

"YELLOW RAIN"

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BEFORE THE
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OPERATIONS AND ENVIRONMENT
OF THE
COMMITTEE ON FOREIGN RELATIONS
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ON
"YELLOW RAIN" AND OTHER FORMS OF CHEMICAL AND
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STATEMENT OF DR. MATTHEW MESELSON, PROFESSOR OF BIO-CHEMISTRY, HARVARD UNIVERSITY, CAMBRIDGE, MASS.

Dr. MESELSON. Thank you, Mr. Chairman. It is a privilege to appear at this important hearing.

My scientific background is in chemistry and biology and I have had, in addition, a longstanding interest in chemical warfare and chemical weapons.

In previous testimony on the very disturbing reports of chemical attacks in Laos and Kampuchea, presented before subcommittees of the House of Representatives in April 1980, I supported the course then being considered by the U.S. Government of bringing this matter before the United Nations. I certainly welcome the efforts of the United States to foster the now ongoing United Nations investigation and to extend its mandate for another year.

However, in some respects, I would recommend caution in concluding whether or not trichothecene mycotoxins have been used in Southeast Asia, although I agree that the preliminary evidence indicates that they have.

I question the initial categorical statement that the Government released in September that "these mycotoxins are not native to warm climates," or even the modified statement that we heard today, that they do not occur naturally in combinations identified in Southeast Asia.

We simply do not know enough about the natural occurrence of mycotoxins in Southeast Asia. I will come to that in a moment.

If we allow our case to hinge on overconfident statements about the origin of these particular mycotoxins, then, if we turn out to be wrong, our case will partially collapse; and if indeed further research shows it to be right, then hasty statements at this time will only create in the minds of those who should be agreeing with us a residue of doubt which will be difficult to overcome.

Now, in the limited time available I will not go very far into the technical details of why I do not think those statements are wholly reliable. But, in general, I would say this: We do not have any control studies from Southeast Asia except for the few samples briefly mentioned by Dr. Watson today. That information, which is not available to the scientific community for examination and review may shift my own evaluation somewhat, but additional control studies are needed.

In the scientific literature of the world there is, to my knowledge, not even a single study of whether these mycotoxins do or do not occur naturally in Southeast Asia. We do know, however that in laboratory tests, all four of the mycotoxins reported are produced in substantial concentrations under warm conditions by strains of the fungus fusarium. Moreover, there are published reports of natural outbreaks in Japan and India of each of the four toxins at warm times of the year. In one case, T-2 and DAS were reported in a natural outbreak on corn at 86 to 104° Fahrenheit. Regarding the claim that these mycotoxins are not native to warm climates, the existence of these and other published reports raises the question of whether the Government availed itself of adequate scientific advice in its initial public assessment of the evidence.

There is a broader biological context in which some of these complexities should be viewed. Trichothecenes are natural products and may well play some widespread ecological role, possibly as insecticides or in competition between plant species, since they are powerful insect and plant poisons.

Nearly everything we know about the natural occurrence of trichothecene mycotoxins is known from food plants. Now, it is obvious that a plant species would generally not be chosen by man as a food plant if it accumulates appreciable amounts of trichothecenes. We must therefore be cautious in drawing conclusions about the natural occurrence of trichothecenes from the studies that have been done so far. Such studies, valuable so far as they go, may bias our judgment regarding the natural occurrence of trichothecenes.

What do we know about the natural occurrence of trichothecenes in nonfood plants? So far as is known to me, there is only one relevant case published in the scientific literature. This plant, *Baccharis megapotamica*, is a shrub, which in Brazil has been found to contain 200 to 300 parts per million of trichothecenes in its leaves. These are not the trichothecenes reported by Dr. Mirocha in samples from Southeast Asia, but related so-called macrocyclic trichothecenes, called baccharins. Last month a study was published in the journal *Science* suggesting that the trichothecenes are made by soil fungi and then taken up by the roots of the plant and translocated to the leaves where they are found as baccharins. Water sampled from the area had about 1,000 parts per million of these trichothecenes, also presumably derived from soil fungi. It would not be surprising to find these compounds on rock surfaces, derived from ground water or leaf exudates or washings. I see the yellow light, Mr. Chairman. Does that mean I must stop?

The CHAIRMAN. Please proceed for another 3 minutes.

Dr. MESELSON. May I have some of my colleague's time?

The CHAIRMAN. Yes; go ahead.

Dr. MESELSON. Thank you.

My point is that we know very little about the normal occurrence of trichothecene mycotoxins in the tropics. While learning more, we should avoid hasty assertions that the trichothecenes detected in Southeast Asia cannot be of normal occurrence.

There is another area of uncertainty, it seems to me, regarding the reported symptoms. There is serious question as to whether any of the mycotoxins Dr. Mirocha has reported, particularly if disseminated in course particles or droplets as described by Mr. Burt would cause the rapid and massive hemorrhage and death Mr. Burt described to us today. A recent publication, coauthored by Dr. Mirocha, is entitled "The Failure of Purified T-2 Mycotoxin to Produce Hemorrhaging in Dairy Cattle." A similar report has been published by a British research group, experimenting with calves and pigs. This is not to say that under some as yet undemonstrated conditions some of the reported trichothecenes, singly or in combination, cannot cause the reported symptoms. The point is, however, that at this time it would not be correct to say, regarding the demonstrated symptoms of the four trichothecenes and the reported symptoms that the fit was perfect.

Another question that must be considered with great care, although probably not at this particular hearing is whether the samples analyzed by Dr. Mirocha are authentic. Is there even a remote possibility

that the U.S. Government has somehow been provided with samples that are not authentic?

I would like to conclude by reading some summaries of interviews with victims or alleged victims of toxic chemical sprays from 10 different highlander villages in Southeast Asia.

First village: Abdominal pains, diarrhea, nasal irritation, coughs lasting more than a month; many children died;

Second village: Abdominal pains, diarrhea, skin rashes looking like insect bites following contact with sprayed vegetation; several children died;

Third village: Diarrhea, cramps, skin rashes, fevers, many children became ill; an estimated 30 died;

Fourth village: Diarrhea, cramps, rashes, fever, coughing blood; 38 children reported to have died.

I could read all 10 of these, but they all sound pretty much the same.

These interviews are not from Laos. They are not from Kampuchea. They are from the Republic of Vietnam representing 5 different tribes in 10 different villages. Each village was close to areas sprayed with herbicides, according to U.S. Air Force flight records. The interviews were conducted by the U.S. National Academy of Sciences under a contract with the U.S. Defense Department as part of a study of the effects of herbicides in South Vietnam, published in 1974.

Now we believe that the herbicides used should not have these effects, unless the villagers had some unusual and quite unexpected sensitivity. I know that, and I agree with what was said about agent orange earlier today.

However, this experience from the Republic of Vietnam tells us that between the reported perceptions of the interviewed villagers who were presumably exposed to herbicide spraying and the perceptions of the officials in Washington who authorized the herbicide program, there was an extraordinary discrepancy. In sifting the various possible explanations of what occurred in Laos and Kampuchea, it may be important to keep this experience in mind. In summary, I would agree that our main interest is to stop what appears to be a serious use of toxic chemicals. Second, a convincing case has yet to be made to the scientific community that trichothecene toxins are in use. The question of their natural occurrence I would say is still at issue, and questions regarding the match of symptoms are also unresolved. And, although problems of confidentiality may arise here, we need full and reliable assurance of the authenticity of the samples.

While we keep studying and accumulating more data, and, importantly, while we put the detailed findings before the scientific community as has not yet been done, we should not hinge our efforts on the unqualified assertion that trichothecene toxins must be responsible for reports of chemical attacks in Laos and Kampuchea.

Finally, for the future, we ought to try to establish reliable national and international machinery for dealing with possible future allegations of chemical warfare.

Thank you, Mr. Chairman, for the extra time.

The CHAIRMAN. Thank you, I guess your bottom line is that we should be very careful about reaching sweeping conclusions at this point.

ELEMENTS NEEDED TO REACH CONCLUSION

What extra evidence do you think would be needed to satisfy you that toxins were being used? What elements do we need that we do not have in order to reach this conclusion, in your judgment? Let me say, too, that I think you are serving a very good purpose here by raising these questions, because we do need to be very careful in analyzing this. But the State Department has made categorical conclusions in its statement today.

What additional elements are missing, in your opinion?

Dr. MESELSON. The first is, if evidence in possession of the State Department is to be convincing, that information has to be put out and made available to the scientific community.

It is understandable that under heavy pressure of events, this may not have been given adequate priority. But maximum effort should now be made to change that situation and to release all relevant information that need not be classified.

Second, as to what else needs to be done, I would say that this is a complex matter to discuss in only a few minutes. The general answer to your question is that carefully thought-out comparisons need to be made of the sort that Dr. Watson was indicating, but which, in my opinion, have not yet been adequate.

The CHAIRMAN. You mentioned negative samples would be required before we could reach some conclusions about toxins in Southeast Asia. What do you mean by this and how long would this take?

Dr. MESELSON. It means you test samples of the same plant species taken some distance from the attack site, trying to reproduce the same ecological conditions. Then you subject the results to critical analysis and review by the scientific community. You try for example, to establish that plants, water and surfaces in the attack site have the toxins and that there is no fungus on the plants or in the ground from which the toxins could have come; but that in the sites where you don't have an attack, you do not find the toxin in plants of the same species growing under matched conditions, or on surfaces, or in water, et cetera. The latter is the sort of background evidence which, until the limited and briefly described work mentioned by Dr. Watson today, never has been done in Southeast Asia. The only published case where a nonfood plant has been analyzed for trichothecenes revealed a plant that apparently takes up trichothecenes through its roots and transforms them and translocates to its leaves, where they occur at high concentrations.

The CHAIRMAN. As I understand your point, it is that we do have some samples, quite a number of them, but unless you had a comparative sample on the same day from the same area—

Dr. MESELSON. Not just one, but a number of samples carefully selected along the lines I have indicated and with results subjected to critical review.

The CHAIRMAN [continuing]. Very well; many.

I don't want to get into a debate with you, but I noted that Dr. Watson had something to say on this.

How many samples did you have? Let me repeat that I do not want to initiate a debate here.

Dr. WATSON. I welcome coming back, Mr. Chairman.

The kinds of studies that Dr. Meselson has just described represent exactly what we did. We went in and took control samples under

the same conditions, matched up the species, took soil samples, and took vegetation samples, and those all have been shown to be negative. We have only six samples, but this is not an area where a troop of scientists can go in and collect samples at random and take all day doing it. The people who go in and do this are in danger every minute they are over there. They have to get out of there in a hurry.

While it is less than the scientific control study that I would like to have, I think we have answered the question of a natural occurrence.

I think Dr. Mirocha would agree with me that the appearance of 150 parts per million of T-2 toxin in something that was scraped off a rock could not possibly be a natural occurrence.

The CHAIRMAN. Dr. Mirocha, would you like to get into this?

SCIENTIFIC COMMUNITY GOOD FORUM

Dr. MIROCHA. Yes. I agree with my colleague that certainly the scientific community is a good forum for asking questions and taking a number of sides. This is healthy. It is a good way to arrive at truth. Sometimes it is painful, and sometimes it is not. Nevertheless, it takes time.

I can only report information on limited amounts of samples that I have had and to the best ability that we could work with the samples that we had. There were not many.

I would agree with Dr. Watson that indeed samples were sent to our laboratory of leaf material from someplace—I cannot say where they came from—and they were negative. We have a lot of these negative samples, and particularly the leaves. Someone else will have to comment about where they originated, because I have no idea which are controls, which are the principals, and so forth.

The comment as far as baccharin from the species baccharus being the macrocyclic trichothecene which is related chemically to the small membered trichothecene, certainly is a good example of a toxic component in leaves. It would appear that this material, based on my biological knowledge, does not come from soil, but rather by some organism that colonizes the roots and therefore then is translocated into the upper portion of the plant.

We have no evidence for this occurring in any of those plants infected by fusarium, which would include bananas, tomatoes, and many of the so-called crop plants. I would not preclude these from evidence to say that because we have not looked extensively on noncrop plants, that this therefore would deny the integrity of our findings on those plants at which we have looked.

Certainly Dr. Meselson is correct in saying that we have not looked extensively on some of the other horticultural varieties of plants, except for those sent to us by Dr. Watson or some other members of the group.

So we have some of this evidence. As a reporter, I would submit to the committee that we certainly have more examples of negative findings in the leaves sent to us as background controls than we have of positives in the principles.

The CHAIRMAN. From what you have seen and heard, what conclusions would you reach? Are these toxins being used? Or are you at the point where you don't know?

UNEQUIVOCAL PROOF OF TOXIN PRESENCE

Dr. MIROCHA. I can only report to you as a scientist that we have found unequivocal proof of the presence of these toxins, T-2 toxin, deoxynivalenol, diacetoxyscirpenol and nivalenol—four of them—on various substrates sent to our laboratory. They are in combinations which, in my opinion, are high and which, again, based on my experience, we do not find normally in that concentration in natural substrates.

Now the rest of a conclusion would be very subjective on my part and I would rather not overextend myself beyond my evidence.

The CHAIRMAN. Dr. Meselson, what is your bottom-line conclusion about what is going on?

Dr. MESELSON. If Dr. Watson and I and other scientists were looking at the same data and had the opportunity to analyze it adequately, we probably would agree more closely than we now seem to.

The CHAIRMAN. I guess, practically speaking, we in Government cannot always wait for absolute evidence. We must do what a jury does sometimes, which is to make conclusions based on evidence that appears to be beyond a reasonable doubt. Have we enough evidence here that we have reached that point? The State Department's statement has apparently concluded that we have. Would you agree with that? Do you think there is enough evidence?

Dr. MESELSON. Considering the evidence that has been made available, I would say that it is not adequate to rule out reasonable doubt.

The CHAIRMAN. Thank you.

There are some additional questions that other Senators will have for you to respond to for the record.

Is there a final statement or conclusion anyone wishes to make? Is there anything you wish to add?

Dr. WATSON. Senator, I would like to add that I have looked at the evidence. In all fairness to Dr. Meselson, some of the evidence has not been presented to him.

All of this evidence will be written up in a scientific publication and with peer review. I think that basically is what the problem is. We have not submitted the evidence for peer review to other scientists, other than those who were directly involved in the analysis, including Dr. Mirocha and several other experts in the mycotoxin field. We have not submitted it to the general scientific community, as yet, though we do intend to do so.

The CHAIRMAN. Can you do that? Is there a restriction on doing that?

Dr. WATSON. No; there is no restriction. As soon as the paper is written, we will submit it. It is just a matter of the events moving more quickly that a scientific publication normally moves toward publication. We very much would like to submit a full paper discussing all of these results.

The CHAIRMAN. Approximately when will that be done?

Dr. WATSON. As soon as all of the controls are finished.

The CHAIRMAN. Are there any further comments?

Dr. MIROCHA. I think in defense here I would say that it is very difficult to answer your question of when because you don't do research or analyses on schedule. It might be early in 1982, it might be