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

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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 ridge-

axes (<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 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.

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 of sediments.

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.

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.

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