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Interactive comment on "A deeper look at the relationship between root carbon pools and the vertical distribution of the soil carbon pool" *by* Ranae Dietzel et al.

Anonymous Referee #2

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Dietzel et al. report on a root study conducted at a field experiment where continuous corn is compared to reconstructed fertilized and unfertilized prairie stands. They have measured: 1) root profiles to depth of 1 m at the end of the growth season for six consecutive years, 2) root production (by regrowth cores) for 2 growing seasons to 30 cm depth, 3) root and soil C and N concentrations to 1 m depth. Extracting root for multiple growing seasons, multiple soils layers and multiple replicated treatments is by no mean easy, and the soil science community can certainly benefit from such precious data. The authors report interesting findings: 1) the C/N ratio of root material increases with depth, which has potential implications for soil C storage, 2) the maize root profile is more uniform with depth than that of prairie species (confirmative), 3) fertilization of

the reconstructed prairie greatly decreases root biomass.

However, I have some significant concerns with the study:

1) The continuous accumulation of maize roots throughout the 6-year period is quite troubling (Fig A3). The authors provide one reference (Dupont 2014) stating that intact prairie root (not maize) can be found in soil several years after cultivation. However, they ignore the substantial literature on maize roots that clearly indicates that maize roots decompose rapidly in soils, starting with the classical study of Mengel and Barber in 1974 (Agron. J. 66: 341-344) and several studies that have followed. Actually, Mengel and Barber (1974) state that root length and fresh weight decrease rapidly after maize has reached the reproductive stage. Here, Dietzel et al. themselves state in the abstract about maize roots that they are "non-structural-tissue dominated root C pool with fast turnover". They also indicate that the site was apparently under maize-soybean rotation prior to starting the experiment, so why was there no accumulated maize root biomass at the start of the experiment (if the root accumulation theory is correct)? Unfortunately, in the present study the roots were not sampled at the same time each year (from early October to early November), and the accumulation of maize roots the last two years also corresponds to the 2 earliest sampling. A possible explanation is that the roots actually decomposed quickly in the field and that by sampling a month earlier by the end of the six-year period a greater number of non-decomposed roots were retrieved. Effects of inter-annual climatic variability on root growth is another potentially contributing effect. There are three implications from this: 1) apparent maize-root accumulation in the field over 6 years is probably an artefact, 2) the pool and rate modelling of Fig 3 and 4 is not justified (it did not bring much to the paper anyway), 3) the paper should have included a much more throughout review of the literature about maize-root dynamics in field soils.

2) The maize-root C profile is presented 3 times in the paper: 1) Fig 1 b, 2) Fig 2, and 3) Fig. A3. The 3 figures are in the same units (Mg ha-1), but I could not reconcile the data between them. The 2013 data of fig. 1 b (with largest root accumulation in

the top soil) do not seem to correspond to the 2013 data of figure A3, which seems to show highest maize-root biomass in the deeper soil. In addition, the maize root profile appears more even in Fig.2 than in Fig. 1 b, while it should be exactly the opposite (e.g. the 5-10 cm should have about half of the 0-5 cm in fig 2, if extrapolated from fig 1). Or are the two figures exactly the same? But why figure 2 then? The 3 figures should have been reconciled and presented as one main figure in logical units, and then the results compared to the literature.

3) Data from the root regrowth cores are not clearly presented, but used for a direct extrapolation of a root turnover rate in the top 30 cm. Summary data (without statistics) are presented in g m-2 in Table 4, making it difficult to compare to the root pool data presented in Mg ha-1. The maize root productivity appears low and I am missing a coherent evaluation of the C input in the context of published studies.

4) The implications for C storage presented in this paper are largely hypothetical and somewhat contradictory. The present paper contains no significant result to link root biomass profiles to soil C profiles. While it is OK to briefly elaborate about possible implication of a higher root C/N ratio with depth, this should not be the main part of the discussion, which should instead focus on actual significant results. In addition, I could not reconcile the two ideas presented here about the effect of root C/N ratio on C storage in soils. On the hand, the authors argue that a lower C/N ratio for maize root favours C storage in soil as compared to prairie roots (p14, line 11-12). On the other hand, they also argue that an increasing C/N ratio of roots with depth in the soil profile also favours C storage (e.g. p1, line 10-12). The potential attempt to reconcile these two contradictory effects of root C/N ratio on soil C was unconvincing (p 14).

In conclusion, Dietzel et al. have collected an impressive data set on maize and prairie roots following maize-soybean rotation. The dataset appears to suffer from some artefacts, but root studies are difficult and shortcomings could have been acknowledged. The data themselves are neither clearly presented nor sufficiently discussed in light of the literature. A main finding is largely ignored, i.e. the dramatic reduction of root

biomass by fertilization in prairie systems. By contrast, the authors focus on an uncertain modelling and non-verifiable considerations about the effect of root C/N ratio on soil C. A focus on significant results and discussion of these results in light of the literature would have better served this study.

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