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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A partially-coupled hydro-mechanical analysis of the Bengal Aquifer System under hydrological loading" *by* Nicholas D. Woodman et al.

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Authors' summary response to HESS editor.

Our thanks for your patience; the first and second authors have both been out of the country since receiving the last of the reviewers' comments.

Reviewer#1

We have addressed each of Reviewer#1's comments directly in our original reply (hess-2018-304-AC1). There we point out that this is the first paper that we are aware of to apply partially coupled poro-mechanical models to a deep and extensive sedimentary

sequence such as the Bengal Aquifer System, which 150 million people rely on for water. We deal with the simplifications that are appropriate to areally-extensive loading types which are common and important in the context of the BAS and therefore have generic impact on groundwater hydrographs and surface displacements throughout the basin. In the article we provide a methodology for identifying key boundary conditions from hydrograph data. We demonstrate numerically for the first time by reference to the Bengal Basin (i) the limits to the conventional use of water levels in piezometers to indicate groundwater recharge and drainage (ii) the conditions under which piezometer water levels respond primarily to changes in terrestrial water storage mass, so may be suitable for 'geological lysimetry', and (iii) the likely scale of ground surface vertical deflections, to complement GPS studies of ground surface motion. We entirely agree with the reviewer that there are also circumstances under which poro-mechanical interactions will need to be addressed in 2D and 3D; these are the subjects of companion papers that will follow. We contend therefore that the paper addresses an obvious 'gap' in the literature, and makes a significant and original contribution. The improvements we have suggested, based on both reviews, we hope will help readers (including Reviewer#1) engage better with the paper by making its contribution and novelty clearer.

Reviewer#2

Reviewer#2 (Dr. Garth van der Kamp) headlined his thoughtful and constructive comments with the judgement that "This paper addresses an important aspect of groundwater management: the fact that groundwater levels do not reflect only the changes of groundwater storage but also reflect groundwater pressure changes due to mechanical loading by changes of total water storage above the formation", and also that "These . . . effects are only starting to be acknowledged in groundwater management practice and this paper therefore can make an important contribution to the literature in groundwater and surface water hydrology." He went on to raise some interesting points and to suggest ways to strengthen the paper.

We concentrate below on a summary of our response to the review of substance pro-

vided by Reviewer#2. The arrangement of the points and the full reasoning are as given in our original author response (hess-2018-304-AC2). Included in this summary are the modifications we propose in the light of Reviewer#2's review, and which we agree will strengthen the paper.

Points proposed to strengthen the paper - We will add to the Introduction (section 1 of the paper) a description of the barometric effect as a useful example of extensive surface loading. - We will augment section 2.4.1 of the paper, expanding on Reviewer#2's point about time-scale of responses in relation to barometric effects and estimation of loading efficiency. - We will add to the Introduction a summary of the climate and seasonality of the region, as context for the subsequent discussion of seasonal changes in terrestrial water storage. - We will include citation of Anochikwa et al (2012) at L 69 in the Introduction, which we omitted in an oversight; it is an important example of geological weighing lysimetry as applied in Saskatchewan, Canada. Also, at L 500 in the Discussion (section 5.2 of the paper) we will expand on the variety of measurements necessary to properly deconstruct the water balance using deep piezometers, such as has been done by Anochikwa et al (2012).

Responses to substantive criticisms - We should like to retain our presentation of the poro-mechanical equations (Re para 3 of Reviewer#2's 'Specific Comments' in which he suggests largely eliminating them), motivated by the three reasons given in our original response to Reviewer#2's comments. (Please note that the appropriate equations for the loading efficiency and specific storage requested by Reviewer#2 are included in the original Discussion paper, see equations 5, 6 and 8). - Re the representation of upper boundary conditions of load and hydraulic head (paras 4 and 5 of Reviewer#2's 'Specific Comments'), we agree that Anochikwa et al.'s (2012) deconvolution of hydraulic and mechanical components is completely valid, but we nonetheless contend that the approach we use in the Discussion paper to solving the equations is also perfectly correct. In the paper, we do not repeat the analysis/discussion over whether hydraulic influences alone may explain deep hydrograph data in the BAS since this

possibility has been comprehensively dealt with in Burgess et al. (2017). We agree it is nonetheless useful to include, so we have done as suggested by Reviewer#2 and addressed each boundary condition separately (in this case the hydraulic boundary, the load boundary, and the pumping effects) in plots such as those shown in Anochikwa et al (2012), which would be suitable for inclusion as 'Supplementary Information'. For the main text, we prefer to retain our approach, for the reasons given in our original author response, and emphasise that our objective is subtly different from that of Anochikwa et al. (2012): ours is to explore the different impacts of the specific styles of surface water load manifestation; Anochikwa et al's is to deconstruct the water balance. Regarding the 'counter-intuitive' amplitude response to the 'load only' upper boundary scenario (para 7 of Reviewer#2's 'Specific Comments'), we would like to add consideration of this interesting point to section 5.1 in the Discussion section of the paper, following the outline given in our original author response. - In our approach to treatment of the field data (para 7 of Reviewer#2's 'Specific Comments'), we do not examine individual short-term rainfall events, although we do comment on rainfall effects at each site, as referenced in our original response to Reviewer#2's comments and in our citation of Burgess et al (2017) for the effects at one of the sites. Emphasising again that our purpose is subtly different to that of Anochikwa et al (2012), we have not measured the individual components of the water balance at our sites in the paper under discussion, so cannot resolve their individual effects on the groundwater heads. Rather, we have tested the proposition that specific piezometers behave as geological weighing lysimeters (the approach is given at L 349-359 in section 4 of the paper 'Applying the partial coupling analysis to field data'), and for this purpose we have applied the appropriate piezometer head record as the upper boundary condition in the model, resolving "all sources of load acting at the site", including barometric loading. - Re "Evaluation of loading efficiency, and use of barometric efficiency" (paras 2 and 6 of Reviewer#2's 'Specific Comments'), there are 3 key issues raised by Reviewer#2 relating to barometric efficiency: (i) we don't include barometric effects in the generic modelling. Response: this is for simplicity, since although it is trivial to include, its inclusion would

serve to de-clarify the analysis. (ii) we don't use the barometric efficiency estimates (although they are available from Burgess et al, 2017) to estimate loading efficiency. Response: please see references to section 2.4.1 LL 186-204 and to the Discussion LL 528-533 in our original author response. In the Discussion we have highlighted the inconsistency between estimates of mechanical stiffness, storativity and barometric efficiencies. As Reviewer#2 points out, barometric efficiency estimates may have a time-dependency due to pressure diffusion in aquitard layers. We will add these points to section 2.4.1 of the paper as a more detailed explanation, as suggested by Reviwer#2. We agree that this remains an "important and poorly resolved issue in geolysimetry". In our Discussion we put it thus: "These differences require attention, but the overall conclusions on the significance of poroelastic behaviour in the BAS and the pattern of poroelastic responses characteristic of specific upper surface TWS boundary conditions are unaffected". (iii) we include the barometric load as part of the total load that we apply to the surface in the field examples, without attempting to de-convolve each contribution. Response: we do this since we do not have shallow water data nor rainfall measured precisely at the field sites, and it is not our objective to deconstruct the water balance (see also the two previous bullet-points). We will add the explanation from our original author response to the discussion section of the paper, where we move from 'lessons learned' to how methods might be applied.

Minor comments and technical corrections For the three points made by Reviewer#2, please see our original author response in which all are readily addressed.

Concluding statement We acknowledge the thoughtful comments made by Reviewer#2, and we are confident that the modifications we have proposed above will strengthen the paper. We would be grateful for this opportunity.

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