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CLIMATIC CONSEQUENCES OF NUCLEAR WAR: WORKING GROUP #1

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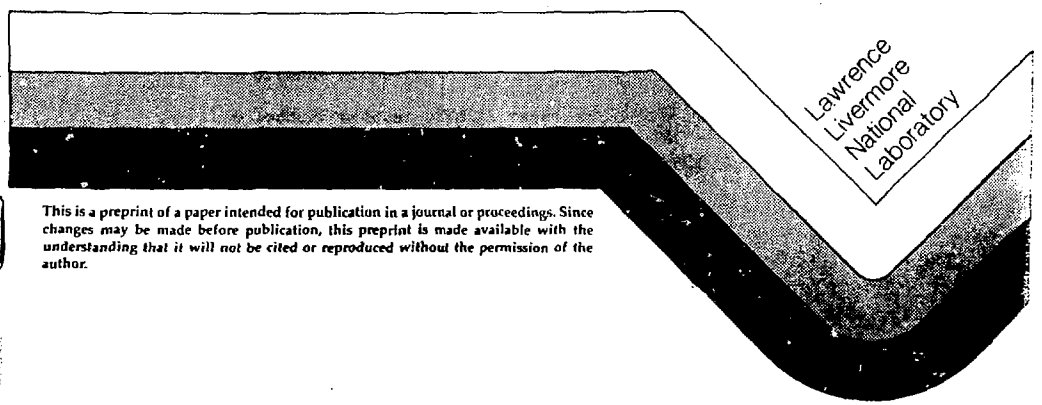

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INTRODUCTION

It should first be noted that I speak as an individual scientist this afternoon; the Department of Energy, the LLNL, and the University of California bear no responsibility for the content, suggested research directions, or opinions herein expressed. This is the third summer that scientists, ambassadors, members of government have gathered in Sicily to consider the consequences of nuclear war and to exchange information in the hope that a timely, free exchange would contribute to global stability. It is a pleasure to accept the challenge of responding to the proposed scope of work for Working Group 1 that Professor Zichichi displayed on the screen this morning. Very briefly, let us review the content of that scope of work.

- To conduct process-physics research to improve the quality of inputs regarding the atmospheric loadings of soot/dust and NO_x from baseline nuclear exchanges; To understand better how material lifetimes in the atmosphere are affected by processes lasting from a few hours to a few days involving coagulation, scavenging, aerosol modification processes, and including injection height effects;

- To study the modification of optical properties as a function of time and the removal rates for aerosol-structures which may at times be non-spherical;

- To understand the forcing functions of climate models; To provide new assessments with higher quality inputs reflected in the above, but not necessarily limited to those items;

- To provide appropriate input at a suitable time to the Biological Impacts Working Group as described in their proposed scope of research;

On August 13, 1983 I stepped off an aircraft in Moscow; I had been asked to lead a small team of American scientists to the U.S.S.R. Academy of Sciences in the "spirit of the World Laboratory". By means of this visit we thought that world tensions might be reduced if reliable information could be shared on the climatic consequences of nuclear war, if reliable information could be transferred upwards to decision makers, and if we could set some framework for cooperative efforts for research and acceleration of the flow of information between governments and societies. All of these potentialities are expressed in the "spirit of the World Laboratory." Dr. Aleksandrov and I co-chaired the meeting in Moscow. Since the Soviets are not here in Erice this year, it is perhaps appropriate to make the events of the Moscow exchange in 1984 a matter of record for the Erice Conference.

The Americans presented, in Moscow, all of the papers that were presented here in Erice in briefer form the following week (of August 1984). Aleksandrov's group presented their work and some 25-30 of the Soviets leading atmospheric scientists were in attendance with several making contributions to the technical exchange. One of them said, "the models of nuclear winter are untested and we must seek data bases, including those from the large natural geophysical events, to test these models before we can accept the present results." After my presentation on the microphysical/mesoscale processes that could change the soot loading and the input to the climate simulation models, another Soviet scientist said, "you are completely right, the atmosphere cleanses itself very efficiently, we have analyzed thousands of satellite retrievals; the evidence is that the atmospheric pollution is never observed above the cloud systems; the cloud systems are scavenging it all."

Professor George Golitsyn presented a theoretical paper which assumed that the soot is instantaneously spread over the globe, resulting in the thermal restructuring of the atmosphere so that the hydrodynamics of the climate system is completely changed; Prof. Golitsyn indicated that the weather would be completely different from that we now experience on our planet after a large nuclear war. To him I responded "you've forgotten the early time processes that modify your assumptions and hence could modify your results -- the completely new behavior may indeed not happen in this manner."

Afterwards Dr. Parker (USA) discussed the tremendous uncertainties in urban smoke emissions. He suggested that the postulated massive urban smoke emissions were overestimated by a factor of several based on his experience. Dr. Moiseev then related the recent history of fire research in the Computing Centre of the USSR Academy of Sciences. After recounting their various attempts at trying to simulate that complex problem, which they apparently abandoned because of the required computer power, he said that the uncertainties looked sufficiently great that the question of smoke emissions should be revisited. And at the conclusion of the conference, we agreed that there were two great uncertainties to be reported to the Erice Conference last year; one - the urban smoke emissions and, second, and the microphysics of the soot particles. Today these two uncertainties remain the two major questions about the nuclear winter assessments. All of this was said in openness in Moscow. No one in the room stood up to discount these two large uncertainties that could modify our views of nuclear winter.

As we all now know, our fellow-scientist V. Aleksandrov disappeared suddenly on March 31, 1985 in Spain. He was an active participant in the ICSU studies of the Environmental Consequences of Nuclear War. Professor Shapiro left one very important thing out of the preceding presentation. When ICSU held their conference in England in June 1985, they sent a telegram to Mrs. Aleksandrov expressing their regrets regarding the disappearance of Aleksandrov. We here in Erice echo those regrets. I regard Aleksandrov as a friend and a colleague who truly recognized both the importance and the major uncertainties of nuclear winter. I wish to pose the following question to the conference and to its chairman; "Is it possible that if we engage in openness to scientists of a closed society and raise questions, as just related to you, do we place those scientists at risk?"

The litany of uncertainty about nuclear winter is long and contains much detail; this litany will not be repeated now. But the study of uncertainties has led to a clearer understanding of new directions in research on the climatic consequences of nuclear war. It should be stressed again that the following ideas are the thoughts of a single individual in that Working Group #1 is yet to be convened, for obvious reasons.

NEW RESEARCH DIRECTIONS:

First, we need a much better definition of the emissions from massive urban fires; we need to know the physical, chemical and optical properties of the evolving aerosol in the perturbed atmosphere. We need improved estimates of the distribution of turbulence in the fire-induced plumes for the purpose of improving

aerosol coagulation calculations at very early times. It is estimated that, when the aerosol leaves the top of the combustion zone, there are approximately 10^{11} particles per cm^3; and approximately 6 minutes later, coagulation has reduced, the aerosol number density to about 10^5. But what was the turbulence model? Probably the wrong one. We need to estimate the correct distribution of turbulence in a massive fire plume so that the particles entering the capping cloud above urban plume can be better determined. Should the number density be less then 10^4, the capping cloud might not be overseeded; the capping cloud could then indeed scavenge the soot, as well as part of the radioactivity. As Professor Pruppacher illustrated last evening, in Case 3 with an aerosol number density $10^4/cm^3$ at cloud base, there was practically no suggested evidence of scavenging minutes into the simulation. In the Nagasaki event, the explosion and fire created a precipitating cloud that delivered "black rain" as well as radioactivity to the ground. If that cloud had been suppressed by similar mechanisms, as in Professor Pruppacher's calculated Case 3, I doubt that the black rain would have reached the ground at early times.

If we return to the list of simulation improvements we see that aerosol properties should be a function of time in the global simulations. Natural cloud systems may scavenge, or modify the entrained aerosol - hence, clouds operate as microphysical processors. They can change the size distribution and the characteristics of aerosol moving through them. We need to be able to estimate how the aerosol is changed with each successive cloud encounters during the first, perhaps, several days. We need to investigate the scavenging of the soot aerosol during the first several days through many cloud encounters, including the effect of impurities from the city fires.

My colleague, Dr. MacCracken, has included patchiness in his simulations, that is, patches of smoke spread from the small smoke clouds over the targets through their evolution in the atmosphere. It is under these patches that the optical depth is large quick cooling can occur, cold columns of air can be formed, and new distributions of mass in the atmosphere can arise and hence, perhaps, new mesoscale fields of motion. These processes are yet to be calculated in sufficient resolution and yet to be included in any assessment.

We need to explore prescribed burns as discussed by Dr. Brian Stocks. The experimental fires in Canada are immensely important for many reasons. First of all, the boreal forests of Canada are very similar to those in the Soviet Union. The second obvious point is that when you "tramp down" the forests (a term used by Dr. Stocks, which means that the wood is collected into large

piles and then ignited under suitable conditions), one approaches the fuel loading that might be present in cities. Hence, these Canadian fires might be some of the most relevant fires to study with well designed instrumental programs, with measurements in, above, and below the plume to understand the atmospheric radiation, the aerosols, and their microphysical properties.

In view of the inherent complexity of "nuclear winter" calculations, the associated uncertainties will be with us for a long time. But at the same time, new environmental issues are arising. For example, if the post-attack atmosphere develops a very strong surface inversion of a depth of 100 or 300 meters in a region adjacent to an urban fire, entrapped carbon monoxide and toxic materials could persist in continental valleys for some days. Smoldering fires combined with trapping inversions in the valleys could create toxic clouds that might affect the survivors.

The so-called dirty cloud problem needs further study in the near future. We should remember Dr. Stock's pictures of capping clouds as this debate progresses in the months ahead. Most of the capping clouds were clean on top (i.e., they appeared white). The soot-particles, which were quite numerous at the cloud base, were being incorporated in cloud droplets, modified, altered, and then transported out of the downwind cloud base and the evaporating edges of the cloud. The soot-particles did not reach the cloud top in significant numbers.

We should continue our microphysical studies of soot in the laboratory as started at the University of Nevada. This is a reasonable beginning to improved understanding of microphysics in the global simulation models, because the microphysics is probably not right at this time. The climatic consequences of nuclear war are created by the residual aerosol surviving plume coagulation, capping cloud alterations and scavenging, and cloud encounters in the early evolution of the soot-aerosol; these matters remain to be studied in careful analyses and special simulations.

My colleague Vladimir Aleksandrov did very extended integrations of the nuclear winter out to roughly 240 days, with relatively simple assumptions. We all recognize that those integrations were carried out for much too long for the assumptions included in the model to be valid; Leon Goure has discussed the problems of agriculture production in the post-attack environmental conditions. It is clear that we must make much longer duration simulations of the nuclear winter with suitable simulation models when they become available. The question is, what are the climatic and growing conditions in the second summer following the postulated large nuclear exchange.

As indicated in my first lecture, I believe that there will be survivors and that there will be survivors the second summer, *following nuclear exchange. Most nations do not have a policy of* storing one year's food supply. So the critical issue is, can agricultural production be restarted in the second summer and what are associated climatic conditions.

There are some larger questions. Included among these is the issue of the simulation of the second summer season after nuclear war. Further, we should revisit the civilian defense issue and revitalize civilian defense as a possible life-saving measure should nuclear deterrence fail. At the appropriate time, we should ask the question, what should the populace know about nuclear winter? Prof. Kapitza asked this question last August in Moscow; he said, "When do we take what we've just discussed to the public"? And fortunately I had listened to Dr. Teller often enough to have learned that there are times to zig and there are times to zag. The response was "I come only as an individual scientist of the United States and I cannot answer that question for our government." Further, I do not want to answer the question at this time as a scientist.

Now just one or two reflections. We ought to remember that the theory of nuclear winter is not proven and it is not disproven. The theory may be questionable, but we've not disproven it. The threshold of 100 megatons is a very popular concept. I believe that the threshold at 100 megatons is a myth, because do you believe that there would be no effect at 99 megatons and a big effect, as big as a 5,000 megatons scenario, at 101 megatons? I do not! Such a threshold or postulation of a threshold implies the precision to the simulation models that is non-existent.

I have already reported to you my views on the extinction of the human species. In the famous World Health Organization study, it is stated that one billion people will die immediately in a nuclear exchange. I will offer you only one comment, - 60% of the stockpile yield of that exchange was apportioned to the non superpower, heavily populated countries. I believe that it is not credible that one would send 60% of the yield to these non-combatant nations by ICBM techniques. Hence, the World Laboratory in regard to nuclear winter research should pursue this aim - that man, nations, and governments need reliable information in order to maintain informed electorates and decision makers, and to permit a balanced and mutual security between the super powers. In speaking of survivors, man survives best when and where he is *informed and hence prepared.*

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