

Interactive comment on “Change of the Asian dust source region deduced from the relationship between anthropogenic radionuclides in surface soil and precipitation in Mongolia” by Y. Igarashi et al.

G. Li (Referee)

ligaoj@gmail.com

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This paper provides a novel approach to trace the source shift of Asian dust in recent years using the anthropogenic radionuclides. By demonstrating the theoretical prediction that the specific activities of ^{137}Cs , ^{90}Sr and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio of the surface soil is largely controlled by precipitation amount based on a case study in Mongolia, the increasing ^{137}Cs , ^{90}Sr activities and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio of dust deposition in Japan during the 2000s is interpreted to reflect a source shift to a relatively humid climate zone.

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Scientifically, the revised version of the paper is largely improved in the manner of logical flow. However, I still have several doubts:

1. I am still not convinced that the higher ^{137}Cs , ^{90}Sr activities and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio of the Taklimakan dust compared to the Taklimakan soil is simply caused by analytical error as listed in Table 1. The analytical error seems very small compared to the difference. Maybe grain size is the answer. The dust has finer grain size composition thus higher ^{137}Cs , ^{90}Sr activities and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio. This is probably also the case for the Japanese dust. The higher ^{137}Cs , ^{90}Sr activities and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio of the Japanese dust is probably simply caused by the preferential transportation of fine particles from Asian arid continent rather than a mixing between local and Asian dust endmembers.

2. I cannot see clear shift in compositions of anthropogenic nuclides between the 1990s dust and 2000s dust based monthly deposit as the author claimed that the 2000s' dust have Asian dust source endmember with higher ^{137}Cs activity. There is just 3 events have significant higher ^{137}Cs activity out of the dozen samples. Could this be captured by chance?

3. It is hard to believe that the wet dust events have significant contribution from local sources similar to the dry deposition based on the two endmember interpretation since the Asian dust source is expected to dominant during the dust event and local emission should be suppressed in wet condition.

4. As the ^{137}Cs and ^{90}Sr have half-lives of about 30 years, should the samples analysed in different time be calibrated when making comparison between each other? The error induced by the decay would not be very large during a course of decade or so but still may be significant. I cannot judge from the manuscript if all the samples are analysed in the same year and how would this effect the interpretation.

The writing of the revised version of the paper is largely improved in a more concise manner, especially for the part before the section 3.2 where intensive comments were

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made in my first review. However the section 3.2 and 3.3 may still need polishing as pointed out below as well as other minor points.

Page 2844

Line 15. Reaching conclusion before analysing? Suggest change 'should be' → 'may be'.

Page 2846

Line 6. Leading to misunderstanding, climate change in a fixed source region or changing source region with different climate condition.

Page 2851

Line 1-13, change the past tense of the verb to the present tense when necessary to describe the characteristics of the nature.

Line 1. Delete 'significant'.

Line 23-26. Delete 'This linear curvein the region'. It is quite obvious that ^{137}Cs and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio should correlate to each other since they are both controlled by precipitation as pointed out in line 20-23.

Line 26. Delete 'in Fig. 4'.

Line 26-27 and line 1-5 of page 2852. → 'The Taklimakan soil samples also exhibit a linear correlation between ^{137}Cs activity and $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio with similar slope to that of the Mongolian soil but higher intercept. Although the higher intercept of the Taklimakan soil is not fully understood, the similar slope of the ^{137}Cs - $^{137}\text{Cs}/^{90}\text{Sr}$ correlation between Taklimakan and Mongolia soil imply that the soil in the wide arid and semiarid areas of North China, Inner Mongolia and Mongolia may exert the same ^{137}Cs - $^{137}\text{Cs}/^{90}\text{Sr}$ correlation as to that of the Taklimakan and Mongolia soil.

Page 2852.

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Line 6. ‘The Taklimakan dust showed a higher $^{137}\text{Cs}/^{90}\text{Sr}$ ratio (4.0 ± 1.0), possibly because of the relatively large error’ → ‘The Taklimakan dust showed a higher $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio (4.0 ± 1.0) than the local soil, possibly because of the relatively large analytical(?) error.

Line 6-7. ‘The Tsukuba soil samples compose another group in the upper left corner of Fig. 4.’ → ‘The Tsukuba soil samples are characterized by low ^{137}Cs activity and high $^{137}\text{Cs}/^{90}\text{Sr}$ activity ratio, which is distinctive to that of the Asian continental soil.’

Line 17-19. Delete ‘The atmospheric depositions in the 2000s’ spring are created by mixing the Tsukuba and continental soils (likely to be two components from relatively dry and relatively humid zones).’

Line 19. Should be four out of the five single precipitation events during spring 2007 exhibited relatively high ^{137}Cs specific activity? See the Fig. 4.

Line 21. ‘could be interpreted to mean’ → ‘could reflect’

Line 24. What’s the ‘nature’?

Page 2853

Line 2. ‘in which’ → ‘when’

Page 2854

Line 1. I doubt there are particles up to several hundred micrometres in the dust deposition.

Line 26. ‘to the additional sources’ → ‘the adjacent source regions’?

Page 2857

Line 2-3. Delete ‘for which conventional isotope tracers would be of no use.’ Conventional isotope tracers still could stand if different climatic zone is coincidentally matches the geologic zone.

Line 15. The appendix has not been mentioned in any place of the main text. It is not relevant to the major argument of this paper. So, I would suggest to delete the appendix.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2843, 2011.

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