Replies to anonymous Referee #2

We wish to thank Referee #2 for her/his comments and suggestions for changes; we have revised the manuscript as follows (our answers are given in blue in the text below)

Kuderer et al. present an analysis of nuclear power plant influences on radiocarbon measurements in CO2 at Heidelberg using emissions data and the Hysplit model at three resolutions. Their main conclusions are that the nuclear correction decreased after the shutdown of Philippsburg BWR, the corrections they estimate are sensitive to model resolution, and nuclear corrections require careful consideration. The authors' work is useful and important to the community. However, some revisions are needed to clarify the details of their study and to expand the conclusions drawn from their results. The methods for model simulations are not very clear and there appear to be several different simulations used that are rather hard to follow.

A table describing the different simulations run for each nuclear site would be helpful.

We added a table (Tab. 2) with the respective information

Details about how the Hysplit runs were conducted, such as the number of particles and release times should be added.

The authors should also clarify that Hysplit was run in forward mode from the locations of the nuclear sites rather in backward mode from the observation site in Heidelberg. There appears to be some details described in the results section 3.2 that would fit better in the methods section.

The requested information is now added in the methods section 2.4 with some technical information moved here from the results section 3.2

The authors report in the abstract that "The mean correction for the period from 1986- 2014, if based on the 0.5◦ x 0.5◦ wind field, which we assume as the most accurate, is 2.3 ‰''. However, it appears high resolution 0.5◦ winds were only used in simulations for 2009 and 2011 – 2014, so it is not correct to say the 1986-2014 correction is from the 0.5◦ x 0.5◦ wind field. The other years were estimated from the coarser 2.5◦ resolution simulations with a correction factor based on comparisons for the years where 2.5◦ and 0.5◦ simulations were run for two reactors.

This is correct, and we changed the abstract as follows:

The finally applied mean $\Delta^{14}CO_2$ correction for the period from 1986-2014 is 2.3 ‰ with a standard deviation of 2.1 ‰ and maximum values up to 15.2 ‰. These results are based on the 0.5° x 0.5° wind field simulations in years when these fields were available (2009, 2011-2014) and, for the other years, they are based on 2.5° x 2.5° wind field simulations, corrected with a factor of 0.43.

Since Fig 4 shows the difference between simulated corrections at different resolution for individual samples is sometimes very small and sometimes very large (even with fixed emissions), is it valid to apply a mean correction to the data before 2009? Particularly if a main argument the authors are making is that the correction is highly variable in time? The authors argue that, since the correction is highly variable in time, monthly emissions data must be used and average emissions cannot be used, but then seem to contradict themselves by saying an average correction can be applied to account for model resolution, when actually this can be highly variable as well.

The referee is absolutely correct, however, we simply do not see an alternative possibility to correct for that obvious bias when using the coarse resolution wind field. We added in section 3.2 the expression … factor of 0.43, "as an attempt" to account for …

Another point is that 0.5◦ is still rather coarse compared to some regional modelling currently being done at 0.1◦ or finer resolution.

Yes, correct, however, higher resolution wind fields than those used here have not been available to us. We would be happy to work together with modelers in the future to refine our corrections …

Why do the authors use fixed emissions in the simulations shown in Fig 4?

We wanted to investigate the sole influence of the different wind fields on the correction (displayed in the more intuitive ‰ units). Further, including variable emissions would have led to a bias for high emission months.

If the authors have simulations with both fixed and monthly-varying emissions, can they include a comparison of these two simulations to quantify variability due to varying emissions vs variability due to varying transport? This comparison would be very useful.

All simulations are done with fixed emissions, which are later scaled. Sole emission variability is displayed in Fig. 2b, sole transport variability in Fig. 4a, and total scaled variability in Fig. 5b.

The authors note the previous estimate of the average nuclear correction by Levin using the plume model is higher than their estimate. Although the plume model is simpler, it might be considered to be at finer resolution than 0.5◦, and therefore a better estimate.

Levin et al. (2003) simply used one single dilution factor taken from the Turner (1970) workbook, and the mean wind statistics at the Philippsburg facility. The estimated factor, especially at the relatively large distance of 25 km from the emission point, may easily be wrong by a factor of two. (See also last comment to Referee #1.)

Can the authors make any inference on the detectability of the Philippsburg shutdown based on the Heidelberg Delta14CO2 data?

We have looked at that, however, as the average NPP contamination is of the same size as the individual measurement uncertainty, and as the variable fossil fuel signal is generally one order of magnitude larger, this is difficult or impossible.

The authors should discuss the impact the Philippsburg shutdown would have on the inferred fossil fuel CO2 at Heidelberg, if the change in the nuclear correction after 2011 was not accounted for. How does the change in the nuclear correction compare to the average fossil fuel signal in Delta14CO2 at Heidelberg?

(2.3 – 0.4 ‰)/(1.8‰/ppm) ≈ 1 ppm, this corresponds to ca. 10% of the total fossil fuel signal.

We added a respective remark at the end of the conclusions.

Section 3.3 Uncertainty in estimated nuclear correction – this needs more detail and seems rather too qualitative. The authors do not seem to include model transport uncertainty also for the high-resolution case.

Yes, but we are, unfortunately, not able to add more quantitative uncertainty estimates here, particularly not of model transport errors.

Do the authors have an estimate for the magnitude of sub-monthly variation in emissions?

No, the measurements in the exhaust air of Philippsburg reactors are integrated monthly values, and we are not aware of high-resolution exhaust data of $^{14}CO_2$. During revisions or fuel element change, there may occur short-term activity maxima, smeared out in the monthly means.

Do the authors think that monthly resolution in emissions data is sufficient, in general? Would this depend on the sampling integration time?

Emission data should ideally be as highly resolved as temporal changes of the meteorology occur, this may be hours or days. However, we think that for ^{14}C emissions we will probably not get higher than monthly or, at best, weekly resolved data, because ^{14}C measurements are (currently) not made continuously in situ, but rather on grab samples. We do not think that the required resolution would depend on the sample integration time, because the contamination at the measurement site varies most with meteorological conditions.

Could Fig 2b show the time series of emissions rather than yearly boxplots? It would be interesting to know if there is any pattern to the emissions over the year – for example, are emissions typically higher in summer potentially related to more maintenance undertaken in summer?

There is no apparent seasonality in the monthly emission data and no clear relationship between emissions and maintenance intervals. We added a respective sentence in the revised manuscript.

The authors should consider the Cattenom reactor in France, to the west and upwind of Heidelberg.

The estimated $^{14}CO_2$ emissions of the 4 PWR blocks of Cattenom (ca. 5.4 GWe) is about 0.2 TBq/year, i.e. half of the emissions from Philippsburg I. These reactors are located around 170 km west of Heidelberg. Therefore, we do expect at least one order of magnitude lower contamination from these facilities compared to those estimated for Philippsburg I.

Shouldn't Eq. 3 have a factor of 1000 for per mil units? What is used for XCO2 in this C3 calculation?

We have added the factor 1000. We used individually measured two-weekly mean CO_2 mole fractions.

First sentence in abstract – last word "fluxes" should be deleted. Radiocarbon measurements quantify fossil fuel derived CO2, but not fluxes.

We have changed "fluxes" into "component"

Also here the Delta notation is used without describing it. The phrase "14CO2 signal" is unclear – do you mean nuclear Delta14CO2 signal? Why are Delta14CO2 and 14CO2 both used in the abstract?

Isn't the Heidelberg site monitoring Delta14CO2 rather than 14CO2?

We think we can use in qualitative cases $\Delta^{14}CO_2$ and $^{14}CO_2$ more or less synonymously; however, we have made it clearer now that we give all numbers on the Δ-scale

Last sentence should be revised to "After operations at the Philippsburg boiling water reactor ceased in 2011, the" . . .

We changed this sentence accordingly

P2, L22: Delete "well".

Not sure why - the signals can really be large close to the facilities, i.e. a few km away !

L24 Comment about "normally quickly disperse" needs reference or should be deleted.

References were added.

P10, L21-24 – Please show some quantitative evidence from the simulations to support these statements.

Statements: “Therefore, and considering the high month-to-month variability of emissions (Fig. 2, lower panel), it is important to use monthly resolved emission data to estimate the Δ14Cnuclear signals originating from KKP I & II. The other four nuclear installations are secondary contributors permitting the use of annual average 14CO2 emission rates in absence of higher temporally resolved emission data.”

As the variability in model transport and NPP emissions are independent, we have to assume that both variabilities contribute to the total variability.

Figure 4b directly shows the low contamination from Neckarwestheim, which is only about 20% of that from Philippsburg (due to the ca. 25% lower emission rate and the larger distance of this NPP from Heidelberg).