Solid Earth Discuss., 3, C227–C229, 2011
www.solid-earth-discuss.net/3/C227/2011/

Solid Earth Discussions

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3, C227–C229, 2011

Interactive Comment

Interactive comment on "The thermal structure of Israel" *by* E. Shalev et al.

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Received and published: 14 June 2011

This paper presents a brief discussion of heat flow measurements in Israel and the resulting implications for the thermal structure of the underlying crust. The authors are properly focused on their stated objectives and provide a well-written overview of the geologic context and previous thermal studies. However, this promising introduction is followed by an inadequate discussion of the thermal data and their analysis. The heat flow measurements represent a wide range of qualities, from equilibrium temperature logs with thermal conductivity measurements from the same borehole to uncertain Bottom Hole Temperature (BHT) measurements with assumed thermal conductivity values. The authors need to discuss these inherent uncertainties and present the heat flow measurements with associated uncertainty estimates. The mapped heat flow values are highly variable, and it is not at all clear to the reader what fraction of the variability represents true variations in background crustal heat flow as opposed to in-

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herent uncertainties in the heat flow determinations. The BHT corrections in particular seem arbitrary and poorly constrained. The correction used by the authors was developed for particular conditions in North America basins dominated by sandstone and shale sequences, and, given the numerous contrasting approaches to BHT correction applied around the world, there is no particular reason to believe that this correction is applicable to boreholes in Israeli limestones. Similarly, the thermal conductivity values used for formation averages are incompletely documented, with no indication given as to the associated uncertainties.

Following on the heat flow measurements, the authors estimate crustal temperatures using the well-known relationship for temperature in a crust with an exponential decrease in radiogenic heat production with depth. This should be a relatively straightforward process, yet the authors make some unusual choices without justification. For example, they assume zero heat production in the sediments, when the value, even if low, should still be consequential for thermal modeling. In addition, they estimate the mantle heat flow to a geotherm constrained by a very high value of radiogenic heat production obtained from the a table of values for the Sierra Nevada batholith in Turcotte and Schubert (1982). This value should properly be referenced to Art Lachenbruch's original work on heat production in the Sierra Nevada, and both the Turcotte and Schubert table and Lachenbruch's work show the value of 3.7 used in this paper to be at the extreme end of heat production values in that province. The resulting geotherms and mantle heat flow values are not shown by the authors, but there should be some anomalously extreme and unrepresentative geotherms given the choices regarding heat production.

Finally, despite the relatively average heat flow in Israel (which does seem to be a robust result), temperatures at depth remain surprisingly high, exceeding 200 Celsius at 10km over most of the country. The authors should explicitly evaluate their deep crustal geotherms in the context of the observed deep seismicity, which implies temperatures no higher than 400 Celsius at depths as great as 32 km or more.

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This paper contains valuable information but needs a clear and consistent description of the data analysis and interpretation, along with an examination of the associated uncertainties.

Interactive comment on Solid Earth Discuss., 3, 431, 2011.

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