Solid Earth Discuss., 3, C168–C170, 2011
www.solid-earth-discuss.net/3/C168/2011/

Solid Earth
Discussions

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

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Received and published: 25 May 2011

This manuscript could make a very interesting paper relating the tectonic framework and heat flux of the region around Israel. Unfortunately, it is absolutely impossible for readers who do not know extremely well the geology of the region and who have not memorized the tectonic map of a much wider region including the Sinai and the Red sea to appreciate the tectonic significance of the data and to understand the proposed interpretation. I am unable to precisely link many of the important geological features mentioned in the paper to the heat flux and temperature maps that are shown. This paper must not be published without a tectonic map of the region. A map showing the total sediment thickness over the study area would also be useful.

Secondly, I have very strong reservations about the approach used by the authors for calculating temperature at depth (as explained in section 4.2). The authors rely on a model with heat generation exponentially decreasing with depth, which is normally based on a linear heat flow heat production relationship $q = q_r + A \times D$ (or $q = q_m + \rho_c H_0 h_r$ in their notation). In this relationship, q_r and D are usually constant for a given province and surface heat production A ($\rho_c H_0$) is variable. The authors have used a fixed (and very high) value of heat production (3.7μW m^{-3}) and varied q_r. This makes little sense if q_r is the mantle heat flux as they state. Incidentally, it has been understood for a long time that q_r is not the mantle heat flux and it is now referred to as the reduced heat flow (heat flux at the base of the enriched upper crustal layer). With the parameters used by the authors (A=3.7μW m^{-3}and D=10km), the total crustal heat production will be 37mW m^{-2}, i.e. it is higher than the surface heat flux in some regions shown on Figure 3, which would then require a negative reduced heat flux! I do not think that the heat flow heat production relationship and the exponential model for heat genaration are good approximations (see the discussion in chapter 7 of *Jaupart and Mareschal*, 2011), but if they are used, they should be used consistently.

Also, I assume that the temperature measurements were made in the sediments. Then I do not know why the authors use equation (2) to fit the temperature profile to the data if they assume that heat production is zero in the sediment . On the same note, equation 2 is not correct for a layered medium. If thermal conductivity varies between layers, one must calculate the temperature through each layer ($z_i < z < z_{i+1}$) with a different conductivity. With no heat production, the equation would be:

$$T(z) = T(z_i) + q_0(z - z_i)/K_i$$

with $T(z_i)$ temperature at the top of the layer and q_0 surface heat flux.

There are a few less important points.

Some more details are needed concerning thermal conductivity values. Were the values given by Eckstein and Simmons measured on samples from the region? In that case, it would be useful to give the range of values and the number of samples measured for each lithology.

Also, the authors could provide a table with all the heat flux estimates and relevant information (geographical coordinates, average temperature gradient and thermal conductivity assumed, depth interval).

What is the coordinate system used for the maps?

Heat production is not 0 in the sediments, although it is smaller than the value used by the authors for the basement. For some of lithologies (shales) it could be as high as 1.5μW m^{-3}.

Where are the xenoliths found? Maybe their locations could be shown on the tectonic map. Or another map could be provided with xenoliths locations and T and P estimates. Incidentally, there is no need to provide so many temperature maps.

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Jaupart, C., and Mareschal, J.C., *Heat generation and transport in the the Earth*, Cambridge University Press, 2011.

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Interactive Comment