

## ***Interactive comment on “Shallow groundwater thermal sensitivity to climate change and land cover disturbances: derivation of analytical expressions and implications for stream temperature projections” by B. L. Kurylyk et al.***

**Anonymous Referee #1**

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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. However, the point is not novel; it has been made before on several occasions, mostly

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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)

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. 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.

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?

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

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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?

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.

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.

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?

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

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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. 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.

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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.

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

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not look internally inconsistent. 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.

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