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Interactive comment on "Application of global models and satellite data for smaller-scale groundwater recharge studies" *by* Rogier Westerhoff et al.

Anonymous Referee #1

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Westerhoff et al attempt to bridge the gap (pardon the pun since they are inspired by WaterGAP) between global and local recharge models which I think is a useful and timely objective. I found the rationale in the introduction quite compelling and exciting; however, I found the assumptions and analysis not very robust in a number of ways that I highlight in my major and minor suggestions below. Since the results could be interesting, but are just not very robust in their current form, I suggest a MAJOR revision.

Major suggestions: 1) The authors state that WaterGAP is a rigorous, accepted method which I only partially agree with. Döll and Fiedler (2008) is a very well referenced paper but this partly because they were the first to model recharge globally. In my opinion,

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Discussion paper

the problem with the WaterGAP method is that it is not directly physically-based - all the physics of variably saturated soil and the water table are excluded. Other more recent approaches to modeling recharge regionally or globally such as de Graaf et al. (2014) are more robust and physically based. I consider the factors (especially fgeol and fsoil in this current manuscript, like the f factors in Döll and Fiedler) to be s falsely or loosely quantified. At a minimum the authors should improve how they implement and describe fgeol and fsoil (as I describe below in more detail) and acknowledge that other approaches to modeling recharge are more physically based. a. The fsoil in table 2 is qualitative/ordinal data such as 'slow over rapid' or 'moderate' translated to interval/ratio numbers like 0.25 or 0.5 (see Bolstad, 2015 figure 2-10 or other spatial data text for discussion of ordinal vs. interval/ratio data). Without further justification, this is a HUGE guess which leads to unsupported results. For example 'slow over rapid' soil does not likely have twice the recharge of 'moderate'. b. Similarly how fgeol is calculated seems arbitrary (and how it is written in the text seems wrong. Line 266 says "fgeol was then calculated as the ratio between 0 and 1 of the potential rainfall recharge and hydraulic conductivity". From this I assume fgeol = R/K which then suggests low K $\rightarrow$ higher fgeol and thus recharge whereas high K $\rightarrow$ lower fgeol and thus recharge. This seems like a mistake and the equation should be fgeol = K/R if anything. But I also don't know what the broader assumption that hydraulic conductivity plays a limiting role in rainfall recharge, or how it is quantified with this ratio. This seems like a major, and unsupported assumption to me. c. Along a similar vein, I consider the quantification of how permeability decreases with age (line 184) totally unfounded. I have never seen a reasonable and robust relationship between geologic age and permeability, and I think it is impossible. Permeability-depth relations, that are actually based on physics, are even hard enough. I suggest the author scrap this entirely, and if it is necessary to alter permeability based on age, just do so deterministically and clearly state how and why units of specific age are modified in the analysis.

2) I think the uncertainty analysis could be the most interesting and compelling part of this paper, if the authors emphasize this in their revision. The uncertainty analysis

could be robust (Section 2.4 starts strong) but could be improved in a few ways: a. The uncertainty results (Fig. 5) are barely discussed. It would be great if the authors could quantify what percentage of uncertainty is due to the various input variables (and their inherent uncertainty). This is just a suggestion, not a requirement. b. The real uncertainty of the authors assumptions are not quantified at all. For example the fsoil is assumed to be 5-10% uncertain when I think (based on arguments) above that this is a massive underestimate of uncertainty. Similarly, assumption of how fgeol uncertainty is quantified is not well justified.

Minor suggestions: 1) The introduction is a little to New Zealand and could examine recharge processes at different scales and in different environments more explicitly by referencing papers like. . .(Sophocleous, 1992; Lerner, 1997; Simmers, 1998; de Vries and Simmers, 2002; Scanlon et al., 2006; Beigi and Tsai, 2014; Cuthbert, 2014; Hartmann et al., 2015; Cuthbert et al., 2016; Wada, 2016). 2) Line 13 "largely inspired" seems colloquial to me 3) Line 25 and throughout paper: state rough scale (in km) everytime you say 'scale'. What do you mean by regional scale, catchment scale, aquifer scale? 4) Line 33: Gleeeson et al 2011 is data rather than a model, while other important global models such as Doll, de Graaf are not included here. 5) Line 59: acknowledge limitation of the assumption that recharge is important to groundwater management (with papers like {Bredehoeft, 2002 #650}). 6) Line 171 forward is not necessary in my opinion 7) Line 194: weak rationale for choice – what about data quality? 8) Line 231: suggest changing R to P (precip) so that R can be recharge in manuscript 9) Table 1: sediments are unconsolidated while sedimentary is only used for consolidated materials. Modify table accordingly. 10) I don't see the purpose of Figure 4.

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