

Interactive comment on “The hydrothermal power of oceanic lithosphere” by C. J. Grose and J. C. Afonso

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AUTHOR COMMENT OVERVIEW: We thank M. Fernandez and F. Lucazeau for their insightful comments. These comments have led to marked improvements in the clarity and explanation of the methods and arguments discussed in our paper. The most significant changes we have adopted after careful readings of the reviewer comments include: 1) We have now included a more explicit discussion of the statistical methods. 2) The reference models GH and GHC are more fully explained so that the reader does not have to read details of our earlier papers. This includes a figure showing the initial, boundary, and other conditions of models GH and GHC. 3) We have added a more elaborate discussion of the use of topography to constrain models and heat flow, including an additional figure showing predicted and measured seafloor topography. 4)

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The discussion of high-resolution heat flow surveys has been reorganized and discussions have been updated to clarify the meaning of the data and our arguments on the use of heat flow to constrain lithospheric cooling.

In the following, we respond to the detailed comments provided by the reviewers and note corresponding changes to the text of the updated manuscript.

REVIEWER COMMENTS BY M. FERNANDEZ, WITH AUTHOR RESPONSE:

This is a very interesting work in which the authors analyze the differences between measured and modelled seafloor heat flow close to oceanic ridges. These differences are interpreted in terms of hydrothermal heat transport, which appears to be less than previously estimated when using recent models of lithospheric cooling, then affecting the thermal budget of cooling oceanic lithosphere. The presented analysis is based on global seafloor heat flow datasets and on well-established oceanic cooling models by using a complete statistical study. Comparisons with regions where high-resolution heat flow surveys are available are also included. The authors conclude that differences between predicted and measured heat flow and hence, the hydrothermal activity, is significantly lower than previously thought and it is concentrated near to the ridgeaxes (<1 Ma). The presented analysis is relevant, reaching sounding conclusions. The paper is clearly written with good quality figures. My main comments-suggestions are:

General comment

My main comment is related to the modelling approach and results obtained, and it is summarized in the first conclusion, where the authors state “We have estimated the power of ventilated hydrothermal heat transport, and its spatial distribution, using a set of recent plate models which highlight the effects of hydrothermal circulation and crustal insulation. The most important conclusion of our study is that a model with both of these effects predicts that the difference between measured and modeled heat flow is significantly lower than previously thought. Consequently, the total heat vented to the

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oceans by hydrothermal circulation is lower, and the fraction of that vented is higher on ridge axes". The question is: if models incorporate hydrothermal circulation and differences between models and observations are attributed to hydrothermal activity, this implies that models are not properly incorporating such processes. Note that according to authors, if the ocean cooling models fully incorporate hydrothermal circulation and reflect perfectly the measured heat flow, then the hydrothermal power would be zero, which is paradoxical. The referred concluding sentence appears in similar ways at different parts of the article generating some confusion to the reader. The authors should comment something about in the Introduction.

AUTHOR COMMENT: We have added more discussion (with a new figure) of the theory involved in the use of the heat flow deficit to estimate the heat removed by hydrothermal ventilation. The reviewer is correct that hydrothermal circulation is not properly incorporated into the models. Specifically, passive hydrothermal circulation, and specifically the ventilation of heat is not incorporated. We only incorporate axial hydrothermal circulation. The assumption in our analysis is that passive hydrothermal circulation does not significantly change the secular cooling of the lithosphere. This may be justified by recognizing that the thickness of the aquifer in long-term circulation occurs is only a few hundred meters thick. Therefore, the heat flow deficit may be underestimated by our analysis because the true lithospheric heat loss is somewhat higher than our estimate due to passive circulation, but the underestimation is probably small unless the hydrogeology of the crust is significantly different than thought. If passive ventilated hydrothermal circulation were included explicitly in the models, a different approach would be needed to estimate the net ventilated hydrothermal power loss.

Specific comments

I suggest to describe very shortly the term 'thermal rebound correction', which as presently called in the text seems to be related to sedimentation (thermal blanketing), when actually it is related to cessation of hydrothermal circulation due to the presence

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of sediments.

AUTHOR COMMENT: We did confusedly, or at least unconventionally, refer to the heating of deposited sediment as a thermal rebound effect. However, in the updated manuscript we more consistently refer to this as a "thermal correction for sedimentation (i.e., recently deposited sediment is initially at ocean-bottom temperature and must be gradually heated by conduction of lithospheric heat)."

The term 'hydrothermal power' is also quite confusing because of: i) in general it is very related to energy resources, but not in this case; and ii) if it really denotes misfit between models and observations, then is not the most appropriate term.

AUTHOR COMMENT: We have wrestled with the idea of changing the references to hydrothermal power, including in the title of the paper, due to this confusion with the energy resources. In our work, hydrothermal power (or ventilated hydrothermal power loss) does not denote the misfit between models and observations, but does denote that which is estimated from the misfit of models and observations. We understand the concern, but we wish instead to emphasize in the work that this is "ventilated" hydrothermal power, and leave the title unchanged, as we hope it will attract a wider range of readers.

Technical corrections

- 1.- There is a problem when referring Figure 3 in the text all along Section 3. Actually, the authors are referring to Figure 2.
- 2.- Fig. 3 is properly cited for first time in Section 6.3 after Figs. 4 and 5.
- 3.- At the beginning of Section 4.1 the authors refer to GC model instead of GHC model.
- 4.- Page 1179, line 19. Add year of publication after Dunn et al.
- 5.- Figure 2: Details like symbols are very difficult to distinguish in a printed version. The Power Deficit should keep the same scale in all the panels. In the caption, should be 'Monte-Carlo' instead of 'monte carlo'.
- 6.- Figure 5: Please include some label in the figure or some text in the caption relative to the location of the region

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AUTHOR COMMENTS: We have corrected these details in the updated manuscript. We have also modified figure 2 for clarity and drawn the figures such that the power deficit uses the same scale.

REVIEWER COMMENTS BY F. LUCAZEAU WITH AUTHOR RESPONSE:

This paper suggests that the hydrothermal contribution to the oceanic lithosphere cooling may have been overestimated in the previous studies and re-evaluates the spatial distribution of the heat loss. The study is based on a sophisticated statistical analysis and on thermal models taking into account the axial hydrothermal cooling (0-0.2 Ma), better integration of petrophysical parameters and the insulating nature of the crust.

Although the topic and the results of this paper are of obvious interest, many aspects would deserve clearer presentation and more detailed explanations, especially those concerning the methodology, which is extremely hard to understand without (and even after) reading a previous paper of the authors (Grose & Afonso, 2013), but fundamental to evaluate the conclusions. The most unclear aspect is certainly the description of the statistical analysis, also complicated by a confusion between figure 2 and figure 3 in the text. In addition, the discussion on high resolution sites is oversized (7 pages, 3 figures) and should be shortened and better related to the first part of the paper. Illustrations are generally overloaded (specially figure 1 & 2), and need to be simplified or separated in several parts.

AUTHOR COMMENT: In our updated manuscript, we have provided a more complete and explicative discussion of the statistical methods, as well as the derivation of the models adapted from Grose and Afonso (2013). The confusion between figures 2 and 3 is regrettable, but has now been checked and fixed. Regarding the discussion of high resolution sites, we have carefully cut out less important details and some unnecessarily exhaustive statements. We have also added text in the opening to this discussion which serves to more clearly explain the purpose of our overview. To help the reader along we have also separated the discussion of high resolution sites to a first

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section which discusses our statistical representations of the site data, and then moves on to a more direct discussion of the meaning of the data and comparison to models. However, we have not attempted to trim the substance of this discussion because, as we attempt to clarify in the new manuscript, the details are important for any attempt to use heat flow data to constrain lithospheric models over young (<10 Ma) seafloor. Although the reviewer later suggests that the "data clearly shows where and how much heat is removed by hydrothermal circulations, and where heat-flow is near conductive", we do not agree. This confusion prompted us to clarify the purpose of our evaluation of the high resolution data and their relationship to model predictions. We have also modified figures 1 and 2 for clarity.

More specific comments: 1) p 1167, dataset filtering. The principal bias in oceanic heat-flow data comes from the deficit of discharge observation sites. If you remove high-resolution studies and near vent sites data, don't you increase rather than decrease this bias?

AUTHOR COMMENT: Yes, we want to bias the data to obtain an average global heat flow for all regions which are not immediately near ventilated hydrothermal circulation. The goal is to obtain a dataset, for which the difference between it and the modeled lithospheric heat flow equals the amount of heat extracted by hydrothermal ventilation, therefore samples of hydrothermal ventilation cannot be included. If we sample sites of hydrothermal venting we 1) will sample a bias in the dataset due to these sites being hydrogeologically interesting and 2) will sample ventilated discharge, which is what we are attempting to calculate by means of a heat flow misfit. If measured heat flow samples include ventilated discharge, then the difference between measured and modeled heat flow will not be equal to the amount of heat discharged by hydrothermal ventilation.

2) p 1168: "thermal rebound correction"? do you mean correction for sedimentation effect? or sediment thickness cutoff?

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AUTHOR COMMENT: We have clarified that the sedimentation effect is a “thermal correction for sedimentation”.

3) As already mentioned, you should detail the statistical analysis methodology. In equation 2, specify than q_m represents models heat-flow.

AUTHOR COMMENT: We have included a more explicative discussion of the details for the statistical methodology in the revised manuscript.

4) p 1170: explain the physics of models GH and GHC explicitly: not all readers want to check in a previous paper. It is not clear if these models are fully 2D or only 2D in the initial conditions (0-2 My).

AUTHOR COMMENT: Models GH and GHC contain a number of different phenomena, so we initially thought it best to briefly describe its characteristics and refer to our previous work for details. However, seeing the point of view of the reviewer, we have now added an explicit overview of the model properties, as well as added a figure illustrating the initial, boundary, and other conditions of the plate models. This includes the solved partial differential equations, Equations of State, thermal diffusivity, specific heat, radiative thermal conductivity, and axial hydrothermal circulation model.

5) p 1171: explain why you used a brittle domain for the Nusselt approximation up to 800 C when it is generally assumed to be 600 C or less. It is not clear if radiative heat transfers is included in the vigorous fluid circulation zone: how is it possible? Is there any reason for choosing 0.2 Ma for the limit of ridge axis rather than 0.1 in Spinelli Harris?

AUTHOR COMMENT: As is now explained in the updated manuscript, the use of 800C is a cracking front cutoff from Manning et al. (2000). Note also that Spinelli and Harris (2011) did not assume a cracking front, instead hydrothermal circulation also occurred in crust at mantle temperatures for their models (~1200C). We include radiative heat transfer everywhere for which the temperature is sufficiently high. We note in

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the manuscript that chemical and microstructural alteration may change the role of radiation, but we do not deal with that complexity here. Our use of a 0.2 Ma limit for cessation of hydrothermal circulation is used since this resulted in geotherms consistent with Dunn et al. (2000) and seafloor topography consistent with the database of Hillier (2010). In the new manuscript we have also provided additional discussion of seafloor topography and subsidence. In a close-up of seafloor topography near ridge axes our models predictions of this off-axis transition are consistent with observations.

6) Figure 1 is not of sufficient quality to distinguish dots of different colours. Figure is incorrectly called page 1172 line 15...

AUTHOR COMMENT: Figure references have been corrected. This figure has also been changed to give a different kind of, hopefully more clear, illustration of the cumulative distribution functions of heat flow as a function of age.

7) Figure 2 is extremely difficult to read! It is almost impossible to distinguish circles, squares, triangles, curves, colours (a_b-c). There is a confusion with figure 3: figure 2 is sometimes called figure 3 in the text (eg line 5 p 1172, p 1173 line 26) or alternatively figure 3 is called figure 2 (e.g line 21 p 1170). The Monte-Carlo analysis only appears in the legend of this figure with no other explanation! it should be detailed explicitly in the text (section 2.2).

AUTHOR COMMENT: The erroneous figure references are regrettable, but have now been fixed. The Monte-Carlo analysis is detailed in the updated section on the statistical analysis. We have also modified figure 2 for clarity by only showing the density functions for the near-axial deficit and the net power deficit, as opposed to showing the power deficit for all ages.

8) p 1176: in Wei & Sandwell, $g = 480$, not 420.

AUTHOR COMMENT: Thanks for this catch. This has been fixed.

9) p 1178 and figure 4: fit for GH and GHC is better for young sea-floor (0-4 Ma) where

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the isostatic assumption is generally considered as non valid, but GDH1 is better for old ages (>25 Ma). Can you comment on that?

AUTHOR COMMENT: In the updated manuscript we discuss this in the following: "An important question is whether or not the decreasing subsidence rate in proximity to ridge axes, and the corresponding misfit of model GDH1, is a real reflection of isostatic balance, or if other contributions, such as flexural effects, are important. If other processes are important, this could indicate that observation and model prediction of a low subsidence rate on ridge flanks is not actually related to crustal insulation and hydrothermal circulation. Cochran (1979) and Watts (2009) showed that isostatic anomalies are present over ridge axes. However, while the anomalies appear somewhat significant over Atlantic ridges, they are small and confined to the immediate vicinity of ridge axes over the EPR. Consequently, while elastic sources of deviation from isostasy may be present on the ridge axis, to our knowledge there is no compelling evidence to believe that non-isostatic effects are responsible for the low subsidence rate on ridge flanks (>0.2 Ma)." We then go on to show that it is at least the case that model GHC closely fits mean seafloor topography. Also, we have expanded the discussion of seafloor topography to include brief discussion of flattening. We argue that while model GDH1 better fits old-age flattening, this may be interpreted as a flaw rather than a success, since seafloor topography over ages 100-130 Ma is clearly anomalous. The subsidence rate here becomes negative around these ages, which cannot be accomplished by passive cooling processes and is not known to occur by means of small scale convection beneath old seafloor (Zlotnik et al., 2008; Afonso et al., 2008).

10) p 1181 line 12: confusion between figures 2 and 3.

AUTHOR COMMENT: This confusion has been corrected.

11) It's not clear what you want to show with high resolution surveys! In the four sites you choose, data clearly shows where and how much heat is removed by hydrothermal circulations, and where heat-flow is near conductive. I think by averaging everything,

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you introduce confusion and biases rather making light!

AUTHOR COMMENT: Thinking about this comment resulted in some significant changes to the discussion of high resolution heat flow surveys as described in our general comments earlier. A problem with the reviewer comment is that the data do not actually clearly (or at least precisely) show where and how much heat is removed by ventilated hydrothermal circulations. For instance, variations in surface heat flow mapped by high-resolution surveys are more strongly related to the thickness of the sediment column and variations in basement topography. In the Costa-Rica Rift survey, arguably the most well characterized region, seafloor heat flow varies considerably, but the variation strongly correlates with basement topography, and ventilated circulation may not even occur here. There are in fact more complicated and inaccessible processes occurring which must be considered, and identifying the conductive heat flow is not straightforward for any of the surveys. In fact, even in the case of the Costa-Rica Rift, while it appears that the region is well sealed from ventilated hydrothermal circulation, borehole geotherms indicate that large-scale ($\gg 1$ km below basement) hydrothermal circulation does occur. Thus, we argue that the measured heat flow in this region is elevated because of thin crust in addition to deep hydrothermal circulation. We characterize the heat flow distributions for the high resolution sites, especially the Costa-Rica Rift, because while it is difficult to extract precise information about the deep-seated heat flux, comparison between the heat flow statistics and the model predictions informs the interpretation of both the estimate of ventilated power and the hydrogeology of the crust in these regions. These details are discussed in the updated manuscript.

12) Can you discuss in more details the insulating role of the crust and how important would be its thickness variations in such an analysis.

AUTHOR COMMENT: In our analysis of the Costa Rica Rift, we found that the thin (5 km) crust in this region does impact the model prediction of seafloor heat flux. We tested the effects of varying the thickness by 0-10 km. In the figure showing the

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seafloor heat flow and diminution rate with seafloor age, the effects of crustal insulation on seafloor heat flow can also be seen. In the updated manuscript, we also include a similar figure showing the effect of crustal insulation on the subsidence rate as a function of age. These observations clearly show that the insulating role of the crust is important, and that variation in thickness is also important for the interpretation of heat flow (at least on young seafloor) on a regional scale.

Conclusion: Interesting approach, which should be improved by a more detailed presentation of the methodology and a better integration of high resolution surveys. Figure 1 and 2 should be improved or simplified, and a careful check of figures call should be done.

AUTHOR COMMENT: Thanks for insightful reviews of this manuscript.

Interactive comment on Solid Earth Discuss., 7, 1163, 2015.