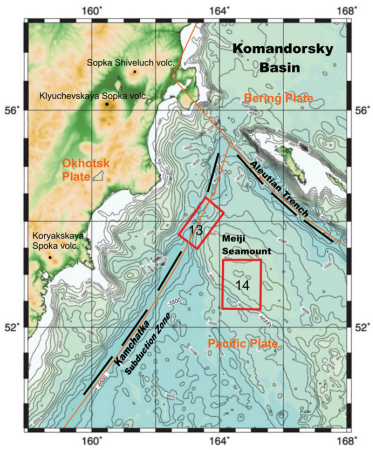
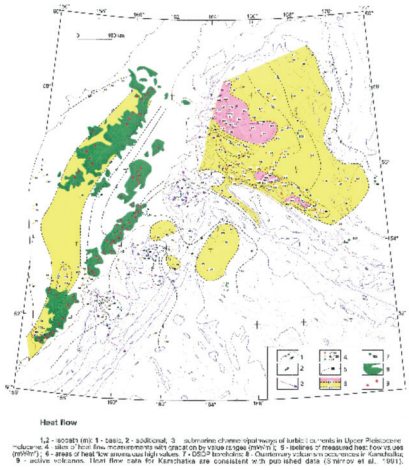
Review by F. Lucazeau

My response:

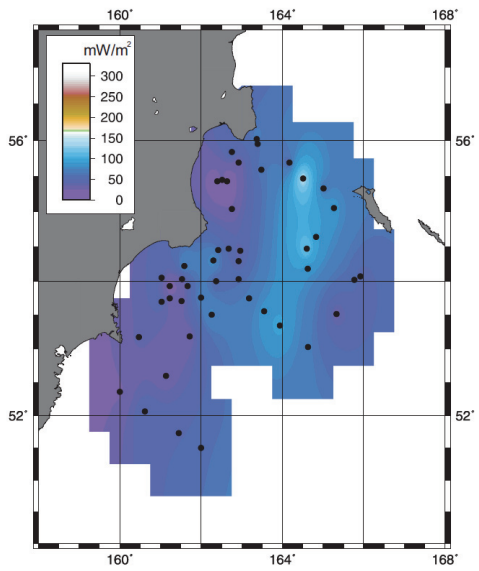
Point 1: Locations and geologic structures are now included as suggested. Contour intervals (500m) will be mentioned in the caption (see comment to Figure 1).



Point 2: The reviewer prefers to see the heat flow map based on the Russian measurements in the section "previous work". The published map by Smirnov (1991)



shows three heat flow anomalies (fitted by eye) in the area (which as a result of my measurements have now grown into one – see my Fig. 9) and secondly, the amplitudes of the "Russian" anomalies are lower than mine. Reason: I have measured in several higher heat flow values than ever measured by Russian workers.
I propose this solution: Since the Russian map (see above) cannot be used due to poor resolution, I put in an additional figure (Fig. 1b) with an isoline plot of the Russian values into "previous work".
The figure looks as reproduced below:



Point 3: The question of seasonal bottom water temperatures (bwt):

- There is sparse information about the details of the Russian heat flow instrument deployed in the late 70ties, early 80ties. To my knowledge by talking to Russian colleagues, the instrument was a rather short one (Bullard type).
- The water temperature inversion as shown in my Fig. 4 actually implies somewhat unsteady temperature distributions with depth. I would argue that significant annual changes in bwt, if existent, will result in either very high or low heat flow values **and** visible curvature in the T-gradient within 1.75 m bsl (for comparison see Davis et al., 2003, therein Fig. 4). I maintain that my measured T-gradients show little evidence for significant disturbance by seasonal bwt. I will follow the suggestion by the referee and will add in Table 1 the standard deviation of the measured values (T-gradient) from the linear regression line passing through all the measured values.

Point 4: This one measured thermal conductivity at site HF 25 is 0.47 W/mK and therefore lower than water. The water thermal conductivity of water is commonly listed as 0.56- 0.6 W/mK. I will remove the sentence: " … which might be explained by the assumption that a pocket of water came to rest between the sensor and the sediment enclosing the sensor rod"

Point 5: The approach I use for terrain correction of heat flow values is discussed in some detail in Delisle & Berner (2002). The approach is based upon a numerical model that mimics the sea floor topography and adds ample space below to include the subsurface, where topography effects on heat flow should have died out. A constant heat flow is assumed at the base of the computational domain, while constant sea floor temperatures are applied on top. By numerical iteration, the by the topography induced deviation of heat flow from the basal value is obtained.
I will include the above reference into the text.

Point 6: Color scale will be added.

Point 7: *"You should also discuss the absence of low values that should appear on the return flow of a convection cell (bias due to the small number of data?) …"*

This is a recurring theme in the literature: (see e.g. Delescluse & Chamot-Rooke, EPSL 276 (2008) on page 149: "… The search for fluid recharge zone was central to the debate on the ODP leg 116 … due to the absence of lower than normal heat flow measurements…"
As far as I know, very few workers, if anyone, have ever found negative heat flow anomalies that match the size of large scale positive heat flow anomalies (land and sea alike). My viewpoint is as follows:
We are dealing here with a situation akin to what happens in a heated tea pot. The heat source at the bottom causes warm water to rise (buoyancy forces). At the same time water is drawn laterally to the bottom of the pot. The latter water is also hot, but not quite as the rising water. No one would ever look for "lower than normal heat flow" in the pot. The whole water surface of the pot is quite hot and so are the sediments offshore Kamchatka in the area of high heat flow.

The last sentence in 5.1 (Therefore, internal convection … heat flow.) will be replaced by:

It is proposed that the most plausible process leading to the observed heat flow anomaly is internal convection within the fractured rock masses, which redistributes heat from depth to near the rock surface. Bending of the subducting oceanic plate is believed to be associated with deep reaching fracturing of the rock masses (Grevemeyer et al., 2005), which is inductive to bring fracture fluids in contact with hot rocks at depth. Hot fluids are then forced via buoyancy forces to near the sea floor. As the fluids cool off they are drawn back - presumably along the lateral limits of the anomaly - to depth for reheating. They might mix during descend with cooler fluids from the outside. It is, however, a question of the relative quantities of cold and warm fluids involved in the mixing which determines, if an accompanying negative heat flow anomaly near the seafloor will evolve or not. In our case, the lack of evidence for such an anomaly favors the model of a largely self-contained system of natural convection as the most likely cause for the observed elevated heat flow.

Point 8: The value 1.7 W/mK is used here as an average value for the whole rock column in question. Lower thermal conductivity values (0.9 to 1.4) are strictly confined to near surface sediments (uppermost meters).

The text will be modified as follows:
…the following rock parameters: $\lambda = 1.7\ Wm^{-1}\ K^{-1}$ (assumed average value for the whole rock column) and thermal diffusivity $a = 0.77 \cdot 10^{-6}\ m^2\ s^{-1}$.

Point 9: As a matter of fact I have not considered mantle hydration – I had looked into this issue many years ago and had come to the opinion that this process in all likelihood will have an only moderate effect. Delescluse & Chamont-Rooke (2007) place the heat flow increase due to serpentinization at somewhere around 20-30 mW/m² which would be insufficient to explain the Kamchatka anomaly. The process takes about 2 to 6 Myr to evolve and is associated with uplift (hydration of mantle rocks) in excess of 1 km (no real evidence for that offshore Kamchatka). Given the high plate velocity rate (9 cm/yr) the process would have to have started about 350 km seawards (let's say 4 Myr ago), which is incompatible with the idea of fracturing due to plate bending. Fracturing would have to occur first for the water to reach deep levels. I am very skeptical that we have a good case for serpentinization offshore Kamchatka.

Point 10: Table 2 in conjunction with Fig. 4 illustrates a so far unexplained feature (the hydrography people on board were surprised as well) and my intention was to quantify the

observed effect as detailed as possible. But since both reviewers did not feel Table 2 to be useful, I will delete.

Point 11: Yes, I assume the reviewer refers to Figs. 2 and 5. I will increase size of labeling.

Review by D. Hasterok

My response:

Comment on section 5.1: Yes, the size of the anomaly is poorly constrained. The only argument that points to a large size anomaly is the fact that every time we (or the Russians in the early 80ties) hit the ground we got anomalously high heat flow. To accommodate this comment, I change the text in 5.1 as follows

All available geothermal data from the region seem to indicate a large area affected by high heat flow with values between 100 -280 mWm^{-2}, though we cannot exclude the possibility that the area is made up of several smaller sized anomalies, primarily bound to fracture systems. Assuming ("extreme case") that we are dealing with only one anomaly and integrating over this area, the annually required water volume involved….

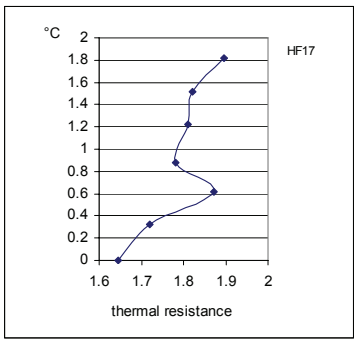
Comment dealing with the mud volcano issue: I have worked myself extensively on mud volcanoes on- and offshore Pakistan and onshore Azerbaijan (several publications on this). Mud volcanoes offshore Kamchatka are absent on the sea floor. I am sure we would have detected them with our bathymetry data. The issue raised by the reviewer centers again around the question of reviewer Lucazeau under point 7: how far away can reasonably be the location, where cold waters are drawn into the fracture system. I hope my new text under point 7 makes it clearer that I greatly doubt that cold waters drawn from great distances play a key role in the case of this Kamchatka anomaly. My clearly favored model is "natural convection" confined by a fracture system.

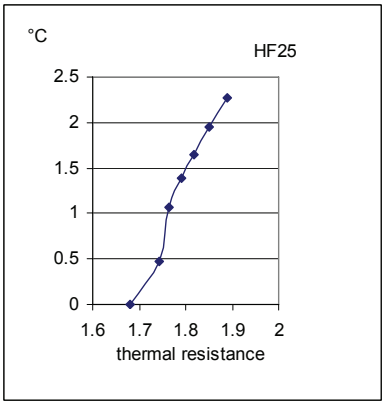
Page 454, line 10: done

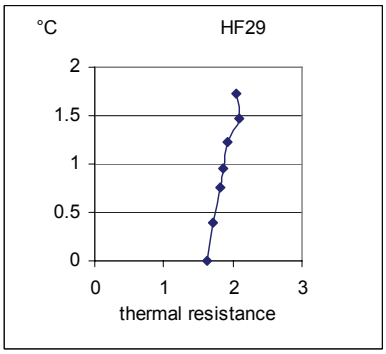
Page 458, first paragraph (uneven sediment compaction): Frankly, we are not at all clear on this and can only speculate. I will change the text as follows:

…not entirely clear. One might speculate that the thermal contact between the penetrating rod and the compacted sediment suffers, the more the sediment is compacted or that uneven sediment compaction plays a role.

Page 458 line 18 and figure 3: The Bullard plot issue: I have done Bullard plots, but not shown, because in my personal opinion they are not as helpful for interpretation in cases with short intervals of measurement (this applies to my hard ground heat flow probe as well as to my conventional heat flow probe with length of about 4 m) in comparison to cases with long intervals (boreholes). I include a few bad and good examples based on my data.







Most important, there is no clear cut systematic evidence for substantial changes in heat flow with depth. My data, I believe, are useful indicators for the "background" heat flow in the region – no more.
As a side note, I feel that the quality of most marine heat flow data is lower than what the usually listed "standard deviation" implies. But they are certainly good enough to tell us, if heat flow is normal in a region or, if anomalies exist.

Page 460 line23: done
Page 462 line 13: done
Page 463, line 22: done

Figure 1: will be done - see my previous remarks under point 2 (previous reviewer)
Figure 2: will be done - see my remarks to point 11 of previous reviewer
Figure 3: done
Figure 4: yes, it is a repeat – only useful for readers which look primarily at figures
Figure 5: will be done – the word "surprisingly" will be deleted.
Figure 6: done
Table 2: will be deleted.