Review of SR 97 Performance Assessment

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This review is an independent technical evaluation of SR 97 by P.D. Glynn. It was carried out in response to a request of SKI as a part of ongoing technical and research cooperation between USGS and SKI. This review is not an official USGS statement on SR 97 or on any of the topics mentioned herein.

Introduction

My review focused on a careful reading of the English version of the main SR 97 reports with occasional referencing to supporting documents. I highlight here what I believe to be the most important questions and issues. Overall, SKB should be commended for obtaining a highly significant amount of scientific information concerning issues of relevance to nuclear waste disposal. In general, SKB's data collection efforts have been quite successful and SKB has gotten many respected scientists and consultants involved in their studies.

The following criticisms of the SR 97 performance assessment should not in any way be viewed as detracting from SKB's real accomplishments over the years.

My training has been primarily in the areas of ground-water flow and ground-water geochemistry and those are the areas in which I can profess some expertise. I have little expertise in other areas, such as canister issues, and in those areas I have taken the perspective of a lay person in reading through the SR 97 performance assessment.

I have grouped my comments on the main report into several general issues and subsequently by pagination within those issues. The issues discussed appear in the following order, which is not necessarily representative of their degree of importance in the SR 97 performance assessment: 1) stability of the bentonite barrier, 2) flow-modeling assumptions, 3) climate change and its effects, 4) independence of SKB's performance assessment, 5) miscellaneous issues and 6) coupling of scenarios. Additionally, a section on simple scientific and editorial mistakes has been added in an appendix. Although not as important as the other topics discussed in this review, editorial and scientific mistakes do affect the way SKB presents its work to the general community and consequently influence the assessment of SKB by both the scientific and lay communities.

1) Bentonite barrier issues

The main report never discusses (e.g. see p. 169) why the canister is not expected to sink through the bentonite. Presumably the canister will be denser than the surrounding bentonite and consequently it should sink, if the medium in which it is placed undergoes plastic deformation, i.e. a property which SKB expects the clay to have given its expected "self-healing" properties. Although this is never said explicitly in the report, it would be my guess that SKB doesn't expect the canister to sink significantly because of the high swelling pressure of the Na-bentonite, which may increase the viscosity of the clay to an extent sufficient to prevent sinking of canister through the bentonite. A question which is not well discussed in the main report is: **How would changing the initially pure Na-bentonite to an almost pure Ca-bentonite affect the properties of the clay?** What would be the effect of an unevenly distributed change in the properties of the bentonite as it comes into contact with $CaCl_2$-rich water at specific fracture/joint openings in the canister vault? Could this result in a tilting of the canister? The contacting water may well be a shield brine with a very high Ca/Na ratio and consequently would be expected to strongly affect the properties of the clay as well as its swelling properties. The heat capacity of the bentonite barrier would also be affected and therefore this could also have an impact on the maximum temperature around the canister. SKB should clarify all these points.

The report mentions the **possible illitization of the bentonite** buffer (p. 50). It is not at all clear that SKB considered the effect of the possible intrusion of seawater into the repository. Sea water has a considerable potassium concentration, many times higher than that of the ground waters presently observed at the 3 sites investigated.

On this same topic, on p. 201, table 8-11, SKB gives estimates for the illitization of montmorillonite based on the supply of potassium. The problem with their assessment is that it doesn't take into account the possibility of Baltic water intrusion (which presently has about 100 mg/L potassium near Aberg) or the possible intrusion of seawater (which would have about 400 mg/L potassium). If the greater possible supply of potassium is taken into account, this would mean that about 30% of the montmorillonite could be converted after 1 million years (assuming present Baltic water concentrations of potassium remain next to the near field through out that time) or even complete conversion of the montmorillonite to illite after 800000 years, assuming potassium concentrations near seawater levels. Therefore, SKB may wish to revisit this issue and assess more carefully the possibility of prolonged seawater intrusion near repository depth at

Aberg and Beberg, as well as examine more carefully the possible rate of illitization and its dependence on various environmental factors.

On p 279, **the issue of colloid transport** is raised and discarded because the "high mineralization" of the ground waters does not result in significant colloid suspension. In this regard the potential intrusion of glacial meltwaters (predicted in the climate scenario) may be problematic because these waters would be expected to be very dilute, and could therefore cause significant colloid suspension. As a result, colloidal radionuclide transport and possibly bentonite buffer erosion are two issues that should be investigated further as part of the climate scenario. These issues do not seem to be adequately addressed in the context of the climate scenario.

2) Flow-modeling issues

The SR 97 report mentions at several points throughout the report (e.g. p. 157, p. 159) the large contrast between the hydraulic conductivity of fracture zones and the hydraulic conductivity of the rock mass. These statements suggest that SKB essentially considers only two flow domains of interest at the 3 different sites. This simplistic view permeates the report. In actual fact, there is a continuum of hydraulic conductors throughout the rock, ranging from "primary" fracture zones to less extensive and possibly less conductive individual fractures. **SKB's view seems to be that so-called "second-order" and "third-order" fractures can be neglected as conductors.** Actually, although such fractures do not, on an individual basis, transport as much water as one of the more extensive fracture zones, their much greater number could make them important water conductors, which should not be neglected in SKB's analysis. The hydraulic importance of second and third order fractures should be properly evaluated by SKB, on an aggregate rather than on an individual basis.

On p. 251, the report mentions that numerical flow modeling calculations were not done with a code capable of simulating density dependent flow. Instead environmental heads were used. It seems to me that this is a significant gap in SKB's modeling effort.

On p. 275, the report mentions that the **maximum matrix penetration depth used for the radionuclide transport model is not important** because the actual penetration depth of the raionuclides is much smaller. **This statement may apply to the transport of strongly sorbing radionuclides, but does not apply to that of non-sorbing or weakly sorbing radionuclides.**

On p. 278, the statement is made that the model-calculated travel times at Aberg (for transport from the repository to the surface) are compatible with observations of very old waters at repository depth. **The fact that the waters are old at repository depth actually has very little bearing on the transit time from the repository to the surface.** At best, the ages only reflect the possible extended travel time between the surficial recharge area and the repository. It does not have anything to do with how long it will take the repository waters to **discharge** to the surface.

On p. 310, the report summarizes the "reasonable" and "pessimistic" flow-wetted surface areas (per volume of water), a_w, used for the transport calculations and the resulting advective travel times. Given the flow-wetted surface areas used in the transport calculations and given the average fracture spacing at each site (equal to twice the maximum penetration depth used in the transport calculations), we can calculate the effective porosities that should be applicable to the transport model at each site. The wetted surface area can be related to the average fracture aperture, f_a, by the relation $a_w = 2/ f_a$. The effective porosity, n, can then be calculated knowing the average fracture spacing, s, by $n = f_a/s$, or $n = 2/(a_w s)$. The results are given in the table below and compared to the porosities assumed by SKB in their transport models of the 3 sites.

Site	a_w in m^{-1}	f_a in m	s in m	n calculated	n assumed
Aberg reasonable	10^4	$2x10^{-4}$	4	$5x10^{-5}$	10^{-3} to 10^{-4} at all sites
Aberg pessimistic	10^3	$2x10^{-3}$	4	$5x10^{-4}$	
Beberg reasonable	10^4	$2x10^{-4}$	4	$5x10^{-5}$	
Beberg pessimistic	10^3	$2x10^{-3}$	4	$5x10^{-4}$	
Ceberg reasonable	10^4	$2x10^{-4}$	40	$5x10^{-6}$	
Ceberg pessimistic	10^3	$2x10^{-3}$	40	$5x10^{-5}$	

The internally consistent porosities that I calculate from the wetted surface areas and fracture spacings used in the SR 97 report are 1 and 2 orders of magnitude lower than the porosities actually used in the SR 97 transport modeling effort. This suggests that 1) the resulting model-calculated advective travel times may either be too long by 1 or 2 orders of magnitude or 2) the wetted surface area and/or fracture spacing (i.e. max. penetration depths) values used for the transport calculations may be too high by 1 or 2 orders of magnitude. The relation that I used to relate the wetted surface area to the average fracture aperture likely provides a minimum estimate of the fracture aperture, since fracture surface roughness is not taken into account, but I don't believe that this would be responsible for an order of magnitude difference.

On p. 314, I disagree with the statement that is made that "only pessimistic values are used for delay time...and maximum penetration depth in the rock matrix". **The maximum penetration depths used are highly optimistic values.**

3) Climate Change and the Oxygenated Glacial Meltwater Issue

Bottom of p. 193, top of p. 194: SKB claims, citing the Gascoyne (1999) and the Guimera et al (1999) reports, that "there are no indications that iron(II) minerals have been oxidized by oxygenated groundwater anywhere at repository depth". The major problems with this statement are: **1) the cited reports did not actually look for, or comment on, any reported field evidence of iron(II) mineral oxidation at depth, and 2) the statement simply ignores the field evidence presented by Glynn et al. (1997), Glynn et al. (1999) and Glynn and Voss (1999).** At the very least, SKB should mention that their opinion, with respect to the possible intrusion of oxygenated meltwaters, is not believed by all scientists. They should definitely cite all the pertinent reports on both sides of the issue. Not to do so is unprofessional and may lead external researchers and eventually the greater public to suspect that SKB has not been forthright in its performance assessment, and that SKB is avoiding mention of issues and facts that may be unfavorable to their selected repository design.

On p. 244-248, there is a nice discussion regarding radionuclide solubilities in the context of the canister defect scenario. Unfortunately, this discussion is not repeated in the climate evolution scenario, with due consideration to the possible impact of the contact between the fuel and oxygenated waters.

On p. 317, the statement is made that "the results should be viewed in the light of the fact that extensive glaciations are to be expected in Sweden within a period of a hundred thousand years, which is the subject of the climate scenario in Chapter 10. A glaciation leads to erosion of virtually the entire soil layer. Aberg can be expected to be under the sea for a large part of the next hundred thousand years." These statements/opinions have no place in this chapter. They should be left for Chapter 10, i.e. for the discussion of the climate scenario. **Scenarios should be discussed entirely within their own limits. If discussion of the interaction between various scenarios is needed (which is actually a good idea), these interactions should then be fully discussed and afforded an entire section of their own in the performance assessment report.**

On p. 356, the report states that glacial meltwater is, like meteoric water, rich in oxygen. This statement appears to be a subtle attempt to disguise the fact that **glacial meltwater is actually likely to be much richer (by 3 to 5 times at least) in dissolved oxygen than normal atmosphere-equilibrated water**(Glynn et al., 1997; Glynn and Voss, 1999; Glynn et al., 1999).

The climate scenario chosen for SR 97 differs markedly from the one chosen by SKI for the SITE-94 project (King-Clayton and others, 1995) from a common starting point based on SKB work. One of the main differences is the smaller extent of the ice-sheet expected during the first two glacial periods, particularly during the second one. Conveniently for SKB's performance assessment, their SR 97 climate scenario has only the ice marginal zone reaching Aberg during that glacial cycle (between 60000 and 70000 years). This is in marked contrast to the SITE-94 climate scenario where the ice sheet was assumed to reach a height of 2 to 3 km over Aberg during the second glacial cycle.

On p. 363, the report makes the statement that the oxygen dissolved in glacial meltwaters will be consumed mainly by reactions with the minerals in the geosphere. While we agree that the geosphere minerals will react with dissolved oxygen, the statement gratuitously implies that **all** the dissolved oxygen will be reacted away, i.e. consumed. This is a misleading statement that is not backed up by any substantial field evidence (Glynn et al., 1999, Glynn and Voss, 1999).

On p. 370, in the discussion of the effect of glaciation on Aberg groundwater flows, it would have been much more useful if the report had given order of magnitude estimates for the expected increase in ground water flow, instead of using purely qualitative statements.

The transport of oxygen-rich meltwater is discussed briefly on p. 379 of the report. The discussion does not mention at all the fact that some scientists (myself in particular) consider that oxygenated meltwaters could indeed penetrate to repository depths and that oxygenated conditions at that depth could remain so for significant periods of time (thousands of years). SKB discounts this possibility, primarily on the basis of 2 reports, one by Guimera et al. (1999)

and another one by Gascoyne (1999). **Given that both reports have significant flaws and that the Guimera et al. (1999) report actually states that the geochemical model used in its calculations was "optimistic", i.e. not at all "conservative", SKB is being disingenuous by ignoring other scientific opinions. The Guimera et al. report actually states, despite its "optimistic" calculations, that fast flowing fractures could indeed bring oxygenated water to repository levels.**

Furthermore the discussion misleadingly implies that the results of the Guimera et al. (1999) report were checked by Gascoyne (1999). In fact, the Gascoyne report did not check the method or calculation results of the Guimera et al. report. The discussion misleadingly states that there are no geological indications that oxidizing water has occurred at repository depth. It also fails to mention that there is no geological evidence proving that reducing conditions have always been maintained at depth over the last 100000 years. **In fact, on balance the geological evidence suggests, albeit does not conclusively prove, that oxidizing conditions probably did occur at repository depth over the past 100000 years (Glynn et al., 1997, 1999; Glynn and Voss, 1999). Despite the earlier statement about lack of geological evidence, the SR 97 report actually mentions possible evidence from Finland (without citing references!) that does suggest that oxidizing conditions possibly penetrated to significant depths during the last glaciation.**

Furthermore, the discussion in the SR 97 report of the conclusions and results of the flow modeling work done by Svensson (1999) supports the earlier modeling results found by Provost et al. (1998) and incorporated in Glynn and Voss (1999). The discussion in the SR 97 report contradicts many of the erroneous statements made by Gascoyne (1999), who primarily discussed the flow and transport modeling work of Provost et al. (1998).

On p. 381 and 382, the report provides some water analyses that may be representative of glacial meltwaters after reactions in a fractured rock environment. The report does not provide references for these analyses, but more importantly misleadingly claims that the two waters (from Grimsel and Taavinunnanen) differ substantially in their chemistry from a glacial meltwater analysis that is also provided. First of all, the report fails to mention that the water at the base of an ice sheet might differ substantially from the water collected from the outlet of a mountain glacier. Secondly, although the Grimsel and Taavinunnanen waters indeed contain more solutes than the glacial meltwater, they nevertheless are still exceedingly dilute compared to most normal ground waters, and more importantly there is absolutely no evidence that they are reducing waters. There are no Fe or Mn or sulfide concentrations given, so the concentrations of these indicators of reducing conditions must not be very important, otherwise they would have been provided. The only redox sensitive compound for which concentrations are provided is sulfate. This suggests that the Grimsel and Taavinunnanen waters are actually oxidizing waters and may even contain dissolved oxygen, although no measurements are given. Therefore, by themselves, the water analyses provided by SKB contradict their own conclusions regarding the potential for deep penetration of oxygenated water.

On p. 441, the report mentions that "there is a very great potential for oxygen consumption in the minerals in the geosphere". This statement, while strictly true, is misleading because it ignores the fact that 1) access of the waters to all this reductive mineral mass may be very limited and 2) the kinetics of reaction of these minerals are generally extremely slow. In other words, there are very good odds that the "very great potential for oxygen consumption" will never be realized.

Finally, on p. 442 the report mentions that oxygenated water is not expected to infiltrate to repository depth other than during "very limited" periods. It would be useful if SKB could quantify the "very limited periods" and also of course provide the appropriate range of uncertainty for their analysis. This would better show SKB's confidence in their statement.

4) Independence of SKB's Performance Assessment Efforts

On p. 440, the report makes the blanket comment "A comparison with safety assessments in other countries shows that the set of scenarios that is analyzed in SR 97 agrees very well with other assessments". The question in this reader's mind is: to what extent were these assessments independent? This is a important question which should be addressed by SKB. In my experience, I get the impression that international "experts" on nuclear waste disposal issues often work for different countries and/or agencies on performance assessments and that the same sources of knowledge are "shared" between assessment efforts. Therefore, the true independence of these efforts is questionable.

5) Miscellaneous issues

p. 140 and 141. It would be nice if the report mentioned why the chemotoxicity of Pu and U, which differs from their radiotoxicity, can be considered insignificant in the safety analysis. Nothing is said about this subject in SR 97.

Although on p. 184, SKB lists processes that are influenced by the composition of pore waters, there is no effort made to list the rate estimates for such processes. The rates of several of these processes are crucial in understanding to extent to which the near-field barriers will be able to maintain their function over the desired lifetime of the repository.

On p. 206, when discussing the potential for sulfide corrosion of the copper canisters, the report mentions that "pyrite is evenly distributed throughout the buffer and that there is no reason to expect local attacks". I don't see why local corrosion could not be expected particularly in a environment (the bentonite) where the migration of ions will proceed only by diffusion rather than by advection. Even if the pyrite appears "evenly" distributed, there is bound to be significant changes in the composition of the porewaters (in sulfide activity and in pH and EH conditions) next to the canister, on a cm scale or smaller. Such changes could cause enhanced corrosion of the canister in some spots.

On p. 223, the report mentions that the requirement is that the k_{eff} value not exceed 0.95. Given that criticality will occur at a value of 1, from a layman's perspective the target k_{eff} value of 0.95 seems rather high, i.e. it does not appear to leave much of a safety factor. The discussion of criticality, the precautions taken to avoid it and the consequences of its possible occurrence, appears a bit meager to me.

On p. 377, the report mentions that the total pressure on the canister during a glaciation is expected to be about 39 MPa and that the canister inserts have been calculated to withstand an external evenly distributed pressure of 80 MPa or 110 MPa, depending on the actual design used. This analysis, however, probably does not take into account the possible weakening of the canister insert because of corrosion or other effects. Consequently, the safety margin regarding the design of the canister insert would appear to be rather small.

On p. 411, the discussion of canister failures and canister damages caused by earthquakes is rather confusing, possibly because a "damaged" canister may not necessarily represent a canister "failure".

6) Lack of Coupling between Scenarios

In general, one of the problems with the climate scenario, is that there is little coupling between the chemical and hydraulic evolution of the near-field and far-field systems and their mechanical evolution (particularly for the near-field).

On p. 414, the SR 97 report mentions that in the earthquake scenario analysis no credit has been taken for the fact that it **will** be possible to reject "unsuitable" canister positions. It is quite interesting to read this statement for this scenario, and to contrast it with the exact opposite statement which is made in the Guimera et al. (1999) report, with regards to the climate scenario. In that report, credit is claimed for the fact that it will be possible to reject "unsuitable" canister positions. All in all, SKB's approach and methodology seems a bit inconsistent and apparently depends on the seriousness of the studied scenario.

As a general comment, I think that the earthquake and climate scenarios should have been explicitely linked together in a separate scenario, given the strong coupling between deglaciation and earthquake frequency.

On p. 442, the report states that "The overall conclusion of the climate scenario is that the climatic evolution does not lead to failure of intact canisters". Again, this is misleading because the climate scenario does not examine the impact of glaciation and deglaciation on corroded or defective canisters. (The corrosion of the canisters does not necessarily have to occur from the presence of oxidizing waters, it could also occur from localized sulfide corrosion. Also, the report never examines the impact of earthquakes as part of the climate scenario, on these corroded canisters).

Conclusions

This review has identified many technical problems in the SR 97 performance assessment. The general impression of this reviewer is that SKB has been disingenuous in its performance assessment effort. It has not cited important differences of opinion with its own views. Furthermore, there are many inconsistencies in the SR 97 report that all together leave the impression that there are many more uncertainties in the SR 97 performance assessment than SKB would perhaps care to admit. Additionally, despite SKB's statements to the contrary, many of the analyses conducted for the SR 97 performance assessment can be clearly shown not to have been based on "conservative" assumptions. Finally, SKB has made little effort to consider possible coupling effects between their different scenarios in SR 97. This is a serious flaw in the SR 97 performance assessment.

The comments in this review should not be taken to imply that the KBS-3 nuclear waste disposal method will not be able to meet the safety and radiation protection requirements which SKI and SSI have specified in recent years (p. 456). Instead, my conclusion is simply that the SR 97 performance assessment of the KBS-3 method would have been more believable had it been based on a forthright and comprehensive discussion of facts, uncertainties and opinions, and on a more conservative choice of assumptions. As it stands, the SR 97 performance assessment is not very credible.

Appendix

Simple Editorial/Scientific Mistakes:

As a general policy, SKB should try to ensure that their public documents receive adequate technical and editorial review to ensure that mistakes such as those mentioned below do not see the light of day in future reports. Although not particularly important from a scientific point of view, the mistakes reflect on SKB's professionalism and will impact the impression that their published documents will give to lay readers and journalists regarding SKB's competence.

SKB should also have a formal mechanism set up by which they could officially (in print and with wide public distribution) retract, or correct, the results of any prior reports that they might have published that they might believe were in substantial error. Any retraction or correction should of course be fully explained and justified. I know of at least one, and possibly two, reports that SKB managers have professed, through verbal communication, to be in error. Leaving this issue aside, the following illustrates some of the scientific and editorial mistakes made by SKB in their SR 97 main reports.

On p. 117, SKB mentions that asphalt occurs as a fracture-filling mineral. This must be a mistake. I know that the deep hole in the Siljan ring was drilled in the exploration for petroleum in the Scandinavian shield, but this observation of natural asphalt is really news to me.

On p. 189 and then again on p. 191, the report mentions that the Aberg and Beberg water compositions are in thermodynamic equilibrium with hematite and goethite. This is for all practical purposes impossible. The water is likely to be at equilibrium either with respect to hematite, or with respect to goethite, but not with respect to both. It is surprising that SKB should not be able to catch significant mistakes such as this one. Mistakes like this probably do not significantly affect the performance assessment, but they nevertheless demonstrate a lack of competent technical and editorial review. If simple matters like this are wrong, how can SKB maintain credibility on more serious matters?

On p. 208, the report mentions that "Models that deal with groundwater composition and evolution in different ways are not used directly for predictions in the safety assessment". This statement is plainly wrong. To cite one example, SKB has used some (but not all) of the model results of Guimera et al. (1999) to predict that oxygenated water will not get to repository depth. On that basis, SKB decided to disregard the possibility of oxygenated meltwater intrusion.

On p. 212, SKB makes the comment that "the buffer material is taken from a natural environment where conditions have for millions of years resembled conditions at repository depth in Swedish bedrock". I would argue that both the environmental conditions responsible for the formation of the bentonite and the present environmental conditions where the bentonite is found are actually quite different from Swedish bedrock conditions.

On p. 330, the report makes the comment "The effects of uncertainties surrounding the properties of the buffer as regards radionuclide transport are small. Provided that the buffer's long-term evolution is as in the base scenario, our understanding of the buffer's role in radionuclide transport is good." This statement is essentially similar to saying that provided that there are no uncertainties regarding our understanding of the buffer's long term evolution, there are no significant uncertainties regarding the properties of the buffer. This is an example of circular reasoning. Actually, there may be significant uncertainties regarding the long-term evolution of the buffer and of its properties, and to this reader it simply sounds like SKB is trying to disguise this fact.

On p. 415, first line, the report reads "The frequency assumptions in the risk analysis are thus not pessimistic if a glaciation with subsequent deglaciation should occur within the next hundred thousand years." In other words, the frequency assumptions are optimistic! It is interesting to see the linguistic convulsions to which SKB will lend itself to avoid admitting possible design vulnerability.

On p. 365, two paragraphs one for Aberg and one for Beberg are repeated by mistake.

References

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