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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Continental-scale enrichment of atmospheric $^{14}CO_2$ from the nuclear power industry: potential impact on the estimation of fossil fuel-derived CO_2" *by* H. D. Graven and N. Gruber

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Dear Heather, dear Nicolas and dear colleagues, The rising awareness of the importance of independent validation of emission inventories both in the emission modeling community as well as in the field of policy making calls for a good approach to independently assess anthropogenic fossil fuel CO2 emissions. In this framework, 14CO2 is usually regarded as an "ideal" tracer to disjoin fossil fuel CO2 from biogenic fluxes, but of course every tracer has its problems. Thus the topic and intend of the paper to investigate the problems of this approach due to emissions of 14CO2 from the nu-

clear power industry (NPI) is urgent and important. Unfortunately, this study falls short in some respects and the current focus of the results might be misleading for some people. This is why I decided to comment on this paper here to suggest necessary changes (from my point of view) and hopefully also start a discussion. The paper includes a concise compilation of anthropogenic 14CO2 sources and also applies a good methodology to derive the 14CO2 fluxes from nuclear power plants. The flaw that real emission data is not always available and thus has to be parameterized using energy production is a general problem that cannot be overcome here. I also second that to obtain a proper estimate of the uncertainty for FFCO2 excess estimates we will have to include spatially resolved fluxes of this type into future modeling studies. The authors note that they want to focus on the gradients on "large" scales of 100 to 10000 km (page 14586, line 9). Given the model resolution of 1.8°x1.8° it is questionable for me if meaningful result of the influence of nuclear power industry (NPI) emissions on the 14CO2 levels can be derived within the same (maybe even adjacent) grid cell of a large emitter can be derived. Therefore I think the lower limit of the focused scale of 100km has to be set significantly higher (e.g. 500km) and the focus of the discussion should be more on sites which are farther away from the sources. Of course it is worthwhile to see if the influence at specific sites: e.g. RYO can be significant, but it is not clear to me how much this result is affected by the coarse model resolution. The authors also rightfully mention that they cannot address "small-scale" gradients in the vicinity of emitters(page 14586, line 11). Yet, the very high possible influences found of b=260% or 22 permil 14CO2 (-8 ppm) which is the key result in the abstract and the discussion section seems to be in the immediate vicinity (within one grid cells) of the major emitters. Thus, this is also on a scale which might not be properly resolved by this approach. I would suggest to focus the discussion more on the average or median influence of NPI emissions for the continental gradients, which presumably lies more in the range of 20% (p14951, line 24) i.e. one order of magnitude smaller. Focusing on the extreme results might give a wrong impression for other readers. Another issue that could be addressed more clearly is the uncertainty of the actual D14C measure-

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ments, which in itself is highly uncertain. This would also put the findings into a better perspective. In the model there are indeed large areas where an influence of NPI emissions is visible, but will not significantly alter any measurement results. I am aware that the local gradient induced by NPI emissions is an offset, which should not be mixed with a statistical uncertainty which does not shift the mean if enough measurements are performed. Yet, the problem appears to be larger if the reader does not keep in mind that the typical uncertainty of current AMS techniques translates to a precision of about 1ppm fossil fuel CO2. This is mentioned in the paper, but should be more pronounced. Imagining the time-series in Figure 4, with error bars of this size it seems questionable that the influence of this correction actually affects the interpretation of any measurement results for CMA, SAB, LUT, SCH, RYO or GSN. I think it would have been worthwhile to choose sites with known and published D14C values (e.g. Levin et al. 2010 mentioned p14590, l14ff) and compare the results to evaluate how well this model actually performs. For me it seems to be the major weakness that the study lacks to display the performance of the model for some results it is focusing on e.g. the strong increase over the English Channel and over Eastern Canada. From our measurements in Ontario, Canada I can indeed support that this might be a "hot spot" as there is a noticeable impact from the local nuclear power plant emissions from the Bruce, Darlington and Pickering nuclear power plant on our measurements. As nicely explained in the paper this is due to the type of reactors used here in Canada. For our measurement design this was one reason to have our urban site and our regional site as close as possible. This is 100% in line with your point that for finer (urban) scales the choice of the background should always be in consideration of a possible local/regional influence of NPI emissions (page 14594, line 21ff). A very positive example is the comparison of the RYO results with Turnbull 2009 and I think this type of interpretation should be much more expanded, to display the difference (and improvement) achieved by including spatially resolved emissions. For interesting sites also a comparison with published results for this influence has to be done. E.g. page 14585, line 6ff mentions the correction used for the D14C time-series in Heidelberg, but lacks to give a result

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from this study and to compare them. Nevertheless, I support that the influence of nuclear power plants has to be taken into account for some regions and agree with the suggested hot spot regions. Nevertheless, this paper in my opinion needs a more extensive discussion and comparison of the results that will have to display the ability of the applied modeling framework. All in all this paper tackles an important issue in the framework of monitoring or understanding fossil fuel CO2 in the atmosphere. Falling short in some aspects it could be refocused according to the abilities of the model used to avoid misunderstanding as its current form might be misleading to some. Exactly as the authors I think that to obtain a better estimate of the influence of NPI emissions on atmospheric measurements on small scales a more sophisticated approach using temporally better resolved emissions and a high-resolution transport model will have to be used (p14594,l28).

Dear Heather, dear Nicolas, I hope that you find some of these comments to be helpful to improve the paper and it would be nice if this triggers further discussions. I hope that other people also see the value of this paper in raising awareness for this issue. As the problem you are tackling is in my opinion crucial for any monitoring network design and interpretation of campaign-based measurements of 14CO2 to derive fossil fuel CO2.

Best regards, Felix

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