropriate for infective agents like
<ex>anthrax. The basic reason for expecting this difference is that pathogens
<ex>replicate within the body and chemicals do not.

<ex>

<ex>Of course there is no good primate (human or non-human) data to test any
<ex>model for an infective agent at low response levels and, if the population
<ex>is distributed smoothly within the cloud area, most casualties are likely
<ex>to be from correspondingly low doses.

<ex>

<ex>Wilkening, however, does produce a qualitative argument against there
<ex>having been a threshold in the dose-response relation at Sverdlovsk. This
<ex>also would rule out any probit model with probit slope greater than
<ex>approximately 1, as that would produce an apparent cut-off, looking like a
<ex>threshold.

<ex>

<ex>Note that Wilkening obtains estimates for Sverdlovsk source strength
<ex>essentially identical to the estimates I gave in Science and, later with
<ex>more detail, in the ASA Newsletter. In "Germs" Bill Patrick is quoted as
<ex>having "hooted" at my Science estimates. At the time of our Science paper,
<ex>Tom Dashiell sent me a note including some tables that he said Bill
<ex>Patrick had used in making his estimates of the Sverdlovsk Source
<ex>strength. But I found that when the tables were used correctly, the
<ex>estimates were virtually the same as mine.

<ex>

<ex>None of this means that we know the actual source strength. Rather it
<ex>means that some people apparently thought, without correctly doing the
<ex>rather simple math, that the models they favored would predict large
<ex>sources even though when the math is correctly done using those same
<ex>models, the results are as I had published.

<ex>

<ex>For an early application of the independent action (exponential) model to
<ex>anthrax data see Druett (1952) Nature 170:288.

<ex>

<ex>I look forward to seeing the notes from your visit to Fredrick.

<ex>

<ex>Best wishes,

<ex>

<ex>Matthew

<ex>

<ex>

<ex>

<ex>

<ex>

<ex>Matthew Meselson

<ex>Department of Molecular and Cellular Biology

<ex>Harvard University

<ex>7 Divinity Avenue

<ex>Cambridge, MA 02138 USA

<ex>email: <msm@wjh.harvard.edu>

<ex>telephone: (617) 495-2264

<ex>telefax: (617) 496-2444

<ex>

<ex>On Fri, 16 Jun 2006, Reid Kirby wrote:

<ex>

<ex>Date: Fri, 16 Jun 2006 16:24:13 +0000

<ex>From: Reid Kirby <rkirby@eximdyne.com>

<ex>To: Julian Robinson <j.p.p.robinson@sussex.ac.uk>,

<ex> Prof M S Meselson <msm@wjh.harvard.edu>, rkirby@eximdyne.com

<ex>Subject: St. Jo

<ex>

<ex>Matthew,

<ex>I interviewed Bud Larson and Joe Jemski yesterday. We spent a
<ex>couple hours in Frederick, Maryland and discussed their activities with
<ex>St. Jo and anthrax. They are both in good health and mentally sharp. Joe
<ex>was unaware of the true aim of St. Jo, but provided a good account of the
<ex>effort to privatize Fort Detrick in 1952 (something that seems to have
<ex>escaped notice by contemporary scholars). We spent a good time discussing
<ex>posology. They gave me a copy of Tom Collins' report A Preliminary
<ex>Analysis of the Respiratory Infectivity of Bacillus Anthracis (Jaycor:
<ex>June 1996). Joe defended his research on anthrax in light of this report
<ex>and discussions he had with you several years ago. Neither seemed aware of

<ex>the exponential model used for dose-response in U of PA's Project SUMMIT
<ex>for ACDA and FM 3-10 Chemical Biological Weapons Employment (1962), and
<ex>stated the probit model was throughout the life of the offensive program.
<ex>I'm still trying to locate why the dose-response model changed on the
<ex>operational side since that has a significant impact on the feasibility of
<ex>biologicals with questionable median infective doses. They made several
<ex>recommendations of other people to interview in the aerobiology community
<ex>on the history of St. Jo and anthrax research. I will send a copy of my
<ex>interview notes to HSP after giving Bud and Joe a draft for comment.
<ex>Sincerely, Reid Kirby 2208 Autumn Trace Parkway Wentzville, MO 63385
<ex>United States Telephone: 01-314-324-0997
<ex>
<ex>
<ex>

Sverdlovsk.pdf	Content-Type: application/octet-stream Content-Encoding: base64
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Estimation of Source Strength

Sverdlovsk Incident

Reid Kirby
EXIMDYNE 2006

Diffusion Model

Using the following point source model and diffusion parameters defined by G. H. Milly¹ estimates were made for the source strength from the Sverdlovsk Incident:

$$D = \frac{Q \cdot \Lambda \cdot B}{\pi \cdot \bar{u} \cdot \sigma_y(x) \cdot \sigma_z(x)} \cdot \exp\left[\frac{y^2}{2\sigma_y^2(x)}\right] \cdot \frac{1}{2} \left\{ \exp\left[-\frac{(z-h)^2}{2\sigma_z^2(x)}\right] + \exp\left[-\frac{(z+h)^2}{2\sigma_z^2(x)}\right] \right\} \cdot \exp\left(-\frac{k \cdot x}{\bar{u}}\right)$$

Where:

D = index dose received (spores/person);

Q = source strength (spores);

Λ = overall source strength correction factor (unitless);

B = rate of breathing (m³/min); ✓

ū = mean wind speed (300 m/min); ✓

$\sigma_y(x) = 3.41 \left(\frac{x}{100}\right)^\alpha$ (unitless cross-wind diffusion parameter);

$\sigma_z(x) = 1.35 \left(\frac{x}{20}\right)^\beta$ (unitless altitude-wise diffusion parameter);

α = stability diffusion parameter (for Pasquill-Gifford D = 0.30);

β = stability diffusion parameter (for Pasquill-Gifford D = 0.88);

y = distance from center-line of windward travel (m);

z = sampling height (assumed 2 m);

h = release height (assumed 10 m); ✓

k = agent decay rate (%/min); and,

x = distance downwind from source (m).

Ceramic Factory

There was a 2% incidence of anthrax at this location about 2,800 m downwind from the suspect release point. Assuming an exponential dose-response

¹ G. H. Milly, Atmospheric Diffusion and Generalized Munition Expenditure ORG-17, US Army Chemical Corps Operations Research Group (1958). [AD223841]

relationship², and an ID₅₀ of 8,000 spores/person, then the index dose received inside this facility is 230 spores/man. Considering several other assumptions, the source strength estimates are as follows:

Q	B	k	Λ
7.1 x 10 ⁹	0.03	None	None
2.1 x 10 ¹⁰	0.01	None	None
1.4 x 10 ¹⁰	0.03	None	Inside blding - 0.5
4.2 x 10 ¹⁰	0.01	None	Inside Blding - 0.5
4.6 x 10 ¹⁰	0.01	1% physical only	Inside Blding - 0.5

Veterinary Cases

There were also veterinary cases up to 50,000 m downwind from the suspected release point. The species and fraction of infections is unknown, but attempting to attain a minimal dose at this distance may also indicate potential source strength. Assuming a rate of breathing of 0.01 m³/min, no physical decay rate, and in the open, then a source strength in the order of 5.5 x 10¹⁰ spores would render a dose of 2 spores/animal – probably a low figure, and thus the lowest acceptable figure. If an aerobiological decay rate of 1% is used for physical loss of aerosol during windward travel, then the figure jumps to the order of 10¹² spores.

Source Strength

The real problem with estimating source strength is translating the number of spores released into a mass or volume figure. To illustrate, I will assume reconciled source strength of 3 x 10¹⁰ spores (range between 10⁹ to 10¹² spores). In the US biological program, dry-type anthrax had a strength of 3.0 x 10¹¹ spores/g, putting this estimate at 100 mg (range 0.3 mg to 3.3 g). This represents what left the facility in a “puff.”

The actual source may have been inside of a building, and thus air change rates would indicate a higher figure. Furthermore, if the release resulted from an industrial accident, the actual figure would require knowledge of the manufacturing process. Nonetheless, given the many uncertainties, it appears what left the facility was in the order of tens to hundreds of milligrams of dry-type anthrax.

² George Shortley and Judd Wilkins, *Independent-Action and Birth-Death Models in Experimental Microbiology*, Bact Rev Vol. 29, No. 1 (March 1965): 102 – 141. The model used was –

$$P(\%) = 1 - \exp\left(-\frac{d_i}{d_0} \cdot \ln 2\right)$$

To: Sandy Ropper <ropper@fas.harvard.edu>
Subject: Re: New YR index
Cc: msm@wjh.harvard.edu (Prof M S Meselson)

Dear Sandy,

Many thanks for this new version of your YR Index. Attached for your possible interest is the current version of the file-listing we are using at the Sussex end to keep track of our YR material. Matt had a quick skim through it when he was here, and we have since mailed him copies of items he asked us for. Hope they've arrived.

You'll see from the attachment that we are currently reordering our holdings quite a bit, both to meet the present Lavoy-conference drafting needs and to identify areas in which the Harvard holdings are far greater than the Sussex ones. For the latter function, we are replicating many of your holding categories, as best we can understand them. So here is reason why you must come to visit us soon!

Best,
Julian

At 13:27 28/06/2006 -0400, you wrote:
New YR index dated 28 June 2006

Sandy Ropper, Archivist
Harvard-Sussex Program on CBW Armament and Arms Limitation
e-mail: <ropper@fas.harvard.edu>
tel: (617)496-3804
fax:(617)496-2444

=====
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SPRU -- Science and Technology Policy Research
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File pockets in YR drawers reordered1.doc

File pockets in YR drawers, reordered

[22 June 2006]

Note: these are K files, denoting private project files, not (yet) part of SHIB or otherwise accessible by people outside the project.

When the project is over, at least some of the material in the files will be dispersed into SHIB, where there is already a lot of YR-related material. This includes:

- *the published items that are held under two locators (both of which have subcategories):*
 - *A1.9.2 (Mycotoxins)*
 - *F5 (Yellow Rain)*
- *the unpublished or other non-public-domain items held within*
 - *H8.2 (drafts provided in confidence),*
 - *H7/F5 (other confidential items) and*
 - *H9 (unpublished proceedings of conferences held under the Chatham House Rule).*

YR Revisited (new project): JPPR project notes &c

- Project correspondence and current project file [separate tray]
- Sussex to-do list
- Lavoy project [separate tray]
- Drafts for contribution to Lavoy project

Yellow Rain (old project): JPPR project notes

- Plans &c [to do, notes, misc papers]
- Index cards registering mycotox/trichoth scientific pubs
- Manuscript notes on early CW reporting: separate sheets on Afghanistan, China/Vietnam, Kampuchea, Cambodia, Laos

YR: JPPR correspondence and memcons

- Letters to/from JPPR
- HMG and Parliament
- Letters copied to/by JPPR
- JPPR MemCons

YR: JPPR worksheets

- Tabulated data, documented, on all samples
- Sheet blanks, and reference decodes
- Enviro sample sheets
- Bio sample sheets
- Incident sheets

YR: JPPR occasional papers, commentaries, &c

- "Haig's Mycotoxins", for *Der Spiegel*, October 1981
- Other JPPR commentaries, &c.
- BBC envelope (dated Nov 2003) containing CD-ROM of Nov 83 *Panorama* on YR

YR: Matthew MESELSON [two pockets]

- Correspondence &c
- Press reporting of MSM activities
- Publications and typescripts submitted for publication (inc Detroit bees handout)
- File containing MSM diskette (1987) of *ForPol* offprint addressee

YR: Jeanne GUILLEMIN

YR chronology [*confchyr* &c]

YR: Amer Acad Conf, 15-16 Apr 83

YR: Nongovt'l inquiries and inquiry options

- Scientific groups
- Laos Inquiry (outstanding)
- WHO
- British working group on mycotoxin warfare
- Schwartzstein Project

YR: Catholic Office for Emergency Relief to Refugees

YR: Indochina Resource Center

- Jacqui CHAGNON
- Roger RUMPF

YR: International Rescue Committee

- Andrea CROSSLAND

YR: Other interested individuals:

- Jim BAMBERG
- Robin BLACK
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- Kit GREEN
- Richard HARRUFF
- Aubin HEYNDRICKX and his conference
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- Graham PEARSON
- Joe ROSEN
- Michael SAURI
- Sterling SEAGRAVE
- Fred SWARTZENDRUBER
- Sharon WATSON
- Henry WILDE
- Jim WILKINSON

YR: Occasional papers & other unpublished commentaries [not by HSP]

The next five items are currently held in the F5 (Yellow Rain) subcategories of SHIB:

- F5.1, Press and other reporting [3 ledburies]
- F5.2, US Government
 - F5.2.1, 'Public diplomacy' releases &c [[1.5 ledburies]]
 - F5.2.2, Congressional hearings [half a ledbury]

The following items, which are currently K files, will eventually enter SHIB.

- F5.2.3, Scientific publications from defence/contractor labs
- F5.2.4, Administration papers (internal)
 - F5.2.4.1, Freedom of Information Act releases [*currently being sorted and filed according to the applicable parts of the Harvard system*]
 - FoIA items received March-July 1987
 - FoIA items received later
- F5.3, Non-US governments
 - Australia
 - Belgium
 - Britain
 - Cambodia/Kampuchea [see also D6.10]
 - China
 - Canada [two pockets]
 - France
 - Germany (Fed Rep of)
 - Laos [see also D6.9]
 - Netherlands
 - New Zealand
 - Norway
 - South Africa
 - Sweden
 - Thailand
 - USSR
 - Viet Nam [see also D6.8]
- F5.4, Intergovernmental organizations
 - UN Group of Experts Investigating Reports
 - Projected BWC Special Conference
- F5.5, Contextual information
 - F5.5.1, Afghan aspects [see also D6.11]
 - F5.5.2, Nightmare Deaths
 - F5.5.3, Dusters and dusting [see also A3.1]
 - F5.5.4, Pollen and sample palynology
 - F5.5.6, Bees
 - F5.5.7, Diatoms
 - F5.5.8, Mycotoxins [see also A1.9.2]
- F5.6, Dark side

To: Sandy Ropper <ropper@fas.harvard.edu>
Subject: Re: New YR index
Cc: msm@wjh.harvard.edu (Prof M S Meselson)

Dear Sandy,

Many thanks for this new version of your YR Index. Attached for your possible interest is the current version of the file-listing we are using at the Sussex end to keep track of our YR material. Matt had a quick skim through it when he was here, and we have since mailed him copies of items he asked us for. Hope they've arrived.

You'll see from the attachment that we are currently reordering our holdings quite a bit, both to meet the present Lavoy-conference drafting needs and to identify areas in which the Harvard holdings are far greater than the Sussex ones. For the latter function, we are replicating many of your holding categories, as best we can understand them. So here is reason why you must come to visit us soon!

Best,
Julian

At 13:27 28/06/2006 -0400, you wrote:
New YR index dated 28 June 2006

Sandy Ropper, Archivist
Harvard-Sussex Program on CBW Armament and Arms Limitation
e-mail: <ropper@fas.harvard.edu>
tel: (617)496-3804
fax:(617)496-2444

=====
J. P. Perry Robinson
Sussex Director, Harvard Sussex Program
SPRU -- Science and Technology Policy Research
University of Sussex, Freeman Centre
Brighton BN1 9QE
United Kingdom

Tel: **44 (0) 1273 678172 or 873627
Fax: **44 (0) 1273 685865
<http://www.sussex.ac.uk/spru/hsp/>



File pockets in YR drawers reordered1.doc

File pockets in YR drawers, reordered

[22 June 2006]

Note: these are K files, denoting private project files, not (yet) part of SHIB or otherwise accessible by people outside the project.

When the project is over, at least some of the material in the files will be dispersed into SHIB, where there is already a lot of YR-related material. This includes:

- *the published items that are held under two locators (both of which have subcategories):*
 - *A1.9.2 (Mycotoxins)*
 - *F5 (Yellow Rain)*
- *the unpublished or other non-public-domain items held within*
 - *H8.2 (drafts provided in confidence),*
 - *H7/F5 (other confidential items) and*
 - *H9 (unpublished proceedings of conferences held under the Chatham House Rule).*

YR Revisited (new project): JPPR project notes &c

- Project correspondence and current project file [separate tray]
- Sussex to-do list
- Lavoy project [separate tray]
- Drafts for contribution to Lavoy project

Yellow Rain (old project): JPPR project notes

- Plans &c [to do, notes, misc papers]
- Index cards registering mycotox/trichoth scientific pubs
- Manuscript notes on early CW reporting: separate sheets on Afghanistan, China/Vietnam, Kampuchea, Cambodia, Laos

YR: JPPR correspondence and memcons

- Letters to/from JPPR
- HMG and Parliament
- Letters copied to/by JPPR
- JPPR MemCons

YR: JPPR worksheets

- Tabulated data, documented, on all samples
- Sheet blanks, and reference decodes
- Enviro sample sheets
- Bio sample sheets
- Incident sheets

YR: JPPR occasional papers, commentaries, &c

- "Haig's Mycotoxins", for *Der Spiegel*, October 1981
- Other JPPR commentaries, &c.
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- China

- Canada [two pockets]

- France

- Germany (Fed Rep of)

- Laos [see also D6.9]

- Netherlands

- New Zealand

- Norway

- South Africa

- Sweden

- Thailand

- USSR

- Viet Nam [see also D6.8]

F5.4, Intergovernmental organizations

- UN Group of Experts Investigating Reports

- Projected BWC Special Conference

F5.5, Contextual information

- F5.5.1, Afghan aspects [see also D6.11]

- F5.5.2, Nightmare Deaths

- F5.5.3, Dusters and dusting [see also A3.1]

- F5.5.4, Pollen and sample palynology

- F5.5.6, Bees

- F5.5.7, Diatoms

- F5.5.8, Mycotoxins [see also A1.9.2]

F5.6, Dark side

To: ropper@fas.harvard.edu (Sandra Ropper)
Subject: YR telegram index
Cc: msm@wjh.harvard.edu (Prof M S Meselson)

Dear Sandy,

While I was visiting you all last August, I captured an electronic file, created 17 Oct 2002, called *yellowraintelegrams1.doc*. It gives particulars of 353 telegrams and, in its right-hand column (it is a huge tabulation), lists the reftels cited in each one. When Matt was just over, I think he said that further work was going to be done on this Harvard index.

Of the 353 telegrams thus identified, I can find only 20 in the collection of YR FoIA papers we have at the Sussex end of SHIB. This doesn't necessarily mean that there are 333 such items unique to the holdings at the Harvard end (because the 353 include a few papers Sussex has not stored as telegrams, and I've only checked telegrams). But there are probably a good 300 or so.

Part of the Sussex collection of YR US FoIA papers has received some indexing effort. I started this because I needed a quick way of citing individual papers in the big Yellow Rain Chronology we have been maintaining since 1986: hence the 6-digit system you will have noticed in manuscript on some of the Harvard holdings, e.g. 'PH86/69'. But only 127 papers have yet been indexed in this fashion. How many we have altogether, I don't know, but I've just been collecting them all together and putting them into chronological order in order to weed out duplicates, to identify possible gaps, and to decide whether to sort them according to the system you are using at the Harvard end. They stack up into a heap that is about eight inches tall (a metric that does not, however, include those "Hart papers" that Matt took and which I am looking forward to receiving back). So our indexing has a long way to go still. I attach a file containing its latest manifestation, called "FoIA documents: citation decrypts" in the version of 3 July 2006.

One thing I have found you maybe should know about. Of the 43 telegrams listed in that 127-item Sussex index, 22 are not listed in the left-hand column of the Harvard *yellowraintelegrams1* index, which suggests either that they still await indexing or that you do not have them at the Harvard end. Of those 22, a total of 15 do, however, appear in the right-hand (reftel) column, some of them more than once.

My hope is to have imposed a bit more order on our YR holdings in time for Matt's and Jeanne's visit next week.

Best,
Julian

=====
J. P. Perry Robinson
Sussex Director, Harvard Sussex Program
SPRU -- Science and Technology Policy Research
University of Sussex, Freeman Centre
Brighton BN1 9QE

United Kingdom

Tel: **44 (0) 1273 678172 or 873627

Fax: **44 (0) 1273 685865

<http://www.sussex.ac.uk/spru/hsp/>



[US FoIA release citations1.doc](#)

YR revisited

FoIA documents: citation decrypts [3 July 2006]

Series PH86/nn

Releases during 1986 to Philip Hilts of the Washington Post

nn Document so designated

- 01 Letter, Lt-Col Herbert E Segal [Director, US Army Medical Component, Armed Forces Research Institute of Medical Sciences, Bangkok] to US Ambassador, Bangkok, 2 January 1980, subject: Trip Report: Chemical Agent Use, Hmong Refugee Populations.
- 02 Excerpted pages from what seems to be a long MIIA briefing on mycotoxins, undated, but probably late 1980 or early 1981.
- 03 Instructions, US Army Armament Research and Development Command [Foreign Intelligence and Threat Projection Division, Requirements and Analysis Office; and Chemical Systems Laboratory]. 6 January 1981, subject: Sampling Instructions for Water, Soil, Vegetation, Blood - animal, Tissue - animal.
- 04 [Not cited in chronology]
- 05 [Not cited in chronology]
- 06 [Not cited in chronology]
- 07 Memorandum for record, Marguerite E Brooks [DRDAR-CLB-R], 2 February 1981, subject: Intelligence Information on Clothing and Tissue Samples.
- 08 Report, DRDAR-CLB-R, 16 March 1981, subject: Analysis/Evaluation of Clothing and Tissue Samples.
- 09 Memorandum to file, Henry Wilde [Regional Medical Officer], 18 March 1981, subject: Report on Visits to Democratic Kampuchea Hospitals. Enclosure no 2 to Intelligence Information Report no 6 895 5051 81.
- 10 Messages, Director-USAMIIA, 061700Z and 231800Z Mar 81, subject: Destruction of biological samples.
- 11 Letter, Arthur E PFISTER [FI&TPD Representative to CSL] to Lt-Col MILLER [USAMIIA], 20 April 1981, subject: Vegetation sample.

- 12 Intelligence Information Report no 6 895 5062 81, Lt-Col Charles D Lane [USDAO AARMA Bangkok], 7 May 1981, title: Possible Chemical Warfare Sample.
- 13 Message, DTG 15...0Z May 81, Dir/USAMIIA to Dir/FSTC, subject: Chemical Warfare Samples.
- 14 DA form 3953, Purchase Request and Commitment, requisition number W23KB1 11524001, 1 June 1981, USAMIIA.
- 15 Interim Progress Report, DRDAR-CLB-R, 30 June 1981, subject: Evaluation of Samples Received from Congressman Leach.
- 16 Message, DTG 271800Z Jul 81, MIIA to CSL, subject: Further Testing of Tissue Samples.
- 17 Memorandum, ASA(RDA) [James R Ambrose] to ACSI and Dep ASA(RDA), 11 Aug 81, subject: ACSI memo, 27 Jul 81, subj: Chemical Warfare in SEA.
- 18 Telegram no 732, AmEmb Vientiane to State Dept, 17 Aug 81, subject: Hmong Villager in Laos Alleges Chemical Warfare Attacks to Mekong Committee Advisor.
- 19 Working Paper. A Wallace Hayes, n.d., title: Samples Analyzed for Dr Sharon
.....
- 20 Message, DTG 171800Z Aug 81, MIIA to DIA et al, subject: Analytical Results on Possible CW Samples.
- 21 Letter, A Wallace Hayes to Sharon Watson, 20 Aug 81.
- 22 DA Form 3161, Request for Issue or Turn-in, voucher no CLB-R-MEB-17, Marguerite Brooks [DRDAR-CLB-R] to Sharon Watson [MIIA], 26 Aug 81.
- 23 Form, Order for Supplies or Services, purchase order no DAMD17-81-M-9428, US Army Medical R&D Command to Dr A Wallace Hayes, 26 Aug 81.
- 24 Memorandum, DIA Director's Staff Group to Secretary of Defense, 31 Aug 81, subject: Chemical Warfare Attacks in Southeast Asia and Afghanistan.
- 25 Memorandum for Record, SGMI-SA (Sharon A Watson), 31 Aug 81, subject: Telephone Conversation with Fred Celek [sic], State Department.
- 26 DD Form 1155, Order for Supplies or Services, purchase order no DAMD17-81-M-A496, US Army Medical R & D Command to Dr A W Hayes, 25 September 1981.
- 27 Letter, A Wallace Hayes to Sharon Watson, 12 October 1981, with enclosures.
- 28 Letter, Cdr/ITAC by IAX-O-PD [Lt Col W W Edwards II, Chief, Projections Division, US Army Intelligence and Threat Analysis Center] to

- Cdr/USAMIIA for SGMI-PR [S Watson], 25 Nov 81, subject: Use of Chemical Agents Overseas.
- 29 Letter, A W Hayes to S Watson, 30 Nov 81, with enclosures.
- 30 Excerpted pages from what seems to be the draft for a long MIIA report on trichothecenes and the sample analyses through 1981
- 31 Informal undated typescript note, apparently to A Wallace Hayes, beginning "HELP!".
- 32 Letter, A W Hayes to S A Watson, 7 Jan 82, with enclosures.
- 33 Letter, A W Hayes to S A Watson, 12 Mar 82, with enclosures.
- 34 Letter, Richard Richter [Senior Producer, Documentaries, ABC News] to R Burt, 17 March 1982, with enclosed letter report from Arthur D Little Inc to ABC, 15 Dec 1981.
- 35 Telegram 08193, AmEmb-Canberra to State, 25 Aug 82, subject: Final GoA Analysis of 'Yellow Rain' Sample.
- 36 Intelligence Information Report no 6 895 5021 81, Lt-Col Charles D Lane [USDAO (AARMA) Bangkok], 5 February 1981, title: Possible Chemical Round.
- 37 Telegram 15629, AmEmb-Bangkok to State Dept, 20 March 1981, subject: Forwarding of Possible CW Samples.
- 38 Memorandum, 6 April 1981, subject: Identification of Samples Water and Foliage.
- 39 Memorandum, CSL Research Division, Analytical Branch, 31 March 1981, subject: Analysis/Evaluation of Water and Foliage Samples.
- 40 Message, DAMI-ISH to Cdr/USINSCOM Ft Meade, copied to Cdr/FSTC, 142200Z Apr 81, subject: Disbursement of GRANITE APPLE Funds.
- 41 SMUEA-FI Form 1, Acquisition of Foreign Materiel, 17 Apr 81.
- 42 DARCOM Form 1095h, Procurement/Work Directive, Cdr/FSTC to Cdr/ARRADCOM CSL/DRDAR-CPB-A, FSTC Project 10027, 27 Apr 81, subject: Foreign Materiel Exploitation.
- 43 Telegram 20562, AmEmb-Bangkok to State Dept, 27 April 1981, subject: Hmong Refugees Provide Alleged Samples of "Poison Gas" Residue.
- 44 Intelligence Information Report no 6 895 5058 81, Lt-Col Charles D LANE [USDAO AARMA Bangkok], 29 April 1981, title: Chemical Warfare Samples.

- 45 Letter, Arthur E Pfister [DRDAR-RAI-C] to Cdr/FSTC, 4 May 81, subject: Vegetation and Water Samples.
- 46 Memorandum for Action Officers, Joint Staff , 4 May 81, subject: Canadian Report of CW Use in Laos.
- 47 Message, DTG 071600Z May 81, US Army Operations Group, Ft Meade, to DIA, subject: Intelligence Information Report 2 201 1181 81 title: Possible Chemical Casualties.
- 48 Memorandum, DRDAR-CLB-R, 12 May 81, subject: Analysis/Evaluation of Yellow Powder Samples (with attached cost estimates).
- 49 Letter, Arthur E Pfister [DRDAR-RAI-C] to Cdr/FSTC, 18 May 81, subject: Possible CW Samples--Final Analytical Results on Samples Seven and Eight.
- 50 Letter, Arthur E Pfister to Cdr/FSTC, 20 May 81, subject: Time and Cost Estimate--Chemical Analysis of Powdery Substance.
- 51 Message, DTG 211302Z May 81, USDAO Bangkok to DIA, subject: Chemical Warfare Sample.
- 52 Message, DTG 211200Z May 81, USDAO Bangkok to DIA, subject: 6 695 0063 81 title: Chemical Warfare Sample.
- 53 DD form 1155, Order for Supplies or Services, purchase order no DAMD17-81-M-6651, 10 June 1981, US Army Medical R & D Command to Dr Wallace Hayes.
- 54 Letter, Arthur E Pfister to Cdr/FSTC, 3 June 1981, subject: Time and Cost Estimate--Chemical Analysis of Powdery Substance (C-168-81).
- 55 Memorandum, DRDAR-CLB-R, 26 May 1981, subject: Analysis/Evaluation of [samples #7 and #8] [ref inferred from item 56 below; the title page has been sanitized away].
- 56 Memorandum, DRDAR-CLB-R, 24 July 1981, subject: Analysis/Evaluation of Water Samples.
- 57 Letter, Rep Jim Leach to Defense Secretary C W Weinberger, 5 June 1981.
- 58 Memorandum, IAGPA-E-DA to ACSI for DAMI-ISH, 7 June 1981, subject: Visit to Congressman Leach.
- 59 Message, DTG 081402Z Jun 81, DRDAR-RAI-C [Pfister] to FSTC. subject: Possible CW Samples--Interim Analytical Results.

- 60 Memorandum for Chief, Foreign Intelligence and Threat Projection Division [of ARRADCOM?] from DRDAR-RAI-C [Pfister], 18 June 1981, subject: Suspect CW Sample Analysis.
- 61 Message, DTG 191031Z Jun 81, USDAO Bangkok to DIA, subject: Chemical Sample.
- 62 Letter, DRDAR-RAI-C [Pfister] to FSTC, 25 June 1981, subject: Time and Cost Estimate for Sample C-178-81.
- 63 Memorandum for Intelligence Community, DRDAR-RAI, 25 June 1981, subject: Administrative Data/Analytical Results on Suspected CW Samples
- 64 Memorandum for the Record, D(E&LS) [Dashiell], 9 July 1981, subject: Establishment of Procedure for CW Sample Exploitation.
- 65 Memorandum, DRDAR-CLB-R, 23 June 1981, subject: Analysis/Evaluation of Powder Sample C-200-81.
- 66 Message, DTG 071400Z Aug 80, OACSI to FSTC, subject: Exploitation of Samples from Alleged Gassing Victims.
- 67 Information Paper, DAMO-NCC, 20 August 1981, subject: Trichothecenes.
- 68 Memorandum thru Vice Chief of Staff, Army, for the Consultant to the Secretary of the Army from ACSI, 24 August 1981, subject: ACSI Memo, 27 Jul 81, Subj: Chemical Warfare on SEA--INFORMATION MEMORANDUM. [Note: a slightly expanded version of this memorandum also exists, dated 3 Sep 81; maybe the earlier version was a draft?]
- 69 Memorandum, R Burt, 9 Sep 81, subject: Interdepartmental Group Meeting on Chemical and Biological Weapons Arms Control.
- 70 Memorandum, [pres DRDAR-CLB-R], 10 September 1981, subject: Evaluation of Samples Received from Ms Karen McKay.
- 71 Unidentified paper entitled 'Talker for Use at CW/BW IG on 11 Sep 81'. See also the Sheila R Buckley declaration of 13 August 1986, pp 2-9.
- 72 Memorandum, DRDAR-RAI-C [A E Pfister] for Cdr/CSL, 14 September 1981, subject: Suspect CW Sample.
- 73 Memorandum, DRDAR-CLB-R, 23 September 1981, subject: Analysis/Evaluation of Powder Samples C-153-A-81 and C-153-B-81.
- 74 Memorandum, DRDAR-CLB-R, 23 September 1981, subject: Analysis/Evaluation of Powder Sample C-168-81.
- 75 Message, DTG 281805Z Sep 81, DAMI-FIT, subject: CW/BW Exploitation Working Group.

- 76 Message, DTG 011430Z Oct 81, Cdr/CSL to Cdr/ARRADCOM, subject: CSL Proposed Study of Mycotoxins.
- 77 Message, DTG 052230Z Oct 81, Cdr/ARRADCOM to Cdr/DARCOM, subject: CSL Proposed Study of Mycotoxins.
- 78 Telegram 47086, AmEmb-Bangkok to State etc, 11 Oct 81, subject: Transmittal of Blood Samples Drawn from Khmer Rouge Victims of 'Poison Gas' Attack.
- 79 Memoranda, Ed McWilliams to Gene Henderson, 10 Oct [81], attached to blood samples.
- 80 Memorandum for record, DAMI-FIT, 9 Oct 81, subject: CW/BW Exploitation Working Group.
- 81 XST HQ Form 4, Acquisition of Foreign Materiel, report no DRXST-14-82, 16 Oct 81.
- 82 Telegram 47836, AmEmb-Bangkok to State Dept, 16 Oct 81, subject: Thai Share Test Results on 'Yellow Powder' from Laos.
- 83 Telegram 48070, AmEmb-Bangkok to State Dept, 22 Oct 81, subject: Transmittal of Blood Samples Drawn from Khmer Rouge Victims of 'Poison Gas' Attack.
- 84 Letter, DRDAR-RAI-C (Pfister) to FSTC DRXST-FMO (Van Zandt), 26 Oct 81, subject: Summary of Analytical Results--Suspect CW Samples.
- 85 Telegram 49054, AmEmb-Bangkok to State, 26 Oct 81, subject: Notes on October 21 Blood Samples Taken from DK 'Gas' Victims.
- 86 Telegram 52374, AmEmb-Bangkok to State, 10 Nov 81, subject: UN Panel Investigating 'Poison Gas' Charges Concludes Visit to Thailand.
- 87 Letter, SGMI-cr to DAMI-FIT, 30 Nov 81, subject: Recommended Changes to Existing Collection Guidance in Support of CW/BW Sample Exploitation.
- 88 Matrix entitled 'CB/BW Sample Exploitation', dated 1 Feb 1982 [evidently originating in MIIA].
- 89 Telegram 55373, USDAO-Bangkok to DIA, 27 Nov 81, subject: Chemical Warfare Sample.
-

Series JG86/nn

<i>State Department releases to Jeanne Guillemin, 8 December 1986</i>	
01	Letter, Ass Sec State W Tapley Bennett Jr to Rep Fish, 8 Mar 84
02	Letter, S Yang, 6 Apr 84 [anti-Meselson abuse]

Series JG87/nn

<i>Releases to Jeanne Guillemin by State Dept on 25 Feb, DIA on 16 Mar, and ACDA on 23 Mar 1987</i>	
01	AmEmb Bangkok telegram 01025, 9 Jan 84
02	AmEmb Bangkok telegram 08848, 17 Feb 84
03	AmEmb Bangkok telegram 11000, 1 Mar 84
04	AmEmb Bangkok telegram 11615, 6 Mar 84
05	AmEmb Bangkok telegram 12037, 7 Mar 84
06	AmEmb Bangkok telegram 13720, 14 Mar 84
07	AmEmb Bangkok telegram 15421, 22 Mar 84
08	AmEmb Bangkok telegram 18465, 19 Apr 84
09	AmEmb Bangkok telegram 20179, 19 Apr 84
10	AmEmb Bangkok telegram 20177, 19 Apr 84
11	AmEmb Bangkok telegram 21018, 24 Apr 84
12	AmEmb Bangkok telegram 21367, 25 Apr 84
13	AmEmb Bangkok telegram 24354, 10 May 84
14	Incoming State: USDAO IR 6 895 0384 83, ca 7 Dec 83
15	Incoming State: USDAO IR 6 895 0383 83, ca 22 Dec 83
16	USACDA memorandum, subj: Meselson point paper, 2 Jul 84

Series MM00/nn

<i>State Department releases to Matthew Meselson, 12 October 2000</i>	
01	AmEmb Bangkok telegram 60572, 22 Dec 80
02	AmEmb Bangkok telegram 41811, 4 Aug 83
03	State telegram 222801, 8 Aug 83
04	State telegram 139713, 12 May 84
05	AmEmb Bangkok telegram 60278, 3 Dec 84
06	AmEmb Bangkok telegram 11549, 28 Feb 85
07	State telegram 274958, 6 Sep 85

Series MM04/nn

<i>State Department releases to Matthew Meselson, 12 May 2004, and DIA release of 16 March 2004</i>	
01	AmEmb Bangkok telegram 14265, 4 Mar 82
03	AmEmb Bangkok telegram 20943, 13 Apr 82
04	AmEmb Bangkok telegram 12037, 7 Mar 84
05	AmEmb Bangkok telegram 22780, 2 May 84
06	AmEmb Bangkok telegram 00018, 2 Jan 85
07	AmEmb Bangkok telegram 56220, 18 Sep 85
08	AmEmb Bangkok telegram 67036, 15 Nov 85
09	AmEmb Bangkok telegram 71462, 9 Dec 85
10	State telegram 391934, 27 Dec 85
11	AmEmb Bangkok telegram 04158, 25 Jan 86
12	AmEmb Bangkok telegram 10899, 28 Feb 86
13	AmEmb Bangkok telegram 10275, 28 Feb 84

Subject: Packages (fwd)
From: "Matthew S. Meselson" <msm@wjh.harvard.edu>
Date: Mon, 31 Jul 2006 14:07:01 -0400 (EDT)
To: "Janet F. Montgomery" <jmontgom@fas.harvard.edu>

Watch for the package.

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 -----
Date: Mon, 31 Jul 2006 17:55:08 +0000
From: Reid Kirby <rkirby@eximdyne.com>
To: j.p.p.robinson@sussex.ac.uk, msm@wjh.harvard.edu
Subject: Packages

Julian/Matthew,

Shipped two FedEx parcels to each of you (figured I'd save a Sussex-to-Harvard trip by going direct).

Tracking numbers are (<http://www.fedex.com/>):

1ZRF57766606505885 (Sussex)

1ZRF57760139840813 (Harvard)

This missive includes -

Photograph of last chemical munition (M23) demilitarized at Johnston Island. Meeting notes with Bud Larson and Joe Jemski (Matthew - caution, Bud made it a point to talk about you... "Let me tell you about your boy Meselson." - still respectful, but you must have made a reputation in the BW community). Anthrax Posology Report. CD with the Spenser Report (Julian - only sent this to you as it is more for someone with a peculiar interest in primary data on WWI chemical strikes against the AEF for OR analysis). Thank you both for your recent assistance. I have two to three more interviews to make (have been out of pocket this July due to a change in jobs) for St. Jo. My article to the Army Chemical Review on CB UXOs was accepted with minor revisions (first time no one tried to change what was said) and will be published this August/September. So now I'm looking for a new article topic to work on after St. Jo - probably Low Observable Attribution properties of biological warfare.

Sincerely,

Reid Kirby

2208 Autumn Trace Parkway

Wentzville, Missouri 63385

United States

Telephone: 01-314-324-0997



Yellow Rain

Matthew Meselson <msm@wjh.harvard.edu>

Mon, Sep 25, 2006 at 1:35 PM

To: mpribbeno@juno.com, Matthew Meselson <msm@wjh.harvard.edu>, Julian Robinson <jppr@hspxsq.u-net.com>

Dear Merle Pribbenow,

I have just run across and read your article on "Yellow Rain" in the International Journal of Intelligence and Counterintelligence and though you might like to see the attached draft of an article that my UK colleague Julian Robinson and I writing for a book being organized by the US Navy Postgraduate School. There will also be a chapter on the yellow rain by Rebecca Katz, whose Princeton thesis you mention in your article. We would be most grateful to have any comment or correction you care to make. We have tried to steer clear of the politics and instead to focus on the evidence or lack of it.

A question on which you may be able to shed some light even if outside the scope of our chapter concerns China's possible involvement in the YR matter. Inasmuch as China was concerned about Soviet influence in the area, opposed to Vietnam and was supporting the Khmer Rouge, it seems reasonable to ask if there is any evidence of Chinese fostering the yellow rain stories among the Hmong and/or the Khmer Rouge. What do you think?

Please do not circulate our draft chapter but let me know if you would like to have a copy of the final version.
Sincerely,

Matthew Meselson



MMrev0923ToJulian.doc

106K



Yellow Rain

Matthew Meselson <msm@wjh.harvard.edu>

Mon, Sep 25, 2006 at 1:35 PM

To: mpribbeno@juno.com, Matthew Meselson <msm@wjh.harvard.edu>, Julian Robinson <jppr@hspxsq.u-net.com>

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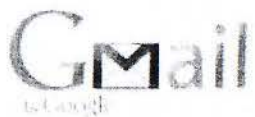
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MMrev0923ToJulian.doc

106K



Matthew Meselson <msmeselson@gmail.com>

Re: Yellow Rain

1 message

Merle L Pribbenow <mpribbeno@juno.com>

Mon, Sep 25, 2006 at 8:21 PM

To: msm@wjh.harvard.edu

Cc: msm@wjh.harvard.edu, jppr@hspsxsq.u-net.com

Professor Meselson,

Thank you for your e-mail. I am gratified that someone has noticed my article.

I have now read your draft article on the Yellow Rain issue. I think it is excellent and very well balanced. I am afraid that there is not a lot I can add in terms of comments or corrections. As you know I am not a scientist, so I cannot address the scientific issues you raise. And as you may or may not be aware because of my previous employment I must obtain prior approval from the CIA's Publications Review Board (PRB) for anything I write on intelligence-related matters or anything related to my previous employment, so I am somewhat limited in saying anything that goes beyond what was contained in my article (the article was approved for publication by the PRB). That said, I can say, as I stated in the article, that professional Agency working-level intelligence collectors on the ground in SE Asia were virtually unanimous in their skepticism about the Yellow Rain reports. After years of work trying to verify these reports, as far as we could tell these reports all seemed to be nothing more than "smoke" - there just wasn't anything solid there. For additional proof, you can check the following web-site for an article on Jerry Daniels, a legendary officer who worked with the Hmong for more than a decade in Laos during the war and then for another seven years (1975-1982) working with the Hmong refugees in Thailand. The article states quite correctly, near the end of the article, that Jerry, who probably knew the Hmong better than anyone else, simply did not believe the Yellow Rain stories that were being told:

http://www.smokejumpers.com/smokejumper_magazine/item.php?articles_id=299&magazine_editions_id=19

By the way, I noticed that in your article you did not have any references to the CIA's Special National Intelligence Estimates (SNIE) on Yellow Rain that were issued in 1982 and 1983. These SNIEs (actually one SNIE with an addendum and an update) have now been declassified and are available for viewing and downloading at the CIA's FOIA website - www.foia.cia.gov - all you need to do is go to the website and type in a search for "yellow rain," then scroll down to the relevant documents. The SNIEs are all titled "*Use of Toxins and Other Lethal Chemicals in Southeast Asia and Afghanistan*" and are dated 2 February 1982 (Volume I), 26 February 1982 (Volume II), and 2 March 1983 (Update) - all are designated SNIE 11/50/37-82. They are fairly long (Volume II is 92 pages).

Merle Pribbenow

From:
Graham Pearson , Graham_Pearson
To : Serguei Batsanov, I NTERNET :batsanov@hotmail.com
To: Julian Robinson, INTERNET : j.p p . robinson@sussex .ac .uk
To : Mat t Meselson, INTERNET:msm@wjh .harvard.edu
cc : Graham, Graham_Pearson
Date: Mon, Mar 5, 2007 9:27: 17 am
RE: Pugwash Workshop Geneva December 2007

Dear All

As promised in my recent e-mail t o all of you about the Pugwash workshop in Geneva *in* December 2007, I have now sent Jean-Pierre Stroot an e - mail on behalf of the Steering Committee asking his to start the ball rolling t o raise the necessary funds from the Swiss authorities, book the Mon Repos and the conference venue.

I also created a first draft of a possible Agenda for the Workshop this is just indicative and I have made it clear to Jean-Pierre that it is not an agreed Agenda. I have suggested the following title for the Workshop "Moving Forward After the Sixth BWC Review Conference " . I have simply indicated what I think we are likely to cover at the Workshop -- clearly the two topics f o r 2007 and the two topics for 2008 as well as CBMs and the universalization initiative.

In the indicative agenda I put the topics for 2007 and 2008 in full, but I think for the eventual Agenda for the meeting we would shorten these to something like:

1. Topics for 2007

- (i) national implementation
- (ii) international cooperation on BWC implementation .

2. Topics for 2008

- (iii) biosafety and biosecurity
- (i v) Oversight, education, awareness raising , and adoption and/or development of codes of conduct I enclose the initial draft of the Agenda and would suggest that the Steering Committee might consider this, the above proposals and move towards an agreed Agenda .

Best Wishes

Graham

- - - - - End Forwarded Message - - - - -

Page 1: Formatted Matthew Meselson

English (U.S.)

Page 1: Comment [q1]

Page:7

qsff6

4/ 4/2007 6:40:00 PM

4/4/2007 6:48:00 PM

This "personal communication" was a private and confidential one. It would be difficult now to ask the person concerned whether, after the long lapse of time, s/he minded being identified as the source of this unpublished information.

Page 1: Comment [GP2] Graham Pearson 4/1/ 2007 11:35:00 AM

You could add the point that "Indeed, CDE Porton Down found that the analysis of trichothecenes required the use of new glassware for each analysis."

Page 1: Inserted Graham Pearson 4/1/2007 11:31:00 AM

Chemical Defence Establishment

Page 1: Inserted Graham Pearson 4/1/2007 11:31:00 AM

Down

Page 1: Comment [GP3] Graham Pearson 4/1/2007 11:32:00 AM

The point is that there are and were two separate establishments at Porton Down and the amendments

makes it explicit which establishment you are referring to

Page 1: Comment [GP4] Graham Pearson 4/1/2007 11:41:00 AM

You could also make the point that "Similar studies carried out at CDE Porton confirmed the rapid metabolism of trichothecenes." The Chemistry in Britain article by Black and Upshall could be referenced.

Page 1: Comment [GPS] Graham Pearson 4/1/2007 11:44:00 AM

Incidentally, I don't see in your paper any mention of the low inherent toxicity of T-2 trichothecene -- of I

to 10 mg/kg body weight which means that to be effective an average 70kg man has to ingest around 70 to

700 mg - and this means that the T-2 should be very easy indeed to find if this were to be the agent used!

The Black/Upshall article in Chemistry in Britain July 1988 pp 659-664 is highly relevant.

Page 1: Comment [GP6] Graham Pearson 4/1/2007 11:56:00 AM

There is another lesson that is well worth drawing. The T-2 trichothecene saga led the US to include T-2

trichothecenes in its list of CBW threat agents against which its protective measures should be effective -

and the wasted resources used to protect against t-2 trichothecene which presented no threat were probably

significant. I have the feeling that the US caused T-2 to be added to the NATO threat list - but this would

need to be checked. The point that needs to be made is that the incorrect identification of T-2 trichothecene

as an

agent has the potential to waste enormous resources.

Page 1: Inserted

TC item 454 rev CSP

Page 1: Deleted

3 meselson_robinson_chapter_MM25 March

Matthew Meselson

Matthew Meselson

4/ 4 /2007 6:48:00 PM

4/4/2007 6:48:00 PM

From: Matthew Meselson [mailto:msm@wjh.harvard.edu]
Sent: Tuesday, March 20, 2007 3:39 PM
To: Carus, Seth; jppr >> Julian Robinson
Subject: Re: FW: Meselson & Pribbenow
Carus, Seth wrote:

Julian,

If you recall, when we met I mentioned that I had recently talked with someone who was involved in the field investigation of the yellow rain story. I passed along the Pribbenow story to him, and this was his response. As you can tell from his comments, Steve still is passionate and has strong opinions about the way the whole yellow rain matter was handled.

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Subject: RE: FW: Meselson & Pribbenow

From: "Carus, Seth" <Caruss@ndu.edu>

Date: Tue, 20 Mar 2007 15 :44:44 -0400

To: "Matthew Meselson" <msm@wjh.harvard.edu>, "jppr >> Julian Robinson"
j.p.p.robinson@sussex.ac.uk

Matt,

Please feel free to contact Cunnion directly -- as you can tell, I think he very much wants to tell his story. What does not completely come out in his account is the extent to which he remains bitter at the way the powers that be handled the whole matter.

I have not seen the paper, but

Seth

Subject: RE: FW: Meselson & Pribbenow

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I have not seen the paper, but

Seth

Subject: Re: Pugwash Workshop Geneva December 2007

From: Graham Pearson <Graham_Pearson@compuserve.com>

Date: Sun, 1 Apr 2007 08:23:48 -0400

To: Julian Robinson <jppr@hspsxsq.u-net.com>, Matthew Meselson <msm@wjh.harvard.edu> -

CC: Graham <Graham_Pearson@compuserve.com>

Ouch! Annotated version omitted from previous message. Here it is!

- ----- Begin Original Message-----Message
text written by Graham Pearson

"Dear Matt

Thank you very much for your thoughtful and useful proposals for Geneva. I will incorporate them into a revised agenda once I have heard from Julian .

My aim will indeed be to have invitations going out far in advance - - so that we can indeed decide whether to commission two or three though- pieces. I shall seek information from Jean-Pierre Stroot in about three weeks time as to how the fundi ng quest is progressing.

Thank you for reminding me to look at your excellent paper on yellow rain . I have added a few comments in track change s and added comments :

A. to make it clear that it was COE Porton Down
B. to point out that I don "t think you pick up the low toxicity of T- 2 trichothecene -- which makes it a virtually incredible agent, and
C . to point out that the US added T- 2 to their CBW threat agent list and hence devoted -- and wasted -- resources into counters to this -- detectors etc -- and I also think they leant on NATO to add T- 2 to the NATO CBW threat list (although you would need to check this) hence wasting resources there too. The lesson to be learnt is that false agent identification wastes scarce resources.
I hope these are helpful.

Best Wishes
Graham

Begin Original Message
Message text written by Matthew Meselson

Dear Graham,

Very good of you to prod us into thinking about Geneva soon enough that perhaps this time will can have proper invitations sent out a few months

before the event.

Your agenda items I and II, the introduction and the steps beyond Revcon VI are certainly essential. In addition, it seems to me that with the Revcon and much else now behind us we should include a substantial further agendum aimed at the encouragement of broad thinking. The intention could perhaps be conveyed with a Roman numeral agendum III "The problem of biological weapons: Where are we now?" In addition to the need for new ideas and analysis, the title might help draw the participants we want as we are all looking for new ideas .

A further possibility would be to commission two or three thought- pieces

4/4/2007 6:36 PM

Re: Pug wash Workshop Geneva December 2007

2 of 3

to be circulated in advance.

In any case, let us ourselves set the pace toward Geneva, with Sergei's assistance when he is able to provide it but without being dependent on it .

On another matter, I wonder if you have any suggestions for improving the draft Yellow Rain paper I emailed to you last week.

As ever,

Matt

Graham Pearson wrote :

Dear Julian

Now that Noordwijk is behind us, it would be good if you and Matt could look at the draft Agenda for the Pugwash workshop in Geneva in December 2007 .

I haven't copied this e-mail to Sergei, but would be glad if any replies and comments on the draft Agenda were copied to all four members of the Steering Committee as our aim is not to exclude his inputs but not to be dependent on his inputs!

Best Wishes

Graham



questions about our MS

Matthew Meselson <msm@wjh.harvard.edu>

Wed, Apr 4, 2007 at 9:49 PM

To: Merle L Pribbenow <mpribbeno@juno.com>, "Matthew S. Meselson" <msm@wjh.harvard.edu>

Dear Merle,

May I ask that you look at the paragraph near the end of the enclosed updated version of our MS that describes your work in Southeast Asia during 1981-3 and let me know if what I have written is accurate. If you spot any errors there or anywhere else in the text, we would be most grateful to know of them.

I have a question about well-poisoning: Was it done so far as you know to any considerable extent in the Hmong areas around Phu Bia mountain?

I was in DC yesterday and had lunch (very spicy Thai) with Dr. Steven Cunnion, a physician member of the State Defence CBW team who investigated CBW claims in SEA during 1984-86. His conclusions, like those of others who served on that team, are exactly in line with your own.

Kind regards,

Matthew

—

Matthew Meselson
Department of Molecular and Cellular Biology
Harvard University
Cambridge MA 02138 USA
Tel: (617) 495-2264
Fax: (617) 496-2444



TC item 454 rev 070405MM.doc

124K



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TC item 454 rev 070405MM.doc

124K



Cunnion

Matthew Meselson <msmeselson@gmail.com>

Waugh

Tue, Apr 10, 2007 at 5:05 PM

Fwd: Mark-up to 'final' draft

Matthew Meselson <msm@wjh.harvard.edu>
To: Julian Robinson <j.p.p.robinson@sussex.ac.uk>

From Steven Waugh, a member of the CBW Team in its middle period. I have hastily taken account of some of his comments in the version I have just sent to Clunan, Lawson and Lavoy.

According to my notes from lunch with Steve Cunnion, the successive teams were

- 1) Michael Sauri, David Lambert Don Stader
- 2) Steven Cunnion, Steven Waugh, Stader
- 23 Cunnion, Ron La Grone, James Brunco

----- Forwarded message -----
 From: Stephen Cunnion <cunnion@comcast.net>
 To: Matthew Meselson <msm@wjh.harvard.edu>
 Cc:
 Date: Tue, 10 Apr 2007 12:28:09 -0400
 Subject: Fwd: Mark-up to 'final' draft
 Matthew,

Steve is the only one of us who has ever seen the still classified documents to which he refers.

Steve

Begin forwarded message:

From: meadows@compuvision.net
 Date: April 10, 2007 10:54:09 AM EDT
 To: cunnion@comcast.net
 Subject: Mark-up to 'final' draft


Hi Doc C,

I have read and thought through the document which you sent me. I have made comments where I could. I can agree with most of the thought processes involved.

However, the authors are traveling on a road that is based only on information which has been "released" from government classification. In short, there are other parts of the story which might contribute to the discussion, yet cannot.

Attached is my mark-up of the paper.

Steve W.

 preFinal070409 SW.doc
120K



Matthew Meselson <msmeselson@gmail.com>

Interviews

5 messages

Matthew Meselson <msm@wjh.harvard.edu>

Sat, Jul 7, 2007 at 2:18 PM

To: Philts@aol.com, "Matthew S. Meselson" <msm@wjh.harvard.edu>, Julian Robinson <j.p.p.robinson@sussex.ac.uk>

Dear Phil,

Good to see you last week. As you know, we want to encourage you to return to the subject of the YR. A good way forward is your proposal to conduct a series of interviews aimed at documenting the experiences and attitudes of those involved. We would be glad to provide information from our archive on the various aspects of the YR, including information on each individual to be interviewed, as well as our large documented chronology of events. This would be on the understanding that the interviews, whether to be kept confidential or not, would be copied to our archive as they are done. Of course we hope that the interview project will stimulate you to write a book on the subject.

So as to guide your interviews most effectively, you will want to have in advance relevant documentary and other information we have in our archive. And as you proceed from one person to the next, additional information will be developed applicable to the conduct of interviews still to be done. With these two criteria in mind, we suggest a modification of your initial priority list, holding Haig and Watson until later and replacing them with James Wilkinson and Ambassador Stapleton Roy, both of whom were in the US Bangkok embassy during the YR events. In any case, we should get together for further discussion and to decide what from our archive should go into your initial dossiers before you start. Pretty much whenever you like. Let us know a couple of times that would be convenient for you.

All good wishes,

Matt and Julian

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Matthew Meselson <msmeselson@gmail.com>

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All good wishes,

Matt and Julian

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Department of Molecular and Cellular Biology
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Tel: (617) 495-2264
Fax (617) 496-2444

Subject: Re: Chapter on the "Yellow Rain"

From: Matthew Meselson <msm@wjh.harvard.edu>

Date: Sun, 04 May 2008 12:11:50 -0400

To: "Donald G. McNeil Jr." <mcneil@nytimes.com>

CC: Stephen Cunnion <cunnion@comcast.net>, Julian Robinson <j.p.p.robinson@sussex.ac.uk>, "Matthew S. Meselson" <msm@wjh.harvard.edu>

Dear Mr. McNeil,

Please find attached Chapter 4 "The Yellow Rain Affair: Lessons from a Discredited Allegation" by Julian Robinson and me.

The chapter will appear in the book

"Terrorism, War, or Disease? Unraveling the Use of Biological Weapons", edited by Anne L. Clunan, Peter R. Lavoy, and Susan B. Martin to appear in June from Stanford University Press. A description of the book can be found at the SUP web site at:

<http://www.sup.org/search/blogthis.cgi?search=0804759774>

and also at:

http://www.sup.org/book.cgi?book_id=5976%205977%20

Robinson and I hold the copyright for our chapter and I am assured by Stanford University Press that advance quotation from it would be agreeable to them, given appropriate citation of the book. You will note that much of the evidence we adduce in reaching the conclusion that there was no chemical or toxin warfare beyond the occasional use of riot control agent was produced through the careful work of US and UK government investigators--but ignored at higher levels. Unfortunately, that evidence was largely kept from public view at the time and some still is. Much more unfortunately, the simple and self-evident "lessons" cited near the end of our chapter were not learned by the senior officials and much of the media in the case of the allegations of WMD in Iraq during the months before the present war.

I highly recommend the attached article by Merle Pribbenow. He is a distinguished Vietnamese language expert and author/translator who for nearly 30 years was a CIA Operations Officer in SEA tasked with interrogating Vietnamese prisoners and defectors about diverse matters including the possible use of chemical weapons. It is important to note that his article was cleared for publication by the Agency, where his remains highly respected. He lives in the D.C. area and can be reached at <mpribbeno@juno.com>. And of course Steve Cunnion and his colleagues were also directly involved in the YR matter at the time and can provide an additional and similarly authoritative perspective from a different angle, as can several other persons with whom Robinson and I have communicated over the years since the allegations were first made.

I should add that there will appear in the same Stanford University Press book a chapter by Rebecca Katz arguing that some sort of chemical or toxin warfare beyond the use of riot control agents did indeed take place. Needless to say, Robinson and I believe that her conclusions entirely fail to follow from or account for the validated facts of the matter.

I will be in Cambridge at the number below until Friday when my wife and I leave for three weeks in France. But the email address below will reach me most every day, even while away.

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Donald G. McNeil Jr. wrote:

So is Matthew Meselson writing the true story then?

Perhaps he'd like to send me the chapters?

Sanaria was absolutely fascinating. We photographed an assembly line of dissectors pulling apart mosquitoes to remove their microscopic, transparent salivary glands -- at the rate of 80 an hour, thank you.

Somewhere in my piles of paper I have a military study of Vietnam-era brain injuries. I wonder if I should try to dig it out.

Donald

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Donald



Matthew Meselson <msmeselson@gmail.com>

'Extra' BZ production facility in the United States (Swannanoa, NC)

2 messages

Innovograph <grant@innovograph.com>

Wed, Nov 28, 2012 at 2:17 PM

To: msm@wjh.harvard.edu, j.p.p.robinson@sussex.ac.uk

Cc: guillemin@mit.edu, c.a.mcleish@sussex.ac.uk, j.revill@sussex.ac.uk

Hi Dr. Meselson, Hi Dr. Robinson

I live near an EPA Superfund site here in Asheville, NC that once produced BZ (150,000 pounds). I am looking for research assistance as this appears to be an 'extra' BZ production site as of now missing from the current CW research literature.

Attached is a 1986 *Associated Press* article with some light details as well as a recent EPA notice with more site environmental details. But onsite BZ production is confirmed on page one of the EPA fact sheet.

I am trying to help tell the story and relate the implications of this amount of BZ being manufactured and moved through our community during the 1962 - 1966 (stockpiled and transported?) period in question. My local newspapers and TV station are unlikely to cover this kind of 'connecting the dots' research as there are many questions to be answered before a better history on this site concerning BZ forms. For now, it does appear that Celanese Corporation of America started the BZ production in this case.

EPA website: <http://epa.gov/region4/superfund/sites/npl/northcarolina/chemtrmc.html>

Note: This EPA site fails to specify BZ (merely 'incapacitating agents') and its community risks during the site's BZ production, storage and transportation phase, but community activists such as myself are working on correcting this omission.

Starting in 1952 Oerlikon made air-air automatic cannons on this site. CS and apparently other weaponry was produced on the site during the Celanese-Northrop period.

In terms of the total US BZ arsenal, one of the things I'm unclear on is how much BZ was destroyed at the Pine Bluff Arsenal. Millmaster Chemical in New Jersey was thought of as the main BZ production facility until now from what I'm finding. I see a total of 100,000 pounds of BZ coming from Millmaster (partly based on *Paradise Lost: The Psycho Agents*, CBW CB No. 71, May 2006). There may have been smaller amounts of BZ produced elsewhere, but conventional wisdom seems to be the total BZ amount of ever produced in the US is 100,000 lbs. I base this on the May 1982 *Mother Jones It's a Mad, Mad War* article as well as the CBW CB article.

The only statement on BZ elimination I can find on the web is that between 1988-1990 BZ there was BZ destruction occurring at the Pine Bluff Chemical Activity in Arkansas. But there's no inventory of raw BZ weight destroyed. I tried to call Pine Bluff but have yet to hear back.

Does HSP have documentation as to how much BZ was destroyed in the late 80's? Of course what I'm trying to verify is whether all the BZ was destroyed and to add this Swannanoa, NC plant into the CW literature. It seems fact that Celanese and possibly Northrop (now Northrop Grumman) produced as much as 68 tons here in North Carolina.

I would like to be recognized as a research consultant should this issue be of interest to HSP. I would like to work with HSP, and if possible OPCW researchers, to create as much of a history of the Swannanoa, NC site as possible with focus on the BZ aspect. I am contacting University of North Carolina Asheville for help as well.

Thank you,

Grant Millin, Innovation Strategist and Owner
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No BZ Found at Chemtronics AP 12.1.86 (via Hendersonville Times-News).pdf
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FS-11-2012 - Chemtronics Third 5-Year Review Report - EPA Community Resources.pdf
743K

Matthew Meselson <msmeselson@gmail.com>
To: "Kirby, Reid" <rkirby@eximdyne.com>

Wed, Nov 28, 2012 at 2:22 PM

Dear Reid,
I thought this might be of interest to you.
Do come by whenever you come within range.
Best regards,
Matthew

[Quoted text hidden]

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—
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3 attachments



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From: Matthew Meselson [mailto:msmeselson@gmail.com]

Sent: 22 June 2015 20:41

To: Julian Robinson

Subject: Re: RCAs

6/25/2015 5:21 PM

Dear Julian

Blumenfeld worked at RAND. You perhaps recall the even more detailed report by Howard. Here in nyc i have only iPhone for email.

M

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Matthew Meselson
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Telephone 617-495-2264



Matthew Meselson <msmeselson@gmail.com>

RE: RCAs

1 message

Julian Robinson <j.p.p.robinson@sussex.ac.uk>

Thu, Jun 25, 2015 at 11:27
AM

To: Matthew Meselson <msmeselson@gmail.com>

Dear Matt,

Thanks! Might I ever have met Blumenfeld, do you think? Probably not, as I was working for SIPRI at that time in Sweden.

The Howard report of 1973 has been on my mind recently because it was done for Deseret Test Center, which as I understand things was established in 1961 for the particular and possibly sole purpose of advancing Secretary McNamara's Project 112. I can't immediately find my copy of Howard, but when I do I shall look to see whether it displays any 112 origins. It is high time, I think, that someone produced a decent history of the project and what its achievements were; one of my grumbles about the Moon MS is that it does not do so.

I now have more to report on how the Blumenfeld draft found its way into Chalfont's orbit. That second copy of the paper was from a file (FO 66/314) of FO Disarmament Department papers entitled "Views of United States of America on Chemical and Biological Warfare". In the midst of its struggle with the MoD about the status of CS under the Geneva Protocol, the Department was at that time nervous that the USG would change its position on the value of CS in Vietnam as abruptly as it had just done on chemical herbicides. The draft paper is attached to a minute from Timms to Summerhayes dated 1 January 1971 that contains this: "Some time ago, Sir Solly Zuckerman told us that he had seen an interesting draft document produced, I think, by Dr Kissinger's office assessing the ways in which CS had been used in Vietnam. We have now managed to get hold of a copy of this document by way of our

