

Review of ESSD-2024-509, Southern Hemisphere radionuclides.

Accepting authors reasoning and accounts of FRN sources, and agreeing that current knowledge focuses more on northern than southern hemisphere, I agree with AVATAR objectives: to understand “fallout chronology and distribution of ^{137}Cs and $^{239+240}\text{Pu}$ in the Southern Hemisphere for various environmental applications”. Does this database provide unique high-quality source information about these isotopes so that researchers can all ‘start from the same page’ in assessing southern hemisphere distributions? Should researchers trust this database to ask further questions, beyond those hinted at here? Have authors missed key information? What other links, e.g. to climate, topographic, geographic, precipitation type or quantity records, might one need, or wish for? Have authors produced a notable outcome, suitable for publication in ESSD?

“Expected”. “Assumed”. Readers would like to follow these authors but, absent clear uncertainty guidelines, we must remain suspicious. AVATAR database represents potentially the best state-of-the-art database of SH FRN, but authors have yet to convince this reader!

- 1) That we do not, no longer have, or - indeed - never had complete accurate records of tests or yields seems unfortunately not surprising. Thanks due to authors for recording and acknowledging these gaps.
- 2) The literature search seems appropriate but - perhaps - limited. Did authors consider other terms besides or in addition to ‘soil’? ‘Sediments’? ‘Particulates’? ‘Fall-out’? ‘Precipitation’? One notes and understands extensive search on isotope terms but wonders about restricted (?) search only on ‘soil’. Particularly since soil depth (‘profile’) and clear evidence of non-disturbance seem key? One presumes that deposition (direct or via particle run-off) into lake sediments might then offer suitable non-disturbed records? Or does ‘run-off’ itself represent a disqualifying erosive process? One understands desire to exclude flooding or draining, but particle run-off from soil into lake seems natural and benign. By choosing reference ‘soil’ exclusively, have authors missed key isotope residence sites probably experiencing relatively low disturbance? Would one expect differences in assumptions based only on soil profiles versus assumptions derived from soil profiles plus lake sediment profiles? The entire ‘dry’ vs ‘wet’ vs ‘flooded’ vs ‘drained’ categorization seems artificial but perhaps necessary? Later (line 469, line 634) authors also mention lake sediments as possible rectifiers or references for these data? Question: do lake sediment data exist in sufficient numbers, reliability and accessibility to impact initial conclusions presented here?
- 3) For calculating delay-induced decomposition of ^{137}Cs during publication process, one hopes that four years represents a maximum rather than mean time. In any case, reflecting relatively short $1/2$ life (fast decomposition) of ^{137}Cs , doesn’t uncertainty in publication time, e.g. 3.5 vs 4.5 years, induce subsequent uncertainties in overall ^{137}Cs concentrations?
- 4) The entire latitude discussion seems - at best - mismanaged here, for several reasons. First, at these latitudes, southern hemisphere surface areas represent primarily ocean. Precipitation happens, in some areas abundantly, with substantial spatial and temporal heterogeneity, over southern hemisphere oceans. How, even using best soil profile data, can authors draw conclusions about southern hemisphere depositions or distributions by only monitoring a fraction, at some latitudes a very small fraction, of geographic surface area? Second, by their own analyses, longitude proved a more important determinant than latitude? In all cases precipitation (wet deposition) proved a determinate factor, followed by longitude; in only a few cases (for ^{137}Cs) or in no cases (for $^{239+240}\text{Pu}$) did latitude play any statistically-important role? Third, again by authors analyses, coastal sites proved much more important to this database than ‘interior’ sites. Because this definition (‘coastal’

vs 'interior') also varies greatly as a function of latitude, should authors have placed less (or, no) emphasis on latitude?

- 5) This reader remains very cautious about two-factor unmixing calculations. I credit the authors for a plausible attempt but: a) readers will eventually learn, if we have not already, that reporting of British and French (two 'local' factors) tests remain largely unknown (lines around 190); b) that documented British and French tests represent less than 30% of reported NWT contributions (line 546, 547); and c) British and French NWT occurred in distinctly different (by longitude, closeness to ITCZ, vegetation, etc.) locations. A rigorous comparison might involve NWT by two different countries from a single location? No longer possible, today, of course, but authors can predict (large) uncertainties?
- 6) Eventually, reader will learn that only 123 of 1526 (<10%) publications discussed outcomes that qualified for inclusion in this database. Good on authors for maintaining high standards! But, RF analyses covered these 123 publications (1122 total profiles) or the full unqualified set? RF analyses, even with 5 20% subsets run 5 times for 22 variables, in no way approaches 10000 RF runs? Or, if run for all 1100+ profile data, greatly exceeds 10k? Clearly, authors have not helped this reader understand their factor assignment process. WorldClim 2.1 only provides data on monthly averages so it will have 'smeared' precip records. Most ESSD readers avoid WorldClim data because of suspected terrestrial biases.
- 7) This reader offers no better alternatives but never-the-less remains very skeptical of analysis via publication records. We know publication records themselves retain substantial biases from construction and exercise. We know that Google Scholar today produces different outcomes than Google Scholar of 2021. Editors and editorial standards also change. We all know that one good 'paper' outweighs 100 weaker papers in terms of care, description, documentation, etc. We also know that publication standards for NWT or FRN data will have changed over time, from initial exploratory reports to subsequent more careful or more thoughtful analysis. The authors acknowledge such changes with their discussions of 'sequences' of "early" vs "late" papers and with their intercomparisons with prior reports. Temporal uncertainty in publication quality plays a large but largely unacknowledged role?
- 8) Line around 320: As n gets very low (43, 7), precision to tenths of a percent (35.0, 5.7) seems more and more fanciful?
- 9) Line 364, Figure 4: Map proves dominance (rightly or wrongly) of northern hemisphere sites in any and all analyses. Do authors really want to show this? Have these authors done similar quality assurance, as described here, for all northern hemisphere data?
- 10) Line 379, Figure 5: Figure shows very large (impracticably large) uncertainties for most southern hemisphere data, particularly when 'sorted' by supposed latitudinal bands. Uncertainties as documented in this figure negate much of the discussion?
- 11) Line 384, Figure 6: Figure documents difference (or, absence of statistical differences) between AVATAR and UNSCEAR data or Hardy 1973 data, but - as for prior figure - latitudinal uncertainties prove disqualifying? This reader might agree with NH vs equatorial vs SH assessments but has not seen anything so far to convince of statistically-valid latitudinal differences within SH? All evidence seems to point in opposite direction: lack of any statistically-valid latitudinal differences within SH?
- 12) Readers would like to trust author descriptions of NWT sources. Isotopic discrimination seems an ideal tool in this regard. But, unfortunately, too many variables cloud the authors' conclusions: location, longitude, elevation, precipitation, temporal evolution of precip and, indeed, of NWT sources, etc. Authors could greatly assist this reader by starting from, and keeping in their minds and in minds of readers, substantial uncertainties! Too easy to focus on details of isotopic analysis while forgetting larger uncertainty factors? For this reader, early declaration of a summary uncertainty, e.g. $\pm 90\%$, $\pm 50\%$, whatever, honored by authors throughout manuscript, would represent a substantial improvement and assistance.
- 13) Line 580, Figure 9: Figure attempts to show prediction skill for areas not covered by measured soil profiles. Good effort. Good luck. This reader might assume some skill for

^{137}Cs , e.g. based on data plot plus particular but un-referenced (panel b) 'purity' factors, but finds no reason from this figure to base any predictions on $^{239}_{240}\text{Pu}$ (panels c, d) data.

- 14) Uncertainty discussion refers entirely to external (weak reporting) factors. Authors apparently assume their work, or their assembly work, introduced no additional uncertainty factors. Certainly not true, but perhaps uncertainty factors introduced here remain small compared to 'external' factors. Unfortunately, readers gain no information to buttress such conclusions?
- 15) Database easy to find, download, read, etc. Compliments to authors!