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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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Response to comments by referee Garth van der Kamp

We are grateful to have these thoughtful and interesting comments from Dr. van der Kamp (GvdK), whose observations of surface moisture loading and subsequent development of geolysimetry as a technique with hydrological application have inspired our analysis applied to the Bengal Aquifer System.

We entirely agree that the issue of mechanical loading by changes in total water storage above the formation is an important aspect of groundwater management; our aim is to bring this to the attention of researchers and practitioners in the Bengal Aquifer System, and to those working in similar hydrogeological environments in the delta regions of SE Asia where groundwater meets the needs of over 300 million people. Suggestions that loading effects might be substantial in the Bengal delta were originally made by W.E. Bardsley and D.J. Campbell in 2000 (cited in the Discussion paper), yet the effects had not been recognised and documented prior to Burgess et al. (2017) in a paper preliminary to the present analysis.

Interesting questions certainly remain about the coupled hydro-mechanical behaviour of aquifers, which merit further attention, and we are grateful to GvdK for signalling these. The magnitude of 'loading efficiency' is one (para 6 of 'Specific Comments' in GvdK's review). The 'counter-intuitive' amplitude response for which GvdK suggests a 'travelling wave' explanation (para 7 of his 'Specific Comments' re L 298-299) is another. GvdK's comments on these points make interesting contributions which we reflect on below.

Rather than giving a linear point-by-point response, below we discuss the points under four titles: 'points to strengthen the paper', 'substantive criticisms', 'points of interest', and 'minor comments & technical corrections'.

Points to strengthen the paper:

The review has shed light on aspects of the paper which we would like to address in order to strengthen it:

- We agree that the phenomenon of groundwater pressure changes in response to changes of atmospheric pressure (the barometric effect) is a well-known concept and is a useful example of extensive surface loading (paras 1 and 2 of GvdK's'Specific Comments'). So we are grateful for the suggestion to add description of the barometric effect in the Introduction – this we will do.

- The comments about barometric effects and loading efficiency (para 6 of the 'Specific Comments') are well-taken. Our paper acknowledges in the Discussion (L 528-533) the interesting discrepancy been estimates of loading efficiency. We should like to give the point about time-scale of responses more prominence in section 2.4.1 of the paper, adding to our description of the relationship between specific storage, Young's modulus, and loading efficiency (L 186-204 of the paper).

- We agree that a summary of the climate and seasonality of the region in the introduction would give a useful context to our discussion of seasonal changes in terrestrial water storage (para 8 of 'Specific Comments'). Annual rainfall is around 2000 mm in this tropical monsoon climate, with individual events of >100 mm/day common during the monsoon season, May-November, during which river levels rise by 2-3 m leading to widespread flooding of the land surface. However we emphasise our aim in the paper under discussion is not to produce a site water balance, for which the data are not adequate. We agree that ultimately there is a need to quantify individual components of the water balance in a manner similar to Anochickwa et al. (2012), and for calibration of a deep piezometer to known changes in the water balance, but this was not our purpose in the paper under discussion. In Discussion at L 500 we write "A true indication of recharge requires . . . a shallow tubewell screened over the depth interval of actual water table fluctuation". We should like to expand this statement to include the variety of measurements necessary to properly deconstruct the water balance using deep piezometers, such as has been done by Anochikwa et al (2012) at their site in Saskatchewan, Canada.

Substantive criticisms:

There are substantive criticisms made with respect to four points (headings in bold below):

Review of poromechanical theory (para 3 of 'Specific Comments', re L 102 onwards) It is suggested that the presentation of the poromechanical equations should be largely

eliminated from the paper. We accept that our presentation of the poromechanical equations presents nothing new beyond the references cited, so might largely be eliminated. However, unless we transgress the length limit we think there is a good case for retaining the equations, thinking it beneficial to keep a more generalised treatment (despite this being available in the earlier literature). We have the following motivations:

(1) The equations provide a succinct reminder of the key assumptions, which although very well-known to those publishing poromechanical research, have been neglected elsewhere (e.g. it is assumed here that the skeleton grains are incompressible). The key insight (which has also been known for a long time), that we would like to re-emphasise is that the 1D simplification leads to a partially coupled system. Moreover, the 1D simplification can then lead to decoupling if the change in total stress can be assumed insignificant. Perhaps it is because of the common applicability of the 1D simplification to groundwater flow that the great convenience of decoupling the flow equations has been so widespread. So, whilst elastic storage and barometric efficiency effects embed the poromechanical nature of aquifers, the decoupling simplification effectively removes the motivation to take loading into account when simulating flow and water storage. This is potentially seriously erroneous, particularly in the context of the BAS. We think it is so important that we have put it in the title, also at LL 57, 126 and extensively throughout the text, and return to it in the conclusions.

(2) In the event of groundwater flows to individual wells, loading by rivers and other more complex boundaries, the 1D assumption breaks down and full poromechanical coupling is likely to be needed. We have a manuscript in preparation which examines 2D effects where the geometric simplifications do not apply. We therefore prefer for the 1D paper not to start with unnecessarily approximated equations since we intend to provide a coherent body of work.

(3) Given the discussion about the operational meaning of barometric efficiency measurements (see below), we think it is doubly useful to have the fundamental equations to hand.

Re: 'The appropriate equations for the loading efficiency and specific storage should be included - they are not given in the text as it stands.' (last sentence, para 3 of 'Specific Comments'): please see equations (5), (6) and (8) in the paper.

The upper boundary conditions of load and hydraulic head (paras 4 and 5 'Specific Comments', re LL 148-359) - which we use to represent the different scenarios of terrestrial water variation addressed in the paper. The reviewer recommends the approach used by Anochikwa et al (2012), and certainly this paper is highly relevant. A similar forensic approach to deconvolving the input terms is precisely what we in time intend for the Bengal Basin. We are aware of the Anochikwa et al. (2012) study; to omit it was an oversight – which we will correct.

We agree that Anochikwa et al.'s deconvolution of hydraulic and mechanical components is a valid way to solve the equations. But we contend that it is perfectly correct to solve the equations as we have done in the paper under discussion. The important aspect is that the boundary conditions are explicitly specified and compared. In that vein, we have noticed an interesting difference in assumptions between our work and the paper by Anochickwa et al: we add a mechanical load due to a moving water table (i.e. we include the weight of the water taken up as unconfined storage) whereas Anochickwa et al do not.

The Anochikwa et al. (2012) paper and ours under discussion have subtly different objectives: ours is to explore the different impacts of the specific styles of surface water load manifestation. We agree that the deconvolution approach is a valuable way to understand the water balance. However we are not clear quite how deconvolution can allow the scenarios we address to be treated "as one", since each overall scenario represents a distinct combination of boundary conditions. Also, the 'load-only' case requires a potentially misleading head boundary to be applied in order to obtain the correct superposition. Therefore, we would argue in favour of our approach, and our preference is to keep it.

Approach to treatment of the field data (para 7 of 'Specific Comments', re LL 337-458) The review suggests including description and analysis of the short-term rainfall events, as a strong demonstration of the reality of the loading effects. We completely agree with this suggestion – see L 375 for the Khulna site, "Episodic deflections on the hydrograph rising limbs, coincident with rainfall events, are likewise simultaneous at all measurement depths" and L 420 for the Laksmipur site, "The hydrographs are characterised by a sequence of episodic increments in groundwater head associated with periods of heavy rainfall". A previous paper by Burgess et al. (2017) showed the episodic increments in groundwater head at the Laksmipur site to be simultaneous with periods of rainfall, and proposed them as evidence for the loading response. However, we have not measured the individual components of the water balance at our sites in the paper under discussion, and cannot deconvolve 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), 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". Again, our purpose is subtly different to that of Anochikwa et al (2012).

Why ignore barometric effects? (para 6 of 'Specific Comments', re L 229) The justification for neglecting barometric effects on the generic simulations that we make is purely that of simplicity, as stated. It is straightforward to superpose a further loading signal on top of the existing one, but this would not bring further insights to the responses characteristic of particular surface boundary conditions, which is our purpose. In terms of the simulations of specific sites, the daily perturbation on water heads by atmospheric pressure changes in the data is of the order of 1cm, which is relatively small compared to the annual perturbation of the order of 1m. It would need to be taken into account when deconvolving deep piezometric signals to make water resources assessments, particularly to include the seasonal atmospheric pressure variations, but we don't make such assessments in the paper under discussion. We would like to add such an explanation to the discussion section of the paper, where we try to move from 'lessons learned' to how methods might be applied. For the real site simulations the point is that the top boundary is given a total load which is the sum of atmospheric and water loads (see L 383 of the paper: "The upper boundary resolves all sources of load acting at the site including from the Rupsa River, which is a linear rather than an areally-extensive load". Our objective was (LL 341-344) to "apply the principles and assumptions of the partially-coupled hydro-mechanical approach to reproduce the characteristic features of the multi-level groundwater hydrographs . . .. , rather than to attempt an exact match by inverse modelling". Therefore our objectives were subtly different from Anochikwa et al (2012). To obtain a proper water balance would indeed require removal of the barometric contribution.

Evaluation of loading efficiency, and use of barometric efficiency (paras 2 and 6, re L 260, of 'Specific Comments') For the generic simulations, making LE=1 neatens the analysis. It could easily have been changed, say to LE=0.8, but this would provide no additional insight.

The reviewer comments: 'Such analyses (of barometric efficiency) are briefly mentioned in the text (L 186-194) . . .., but are not further described or used in the paper although they are surely relevant. At the very least a more detailed explanation should be provided of why these results are not used.' In the paper we explain why we did not use barometric efficiency as our basis for loading efficiency – see LL 186-204 in the paper, which concludes "Therefore for the purposes of this paper we adopt Ss estimates based on field measurements and use the corresponding ðİŻ¡ and E values." Later, in the Discussion, we return to this as a topic which requires further attention in agreement with the reviewer's point– see LL 528-533: "In our analysis we have based values for the 3D loading efficiency, β (0.961-0.996) and Young's Modulus, E (82-851 MPa) in the BAS on field measurements of Ss, for the sake of internal hydro-mechanical consistency, but we have noted a discrepancy with lower values for the 1D loading efficiency ðİIJĽ (0.69-0.87) derived from determinations of barometric efficiency(Burgess et al., 2017). These differences require attention, but the overall conclusions on the signif-

icance of poroelastic behaviour in the BAS and the pattern of poroelastic responses characteristic of specific upper surface TWS boundary conditions are unaffected."

Therefore, we completely agree with the reviewer's important point that barometric efficiency measurements operationally consider timescales corresponding to short-term periods governed by the pore pressure changes in the relatively stiff sediments (para 6 of 'Specific Comments', re L 260). In Burgess et al (2017) short-term moisture loading effects were a key interest, so loading efficiencies based on barometric efficiency estimation are appropriate. However, in the paper under discussion we are concerned over poromechanical consistency, and contend that we should remain sceptical about using barometric efficiencies derived from short-term responses to address water load changes operating over the longer term. Interestingly, longer-term loads are potentially more readily determined in the Bengal Basin than in drier, temperate environments, since there is the possibility of measuring time-series of flood inundation over the monsoon season, rather than individual components of the soil/vegetation water budget. Therefore we also agree with the reviewer that this is an important and poorly resolved issue – see L 528-533 in the Discussion section of the paper, and below under 'points of interest'. We can augment our discussion of this point in the paper.

Points of interest: 'loading efficiency' and the 'counter-intuitive' amplitude Loading efficiency We felt that the discrepancy that we haven't resolved between field measurements of Ss (via pumping tests), anticipated material stiffness and barometric efficiencies is sufficiently interesting that we have made it prominent. We agree with the reviewer on this – also see Discussion L 528-533.

The 'counter-intuitive' amplitude response to the 'load only' upper boundary scenario: The reviewer makes a very interesting point here (para 7 of 'Specific Comments, re L 298-299), and to build on it we would like to consider adding a paragraph to the Discussion section of the paper. The decomposition described is a helpful way to mathematically picture how the apparently anomalous amplitude and phases come about in the 'load only' case. We think our partially-coupled solution is also useful, however. For example, for the 'load-only' case one can take the phase solution we give in equation [12] and set α=0 and γ=1; in the asymptotic limit as z tends to 0, the phase tends to $-\pi/4$. By differentiating we get the dimensionless depth corresponding to the peak amplitude, i.e. the solution to cos(θ)+sin(θ)=exp(-θ) which is $\sim$2.284 and the peak amplitude at this depth is $\sim$1.07. So, we would argue that both approaches can be useful in different ways.

Minor comments and technical corrections The reviewer makes some minor comments which we address as follows:

- 'The appropriate equations for the loading efficiency and specific storage should be included - they are not given in the text as it stands' (para 3 of 'Specific Comments' in the review) – please see equations (5), (6) and (8) in the paper.

- We will give full details for Burgess et al (2017), published in Scientific Reports, 7(1), 3872. doi:10.1038/s41598-017-04159-w

- Units as Pa and MPa (final point in GvdK's review, re LL 110-115): Yes, we accept – but emphasise that all the same units were included in application of equation (1) so no corrections to our working are needed.

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