In this brief manuscript, the authors (LZZL) present a new semi-analytical solution for flow to a slanted well (including horizontal and vertical) of zero radius in a uniform anisotropic unconfined aquifer considering unsaturated flow above the water table. Details of the solution are included in supplementary material and appear to be correct. The underlying conceptual-mathematical model is similar to that of Tartakovsky and Neuman (2007) for a vertical well, with the exception that LZZL take the unsaturated zone to be finite (TN took it to be infinite). Their method of solution is somewhat different from that of TN. The authors evaluate their solution numerically and present it in the forms of time-drawdown curves in the saturated zone, synoptic profiles of dimensionless drawdown in both the saturated and the unsaturated zones, and flow rate across the water table, for a range of dimensionless parameters, concluding that the unsaturated zone has a significant effect on system behavior in all cases.

I find the paper to be clearly written and the mathematics well explained. I do, however, have a few fundamental questions to the authors:

1. LZZL are aware that the conceptual-mathematical model of TN has been superseded by a more general model due to Mishra and Neuman (2010, 2011). In addition to having rendered the unsaturated zone finite (as do LZZL), the most important extensions introduced by MN are representations of unsaturated material properties by 4 (instead of 2) parameters, in a manner similar to that of Mathias and Butler (2006); accounting for storage in the pumping well (rather than treating this well as a line sink); and accounting for delayed response of piezometers and observation wells due to storage in these devices. TN have demonstrated that the four-parameter representation leads to more realistic estimates of aquifer parameters, based on observed drawdowns, than does the two-parameter model. They also demonstrated that storage of water in pumping and observation wells have significant impacts on drawdowns below and above the water table. My question to LZZL: Why have you not worked with a four-parameter model, and why have you not accounted for pumping and observation well storage?
2. MN have demonstrated that the unsaturated zone may or may not have a significant impact on drawdown below the water table depending on the choice of system parameters and mode of observation. My question to LZZL: What justifies your blanket statement that the unsaturated zone has a significant effect on system behavior, without any qualifications?
3. MN have shown time-drawdown curves for the unsaturated zone; why have LZZL not done likewise? New developments in unsaturated zone sensor technology will likely make it practical, in the not too distant future, to observe unsaturated zone behavior at depth and use the MN (or LZZL) solution to interpret such observations quantitatively.
4. MN have used their solution to analyze published pumping test results, demonstrating (as already noted) that their new model provides more realistic parameter estimates than did earlier models, including that of TN (and hence, I conclude, LZZL). Why have LZZL not done the same?
5. MN have shown that their solution allows estimating unsaturated zone properties based on observed drawdowns in the saturated zone. Why have LZZL not attempted to do the same?

In summary, the LZZL solution is interesting in that it is the first to consider slanted wells in the unconfined aquifer context. It however rests on a somewhat limited and outdated conceptual-

mathematical model of the system; would it be possible for the authors to remedy this? In any case, the authors should show graphically how the unsaturated zone responds to pumping and, if at all possible, use their model to analyze real (or at the least synthetically generated) pumping test data.

Review by Shlomo P. Neuman