Solid Earth Discuss., 7, C1258–C1261, 2015
www.solid-earth-discuss.net/7/C1258/2015/

Solid Earth Discussions

Interactive comment on "On the thermal gradient in the Earth's deep interior" *by* M. Tirone

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Received and published: 29 October 2015

General Comments

In the current manuscript the author explores the influence of irreversible energy transformations in the Earth's mantle on the temperature gradient therein. He assesses two contributions to irreversible energy conversion: (i) viscous dissipation and (ii) Joule-Thompson (JT) gravitational effects. This is an interesting perspective that goes beyond the typical treatment of the geotherm in geodynamics textbooks where it is said that "irreversible processes can be neglected" (to quote one of them). The results are presented along two lines: (i) 2D convection models that include viscous and JT contributions, (ii) a mantle model with certain footing temperatures for upwellings and downwellings to get the influence on the actual mantle geotherm. The general presentation made here is worth considering, while significant further discussion is required before this manuscript can be published (see specific comments below).

Specific Comments

2D convection simulations:

1. In the 2D simulations (isoviscous, bottom heated) T-profiles are given for an upwelling and a downwelling, with the different irreversible contributions plotted separately. Both the viscous dissipation and the gravitational effect. The average T-profile of the simulation – including the thickness of the boundary layers – must be affected by these contributions as well, and an additional panel showing the average T-profile and associated discussion would be helpful.

2. The b+w representation of the results here is an impediment to easily understand the results; a less puritan approach (=using colors) would be helpful to the reader.

3. The 2D simulations are described as purely bottom heated with a temperature of 3500 K at the bottom. However, not even the upwelling shown comes near that temperature (T<2900 °C). Temperature scales used (K vs. °C) should be consistent through the paper. Right now K is used in the text, °C in the figures.

For the computation of the mantle geotherm many questions remain open:

1. First of all, the choice of composition model should be discussed. It is not enough to say that it is restricted to the MgO-SiO2-FeO system. What are the proportions? Similarly, the choice of initial temperatures of upwellings (3100 K at the CMB) and the downwellings (1425 K) require – at least – a reference.

2. In the 2D convection simulations viscous dissipation is shown to play an important role, at the level of the JT gravitational effects. However, the presence and influence of viscous dissipation is not discussed at all for the mantle. Is it negligible due to the small stresses?

3. The adiabats shown for reference gain relatively little temperature over the depth of the mantle. The 1200 °C adiabat (taken for the downwelling) reaches ∼1800 °C at the core-mantle boundary, while other self-consistently determined adiabats (e.g.,

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Stixrude + Lithgow-Bertelloni, 2011) reach ∼2200 °C. This large discrepancy requires some attention.

4. From the 2D convection simulations it appears that the zeta value stays relatively close to 1.0 with deviation of 0.01 at most. I find therefore somewhat misleading to show results for larger/smaller values in Fig. 4.

5. The excess temperature of mantle upwellings has long been determined to be rather small (200-300 K, e.g. Schilling, 1991). A large "potential temperature" of the upwellings as predicted here (1800 °C or larger) seems in stark conflict with the geophysical observations.

6. The presence of the post-perovskite phase in the lowermost mantle seems to play a huge role in the thermal structure (leading to a sharp T increase with depth). However, the presence of the post-perovskite phase at temperatures above 3000 K at CMB pressures is highly questionable. The occurrence of the ppv phase needs to be critically considered and discussed here.

7. The discussion of a saturation of the heat capacity (c_P) is bewildering to me here. Two aspects: (i) No physical model predicts such a behavior; for the purpose of the current review I have looked at this explicitly, and the Mie-Debye-Grüneisen model rather predicts a decrease in c_P with P along an isotherm. (ii) The connection to the current considerations is not clear to me.

Technical Comments

1. Throughout the text Kelvin (K) is used as the unit for temperature, but in the figures it is Celsius (°C). This should be unified.

2. As pointed out by the author already the x-axis on one panel of Figs. 2 and 3 has the wrong label. It should be P_g/P rather than P/P_g.

3. Colors would help significantly to make the content of figures accessible. At this point it is, for example, almost impossible to distinguish the different curves in the

middle (right) panel of Fig. 2.

4. The x-axis ticks and tick marks in Fig. 1 (lower panel) is chosen in an awkward way. Why dT=40 K for the minor ticks? This does not make much sense.

5. There a number of occasion at which the reference to Figs. In the text is wrong. These are pg. 2509, l 19 (Fig. 3 -> Fig. 2) and pg. 2512, l 24+25: Fig. 2 -> Fig. 4

6. Typos: pg. 2505, l 5 combine -> combining; pg. 2511, l 8 pressure ration -> pressure ratio; pg. 2512, l 15 geothers -> geotherms

Other suggested changes to text:

7. Pg. 2505, l 11-12: . . . and other potential sources of entropy changes are ignored for simplicity or because they are negligible . . .

8. Pg. 2506, l 2 delete "for which only a brief description is given here".

Interactive comment on Solid Earth Discuss., 7, 2501, 2015.