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Interactive comment on "A model of potential carbon dioxide efflux from surface water across England and Wales using headwater stream survey data and landscape predictors" *by* B. G. Rawlins et al.

Anonymous Referee #2

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General Comment

1. The authors present a model containing 10 variables which predicts 24% of the spatial/temporal variation in pCO2. This suggests it has limited value. I believe that one of the main problems with this approach is that the model is predicting pCO2 values which have been modelled themselves.

2. There is a significant N American, European (inc. UK) literature on pCO2 and CO2 evasion, which is poorly represented in the manuscript.

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Specific Comments

16455, L1: Several papers (see Kling et al. 1991 Science; Hope et al. 2001 L&O) already show that CO_2 evasion is likely to account for much, much more than 10% of NEE.

16456, L16-17: it would be better to turn this into a testable hypothesis.

16457, L3-6: what is the justification for this statement or assumption? These are the areas of the catchment (headwaters) where evasion rates are known (and have been measured) to be highest.

16457, L11-14: the authors are making a huge assumption here, that all free CO_2 evades downstream so there is no need to calculate k values. There is a significant body of literature to show that CO_2 concentrations (even in many large rivers systems) never reach equilibrium with the atmosphere. It is also unclear what the authors mean by "limited downstream changes in water chemistry". The statement needs to be clarified, particularly as rivers typically show significant spatial changes in water chemistry.

16457, L21: this is a misrepresentation of Dinsmore et al. (2010) – I believe their work is based on one headwater site only.

16459, L19: "theoretical pCO_2" is being modeled by the authors, from several variables, but it is unclear which ones (pH? temperature? DIC?). The authors need to clearly state how they are doing this in their model, so readers can evaluate it's usefulness for themselves. It would also be appropriate (like most models) to validate it against real pCO_2 data.

16461, L6: the authors need to define "dominant" land cover class. Does this mean >50% coverage?

16461, L16: what is the BFIHOST value?

16464, L8-10: It's not clear why the authors expect to find higher pCO_2 in agricultural streams; pCO_2 is not just based on soil productivity. These systems typically contain well-aerated, highly managed soils, which lose soil CO_2 rapidly to the atmosphere. Poorly drained, organic-rich natural systems are where highest pCO_2 occur because of their poor drainage and their ability to accumulate significant sub-surface CO_2 stores which connect to the aquatic pathway.

16467, L2-3: CO_2 evasion = pCO_2 x flow is a gross over-simplification, because there are so many factors which influence CO_2 concentration in the aquatic system.

Figure 7: Is the data presented in this figure realistic? The suggestion is that the highest evasion rates occur in the higher pH soils and not in the organic-rich upland areas of the UK. High pH soils produce circum-neutral or high pH streamwater, which based on the carbonate equilibrium, contains little or no free CO_2. I suggest the authors revisit their model as it appears to calculate pCO_2 incorrectly. Further underlying data or variables like pH and DIC, would help the reader sense-check these regional differences.

Interactive comment on Biogeosciences Discuss., 10, 16453, 2013.