

Reply to comments by Reviewers # 1 and #2 of “Technical Note: A simple generalization of the Brutsaert and Nieber analysis”

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1 Introduction

We would like to thank the Reviewers and the Editor for their valuable comments. We give our answers in a single file.

Apart from the answers that are discussed below and that are specific for the comments raised during revision, some re-wording was made to better suit the context of the discussion and that can be seen in blue in lines 21, 31 and 182.

2 General answers and description of numerical experiment

In our original manuscript, we believed that our analytical results might alone be enough to carry our argument. In retrospect, we agree that an independent verification can strengthen it.

Indeed, our previous version shows results from estimates that amend the original BN77 theory for $\phi_0 \neq 0$, but still rely on Boussinesq [1903]’s *linearized* solution. It is well worth comparing how our (still approximate) estimates in Eqns (10) and (11) perform against true values resulting from the more physically accurate Boussinesq nonlinear differential equation.

However, we do not believe that field data exist where independent values of n_e and k_0 can be obtained with enough confidence — *i.e.* small enough uncertainty. After the reviewer’s comments, it has come to our attention that some laboratory experiments have been performed that might be useful as validation sets [Hewlett and Hibbert, 1963, Sanford et al., 1993, Mizumura, (2002)], but we haven’t yet looked at those data in detail. Moreover, and importantly, it is not clear that a wide enough range of carefully controlled values of ϕ_0 would be available to validate our results.

Therefore, we have decided to run an extensive set of numerical simulations of the full nonlinear Boussinesq equation. We would like to argue that this is a valid alternative, that has been used in important research related to the theme [see Szilagyi et al., 1998, Rupp and Selker, 2006].

We describe our simulations here but chose not to include this description in the manuscript; the description is relevant to our answers to Referees # 1 and # 2. With an implicit finite-difference method, we solved the fully nonlinear Boussinesq equation in dimensionless form, *i.e.*

$$\frac{\partial \phi}{\partial \tau} = \frac{\partial}{\partial \eta} \left(\phi \frac{\partial \phi}{\partial \eta} \right); \quad \phi(\eta, 0) = 1, \quad \phi(0, \tau) = \phi_0, \quad \frac{\partial \phi}{\partial \eta}(1, \tau) = 0, \quad (1)$$

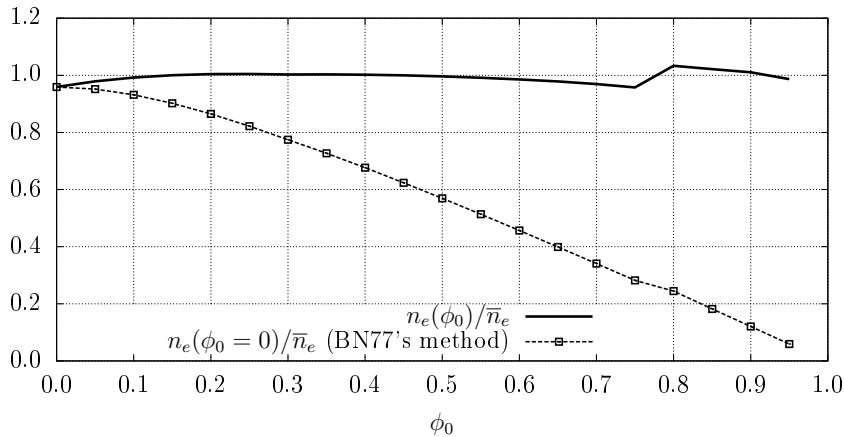


Figure 1: Performance of n_e/\bar{n}_e using the proposed equation (10) (solid line), and error incurred by the traditional BN77 analysis (dotted line and symbols).

with

$$\phi \equiv \frac{h}{H}, \quad (2)$$

$$\eta \equiv \frac{x}{B}, \quad (3)$$

$$\tau \equiv \frac{k_0 H}{n_e B^2} t. \quad (4)$$

Before anything else, we checked (with excellent results) the numerical solution against the only known analytical solution of the nonlinear PDE, which is valid for $\phi_0 = 0$, as was obtained by Boussinesq [1904]. We do not show the comparison here, but it is available upon request. Then we varied ϕ_0 from 0 to 0.95 in increments of 0.05. For each ϕ_0 , a Brutsaert-Nieber recession analysis was performed and k_0 and n_e were estimated against their true values \bar{k}_0 and \bar{n}_e using Equations (10) and (11). The evident advantage of the dimensionless form is economy: one need not “vary” k_0 and n_e (which are kept at nominal unity values, as well as H and B), but only ϕ_0 : all that matters are the ratios of the estimates, namely n_e/\bar{n}_e and k_0/\bar{k}_0 . In order to be consistent with the estimates given by equations (10) and (11), the slopes of the recession analyses were fixed at $\beta_1 = 3$ (early time) and $\beta_2 = 1$ (late time), and only α_1 and α_2 were estimated using a nonlinear Levenberg-Marquardt least squares method.

We now re-plot our former Figure 2 in the first version of the manuscript as two new figures: one for n_e and the other for k_0 (which are Figures 1 and 2 in this reply, but correspond to Figures 2 and 3 of the current version of the manuscript).

Notice the change in the choice of (for instance) k_0/\bar{k}_0 instead of the former $k_0(\text{BN})/k_0$, which avoids the log scale of Figure 2 and allows a more clear picture to emerge. Moreover, while our Figure 2 was actually the ratio of two *estimates*, we are now able (after our numerical simulations) to plot the results against true known values \bar{n}_e and \bar{k}_0 .

As can be seen, the k_0 estimate using the original equations remains “robust” up to $\phi_0 = 0.4$ approximately. On the other hand, there is a more or less linear trend in n_e estimated with the original equations all the way from $\phi_0 = 0$. Our modified equations (10) and (11) give estimated values of k_0 and n_e that differ very little from the true ones for the whole ϕ_0 range, and as such represent a considerable improvement over the original equations.

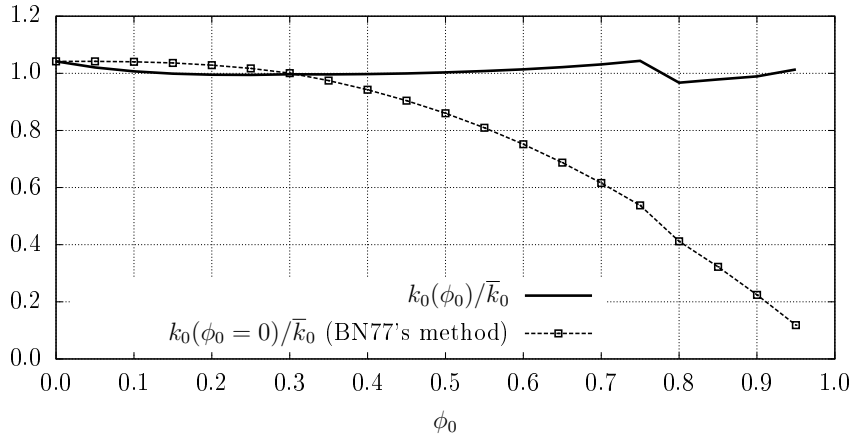


Figure 2: Performance of k_0/\bar{k}_0 using the proposed equation (11) (solid line), and error incurred by the traditional BN77 analysis (dotted line and symbols).

The small kinks between $\phi_0 = 0.7$ and $\phi_0 = 0.8$ are an artifact of the choice of the range of the streamflow Q for fitting α_1 and α_2 used in the recession analysis. This (to the best of our knowledge) is still a subjective part of the BN77 analysis: the ranges were chosen to fit the *recession* plots $dQ/dt \times Q$ reasonably well, but they were not “fudged” to “optimize”, in any way, the estimated k_0 and n_e . Our recession data are also available so that these results can be verified independently.

The last two paragraphs (with a few modifications as well as a brief introduction about the numerical experiment) were added in lines 158–175 of the main document. Line 10 in the abstract was also modified to include this new comparison.

3 Answer to Referee #1

We thank again Reviewer # 1 and address his specific comments.

Section 1: “... can be compared to the predictions from analytical solutions...” The authors may want to extend this paragraph by showing explicitly which analytical solutions they have in mind and how they are used.

The following replaced the previous sentence in line 36:

can be compared to the predictions from the above-mentioned analytical solutions by Polubarinova-Kochina [1962], Boussinesq [1903] and Boussinesq [1904], among many others [see Rupp and Selker, 2006].

And the explicit reference to the solutions are made in line 40 with the following text:

In this work, the solutions used are the ones by Polubarinova-Kochina [1962] and Boussinesq [1903]

Section 1: “... does not account for that case.” and “... is not strictly true ...” The two statements seem to contradict each other and need clarification.

Paragraph in lines 39–43 was replaced by:

If one wishes to estimate only the soil hydraulic conductivity k_0 and the drainable porosity n_e , two of the three aforementioned solutions can be used. In this work, the solutions used are the ones by Polubarinova-Kochina [1962] and Boussinesq [1903]. However, the solution by Polubarinova-Kochina [1962] is only valid for the case $H_0 = 0$: it is therefore important to assess how much this assumption affects the estimate of k_0 and of n_e for cases where it does not hold.

Section 2: As the solutions of Chor (2013) and Dias (2014) are essential in this paragraph it may be worth noting the equations together with one or two sentences of explanation. This will give the reader the possibility to focus on the text rather than getting distracted by consulting the references to understand what follows.

Text in line 85 was extended until line 96 with:

where $h(x, t)$ is the water table height, x is the horizontal distance from the water stream and t is the time. Under the above change of variables, the Boussinesq equation is reduced to the dimensionless ordinary differential equation

$$\frac{d}{d\xi} \left(\phi \frac{d\phi}{d\xi} \right) + 2\xi \frac{d\phi}{d\xi} = 0 \quad (5)$$

together with the boundary conditions $\phi(0) = \phi_0$ and $\phi(\infty) = 1$. Due to the second boundary condition, the solution is only valid for the initial phase of aquifer drawdown. For $\phi_0 = 0$, as already noted, the solution by Polubarinova-Kochina [1962] suffices for the BN77 analysis; for $\phi_0 \neq 0$, a series solution of the form

$$\phi(\xi) = \sum_{n=0}^{\infty} a_n \xi^n \quad (6)$$

has been proposed by Dias et al. [2014], with a recursion relation for the a_n 's. An important result in that work is an empirical equation, fitted to numerically obtained values of a_1 in the series above, for the value of ψ_0 , defined below. This is given as equation (12) in the present work.

After eq. 12: Where do the numerical values for eq 12 come from? Please clarify.

Text below was added in line 152–157.

As explained in Dias et al. [2014], even after a general recursion relation for the a_n 's in (6) has been obtained, the values of the a_n 's still cannot be obtained analytically, essentially because the series' radius of convergence is limited so that the boundary condition $\phi(\infty) = 1$ cannot be imposed analytically. Instead, they must be obtained numerically with the aid of numerical solutions of (5). The coefficients above have been obtained in Dias et al. [2014] by curve fitting with a large number of numerical solutions.

4 Answer to Referee # 2

We once again thank the referee for his comments. It appears to us that the Referee believes that the Brutsaert-Nieber analysis is now somewhat outdated, or rendered inapplicable, due to recent findings.

We believe that most of the criticism by the Referee can be traced back to the paper by Rupp and Selker [2006]: because it is well known by us, and because it is already in our list of references, we would like, in the following, to argue on the basis of this reference.

First of all, we realize that our choice of words may lead to the optimistic impression that our results are all that is needed to “fix” the BN77 recession analysis. We know better than that, and are ready to admit that the issue of $\phi_0 \neq 0$ that we address in this manuscript does not, by any means, exhaust the subject. [We make that clear now in the sentence in line 68.](#)

As Rupp and Selker [2006] argue convincingly, there are at least two other issues that can compound the difficulty of BN77 recession analyses considerably: steep slopes and the k_0 dependency on aquifer depth h . The latter leads to an even more general non-linear equation than (1).

We also note, however, that exactly as we do here, Rupp and Selker [2006] resorted to numerical simulations. This may well have been chosen wisely, as (sadly!) real field data are bound to complicate the picture even more with measurement error, the existence of many more flow components contributing to the measured streamflow, complicated geometry, etc..

At any rate, Rupp and Selker [2006] results do not by any means sound a death knell on BN77. In particular, we call attention to their conclusions in paragraphs 60 and 62, reproduced in part below:

§ 60 “A definition for the recession parameter a was also derived for late time, meaning that in theory the Brutsaert and Nieber method can be used to determine the hydraulic properties of a sloping aquifer.”

§ 62 “In the case of a horizontal or very mildly sloping aquifer, Szilagyi et al. [1998] found the assumption of a representative single rectangular aquifer to be robust, based on numerical solutions of the 2-D Boussinesq equation in a synthetic catchment. The general shape of the recession slope curve for catchment discharge was similar to that for discharge from a 1-D rectangular aquifer, though with a smoother transition between the early and late time domains. Furthermore, the basin-scale hydraulic and geometric aquifer parameters were reasonably estimated by recession slope analysis using (2), including cases where the saturated hydraulic conductivity varied across the catchment. As of yet, however, we are not aware of numerical experiments similar to that of Szilagyi et al. [1998] for catchments composed of hillslopes of moderate to steep gradient.”

In short, it seems to us that these remarks: (i) do not by any means consider BN77’s idea to be discarded, but only call our attention, very correctly, to possible complicating factors and (ii) also call our attention to the fact that there is good evidence supporting the approach for mild slopes.

None of the aforementioned complications dishearten us, and on a fundamental level we believe that they should not be used as arguments against conducting research using simplifying assumptions. In our manuscript, we chose a very simple approach: we studied a zero-slope, constant k_0 aquifer. These assumptions may, to varying degrees, not correspond to real watersheds, and we are more than ready to acknowledge this. Of course, such caveats will be incorporated in the manuscript if it is accepted. On the other hand, these simplifying assumptions allow us to concentrate on the issue of how $\phi_0 \neq 0$ affects the estimates of the physically-based parameters k_0 and n_e . Again, we would like to argue strongly, but cordially, that the simplifications are valid if one is — for the first time — investigating a new issue.

We made some changes in the text to make it more in accordance with this discussion. The changes can be seen in blue in lines 7–8, 179–181. Some of the issues raised here were also addressed openly in lines 48–78.

5 Answers to editor’s comment

We thank the editor once again.

Following his request, we included a brief summary of the current use of the BN77 analysis, wrote a few more lines and rephrased some sentences in order to address the issues raised by reviewer 2. These changes can be seen beginning in line 48 and that extends until line 78, and also on the already mentioned lines 179 to 181.

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