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The Heart Mountain Conundrum, Part 1: Models of Low-Friction Sliding Have Major Problems

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Abstract

Heart Mountain, Wyoming, USA, is a geological puzzle. Paleozoic carbonates overlie much younger Lower Cenozoic rock. So, either several mountain-sized blocks broke apart and slid up to 45 km from where they were originally deposited onto the younger rock or the way the relative ages of the base layer and the blocks are assessed is wrong. This paper, with its partner, examines the issues and problems explaining this proposed event. Part 1 examines three (uniformitarian) models of a low-angle slide on a low-friction cushion. In Part 2, another six essential issues, including how the movement started and was sustained over uneven terrain, are examined. The joint conclusion is that present models are seriously inadequate.

Key Words: Heart Mountain, Décollement, Detachment, Slide, Uniformitarianism, Low-friction sliding, Geologic column, Flood models

Introduction

Heart Mountain, Wyoming, USA, has been recognized as a geological puzzle for over 100 years (Hauge, 1993). Paleozoic carbonates overlie much younger Lower Cenozoic rock. Either numerous mountain-sized blocks broke apart, with or without the involvement of volcanic rocks, and slid up to 45 km onto the younger rock, or the way the relative ages of this sequence are assessed is wrong. Many studies of the area have focused on trying to explain how several portions detached from a "mother area," broke apart, and moved up to 45 km. No uniformitarians have questioned the stratigraphic assumptions underlying the dating.

Whitcomb and Morris (1961) challenged the uniformitarian geologic column because of what the authors saw as anomalies and its intimate association with evolution. They had used the sequence at Heart Mountain as an example of the failings of the column because they saw no physical evidence of an overthrust. Other creationists (e.g., Garner, 2011; Clarey, 2013) accept the uniformitarian claim that this detachment and sub-aerial sliding (typically abbreviated to HMD) took place and that it does not compromise the chronostratigraphic column, although they do not accept its geochronologic timescale.

This series examines the issues surrounding the HMD. Part 1 examines the three latest uniformitarian models

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that attempt to explain how the HMD block moved down a slight slope on a low-friction cushion. Part 2 examines six key issues, including how the supposed movement started (the break-away fault), how the break-up occurred, and how movement was sustained over uneven terrain. The conclusion of both is that all three models are inadequate and that they reveal major problems regarding the understanding of the chemistry, geology, petrology, physics, and rock mechanics of the event.

These three models are uniformitarian. Currently, there is only one creationist model that attempts to explain the HMD movement (Oard, 2011). It is also reviewed in Part 2. Other creationist low-friction models might be developed (Sydow, 2020), but until such time, Whitcomb and Morris' (1961) view that the "Paleozoic" rocks of Heart Mountain are younger than the "Cenozoic" rocks they rest on remains a distinct possibility, implying that 60 stages of the geological column are suspect.

The hope is that these two papers will encourage further discussion on Heart Mountain (including newer ideas on how the slide might have happened) and a willingness to reconsider the geological column if no new ideas are forthcoming. It is disappointing that a carefully laid-out discussion with new data has not taken place since 2006 (Reed and Oard, 2006), since it is crucial for modeling the Flood and challenging evolution.

The Heart Mountain Conundrum

The sequence of rocks at Heart Mountain, Wyoming, USA, has puzzled geologists for over 100 years. Uniformitarians have proposed that the older rocks, "Ordovician and Mississippian" carbonates, detached from a larger "mother mass," broke off, and slid in separate groups, or in a volcanic/carbonate coherent mass, up to 45 km into their present positions atop "Eocene" rock (Pierce, 1957; Hauge, 1990). Thus, based on the order in the geological column, older rocks overlie younger rocks. If the order at the HMD is a result of a catastrophic décollement, then the mechanism for detachment and motion must be addressed.

Some claim that there is separate petrological evidence for the slide. For example, certain layering is interpreted as "microbreccia" suggesting a period of sliding. But there are stratigraphic features within the complex that show a "sedimentary character that appear[s] to record deposition from suspension rather than friction" (Beutner and Gerbi, 2005, p. 724). So, this evidence is equivocal. There are four other points offered by uniformitarians, and one by creationists, also largely equivocal, which are addressed in detail in Part 2.

A simple cross-section is shown in Figure 1 (NW to SE, left to right) based on Beutner and Gerbi (2005) and other references. Note that the cross-section is not a perfect straight line, see Figure 2 of Aharanov and Anders (2006).

Review of Uniformitarian Explanations

The literature on Heart Mountain is extensive. Beutner and Gerbi (2005) reviewed much of it and offered their own uniformitarian model, proposing that high-pressure carbon dioxide could have formed a low-friction cushion for the HMD block to slide on down the low-angle slope. They noted that similar ideas had been proposed previously, but none were robust models since they failed to address the question of how any cushion(s), gaseous or liquid, was/were sustained over long distance. They therefore suggested a continuously self-generated layer of CO_2. Since then, two other models of a low-friction cushion have been proposed, including a water-layer (Aharanov and Anders, 2006), and a melt-layer (Craddock et al., 2009). In view of this recent interest in a low-friction cushion, this paper evaluates these three models.

Beutner and Gerbi (2005) also recognize that there is more to explaining HMD than a low-friction slide. They identify six key questions addressed in Part 2. Uniformitarian answers are vague or not scientifically robust. It is not possible, in a journal article, to provide all the detail (chemical, geological, mathematical, and physical) of this assessment for rejecting HMD as a detachment and slide. That material is available in an unpublished document of 40 pages from the author (Matthews, 2019). This series is its summary.

Gaps in the Slide-Explanations

Seven questions listed by Beutner and Gerbi (2005, p. 724) were circulating years earlier in prototype form (Prostka, 1978).

1. What allowed or caused a mass of rock more than 1,100 km in area and several km in thickness to detach approximately along a bedding plane and slide on a slope of <2° while spreading to cover more than three times its original area?
2. What force, or forces, initiated movement?
3. What caused the detachment to form near the base of the Ordovician Bighorn Dolomite rather than in weaker underlying rocks?
4. What was the rate of displacement?
5. What process or processes reduced friction on the sliding surface sufficiently to allow sliding on such a low slope?
6. What role did contemporaneous volcanism play?
7. Why is rock immediately below the slide surface so commonly undeformed?

The authors continue (p. 724):

> …during the last century numerous geologists [they list six] attempted to answer these questions. None

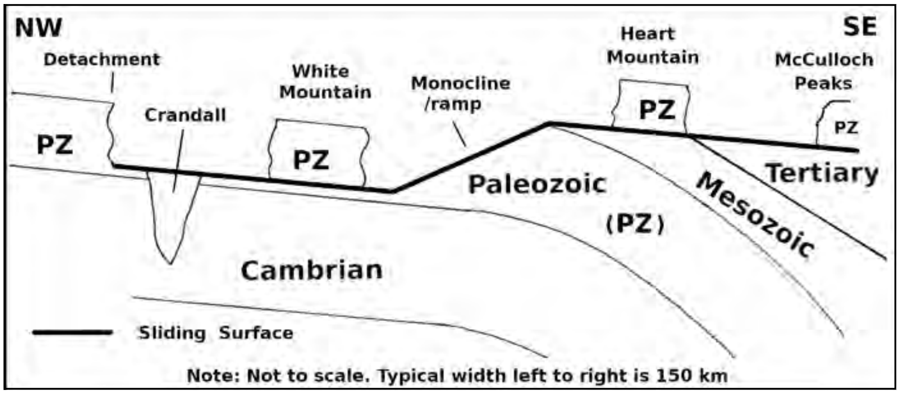

Figure 1. Simplified cross-section of Heart Mountain, Wyoming, USA.

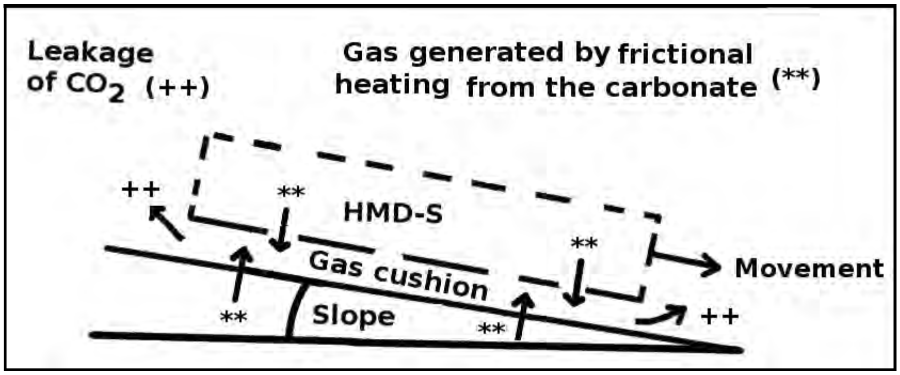

Figure 2. Heart Mountain sliding on a gas cushion.

> of the resulting hypotheses [they list eleven] attracted a quorum of supporters because evidence was not offered that unequivocally accorded with one hypothesis and negated others.

Since there are no obvious reasons to reject Beutner and Gerbi's (2005) conclusion that these earlier ideas are inadequate, only these three later models need to be studied.

One of the more recent papers on the HMD, (Craddock et al., 2009), admits that the absence of significant breccia across the assumed sliding surface is a serious challenge. This was Whitcomb and Morris' (1961, p. 181) original point. To that extent, the focus of those anxious to explain HMD is on Question 5, and all propose a low-friction regime which could explain the absence of breccia. In reality, the seven questions contain multiple and indirect questions; some overlap. It is therefore essential to relate the questions to the proposed full models for HMD.

Oard (2006) noted the questions set out by Beutner and Gerbi (2005) and offered tentative answers, explaining HMD as a subaqueous detachment and slide during the Flood. The model does not specifically rely on a low-friction cushion. It is more appropriate therefore to discuss that model in Part 2.

The Low-Friction Models

Leaving aside six of the questions, we focus first on Question 5, namely: what provided the low- friction cushion? The reason for going straight to Question 5 is that recent uniformitarian models follow this approach. The models assessed include: 1) the Carbon Dioxide Cushion, 2) the High-Pressure Water Cushion, and 3) the Molten Calcite Model. These can be critiqued in detail, whereas many of the issues surrounding the other six questions cannot. Obviously, answers to all seven questions must be internally consistent with any proposed low-friction cushion.

The Carbon Dioxide Cushion

Since there is little evidence of rock-to-rock contact during the supposed movement, Beutner and Gerbi (2005, p. 732) claim that carbon dioxide could have been released during movement thus providing a low-friction cushion. Their model is not described in detail nor in a systematic manner, but three statements reveal their weak points.

First:

> In order to maintain this system, motion along the detachment is *interpreted to have been rapid*, [emphasis mine] with displacement taking place in no more than a few tens of minutes. [p. 734]

Obviously, unless the movement was rapid, the CO_2 cushion would have quickly escaped, and so they offer a velocity of many tens of kilometers per hour (kph), but without supporting calculations. They also remind readers that one authority suggested 150 kph (~40 m/s) but with "tongue in cheek." But a focus on velocity prior to confirming motion is tantamount to forcing the desired answer and avoiding conflict with the geological column. By focusing on the low-friction aspect, the authors are trapped in circular reasoning.

Second:

> The kinetics of dissociation by frictional heating *clearly need further study* [emphasis mine]. How much calcareous dolomite [i.e., the Paleozoic carbonates] would have to dissociate in order to float the Heart Mountain allochthon remains an important question…. a simple calculation using the ideal gas law suggests that several centimeters of dolomite would have to dissociate to generate an ~10-cm-thick cushion of CO_2…. Our model *requires explanation* [emphasis mine] of the nature and source of the gas-like fluid present along the fault during movement. We *suggest* [emphasis mine] that CO_2 released by dissociation of carbonate as a result of frictional heating is a likely candidate for the fluid phase along the slide surfaces of the Heart Mountain…. [p. 734]

Their triple equivocations are clear.

Third:

> A detailed thermodynamic and kinetic study *remains to be done* [emphasis mine] for the Heart Mountain structure. [p. 734]

So, in three respects, they admit that their model is tentative. Only by taking their model to the next level of detail could it be validated. The following are reasons for rejecting it completely.

A robust model needs to show that, if the block is moving steadily down the slope as shown in Figure 2, the change in potential energy during movement could balance the frictional heating necessary to sustain the generation of a 10-cm gas cushion, leaking continuously at the edges *(which the authors failed to mention)*. Using the quoted parameters in Beutner and Gerbi (2005) relating to temperatures, heat capacities, ideal gas law, etc., and favorable parameters where they were silent, we calculate that a velocity of ~25 m/s meets these conditions for the dolomite (Matthews, 2019). While this velocity is consistent with their range of 10–40 m/s, there are five major problems with their explanation. Any of them separately rule out a low-friction gas cushion slide and prompt several additional questions.

First, why a 10-cm gap? The irregularities on the fault plane could reach 5 m, judging by the maps of the area, and unevenness created by the Crandall Intrusive Complex and the Blacktail Thrust. A gap of only 10 cm would result in the huge block dragging on the substrate because of vibration, swaying during movement, and this natural unevenness. Rock-to-rock contact would rapidly increase friction, which would halt motion. But a larger gap would result in faster escape of the gas from beneath the rock mass. To balance gas generation with leakage from a larger gap with continued motion would require a velocity of Mach 3 (Matthews, 2019). At that velocity, the air resistance would severely interfere with the gas cushion in such an extensive gap.

Second, why did the HMD travel southeast rather than southwest? The slope to the SE is <2°, while the slope to the SW is 10° (Prostka, 1978). This dramatic difference in gradient is one reason Prostka (1978) ruled out a gravity slide. Thus, the block should have ended up many kms to the SW if a low-friction cushion allowed free movement. In a simple detachment and sliding, the "mountain" would have moved SW (solid line track, Figure 3). If an initial velocity of 25 m/s had somehow been achieved moving SE, then a curved track would have resulted (long dashes) due to gravitational acceleration to the SW on the moving block offsetting the initial SE motion. There is no reason for it to have moved as proposed.

Third, how did it *continue* to slide up a monocline of Paleozoic and Mesozoic rocks before ending up on the Lower Cenozoic? Sliding across this monocline would require a higher velocity, ~40 m/s, because extra heat is needed to liberate sufficient gas (Matthews, 2019), even with Beutner and Gerbi's (2005) 10-cm gap, which was probably not sufficient to avoid grinding contact. So, the block would have had to encounter the top of the monocline moving at a velocity of around 40 m/s.

This leads to the fourth question—how did it climb the monocline? The Mesozoic and Cenozoic could have been between 300 and 500 meters higher than the Paleozoic 'fault plane' at the time of movement (see Part 2 discussion of rock mechanics associated with the initial detachment and fracturing). If the block arrived at the *base* of the monocline at 25 m/s, it would come to a halt on the monocline *even with* a low-friction cushion because the kinetic energy would have been absorbed by the increase in potential energy. The monocline/ramp may have been different at the time of movement; it could have *higher* or *lower*, hindering or helping the model. Beutner and Gerbi (2005) mention but do not explore this point, and so I will leave it here. Given all of the problems noted, the original state of the ramp does not affect my basic conclusion that their model cannot account for the outcome.

Furthermore, if the block needed to cross the Mesozoic and Cenozoic strata at 40 m/s, it must have been moving between 90 and 110 m/s when it reached the monocline (unless there is some mechanism for injecting more kinetic energy into the block). Otherwise, the model is not robust. But that higher velocity across the Paleozoic strata could not have been sustained since the gap generated would no longer be in equilibrium with the heating rate. Summarizing, the CO_2 model would have required three phases of movement (across the Paleozoic, up the monocline, across the Mesozoic and then across the Lower Cenozoic of the Bighorn Basin), but uniformitarians insist it was a single-pulse event. More detail on the issue of kinetic energy is addressed in Part 2.

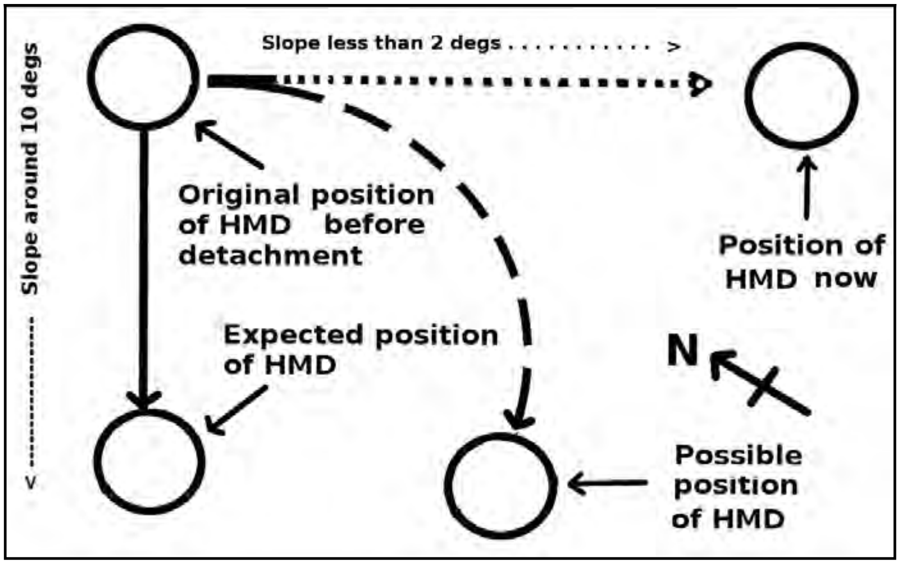

Figure 3. Trackway reconstruction of Heart Mountain, Wyoming, USA.

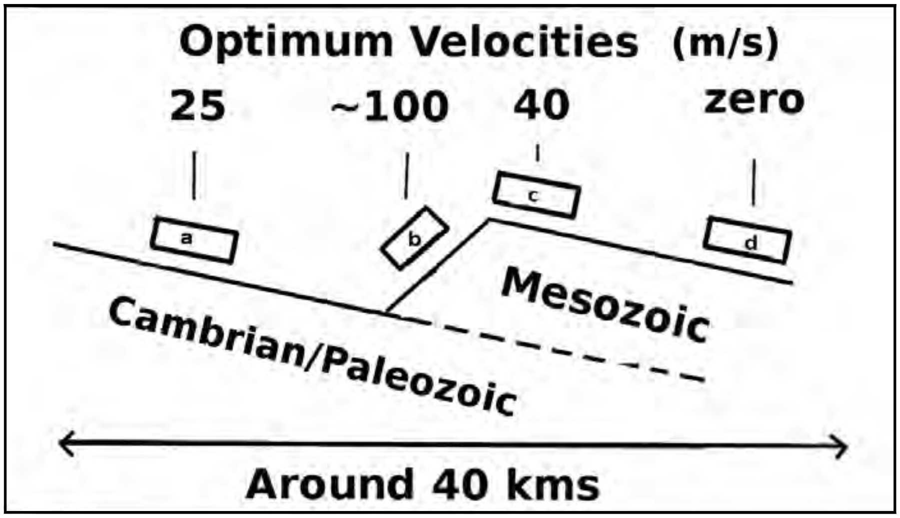

Figure 4. The velocity dilemma

There are other problems with the monocline. As the block reached the base of the monocline, it would have had to shift from moving 2 degrees downslope to as much as 30 degrees *upslope*. A huge gap between its base and the substrate would have opened, allowing more rapid CO_2 escape alongside substantial physical damage.

How did the changes in velocities needed by the different portions of movement occur? Figure 4 (not to scale) shows the stark reality of the problem. It emphasizes that there are at least three different portions of movement at different velocities (positions a, b, and c) where changes in velocity *must* have occurred. In reality, there are four, because we must include the location where Heart Mountain came to a halt (d). What is so special about that position? Why did the CO_2 cushion fail at that specific point when it had been so successful for 40 km? Why could the carbonate pieces found at McCulloch Peaks pass that point and not Heart Mountain? Without answers, we do not have a robust model.

Fifth, as the dolomite releases CO_2, it turns to lime in this model. That lime would be expelled from beneath the block by escaping gas, creating a depressed trackway on the fault plane and levees at the edge (Figure 5). There is no evidence for these. Beutner and Gerbi (2005) suggest that the lime could have turned back to carbonate by interaction with air-borne and other local sources of CO_2. But the argument is one from a lack of evidence, and therefore ad hoc. Also, since dolomite includes magnesium, magnesium minerals, sparse in CO_2, would also have been deposited in the levees. These are neither soluble nor react back to their original state. Beutner and Gerbi (2005) admit that they are not there.

Commenting on this CO_2 self-generated cushion, several creationists accept that HMD was a real, sub-areal slide. For example:

> Beutner and Gerbi have … made a strong case for catastrophic movement of HMD (involving supercritical CO_2 as the suspending medium.) (Clarey, 2013, p. 5)

In light of the hidden and unreasonable assumptions identified in Beutner and Gerbi's work herein, if HMD moved it was not by CO_2.

The High-Pressure Water Cushion

Aharonov and Anders (2006) address the friction problem with a high-pressure water cushion. They suggest that dikes rose from depth at high pressure along what would become the fault plane. Water from those dikes could explain how the slide started on a near-zero-friction surface of high-pressure water. Their calculations using rock mechanics and overpressures show that, given certain assumptions, the pressure of the interstitial water could have been raised by the Skempton effect to the lithostatic load. The block could then start to move down the small slope without the need for an arbitrary injection of kinetic energy (which Beutner and Gerbi require). With a slope of <2° it would have moved slowly. Meanwhile, the high-pressure water would have escaped along the glide plane. Thus, their key problem would be maintaining the slide. The block would have moved away from the high-pressure water source and onto an unpressurized surface. The high-pressure water should have rapidly escaped as the block accelerated, resulting in abrupt rock-rock friction, and termination of the motion. This is why Beutner and Gerbi (2005) rejected earlier models of low-friction sliding. Another major problem for Aharanov and Anders' (2006) model is that HMD should have slid SW (Figure 3) since gravity provided the energy for their slide.

Other problems include not assessing the viscous drag from the water gap even if movement could have started. Also, high-pressure water along a fault plane creates other problems (see Part 2). The paper references Beutner and Gerbi's (2005) idea on this point without comment. The fact that it is a very different idea is an implicit criticism of Beutner and Gerbi's CO_2 gas cushion, but it fares no better than theirs.

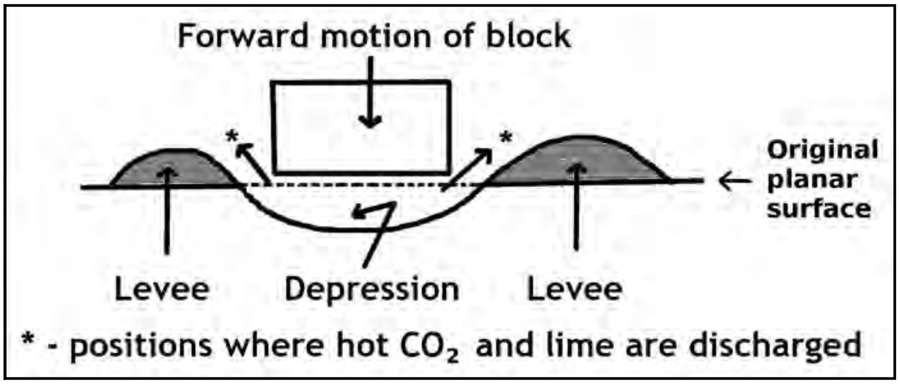

Figure 5. The formation of levees and depressions

The Molten Calcite Model

The paper by Craddock et al. (2009) is primarily about White Mountain (about 30 km "upstream" of Heart Mountain). Since it is also judged to have experienced catastrophic movement, their model could resolve the problems understanding HMD.

Like the others, this model starts with the assumption that the detachment occurred. The moving block has an unknown velocity that can be calculated by assuming that the slide took place over a low-friction layer of molten carbonate. A value of ~0.06 is quoted compared with a normal rock-to-rock value around 0.6 (Jaeger et al., 2007). The heat is generated by the friction of sliding. Of the seven questions listed by Beutner and Gerbi (2005), Questions 1 to 3 are ignored, and Question 4 is answered by assuming that the slide took place (*circular reasoning*). Question 5 is answered by a less-than-sophisticated heat transfer calculation, and Questions 6 and 7 are ignored.

Furthermore, in their heat-transfer calculations, there is an assumption that the heat required to melt the rock is *fully available* at the beginning, but it would not be until the end that there would be sufficient heat to melt the calcite. There are also *numerical inconsistencies* (see Matthews, 2019). There are too many assumptions to call this a viable model. Like Beutner and Gerbi (2005), the focus was on a petrological examination of the rocks, with too little thought on the physical mechanisms needed for the large-scale process.

This model cannot help explain how Heart Mountain traversed the Paleozoic, climbed the monocline, or traversed the Mesozoic and part of the Cenozoic.

Summary, Conclusions, and Recommendations

Geologists have been baffled by the HMD for over 100 years (Hauge, 1993). They claim that it detached from its "mother mass," sliding up to 45 km to the east-southeast. Recent uniformitarian studies reveal seven questions that need answering before a viable explanation can be made. But the focus has solely been on only one—namely the nature of the low-friction cushion needed to explain the lack of breccia from the supposed movement.

Three very different recent models of low-friction sliding have been examined herein. They include cushions formed by carbon dioxide, high-pressure water, or melted calcite. Numerous problems exist with these, including the lack of consistency *between* the three distinctive ideas. Therefore, this leads to two obvious questions: Did Heart Mountain really move? And, if so, how did it move?

Leaving aside these fundamental questions for the moment, the key conclusions and recommendations from this study are:

1. In spite of intense effort to explain HMD as a low-friction slide, current models are woefully inadequate. They fail fundamental tests of physics and chemistry. All three assume movement, then try to justify it with ad hoc mechanisms.
2. While there is always opportunity for new perspectives on HMD that might answer the seven questions, it grows less likely in view of the effort so far and the lack of consistency between different researchers. There has been no major work in the last 10 years.
3. Creationists, especially those who consider the geological column to be a robust model of Earth's history (though without its uniformitarian timescale), should be wary of using or endorsing any of these low-friction models and need to note the *a priori and* often unstated assumptions in them.
4. Since Whitcomb and Morris (1961) seem to have had a valid point, creationists should revisit the geologic column, perhaps in a forum like Reed and Oard (2006), for two reasons. First, if the column is not robust, evolution is more readily challenged. Second, establishing whether the column is or is not robust affects how Flood models are constructed.

Personal Note

This paper has been many years in the making. During that time, I have had extensive discussions with those having an interest in the subject and those I have pressured to check my physics. I am grateful for the time they have spent with me clarifying things and alerting me to factors I had not previously noted. Some support the column; some do not; others (two physicists) have no independent view. I am also grateful to the formal reviewers helping me clarify my ideas and being sensitive to those who hold alternative views on the column.

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