Document: Catastrophic Wildfire in the CEZ, December 11, 2010 Review Copy

Comment number and (line number)	COMMENTS
1	Line 59: ……. a chain reaction……. is not a good description of this accident. A rapid accidental power excursion is more appropriate.
2	Lines 50, 80: …..most likely over-estimates…….: This statement needs clarifying if the overly conservative claim is valid. If all the assumptions are clearly listed and reasons why all (or most) can be claimed conservative then the claim may be made, the paper does not currently do this.
3	Line 60: There are many literature estimates of this activity release; the one quoted is not the most definitive reference although it comes within the best estimate range. Quoting 3 significant figures precision is not realistic (IAEA best estimate error factors are about a factor of 2). The number should state either noble gases are included or not and, more useful for the reader, would be the fraction and quantity of the total fuel released.
4	Line 81: Sv / a is not a dose measure but a dose rate.
5	Line 74-76: Doses for ground based and air-borne fire-fighters and people living in the CEZ are not addressed, as well as the public at distances < 25 km. This is a major omission in the reviewer's opinion and should logically be part of the scope. There is no rationale provided for excluding these dose recipients, particularly as the report states specifically 'critical' groups (but only at > 25 km) are dealt with. There is no evidence provided that > 25 km represents critical dose recipients. In view of the high profile of the original Chernobyl fire-fighter tragedy, potential doses to this group in particular should be addressed.
6	Line 79: The evidence for extreme conservatism is not clearly outlined in the report. Only by listing all model main assumptions and for each assumption clearly explaining why each assumption is conservative can this be justified. Ideally some simple error factor analysis might be provided by assigning individual error factors for each assumption. It is also important that dose estimates not be misinterpreted by being associated with an unrealistic degree of precision. All final summary dose/dose rates in the report are stated with a degree of precision that is not realistic.
7	Line 101: Terminology: 'two most common isotopes' is not helpful to appreciate the dose importance. Would be more useful to

	state the most dominant isotopes for dose considerations, accounting for the activity and dose conversion factor significance.
8	Line 111, 113, 114: 'abstractions of reality' and parsimonious' would be better phrased in clearer English. 'Not fully understood' would be better stated as……. cannot be accurately modelled.
9	Lines 165/335: The validity/accuracy of the Gaussian plume model at 25 km and certainly at 100 km is extremely uncertain. The application of this model is particularly questionable for a forest fire dispersion, where by its nature the release will be far from a point source and, most importantly, is subject to large plume rise due to thermal effects. The literature discusses numerous coupled forest fire emission and dispersion models specifically designed for this scenario and these more applicable models should be discussed. It may well be that due to the thermal rise of the plume that the Gaussian model will be conservative under all conditions considered here and that the GP model is justifiable, but this is not discussed in the report. The coupled fire emission and dispersion models also usually present discussions on, and experimental data for emission factors, and associated soil depth for releases of activity.
10	Line 225: It is not clear what 'periodic' refers to. Line 229: the quote of 5 d appears to be the duration of the fire? This should be more clearly stated in the section on inhalation and reference back to line 160. The unit for R_{inh} is then incorrect; it should be in m^3 not (m^3/a) as quoted. Line 223 unit is incorrect as this should then be in Sv not (Sv/a).
11	Line 229: The definitive reference sources for adults and children for breathing rate (ICRP) should be stated and used.
12	Lines 232, 233: The summary definition of the GPM as radiation emitted being in radiative equilibrium and the note on energy absorption is not a very common, nor useful description, of such a model.
13	Line 237: The 'highest risk group possible' is certainly not beginning at 25 km. Instantiation does not seem to be a suitable description here. The example quoted of the building effect here is not stated clearly, if it means a shielding effect due to buildings, of the surrounding active plume, then that is only one of many conservative factors that the plume model may provide. If it means the building wake correction factor for dose recipients outside a building, then that particular correction factor is of little consequence or importance at 25 km and beyond. The conclusion of line 442 that doses in the CEZ will exceed those in the report leaves the reader wondering why this was out of scope.

14	Line 178 -180: As the reference to infinite plane may be confused with the more commonly semi-infinite plume model for dispersion a source reference should be given.
15	Line 261: Typo in Sv/e?
16	Title: A worst case scenario: The report does not appear to provide any justification for a so-called worst case scenario, as the scope has been limited (lines 73-76). Additionally, the use of so-called worst case terminology is usually avoided in safety analyses as there is no such scenario can ever be defined (hence beyond design basis and other terms are used). Whatever worst case the authors can define I can guarantee that I can always define something worse.
17	Line 349: It is not clear why the predicted dose at 100 km is being quoted in the summary with no mention of the 25 km summary dose.
18	Line 349: 100 km: Table 8 does not quote directly the 4.7×10^{-4} Sv/a, the reader is left to derive this from the table. There is no column separating out ingestion from the sum of the other types of dose. It is not clear why only 100 km is quoted in this results summary, as this is not the highest dose. Furthermore, line 343 indicates doses, whereas the values in Table 8 are all dose rates. Also 'exposure' as stated in line 349 is not a dose rate (Sv/a), so the terminology is confusing. The use of a dose rate Sv/a in Table 8 for immersion and inhalation does not appear logical or practical for these types of exposure. One would expect just a dose in Sv to be provided as result of a short term fire, this would be at most a few days (5 days is assumed but if a dose is calculated the actual burn time is not needed to be defined if the exposure time exceeds the burn time. After the plume has passed the immersion and inhalation dose types then becomes zero, ingestion and ground exposure (rates) may continue for a year or more, so that a dose rate is logical for these.
19	Line 385: The numbers quoted are not doses but dose rates. The reader is left to work out these numbers from Table 8. If it is important to combine the contribution as mentioned in a summary then Table 8 should also state these numbers as well. The 2.4 mSv/a for adults and the 1.7 mSv/a for infants quoted for 25 km also do not appear to be correct, when derived from Table 8?
20	Line 390: Some overall uncertainty estimate should be provide in an Appendix by listing all assumptions with estimated error factors; showing all or most assumptions to be conservative would then justify then the statement of 'worst case'. Without such details 'worst case' cannot be justified.
21	Line 397: …..could exceed contaminated levels…….The doses are not clearly summarized here, the statement 'up to 150 km' should surely say at all distances greater than 25 km and the dose quoted be summarized and Table 8 referenced?
22	Line 382: It is not clear where the isotope contamination limits come from.

23	Line 403: It is not clear where the references are to the acceptable activity levels of the isotopes quoted.
24	Line 437: Summarizing the possible release (quoted to a 2 figure precision which cannot be justified by literature sources) does not provide a useful conclusion for the reader. While the total release is of interest there is no clear reference in the text where this number came from. It appears to be the sum of the combustible material in 2010 of mainly Cs^{137} and Sr^{90} of Table 1 for which no totals are provided.
25	Line 439: A conclusion should be provided of the measure of how much below the threshold is predicted. Quoting dose / year, as noted in previous comments for these pathways for 1 year when inhalation and immersion will be only a very limited time is confusing.
26	Line 441: A conclusion summary should be provided with a measure of by how much the ingestion level threshold is exceeded i.e. how long would the residence time be, in order to reach the dose limit, to provide the reader with a practical limit.
27	Line 462: Table 2: the specific location of the reference in IAEA 201 would be helpful. The units stated for inhalation and ingestion appear to be incorrect? The dose coefficients for $Pu^{239 / 240}$ are not always the same, as quoted?
28	Line 274/275: units / e is not clear? Line 274; The units indicate that $E_{ing,p}$ is a dose rate, not a dose.
29	Line 305: Some of the contributors to E_{tot} are dose rates some doses and thus cannot be logically added and line 306 references only a dose.
30	Footnote 1 and elsewhere: Terminology ‘stock’ is very unusual for an activity measure; suggest use inventory or amount.
31	Table 1: Emission factors are generally used to define the emission from a ground concentration. This factor is where a clear conservatism can be used but it is not clearly mentioned. Table 1 appears to use some sort of emission factor but is not well explained.
32	Footnote 6: Should mention plume rise from thermal effect to explain ‘why one would expect’ a non-ground level release height.
34	Table 8: Dose precision, for the totals in particular, is not justified.