Ocean Sci. Discuss., 11, C1000–C1006, 2014
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Ocean Science
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Interactive comment on "Reconstructing bottom water temperatures from measurements of temperature and thermal diffusivity in marine sediments" *by* F. Miesner et al.

Anonymous Referee #1

Received and published: 31 October 2014

This paper describes an inversion method for reconstructing bottom water temperature variations from temperature-depth measurements made in marine sediments. The authors parameterize their solution in terms of a single frequency and amplitude and then invert temperature, thermal conductivity, and thermal diffusivity, all as a function of depth, for the background thermal regime (mean, or long-term, bottom water temperature and gradient, the authors call this steady-state), the amplitude of the wave, and phase. The authors apply their algorithm to a synthetic model and to real data and argue that these parameters can be adequately estimated.

Comments are numbered below:

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1. The main point of the paper is to present an inversion scheme to reconstruct bottom water temperature variation over one annual cycle. The authors argue that this is useful for climate change studies though I would argue that one annual cycle is not nearly long enough to resolve much about climate. Further, the fact that solutions are parameterized in terms of a one-year-periodic function, discounts the usefulness for climate studies. It is likely that the bottom water is slowly warming (or cooling) in time so that a small linear trend would be added (subtracted) to the periodic annual wave. As the inversion algorithm is presently parameterized warming (or cooling) trends would not be recovered. Would this linear trend be included in the determination of the background (steady state) gradient?

The authors are limited to recovering about 1 year of bottom water temperature variation because their measurements only extend to about 6 m depth.

2. I also think this paper would benefit from being more general. The examples are based on a single probe design, constructed by FEILAX with 22 thermistors over a 6-m length. How would the results change as a function of probe length or number of thermistors? If one is really interested in the amplitude of annual cycles on the seafloor can the probe design be optimized for this problem?

3. This problem is extremely similar to that of inverting continental borehole temperatures for ground surface temperature histories. There are already quite a few inversion schemes to handle this problem and I think this paper would be better if it built on this previous work. Previous papers have already explored many aspects of this problem (noise level, tradeoffs between resolution and variance, measurement spacing, etc.) Good examples of these studies include Clow (1992), Shen and Beck (1991), and Shen et al., (1996) among others. What insight does this new inversion scheme give us? Is this current inversion scheme better in someway than previous inversion schemes?

Clow, G. D., 1992, The extent of temporal smearing in surface-temperature histories

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derived from borehole temperature measurements, Glob. Planet. Change, 98, 81-86.

Shen, P. Y., and A. E. Beck, 1991, Least squares inversion of borehole temperature measurements in functional space, J. Geophys. Res., 96, 19,965-19,979.

Shen P. Y., H. N. Pollack, and S. Huang, 1996, Inference of ground surface temperature history form borehole temperature data: A comparison of two inverse methods, Global Planet. Change, 14, 49-57.

4. Discounting advection. The authors are working at shallow seafloor depths where bottom water temperature variations are large. They present the full forward equation (one-spatial dimension) on page 2394 and then simplify. The authors discount the advection term, because the fluid flow is likely to be low, claiming the pore volume is rather low (line 10, page 2395). In fact porosity in the upper 6 m is typically quite high with values greater than 50% (see IODP drilling results). I agree that advective fluid flow is likely negligible, but would not rule it out a priori because of the presence of seeps, etc. However, of greater consequence in these shallow continental margin settings may be heat advection due to sedimentation effects. The sedimentation effect is not discussed but should be mentioned and criteria for when both of these effects can be neglected should be given. In the context of the problem defined by the authors (in my view overly restrictive), i.e., large seasonal amplitudes, neglecting advection is probably okay. The issue is really at deeper depths where seasonal amplitudes are significant but smaller than those addressed by the authors.

5. Section 5.1 is an analysis of how well the inversion algorithms work with synthetic data. One of the powerful attributes of inversion is the ability to assess solutions. I think this aspect is under utilized in this study and am curious about a number of aspects.

The authors add various amount of noise to the synthetic data. Although not stated I assume they are adding zero-mean Guassian noise to the temperature measurements. I do not see that the authors give the background gradient for the synthetic example or how well the inversion scheme recovers this value. This aspect is very relevant if there is also a long term warming or cooling trend. What is the impact of uncertainties in the thermal diffusivity with depth?

Line, 16, page 2400, the authors state that the geothermal gradient is a priori known, but I do not understand how this can be the case. The temperature depth profile is a combination of both the bottom water temperature variation and the background gradient. Similarly the heat transfer coefficient is not a priori known. How are these parameters known a priori?

For the synthetic example I would find the paper more compelling if a real bottom water temperature data set (say of a decade or more) were used as a forcing function at the seafloor to generate the temperature-depth profile and then inverted. How well would this more realistic case (that may or may be well characterized by a cosine) recover reasonable parameters.

One issue with seafloor temperature-depth measurements is that one only knows the absolute depth of the thermistors to within the thermistor spacing. That is you can tell whether or not a thermistor is in the sediment, but you don't know how far into the sediment the top thermistor has penetrated. With a thermistor string of 6-m and with 22 thermistors gives a thermistor spacing of about 29 cm. So the depth of any individual thermistor is not known to better than about 29 cm. Is this uncertainty included in the analysis and tested? These are clearly small uncertainties given the amplitude of the annual signals used in this paper, but will be relatively larger in regions with small (but significant) annual variation. If the background gradient is say 60 mK/m then a depth uncertainty of 29 cm translates to a temperature bias of X° C. How does uncertainties in the thermal conductivity (or thermal diffusivity) impact the solution.

How does the layering relate to the depth of thermistors. Where do layer boundaries occur?

Comments of a somewhat more editorial nature are listed below.

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8. The abstract reads more like an introduction than abstract. There is very little quantitative information here. No results or conclusions are given. The first sentence does not convey much. I do not know of any studies where marine heat flow measurements are made to document steady state heat flow as a source of energy (2nd sentence). This point is not discussed in the main text. In the second paragraph the abstract states that the aim of the paper is to reconstruct bottom water temperature variations over the past two years. This statement should come at the top of the abstract, and in reality the paper discusses reconstructions over one annual cycle, not two. On line 3, page 2394, the paper says several years, and that deeper measurements are used to reconstruct older climate history. This statement is confusing because there is no deeper data presented in this paper. The third paragraph states that an inversion operator and two common inversion schemes are used, but doesn't specify them. Is there a reason to not to specify these?

9. I think this paper could be improved by a reorganization. In that way there would not need to be so many parenthetical statements that refer to other sections. The forward model can be completely discussed before the inverse problem is introduced. Personally, I would like to see the synthetic example fully developed before moving to real data. In this way one would defer the discussion of example data (line 12, page 2397) until after the synthetic data had been analyzed.

10. I am wondering if a different terminology for steady state can be used. In marine environments the thermal field is never really in steady state.

11. Line 9, page 2393. The statement that periodically changing water temperatures propagate into the sediments to different depths is incorrect. The heat equation shows that the earth is a low-pass filter with different frequencies attenuating at different depths. This phrasing is repeated multiple times through the paper.

12. Line 15, page 2393. The statement that constant surface temperature leads to a linear increase of temperature with depth neglects heat sources or sinks, advec-

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tion, and potential changes in material properties. The second part of this sentence that heat production of the lower earth can be determined from a linear temperature increase is also wrong. Wish it were true though.

13. Line 7, page 2399. Can references be given for the determination of thermal diffusivity from the temperature decay of a heat pulse? 14. Line 9, page 2399. The statement that the in-situ thermal gradient can ‘then’ be calculated should probably come before the discussion of the thermal conductivity determination.

15. Line 18, page 2399. The authors cite the accuracy of the thermistor string just after discussing the vibrocorer system. I assume that the accuracy of the thermistor string is for both systems, but it is not clear. Also the accuracy of the thermal conductivity and thermal diffusivity need to be given since these quantities play a role in the determination of inverted parameters.

16. The paragraph starting line 1, page 2402 seems out of place.

17. Why are so many decimal places retained in the solution parameters, line 1, page, 2405, and Table 1? Are the authors really implying their results are good to tenths of milliKelvin?

18. Section 5.2, page 2405 and Table 1. Here the Fourier series is first introduced but again only a single annual cycle is estimated.

19. Section 6, line 24, page 2408. Here the authors state they are assuming a homogenous half space. I thought we were dealing with a layered media.

20. Line 2, page 2409. Here the authors state that the mathematical model in equation 6 neglects a lot of short periodic noise. I am not sure what they mean by noise in this context. Is not this part of the signal? Given that the model solves for only one period I would say it neglects a lot of short and long period information.

21. I do not understand the point of section 6.1. Here the authors are discussing the accuracy of reconstructed parameters and contend that an accuracy of less than 1 K in

their A and B parameters is all that is needed. I think this statement needs to be better supported.

22. The Conclusion section reads more like a section on future work.

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