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***Interactive comment on* “Evaluation of anthropogenic emissions of carbon monoxide in East Asia derived from observations of atmospheric radon-222 over the Western North Pacific” by A. Wada et al.**

Anonymous Referee #1

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Anthropogenic emissions of carbon monoxide and other trace species have grown tremendously in eastern Asia during the past decades. For researchers who establish emissions databases for modeling purposes this growth is hard to keep up with, and often the primary information about polluting activities and emission factors is incomplete. Therefore, the development of alternative activities and methods to estimate the emissions, as offered by Wada et al., is very welcome. The present study uses measurements of CO and Rn-222, the latter being a tracer of continental air, to locate the CO emissions from the East Asian continent. The measurements were performed

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on three islands at different locations in the western Pacific, and the results are accompanied with a tagged-tracer study with an atmospheric transport model. This work and methodologies used are appropriate for ACP, and the results are interesting. I support publication of this work in ACP though have some important reservations which could be met in a revised version of the manuscript. Although the English language use is generally good, the manuscript would benefit from corrections by a native speaker.

My main concern is the treatment (or better neglect) of biomass burning emissions, which are known to be an important CO source worldwide. For example, table 1 presents the results of a number of studies, of which some do and others do not include biomass burning, which seems to have overall little effect on the estimated emissions, which range from 118 to 206 Tg/yr from East Asia. Wada et al. also assume that biomass burning emissions are of minor relevance. However, I find it hard to believe that at least in some seasons, e.g., due to wild fires during periods of dryness and agricultural waste burning, biomass burning emissions can be fully neglected. Therefore, I would like to see this assumption either corrected or substantiated by stronger arguments.

Specific comments:

1. p.15339, l.28/29 mentions that the radium-226 distribution and Rn-222 emissions are uniform/homogeneous. Although this assumption is reasonable, it is factually not correct. Please discuss this as a source of uncertainty in the applied method.
2. p.15340, l.19: replace “observed” by “reflected”
3. p.14341: Sampling sites. To what extent is RYO affected by emissions from Taiwan? Can these simply be neglected? It would be useful to characterize the sampling sites based on an air mass back-trajectory study. One could, for example, plot the origin of back-trajectories on a map for the three stations, color-coded for different transport periods (e.g., from 1 to 5 days).

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4. p.15342, l.25: please indicate what is meant by STAG
5. p.15343, l.17: biomass burning not included (see above general comment)
6. same page, l.19. Assuming that the contribution by NMHCs is constantly 40 ppbv is challenging. Why not relate this to the seasonal Spivakovski et al (2000) OH concentrations, as also done for CO oxidation?
7. p.15350, l.18-21: Based on the tracer study by Sawa et al. (2007) it is assumed that biomass burning is a negligible source of CO. This needs to be substantiated. For example, you could use the publicly available GFED biomass burning emissions database (based on satellite observations) to show to what extent these emissions can be neglected in East Asia.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15337, 2012.

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