This manuscript describes a new framework for joint d^{13}C / CO$_2$ inversions, and its application to a recent time period. Novel about this work is the use of a new biogeochemical model (BEPS) to simulate the 13C cycling through the terrestrial biosphere, which is a valuable tool as few of such models currently exist. The use of this model in an inverse framework follows methodology introduced before, but comes to conclusions that are not in line with our knowledge of the current 13C budget. This is very likely due to a number of flaws in the design of the inverse modeling problem, that need to be resolved before the paper can be judged on scientific merits. Since the results are likely to substantially change the conclusions, I can not recommend publication of the current manuscript in ACP and I think that a new manuscript should be created based on all suggestions and new results.

I will write detailed comments on the methodology and conclusions below, to guide the authors in re-investigating their approach. I hope they will consider improving their framework, as I think it is important for more modelers, and more different models, to contribute to the effort of interpreting the d13C records.

Detailed comments:

- Please pay close attention to the equation numbering. The matrix formulation is not numbered, but is referred to as eq. 6 (e.g. in 2.1.3., line 18). From there on all other equations are wrongly referred to as 7,8,9,10, etc, but are numbered as 6,7,8,9, etc.

- I have strong doubts whether Eq. 6 & 7 are correct. First of all, the units on the left hand side are not consistent with the right hand side. Rj is given as an absolute ratio (13C/12C), whereas Ra, alpha, Rbe and others are given in permil relative to PDB. These formulations are indeed the same as presented in Ciais et al. (1995), but in their study R is portrait as an absolute ratios 13/12C, and not in permil relative to PDB as done in this manuscript. Alpha should be portrait as the ratio of two carbon ratios in different reservoirs (e.g. alpha_ph=Rb/Ra). For example, the term alpha_ph*Ra should represent the ratio in newly assimilated plant material (Rb). But in this manuscript (at least according to the text), the term is solved as the product of two permil values. Also, I cannot follow the way alpha_ph (photosynthetic fractionation) is calculated from the atmospheric signature (now called d_a and not Ra?!) and plant discrimination (Delta). I would expect the plant discrimination signature being related to alpha_ph through the formulation -Delta/1000+1. I am wondering whether these issues explain some of the unexpected results in this manuscript. I ask the authors to elaborate on this.

- In those same equations, why are the authors treating net exchange and disequilibrium in one parameter, and not simply as two separate 13CO2 flux terms: one for net exchange, and one for disequilibrium isoflux? If the net exchange flux approaches zero the 1st term will simply disappear (or become very small), but disequilibrium isoflux always remains present. You avoid all problems with dividing through zero NEE, and also make sure that moles of ^{13}CO$_2$ are exchanged even when photosynthesis and respiration balance.

- The different case studies investigate the sensitivity of the surface CO2 fluxes to changes in discrimination and/or disequilibrium. If I understood correctly, the authors only make their changes in Eqs. 6 & 7 but not in the presubtracted concentrations. I think this is a wrong assumption, and the residual concentrations used in the inversion will become much larger when for instance the disequilibria fluxes are assumed zero. This must change the flux partitioning quite a lot to still satisfy the carbon balance, as an over- or underestimation of the disequilibrium isoflux by 30% can change the uptake over land and oceans easily with 1-2 PgC/yr (see Alden et al., 2010). So neglecting this disequilibrium flux, either over land or oceans, would cause much larger changes in the fluxes than magnitudes of 0.01-0.02 PgC/yr presented in this manuscript.

- Can the authors elaborate on how large the uncertainty is in the R matrix in terms of permil? When I calculate the uncertainties myself from your information the values become very large. For example, a land station is assigned a standard deviation of 1.25 ppm for CO2. The authors use the relationship Ra*sigma to calculate the uncertainty in 13CO2 in units of ppm. So for a land station I would write:

sigma*Ra=1.25*0.011=0.014 ppm

In ppm space, the mean and uncertainty in observed 13CO2 would thus become for instance 4.236±0.014 ppm. In permil space this translates into -8± 3.3 permil. I think an uncertainty of ±3.3 permil is quite a lot, especially when you consider that measurement precision is ~0.02 permil, and seasonal variability is within 1 permil. This could also be an explanation of why adding $^{13}CO_2$ to your inversion only had a minor impact on estimated fluxes.

- In Eqs 8, 9, and 10 I do not see the logic of the calculations, and especially this “VAR” correction term is of unclear origin. What is represented by this term, and why is it needed? Please correct me if I am wrong, but it seems like this has something to do with the smooth-curve nature of GlobalView, forcing you to use some time-averaged CO_2 value to back out the 13C/12C ratio from the reported smooth d13C values? If so, this seems very dangerous to me as small variations in CO_2 would drive large changes in 13C/12C which were not actually ‘observed’, but were part of the separate curve fitting procedures for d13C and for CO_2. And this would mean the 13CO2 signals going into the inversion were not real. Could you please confirm that you have used actual observed d13C and CO_2 pairs from the flasks to back out the 13CO2? If not, can you please explain why your method is valid, and if I even understood it correctly?

- The method to simulate the disequilibrium of the land biosphere (Eq 15) is in my view incorrect. The equation states that carbon coming out of todays carbon pools carries the signature of tau years ago, with tau being the pool turnover time. This seems conceptually appealing, but the turnover time tau is not a “lag time” for signatures to return, but rather an integration time of the pools. In other words, carbon coming out of wood pools with a 30-year turnover time have accumulated 30 years of atmospheric signatures, assimilated through NPP, into the wood that is respired. The respiration signature is thus some weighted average of the atmosphere, NPP, and discrimination over each of the past 30 years. That is why one preferably should use a biosphere model to spin-up these pools consistently over time. With the current ‘instantaneous’ treatment, the isodisequilibrium forcing coefficient (see Scholze et al.,2003, Alden et al., 2010, van der Velde et al., 2013) would likely be too large, and especially too variable from year-to-year. A tabulated comparison of this value to previous work (including a discussion of C3/C4 land-use and NPP fractions, and of global average d13C isofluxes) would be helpful.

- Overall, it would help the readers if the authors can assess the performance of their a-priori modeling system better before being shown the inverse results. For instance by showing observations vs. model results (including uncertainty) from forward modeling of the a-priori fluxes over a few years. One can then quickly see if the simulated $^{13}CO_2$ disequilibria match the observed trend of d13C which is most strongly pulled down by fossil fuels. In a forward simulation, cases IV through VI where disequilibria were neglected, should give a rather big offset between observed and modeled 13CO2 mole fractions/signatures. Then in the inverse simulations, readers must be able to see the improvements to the simulated mole fractions/ signatures, and have access to some statistics (chi-squared) to judge the chosen uncertainties versus model perfromance.

- Related to the a-priori mole fractions is the global CO_2 and 13CO2 flux budget. Considering the separate use of GFED2 fires and BEPS biospheric fluxes, I wonder to what extent the global CO_2 balance is closed a-priori and a-posteriori? And is the global 13CO2 balance closed if you add up the global mean values of each of the bottom-up flux terms? The disequilibrium flux (combined ocean + land) is the largest term in the budget after fossil fuels. So, either an over- or underestimation of the disequilibrium isoflux can deteriorate the carbon uptake patterns quite a lot over land and oceans. A closed budget from bottom-up estimates is difficult to create though and often requires additional scaling due to large model uncertainties in respiration, pool turnover and ocean exchange (Alden et al. 2010, van der Velde et al., 2013). The latter authors suggest that matching the observed $^{13}CO_2$ variability is even harder because models tend to underestimate variations in discrimination. Do you also see this?

- Table 3 is very hard to read, and must be presented differently. Overall, I also would like to see fewer global maps in the figures, and more relevant statistics or temporal behavior of fluxes. Also, the work would profit from considering a longer time period since much information is contained in the year-to-year variations of d13C. What is preventing you from doing this for a full decade?