SUBMISSION TO THE SENATE STANDING COMMITTEE ON FOREIGN AFFAIRS, DEFENCE AND TRADE

re

Inquiry into the Provisions of the Australian Participants in British Nuclear Tests (Treatment) Bill; and …….the (consequential Amendments and Transitional Provisions) Bill 2006

By

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I would welcome the opportunity to appear before the Committee to respond to any issues the Members might wish to raise with me.
JPL

Note: Attached to this submission are two pages containing tables which show the wide spread of **excess** cancers among participants in these tests

1. Thrust of Submission.

1.1 **Provision of health care as proposed is welcome and supported, and I would not want anything to delay its implementation.**

1.2 **But the intended action does not go far enough.** There is mounting evidence about health problems other than cancer caused by nuclear radiations. I am not in a position to argue this issue but urge the Committee to investigate it.

1.3 Apart from what appears to me to have been a meaningless quest to determine whether or not cancers were correlated with ionising radiation (see sections 3.6 to 3.8 below) I have no quarrel with most of the statistical work, qua statistics, as published in the study reports. However I have seen evidence that the nominal roll of participants is deficient to the extent that some participants have been left out and others have not been correctly credited with the circumstances of their actual involvement. Again, I am not arguing this issue but urge the Committee to heed what others may have to say about it.

1.4 My concerns lie in the area of scientific methodology underlying the cancer study and the interpretation of results. These aspects are flawed and have been directly responsible for the Government reneging on an earlier decision agreeing in principle to compensating the veterans under the terms of the Veterans Entitlement Act. The veterans themselves who are victims of cancer are thereby deprived of a disability pension and the widows of such veterans who die of cancer are deprived of the war widows pension.

1.5 In his second reading speech the Minister defended the integrity of the reports by referring to the eminence of the people responsible for them. I have no doubt at all about the eminence of those involved. It is the inference from that fact that needs to be treated with caution. Scientific eminence is a good base from which to start but it does not give a cast iron guarantee that it will lead to infallible results. Albert Einstein, scientific giant that he was and partially responsible for the discovery of Quantum Mechanics, has been shown to have been completely wrong in his understanding and interpretations of that branch of modern physics.

1.6 I will explain in section 3 below how the scientific methodology in these studies is flawed. What can be extracted from those theoretical considerations is a relatively simple picture of their practical outcome, namely the conclusions that correctly flow from the studies. That picture I present in section 2 following.

1.7 One of the conclusions I reach is the likelihood that the dosages allocated to participants by the Dosimetry Panel are underestimates. That such underestimates were in fact made is shown in section 4.

1.8 The issues canvassed in section 5 are peripheral, but nevertheless relevant I believe, to the Committee's considerations

2. Conclusions from the Studies

2.1 The escape clause used by the Government to deny compensation under the VEA was the claim that the studies showed no link between the excess cancers experienced by the participants and the dosages of ionising radiation they

received. This “justification” is misleading in the extreme.

2.2 The proper conclusion emerging from the studies is that, with high probability, ionising radiation was responsible for a great many of the excess cancers and that the lack of connection between cancers and dosages was due to underestimating the dosages experienced by the participants and/or underestimating the effect of those dosages**.** Understanding this calls only for careful attention to the explanation that follows.

2.3 The theory underlying the studies entailed that the dosages estimated for the participants would lead to a maximum of 3 cancers among all the 10,000 or so participants throughout their collective lifetimes..

2.4 The statistical studies showed that, at a very high confidence level, there were 456 excess cancers among the participants (military and civil combined).

2.5 If ionising radiation was not to blame, then causes needed to be found for 453 excess cancers.

2.6 Looking first at the civilians we find a total of 116 excess cancers (19%) compared with their peers in the general community. No substantive evidence was produced in the reports to show that there was something about this group of civilians that made them more susceptible to cancer than were their civilian peers. The only certain distinguishing mark was that this group participated in the nuclear tests, and the only factor relevant therein is ionising radiation.

2.7 Turning now to the military participants we find a total of 340 excess cancers (25%) compared with their peers in the general community. The higher rate among the military compared with the civilians is not surprising since there is an obvious possibility that something associated with their military service (apart from involvement in the nuclear tests) could be intruding. Indeed mesothelioma among naval participants resulting from asbestos in the ships of the time is one such, and 10 of the excess cancers can properly be put down to this cause.

2.8 About 330 excess cancers among the military remain to be explained. The epidemiologists tried to blame a swag of these on excess smoking. But their argument which runs like this is circular - smoking is a common cause of this set of cancers, therefore these military people must have smoked excessively, therefore they developed these cancers. Such an argument is only valid if independent evidence of excessive smoking is produced. No such evidence was produced.

2.9 Significantly, a 60% increase in the incidence of non-CLL leukaemia (which is commonly thought to be caused by radiation) was left “unexplained”.

2.10 I submit that the conclusion from the studies that there was no link between ionising radiation and the incidence of excess cancers is seriously called into question by the above considerations. The proper conclusion is that: **with high probability, ionising radiation was responsible for a great many of the excess cancers and that the lack of connection between cancers and dosages was due to underestimating the dosages experienced by the participants and/or underestimating the effect of those dosages**.

2.11 The above analysis is presented more rigorously in the account below which links the conclusion highlighted to examination of the studies in the light of the scientific methodology underlying them.

3 Scientific Methodology and the Cancer Studies.

3.1 In his press release of 28th June announcing that the Government would now provide free medical treatment for participants in the British nuclear tests in Australia who have or develop cancer, the Minister for Veterans' Affairs stated that this decision was taken "*Despite the lack of association between cancer rates and radiation exposure*." The qualification quoted is literally incorrect and should read "*Despite the lack of association between cancer rates and estimates of radiation exposures claimed by the Dosimetry Panel."* **Much hangs on the distinction between estimated doses and actual doses.**

3.2 The commentary that follows needs to be prefaced by a cornerstone of the Philosophy of Science, namely that, **in any investigation, whether theoretical or practical, no basic belief can be taken as inviolate. Every one of them is called into question and open to refutation if fresh evidence contradicts them.** Such epistemological principle applies to the investigation discussed here.

3.3 **The nature of a study such as this one does not admit of absolute certainty in any of its essentials**. The best we can aspire to are provisional conclusions of varying degrees of probability. And it is here where I part company from the study teams . Weighing and balancing all the evidence that I am able to adduce from the reports I come to the conclusion that ***many of the excess cancer cases and many of the excess cancer deaths must be attributed to ionising radiation.***

3.4 The dosages coming from the Dosimetry Panel are presented by the epidemiologists as factual. But they were all estimates, the set taken as a whole having no more status than that of an as yet unproven hypothesis. **In particular, there was no way of verifying the estimates of inhalation and ingestion doses by any direct measurements; they could only be tested by deducing their consequences and then checking whether these consequences are borne out or invalidated in actual experience**. This is an important factor in the whole project. Moreover, much the same applies to many of the calculated external dosages, as I shall demonstrate in section 4 below

3.5 The epidemiology study (Vol 2) implicitly included in part just such a verification test on (i) the assumed risk factor associated with ionising radiation (approximately 1 cancer in the whole life times of 17,000 people exposed to one millisievert of radiation, and proportionately higher for larger doses), and (ii) the set of dosage estimates hypothesised for the tests. The combination of these two factors in the present study entails that, for the sum total of participants admitted to the studies, as a result of ionising radiation no more than 3 will contract cancer and no more than 3 will die from cancer.

3.6 **In both the mortality study and the cancer incidence study, the epidemiologists investigated whether there was a correlation between the number of cancers experienced and the assigned dosages. They reported no**

correlation. But this part of their work seems to be seriously flawed. Any correlation sought should have been between the excess cancers and the dosages, since the expected number of cancers, being derived from the general population who had no direct involvement in the tests, would certainly exhibit no correlation with the dosages received by participants in them. But there would have been an insurmountable problem in correlating excess with dosage. It was not possible to determine who among the cancer victims were the actual subjects of excess cancers, and so the excess cancers as such could not be distributed among the dosage categories. In short, the necessary data for testing whether there was a relationship between radiation dosage on the one hand and excess cancer incidence and mortality on the other could not be extracted

3.7 As I read their report it appears that, for each correlation test they did, the epidemiologists looked for correlation between the total number of cancers observed and the estimated radiation dosages. **In short, they ignored the fact that some 80% of the cancers could not possibly be correlated with radiation dosages and that this would have completely masked any possible observable correlation** between dosages and the relatively small number of cancers that might conceivably have been correlated with them.

3.8 In the mistaken belief that they had done a meaningful analysis and had shown that there was no connection between cancers and radiation, the epidemiologists then looked briefly at whether the dosages might be underestimates, but their conclusion that they were not is unconvincing. They took the result as decisive and then set out on the path of searching for other possible causes of the excess cancers. Even if they had achieved what they claimed, **the proper thing to have done was, for the time being, to still leave it open that the dosages might have been wrong or that the risk factor used might have been too low.**

3.9 The epidemiologists now had to find explanations for 223 excess cancer deaths and 453 excess cancer incidences. The logic underlying their inquiry was to identify a known cause of a particular cancer and then attribute the excess cancers to that cause. Where independent evidence was not produced to show the factual presence of the identified cause the procedure is no more than a case of *begging the question*. An example that is not in dispute will help to illustrate the point. In the case of RAN participants and the incidence of mesothelioma, the epidemiologists identified asbestos in RAN ships as the cause of the cancers. RAN ships of the day are now notorious for the large quantities of asbestos carried in various spaces and no-one would quibble at the cause of mesothelioma cases in the RAN being put down to asbestos rather than ionising radiation. This reduces the number of excess cancer incidences requiring explanation from 453 to 443.

3.10 Adequate explanation for other cases seems to be lacking. Consider lung cancer. The case presented in the report is just this: "*Lung cancer is strongly related to smoking, and the excess could be due to a higher smoking prevalence in test participants."* (*Main Findings*, Vol 2, page vi). This is a classic case of begging the question and it inevitably leads the reader to take it that the excess lung cancers were in fact (rather than in speculation) caused by high smoking prevalence. But to move to that conclusion it has to be actually shown that the test participants did in fact exhibit a high smoking prevalence, and that the

epidemiologists did not do.

3.11 Further to the last point, consider the following: 0.96% of the civilians developed excess lung cancers, and 0.87% of the service participants did the same. What possible reason could be adduced for the civilian participants having a higher smoking prevalence than the parent population with which they are being compared? The epidemiologists overlooked this anomaly.

3.12 As far as the service participants are concerned, it is easy to succumb to the folklore that servicemen smoked heavily. But folklore won't do here. The epidemiologists have to provide the evidence that, in fact, these participants did smoke heavily. Moreover in regard to what constitutes *heavy smoking* they should use the same criteria linking smoking with lung cancer as DVA uses (in the relevant SOPs), unless DVA is to resort to double standards.

3.13 Note also that the quote from the *Main Findings* in 3.10 above does not sit well with what we read in section 12.1 of Vol 2 (page 105) and section 5.11 (page 55). In the former, the speculation that increased smoking prevalence in the cohort could account for excess incidences of cancers of the oral cavity, oesophagus and lung is discussed but substantially negated *"because the mortality study of nuclear test participants has shown no excess mortality from chronic obstructive pulmonary disease (COPD), a finding that would be unexpected in a population with a high smoking prevalence"* . But then this qualification gets watered down by heaping another speculation on the pile: "*there was some under-ascertainment of deaths, particularly for causes other than cancer, so that a small true excess mortality from COPD is possible".* The second quote might be perceived as a patch to keep the smoking speculation afloat. Note also that in section 5.11 (page 55) smoking as cause of cancers of lip, oral cavity and pharynx is scuppered but the possibility is still canvassed in the *Main Findings* on page vi.

3.14 The epidemiologists could find no cause for excess non-CLL leukaemia which they point out is "*commonly found to be increased in groups exposed to radiation*" (page vi). This circumstance seems to me to be a strong pointer to the possibility that either the estimated dosages they were working with were wrong or that the assumed risk factor for triggering cancer was too low. Hence the advice I offered in section 3.8.

3.15. As it turns out, none of the explanations for excess cancers, offered as alternatives to ionising radiation, are acceptable, and for the simple reason that they are not backed up by facts linking the participants with speculated causes. For example, in an attempt to link civilian participants to asbestos-related diseases we find, again in the *Main Findings* on page vi, the statement that "*many of the civilian subjects in the cohort were in the construction industry, where asbestos was commonly used, at a time when less caution was exercised than in recent years. Whether any of these subjects were exposed to asbestos during the nuclear tests is not known*". Nor is any evidence produced to show whether they were exposed to asbestos in the work they did before the nuclear tests, or after them, or whether they differed in this or any other way from their parent population. **In short, the only fact we have to go on is that they participated in the tests, and that identifies ionising radiation as cause of their excess cancers with very high probability. Again refer to the advice I offered in section 3.8**

3.16 **To summarise the case of the civilians**: The only criterion differentiating

them from their parent population is that of participating in the nuclear tests. *Prima facie*, the causes for their excess cancers must be found in that participation and, unless other compelling factors can be found resulting from participation (or from external factors they imported with them into the tests which make them differ from the parent population), ionising radiation must be accepted as the cause of their excess cancers. No such causes were adduced, so it follows that **ionising radiation must be accepted as responsible for the excess cancer deaths numbering 87 and the excess cancer incidences numbering 116.**

3.17 This takes us back to what was said in 3.2 (basic beliefs are always open to refutation if fresh evidence contradicts them), 3.5 (the risk factor and the estimated dosages were subject to investigation in these studies) and 3.4 (the dosages - especially the inhalation doses but also many of the external doses- , provided by the Dosimetry Panel were substantially hypothetical and could only be validated or otherwise by comparing their consequences with actual observations). Their consequences were 3 forecast cancer deaths and 3 forecast cancer incidences for the whole cohort compared with the observation just noted of 87 deaths and 116 incidences among the civilians with only ionising radiation standing as highly probable explanation for most of them. **The conclusion that follows is that the doses were underestimated and/or that the attributed risk factor was too low.**

3.18 **To summarise the case of the services participants**, There are two criteria differentiating them from the general population with which, in respect of cancer incidence and cancer mortality, they were compared. One is that they participated in the nuclear tests. The other is that something specific to service life could be responsible. As shown above the one and only acceptable factor in service life that was presented in the study related to naval personnel and exposure to asbestos in ships. That left a total of 443 excess incidences of cancer in the cohort to be explained by the epidemiologists, and most of those remain unexplained. For the same reason as given in the case of the civilians, the bulk of the excess 139 cancer deaths (or 129 if 10 are accepted as resulting from mesothelioma) and the bulk of the excess 327 cancer incidences among the service personnel must be attributed to ionising radiation.

3.19 **A comment on RAN deaths.** Mortality of the whole cohort is summarised in the conclusion that it conformed pretty much to that of the general population. However, one important conclusion in this part of the studies was mentioned and then dropped. In Table 5.1 (page 46, Vol 2)) we find RAN participants with an observed death total of 1173 compared with an expected number of 1026 - an excess of a *statistically significant* 147. This result should have appeared among the *Main Findings*, but it does not. No explanation for this excess, or detailed examination of it, is offered. Working our own way through the report we find that 93 of the deaths were from cancer (Table 5.8, page 51) but we do not find any information on the causes of the other 54 deaths or explanations for their occurrence. Re lung cancer and mesothelioma, smoking and asbestos are discussed - smoking with no more success than outlined above, and asbestos offered for an unknown number of mesothelioma deaths that might have added up to 10 Again, in the absence of any concrete evidence to the contrary, **the bulk of the 147 excess RAN deaths (or 137 if 10 mesothelioma deaths are accepted) should be put down to ionising radiation**

3.19 **To summarise the studies**.
(**a**) Had only the epidemiological study been carried out, the conclusion would have been uncomplicated - namely that ionising radiation was responsible for the bulk of the cancer incidences and deaths.
(**b**) Conjoined to the epidemiological study, the dosimetry study led the epidemiologists astray. **They failed to see that the dosages were unproven estimates that would, in fact, be tested by the results of their own study**.
(c) Taking the estimates as factual, the epidemiologists, in a completely nugatory exercise, found no correlation between dosages and cancer numbers, and so they **incorrectly reported that ionising radiation was not responsible for the excess cancers, when in fact it remained an open question whether or not there was such a relationship.**
(d) The consequence of accepting the dosage estimates and the conventional risk factor was that a maximum of 3 cancer incidences and 3 cancer deaths would result from participation in the nuclear tests. We have seen that **the only feasible explanation for the bulk of excess cancers observed is participation in the tests and the effect of ionising radiation.**.
(d) This outcome conflicts with the deductions made from the dosimetry estimates and the currently accepted risk factor, so one or both of these must be rejected.

4.Underestimated Dosages in the Dosimetry Study

4.1 My original intention was to work through the report and compose a detailed critique. However the Government announced its decisions long before I could have completed this task so I have downed tools and limited myself to commentary on what I have studied so far.

4.2 To provide context for my claim that dosages were in fact underestimated I need first to outline the dosimetry methodology. The transition from the characteristics of the atomic explosion to dosages experienced by individuals and groups is effected in two steps: In the first, two tables (Vol 1, Tables 6.3 and 6.4) are presented that are believed to be applicable to all the tests carried out and provide the essential starting point for application to the particular involvements of individuals and groups in all the tests.

4.3 Table 6.3 comprehends radioactivity levels, external effective dose rates, and doses integrated over the time spent exposed to the radiation. It applies to exposure at any time from one half hour to 10 years after detonation. Table 6.4 is concerned with determining committed doses from inhalation and/or ingestion of radioactive materials. It applies to exposure at any time from one half hour to 10 years after detonation. The committed dose is the total dose a person would receive over a lifetime from the material inhaled or ingested. Here the committed dose is calculated for 50 years after exposure.

4.4 The second of the two steps involved in dealing with particular dosages is to select the appropriate data from the Tables and manipulate the figures as required by the circumstances of the case.

4.5 In the draft of the dosimetry report discussed at the Forum meeting of 27 April there was minimal explanation of the transition from explosion to Tables 6.3

and 6.4. I objected to this defect and asked that an appendix be included remedying the deficiency. Despite opposition from the Chairman of the Scientific Advisory Committee, an appendix was subsequently prepared by Dr Keith Wise who sent the draft to me for comment. The appendix comprises a lucid explanation of the methodology. It also incorporates both the necessary primary physical data and also the algorithm (and the mathematical theory underpinning it) by which the tables are computed.

4.6 The full story behind the derivation of Tables 6.3 and 6.4 is now on the record and available for open peer review. For my part I am confident that they provide the correct starting points for the derivation of dosages experienced by participants in their various activities. If the dosages estimated for participants in the tests are wrong, it seems that the errors must lie in the application of the results in Tables 6.3 and 6.4 to practical cases. Examples are not hard to find. One conclusion that will emerge is that it was folly beyond belief for the practical calculations to be carried out without full involvement of, and indeed a guiding hand from, someone familiar with all the details of the practical operations particularly by being involved in them in both a supervisory way and a hands-on way at the time they occurred. Such a person is Major Alan Batchelor whose attempts to assist were largely rejected.

4.7 Direct measurements of internal doses (inhaled and/or ingested) are not possible so the best that can be done is to calculate them as purely hypothetical data. To this end in the Dosimetry report, surface contamination activity per square metre is converted to airborne contamination per cubic metre by multiplying the former value by 10^{-5} , the **assumed *resuspension factor***. I located pertinent data in a report by C Walsh , *Calculation of Resuspension Doses for Emergency Response,* issued by the UK National Radiological Protection Board in Jan 2002. Walsh reports resuspension factors:

"*inside cab of land rover*" in the Maralinga trials:	$5.0\ 10^{-5}$ to 10^{-4}.
"*vehicular traffic*" at Maralinga:	10^{-7} to 10^{-4}.
"*sweeping vigorously* " in a room:	10^{-4} to 10^{-2}

4.8 As would be expected resuspension is sensitive to activity. Use of a blanket figure of 10^{-5} should be judged unacceptable. If the dosimetry report were to apply to the resuspension factor the criterion of always using worst case doses as, in many other places, it says it does, it should use 10^{-5} as its rock bottom lowest value and higher values elsewhere spaning the range $5.0\ 10^{-5}$, through 10^{-4} and possibly even higher, perhaps in some cases going up to 10^{-3}. **Use of a manifestly inappropriate resuspension factor means that many internal doses are underestimated in the Dosimetry report**.

4.9 But the weaknesses do not end there. One I am unsure about is this. In the draft report discussed on 27 April I showed how some dosages were underestimated by a factor of 925 due to incorrect conversion from radioactivity as measured on instrumentation used in the tests. The calculation has been excised from the final report, but **I do not know if the flawed conversion lives on by replication in any of the later dosage calculations.**

4.10 There is a table in the report (Table 6.7) giving a detailed procedure for calculating the dosages experienced by participants engaged in general engineering tasks "*undertaken in and around contaminated areas, including cable*

laying, fence construction, installation and recovery of instrument bunkers, and sandbagging of equipment for its protection. These tasks were performed before and after major tests, and sometimes in areas contaminated by previous explosions." Whilst this table might have been applicable to some limited operations, it was inappropriate in the extreme for general usage. As an example, its use by the Dosimetry Panel to calculate the doses suffered by Major Batchelor's engineering team in Antler not only had the team in the wrong place and suffering the wrong dosage rate, but also had them working for the wrong number of hours over the wrong number of days. According to the Panel's calculations a total dose of 13.5 millisievert (mSv) was supposed to be a generous estimate, based as it was on 115 days work of 10 hours per day. Major Batchelor's team was in the field near ground zero in less than one hour after one of the bombs had been detonated, engaged in a variety of tasks with dust swirling around them for just on one hour. I have seen the official records of the radioactivity in the vicinity of where this team was working and they show a reading of over 29 roentgens per hour, equivalent to a dosage rate of 290 mSv per hour. For a 10 hour day that the team did not work, the Dosimetry Panel allowed the members a dose rate of 0.01 mSv per hour, amounting to 0.1 mSv for the whole day, when the actual dosage suffered for the one hour they did work was 290 mSv, nearly 3,000 times greater. As a footnote, the 13.5 mSv estimate included both the external and the internal dosages, whereas the 290 mSv measurement was for the external dose only.

4.11 **Dosages for travel in contaminated vehicles** for both Buffalo and Antler were estimated and allocated to participants. I dispute a host of issues relating to this work. The estimates suffer from a combination of (small) arithmetic errors, unjustified assumptions that reduce dosages, and the omission of operational and theoretical factors that cause augmentation of doses. I shall comment on just a few.

4.12 The report says: "*Measurements on the floor and cabins of Land Rovers, using a 1320 counter, commonly showed readings of over 1000 cps (the upper limit of detection)*". This means that the actual contamination was *commonly* greater than that calculated for a reading of 1000 cps. How much greater?. Official figures that I have seen have readings at maximum on day 5. This could mean an underestimate on day 1 of up to a factor of 5 or 10. **All we can be sure of is that these dosages were underestimate**s.

4.13 The Dosimetry Panel then went on to reduce the 1000cps count by a factor of 4 because "*It is considered that the contamination entered the cabins of the vehicles on footwear and was in the form of discrete spots, therefore the average level of contamination would be expected to be considerably less than the spot readings recorded*". What evidence is there that the readings were in fact "spot readings" directly related to localised discrete sources of contamination and that they were less elsewhere? What evidence is there to back the assumption that the contamination was localised to discrete spots? Here is some counter-evidence. AWRE Report T22/57, referring to Yellow Canadian vehicles engaged in Buffalo has this to say: "*Three jeeps and three Dodge 15 cwt closed trucks were engaged on an exercise following Round 4. The exercise took these vehicles across country and in dose rates up to 10 r/h"* (= 100 mSv per hour) "*On D4 + 2, beta gamma counts of up to and above 1000 counts/sec inside and underneath were found. The*

interiors were uniformly contaminated at this level."

The contamination would have been brought into the vehicles not only as blobs on footwear but also as dust on footwear, clothing, and tools by all of the operatives in the course of their various activities during the period of their excursion. It would also have come in as described in 4.14 below. The dust thus imported would be expected to be scattered all about and not isolated into discrete spots. **There was no justification whatever for reducing these dosages by a factor of 4.**

4.14 **Swirling dust sucked into vehicles** The report says: "*When the vehicle is driven over contaminated areas, there will be additional contributions, internal and external, from the radioactivity on the ground and resuspended by the vehicle*". There is no doubt about this. Passage of the vehicle will be accompanied by the stirring up of dust. This will also enter the vehicle in many different ways. Among the worst will be swirling dust, concentrated and sucked into the vehicle at the back, and distributed throughout the whole of the platform, cabin included. Quite a bit of this dust will be inhaled and ingested.

4.15 **Allowing for dust entering the vehicle during passage**. This is obviously a situation where the resuspended radioactivity is very large, and the resuspension factor used to calculate dosages must be correspondingly large. Walsh's figures of 5.0 10^{-5} to 10^{-4} should be contemplated, and if the worst case scenario is to be considered, an eye should be cast on his figure for sweeping vigorously in a room, namely higher than 10^{-4}. **In the case of a vehicle travelling over a contaminated area, any calculation of inhalation dosage and/or ingestion dosage using the resuspension factor of 10^{-5} is going to lead to underestimates of dosages received**.

4.16 **Discounting for beta particles**. The Dosimetry Panel went on to reduce the intra-vehicle count by a further factor of 5: *"external dose rate is based on a beta/gamma ratio of 4:1, (the 1320 monitor was most frequently used with the beta window open. Therefore only 20% of the measured count rate is from gamma radiation)*". But 100% of the measured count rate was from gamma rays when the beta window was shut and no allowance for this circumstance is built into the procedure. That the window was sometimes shut, and the 5:1 reduction wrong, is evident from the following entry in Table 4 of AWRE Report T 22/57 relating to decontamination of trucks during Buffalo: *"3-ton, D1 + 6, (contamination) > 1000 gamma all over*" **Without doubt, some underestimates of dosages arose from this reduction**. I am not at all sure that any reduction was warranted anyway. No information seemed to be given on how the instrument was being held at the time of measurement. If it were anywhere near head height in the cabin then the beta particles would have been there for ready inhalation.

4.17 **I do not believe that there is a shadow of a doubt that the dosages assigned for travel in contaminated areas have been considerably underestimated.**

5 Some incidental matters

5.1 Questioning DVA's Handling of this Project from November 2005 till the disbanding of the Consultative Forum on 27 April 2006

5.2 In November 2005 the CF, after being in uninformed recess for 18 months, was advised that the study was in its final stages, although there was a hold-up while some re-writing took place.

5.3 On 10 April 2006 the CF was advised that a meeting was scheduled for 27 April to receive the final study reports. I received my copy of the Dosimetry report on Good Friday, 17 April. The epidemiological report arrived on 20 April. Previously sight-unseen, it was an impossible task to analyse these long complicated reports in what in effect amounted to less than one week. Despite protestations the DVA Project Manager insisted on going ahead. What is more he was determined that the 27 April meeting would be the final meeting of the Forum and that it was being held primarily to brief members on the study outcomes. That the members might have commentary to make on the reports that could necessitate some re-writing if not re-thinking was not an item in his agenda. In fact the intention was clearly to sideline the Forum and use the meeting as window dressing..

5.4 In a series of emails I had made it clear to DVA that I did not intend to be a stuffed dummy or a rubber stamp for the reports that were to come from the scientific groups, and I gave them chapter and verse on my competence to second-guess the scientists. All of this they chose to ignore.

5.5 Meanwhile Major Alan Batchelor (Retired) and Ms Ann Munslow-Davies had also been extremely critical of the way things were going. They were more than fully entitled to remonstrate since their knowledge of the tests, of the subsequent inquiries, and of the vast literature written on the subject was orders of magnitude greater than that of the investigators, even if taken collectively. Major Batchelor had also been a participant in the tests.

5.6 I had become so concerned at DVA's failure to address Major Batchelor's concerns that I sent an email on 17 December in which, among other things, I said :

> *" ….your response to those e-mails (from Alan and myself) erects a barrier, not only against attempting to resolve issues of concern, but even against any debate on anything……. . It does you no credit to bury his concerns, not suddenly expressed for the first time, on the expedient grounds of great urgency for completing the project when I believe that, with another approach altogether, those concerns could have been allayed with no threat to the timely appearance of an agreed report*".

5.7 Early in April Ms Munslow-Davies let it be known that she was very concerned about the way the project was heading. She was a member of the Scientific Advisory Committee (SAC) by ministerial appointment, and she was also the elected representative of the CF on that committee. DVA muzzled her from discussing her concerns with the CF.

5.8 On 21 April and 24 April Alan emailed DVA with substantive criticisms of the Dosimetry Study. Since the many earlier fully justified complaints he had sent in had not even been acknowledged let alone answered it was no surprise that the treatment in this case was the same.

5.9 On 26 April I circulated a criticism of Chapter 6 of the Dosimetry report. This was the critical chapter in which dosages were calculated for application to such scenarios as were experienced by various participants. The focus for these subsequent calculations rested in two Tables (6.3 and 6.4). The explanation for the derivation of these tables was woefully inadequate. The report also needed a substantial re-write to make it understandable and to remove errors in physics and arithmetic that I had discovered.

5.10 Earlier I had emphasised that both reports should be published and released to the public, not the least reason for this being the desirability that the reports later survive unfettered peer review. As things stood such peer review was completely written out of the script. I had specifically pointed to the need for the peer reviewer to have the necessary information to permit calculation from bomb blast to dosages and I had outlined how I saw this happening via three inter-related algorithms solved on digital computer. To this end the peer reviewer would need information on all the fission pair products of the explosion and their relative intensities, plus data on unburnt fuel, plus data on dose conversion factors for the different radiations occurring over the lifetimes of all the radionuclides produced in the various decay chains that would ensue. In principle none of this was mathematically difficult although the various inter-relationships were complex.

5.11 Mr Ric Johnstone, who had been a participant in the tests, and who throughout the course of the project, had also been prominent in his criticisms of it, also submitted a weighty criticism of the reports in which he too sought substantial re-writing and urged independent peer review of the dosimetry calculations.

5.12 The meeting of 27 April was a robust affair. The actual briefings were very well presented. Then began a debate between Batchelor, Munslow-Davies, Lonergan and Johnstone on the one side and the scientists on the other, with the DVA representatives reluctant participants as they were all for maintaining the status quo. The chairman of the SAC was unwilling to agree to including the extra information I wanted (in an appendix, I had suggested, so as not to create any major additional need for re-writing). None of the many concerns of Alan Batchelor, Ann Munslow-Davies and Ric Johnstone all relating to anomalies and omissions in the applications to participants, got more than a passing nod. Meanwhile the scientists had decided that some of the dosimetry report should be re-written. There was also an understanding at least informally that the scientists, in going about the re-write, would liaise with Alan, Ann, Ric and myself. DVA refused point blank my suggestion that, when the report(s) had been re-written, the Forum assist the DVA project team in interpreting the results and putting together a report to the Minister.

5.13 An impossibly short period for all of this to occur was specified by the Project Manager. He also refused to agree to let the Forum members see the revised reports. The Forum then and there was unceremoniously dissolved.

5.14 After the meeting the working scientists decided that they would have an appendix prepared and included in their report incorporating just what I had requested for bridging the gap between explosion and Tables 6.3 and 6.4. The mathematician responsible (Dr Keith Wise) subsequently sent me a copy of what he proposed to include in this appendix. It was excellent, incorporating an elegant single algorithm (to serve the purpose of the three I had suggested) together with all the information needed to feed into the algorithm to repeat the calculations, and a lucid explanation to boot. Dr Wise's account appeared in the final report as an Addendum.

5.15 The Consultative Forum had been disbanded on 27 April and was excluded from seeing a draft of the revised report. The first we knew of the Government decisions, released on 28 June, came from the Media. I did not get a hard copy of the final report from DVA till 18 September, after I had inquired about it. I was given to understand that the report had been sent to me but to an old address. This was surprising since I had given DVA formal notice of change of address, and the drafts that I got on 17 and 20 April both came to my new address and were accompanied by a phone call telling me that they were in course of delivery.

5.16 Meanwhile I had downloaded a copy of the final report via the internet and printed it out. Most but not all of my criticisms of Chapter 6 had been acted on, including the provision of the Addendum. Most of the concerns expressed by Alan Batchelor, Ann Munslow-Davies and Ric Johnstone had gone unheeded.

5.17 After the meeting of 27 April I started giving the published reports close scrutiny. The foregoing criticisms stem from that work. On 30 July I sent a critique to Admiral Harrington in his capacity as the Services member of the Repatriation Commission. He sent me a courteous reply and said he had referred it on for attention

5.18 In the debate on this bill in the House of Representatives the Hon. Alan Griffin referred to the work done by Alan Batchelor and me, and suggested that the Minister might have our concerns responded to. The outcome of that suggestion is summarised in the copy of the text of a message I sent to Mr Billson as a response to his remarks in the House:

> In Parliament yesterday you made the following remark:
>
> - "Another person, by the name of Jack Lonergan, was mentioned by the member for Bruce. Again, the member for Bruce was asserting that he had not been responded to. That is not correct either: Mr Lonergan has been responded to. We have recognised his great interest in this work as well as pointing out to him the government's response, the circumstances that gave rise to it and the availability of the detailed report".
>
> I am the person whom you describe as "by the name of Jack Lonergan"
>
> - (Brief CV)
>
> I suggest you correct the **substance** of the above statement in the Parliament where you made it. I am not concerned at the manner in which you choose to name me, but at least you are now better informed on that matter.

In case you have not bothered to check the facts or have been misinformed about them I list them below. I have copied the e-mail to Admiral Harrington (Repat Commission) and Dr Keith Horsley, DVA, who will be able to confirm what I now assert.

On 30 July I sent a detailed critique of the published study reports to Admiral Harrington (copy attached). I disputed the conclusions that had been drawn from these studies and explained in detail the causes of my concerns. Admiral Harrington replied and advised that he had sent the critique on to other people who, he thought, would be better able to respond than he.

All went quiet.

In mid September I had occasion to ring Dr Horsley about another matter and I inquired if he had heard about my correspondence with Admiral Harrington. He said that he had and that he would be coordinating a response from the study teams. He then arranged to call me at 3 pm on 21 September. This commitment he honoured.

In his call Dr Horsley referred to the complexity and breadth of the issues involved and asked if I would identify one important issue that he could put to the appropriate member of one of the study teams. He thought that resolution of our differences might be achievable through direct dialogue/debate over the full complement of matters in dispute. I said I was quite happy to proceed in this way and nominated the following issue to begin with : the fact that most of the estimated external dosages were not verified by measurement made at the time of the tests and that all the internal dosages were in fact unverifiable. He said he would put this point for a start to Dr Gun. He said that I could expect to hear from Dr Gun within a period of about 10 days. Three weeks on I have not heard a word from Dr Gun.

Not a single one of the issues I raised in the communication with Admiral Harrington has been the subject of any discussion by DVA with me or of any response other than the oral non-specific one by Dr Horsley.

I request that you correct the parliamentary record on this matter.

Yours sincerely

Jack Lonergan

EXTRACT FROM NOTES AND TABLES PREPARED BY JPL CONCERNING THE GENERAL CONCLUSIONS OF THE EPIDEMIOLOGY STUDY

Some remarks on terminology

Before looking at conclusions it is important to note that, in this study, to say that a result is *statistically significant* is to say that there is a 95% probability that the result points to some (possibly unidentified) factor that is responsible for making the study group different from the general population, and that the difference noted is not just a chance variation. Frequently in such studies the factor responsible for the difference will be determined by the criterion that distinguishes the study group from the population at large. In the case of the civilians considered here the difference is that they participated in the tests. The same applies to the military participants with the added distinction of being members of the armed forces.

When the word ***cohort*** is used it means *civilians plus all military combined.*

RESULTS

Five main results came from the combined dosimetry and epidemiological studies and these are summarised below

1. **Deaths due to cancer were considerably higher (over 18% more) among participants in the nuclear tests than expected by comparisons with their fellows in the population at large. See Table 1 below. The excess cancer deaths for the cohort are statistically significant, as is the case for the civilians, the military as a whole, and the RAN. The RAAF and the Army have excess deaths, both just short of being statistically significant. The deaths recorded are spread over a large number of cancer types and a very large number of them are statistically significant.**

1. ***Increases in Mortality of Participants in Nuclear Tests***

	* indicates excess of observed deaths relative to expected deaths ** indicates statistically significant excess *# excess marginally below statistical significance					
Cancer Types	**TOTAL**	**CIVIL**	**SERVICE**	**RAN**	**RAAF**	**ARMY**
All Cancers	**	**	**	**	*#	*#
Lip, Oral Cavity. Pharynx	**	*	**	*	*	**
Colorectal	**	*#	**	**	*	
Lung	**	**	**	**		
Melanoma	*		*#	*	**	
Prostate	**		**	*#	*	*
All leukaemias	*		*		*	
Non-CLL leukaemia	*	*	*	*	*	

2. The incidence of cancer in the case of the nuclear tests participants was considerably higher (23% more) than in the general population. The results are summarised in Table 2 below. The table reveals a very wide distribution among cancer types and a very large number of instances that are statistically significant, including the cohort, the civilians as a group (19%), the Services as a group (25%), and the three Services considered separately – the RAN (31%), the RAAF (20%) and the Army (22%)

2. *Increases in Cancer Incidence for Participants in Nuclear Tests*

*** indicates excess of observed cases relative to expected cases**

**** indicates statistically significant excess**

***# excess marginally below statistical significance**

Cancer Types	TOTAL	CIVIL	SERVICE	RAN	RAAF	ARMY
All Cancers	**	**	**	**	**	**
Oral Cavity.	**	**	**	**	*	*
Oesophagus	**	*	**	*#	*	
Stomach	*		*#	*	*	*
Colorectal	**	**	*#	*#	*	*
Liver		np	*	*		*
Pancreas	*		*	*		**
Larynx	*	np	**	*	*	*
Lung	**	**	**	**	*	*
Melanoma	**	*	**	**	**	*
Mesothelioma	*#	*	*	**		
Prostate	**	*	**	**	**	*
Bladder			*	*	*	*
Brain	*#	*	*		*	*
Thyroid	*	np	*	np	np	
Lymphomas	*	*#			*	
Multiple myeloma	*	**			*	
All leukaemias	**	*	**	*	**	*
Chronic lymphatic leukaemia	*	*	*		*	
Leukaemia excluding CLL	**	*	**	**	**	
	np = not presented					