

Peer Review File

Weak magnetism of Martian impact basins may reflect cooling in a reversing dynamo



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REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

Dear authors,

I enjoyed reading your research article "Could the weak magnetism of Martian impact basins reflect cooling in a reversing dynamo?". In the following I am providing my review roughly following the structure proposed by Nature Communications.

Key results/summary

This article addresses an important open question about the history of the Martian core magnetic field with important implications for the history of Mars' climate: How to reconcile the absence of magnetic fields in large impact basins with the presence of magnetic fields in young lava flows. The former has been interpreted as an indication that the Martian core magnetic field ceased 4.1 billion years ago, but the latter has been interpreted that the core magnetic field was active 3.7 billion years go. A possible explanation presented by Rochette et al (2006, cited in the article), was that the impact basins may have formed in an active but reversing core magnetic field, similar to Earth's core magnetic field. The work of Rochette et al (2006) presented this hypothesis together with a crude back of the envelope demonstration that this hypothesis may be valid.

The article in review presents a substantially more sophisticated investigation to test the hypothesis whether the absence of a magnetic field over impact basins at spacecraft altitude can indeed be explained by a reversing magnetic field. To test this hypothesis, the authors first use hydrocode impact simulations to determine the temperature structure in an impact basin for a vertical impactor. The temperature profile once the movement of the impact has settled is then used as a starting point for a cooling process simulation. The simulated cooling history exposed to a reversing core magnetic field is used to determine the remanent magnetization within the impact basin. Finally, the shallow-most layer is partly demagnetized by random late impact events after core field shutdown. From the resulting magnetization, the radial component of the magnetic field as observed by spacecraft is calculated. These calculations were repeated for a range of impact basin sizes and random reversal histories for a wide range of mean reversal frequencies.

The key result of this article is that based on the presented simulations, the absence of substantial magnetization over the larger impact basins at spacecraft altitude can indeed be explained as a consequence of a reversing core magnetic field together with the slow cooling of the impact basins at depth and late demagnetization at shallow depth. This article also presents an intriguing finding that for large impact basins which form in reversing core fields, the remanent magnetic field peaks close to the basin rim and not at the center as is the case for smaller craters.

Noteworthy results

The presented work is of high value for the study of the Martian magnetic field and has substantial implications on the potential climate history of Mars, as was elaborated in the article.

The hypothesis tested in this paper, that a reversing dynamo could explain the weak magnetic field in larger impact basins, was published by Rochette et al (2006), but the method of Rochette et al to demonstrate the hypothesis was not elaborate enough to be convincing. The approach presented in the submitted article uses state of the art software to take into account the dynamics of an impact, cooling, and magnetization.

Validity

The methodology is appropriate and to the best of my knowledge well designed. Hydrocode and cooling simulations are outside of my field of expertise, however, most of the parameters chosen for these simulations appear well justified. The method is described with enough detail for reproducing the results. One choice that is implied when using 2D impact simulations, as by the authors, is that the impact is assumed to have occurred vertically. However, probable impact angles deviate substantially from the vertical and the resulting temperature structure from an oblique impact may show substantial asymmetry. I propose how to address this in the section "Suggested improvements".

A second point that I think should be addressed is the comparison of the results with the Zhurong Mars rover (Du et al, 2023, cited in the article). The Zhurong Mars rover collected surface magnetic measurements within Utopia Basin, a major impact basin. Utopia Basin has a weak magnetic field at spacecraft altitude and would be in the class of basins with magnetic field peaks close to the edge of the basin. The Zhurong Mars rover collected magnetic field data on the surface inside Utopia Basin in the vicinity of the edge of the basin. The key finding of Du et al (2023) is that the horizontal component of the crustal magnetic field along the approximately 1 km long transect of the rover is close to zero on Mars' surface (the radial component could not be determined from the rover measurements). The submitted article does touch on these results, but notes that longer transects would be required to interpret the surface data. I argue that the Zhurong rover results should be addressed more quantitatively. I propose how to address this in the section "Suggested improvements".

Suggested improvements

My first suggested improvement pertains to the likelihood of oblique impacts and their effect on the remanent magnetic field. I want to stress that I do not propose to use 3D impact simulations of oblique impacts, as the computational cost of such additional experiments would be too burdensome. However, a discussion of expected asymmetries in the temperature field of an oblique impactor and resulting magnetic field pattern would greatly help. Spatial patterns of the temperature profile of oblique impacts for the relevant basin sizes may be found in the literature. I could imagine that one side of an oblique impactor may not heat up as much and therefore cool quickly and have a higher chance of assuming the magnetic direction of a single polarity. However, the asymmetry may be limited to only the shallow-most portion of the impact basin, which is removed by random late impacts.

My second suggested improvement pertains to the interpretation of the Zhurong Mars rover magnetic data collected on Mars' surface inside Utopia Basin close to the rim. I propose to use the magnetization calculated for simulations relevant for Utopia Basin to obtain the intensity of the tangential magnetic field on Mars' surface within Utopia Basin. I propose to then use Zhurong data as a point location, because of the very short transect length. The calculated intensity of the tangential magnetic field obtained from the simulations can then be evaluated at all locations on a circle representing the location of the Zhurong rover relative to the basin edge. Some of these magnetic field values may be large, but others may be close to zero (e.g. at the peaks of the radial field in the right panel of figure 4c, because the radial field typically resembles the potential field and the peaks of the potential field correspond to zero in the tangential field). From these magnetic field values, a probability can be calculated for recording a near-zero tangential field at the distance from the center of the basin that corresponds to the location of the Zhurong rover in Utopia Basin. If that probability is high, then this would present a strong confirmation for the hypothesis presented in this article. If, however, the probability is low, then this would be an indication that the reversing magnetic field hypothesis may not be a valid explanation for the weak fields in the larger basins. In my personal opinion, both outcomes are equally exciting and clearly worthy of publication in Nature Communications. I also want to note that disproving this hypothesis would not disprove that Mars had a reversing core field, only that this may not be the explanation for the weak fields over the basin.

To summarize my review, I believe that this article is a significant and exciting contribution to the field. I propose two improvements that I believe can be carried out without substantial reanalysis. My second proposed improvement, the assessment of the probability of measuring a zero tangential magnetic field at the relative location of the Zhurong rover, may change the outcome of the investigation. The result may be that even though spacecraft data can not refute the reversing dynamo hypothesis, the rover data may. I think that the substantial magnetic field on the surface close to the basin rim (Figure 4c right panel as well as supplemental figure S5B) may be an early indication that this could be the case. Should this indeed be the case after the proposed investigation, then I believe this article would be just as relevant and publishable by Nature Communications.

Please reach out if my explanations of the second suggested improvement are not clear and I am happy to elaborate.

With my very best wishes,
Alain Plattner

Reviewer #2 (Remarks to the Author):

In this study, the authors investigate an alternative scenario for the observation that little or no magnetization is measured over the large impact basins on Mars. So far it has been interpreted that there was no dynamo action at the time of the impacts. The authors of this paper are investigating whether a reversing dynamo could have been responsible for the fact that no magnetic field is detected at orbital altitude - reversing dynamo is also observed on Earth and is in principle also to be expected on Mars. A reversing dynamo leads to the formation of layers with different polarity in the magnetization when the crust heated by the impact cools below the Curie temperature, some of which cancel each other out. The authors note that the magnetic field strength is strongly reduced by reversals over large impact basins and cannot be detected at 200 km orbit altitude, depending on the material to be magnetized, i.e. the saturation remanence. This would explain the observations and still allow for a Mars dynamo in the later evolution, i.e. at the time of the impacts. The authors also show that magnetic field measurements would have to be carried out at much lower altitudes in order to test this theory.

This is an interesting work that takes up an idea from Rochette (2006) and is now being systematically investigated using numerical models. The result is of great relevance for a better understanding of the thermal and magnetic evolution of Mars, but also for the evolution of the atmosphere, as explained in detail by the authors. I recommend publishing the study, but have a few criticisms that the authors should consider or discuss, as it may reduce the parameter range for successful scenarios.

The reversal and cooling rates are important for the analysis. The higher the reversal rate, the thinner the layer with homogeneous magnetization and the smaller the magnetic field strength at orbital height. The opposite is true for the cooling rates: The higher the cooling rate, the thicker a layer that can be magnetized during the stable magnetic field phases between reversals. While a large parameter range of the reversal rate was investigated in the study, the cooling rate and its influence on the results is less discussed. I also have some questions/concerns regarding the thermal model in particular.

1. On the one hand, this concerns the temperature distribution of the 'post-convective thermal profile'. It is argued that at the beginning, convection cools the melt pool efficiently up to a

temperature limit of 1175 °C. At this temperature, the crystal fraction is about 0.6 and the viscosity increases so much that convection no longer takes place. The reasoning is understandable so far, but not the implementation. So was the temperature set to a maximum of 1175 C everywhere where the temperature was higher due to the impact? If convection takes place in the melt pool and then stops, the temperature distribution would certainly be more complex.

2. What is certainly much more critical, however, is the assumption that heat transport is purely conductive below this temperature. At these temperatures, melt is still present (max. 40%) and heat can also be transported effectively by melt transport. This means that the cooling rates are underestimated! Since the cooling rates are relevant for the strength of the magnetic field, it would be very important to investigate in more detail how large the heat transport can be and what effects this has on the conclusion of the study.

3. The top layer seems to play the most important role for the strength of the magnetization. What is the maximum thickness of this layer - depending on the saturation of remanent magnetization - so that the field is not measurable at an orbital altitude of 200 km? That would be an interesting piece of information, but so far is not given in the manuscript.

4. What is the crust thickness distribution after the impact and do you assume a regolith layer on top of the crust after the impact? With other words, what is the assumed conductivity distributions when cooling sets in and does it vary for the scenarios?

5. See Table 1: Why is the conductivity in the mantle 100 W/mK?

6. A constant temperature at the lower boundary of your thermal model is assumed. How reasonable is this assumption for the large impacts and/or if the effective thermal conductivity is higher? What is the depth range of your model?

7. Line 360: It is written "Most features cool much more quickly than large impacts ... " Why is this the case and what are the expected differences in the cooling rates?

8. Lines 369-371: It is also claimed here that emplacement of layered volcanic deposits cools faster. Why is this the case?

9: What influence do magmatic intrusions have on your model and not just the remagnetization or demagnetization of the surface layer?

10. In large impacts, the fields in the interior are reduced but still relatively strong at the crater rim, wouldn't there be specific structures if the material outside the impact region is magnetized and wouldn't they also be visible in today's observations, i.e. 200 km orbit altitude?

Reviewer #3 (Remarks to the Author):

This well-written manuscript helps solve a major problem in our understanding of Mars. Scientists need to know when Mars lost its global magnetic field, relative to the climatic transition that occurred when most of Mars's early atmosphere vanished. Several studies suggest that Mars's dynamo died as late as ~3.9–3.7 billion years ago (Ga). However, other observations oppose this inference. That is, large impact basins appear weakly magnetized in crustal maps derived from measurements at orbital altitudes. Much of the community has cited these observations as evidence that the dynamo died perhaps as early as the age of the oldest basins (~4.1 Ga).

This new study is novel and impactful because it shows that the large impact basins could be strongly magnetized, in contrast to previous assumptions. Large impacts melt huge volumes of the crust, which take tens to hundreds of millions of years to cool and resolidify. If the Martian dynamo reversed its polarity at Earth-like rates, then each impact basin would eventually preserve strong magnetization from multiple layers of rocks with alternating polarities. The magnetic anomalies from these layers would effectively cancel out when measured at orbital altitudes. Using sophisticated models of basin cooling and magnetization, the authors convincingly demonstrate that this process is highly likely to explain the non-detection of strong magnetization (so far) above the largest basins on Mars. Importantly, they show how future missions (using helicopters, rovers, and/or humans) could detect and characterize basin magnetization with long (>100 km) traverses at low (<10 km) altitudes. This work thus has broad implications for Mars's geodynamic evolution, climate history, and future exploration.

I wholeheartedly recommend this manuscript for publication after minor revisions. I could only formulate a few comments and suggestions for the authors, which should be easy to address.

In order of appearance:

Lines 36–37. I suggest unpacking here the phrase “basins formed in a strong, stable field are expected to produce strong magnetic fields.” What is magnetized in the basin? How?

Line 54, 328–329. Do we love “in light of evidence that Mars presently has no inner core”? The cited reference states “The existence of an inner core cannot be determined by current data” (Figure 3 caption). Additionally, two recent studies in *Nature* (refs. 24, 25) suggest that the core is smaller than reported by Stähler et al. (2021), which weakens Stähler's (reasonable) reasoning against the existence of an inner core. That is, a smaller core would thus contain fewer light elements, and hence probably nucleate an inner core at relatively higher temperatures. Finally, core cooling is still very much required to drive a dynamo even in the presence of an inner core. Hemingway & Driscoll (2021, *JGR*, <https://doi.org/10.1029/2020JE006663>) discussed how Mars could have an inner core but no dynamo today. I'm not saying that an inner core exists, but I don't think that we can 110% rule it out.

Line 56. Did they “struggle” or were they simply tuned to predict that the dynamo would die by 4.0 Ga? My impression of cited reference 20, at least, is that they can make the dynamo live as long as they want by adjusting the initial temperature of the core (or the parameterization of the lower

boundary layer in the mantle or whatever or a combination of whatever). I would not struggle at all to change the predicted lifetime of the dynamo by a few hundred Myr in models.

Lines 112, 126–127: Since this section mentions the Curie depth and the unblocking temperature spectrum, it might be nice to have a hint about the mineralogy of the magnetic carrier(s) somewhere in these paragraphs.

Line 143: Might want to clarify again that “lower altitudes” means ~10 km here?

Line 148: Transects at what altitudes? Conducted by Mark Watney, a helicopter, or a spacecraft?

Lines 251–254: A non-expert reader might appreciate quoting the Curie temperatures of pyrrhotite and magnetite here. Additionally, the unblocking spectra of Tissint and magnetite seem similar. While Tissint is “pyrrhotite-dominated”, Figure 8 indicates that it retains a substantial magnetization fraction up to temperatures of >500 °C (i.e., above the Curie temperature of pyrrhotite), like magnetite (because Tissint has substantial amounts of titanomagnetite?). I wonder if, in these models, the basin fields would appear more sensitive to the assumed unblocking spectrum if more divergent endmembers (like pure pyrrhotite or pure hematite) were tested?

Lines 260–279. When you model late modification processes, you are setting the magnetization to zero in a fraction of the uppermost crust? You are not calculating how it would re-magnetize from the magnetization of the underlying crust? I can imagine the possibility that (1) aqueous alteration produces a (figurative) ton of magnetite near the surface; (2) that magnetite can acquire and retain a chemical remanent magnetization from the magnetization of the underlying basin material, which was magnetized in the dynamo field; (3) the altered crust therefore has strong magnetization traceable to a dynamo, although the magnetic minerals were formed after the dynamo died. These paragraphs seem to gesture at this possibility, but “remagnetize” and “demagnetize” are used interchangeably in a way that is perhaps confusing (to me, haha). Maybe additional clarification would be useful?

Line 327: Is this first sentence a tautology? The magnetic diffusion time for Mars is pretty short, so a dynamo requires core convection for essentially as long as it exists.

Lines 328–329: Same quibble as above about the inner core. Maybe “...Mars may not have a solid inner core...”?

Figure 3C. Due to the double arrow on the right side, the location of the surface is a bit ambiguous. I assume the surface is at the top of these figures? (Same comment for the supplementary figures, of course.)

Reviewer #1 (Remarks to the Author):

My first suggested improvement pertains to the likelihood of oblique impacts and their effect on the remanent magnetic field. I want to stress that I do not propose to use 3D impact simulations of oblique impacts, as the computational cost of such additional experiments would be too burdensome. However, a discussion of expected asymmetries in the temperature field of an oblique impactor and resulting magnetic field pattern would greatly help. Spatial patterns of the temperature profile of oblique impacts for the relevant basin sizes may be found in the literature. I could imagine that one side of an oblique impactor may not heat up as much and therefore cool quickly and have a higher chance of assuming the magnetic direction of a single polarity. However, the asymmetry may be limited to only the shallow-most portion of the impact basin, which is removed by random late impacts.

Although oblique impacts could introduce some asymmetry into the post-impact temperature profile, for a given final basin diameter we do not expect impact angle to have a strong effect on the volume of quickly cooled near-surface material which dominates the field at relatively rapid reversal rates. Although field morphologies may be more complex or offset with respect to the basin rim, oblique impacts should be similarly likely to produce basins that appear demagnetized at reversal rates $> 2 \text{ Myr}^{-1}$. We elaborate on this reasoning in a new section, *Sensitivity of results to impact angle*, beginning at line 344.

My second suggested improvement pertains to the interpretation of the Zhurong Mars rover magnetic data collected on Mars' surface inside Utopia Basin close to the rim. I propose to use the magnetization calculated for simulations relevant for Utopia Basin to obtain the intensity of the tangential magnetic field on Mars' surface within Utopia Basin. I propose to then use Zhurong data as a point location, because of the very short transect length. The calculated intensity of the tangential magnetic field obtained from the simulations can then be evaluated at all locations on a circle representing the location of the Zhurong rover relative to the basin edge. Some of these magnetic field values may be large, but others may be close to zero (e.g. at the

peaks of the radial field in the right panel of figure 4c, because the radial field typically resembles the potential field and the peaks of the potential field correspond to zero in the tangential field). From these magnetic field values, a probability can be calculated for recording a near-zero tangential field at the distance from the center of the basin that corresponds to the location of the Zhurong rover in Utopia Basin. If that probability is high, then this would present a strong confirmation for the hypothesis presented in this article. If, however, the probability is low, then this would be an indication that the reversing magnetic field hypothesis may not be a valid explanation for the weak fields in the larger basins. In my personal opinion, both outcomes are equally exciting and clearly worthy of publication in Nature Communications. I also want to note that disproving this hypothesis would not disprove that Mars had a reversing core field, only that this may not be the explanation for the weak fields over the basin.

To address the constraints from Zhurong data more quantitatively, we added a section to the Supplementary Information (S4) including calculated horizontal field intensities over simulated basins, following the recommended process in this comment, and added a more extensive discussion about Utopia and the Zhurong measurements. We find that the low horizontal field intensity measurements from Zhurong are compatible with a reversing dynamo during Utopia's formation given the likely extensive late alteration within Utopia and beneath the Zhurong site. Without better constraints on the magnetic properties of the basin material or the extent of late remagnetization, the observations from Du et al. (2023) neither support nor refute the possibility of a long-lived and reversing Martian dynamo.

Reviewer #2 (Remarks to the Author):

The reversal and cooling rates are important for the analysis. The higher the reversal rate, the thinner the layer with homogeneous magnetization and the smaller the magnetic field strength at orbital height. The opposite is true for the cooling rates: The higher the cooling rate, the thicker a layer that can be magnetized during the stable magnetic field phases between reversals. While a large parameter range of the reversal rate was investigated in the study, the cooling rate and its influence on the results is less discussed. I also have some questions/concerns regarding the thermal model in particular.

1. On the one hand, this concerns the temperature distribution of the 'post-convective thermal profile'. It is argued that at the beginning, convection cools the melt pool efficiently up to a temperature limit of 1175 °C. At this temperature, the crystal fraction is about 0.6 and the viscosity increases so much that convection no longer takes place. The reasoning is understandable so far, but not the implementation. So was the temperature set to a maximum of 1175 C everywhere where the temperature was higher due to the impact? If convection takes place in the melt pool and then stops, the temperature distribution would certainly be more complex.

We set the post-convection temperature to the temperature 40% of the distance (in temperature space) between the solidus and liquidus at a given depth, so this increases moderately with pressure (about a 100-200 K difference between the top and bottom of the melt pool). To clarify this point, we revised lines 544-548 of the text to "To produce a post-lockup temperature distribution, we truncated the post-impact temperature distribution at the lockup temperature, which we defined as 40% of the distance between the solidus and liquidus at the relevant depth (e.g. Fig. 2B; see Supplementary Information S6.2 for more details). For basaltic

and Martian mantle compositions at relevant depths, typical lockup temperatures were $\sim 1400\text{--}1500\text{ K}$ ⁸¹⁻⁸⁶.” We also added details of the assumed solidus, liquidus, and lockup temperature profiles to Supplementary Information S6.2.

This approach likely captures the broad vertical variation in post-lockup temperature within the melt pool. We do not expect significant thermal heterogeneity at the time of lockup since the rapid convection should maintain relatively homogeneous temperatures throughout the early melt pool.

An important related question is whether other areas of the impact structure experience significant thermal evolution before lockup. Although crystallization timescales can vary depending on a number of factors, estimates of the time for large magma ponds to cool to the critical melt fraction are on the order of $100\text{--}5,000$ years (Reese & Solomatov, 2006). This is much faster than virtually any cooling timescales outside the melt pool; only the shallowest $50\text{--}500$ meters have shorter conductive cooling timescales, and this is below the spatial resolution of our cooling model. In summary, lockup happens much too quickly either for structure to develop within the melt pool or for appreciable evolution of the post-impact thermal profile in the surrounding conductive regions.

To address these points in the paper, we revised the beginning of the *Modeling basin cooling* section of the main text (now at line 537) to “This process for computing post-impact heating yields temperatures above the melting point of basalt where material is melted or vaporized by the impact. After the impact, convection within the melt pool would efficiently mix and cool the material, homogenizing its temperature distribution until the crystal fraction in the melt reaches ~ 0.6 and the viscosity of the material increases by two orders of magnitude ^{80,81}. Since cooling to this lockup temperature occurs over timescales of just $10^2\text{--}10^4$ years ⁴⁸, the thermal profile outside the melt region does not evolve appreciably before lockup. We therefore began our cooling models immediately after convection ceased.”

2. What is certainly much more critical, however, is the assumption that heat transport is purely conductive below this temperature. At these temperatures, melt is still present (max. 40%) and heat can also be transported effectively by melt transport. This means that the cooling rates are underestimated! Since the cooling rates are relevant for the strength of the magnetic field, it would be very important to investigate in more detail how large the heat transport can be and what effects this has on the conclusion of the study.

Melt migration may play an important role in cooling the basin, but we expect the net effect to actually be *slower* cooling of shallow material. This could make the process proposed here moderately more efficient.

To understand the impact of melt migration on our models and results, there are two key points to consider. First, the mechanism by which melt migration transports heat is by emplacing deep melt closer to the surface. Cooling will therefore be more rapid in melt source regions which, by definition, are at temperatures above the solidus. Second, the solidus is typically $\sim 500\text{ K}$ above the Curie temperature of magnetite (and $\sim 700\text{ K}$ above that of pyrrhotite).

In our problem, melt migration will behave in a way that is conceptually similar to convection: it will decrease the cooling timescale of the basin as a whole and make cooling to the solidus more rapid, but it cannot decrease cooling timescales anywhere where temperatures are below the solidus. **Since magnetization acquisition begins at much lower temperatures, melt migration will ultimately not cause more rapid cooling during magnetization acquisition.**

Another important point here is that modeling suggests that melt migration can persist in large basins over timescales of 10^7 - 10^8 years (Reese & Solomatov (2006); Black & Marchi (2024), after which point cooling will still continue via conduction and solid-state convection. The fact that these melt migration timescales are comparable to the conductive cooling timescales in this work affirms that this process does not substantially reduce overall basin cooling timescales. **However, this long timescale introduces a key conceptual difference between melt migration and early convection in the melt pool: melt migration can move hot material to shallow depths where magnetization has already been set. Accordingly, while this process makes the cooling of deep material more rapid, it yields longer effective cooling timescales in shallow material.** Given that the longest-lived source of melt will be material well below the Curie depth, this process could effectively slow magnetization acquisition within the entirety of the basin. In this case, a reversing dynamo would more efficiently reduce basin field strengths than we predict in this study.

Perhaps more importantly, since melt can be brought to very shallow depths or erupted, the strongly magnetic shallow material in our models would likely experience the greatest increase in effective cooling timescales. Because the shallow, quickly cooled layer contributes most of the measured magnetic fields, this could make the process proposed in this work substantially more efficient at reducing basin fields than implied by our current models.

Since incorporating melt migration may provide an efficient mechanism for increasing the cooling timescales of shallow material or resetting near-surface magnetization, it would be valuable to include it in future work aiming to use more sophisticated thermal models targeting specific basins. However, the simple cooling models used in this work are sufficient to capture the first-order effects of reversing dynamo on basin magnetism more broadly, which was the ultimate goal of this study.

We added two paragraphs (lines 277-294) summarizing this discussion to the section *Estimating the likelihood that basins are misclassified as demagnetized* and revised the sentence beginning at line 447 to “More sophisticated modeling including melt migration and better estimates of the extent of late remagnetization will ultimately be necessary to fully characterize expected low-altitude fields above specific basins.”

3. The top layer seems to play the most important role for the strength of the magnetization. What is the maximum thickness of this layer - depending on the saturation of remanent magnetization - so that the field is not measurable at an orbital altitude of 200 km? That would be an interesting piece of information, but so far is not given in the manuscript.

Since the field from the top layer of magnetization depends not just on the thickness of the layer but also on its diameter, the critical depth for the field from this layer to be observable varies with basin size. To address this point, we added Figure S7 to the supplement which shows the predicted field strength from this top layer as a function of Mr_s and reversal frequency for 200-1000 km basins (above this size the peak fields did not appear to increase significantly due to the changing field horizontal scale). We marked on this plot the regions of parameter space where the field from the top layer alone would exceed our 5 nT threshold, which corresponds to the maximum layer thickness for which the field would not be observable at 200 km altitude.

4. What is the crust thickness distribution after the impact and do you assume a regolith layer on

top of the crust after the impact? With other words, what is the assumed conductivity distributions when cooling sets in and does it vary for the scenarios?

For analytically derived temperature profiles, we use the material properties in Table 1 to model post-impact cooling. For the 1800 km diameter basin we deviate from the conductivity profile in Table 1 to ensure the pre-impact thermal profile is in thermal equilibrium. Since we considered purely conductive cooling, this is necessary to establish self-consistency between the mantle temperature structure and the surface temperature and to avoid introducing magnetic artifacts from re-equilibration of the background temperature profile. More sophisticated thermal models would be necessary to study specific basins in detail, but this approach is appropriate for the purpose of characterizing the broad, first-order effects of reversing fields on basin magnetism.

In terms of a regolith layer, we also included a 1 km relatively insulating regolith layer for all basins. However, we note that this layer still has a relatively high conductivity ($2 \text{ W m}^{-1} \text{ K}^{-1}$) which is similar to the assumed crustal values, and likely does not significantly influence the cooling.

To clarify these points, we added a paragraph at line 580 more explicitly detailing our assumptions and Fig. S8 plotting the conductivity profiles for analytical and iSALE basins.

5. See Table 1: Why is the conductivity in the mantle 100 W/mK ?

We assumed a high mantle conductivity to approximate the efficient heat transport associated with convection in the mantle without explicitly modeling convection, which would be much more computationally expensive. Effectively, this conductivity value maintains a relatively flat temperature profile in the mantle that is relatively similar to the expected mantle adiabat in modeled equilibrium thermal profiles (e.g. Plesa et al. (2015)). Because the Curie depths of all major minerals are very shallow compared to the 100 km lithosphere-mantle boundary, we expect our results to be insensitive to this simplification. To clarify this in the paper, we added the sentence “Since our thermal models were purely conductive, for these basins we parameterized convection in the mantle with a high conductivity ($k \sim 100 \text{ W m}^{-1} \text{ K}^{-1} \dots$ ” at line 585. We also added Supplementary Information S6.3, which includes a more thorough explanation of and justification for this assumption.

6. A constant temperature at the lower boundary of your thermal model is assumed. How reasonable is this assumption for the large impacts and/or if the effective thermal conductivity is higher? What is the depth range of your model?

The maximum depth in our models ranges from 100 km for a 200 km diameter basin to 1000 km for an 1800 km diameter basin. These depths were chosen such that the thermal perturbation of the impact at the bottom boundary was on the order of tens of K (equivalent to a temperature increase of $\sim 5\%$). This allowed us to capture nearly all of the heating from the impact without sacrificing spatial resolution in our cooling models.

This approximation is likely also reasonable because the impact induces a temperature inversion at the top of the mantle. Since this would be relatively buoyant, downward heat transport would be less efficient and the thermal anomaly from the basin would likely not propagate substantially deeper into the mantle. Since we assume relatively high conductivities in the mantle to parameterize the convection that does occur, it is more likely that we *overestimate* downwards heat transport. If this were the case, the true cooling timescales would be slightly longer and lead to weaker magnetic signals. However, the upper 50 km region where

magnetization is actually acquired is probably not very sensitive to the thermal conditions of the mantle.

In summary, because we selected model depths where temperatures were already within ~5% of constant and heat transport downwards is likely inefficient, this constant temperature boundary condition is appropriate. We note that a more sophisticated approach would be necessary for much larger impacts where heating of the mantle is much more extensive (e.g. Roberts & Arkani-Hamed (2014)).

To address this point, we expanded our discussion of our chosen model domain (lines 551-557) and added a section to the Supplementary Information (S6.3) explaining the points discussed here.

7. Line 360: It is written "Most features cool much more quickly than large impacts ... " Why is this the case and what are the expected differences in the cooling rates? & 8. Lines 369-371: It is also claimed here that emplacement of layered volcanic deposits cools faster. Why is this the case?

It has to do with the thickness of each single cooling unit. If the lateral extent of heating is much larger than the depth, such that cooling through the surface is much faster than cooling through the sides, the cooling time is approximately given by the conductive cooling timescale

given in the text ($\tau = \frac{d^2}{\kappa}$, with a thermal diffusivity $\kappa \sim 10^{-6} \text{ m}^2 \text{ s}^{-1}$). Using this expression, the κ conductive cooling time for a 50 km thick layer is ~80 Myr. However, the timescale for a 1 km layer is 0.03 Myr. In principle, the time to emplace and cool fifty 1-km-thick layered lava flows would be ~1.5 Myr—just 2% of the time to cool the same volume of hot material all emplaced at once. If the flows were 100 m thick, the time to successively emplace and cool 500 layers (a total of 50 km) would be just 0.1 Myr, or ~0.1% of the time to cool a single 50 km layer. Even if the cooling time were moderately longer to account for each flow cool incompletely before emplacement of the next, the cooling times would still be much shorter than for a basin, especially since basin heating can penetrate well into the mantle.

There are second-order effects from the thickness and lateral extent of heating, since smaller bodies may also cool conductively through the sides and/or bottom. This is why most intrusive bodies, which are typically thinner and smaller than the thermal anomalies of large impacts, also cool more quickly than large impact basins despite being at depth. On Earth, even batholiths typically cool over timescales of ~10 Myr (e.g. Housen, Beck Jr., & Burmester (2003)).

To provide context for this point, we added the note "(virtually all other geologic features are much smaller and/or episodically emplaced in thin, fast-cooling layers)" to this sentence, which begins at line 421.

9: What influence do magmatic intrusions have on your model and not just the remagnetization or demagnetization of the surface layer?

We would expect the effects of magmatic intrusions to be qualitatively similar to those of late impacts, which introduce relatively small-scale field heterogeneities visible primarily at low altitudes. However, the impact to the field would typically be less than that of a late crater of the same size because intrusions typically affect deeper, less magnetic material. Although the intrusion itself may acquire magnetization either from the surrounding material or a dynamo field (if it occurs before the dynamo's cessation), the impact of this would depend entirely on the properties of the field and intrusion.

We added the text “Deeper material could also be remagnetized by deep alteration and magmatic intrusions. These processes would likely have a smaller effect on the overall magnetic field strength since material at depth is typically already less magnetic, but could introduce additional heterogeneities in the field (similar to those produced by late impacts) which could be observable at low altitudes,” at lines 317-321 to clarify the likely effect of deeper remagnetization processes.

10. In large impacts, the fields in the interior are reduced but still relatively strong at the crater rim, wouldn't there be specific structures if the material outside the impact region is magnetized and wouldn't they also be visible in today's observations, i.e. 200 km orbit altitude?

These structures may well be observable in today's observations depending on the magnetizing field geometry and relative magnetic properties of material within and outside of the basin. In particular, if the magnetic mineralogies of the crust and mantle are substantially different (as we might expect) the magnetic signature from the basin may be of much smaller magnitude than the surrounding heterogeneous crustal fields. There should certainly be future work to try to identify what specific basin field features might be observable today and diagnostic of the field state during cooling, but this would be a major and necessarily separate effort.

Reviewer #3 (Remarks to the Author):

Lines 36–37. I suggest unpacking here the phrase “basins formed in a strong, stable field are expected to produce strong magnetic fields.” What is magnetized in the basin? How?

We changed this to say “Since basin formation heats large volumes of material which then acquires magnetization during cooling, basins formed in a strong, stable field are expected to produce strong magnetic fields. It is therefore typically argued that these weakly magnetic basins formed in the absence of a dynamo ^{5,6}.”

Line 54, 328–329. Do we love “in light of evidence that Mars presently has no inner core”? The cited reference states “The existence of an inner core cannot be determined by current data” (Figure 3 caption). Additionally, two recent studies in Nature (refs. 24, 25) suggest that the core is smaller than reported by Stähler et al. (2021), which weakens Stähler's (reasonable) reasoning against the existence of an inner core. That is, a smaller core would thus contain fewer light elements, and hence probably nucleate an inner core at relatively higher temperatures. Finally, core cooling is still very much required to drive a dynamo even in the presence of an inner core. Hemingway & Driscoll (2021, JGR, <https://doi.org/10.1029/2020JE006663>) discussed how Mars could have an inner core but no dynamo today. I'm not saying that an inner core exists, but I don't think that we can 100% rule it out.

We appreciate the point that revised core size estimates weaken the argument against an inner core! Line 55 now reads “in light of evidence that Mars may have no inner core.” We also added a reference to Le Maistre et al. (2023), which suggests that an inner core is unlikely on the basis of InSight radio tracking data, at the end of this sentence.

Line 56. Did they “struggle” or were they simply tuned to predict that the dynamo would die by 4.0 Ga? My impression of cited reference 20, at least, is that they can make the dynamo live as long as they want by adjusting the initial temperature of the core (or the parameterization of the

lower boundary layer in the mantle or whatever or a combination of whatevers). I would not struggle at all to change the predicted lifetime of the dynamo by a few hundred Myr in models.

We changed this sentence to “Although many models of the planet’s thermal history assume the dynamo shut down at 4.0 Ga ²²⁻²⁵, some imply that certain ranges of mantle reference viscosity, core conductivity, and initial core superheating could permit a later cessation ^{22,25}.”

Lines 112, 126–127: Since this section mentions the Curie depth and the unblocking temperature spectrum, it might be nice to have a hint about the mineralogy of the magnetic carrier(s) somewhere in these paragraphs.

We revised this sentence, now beginning on line 120, to “The upper bound on field strength is set by the Curie depth (about 50 km for the expected magnetite-bearing mineralogy ⁴⁴), where equilibrium temperatures are too high to retain magnetization,” and added further discussion of Martian magnetic mineralogy in lines 264-269.

Line 143: Might want to clarify again that “lower altitudes” means ~10 km here?

We changed the beginning of this sentence to “At 10 km altitude...”

Line 148: Transects at what altitudes? Conducted by Mark Watney, a helicopter, or a spacecraft?

Under the perhaps unfair assumption that Mark Watney (or any human for that matter) would be ill equipped for a 100 km transect, we revised this sentence (now at line 157) to “Long (~100 km) magnetic mapping transects at or below 10 km altitude (e.g. by balloons or helicopters ⁴⁶) may therefore be required to determine the strength and variability of the magnetic field during basin formation.”

Lines 251–254: A non-expert reader might appreciate quoting the Curie temperatures of pyrrhotite and magnetite here. Additionally, the unblocking spectra of Tissint and magnetite seem similar. While Tissint is “pyrrhotite-dominated”, Figure 8 indicates that it retains a substantial magnetization fraction up to temperatures of >500 °C (i.e., above the Curie temperature of pyrrhotite), like magnetite (because Tissint has substantial amounts of titanomagnetite?). I wonder if, in these models, the basin fields would appear more sensitive to the assumed unblocking spectrum if more divergent endmembers (like pure pyrrhotite or pure hematite) were tested?

We added text at lines 264-269 including the Curie temperatures of magnetite and pyrrhotite and clarifying the significance of this for magnetization acquisition.

Although considering endmembers like pure hematite or pure pyrrhotite could give significantly different final basin fields (mostly reflecting the different Curie depths), these endmember mineralogies are dissimilar to the Martian meteorites we have (e.g. Rochette et al., 2005), suggesting they probably would not be realistic. We also note that another key difference between a spectrum like Tissint’s and pure magnetite is the range of temperatures over which unblocking occurs. Tissint’s shallow unblocking spectrum means magnetization acquisition occurs over longer timescales compared to a pure endmember composition that unblocks near that mineral’s Curie point. Considering compositional endmembers could exaggerate the impact of characteristic Curie depth while underestimating the effect of having a range of unblocking temperatures.

Lines 260–279. When you model late modification processes, you are setting the magnetization to zero in a fraction of the uppermost crust? You are not calculating how it would re-magnetize from the magnetization of the underlying crust? I can imagine the possibility that (1) aqueous alteration produces a (figurative) ton of magnetite near the surface; (2) that magnetite can acquire and retain a chemical remanent magnetization from the magnetization of the underlying basin material, which was magnetized in the dynamo field; (3) the altered crust therefore has strong magnetization traceable to a dynamo, although the magnetic minerals were formed after the dynamo died. These paragraphs seem to gesture at this possibility, but “remagnetize” and “demagnetize” are used interchangeably in a way that is perhaps confusing (to me, haha). Maybe additional clarification would be useful?

Yes, we set the magnetization fraction to zero. This was partly a practical choice—it would be too underconstrained of a problem to explicitly compute remagnetization by crustal fields—but it is also a good and useful assumption here for a few reasons. First, although aqueous alteration can form magnetite, CRM efficiencies are typically low in Earth’s oceanic crust (e.g. Dyment et al., 2015). Second, for basins cooled in reversing fields, we find that near-surface crustal fields would likely be highly heterogeneous and significantly weaker than the original dynamo field (e.g. Figure S6), especially if characteristic saturation remanence intensities are low. So alteration within these basins may be less likely to produce bodies of strong, coherent magnetization. Since alteration can either create or remove magnetic minerals depending on the conditions, this introduces another important uncertainty.

Given these complicating factors, we chose to consider two endmember cases: one where late modification processes completely remove magnetization (Figure 6, bottom), and one where late modification processes are insignificant (Figure 6, top). These likely capture the range of possible outcomes.

To clarify these points in the paper, we added the sentences (beginning at line 307) “If these late modification processes either removed magnetic material or produced extensive demagnetization, they could substantially reduce the magnitude of basin fields. The heterogeneous and relatively weak near-surface fields may make late modification processes more likely to result in demagnetization (Figs. 3, S1-S5).”

Line 327: Is this first sentence a tautology? The magnetic diffusion time for Mars is pretty short, so a dynamo requires core convection for essentially as long as it exists.

Yes, this is redundant, but we include this point as clarification for readers who may be less familiar with dynamos.

Lines 328–329: Same quibble as above about the inner core. Maybe “...Mars may not have a solid inner core...”?

This sentence (now at line 387) now begins “Although core crystallization is not a likely power source for the dynamo, especially if Mars does not have a solid inner core ^{20, 21...}”

Figure 3C. Due to the double arrow on the right side, the location of the surface is a bit ambiguous. I assume the surface is at the top of these figures? (Same comment for the supplementary figures, of course.)

We corrected this on all Figures 3 and S1-S5.

REVIEWERS' COMMENTS

Reviewer #1 (Remarks to the Author):

Dear Authors,

I have re-reviewed your research article and am excited about the updated results and their discussions. All of my suggestions from my previous review were well addressed. I thus fully support the publication of this article. The results are timely, noteworthy, and well supported.

I found only a single typo in the supplemental materials: Section S4, paragraph 3 line 4: "Additionally, theating" should read "Additionally, heating".

Best wishes,
Alain Plattner

Reviewer #1 (Remarks on code availability):

The software provided on <https://github.com/ssteele1111/BasinCoolMag> consisted of two components:

BasinCooling and BasinMagCalc. I did not succeed in running either, but believe that a motivated user will be able to fix the issues I encountered and should be able to reproduce the results after some work.

BasinCooling:

The provided README instructions are rather short and appear to skip a few important instructions. For example, the instructions "Change the run target in mars_heat.cc main, then compile and run mars_heat.cc." I found that the following steps were required:

To compile the code, two steps are needed:

cmake .

make

This then reveals, that other locations within the mars_heat.cc need to be adapted: The directory to support_code in mars_heat.cc on lines 54 and 53 need to be adapted. Also, an additional package,

libconfig++-dev, needs to be installed

After this, the compilation ran smoothly (perhaps some unspecified packages that I already had installed on my machine were also missing).

To run the code, the config.cfg file that was automatically created using the Preprocessing folder needs to be repaired: A closing curly bracket is misplaced and appears at the end of the file instead of at the end of the block "heat_solve_parameters". Also, there is an extra curly bracket that needs to be deleted. Also, T_surf within the heat_parameter block appears twice and one of these duplicates needs to be deleted.

After these fixes, the code ran, but aborted. I tried increasing the parameter iteration_coefficient in the config.cfg file, but then the code crashed for a different reason with the error message

"The violated condition was:
dealii::numbers::is_finite(sum)"

I gave up after I couldn't figure out what led to this error.

BasinMagCalc:

I needed to edit the file late_remag_numba.py to use the correct type of slashes for subdirectories (this could be implemented to work automatically based on the operating system).

After correcting this, I got the error message:

```
..... basiniter.py", line 24, in <module>
imgridZ=np.array(np.cos(np.linspace(0,2*np.pi,100)),np.sin(np.linspace(0,2*np.pi,100)))
TypeError: Cannot construct a dtype from an array
```

Reviewer #2 (Remarks to the Author):

The authors have responded in detail to my questions and comments. I agree with most of the answers and think that the manuscript has improved a lot. This work is certainly interesting and can stimulate further discussion about Mars dynamo.

But as note, after they provided more information on the thermal model, I realized that the model is very simple and I am not absolute certain whether it really captures the thermal evolution of an impact heated region. In particular, the way in which the thermal evolution of the mantle was

implemented with a fixed effective thermal conductivity (chosen to match the background temperature profile) and the fact that the model does not take into account the cooling of the background temperature, as the lower temperature of the mantle is fixed, raises concerns. However, since I'm not sure if a more appropriate approach for the ~100 Myr time period under consideration would make a significant difference, I would accept the current approach for this study.

A few minor points that should be changed:

Figure S8: Here the caption is not consistent with the legend (blue and orange line). This should be corrected accordingly.

In the text it says pre-impact temperatures and then further down in the figure caption post-impact conditions. Why are the pre-profiles different and why they are different conductivities used in the mantle? In the text they only mention $K \sim 100 \text{ W/mK}$ to fit the background profile.

Eq. 4 needs a bit more information of the parameters, definition of σ , u and t ? σ what values have been used? Why you use differ between n and t ?

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Best wishes,
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The typo in the supplement has been corrected.

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```

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After these fixes, the code ran, but aborted. I tried increasing the parameter iteration_coefficient in the config.cfg file, but then the code crashed for a different reason with the error message

```
"The violated condition was:  
dealii::numbers::is_finite(sum)"
```

I gave up after I couldn't figure out what led to this error.

We have corrected bugs in the cooling code and updated the README to more thoroughly describe the process to install required dependencies and go through the full process of modeling basin cooling. With these changes, we were able to download deal.II and the required dependencies, download the codes from GitHub, and run all steps of the thermal modeling for a test analytical basin on a fresh machine.

BasinMagCalc:

I needed to edit the file late_remag_numba.py to use the correct type of slashes for subdirectories (this could be implemented to work automatically based on the operating system).

After correcting this, I got the error message:

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TypeError: Cannot construct a dtype from an array
```

We have changed the code to automatically detect slashes for subdirectories and corrected the error in line 24. We tested the process of running magnetic calculations for a test basin on a fresh machine to confirm that the version of the code in Github works without correctly.

Reviewer #2 (Remarks to the Author):

The authors have responded in detail to my questions and comments. I agree with most of the answers and think that the manuscript has improved a lot. This work is certainly interesting and can stimulate further discussion about Mars dynamo.

But as note, after they provided more information on the thermal model, I realized that the model is very simple and I am not absolute certain whether it really captures the thermal evolution of an impact heated region. In particular, the way in which the thermal evolution of the mantle was implemented with a fixed effective thermal conductivity (chosen to match the background temperature profile) and the fact that the model does not take into account the cooling of the background temperature, as the lower temperature of the mantle is fixed, raises concerns. However, since I'm not sure if a more appropriate approach for the ~100 Myr time period under consideration would make a significant difference, I would accept the current approach for this study.

We have added a paragraph at the end of the discussion section recognizing the limitations of this simple thermal modeling and emphasizing the role more sophisticated models will play in answering many of the remaining questions.

To address the point about secular cooling, we also added two paragraphs to Supplementary Information S6.3 discussing the expected impact of changing background temperatures in the mantle.

Figure S8: Here the caption is not consistent with the legend (blue and orange line). This should be corrected accordingly.

This has been corrected.

In the text it says pre-impact temperatures and then further down in the figure caption post-impact conditions. Why are the pre-profiles different and why they are different conductivities used in the mantle? In the text they only mention $K \sim 100 \text{ W/mK}$ to fit the background profile.

We added text to the caption of this figure to clarify the thermal conditions we assume for the iSALE simulations and refer to parts of the text which provide more detail.

Eq. 4 needs a bit more information of the parameters, definition of sigma, u and t? Sigma what values have been used? Why you use differ between n and t?

We changed the qualifier after Eq. 4 (now Eqn. 11) to “for position $\mathbf{x} = (x,y,z)$, time t , temperature T , equilibrium surface temperature T_{eq} , domain Ω with boundary $d\Omega$, top boundary Γ_1 , and left boundary Γ_2 (the axis of symmetry). Here, σ is the Stefan–Boltzmann constant, ρ is the density, C_p is the specific heat capacity at constant pressure, and k is the conductivity.” We also corrected dT/dn to $\hat{\mathbf{n}} \cdot \nabla T$.