Record of Proceedings of Session D-19

Environmental Aspects of the Large-Scale Use of Atomic Energy

THURSDAY MORNING, 11 SEPTEMBER 1958 (1958)

Chairman: Mr. W. G. Marley (UK)

Vice-Chairman: Mr. G. W. C. Tait (IAEA)

Scientific Secretaries: Mr. T. Carr and Mrs. T. Bernert

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The CHAIRMAN: This morning's session is devoted to environmental aspects of the large-scale use of atomic energy. The environmental aspects involve protection of populations, members of the general public, and involve control of radiation exposures of such populations and over wide areas. Necessarily, this cannot be done by personnel monitoring, and in many instances the protection must be measured at a site, in a location, or computed. This involves questions of meteorology, circulation of ocean currents, as well as biological build-up factors. We therefore have papers in two broad divisions, the first being terrestrial aspects, including meteorology, the second being marine aspects.

DISCUSSION OF P/316

Mr. B. H. KETCHUM (USA): I would like to ask Mr. Templeton if he could please explain why the half-life in the milk was shorter than the half-life in the grass samples. It would seem to me that the

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in the milk should bear a constant relationship to the concentration in the feed.

The CHAIRMAN: Before Mr. Templeton answers that, there is an extension of that question from Dr. Dopchie of Belgium. Perhaps he would ask his question so that Mr. Templeton can reply to the two together.

Mr. M. H. DOPCHIE (Belgium): I would like to know on what hypothesis the relation 0.1 μc/l of I^{131} in milk leads to a dose of 20 r in the child's thyroid gland.

Mr. W. L. TEMPLETON (UK): I think the answer to the question which has been asked on the dose to the thyroid is answered adequately in the paper.

The CHAIRMAN: Could you explain it in just a few words? I think it should be stated verbally, if you please.

Mr. TEMPLETON (UK): From the constants which are taken from the recommendations of the ICRP, the total dose delivered by a concentration of 1 μc/g of I^{131} in the thyroid is 130 rads. The mass of an adult thyroid is given as 20 g and that of a child has been taken as 5 g. Therefore, one microcurie in the adult thyroid delivers 6.5 rads whilst the same amount in the child delivers 26 rads. The child's thyroid is assumed to retain 45% of the ingested iodine with an effective half-life of eight days. The total intake required to give a dose of 20 rads can be calculated from these figures.

The CHAIRMAN: I think the point here is that all these parameters come into the calculation, and I think it is very important to recognize the complexity of the calculation of dose and the great attention which must be paid to this by the biologists and medical persons who must assist in this evaluation. Could you answer the second question, please?

Mr. DOPCHIE (Belgium): May I just put another question on the same subject? Is not the 45% that Mr. Templeton mentions included in the ICRP dose? In other words, does this 45% still have to be applied?

Mr. TEMPLETON (UK): The answer to Dr. Dopchie's question is "yes", the 45% does still have to be applied.

In answer to Dr. Ketchum's question, the half-life of the deposition on the ground, measured with a gamma instrument, was eight days. The half-life of the grass was between five and six days, and this again was reflected in the milk, which had a half-life of five days.

Mr. T. SCHÖNFELD (Austria): I would like to ask how the Sr^{90} levels changed after November 1957. They were only shown in the table up to that date. The reason I ask that question is that I should like to know whether the long-term change can be evaluated to give an indication of the relative importance of the various paths of strontium to the grazing animal; that is, on the one hand, the path-surface adsorption from the grass and direct uptake by the animal and, on the other hand, Sr^{90} to the ground, dilution by calcium and then to the grass.

The CHAIRMAN: There is another question on the same lines, by Dr. Sahasrabudhe of India. Perhaps in his reply Mr. Templeton will speak about the levels of strontium in vegetation—what they were. Perhaps it is in the paper.

Mr. TEMPLETON (UK): The levels in vegetation are given in the paper. On the slide I showed of a farm which was about three and a half kilometres south of the site of the area of maximum deposition there was an increase of about twenty-five strontium units in the milk. Subsequent to November 1957 the levels have fallen back again to the pre-accident levels. It was such that the deposition which did occur, was deposited on the grass and was eaten by the cow. Presumably with the onset of winter and the dying-back of the grass the strontium now is back in the soil, with the subsequent dilution.

Mr. T. MIYAKAWA (Japan): I would like to ask about contamination in rain and drinking water.

The CHAIRMAN: There is a further question, from Dr. Spitsyn, on the same subject.

Mr. V. I. SPITSYN (USSR): I should like to ask whether Mr. Templeton has any information on the behaviour of radiostrontium and other radioisotopes absorbed on the surface of the soil after rain.

Mr. H. J. DUNSTER (UK): In reply to the first question, we have no direct measurements of rainfall, prior to its deposition on the ground, associated with this accident. The levels in water running off land into reservoirs, the levels of water in the reservoirs and the radioactivity in the water at the supply point —the tap—were measured for I^{131} particularly and the highest figure was about 100th of the ICRP occupational maximum permissible level.

I think the only answer we can give to the second question is that, associated with this accident, we have no information to offer at all. The amount of deposition, apart from the iodine, was not such as to enable us to do any useful scientific experimental work with the data.

Mr. F. BEHOUNEK (Czechoslovakia): I have three questions. First, has an attempt been made to calculate the maximum dose which the local population takes in by way of contaminated milk?

Secondly, were weather conditions rather favourable for the diffusion of contamination, and was there more contamination than there would have been if the weather conditions had been different?

Thirdly, for how long was radioactive material escaping into the atmosphere? I think there is in fact a certain difference between this Windscale incident and the explosion of an atomic bomb.

In this case, we have the formation of a cloud extending according to more or less known laws. On the other hand, I have the impression that here the radioactive material must have been escaping over a fairly long period, so it is not possible to conclude that this escape was immediate, as in the case of the atomic bomb, and we therefore think that this escape must be different from those we know about in the case of atomic fall-out.

Mr. **Dunster** (UK): The maximum dose to a thyroid of a child, estimated for this incident from the duration for which contaminated milk was drunk, was 25 rads. The highest activity in a child's thyroid found by direct measurement corresponded to a total dose of 16 rads.

The second question, I am afraid, I was unable to understand from the interpretation.

The third question: the duration of the persistence of the activity in the air is something on which we do not have very good information, because with the relatively short period of release of this type, the cloud moves from place to place and it is difficult to be sure of its total time of persistence. Some information is available from the work done through the International Geophysical Year. My own impression is that the maximum duration was a few days rather than a longer period, certainly not more than a week or so. There is a considerable difference between the behaviour of fission products released at low altitudes in this way and those released in a weapon explosion.

May I ask you to repeat the second question?

Mr. **Berounek** (Czechoslovakia): I don't know the climatic conditions in the British Isles. Consequently I do not know if the weather in the days following the incident can be regarded as good, bad or very bad from the point of view of the contamination of the largest zone. So I wonder if the situation might have been better or worse.

Mr. **Dunster** (UK): I think the conditions at the beginning of the incident were on the bad side of average, from the public health point of view. In this case deposition from the early stages of the incident was greater, that is, worse than the average conditions. Later conditions were reasonably close to average. In extreme conditions one might expect something worse than we had, but it is a little difficult to estimate how much worse.

The **Chairman**: In that connection, I believe there are two articles in the current edition of *Nature* relating to this incident, one concerning the biological data which has just been presented, and one concerning longer range meteorological dispersion of the activity. I have not seen the article myself but there is a publication there which gives further details about the meteorological situation.

Now I regret I must terminate the questions on this. There are many more questions, and I would request the persons formulating them to address them directly to the authors who can then reply directly.

DISCUSSION OF P/177, P/2310 AND P/1536

The **Chairman**: Now we have questions on the last three papers, beginning with one from Dr. Klechkovsky to Dr. Fredriksson.

Mr. V. M. **Klechkovsky** (USSR): (1) In your experiments on the transfer of fission products in the soil, a considerable change in the Ru : Ce ratio was observed in the course of transition over 0-0.5 cm to 2 cm. Can you not tell what fraction of the overall gamma radiation (Table 2B) was due to ruthenium at depths of 2.5 cm and more, in the course of a year? (2) In your paper, there is a reference to an experiment with barley, in which the Sr^{90} : Ca ratio in the grain was 2.4 times higher than the Sr^{90} : Ca ratio in the soil. Were similar cases observed in your experiments with such plants? (3) The observations with regard to the effect of saturating the soil with calcium on the uptake of Sr^{90} by plants from the soil agree with the results of experiments conducted in the USSR by Prof. Gulyakin and others. Do you think it is possible to reach any conclusion about the advisability of heavily liming acid soils as a means of reducing the Sr^{90} uptake by plants and thereby reducing its concentration in the food chain?

Mr. L. J. G. **Fredriksson** (Sweden): I will answer the two last questions first. With regard to the ratio between Sr^{90} and calcium the high value for grain was specific for that plant in our experiments.

Regarding the third question, I think on soils which have a calcium content which is about 50% of the total basic in change capacity or less, it would be possible in practice to depress the uptake of strontium by liming, but I think we ought not to over-estimate the possibilities on other types of soil. Of course, soils which have such a low content of lime are not very common in many countries.

Regarding the first question I wonder if Dr. Edvarson would like to give an answer to that as he has been in charge of that part of the work.

Mr. K. **Edvarson** (Sweden): As regards the ratio between the different isotopes, we have not yet concluded this work, and we have found considerable variations between different samples of the same sort.

The tendency is that cesium is strongly retained in the uppermost layer between 0 and 1 cm whereas ruthenium is concentrated in the lower layers.

I think that is all I can say at present, as we are not yet ready with the statistical evaluation of these measurements.

Mr. R. **Scott Russell** (UK): With regard to the interaction of strontium and calcium in soil, the two papers we have listened to this morning seem to justify the conclusions that in the United States, Europe (both in Sweden and Russia) and in the United Kingdom this problem is now very similarly assessed. The apparent differences that exist in opinions on strontium-calcium relations in soil now seem to be because scientists in different countries are dealing with rather different situations.

However, with regard to the interaction of cesium and potassium it seems we have not yet quite seen the situation in the same light. Dr. Klechkovsky has suggested that the interaction between potassium and cesium is of a rather totally different character to that between calcium and strontium.

I should like to ask him if he would agree that we could helpfully analyse this into two components. The fact that potassium and caesium can be fixed in clay lattices, particularly the illites, and the extent to which they participate in these reactions, very much depends on ionic size—but, this apart, potassium and caesium do in fact show in the soil solution of the plant precisely similar non-isotopic dilutions (as he would call it) as he has demonstrated so successfully with strontium and calcium.

Mr. **Klechkovsky** (USSR): I should like to say that I agree with Mr. Scott Russell's remarks with regard to cesium and potassium. As I said in my paper, the question of the Cs : K ratio calls for further study.

As regards our own experiments, which we have described here, these relate to three types of soil, and it may be that under other conditions we shall obtain fresh data on the joint behaviour of these elements. But the fact that *one* pair (microquantities of strontium and macroquantities of calcium) behaves differently from the other pair (microquantities of caesium and macroquantities of potassium) can no longer be doubted.

Mr. B. P. **Leonard** (USA): I would like to venture the remark that cloud diffusion data obtained in the United States indicate that the values of certain parameters given in Dr. Davies's paper are applicable only at short distances from the release point. The first data I might mention have been taken by Dr. Hiltson, and workers, at Hanford plant in the United States. They find a transition of the diffusion in the vertical which implies the transition of the variants in the vertical, which means that no single set of parameters will represent the effects in that region. There is a second set of data of Carpenter and Thomas of Wilson Dam. They obtained data at 150 miles from the release point under inversion conditions. The indications are that the inversion conditions affect the horizontal diffusion very little but the vertical diffusion radically. This can be represented properly only by modifying Sutton's equation to the extent of taking different values of N in the vertical and horizontal planes. Some of these data are presented in paper P/428.

The **Chairman**: I think our discussion shows the enormous complexity of the evaluation of hazards from release material from both the point of view of meteorological complexity, and complexity in the behaviour of the soil-plant-animal relationships. There is enormous scope for a great deal more scientific work which will be very profitable in this field.

DISCUSSION OF P/297, P/402, P/2053 AND P/2128

Mr. P. **Groen** (Netherlands): My first question is to Dr. Kreps and it refers to the density distribution about which he spoke. In the paper it is stated that below 4000 m the density decreases in such a deep sea depression as the Tonga trench. I suppose the authors do not mean the real density but the density which the water would have if it were under atmospheric pressure, having the same temperature and salinity. The real density increases toward the bottom. As a matter of fact it is the potential temperature which determines stability.

Secondly, do the authors have a quantitative idea of the amount of time needed for the bottom water to rise to the surface of the sea? I think that it will probably be much longer than the time required for the replacement of the water at one particular place in a deep sea depression.

Mr. Y. M. **Kreps** (USSR): The complete text of the paper gave the data from which actual temperatures with corrections for pressure and density were calculated. Of course, with depths greater than 4000 m, changes in density are negligible, but all the same, they show that deep layers, from 4000–5000 m to the bottom, possess an instability which permits slight mixing.

For more detailed discussion we would have to refer to the table.

On the second question, the periods needed for water particles to fall from the surface to the bottom or to rise from the bottom to the surface, we have no direct data.

When we visited in February this year the Willington Laboratory, where Dr. Rafder works, we were greatly impressed by the calculations of the turnover time of water particles using C^{14}. Only we must bear two facts in mind: first, we do not know what fraction of these long periods is taken by the fall of water from surface to bottom, and what fraction is required for the rise; secondly, the ocean is a very complicated system, which is washed by a whole series of currents, travelling from north to south, from south to north, from surface to bottom and from bottom to surface. The direct measurements of current depths carried out on the VITYAZ have shown that these currents have velocities greater than we had thought. All this greatly complicates these calculations.

Mr. J. **Blok** (Netherlands): I am very much impressed by the considerable body of information gained from the experiments concerning the possibility of dispersal of radioactive waste in coastal waters. However, there remain two questions.

The International Commission on Radiological Protection recommends limiting any radiation dose to levels as much as possible below the maximum permissible levels. What I wish to ask is, are there any purification treatments of low-level wastage carried out before release to comply as far as possible with this recommendation?

Secondly, is it quite certain that no accumulation of radioactivity can occur from the total release to be expected in the future at great distances from the disposal sites, for instance, by means of a now unknown mechanisms? Would it not be wise to restrict dispersals in common seas such as the North Sea and the Channel as much as possible until more data have become available?

Mr. H. J. Dunster (UK): The first question, concerning the ICRP, is perhaps not quite unexpected. This recommendation of the ICRP is a difficult one to interpret quantitatively. In fact the discharges at Windscale have been treated from time to time—they are not treated continuously; it depends on the amount of radioactivity arising for disposal—but in general terms the intention has been to discharge fairly substantial amounts of radioactivity as part of an organized and deliberate scientific experiment and the aims of this experiment would in fact have been defeated if the level of activity discharged had been kept to a minimum.

This leads to the second question, that it is actually unwise to discharge radioactivity into the seas indiscriminately, and only careful scientific evaluation of these problems can demonstrate the safety. One of the principal and, I believe, the most effective methods of carrying out these investigations is indeed to use radioactivity and discharge it and find out what happens to it.

This leads to information a great deal more sound than that which can be obtained by small-scale and laboratory experiments.

As for the possibility of a mechanism which might produce large concentrations at great distances, such considerations have to be borne in mind when assessing the problem, and the investigations carried out in the Irish Sea have been extended to quite remote coasts and there is no reason at all to believe that any mechanism of such a disastrous nature that it can cause reconcentration after many tens of kilometres can occur in the Irish Sea. It is unlikely, in my opinion, that such processes will occur in any coastal waters where there is a reasonable amount of turbulent diffusion.

Mr. D. E. Hull (USA): Dr. Ketchum, in referring to his Table 1, mentioned that nearly all the elements in the sea are concentrated in one or another type of organism, but cesium appears to be an exception to this case. I wonder if this might be attributed to the chemical univalent nature of cesium as distinguished from the other elements in this table and whether Dr. Ketchum thinks there might be other organisms as yet untested which would concentrate cesium. Is there any reason to expect such an organism to exist?

Mr. B. H. Ketchum (USA): To answer your second question first, I do not think there is any reason to expect such an organism to exist except that there is a tremendous variety of organisms in the sea and we have looked at very few of them. Unquestionably cesium, as indeed I should have mentioned, is an exception to the general concentration rule. Sea-water, of course, is a 3% salt solution; a good bit of this is sodium and potassium, and I think that as with the soil equilibria similar phenomena are probably going on with regard to cesium in the concentration factors by marine organisms.

Mr. A. Persano (Italy): Do the authors of the papers think that it is possible to reduce the concentration of fission products in organisms living in the seas by complexing the fission products before discharging them into the sea, or by methods similar to those used to avoid fixation of fission products in the human organism?

Mr. Dunster (UK): I do not think I know the answer to this question.

Mr. Ketchum (USA): I do not believe I know the answer either, but there are a few data. Some of these complexes in sea water are utilized indeed by the marine organism. This has been studied particularly in a variety of copper complexes in terms of their toxicity to marine forms, and several of the complexes, copper materials, are ultimately assimilated by marine organisms; others are removed completely—it depends, of course, upon your complexing agent—and the toxicity of these can be reduced. I regret to say I have no information about the internal consumption of the entire complex unit under these circumstances, but I presume that it is excluded from the living tissue of the organism.

The Chairman: If I might make the remark, the subject of Dunster's paper was the disposal of radioactive waste, and the economic problem of changing the chemical form in this way would be a very material factor in the overall assessment of the value of the disposal method. I think the subject is obviously worth looking at by our chemists concerned with this problem.

There is one more question, from Mr. de Laboulaye of the International Atomic Energy Agency, addressed to Mr. Dunster.

Mr. H. de Laboulaye (IAEA): Is it anticipated that the totality, or at least a large part, of the irradiated fuel in the United Kingdom will be treated at Windscale, and must we therefore assume that the total discharge of slightly radioactive effluents into the Irish Sea at Windscale will increase in somewhat direct proportion with the total installed atomic power in the United Kingdom?

Mr. Dunster (UK): The categoric answer to the last part of the question is, No, you need not anticipate that the discharges from Windscale will increase in direct proportion with the total nuclear power capacity. For the time being, at least, the processing from the nuclear power producing plants will be done at Windscale, but this will involve new processing plant and the extent to which the radioactivity of the fission product is segregated into the main fission product stream for storage will be considerably greater than with the existing plant. There is no intention of increasing the discharge, in any case, beyond what the present work has shown to be safe, and there is no reason to suppose that discharge need be increased significantly at all with the development of the power programme.