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Interactive comment on "Global CO_2 uptake of cement in 1930–2019" *by* Rui Guo et al.

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In response to 'SUMMARY and MAIN IMPRESSIONS': Thank you for recognising the importance of this piece of research. Regarding the two issues you mentioned, on the former, we have presented the assumptions, data, statistical distributions and other information in the Monte Carlo simulations, run for calculating the uncertainties in the dataset at https://doi.org/10.5281/zenodo.4064803. On the latter, we will address your concerns in the following texts.

In response to 'MORE DETAILED COMMENTS': Abstract: Including the dataset DOI complies with ESSD guidelines. It is also already included in the references. Line 32: We are happy to update this figure in the revised manuscript. Just to clarify, we simply stressed the share of cement industry emission of the total industrial emission. Line 42: First, this is merely a background introduction of historically high clinker ra-

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Discussion paper

tios hence emission factors. As part of this work, we explicitly estimated the process emission based on the newest available data. Secondly, the GNR project is known to skew the clinker ratios to the lower end because of its limited coverage. Instead, we used databases of higher resolution and accuracy. Line 42: You are talking about the maximum amount of CO_2 that can be absorbed for Portland cement (mainly CEM I).In the manuscript, we referred to two important papers discussing the historically high emission factors of around 0.5 t CO_2/t cement. Line 45 (1), (2): Thank you for your advice here. We are aware that the actual carbonation mechanisms are more complicated than outlined in the manuscript, and will change the wording to something like 'the main mechanism' or simply add more equations. Nevertheless, the fact that the amount of CO_2 that can be taken up depends on the Ca content (active) won't change. Line 49: We don't think what you argued here is in contradiction to what we stated in the manuscript. The '...multi-giga-tonne potential of CO_2 abatement...' is partly owing to the low content of clinker due to high mixing. Line 50: We agree with you here. We should have and will stress that reducing clinker ratio is the key to lower the PROCESS emission level. Line 59: As I mentioned above (Line 42), we think the coverage of the GNR project is quite limited hence not representative of the reality. Line 61: This sounds like a good idea. We will add the relevant information in the revised manuscript. Line 140: We considered this parameter and its range explicitly in the Monte Carlo simulations. In the literature you referred to here, this ratio is however fixed at 65%. Line 147: When we say 'fully carbonated', we mean carbonated considering the degree carbonation e.g. clinker content, active CaO content etc.. 'Fully' here simply suggests that we don't tend to complicate our global-scale model by considering the dynamic evolution of carbonation, like partly-carbonated zone etc. However, we realise that 'fully' is a confusing term and will change the wording accordingly in the revised manuscript. Line 187: Although an assumption, here we applied the uniform distribution between a and b in terms of particle sizes not their carbonation behaviour. Line 395: Can you clarify your question here? As we understand it, the literature you referred to is only concerning Spain using a Tier 1 approach. Line 398: Yes. We will add the dataset at https://doi.org/10.5281/zenodo.4064803 as the reference as the ratios are listed there. Line 410: Sure. However, since we are only comparing the overlapping 1930-2017 (cumulative) and 2017 (yearly) process emission, either literature would suffice. Line 416: We could but we doing so seems redundant to me the reasoning being we have laid out the differences in the model in line 56,72,367 and 476 between these two studies. The results hence differ accordingly. Therefore, a discussion in this respect would only be comparing numbers with little insights, given the statistical nature of the estimation method. Line 417: We decided not to publish the code at this stage. The uncertainties are estimated using Monte Carlo methods with all the variables, ranges and distributions considered listed in the 'SI table-Variables considered in the uptake uncertainty analysis'. Line 429: This is provided in SI data 9 and SI data 14 tab in 'Input model parameters of cement carbon emission and uptake'. Line 240: Which line are you referring to? Line 440: Noted. They are clearly labelled in the schematics though. Line 447: This is addressed above (Line 395). Line 467: We decided not to publish the code at this stage. Line 474: This seems reasonable, we will do so in the revised manuscript. Line 475: We haven't seen other literature reporting such an index using different models (methods) at global scale. Line 477: Initially we intended to make such a comparison schematically, however, we don't have access to the yearly uptake data as reported in Xi et al. 2016 any more. Line 479: Can you explain the reason? Line 480: It is a good point to include the uncertainties in the conclusion sector, maybe the cumulative results only, given they were already explicitly stated in the preceding sections (Results) and in the SI tables. However, in our opinion, the Conclusion section mainly serve as a summary to catch the trends found in this study. Line 484: this is a proposal for ways to increase the accuracy and reliability of the estimates. Mass spectroscopy and nuclear magnetic resonance, among other experimental methods are useful in determining the conversion factor experimentally.

In response to 'RECOMMENDATION': We believe that the manuscript, together with the dataset provided at https://doi.org/10.5281/zenodo.4064803, is self-contained. We will work on the necessary data update and wording issues the Reviewer pointed out.

Regarding the uncertainty calculation bit, we decided not to publish the code at this stage. The relevant methodologies are in line with Xi et al. (2016) and should be referred to.

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