Further response to Andreo's letter "On the p_{dis} correction factor for cylindrical chambers"

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In addition to our letter published in PMB(Wang and Rogers 2010), this document is a further set of comments in response to Pedro Andreo's Letter to the Editor of PMB (Andreo 2010) concerning our earlier paper(Wang and Rogers 2009),

1. We stand by dating TRS-398 as being published in 2001 despite the fact that the cover says 2000. The IAEA web site shows the publication date as 31 May, 2001 (go to http://www-pub.iaea.org/MTCD/publications/publications.asp and click on the Technical Report Series)

2. The methods we use to calculate $P_{\rm repl}$ do not correspond to the "Gedankenexperiment" reported by Andreo et al in 1991. In fact, that paper reported a failed calculation not a 'Gedankenexperiment". More importantly, that paper made use of what we referred to, in our original paper, as the indirect SPR method of calculating $P_{\rm repl}$ (Wang and Rogers 2008) whereas our main contribution has been to devise more direct methods of calculating $P_{\rm repl}$ and in this case we clearly stated we were using the LDW (low-density water) method.

3. Andreo suggests we were unaware of the need for detailed models of ion chambers. However, as we say in our letter, (a) we have published papers with detailed models of ion chambers (which Andreo doesn't refer to, thereby suggesting we don't know how to do it); (b) we have demonstrated that a detailed model did not affect the calculations at hand; and (c) by definition, the $P_{\rm repl}$ factor is defined for just a bare air cavity. Thus, this is red-herring argument.

4. Andreo states that the data of Johansson et al were "the only set of experimental data available for decades", whereas the data used by the AAPM, viz that of Cunningham and Sontag are also at least partially based on experiments, albeit also wrongly interpreted and used in conjunction with calculations to extend the coverage.

5. Andreo quotes part of TRS-398 to imply that the changes in calculated k_Q values implied by our new values of $P_{\rm repl}$ don't really matter since TRS-398 prefers using measured values (of $N_{D,w}$) for the chamber. This is misleading since virtually no-one using TRS-398 does anything except use the calculated values. Most of the major countries which can provide direct calibrations in accelerator beams (eg. Germany, UK, Canada, Switzerland) use either their own national protocol or the AAPM's TG-51 protocol (the exception is France).

6. The criticism for using the colloquial "18 MV" as a beam quality specifier overlooks the fact that we clearly identify that beam as having a value of $\%dd(10)_x$ of 82% in the figure.

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7. Andreo's criticism for using a cavity length of 20 mm is correct as far as it goes, but he should have pointed out that in Table 3 of our first paper on the subject of $P_{\rm repl}$, we established that there is only a very small effect of cavity length. Furthermore, to the extent that there are any differences, they would have made the discrepancy with TRS-398 even larger since $P_{\rm repl}$ may go up slightly for the longer cavities.

8. Andreo emphasizes that the fit we provided does not yield a value of 1.0 for a zero chamber radius, a point which he grants we also made in our paper. He makes an issue of this, implicitly suggesting the calculations are incorrect but he fails to mention that we verified our codes are working properly by doing the calculation for Fano conditions and demonstrating a unity value within an average of 0.06%. This suggests that the 0.21(7)% difference from unity for the smallest radius studied (0.5 mm) may be real and indicate a non-linear approach to the unity value. On the other hand, the data can be fit with a straight line through unity and all calculated $P_{\rm repl}$ values are on the line within the 0.2% systematic uncertainty we assigned to our calculated values.

9. Andreo makes the point that these new values don't reduce the uncertainty in calculated k_Q values significantly. While we agree this may be correct, it ignores the issue of now having the more correct values of k_Q, by anywhere from 0.13% to 0.5%. Surely this was a worthwhile goal, especially as TRS-398 states that the uncertainty in this factor represents the largest uncertainty in the calculation of k_Q.

10. Andreo makes an issue about the Co-60 spectrum that we used, again implicitly suggesting that there is considerable uncertainty in our results. While we agree that we have not explicitly tested the sensitivity to the spectrum in this case, there are two publications from our group which have explicitly discussed this issue and compared the results for realistic spectra, like those of Mora *et al* (1999), to results for either a 1.25 MeV spectrum or a bare Co-60 spectrum (2 lines). La Russa and Rogers (2009) showed a 0.02% effect from these extreme cases (their fig 10) and Rogers and Kawrakow (2003) showed variations of less than 0.1% on similar corrections (their fig 5 and 6). If the variations for these very extreme cases are so small, the expected uncertainty from the variation in various realistic Co-60 spectra is presumably negligible. Another indication of the small effect expected is the fact that standards labs do not normally account (at least directly) for such differences in Co-60 beam comparisons. Furthermore, neither TRS-398 nor TG-51 requires any further beam quality specification other than specifying that the beam is ^{60}Co. This means that there is little sensitivity to the details of the ^{60}Co spectrum in different laboratories or clinics.

To reiterate the final statement in our published letter: "We do agree with Andreo that our new values of $P_{\rm repl}$, despite being substantially different from the values used in TRS-398, will have little effect on the final dose values produced using TRS-398 for photon beams, except for the highest-energy beams, and figure 5 in our paper made exactly that point."

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References

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