<u>Comments from Reviewer 1 and Authors' replies</u>

We thank Reviewer 1 for a very detailed review that raises several points that can be addressed in a revised version of this paper. We have separated the original text by Reviewer 1 into distinct, enumerated comments to facilitate our replies. The original comments are listed below, and our comments (including changes to the text) are provided in bold. Unless otherwise noted, all citations refer to papers referenced in our *HESSD* paper.

Comment 1. This manuscript addresses the point that short-term analyses of stream temperature sensitivity do not account for long-term responses of groundwater (and discharge to streams) to increasing air temperatures. The manuscript nominally treats groundwater temperature sensitivity to climate change as an eventuality rather than sensitivity, drawing on the rough equivalence between shallow groundwater temperature and mean annual air temperature. This is probably an important point, given the number of papers saying that groundwater dependent streams might be less sensitive to climate change.

Reply to Comment 1: This statement generally summarizes the content of our paper, and we agree that the topic is important given the large number of emerging papers that do not consider groundwater warming when projecting future stream temperatures.

However, our *HESSD* paper does not suggest that mean annual air temperature and shallow groundwater temperature are equivalent. On the contrary, we note in several locations, that *land surface temperature* drives shallow groundwater temperature (e.g., P12578, L6; P12579, L2, 9 and 16; P12582, L17; P12584, L6; P12586, L4). Furthermore, the presented solutions do not merely suggest an equivalency between land surface and groundwater temperature but rather account for both damping and lagging effects. We acknowledge that Figure 2 may be a bit confusing in this context (i.e. air temperature trends are presented rather than surface temperature trends), but as we indicate on P12586, L20-25, this approach is using air temperature trends to represent surface temperature trends. This is usually considered to be reasonable in the absence of snowpack evolution.

Also note that we use the term 'sensitivity' in the same manner as stream temperature analysts who have applied the concept of stream thermal sensitivity proposed by Kelleher et al. (2012, *HP*) to project decadal scale stream sensitivity.

<u>Changes</u>: We will include an additional statement in Section 2.4 indicating that air temperatures are used as a proxy for surface temperatures in this context and that the point is simply to illustrate the application of the equations.

Comment 2: However, the point [see above] is not novel; it has been made before on several occasions, mostly by the same authors. Here are quotes from the abstracts of two of the papers (using the citations from the manuscript):

"The simulated increases in future groundwater temperature suggest that the thermal sensitivity of baseflow-dominated streams to decadal climate change may be greater than previous studies have indicated." (Kurylyk et al, 2013)

"Thus, the simulations demonstrate that the thermal sensitivity of aquifers and baseflow-dominated streams to decadal climate change may be more complex than previously thought. Furthermore, the results indicate that the probability of exceeding critical temperature thresholds within groundwater-sourced thermal refugia may significantly increase under the most extreme climate scenarios." (Kurylyk et al., 2014a)

Reply to Comment 2: We agree that the Kurylyk et al. (2013, *HESS*) and (2014a, *WRR*) papers above have also addressed groundwater warming in response to climate change. However, the thermal sensitivity of shallow groundwater to climate change remains a very under-researched topic in comparison to surface water warming. Although dozens of papers have been published that consider surface water warming in response to climate change, to our knowledge, only papers involving authors of this manuscript have directly addressed the potential of groundwater warming to produce additional stream warming. There is also a difference in focus between this study and the two studies listed above. For example, as the quote selected by Reviewer 1 indicates, the Kurylyk et al. (2014a, *WRR*) paper primarily addresses how groundwater-sourced thermal refugia (discrete groundwater discharge points) will warm in response to climate change and how those changes could influence thermal diversity in rivers. The Kurylyk et al. (2013, *HESS*) paper primarily considers (1) land surface temperature changes due to climate change and snowpack evolution and (2) the empirical relationship between seasonal surface and groundwater temperature. Only 1 short paragraph in Kurylyk et al. (2013) (see P2713) addresses the differences between short term and long term groundwater sensitivities. This is the phenomenon that is the focus of the present study.

There are many unique facets of the present *HESSD* paper. The most important are:

1. **This contribution presents and demonstrates the utility of analytical solutions that can be readily implemented by other researchers for different climates and aquifer configurations. The spreadsheet is provided as an electronic supplement to allow others to readily implement these solutions. Conversely, the results presented in the 2013 *HESS* and 2014 *WRR* papers are site specific. The methods suggested in these**

previous papers require either extensive monitoring of surface and subsurface thermal regimes (to parameterize the empirical function proposed in the 2013 *HESS* paper) or detailed numerical modeling that requires extensive hydrogeological expertise, numerical codes, and several months of pre-processing, simulations, and post-processing.

Thus, the two previous papers identified a weakness in stream temperature papers in that many do not consider groundwater warming; however the results are not easily transferable. Here we provide a first-order approach for considering groundwater warming that can be applied without the limitations noted above. We expect that these solutions will be used in future studies to estimate groundwater warming and the associated changes to streambed heat fluxes in deterministic stream temperature models. These solutions can also be used to gradually adjust the coefficients found in empirical stream temperature models. For example, the possibility of altering coefficients in regression-based stream temperature models to account for long term groundwater warming is investigated in more detail in a recent paper by Synder et al. (2015). Our revised manuscript will discuss how the concepts from this paper can be integrated with the concepts proposed by Snyder et al.

Snyder CD, Hitt NP, Young JA. 2015. Accounting for groundwater in stream fish thermal habitat responses to climate change. *Ecological Applications* Published online, DOI: 10.1890/14-1354.1.

2. **A large portion of this study (e.g. Section 1, 2.3, 2.7.1, and 3.2 as well as Figs. 3a, 3b, and 7) addresses the influence of land cover disturbances on groundwater temperature. Kurylyk et al. (2013, *HESS*) and Kurylyk et al. (2014a, *WRR*) do not include discussion on this important topic.**

3. **Although, the analytical solutions (Eqs. 5, 11, 13, and 15) are modified from previous publications, the analytical expressions for *groundwater thermal sensitivity* in response to long term climate change or land cover disturbance (Eqs. 16-19) have not been previously proposed. These expressions facilitate the comparison to short term (i.e., the seasonal damping factor, Eq. 8) and long term groundwater thermal sensitivity and thus very clearly illustrate the limitations of employing short term stream thermal sensitivity to infer long term stream warming. They also allow the user to investigate thermal sensitivity of groundwater to a few parameters (e.g. depth, time, thermal diffusivity, and groundwater velocity), and thus a range of results based on the parameter uncertainty, can be readily obtained.**

4. **This contribution contains far more discussion on streambed heat fluxes (e.g., Fig.1, first 2 paragraphs of introduction, and P12604, L12-25), their influence on surface water temperature in upwelling streams, and their dependency on groundwater temperature. The intent of this text was to more clearly demonstrate the interrelationships between climate change, groundwater temperature, and stream temperature. This is also reflected in the title.**

<u>Changes</u>: Changes in response to Comment 2 are included within our response to Comment 3.

Comment 3: More thorough reading of the papers shows very similar discussion, figures, and conclusions about the inappropriateness of ignoring groundwater warming when considering climate change impacts.

Response to Comment 3: We agree that the text Section 3.4 contains similar concepts similar to previous papers, although we note that new ideas and comparisons to new studies are presented in this section. It could be argued that Figs. 1 and 2 review material that is similar to previous papers, but these are merely introduction/methods figures rather than results/discussion figures. The other figures presented are unique from previous contributions and demonstrate the utility of the analytical solutions and thermal sensitivity expressions.

<u>Changes</u>: We will alter the introduction to clearly explain how this contribution differs from the other papers noted by Reviewer 1. In particular, we will emphasize that the main objective is to present the analytical solutions and associated groundwater thermal sensitivity expressions and to demonstrate their utility. Section 3.4 will be slightly reduced to limit redundancy with these previous papers.

Comment 4: Note that the current manuscript still only simulates aquifer temperatures, not stream temperatures, so does not go much beyond these and the related earlier papers in pointing out the potential additional warming.

We agree that a study that considers the thermal regimes of aquifers and stream holistically would be a useful contribution. However, this manuscript is already long and we believe that there is sufficient content in this manuscript to warrant publication without considering surface water thermal regimes directly. This paper goes beyond earlier papers by providing surface water temperature modellers with a set of equations that can be applied to estimate future groundwater warming. Other unique aspects of this paper are listed in our response to Comment 2.

Comment 5: The arguments presented in the current manuscript rely on analytical solutions of the conduction-advection equation (the commonly used version with constant diffusivity and velocity), whereas previous papers have used numerical models to estimate the effects of climate change on groundwater temperatures. What new information is learned from applying analytical solutions instead of numerical solutions?

Response to Comment 5: To our knowledge, the Kurylyk et al. (2014a) paper is the only previous study to apply numerical methods to study shallow groundwater warming in response to climate change. Thus, we do not believe that the present study addresses a scientific question that has been thoroughly investigated using numerical methods. Numerical models generally require extensive subsurface data for parameterization, modeling expertise, and time. For example, the modeling presented in the Kurylyk et al. (2014a, *WRR*) required very lengthy simulations, and even these were based on idealized aquifers. The findings of the *WRR* paper were valuable, but it is unlikely that these resources would typically be dedicated to a study of a stream temperature response to climate change. Conversely, the spreadsheet provided in the electronic supplement allows the user to quickly conduct a simple sensitivity study to consider a range of potential groundwater warming based on relatively few input parameters.

Also, the mathematical forms of the solutions allow for the derivation of the groundwater sensitivity formulae (Eqs. 16-19). These analytical expressions illustrate the difference between the subsurface thermal response to short term and long term surface temperature changes. These expressions can be applied to test how system conditions (depth, time, etc.) can influence subsurface thermal sensitivity.

Comment 6: There are some technical points that leave a little confusion as well. They briefly mention one issue with snowpack, where shallower snowpacks can actually lead to cooler ground surface temperatures in part of the season. In addition because of the latent heat of fusion, the snowpack pins temperatures to near 0˚C for a portion of the season (getting shorter under a warming climate of course), and much water input (the downwelling contribution) still occurs at or near freezing. Wouldn't this mean that even if the winter temperatures are warmer, the "mean temperature" of the ground may not shift as much as the mean of the air temperatures for the year. Despite noting a few concerns with how one would factor snow cover into the proposed conceptual model for groundwater temperature, the authors are critical of work from areas with substantial snow cover. Is this really appropriate, or should the authors be a little clearer about where they can make such inferences and where they cannot?

Response to Comment 6: The boundary conditions for these solutions are at the ground surface not the atmosphere, and thus they must be driven by land surface temperature. In many areas, mean annual air temperature and surface temperature trends are coupled. In fact, the whole field of borehole paleoclimatology is predicated on this assumption. Reviewer 1 is correct in noting that snowpack evolution can decouple trends in mean

annual air and ground surface temperature, and this is discussed in far more detail in Kurylyk et al. (2013).

In snowpack-dominated regimes, a surface energy flux model should be applied to consider the influence of changing air temperature and snowpack evolution on surface temperatures. These surface energy flux models are typically easy to run and can reproduce measured surface temperature data under snowpack conditions (see Fig. 4, Kurylyk et al. 2013, *HESS*). Surface energy models are parameterized/driven with more commonly available data (e.g., leaf area index, latitude, precipitation) than subsurface models. Each simulation in surface energy flux models can typically be performed in seconds as opposed to the days required to run each subsurface numerical model simulations presented in Kurylyk et al. (2014a). The need for surface energy budget considerations is alluded to on P12603, L10-17.

Kurylyk et al. (2014, WRR) found that the latent heat of fusion in seasonally freezing soils did not influence the rate of decadal scale groundwater warming (although in permafrost soils this would be the case).

<u>Changes:</u> We will enhance the text on P12603 to more clearly explain the approach for obtaining surface temperature trends in snowpack dominated regions.

Comment 7: There is a non-constant water velocity through the course of the year, and most of the analytical solutions (and the initial equation used) are derived based on a nominally constant velocity. In many places in the world, recharge is seasonal. In particular in snowpack dependent climates, the recharge is associated with near 0◦C meltwater. This only means that the approximations are off, and does not broadly contravene the conclusions, but it would dampen the degree of effect in some situations.

Response to Comment 7: We agree, to an extent, and acknowledged this on P12605, L8-11. The primary author is currently conducting a study of how seasonal recharge impulses influence shallow groundwater temperature. The analytical solutions assume that recharge enters the subsurface at the mean annual land surface temperature. In regions where snowmelt dominates recharge, the mean annual recharge temperature is less than mean annual surface temperature. Conversely, in regions where recharge tends to be dominated by irrigation during the warm season, the recharge temperature is higher than mean annual surface temperature. However, preliminary numerical modeling results suggest that the resultant influence of recharge pulses on the seasonal groundwater temperature or the rate of shallow groundwater warming tends to be minimal. For example, numerical model (SUTRA) runs have been conducted assuming saturated sand thermal properties, a typical maximum basin recharge rate (25 cm/year), a seasonal range of surface temperature of 30°C, a linear surface warming of 5°C per century, and that the entire recharge pulse occurs during the coldest month. The difference in the groundwater

warming produced by the numerical model compared to that predicted by the Taniguchi et al. (1999) solution (assuming constant recharge) is about 10% in the upper 40 m. The results are presented on the following figuree. Note these simulations were run for deeper conditions and thus contain a thermal gradient. This is the extreme case, and for most basins, the influence would be less as recharge events still occur in the summer and fall. Thus, this appears to exert minor control on the rate of groundwater warming, at least for typical basin conditions.

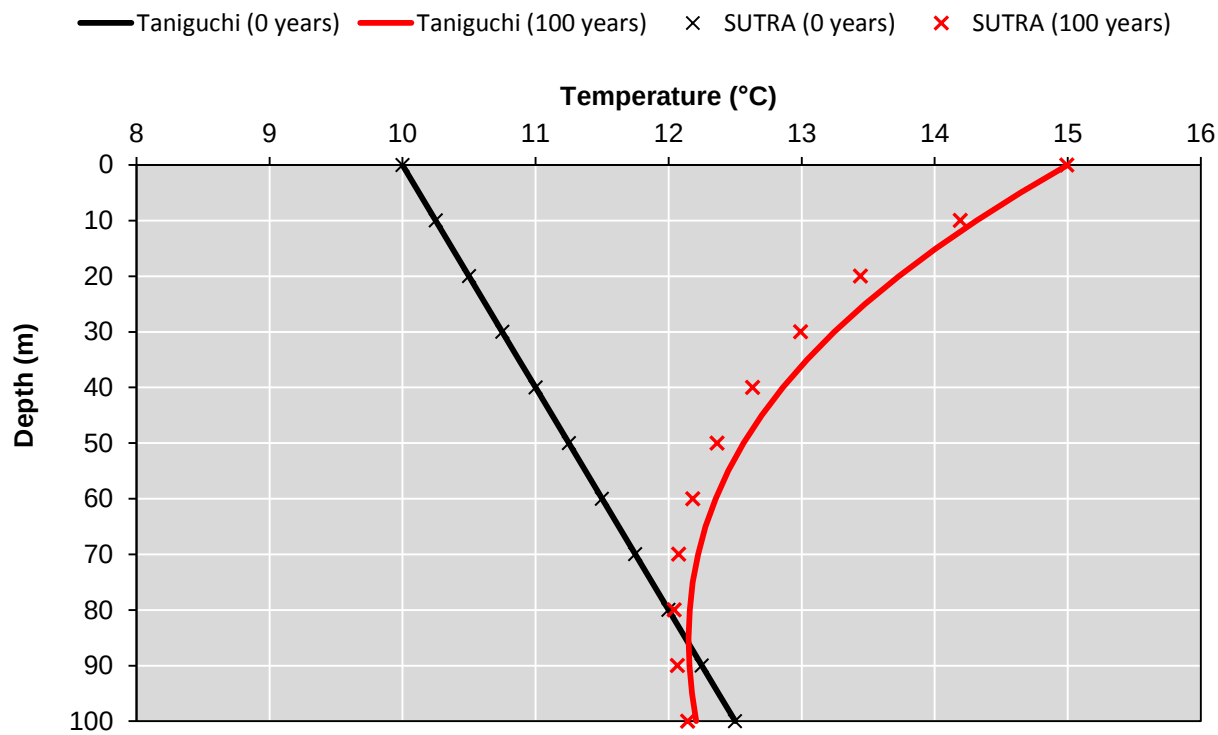

Figure: Temperature-depth profiles for 0 years and 100 years (due to 5°C per century linear warming) simulated with SUTRA and the Taniguchi et al. (1999) solution. The only difference between the results is that the Taniguchi et al. (1999) assumes constant recharge, whereas the SUTRA runs accommodate seasonal recharge at the ground surface (all recharge enters SUTRA domain during coldest month).

<u>Changes:</u> In flashy karst aquifers, groundwater may not have time to thermally equilibrate with the surrounding rock, and hence recharge seasonality would have more influence on groundwater discharge temperature. We will include a sentence acknowledging this in our limitations sections.

Comment 8: Why did the authors apply a recharge rate of 0.2 m/yr to generate figure 5? Shouldn't this be on a par with runoff? Is this just an estimate of the recharge to deeper

groundwater systems? If it were higher, deeper layers would respond more rapidly. This does not seem like it would be a substantial issue for the arguments presented, but the seemingly small recharge rate leaves one asking the question.

Response to Comment 8: This recharge rate was applied for all results (Figures 5-8). This is a typical basin recharge rate. In fact, for many surficial aquifers the recharge rates are much lower than this. For example, Döll and Fiedler (2008) estimated global (minus Antarctica) terrestrial recharge to be 12,666 km^3/yr. Dividing through by the land area of 135,000,000 km^2 (149,000,000 – 14,000,000) yields a mean annual recharge rate of about 10 cm/yr.

We agree that faster recharge rates would lead to more rapidly transmitted surface temperature signals. However, it is uncommon for recharge rates to greatly exceed 20 cm/yr except during intense irrigation or when water is sourced from a draining surface water body.

<u>Changes:</u> We will include a reference to Döll and Fiedler (2008) to justify our choice of recharge.

Döll, P. and Fiedler, K. 2008. Global-scale modeling of groundwater recharge. *Hydrol. Earth Syst. Sci.* 12: 863-885.

Comment 9: An additional point of noting these approximations used by the authors is that the models they apply have error as well. So if the work ignoring the groundwater effects is an approximation to some order, then the authors are not, per se, correcting these, but improving the order of error of the approximation (one hopes that is the case, in any event, but it has only been argued not demonstrated). In the context of improving projections of future temperature, then, is the additional effect noted here a small term in the overall uncertainty in future stream temperatures or a large term?

Response to Comment 9: Any thermal modeling of hydrologic systems invokes assumptions. Also, introducing model complexity (e.g. such as considering groundwater temperature warming in stream temperature models) never eliminates error but only improves the approximations. We agree with this point, but the same statement could be made of most (if not all) modeling studies of river, stream, or aquifer thermal regimes that incorporate some degree of increased complexity in comparison to previous studies.

The degree to which ignoring groundwater warming influences the overall uncertainty in stream temperature projections is dependent on many things such as the stream canopy, local climate, degree of groundwater contribution to stream, and time. Thus, this must be investigated using stream temperature models and is outside the scope of this study.

Comment 10: In summary, the general point is good to note, but it seems repetitive considering earlier work by the same authors. The current manuscript almost seems to present a weaker argument than in the earlier papers. The manuscript presents a strictly modeling exercise, and as such lays out a good hypothesis, but it is presented as a one-sided debate, where the authors do not really challenge their hypothesis so much as advocate it.

Response to Comment 10: As we note in our response to Comment 2, the previous papers referred to by Reviewer 1 essentially demonstrated that shallow groundwater temperature can be very sensitive to climate change. The main objective of the present study is to equip stream temperature modellers with equations that can be applied to overcome the limitations of ignoring shallow groundwater warming (see Objective 1). The results are illustrative and are primarily intended to demonstrate the utility of the solutions. Hence, Objective 2 will be amended to reflect this. It is a bit difficult to challenge the hypothesis that shallow groundwater will warm in response to climate change. As we note in our introduction (paragraph 6), this has been shown with field data as well as physically based models. Our intention is not so much to advocate this hypothesis as it is to demonstrate how known analytical solutions can be applied to consider groundwater warming in stream temperature models. Thus this is more of a 'methods' paper.

<u>Changes</u>: We will rewrite the objectives to be clearer regarding the point of this contribution. In particular, we will use objective 2 to indicate that our results are merely illustrative in nature and intended to demonstrate the utility of the solutions.

Comment 11: On the net, the argument has a certain irony as well. The authors complain about lax assumptions of quite a few other works, but end up using a number of rough approximations themselves. They argue that these rough approximations are better than ignoring the problem (which may well be true), but we have to take their word for it.

Response to Comment 11: It is still very common to study subsurface thermal regimes using analytical solutions. For example, most recent papers in borehole paleoclimatology employ analytical solutions to the 1D conduction equation. Also, many studies in aquifer thermal energy storage or borehole heat exchangers employ analytical solutions to a similar governing equation as this study, albeit in radial coordinates. The continued use of these solutions is partially due to the fact that the variability in subsurface thermal diffusivity is generally constrained in comparison to aquifer hydraulic diffusivity. Also, conduction is usually more important in subsurface energy transfer than advection, and this tends to minimize uncertainties induced by temporal or spatial variation in groundwater advection. The equations are also derived from understood physical processes, and are not merely arbitrary functions based on some correlation between air and subsurface temperature (see Comment 1). Thus, using these solutions is better than the two common alternatives for considering groundwater temperature response to climate change in stream temperature models: (1) groundwater temperature is completely resilient

to climate change or (2) changes in groundwater temperature equal changes in air temperature without consideration of surface temperature, damping, or lagging. As we note in our response to the last comment below, the recent paper by Menberg et al. (2014) illustrates these points using measured decadal trends in groundwater temperature.

<u>Changes</u>: Two sentences/paragraphs have been altered to be less critical of how groundwater temperature changes have been considered in previous studies (e.g., statements originally found on P12576, L27 to P12577, L2 and P12601, last paragraph). We are amenable to altering other critical statements, but these must be clearly identified to us.

Comment 12: Section 2.2 (specifically equations 4 & 5) and Section 3.1: Stallman (1965) attributes equation (5) to Suzuki (1960), which makes quite a bit of the language in these sections a bit awkward. Equations 6 and 7 are irrelevant to this paper, and are solutions to the inverse problem of finding downwelling infiltration rates. If one were going to attach a name to equation (5), Suzuki (1960) seems more appropriate, although I am unfamiliar enough with the literature to know whether there is an earlier solution. It would not be surprising, however. Equations 6 and 7 are most appropriately attributed to Stallman, but they are not used in this paper.

Response to Comment 12: We agree that Suzuki (1960) proposed the original form of Equation 5, but Stallman (1965) provided more accurate expressions for obtaining *d* and *L* (Eqs. 6 and 7). Hence, these equations (5-7) are often collectively referred to as 'Stallman's solution'. It is not true that Eqs. (6) and (7) are irrelevant to this paper and are only useful in the inverse solutions. Eqs. (6) and (7) can be applied in a direct manner to parameterize Eq. 5 (see *L* and *d*). Indeed this is the only way to obtain the solution presented in Eq. (5) using commonly available data. Hence. Eqs. (6) and (7) were used to generate Figures 5 and 6 and are integral to the results and discussion of this paper.

<u>Changes</u>: We will include a citation to Suzuki's (1960) paper before Eq. (5). The caption for Figure 5 will be amended to clearly indicate that Eqs. (6) and (7) were used.

Comment 13: 12602 Lines 3-4: criticize the use of time series of two decades length on the basis that groundwater could take a century to respond, but at the same time on 12577, lines 2-7 the authors are critical of papers suggesting long lags in groundwater response. It gives the impression that they are arguing in the introduction that the lags are short enough that it should be considered a more important process, but then they discount long term sensitivity work for not considering a long enough lag. In a similar vein they criticize another paper that deals with a very similar topic (Meisner 1988) for not considering the lag at all, but nominally treating the groundwater increase as an eventuality as well. All of this comes across as inconsistent. Perhaps a different tone, recognizing that most of the previous work is built on approximations, and that the current work is yet another set of approximations extending the earlier approximations would create a text that does not look internally inconsistent.

Response to Comment 11: Our point is that one cannot assume (1) the lag = 0 years or (2) the lag = 100's of years. Rather the lag should be determined using a physically based approach, and we present and demonstrate such an approach. There is another distinction here as well. The paper by Chu et al. (2008) assumes centuries before any response, whereas we show that the response increases over time and may realize its 'full potential' after 100 years.

<u>Changes:</u> We will include a sentence explaining this in Section 3.4. In our response to Comment 11, we indicate that our tone will be altered in a couple of places.

Comment 14: Again it ties back to thinking in terms of degrees of error propagating from climate models through to ground surface temperatures, groundwater temperatures, and ultimately stream temperatures. This would involve the use of data to substantiate their hypothesis and demonstrate that it is a sizable effect. Based on my reading of the literature, I would guess that analysis of observed data would put their work in a very favorable light.

Response to Comment 14: Wee agree, and such a study was published in *HESS* in late November (Menberg et al., 2014). That study compared measured groundwater temperature warming over several decades to results obtained with an analytical solution to the same governing equation that is employed in this study. The analytical solution performed very favourably for the wells with the most data (see Figure 4, Hardtwald 1 and 2, Menberg et al. 2014). The inter-annual variability in the groundwater temperature stemmed from a seasonal sampling bias. There was no surface temperature warming data available for the other two wells, and this likely resulted in the underestimation of the Sinthern warming (Fig. 4). We will include a statement and citation in the limitations section indicating that these methods have been shown to generally match measured subsurface warming trends.

http://www.hydrol-earth-syst-sci.net/18/4453/2014/hess-18-4453-2014.pdf

Menberg K, Blum P, Kurylyk B, Bayer P. 2014. Observed groundwater temperature response to recent climate change. *Hydrol. Earth Syst. Sci.* 18, 4453–4466.