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**UNITED KINGDOM NIREX LIMITED**

**Rock Characterisation Facility**

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**SUPPLEMENTARY  
PROOF OF EVIDENCE**

**OF**

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**REPOSITORY PERFORMANCE**

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## 1. SUMMARY

See [PE/NRX/15/S1 SUMMARY](#)

## 2. INTRODUCTION

2.1 In this Supplementary Proof I comment on the Proofs of Evidence which oppose the planning application for the RCF on the grounds that aspects of safety assessment research and modelling have not been adequately addressed by Nirex. I include comments on the Proofs and the Evidence led by:

	i.	Dr Starmer (PE/CCC/5 for Cumbria County Council);
	ii.	Dr Salmon (PE/FOE/5 for Friends of the Earth) (paragraphs 4.20 to 4.26);
	iii.	Dr Hencher (PE/FOE/6 for Friends of the Earth);
	iv.	Dr Allison (PE/FOE/7 for Friends of the Earth);
	v.	Dr Wogelius (PE/FOE/8 for Friends of the Earth);
	vi.	Dr Mackay (PE/GNP/2 for Greenpeace Ltd);
	vii.	Dr Haszeldine (PE/GNP/3 for Greenpeace Ltd) (Sections 13 and 14); and
	viii.	Mr Richardson (PE/GNP/4 for Greenpeace Ltd) (paragraphs 8.4 to 8.7).

### Principal Points of Agreement

2.2 There are four main areas in which there appears to be substantial agreement or, at least, no dispute between all the principal parties, namely:

	i.	the performance of barriers in the repository system must be evaluated carefully to ensure that radionuclides would be sufficiently contained by the system to assure safety of the people living at the time of repository closure and in the future;
	ii.	the groundwater pathway for release of radionuclides from the repository is the most significant pathway against which barrier performance should be assessed;
	iii.	calibration and validation of models is of importance in assessing repository performance; and
	iv.	Nirex is not yet in a position to make a safety case for the disposal of radioactive wastes, nor yet to give the regulatory authorities a sufficiently firm view of the suitability of locating a repository at the Sellafield site to warrant an application for its development.

### Principal Points of Disagreement

2.3 The assertions made in the Objectors' Proofs of Evidence which form the principal points of disagreement that I address in this Supplementary Proof of Evidence, fall essentially into three categories:

	i.	Nirex's evaluation of the post-closure performance of a repository at Sellafield does not properly reflect current knowledge of the site and processes influencing radionuclide return: the 'true' performance is likely to give a less favourable comparison against the regulatory target;
	ii.	a detailed understanding of all processes potentially influencing radionuclide return is required: such understanding has not been achieved and will require substantial further work; and
	iii.	in both the above regards, the RCF will not take us significantly forward.

2.4 The various points raised by the Objectors within each of these categories are addressed by Sections 4 to 10 of this Supplementary Proof of Evidence as outlined in paragraphs 2.7 to 2.13 below. In summary my response to each of the three categories is as follows:

i.		In its evaluation of Sellafield, Nirex takes a balanced approach to reflecting current knowledge of the repository system. I believe that we have properly reflected current knowledge in our performance assessment work and that the processes and features identified by the Objectors are adequately addressed for the current stage of development in our programme. Given the agreed significance of the groundwater pathway, I believe our performance assessment gives a good indication of the system performance and that the site shows good promise. However, further work is needed to help resolve uncertainties and the RCF has an essential role to play in this. Resolving these issues to a satisfactory level will demonstrate the confidence required to reach a decision on whether to submit a planning application for repository development.
ii.		Nirex does not accept that a complete understanding of all processes is essential to assessing system performance. Dealing with uncertainty is recognised by Nirex and HMIP as an integral part of carrying out post-closure performance assessments. When knowledge is incomplete it is accepted that this uncertainty is dealt with either explicitly, within a range of models, or through the use of appropriate parameter distributions in a probabilistic assessment.
iii.		The RCF will provide an opportunity to gain further information and reduce uncertainties in the key areas identified in my Proof of Evidence ( <a href="#">PE/NRX/15</a> ). The factors directly affecting repository performance include groundwater flow and radionuclide transport and the engineered sealing system needed at repository closure. In certain key areas, the RCF is the only route to obtaining this information and to demonstrating clearly to decision makers and peer reviewers that Nirex is able to validate its assessment modelling approach.

### **Structure of Supplementary Proof of Evidence**

2.5 The remaining paragraphs of this Introductory Section provide a guide to where points raised by the Objectors are addressed.

#### **Section 3: Repository Capacity**

2.6 This Section provides the information on the content of key radionuclides in respect of the revised repository capacity, as advised by Mr Folger in his Supplementary Proof of Evidence ([PE/NRX/12/S1](#), [paragraph 3.3](#)).

#### **Section 4: Treatment of Uncertainty**

2.7 This Section responds to points made by Dr Starmer ([PE/CCC/5](#)) concerning the treatment of uncertainty and probabilistic analyses. The inappropriate use both of arbitrarily defined quantitative criteria and of invalid numerical analyses is addressed.

#### **Section 5: Using Site Investigation Data to Construct Safety Assessment Models**

2.8 This Section responds to the points made by Dr Mackay ([PE/GNP/2](#)) concerning the relationship of site characterisation strategy and the construction of safety assessment models. The relevance of the case study cited by Dr Mackay and the conclusions drawn from it are addressed.

#### **Section 6: The Nirex Repository Concept**

2.9 This Section responds to the points made by Dr Allison ([PE/FOE/7](#)), Dr Salmon ([PE/FOE/5](#)) and Dr Wogelius ([PE/FOE/8](#), Section 4) concerning the Nirex Repository Concept. The inappropriate use both of arbitrarily defined qualitative criteria and of invalid comparisons is addressed.

#### **Section 7: Effect of Geochemistry on Radionuclide Transport**

2.10 This Section responds to the points made by Dr Wogelius ([PE/FOE/8](#), Sections 5 to 10) and Dr Haszeldine ([PE/GNP/3](#), Sections 13 and 14) concerning various aspects of the role of geochemistry in performance assessment. Dr Wogelius' Evidence is shown to be either of marginal relevance to performance assessment or to require the

kind of work that Nirex has in progress or planned for the RCF. Dr Haszeldine is shown to have misinterpreted the controls on the deep hydrochemical environment of the repository.

## **Section 8: Treatment of the Biosphere in Performance Assessments**

2.11 This Section responds to points made by Dr Starmer (PE/CCC/5) concerning the treatment of the biosphere in performance assessments, and in particular in *Nirex 95* [COR/522]. The validity of the single result on which Dr Starmer bases much of his Evidence on climate change is addressed. The validity of other statements on climate change and water abstraction in wells is also examined.

## **Section 9: Results of Groundwater Flow Modelling**

2.12 This Section responds to points made by Dr Starmer (PE/CCC/5), Dr Salmon (PE/FOE/5), Dr Hencher (PE/FOE/6) and Dr Haszeldine (PE/GNP/3) concerning results of Nirex's groundwater flow modelling. I show the Nirex approach to model development and application to be in line with international practice and I give reasons for the way in which features and processes at the site have been identified and incorporated into the performance assessment work.

## **Section 10: Validation of Models and the Role of the RCF**

2.13 This Section responds to the points made by Dr Salmon (PE/FOE/5), Dr Hencher (PE/FOE/6), Dr Mackay (PE/GNP/2) and Mr Richardson (PE/GNP/4) concerning validation and the utility of the RCF in the validation of hydrogeological models in particular. I place the Nirex strategy for validation in the context of accepted waste management practice. I provide details of the relevant work that Nirex has carried out, or that is in progress, and show how the cyclic approach to validation is being implemented and how it is essential that it be carried forward using the RCF programme.

## **3. REPOSITORY CAPACITY**

3.1 [Paragraph 3.1](#) of Mr Folger's Supplementary Proof of Evidence (PE/NRX/12/S1) explained that:

*"In late September 1995, in the light of updated indications of customer requirements which have been provided for conceptual design work, the Company adopted a new waste volume for its future work. The new planning basis to 2060 is a range of 200,000m<sup>3</sup> to 275,000m<sup>3</sup>, including 15,000m<sup>3</sup> of LLW."*

3.2 In this Section of my Supplementary Proof I comment on the radioactivity content of the reduced volume in respect of the three radionuclides chlorine<sup>36</sup>, iodine<sup>129</sup> and uranium<sup>238</sup> which are found to be of greatest importance to the safety of disposal (PE/NRX/15, [paragraph 5.6](#)).

3.3 Radioactivity contents of chlorine<sup>36</sup>, iodine<sup>129</sup> and uranium-238 for the 275,000m<sup>3</sup> volume are estimated to be the same as those given previously at [Table 5.1](#) (page 63) of my Proof of Evidence (PE/NRX/15) for a repository capacity of 400,000m<sup>3</sup>. This is because the reduction to the new figure of 275,000m<sup>3</sup> involves:

the elimination of 85,000m<sup>3</sup> of low-level wastes (LLW), having a relatively low content of chlorine<sup>36</sup>, iodine-129 and uranium-238; and the reduction of the volume of various intermediate-level wastes (ILW) through more efficient packaging and the radioactive decay of some ILW to LLW that will not require deep disposal.

Estimated radioactivity contents for the low end of the range, 200,000m<sup>3</sup>, are given in the following paragraphs 3.4 to 3.6. The reductions in radioactivity contents come about because of the potential elimination of notional future waste arisings that are included in the figures given for the 275,000m<sup>3</sup> volume.

3.4 The figure for the radioactivity of chlorine<sup>36</sup> in the disposal inventory given in [Table 5.1](#) (page 63) of my Proof of Evidence (PE/NRX/15) is 16.6 TBq. This value was the central case determined in an exercise to take account of uncertainties in the quantities present, as reported in *Nirex 95* (Volume 3, Table 6.3, page 6.25) [COR/522]. The corresponding value for 200,000m<sup>3</sup> of waste is 11.7 TBq.

3.5 The figure for the radioactivity of iodine129 in the disposal inventory given in [Table 5.1](#) (page 63) of my Proof of Evidence (**PE/NRX/15**) is 0.919 TBq. The equivalent value for 200,000m<sup>3</sup> of waste is 0.51 TBq.

3.6 The figure for the radioactivity of uranium238 in the disposal inventory given in [Table 5.1](#) (page 63) of my Proof of Evidence (**PE/NRX/15**) is 35.8 TBq. The equivalent value for 200,000m<sup>3</sup> of waste is 30 TBq.

3.7 Peak risks calculated in *Nirex 95* (Volume 3) [COR/522] in relation to release of chlorine36 and iodine129 would be expected to be reduced somewhat as a consequence of any reduction in the quantities of these radionuclides in the disposal inventory.

3.8 Peak risks calculated in *Nirex 95* (Volume 3) [COR/522] in relation to release of daughter radionuclides of uranium238 would be expected not to change significantly. This is because the concentration of uranium dissolved in the porewater in the repository is still expected to be at the solubility limit.

#### **4. TREATMENT OF UNCERTAINTY**

4.1 Dr Starmer's Proof of Evidence (PE/CCC/5) deals primarily with Nirex's preliminary assessment of the groundwater pathway for a deep repository at Sellafield in *Nirex 95* [COR/522]. He questions a number of aspects of the Nirex assessment including the modelling of the biosphere and groundwater flow. I respond to those matters in Sections 8 and 9 respectively of my Supplementary Proof of Evidence. In this Section, I focus on his discussion of the issue of uncertainty in *Nirex 95* [COR/522].

4.2 In summary, Dr Starmer (PE/CCC/5) attempts a quantification of the "*errors and uncertainties*" associated with the preliminary assessment of the groundwater pathway presented in *Nirex 95* COR/522. Within his analysis, he draws on cases set out in *Nirex 95* COR/522 to develop "*range bars*". These purport to show the uncertainty associated with the Nirex risk assessment. They are presented (PE/CCC/5, Figure 3) as resulting in an order of magnitude uncertainty about the highest risk curve presented in *Nirex 95* COR/522 for the groundwater pathway. By comparing these risk curves incorporating range bars with the regulatory target, he concludes (PE/CCC/5, paragraph 4.1.1):

*"The nature and extent of the uncertainties associated with the Nirex assessment call into question the likelihood that those estimates will remain below the risk target as a more complete assessment of those uncertainties is undertaken."*

4.3 In this section, I explain why the basis of his quantification and steps in his argument are flawed. First, I outline the nature of the risk target and the manner in which Nirex performs its risk calculations. Then I address Dr Starmer's treatment of uncertainty.

#### **Risk Target and Risk Calculations**

4.4 The *HMIP 1995 Consultation Draft, GRA2, Disposal Facilities on Land for Low and Intermediate Level Radioactive Wastes: Guidance on Requirements for Authorisation, September 1995* (HMP/1/1, paragraph 6.7) states as a radiological requirement of the disposal facility:

*"After control of the facility is withdrawn, the assessed radiological risk to a representative member of the critical group should be consistent with a risk target of 10<sup>-6</sup> per year (i.e. 1 in a million per year)."*

4.5 The document also defines radiological risk (paragraph 6.10):

*"Radiological risk to a member of a critical group is the product of the probability that a given dose will be received and the probability that the dose will result in a serious health effect, summed over all situations that could give rise to exposure to the group."*

This describes the method for the calculation of the expectation value of risk (which is, therefore, the value which is to be compared with the regulatory target) using probabilistic safety assessment methods such as those employed by Nirex, as I described in my Proof of Evidence (**PE/NRX/15**, [paragraphs 4.10](#) and 4.11).

4.6 In confirming the  $10^{-6}$  risk target the Government recognised the advice of the RWMAC/ACSNI Study Group which advised on the general criteria against which disposal facilities should be assessed in order to provide for an appropriate level of public safety. The Government noted in *the July 1995 White Paper* (page 21, paragraph 76) [GOV/208]:

*"The Group recognised that the confidence limits placed on an estimate of risk arising from a repository would be wider than for other nuclear plant, particularly at long times in the future, but felt that this should be taken into account in the way the potential risks were estimated and assessed rather than by applying different criteria of acceptability."*

4.7 Therefore, in carrying out performance assessments Nirex is required to incorporate uncertainty into the assessment of risk and not to consider it separately. Nirex has stated in *Nirex Science Report S/94/001, Post-closure Performance Assessment - Probabilistic Safety Assessment: Overview* (page 5) [COR/507]:

*"The approach developed for Nirex by the DSAT [Disposal Safety Assessment Team] involves:*

- *preparing an assessment in which acknowledgement and treatment of uncertainty is central."*

4.8 The major technique for addressing uncertainty used by Nirex is probabilistic safety assessment. In this technique models of the disposal system are set up in which parameter values are specified as ranges rather than exact values. The equations are solved using values selected from these ranges by a Monte-Carlo sampling technique. The outcome of such calculations is a distribution of possible consequences such as dose or risk. The technique is outlined in my Proof of Evidence, [paragraphs 4.10](#) to 4.16 (**PE/NRX/15**).

4.9 A key aspect of these calculations is that some values of dose or risk are more probable than others. From the many repeated realisations constituting an assessment, some values occur more frequently and thus have a higher probability. From the distribution of all the values the mean value of risk is calculated as a function of time, also known as the expectation value. This is calculated and compared with the regulatory target. As noted in paragraph 4.5 above, it is this comparison and no other which is the recognised way of addressing the regulatory target.

4.10 This technique was applied in the preliminary assessment of the groundwater pathway described in *Nirex 95* [COR/522]. This assessment focused on the groundwater pathway because it has been identified to be the most important pathway at the current stage of the investigation of the Sellafield site. This pathway is a key contributor to risk and to understanding of site performance.

4.11 In the *Nirex 95* [COR/522] probabilistic safety assessment, calculations were undertaken for a base-case conceptual model of the hydrogeology of the Sellafield site. This base case was developed by an expert group taking account of the data obtained from the Nirex Site Characterisation Programme at Sellafield. It was judged to be the most probable model of the hydrogeology of the Sellafield site based on the information available when the study was carried out.

4.12 In his Proof of Evidence (PE/CCC/5, Section 6) Dr Starmer has used the information set out in *Nirex 95* [COR/522] to develop a treatment of "*errors and uncertainties*" in a way which I consider is incorrect and which fails to follow the guidance laid down by Government, particularly as identified at paragraphs 4.5 and 4.6 above. Dr Starmer appears not to understand the basis of risk calculation and regulation. I illustrate this with two examples in the following paragraphs.

#### Example 1. Sub-Surface Routing in the Biosphere

4.13 As part of the analysis reported in *Nirex 95* [COR/522] one test of sensitivity looked at model uncertainty associated with a process in the biosphere termed sub-surface routing. This was identified as a pessimistic bounding calculation to address the uncertainty in the quantification of the sub-surface routing parameter used to describe the

distribution of radionuclides between soil and water on emerging into the biosphere. This resulted in risks about ten times higher than calculated for the base case.

4.14 As explained in Sub-section 5.2.5 of *Nirex 95*, Volume 3 [COR/522], there is good evidence that sub-surface routing will occur and this is now supported by *Environmental Science and Technology, Volume 29, No 7*, (pages 1719 - 1720) [NRX/15/4]. The pessimistic bounding calculation was undertaken because of then current uncertainties about the value that should be assigned to the fraction of the total radionuclide flux entering the surface soils in the biosphere that is translocated to the soils. The value chosen for this parameter for the *Nirex 95* base case assessment was believed to be conservative. The pessimistic bounding calculation assumed that the value was one.

4.15 Dr Starmer employs the calculated risks from this pessimistic case for comparison with the risk target in his Proof of Evidence (PE/CCC/5, Figure 3). He gives them equal weight in comparison to the risk target as he give to the calculated risks from the *Nirex 95* base case. For this to be valid he must assume that there is the same probability that the sub-surface routing factor is one as that it is the value used for the *Nirex 95* base case. This is incorrect. The probability that the value is one was known to be effectively zero and this has now been confirmed by [NRX/15/4], whereas the probability of the value being the same as that employed in the base case or even less was believed to be high and that has now been confirmed. In simple terms there is zero probability of a risk being incurred under the conditions of the case promoted by Dr Starmer and his comparison with the risk target is meaningless.

#### Example 2. Numerical Analyses

4.16 In his Proof of Evidence (PE/CCC/5, paragraph 6.5) Dr Starmer uses the histograms showing the distribution of conditional risk for the base-case probabilistic calculation reported in *Nirex 95* (Volume 3, Figures 6.9 and 6.10) [COR/522] as a presentation of the "variability of risk". He uses this to generate an order of magnitude which is translated into "range bars". This seems to reflect a failure to understand probabilistic assessments.

4.17 These histograms show the varying probabilities of realising risks of different values. The products of the probabilities and the risk values are summed to give the expectation value of risk. There may well be individual realisations giving values of risk above  $10^{-6}$  as in *Nirex 95* (Volume 3, Figure 6.10) [COR/522]. Provided that these have a low probability, the expectation value of risk which takes these realisations into account, will be below the risk target, as in the case considered in *Nirex 95* [COR/522].

4.18 The risks calculated from individual realisations are only comparable with the risk target after they have been multiplied by the probability of their occurrence. The selection of extreme values of risk shown on the histograms in *Nirex 95* (Volume 3, Figures 6.9 and 6.10) is a meaningless exercise in terms of addressing the risk target unless the probability of occurrence is accounted for.

4.19 Dr Starmer repeats this meaningless exercise in his Proof of Evidence (PE/CCC/5, paragraph 6.12) by claiming that a distribution of results shown in *Nirex Report S/94/001* [COR/507] reflects two orders of magnitude variation. In the probabilistic assessments analysed, the realisations giving risks much greater, or less, than the central value have very low probabilities which must be taken into account when making statements on their true significance.

#### Summary

4.20 Dr Starmer has built in his Proof of Evidence (PE/CCC/5) a description of the impacts of uncertainties by combining speculation on the significance of certain cases, of which the sub-surface routing case is an example, with semi-quantitative treatments and overlaying these confusingly onto the results of a probabilistic risk analysis. This segregation of uncertainty from the risk calculation is contrary to the requirements of the *Government White Paper* [GOV/208] as described in paragraph 4.7 of my Supplementary Proof of Evidence. In addition, Dr Starmer has:

been selective in his choice of cases (taken from the *Nirex 95* [COR/522] study), not reflecting the relative probabilities of different cases, and attempting to compound a variety of unconnected cases (PE/CCC/5,

paragraph 4.5.5). This is contrary to the requirements of a properly structured probabilistic analysis and inevitably introduces unquantifiable biases into his presentation;

- established no sound basis for the mathematical meaning or the magnitude of his 'range bars' which, in any case, appear to convolute uncertainties and probabilities which are neither additive or multiplicative; and
- attempted to use these 'range bars' to invite comparisons of risks with the regulatory target. This is contrary to the requirements of the regulatory guidelines (paragraph 4.5 of this Supplementary Proof of Evidence) which indicate that it is the expectation value of risk, derived from a proper probabilistic analysis that is to be used for comparison, not outlying values with low probability.

4.21 Dr Starmer's attempted comparison to the regulatory target is thus not based on a sound scientific treatment of risk, being essentially a product of manipulation of random and incorrectly derived values.

## 5. USING SITE INVESTIGATION DATA TO CONSTRUCT SAFETY ASSESSMENT MODELS

5.1 This Section of my Supplementary Proof of Evidence addresses points raised in the Proof of Evidence of Dr Mackay (PE/GNP/2) concerning the relationship of models used in performance assessments to field observations and the conduct of site investigations.

5.2 In Section 4 of his Proof of Evidence (PE/GNP/2), Dr Mackay refers to a theoretical exercise undertaken by him on behalf of HMIP [GOV/622, GOV/623, GOV/628]. In Sections 4 to 7 of his Proof of Evidence (PE/GNP/2), Dr Mackay then makes a number of general points about modelling (based largely upon that exercise) and asserts that:

i.	it cannot be simply assumed that collecting more data will necessarily improve risk predictions (PE/GNP/2, paragraph 4.12) and, that using increasing amounts of data can, counter-intuitively, lead to worse predictions of risk (PE/GNP/2, paragraph 6.3);
ii.	a concentration of investigations in the RCF will not necessarily improve predictions of groundwater flows (PE/GNP/2, paragraph 5.7) and that focusing on a small region close to the repository can produce worsening estimates such that regional measurements are crucial (PE/GNP/2, paragraph 6.5);
iii.	calculated error bounds do not necessarily encompass the true risks and conservative model assumptions do not necessarily lead to conservative risk predictions (PE/GNP/2, paragraph 6.4);
iv.	calibration against hydraulic head data is not necessarily sufficient to distinguish between conceptual models; predictions of other hydraulic properties must also be compared (PE/GNP/2, paragraph 6.6); and
v.	models can produce good predictions even though they are incorrect (PE/GNP/2, paragraph 7.1); if significant tuning is required to validate a model then the model must be regarded as invalidated (PE/GNP/2, paragraph 7.4). In Section 8 of his Proof of Evidence (PE/GNP/2), Dr Mackay suggests a procedure for a staged investigation process and asserts that it is necessary to identify a procedure in advance to demonstrate the appropriateness of predictions (PE/GNP/2, paragraph 8.1). In Section 10 of his Proof of Evidence (PE/GNP/2), Dr Mackay then criticises Nirex's approach to site investigation and, in particular asserts that:
vi.	Nirex proposes to validate only " <i>component conceptual</i> " models (PE/GNP/2, paragraph 10.5) and not the " <i>macroscopic</i> " model (PE/GNP/2, paragraph 10.3);
vii.	Nirex has not identified prior criteria against which later predictions can be judged in a multi-staged process (PE/GNP/2, paragraphs 8.1 and 10.6); and
viii.	Nirex does not have a mechanism to ensure that alternative conceptual models are identified and explored (PE/GNP/2, paragraph 10.8).

5.3 I respond in detail to these points in paragraphs 5.4 to 5.29 of my Supplementary Proof of Evidence, but in outline, my response is as follows:

i.	Although in a general sense increasing amounts of data do not necessarily improve the quality of predictions, the particular problems in this respect which Dr Mackay experienced in the course of his theoretical exercise arose because of limitations in his exercise which are not present in Nirex's programme.
ii.	Investigations in the RCF will complement regional investigations to ensure that the potential limitations identified by Dr Mackay are overcome.
iii.	Dr Mackay has developed a study-specific point into a general observation without sound justification. I do not accept his conclusion on what do and do not comprise conservative assumptions.
iv.	Nirex will validate its models against a range of data and not just hydraulic heads.
v.	The potential problems identified by Dr Mackay are recognised and Nirex's programme is designed to ensure that validation of models is taken to the point where there is sufficient confidence in their application as described in Section 10 of this Supplementary Proof of Evidence.
vi.	As discussed in Section 10 of this Supplementary Proof of Evidence and in <a href="#">Sections 7 and 10</a> of Dr Chaplow's Supplementary Proof of Evidence (PE/NRX/14/S1), Nirex has an extensive current and future programme of model validation, model development and model testing. This covers a variety of scales including the "macroscopic" and "component" levels identified by Dr Mackay.
vii.	Nirex proposes to use a multi-staged process. At each stage predictions will be made and subsequently evaluated against observations in relation to previously identified criteria. The way in which criteria for such testing will be set is identified.
viii.	Nirex has clear mechanisms for appraising alternative conceptual models.

#### **Point i: The Effect of Additional Data on Risk Calculations**

5.4 From the theoretical exercise to which he refers (PE/GNP/2, paragraphs 4.1 and 4.2), Dr Mackay draws the conclusion that:

*"....it cannot be simply assumed that collecting more data will necessarily improve risk predictions."(PE/GNP/2, paragraph 4.12)*

He further asserts that:

*"Using increasing amounts of data to develop the model can lead to worsening predictions of risk. This unexpected result is counter intuitive but of the utmost importance for site characterisation."(PE/GNP/2, paragraph 6.3)*

5.5 These conclusions are not relevant to the Nirex Site Investigation at Sellafield because of the material differences between Dr Mackay's rather unrealistic exercise and the approach taken by Nirex.

5.6 The sequence of modelling and synthesised investigations developed in Dr Mackay's study [GOV/662, GOV/623 and GOV/628] involved using data from old regional boreholes, followed by boreholes in the repository region and finally from new boreholes in the region. This is rather an unusual approach and quite different to the way in which regional data have been obtained and included in the Nirex assessment. Dr Mackay acknowledges (PE/GNP/2, paragraph 6.1) the fact that:

*"The Harwell site is geologically very different from Sellafield and the way the investigations and modelling were conducted may differ from the methods used by Nirex."*

Indeed, the way they were conducted does differ substantially, but Dr Mackay does not discuss how the differences between the phasing of data acquisition in his synthetic "*site investigation*" and the long-term programme at Sellafield would affect his conclusions.

5.7 In paragraph 4.9 of his Proof of Evidence (PE/GNP/2), Dr Mackay describes the conduct of his own exercise and notes that:

*"The site investigation team did not have sight of the risk predictions at the end of each assessment exercise and were given only a short amount of time in which to develop the assessment analysis."*

Dr Mackay's procedure is thus in marked contrast to the assessment cycle outlined in my Proof of Evidence (PE/NRX/15, [paragraphs 4.17](#) to 4.19) and described in *Nirex Report 525* (Sub-Section 5.2, pages 30 to 31) [COR/505]. Over a period of seven years since the initiation of site investigations at Sellafield, assessment studies have enabled the priorities for site investigations to be identified with the specific purpose of improving risk predictions. As a result of this, the difficulties which Dr Mackay encountered do not, in practice, arise in Nirex's work.

### **Point ii: The Use of RCF Investigations in the Prediction of Groundwater Flows**

5.8 In Section 5 of his Proof of Evidence (PE/GNP/2), Dr Mackay continues his account of the theoretical study undertaken on behalf of HMIP, concluding in paragraph 5.7:

*"With reference to Nirex's planned investigations in the RCF, it should be noted that a concentration of investigations in the RCF will not necessarily improve predictions of groundwater flows."*

5.9 By ensuring that the RCF programme complements the ongoing regional characterisation programme, and by focusing its model validation activities on key uncertainties identified in previous assessment cycles, the limitations of Dr Mackay's exercise will be overcome. The role of the RCF in improving predictions of groundwater flows is described at [paragraphs 6.15](#) to 6.27 in my Proof of Evidence (PE/NRX/15, pages 40 to 44) and at [paragraphs 8.79](#) to 8.88 of Dr Chaplow's Supplementary Proof of Evidence (PE/NRX/14/S1, pages 84 to 87).

### **Point iii: The Use of Conservative Model Assumptions**

5.10 Dr Mackay asserts that:

*"Calculated error bounds in predicted risks do not necessarily encompass the true risks and false confidence in risk predictions can be obtained. Conservative model assumptions such as uniform hydraulic properties do not necessarily lead to conservative risk predictions."* (PE/GNP/2, paragraph 6.4)

5.11 While the problem identified in the first sentence could apply, and did in Dr Mackay's exercise, it can be overcome by careful evaluation of uncertainty. As discussed in Section 10 of this Supplementary Proof, the methodology adopted by Nirex to test alternative conceptual models and to reduce biases means that our combined site investigation and safety assessment programmes are designed so as not to suffer the limitations imposed by Dr Mackay in his synthetic exercise, limitations which, Dr Mackay notes, included the inability to validate any of the models of formation properties being used (*A Study of the Effect of the Extent of Site Investigation on the Estimation of Radiological Performance: Volume 1*)[GOV/622, page 145].

5.12 Dr Mackay's second sentence is unsupported by any information provided in his Proof of Evidence and, indeed, I have not been able to locate any similar statement in the report on which his evidence is based. I would, in any case, disagree that an assumption of uniform hydraulic properties is actually a conservative assumption, in safety assessment terms (see definition of a conservative assumption in *Glossary of Scientific Terms* [COR/519]). In the case of Sellafield, as *Nirex 95* (Volume 3, Chapter 3) [COR/522] has shown, the inclusion of heterogeneous properties and fracture networks can lead to channelled flow. This leads to higher calculated risks than would an assumption of uniform properties.

### **Point iv: Calibration and Validation of Models**

5.13 In paragraph 6.6 of his Proof of Evidence (PE/GNP/2), Dr Mackay asserts that:

*"Calibration against hydraulic head data is not necessarily sufficient to distinguish between different conceptual models. It is important to also compare model predictions with measurements of other hydraulic properties."*

5.14 Nirex envisages a wide range of validation tests, consistent with Dr Mackay's view, and as identified in *Nirex Report S/94/004* (Section 2.4, page 18) [COR/510]:

*"The final stage of the detailed multi-dimensional groundwater flow and transport calculations is to compute various quantities that can be compared against experimental data to test the models. Suitable quantities are the groundwater head, the groundwater salinity, locations of recharge and discharge, values of recharge and discharge, and groundwater age (which can be estimated by calculating back along pathlines to the point on the surface at which they start). Testing the results of the model against experimental data is a very important part of the process of validation for the underlying conceptual model. A reasonable match helps to build confidence in the model, and consequently in the results of the assessment."*

5.15 Thus, it is seen that, in accordance with the approach endorsed by Dr Mackay, Nirex is indeed comparing model predictions with measurements of other hydraulic properties.

#### **Point v: The Predictive Value of Models**

5.16 I cover the issue of confidence in the "correctness" of models and their validation in detail in Section 10 of this Supplementary Proof of Evidence.

#### **Point vi: Validation of the Macroscopic Conceptual Model**

5.17 In paragraph 10.3 of his Proof of Evidence (PE/GNP/2), Dr Mackay criticises Nirex for not proposing to validate its "macroscopic" model of the site's geology and hydrogeology:

*"....the description of tasks to be undertaken in the RCF programme has not included a framework in which validation of the current conceptual macroscopic model of the site's geology and hydrogeology is attempted."*

5.18 Furthermore, in paragraph 10.5 of his Proof of Evidence (PE/GNP/2), Dr Mackay asserts that:

*"The model validation described in Nirex's evidence as taking place in the RCF is therefore only that of 'component conceptual models' ...."*

5.19 I understand Dr Mackay's term "macroscopic model" to refer to the regional hydrogeological model (PE/GNP/2, paragraph 2.14). The RCF has an important role in validating the regional model, as described in [paragraphs 8.79](#) to 8.88 of Dr Chaplow's Supplementary Proof of Evidence (PE/NRX/14/S1, pages 84 to 87).

#### **Point vii: Procedure for a Multiple Staged Investigation Process**

5.20 In Section 8 of his Proof of Evidence (PE/GNP/2), Dr Mackay advances a "multiple" staged (as opposed to "single" staged) procedure for site investigation and model validation. In particular, he asserts that (PE/GNP/2, paragraph 8.1):

*"Although a single stage of site investigation may yield the conceptual macroscopic model, if all data are used for its development, validation cannot be attempted. Consequently a staged investigation is preferable in which the results of each new investigation are used to examine the performance of the models previously developed from prior site investigations. This is a minimum requirement for validation in the weak sense of improving confidence in the conceptual model as investigation proceeds."*

5.21 By way of criticism of Nirex's programme, Dr Mackay then states in paragraph 10.6 of his Proof of Evidence (PE/GNP/2) that:

*"This raises the concern that criteria have not yet been established in advance to say what level of prediction is acceptable. It is not sufficient for Nirex to establish such criteria after the comparison has been made as this makes failure to recognise problems with the model predictions far more likely. There must be scientific peer review and open publication of such criteria before site characterisation proceeds."*

5.22 The Nirex approach, involving multiple validation stages, prior establishment of performance measures, and peer review is described in Section 10 of this Supplementary Proof of Evidence.

### **Point viii: The Development and Assessment of Alternative Conceptual Models**

5.23 Dr Mackay raises the issue of alternative conceptual models in paragraph 10.8 of his Proof of Evidence (PE/GNP/2):

*"There is no mechanism to ensure that alternative conceptual models are identified and explored. Nirex has produced results from only a single conceptual model of the hydrogeology of the site in their reports,..."*

and:

*"It is clear from the Harwell study described above that Nirex's focus on a single conceptual model limits their chances of providing improved confidence in the assessment predictions as site investigation proceeds."*

5.24 In paragraph 11.2 of his Proof of Evidence (PE/GNP/2), Dr Mackay concludes:

*"The method of demonstrating the adequacy of the conceptual model must be clearly and openly stated in advance of any construction of an RCF and should be agreed in advance with an appropriate authority. It is not apparent that this is the case at present. If such a method cannot be established then the development of the RCF should be rejected until such a method has been developed."*

5.25 The process of development of the hydrogeological conceptual models used as the 'base case' for risk calculations in *Nirex 95* [COR/522] was described in considerable detail in Volume 1 of that document. Section 6 of Volume 1 (page 6.1) states:

*"At that time it was also recognised that a number of alternative choices could have been made in the formulation of the model and that some of these alternatives could also lead to a model that was as compatible with the available data. The following issues were noted by the expert group in the course of the meetings held to formulate the conceptual model and are recorded here for reference and for possible consideration in later work.....Various mechanisms which could lead to the production of the observed head values were discussed by the group...A number of these possibilities, as well as the effect of explicitly imposing the observed head gradients on the base of the model were investigated in the variant calculations performed in *Nirex 95* (see Section 7 of Volume 3)."*

5.26 Section 10 of this Supplementary Proof of Evidence describes the Borehole RCF3 Pump Test. This exercise has been used to compare six alternative conceptual models of groundwater flow within the BVG.

5.27 Thus, there is ample evidence to contradict DrMackay's statements on alternative conceptual models as reproduced at paragraph5.23 of this Supplementary Proof.

5.28 The Nirex validation approach, as described in Section 10 of this Supplementary Proof of Evidence, meets the concerns expressed by DrMackay at paragraph 11.2 of his Proof of Evidence (PE/GNP/2) as reproduced at paragraph 5.24 above. Hence his reservation regarding the development of the RCF is unfounded.

5.29 The adequacy of models used in repository post-closure performance assessments that will be submitted in support of an application for authorisation under RSA 93 will be a matter for the appropriate regulatory bodies.

Nirex expects the regulator to have an interest in the methods that were used to test the adequacy of the relevant models; models which will almost certainly be different from those that have been developed to date, and plans to present an appropriate submission containing such information. The RCF is an essential facility for the calibration, validation and improvement of models to be used in post-closure performance assessments and is therefore integral to the method of demonstrating their adequacy to the regulator.

## Summary

5.30 In summary, I conclude that:

	i.	the Nirex Science Programme is designed to ensure that the problems encountered in the exercise carried out by Dr Mackay for HMIP will not arise; and
	ii.	Nirex's approach to model validation meets the concerns identified by Dr Mackay.

## 6. THE NIREX REPOSITORY CONCEPT

6.1 Dr Allison, for Friends of the Earth, makes a number of points concerning the engineered barriers in the Nirex repository concept and the relationship of the concept to the experimental programme proposed for the RCF in Sections 4, 5 and 6 of his Proof of Evidence (PE/FOE/7). He asserts that the safety of a repository at Sellafield will rely on the performance of the engineered barrier as a means of physical containment (PE/FOE/7, paragraphs 4.14 and 4.15). Dr Wogelius, also for Friends of the Earth, makes points concerning the value to the Nirex repository concept of geochemical controls on radionuclide migration in Section 4 of his Proof of Evidence (PE/FOE/8). He asserts that the engineered barrier cannot be relied upon to meet the long-term safety requirements of a radioactive waste repository (PE/FOE/8, paragraph 4.2) and that the beneficial operation of geochemical processes will be essential. Dr Salmon in Section 4 of his Proof of Evidence (PE/FOE/5) raises similar points to those of Dr Allison in respect of sealing of RCF shafts and the potential impact on repository performance.

6.2 The points which Dr Allison raises in his Proof of Evidence (PE/FOE/7) in relation to:

siting and future use of the RCF (paragraphs 4.36 and 4.44 to 4.54);

methods of excavation of the RCF (paragraphs 4.37 and 4.55 to 4.60); and

design of rock support for the RCF shafts and galleries (paragraphs 4.38 and 4.61 to 4.68)

will be dealt with in a Supplementary Proof of Evidence from Dr Mellor ([PE/NRX/16/S1](#)).

6.3 The remaining points which Dr Allison raises in Sections 4, 5 and 6 of his Proof of Evidence (PE/FOE/7) together with Dr Salmon's concerns on sealing are dealt with in this Section of my Supplementary Proof of Evidence.

6.4 The points which Dr Wogelius raises on geochemical processes and related experiments in the RCF in Section 5 to 9 of his Proof of Evidence (PE/FOE/8) are dealt with in Section 7 of my Supplementary Proof of Evidence.

6.5 The points which Dr Wogelius raises in Section 4 of his Proof of Evidence (PE/FOE/8) are dealt with in this Section of my Supplementary Proof of Evidence.

6.6 The overall theme of Sections 4, 5 and 6 of Dr Allison's Proof of Evidence (PE/FOE/7), as set out at paragraphs 4.14 to 4.16, is that, in view of the current picture of the hydrogeology at Sellafield, the engineered barriers for a repository at Sellafield require an advanced level of development. In particular, he asserts (PE/FOE/7, paragraph 4.14):

*"Nirex's evidence, particularly that of Dr Chaplow [[PE/NRX/14](#)] and Dr Hooper [[PE/NRX/15](#)], demonstrates that the geological setting at the Sellafield site is complex and that the rocks within the PRZ are variable, both in terms of their intrinsic properties (smallscale) and mass properties (largescale). By*

*comparison with the attributes of the ideal repository site, it is evident that the degree to which uncertainties can be resolved at Sellafield, even through construction of an RCF, is itself uncertain."*

and goes on to say (PE/FOE/7, paragraph 4.15):

*"In these circumstances, the multibarrier philosophy indicates that the level of attention to be focused on engineered barriers should assume a particularly high level of importance in studies directed towards the planning of a final repository at Sellafield".*

6.7 Dr Allison is wrong to make reference to "the ideal repository site". As explained by Dr Chaplow (PE/NRX/14/S1, [paragraphs 5.5](#), 5.6 and 5.7) in response to this statement by Dr Allison, there is no basis for citing "complexity" as an issue either from the point of view of Nirex, provided that it is willing to deploy the necessary resources to generate an adequate understanding, or from the point of view of HMIP, provided that "a complex geological structure" was "addressed thoroughly in any future application" (PE/HMP/1, page 9, paragraph 4.9).

6.8 I believe that the "level of attention to be focused on engineered barriers" should assume a "high level of importance" at any repository site. Further, I believe that the engineered barriers should be designed to take account both of the wastes to be disposed of and of the characteristics of the site, and to be in accordance with the regulatory requirements for the adoption of best practicable means, as laid down in "the July 1995 White Paper" (page 22, paragraph 78) [GOV/208].

6.9 It is clear that Dr Allison has based his thesis upon an incorrect reading of "Euradwaste series No 6" which he cites (FOE/7/14 page 20, paragraph 4.13) as follows:

*"The site should be such that significant water outlets surrounding the repository can be easily identified and the water pathways can be modelled."*

This citation occurs within the section of the EC report dealing with "near surface repositories" [FOE/7/14, page 7] "for short lived, low and medium level waste" [PE/FOE/7, page 5] and is therefore inappropriate to a deep geological repository.

6.10 The overall theme of his evidence in general and in particular Section 4 of Dr Wogelius' Proof of Evidence (PE/FOE/8), as set out in paragraph 4.2, is that in view of "the limitations of the engineered barrier, the long-term safety requirements of a radioactive waste repository make it necessary for the geological setting of a proposed site to demonstrably contribute to the immobilisation of the radionuclides."

6.11 It is clear that Dr Wogelius has based his thesis upon *Brady (1994)* (FOE/8/1), a paper submitted to the scientific journal *Waste Management* but not yet accepted for publication. The statements abstracted from the submitted paper in paragraph 4.1 of Dr Wogelius' Proof of Evidence (PE/FOE/8) have no relevance to a deep geological repository since the paper deals with "near-surface disposal facilities" (page 3) (FOE/8/1).

6.12 However, I am in full agreement with the general principles expressed at page 5 of *Brady (1994)* (FOE/8/1):

*"Using cement-based material as a waste form or backfill material may potentially lead to a simpler, more defensible performance assessment for a number of reasons....Hydrogeochemical modelling of radionuclide mobility in the near-field is much more straight forward if the pH is known to be "fixed" by hydrolysis of cement".*

This is a concise statement of a principal reason for the selection by Nirex of a cementitious backfill for a deep repository, Nirex Reference Vault Backfill.

6.13 I am unable to reconcile the theme of Dr Allison for Friends of the Earth, that great reliance must be placed on engineered physical barriers, since (PE/FOE/7, paragraph 5.7):

*"Physical systems reach states of equilibria which are much more readily identified than the geochemical 'equilibria' which evolve within the repository environment"*

with the theme of Dr Wogelius, also for Friends of the Earth, that reliance must be placed on geochemical processes because, he says (PE/FOE/8, paragraph 4.2), of:

*"...the limitations of the engineered barrier..."*.

6.14 Furthermore, I reject both these themes because they both over-emphasise the importance of the performance of one component of the multi-barrier containment system. I have laid down the principles of multi-barrier containment in the Nirex repository concept in [paragraphs 4.3](#) to 4.6 of my Proof of Evidence (PE/NRX/15). The Nirex repository concept has been developed to take account of the volumes and natures of the wastes that it is intended to contain and the statements repeated in "*the July 1995 White Paper*" (pages 22 and 23, paragraphs 80 and 81) [GOV/208] concerning the unlimited period into the future over which risk should be assessed.

6.15 Good physical containment, as proposed by Dr Allison (PE/FOE/7), and good chemical containment, as proposed by Dr Wogelius (PE/FOE/8), are afforded by the barriers of the Nirex repository concept. The time-frames over which these barriers can be relied upon has been taken into account. The longer term reliance on chemical containment, rather than complete physical containment, is supported in principle by Dr Wogelius who states (PE/FOE/8, paragraph 4.2):

*"However, geochemical processes may act to keep radionuclides in solution low and effectively limit the release of radionuclides even after the engineered barriers have failed."*

6.16 The specific assertions which Dr Allison makes in Sections 4, 5 and 6 of his Proof of Evidence (PE/FOE/7) can be summarised as follows:

- i. there are uncertainties about the performance of the containment concept that will not be adequately addressed by the proposed programme of works in the RCF (paragraphs 4.30 to 4.32); and
- ii. inadequate attention is being paid to the design of seals for the RCF (paragraphs 4.69 to 4.80) and for the repository access and underground excavations, other than the disposal vaults (Section 5), and inadequate attention is being paid to "*quality objectives*" and large-scale tests of backfilling and sealing designs (Section 6).

6.17 The specific assertions that Dr Wogelius makes in paragraphs 4.5 to 4.9 of his Proof of Evidence (PE/FOE/8) can also be summarised as referring to uncertainties about the performance of the containment concept, as at i. in paragraph 6.16 above. My response to point i is given at paragraphs 6.18 to 6.44 below, and to point ii at paragraphs 6.45 to 6.94 below.

### **Point i: Uncertainties about Performance of the Containment Concept**

6.18 I respond in detail to these points below but my overall response is that:

the uncertainties which Dr Allison lists are being addressed by the long-established Nirex Safety Assessment Research Programme or will be addressed by the RCF; and the assertions which Dr Wogelius makes are based on incorrect assumptions concerning the Nirex repository concept and assessment of its long-term performance that I identify.

6.19 In paragraphs 4.30 and 4.32 of his Proof of Evidence (PE/FOE/7), Dr Allison asserts that there are uncertainties about the performance of the containment concept that will not be adequately addressed by the proposed programme of works in the RCF. These fall into two categories:

- areas of uncertainty which Nirex has identified - as raised by Dr Allison (PE/FOE/7); and
- areas of uncertainty which Dr Allison suggests Nirex has overlooked.

I deal with these in turn in paragraphs 6.20 to 6.24 and 6.25 to 6.33 below

Areas of Uncertainty which Nirex has Identified

6.20 At paragraph 4.30 of his Proof of Evidence (PE/FOE/7), Dr Allison sets out a list of uncertainties which, in the main, he has extracted from my original proof ([PE/NRX/15](#)) without further comment. However, in two instances he elaborates on the original point.

6.21 In the third bullet point of paragraph 4.30 (PE/FOE/7), Dr Allison refers to the uncertainty concerning:

*"the reliability of the predicted groundwater flow through the repository of 140m<sup>3</sup> per year and the risk that higher rates of flow (approximately five times greater) could deplete the buffering properties of the backfill to an extent which undermines confidence in postclosure safety performance [[PE/NRX/15](#), [paragraphs 5.21](#), 5.22, 5.23, [5.51](#)]."*

6.22 Dr Allison does not give any justification for considering a rate of flow approximately five times greater than the figure of 140m<sup>3</sup> per year given in [paragraph 5.21](#) of my Proof of Evidence ([PE/NRX/15](#)). However, as I note in that paragraph, simple calculation shows that for a flow rate of 140m<sup>3</sup> per year the pH of the porewater in the near-field of the repository would be maintained at a value greater than the desired value of 10 for many millions of years. The flow rate would have to be considerably greater before it "could deplete the buffering properties of the backfill to an extent which undermines confidence in post-closure safety performance". If such a concern existed, the repository design and in particular the quantity of backfill would be optimised in order to ensure that the pH buffering capacity of the repository would be maintained for an adequately long period of time. Dr Allison is therefore wrong to raise this as an uncertainty.

6.23 In the fourth bullet point of paragraph 4.30 (PE/FOE/7), Dr Allison refers to the uncertainty concerning:

*"the extent to which spreading and dilution in the 'far field' meets the predicted dilution of one part in two thousand [[PE/NRX/15](#), [paragraphs 4.5](#), 4.6, [5.28](#), 5.29, [5.52](#), [6.17](#)] - this implies that no express route for groundwater will occur within the 'nearfield'."*

6.24 The extent to which spreading and dilution in the "far-field" meets the predicted dilution of one part in two thousand is an important area of uncertainty. The RCF is needed to address this. The dilution factor of one part in two thousand is not given in the paragraphs of my Proof of Evidence ([PE/NRX/15](#)) that are cited by Dr Allison but is that which applies in calculations for the inventory-limited radionuclides chlorine-36 and iodine 129 in *Nirex 95* (Volume 3, Section 2.3, page 2.13) [COR/522]. However, the relevance of an express route for groundwater within the "near-field" is not clear. An "express route" implies a preferential flow path which would result in the groundwater that would move along such a path having access to only a small fraction of the total inventory of radionuclides. The flow of groundwater through the repository would thus contain a lower dissolved level of radionuclides. Since dilution is afforded by the flow in the overlying sandstones, which is independent of flows within the repository, a higher dilution factor would result. *Nirex Safety Assessment Research Programme, Nirex Near-Field Research: Report on Current Status in 1994, July 1995 ("Nirex Report S/95/011")* (Figure 9, page 57) [COR/529] illustrates results where the iodine-129 inventory is released more rapidly from the repository near field when using a homogeneous model of a repository vault with groundwater flow accessing the entire volume of the vault, as compared with models containing various preferential flow paths. Therefore, I conclude that Dr Allison is wrong in raising the issue of an "express route" within the near field as a means of lowering radionuclide dilution.

Areas of Uncertainty which Nirex has Allegedly Overlooked - as Raised by Dr Allison (PE/FOE/7)

6.25 In paragraph 4.32 of his Proof of Evidence (PE/FOE/7), Dr Allison sets out a list of uncertainties which he says are not addressed by Nirex. In the following paragraphs 6.26 to 6.33 I address, in turn, each of the uncertainties he identifies.

6.26 Uncertainties arising from: "the rate of generation and dispersion of gas within the 'nearfield' as a function of the rate of groundwater flow, the spatial arrangements within the backfilled vaults, and the possibility of utilising the generation of gas overpressures as a means of preventing groundwater ingress." (PE/FOE/7, paragraph 4.32, point 1) have been addressed by Nirex. In particular:

*Nirex Report NSS/G120, NSARP Reference Document, Gas Generation and Migration January 1992 [NRX/15/5]* states on page 18:

*"The gas generation model couples gas generation from the corrosion of metals and from the microbial degradation of cellulosic wastes. This model is encoded in the GAMMON computer program"*

and on page 22:

*"GAMMON has been developed to take account of the movement of groundwater through the repository environment.";*

*Nirex Report S/95/011* (Sub-section 5.23, page 57, entitled "*Modelling of interactions between packages or between vaults*") [COR/529] describes the work that has been carried out on the spatial arrangements within the vaults; and the possibility of utilising the generation of gas over-pressures as a means of preventing groundwater ingress has been considered and rejected as a concept which could not be demonstrated to give any benefit in terms of radiological protection.

Therefore, I conclude that Dr Allison is wrong to assert that these are "*uncertainties not addressed by Nirex*".

6.27 Uncertainties arising from the: "*means by which the vault backfill will be introduced to fill all the voids of the waste emplacement chambers, including those at the crown of the vaults.*" (PE/FOE/7, paragraph 4.32, point 2) have been addressed by Nirex. Indeed, Dr Allison quotes the relevant sections of the *Nirex Vault Backfill* (FOE/7/17) Patent Application in paragraph 5.30 of his Proof of Evidence (PE/FOE/7) :

*"The backfilling material slurry is suitable for mixing, handling, pumping and remove [sic - "remote" in original document] vault filling operations. The slurry is self-levelling and compacting and able to infill the spaces between waste packages. Bleed should not be greater than 2% to minimise the formation of voids at waste package interfaces.*

*The backfilling material slurry may be mixed underground at a mixing station within the repository vault. The vault slurry could be pumped directly along a long pipeline for placement in the vault as required, or pumped into tanks and transported into the vault."*

In *Hooper (1995)* (pages 8 and 9 and Figure 5) [GNP/3/5], a large-scale test is reported which showed that the Nirex Reference Vault Backfill satisfied aspects of the specification that Dr Allison claims are not addressed. Finally, the models of the repository near field used in *Nirex 95* (Sub-section 3.2.1, page 3.2) [COR/522] assumed that the vaults have open crown spaces. This has been shown to be a beneficial arrangement in reducing radionuclide release in work carried out to date and illustrated in *Nirex Report S/95/011* (Figure 9, page 57) [COR/529].

Therefore, I conclude that Dr Allison is unjustified in claiming that these are "*uncertainties not addressed by Nirex*".

6.28 Uncertainties arising from the: "*means of enhancing radionuclide retention within the waste emplacement vaults, by physical restriction of groundwater flow through the repository backfill.*" (PE/FOE/7, paragraph 4.32, point 3) are being addressed. Referring to the main channels for groundwater flow in the BVG, *Nirex Report 525* (page 43) [COR/505] states:

*"In practice, of course, if a repository were constructed at Sellafield at some time in the future, then considerably more would be known about the hydrogeological properties of the rocks in the Potential Repository Zone than at present. In particular, the intention would be to establish the frequencies and specific positions of zones of locally enhanced conductivity. The repository could be designed to avoid them or, if found to intersect vaults, it may be possible to effectively seal them off. Meanwhile the conservative assumption made in current models is that unsealed zones of locally enhanced conductivity cross some of the vaults. This is likely to over-estimate risks."*

In my Proof of Evidence ([PE/NRX/15](#), page 56) I identify the features that Nirex wishes to consider sealing and for which work in the RCF is required. The first feature is given as :

*"flowing fractures intersecting repository vaults (which are assumed not to be sealed in current models);"*

Therefore, I conclude that Dr Allison is wrong to claim that there are *"uncertainties not being addressed by Nirex"* and I identify that the RCF is planned to play an important role in their resolution.

6.29 Uncertainties arising from: *"the types of materials and designs for special seals which can prevent development of preferential flowpaths through the disturbed rock, through imperfections in backfilled openings, and at the interface between the BVG and the overlying materials which are penetrated by access routes to the emplacement horizon."* (PE/FOE/7, paragraph 4.32, point 4) are being addressed, as is made clear in [paragraph 7.2](#) of my Proof of Evidence ([PE/NRX/15](#)).

6.30 Uncertainties arising from: *"the merits of alternative methods of rock excavation in limiting the amount of peripheral rock disturbance."* (PE/FOE/7, paragraph 4.32, point 5) can only be addressed when excavation disturbance of the rocks at Sellafield has been investigated through the RCF. In the interim Nirex has sought to gain an understanding of the relevant principles through participation in the international Äspö Hard Rock Laboratory Project. In *GEOVAL '94 Validation Through Model Testing, Proceedings of an NEA/SKI Symposium Paris, France, 11-14 October 1994* [NRX/15/6] it is stated (page 24):

*"To obtain a better understanding of the properties of the disturbed zone and its dependence on the method of excavation ANDRA, Nirex and SKB decided through their participation in the international Äspö Hard Rock Laboratory Project, to perform a joint study of disturbed zone effects. The project is named ZEDEX (Zone of Excavation Disturbance Experiment)."*

6.31 Uncertainties arising from: *"the longterm stability of repository openings including shafts and galleries, and the need for linings and/or backfills to provide the rock support that is required."* (PE/FOE/7, paragraph 4.32, point 6) are being addressed, as set out in [paragraph 8.11](#) of Dr Chaplow's Proof of Evidence ([PE/NRX/14](#)) and references cited therein.

6.32 Uncertainties arising from: *"the need to verify that the selection of concrete as a material for backfilling the repository vaults and for lining the RCF shaft is justified in terms of material longevity"* (PE/FOE/7, paragraph 4.32, point 7) are being addressed in respect of the backfill and of the hydrostatic lining to be placed in the upper section of the RCF shaft. *"The selection of concrete as a material for backfilling the repository vaults"* is taken to refer to the Nirex Reference Vault Backfill. The *"material longevity"* is covered in part by tests simulating long-term exposure to groundwater in the BVG rocks of the PRZ at Sellafield, the results of which were summarised in *Hooper (1995)* (page 9) (GNP/3/5) as follows:

*"Long-term buffering properties substantially exceed those required in accelerated groundwater leaching tests;*

*The cured mix retains high sorption capacity for sorbing radionuclides initially dissolved in pore water.*

*The mix specimens subjected to accelerated leaching and groundwater reactions retained physical integrity and a suitably high permeability to gas (to allow gas formed in the repository to escape without overpressurising the material)."*

Further evidence is obtained from natural analogues of the key components of the Nirex Reference Vault Backfill and in particular the natural analogue at Maqarin in Jordan where the mineral phase calcium silicate hydrate gel, which is responsible for the later stages of the repository long-term alkali buffering and sorption properties, has been stable in relatively high groundwater flows for tens of thousands of years. The longevity of the shaft lining in terms of its hydraulic properties requires to satisfy a relatively undemanding standard, given its location, well above any potential repository horizon, and the information on sealing standards required throughout the shaft in [paragraph 7.2](#) of my Proof of Evidence ([PE/NRX/15](#)). Finally, I note in my Proof of Evidence ([PE/NRX/15](#), [paragraph 5.23](#)) with respect to long term reactions with the backfill that account is taken of the resulting

uncertainties in current safety assessment studies. Again, I must conclude that Dr Allison's assertion that these are "*uncertainties not being addressed by Nirex*" is unjustified.

6.33 Uncertainties arising from: "*the need to carry out confirmation trials on backfilling and sealing for which practical operational methods have already been tried, tested and validated, and shown to be capable of producing consistently a prescribed set of quality objectives.*" (PE/FOE/7, paragraph 4.32, point 8) are addressed by the strategy for sealing studies proposed for the RCF in my Proof of Evidence (PE/NRX/15, [paragraphs 6.68](#) and 6.69). In the case of backfilling as indicated in the RCF Environmental Statement [COR/101] at paragraph 1.52, a backfilling trial is under consideration for the RCF. However, it may be more appropriate to carry out such trials in a surface-based facility or as part of non-radioactive commissioning trials when a repository vault has been constructed.

Matters raised by Dr Wogelius (PE/FOE/8)

6.34 In paragraphs 4.5 to 4.9 of his Proof of Evidence (PE/FOE/8), Dr Wogelius asserts that there are inadequacies in the performance of the Nirex repository concept in respect of:

enhanced transport conditions for radionuclide release as a consequence of the "high permeability" grouting material (paragraph 4.5, PE/FOE/8) and;

inadequate dilution of radionuclides that do not have a low solubility under repository conditions.

I deal with these in turn in paragraphs 6.35 to 6.38 and 6.39 to 6.44 below.

Enhanced Transport Conditions

6.35 Referring to the Nirex Reference Vault Backfill, Dr Wogelius (PE/FOE/8, paragraph 4.5), notes that it has been selected to provide "*relatively* [my emphasis] *high permeability and porosity*" and he lists some, but not all, the reasons for Nirex's requiring these properties. He then goes on to state, omitting the word "*relatively*":

*"This high permeability will also provide enhanced transport conditions for radionuclide release."*

This is wrong because the release of radionuclides dissolved in the porewater of the backfill will be determined by the rate of groundwater flow through the near field of the repository, which will in turn be determined by the hydrogeological characteristics of the host rock.

6.36 Dr Wogelius omitted to note that one of the reasons given in my Proof of Evidence (PE/NRX/15, [paragraph 5.18](#)), for selecting a relatively high porosity material is:

*"long-term maintenance of a high active surface-area for sorption of key radionuclides"*.

This is a powerful process for the retardation of release of many radionuclides, as noted in the paper *Brady (1994)* (pages 13 and 14) [FOE/8/1] that Dr Wogelius cites.

6.37 The backfill is not "*highly permeable*" but does have a "*relatively high permeability*" in comparison with structural concrete and the BVG. The permeability of the backfill has been measured as  $10^{-16} \text{ m}^2$  (*Nirex Report NSS/309, The Mass Transport Properties of Cementitious Materials for Radioactive Waste Repository Construction, July 1995* (page 6) [NRX/15/7]) (equivalent to a hydraulic conductivity of  $10^{-9} \text{ m s}^{-1}$ ), some orders of magnitude less permeable than sands and gravels for example.

6.38 In conclusion, Dr Wogelius seems not to have understood the nature of the Nirex Reference Vault Backfill and some of the processes that will contribute to its performance in containing radionuclides within the repository. Far from enhancing "*transport conditions for radionuclide release*", the Nirex Reference Vault Backfill will reduce such a release, as it is designed to do.

Inadequate Dilution

6.39 Referring to the role of dilution in solving "*the problem*" of radionuclides that do not have a low solubility under repository conditions, Dr Wogelius (PE/FOE/8, paragraph 4.9) uses dose-based limiting concentrations (DBLC's) to imply that the dilution factors cited in my Proof of Evidence (PE/NRX/15, [paragraph 5.32](#)) gave a misleading impression of safety.

6.40 In order to make this point, Dr Wogelius focuses on some calculations for the radionuclide caesium-137 (PE/FOE/8, paragraph 4.9), stating that the dilution argument has been applied in evaluating the safety of disposal of this radionuclide. This is not the case. Caesium-137 is contained within the repository system by physical containment within the waste containers and additional containment would be afforded by sorption in the near field and the geosphere as explained in *Nirex 95* (Volume 3, Sub-section 8.1, pages 8.1 to 8.4).

6.41 The safety of disposal of the radionuclides chlorine-36 and iodine-129 does rely on the extent of dilution in near-surface groundwaters. The calculated risks arising from these radionuclides given in my Proof of Evidence (PE/NRX/15, [paragraph 5.37](#)) are consistent with the dilution factor of about 2,000 (PE/NRX/15, [paragraph 5.23](#)).

6.42 The argument presented by Dr Wogelius is entirely inappropriate to evaluations of safety of deep geological disposal of radioactive wastes. For a deep geological repository it is appropriate to take account of some delay in time between disposal and the first possible radiological exposure of people. Taking a notional total containment time for the disposal inventory of caesium-137 (PE/NRX/15, [Table 5.1](#)) of just one thousand years to account for physical containment in the waste containers and travel through the geosphere, the dilution factor of 2,000 would lead to a concentration of caesium-137 in groundwater emerging into the biosphere approximately one thousandth of Dr Wogelius' DBLC.

6.43 For this simplified containment model, the radioactive decay of the caesium-137 in one thousand years accounts for most of the reduction in the concentration in the groundwater. This gives a simplified example of the merits of deep geological disposal of radioactive wastes.

6.44 Dr Wogelius' statements on dilution are irrelevant because he has made unjustified assumptions about the principles of deep geological disposal both of caesium-137 in particular and of radioactive wastes in general.

### **Point ii: RCF and Repository Sealing and Backfilling**

6.45 In his Proof of Evidence (PE/FOE/7), Dr Allison asserts that inadequate attention has been given by Nirex to repository backfilling and sealing issues in relation to:

waste emplacement and backfilling tests to be carried out in the RCF (paragraphs 4.41 to 4.43 and paragraphs 4.69 to 4.80);

general principles of repository backfilling and sealing (paragraphs 5.1 to 5.10);

sealing shafts and galleries (paragraphs 5.13 to 5.26);

the Nirex Reference Vault Backfill (paragraphs 5.27 to 5.38);

gas generation in the repository (paragraphs 5.39 to 5.41);

"*peripheral areas*" of the repository (paragraphs 5.48 to 5.42); and

"*special seals*" in the repository (paragraphs 5.53 to 5.55).

6.46 Given the wide range of Dr Allison's comments in these areas, it is not appropriate to attempt to summarise my response to each of his points at this stage. Rather, I deal with each of these areas in turn, and the detailed points raised in relation to them below.

6.47 In respect of the third bullet, Dr Salmon at paragraphs 4.20 to 4.26 of his Proof of Evidence (PE/FOE/5) also raises issues concerned with the sealing of the RCF and the potential impact on the repository safety case. Dr

Salmon concerns are addressed, along with the response to Dr Allison's third bullet point at paragraphs 6.56 to 6.61 below.

#### Waste Emplacement and Backfilling Tests in the RCF

6.48 Considering waste emplacement and backfilling operations in paragraphs 4.41 to 4.43 of his Proof of Evidence (PE/FOE/7), Dr Allison concludes:

*"A substantial programme of background generic research in this area is required prior to the selection of best practical methods for verification within an RCF. Nirex gives no identification that this research work has been carried out."*

6.49 I have already described in paragraph 6.27 of this Supplementary Proof of Evidence the large-scale test work that has been carried out. *Hooper (1995)* (page 7) (GNP/3/5), discussing the Nirex Reference Vault Backfill Development Programme, states:

*"Initially TEL carried out a desk study, supported by bench-top trials on ninety-six formulations covering combinations of OPC, lime, PFA, ground granulated blast-furnace slag, sodium silicate, bentonite and filler (limestone flour), to identify formulations showing sufficient promise to be carried forward into a laboratory testing programme."*

thereby summarising a key stage in a substantial development programme that has been underway for over six years in direct contradiction of the assertion made by Dr Allison.

#### General Principles of Repository Backfilling and Sealing

6.50 In paragraphs 5.1 to 5.10 of his Proof of Evidence (PE/FOE/7), Dr Allison makes a number of general comments about repository seals which I set out and deal with in turn below.

6.51 In paragraph 5.4 of his Proof of Evidence (PE/FOE/7), Dr Allison asserts that:

*"...in general it is tacitly assumed that the backfilling and sealing system within the 'redundant' areas will (as a minimum) provide containment properties equal to those of the undisturbed natural barrier [FOE/7/6]."*

I have searched the reference (FOE/7/6) for evidence of a tacit assumption but found no information to justify this proposal.

6.52 In paragraph 5.5 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"The fact that safety assessment models may take no credit for the physical contribution of backfilling and sealing systems to containment of radionuclides within the 'active' zone, does not mean that this aspect may be ignored [FOE/7/7]. Indeed, within the waste emplacement areas, the backfilling and sealing system should seek to achieve:*

*maximum delay to the establishment of saturation conditions;*

*minimum rate of groundwater through-flow in order to limit the rate of replenishment of corrosive agents and the outflow of contaminated water; and*

*high levels of effectiveness in eliminating interface flow paths and peripheral flow paths (see Figure 4.3)."*

Dr Allison acknowledges that the benefit of meeting these requirements may not be reflected in assessments of radionuclide containment. I conclude that Dr Allison's prescriptions are not shown to be relevant to affording radiological protection.

6.53 In paragraph 5.8 of his Proof of Evidence (PE/FOE/7), Dr Allison proposes that:

*"One of the fundamental objectives of Nirex's programme should be to validate best practicable means of achieving the maximum containment of radionuclides at or near to the point of burial, using engineered backfilling and sealing systems."*

This is a different objective to that given by the regulatory authorities in *the 1995 HMIP Consultation Draft* (page 23, paragraph 6.15) [HMIP/I/1]:

*"Requirement R3 - use of best practicable means*

*The best practicable means shall be employed to ensure that any radioactivity coming from a facility will be such that doses to members of the public and risks to future populations are as low as reasonably achievable"*

Nirex works to the objective of satisfying this requirement in respect of best practicable means rather than to alternative prescriptions offered in this instance, by Dr Allison, which therefore are not relevant.

6.54 In paragraph 5.9 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"A pre-requisite for effective backfilling and sealing of radioactive waste repositories is that there is demonstrable evidence of the longevity of the materials used for this purpose [FOE/7/2, FOE/7/7, FOE7/10]. Considering the source term for the models described in Dr Hooper's evidence [[PE/NRX/15](#)], 'near-field' barriers are presumed to contribute to repository performance over a period of up to one million years (albeit only as a geochemical barrier). Backfilling and sealing systems within 'redundant' parts of the repository will be required to function as a physical barrier over even longer periods if preferential flowpaths are to be permanently eliminated."*

It was made clear in my Proof of Evidence ([PE/NRX/15](#), [paragraph 7.2](#)) that shafts have been evaluated as if sealed against the flow of groundwater to only a very low standard, resulting in the calculation of no detrimental effect on transport of radionuclides. Therefore, there is no justification for the condition proposed by Dr Allison.

6.55 In paragraphs 5.10 of his Proof of Evidence (PE/FOE/7), Dr Allison states:

*"...Apart from the Nirex Reference Vault Backfill, the proposals in Nirex's evidence do not identify the various materials to be used for backfilling and sealing, and have not provided evidence that the appropriate longevity criteria will be met."*

I dealt with the issue of longevity in paragraph 6.32 of this Supplementary Proof of Evidence. "Criteria" would be expected to be developed in relation to a firm repository design proposal, which requires inputs of information from the RCF.

### Sealing of Shafts and Infilling of Galleries

6.56 In paragraphs 5.13 to 5.26 of his Proof of Evidence (PE/FOE/7), Dr Allison sets out a number of general observations in relation to the sealing of shafts and infilling of galleries. I outlined the Nirex work in relation to these aspects in paragraphs 6.29 and 6.27 respectively of this Supplementary Proof.

6.57 At paragraphs 4.20 to 4.26 of his Proof of Evidence (PE/FOE/5), Dr Salmon discusses the potential "Damage of the Repository Safety Case" that could arise from construction of the RCF if the shafts were not subsequently sealed adequately. At paragraph 4.22 he refers to the scoping studies of the impact of underground investigations on the performance of a repository discussed at [paragraph 6.47](#) of my Proof of Evidence ([PE/NRX/15](#)). At paragraph 4.20 Dr Salmon indicates, "No details of these scoping studies or references are provided".

6.58 At paragraph 4.24 of PE/FOE/5 Dr Salmon points to apparent contradictions between Nirex witnesses in respect of the impact of the RCF, and at paragraph 4.26 he concludes:

*"It therefore appears that Nirex has prematurely dismissed the possibility of the RCF damaging the repository PCPA, or at least has not provided sufficient evidence for independent peer review".*

6.59 The scoping studies referred to at [paragraph 6.47](#) (and also at [paragraph 7.2](#) and [7.7](#)) of my Proof of Evidence (PE/NRX/15) are described in *Nirex Report 560* (the conclusions of this report comprise reference (NRX/15/19). This report has now been made available for reference at the Greengarth Library of Nirex Science Programme reports, so meeting Dr Salmon's concerns regarding the availability of details of the scoping studies.

6.60 The scoping studies described in *Nirex Report 560* (NRX/15/19) considered the consequences for predicted post-closure radiological risk of varying the performance (in effect, the permeability) of drift and shaft seals for a repository in the PRZ at Sellafield which had incorporated the RCF shafts as ventilation shafts. The studies established that provided a seal permeability  $10^{-14} \text{ m}^2$  could be achieved falling well within the range of values expected for sandstones as shown in Table 2.2, page 29 of the textbook "*Groundwater*" by P A Freeze and J A Cherry (FOE/4/5), then there was no significant difference in predicted risk compared to the reference case in which the shafts and drifts were neglected. It is considered that this permeability should be achievable, although as recognised by Dr Salmon at paragraph 4.25 of PE/FOE/5, the RCF has an important role in demonstrating that seals of the appropriate quality can be emplaced.

6.61 The scoping studies have provided us with confidence that the RCF shafts should not compromise the repository safety case, but, as indicated at [paragraph 7.1](#) of my Proof of Evidence (PE/NRX/15), this is ultimately a matter on which Nirex will have to satisfy the regulators at the appropriate stage of an authorisation procedure under RSA 93. That safety case will be informed by tests in the RCF particularly in respect of the nature of the excavation disturbance zone, the characteristics of the networks of connected fractures, and the sealing tests. There are no contradictions in the evidence of the Nirex witnesses as claimed by Dr Salmon at paragraph 4.24 of PE/FOE/5. We have not "*prematurely dismissed the possibility of the RCF damaging the repository PCPA*" as claimed by Dr Salmon at paragraph 4.26 of PE/FOE/5.

#### The Nirex Reference Vault Backfill

6.62 In paragraph 5.32 of his Proof of Evidence (PE/FOE/7), Dr Allison sets out a series of "*areas of concern*" which I shall deal with in sequence below. I shall also deal with the points he makes at paragraphs 5.34, 5.36 and 5.38.

6.63 The concern that: "*Reliance has been placed upon geochemical conditioning and sorption as the only contribution to be made by the backfill to radionuclide retention*" (PE/FOE/7, paragraph 5.32, point 1) is a misleading generalisation which obscures the reductions in corrosion, gas evolution and natural colloid stability that result from the use of a cementitious backfill. However, the result of the two contributions identified by Dr Allison is exemplified by the chemical containment of the long-lived radionuclide plutonium-239 given in my Proof of Evidence (PE/NRX/15, [paragraphs 5.25](#) and [Figure 5.2](#)). With the operation of the backfill just one part in two thousand of the initial disposal inventory of plutonium-239 would be released from the repository near field. The Nirex Reference Vault Backfill will afford a high level of radionuclide containment and I conclude that Dr Allison's concern has no grounds.

6.64 The concern that: "*it is assumed that homogeneous geochemical conditions will be established within the backfill*" (PE/FOE/7, paragraph 5.32, point 2) is ill-founded since inhomogeneities in the repository near field are considered explicitly in a section devoted to the subject in *Nirex Report S/95/011* (Section 5, pages 53 to 58) [COR/529].

6.65 The concern that: "*The use of permeable material for the backfill would not impose significant delay to the saturation process and would not make any contribution to the reduction of groundwater flow through the repository*" is (PE/FOE/7, paragraph 5.32, point 3) not justified by any evidence that Dr Allison presents with respect to the Nirex Reference Vault Backfill. The permeability of the Nirex Reference Vault Backfill is approximately  $10^{-16} \text{ m}^2$ , which is not high in an absolute sense. A lower permeability material would not satisfy a number of the requirements of the vault backfill given in my Proof of Evidence (PE/NRX/15, [paragraph 5.18](#)) that are important to the eventual development of a safety case for a repository. Furthermore, a substantial delay in saturation as proposed by Dr Allison would allow the maintenance of aerobic conditions in the repository, which

would be detrimental to the corrosion-resistance of steel waste containers. Therefore, I conclude that Dr Allison would raise more concerns about repository safety if his proposals were implemented.

6.66 The concern that: "*Nirex has not identified any measures to eliminate the interface flowpaths and peripheral flow paths*" (PE/FOE/7, paragraph 5.32, point 4) does not recognise that the design concept giving calculated risks below the risk target in *Nirex 95*, Volume 3 [COR/522] has an unfilled vault crown space. I conclude on this basis that elimination of the flow paths identified by Dr Allison is not a significant issue in respect of safety.

6.67 The concern that: "*cementitious materials are prone to shrink rather than swell on setting*" (PE/FOE/7, paragraph 4.32, point 5) is a generalisation. Information in *OECD/NEA International Stripa Project 1980-1992, Overview Volume III Engineered Barriers, January 1993* ("*the Stripa Overview Report*") (pages 185 and 186) (NRX/15/8) shows that the generalisation is not true of cementitious materials specified for sealing purposes in contact with rock.

6.68 The concern that there is: "*the lack of consideration of the need to contribute to the support of the repository openings*" (PE/FOE/7, paragraph 4.32, point 6) is mis-directed to consideration of the Nirex Reference Vault Backfill. The support requirements are to be met by other means as described in *Nirex Report 800* (pages 1.5 and 1.9) (FOE/5/16).

6.69 The concern that: "*there is no clear evidence that the Nirex Reference Vault Backfill (which is not an analogue of a naturally-occurring material) will retain its anticipated physical and chemical properties over the timescales required.*" (PE/FOE/7, paragraph 4.32, point 7) is not borne out by the results of Nirex's long-term tests and natural analogue studies as described in paragraph 6.32 of this Supplementary Proof of Evidence.

6.70 The concern that: "*Nirex's patent for the Nirex Reference Vault Backfill acknowledges that the method is contrary to the backfilling and sealing concepts developed in other countries, where vault backfills are intended to provide a physical as well as a geochemical barrier.*" (PE/FOE/7, paragraph 5.34) is ill-founded because it is not true of all other countries, as described in paragraph 6.84 of this Supplementary Proof of Evidence and it may be taken to imply that Nirex believes the intended results will be obtained by others, whereas the Patent Application (page 2) (FOE/7/17) notes:

*"However in spite of proposals to backfill with impervious material, there remain concerns with the possibility of groundwater ingress".*

6.71 The concerns that: "*It is clear that the Nirex Reference Vault Backfill would make no contribution to the retention of the long-lived radionuclides chlorine-36 and iodine-129. Enhanced physical retention of these radionuclides and/or research on other methods of sorption are therefore warranted.*" (PE/FOE/7, paragraph 5.35) is not justified. In [paragraph 5.27](#) of my Proof of Evidence (PE/NRX/15) I state of chlorine-36 and iodine-129 that in experimental work to date they have been found not to sorb strongly to cement phases. However, taking this into account in assessment studies, the risks contributed by the natural discharge of these radionuclides are calculated to be below the risk target.

6.72 The concern that: "*Furthermore, it is clear that there is a relationship between the rate at which groundwater can flow through the repository and the rate at which aggressive chemical species are replenished to promote radionuclide release. The Nirex proposals fail to recognise any physical contribution which the vault backfill can make in this respect.*" (PE/FOE/7, paragraph 5.36) is wrong.

The Nirex Reference Vault Backfill is designed specifically to deal with this issue, amongst others. The nature of the "*aggressive chemical species*" proposed by Dr Allison is unclear but the major negatively charged species in groundwater in the BVG will not complex significantly with radionuclides provided that the Nirex Reference Vault Backfill buffers the water at high pH conditions.

6.73 The concern that: "*...there is no published evidence to demonstrate that Nirex has validated its [backfill]model through large-scale experiments or that it has completed a satisfactory programme of research on*

*alternatives.*" (PE/FOE/7, paragraph 5.38) is wrong. In paragraph 6.27 of this Supplementary Proof I draw attention to the publication of relevant elements of the Nirex Reference Vault Backfill Development Programme.

Gas Generation in the Repository.

6.74 In paragraph 5.39 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"In evidence given by Dr Holmes and Dr Mellor [[PE/NRX/15](#), [PE/NRX/16](#)], the release of gases (predominantly hydrogen and methane) is given as the reason why the vault backfilling material should be relatively permeable. It is suggested in their evidence that if backfill permeability were to be low, gas overpressures could be generated which could disrupt the backfill and the host rock, thus giving rise to the concentrated release of gases and the creation of detrimental groundwater pathways."*

6.75 Dr Allison's references are in error in this statement and it is not clear from where he draws his material. The correct position, as stated by Dr Holmes in his Proof of Evidence ([PE/NRX/13](#)) at [paragraph 4.8](#) is that:

*"The gas must not build up pressures that would damage the repository and surrounding rocks, and hence the backfill and surrounding rocks need to be sufficiently permeable to gas to permit it to escape from the vaults."*

Indeed, there is no link of the backfill permeability to disruption of the host rock as proposed by Dr Allison. The permeability to gas of the host rock itself will determine its response to gas pressures. There is no reference to a link to the concentrated release of gases. Indeed, it is reasonable to envisage that disruption of the rock mass could lead to a spreading of gas release rather than a concentration.

6.76 In paragraph 5.40 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"In examining this proposition at a conceptual level, I contend that it does not represent a well-engineered solution and that it presents Nirex with fundamental difficulties which remain to be resolved."*

He then sets out a number of "issues" which he states are "pertinent", the importance of which I shall deal with in turn below. Some appear to derive from the mis-conception he states in paragraph 5.39 of his Proof of Evidence (PE/FOE/7).

6.77 The issue that: "*reduced permeability of the backfill would reduce the flow of groundwater and hence would reduce the rate at which gas generation is promoted; conversely, increased backfill permeability would increase the rate of gas generation.*" (PE/FOE/7, paragraph 5.40, point 2) shows incorrect scientific reasoning. In fact, the predominant mechanism for the generation of gas is the corrosion of steels and resulting release of hydrogen. The corrosion and hence hydrogen gas generation is dependent upon the presence of water at the corroding steel surface; as long as the steel surface is in contact with a water-saturated medium, the corrosion rate will be fixed for a given set of chemical conditions. I am not aware of any material that could be used as a vault backfill that would preclude the contact between the steel surface and water. The Nirex Reference Vault Backfill controls the chemical environment in the repository near field so that corrosion is independent of the concentrations of salts dissolved in the groundwaters in the BVG of the PRZ at Sellafield. The use of the Nirex Reference Vault Backfill controls the rate of corrosion of carbon steels in BVG groundwater at a level some two orders of magnitude lower than for the rates observed at neutral pH. The permeability has been selected such that these chemical conditions will be present reliably at the steel surfaces. Therefore, I conclude that the approach adopted by Nirex will result in a lower rate of gas generation than is likely to be obtained by the approach proposed by Dr Allison.

6.78 The issue that: "*the creation of a highly permeable pathway for gas release within the vault backfill would have significant potential to create groundwater pathways in the rock due to the resultant interconnection of rock fractures*" (PE/FOE/7, paragraph 5.40, point 3) is not justified by any scientific basis presented by Dr Allison or that I can identify independently. The creation of groundwater pathways would be dependent upon the response of the rock mass to gas pressures and not upon transport processes within the backfill which I discuss in the following paragraphs.

6.79 The issue that: "*the pressure gradient which could drive any generated gas outwards depends on the size of the excavation, the amount of waste placed within it, and the relative permeabilities of the backfill and the surrounding rock*" (PE/FOE/7, paragraph 5.40, point 4) has no scientific basis of which I am aware. Dr Allison does not define the points between which the gradient, to which he refers, applies. I assume that he is referring to a pressure difference between the repository vault and hydrostatic pressure at the repository depth. When considering a relative overpressure within the repository vault, the maximum value of this overpressure will be determined by the ease with which gas can escape into the surrounding rock and not by any of the factors listed by Dr Allison. This is explained in *Nirex Report S/94/003* (pages 13 to 17) [COR/509] which shows that for calculations carried out on an initially unsaturated repository, the repository pressure does not rise above the hydrostatic pressure at the repository depth.

6.80 The issue that: "*the generation of a gas overpressure could serve to prevent the ingress of groundwater*" (PE/FOE/7, paragraph 5.40, point 7) is rejected in paragraph 6.26 of this Supplementary Proof of Evidence.

6.81 Summarising his identification of "*pertinent issues*" in paragraph 5.41 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"Collectively, the above points present a paradox whereby the Nirex proposals for promoting the release of gas from the backfilled repository would also serve to promote the rate of gas generation and the creation of groundwater pathways for radionuclide migration".*

I present evidence in paragraphs 6.77 to 6.80, in dealing with these issues, to show that there is no "*paradox*". The Nirex repository concept reduces the rates of gas generation and does not "*promote*" them and "*the creation of groundwater pathways*" is an unsupported assertion by Dr Allison.

6.82 Dr Allison continues in paragraph 5.41 of his Proof of Evidence (PE/FOE/7):

*"Furthermore, it would appear that the introduction of a lime-rich backfill (in the form of the Nirex Reference Vault Backfill) would serve to promote the generation of additional gases (predominantly carbon dioxide) from the backfill itself."*

Dr Allison is wrong in his assertion that gas generation would be "*promoted*". "*Lime water*" (which has a composition similar to cement porewater) extracts carbon dioxide from the gas phase. In correcting this erroneous statement I draw attention to the removal of carbon dioxide gas generated from the wastes through reaction with the Nirex Reference Vault Backfill. The carbon dioxide would incorporate a part of the carbon-14 radionuclide inventory which would therefore be contained in the repository.

6.83 In paragraph 5.42 of his Proof of Evidence (PE/FOE/7), Dr Allison states:

*"In effect, the geochemical model associated with the Nirex Reference Vault Backfill concept appears to ignore many factors associated with the physicochemical environment that would arise within the repository."*

This appears to be an unsubstantiated assertion but importantly it fails to recognise that the Nirex Reference Vault Backfill is designed to establish the physico-chemical "*environment*" in the near field of the repository, and that this position is supported by a large body of research most recently summarised in *Nirex Report S/95/011* [COR/529] and many of the 112 documents cited therein.

6.84 Dr Allison continues in paragraph 5.42 of his Proof of Evidence (PE/FOE/7):

*"As previously noted, the concept of a permeable backfill is also contrary to the principles generally embodied in backfilling and sealing concepts which are being developed by other countries."*

In *Gas Generation and Release from Radioactive Waste Repositories, Proceedings of a Workshop organised by NEA in co-operation with ANDRA, Aix-en-Provence, 23-26 September 1991* [NRX/15/9] there are the following statements:

Concerning the operational Swedish Final Repository for Radioactive Waste (SFR) on page 84:

*"The waste packages emplaced in the compartments will be surrounded by porous concrete. A porous concrete has been chosen, which will ensure that conductive paths for the gases are easily formed and that displacement of water can take place without extensive pressure build-up".*

Concerning the now-operational VLJ Repository in Finland on page 90:

*"For medium level waste, a separate silo of reinforced concrete with a 60cm thick wall, has been constructed inside the rock silo. No backfilling will be used inside the concrete silo."*

Concerning "a low/Intermediate Level Nuclear Waste Repository" concept of NAGRA (Switzerland) on pages 254-255:

*"...each cavern consists of: (a) waste packages placed within (b) a porous and permeable backfill..."*

Dr Allison's claim to "have monitored closely the development of international research programmes in this field" (PE/FOE/7, paragraph 1.6) and presumably therefore to speak with authority does not appear to be supported by this contradiction of his statement on backfilling and sealing concepts which are being developed by other countries.

6.85 Overall, therefore, my conclusion on the points made by Dr Allison in relation to the issues he identifies in relation to gas generation and migration are:

the Nirex Reference Vault Backfill will reduce the release of gas by maintaining a high pH water chemistry in which steel corrosion rates and hence hydrogen gas generation are much lower than would otherwise be the case;

the Nirex Reference Vault Backfill will trap carbon dioxide generated from the waste and therefore reduce potential releases of carbon-14;

by designing the Nirex Reference Vault Backfill to have a sufficiently high permeability its desirable properties will not be lost through physical disruption caused by over-pressurisation; and

other agencies (SKB and NAGRA) dealing with radioactive wastes that may generate gas have adopted or proposed a similar cementitious repository concept.

Sealing "Peripheral Areas" of the Repository

6.86 In paragraph 5.50 of his Proof of Evidence (PE/FOE/7), Dr Allison refers to two areas of concern in relation to Nirex's proposals for rock grouting.

6.87 The first concern is:

*"grouting materials used to achieve a high degree of penetration into the finer rock fissures are typically non-particulate grouts of a chemical formulation which seldom have a natural analogue - their longevity is therefore open to question."*

This is in direct contradiction of the statement in *International Stripa Project Overview Report* (pages 198 and 199) [NRX/15/8]:

*"Archaeological and geological analogues examined gave no indication that, if used appropriately, the clay - and cement-based sealants would not persist in repository settings. The results of laboratory tests and geochemical modelling coupled with experiments were consistent with this finding. The processing involved with the production of bentonites and cements render the materials inherently thermodynamically unstable. However, under the low hydraulic gradients expected in the groundwater in a sealed repository site, chemical transformation of the minerals in the groundwater in a sealed repository site, chemical transformation of the minerals in the sealants can be predicted, reasonable, to extend over tens of*

*thousands of years to millions of years. The predicted period depends on the porosity of the as-placed materials and the ionic concentrations in the groundwater. Understanding of the materials and models for effecting the longevity calculations have been developed. For bentonite, longevity is primarily controlled by the diffusion of  $K^+$  in the clay. The extremely low hydraulic conductivity of high-performance cement-based materials ( $k < 10^{-14}$  m/s) controls their longevity."*

6.88 The second concern in paragraph 5.50 of Dr Allison's Proof of Evidence (PE/FOE/7) is:

*"grouting operations are highly operator-dependent, and it is not possible to verify the results achieved other than by intrusive methods of testing - thus, successful tests in the RCF will not provide full assurance that grouting under different conditions will yield comparable results".*

This "concern" about different conditions is intended to be met by the RCF where the conditions are those that would pertain in a repository that might subsequently be developed in the PRZ. In my Proof of Evidence (PE/NRX/15, [paragraphs 6.68](#) and 6.69) referring to sealing, I state:

*"The achievement of the required properties relates strongly to the interaction between the materials and the precise geological setting specific to the site and location ..... The requirement is that this work should be carried out in representative geological settings, and in particular in the BVG at depth where the levels of in situ stress and rock quality will be appropriate."*

6.89 In paragraph 5.51 of his Proof of Evidence (PE/FOE/7), Dr Allison asserts that:

*"...sole reliance on grouting methods to rectify damage to the peripheral rock mass is inadequate in a repository context. Other techniques, such as the use of cut-off collars, comprising high-integrity seals constructed in carefully excavated enlargements are needed to interrupt the peripheral flow zone at strategic locations[FOE/7/8]. Such methods are amenable to verification as the seal placement operations are direct and not remote, as in the case of grouting [FOE/7/3]"*.

I agree that the use of "cut-off collars" has the merits described by Dr Allison. Such seal designs are under active consideration by Nirex.

#### Special Seals in the Repository

6.90 At paragraph 5.55 of his Proof of Evidence (PE/FOE/7), Dr Allison states that:

*"The materials and concepts for creating such [special and composite] sealing systems are reported in the international literature [FOE/7/7]. Yet the Nirex proposals for the RCF do not indicate what progress has been made in evaluating these sealing systems and provide no information about validation tests to be performed either within the RCF or elsewhere before submitting a planning application for a final repository."*

I agree that the published proposals for the RCF give little indication of our progress in evaluating sealing systems. Our current work programme is not scheduled to reach any firm conclusions until important geotechnical information is synthesised in 1996. However, I do not agree that no information about validation tests has been provided since such information can be found in Dr Mellor's Proof of Evidence (PE/NRX/16, [paragraphs 5.29](#) to 5.34).

#### Quality Assurance of Waste Emplacement and Backfilling

6.91 In Section 6 of his Proof of Evidence (PE/FOE/7), Dr Allison asserts that inadequate attention is being applied to quality assurance aspects of waste emplacement and backfilling operations and he develops a thesis of what he calls "*best practical methods*".

6.92 As I noted in paragraph 6.53 of this Supplementary Proof of Evidence, this approach does not seem consistent with the best practicable means promulgated by the regulatory authorities, where the means are linked to the radiological protection to be afforded.

6.93 At paragraph 6.11 of his Proof of Evidence (PE/FOE/7), Dr Allison states:

*"At Sellafield it is appropriate that particularly high standards (quality objectives) should be established for the backfilling and sealing system to maximise its contribution to safety".*

Again, as I note in paragraph 6.53 respectively of this Supplementary Proof there is no basis for this thesis which appears to be a contradiction of the regulatory position as founded on radiological protection principles.

6.94 The main conclusions drawn from this seemingly incorrect thesis are that an iterative programme including large-scale tests should be conducted prior to carrying out work in the RCF (paragraphs 6.11, 6.17 and 6.20). I have shown that such a process has been followed to date. There is no reason that the process will not be followed in the future period of several years leading up to validation testing in the RCF.

## Summary

6.95 In summary, I conclude that the concerns raised by Dr Allison, Dr Wogelius and Dr Salmon in respect of the performance of the Nirex repository concept are not well founded. I have presented further evidence to establish that the engineered barriers of a repository at Sellafield should perform consistently with meeting regulatory requirements for the post-closure performance of a repository. I have shown that issues identified by the Objectors, have been, or will be addressed in the Nirex Science Programme. The RCF has an essential role in the forward Programme.

## 7. EFFECT OF GEOCHEMISTRY ON RADIONUCLIDE TRANSPORT

7.1 Sections 13 and 14 of Dr Haszeldine's Proof of Evidence (PE/GNP/3) and Sections 5, 6, 7, 8 and 9 of Dr Wogelius' Proof of Evidence (PE/FOE/8) raise points concerning the effect of geochemistry on radionuclide transport. These points are dealt with in this Section of my Supplementary Proof of Evidence.

7.2 The points can be grouped within four topics:

- the presence and effect of oxidising or reducing conditions in groundwater (PE/GNP/3, Sections 13 and 14);
- radionuclide solution chemistry (PE/FOE/8, Section 5);
- mineral surface chemistry (PE/FOE/8, Sections 6 and 7); and
- sorption measurements and the use of experiments in the RCF (PE/FOE/8, Sections 8 and 9).

These four topics are dealt with separately in this Section. First, I give a breakdown of the major issues within each topic and an outline of my response to these issues, then, from paragraph 7.11 onwards, I deal with the four topics in detail.

### The Presence and Effect of Oxidising or Reducing Conditions in Groundwater - Summary of Points Raised

7.3 In paragraph 13.1 of his Proof of Evidence (and elsewhere in Section 13 of his Proof of Evidence), Dr Haszeldine (PE/GNP/3) asserts that:

	i.	the presence of hematite in the rocks of the Sellafield region indicates oxidising conditions at depth, which are deleterious to repository performance. Nirex's knowledge of the natural oxidation state of groundwater in the PRZ rests upon the assumption that the mineral pyrite geochemically controls (buffers) the groundwater redox state (Eh : a measure of the chemical oxidising potential of a solution, which can influence which ions are stable in that solution); and
	ii.	the oxidation state of the deep geological environment is important because it controls the solubility of uranium in the repository and because it may lead to the corrosion of the steel waste containers (a point also made by Dr Wogelius in paragraphs 5.4 to 5.6 of his Proof of Evidence, PE/FOE/8).

7.4 My detailed response to these points is given in paragraphs 7.11 to 7.39 of this Supplementary Proof. In summary, my response is:

	i.	Dr Haszeldine has misunderstood, or is unaware of, the fundamental thermodynamic data defining the geochemical stability field of hematite and has, consequently, reached an erroneous conclusion. Nirex has demonstrated the presence of both hematite and pyrite, as well as other iron-rich minerals, in the PRZ. The in situ redox state is buffered by a combination of these minerals and dissolved sulphate in the groundwaters at a reducing value of Eh of about -230 mV (page 33, paragraphs 6.7 to 6.9)[COR/525]. It is the <b>presence</b> of a mineral, not the <b>amount</b> (as Dr Haszeldine implies), that is important in establishing in situ geochemical equilibria.
	ii.	In any event, Dr Haszeldine has misunderstood the role of groundwater flowing into the repository (i.e. the near field) in controlling the release of uranium from the repository. The redox conditions in the repository are controlled by the components of the engineered structure (i.e. the waste and the steel containers). Geochemical modelling ( <i>Nirex Report NSS/R398 "Development of a Methodology for Modelling the Redox Chemistry and Predicting the Redox Potential of the Near Field of a "Cementitious Radioactive Waste Repository"</i> , page 19, NRX/15/10) has shown that the redox conditions in the near field are not dependent on the redox conditions of the inflowing groundwaters for all groundwaters with redox potentials between +400 mV and -400 mV. This range encompasses the values considered realistic by Nirex as well as the range considered plausible by Dr Haszeldine.

### Radionuclide Solution Chemistry - Summary of Points Raised

7.5 In paragraphs 5.12 to 5.30 of his Proof of Evidence (PE/FOE/8), Dr Wogelius raises various concerns relating to:

	i.	the role and effect of organic chemicals and, in particular:
		naturally occurring organic compounds which, he says, will form stable complexes with radionuclides and enhance their mobility; and
		organic waste degradation products which, he says, will have a marked effect on the concentrations of radionuclides in the groundwater.
	ii.	the role and effect of colloids which, he says, will invalidate all transport calculations involving solubilities and exchange equilibria (see paragraphs 5.24 and 5.25 of Dr Wogelius' Proof); and
	iii.	the role of ionic strength and temperature on radionuclide sorption, solubility and speciation which, he says, will make prediction of these factors difficult.

7.6 My detailed response to these points is given in paragraphs 7.40 to 7.58 of this Supplementary Proof. In summary, my response is:

	i.	Nirex has undertaken an extensive research programme since the 1980's to identify the sources and effects of organic materials in the repository on the performance of the near field. The Nirex programme is a world leader in this field. This programme has identified that organic degradation products of cellulose can, indeed, have a significant effect in reducing radionuclide sorption and increasing radionuclide solubility. However, this effect is being taken into account by Nirex in its performance assessments. It has also been shown that naturally-occurring humic acids cited by Dr Wogelius, will not be mobile in a cementitious near field, <i>Nirex Report S/95/011</i> (page 38)[COR/529].
	ii.	Nirex recognises that there are still uncertainties about the potential role of colloidal material derived from the cementitious barriers in transporting radionuclides. However, the nature and behaviour of colloids must meet a number of criteria before they can be demonstrated to affect radionuclide transport. There is a growing body of evidence from natural analogue and other studies that colloid transport will not be a major contributor to radionuclide releases from a

		repository, but it is essential to test some of the indications by a combination of laboratory work and experiments and observations in the RCF.
	iii.	Dr Wogelius quotes the state of knowledge in this area in 1992 (review cited as FOE/8/12) and has consequently not recognised the amount of new work that has been carried out on the effects of ionic strength and temperature. In addition, he overestimates the importance of the effect of ionic strength, as the radionuclides to which he refers are known not to complex strongly with chloride or sulphate, principal anionic constituents of the Sellafield groundwaters.

### Mineral Surface Chemistry - Summary of Points Raised

7.7 In Sections 6, 7 and 8 of his Proof of Evidence (PE/FOE/8), Dr Wogelius asserts that:

	i.	the effects of ageing on mineral surfaces are virtually unknown, that this is " <i>perhaps</i> " the most important problem in terms of quantifying the geochemical safety case (paragraphs 7.7 to 7.9) and that such ageing will reduce the beneficial effect of sorption of radionuclides on the mineral surface (paragraphs 6.6 and 6.14);
	ii.	the effect of the high pH environment generated by the alkaline fluids in the vicinity of the repository will significantly affect rock physical and chemical properties with respect to radionuclide transport (paragraphs 6.19 to 6.22); and
	iii.	that there is a need for further laboratory tests before " <i>moving on to test hypotheses in the field</i> " (paragraph 8.1)

7.8 My detailed response to these points is given in paragraphs 7.59 to 7.70 of this Supplementary Proof. In summary, my response is:

	i.	Sorption data used in safety assessments are selected conservatively to provide a range of possible values which reflect the whole spectrum of likely sorption states of a particular radionuclide, encompassing all degrees of surface characteristics of minerals. They are not based on hypothetical 'clean' states of pure minerals. These data are obtained from field and laboratory experiments on rocks which already exhibit varying degrees and mechanisms of alteration and ageing which reflect the past history of the site. Dr Wogelius' comments on surface chemistry changes, whilst geologically interesting, are not of direct significance in safety assessment as the potential impacts are accounted for by a combination of conservative assumptions and adequate ranges of data derived from real materials which reflect the range of likely states to be encountered in the geological environment.
	ii.	Nirex recognises that high pH fluids will affect the rock around the repository. It is undertaking a research programme, involving laboratory experiments, natural analogue studies and geochemical modelling, to study the results of this interaction and its impact on radionuclide transport. Current results suggest that, in general, porosity of the rock will reduce as a result of interaction with the high pH fluids. In addition, experimental results indicate that actinide element retardation in the alkaline disturbed zone will be greater than or comparable to retardation in the unperturbed geosphere.
	iii.	The RCF research programme is intended to complement, and not to replace, a laboratory-based research programme to study radionuclide transport. It is intended that the proposed experiments in the RCF to address processes of radionuclide transport (the rock-matrix diffusion experiment and the colloid transport experiment described at <a href="#">paragraphs 6.33</a> to 6.38 of my Proof of Evidence, <b>PE/NRX/15</b> ) will be supported by laboratory experiments and geochemical modelling, a programme of which is already ongoing.

### Sorption Measurements and the Use of Experiments in the RCF - Summary of Points Raised

7.9 In Sections 8 and 9 of his Proof of Evidence (PE/FOE/8), Dr Wogelius makes a number of points in relation to geochemical studies (especially sorption measurements) in the laboratory and the RCF which I deal with in this Section of my Supplementary Proof. In particular, Dr Wogelius asserts that:

i.	further laboratory experiments are needed before moving on to field experiments (paragraph 8.1);
ii.	there are shortcomings in Nirex's experiments on sorption and discrepancies in the results (paragraphs 8.5 to 8.15); and
iii.	there are deficiencies in the geochemical research proposed for the RCF (Section 9).

7.10 My detailed response to these points is given in paragraphs 7.71 to 7.100 of this Supplementary Proof. In summary, my response is:

i.	The data that are required in the areas of interest to Dr Wogelius would, indeed, be obtained largely from laboratory experiments, albeit using well-characterised geological samples of rock and groundwater taken from the Sellafield site. The highest quality samples of some materials can only be obtained from the RCF. The Nirex laboratory programme has been in progress for many years already. Nirex believes that it is now essential to evaluate some of the processes in situ, in the RCF.
ii.	Nirex has an integrated programme of sorption measurements and supporting statistical and conceptual sorption modelling work which, together, use a variety of complementary approaches for deriving sorption data. This allows the incorporation of a simple, yet realistic conservative treatment of sorption into safety assessment work. Dr Wogelius' attribution of errors seems to arise from an inappropriate comparison of databases.
iii.	I disagree that there are deficiencies in the RCF experimental programme. Dr Wogelius is concerned about issues such as the spatial variability of the system, uncertainties in interpretation of the results and working in a perturbed environment. One of the main aims of the RCF programme is to test predictions of models which address precisely these issues. Dr Wogelius has underestimated the value of in situ experiments, in particular their ability to address processes and systems which are simply inaccessible in laboratory studies. They serve an essential role in the validation process which I discuss in detail in Section 10 of this Supplementary Proof. Whilst agreeing that timescales are limited, this has been taken into account in preparing an integrated programme of field, laboratory and natural analogue studies of geochemical processes. Each has its own role to play. The issue of contamination will not be a problem, as discussed in <a href="#">paragraph 10.39ii.</a> of Dr Chaplow's Supplementary Proof of Evidence (PE/NRX/14/S1).

## The Presence and Effect of Oxidising or Reducing Conditions in Groundwater

Point i: Hematite and the Oxidation State of the Groundwater

7.11 **The Significance of Hematite:** Dr Haszeldine's thesis throughout Section 13 of his Proof is that the presence of hematite indicates oxidising conditions. In paragraph 14.6 of his Proof of Evidence (PE/GNP/3), Dr Haszeldine notes with respect to the "*geochemical parameter Eh*":

*"Oxidising Eh has positive values, reducing Eh has negative values."*

The standard geochemical text book, "*Solutions, Minerals and Equilibria*" by Garrels and Christ, published by Freeman, Cooper and Company, 1965 [NRX/15/11] shows, on Figure 7.23 (page 228), which is redrawn as Figure 7.1 of this Supplementary Proof, that hematite can be in equilibrium with 'reducing conditions' (adopting the convention of Dr Haszeldine) when the pH of the groundwater is at a pH greater than 6.2, a condition that is always met by groundwater samples taken from the BVG, as shown in *Nirex Report NSS/R397, Groundwater*

*Compositions for The Borrowdale Volcanic Group, Boreholes 2, 4 and RCF3, Sellafield, Evaluated Using Thermodynamic Modelling, 1995 (Tables 2, 9 and 13, pages 14, 21 and 25) [NRX/15/12].*

7.12 Therefore, I conclude that Dr Haszeldine is incorrect in his assertion that the presence of hematite indicates oxidising conditions in the BVG; it can also exist in a stable state in reducing conditions. The following paragraphs show how a proper evaluation of the minerals present in the PRZ should be used to develop a thermodynamically correct understanding of in situ redox conditions.

7.13 **Evidence on the oxidation state of the groundwater:** In addition to the incorrect arguments based on the presence of hematite, Dr Haszeldine makes two further claims about evidence for the oxidation state of the groundwater:

- that Nirex relies on assumptions that are inconsistent with the observed mineralogy of the PRZ; and
- that the results of his own modelling indicate that oxidising waters will exist in the PRZ.

I deal with these below in paragraphs 7.14 to 7.22 and 7.23 to 7.24, respectively of this Supplementary Proof.

7.14 Dr Haszeldine states (PE/GNP/3, paragraph 13.1) that :

*"Nirex assume that the mineral pyrite (iron pyrites  $FeS_2$ ) geochemically controls (buffers) the groundwater Eh system".*

7.15 This is a selective view of the information presented in *Nirex Report S/95/008 [COR/525]*, which states (page 33, paragraph 6.8) :

*"These results are reasonably consistent with Eh buffering by equilibria involving ferroan carbonate, pyrite and hematite."*

and (page 35, paragraph 6.23) :

*"The in-situ redox conditions in the groundwaters within the Potential Repository Zone are uncertain, but the sulphur system with a sulphate-pyrite-hematite buffer seems closest to representing in-situ redox conditions, giving a calculated Eh of -232 mV."*

7.16 Thus, the true position is that Nirex has taken a proper, thermodynamically correct geochemical approach to evaluating the redox state of the system, based on knowledge of the mineralogy and hydrochemistry of the rock/ groundwater system.

7.17 Dr Haszeldine's misunderstanding of the geochemistry of the system is further illustrated at paragraph 13.2 of his Proof of Evidence (PE/GNP/3) where he states, of the visibly reddened parts of the BVG core :

*"It is very important to note that no sulphides are recorded in such samples and so I interpret these to have been removed."*

7.18 Dr Haszeldine does not give any specific reference for this claim. Instead he gives only a very general quote that *"Nirex have made detailed examinations of the mineralogy of the bulk BVG rock"* (PE/GNP/3, paragraph 13.2). The interpretation is incorrect as pyrite is ubiquitous in fractures in the BVG, even though the quantities are often small.

7.19 Dr Haszeldine himself, in the following paragraph (PE/GNP/3, paragraph 13.3) cites *Nirex Report S/95/006 [COR/523]* and notes the presence of *"iron pyrites or other metallic sulphides"*. He then goes on to discount these as they *"form less than 1% present by volume"*. This suggests that Dr Haszeldine does not understand fully the geochemical equilibrium established in this system. The important fact for establishing the geochemical redox control (buffer) is the presence of iron sulphide phases. The amount of mineral is only significant in determining how the buffer will respond if the redox conditions in the system are disturbed; that is, if the redox of the system is

perturbed such that more oxidised iron is present in the groundwater, pyrite will dissolve to re-establish the original balance.

7.20 Dr Haszeldine makes the same mistake in paragraph 13.6 of his Proof of Evidence (PE/GNP/3), in which he discusses mineralogy in flowing zones in the PRZ. Here he says "*pyrite and siderite are mentioned as subordinate minerals, but no data on mineral abundances or locations are given*". He fails to note that these samples show the presence of iron sulphide on the surface of fractures in contact with present-day groundwater.

7.21 As noted in paragraph 7.11 of this Supplementary Proof, even in the absence of sulphide minerals, the groundwaters need not be oxidising, as the hematite stability field extends to negative Eh values in the pH range found in BVG waters.

7.22 I conclude, from the discussion in paragraphs 7.11 to 7.21 of this Supplementary Proof, that Dr Haszeldine's statement at paragraph 13.18 of his Proof of Evidence (PE/GNP/3):

*"My basic message here is that no other piece of the British landmass contains so much geological evidence of oxidised groundwater chemistry. This evidence is the alteration of BVG rock and vein minerals to hematite, as well as the existence of Britain's largest iron oxide ore deposit. There are only minimal quantities of pyrite or other sulphides, even in the minerals lining the youngest fractures. The small quantities of such sulphides make it difficult to envisage a geochemically reducing water in the past or today. This may be one of most difficult parts of the UK in which to build a Repository, which requires the maintenance of a geochemically reducing groundwater for time periods which are very long even by geological standards."*

is without foundation because his evaluation of the geochemistry of the Sellafield region is fundamentally flawed. Nirex already has the requisite level of understanding of redox conditions at the site for the present level of investigations and Dr Haszeldine's requirement:

*"to check these assertions of in-situ oxidising groundwater by direct experiment before disturbing the waters by excavating an RCF"*

is, thus, not only difficult to understand (there is no description of what is meant by 'experiments') but takes no account of the methodology used by Nirex to evaluate redox conditions or the results of the existing Nirex evaluation of the reducing potential of the repository near field (see paragraph 7.27 of this Supplementary Proof).

7.23 In paragraphs 14.16 to 14.22 of his Proof of Evidence (PE/GNP/3), Dr Haszeldine describes geochemical modelling work undertaken at Glasgow University. I am unable to comment on the validity of the approach without more detail, but there are two conclusions that I find important in paragraph 14.22:

*"In summary, I consider that the PRZ groundwater may be oxidising, in contrast to the simulations of Nirex"*

and in paragraph 14.19:

*"Implicitly, tiny amounts of pyrite can co-exist with a fluid ranging from Eh +50 to -258 mV and need not be a mineral dominating and controlling Eh in the real water."*

7.24 In this summary of his findings, Dr Haszeldine is providing evidence that the groundwater could indeed be reducing and that the most oxidising Eh consistent with his approach is +50 mV. In a rigorous study of the redox chemistry of the repository engineered barriers, *Nirex Report NSS/R398* (figure 2)[NRX/15/10], showed that groundwater at this Eh flowing into the repository near field would be buffered to the reducing potential of -440 mV by the repository contents (see also paragraph 7.27 of this Supplementary Proof). I therefore conclude that Dr Haszeldine's conclusions about the oxidising nature of the repository volume are wrong.

Point ii: The Importance of Oxidising Conditions

7.25 Dr Haszeldine (PE/GNP/3) claims that oxidising conditions within the BVG groundwater will cause oxidising conditions to become established in the repository (paragraph 13.13, page 44). He goes on to state that there are two adverse consequences of oxidising conditions in the repository from the point of view of repository performance:

- enhanced solubility of uranium (paragraph 13.15); and
- oxidation of the steel waste containers (paragraph 14.25).

7.26 Dr Wogelius notes the importance of maintaining reducing conditions in the repository at paragraphs 5.4 and 5.6 respectively of his Proof of Evidence (PE/FOE/8) and, at paragraph 5.7, he too addresses the issue of container corrosion, although from a somewhat different viewpoint.

7.27 Before addressing the specific points above, I wish to refute the suggestion made by Dr Haszeldine in Section 14 of his Proof (PE/GNP/3), that the hydrochemistry of the groundwater in the BVG can affect the redox conditions that will be established in the repository. The redox conditions in the repository are controlled by the components of the engineered structure (i.e. the waste and the steel containers). The redox conditions in the near field are reducing, and the Eh is predicted to be approximately -440 mV, *Nirex Report NSS/R398* (page 19) [NRX/15/10]. This value is not dependent on the redox conditions in the surrounding geosphere for inflowing groundwaters with redox potentials lying in the range between +400 V and -400 mV. This range encompasses the values considered realistic by Nirex as well as the range considered plausible by Dr Haszeldine. Therefore, a detailed hydrochemical understanding of the undisturbed BVG groundwaters is of limited relevance to the repository Safety Case (see also Dr Chaplow's Supplementary Proof of Evidence at [paragraph 10.30, PE/NRX/14/S1](#)). The argument developed by Dr Haszeldine in Section 13 and 14 of his Proof of Evidence (PE/GNP/3) is misconceived.

**7.28 The Oxidation State of Uranium in the Repository:** At paragraph 13.15 of his Proof of Evidence (PE/GNP/3), Dr Haszeldine states that:

*"..it is also important to maintain Uranium in its reduced state of  $U^{4+}$  which is less soluble in water than  $U^{6+}$ ."*

7.29 I would agree that uranium (IV) [ $U^{4+}$  in the notation of the above quotation] is generally less soluble than uranium (VI). At high pH in the repository near field, even under reducing conditions, the predicted oxidation state of uranium is a mixture of U(IV) and U(VI). The values of uranium solubility in the near field used in the Nirex performance assessment reflect this mix of oxidation states. Furthermore, I note that the uranium solubilities calculated by Dr Haszeldine range from  $8 \times 10^{-12}$  M to  $6.5 \times 10^{-8}$  M (PE/GNP/3, paragraphs 14.21 to 14.24). It is assumed in the performance assessment reported in *Nirex 95* [COR/522] that the solubility for uranium in the repository lies in the range  $1 \times 10^{-8}$  M to  $1 \times 10^{-2}$  M, with a central value of  $3 \times 10^{-6}$  M (*Nirex 95*, Volume 3, Table 6.6, page 6.27) [COR/522], where the solubilities are given in units of  $\text{mol m}^{-3}$ , equivalent to  $1 \times 10^3$  M. Thus, the uranium solubilities assumed by Nirex are always significantly higher than those calculated by Dr Haszeldine, and still produce calculations of risk below the target of  $10^{-6}$  per annum to an individual (see [Figure 5.3](#) of my Proof of Evidence, **PE/NRX/15**).

**7.30 The Retardation of Uranium in the Geosphere:** In paragraph 13.17 of his Proof of Evidence (PE/GNP/3), Dr Haszeldine states:

*"A possibility still exists that sorption of Uranium may occur onto these iron oxides lining the fractures, and that this may retard Uranium moving with the groundwater towards the surface. Although such effects are regarded as minor, this is speculation, as I know of no accepted values to enable these to be quantitatively evaluated [e.g. COR/605 page 138]. Thus, I doubt that the BVG rocks surrounding an engineered Repository will be adequate to contain any leakage of Uranium from a Repository. Consequently any leakage of Uranium from an engineered Repository may have an unhindered path towards the surface. Further work is still needed here, in particular the simulation of fluid flow through these rocks, coupled to the geochemical evolution of such a fluid."*

7.31 In dismissing sorption of uranium in the geosphere, Dr Haszeldine ignores the extensive geochemical and radioactive waste research on this subject. Indeed, Dr Haszeldine cites, in his support, *The Royal Society Study Group Report* (page 138) [COR/605] even though that Report does not substantiate the statement. I also note that there is a difference in interpretation of the importance of sorption in the geosphere between Drs Haszeldine and Wogelius. Whilst Dr Haszeldine states that evidence for sorption of uranium is "*speculation*" (PE/GNP/3, paragraph 13.17), Dr Wogelius states that "*Some minerals are extremely good at adsorbing radionuclides*" (PE/FOE/8, paragraph 6.2). Reference FOE/8/20 includes an extensive compilation of international studies of sorption relevant to radioactive waste disposal. Nirex has undertaken an extensive research programme to measure sorption of uranium and other actinides onto rocks from the Sellafield site (e.g. FOE/8/13). The results have been used in the derivation of sorption values used in the recent Nirex assessment.

7.32 I treat the subject of sorption in more depth in paragraphs 7.71 to 7.87 of this Supplementary Proof.

7.33 At paragraph 13.13 of his Proof of Evidence (PE/GNP/3), Dr Haszeldine asserts that:

*"If oxidising conditions in groundwater remain similar today, then this same chemical buffer will easily be able to oxidise the few hundred tons of steel barrels proposed by Nirex to maintain their engineered Repository at reducing  $E_h$ . This will turn the steel to hematite (rust), and the Repository will become oxidising."*

7.34 This argument is flawed for several reasons:

- it is geochemically flawed because the assumption concerning oxidising conditions is incorrect (as noted in paragraph 7.27 of this Supplementary Proof);
- it is chemically flawed as it assumes that the only reaction between the steel containers and the groundwater is a thermodynamic one. The electrochemical reactions that control the corrosion of the steel are not mentioned; and
- this statement also incorrectly gives the mass of iron that would be present in the repository, underestimating it by three orders of magnitude. *Nirex Report NSS/R398 "Development of a Methodology for Modelling the Redox Chemistry and Predicting the Redox Potential of the Near Field of a "Cementitious Radioactive Waste Repository"* (Appendix 4 (e), page A4.5) [NRX/15/10] shows the amount for the inventory at that time to be  $2.89 \times 10^9$  moles, which equates to more than 100,000 tons.

7.35 I thus conclude that Dr Haszeldine's statement about the ease with which the repository volume would be oxidised is incorrect. The mass of iron present in the repository, combined with the low calculated groundwater Eh, will ensure that reducing conditions are maintained in the system.

7.36 In paragraph 5.7 of his Proof of Evidence (PE/FOE/8, page 17), Dr Wogelius states:

*"The corrosion (oxidation) rate of iron is heavily dependent on the concentration of the hydroxide iron because the oxidation rate of iron depends on the hydroxide concentration raised to the second power. Thus if oxidising conditions do occur, the corrosion rates of the primary waste drums will be increased dramatically over currently assumed corrosion rates".*

7.37 I am unaware of the cited dependence of the oxidation rate of iron on the hydroxide concentration and this is not substantiated. This contradicts the well-established role of the alkalinity in concrete which affords corrosion protection to steel reinforcing bars against atmospheric oxygen and water.

7.38 Corrosion rates of the steels used to fabricate waste containers have been measured, not assumed. Carbon and stainless steels were placed in contact with cement pore water under both oxidising (aerobic) and reducing (anaerobic) chemical conditions. The results are reported in *Nirex Science Report S/95/011* (Sub-Section 2.2.1, page 18) [COR/529]:

*"The most recent estimates for the rates of aerobic corrosion of carbon steel under alkaline conditions are 0.08 μm per year at 30°C and 1.6 μm per year at 80°C. For stainless steel, the estimated corrosion rates at these two temperatures are 0.03 μm per year and 0.5 μm per year respectively."*

The symbol μ m represents the micron unit of measure - one thousandth of a millimetre.

7.39 I conclude that the corrosion protection provided by the alkalinity will dominate the corrosion behaviour of the steel containers and that these corrosion rates are well-established. This was described at [paragraphs 5.14 to 5.15](#) of my Proof of Evidence (**PE/NRX/15**).

## **Radionuclide Solution Chemistry**

Point i: The Role and Effects of Organic Chemicals

7.40 In paragraphs 5.12 to 5.22 of his Proof of Evidence (PE/FOE/8), Dr Wogelius discusses the role of organic chemicals without reference to *Nirex Report S/95/011* [COR/529]. This report was made available to Friends of the Earth in July 1995. Organic chemicals are dealt with extensively in pages 33 to 52 of the report. Paragraphs 7.41 to 7.51 of this Supplementary Proof address Dr Wogelius' concerns about:

- naturally occurring organic compounds; and
- the presence and impact of organic degradation products.

Naturally Occurring Organic Compounds

7.41 At paragraph 5.12 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states that:

*"The presence of organic materials that are able to form stable complexes with radionuclides can be the most important feature of a geochemical system and can result in completely different behaviour from that expected from consideration of inorganic reactions alone."*

7.42 At paragraph 5.17, he states:

*"These results are important because the strontium in the wastes is a long-lived radionuclide [sic: it is not long lived, having a half life of only 29 years] and because europium is a reasonable chemical proxy for the radionuclides americium and plutonium. Given the potential for reactions with organic acids to significantly influence the concentration of radionuclides in the groundwater, research prepared for the HMIP has concluded that there is a "pressing need" for experiments to quantify the importance of this effect. (Higgo 1992)[FOE/8/8]."*

7.43 However, Nirex's experimental work shows that these fears are unfounded. In particular, *Nirex Report S/95/011* [COR/529] states (page 38):

*"The high calcium concentration in the near-field porewater is expected to destabilise natural colloids (comment: such as humic acids)migrating into the repository and to compete with radionuclides for sorption onto colloids generated from the backfill. It was shown that naturally-occurring humic acids will not be mobile in a cementitious near field"*

7.44 Thus, in the engineered barriers of the repository, the high ionic strength, calcium-rich waters will destabilise naturally occurring organic compounds such as humic acids, which may be present as colloids, so that they do not present a potential means of radionuclide complexation and transport. In addition, it is anticipated that repository-derived organic compounds will dominate the organic geochemistry of the near-field system for a considerable period of time after closure of the repository. This is discussed at paragraphs 7.45 to 7.51 of this Supplementary Proof.

The Presence and Impact of Organic Degradation Products

7.45 At paragraph 5.18 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states that:

*"In addition to naturally occurring organic products, organic degradation products of the waste itself must also be accounted for. During the decomposition of the waste, cellulosic degradation products may be produced in quite a high yield (Jefferies 1992)[FOE/8/9]. These organic chemicals may have a marked effect on the concentration of radionuclides in the ground water. (Barton [sic: Baston]1994)[FOE/8/10]. However the chemical reactions that produce this effect are not well understood."*

7.46 I agree that such products must be accounted for and, indeed, will dominate the organic geochemistry of the near-field system. However, discussing the role of the degradation products of cellulose in increasing the solubility of plutonium in the repository, *Nirex Report S/95/011 [COR/529]* states (page 35) that recent treatments in preliminary performance assessments of the degradation of cellulosic materials in the near field have taken account of this behaviour.

7.47 As described in *Nirex Report S/95/011 [COR/529]* on pages 40 to 44, an extensive programme of work has led to a good understanding of the potential impact of the degradation of cellulose on the concentrations of radionuclides in groundwaters.

7.48 In paragraphs 5.19 to 5.21 of his Proof of Evidence (PE/FOE/8), Dr Wogelius describes a chemical study carried out in order to investigate the effect of organic chemicals on the amount of radionuclides that could be carried by water surrounding the rock. At paragraph 5.21, he then states:

*"... it is clear that organic species can strongly change the behaviour of both of these radionuclides and potentially increase their mobility. Complexation reactions such as this must be fully understood in order to be able to make accurate geochemical predictions."*

7.49 The last sentence overstates the importance of having a complete understanding. *Nirex Report NSS/G118, NSARP Reference Document Radionuclide Transport Through the Geosphere*, January 1992 [FOE/8/9], which Dr Wogelius cites, deals with this issue on pages 59 and 60 by making conservative assumptions, a practice common internationally in safety assessments in dealing with issues where there are currently insufficient data. The conservative nature of the approach adopted by Nirex, in using "*organic enhancement factor*", is explained and the likely reduction of the effects of organic chemicals by degradation and dilution is identified.

7.50 In paragraph 5.22 of his Proof of Evidence (PE/FOE/8), Dr Wogelius raises the concern that :

*"an unaccounted-for organic agent may form a complex with a radionuclide and bring it into solution in the groundwater"*

7.51 The report *DOE/RAS/92.012, UK Nirex Report No. 286, The Physical and Chemical Characteristics of UK Radioactive Waste Sources*, November 1992, referred to in my Proof of Evidence (**PE/NRX/15, paragraph 5.3**), ensures that the presence of any major organic constituents of the wastes can be identified. These can then be screened for significant effects upon radionuclide chemistry, as described in *Nirex Report S/95/011 [COR/529]* on pages 43 and 44. In addition, as part of the ongoing hydrochemical characterisation of the region, Nirex is carrying out analyses of naturally occurring organic compounds in the groundwaters.

#### Point ii: The Role of Colloids

7.52 Dr Wogelius considers the adequacy of data on colloids in paragraph 5.25 of his Proof of Evidence (PE/FOE/8):

*"However, if colloid formation does occur all transport calculations involving solubility limits will be invalidated. Completely new measurements based on the chemistry and physics of the colloid will be needed in order to accurately quantify the role of the geochemical barrier."*

7.53 The Nirex approach to colloid transport is outlined in *Nirex Report S/95/011 [COR/529]* on pages 39 and 40. Nirex accepts that there are uncertainties surrounding various aspects of colloids. However, as noted in this report, the need for studies of radionuclide association with colloids stems from the potential for the generation of

cementitious colloids in the near field and in the alkaline groundwater that migrates into the surrounding rock. This is the subject of ongoing studies at Nirex and of proposals for work in the RCF (see my Proof of Evidence, **PE/NRX/15**, [paragraphs 6.36](#) to 6.38) but, by itself, does not suggest that colloid transport of radionuclides could be a significant factor in the Nirex safety case. Indeed, the *Royal Society Study Group Report* [COR/605] states (page 125) that:

*"...current indications are that transport by colloidal attachment will not be serious so long as high pH persists."*

7.54 In fact, there is a growing body of evidence that colloids will not significantly increase calculated values of risk. This can be summarised briefly as follows:

- In order for colloids demonstrably to be a problem in a safety assessment (i.e. to transport significant quantities of radionuclides to the biosphere) they have to overcome several "hurdles":

- they must be present in significant concentrations;

- radionuclides must sorb irreversibly onto them; reversible sorption (when radionuclides from solution attach "temporarily" to the surface of a colloid before going back into solution) may become irreversible if colloids aggregate (stick together) but, if they do, they are likely to become large enough to sediment or be filtered out and irreversible sorption then becomes an advantage because the radionuclides are immobilised;

- they must be physically and chemically stable in all the hydrochemical environments along the pathway to the biosphere; and

- they must be able to move through the pore and fracture structure of the potential transport pathway.

- Preliminary evaluations of natural colloid populations in the groundwaters at Sellafield (not yet published) suggest them to be low and comparable to other sites and environments being studied internationally for deep disposal of radioactive wastes. However, these measurements need to be verified by better quality sampling that would only be facilitated by the access provided by the RCF.
- From natural analogue studies (studies of natural geochemical systems analogous to those found in a repository or at a repository site), the observed association of uranium with natural colloids appears to be small. This has been observed in the fractured granitic rocks of the El Berrocal site in Spain (*El Berrocal Project: Summary Report on Phase I, Rivas et al.; Report of the European Commission, EUR 15908, 1995*, page 93, NRX/15/17) and of Grimsel in Switzerland (*Nirex Report NSS/R165*, page i, NRX/15/18), in fractured slates from Cornwall (*Nirex Report NSS/R165*, page i, NRX/15/18) and in the highly alkaline groundwaters of the Maqarin site in Jordan (*Nagra Technical Report 91-10*, page 76, NRX/15/13) which are analogous to those which might transport radionuclides from the near-field.
- I know of no clear evidence for long-distance colloid mobility in low conductivity fractured rocks at repository depths. It is believed that the tortuosity of the pore and fracture system in the rock effectively filters colloids out from flowing groundwater.

7.55 There are clearly uncertainties which must be explored by a combination of continued laboratory experiments, natural analogue studies and in situ observations and experiments in the RCF. I consider the latter to be crucial to a clearer resolution of the issue. Work in the RCF, both in the colloid transport and the chemical disturbance experiments, which I outline in my Proof of Evidence (**PE/NRX/15**, [paragraphs 6.36](#) to 6.38 and [paragraphs 6.51](#) to 6.53 respectively) will be essential in studying each of the four points above.

Point iii: The Importance of Ionic Strength

7.56 In paragraph 5.29 of his Proof of Evidence (PE/FOE/8), Dr Wogelius sees the statement :"*the effects of increasing ionic strength and temperature on radionuclide sorption, solubility and speciation have generally not been considered*" as "*a tremendous problem*" (PE/FOE/8, paragraph 5.30) in respect of ionic strength.

7.57 A considerable amount of work has been carried out by Nirex since material was compiled for the report referenced by Dr Wogelius as FOE/8/12 and published in 1992. Experimental work has been carried out on the

effects of salinity and temperature in a repository environment. For example, *Nirex Report NSS/R311*, (Figure 7) [NRX/15/14], shows data for U(VI). It shows that salinity does not significantly affect the solubility of uranium. This observation is supported by geochemical modelling, which predicts that the speciation of radionuclides is dominated by hydroxy complexes. There is no indication that radionuclides complex significantly with the main ionic components of Sellafield groundwater, chloride and sulphate, over the expected chemical conditions and concentration ranges found in the rocks at Sellafield. It is therefore clear that ionic strength is not a key variable and that the "*tremendous problem*" in paragraph 5.30 of Dr Wogelius' Proof of Evidence (PE/FOE/8) is non-existent.

7.58 In paragraph 7.3 of Dr Wogelius' Proof of Evidence (PE/FOE/8), the effects of temperature are said not to have been considered in safety assessment calculations in respect of the solubilities of plutonium, neptunium and americium at elevated temperatures. Again, this is justified by reference to FOE/8/12. Much work has, in fact, been carried out since the statements in that report were formulated and the results are due to be reported in 1996.

### **Mineral Surface Chemistry**

7.59 At paragraph 6.2 of his Proof of Evidence (PE/FOE/8), Dr Wogelius sets out his general thesis that the precise nature of the mineral surface must be known:

*"Some minerals are extremely good at adsorbing radionuclides. For example clays and amorphous iron-hydroxides have a strong tendency to adsorb a number of radionuclides under certain conditions. However many minerals, such as quartz, are extremely poor sorbers. Thus in order to extrapolate data from experimental results for use in geochemical predictions the precise nature of the mineral surface concerned must be known".*

Again, the contrast between Dr Wogelius and Dr Haszeldine (at paragraph 13.17 of his Proof of Evidence, PE/GNP/3), in the assumed importance of sorption, is noted.

7.60 My overall response to this is that:

- I accept that mineral surfaces are important controls on the groundwater chemistry and on sorption of radionuclides in the geosphere;
- however, the definition of "*precise nature*" is not given and I believe this is an elusive criterion; moreover
- I believe the nature of geological mineral surfaces has only to be understood to the extent necessary to demonstrate that adequate models and parameters are used in performance assessment studies.

In the remainder of this Section 7 of my Supplementary Proof I deal with the specific concerns raised by Dr Wogelius in this general area.

#### **Point i. The Effect of Ageing**

7.61 In paragraphs 7.8 to 7.9 of his Proof of Evidence (PE/FOE/8), Dr Wogelius makes statements on how changes to mineral surfaces and sorption on minerals might be of significance in performance assessment. In summary he states (paragraph 7.9):

*"The ageing effects on mineral surfaces for the whole range of pertinent chemical reactions is virtually unknown. This is perhaps the most important problem in terms of quantifying the geochemical safety case."*

7.62 Whilst accepting that there is sparse information on the ageing of mineral surfaces, this appears to reflect a misconception concerning work by Nirex on the role played by rock surfaces in assessments of long-term safety. Sorption data used in safety assessments are selected conservatively to provide a range of possible values which reflect the whole spectrum of likely sorption states of a particular radionuclide, encompassing all degrees of surface characteristics of minerals. They are not based on some hypothetical 'clean' state of a pure mineral. These data are obtained from field and laboratory experiments on rocks which already exhibit varying degrees and mechanisms of alteration and ageing which reflect the past history of the site. There is good confidence that the far field fracture surface mineralogy (outside the influence of the concentrated alkaline plume) will not change significantly in the geologically short timescale of interest to repository performance unless there are significant

changes to deep hydrochemistry. Furthermore, any possible perturbations are only likely to result in the same types of surface changes for which there is already evidence in the samples used in experiments.

7.63 In paragraph 6.6 of Dr Wogelius' Proof of Evidence (PE/FOE/8) he cites the work of Seitz (FOE/8/17) to support his conclusions about mineral surface alteration, saying that:

*"....under conditions analogous to those expected in a waste repository mineral surfaces can become significantly less able to sorb radionuclides."*

It should be noted that the "*conditions analogous to those expected in a waste repository*" in this work relate to the hydrothermal alteration of basalt at 320°C [FOE/8/17, page 111]. Such high temperatures are of no relevance to the Nirex repository concept for ILW and some LLW.

7.64 I would also point out that the behaviour of radionuclides at the Sellafield site over geological timescales is addressed through studies of the geochemical behaviour of naturally-occurring radionuclides. In *Nirex Report NSS/G118* (page 22) [FOE/8/9] it is stated :

*"Many elements of interest in repository assessments occur naturally. Some of the radioactive fission and activation products are radioisotopes of elements that have stable isotopes occurring in nature, such as the halogens or the transition metals. In addition, small quantities of the members of the long-lived  $^{238}\text{U}$ ,  $^{235}\text{U}$  and  $^{232}\text{Th}$  decay chains are found in all rocks. The distributions of all these isotopes within a given part of the earth's crust are a function of changing hydrogeological and hydrochemical conditions. Therefore the current distributions of such radionuclides have the potential to yield information on long term groundwater flow, geochemistry and radionuclide transport."*

7.65 In the near field, ageing or leaching of the cementitious Nirex Reference Vault Backfill (NRVB) over long timescales may result in a reduction in the sorption capacity of NRVB. This ageing effect is incorporated in the range of parameter values represented in probability density functions (PDFs) in assessment models for radionuclide sorption onto NRVB (see *Nirex Science Report S/95/011*, page 32)[COR/529].

7.66 Nirex recognises that, over long timescales, radionuclides may become irreversibly incorporated in minerals within the repository or surrounding rocks. This is a beneficial process that could lead to reductions in the dissolved concentration of the radionuclide. However, Nirex currently takes no account of this process in its safety assessments and this is considered to be a conservative assumption. Because Nirex uses a conservative, reversible sorption model, the potential for release of irreversibly sorbed radionuclides as a result of mineral surface ageing is not relevant.

7.67 I conclude that Dr Wogelius' comments on surface chemistry changes, whilst scientifically interesting, are not of direct significance in safety assessment as the potential impacts are accounted for by a combination of conservative assumptions and adequate ranges of data derived from real materials which reflect the range of likely states to be encountered in the geological environment.

#### Point ii. Groundwater Chemistry and Mineral Surface Reactions

7.68 At paragraph 6.19 of his Proof of Evidence (PE/FOE/8), Dr Wogelius asserts that a reliable quantification of how sorption changes as a function of pH is critical for a reliable safety assessment. This is indeed of significance and Nirex has addressed this by carrying out experiments on sorption over a range of pH conditions from pH 6-12 (see FOE/8/10, page 521 and FOE/8/9, page 24).

7.69 In paragraphs 6.21 and 6.22 of his Proof of Evidence, Dr Wogelius discusses dissolution of minerals by alkaline fluids released from the repository, proposing "*possibly significant changes in the regional hydrology.*"

7.70 The high pH of the groundwater derived from the repository is predicted to be reduced to near-neutral pH (i.e. the chemical conditions of the unperturbed BVG groundwaters) through dilution and reaction of the alkaline plume in the BVG before it reaches the sandstones discussed in Dr Wogelius' paragraph 6.21. Work conducted on this topic by Nirex using laboratory experiments, natural analogue studies and geochemical modelling has shown that,

for the minerals present in both the BVG and the sandstones, the precipitation of new minerals resulting from reaction with alkaline fluids generally causes a reduction in porosity, and not an increase. These changes will be localised around the repository and will not affect the regional hydrology, as Dr Wogelius proposes. As described in Dr Mellor's Proof of Evidence (PE/NRX/16) at [paragraphs 5.22](#) and 5.23, Nirex intends to carry out in situ experiments to support our ongoing programme of laboratory and natural analogue studies of this process.

## **Sorption Measurements and the Use of Experiments in the RCF**

### **Point i: Further Laboratory Studies**

7.71 In paragraph 8.1 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states that:

*"As discussed above, it is clear that many key pieces of data remain to be acquired in order to enable a reliable chemical safety case for nuclear waste disposal to be produced....it is advisable for many of the fundamental chemical data to be measured under well-constrained laboratory conditions before moving on to test hypotheses in the field"*

7.72 My broad response to this is that, although Dr Wogelius discusses some interesting issues up to this point in his Proof of Evidence (PE/FOE/8), it is far from "*clear that many key pieces of data remain to be acquired*". In particular, Dr Wogelius has failed to quantify the effects of these issues on a repository performance assessment. Further, he has not recognised that many of the uncertainties that he has raised are already encompassed in the parameter ranges used as input to the performance assessment. This suggests that he has misunderstood the philosophy underlying a probabilistic safety assessment namely that it is not necessary to have a complete understanding of all processes (in a similar way that it is not necessary to have a complete deterministic description of the system; see Dr Chaplow's Supplementary Proof of Evidence, PE/NRX/14/S1, [Section 5](#)). Rather, it is necessary that the possible effect of the process on the parameter has been considered. Nirex, in line with international practice, deals with uncertainties by making conservative assumptions until such time as these uncertainties can be better resolved.

7.73 The data that are required in the areas of interest to Dr Wogelius would, indeed, be obtained largely from laboratory experiments, albeit using well-characterised geological samples of rock and groundwater taken from the Sellafield site. The RCF offers the opportunity to obtain the highest quality samples of some materials to feed into this laboratory programme. The *Royal Society Study Group Report* [COR/605] noted (Section 1.3, pages 2-3) that there were a number of areas where scientific understanding is not yet sufficiently advanced to support the construction of detailed post-closure performance assessments, whilst observing that these will be addressed in the Nirex research programme. Referring to the chemical containment issues, the *Royal Society Study Group Report* [COR/605] noted (Section 1.3.4, page 5) that:

*"Nirex are making progress with assessing these very complex effects, and there is increasing international work on what was formerly a predominantly UK interest."*

7.74 A substantial research programme has been underway since field samples were first available from the site and is envisaged to continue up to, and overlap with, Phases 2 and 3 of the RCF. Many of the areas addressed by Dr Wogelius are already under study or will be studied in this period. I believe that it is now essential to extend the laboratory study of important geochemical processes into in situ conditions in the RCF.

### **Point ii: Alleged Discrepancies in Nirex's Sorption Data**

7.75 From statements made in paragraphs 8.5 to 8.15 of his Proof of Evidence (PE/FOE/8), it appears that Dr Wogelius doubts the validity of the Nirex sorption data. He makes a number of specific points which I shall deal with below in turn.

7.76 In paragraph 8.5 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states:

*"... the types and proportions of the organic chemicals present in the 'authentic degradation products' may not have been the same in the experiments ..."*

7.77 This conclusion is incorrectly attributed by Dr Wogelius as a finding of *Jefferies (1992)* [FOE/8/9]. In any event, this statement is incorrect. In particular, in Nirex's work, the organic degradation products are prepared by a standard method which is subject to quality assurance and the same batch of material is used for a group of experiments which is going to be the subject of comparative evaluation.

7.78 In paragraph 8.7 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states:

*"Discrepancies between these experimental results 'raise questions over the applicability of batch data to in situ conditions' (Jefferies 1992)[FOE/8/9]. Clearly, extrapolations from these data sets to the long-term geochemical safety case would contain significant errors."*

7.79 The report from which Dr Wogelius quotes is Nirex's own evaluation [*Nirex Report NSS/G118*, FOE/8/9] of the adequacy of sorption data. The issue of the potential over-estimation of sorption by the batch technique has long been recognised by Nirex. In the annex of *Nirex Report NSS/G118*, page 103 [FOE/8/9], it was recognised that further work was required to build a robust argument for correcting batch sorption data for use in assessment calculation. Dr Wogelius fails to note the treatment of this issue in the most recent assessment, *Nirex 95* (Volume 3, page 6.8) [COR/522], where a correction factor was used to account for the potential lower sorption on intact rock samples.

7.80 In paragraph 8.9 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states that:

*"In contrast to the batch and diffusion experiments considered above, studies with single mineral phases allow critical measurements to be made concerning the rates and energetics of important reaction mechanisms. Questions about whether sorption or precipitation occurred are answered, the relative importance of various phases in adsorbing contaminants are determined, and the reversibility of the sorption reaction unambiguously measured. "*

7.81 Such work has been underway for several years in the Nirex research programme and results will be reported in 1996.

7.82 In paragraphs 8.11 to 8.15 of his Proof of Evidence (PE/FOE/8), Dr Wogelius seeks to undermine the use of bulk sorption coefficients by Nirex. In particular, in paragraph 8.12, Dr Wogelius states:

*"Estimates of  $K_d$  used in Nirex performance assessments have exhibited errors of up to 10,000 fold. (Thomas 1993) [FOE/8/21]"*.

7.83 Nirex recognises that the treatment of radionuclide sorption in the assessment is simple, in that it is treated as a reversible linear process characterised by a distribution coefficient ( $K_d$ ) describing the partitioning of radionuclide between the groundwater and the rock or mineral. FOE/8/9 (page 17), states: *"Although simple, the  $K_d$  approach can be shown to lead to conservative estimates of sorption behaviour through carefully considered justification of the choices of  $K_d$  values for particular rock types and radioelements."* A number of approaches are adopted to obtain such conservative estimates of sorption:

- sorption measurements on intact rock samples in which the natural rock surfaces have not been disturbed by crushing the sample;
- application of surface analytical techniques to identify the important sorbing minerals in the rock;
- batch sorption experiments using well-characterised single mineral phases, to build confidence in the mechanisms of sorption;
- geochemical modelling to interpret solution data from experiments and to extrapolate results to a wider range of geochemical conditions; and
- observations of the behaviour of naturally occurring radionuclides in geological systems (natural analogues).

7.84 As to the assertion of 10,000 fold errors, I cannot find the statement which Dr Wogelius quotes or evidence for that statement in the reference cited [FOE/8/21]. The reference seeks a comparison of "*Nirex Data*" with "*Nagra Data*" with respect to far field and near-field sorption data. There is only one written comment in FOE/8/21

that compares the Nirex sorption data with the other published sorption data. It is stated [FOE/8/21, page 12] that "*The majority of the sorption and solubility values quoted by Nagra and SKB fall well within the ranges quoted by Nirex and those provided for Phase 1 calculations from HMIP databases*". There is no comment to the effect that Nirex data have exhibited errors up to 10,000 fold, as implied by Dr Wogelius. It is, therefore, unreasonable to say that "*estimates of  $K_d$  used in Nirex performance assessments have exhibited errors*" let alone "*of up to 10,000 fold*" (paragraph 8.12, PE/FOE/8). The Nirex and Nagra/SKB data presented in FOE/8/21 are, simply, different; both sets are appropriate for the rock types and experimental conditions.

7.85 The Nagra data given in Tables A3 and A4 of FOE/8/21 are the same for "*high capacity rocks*" (Table A3) and "*low capacity rocks*" (Table A4). On consulting the source reference, I have found that these data are given for one rock type only, "*marl*." I am unable to reconcile their use to describe the behaviour of two distinct rock types.

7.86 In paragraphs 8.14 and 8.15 of his Proof of Evidence (PE/FOE/8), Dr Wogelius questions the reliability of  $K_d$  values for use in performance assessments stating, at paragraph 8.14:

*"... when rock samples as either powders or larger aggregates are used to determine the  $K_d$  for an element, implicit in the result is that the lithology is homogeneous with respect to the amounts of various mineral components present. Thus  $K_d$  values cannot be relied upon to provide unequivocal data for use in reliable geochemical safety assessments."*

7.87 Dr Wogelius' concerns are unjustified. In particular, in reaching his view he appears not to have considered the measures that Nirex has adopted to build confidence that the data used in performance assessments are reliable, in the sense that they do not lead to underestimates of risk. These measures are as follows (see *Jefferies, 1992*; FOE/8/9, Section 3.3.1):

- the use of probability density functions for  $K_d$  values to represent natural variability. The Nirex sorption programme involves measurements on a range of rocks from the Sellafield site using a range of groundwater compositions in order to describe this variability;
- an extensive programme of work directed at understanding the mechanisms of sorption so that geochemical modelling can be used to provide an interpretative model consistent with experimental data; and,
- support for the conceptual model of sorption by observations of mineralogical samples, single phase batch experiments and natural analogue data.

I therefore conclude that a global  $K_d$  may be inherently inaccurate in an absolute sense, but if it is chosen conservatively, informed by such work, it will allow a robust estimate to be made of the performance of the repository. It will not be "*unreliable*" in respect of its purpose.

Point iii: Geochemical Research Proposed for the RCF

7.88 In Section 9 of his Proof of Evidence (PE/FOE/8, pages 43 to 46), Dr Wogelius criticises the geochemical research proposed for the RCF.

7.89 In paragraph 9.2 (and effectively, again, in paragraph 9.6) Dr Wogelius asserts that:

*"The rock matrix diffusion tests will be completed by injecting sorbing and non-sorbing tracer chemicals into a portion of the site. Fluids will be collected at a distance from the injection point, and analysis of the collected fluids will be used to determine how much of the chemical tracer has been adsorbed. This type of approach is simply a large scale  $K_d$  experiment."*

7.90 In this, Dr Wogelius seems not to appreciate the benefit of in situ experiments in the RCF in that they allow processes to be studied under natural geochemical conditions on a length scale greater than can be studied in the laboratory. Additionally, they allow experiments to be performed that address groundwater flow and transport

through fractures in the BVG. Such experiments are not possible in the laboratory, because of the changes to the hydraulic properties of the fracture that inevitably occur when the fracture is removed from the ground.

7.91 The scope of the rock matrix diffusion experiment is wider than simply measuring the sorption properties of selected radionuclides in the fracture. The experiment will address the geometry of the groundwater flow pathway and the extent of rock matrix diffusion. The results will be used to ensure that an appropriate representation of channelling of flow and of rock matrix diffusion is incorporated in performance assessments.

7.92 In paragraph 9.3 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states:

*"Nirex funded and other laboratory research has had great difficulty understanding radionuclide sorption data even when the experiments are fairly simple. Conflicting results have been found in a number of studies, due to problems with effective surface areas, slight shifts in chemical equilibria and surface ageing effects".*

7.93 In this, Dr Wogelius has correctly identified a problem if, as I believe, he is saying that sorption is a complex process and it will be difficult to understand in detail. I agree, but I have outlined above why this is not an issue.

7.94 The value of the proposed experiments in the RCF is that they will allow predictions, based on the treatment of uncertainty in sorption models and parameter values derived from laboratory-scale experiments, to be tested. The extent of agreement between model prediction and experimental result will demonstrate the degree of understanding of the process. This is the process of model validation described in detail in Section 10 of this Supplementary Proof of Evidence and it is one of the key aspects of the experimental programme in the RCF.

7.95 In paragraph 9.5 of his Proof of Evidence (PE/FOE/8), Dr Wogelius states that:

*"Resolution of these problems is fundamental to understanding radionuclide geochemistry and making accurate predictions. Large-scale field tests however, will have difficulty contributing to the solution of these problems for the following reasons:*

- the time scale of the experiments will be limited and therefore it will not be certain that the observed behaviour is not a transient phenomenon;*
- there will be uncertainty about the cause of the observed results;*
- such an approach does not account for possible inhomogeneities in the host rock which can drastically change geochemical behaviour;*
- finally, the short-term chemical state of the site may be changed by RCF construction, such that the measurements may be compromised by contamination."*

7.96 My response to the bullet points he sets out is given in paragraphs 7.97 to 7.100 of this Supplementary Proof, respectively:

7.97 I agree that *"the time scale of the experiments will be limited."* Therefore, only relatively rapid geochemical processes will be addressed. Neglect of long-timescale processes such as mineralisation of radionuclides is a conservative assumption in performance assessment modelling. Nirex recognises that each of the three types of experimental methods to acquire data - laboratory experiments, field experiments and natural analogues - has advantages and disadvantages relative to the others. The in situ experiments in the RCF will be supported by a continuing laboratory scale programme and by observations on the behaviour of naturally-occurring radionuclides in the Sellafield groundwater system.

7.98 I agree that there may be some *"uncertainty about the cause of the observed results"* in terms of the detailed mechanisms. However the important test is whether an adequate understanding can be gained of the causes and effects that are important to repository performance. The integrated modelling and testing will be designed to ensure that requirement is met.

7.99 I disagree that *"such an approach does not account for possible inhomogeneities in the host rock which can drastically change geochemical behaviour"*. This is a major purpose of conducting large scale, in-situ experiments. A fracture will be selected for study to be representative of water-conducting features in the BVG. It is recognised

that the fracture under test may be heterogeneous. Excavation of parts of the fracture will address this mineralogical heterogeneity. Heterogeneity is an important aspect of the real system and the approaches taken to predict the results of the experiment and to extrapolate the results for use in a performance assessment will need to take this geochemical heterogeneity into account.

7.100 I disagree that "*the short-term chemical state of the site may be changed by RCF construction, such that the measurements may be compromised by contamination.*" A small volume of rock surrounding the excavation may be subject to such "*contamination*" but the system will remain uncontaminated on the site scale. Experiments will be performed in boreholes drilled from the galleries in the RCF extending beyond the zone of any significant stress relief and likely "*contamination*". Since natural groundwater flow will be from the rocks towards the RCF, it is unlikely that there will be "*contamination*" of the groundwater in the fractures by the atmosphere in the RCF, as proposed in paragraph 9.7 of Dr Wogelius' Proof of Evidence (PE/FOE/8). I thus find no grounds to support the statement in paragraph 9.8 of his Proof of Evidence that:

*"These perturbation effects will directly effect the geochemical equilibria under investigation."*

Rather, the perturbation effects will be limited and the experiments will be located to avoid these effects.

## **Summary**

7.101 The Objectors have raised a range of concerns about the geochemical aspects of performance assessment. Many of these are generic to radioactive waste disposal in its broadest sense or to the conceptual design of the proposed Nirex repository. None of the concerns raises a new issue. Nirex is already familiar with all the arguments raised and is, or has been, addressing them to varying extents in its research and development programme for more than a decade.

7.102 I have shown the statements made by Dr Haszeldine concerning the redox state of the repository environment to be incorrect.

7.103 Dr Wogelius suggests that a more detailed, mechanistic understanding is required for all the processes which he raises. However, he misunderstands the philosophy underlying a probabilistic safety assessment. A complete understanding of all processes is not required. Rather, it is necessary that the possible effect of the process on the parameter has been considered. The uncertainty in parameters must be encompassed fully in the probability density function (PDF) of parameter values. Nirex, in common with international practice, deals with uncertainties by making conservative assumptions until such time as these uncertainties can be better resolved. The current and future Nirex programme is addressing many of the issues raised by Dr Wogelius and the RCF will play a central role in their evaluation.

7.104 It is my view that all of the issues raised by Dr Wogelius are best treated by a programme of laboratory experiments and natural analogue studies which runs in parallel with, and is eventually integrated with, in situ experiments in the RCF. Such a programme of work has been in hand for many years and, in most areas, is at a mature stage of development and has produced high-quality information that has been used in safety assessment. A number of issues, such as colloid transport and alkaline plume/rock interactions are ready now to be evaluated using a parallel programme of underground experiments. In fact, it is essential that in situ experiments in the RCF do take place. We do not need to wait any longer before this integration takes place.

## **8. TREATMENT OF THE BIOSPHERE IN PERFORMANCE ASSESSMENTS**

8.1 Dr Starmer's Proof of Evidence (PE/CCC/5, Sections 3, 4, 5 and 7) makes a number of observations and criticisms regarding Nirex's treatment of the biosphere in performance assessments which are dealt with in this Section of my Supplementary Proof of Evidence.

8.2 Dr Starmer's points fall into two groups which I consider in turn below:

a series of claims that Nirex has omitted to consider (or at least to present) a number of analyses and scenarios which would lead to worse risk predictions than the scenarios actually presented; and

specific criticisms of Nirex's "agricultural well" analysis.

8.3 Before dealing with Dr Starmer's specific concerns, I draw attention to Nirex's approach to biosphere assessments as described in *Nirex Report S/95/002, Post-closure Performance Assessment, Treatment of the Biosphere, May 1995* (Section 3.1, page 6) [COR/526]:

*"Nirex's overall approach to biosphere assessment modelling is based on existing methods for simulating the behaviour of trace substances in the environment, coupled with scientific knowledge and understanding obtained from the NSARP [S/95/003]. However, whereas the NSARP provides an appropriate level of understanding of the underlying processes of relevance to biosphere assessments for deep geological disposal, it is clear that there is only limited scope for validating research models describing the long-term evolution of the biosphere. This is also the case for models used to represent other environmental processes of potential importance in the context of releases from deep geological repositories, for direct use in the context of radiological assessment. This consideration, together with uncertainties associated with the potential impact of future human activities, dictates that a pragmatic strategy be adopted in performance assessment modelling. The essence of this approach is to make clear the underlying assumptions of the assessment, and to advance a rational and self-consistent basis for evaluating possible radiological exposures associated with future discharges from the disposal facility."*

8.4 Hence, my overall response to Dr Starmer's concern is that the declared objectives of the Nirex approach to biosphere assessment modelling are consistent with ensuring that there are no important omissions such as those which he proposes. The work programme (which has been underway for more than seven years) has a declared framework, as given above, in which the underlying assumptions are stated clearly and provide a rational and self-consistent basis for evaluating repository post-closure performance. The programme is managed for Nirex by Dr Thorne of Electrowatt Engineering Services, an internationally recognised scientist in this field.

#### **Alternative Analyses and Scenarios**

8.5 In summary, Dr Starmer asserts in his Proof of Evidence (PE/CCC/5) that:

i.	the Nirex base case may well be non-conservative when it comes to dealing with the effects of climate change (PE/CCC/5, paragraphs 5.1.1 and 7.1). Calculations carried out for HMIP indicate that the risk estimated by time dependent simulations could be two orders of magnitude greater than for constant conditions (PE/CCC/5, paragraph 5.1.2);
ii.	Nirex has input a constant base case radionuclide flux into its modelling of climate controlled biospheres and does not consider differences in hydrological conditions due to climate changes (PE/CCC/5, paragraphs 3.1.3 and 5.1.6);
iii.	Nirex has not adequately treated the effect of "down-cutting" of streams in the Boreal climate stage (PE/CCC/5, paragraph 5.1.8);
iv.	Nirex does not present results from variant groundwater flow models for Temperate terrestrial discharge (PE/CCC/5, paragraph 3.2.19); and
v.	Nirex has not addressed the possibility that "up-welling" of ground water offshore will result in contaminated sediments which will subsequently be exposed under boreal climate conditions (PE/CCC/5, paragraph 5.1.7).

8.6 I deal in detail with these assertions at paragraphs 8.7 to 8.29 but in summary my response is that:

i.	the first is ill-founded because the calculations carried out for HMIP on which Dr Starmer depends are known to be incorrect owing to inappropriate critical group assumptions and artefacts introduced by the modelling approach that was used to obtain the result;
ii.	the second is correct, but is likely to be of little significance to the calculated risk for a repository at Sellafield;
iii.	the third is incorrect because the treatment is adequate for the purposes of performance assessment;
iv.	the fourth is incorrect in substance because the results would not be substantially different from those presented for a Boreal terrestrial discharge; and
v.	the fifth is not correct because the possibility has been addressed in the results presented in <i>Nirex 95</i> (Volume 3) [COR/522] and associated risks are not underestimated.

Point i: Climate Change and the Nirex Base Case

8.7 Dr Starmer asserts that the Nirex base case may well be non-conservative when it comes to dealing with the effects of climate change (PE/CCC/5, paragraphs 5.1.1 and 7.1).

8.8 He further notes that (PE/CCC/5, paragraph 5.1.2):

*"HMIP has conducted a trial assessment of hypothetical LLW/ILW repositories at Harwell in Oxfordshire in which a probabilistic model of climate change has been developed and used to provide time-varying boundary conditions to the groundwater flow and transport calculations (CCC/5/1). A key result of the study is that:*

*'For the Harwell site, results indicate that the risk estimate obtained by time-dependent simulation is about two order of magnitude greater than that obtained by an equivalent simulation for constant (temperate) environmental conditions.'*

8.9 This conclusion is taken from CCC/5/1, the overview report of the HMIP study, and is based on Figure 4.25 (page 67). However, this figure relates only to iodine-129 in a geological system that bears no resemblance to Sellafield.

8.10 On page 66 of CCC/5/1 the difference between the time-dependent and constant conditions results is attributed to:

*"the sharp dose peaks which are characteristic of the time-dependent runs".*

8.11 Page 66 and Figure 4.24 of CCC/5/1 show that these sharp dose peaks are associated with springwater pathways in Devensian Glacial and Tundra periods when near surface groundwater dilution was taken to be lowest. However, these peaks are entirely an artefact of the modelling approach adopted.

8.12 These results are wholly conditioned by inappropriate critical group assumptions. Examination of the input data given for this exercise on page 65 of *DOE/HMIP/RR92.059: Dry Run 3*, Volume 5 shows that a very small recharge area of  $5.5 \times 10^4 \text{ m}^2$  was used. This was based on the concept of recharge to the surface only through a talik (put simply, a break in the permafrost) and not via any runoff or interflow from ice sheets.

8.13 This area of  $5.5 \times 10^4 \text{ m}^2$  (5.5 hectares) appears to have been equated to the resource area for the critical group. By comparison, Nirex estimates the minimum resource area for a community of 30 people in Periglacial conditions to be  $10^9 \text{ m}^2$  in *Nirex Report S/95/002* (page 9, Box C) [COR/526]. This is 18,000 times greater than the area used in *Dry Run 3* (CCC/5/1). The immediate implication is that the area used in the latter report could not support a single person. With the appropriate resource area taken from *Nirex Report S/95/002* [COR/526] a surface water flow rate, and hence dilution factor, about 10,000 times that calculated in *Dry Run 3* (CCC/5/1) would be available to the critical group supported by the minimum realistic area. This would completely suppress the peak in

risk reported in Dry Run 3 (CCC/5/1) invalidating the main finding on the differences between the case of constant Temperate conditions and the time-dependent simulation.

8.14 Dr Starmer seeks to gain authority for the importance of this (incorrect) finding from *Dry Run 3*, (CCC/5/1) stating (PE/CCC/5, paragraph 5.1.3):

*"HMIP warns that direct transferral of the Harwell site results to other sites because the travel times estimated for Harwell are "..... considerably greater than the time over which climate changes may occur, .....". The Sellafield travel times are also long compared to the predicted climate change times ....."*

This is an important mis-quotation of the study report which should not be interpreted to mean that the results are considered transferable. The statement on page 101 of *Dry Run 3* (CCC/5/1) is:

*"This result is not necessarily directly transferable to other sites. **For example**, [my emphasis] a significant feature of the Harwell site is that estimated geosphere transit times are considerably greater than the time over which climate changes may occur"*.

8.15 Finally, I draw attention to the comment on page 93 of CCC/5/1 that:

*"The Project Team agreed that the potential bias associated with the modelling approach in *Dry Run 3* was not adequately assessed and therefore it is not possible to confirm that the modelling approach has not introduced a significant bias **on the quantitative results**" [My emphasis].*

It is therefore inappropriate to use these quantitative results to argue that the quantitative results in *Nirex 95* Volume 3 [COR/522] are in error.

8.16 In conclusion, the result cited by Dr Starmer to justify his statement on increase in risk due to climate change is an artefact of modelling error. In addition the result would not be transferable to the calculations of risk for a repository at Sellafield, even if it were correct.

Point ii: Base Case Radionuclide Flux

8.17 Dr Starmer indicates that Nirex has input a constant base case radionuclide flux into its modelling of climate controlled biospheres (PE/CCC/5, paragraph 3.1.3). Further, at paragraph 5.1.6 of his Proof of Evidence (PE/CCC/5) he states:

*"Nirex do not adjust the base case hydrology for climate conditions. The differences observed for the various climate state simulations are due to changes in the biosphere model, such as changes in the crops grown by the critical population group. Increased infiltration in the highlands east of the Sellafield site due to melting of permafrost has been cited by the RWMAC (GOV/406). Other climate controlled changes in the hydrologic flow regime affect flow velocities and volumes. Under both a Mediterranean and periglacial climate scenario, for example, the infiltration of water might be reduced leading to a change from advective flow to diffusive flow, and flow paths could change at that point."*

8.18 Dr Starmer is correct in saying that Nirex does not adjust the base case hydrology for climate conditions. However, the period to the onset of Periglacial climate conditions will be characterised by a slowly cooling regime in which only very limited changes of flow pattern are expected. The levels of discharge to surface of the most significant radionuclides, chlorine-36 and iodine-129, should not at that time lead to unacceptable risks.

8.19 Nirex has previously acknowledged and justified its use of constant base case hydrology and considered more generally the potential effects of transitions between states. These potential effects are reviewed at Section 2.2 of *Nirex Report S/95/003* (pages 12 to 14) [COR/527]. This reports on a study undertaken to determine whether any important processes or events had been neglected because of the representation of climate change as a succession of discrete climate states, which concluded:

*"Overall, the authors were unable to identify any major features of transition periods which invalidate the treatment of future climate change as a succession of discrete climate states."*

Also, in evaluating the potential for increased risk from climate transitions, the timing of the changes in relation to the peaks in risk (a period of warming after the next ice age is likely to be after the peak associated with chlorine-36 and iodine-129) and the lower risks associated with Periglacial conditions due to lower agricultural productivity (as shown in *Nirex 95*, Volume 3 (Figure 6.6) [COR/522]) need to be taken into account. Nevertheless, further studies are planned to consider transient effects in more detail.

8.20 The effects of climate change on the hydrogeology at depth is treated in paragraphs 9.67 to 9.70 of my Supplementary Proof of Evidence.

Point iii: The Effect of "Down-Cutting" of Streams in the Boreal Climate

8.21 Dr Starmer asserts that (PE/CCC/5, paragraph 5.1.8):

*"The Boreal climate state is not yet adequately treated in Nirex's analyses in terms of the effect of down-cutting of streams due to reduced sea levels. Whilst recognising this phenomena, Nirex does not at this point fully incorporate the potential effects of down-cutting."*

8.22 In general terms, the presence of incised streams is advantageous, as they provide a direct hydraulic connection between groundwaters and surface water bodies. Such a direct connection enhances the degree of subsurface routing that will occur because it provides a pathway by which contamination can enter surface waters without passing through the soil system. This results in a reduction of exposure to radioactivity and therefore lower risks. The hypothetical catchment studies undertaken by Nirex include streams incised into the associated catchments with channel depths and widths commensurate with the water flows appropriate to those streams. The degree of incision required to accommodate the calculated water flows is very limited as shown in *Nirex Report S/95/002* (Figure 6, page 16) [COR/526]. Thus, contrary to Dr Starmer's assertion, incised rivers and streams are currently incorporated into Nirex's assessments. Furthermore, any increase in the degree of incision would tend to decrease rather than increase risk.

Point iv. Temperate Terrestrial Discharge Variants

8.23 Dr Starmer asserts that Nirex does not (but should) present results from variant groundwater flow models for a Temperate terrestrial discharge which would show higher risks (PE/CCC/5, paragraph 3.2.19).

8.24 Dr Starmer is correct in stating that Nirex did not present results from variant groundwater flow models for a Temperate terrestrial discharge in *Nirex 95*, Volume 3 [COR/522]. However, the fact that the higher productivity of the land surface in the Temperate terrestrial biosphere state leads to a small increase in risk over that for the Boreal terrestrial biosphere is clearly shown in *Nirex 95*, Volume 3 (Table 6.18, paragraph 6.36) [COR/522]. Moreover, the biosphere pathways do not change as a consequence of different groundwater flow models and the same relationship as shown in that Table would be expected between calculated risks for Boreal and Temperate climate states for the variant cases. Hence, there would be virtually no difference for the early peak due to chlorine-36 and a calculated risk higher by a factor of approximately two for the peak occurring at about a million years in the future due to uranium-238. The Boreal terrestrial biosphere state is chosen because:

the discharge of radionuclides would be 100% to land rather than the mix of marine and terrestrial discharge appropriate to a Temperate climate; and

in the long term future the Boreal climate state will be prevalent over the Temperate at the Sellafield location.

Point v: Contaminated Marine Sediments

8.25 Dr Starmer asserts that Nirex has not addressed the possibility that "up-welling" of ground water offshore will result in contaminated sediments which will subsequently be exposed under Boreal climate conditions (PE/CCC/5, paragraph 5.1.7).

8.26 This is not correct. The approach adopted by Nirex is as described below.

8.27 Given the high chloride levels in the marine environment, very little chlorine-36 would be expected to be sorbed to these sediments. Thus, the important case is the one used by Nirex, in which chlorine-36 enters and is sorbed to the soils subsequently generated from the sediments in a terrestrial environment. For comparison, the  $K_d$  value recommended by the International Atomic Energy Authority (IAEA), in *IAEA Technical Report Series No. 247, Sediment  $K_d$ s and Concentration Factors for Radionuclides in the Marine Environment, 1985* ("The IAEA  $K_d$ s Report") (NRX/15/15, Table III, page 15) for coastal marine sediments for chlorine-36 is  $3 \times 10^{-5} \text{ m}^3 \text{ kg}^{-1}$ , whereas a value of  $1 \text{ m}^3 \text{ kg}^{-1}$  (some 30,000 times greater) is used by Nirex for terrestrial soils (Gleys, Peats and Spodosols) (*Nirex 95*, Volume 3, page 5.4) [COR/522]. A higher value of  $K_d$  results in a greater uptake of radioactivity in the food chain and hence a higher dose.

8.28 In the case of iodine-129, modest concentrations could build up in offshore marine sediments (the value given in the *IAEA  $K_d$ s Report* (NRX/15/15, page 17) is  $0.02 \text{ m}^3 \text{ kg}^{-1}$ ) but comparable  $K_d$  values are adopted for Terrestrial soils by Nirex. *Nirex 95*, Volume 3 (page 5.4) [COR/522] gives values of  $0.0065 \text{ m}^3 \text{ kg}^{-1}$  for Brown Earths and Sands and  $0.13 \text{ m}^3 \text{ kg}^{-1}$  for Gleys, Peats and Spodosols.

8.29 In conclusion, there is not strong sorption of significant radionuclides to submerged marine sediments and the approach adopted by Nirex does not lead to an underestimation of risk as proposed by Dr Starmer (PE/CCC/5, page 33, paragraph 5.1.7).

### The Agricultural Well Scenario

8.30 In summary, Dr Starmer makes the following assertions in his Proof of Evidence (PE/CCC/5) concerning Nirex's agricultural well scenario:

i. As a better picture of the site develops, it is likely that the calculated risks in Nirex's analysis of the agricultural well scenario (which already shows risks which are above the "*risk limit*" [sic: should be risk target]) will increase (PE/CCC/5, paragraph 4.4.2). In particular:

i.	the well in Nirex's analysis is only 50m deep and is not likely to have been set at the maximum plume concentration (PE/CCC/5, paragraph 4.4.1); and consideration of nuclides (such as nickel-59, selenium-79 and tin-126) which Nirex has not so far included in the analysis will add to the risk peak (PE/CCC/5, paragraph 4.4.1).
ii.	By using the " <i>transient case</i> " Nirex has shown a dilution in the biosphere of 2,000 rather than the " <i>standard 600 for the base case analyses</i> " (PE/CCC/5, paragraph 4.4.1).
iii.	In future drier conditions, there could be increased reliance on groundwater wells as a source of water for crops and livestock (PE/CCC/5, paragraph 5.1.5).
iv.	The risk may be higher if well water use were added to " <i>exposure pathways using only biosphere models</i> " (PE/CC/5, paragraph 4.4.4).

8.31 I deal in detail with these assertions paragraph 8.32 to 8.49 but in summary my response is that:

i.	calculated risks are unlikely to increase as a result of the reasons quoted by Dr Starmer;
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	ii.	the second is correct because it is the appropriate treatment as explained in information provided in <i>Nirex 95</i> , Volume 3 (Section 2.3, pages 2.9 to 2.13) [COR/522];
	iii.	the third is correct, but even if the changes envisaged were to materialise they would not have a significant effect on risk because the dominant exposure pathways are not irrigation-dependent; and
	iv.	the fourth is ill-founded and reflects a failure to understand that the agricultural well scenario is an alternative and conservative biosphere model; the risks would decrease if all the water requirements were not met by the agricultural well, as is likely to be the case.

#### Point i: The Effect of Further Information on the Calculated Risk

8.32 Dr Starmer asserts that, as a better picture of the site develops, it is likely that the calculation of the risk in Nirex's analysis of the agricultural well scenario (which already shows risks which are above "*the risk limit*" [sic: should be risk target]) will increase (PE/CCC/5, paragraph 4.4.2). This is because, so he states (PE/CCC/5, paragraph 4.4.1):

*"The details of the scenario are sketchy, but the well is only 50m deep and is therefore, not likely to have been set at the maximum plume concentrations available only about 50m deeper, around 100m, in the aquifer (Figure 2.11 of COR/522 Vol. 3). The analysis only includes the radionuclides Cl-36 and I-129 and uses the transient case for these..."*

8.33 As described at pages 6.18 to 6.20 of Volume 3 of *Nirex 95* [COR/522] the risk calculations were based on average concentrations at a depth of 50m bOD which, in terms of the West Cumbrian coastal profile, involves depths of approximately 100m. The case was chosen to be conservative and samples the "*maximum plume concentrations*" indicated by Dr Starmer. He seems not to have recognised this. It should however be noted that the wells referred to are agricultural wells. As substantial volumes of water would be encountered in the regional aquifer at depths above 50m bOD there would be no need to go deeper.

8.34 I agree that there is a need to treat a wider range of radionuclides and to provide a probabilistic treatment. This work is planned, as noted in *Nirex 95*, Volume 3 (page 6.19) [COR/522]. The radionuclides identified by Dr Starmer as possibly adding to the peak are predicted to be strongly retarded in the near field of the repository and the geosphere (tin [Sn]-126) or in the geosphere (nickel [Ni]-59 and selenium [Se]-79) and would decay before completing their transport to within 100m of the surface, as indicated in *Nirex 95*, Volume 3 (page 8.4) [COR/522].

8.35 However, Dr Starmer has failed to mention an important factor that would lead to a reduction in risk from the agricultural well pathway. This factor is explicitly discussed in *Nirex 95*, Volume 3 (page 6.19) [COR/522]. The relevant text is reproduced below:

*"Radionuclide concentrations used in assessing risks to critical groups utilising water from agricultural wells are those calculated for the upper few tens of metres of the regional aquifer. Although such wells are likely to be cased through the overlying Quaternary deposits, it is likely that their yield will be a mixture of aquifer water and recent meteoric water. Since the wells are not represented explicitly in the 2D cross section used for radionuclide transport calculations (see subsection 2.3 of this volume), the degree of dilution of aquifer radionuclide concentrations by recent meteoric water may have been underestimated. Thus the radionuclide concentrations and hence the associated risks are thought to have been overestimated in this respect.*

*The degree of possible overestimation will be investigated in the future work programme, taking account of the structure of the superficial sediments determined in the Site Characterisation Programme."*

8.36 Specifically, if there is effective mixing of recent meteoric water and aquifer water, risks can be expected to decrease.

8.37 It should also be pointed out that the assessed risk assumes all irrigation, animal drinking and domestic supplies are drawn from the agricultural well in *Nirex 95*, Volume 3 (page 13) [COR/522]. In practice, a mix of

well, local meteoric and externally supplied waters is likely to be used to resource one or more of these requirements, thereby reducing risks further.

8.38 Hence, the assertion that, as a better picture of the site emerges, the risks are likely to get higher, is unfounded. The reverse is true as more detail of the superficial deposits in particular will allow the current conservatism in the treatment of near-surface mixing to be redressed.

#### Point ii: Dilution and the Use of the "Transient Case"

8.39 Dr Starmer criticises Nirex for using the "*transient case*" "*invoking in the process a factor of 2000 dilution, rather than the standard 600 for the base case analysis*" (PE/CCC/5, paragraph 4.4.1).

8.40 The transient calculations referred to provide a more realistic estimate of dilution for chlorine-36 and iodine-129 than do the steady state calculations. Specifically, as stated in *Nirex 95*, Volume 3 (page 2.12) [COR/522], the steady state calculations neglect:

*"the effects of the finite duration of the source term and spreading in the geosphere, which would lead to a greater dilution"*.

8.41 Therefore the transient calculations (the "*transient case*") should be used as they are more realistic; the steady state calculations neglect these important effects on chlorine-36 and iodine-129 behaviour.

#### Point iii: The Effect of Climate Change on Water Usage

8.42 At paragraph 5.1.5 of his Proof of Evidence (PE/CCC/5) Dr Starmer states that:

*"COR/522 asserts that the greenhouse effect is expected to last for at least the next 10,000 years (page 6.10 of COR/522, Vol 3) and, further, that the effect will push the local climate in the direction of a Mediterranean climate similar to that experienced at Bordeaux in France. This hotter, dryer climate could change basic usage of water and increase reliance on groundwater as a resource. For example, the 50 mm of water assumed by Nirex for irrigation of drought-sensitive crops (pages 5.13 and 5.14 of COR/522, Vol 3) might have to be increased and the use of irrigation might be required for general agricultural use. For example, the drought this summer has severely stressed the surface water supply and further stress could require increased reliance on groundwater wells as a source of water for drinking and domestic use, livestock and irrigation."*

8.43 Dr Starmer's assertion is not strictly correct in that *Nirex 95*, Volume 3 (page 6.10) [COR/522] actually states:

*"the future climate might be either Mediterranean or Temperate for the next 25,000 years"*

8.44 However, this is largely irrelevant because, as examination of *Nirex 95*, Volume 3 (Table 5.8, page 5.25) [COR/522] shows, the dominant pathways are consumption of animal products for chlorine-36 and the ingestion of drinking water for iodine-129. Neither of these pathways is irrigation dependent. 100% reliance on groundwater resources from the agricultural well is already assumed for the assessment calculations, so the reliance cannot be increased.

8.45 Thus, even if the changes envisaged by Dr Starmer were to materialise, they would not have the effect which he fears.

#### Point iv: The Effect of Biosphere Exposure

8.46 Dr Starmer asserts that the risk may be higher if well water use were added to "*exposure pathways using only biosphere models*" (PE/CCC/5, paragraph 4.4.4).

8.47 I state in paragraph 8.44 of this Supplementary Proof of Evidence that 100% reliance on groundwater resources from the agricultural well is already assumed for the assessment calculations. Therefore, the proposal that risks could be increased by adding the risks from the agricultural well scenario to the risks from biosphere

modelling is ill-founded and reflects a failure to understand that the agricultural well scenario is an alternative and conservative biosphere model. The risks would decrease if all the water requirements were not met by the agricultural well, as is likely to be the case in reality.

## Summary

8.48 In summary, I conclude that:

i.	the alternative scenarios and analyses proposed by Dr Starmer are unlikely to result in significant increases in risk as they are already addressed in the Nirex 95 [COR/522] evaluation or have been considered separately and shown to have little impact; and
ii.	Dr Starmer's view that further analysis will lead to increased estimates of risk for the agricultural well is incorrect as it is shown that the associated risk is not sensitive to the scenarios he proposes or that he has misunderstood the information provided.

## 9. RESULTS OF GROUNDWATER FLOW MODELLING

9.1 The Proofs of Evidence by Professor Mather (PE/CCC/4, paragraphs 1.30, 6.65 and 6.4.11), Dr Starmer (PE/CCC/5, paragraphs 4.2.1 to 4.2.5, 4.3.1, 4.3.2, 4.5.1 to 4.5.5, 4.6.1 to 4.6.3, 5.1.4, 5.1.6, 5.1.9 and 5.3.1 to 5.3.3), Dr Salmon (PE/FOE/5, 3.17, 3.18, 3.22, 3.24 and 7.9) and Dr Hencher (PE/FOE/6, 7.9, 7.11, 9.8, 9.9 and 9.11) make a number of observations on, and criticisms of, the results of the groundwater flow and radionuclide transport modelling performed in *Nirex 95* [COR/522] and on the approach to modelling adopted in the analysis. These comments fall into three groups of points related to:

modelling approach: it is asserted that aspects of the modelling work are inappropriate or in some way deficient;

specific results of the modelling: it is asserted that the assessment models do not deal appropriately with aspects of the behaviour of or structure of the site; and

features alleged to be excluded from the models: it is asserted that certain, potentially important, physical processes have not been included in the models.

9.2 I will respond in detail to these points in paragraphs 9.3 to 9.71 of my Supplementary Proof of Evidence but, in outline, my response to these points is, respectively:

- that the modelling approach is appropriate and not deficient and that the points raised generally indicate that important aspects of the Nirex approach have not been recognised by the Objectors;
- that the models do deal appropriately with these aspects of the behaviour of or structure of the site and that the criticisms generally indicate that the basis for the results or the nature of the results has not been appreciated; and
- that the features or processes which the Objectors argue should be included actually are included in the modelling, when they are realistic features or processes. When this is not the case it is because there are no sound physical arguments for their inclusion.

Each of these topics is dealt with below.

### Modelling Approach

9.3 The Proofs of Evidence by Professor Mather (PE/CCC/4), Dr Starmer (PE/CCC/5), Dr Salmon (PE/FOE/5) and Dr Hencher (PE/FOE/6) contain a number of comments on and criticisms of the approach to hydrogeological modelling used in *Nirex 95* [COR/522]. These criticisms fall into three groups, relating to the following topics:

i.	development of the conceptual model of the system and the method of deriving the parameters of the models (PE/FOE/5, pages 25 and 26, paragraphs 3.17 and 3.18, and PE/FOE/6, page 20, paragraph 7.9) - addressed at paragraphs 9.11 to 9.17 of my Supplementary Proof of Evidence;
ii.	fracture-network modelling (PE/FOE/6, pages 21, 26 and 27, paragraphs 7.11, 9.8, 9.9 and 9.11, and PE/FOE/5, page 28, paragraph 3.24) - addressed at paragraphs 9.18 to 9.27 of my Supplementary Proof of Evidence; and
iii.	regional flow modelling (PE/FOE/5, page 27, paragraph 3.22, and PE/CCC/5, pages 35 and 36, paragraphs 5.2.2 and 5.2.3) - addressed at paragraphs 9.28 to 9.33 of my Supplementary Proof of Evidence.

9.4 It will be demonstrated in the following paragraphs that these criticisms generally arise from a misunderstanding of the modelling approach and, in particular, from a failure to appreciate the roles played by the various models used in the performance analysis. These topics are addressed in turn in the following sections. However, in order to set my responses in context it is appropriate to make first a few preliminary comments on the overall modelling approach that was adopted in *Nirex 95* [COR/522].

9.5 The first stage in the development of the models encompassed the development of the conceptual model of the system and of parameters for that model together with the derivation of the effective hydrogeological parameters for regional-scale models. This process is described in Volumes 1 and 2 of *Nirex 95* [COR/522].

9.6 Numerical fracture-network models were used in the upscaling process to derive the regional-scale effective hydrogeological parameters for the BVG. This is described in Section 5.1 (pages 5.2 to 5.10) of Volume 2 of *Nirex 95* [COR/522]. Fracture-network models were also used to model the flow and transport in a region around the repository (Section 2.2, pages 2.6 to 2.9, Volume 3 of *Nirex 95* [COR/522]). The results of these models were used to develop the parameter distributions for the MASCOT submodels of radionuclide transport in the BVG that were used in the calculation of risk.

9.7 The NAPSAC fracture-network code that was used for this work is a widely used computer program that was developed by AEA Technology. Licenses for its use have been purchased by several agencies with an interest in nuclear energy and radioactive waste disposal such as the Nationale Genossenschaft fuer die Lagerung radioaktiver Abfaelle (NAGRA) in Switzerland, the Nuclear Environment Management Centre (NEMAC) in Korea and the Japanese Ministry (MITI) responsible for these matters. NAPSAC was also available to and utilised by participants in the Stripa project (see FOE/6/15 for a discussion of the Stripa project). Copies of NAPSAC have been obtained by VTT (Technical Research Centre of Finland) and Atomic Energy of Canada Ltd. Its use is also being considered by the Gesellschaft fuer Anlagen und Reaktorsicherheit (GRS) in Germany. A copy has also been obtained by the University of Birmingham in the UK to explore the possibility of using the advanced techniques developed within radioactive waste disposal programmes in water resources analysis.

9.8 NAPSAC has been well-verified (*Nirex Report S/94/004*, page 6) [COR/510], both by comparison against simple analytical cases and by comparison with the results of other fracture-network programs. This means that it has been confirmed that the code produces correct results for a range of test cases and is solving correctly the system of equations that are implemented in the code. A cross-code verification exercise was an initial stage of the modelling work in the Stripa Project (NRX/16/2, page 47). An overview of the successes of the Stripa Project is given in paragraphs 9.24, 9.25 and 10.16 of my Supplementary Proof of Evidence.

9.9 In *Nirex 95* [COR/522], two-dimensional continuum porous medium models of vertical cross-sections were used to calculate groundwater flow and radionuclide transport on a regional scale. A sensitivity analysis of the results of the groundwater flow models was used to develop the parameter distributions for the MASCOT submodels of radionuclide transport in the sedimentary units. The results of the radionuclide transport calculations were used in the calculations performed for the agricultural well scenario.

9.10 The NAMMU groundwater flow and transport code that was used for the work described in paragraph 9.9 above is a well verified and widely used computer program that was developed by AEA Technology. Licenses for its use have been purchased by several agencies with an interest in nuclear energy and radioactive waste disposal such as the Nationale Genossenschaft fuer die Lagerung radioaktiver Abfaelle (NAGRA) in Switzerland, the

Nuclear Environment Management Centre (NEMAC) in Korea, the Gesellschaft fuer Anlagen und Reaktorsicherheit (GRS) in Germany and the Swedish Nuclear Fuel and Waste Management Company, SKB. Her Majesty's Inspectorate of Pollution (HMIP) also owns a licence for NAMMU and it has been used by HMIP consultants. A NAMMU User Group, with participants from these and other countries meets regularly. NAMMU has also been well-verified (*Nirex Report S/94/004*, page 6) [COR/510], both by comparison against simple analytical cases and by comparison with the results of other groundwater flow programs in international intercomparison exercise such as the HYDROCOIN Project.

Point i: Development of the Conceptual Model and Parameters

9.11 Criticisms in these areas were raised by Professor Mather (PE/CCC/4) and Dr Salmon (PE/FOE/5). The majority of these points have been addressed in Dr Chaplow's Supplementary Proof of Evidence ([paragraphs 8.10 and 8.17](#) to 8.26 of **PE/NRX/14/S1**, respectively).

9.12 Therefore, in my Supplementary Proof of Evidence I will only comment on assertions made by Dr Salmon and Dr Hencher that are related to the elicitation and modelling procedures followed in *Nirex 95* [COR/522]. Specifically, these assertions relate to the elicitation procedures adopted in *Nirex 5* (PE/FOE/5, paragraph 3.17), the process of upscaling (PE/FOE/5, paragraph 3.18) and the use of continuum porous-medium models to represent flow and transport in fractured rock (PE/FOE/6, paragraph 7.9). These assertions are addressed in turn in paragraphs 9.13 to 9.17 of my Supplementary Proof of Evidence.

9.13 In paragraph 3.17 (page 25) of his Proof of Evidence (PE/FOE/5), Dr Salmon comments on the elicitation procedures in *Nirex 95* and suggests that

*"the apparent omission by Nirex to elicit the individuals estimates of the CDFs by means of a questionnaire prior to the main elicitation meetings may have artificially suppressed the range of possible CDFs identified."*

In fact, experience of data elicitation indicates that precisely the opposite is true. It is important that participants should not be anchored to a particular position (which may be biased). The danger of anchoring on best estimates is recognised in the elicitation procedure and discussion of extreme outcomes is an important part of the process. In addition, the careful definition of the quantity to be elicited, in a way that is agreed by all participants to be appropriate, is a vital part of the elicitation process and can only be achieved when all the participants can interact (*Nirex Report S/94/002*, page 9, Box D) [COR/508]. The approach suggested by Dr Salmon would result in the definition being pre-judged by the person formulating the questionnaire, leading to potential ambiguities in interpretation.

9.14 In paragraph 3.18 of his Proof of Evidence (PE/FOE/5), Dr Salmon states that

*"The process of scaling up the hydraulic CDFs and PDFs from the individual fracture to the effective parameter scale introduces additional uncertainties."*

He does not indicate the supposed source of these "*additional uncertainties*". In any event, his statement is incorrect. The process of upscaling does not, in itself, give rise to additional uncertainty. Rather it is important to ensure that the uncertainties in the properties identified in the smaller scale models are quantified and carried through the upscaling analysis so that the calculated effective properties take account of these uncertainties. As described in *Nirex 95* (Section 5.1.8, page 5.10 of Volume 2) [COR/522], this is what was done in the *Nirex 95* analysis.

9.15 In paragraph 7.9, page 20 of his Proof of Evidence (PE/FOE/6) Dr Hencher gives the following quotation from *Geier 1992* (FOE/6/4):

*"even a set of 5 packer tests, all near the centre of a block, gives very little information about the block conductivity on a 40m scale. This means "conditional" SC [Stochastic Continuum] models based on such data may not be "well conditioned"."*

and states that this:

*"reaffirms the difficulties in using a continuum model to represent a heterogeneous, fractured rock mass"*

However, it is clear from the content of the quotation itself that Geier is not addressing the general issue of the use of continuum models to represent fractured rock, but is making a much more restricted specific point about the difficulty of conditioning a particular type of stochastic continuum model using a particular type of data (which is not expected to be suitable for that particular purpose (FOE/6/4, page 86). Thus the quotation does not support the point made by Dr Hencher.

9.16 In addition, Dr Hencher's assertion concerning the difficulties of using a continuum model are not supported by the following quotation, also taken from *Geier 1992* (FOE/6/4). It is stated (page 189) in connection with the outcome of the project (which used Discrete Fracture Network (DFN) models) that:

*"A major result has been the development and demonstration of a DFN methodology to interpret packer tests in terms of fracture network properties and predict relationships between packer test results and block scale properties".*

Thus, the authors conclude that the study has demonstrated a methodology to predict relationships between test results and block scale properties and indicate that this methodology is based on the use of discrete fracture-network models. This methodology is, therefore, similar to the approach used in *Nirex 95* [COR/522]. Thus, the work in *Geier 1992* (FOE/6/4) actually provides support for the methodology adopted in *Nirex 95* [COR/522].

9.17 In any event, I am aware of the advantages and disadvantages of both continuum porous medium models and fracture-network models. It is, of course, important that continuum models of fractured rock should only be used in regimes in which the continuum approximation is appropriate and that the effective parameter values for continuum models should be carefully calculated. This is precisely the approach that has been adopted in *Nirex 95* [COR/522], in which continuum models of the fractured host rock were used where appropriate and the parameter values for the continuum model were obtained by a careful process of upscaling (Section 5.1, pages 5.2 to 5.10 of Volume 2 of *Nirex 95*) [COR/522]. However, fracture-network models were also used for the region of host rock around the repository, in order to allow explicit representation of the fracture system in this domain.

Point ii: Fracture network modelling

9.18 In their Proofs of Evidence Dr Salmon (PE/FOE/5) and Dr Hencher (PE/FOE/6) make a number of assertions related to the modelling of fractured rocks in general and in *Nirex 95* in particular. It is suggested that fracture-network models (also called "*discontinuum models*" by Dr Hencher) have an unproved track record (PE/FOE/6, paragraphs 7.11 and 9.11), that the results of projects which sought to validate such models were not encouraging (PE/FOE/6, paragraphs 9.8 and 9.9) and that the treatment of travel times in fractured rock in *Nirex 95* was inadequate (PE/FOE/5, paragraph 3.24). These points are refuted in paragraphs 9.19 to 9.27 and 10.16 of my Supplementary Proof of Evidence.

9.19 In his Proof of Evidence (PE/FOE/6, paragraphs 7.11 and 9.11) Dr Hencher suggests that fracture-network models (also called discontinuum models by Dr Hencher) have an "*unproved track record*". However, this view is not supported by the fact that fracture-network modelling is widely recognised as being an appropriate technique for modelling groundwater flow and transport in low-permeability fractured rocks such as the BVG, in which the flow is predominantly within the fractures. Fracture-network modelling is used within the Swedish, US, Swiss, Finnish, and Japanese national radioactive waste disposal programmes, and of course the UK programme.

9.20 Dr Hencher's view that fracture-network models have an unproved track record contrasts with that expressed by Olsson and Gale in a reference which Dr Hencher himself quotes (FOE/6/15). These authors state that in connection with the Stripa Site Characterisation and Validation Project (SCV) that (page S29):

*"During the course of the SCV project, the 3D discrete fracture flow codes evolved from being research tools to practical tools capable of representing flow through physically realistic fracture systems. Application of these codes to the SCV site demonstrated that the discrete fracture flow models can be*

*constructed from field data normally obtained during a comprehensive site investigation programme. In addition, using the discrete codes to generate the properties for large-scale porous medium models provides a tool for bridging a range of scales while retaining much of the inherent variability in the flow and transport properties of fractured rock masses."*

It should be noted that this comment refers to the Stripa project which ended in 1992. This view, by acknowledged experts on fracture flow systems, in a publication to which Dr Hencher himself refers, together with the widespread use of fracture-network models, would suggest that Dr Hencher's view is not widely supported.

9.21 Continuing with the theme of the limitations of fracture-network models, in paragraph 7.11 of his Proof of Evidence (PE/FOE/6), Dr Hencher states that:

*"Geier (1992) [FOE/6/4] reports that discrete fracture models rely on the geotechnical model being "statistically homogeneous" which may limit their application. For example Kulatilake (1990)[FOE/6/7], in his work at Stripa, found the largest "statistically homogeneous" region to be 10m long."*

No precise reference is given for the reference from Geier 1992, but it may be that Dr Hencher is referring to the passage on page 39, which states:

*"Strictly speaking, valid DFN models are restricted to the simulation of regions composed of domains within which the fracture population is statistically homogeneous. Most theoretical developments and applications to date have defined statistical homogeneity in very strict terms of spatially uniform distributions of fracture properties, either univariate (e.g. size) or bivariate (e.g. orientation). When this narrow definition of statistical homogeneity is applied, field studies suggest that fracture populations are often not "statistically homogeneous" over any practical modelling scale. For example, in a recent study of a 36m section of tunnel in the Stripa mine (Kutilake et al 1990), the largest "statistically homogeneous" region found was 10m long."(My emphasis)*

The very next paragraph on page 39 of Geier 1992 puts the matter in a rather different perspective. It states:

*"Practical DFN modelling requires a broader definition of statistical homogeneity, based on a statistical description of heterogeneity in the fracture population."*

The rest of the paragraph is concerned with a description of several models that are used to represent the fracture intensities in practice. It is also stated that four of these models are included in the FracMan fracture-network model which Dr Hencher indicates that he uses in his current research (PE/FOE/6, paragraph 1.2).

9.22 Dr Hencher also states (PE/FOE/6, paragraph 9.11) that:

*"Most of the available software has only recently been developed"*

As indicated on page 1 of FOE/5/8, the original version of the NAPSAC fracture-network code, for example, was written in 1985. Formal releases of the software have been made since 1988. Of course, the capabilities of NAPSAC are continuously being extended, but it is not the case that the fracture-network modelling software used by Nirex has the very recent origin suggested by Dr Hencher.

9.23 In paragraphs 9.8 and 9.9 of his Proof (PE/FOE/6, page 26), Dr Hencher implies that the results of studies that sought to validate fracture network codes have not been encouraging and, in particular, in connection with a particular component of the Stripa calibration and validation (SCV) experiment known as the inflow validation tests that (PE/FOE/6, paragraph 9.9) that:

*"The failure of this inflow validation test of the NAPSAC model which is used by Nirex, clearly has significant implications for the RCF proposal"*

This comment is extremely misleading and is presented out of context with the background from the SCV experiment. The test that he refers to was not a test of the ability of the NAPSAC code itself to model groundwater flow, but a test of a particular hypothesis (FOE/6/15) concerning the effects of stress redistribution following

excavation on the hydraulic properties of fracture networks in a gallery wall. It is this particular hypothesis that failed the test, not the capability of NAPSAC to model flow. The successes of the Stripa project can be clearly appreciated by considering the brief account given below of some of the different experiments performed at Stripa.

9.24 The tone of the account given by Dr Hencher of the Stripa project contrasts significantly with the positive comments by Olsson & Gale that were quoted in paragraph 9.20 of my Supplementary Proof of Evidence, and were taken from a reference which Dr Hencher himself quotes (FOE/6/15). The account of the "*significant achievements*" of the Stripa project given in pages 55 to 57 of the Executive Summary of the *Overview Report of the Stripa Project* (NRX/16/2) is also more positive than Dr Hencher's account. On page 57 of NRX/16/2 one of the "*significant achievements*" of the Stripa project is listed as:

*"Demonstration of the applicability of the fracture flow modelling for simulating groundwater flow and solute transport"*

Thus it is clear that Dr Hencher's view is not supported.

9.25 Inflows to the D-hole experiment at Stripa were predicted using a NAPSAC model and other fracture-network models. It was found that the results of all the models were in reasonable agreement with observations (NRX/16/2, page 49, Section 4.5.4). The various fracture-network models were also used to predict tracer movement. It was found that once it had been calibrated against the flow (clearly a necessary stage in any transport modelling), the NAPSAC model was able to predict the movement of tracer (FOE/6/15, Figure 11). The observed dispersion of the tracer was explained simply in terms of the dispersive effects of the fracture-network geometry. Taken together, the validation studies carried out for the Stripa project build confidence in our ability to characterise and explain groundwater flow and the movement of tracers in fractured rocks. The use of progressive cycles of validation work in the Nirex Programme and, in particular, the role of the RCF in this process, is described in Section 10 of my Supplementary Proof of Evidence.

9.26 In paragraph 3.24 of his Proof of Evidence (PE/FOE/5), Dr Salmon makes several related criticisms of the calculation of travel times in fractured rock performed in *Nirex 95* [COR/522]. His argument is that:

- the *Royal Society Study Group Report* [COR/605] has expressed concern that Nirex is not exploring the scale-up problems sufficiently;
- the impact of this on calculated travel times used in the performance assessment could be considerable, because potential pathways from the repository to the surface could be overlooked and because calculated flow velocities may not be adequately "*corrected*" to take account of relatively rapid groundwater flow through larger fractures; and
- travel times in fractured rock are obtained by scaling results obtained using a matrix porosity and that the scaling may be inadequate.

9.27 My response to these three points is as follows:

- Dr Salmon refers to *The Royal Society Study Group Report* [COR/605]. The relevant paragraph (page 135) starts:

*"A further concern relates to the scaling up of parameters"*.

This paragraph is referring to a study conducted in early 1993, based on limited data (see [PE/NRX/14/S1, paragraph 8.25](#), page 65 for more information on this). Concerning the procedure in the 1993 study, the paragraph in [COR/605] subsequently continues:

*"Ideally it would be largely replaced by one of scale-up calculations based on extensive field data. However, because Nirex cannot compromise the integrity of the site by drilling a large number of boreholes, the field data available will always be limited, both in absolute terms and relative to some other applications of groundwater flow modelling. The RCF should help in this respect."*

This is the approach being adopted by Nirex and used in *Nirex 95* (see Section 5.1 (pages 5.2 to 5.10) of Volume 2 of *Nirex 95*) [COR/522]. Data obtained from the RCF will indeed enable this approach to be pursued further (see [paragraphs 5.8](#) to 5.10, pages 36 to 38, of Dr Chaplow's Supplementary Proof of Evidence, **PE/NRX/14/S1**).

- Dr Salmon is incorrect in asserting that, because upscaling of properties for the fractured rock has not been treated appropriately, important pathways could be neglected and travel times underestimated. This is because the upscaled effective properties (which are in any case appropriate) were not used in the calculation of the pathways and travel times in the fractured BVG in *Nirex 95*. These quantities are calculated directly using the NAPSAC discrete fracture-network code (as is made clear in Section 2.2 (pages 2.6 to 2.9) of Volume 3 of COR/522).
- Again, Dr Salmon is incorrect because the travel times in the fractured rock are obtained directly using NAPSAC and not by the scaling approach that he suggests. In any case, his comment on the inadequacy of the scaling approach for individual fractures is not relevant to transport through a large network of fractures, providing that the effective properties of the network have been calculated appropriately.

Point iii: Regional flow modelling

9.28 When discussing the regional scale continuum porous medium modelling performed in *Nirex 95* [COR/522], Dr Starmer and Dr Salmon make the following two assertions:

that no sensitivity analysis was performed as part of the study (PE/FOE/5, paragraph 3.22); and

that results from earlier models of this type were difficult to believe and that the results from the *Nirex 95* model show modified behaviour (PE/CCC/5, paragraphs 5.2.2 and 5.2.3).

These assertions are refuted in turn in paragraphs 9.29 to 9.33 of my Supplementary Proof of Evidence.

9.29 When discussing the results of the NAMMU regional-scale flow models Dr Salmon states that (PE/FOE/5, paragraph 3.22):

*"Nirex has not even reported the results of any sensitivity analysis"*

This statement is incorrect. In the course of *Nirex 95* [COR/522] many groundwater flow calculations were performed in which parameter values were systematically changed from those used in the base case. These calculations were used to explore the sensitivity of results of interest (e.g. travel times) to the input values of the hydrogeological parameters. This is discussed in Section 2.4.2 of Volume 3 of *Nirex 95* and the sensitivities of the travel times are given in Table 2.6 of that document. This procedure is used to ensure that the uncertainties in the hydrogeological parameters are taken account of appropriately in the PSA calculations of risk. The probability density functions for the groundwater flow parameters that are used in the MASCOT calculations of risk are based on the sensitivities derived from the set of groundwater flow calculations with changed parameter values (see Section 2.4.2 and Figure 2.13 of Volume 3 of *Nirex 95*). I would therefore agree with Dr Salmon (PE/FOE/5, paragraph 3.22) that:

*"Sensitivity analysis is standard modelling practice when trying to represent such complex hydrogeological conditions, and would assist in quantifying the uncertainties associated with the model results"*

The results of the NAMMU sensitivity analysis are used in precisely this way.

9.30 In paragraph 5.2.2 of his Proof of Evidence (PE/CCC/5, page 35), Dr Starmer states that

*"Early model results presented by Nirex (in Nirex Report No. 525 - COR/525) were frankly difficult to believe."*

In support of this statement Dr Starmer refers to the *Fourteenth Annual Report of RWMAC* (page 16) [GOV/406] which stated (referring to a flow path shown in Figure 2 of GOV/406) that:

*"This modelled flow path is inconsistent with the totality of field observations. As noted in paragraph 3.18b, there is an upward hydraulic gradient within the SSG immediately to the west of the repository and it would be expected that the flow path would remain in the upper part of the SSG (Calder Sandstone). Indeed, the Nirex observations (3.18b) demonstrate discharge from the top of the SSG above and to the west of the repository zone. Further, the modelled flow path takes a lower density fresh water downwards into a higher density brine. It is difficult to conceive a natural process, in these overall circumstances, which could draw the pathline downwards so deeply within the Seascale Fault Zone. Nirex explains this situation as resulting from the input into the model of a higher hydraulic conductivity for the Seascale Fault Zone. A possibly more plausible hydrogeological flow pathline, consistent with all the available field data, tracks upwards, over a distance of just over a kilometre from the repository zone in the BVG into the SSG to discharge from the SSG into the drift or to the ground surface west of the repository."*

9.31 By quoting this passage in his paragraph 5.2.2 (PE/CCC/5, page 35) in support of his point that early model results were "*difficult to believe*", Dr Starmer appears to endorse the following propositions that are contained in the quotation:

that the modelled flow path takes lower density fresh water downwards into a higher density brine;

that it is "*difficult to conceive*" why the flow path should behave as it does in the Seascale Fault Zone; and

that a more reasonable flow path, that would be consistent with field observations, would discharge just west of the repository zone.

9.32 My response to these points is as follows:

The first point is a misunderstanding which arises from the fact that in Figure 2 (page 15) of GOV/406 (*Fourteenth Annual Report of the RWMAC*), pathlines derived by Nirex from a NAMMU numerical groundwater flow model of one geological cross-section (*Nirex Report 525*, Figure 17, page 36) [COR/505] are superimposed onto a conceptual view of the groundwater flow system given on a different cross-section (*Nirex Report 525*, Figure 13, page 21). This is clearly demonstrated by Figure 9.1 of my Supplementary Proof of Evidence which shows both the position of the NAMMU cross-section on which the pathlines were calculated, (red line) and the line of the geological cross-section on which the conceptual view of the system was presented (blue line). The two cross-sections contain different geological features, in particular, one intersects the Seascale Fault Zone, whereas the other does not. It is, therefore, invalid to draw conclusions from the superposition of these two sets of information. The model did not show pathlines penetrating deeply into the highly saline brines, as incorrectly concluded from this superposition. Instead, it showed some mixing between fresh and saline waters, as may be expected in the transition zone between freshwater and more saline water.

The behaviour of the pathline in the Seascale Fault Zone follows the predicted direction of groundwater flow which was calculated by solving the standard equations for groundwater flow in conjunction with information on rock properties. The model reflects the physically realistic behaviour of flow in the aquifer moving down a fault zone represented, in this case, as being more permeable than the surrounding rock (see *Nirex Report S/94/004*, Box H, pages 14 and 15) [COR/510].

There is no evidence for a near-vertical groundwater path of the type implied. It would not be consistent with the observations of the site, in particular that the sandstone formations carry significant horizontal flows. As discussed in [paragraphs 8.53](#) to 8.59 of the Supplementary Proof of Evidence of Dr Chaplow (PE/NRX/14/S1) observed terrestrial discharges of groundwater are consistent with the predictions of the current groundwater flow models (see also paragraphs 9.37 to 9.39 and 9.49 below).

9.33 I would also note that differences between the results of the models reported in *Nirex Report 525* [COR/505] and *Nirex 95* [COR/522] reflect the fact that considerably more site data, particularly on the distribution of salinity, were available when the Nirex 95 models were developed. Thus, the modified behaviour to which Dr Starmer refers (PE/CCC/5, page 36, paragraph 5.2.3) is not an indication that the earlier models did not behave in a

physically reasonable fashion, but indicates the incorporation of increased knowledge of the site into the modelling process.

### Specific Results of the Modelling

9.34 Two related results of the modelling work are criticised in the Proofs by Dr Starmer (PE/CCC/5, paragraphs 4.2.1 to 4.2.5, 5.1.9 and 5.2.3) and Professor Mather (PE/CCC/4, paragraphs 1.30 and 6.6.5). These are:

	i.	the locations of the groundwater and radionuclide discharges. It is asserted that a marine discharge of radionuclides has been assumed and that evidence of terrestrial discharge of groundwater has been neglected; and
	ii.	the location and geometry of the radionuclide plume. It is asserted that " <i>up-welling</i> " associated with the Fleming Hall Fault Zone has not been addressed in the performance analysis.

I will demonstrate in paragraphs 9.35 to 9.49 of my Supplementary Proof of Evidence that neither of these assertions is correct.

#### Point i: Marine Discharge

9.35 In his Section 4.2 of his Proof of Evidence (PE/CCC/5), Dr Starmer makes a number of claims that relate to the location of radionuclide discharge. Dr Starmer asserts that:

the "*Nirex safety case*" assumes that there will be marine discharge of radionuclides for the Temperate (current) case and ignores evidence of discharge of groundwater inland or at the coast (paragraphs 4.2.1, 4.2.2, 4.2.3 and 5.1.9);

the assumption of marine discharge is important to the "*safety case*" (paragraph 4.2.2);

a "*more reasonable approach*" is to accept that radionuclide discharge is along the shore or further inland (paragraph 4.2.5); and

such an approach would make the well use scenario more important, (paragraph 4.2.5).

9.36 My detailed rebuttal of these points is given in paragraphs 9.37 to 9.41 of my Supplementary Proof of Evidence. In summary, my response is that:

marine discharge of radionuclides in Temperate (current) conditions is not assumed, it is a result of models that are based on the physics of the system. Evidence of discharge of groundwater inland is consistent with these results;

marine discharge is not crucial to the "*safety case*" and terrestrial discharge is also considered;

a predominantly terrestrial discharge of radionuclides in present day conditions (the "*more reasonable approach*" proposed) is not consistent with knowledge of the groundwater flow system; and

a well calculation for an unrealistic situation is not relevant.

9.37 The suggestion that marine discharge of radionuclides is an assumption was also raised by Professor Mather (PE/CCC/4, page vii, paragraph 1.30, and page 39, paragraph 6.6.5) to whom Dr Starmer refers. The Supplementary Proof of Evidence by Dr Chaplow (PE/NRX/14/S1) gives a detailed response to this point at [paragraphs 8.53](#) to 8.59 (pages 74 and 75). Dr Chaplow points out that the locations of groundwater and radionuclide discharges are predicted as an output from the modelling and are not arbitrary assumptions. The models are based on an understanding of the site hydrogeology in current (Temperate) conditions, and indicate that, although the main region of groundwater discharge extends to about 100m offshore, the main region of radionuclide discharge extends from about a hundred metres offshore to about a kilometre offshore (see page 2.12 of Volume 3 of *Nirex 95*) [COR/522]. The models used in *Nirex 95* do predict some level of terrestrial discharge of groundwater and radionuclides under the groundwater and sea level conditions in a Temperate (present day) climate

(see Figure 2.6b of Volume 3 of *Nirex 95*). As will be explained in paragraph 9.40, this is taken into account in the assessment.

9.38 As explained by Dr Chaplow in his original Proof of Evidence ([PE/NRX/14](#)), the behaviour predicted by the models is entirely consistent with the pattern of observations made at the site which suggest that

*"the upper 150 to 200 metres of the Sherwood Sandstone Group is an active aquifer system receiving recharge in the higher ground (Borehole 8) and containing coastward and upward moving freshwater for much of the area where ground level is below about 100 metres aOD." (PE/NRX/14, page 41, [paragraphs 6.55](#) paragraph 6.55).*

Dr Chaplow explains in his Supplementary Proof of Evidence ([PE/NRX/14/S1](#), pages 74 and 75, [paragraphs 8.53](#) to 8.59) that the predictions of the models are also consistent with the observations quoted by Professor Mather. As explained in paragraph 9.39 of my Supplementary Proof of Evidence, this behaviour is also what would be expected on the basis of the physics of the system.

9.39 The radionuclides are carried into the upper sandstones by a weak flux of groundwater from the lower rocks. The upward flux is much weaker than the much larger nearly-horizontal flux in the upper sandstones. This is because the upward flux is controlled by the effective permeability of the BVG, whereas the nearly horizontal flux in the upper sandstones is controlled by the effective permeability of the upper sandstones, which is much larger than the effective permeability of the BVG. The pathlines originating from the repository are, therefore, effectively refracted to lie close to the base of the upper sandstones and the paths discharge at the outer edge of the groundwater discharge region. The radionuclides follow the general route of the pathlines and spread about this as a result of dispersion.

9.40 In any case, the condition of marine discharge of radionuclides is not "*important to the Nirex safety case*" ([PE/CCC/5](#), paragraph 4.2.2). In *Nirex 5*, Volume 3 [COR/522] no claim for marine discharge is made. Indeed, the concept of a fully marine discharge is explicitly discounted in Sections 6.4(a) and 9.1 (pages 6.11 and 9.2, respectively) of that reference. For example the relevant text (items (g) and (h)) from page 9.2 is reproduced in full below:

*"(g) For discharge of all radionuclides to the marine environment, risks would be at least three orders of magnitude below the regulatory target (see Section 6). However, in the Temperate climate state, groundwater flow and radionuclide transport calculations show that it is likely there would be a certain proportion of terrestrial discharge of radionuclides. The risks in the Temperate climate state would then be given to a good approximation by the Temperate terrestrial risk multiplied by the proportion of the discharge that is terrestrial. For instance, the peak risk from  $^{36}\text{Cl}$  for a case in which 25% of the discharge of radionuclides is to a Temperate terrestrial environment would be  $3 \cdot 10^{-8}$ .*

*(h) The Boreal climate state will be associated with a lower sea level than the current sea level. Discharge is therefore likely to be terrestrial. The peak risks in the Boreal terrestrial state are  $1.1 \cdot 10^{-7}$  at 20,000 years after closure and  $3.3 \cdot 10^{-7}$  at 4 million years after closure."*

9.41 Therefore, the statement that a marine discharge is assumed and is relied on, is unjustified. However, the validity of the prediction of a largely marine discharge of radionuclides under Temperate climate conditions is established in paragraphs 9.37 to 9.39 above. Thus, a predominantly terrestrial discharge of radionuclides in present day conditions (the "*more reasonable approach*" proposed by Dr Starmer, ([PE/CCC/5](#), paragraph 4.2.5)) is not consistent with knowledge of the groundwater flow system and hence is not more reasonable. The effect of an unrealistic scenario on the well calculation is, by definition, not relevant. That a certain proportion of the radionuclide discharge in temperate conditions will be terrestrial is accounted for in the calculations performed in *Nirex 95* [COR/522].

Point ii: Plume Location and Geometry

9.42 In his Proof of Evidence (PE/CCC/5, paragraphs 4.3.1 and 4.3.2, pages 24 and 25, Section 4.4, pages 25 to 27) , Dr Starmer discusses a number of issues related to the location, geometry and concentration of the radionuclide plume. In particular he asserts in PE/CCC/5 that:

"*up-welling*" associated with the Fleming Hall Fault Zone is not addressed in the *Nirex 95* [COR/522] analysis (paragraph 4.3.1);

water could move up preferential pathways associated with the Fleming Hall Fault Zone and "*short circuit*" the plume (paragraph 4.3.1);

the Seascale Fault Zone has been identified as a low permeability barrier and could force the plume closer to the surface (paragraph 4.3.2);

these effects could increase predicted risk dramatically (paragraph 4.3.2);

more realistic well calculations are likely to lead to higher risks (paragraph 4.4.2); and

evidence exists for a regime of significant groundwater upflow that could bring "*contaminants*" to the surface (or near the surface) in the vicinity of the repository (paragraph 4.3.2).

9.43 My response to these points is given in detail in paragraphs 9.44 to 9.49 of my Supplementary Proof of Evidence. In brief my response is that:

upflow in the Fleming Hall Fault Zone is actually accounted for in *Nirex 95* [COR/522];

the inclusion of this upflow may be a conservatism in the *Nirex 95* models. Dr Starmer suggests no mechanism to "*short circuit*" the plume;

although the evidence quoted by Dr Starmer (PE/CCC/5, paragraph 4.3.2) for the Seascale Fault Zone being a barrier does not support the proposition, the possibility of reduced permeability across this zone is also accounted for in *Nirex 95*;

there is therefore no support for the dramatic increase in risk suggested by Dr Starmer;

there is no evidence for this proposition, current risks are thought to have been overestimated; and

there is no evidence to support the regime of groundwater upflow suggested by Dr Starmer.

9.44 It is difficult to understand why Dr Starmer should suggest that upflow in the Fleming Hall Fault Zone is not addressed in *Nirex 95* [COR/522], given that his quoted evidence for the effect is Figures 2.6a and 2.11 of Volume 3 of *Nirex 95*. These Figures show, respectively, the groundwater flow field of the base-case groundwater flow model and the calculated radionuclide plume used in the agricultural well calculation. A submodel representing flow and transport in the Fleming Hall Fault Zone is explicitly included in the MASCOT model (see *Nirex 95*, Volume 3, page 2.15). The development of the radionuclide plume calculated in *Nirex 95* (see *Nirex 95*, Figure 2.12 of Volume 3) clearly shows the effect of upflow in the Fleming Hall Fault Zone carrying radionuclides towards the surface. Thus the effects of "*up-welling*" in the Fleming Hall Fault Zone is explicitly included in the performance analysis.

9.45 As noted in paragraph 9.44 of my Supplementary Proof of Evidence, the performance analysis does take account of movement up the preferential pathway associated with the Fleming Hall Fault Zone. It is worth noting that inclusion of this pathway may be a pessimism in the current assessment, as the Fleming Hall Fault Zone may well not behave as a high permeability feature, as was assumed in *Nirex 95* [COR/522]. This would lead to lower risks from the agricultural well as the plume would not be preferentially carried up the flow path associated with the fault zone.

9.46 Dr Starmer also suggests (PE/CCC/5, paragraph 4.3.1) that water may "*short circuit even the plume*". Clearly, for this to happen there would have to be a physical mechanism to move the radionuclide. The movement of the

plume is the result of groundwater flow and dispersive and diffusive processes. Thus, if the plume is to be short-circuited, a physical mechanism in addition to those listed in the last sentence must operate. However, Dr Starmer does not suggest any physical mechanism to produce the "*short circuit*" that he suggests. I would draw attention to the fact that the agricultural well is a mechanism to produce such a "*short circuit*" and that this effect is included in the *Nirex 95* [COR/522] analysis.

9.47 The suggestion that the Seascale Fault Zone is a "*low permeability barrier*" (PE/CCC/5, paragraph 4.3.2) refers to the Proof of Evidence of Professor Mather (PE/CCC/4) but does not give a precise reference. The intended reference may be at paragraph 6.4.11, page 28 of PE/CCC/4 which states:

*"For example, the representation of head contours across the Seascale Fault Zone in Nirex Report No. 524 Vol. 3 (COR/517) interprets the fault as a barrier or a low hydraulic feature....."*

Again, the reference is not precise but the intended passage may be that on page 48 of Volume 3 of *Nirex Report 524* [COR/517] in which Figure 4.5 of that Volume is discussed. This Figure shows head contours on an approximately north-south cross-section. The text on page 4-8 discusses a stepped increase in head of 60m observed across the top part of the Brockram in Borehole 11. The relevant statement from page 4-8 of *Report 524* [COR/517] is:

*"This representation of the head contours interprets the Seascale Faults and Brockram as low hydraulic conductivity features which confine the underlying heads."*

It is clear that the reference is to a low permeability capping feature which confines head values below the level of the Brockram. Thus, there is no reference to low permeability barriers, in the sense of features that could drive flow upwards within the sandstone units nearer the surface, which would be the region relevant to the discussion by Dr Starmer. However, on the recommendation of the expert group that developed the conceptual model for *Nirex 95* [COR/522], a reduction in permeability across the Seascale Fault Zone was actually included in *Nirex 95* (Figure 5.1 of Volume 2). In the *Nirex 95* calculations of radionuclide transport the highest near-surface concentrations in the plume at early times lie between the Seascale and Fleming Hall Fault Zones (*Nirex 95*, Figure 2.12 of Volume 3), carried by the flow in the high permeability zone associated with the Fleming Hall Fault Zone. As noted in paragraph 9.45 above this representation of the Fleming Hall Fault Zone may be conservative. Dr Starmer suggests (PE/CCC/5, paragraph 4.3.2) that if the effects of the fault zones discussed above, (i.e. the Seascale Fault Zone being a low permeability barrier and upflow occurring in the Fleming Hall Fault Zone) were incorporated into the risk calculations, risk could increase dramatically. It has been demonstrated in paragraphs 9.44 to 9.47 that these effects are already accounted for in the assessment. Thus, there is no argument on this basis for a dramatic increase in risk.

9.48 In his Proof of Evidence (PE/CCC/5, Section 4.4), Dr Starmer makes several comments on the well scenario. In particular, Dr Starmer states (PE/CCC/5, page 26, paragraph 4.4.3) that:

*"The results of the variant calculations presented in Chapter 7 of COR/522, Vol 3 indicate that radionuclide concentrations in groundwater may be higher than base case concentrations and could easily increase risk by an order of magnitude (Figures 7.23-7.26 of COR/522, Vol 3)."*

It is difficult to understand the basis for this statement. Table 7.7 of Volume 3 of *Nirex 95* [COR/522] presents the deterministic peak risk for the base case and variant calculations. It can be seen that in those cases in which there is an increase in risk, the increase is a factor of about 2 to 3. In some variants the risk is actually lower than in the base case. Therefore, calculations for the well scenario performed on the basis of the variant groundwater flow calculations could either slightly decrease or slightly increase the calculated risk. When considering possible well calculations, it is important to bear in mind that the current well calculation may contain several pessimisms. As noted in paragraphs 8.35 and 8.36 of my Supplementary Proof of Evidence, the radionuclide concentrations and hence the associated risks are thought to have been overestimated, because the degree of dilution of radionuclide concentrations by recent meteoric water may have been underestimated (Section 6.7, page 6.19, Volume 3 of *Nirex 95*). In addition, as noted in paragraph 9.45 of my Supplementary Proof of Evidence, the treatment of the Fleming Hall Fault Zone as a high permeability feature may be a pessimism in the current assessment. Thus, the contention

by Dr Starmer (PE/CCC/5, page 26, paragraph 4.4.2) that the risk "is likely to move to higher risks rather than lower risks as a better and more realistic picture of the site emerges." is not justified.

9.49 Dr Starmer also suggests (PE/CCC/5, paragraph 4.3.2) that there is evidence of an upward flow and transport regime that could bring contaminants to the surface in the vicinity of the repository. This general topic has already been discussed in paragraphs 9.32 and 9.37 to 9.39 of my Supplementary Proof of Evidence. In paragraph 4.3.1 (page 24) of PE/CCC/5, Dr Starmer refers to the suggestions in the *Fourteenth Annual Report of RWMAC* (paragraph 3.23, page 16 of GOV/406) that were discussed in paragraphs 9.30 to 9.33 of my Supplementary Proof of Evidence. As indicated in those paragraphs, there is no evidence for the flow paths suggested in that report [GOV/406]. In paragraph 4.3.2 (page 25) of PE/CCC/5, Dr Starmer refers to conclusions drawn by Professor Mather regarding upward flow paths (no precise reference is given but Dr Starmer may be referring to paragraphs 6.5.5 and 6.5.6, page 31 of PE/CCC/4). A detailed response to Professor Mather's comments in paragraph 6.5.5 (page 31) of PE/CCC/4 is given in Dr Chaplow's Supplementary Proof of Evidence (PE/NRX/14/S1, pages 95 to 98, [paragraphs 9.11](#) to 9.20). Dr Chaplow concludes that:

*"it is inappropriate for Professor Mather to conclude that the observed salinity profiles are indicators of significant upward flow."*

Thus there is no support for the type of flow regime suggested by Dr Starmer.

### Features Alleged to be Excluded from the Models

9.50 This Section addresses a number of comments by Dr Starmer (PE/CCC/5) in which he claims that various features have not been appropriately addressed in the modelling performed in *Nirex 95* [COR/522]. It is asserted that:

i.	potential short, faster pathways have been neglected and excessive credit has been taken for the process of rock matrix diffusion (PE/CCC/5, pages 27 to 29, paragraphs 4.5.1 to 4.5.5) - addressed in paragraphs 9.51 to 9.59 of my Supplementary Proof of Evidence;
ii.	processes that could reduce dilution have been neglected (PE/CCC/5, pages 29 and 30, paragraphs 4.6.1 to 4.6.3) - addressed in paragraphs 9.60 to 9.64 of my Supplementary Proof of Evidence;
iii.	unreasonable screening of the radionuclide inventory has occurred (PE/CCC/5, pages 37 and 38, paragraphs 5.3.1 to 5.3.3) - addressed in paragraphs 9.65 and 9.66 of my Supplementary Proof of Evidence; and
iv.	certain specified effects of climate change on the hydrogeology should be addressed as they could have an impact on performance (PE/CCC/5, pages 32 and 33, paragraphs 5.1.4 and 5.1.6) - addressed in paragraphs 9.67 to 9.70 of my Supplementary Proof of Evidence.

I will demonstrate in the following paragraphs that these assertions are not valid.

#### Point i: Short Paths and Rock Matrix Diffusion

9.51 In Section 4.5, pages 27 to 29 of his Proof of Evidence (PE/CCC/5), Dr Starmer discusses various issues which he suggests are not treated appropriately in the modelling presented in *Nirex 95* [COR/522]. He suggests that if these issues were taken into account, the calculated risk would be considerably increased. The main points made by Dr Starmer appear to be:

- shorter flow paths result in shorter travel times (PE/CCC/5, pages 27 and 28, paragraphs 4.5.1 and 4.5.4);
- flow paths derived from NAPSAC may overestimate travel times because they are longer than the straight line distance between points (PE/CCC/5, page 28, paragraph 4.5.3);
- the choice of parameter distributions unreasonably screens out extreme values and fast paths (PE/CCC/5, page 28, paragraph 4.5.3);
- maximum benefit from matrix diffusion is assumed (PE/CCC/5, page 28, paragraph 4.5.3);

- fracture flow models give earlier breakthrough than equivalent porous medium models, and the resulting fast paths could increase risk (PE/CCC/5, page 28, paragraph 4.5.4); and
- putting all of these effects together could give a considerable increase in risk (PE/CCC/5, page 29, paragraph 4.5.5).

In paragraphs 9.52 to 9.59 of my Supplementary Proof of Evidence I will rebut each of these points in turn and thus demonstrate that, as the steps in Dr Starmer's argument are invalid, his conclusion is not valid.

9.52 Dr Starmer states (PE/CCC/5, page 27, paragraph 4.5.1) that:

*"The length of the flow path is an important factor in determining the length of time for radionuclides released from the repository to reach the surface."*

and (PE/CCC/5, page 28, paragraph 4.5.4) that:

*"Short flow paths lead to shorter travel times due to distance,"*

9.53 The second statement is wrong. A shorter path does not automatically imply a shorter travel time. Both the path length and the travel time are determined by the groundwater velocities along the path that is followed. These determine where the path goes. The groundwater velocities will, in turn, depend on the properties of the hydrogeological units that are encountered along the path and the forces driving the flow. It is the physics of the flow system that determines the travel time and path length, not simply the geometry of the path.

9.54 In connection with the flow paths derived from NAPSAC, Dr Starmer states (PE/CCC/5, page 28, paragraph 4.5.3) that:

*"The flow paths derived from NAPSAC runs are likely to be longer than the "straight line" distance from point to point."*

This is not disputed. It simply reflects the fact that the flow and transport takes place in the network of transmissive features. The NAPSAC calculations will tend to find the easiest way through the system, but will also explore the potential variability around these preferred routes. The categories of path obtained from the NAPSAC calculations are described on page 2.8 of Volume 3 of *Nirex 95* [COR/522]. However, Dr Starmer seems to be suggesting that there are shorter "*straight line*" paths, which will be faster because they are shorter and which should be considered. This argument is not valid for two reasons. First, as noted above, the idea that a shorter path will necessarily be faster is not valid. Second, the fracture-network models have been constructed to represent current understanding of the site, based on the data from the site investigation. Thus, high permeability features will appear in the models at a frequency consistent with current understanding of the site. There are no scientific grounds for adding arbitrary extra paths to the models. To do so would be an unwarranted assumption, with no basis in the site data. Investigation of alternative conceptual models of the fracture system and the evaluation of these models by comparison with site data is part of the ongoing programme of scientific work at the site. The RCF will play an important role in the development of these models.

9.55 Dr Starmer suggests (PE/CCC/5, page 28, paragraph 4.5.3) that because the probability density functions for effective permeability used in *Nirex 95* [COR/522] are log-triangular, extreme values of permeability have "*much less chance of being picked*". The probability density functions used represented the uncertainty about the corresponding parameter values. The fact that values of, for example, effective permeability away from the centre of its distribution are less probable than values at the centre is to be expected. It reflects the fact that values near the centre were considered to be more likely, the extent to which they were more likely being quantified by the probability density function. The probability density functions were derived by a process of structured data elicitation (see *Nirex Report S/94/002*, page 9, Box D) [COR/508] and Appendix 1 of Volume 1 of *Nirex 95* or by analysis of the available data and appropriate upscaling procedures (see Volume 2 of *Nirex 95*). In the latter case, in particular, the fact that the probability density function decreases towards its tails reflects the fact that parameter values far from the central values are less likely to be consistent with the experimental measurements of permeability from which the distributions were determined, than values near the centre of the distribution.

9.56 Log-triangular probability density functions were considered to be reasonable approximations to the actual distributions of the individual parameters considered in isolation. It is worth noting that the fact that the probability density functions used decrease towards their tails is not just a property of the log-triangular distributions used. Similar behaviour would be shown by other realistic possible forms of distribution, such as log-normal probability density functions.

9.57 In connection with *Nirex 95* [COR/522], Dr Starmer states that "*Matrix diffusion is used at a maximum value by Nirex*" (PE/CCC/5, page 28, paragraph 4.5.3). This is not the case. In the MASCOT calculations of risk, the process of diffusion into the rock matrix away from fractures is modelled explicitly in those hydrogeological units in which groundwater flow was considered to occur predominantly in fractures (see Section 2.1.3 (page 2.6) and Sections 4.2 and 4.4 (pages 4.3 to 4.7) of Volume 3 of *Nirex 95*). Therefore, the extent to which radionuclides diffuse into the matrix is determined during the MASCOT calculations, and depends on the probabilistically selected values of the parameters that control rock-matrix diffusion, such as the intrinsic diffusivity. The distributions of values used for these parameters were based on experimental data from the Nirex Safety Assessment Research Programme (see Section 5.12.2, pages 5.20 and 5.21 of Volume 2 of *Nirex 95*). Exclusion processes were taken into account; the effects of channelling within fractures were also taken into account using a conservative treatment (see Section 4.2, page 4.5 of Volume 3 of *Nirex 95*).

9.58 Dr Starmer states (PE/CCC/5, paragraph 4.5.4) that:

*"flow along fractures also yields earlier breakthrough than the equivalent porous medium model"*

His argument would appear to be that the *Nirex 95* [COR/522] analysis will be predicting travel times that are unrealistically long because of this effect. Dr Starmer's point is not relevant to the *Nirex 95* analysis in which, for example, the travel times in the BVG were obtained from the NAPSAC model which explicitly represents the network of transmissive features in the BVG (see Section 2.2.2, pages 2.7 to 2.9 of Volume 3 of *Nirex 95*).

9.59 I have demonstrated that the steps of Dr Starmer's argument are not valid. Dr Starmer's conclusion that putting all these effects together could give a considerable increase in risk is therefore unsustainable.

Point ii: Processes That Could Reduce Dilution

9.60 In Section 4.6 of his Proof of Evidence (PE/CCC/5, pages 29 and 30) Dr Starmer notes the analysis of dilution factors that is performed in *Nirex 95* [COR/522]. In connection with these, he asserts that:

the lower end of the range of the dilution factors (which he implies has been eliminated in *Nirex 95*) could "*readily reduce the beneficial effect of dilution incorporated into the risk calculations*"; and

various phenomena could reduce the dilution factor and give rise to greater calculated risks.

My response to these points is given in detail in paragraphs 9.61 to 9.64 of my Supplementary Proof of Evidence. In outline, my response to the first point is that, the lower end of the dilution range is not actually eliminated and to the second that, on the whole, the phenomena suggested are already included in the assessment.

9.61 Dr Starmer's paragraph 4.6.2 (PE/CCC/5, page 29) suggests that he believes that the range of dilution factors of 4 to 100,000 stated by Nirex was actually used in the calculations of risk reported in Volume 3 of *Nirex 95* [COR/522], but that the lower end of the range had been eliminated. This is not the case. The quoted range of dilution factors was derived from an analytical calculation performed to aid understanding and only took account of uncertainty in one of the effective hydrogeological parameters. As the MASCOT realisations take account of the uncertainty in many more parameters, the range of effective dilution factors included in the MASCOT risk calculations is potentially wider than that quoted above. It is likely that the MASCOT calculations will include cases with dilution factors that are too small to be compatible with the site data. Considerations of the validity of the extremes of the quoted range therefore have no impact on the risk calculations. This is a potential conservatism in the calculations of risk.

9.62 Dr Starmer suggests (PE/CCC/5, page 29, paragraph 4.6.2) that flow along preferential pathways, short circuits to the surface, a reduced release area, or vertical movement along the Fleming Hall Fault Zone could

considerably reduce dilution. This is a similar point to that made in his paragraphs 4.3.1 and 4.3.2 and the reply is thus similar to that given in paragraphs 9.43 to 9.49 of my Supplementary Proof of Evidence. Many of these phenomena are already included in the *Nirex 95* [COR/522] models, so that there is little basis for the arbitrary numerical factors suggested by Dr Starmer to increase risk further.

9.63 Figures 2.6a and 2.11 of Volume 3 of *Nirex 95* [COR/522] show, respectively, the groundwater flow field of the base-case groundwater flow model and the calculated radionuclide plume used in the agricultural well calculation. Flow up the preferential pathway associated with the Fleming Hall Fault Zone can be seen in Figure 2.6a. The development of the radionuclide plume calculated in *Nirex 95* (see Figure 2.12 of Volume 3 of *Nirex 95*) clearly shows the effect of this upflow in the Fleming Hall Fault Zone carrying radionuclides towards the surface. Thus preferential flow along the Fleming Hall Fault Zone is explicitly included in the performance analysis. Inclusion of this pathway may be a pessimism in the current assessment (see paragraph 9.45 of my Supplementary Proof of Evidence).

9.64 As noted in paragraph 9.46 of my Supplementary Proof of Evidence, if there is to be a "short circuit" there must be a physical mechanism to move the radionuclide. However, Dr Starmer again does not suggest any physical mechanism to produce the short circuit that he suggests. I note that the agricultural well is a mechanism to produce such a short circuit and that this effect is included in the *Nirex 95* [COR/522] analysis. Similarly, the predicted release area offshore is obtained from a model that incorporates the physics of the system. A suggestion that this area might be an overestimate should be based on a physical argument. As it stands, paragraph 4.6.2 (PE/CCC/5) is speculation, with no basis in a physical description of the system.

#### Point iii: Screening of the Radionuclide Inventory

9.65 Dr Starmer's comments in Section 5.3 (PE/CCC/5, pages 37 and 38) indicate a fundamental misunderstanding of the treatment of the radionuclide inventory in *Nirex 95*[COR/522]. He states in paragraph 5.3.1 that:

*"the **screening** of radionuclides left the suite Cl-36, Tc-99, I-129 and (U-238) as the important radionuclides. An important point is that this **selection** has been predicated by assumptions that have not been fully tested"* [my emphases].

Thus he appears to believe that only these four radionuclides were considered in the risk calculations. This is not the case. The wide range of radionuclides used in the MASCOT calculation of risk is given in Table 6.1 of Volume 3 of *Nirex 95* [COR/522]. Of these, only the four discussed by Dr Starmer were found to make a significant contribution to risk in the various Biosphere states.

Dr Starmer refers to page 8.4 of Volume 3 of *Nirex 95* in connection with the "selection" of the radionuclides and does not seem to have appreciated that the purpose of Section 8 of Volume 3 of *Nirex 95* is only to aid understanding of system performance. The calculations of risk reported in Section 6 of Volume 3 of *Nirex 95* were not carried out using this description, rather they aided its development. The same misunderstanding seems to have arisen concerning the dilution analysis discussed in Sections 2 and 8 of Volume 3 of *Nirex 95* (see paragraph 9.61 of my Supplementary Proof of Evidence).

9.66 That this misunderstanding of the inventory that is included in *Nirex 95* [COR/522] has indeed occurred is confirmed by the comment that Dr Starmer makes in (PE/CCC/5, page 38, paragraph 5.3.3) that:

*"it is notable that Tc-99 shows up in the Boreal terrestrial and Temperate marine analysis but is **not included**"* [my emphasis] *in the analysis of the Temperate terrestrial or periglacial analyses (Table 6.18 in COR/522)"*

The Table referred to (in Volume 3 of *Nirex 95*) shows the radionuclides which were found from the results of the MASCOT calculations to make a significant contribution to risk in the various Biosphere states. Thus, Dr Starmer's comments are based on a misconception of the work that was performed.

#### Point iv: Effects of Climate Change

9.67 The Proof of Evidence by Dr Starmer (PE/CCC/5) contain discussions of the potential effects of climate change. Several of these are addressed in Section 8 of my Supplementary Proof of Evidence. However, two further effects relating to the groundwater flow regime are addressed in this Section. First, the suggestion that melting of permafrost would give rise to increased infiltration (PE/CCC/5, paragraph 5.1.6). Second, that a period of a diffusion dominated transport could lead to changes in flow paths and in repository performance (PE/CCC/5, paragraphs 5.1.4 and 5.1.6).

9.68 In paragraph 5.1.6 of his Proof of Evidence (PE/CCC/5) Dr Starmer refers to the *Fourteenth Annual Report of the RWMAC* (page 19, paragraph 3.30) [GOV/406] and claims that:

*"increased infiltration in the highlands east of the Sellafield site due to the melting of permafrost has been cited by RWMAC".*

However, the RWMAC reference (page 19, paragraph 3.30) [GOV/406] is not arguing for increased infiltration associated with melting but decreased infiltration associated with permafrost, followed by a return to present day infiltration conditions. This can be seen from the statements in the same paragraph that:

*"during the Pleistocene Ice Age and the following period, when frozen ground tundra conditions existed, there would have been little fresh water infiltration into the BVG ..... only when the ice melted and the ground thawed, ten thousand years ago or so, would infiltration of fresh water, through rainfall have started."*

Thus the argument for increased infiltration is not supported.

9.69 Dr Starmer suggests (PE/CCC/5, page 32, paragraph 5.1.4) a number of possible effects of a period of diffusion dominated transport. He suggests that:

*"A period of slow diffusion of radionuclides to the flow system in the repository during reduced flow conditions followed by increased flow driven by increased fresh water infiltration in the highlands, as appears to be the case now, could lead to flushing a pulse of more concentrated pore water from the repository than is now considered under the base case or variants. This could change the overall conceptual model of repository evolution and performance in the ice ages timeframe (10e4 to 10e5 years)."*

The scenario suggested by Dr Starmer would lead to a reduction in calculated risk. This can be seen from the simple understanding of system performance given in Sections 8.2.2 and 8.2.3 (pages 8.6 to 8.12) of Volume 3 of *Nirex 95* [COR/522]. This discussion demonstrates that the important factors influencing performance are the geosphere and source term spreading times. The source term spreading time for a radionuclide is a measure of the timescale for release of the radionuclide from the repository. The geosphere spreading time is a measure of the timescale for radionuclide transport through the geosphere. It can be seen from Figure 8.8 of Volume 3 of *Nirex 95* that a significant increase in the source term spreading time leads to a reduction in the risk arising from chlorine36, which is the most significant radionuclide in the timescale that Dr Starmer discusses. Dr Starmer's scenario of a period of low flow would lead to a significant increase in the source term spreading time and hence to a reduction in risk. I also note that the time frame which he proposes for the period of "*slow diffusion*" roughly coincides with the time of the early risk peak (largely due to chlorine36) in the base case calculation (Figure 6.6 of Volume 3 of *Nirex 95*). A period of low flow would delay the arrival of this peak. With the combination of these effects, Dr Starmer's scenario could lead to a significant reduction in predicted peak risk.

9.70 A related suggestion by Dr Starmer (PE/CCC/5, paragraph 5.1.6) is that:

*"under both a mediterranean and periglacial climate scenario, for example, the infiltration of water might be reduced, leading to a change from advective flow to diffusive flow, and flow paths could change at that point"*

It is certainly true that a change to diffusion dominated transport would lead to different transport pathways, because of the differences between advective and diffusive transport. However, such a change would also lead to a much smaller rate of removal of radionuclides from the repository and a much slower rate of transfer of

radionuclides to the biosphere, that is a significant increase in both the geosphere and source-term spreading times discussed in paragraph 9.69. As can be seen from Figure 8.8 of Volume 3 of *Nirex 95* [COR/522], such a scenario would lead to lower risks. In suggesting that these scenarios are worthy of consideration, Dr Starmer is effectively suggesting that the analysis presented in Volume 3 of *Nirex 95* [COR/522] could have significantly overestimated risk, not underestimated it.

## Summary

9.71 In summary, I conclude that:

i.	the Nirex approach to groundwater modelling is appropriate to the characteristics of the Sellafield site and that the criticisms raised by the Objectors generally indicate that: important aspects of the Nirex approach to groundwater flow modelling have not been recognised by the Objectors; or important aspects of the current status of modelling techniques have not been recognised by the Objectors; and
ii.	that particular features or processes that the Objectors claim have been neglected in the Nirex modelling are actually included, when they are physically realistic.

## 10. VALIDATION OF MODELS AND THE ROLE OF THE RCF

10.1 Several of the Objectors' Proofs of Evidence raise the issue of 'validation'. The purpose of this section of my Supplementary Proof of Evidence is to explain the accepted definition and approach to validation in the context of radioactive waste safety assessment and to explain how Nirex is addressing the issue, in particular with respect to the RCF work programme. The passages with which this section is concerned are:

- paragraphs 3.28 and 4.27 to 4.29 of Dr Salmon's Proof of Evidence (PE/FOE/5);
- paragraphs 8.4 to 8.7 of Mr Richardson's Proof of Evidence (PE/GNP/14);
- paragraphs 3.6, 8.5 and 9.4 and Section 10 of Dr Hencher's Proof of Evidence (PE/FOE/6); and
- Section 7 of Dr Mackay's Proof of Evidence (PE/GNP/2).

10.2 In the next four paragraphs of this Section I summarise the argument put forward by each Objector in turn along with my summary response. I will then set out, at paragraphs 10.8 to 10.26 in more general terms, an explanation of the role of validation in radioactive waste safety assessment followed by an outline of the process of validation in the Nirex programme. An outline of the validation cycles for groundwater flow models which Nirex has performed and is undertaking is presented. Future validation cycles, both prior to and during the course of Phase 1 RCF are set out in the Appendix to this Supplementary Proof of Evidence.

### Dr Salmon (PE/FOE/5)

10.3 Dr Salmon raises the question of validation as follows:

i.	At paragraph 3.28 of his Proof of Evidence (PE/FOE/5), Dr Salmon asserts that " <i>there is an urgent need to further validate the model</i> " (NAMMU) and states that Nirex has ignored opportunities to do this. In particular, he identifies work associated with the Borehole RCF3 Pump Test and the impact of the RCF shafts as missed opportunities. I explain at paragraph 10.18 of my Supplementary Proof why Dr Salmon is mistaken in these assertions and that such work is already underway or is planned. Dr Salmon asserts, in paragraph 3.92, that " <i>I nevertheless remain doubtful that an adequate validation can be achieved....</i> ". I explain in paragraphs 10.17 to 10.26 below why this is incorrect by showing how such validation is actually taking place at present.
ii.	At paragraphs 4.27 to 4.29, Dr Salmon (PE/FOE/5) re-asserts the value of using RCF shaft construction to validate groundwater flow models, stating (paragraph 4.28) that the authors of GOV/610 were " <i>....led to believe that modelling of the expected influence of the RCF....would be undertaken before shaft construction proceeds</i> ". I describe at paragraph 10.18 and in Section A3

		of the Appendix to this Supplementary Proof of Evidence how such an exercise is to take place, before shaft construction begins.
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**Mr Richardson (PE/GNP/4)**

10.4 Mr Richardson, at paragraph 8.7 of his Proof of Evidence (PE/GNP/4) also asserts that there is no indication that Nirex will follow a formal validation process incorporating peer review. I explain at paragraph 10.10 below that we are currently following a validation process incorporating appropriate peer review.

**Dr Hencher (PE/FOE/6)**

10.5 Dr Hencher raises the question of validation as follows:

i.	At paragraph 3.6 of his Proof of Evidence (PE/FOE/6), Dr Hencher concludes that " <i>...to proceed to the RCF stage of repository development prior to satisfactory completion of underlying generic research work and validation of numerical models would be premature and ill-conceived.</i> " I believe that this shows a misunderstanding of the process of validation, as set out below at paragraphs 10.8 to 10.16. It also fails to appreciate the essential role that the RCF itself is designed to play in the validation process, which is explained below;
ii.	At paragraph 8.5 of his Proof of Evidence, Dr Hencher quotes <i>Dr Long</i> (FOE/6/11) on the modelling of fractured rock hydrogeology. That quotation can be taken as strong support for the need to validate flow modelling approaches. <i>Dr Long</i> (FOE/6/11) also says that " <i>Probably the most important progress will be made simply by trying to create predictive models for an increasing number of sites.</i> " (Section 11.10, page 292). The work which we propose for the RCF (as described in the remainder of this Section of my Supplementary Proof), may be seen as a key contribution to the progress which she envisages;
iii.	At paragraph 9.4 of his Proof of Evidence (PE/FOE/6), Dr Hencher asserts that " <i>Reported success rates of such validation tests are not high</i> " (referring to flow in fractured rocks) and goes on to quote a number of examples which he asserts illustrate that (PE/FOE/6, paragraph 9.11) " <i>...processes of model validation for flow through fracture networks is at an early stage.</i> " He also states (paragraph 11.6) that " <i>... the simple answer is that discrete fracture flow models have not yet been validated in the fields.</i> " I disagree with Dr Hencher and address this issue of the success or otherwise of previous validation exercises at paragraph 10.16 below; and
iv.	In Section 10 of his Proof of Evidence (PE/FOE/6), Dr Hencher appears to be saying that validation work is only appropriate to an underground research laboratory (URL) and not an RCF, which he describes (paragraph 10.5) as " <i>merely the confirmatory underground stage of geological investigation</i> " (for a repository). This, together with his assertion (paragraph 11.2) that " <i>the emphasis given by Nirex is on confirmation rather than experimentation or validation</i> " again displays a lack of understanding of the process of validation and the way in which it is an integral part of Nirex's RCF programme. This is discussed further in paragraph 10.10 of my Supplementary Proof. His assertion (paragraph 11.7) that " <i>...the prognosis of Nirex achieving validation during the sinking of shafts is not good</i> " is subjective and unjustified.

**Dr Mackay (PE/GNP/2)**

10.6 In paragraphs 2.18 to 2.22 of his Proof of Evidence (PE/GNP/2), Dr Mackay raises a number of issues concerning the extent to which validation of flow models will be achieved in Phase 1 of the RCF programme and whether acceptance criteria have been developed for use in the validation process. I discuss these matters in detail below at paragraphs 10.11, and in Sections A2 and A3 of the Appendix to my Supplementary Proof of Evidence and explain that work is underway or planned which will answer Dr Mackay's concerns.

10.7 Paragraphs 10.8 to 10.16 of my Supplementary Proof set out an explanation of the role of validation in radioactive waste safety assessment and paragraphs 10.17 to 10.26 then outline of the process of validation in the Nirex programme.

## The Validation Process

10.8 In radioactive waste safety assessment, validation is the term applied to the iterative process of building confidence in the fitness for purpose of models used in developing a performance assessment for a repository and in the predictions they make. As discussed in [paragraph 6.11](#) of my Proof of Evidence (**PE/NRX/15**), the validation process can be used to discriminate between alternative conceptual models, to refine a given conceptual model and to confirm that a conceptual model is fit for the purpose for which it is intended. The process of validation involves testing model predictions against independent observations and evaluating them against a set of performance measures within a peer review framework.

10.9 The International Atomic Energy Agency defines validation as [COR/519], (page 11):

*"..... a process carried out by comparison of model predictions with independent field observations and experimental measurements. A model cannot be considered validated until sufficient testing has been performed to ensure an acceptable level of predictive accuracy. (Note that the acceptable level of accuracy is judgmental and will vary depending on the specific problem or question to be addressed by the model)."*

10.10 Nirex has addressed the process of validation within a formal framework which, for any given process or mechanism of interest in an assessment, will involve a series of steps, undertaken in an iterative manner. The completion of the sequence of steps comprises a 'validation cycle' and successive validation cycles should result in a gradual and demonstrable improvement in important models underpinning the performance assessment. A validation cycle comprises the following steps:

	i.	review the various models which could be used to describe the process or system;
	ii.	review the data required by these models and available for use in testing them;
	iii.	calibrate each model against a sub-set of the available data;
	iv.	define the performance measures. These will later be used to assess whether the model is acceptable with due regard to its purpose. Such performance measures will be qualitative judgements or quantitative criteria by reference to which the predictions made by the model will be compared to the observed data;
	v.	make predictions using the model and test them by reference to the performance measures against observations using data independent from those used to calibrate the model;
	vi.	compare the results with those obtained using alternative models;
	vii.	analyse discrepancies between predictions and observations and between models;
	viii.	identify those parameters which are either poorly matched or to which the model predictions are particularly sensitive in order to identify key issues;
	ix.	present the results of the study for peer review, ensuring that the ultimate use of the model is clearly stated;
	x.	consider implications of the identified key issues for producing a post closure performance assessment;
	xi.	suggest improved experiments to address the key issues during further cycles of validation; and
	xii.	review consistency of models with related or linked models used in performance assessment.

10.11 The performance measures, which will enable assessment of the acceptability of a model, should be set in advance of the tests. These measures must be defined on a model specific basis and with knowledge of the model's intended use. The measures must also take account of a range of possible uncertainties which may arise from:

measurement uncertainty; conceptual uncertainty; the simplifications imposed by the manner in which the numerical modelling code abstracts a specific problem; and parameter uncertainty.

10.12 Predictions may be made prior to, during or after independent data are gathered. If made during or after data acquisition then special care must be taken to ensure that independence is not compromised. The most powerful technique in terms of building confidence is that of making forward predictions prior to data gathering. In this case, the actual observations against which predictions are being made may be kept from the modelling group(s), who are only supplied with sufficient information to make the predictions, in a 'knowledge environment' similar to that in which they may be using the models to make predictions in a safety assessment. This technique is known as 'blind predictive modelling', and is used by Nirex.

10.13 These steps are repeated in a cyclical fashion to accommodate improvements in understanding, in the scope and descriptive capability of the models (e.g. their ability to predict behaviour at different scales) and in the quantity and quality of site investigation and other data available to them. It is important to appreciate that validation is not intended to produce an absolute result, but rather to achieve an acceptable level of predictive accuracy (*Glossary of Scientific Terms*, page 11) [COR/519]. Improvements to conceptual models are always possible, but it is important to have confidence that the models used are fit for the purpose which they are required. Such confidence can be enhanced by exposing the validation process to peer review.

10.14 A key aspect in validation is the progressive identification and, where possible, reduction of uncertainties about the models and their predictions. *Dr Long* (FOE/6/11) discusses the need for this cyclical approach as a means of identifying and reducing uncertainties at page 290:

*"There are never enough data and the conceptual model can always be questioned. Consequently, it is not possible to construct a unique model of fracture flow. Given this situation, it is probably better to view the data as determining a series of possible models from which a series of predictions can be made.....With a series of models, it is possible to quantify the error associated with using the models to make a prediction. This means that the model is judged by its ability to predict the system response accurately.....The more numerous and diverse types of predictions that can be included in the estimate of prediction error, the more confidence is generated in the model.....This is the essence of the iterative approach: cycles of measurement and prediction are repeated until the addition of new data does little for the ability to make predictions."*

As discussed in the following paragraphs of this Supplementary Proof, this is the approach currently being used by Nirex.

10.15 Some of the Objectors, for example *Dr Hencher*, (PE/FOE/6, paragraph 3.6) appear to believe that modelling approaches have to be completely "*pre-validated*" in a generic sense before Nirex uses them at the Sellafield site. The essence of validation is to quantify uncertainties until the modelling approach is fit its for purpose when required. International experience, as recently focused in the INTRAVAL project (see *Royal Society Study Group Report*, page 88) [COR/605] in which Nirex was actively involved, has identified a general agreement that models cannot be validated generically and that it is thus necessary to demonstrate that models are applicable to a specific site of interest. In other words, validation cannot be divorced from the site to which it is intended to apply the models.

10.16 Statements in *Dr Hencher's* Proof of Evidence (PE/FOE/6, paragraphs 9.4, 9.11 and 11.6) assert that the process of validating fracture network models is at an early stage and that such models have not been validated in the field. This issue is discussed in more detail in Section 9 of this Supplementary Proof, but I reiterate that *Dr Hencher* has presented examples from the Stripa Site Characterisation and Validation Programme out of context, and has, consequently, misrepresented the conclusions of *Olsson and Gale* (FOE/6/15). Indeed, the Overview Document published by the OECD/NEA International Stripa Project (NRX/16/2), concludes (page 57) that one of the significant achievements of the Site Characterisation and Validation Project, of which the inflow tests quoted by *Dr Hencher* formed a part was the "*demonstration of the applicability of the fracture flow modelling for simulating groundwater flow and solute transport*".

## Validation In The Nirex Programme

### Validation Cycles

10.17 Validation cycles as described in paragraph 10.10 of my Supplementary Proof have already been implemented by Nirex. The cycles have been designed to build on the understanding established following the drilling of the regional boreholes. This understanding is described in *Nirex Report 524* [COR/517]. An initial validation cycle was conducted around the Borehole 2/4 crosshole testing programme (**PE/NRX/14**, Appendix A, [Table A8](#)). This initial validation cycle tested qualitatively a range of conceptual models. This work has been described in a paper presented to a European Commission Workshop held in Brussels, 12-13 January 1995 (NRX/15/16).

10.18 Nirex envisages completing two more main validation cycles for these models of groundwater flow by the time RCF Phase 1 is completed. These two cycles will be centred around the Borehole RCF3 Pump Test and the shaft drawdown experiment respectively. The Borehole RCF3 Pump Test cycle, and the initial predictions for the shaft drawdown cycle will be completed prior to commencement of shaft excavation. Further cycles are planned, which may commence during Phase 1 of the RCF, and extend into Phases 2 and 3.

10.19 Within each main cycle, there may be the opportunity for repeated testing of specific aspects of some of the model, adding further to the iterative validation process. In addition, as discussed at paragraph 10.10 above, peer review is an important part of the process. It is intended to use a panel of independent experts in the field of groundwater flow modelling to assist with the development of performance measures and to review the performance of models against them.

10.20 As noted in my Proof of Evidence (**PE/NRX/15**) at [paragraph 6.3](#) to 6.5, the shaft sinking phase of the RCF is anticipated to allow the model validation required to underpin a preliminary decision on the repository depth (and RCF gallery depth). It is expected that sufficient confidence in the groundwater flow models could be gained by the validation cycles completed by the end of Phase 1 of the RCF to permit a decision on whether to propose repository development. The work involved in the groundwater flow model validation cycles leading to this decision point is described in the Appendix to this Supplementary Proof.

### Validation Using The RCF

10.21 In this Supplementary Proof I concentrate on describing the Nirex approach to validation of groundwater flow models as an example of how the Nirex strategy will be applied using the RCF. A similar process to that described in the remainder of this section is applied in other parts of the Nirex programme such as modelling gas generation (*Nirex Report S/94/003*, page 5) [COR/509].

10.22 [Paragraph 6.3](#) of my Proof of Evidence (**PE/NRX/15**) outlined a requirement to build confidence in key models used in assessments of repository post-closure performance and to enable confident extrapolation of descriptions of the rock mass and groundwater systems across the PRZ for use in developing the repository design. [Paragraph 6.26](#) of Dr Holmes' Proof of Evidence (**PE/NRX/13**) identifies the shaft sinking phase of the RCF as playing a major part in the validation process, providing the first opportunity to test groundwater flow models on a large scale. Dr Mellor (**PE/NRX/16**, [paragraphs 7.11](#) and 7.12) describes how the programme of forward predictions relate to the Sector Test Plans which will form the basis of measurements to be made in the RCF and that will form the basis of validation exercises using the RCF.

10.23 Forward predictions during the excavation phase of the RCF will allow the validation process to be applied to a variety of contributing conceptual models relevant to developing the necessary understanding of the behaviour of the site, including:

	i.	models of groundwater flow in the BVG and cover rocks;
	ii.	the description of spatial variability in key features of the rock mass, in particular lithology, fracture type, fracture orientation, and fracture frequency and fracture mineralisation patterns;
	iii.	models of rock stress and stress distribution;
	iv.	hydrochemistry; and
	v.	models of excavation damage effects.

10.24 The simultaneous and complementary validation of all these models during Phase 1 of the RCF will provide a powerful approach to building confidence in key models and in post-closure performance assessment. The aim is to develop a demonstrable understanding of the geometry of the flow system, the physics controlling the flow system and the volume of flow occurring through the system, in order to predict groundwater flow through the geosphere barrier. This requires a wide range of information to be obtained from the RCF in order to test predictions from these models. The range of information that will arise during Phase 1 of the RCF is described by Dr Mellor in [paragraphs 4.3](#) to 4.14 of his Proof of Evidence (PE/NRX/16).

10.25 The RCF provides the opportunity for the direct observation of features which have not been observable from surface boreholes. In addition, it will extend the length scales over which tests are performed. As such, it will provide independent and new data against which to validate models developed from the surface investigation programme. The differing nature of this information to that obtainable from surface boreholes will assist in increasing confidence in the models which perform well against the performance measures for the comparison of predictions with observations.

10.26 The RCF shaft drawdown has been accepted as one of two potential major contributors to the successor programme within the international DECOVALEX validation exercise, aimed at evaluating coupled models of rock stress and hydrogeology in fractured rocks. This programme is intended to expose models applied by several independent international groups to extensive testing against data from the RCF. Like its sister projects, INTRAVAL and the early phase of DECOVALEX, this exercise will form a major contribution to the international scientific community working on the issue of validation and to the peer review of the Nirex programme of validation.

## Conclusion On Validation

10.27 My overall conclusion in respect of the matters raised on validation by the Objectors is that Nirex has both a well developed approach to validation and an extensive programme of validation exercises completed, underway and planned for the RCF itself. Nirex is involved in international validation exercises and is committed to ensuring that its validation work is thoroughly peer reviewed. Conceptual models used to support safety assessments submitted to the regulators will have to be shown to be acceptable and to be fit for purpose. As a consequence, the concerns raised by the Objectors are considered to be ill founded and to be addressed by work which Nirex already has in hand. The RCF is an essential component of this work.

## APPENDIX: VALIDATION CYCLES FOR THE GROUNDWATER FLOW MODELS (SECTION 10)

### A.1. Crosshole Testing Between Boreholes 2 and 4

A.1.1 This initial cycle of validation was completed by Nirex in 1994, and was conducted around the Borehole 2/4 crosshole testing programme (PE/NRX/14, Appendix A, [Table A8](#)). This cycle tested qualitatively a range of conceptual models of groundwater flow in the BVG by considering their compatibility with observations of crosshole and single borehole responses to six pumping tests performed in Borehole 2. This work has been described in a paper presented to a European Commission Workshop held in Brussels, 12-13 January 1995 (NRX/15/16).

### A.2. The Borehole RCF3 Pump Test

A.2.1 Nirex is carrying out a major experimental exercise focused on flow model validation in the PRZ rocks by means of the Borehole 'RCF3 Pump Test'. This exercise is a smaller scale version of the type of exercise which would be possible during RCF shaft construction.

A.2.2 One of the principal aims is to assist in the characterisation of the flow system in the BVG and overlying strata by distinguishing between alternative conceptual models of flow in the PRZ and assessing the applicability of available models to that system. In particular, the applicability of fracture network models to the fracture data from the BVG is being studied. To this end, six alternative conceptual models of flow within fractured rock were developed and compared. These models described the rock using various combinations of deterministic and stochastic properties. Numerical simulation of the models was achieved using four, internationally well-tested and verified computer codes (NAMMU, NAPSAC, FracMan/MAFIC and ECLipse) designed to evaluate flow in fractured and porous rocks, set up in different ways and in different combinations.

A.2.3 The test (which is now completed) involved pumping groundwater from Borehole RCF3 and monitoring the hydraulic response of selected zones in 15 boreholes in the vicinity of the PRZ. Modelling utilised results from 120 monitoring points within 600m of Borehole RCF3 in all formations. A modelling and prediction protocol was drawn up in advance to specify exactly what was to be predicted and how the predictions were to be presented. Three classes of predictions were defined for the Borehole RCF Pump Tests:

i.	<b>Class A</b> 'predictions' were carried out 'blind' before the pump test itself, and, by using clearly independent data, have the greatest potential strength in building confidence in the conceptual models;
ii.	<b>Class B</b> 'predictions' were carried out after the start of the pump test, but without knowledge of the outcomes gained to date. Class B predictions were not as rigorous for validation as Class A predictions. These predictions were needed owing to the practical need for modifications to the pump test during its implementation.
iii.	<b>Class C</b> 'predictions' are currently underway and have been made after the event. While of less value than forward predictions, this part of the process is important as it permits the development of understanding arising from the predictions, and allows any necessary refinements to the model.

A.2.4 These predictions were tested against both 'direct' (where direct comparison could be made with a measured parameter) and 'interpreted' (which require interpretation of field results to produce a derived parameter or other factor) performance measures. The measures used take account of the stochastic nature of flow in fracture networks (discussed by Dr Chaplow at [paragraph 5.8](#) in his Supplementary Proof of Evidence, (PE/NRX/14/S1)) and include assessment of modes, trends, ranges and distributions of predictions as well as actual values. Seven direct and four interpreted performance measures were used, including hydraulic response locations, magnitudes and times, drawdowns and drawdowns versus distances and interpretations of flow magnitudes and flow dimensionality.

A.2.5 The stochastic nature of the flow regime means that the performance measures were, of necessity, probabilistic. Statistical 'scores' will, on the basis of the assessment of the test be attributed to each prediction based on their correspondence with measurements. Correspondence is being gauged by:

i.	visual comparison of trends, curves and spreads;
ii.	tabulations of differences and their means and standard deviations curve fitting by non-linear regression and comparison of residuals and correlation coefficients; and
iii.	plotting probability distribution functions for predictions and measurements.

A.2.6 For the Borehole RCF3 Pump Test, which is part of an evolving process of validation it was not considered appropriate to develop quantitative criteria whereby a model could be accepted or rejected based on its performance with respect to the statistical tests described above. Qualitative judgements will be used to assess the model predictions against the established performance measures.

A.2.7 The test itself was designed to provide the data, required at an adequate density and frequency, to allow predicted values from the computer codes for each model to be compared with the performance measures. Experimental design is thus a critical step in the validation cycle. The tests involved pumping from four intervals in Borehole RCF3, two in the Sherwood Sandstone Group, one in the Brockram and one in the BVG, results from the latter being the main focus of the exercise and involving a drawdown period of 90 days. The upper sandstone test, performed above the North Head Member was introduced at the end of the test programme .

A.2.8 The predictions of the responses is an important step in a full validation cycle as described at paragraph 10.10 of my Supplementary Proof of Evidence. The results are still being analysed and it is not yet possible to present our conclusions. However, the differences between the predictions will help to distinguish the most appropriate conceptual models.

A.2.9 The whole exercise is the subject of a peer review process, involving the appointment of an independent referee or 'banker' (John Lloyd, Professor of Hydrogeology at the University of Birmingham) who holds the forward prediction results and who will review the comparison of results with the performance measures.

### **A.3. The RCF Shaft Drawdown Experiments**

A.3.1 The Borehole RCF3 Pump Test looks to have provided a firm basis on which to build subsequent validation cycles for flow models of the rocks within the PRZ. It will provide (as described above) initial discrimination between alternative conceptual models of flow in the BVG and confirmation that one or more of the models may be fit for the purposes of future safety assessment calculations. These models will be refined and carried forward in readiness for the next validation cycle, the RCF shaft drawdown.

A.3.2 Like the Borehole RCF3 Pump Test, the RCF shaft drawdown cycle will use hydraulic response data from the network of monitoring points. Hydraulic pressure changes will be induced in the monitoring network during the course of shaft excavation, as described at [paragraph 4.7](#) of Dr Mellor's Proof of Evidence (**PE/NRX/16**). These effects are termed the shaft drawdown effects, and they will be used in the next main cycle of validation. The use of excavation drawdowns for model validation has been tested in Canada at the Underground Research Laboratory of AECL between 1980 and 1986, as discussed by Mr Reeves (PE/FOE/4, paragraphs 6.13 to 6.15) and described in more detail in Davison (PE/FOE/4) (FOE/4/5).

A.3.3 The Canadian experience showed the utility of using large-scale borehole pumping tests (similar in concept to the Borehole RCF3 Pump Test described above) combined with piezometric pressure monitoring records to calibrate the models which were subsequently tested during shaft excavation. The Nirex activities go well beyond this simple pre-calibration and testing of the models because, as described above, two full validation cycles will have taken place to compare alternative models prior to the shaft drawdown experiment. In addition, the range and sophistication of models available and the consequent extent and variety of performance measures go far beyond what was available to AECL in the early 1980's.

A.3.4 The RCF shaft drawdown exercise will proceed in a similar manner to the Borehole RCF3 Pump Test and following the steps outlined in paragraph 10.10 of my Supplementary Proof. Our forward programme, between now and the commencement of excavation, will consist of the following steps:

	i.	Completion of the analysis of the Borehole RCF3 Pump Test results;

	ii.	Design of the RCF validation test programme, taking account of the larger volumetric scale, the opportunity to modify the configuration of the monitoring network to optimise data collection;
	iii.	Definition of sets of models appropriate to analysis of flow in the cover rocks;
		Selection of a revised set of models for the BVG, taking account of those which performed best and the need to evaluate performance at different scales;
	iv.	Development of a set of performance measures, which will include quantitative criteria to assess the acceptability of the selected models and which will be appropriate to the revised set of models and the different stages of the shaft excavation;
	v.	Definition and reporting of prediction protocols and acceptance criteria for models, suitably peer reviewed using a panel of independent experts;
	vi.	Production of a range of blind predictions; and
	vii.	The detailed design of the experimental measurement programme.

A3.5 It is our intention to release hydrogeological data as the RCF proceeds and the whole process of validation using the shaft drawdown experiment will be carried out under an appropriate peer review regime. However, the requirements to make 'blind' predictions, as discussed above, mean that data flow must be strictly controlled during the whole of this exercise, if it is to have proper validation significance.

A.3.6 An additional aspect of the shaft drawdown experiment will be the ability to link the testing of hydraulic models to regional scale models of hydrochemistry and to models of rock stress in the excavation damaged zone (EDZ) around the shaft. Limited studies of the hydrochemical effects of the Borehole RCF3 Pump Test have been done, and these are being used to confirm Nirex's concepts of groundwater regimes and mixing relationships (**PE/NRX/14/S1**, [paragraph 8.33](#)). The proposed injection of tracers into the rock mass surrounding the shaft, and the associated monitoring of tracer entries into the shaft during shaft sinking will permit the simultaneous validation of coupled flow and transport models to take place.

#### **A.4. Subsequent Validation Cycles: Work Within the RCF**

A.4.1 When access to the BVG is gained it will be possible to make detailed refinements to the stochastic descriptions of spatial variability in fracture densities and properties, using direct observations from fracture mapping, initially on the shaft walls. It will be possible to use these refinements to make and test revised predictions for the final stages of the shaft drawdown experiment during the 912 months between first entering the BVG and reaching the shaft bottom. In addition, further validation cycles can commence, centred on other RCF data acquisitions activities.

A.4.2 The main part of these subsequent validation cycles for the selected conceptual models of flow in the BVG would involve:

	i.	further refinement of the models based on additional fracture data from mapping the shaft connection adit and the galleries to be constructed in Phases 2 and 3 of the RCF. In particular, these cycles will have the benefit of direct observational testing of the conceptual models of 'flowing feature' types; and
	ii.	input from cross-hole hydraulic tests in various regions of the BVG to evaluate in detail the properties of the fracture network and the behaviour of different flowing features.

A.4.3 The types of validation experiments performed in Phases 2 and 3 of the RCF are envisaged to build on experience drawn from the Stripa SCV Project in particular, as outlined in Dr Mellor's description of the proposed RCF-SCD test at [paragraphs 5.7](#) and [5.8](#) of his Proof of Evidence (**PE/NRX/16**). A central aim will be to test the ability to apply the, by then, preferred conceptual models of the BVG at various scales. The techniques for 'upscaling' were described by Dr Chaplow at [paragraphs 5.8](#) and [7.19](#) to 7.27 of his Supplementary Proof (**PE/NRX/14/S1**).

A.4.4 Validation cycles will also be applied to models which simulate radionuclide transport.

## **A.5. Input to Safety Assessment**

A.5.1 Validation cycles completed by the end of Phase 1 of the RCF will permit:

- definition of the most appropriate models to apply to flow in each of the formations and selection of preferred models for assessment purposes;
- definition of tried and tested upscaling rules for the preferred models; and
- definition of an updated flow model for use in the safety assessment work.

A.5.2 The decision at the end of RCF Phase 1 as to whether to propose development of a repository will depend on the outcome of this work. The safety assessments supporting such a decision will build on *Nirex 95* [COR/522] and will utilise updated conceptual models of groundwater flow derived from the validation cycles described in this Section of my Supplementary Proof.

## **REFERENCES**

GOV/406

The Radioactive Waste Management Advisory Committee, Fourteenth Annual Report. HMSO, June 1994.

GOV/622

A Study of the Effect of the Extent of Site Investigations on the Estimation of Radiological Performance; Volume 1 - Development of the Synthetic Site Model. Mackay R, DOE/HMIP/RR/93.052, DOE 1993.

COR/501

Nirex Report 71, Deep Repository Project - Preliminary Environmental and Radiological Assessment and Preliminary Safety Report, 1989.

COR/505

Nirex Report 525, Scientific Update 1993, December 1993.

COR/507

Nirex Science Report S/94/001, Post-closure Performance Assessment, Probabilistic Safety Assessment: Overview, October 1994.

COR/508

Nirex Science Report S/94/002, Post-closure Performance Assessment: Information Management, October 1994.

COR/509

Nirex Science Report S/94/003, Post-closure Performance Assessment, Gas Generation and Migration: November 1994.

COR/510

Nirex Science Report S/94/004, Post-closure Performance Assessment, Modelling of Groundwater Flow and Radionuclide Transport, February 1995.

COR/517

Nirex Report 524, The Geology and Hydrogeology of the Sellafield Area: Interim Assessment: December 1993.

COR/519

Glossary of Scientific Terms.

COR/522

Nirex Science Report S/95/012, Post-closure Performance Assessment, Nirex 95: A Preliminary Analysis of the Groundwater Pathway for a Deep Repository at Sellafield, July 1995.

COR/523

Nirex Science Report S/95/006, Sellafield Geological and Hydrogeological Investigations: The Flow Zone Characterisation of the RCF Area Summary Report, July 1995.

COR/525

Nirex Science Report S/95/008, Sellafield Hydrogeological Investigations. The Hydrochemistry of Sellafield 1995 Update.

COR/526

Nirex Science Report S/95/002, Post-closure Performance Assessment: Treatment of the Biosphere. May 1995.

COR/527

Nirex Science Report S/95/003, Nirex Safety Assessment Research Programme: Nirex Biosphere Research: Report on Current Status in 1994, July 1995.

COR/529

Nirex Science Report S/95/011 Nirex Safety Assessment Research Programme: Nirex Near Field Research: Report on the Current Status in 1994, July 1995.

COR/605

The Royal Society, Disposal of Radioactive Wastes in Deep Repositories, November 1994.

NRX/15/4

Environmental Science and Technology, Volume 29, No 7 1995.

NRX/15/5

Nirex Report NSS/G 120, NSARP Reference Document Gas Generation and Migration January 1992.

NRX/15/6

Geoval '94 Validation through Model Testing. Proceedings of an NEA/SKI Symposium, Paris, France 11-14 October 1994.

NRX/15/7

Nirex Report NSS/R309 The Mass-Transport Properties of Cementitious Materials for Radioactive Waste Repository Construction, July 1995.

NRX/15/8

OECD/NEA International Stripa Project 1980-1992 Overview Volume III Engineered Barriers.

NRX/15/9

Gas Generation and Release from Radioactive Waste Repositories Proceedings of a Workshop organised by NEA in co-operation with ANDRA, Aix-en Provence, 23-26 September 1991.

NRX/15/10

Nirex Report NSS/R 398, Development of a Methodology for Modelling the Redox Chemistry and Predicting the Redox Potential of the Near Field of a Cementitious Radioactive Waste Repository September 1995.

NRX/15/11

Solutions, Minerals and Equilibria by Robert M. Garrels and Charles L. Christ Freeman, Cooper and Company, 1965.

NRX/15/12

Nirex Report NSS/R 397, Groundwater Composition for the Borrowdale Volcanic Group, Boreholes 2, 4 and RCF3, Sellafield, Evaluated using Thermodynamic Modelling, September 1995.

NRX/15/13

NAGRA Technical Report 91-10, A natural analogue study of the Maqarin hyperalkaline groundwaters.

1. Source term description and thermodynamic database testing.  
December 1992.

NRX/15/14

Nirex Report NSS/R 311, Thermodynamic Modelling of Radioactive Waste Disposal: Assessment of Near-Field Solubility, August 1995.

NRX/15/15

IAEA Technical Report Series No. 247 Sediment  $K_d$ s and Concentration Factors for Radionuclides in the Marine Environment 1985.

NRX/15/16

European Commission: Nuclear Science and Technology, Testing and modelling of thermal, mechanical and hydrogeological properties of host rocks for deep geological disposal of radioactive waste. Proceedings of a workshop held in Brussels 12-13 January 1995.

NRX/15/17

European Commission: Nuclear Science and Technology, El Berrocal project Characterization and validation of natural radionuclide migration processes under real conditions on the fissured granitic environment. Summary report on Phase 1 (1 March 1991 to 28 February 1993).EUR 15908 EN, 1995.

NRX/15/18

Nirex Report NSS/R165

The Sampling and Characterisation of Natural Groundwater Colloids: Studies in Aquifers in Slate, Granite and Glacial Sand, July 1989.

NRX/15/19

Nirex Report 560. An Assessment of the Rock Characterisation Facility on Groundwater Flow and On Risk from the Groundwater Pathway, February 1994.

CCC/5/1

Dry Run 3. A Trial Assessment of Underground Disposal of Radioactive Wastes Based on Probabilistic Risk Analysis. Overview. DOE Report No: DoE/HMIP/RR/92.039

CCC/5/3

