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678070

The Windscale Inquiry

Report by
the Hon. Mr. Justice Parker

Presented to the Secretary of State
for the Environment on 26 January 1978

Volume 1
Report and Annexes 3 - 5

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dangerous until the limit had been exceeded by 100 per cent or more.

10.40 I limit mention of BNFL's past errors to the two foregoing examples for, although their past record was [illegible] close scrutiny as a that they could not be relied on to fulfil their intentions sufficiently for the protection of the public, I am satisfied that, with the large margin of safety which exists, the submission is ill-founded.

Risk levels—Suggested inadequacies of current estimates and limits

10.41 Having concluded that BNFL are likely to achieve their intentions I now consider whether there is a real likelihood that risks currently estimated are so far wrong that THORP could not be built and operated at tolerable levels. To cover every suggestion advanced concerning alleged defects in the limits would be inappropriate. I shall take in turn only those suggestions principally relied on by objectors and consider them as briefly as possible.

Dr Alice Stewart

10.42 Dr Stewart and her co-workers concluded that cancer risks might have been under-estimated as much as 20 times. This conclusion was reached on the basis of a paper (the Mancuso, Stewart and Kneale paper) (IOM66) which had not been published at the conclusion of the Inquiry but which has since been published (Health Physics *33*pp. 369–385 1977). It was, however, known about in scientific circles by December 1976, and had attracted criticism from a number of sources. It was based on data relating to workers at the American nuclear establishment at Hanford. Dr Stewart herself and her co-worker Mr G Kneale both gave evidence before me. There can be little doubt that if Dr Stewart's conclusion is valid it would seriously affect the whole picture. This was expressly accepted by Dr Dolphin of NRPB. It would not however necessarily mean that THORP could not be built to tolerable levels. If the permitted dose were reduced to 1/20th of its present level it might still be possible to build and operate the plant to comply with that level. If it proved to be impossible then it would have to be abandoned.

10.43 I have mentioned that the Mancuso, Stewart and Kneale paper had met with criticism from various sources. As a result Dr Stewart had already made a number of amendments to the original paper prior to giving evidence at the Inquiry. One source of such criticism was Professor J Rotblat, Emeritus Professor of Physics at the University of London, who, like Dr Stewart, was called by TCPA. At the public hearings on the projected CFR-1 at the London International Press Centre on 13 and 14 December 1976, Professor Rotblat stated that he did not accept 'the report which came out a few days ago that, in the United States, (radiation) workers have had an increase in the incidence of cancer'. He thought that the samples were too small at that stage to enable an opinion to be expressed one way or the other. The report to which he referred was either the original or an early version of the Mancuso, Stewart and Kneale paper. When asked about this matter in evidence he stated (i) that [illegible] the paper and could not quite understand it, (ii) that he had since discussed the paper with Dr Stewart on several occasions to try to understand it, (iii) that Dr Stewart had on such occasions been very convincing but he still did not understand it fully and (iv) that he still did not accept the results, albeit his non-acceptance was less emphatic. Such a view, coming from such a person, appears to me of considerable significance. Dr Stewart was convincing in evidence in the sense that she rejected with supreme confidence suggestions that her results were wrong but she failed to deal with a number of what appeared to me to be valid criticisms. It is right that having made such a statement I should give instances. I shall give three.

10.44 *Example 1*

a. In Table 2 of the paper as presented at the Inquiry Dr Stewart set out data relating to 3520 Hanford workers who had died. This group was divided into those who had died of cancer and those who had died of some other cause. The table recorded that workers who had been exposed to radiation accounted for 66 per cent of the cancer deaths but only 61.1 per cent of non-cancer deaths and that this difference was statistically significant. Dr Stewart explained in evidence that this result, which was noted at an initial stage in the research, was such as to indicate that there might be something happening—i.e. some connection between radiation exposure and cancer deaths, and that it led her and her co-workers to go further.

b. In April 1977 Dr Ethel S Gilbert of the Battelle Memorial Institute USA commented upon the paper in its revised edition as at March 1977 (BNFL 311). Amongst her comments she included comment on Table 2. This comment was that if the exposed workers, who had been taken by Dr Stewart as one entire group, were divided into two groups, those employed for less than two years and those employed for two years or more, the apparent difference in Table 2 was eliminated. Thus

	% occurring in exposed workers	
	Cancers	*Non-cancers*
All deaths (from Table 2)	66.0	61.1
Deaths among those employed < 2 years	33.9	33.9
Deaths among those employed 2+ years	87.4	87.6

c. Dr Stewart was aware of this comment in April 1977. She did not, however, mention it or seek to refute it in her evidence in chief although she both mentioned, produced and sought to deal with an

earlier (July 1976) report by Dr Gilbert on the Hanford data in which the conclusion had been reached that 'analysis of the full data does not exhibit any evidence of a relationship of radiation exposure and cancer as a cause of death'.

d. When Dr Gilbert's ev[illegible] to Dr Stewart in cross-examination her observation was merely that lines two and three of the table at b. above were totally incompatible with the first line 'because an overall figure of 66 per cent could not arrive at a sub-division of 33.9 and 87.4 when a 61.1 gives you 33.9 and 87.6. I think that Miss Gilbert probably made a little arithmetical error'. In Dr Stewart's view the figure could not possibly be correct.

e. Dr Stewart's observation was made with complete confidence and was thereby on the face of it 'convincing'. Indeed it might be thought somewhat patronising towards Dr Gilbert. It was, however, clearly wrong. The figures are plainly algebraically *possible* and the apparently significant difference in the overall rates (66 per cent and 61.1 per cent) could be accounted for and shown not to be significant if (i) there was a higher percentage of cancers in the 2+ year group (both exposed and non-exposed) than in the < 2 year group and (ii) the percentage of exposed workers in the 2+ year group was higher than in the < 2 year group.

f. Although I was able to satisfy myself algebraically that Dr Stewart's answer was untenable I had not the basic Hanford data available to me. I therefore asked that I be provided with a revised version of Table 2 dividing the exposed workers into the same two groups as Dr Gilbert had used and giving the basic data.

g. As a result I was provided on the last day of the Inquiry with two further tables prepared by Dr Stewart. The first was a revised Table 2 sub-divided on the basis which I had requested. This contained figures which, although not exactly the same as Dr Gilbert's, confirmed that the division into the two groups eliminated any significant difference between the percentages of deaths from cancers and non-cancers occurring in exposed workers.
The second table consisted in a further revision of Table 2 in which causes of death, instead of being divided merely into cancers and non-cancers, were divided into RES Neoplasms, other cancers, accidents and other causes of death. This Table showed on its face that in both the 2+ year group and the <2 year group those dying of RES neoplasms had the highest radiation doses. Surprisingly, however, whilst the percentage of deaths from such neoplasms occurring in those who had been employed, and thus exposed, for the shorter period (<2 years) was shown to be statistically significant this was not the case in those who had been employed, and thus exposed, for longer periods (2+ years).

h. I was unable to investigate whether the last Table had any greater significance than the earlier [illegible] but the somewhat surprising result mentioned above coupled with the manner in which Dr Stewart had dealt with Dr Gilbert's criticism gave me no confidence that it had.

10.45 *Example 2*

(a) *Table 11* of the paper was designed to test for a correlation between the percentage of cancer deaths and the cumulative radiation dose after standardisation for age at death. The final figure in the Table was 0.46 ±0.22 and this was regarded by Dr Stewart as sufficiently high to be significant in establishing a correlation between cancer deaths and cumulative radiation dose.

(b) The Table was divided, as its title suggests, into age groups, one of which was the group aged from 60–69. There were 239 deaths from cancer in this group. The group itself, like all other groups, was divided into five sub-groups according to the radiation doses received. It was demonstrated in cross-examination of Mr Kneale that if one of those dying of cancer in the sub-group with an accumulated dose of 500+ centirads, had reached his 70th birthday, before dying, thus reducing the number in that group by 1 (from 12 to 11) and increasing the number in the next group by 1 (from 5 to 6) the final figure would have been 0.32 instead of 0.46. Mr Kneale agreed that such lower figure would not be significant in establishing a correlation.

(c) It was later established that a different move of a single person from one group to another could also increase the figure of 0.46 and thus show an apparently more significant correlation.

(d) It may be that in reaching such a conclusion I am flying in the face of statistical theory but my own conclusion is that if the significance or otherwise of an apparent result can depend on the chance that a single man died just before rather than just after a particular birthday, the result shown is not convincing.

10.46 *Example 3*

(a) Table 4 of the paper was entitled 'Observed and Expected Numbers of Specific Neoplasms listed according to Mean Cumulative Radiation'. It listed 18 types of neoplasms and used for expected deaths the figures from NCI Monograph 33 for cancer deaths for White US males in 1960.

(b) Dr Stewart, when cross-examined about this, at first asserted that the year 1960 had been carefully chosen because over 50 per cent of he deaths were before 1960; but she later agreed that there were a substantially higher number of deaths after 1960 than before. She also agreed that cancer rates in the United States had been increasing since 1960.

(c) I am unable to attribute much, if any, value to figures which do not correlate observed deaths with deaths expected at the dates when the

observed deaths occurred, the more particularly when the attempt to justify the [illegible] comparison as carefully chosen as a 50:50 figure was swiftly acknowledged to be wrong.

10.47 [illegible]

certain respects was acknowledged by Mr Kneale) unconvincing, I should perhaps stress that I have no doubt about either the importance of such data or the desirability of accumulating data of the same nature about radiation workers in the United Kingdom, a matter which was referred to in paragraphs 74 and 75 of the Sixth Report. Arrangements for such accumulation are already in hand. I therefore say no more on the subject except that I have not relied upon the paper by Dr Dolphin of NRPB (NRPB R54)–(BNFL 199) reviewing figures relating to Windscale workers in reaching my conclusions on Dr Stewart's evidence or indeed on any other matter.

Professor Edward Radford

10.48 Professor Radford, a member of US National Academy of Sciences Advisory Committee on the Biological Effects of Ionising Radiation and of that Academy's present committee which is engaged in up-dating the Report of the earlier committee (the BEIR report), was called on behalf of NNC. He was not an opponent of nuclear power nor did he advocate a 'nil' release of radioactivity in the course of operating nuclear establishments. He did however consider that the present risks of cancer, in particular the lung cancer risks, were underestimated and that the MPC_a for insoluble plutonium and americium should be reduced by a factor of 200.

His recommendations may be summarised as follows:—

i. The whole body dose limit should be reduced to 25 mrems p.a., i.e. to 5 per cent of the present ICRP limit.
ii. the MPC_a for plutonium and americium should be reduced by a factor of 200.
iii. All releases of tritium and plutonium should so far as practicable be to sea and not from the stacks.
iv. There should be included in THORP, if built, plant for the containment of krypton 85.

10.49 If THORP were built and operated so as to comply with these recommendations he would have no objection to it. He considered, however, that before final design and construction of THORP was permitted the magnox facility should operate to his recommended limits for at least three years in order to demonstrate BNFL's ability to operate to such limits. He finally invited me to request the Secretaries of State for Energy and the Environment to call an international meeting of countries concerned with nuclear power development to resolve differences concerning policy and standards. I record the invitation, but I do not accept it. Any recommendation for such a meeting could only be made if there was no adequate international machinery for fixing standards. I consider that there is such machinery.

10.50 I am satisfied that there should be, and that there [illegible]

radionuclides, but the material upon which Professor Radford's conclusions were based appeared to me unsatisfactory in a number of respects. He referred for example to studies in relation to uranium miners in Sweden and Czechoslovakia. The Swedish results had not yet been fully evaluated and he accepted that they were of too preliminary a nature to be of value. In the case of the Czechoslovak miners the studies did not disclose the numbers of exposed miners. He also referred to studies relating to patients suffering from ankylosing spondylitis who had been given radiotherapy but there are as yet no valid estimates of the doses to the lungs and bronchi of such patients.

10.51 I do not therefore consider that his conclusions have sufficient weight to justify me in finding that the present limits are likely to be changed so radically as to suggest either that THORP cannot be built to tolerable limits or, equally important, that the public are or have been under any serious risk from present or past releases.

10.52 With regard to tritium and krypton, Professor Radford regarded his suggestion as to tritium as an additional precaution only and in this context I need say no more about it, the more so as it is BNFL's intention to discharge all tritium to sea in any event or, at most, only a small fraction to atmosphere. With regard to krypton it is accepted by BNFL that krypton removal plant will be incorporated if the technology for its removal and safe retention is available. I am satisfied that it should. I also consider that BNFL should not merely stand by and instal such a plant if and when others develop it. They should themselves devote effort to its development.

Dr Sadao Ichikawa

10.53 Dr Sadao Ichikawa, Professor at the Laboratory of Genetics, Kyoto University, Japan was also called on behalf of NNC. He was wholly opposed to nuclear power and was not personally prepared to accept any amount of radiation exposure, however small, from nuclear establishments. He considered that the doubling dose for genetic effects should be 10 rem or lower. The doubling dose is the dose estimated to double the number of naturally occurring mutations. Existing estimates range from about 10 rem to 100 rem. Dr Ichikawa's figure is therefore at the lower end of the range. As the Royal Commission pointed out, less is known about the incidence of genetic effects per unit of dose than in the case of cancer induction. Dr Ichikawa based his conclusions largely on work which he had carried out on the plant Tradescantia but accepted that a similarity