Strål
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Swedish Radiation Safety Authority

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Technical Note

2013:33

Seismology – Frequencies and mechanisms

Initial review phase

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SSM perspektiv

Bakgrund

Strålsäkerhetsmyndigheten (SSM) granskar Svensk Kärnbränslehantering AB:s (SKB) ansökningar enligt lagen (1984:3) om kärnteknisk verksamhet om uppförande, innehav och drift av ett slutförvar för använt kärnbränsle och av en inkapslingsanläggning. Som en del i granskningen ger SSM konsulter uppdrag för att inhämta information i avgränsade frågor. I SSM:s Technical note-serie rapporteras resultaten från dessa konsultuppdrag.

Projektets syfte

Uppdraget är en del av granskningen som rör den långsiktiga utvecklingen av bergmassan omgivande det tilltänkta slutförvaret i Forsmark. Detta uppdrag fokuserar på att studera SKB:s hantering av jordskalv som skulle kunna påverka slutförvaret och dess närområde. Frågor som berörs är mekanismer bakom jordskalv och beräkningar av parametrar och frekvenser. Uppdraget går även ut på att granska tillförlitligheten på utförda analyser av sprickrörelser till följd av jordskalv.

Författarnas sammanfattning

Denna rapport gjord av NORSAR sammanställer granskningen av redovisningen av den långsiktiga säkerheten för ett slutförvar för använt kärnbränsle i Forsmark ur ett seismologiskt perspektiv. Huvudrapporten och ett antal tidigare rapporter, mestadels framtagna av SKB, har granskats av NORSAR.

SKB:s rapporter reflekterar många år av detaljerade multidisciplinära studier av komplexa problem, till stor del relaterade till att förutse framtiden. Det finns en risk att NORSAR:s kommentarer i denna rapport upplevs som negativa. Det poängteras härmed att detta är standardförfarande inom vetenskapen. Kommentarerna erbjuds i en anda av konstruktiv vetenskaplig kritik och betyder inte att SKB:s arbete generellt sett inte är av hög kvalitet.

Sammanfattningsvis pekar NORSAR på några ämnen som inte verkar vara tillräckligt väl underbyggda eller omhändertagna i nuvarande studier av SKB:

- Även om huvudrapporten siktar mot att förklara hur olika studier och rapporter interagerar med varandra så är det fortfarande svårt att följa hur mellanliggande resultat framskrider mellan olika studier, och vad som påverkar mest på slutresultatet. Eftersom det inte är ett linjärt framtagande av rapporterna är det utmanande för läsaren att spåra hur en del av besluten fattas.
- Relaterat till ovan nämnda punkter är en avsaknad av en systematisk överföring av osäkerheter från mellanliggande resultat till slutresultat och rekommendationer för framtiden. Men, eftersom detta är svårt att göra, borde ett mera systematiskt tillvägagångssätt ha använts för att få en jämnare nivå av konservatism.
- En fundamental bas för nuvarande arbete är att de potentiellt aktiva förkastningszonerna i och runt slutförvaret är endast de som redan är karterade, möjligen utökat med information från uppförandefasen. Maximala magnituder är endast beräknade utifrån zoners dimensioner. Ett bättre övervägande av vad som driver förkastningsrörelser behövs, inkluderat uppkomst av nya sprickor,

tillväxt av förkastningszoner med tiden och en potentiell inblandning av en seismisk riskanalys (se nedan).

- Ett av NORSAR:s stora vetenskapliga problem med SKB:s redovisning är att nivåerna på seismisk spänningsuppbyggnad baseras på medelvärdet över en glacial cykel (cirka 100000 år) i riskberäkningen för kapslarna. Att använda ett medelvärde baserat på en period med mycket hög seismisk aktivitet under isavsmältningsfasen och en period med låg seismisk aktivitet kan vara icke-konservativt.
- NORSAR har ett stort problem med de nivåer på spänningsuppbyggnad som används, då dessa verkar lägre än vad som kan härledas från andra källor, även om man ser på ett mycket långt perspektiv. Uppskattningen av frekvensen baseras på ett medeltal över 100 år av nutida seismisk aktivitet i mellersta och södra Sverige, ett tal som utan bevisning sägs representera en hel glacial cykel. Fortsättningsvis, den beräknade relationen mellan magnitud och frekvens extrapoleras linjärt över tre storleksordningar för magnitud (från 4 till 7) utan att hänsyn tas till trunkering för den uppskattade maximala magnituden.
- Den seismiska aktiviteten skalas linjärt ned till ett litet område runt slutförvaret (5 km radie), vilket kan vara konservativt. Men när denna aktivitet fördelas lika över 30 zoner och där de flesta sedan tas bort med hänsyn till deras stabilitet är detta mycket icke-konservativt. Denna reducering, och dess rättfärdigande, är inte tillräckligt väl argumenterad. Det vore mera rättfärdigat att fördela hela aktiviteten på de potentiellt instabila zonerna (gissningsvis mellan 1 och 5), vilket skulle leda till en risk som är sex gånger större än beräknat av SKB.
- Nivån på seismisk aktivitet, som baseras på nutida data, extrapoleras linjärt över fyra storleksordningar till att inkludera 6 st magnitud 8 skalv över en hel glacial cykel för hela Sverige. Man kommer även fram till, baserat på töjnings-hastighet, att det kommer att ta ungefär 500000 år att ackumulera tillräcklig spänning för att åstadkomma två magnitud 5 skalv. Detta tidsintervall motsvarar cirka fem glaciala cykler lika långa som den tid Weichsel-isen varade. Dessa två mycket olika hastigheter är inte väl motiverade och de behöver därför bli bättre utredda och förklarade.
- Utvärderingen av de potentiellt inaktiva förkastningszonerna förlitar sig på ett antal antaganden, så som modellen för istäcket, jordskorpemodellen, modellen för portryck samt tillståndet för bakgrundspänningen. Även om detta diskuteras ordentligt i underliggande rapporter så återspeglas inte osäkerheterna i riskberäkningen utförligt. Alternativa modeller, speciellt med hänsyn till samverkan mellan spänningsriktning och förkastningsgeometrier, skulle ha kunnat komplettera nuvarande modell för utvärdering av förkastningsstabilitet. Detta skulle kunna bidra till en bättre utvärdering av osäkerheter i modelleringarna.
- Numeriska modelleringar av förkastningsrörelser bygger alltid på en bra kalibrering med empiriska data, som till exempel spänningsavlastning, skjuvhastigheter och markrörelser, och i detta fall är det i stort sett endast den taiwanesiska Chi-Chi-jordbävningen

som används. Flera fall borde ha använts. En sådan kalibrering är speciellt viktig eftersom de tilltänkta förkastningarna är modellerade som plan utan tjocklek. En diskussion om eventuella effekter av en sådan förenkling hade varit användbar, speciellt i en ansökan där förskjutningar på sekundära förkastningar är av intresse.

- Det är inte acceptabelt att slutsatser om markrörelser vid kapslarna baseras på den gamla och daterade GSHAP-studien (Wahlström & Grünthal, 2000) samt överskridning av sannolikheten med 10% över 50 år. Nuvarande arbete lider därför av frånvaron av en probabilistisk seismisk riskanalys som hade kastat bättre ljus över upprepning, karaktärisering av källan (nutida seismicitet), markrörelse-modeller och maximal magnitud. Detta skulle också avsevärt bidra till en bättre underbyggd relation till upprepning än de som används nu och som vi anser är dåligt underbyggda. En sådan riskanalys skulle i första hand täcka tiden fram till förslutning (cirka 100 år).
- En seismisk riskanalys skulle även kunna utökas med en glacial cykel med hänsyn tagen till maximal magnitud, vilken antas kunna vara annorlunda än för nuvarande förhållanden.

Kompletteringar enligt ovanstående punkter skulle bättre stödja SKB:s slutsatser.

Efter utvärdering av rapporterna bedöms det att många val är gjorda med hänsyn till konservativa värderingar, men även att det finns andra viktiga faktorer som har blivit undervärderade. Med stor sannolikhet kan dessa utvärderingar vägas upp av de konservativa elementen när de kombineras.

Projektinformation

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Diarienummer avrop: SSM2012-5697
Aktivitetsnummer: 3030012-4043

SSM perspective

Background

The Swedish Radiation Safety Authority (SSM) reviews the Swedish Nuclear Fuel Company's (SKB) applications under the Act on Nuclear Activities (SFS 1984:3) for the construction and operation of a repository for spent nuclear fuel and for an encapsulation facility. As part of the review, SSM commissions consultants to carry out work in order to obtain information on specific issues. The results from the consultants' tasks are reported in SSM's Technical Note series.

Objectives of the project

This assignment is part of the review regarding the long-term evolution of the rock surrounding the repository. This assignment focuses on the handling by SKB on the impact of earthquakes on repository structures. Issues that concern mechanisms for occurrence of earthquakes including determination of input parameters and earthquake frequency are considered. The assignment includes the assessment of the robustness of the analyses performed.

Summary by the authors

This report summarises the review from a seismological perspective by NORSAR on behalf of SSM (Swedish Radiation Safety Authority) for the assessment of long-term safety of the final repository for spent nuclear fuel at Forsmark. For this purpose, NORSAR has reviewed the main report and a number of earlier reports, mostly developed by SKB.

SKB's reports reflect many years of detailed multidisciplinary studies of complex problems, largely related to predicting the future. A danger in reading NORSAR's comments is that they may appear to be generally negative. It is emphasized here that this is a standard scientific process. Comments were offered in the spirit of constructive scientific criticism and do not imply that SKB's work is not generally of high quality.

In summary NORSAR points to some issues that do not seem sufficiently well justified and covered in the present SKB studies:

- Even though the main report aims at explaining how the different studies and reports interact with each other, it is still difficult to follow the way in which intermediate results propagate between the different studies, and what counts the most for the end result. Since there is not a linear progression of the reports, it becomes challenging for a reader to trace the way in which some of the decisions have been developed and taken.
- Related to the point above is that there is a lack of systematic propagation of uncertainties from intermediate results into the final result and recommendations. However, since this admittedly is difficult to achieve, there should at least have been a more systematic approach towards the use of a more consistent level of conservatism.

- A fundamental basis for the present work is that the potentially active fault zones within and around the repository are those, and only those, that are already mapped geologically, potentially supplemented by further studies during the excavation phase. Maximum magnitudes are derived solely from the dimensions of the zones. What is needed is a better consideration of the dynamics of earthquake faulting, including breaking of new fractures, growth of faults over (geologic) time and necessary inferences from the result from a seismic hazard analysis (see below).
- One of NORSAR's main technical concerns with SKB's study is that the seismic loading levels as applied in the risk assessment for the canisters are based on an average over one glacial cycle (on the order of 100ka). Given that this is an average between a very high seismic activity level during the deglaciation phase and much more quiet, pre-glacial periods. This assumption seems to be non-conservative.
- NORSAR's major issue with the seismic loading levels used, which seem to be lower than what can be justified, for a long term average. The main frequency estimate comes from the average of 100 years of present day seismicity in central and southern Sweden, a number which without justification is claimed to properly represent the average over a full glacial cycle. Also, the derived magnitude-frequency relation is extrapolated linearly over about three orders of magnitude (from 4 to 7) without any independent consideration of truncation by a maximum magnitude assessment.
- This seismicity is linearly scaled down to a small (5 km-radius) area around the repository. This may be conservative, but when this activity is distributed linearly on 30 zones and most of them are removed later on the grounds of stability, that would be strongly non-conservative. The way this reduction of the number of zones is done, is not sufficiently well justified. In fact, it would be more justifiable to distribute the full activity on only the potentially instable zones (say between 1 and 5). This accounts for an underestimation by a factor of six in SKB's analysis.
- The seismicity level used, based on present day data, is extrapolated linearly four orders of magnitude to include 6 magnitude 8 earthquakes over a full glacial cycle, for all of Sweden. It is also found, however, based on strain rate arguments, that it will take about 500,000 years to accumulate sufficient stress for two magnitude 5 earthquakes (i.e. five glacial cycles of Weichselian lengths). These two very different rates seem difficult to reconcile and should therefore be better supported by SKB.
- The assessment of potentially instable fault zones hinges on a number of assumptions, including the ice sheet model, the Earth crust model, the pore pressure model and the background state of stress. While this is properly discussed in underlying studies, the associated uncertainties are not well reflected in the final assessment. Alternative models, in particular for the interplay between stress directions and fault geometries, could therefore have complemented the current analyses for the fault stability assessments. This would in particular contribute to a better assessment of modelling uncertainties.

- Numerical modelling of fault motion always depends on a proper calibration against empirical data, such as for stress drop, slip velocities and ground motions. SKB essentially uses only the Chi-Chi earthquake in Taiwan. More data should have been brought in here. Such calibration is particularly important since the causative fault is modelled as a single plane without thickness. In an analysis where the rupture effect on secondary fractures is the focus it would be useful to discuss what the possible effects of this simplification could be.
- The inferences concerning shaking levels for the canisters are based on the old and outdated GSHAP study (Wahlström & Grünthal, 2000) and for an exceedance probability of 10% in 50 years, which is not acceptable. The present work therefore suffers from the lack of a probabilistic seismic hazard assessment that would shed better light on recurrence, source characterization (contemporary seismicity), ground motion models and maximum magnitude. This would also substantially contribute to better justified recurrence relations than those currently used and which NORSAR considers poorly founded. Such a hazard assessment should primarily cover the time until closure (~100 years).
- A probabilistic seismic hazard assessment could also be extended to include a glaciation with respect to maximum magnitude, which should be expected to be different from present conditions.

Improvements in the comments above would substantiate SKB's conclusions.

After evaluating the reports, it seems that many choices made by SKB would contribute to conservative estimates, but also that some other important factors might have been significantly underestimated. It is quite possible that these underestimations may outweigh the conservative elements when combined.

Project information

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Contents

1 Review assignment

The present NORSAR review for the Swedish Radiation Safety Authority (SSM) of the plans and strategies for safe and permanent subsurface storage of high- and intermediate radioactive waste at Forsmark, Sweden, is the second one done by NORSAR. The first review was conducted in the spring of 2010 and included the following reports:

- Bäckblom & Munier, SKB TR-02-24, 2002.
- Munier & Hökmark, SKB R-04-17, 2004.
- Lampinen, POSIVA Report 2007-69.
- Hedin, Math. Geosci., 2008.
- Lagerbäck & Sundh, SGU Paper C 836, 2008.
- Lund, Schmidt & Hieronymus, SKB TR-09-15, 2009.

The present review is conducted under the framework agreement SSM2012-4733 with main attention on "Seismology – Frequencies and Mechanisms". The following reports and sections were selected by SSM for this review (listed chronologically on report date):

- Munier & Hökmark, SKB R-04-17, 2004: Respect distances; Rationale and means of computation.
- Bödvarsson, Lund, Roberts & Slunga, SKB R-06-67, 2006: Earthquake activity in Sweden; Study in connection with a proposed nuclear waste repository in Forsmark or Oskarshamn.
- Fälth, Hökmark & Munier, SKB TR-08-11, 2010: Effects of large earthquakes on a KBS-3 repository; Evaluation of modelling results and their implications for layout and design [Sections 1, 3, 4.4, 5, 6.1-6.5, 7.1, 7.2 and 8.2-8.3].
- Hedin (project leader), SKB TR-10-48, 2010: Geosphere process report for the safety assessment SR-Site [Sections 4.1.1-4.1.3, 4.3].
- Hedin (project leader), SKB TR-11-01 SR-Site Report, (Vol. 1-3), 2011: Long-term safety for the final repository for spent nuclear fuel at Forsmark; Main report of the SR-Site project [relevant sections].

Additionally, several reports and publications are listed by SSM because they could contain relevant information for the present review assignment (SKB TR-08-05; SKB TR-10-21; Saari, 2008; SKB R-06-89). In addition the reviewers have also accessed several other reports and publications, including the main geology report (Stephens et al., SKB R-07-45).

The review instructions are comprehensive and we repeat the items that all of the external experts have been asked to consider:

- Completeness of the safety assessment
- Scientific soundness and quality of the SR-Site
- Adequacy of relevant models, data and safety functions

- Handling of uncertainties
- Safety significance: Quality in terms of transparency and traceability of information in SR-Site and in the associated references
- Feasibility of manufacturing, construction, testing, implementation and operation of repository and engineered barrier components (if relevant for the specific assignment)

Only some of these points are pertaining to the present assignment which primarily is concerned with frequencies and mechanisms within seismology.

In addition to these general questions a number of more specific questions have been listed by SSM as potentially helpful for the elaboration of the above points. These questions are as follows:

1. Are there any obviously missing pieces of information in the SR-Site and its supporting references within the area covered by the specific review assignment?
2. Are key scientific conclusions adequately supported and justified? Are the necessary references provided and are they sufficiently specific?
3. Are there any alternative results or alternative scientific explanations published in the open scientific literature related to the assignment area which have not been addressed or mentioned by SKB? If so, please provide those references.
4. Is the source information of key datasets related to the assignment sufficiently described and referenced? Is any data treatment explained and justified (e.g. derivation of effective parameters)?
5. Are mathematical models including utilised assumptions related to the assignment sound and sufficiently explained and justified?
6. Has SKB defined any safety function(s) that is closely related to a specific review assignment? If so are safety functions and their associated safety function indicators and criteria adequately explained and justified in the SR-Site?
7. Are all known and relevant uncertainties related to the scientific area of a specific review assignment identified, analysed and discussed in sufficient detail?
8. Is the overall safety relevance of the specific review assignment within its scientific area explained and justified?
9. Is the safety assessment strategy for handling of issues related to a specific review assignment explained in a clear manner?
10. Is information at different levels in the safety assessment consistent and logically subdivided (e.g. main SR-Site report, main supporting references and other references)?
11. Are there any particular aspect of manufacturing, construction, testing, implementation and operation of the repository facility or its engineered barriers that might challenge the long term safety as presented in the SR-Site?

2 General review comments

Initially it is important to emphasize again that our comments are offered in the spirit of constructive scientific criticism, which means that we concentrate not so much on what is found to be well covered but rather on areas where improvements still can be achieved, for example through better justifications of the choices made. Critical comments should therefore not be viewed as detracting from the high quality of the work. The reports are also generally well written and comprehensive.

While the present review is done in a (chronological) report-by-report order we also have a number of more general comments which apply to the entire project, even if they may be listed under a specific report heading.

The work conducted by SKB and cooperation partners in this project is both extensive and impressive, and generally with a high scientific quality and justification of choices. The task is, however quite complex, and a lot of work has obviously been invested in the overall design and planning, aiming at an optimal combination of the different efforts. We return to this in a comment to the main SR-Site report, recognizing the efforts to document the way this large project has been organized and conducted, based on 11 steps. Our point here is that the overview is more organizational than dynamic, and that more efforts could be done to trace the way in which the different studies and intermediate results are passed on to other studies along the decision chain, leading up to the final results. This is important not the least because the investigations have been conducted over a long time period, and new insights may exist today which were not available earlier in the project. Admittedly this will be different for the different project components, where we have concentrated on seismology (frequencies and mechanisms).

In this project the stability of the mapped faults in the Forsmark region is a major issue and decisive for the final layout of the repository. The basis for the stability assessments largely come from Lund et al. (2009, TR-09-15), where the underlying assumptions and uncertainties are discussed in detail, including the ice sheet model, the Earth model, the pore pressure model, and the background state of stress, where both reverse and strike-slip stress fields were modelled. The key to the fault stability lies in the combined stress field originating from the ice sheet effects and the tectonic background stress, and we find that the model sensitivity in the fault stability assessments are not well enough covered and discussed, possibly resulting in a too small number of potentially unstable faults. We return to this under our comments to specific reports.

For obvious reasons the focus is set on fault stability at shallow depths (< 5-6 km). While this is understandable, there is a slight contradiction between this and the occurrence of larger 6+ earthquakes. Larger earthquakes (like Pärvie) break through large parts of the crust and may originate in the deeper crust before it ruptures towards the surface. For the larger earthquakes the evaluation of fault stability at shallow depths may therefore not be fully appropriate. Lund et al. (2009, TR-09-15) modelled fault stability under varying conditions, and it seems (see

Figs. 7.14 and 7.15 in TR-08-11) that primarily the more shallow stability estimates have been used in the later applications.

Lund et al. (2009, TR-09-15) found a significant sensitivity to the ice sheet model, the Earth model, the pore pressure model and the background state of stress, and they state that it is unfortunate that they have not been able to investigate an alternative ice history (to that of Näslund). One such model is shown in Figure 2.1. When looking at one of the computations for deeper (9.5 km) reverse fault stability (Lund et al., 2009, Fig. 9-13), it can be seen that fault stability is quite sensitive to stress direction (or fault geometry). Essentially, the same message of instability in a reverse regime for different depths is given in Fig. 7.11 in Fälth et al. (2008, TR-08-11). From this model (Fig. 9-13 showing the deeper reverse faulting model stability for Forsmark) the following question/suggestion emerges:

- Would it be appropriate to investigate how sensitive the models are for changes in stress direction/fault geometry at deeper levels (e.g. at 10-15 km)? It seems quite sensitive to such changes, and it is also possible that this deeper reverse model could be more representative for the larger earthquakes than a mixed or strike slip regime.
- If the above assumptions are correct it could be possibly that new faults within the repository area could be considered unstable (where currently only one fault, ZFMA2, is considered unstable).

Another component which warrants a more general comment is the way in which uncertainty is handled, and the associated levels of conservatism. It appears now as if conservatism is applied in different ways in the different reports, even with different terminologies (see our comment on this to for SKB TR-11-01). With this structure it is difficult to evaluate the final uncertainties in a consistent way. Uncertainty propagation has not been done in any systematic way, and is admittedly very difficult to do in a complex project like this. An alternative could have been to use best estimates in all of the underlying and intermediate models, and to impose the desired level of conservatism only at the very end, possibly through different scenarios. The approach currently followed may lead to over-conservative results, but due to the lack of systematic error propagation it is not possible to predict the actual level of conservatism in the final results/recommendations.

It would have been reasonable to expect that for a major risk analysis of the present kind one of the starting points should have been a seismic hazard analysis conducted in order to assess the present seismic hazard levels, subsequently combined with the considerable SKB efforts to assess the potential developments over subsequent glaciation cycles. Such a hazard analysis may not necessarily be as elaborate as the one for the Yucca Mountains repository (Stepp et al., 2001), but it is far from satisfactory not to have any such analysis all (at least nothing has been presented to us). Detailed, well documented and well justified methodologies for such hazard analyses for critical facilities are now available (SSHAC 1997; Hanks et al., 2009; Coppersmith et al., 2010; USNRC, 2012), and there are many examples of projects where these are applied, including eastern North America and South Africa which both carries many tectonic similarities with Fennoscandia. One of the aims in the SSHAC process is to develop models that properly cover the centre, body and range of technically-defensible interpretations

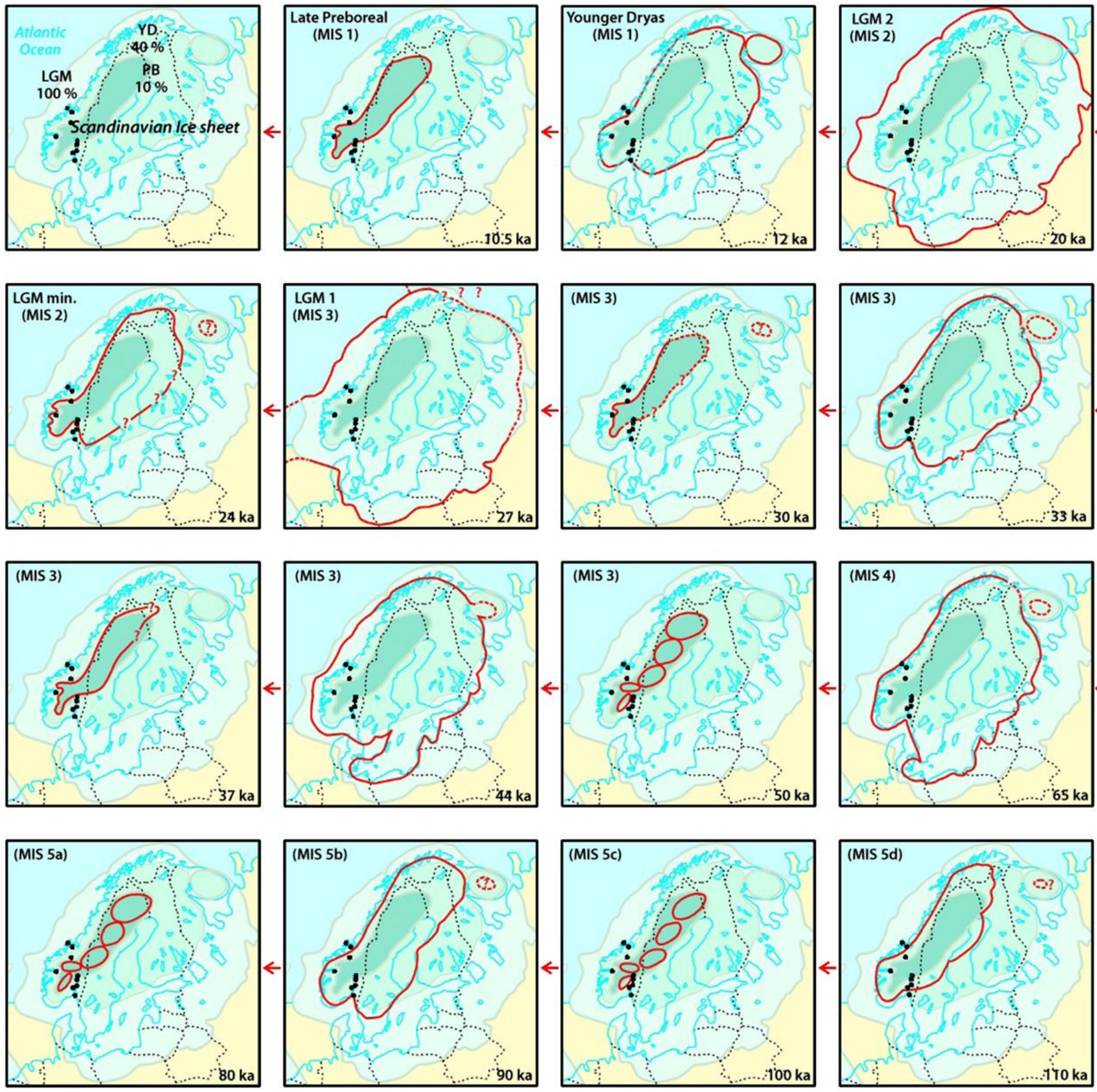

Figure 2.1. Ice model of Slagstad et al. (2009), based on Olsen (2006).

At present only a simple earthquake recurrence assessment based on contemporary seismicity is done (Bödvarsson et al., 2006, SKB R-06-67), including some rather weakly supported assumptions about seismicity uniformity (see specific comments to this investigation). A hazard study will presumably also be required for the repository filling period (before closing), since earthquake shaking may then more easily lead to failures and damages.

One of the most important elements that a hazard analysis would be able to provide is an assessment of maximum magnitude, which is not done at all at present. Instead, a spatially downscaled log-linear magnitude-frequency relation is extrapolated up to a magnitude level which is set by the maximum length of a number of deformation zones that could be potential host zones for future (postglacial-type) earthquakes. The problem related to maximum magnitude in stable continental regions has been the subject of extensive studies for more than 30 years (e.g., Johnston et al., 1994), providing an understanding that would be potentially very important for the present project. An investigation to this side would complement the present assessments which are largely based on geophysical and geological assumptions.

Other basic elements in a seismic hazard analysis would be a detailed seismic source characterization, including potential fault sources, and an earthquake ground-motion model that would provide both excitation levels and attenuation characteristics. As already noted, however, the results from such an analysis would still have to be combined with the non-Poissonian ground-motion contributions from future glaciation cycles.

The final risk assessment seems to be largely based on postglacial earthquakes taking place in an E-W stress regime, where the faults are loaded from long lasting stable background tectonic compressional stress state, and then they are additionally loaded during a deglaciation phase, triggering earthquakes. While this seems reasonable, there are limited discussions of studies that have investigated fault loading and earthquake triggering under different stress regimes, and not the least how sensitive the mapped faults are to alternative stress fields, even if this is to some extent covered in Lund et al. (2009, SKB TR-09-15). This seems important because small variations in the σ_H direction can potentially activate faults in the deposition region which are now considered stable in an E-W stress regime.

As a general comment in closing we note that the reports reviewed do to a large extent build on earlier SKB reports and less on external references. While this is understandable (the reports are comprehensive), the danger of giving too much weight to internal references is obvious. Examples of this can for example be seen in the reference list of Fälth & Hökmark (2008, TR-08-11), showing that 41 out of 92 references (44%) are earlier SKB reports.

3 SKB R-04-17: Respect distances (Dec., 2004)

The concept of respect distances is fundamental both in the SR-Site and the SR-Can projects, where it is justified and used in a clear and consistent way. However, in terms of the numerical evaluation of the concept the project now seems to depend more or less entirely on Fälth et al. (2008, SKB TR-08-11), replacing the earlier analysis in the present report. In our comments to this report in 2010 (Bungum, SSM report, 2010; Lindholm, SSM Report, 2010) we made a note of the fact that hydro-geological conditions seemed to be less considered and discussed; this has now been covered in a better way in Fälth et al. (2008, SKB TR-08-11).

Like for Fälth et al. (2008, SKB TR-08-11), the results are critically dependent on the mapping and detectability of the larger deformation zones, as detailed in the geology report by Stephens et al. (2007, SKB R-07-45). This is a critical prerequisite, and as we understand it, a final assessment here will be done along with the drilling and excavation of the sub-surface repository. Another critical assumption that should be revisited is that the primary ruptures will be limited to the structures already discovered and mapped. Even if earthquakes clearly originate on pre-existing structures it is now seen more and more often that ruptures for large earthquakes (including M_w 7.5) often jump between mapped faults that can be separated by quite some distance (e.g., Schwartz et al., 2012; Lozos et al., 2013; Youngs et al., 2003), while jumping segment step-overs is even less unlikely (e.g., Kase, 2010). There is a rich body of literature on this. Another similar issue is that it has been observed that large earthquakes may break the surface in new ruptures several kilometres from previously mapped surface ruptures, but apparently with the same roots as the surface rupture mapped before the earthquake (the 2005 M=7.6 Muzaffarabad earthquake in Pakistan is one recent example of this, e.g., Fujiwara et al., 2006). This implies that a new surface rupture can be created *near* an earlier mapped fault and with the same roots and tectonics as the old fault.

There also does not seem to be much discussion of the way faults and fractures develop and grow over time, which may not be a big issue given the geologically short time spans in this project (less than a million years). Even so, it would have been useful to discuss this.

The discussion in Section 6 on conservatism is a bit limited in scope and subject. We would like to note here that during the last few years many tools for more advanced finite fault modelling has been developed based on both kinematic and dynamic approaches (e.g., Song and Somerville, 2010), including also the near field and the influence of various non-linear effects. For example, the rupture velocity is very important, and in general the variability of stress drop and thereby the 'patchy' distribution of slip across and along the fault. This even includes super-shear ruptures (e.g., Andrews, 2010), which now are considered to be less unlikely than what was judged earlier. These or similar references are also largely missing in the subsequent reports.

The overarching philosophy in this project, starting from the Munier & Hökmark (2004, R-04-17) report, is that earthquake damage to the canister integrity is exclusively limited to direct fracture hits of the canister. This means that the (elastic) shaking from an earthquake is not foreseen to jeopardize the canister integrity. It is continued as the fundamental basis for the investigation that earthquakes may trigger reactivation (slip) on structures away from the causative fault, which is the reason for the quest for the “respect distance”, which essentially is a tool for identification of a volume within which deposition of canisters is prohibited. However, the conclusion that earthquake shaking during repository filling (before closing) is considered to be risk insignificant seems not to come from Munier & Hökmark (2004, R-04-17) but rather from the earlier report by Bäckbom & Munier (SKB TR-02-24). The apparent rejection of earthquake shaking as a risk during repository filling is commented on below..

With the above outset it is aimed at modelling on how and at what distance a major earthquake (on a known fault) can lead to second order displacements on “target fractures” that are too small to be the site of the major earthquake, but are large enough to be slipping under the influence of the stress-field changes from a nearby main rupture.

The parameter setting of the modelling seems to be appropriate but with a bias to the conservative side. Target fractures of 100 meter radius are used. Variations in initial stress state, geometry and other parameters have been used to explore how the parameter space influences the results. In conclusion a respect distance of 100 meters is defined as adequate. This respect distance is the same for 3 and 10+ km long fault zones.

The modelling conducted in this report has later been followed up with newer analyses and assessments (Fälth et al., 2008, SKB TR-08-11).

Specific comments

Since the report is relatively old and largely replaced by newer studies of the same nature we limit ourselves in the following to more general concepts.

- As already stated, earthquakes occur predominantly on pre-existing deformation zones. The report still claims, however, that fracturing within virgin rock cannot be excluded, but is very unlikely (not quantified). However, as indicated above, this is a lot more likely in cases where the rupture initiates on the roots of a pre-existing fault and continues into more pristine rocks.
- Based on seismological knowledge of subsurface vs. surface shaking from earthquakes we do agree (without knowing the physical specifics of the canisters) that shaking alone is extremely unlikely to cause mechanical canister failure. Even so, this should be supported by investigations (unless we have overlooked work addressing this).
- The earthquake shaking (or other impacts) from large earthquakes taking place during the repository filling does not seem to have been an issue of consideration. We consider this as a risk situation that should also be investigated.
- The M=6 modelling is recognized as a limitation, since larger magnitude scenarios were not used at that time. Later results have demonstrated the big difference between magnitudes 6 and 7.5, and this is not only because larger magnitudes activate larger faults and larger volumes.
- For the above reason the original respect distance of 100 m has later been modified and a more flexible definition adopted.

- The difference and uncertainly regarding earthquake frequency in postglacial and “normal” times is important. We return to this elsewhere.

4 SKB R-06-67: Earthquake activity in Sweden (Feb., 2006)

This is a small report and it is generally transparent. However, there are two problems with it that are quite apparent. Firstly, the report is old (2006) and therefore does not include newer seismicity data from the upgraded Swedish network. Secondly, and this is a greater problem, there is no consideration at all about maximum magnitude, leaving the G-R distributions as unbounded (i.e., linearly extrapolated over three orders of magnitude). This is referred to also in some comments to the main report (SKB TR-11-01), and the problem becomes even more serious since the b-values derived are low and therefore will affect strongly the three orders of magnitude extrapolation used. In a situation where uncertain b-values strongly influence extrapolation it is not uncommon to use a global b-value of 1, which actually would be less conservative.

The report is concerned with analysing natural earthquake occurrence. It is largely based on the available earthquake catalogue(s), heterogeneously generated over approximately the past 100 years (FENCAT). It is stated that magnitudes were homogenized using a relation from Slunga et al. (1984) developed for southern Sweden. Without judging the quality of this relation it may certainly be stated that a lot of relevant data have been recorded since 1984 that can be used for substantiating magnitude regressions. A discussion of the catalogue in terms of potential explosion contamination, completeness, magnitudes, depth distribution, time-variations etc. would have been helpful for evaluating the reliability of the regression results. As this is not done, the report appears as a quick analysis (as also stated in the introduction) that can only serve as a first indication of the contemporary seismic activity. The above deficiencies could have been covered in a much more extensive and satisfactory way if a site specific earthquake hazard analysis had been conducted as a basis for the present risk analysis.

We have for the sake of demonstration tested a different earthquake catalogue, SHEEC (http://www.emidius.eu/SHEEC/), from the pan-European SHARE project (http://www.share-eu.org/), which is also largely based on FENCAT, but using revised location and homogenized M_w magnitudes) for computing an alternative G-R recurrence relation. A comparison is made in Fig. 4.1. The relation developed from the SCEEC data reads $log(N) = 4.23 – 1.06M$ and is very different from the Bödvarsson et al. (2006, R-06-67) relation of $log(N) = 2.63 – 0.81M$. In both cases data from the same area (and the same original catalogue!) has been used for deriving the relation. Since different time periods are used both relations are normalized to 1 year.

Figure 4.1 also contains a (properly scaled) relation from Bungum’s contributions (in Appendix 6) to Hora and Jensen (2005), based on summing the seismic moments from 12 Weichselian postglacial earthquakes, converting this to a G-R distribution under the assumption that the moment release took place over 2500 years. While this curve is higher than what a 100,000 year average would be it serves as a useful reference since it provides an average over the most active 2.5 deglaciation millennia.

Figure 4.1 is used mainly to illustrate the uncertainty of such relations and the need for thorough analysis of uncertainties stemming from inhomogeneous catalogues. It is noted that the Bödvarsson et al. (2006, R-06-67) report acknowledges differences in b-values for two catalogues, and also references other authors that arrived to different values (Kijko et al., 1993; missing in reference list), however, at the end the relation referenced above was used for Forsmark. It may also be noted that the exact numbers could not be reproduced, which may be caused by some missing decimals.

The occurrence of future earthquakes is estimated by extrapolating the derived G-R relation to magnitudes far above the observed ones, and then downscaling it from 650 km radius to 100 km radius and then again to 10 km radius and finally to 5 km radius in later applications. The scientific justification for this downscaling is not well discussed (even if the pragmatic need is recognised). This lack of scientific justification is particularly important since these results have been used in later SKB reports and in the final risk assessment for the canisters.

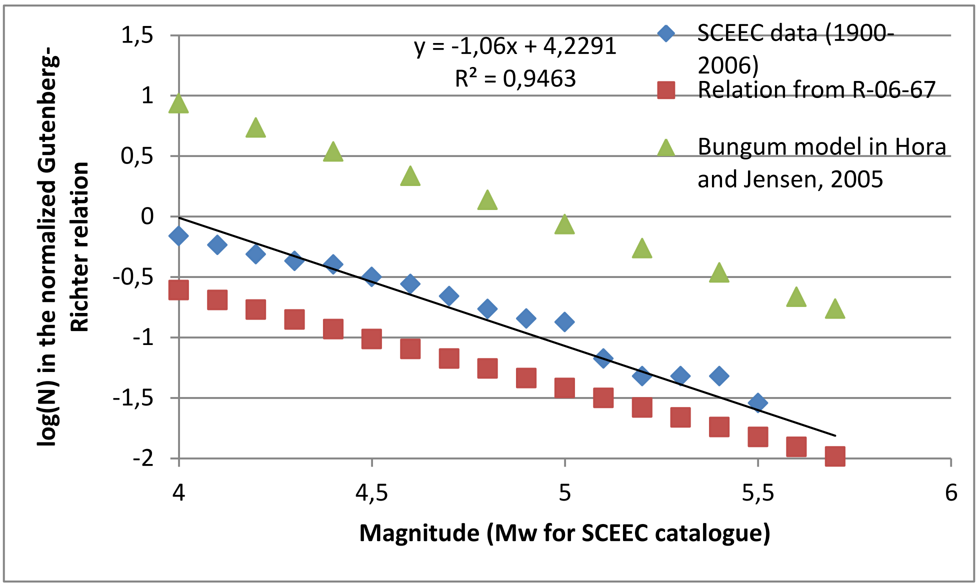

Fig. 4.1. Data from the SCEEC catalogue covering an area within 650 km radius from Forsmark were used to compute the blue dots (Mw) with a regression line indicating log(N) = 4.2291 – 1.06M. The red dots show the relation developed for the same area with the FENCAT data and the achieved R-06-67 relation of log(N) = 2.63 – 0.81M.

In view of the inhomogeneous spatial earthquake distribution the chapter on “Expected distances to large earthquakes” is not really contributing much.

The Bödvarsson et al. (2006, R-06-67) report contains an important qualitative discussion on seismic versus aseismic deformation of the crust in Scandinavia, based on a fairly speculative but still interesting model by Slunga (1991). It seems that this discussion is not leading to quantitative estimates, which is recommended to be done in the SKB analysis of deformation along and outside known faults. We return to this in our discussion of the main report (SKB TR-11-01).

Specific comments

The report is a fairly simple analysis of the present day seismicity with extrapolations and scaling to the Forsmark site.

- The use of a heterogeneous catalogue (many sources), lacking a clear discussion of potential explosion contamination, completeness, magnitude conversions and aftershock removal represent an obvious problem for extracting reliable recurrence parameters.
- The lack of a quantitative discussion or even an attempt to define maximum magnitude (Mmax) is an obvious problem of clear practical importance.
- The earthquake distribution in Scandinavia is recognized as spatially inhomogeneous. Nevertheless a 650 km-radius region of inhomogeneous seismicity is used to quantify the seismicity at Forsmark through spatial downscaling. This is not scientifically justified, even if it may possibly be regarded as conservative.
- The report is based on contemporary seismicity (short catalogues). While this is the purpose of the report, it seems that later use of the Bödvarsson et al. (2006, R-06-67) results do not clearly distinguish between postglacial seismicity bursts and the quantified contemporary seismicity.
- For a permanent nuclear repository it is strongly recommended to conduct a comprehensive and in-depth study of earthquake hazard (a PSHA) covering both postglacial earthquake burst activity and present-day tectonic conditions, including estimation of frequencies as well as maximum magnitude and based on well analysed earthquake catalogues. This is recommended not only because the present seismic loading (‘frequency’) estimation is weak but also because a separate PSHA study is a necessary safety measure for such projects.

5 SKB TR-08-11: Effects of large earthquakes (June, 2010)

This investigation is an advancement and deepening of the modelling conducted in the Munier & Hökmark (2004, SKB R-04-17) respect distance report. The scenario that cause most of the concern is a postglacial large reverse faulting earthquake (M>5) taking place on a mapped fault and triggering slip on a neighbouring fracture that may damage the canister integrity. With the attention on this scenario a number of other possible scenarios have implicitly been put to the side. While this approach is understood and accepted it is still important to keep this in mind.

Our comments in the following are largely limited to the sections that we have been specifically asked to concentrate on [1, 3, 4.4, 5, 6.1-6.5, 7.1, 7.2 and 8.2-8.3], even if we have been reading other sections as well, and also other SKB reports, when this was needed to understand the setting.

Section 1: Introduction and background

It is recognized that earthquake activity in Sweden is generally low, and that the activity at the Forsmark site is lower than the average. The new Swedish National Seismic Network (SNSN) was installed more than ten years ago and was a great monitoring improvement (supported by SKB). It is expected that the regional earthquake mapping of Forsmark and surroundings has improved gradually during this period and that the monitoring could reveal interesting seismicity information (or lack of). It is therefore somewhat surprising to see that the only report and/or paper coming out of this major effort has been quarterly reports and the old report by Bödvarsson et al. (2006, SKB R-06-67), based on a fairly simple assessment (see our comments to this report). If there are more recent reports covering the Forsmark regional earthquake activity both SKB and SSM should have seen these.

With background in the large postglacial faults in northern Fennoscandia the attention is set on postglacial earthquakes and many of the results are based on the Lund et al. (2009, SKB TR-09-15) glacial modelling results (which therefore play an important indirect role). Lagerbäck & Sundh (2008) is quoted on stating that only the last (Weichselian) glaciation is likely to have produced large earthquakes and that these most likely did not occur in southern Sweden. This is an important background for the modelling. A discussion on how the stresses leading to large earthquakes are generated in the last phase of the glaciation is largely similar to the Munier & Hökmark (2004, SKB R-04-17) study.

Tangential stress generation due to heat is recognized, but has apparently been analysed and found to be “kept at bay” through confining pressure around canisters (Lönnqvist et al., 2010, SKB R-10-36). Similarly, that study shows that the repository will not be likely to act as a plane of weakness as a whole, and it is concluded that the only seismic risk relevant to the repository is that of fracture shear displacements across deposition holes.

It is also stated that the shear velocity is very important (reflecting on bentonite properties). First order, second order and third order displacements are discussed in a logical structure leading to the “respect distance” concept, however, in this case more detailed and more refined than in the Munier & Hökmark (2004, SKB R-04-17) report.

We largely agree that direct shear through the deposition hole is a damage scenario that may be caused by an earthquake (directly or indirectly). We also support the increased strictness in the application of respect distances.

Specific comments:

- Page 12-13: The causes for postglacial seismicity are, presumably, largely building on models from the Lund et al. (SKB TR-09-15) report. Three possible causes of post-glacial seismicity are discussed, and later we find that it is the first of them (the tectonic model) that is the most important one, combined with a critically important assumption on the strain rate, which has a several orders of magnitude uncertainty.
- Page 14: The direct and unquestioned use of numbers and extrapolations from the Bödvarsson et al. (2006, SKB R-07-67) report is questionable; see also comments elsewhere on that report. Note that the probabilities from this report are valid only under the assumption of a Poissonian seismicity distribution (present day seismicity, not different tectonic regimes).
- Page 15: Also in this report it is stated that shaking will not have any impact on the integrity of the buffer-canister system. In Bäckbom & Munier (2002, SKB TR-02-24) it is stated that such damage will not appear (before tunnel closing) for shaking below 0.2 g, and that expected accelerations in Sweden will be at least 10 times below this level, based on the GSHAP project (Wahlström & Grünthal, 2000). These results are outdated and moreover estimated for a 10% exceedance probability in 50 years, which is the same safety level as now required by the Eurocode 8 for normal housing. We strongly recommend that the present seismic hazard level used should be based on a new high-quality site specific earthquake hazard study.
- Page 18: The Bödvarsson et al. (2006, SKB R-07-67) study is referenced for suggesting a typical M 5 earthquake in Sweden to have a peak displacement of about 0.5 m and a rupture area a little less than 1 km^2, and it is assumed that shallow depth earthquakes would also be well approximated by this. These numbers are not consistent with Figure 1-6, and if this is a critical assumption, a much better basis for it should be developed.
- Page 18-19: As noted elsewhere a major update of Wells & Coppersmith (1994) has been done by Leonard (BSSA, 2010), which may or may not change the scaling relations adopted in this study. As a minimum this should be checked. For example, it is stated that the regressions regard surface displacements, which is not really correct since Wells & Coppersmith’s Figure 2 actually shows the relation between surface and subsurface displacements.
- Page 19: The concluding sentence that "*potential earthquake faults can be concluded to be either too small to produce displacement larger than the damage threshold or too large to elude detection during construction*" should be reconsidered, given that

seismology is (also) a field within which "the exception is the rule" (Houston, 1992). Since that paper was written more than 20 years ago the earthquake 'exceptions' have continued to show up.

- Page 20: It is stated here that secondary movements would occur without any time delay as soon as the seismic waves arrive. Many examples show that this is not necessarily the case, even if the time delay should be expected to be much shorter for dynamic triggering than for static triggering.

Section 3: Reference seismic events

Other SKB reports highlight that a large postglacial earthquake is the most likely damaging scenario earthquake (with frequent references to the Pärvie fault rupture(s) from the end of the last glaciation). The target scenario is therefore to model primary and secondary effects of such an earthquake. While this scenario is the most important target, very little constraining data from this event (or events) are available. Instead, the well-documented M_w 7.6 1999 Chi-Chi (Taiwan) earthquake is used for comparison and calibration of input model parameters.

It is quite possible that the 1999 Chi-Chi earthquake is a good choice in this case, but what is missing here is nevertheless a discussion of alternatives and also which of the selection criteria (magnitude, mode of faulting, slip velocities and data coverage) that are most important. A useful overview of events and studies (albeit not recently updated) is available at http://www.seismo.ethz.ch/static/srcmod/Events.html.

Missing from this section is also a discussion of the potential level of conservatism implied in the modelling when based on the Chi-Chi earthquake, which after all shows fairly high ground-motions and slip velocities. It is notable in this respect that the number of observed 'extreme' ground motions has increased considerably since 1999 (e.g., Strasser & Bommer, 2009a,b).

Section 4: Modelling approach

This section is essential in that is comprises the selection of scenarios used in the modelling, where there however does not seem to be any clear justification of the selected magnitudes (5.5, 6.2 and 7.5). It would have been useful to have a discussion here of the way in which these scenarios have been selected and what kind of philosophies that have been applied in this process. If this is done in other reports (such as Fälth & Hökmark, 2007, SKB 06-89) it should still be repeated here. For example, does the selection process reflect any particular approach to the level of conservatism?

The modelling investigation was conducted with the 3DEC software, one of several programs available for this type of investigations. The present reviewers do not have first-hand experience in FE modelling and we therefore refrain from commenting on the software selection. The dynamic rupture modelling is conducted radially from a hypocentre. A key parameter is the "stress reduction time" (*rt*) which has a significant bearing on the later secondary fracture activation. For technical reasons the model setup is different for M=7.5 than for the smaller events. We trust this has been adequately covered. By and large we support the approach used, where the dip-slip main event in a postglacial scenario provides the overall setting. The problem statement is clear and rational and highlight induced ruptures on secondary fractures.

Specific comments:

- In this report there does not seem to be any justification for the selection of scenario magnitudes (5.5, 6.2, 7.5), which is important since this should be tied to the earthquake potentials, from the postglacial seismicity as well as from the present seismicity (where a hazard analysis and thereby also a magnitude assessment is missing). Specifically we question if Mw=7.5 is sufficiently conservative for another Weichselian type glaciation.
- The causative fault is modelled as a single plane without thickness (page 36, etc.). In an application where the rupture effect on secondary fractures is a target we are not sure what the possible effects of this simplification could be and if this therefore is an appropriate approximation (see also Papageorgiou, 2003 on this issue). In the far field it is more likely to be acceptable, but in the near field more complex fault approximation models would be more adequate.
- The three initial stress models for the Mw 7.5 model are very different and even if it is briefly stated on page 54 how they are developed it would have been useful to have a more thorough justification, especially it is stated on page 79 that "*the initial stress field appears to have a significant influence*" (cf. Figure 5-20).

Section 5: Modelling results

The modelling is using the 3DEC software (from Itasca), and the first part is a modelling of three magnitude earthquakes (5.5, 6.2 and 7.5) in which influence of a number of parameters are studied. An important issue is the derivation of slip velocities from displacement histories along with models of stress change, deformation patterns and ground motion at varying distances. The second part of the modelling is concerned with induced secondary slip on fractures at various distances with varying parameterization.

The modelling is conducted in a multi-parameter space. The description of the different tests and parameters is clear, but synthesizing the results of this section is difficult (partly because on the model naming). Slip velocity, which is later shown to be very important for secondary fracture shear, is derived from the slip histories through time derivative of the displacement. The strength reduction time (*rt*; the time needed for shear strength to be zero) is input and directly reflects in slip velocity. Target fracture stress stability analysis shows convincingly how secondary fractures can be brought to slip. It is also demonstrated that stress oscillations is a contributing factor.

Specific comments:

- It would be useful for Table 5-1 (page 60) to be introduced by referring back to the Section 4 where they are developed (see our comment on that section). It could also have been useful to have less cryptic names for the models.
- In Section 5.1 (page 59-64) it seems that the Chi-Chi slip velocity has been used primarily as a reference or calibration, such as for Figure 5-5, where a value of 4.5 m/s is used. As stated on page 33, the Ma et al. (2003) slip velocities (see their Figure 3B) are peaking more in the range 3-4.5 m/s, so it could have been more useful to use that range in Figure 5.5 and not only the extreme value of 4.5 m/s. If so, the match for the Mw 6.2 earthquake would be even better.
- In Figures 5-7 and 5-8 (page 66-67) the units neither for the colour scales nor for the vectors seem to be given. The same is the case for the legend in Figure 5-9 even though in that case it is clear from the figure that the unit is MPa. Also, it would have been useful if horizontal axes had been included so that one could judge the distance effects.

Section 6: Relevance and validity

Section 5 is very interesting and also important since it contains the main modelling results, based on the models defined in Section 4. However, the section is hard to read since it is a long sequence of modelling results for different combinations of assumptions, making it difficult to judge the relevance of the different results. This is where Section 6 comes in, and we understand that the authors have preferred to present this overview in a different section instead of integrating it with Section 5, which also could have been done.

Section 6.1.1 (stress drop): It is an aim of this section to compare with results from other studies, and it is therefore somewhat disappointing to see that the only earthquake used in addition to 1999 Chi-Chi is the 1992 Landers earthquake. It is stated (page 97) that "stress drop is difficult to measure", which is correct of course, except that we would have preferred to say that "stress drop cannot be measured" (only indirectly inferred). For that reason it would be useful to have some more (but carefully selected) examples, which would have shown a much greater range than at present, also in fact complying better with the 3DEC modelling results. To bring in Lansjärv and Pärvie stress drops into this discussion is not really contributing much given the simple assumptions that those estimates are based on, irrespective of their relevance otherwise. Also, as stated earlier, a range of peak slip velocity values (like 3 to 4.5 m/s) would have been better than the single (extreme) peak value in Figure 6-2.

Section 6.1.2 (slip velocities): Similar comments apply for slip velocities in Section 6.1.2; again only Landers is brought in in addition to Chi-Chi. In a situation where empirical calibration is essential this is not really sufficient, also when balanced against the very large efforts that have gone into the modelling work.

Section 6.3 (fault displacement, rupture area): This section would, as stated also elsewhere, have benefitted from an update (particularly by use of Leonard, 2010) of the 20-year old Wells & Coppersmith (1994) model, also including a specific discussion of the most interesting and relevant earthquakes in those data bases. This would have the potentials of actually strengthening the modelling results.

Section 6.4 (ground velocities): In the case of ground velocities there are even stronger reasons for a wider comparison with empirical data, partly because these are measured values and partly because they are in abundance, such as from the data bases used in deriving empirical ground-motion prediction equations (GMPEs). One common observation now is that the range of (spatial) variation of ground motions for a particular earthquake increases with sampling density (as demonstrated by Chi-Chi), and it is therefore necessary to consider the whole distribution and not only the peak values (e.g., Strasser & Bommer, 2009a,b).

Section 6.5 (seismic attenuation): Even if empirical GMPE models are fairly robust it would be reasonable in this case to expect a better update of such models, introduced for comparisons, in particular since the great breakthrough on such models came with the NGA (Next Generation Attenuation) models in the US (http://peer.berkeley.edu/ngawest), now including also an (ongoing) intraplate eastern US project (http://peer.berkeley.edu/ngaeast). Some of these studies also include footwall/hanging wall effects which could have been compared directly to the modelling results.

Section 6.13.2 (conservatism): We reiterate that it is a notable limitation in the present study that its empirical calibration is limited only to one, albeit well-recorded, earthquake. It is a fact that high-quality data have significantly increased in availability over the recent years, and this should have been better exploited in the present study.

Section 6.13.3 (confidence): Numerical modelling is known to be challenging not the least because of the problems with combining parameters where the joint probabilities may be difficult to assess, making empirical calibration essential. The effort in this section to assess the level of conservatism more systematically is positive, but not particularly successful, stating that most of the parameters are on the conservative side. How conservative the models become when these parameters are combined is admittedly a very difficult question, but as stated earlier one could get a long way towards a resolution of this if a better comparison with empirical data had been conducted.

Finally, the shear strength reduction time (*rt*), shown for example to be important for slip velocity, is discussed repeatedly in Section 5, but hardly mentioned in Section 6. When considering the importance of this parameter this is a problem, and again a better connection to other studies could have been made.

Section 7: Recommendations

Section 7.1 (interpretation model): This summary of a variety of modelling results is quite good especially since it also includes an overall assessment of uncertainties. It is intuitively difficult to understand, however, how and why the spread of the 'schematic representation' of the results in Figure 7-1 (lower) shows a smaller spread (between 200 and 600 m) for M_w 7.5 than for M_w 5.0. The methodology used in deriving the idealised linear distributions should also be better explained, not the least since these are the ones that are used in the subsequent analyses in Section 7.2. It would also be useful to have some uncertainty estimates in those figures.

Section 7.1: A percentage ratio relation for the critical fracture radius is proposed. Critical radii are determined for faults 3-5 km (small) and from faults greater than 5 km (large). A scheme for avoiding displacement exceeding 50 mm (or other) based on fault size, rupture dip and rupture size is created. It is presumed that this new relation modifies the “rules” specified on page 20 (100 and 200 meter distance for 75 and 150 radii).

Section 7.2 (critical fracture radius): This is an important section since it summarizes the modelling results, but for an outside reader it is not easy to grasp the essentials since the text is so limited, consisting mainly of references to the figures (which however are very instructive). A better and more extensive discussion here would be very useful.

Section 7.3 (site application): As shown by Lund et al. (2009, TR-09-15) the stability of the faults is critically dependent on several modelling assumptions, including the combined stress effects of glacial and tectonic processes. While Lund et al. (2009, TR-09-15) demonstrated the very different effects of a reverse and a strike-slip background tectonic model, which has been approached in the present case through a so-called mixed stress model in which the stress field is reverse down to 2.4 km (said to be arbitrarily selected) and strike-slip below that depth. Figures 7-14 and 7-15 illustrate the sensitivity to this assumption, and even if the mixed regime gives many more potentially unstable zones we find that this uncertainty, which also includes stress directions, should have been better covered.

Specific comments:

- One of the important results is that the tectonic stress accumulation following an earthquake is so slow that another event at the same fault zone is not likely to occur until after another glaciation (mentioned also elsewhere). In that case, how is cumulative slip on fractures by reactivation through several glaciations handled?

Section **8: Conclusions and discussion**

This section is clear and instructive, summarizing the results and the way they are applied in a consistent way. In the discussion it would be useful, however, to go back to the sensitive dependence of the results on the deformation zones, which in turn are relying also on the quality of the mapping of those zones. It is important always to keep in mind that (also) in seismology, the variability generally increases with access to more data.

Specific comments:

- Horizontal and gently dipping fractures slip more. Could such fractures potentially be affected (growing) due to the drilling of the deposition tunnels?
- Near field calibration of PGA and PGV is claimed, but not documented. There is now a wealth of data freely available since 1999 that also include near-field, in addition to Chi-Chi. Christchurch 2011 is one example. There are few references to near field shaking and related effects.

6 SKB TR-10-48: Geosphere process report (Nov., 2010)

This report summarizes results from many previous reports and puts the results in a practical context. The report parts that the present reviewers have covered in particular are Sections 4.1.1-3 and 4.3.

The geotectonic descriptions of the Forsmark location follow available knowledge and depict a generally very old and stable part of the Fennoscandian shield. Deformations around Forsmark are old, well mapped and regionally the deformation is caused by two processes: plate-related stresses (ridge push) and isostatic rebound after the last glaciation.

A review of the regional earthquake history is adequately made and the postglacial faulting related to future glaciations is identified as a main safety concern in the earthquake context. It is also stated that earthquakes “overwhelmingly frequently” occur on pre-existing faults, and rarely break virgin rock volumes. This is common knowledge, but there are exceptions.

The arguments in this section are leaning heavily on the SKB reports by Muir Wood (1993, 1995). These were important contributions when they came and they are still interesting, but they are at times quite speculative, they are not peer reviewed and they are about 20 years old. A better balance with newer publications would have been both desirable and useful.

Specific comments:

- Section 4.1.2, page 86: It is stated here (5th paragraph) that the current stress field in the south and central parts of the Baltic Shield show clearly the influence of ridge-push. While also the present reviewers support this (also in many publications), it could also be admitted that this conclusion is based essentially on a similarity between theoretical and observed (mostly from earthquakes) compressive stress directions and that there are other sources of stress that would be oriented largely the same way (e.g., Fejerskov and Lindholm, 2000), such as some of those relating to sedimentary loading and to lateral density differences across the continental margin. At any particular place the relatively weak ridge push effect could easily be significantly modified by other sources of stress, thereby possibly changing the σ_H direction (Hicks et al., 2000).
- Section 4.1.3 (earthquakes), page 86-87: It is noted that the ridge push is a main source for stress generation and it is estimated that a strain rate of 10^{-10}/year would lead to a stress change of 0.5 MPa over 100 ky, given an elasticity of 50 GPa. This stress may be released at the time of deglaciation. It should be noted here that computation of stress generation from very low strain rates over such long time periods is likely to require a visco-elastic crustal model and not only elastic as used in the present simplification. Moreover, strain rates in such regions are uncertain by several orders of magnitude and with large expected regional variations. If this strain rate can be used also for northern Scandinavia then it should be tested against the repeated earthquakes with M>7.5 during the last deglaciation. These comments have also relevance for the main report (SKB TR-11-01, page 468-469) where similar arguments on strain rates has been used to estimate the occurrence of only two M≥5

earthquakes over a 500 ky period. How can this be reconciled with the high activity rate during the last deglaciation?

- Section 4.1.3 (earthquakes), page 87-88: This review is reasonable well balanced, but it still warrants a few comments: (1) The "ridge push" force is again considered to be the only viable tectonic source of stress in Scandinavia, ignoring more regional and local sources (e.g., see Olesen et al., 2012); (2) It is stated unequivocally that "*the reason why postglacial fault displacements were concentrated within a small (Lapland) region is that the glaciation had a longer and unbroken duration here, so that larger amounts of energy could accumulate*". Since this has long been considered to be an open question in the peer reviewed literature it needs to be better supported; (3) The references used here are not particularly well updated and also mostly comprising SKB reports; (4) Even so, we fully support the conclusion that the possibility of future postglacial earthquakes near the repository cannot be excluded.
- Section 4.3.5, page 101: As noted earlier, Wells & Coppersmith (1994) should now be replaced by Leonard (BSSA, 2010), unless specific reasons are given for keeping the old relations.
- Section 4.3.5 (analogues), page 101: We are quite surprised by the statement that "*Similar observations [as Landsjärv and Pärvie] have been made for normal faults in Hanöbukten and in the North Sea, interpreted from marine seismic investigations (see Wannäs and Flodén 1994, Muir Wood 1995)*". That would have been quite sensational and we cannot see that such a statement can be supported by the 1994 report, which is based on data from low resolution offshore reflection profiling. This should be resolved, including what is meant by "similar".
- Section 4.3.5 (analogues), page 101: It appears as if the last sentence on this page is a conclusion on earthquake scaling from the project; as is well known, this is a fundamental earthquake scaling relation supported by a wealth of theoretical and empirical studies. Also, Figure 4.5 is from Wells & Coppersmith (1994; Figure 12a) and not from La Pointe et al. (1997), who only have redrawn it.
- Section 4.3.7 (earthquakes), page 104: Paragraph 3 here contains the calculation commented on also for the main report (SKB TR-11-01, page 466), in which the contemporary seismicity assessed by Bödvarsson et al. (2006, SKB R-06-67) for a 650 km radius area is scaled down to a 5 km radius and distributed equally on 30 zones. The uniform distribution over 650 km (which is conservative) is scaled down and distributed over the 30 zones (which is underconservative). This is not well justified. A model where all of the normalized activity within 5 km is distributed on only the potentially instable zones (such as between one and 5) instead of all 30 appears to be a more justifiable model. We refer back to these comments later, reiterating also that we cannot see how these numbers can apply also to a future postglacial period (which we agree could be assumed similar to Weichsel).
- Section 4.3.7 (earthquakes), page 104: The frequency of earthquake occurrence from Bödvarsson et al. (2006, SKB R-06-67) is reduced by 50% with the argument that earthquakes are normally deep, and that the Forsmark area will only be affected by

more shallow earthquakes. This reasoning may be correct for smaller earthquakes but not necessarily for the large magnitudes that are most important for the present project.

- Section 4.3.7 (earthquakes), page 104-105: The main problem with the arguments leading up to Table 4.3 is again the fact that it is based on the Bödvarsson et al. (2006, SKB R-06-67) recurrence relation from contemporary seismicity. It should be noted that the main problem with using a memory-free (Poisson) model is that the burst of postglacial (Weichselian) seismicity cannot be modelled like that. It may be better to use a linear extension of the average recurrence (equation 4-1), provided of course that the average includes the large postglacial earthquakes.
- Section 4.3.7 (earthquakes), page 104: As repeatedly noted the use of the recurrence values from Bödvarsson et al. (2006, SKB R-06-67) is not well justified and supported. The 36 local faults (reduced to 30 as potentially unstable and further reduced to 5 within the repository area) are used as host faults for which recurrence parameters are estimated. The values reported by Bödvarsson et al. (2006, SKB R-06-67) are in this section directly used for deriving the occurrence of larger earthquakes for different time periods. In addition to the uncritical use of the Bödvarsson et al. (2006, SKB R-06-67) values we note the following:
    - How dependent are the selected faults on small changes in the regional stress field? It seems possible that small changes in the regional stress field (say 10-30 degrees on σ_H) could possibly change the stability of the faults. This regional stress field is influenced among other factors by glaciation models for which a number of possibilities exist. Lund et al. (2009, SKB TR-09-15) discuss two Weichselian ice sheet models (Lambeck et al., 1998 and Näslund et al., 2006) and chose one model (Näslund) in their modelling. There exists also other models (e.g. Slagstad et al. 2009), and we question to which extent regional and local stress fields should have been modelled from a suite of alternative ice models.
    - The simple reduction of potential from the original 36 faults to the final 5 and dividing the activity equally is not well justified and seems non-conservative.
- Section 4.3.7 (earthquakes), page 105-108: The arguments in this section relating to the long term (say 500,000 years) probabilities depend on a number of assumptions, and it should in particular be better elaborated how the results depend on the effective strain rates, which are uncertain for two reasons, the large scale (background) tectonic strain rate is very uncertain, and so is the way that the effects of it is reduced through aseismic slip, which we have commented on elsewhere. An improvement of these arguments would also benefit from including ranges, reflecting the uncertainty, and showing the sensitivities.
- Section 4.3.8 (uncertainties), page 108-109: The uncertainties covered in this section are concerned with the mechanical modelling and not with the underlying driving forces from earthquakes, which is based on Bödvarsson et al. (2006, SKB R-06-67)

and which is much simpler and less well justified than the rest of the analysis (see several earlier comments on this).

We have above provided a number of potentially important comments to the Bödvarsson et al. (2006, SKB R-06-67) report which need to be resolved. This is particularly important since the results from this report are further used as a basis for some of the risk assessments.

7 SKB TR-11-01, Main report, SR-Site (March, 2011)

This is an extensive report where the whole SR-Site project is integrated and summarized and where one of the aims has been to make the report self-contained while at the same time referring extensively back to the many individual reports. This is challenging since extensive cross-referencing will often impair the readability and understanding, and in this sense the overview of results on page 39-49 is very useful, summarizing the results in an ordinary language and without any cross-references at all. As an executive summary this is commendable.

We also recognize the efforts to provide an understandable overview of the project, such as in Section S3 (page 23-39) where the 11 main steps are summarized, and then in more detail in Section 2.5 where the same steps are discussed in more detail, including the overview flow chart in Figure 2-2. This flow chart is mainly organizational, however; for an external reviewer one of the main challenges is to get an overview of the *interaction* between the different components (the "principal references"), which would call for a different flow chart aiming at tracing the way decisions and conclusions are made. The chronology of the different studies is of less importance in this sense.

Throughout the report, the unconventional word "pessimistic" is extensively used instead of "conservative", which in comparison is well-established and with a clear understanding of what it implies. There is even a case of "pessimistic data". Similarly, the word "cautiously" is also used in cases where "conservative" would be more appropriate.

On the same level we have noticed also an extensive use of the word "hypothetical", which is implicit in work of the present kind where one is aiming at predicting events in the future. The problem here is that "hypothetical" is often used in a diminutional sense, which hopefully is not the intent of the authors.

Specific comments:

- Section 7.3 (process documentation), page 222, where it is stated that "*Canister failures and earthquakes of a magnitude that could affect the deposition hole or tunnel geometry are not expected during the several thousands of years after deposition when temperate conditions are likely to prevail*". This statement disregards the earthquake potentials and the expected maximum magnitudes within stable continental regions at present, which a high-level seismic hazard study (as recommended above) would help to disclose and discuss.
- Section 10.2.2 (induced seismicity), page 297, lines 4-5, states that "…*seismic events that could impair the integrity of the already deposited canisters, would require an induced earthquake of approximately magnitude 5*". This is a statement that presumably is based on an assessment of inferred displacements less than 5 cm for M<5. The uncertainty here is appreciable and therefore should be better discussed (including sensitivity to rigidity).

- Section 10.2.2 (induced seismicity), page 297, lines 7-9, states that "*Furthermore, there is no evidence that present-day deviatoric stresses in Swedish bedrock at repository depth are sufficient to power seismic events of magnitude 5*". This is again a disregard of the seismic hazard levels at present, which have not been investigated in this project.
- Section 10.4.5 (Probability of future large earthquakes), page 466: The exponential Gutenberg-Richter relation is fine at first order, but with one important qualification; it needs to be a truncated distribution against a maximum magnitude. For yearly frequencies of contemporary seismicity this is no problem, but if seismicity is predicted on longer time scales it becomes imperative to consider maximum magnitude, as derived from a full scale PSHA study. Furthermore, if the time scale is so long that a Poisson distribution no longer can be assumed (due to changes in the underlying stress field) the extended GR relation is inadequate.
- Section 10.4.5, page 466: In Table 10-14 there are comparisons between different studies, including Bödvarsson et al. (2006, SKB R-06-67) which covers contemporary seismicity and Hora & Jensen (2005) which addresses the seismicity following next glaciation, surprisingly with quite similar results. The implication of this is that the postglacial seismicity is comparable with the present seismicity in Sweden. By checking Bungum's Appendix 5 in Hora & Jensen (2005) we find that he concludes with 260 M≥5 events over 2-3000 (say 2500) years and 400x400 km, assuming a maximum magnitude of 8.2 (for Mmax 7.6 the rate is 560 for M≥5). This corresponds to a rate for an area of 5 km radius, which is about 20 times higher than what is reported in Table 10-14 for Bödvarsson et al. (2006, SKB R-06-67) and Hora & Jensen (2005). Values similar to those used in Table 10-14 would be obtained if assuming that the 2500-year average is representative for the entire 100,000 years interval, thereby assuming that there is no additional seismic moment added during the 97,500 years outside of the documented postglacial burst of seismicity. We find this very problematic to accept and we are calling for a further investigation and a better justification. Further to this point we note also that the rate from Bödvarsson et al. (2006, SKB R-06-67), which reflects 100 years of contemporary seismicity, is also taken as being representative for the entire glacial cycle. We return to this also below.
- Another issue for Table 10-14 is, as mentioned earlier also, that the seismicity is distributed equally on the 30 identified deformation zones in the Forsmark region, in spite of the fact that it is shown in Section 4.4 (and also in TR-08-11) these zones are quite different in nature, ranging from brittle to ductile and from unstable to stable. In Stephens et al. (SKB R-07-45) a larger number of zones are delineated, and it is not even clear how the 30 zones are selected. This needs to be better documented, including the important assumption that future earthquakes will be limited to the 30 zones and that the seismicity is distributed uniformly on these even if only a small number of them are considered potentially active (see earlier comments on this).
- Section 10.4.5, page 468: Under point G a magnitude-frequency relation from Bödvarsson et al. (2006, SKB R-06-67), developed based on about 100 years of data,

is claimed to be representative for the average over a glacial cycle, characterized as a fundamental assumption, and it is even claimed that this can be reliably extrapolated to longer time frames. One estimate derived from Bödvarsson et al. (2006, SKB R-06-67) is that 40 and 6 earthquakes over the glacial cycle will occur for M≥7 and 8, respectively, for all of Sweden. There is, however, no discussion of the large temporal variations in seismicity over the glacial cycle, and similarly there is no discussion of the importance of the maximum magnitude, which also should be expected to vary significantly over the same time period.

- Section 10.4.5, page 468: Further down on the same page it is referred to an assessment by Slunga (1991) that aseismic slip in Sweden is overwhelmingly (~20,000 times) larger than seismic slip. This issue is important since under point B on page 467 it is stated that only seismic and not aseismic slip is considered to be a potential problem for the canisters. There it is, however, also stated that the aseismic/seismic slip ratio at Forsmark indeed is an open issue, which we support. Slunga's (1991) arguments are interesting, but it appears that the implied effective strain rates from that study have not been used.
- Section 10.4.5, page 468-469: As just mentioned, Bödvarsson et al. (2006, SKB R-06-67) find a seismicity level which is extrapolated to 6 M=8 earthquakes over a full glacial cycle, for all of Sweden. In this section it is also found, however, based on strain rate arguments, that it will take about 500,000 years to accumulate sufficient stress for two M≥5 earthquakes (i.e. five glacial cycles of Weichselian lengths). These two very different rates seem difficult to reconcile and should therefore be better elaborated on and explained. Also, the tectonic strain rate is uncertain by orders of magnitude, and with an uncertain stress relaxation assumption the two events per million years becomes highly uncertain. As noted earlier this estimation seems to be based on fully elastic crustal conditions, which is very uncertain and not recommended by some stress modellers (S. Buiter, pers. comm.). A visco-elastic crustal model would be more appropriate.
- Further to Section 10.4.5, page 468-469: Assuming E=64 GPa, strain rate=10^{-10} /year and $\Delta\sigma$=3 MPa, a simple calculation indicates a 500,000 year loading time for each M≥5 earthquake. Following contact with a leading strain modeller (Corné Kreemer, University of Nevada, pers. comm.) an alternative model was indicated as possible for Sweden: strain rate=$2*10^{-9}$/year, 20 km crustal thickness, shear modulus=30 GPa. From this model a return period of one M3.8/yr, one M5.8/kyr or one M6.5/10kyr would be indicated for a 800x300 km area. This alternative model is not claimed to be superior or more credible in any way; the point here is only that it demonstrates how uncertain the estimates are.
- Further to Section 10.4.5, page 468-469: The simple loading/unloading model depending solely on shear strain and used as a basis for assessing return periods has been challenged by R. Sibson (1995, 1991), related to changes in mean stress and fluid migration during the loading cycle. One should therefore be careful with basing design decisions on estimates as uncertain as those developed in this section of the report.

- Section 10.4.5, page 470: In Table 10-15 Wells & Coppersmith (1994) Table 2A has been used, but without specifying which relation that has been used. From the numbers it appears, however, that it is the one for surface rupture length (SRL) and all slip types, which gives magnitudes of 6.8 and 7.2 for rupture 30 and 70 km, respectively. This choice of relation should be explained and justified, since there is some sensitivity involved. Furthermore, the Wells & Coppersmith (1994) paper has now largely been replaced by Leonard (2010) who used a larger data set as well as theoretical constraints, and who specifically addresses SCR (stable continental region) conditions, like Sweden. The choice of relation will clearly affect the magnitudes.
- Section 10.4.5, page 470: In Table 10-15 the magnitude is derived directly from the fault dimension, with no consideration of possible segmentation and/or more generic Mmax assessments. While this approach is not uncommon (albeit not advisable) in normal tectonic situations there are many more open issues when applied to postglacial situations like in the present case.
- Further to Section 10.4.5, page 471-472: Figs. 10-117 and 10-118 indicate stable and unstable faults around the repository volume. As indicated by Sibson (1991) and in the discussion on sensitivity to σ_1 in Section 2 the sensitivity to changes in stress directions and fault geometry at greater depths can be much larger than what is indicated in these figures.
- Section 10.4.5 (Cases of shear load to consider), page 474: Growth of fractures and cumulative slips are considered here (Fig. 10-120), which is positive. This, however, is in contrast to the rigid fault interpretation used for Table 10-15, where the mapping of the faults controls the earthquakes completely. If fractures can grow, then faults can also grow. If faults can grow (under certain conditions) and if rupture can jump across mapped faults then this Table 10-15, and respect distances, needs to be reassessed.
- Section 10.4.5 (Number of canisters in critical positions), page 477: It is stated here that the reverse stress regime affects only one deformation zone (ZFMA2). There are, however, four other low-dip faults distinct only by strike. The important decision to select one and only one fault here needs to be better justified and possibly also reconsidered. For one thing, small changes in the presumed (glacio-tectonic) stress direction could change significantly also the fault stabilities, and we would recommend investigating how small (10-30 degree) changes in the glacio-tectonic stress direction could influence shear stress on the mapped faults. See also further comment below.
- Section 10.4.5 (Number of potentially failed canisters by shear load), page 477-480: It is concluded here that on average between $8.3 \cdot 10^{-4}$ and $5.7 \cdot 10^{-3}$ canisters may fail during a glacial cycle. For the 1,000,000 year time frame, two seismic events are assumed and it is estimated that on average between $8.3 \cdot 10^{-3}$ and $7.9 \cdot 10^{-2}$ canisters may fail. These results are hinging on how the uncertainties (min-max range) are estimated, including the Bödvarsson et al. (2006, SKB R-06-67) earthquake frequencies, the 50% frequency reduction developed in the Geosphere report (2010, TR-10-48), a glaciation cycle of 120,000 years, two M=5 earthquakes within one

million years, and the fairly rigid causative fault model. Also, which relaxation time (T) is used at page 480?

- To the same issue: With respect to the canister risk assessment we are questioning here the use of earthquake frequencies as an average over a full glacial cycle; is it unconservative to use a linear average when this incorporates seismically very active periods interchanged with almost complete quiescence?
- Section 10.4.5 (Number of canisters in critical positions), page 477-479: It is referred here to how the fault stability depends on the background stress model (reverse or mixed) but it is not mentioned that this comes from the modelling in Fälth et al. (SKB TR-08-11) and in Lund et al. (2009, SKB 09-15). In the calculation of number of canisters one comes down here to only one zone, ZFMA2, not including any of those that are unstably in a mixed stress regime but not in a reverse regime. We are not convinced that this is sufficiently conservative, given the sensitivity to the different modelling components, particularly the combined stress regime and its direction, where also the nature of the ice sheet and the pore pressure is important (e.g., Lagerbäck and Sundh, 2008).
- Section 15.6.1 (Further characterisation of the deformation zones with potential to generate large earthquakes), page 834: The Site description Forsmark report (SKB TR-08-05) is cited on the conclusion that only a few deformation zones exist with the potential to host larger earthquakes and that even fewer intersect the repository volume in such a way that respect distances need to be considered. The report therefore recommends that a more detailed investigation programme should concentrate on determining the extent of the damage zone for these few deformation zones, and identifying and characterising splays from these zones. Has a plan for this been worked out?

  The approach in this report reflects a somewhat static view of a dynamic situation, assuming that earthquake ruptures can only occur within the delineated zones and not elsewhere. This is problematic for two reasons: (1) Large earthquakes often disregard existing fault zones and may jump between several, breaking also more pristine rocks; (2) Given the very long time scales for this repository the approach does not properly discuss that deformation zones and faults can develop over time, in particular in response to changing stress fields, which could be expected here in connection with glacio-isostatic adjustments.

8 Recommendations on topics to be further investigated

The present review report comprises a large number of specific comments and recommendations that could and should be studied and considered within the project. The following list of recommendations is therefore not complete, containing only some of the points that are of principal importance.

- The simplest recommendation to implement is to update calculations that presently are based on Wells & Coppersmith (1994) with the relations from Leonard (2010). This will in most cases not lead to significant changes, but a nearly 25 year old reference is not appropriate to be used when a 3 year old reference exists (based on more data).
- A crucial part of the study is the derivation and identification of potentially instable fault zones in the vicinity of the repository. Given the importance of this we recommend to investigate more closely how alternative assumptions on glaciation (ice sheet) models and background stress models may influence the resultant stress field and the resulting fault stabilities, thereby possibly leading to the triggering of earthquakes on faults other than those expected in the present study.
- Considerations about level of conservatism are present almost everywhere in a study like this, and maintaining consistency here is not easy. Even so it is recommended to attempt developing a more transparent propagation of uncertainties and from initial model parameters until end estimation of probability for damaged canisters.
- A major problem with this study is the lack of a more rigorous seismic hazard analysis, where at present only a very simple and questionable analysis has been used as a basis for the seismic loading assessments that enters the subsequent risk assessments for the canisters. In lights of this, and also because high-quality tools are available, we recommend also to conduct a high level (preferably SSHAC based) PSHA study with the aim to
    - Develop a source zones characterization model to be used for predicting future seismic activity in the vicinity of the repository
    - Develop a ground-motion characterization model to be used in the computation of seismic hazard
    - Estimate Mmax under present tectonic conditions and revisit the Mmax and recurrence relations expected under deglaciation periods (including quantified uncertainties).
    - Estimate earthquake shaking hazard during the filling period (before closing).

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10 Appendix 1: Coverage of SKB reports

This appendix is supposed to list the reviewed reports, the reviewed sections and our comments. Since our main review report is organized report by report, with references to sections and page numbers, we find that this fully covers what was intended for Appendix 1. In addition, the report has a Summary, a section with General Comments, a section on Recommendations, and a comprehensive list of References.

11 Appendix 2: Suggested needs for complementary information

This is a list of suggested questions to SKB requiring clarifications, complementary information, complementary data, etc. All of the points are covered in more detail in the main NORSAR report.

- One of our main technical problems with the study is that the seismic loading levels as applied in the risk assessment for the canisters are based on an average over one glacial cycle (on the order of 100ka). Given that this is an average between a very high seismic activity level (during the deglaciation phase) and much more quiet periods it seems to be unconservative to use such an average. The principle of this approach is not justified.
- We have a major issue with the loading (seismicity) levels used, which seem to be lower than what can be justified. The main frequency estimate comes from an average of 100 years of seismicity in central and southern Sweden, a number which moreover is claimed to properly represent the average over a full glacial cycle. Also, the derived magnitude-frequency relation is extrapolated linearly over about three orders of magnitude (from 4 to 7) without any independent consideration of truncation as resulting from a maximum magnitude assessment. While a seismic hazard analysis could have provided all of this, it is not explained or discussed why a very unpretentious seismicity assessment has been used instead.
- The seismicity used in the risk analysis is linearly scaled down to a small (5 km-radius) area around the repository, which may be conservative. This activity is distributed linearly on 30 zones where most of them are removed later on the grounds of stability, which is strongly unconservative. The way this reduction is done, and its justification, is not sufficiently well discussed and documented. In fact, it would be more justifiable to distribute the full activity on only the potentially instable zones (say between 1 and 5). This accounts for an underestimation by a factor of six.
- The seismicity level used, based on contemporary data, is extrapolated linearly four orders of magnitude to include 6 M=8 earthquakes over a full glacial cycle, for all of Sweden. It is also found, however, based on strain rate arguments, that it will take about 500,000 years to accumulate sufficient stress for two M≥5 earthquakes (i.e. five glacial cycles of Weichselian lengths). These two very different rates seem difficult to reconcile and should therefore be better elaborated on and explained.
- Numerical modelling of fault motions depend always on a proper calibration with empirical data, such as for stress drop, slip velocities and ground motions, and in this case essentially only the Chi-Chi earthquake is used. More empirical data should have been brought in here. Such calibration is particularly important since the causative fault is modelled as a single plane without thickness. In an application where the rupture effect on secondary fractures is a target it would be useful to discuss what the possible effects of this simplification could be.

- Considerations about level of conservativeness are present almost everywhere in a study like this, and maintaining consistency here is not easy. Even so it is recommended to attempt developing a more transparent propagation of uncertainties and from initial model parameters until end estimation of probability for damaged canisters.
- The calculations that presently are based on Wells & Coppersmith (1994) should be checked against the newer relations of Leonard (2010).

Further points are listed in Appendix 3.

12 Appendix 3: Suggested review topics for SSM

This is a list of suggested topics requiring substantial additional work on the part of SSM and SSM's external experts during the main review phase. All of the points are covered in detail in our main report. We consider these points to require new investigations, presumably under the responsibilities of SKB.

- The potentially active fault zones within and around the repository are those, and only those, that are already mapped geologically, potentially supplemented by further studies during the excavation phase. Maximum magnitudes are derived solely from the dimensions of the structures. What is needed is a better consideration of the dynamics of earthquake faulting, including breaking of new fractures, growth of faults over (geologic) time, and potential inferences from a seismic hazard analysis (see below).
- The assessment of potentially instable fault zones hinges on a number of assumptions, including the ice sheet model, the Earth model, the pore pressure model and the background state of stress. While this is discussed in underlying studies the associated uncertainties are not well reflected in the final assessment. Alternative models, in particular for the interplay between stress directions and fault geometries, could therefore have complemented the current models for the fault stability assessments. Given the importance of this we recommend to investigate more closely how alternative assumptions on glaciation (ice sheet) models and background stress models may influence the resultant stress field and the resulting fault stabilities, thereby possibly leading to the triggering of earthquakes on faults other than those expected in the present study. This would also contribute to a better assessment of modelling uncertainties.
- A major problem with this sequence of SKB investigations is the lack of a more rigorous seismic hazard analysis, where at present only a very simple and questionable analysis has been used as a basis for the seismic loading assessments that enters the subsequent risk assessments for the canisters. In lights of this, and also because high-quality tools are available, we recommend also to conduct a high level (preferably SSHAC based) PSHA study with the aim to
    - Develop a source zones characterization model to be used for predicting future seismic activity in the vicinity of the repository
    - Develop a ground-motion characterization model to be used in the computation of seismic hazard
    - Estimate Mmax under present tectonic conditions and revisit the Mmax and recurrence relations expected under deglaciation periods (including quantified uncertainties).
    - Estimate earthquake shaking hazard during the filling period (before closing).

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The Swedish Radiation Safety Authority works proactively and preventively to protect people and the environment from the harmful effects of radiation, now and in the future. The Authority issues regulations and supervises compliance, while also supporting research, providing training and information, and issuing advice. Often, activities involving radiation require licences issued by the Authority. The Swedish Radiation Safety Authority maintains emergency preparedness around the clock with the aim of limiting the aftermath of radiation accidents and the unintentional spreading of radioactive substances. The Authority participates in international co-operation in order to promote radiation safety and finances projects aiming to raise the level of radiation safety in certain Eastern European countries.

The Authority reports to the Ministry of the Environment and has around 270 employees with competencies in the fields of engineering, natural and behavioural sciences, law, economics and communications. We have received quality, environmental and working environment certification.

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