Solid Earth Discuss., 5, C90–C95, 2013
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Solid Earth
Discussions

Interactive comment on "Energy of plate tectonics calculation and projection" *by* N. H. Swedan

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Received and published: 29 April 2013

Author reply to Short Comment, C. Grose

Each comment will be itemized and listed in the order in which the comments were presented. Under each item, Mr. Grose's comment will be presented first followed by author comment.

1) General comments:

There are many misspellings (e.g. "mantel") and odd word associations (e.g. "the plate tectonics"). The author shows no knowledge of the literature on the problem of the energy of the Earth's interior and the driving forces of plate tectonics. The authors attempt to characterize the thermodynamic behavior of the Earth is very difficult to follow, and numerous nontrivial contradictions with contemporary thought are not

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Interactive Discussion

Discussion Paper

discussed. Many statements have dubious meaning or credibility. For example:

Author comment

This Short Comment should, with all due respect, be ignored and given no consideration. Because subjects or issues raised could not be located in the manuscript where indicated by Mr. Grose. 1) The misspellings could not be found so that they can be corrected, 2) The comments have line numbers but do not have page numbers and the issues raised could not be located so that they can be addressed, 3) line numbers such as 53, 55, and 67 do not exist in the submitted manuscript, which suggests that Mr. Grose may have not even read the manuscript, and 4) Mr. Grose has exhibited poor skills in thermodynamics and his explanations could not be followed or addressed. He failed to define system and surroundings which is a basic requirement in thermodynamic discussions.

Line number 26 of page 138 of the manuscript states clearly that other tectonic models were examined and found to be inadequate for objectives of this manuscript. What makes Mr. Grose so sure that am unaware of the current state of the science and/or contradictions?

Before closing this comment the subject of the comment “Primary hypothesis is not feasible” will be addressed: The manuscript suggests more than a hypothesis. The work presented in the manuscript is in good agreement with observations, experiments, and the work of others. The calculated magma latent heat of melting and percent of mantle partial melting is in agreement with the work and experiments of Yoder (1976 p. 107). The thermodynamic approach suggests an upper limit of magma partial melting that cannot exceed 53% which is in agreement with observations, experiments, and the work of Yoder (1976 pages 112 and 113). The calculated average force of plate tectonics of 5.4 E12 N/m is in line with those calculated by McKenzie (1969) Or Harper (1975). The calculated annual energy of geological activities, 1.29 E19 Joules, and the observed energy radiated by earthquakes, 7.55 E18 Joules in Table 2, are in reason-

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able agreement. The work presented in the manuscript suggests an increase in the geological activities with climate change and surface temperature rise, and they are increasing: Table 2 clearly shows an increase in the energy radiated by earthquakes with time based on the US Geological Survey, Earthquake Facts and Statistics. The observed energy for 2012 is in line with the trend. Using the Smithsonian Volcano Research Database and sampling every five years, the volcanic activities have an increasing trend with time as well. The following is an annual average per two decades of the samples: from 1990 to 2010, the annual average number of volcanoes is 65; from 1970 to 1990, the annual average number of volcanoes is 56; from 1950 to 1970, the annual average number of volcanoes is 52. Based on observations, midocean ridges are rising with climate change. Iceland, which is located at midocean ridges, rose by 5-10 millimeters annually between 1992 and 1999, (Sjoberg L. E. et. al 2004). Nature reported lately that Iceland rising trend has increased to 12 millimeters annually. All of these trends of geological activities are anticipated by the thermodynamic model. In fact one can calculate the volcanic activities and the observed rising trend of Iceland with reasonable curacy by simple manipulation of the equations presented in the manuscript. The agreement between the work presented in the manuscript and observations, experiments, and the work of others is robust; it goes well beyond a hypothesis.

2) Ln 20-22: It is unclear to me how surface topography increasing the surface area of the Earth is thermodynamically important.

Author comment

Page number is not shown in this comment. The subject raised by Mr. Grose could not be located in the manuscript and addressed.

3) Ln 22-29: The attempt to link heat loss of the Earth to atmospheric thermodynamics is dubious since the heat loss of the Earth is negligible compared to the solar budget.

Author comment

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Page number is not shown in this comment. The subject raised could not be located in the manuscript and addressed.

4) Ln 24-26: Asserting that the arrangement of continents on the surface of the earth is such that heat loss is somehow optimized is unsubstantiated. Furthermore, that transport of Earth's "internal" heat, whether to space or the atmosphere, occurs primarily by radiation and not conduction is also unsubstantiated.

Author comment

Page number is not shown in this comment. The issues raised could not be located and addressed. The word optimized does not exist in the introduction.

Our observations suggest that mantle convection transports heat from the subduction zone to midocean ridges, not the other way around. This suggests that land is located on a warmer mantle, most likely, to radiate earth's internal heat more efficiently. This discussion of land radiations does not impact the content of the manuscript because the submitted work is related to ocean crust and heat transfer in the ocean, not land.

5) Ln _67: Although the author seems to agree that plate tectonics is driven by heat loss of the earth related to lithospheric cooling, this heat loss is attributed to the latent heat of solidification. This is a major contradiction with contemporary thought, and it is unclear what roles the author attributes to conductive cooling and density changes associated with temperature changes in the lithosphere or convective mantle generally.

Author comment

Page number is not shown in this comment and Line 67 does not exist in the manuscript. The subject raised could not be located and addressed.

Other tectonic models were examined. While they are correct models, they are not sensitive enough to variations in the temperature of ocean floor and cannot be used to achieve objectives of the manuscript that include projecting the observed increase in the geological activities with climate change.

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6) Comments on the primary suggestion of the work:

The author suggests that the force driving plate tectonics is a pressure generated by partial melting near ocean ridges (Ln 53-55). This is almost certainly incorrect. Although it is correct that partial melting will generate a pressure, melting is not an isochoric process and pressure associated with the volume expansion on melting will be quickly relieved by a displacement of surrounding mantle below the ridge. Thus, along with a local decrease in mantle temperature, some work done by the expansion related to the enthalpy change on melting (dH=dQ+VdP) will go into gravitational potential energy, which might be roughly modeled as a positive topographic anomaly above ridges and may be on the order of a few hundred meters at most for 'normal' seafloor. Most relief will occur over the timescales of the solidification of oceanic crust (<0.1 Ma, or a few kilometers from the ridge). I fail to understand how it is mechanically feasible to suggest that such a stress generated in hydrostatic mantle will be converted to a sustained stress on plates. In comparison, the topographic relief related only to conduction over the age of the lithosphere is on the order of 3 km and occurs over thousands of kilometers. If this gravitational potential energy is available to drive plate tectonics, it is negligible.

Author comment

Page number is not shown in this comment and lines 53 and 55 do not exist in the manuscript. The issues raised could not be located where indicated and addressed. Therefore, the comments below are for the sake of discussion only:

Most of the issues raised in this comment are discussed in author reply to comments of Referee #1. Please refer to items 2, 8, and 9 of the discussion.

The thermodynamics presented by Mr. Grose is unclear for system and surroundings are not defined. Regardless of whether he is right or wrong, he misses the point: An objective of the manuscript is to project the geological activities with climate change. The available models cannot achieve this objective because they are not sensitive enough

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to variations in the temperature of ocean floor. Conversely, the thermodynamic model is very sensitive to infinitesimal changes in the temperature of ocean floor and it is adequate for the objectives of the manuscript. The suggested thermodynamic approach is therefore based on necessity and does not proof that other models or concepts are wrong. On the contrary, it cross-checks, confirms, and agrees with earlier findings and other tectonic models.

Interactive comment on Solid Earth Discuss., 5, 135, 2013.