nature portfolio

Peer Review File

A Modern Dynamo at Triton as a Consequence of Its Capture by Neptune

Corresponding Author: Ms Lana Tilke

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Comments for the Author)
This manuscript presents an investigation into the potential for a present-day dynamo within Triton, motivated by how its capture history could have initiated early internal differentiation. By modeling the evolution of Triton's interior, the authors seek to calculate when a dynamo may exist and assess its maximum lifetime. They explore three plausible core crystallization regimes and demonstrate the conditions under which Triton may still have an active dynamo. They find that a nominal FeS-layer (S-rich) model could sustain a partially molten core for more than twice the age of the Solar System.
The topic is highly relevant: Triton is a prime target for future outer solar system missions, and constraining its internal structure and magnetic potential provides new context for understanding captured and differentiated icy satellites. The manuscript is clearly written, and the modeling framework is thoughtfully constructed.
However, several key assumptions, particularly regarding the initial thermal state following capture, require fuller justification and quantitative grounding. The dynamo lifetime, which is the primary result, appears highly sensitive to the assumed initial conditions. I strongly encourage the authors to expand their discussion and analysis of these parameters.

Major comments

1. Initial thermal state

The results hinge on the adopted initial core and mantle temperatures (1600-2400 K), yet these values are assumed, not derived. Because the authors note that a hotter initial state extends the dynamo lifetime, the results depend critically on the adopted starting temperatures.
I recommend the addition of a dedicated methods section that derives plausible initial core and mantle temperatures from an energy budget, and evaluates how circularization timescales (10-100 Myr) influence whether such heating would actually produce or preserve a "hot start," given concurrent cooling. It would be useful to state explicitly whether the assumption of a well-mixed, fully liquid core is physically consistent with the derived temperature range and core composition. While I recognize that the models are aimed at estimating dynamo longevity rather than reconstructing exact initial conditions, the adopted starting temperatures are presented without clear physical justification, and a quantitative discussion of their origin and plausibility is necessary to substantiate the main conclusions.
I acknowledge that Figure 4 presents a useful sensitivity analysis, but the physical motivation for the adopted initial temperatures and compositions remains unclear. The parameter ranges appear somewhat arbitrary, and without a derivation from an energy budget, it is difficult to assess whether the tested values are representative of realistic post-capture conditions.

2. Core composition

The three end-member crystallization modes (FeS layer, iron snow, and central Fe solidification) provide a useful framework, but represent discrete points within a continuous Fe-S compositional space. The models use cores containing 33 wt% S or 12.5 wt% S: how were these values selected? Given the sensitivity of chemical buoyancy and electrical conductivity to sulfur content, clarifying the chosen Fe/S ratios and their motivation (especially when extrapolating from Ganymede to Triton) would strengthen the analysis.

The three crystallization regimes are adapted from Ganymede-based models, yet Ganymede's formation environment and thermal history differ substantially from Triton's. While Ganymede provides a valuable comparative example because it maintains an active dynamo, this alone may not justify adopting its interior regimes as direct analogues. Transitional Fe-S compositions, rather than the Ganymede-style end members, could be more realistic for Triton given its capture history, mixed volatile inventory, and likely higher oxidation state. If feasible, a qualitative exploration or scaling across intermediate compositions would show whether the dynamo outcome is robust across plausible Fe/S values or depends on a narrow range.

Minor comments

- A brief comparison with Pluto (Triton's closest analogue in size and composition) would strengthen the discussion. If capture-induced tidal heating and differentiation are responsible for a long-lived metallic core and dynamo at Triton, then Pluto's apparent lack of such features provides a valuable control case. Discussing how compositional or volatile differences might explain their divergent outcomes would place the results in a broader planetary-formation context.

- The study's motivation centers on the detectability of Triton's magnetic field, yet no quantitative field strengths are reported. I encourage the authors to estimate surface and external magnetic field strengths for each end-member model using established dynamo scaling laws. The quantities already available (core size, convective power, density, rotation rate) should allow approximate dipole field estimates at the core/mantle boundary, Triton's surface, and at Voyager 2's closest approach. Comparing these values to instrument detection limits would clarify the observational implications. A brief discussion of how such intrinsic fields could be distinguished from induction responses to Neptune's magnetosphere would further enhance the paper's relevance to future mission planning.

- Very minor: It may also be helpful to define the Rossby number alongside the magnetic Reynolds number, since both are discussed as critical dynamo criteria. This would improve clarity for readers outside the immediate subfield.

Recommendation

The paper presents an interesting and potentially impactful advance in our understanding of Triton's interior evolution. The modeling framework itself appears sound, and the results offer genuinely novel insights into Triton's possible magnetic history. With the inclusion of a physically motivated derivation of initial thermal conditions and a clearer treatment of compositional sensitivity, it would be suitable for publication in a high-impact journal. At present, however, the conclusions rely on assumptions that require further quantitative justification.

(Remarks on code availability)

Reviewer #2

(Comments for the Author)
Summary

This manuscript argues that Triton's "hot start" and subsequent core crystallization pathways can drive and sustain a present-day core dynamo, yielding surface fields of the order of a few microtesla and complicating ocean-induction responses. The paper is timely and potentially impactful: a live dynamo at Triton would reshape priorities for the Neptune–Triton system and for interpreting magnetometer data. The modeling framework is clear and the three crystallization regimes (FeS top-down layer, iron snow, Earth-like inner core) cover the most important parts.

However, in its current form, the claim of a dynamo on Triton feels a little bit weak on several core aspects (critical magnetic Reynolds number, type/morphology of the dynamo, and explicit observational discriminants versus induction). Addressing the major comments below would, in my view, lift this to Nature Astronomy level.

Overall recommendation: Major revision

Major comments

1) Analysis of your produced field (a few microtesla)

You state that a roughly ~1 μT surface field could overwhelm an induction signal and would have been undetectable by Voyager 2 at its closest approach (~0.04 nT at the spacecraft), but there is no quantitative comparison between plausible induced responses and your intrinsic field predictions. Perhaps you could add synthetic magnetometer time series or, at minimum, comment on the expected amplitude of an inductive response for a range of ocean salinities/conductivities. Your work provides valuable predictions for the dynamo field, and it would be useful to understand how it can be quantitatively compared to the ocean's induction response. This would clarify where/when an intrinsic field biases induction inversions.

2) Onset of the dynamo

You adopt a critical Rm of nearly 40 (with a stated plausible range 10–100) and implement an on/off threshold. I understand that the cited literature goes along with these values. However, this typical value must be taken cautiously: it is well known that the onset of dynamos can significantly increase with turbulence and with small magnetic Prandtl number (here Pm~ 10^{-6} in metallic cores). In the range reported by Christensen, Pm in numerical simulations remains relatively large (around 0.1–10) and no clear increase of the onset is observed. But there is evidence that the onset increases rapidly as Pm decreases (see, for instance, Schekochihin et al. 2004, Phys. Rev. Lett. 92, 054502). Interestingly, all experimental turbulent dynamos (with the exception of the VKS dynamo, due to ferromagnetic impellers) have failed to observe dynamos up to Rm \sim 200, possibly due to the effect of turbulence. This matter on the possible higher value of the critical Rm should be discussed.

3) Scaling laws and proximity to onset

Similarly, you could discuss the fact that the scaling laws used for predicting the field strength might differ depending on whether we are dealing with a thermal-convection dynamo, an iron-snow dynamo, or other mechanisms. Please also discuss the applicability of these scalings depending on the parameters (rotation, etc). Similarly, given that your solutions sit quite close to dynamo onset, scaling laws may change compared to the one used here. A clarification would help in understanding the consequences of the work reported here.

4) Spectral imprint of a stratified top layer

Please discuss how a stratified top layer (thermal, FeS, or snow) would imprint the spectrum via a skin effect that damps high-degree harmonics. You briefly allude to this in the Discussion, but the paper would benefit from expanding this quantitatively for Triton's parameters. Again, this might be interesting for analyzing future magnetometer data.

5) Narrative of Fig. 3

I think you should clarify the four evolutionary stages when discussion Fig.3. In Fig. 3 you alternate between super-adiabatic and sub-adiabatic phases. Please make explicit that it is the super-adiabatic heat flux (not absolute T) that sets buoyancy power and thus Rm; hence why early rapid cooling boosts the dynamo before mantle heating temporarily suppresses it. A short, self-contained explanation near Fig. 3a–b would help non-specialists.

6) Discontinuities in Rm and B near ~2.5 Gyr
I am not sure I understand the origin of the strong jump in Rm at t=2.5Gyr.: why does the onset of chemical buoyancy produce such a large change in Rm? As the FeS layer is small at first and gradually increases, I would expect a similarly gradual effect on Rm. Please clarify whether the jump is physical, numerical (e.g., step-function thresholding), or both.

7) Dimensionless numbers for modeling (near-onset case)

People doing numerical simulations of astrophysical dynamos might be interested in having an example of a dynamo operating very close to its onset. You might consider giving additional estimates for Triton, such as the Rossby number and Ekman number, in order to facilitate modeling of this problem (along with Re, Pm, and, if relevant, the Elsasser number).

8) Double diffusion vs. iron snow

"Iron snow" in your models is not, per se, classical double-diffusive convection. Please clarify whether additional double-diffusive layering is expected, and whether such layers could delay convective restarts or modulate the chemical buoyancy flux in your FeS and inner-core regimes. A brief scope note would be sufficient.

9) Connect model outputs to observations

You should relate your model outputs more closely to observations: details of the magnetic environment near Triton; Neptune's rotating, non-axisymmetric field as a driver of induction; mission durations and instrument sensitivity; and how these tie to your outputs B_surf, expected topology given small Rm, secular variation, etc.). This would make the measurement implications more concrete.

10) Time-evolving onset conditions

I was wondering whether your data on the size and composition of the core can be used to estimate more accurately how the critical superadiabaticity for dynamo action changes over time. In other words, as the interior evolves, the structure may change sufficiently for the size and conductivity of the core to change; therefore, the onset may not be just a fixed line in the

heat-flow plots? Maybe this effect is negligible ?

Summary: The manuscript links Triton's hot start and subsequent crystallization pathways to a present-day core dynamo, predicting surface fields of a few microtesla and clear implications for ocean-induction measurements.

Originality & significance: The work is timely and valuable, integrating three crystallization regimes into mission-relevant magnetic predictions; its novelty lies in connecting capture-heating, interior evolution, and dynamo observables for Triton, which is significant even if individual ingredients (thermal evolution, dynamo scalings) are known.

Data & methodology: The 1-D interior evolution plus buoyancy-flux/dynamo scaling is appropriate and clearly implemented, but would benefit from a compact table of parameters and dimensionless numbers (Rm, Re, Pm, Rossby, Elsasser) and a more prominent k_c/\eta sensitivity.

Statistics & uncertainties: Treatment of uncertainty is currently limited around the critical Rm, small-Pm regimes, and near-threshold behavior; quantify these (and the effect of a stratified top layer) and compare intrinsic vs. induced signals with synthetic tracks.

Conclusions: The FeS-layer scenario looks promising, but robustness remains conditional on Rm_c, Pm, k_c, and the onset model; claims of an active dynamo are plausible yet not definitive without the expanded uncertainty analysis.

Suggested improvements: (i) add synthetic magnetometer predictions contrasting intrinsic and induced fields; (ii) report dimensionless numbers/regime; (iii) explore higher Rm_c and arguments on near-threshold scaling; (iv) quantify spectral damping from a stable top layer; (v) clarify Fig. 3's staging and the ~2.5 Gyr discontinuities; (vi) briefly discuss double-diffusive layering.
References: Generally appropriate; consider citing low-Pm onset/experimental constraints (e.g., small-scale dynamo and VKS context).

Clarity & context: The paper is well written; the abstract and introduction are clear, but the Results/Discussion should more explicitly narrate the dynamo phases, spell out observation-facing diagnostics, and foreground the limitations/assumptions that control the main claim of a dynamo still active now on Triton

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Comments for the Author)
I have reviewed the first revision of "A Modern Dynamo as a Consequence of Triton's Capture at Neptune" submitted to Nature Astronomy. The authors have clearly taken my previous comments into account, and I thank them for the care taken in both the revisions and the detailed response provided.

The newly added Methods section, Internal Heating from Formation, Capture, and Differentiation, directly addresses my concerns regarding Triton's initial heat budget. The section is well constructed and provides clear physical justification for the adopted initial temperatures. I also appreciate the discussion of similarities between Triton and Pluto in this context, which helps anchor the assumptions in a broader planetary framework.

I am glad to see that the authors explored a broader range of core compositions, both overall and in Figure 4. That said, I found this figure difficult to interpret, as the differences between models are not visually clear to me.

Relatedly, the response file notes that Ganymede may represent a useful analogue for Triton due to the possibility of a transitional Fe-S core composition. I believe this is a valuable clarification and would suggest that this motivation be stated explicitly in the manuscript itself. Even a single sentence added after "We use three end-member models of core crystallization (Figure 2), motivated by other models that have been developed for application to Ganymede" would help make the rationale for using Ganymede-style end members clearer to readers.

I also wish to note that I appreciate the new Extended Data Figure 4.

Overall, I am satisfied with the modifications the authors have made and consider that my main scientific concerns have been addressed. I will therefore follow the editor's recommendation regarding the disposition of the manuscript.

(Remarks on code availability)

Reviewer #2

(Comments for the Author)
The authors have satisfactorily addressed all my comments and questions. I therefore consider that the manuscript is now suitable for publication in Nature Astronomy.
Christophe Gissinger

(Remarks on code availability)

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Thanks to both reviewers for the very helpful comments! The reviews are copied below verbatim and our point-by-point responses follow in purple text. In general, we expanded the text and added new figures and a table to provide the context and elaboration desired by the reviewers. Our top-level conclusions remain the same. That is, Triton could (but is not guaranteed to) host a dynamo that produces magnetic fields much stronger than the induction response from a putative ocean.

Reviewer #1 (Comments for the Author):

This manuscript presents an investigation into the potential for a present-day dynamo within Triton, motivated by how its capture history could have initiated early internal differentiation. By modeling the evolution of Triton's interior, the authors seek to calculate when a dynamo may exist and assess its maximum lifetime. They explore three plausible core crystallization regimes and demonstrate the conditions under which Triton may still have an active dynamo. They find that a nominal FeS-layer (S-rich) model could sustain a partially molten core for more than twice the age of the Solar System.

The topic is highly relevant: Triton is a prime target for future outer solar system missions, and constraining its internal structure and magnetic potential provides new context for understanding captured and differentiated icy satellites. The manuscript is clearly written, and the modeling framework is thoughtfully constructed.

However, several key assumptions, particularly regarding the initial thermal state following capture, require fuller justification and quantitative grounding. The dynamo lifetime, which is the primary result, appears highly sensitive to the assumed initial conditions. I strongly encourage the authors to expand their discussion and analysis of these parameters.

Major comments

1. Initial thermal state

The results hinge on the adopted initial core and mantle temperatures (1600-2400 K), yet these values are assumed, not derived. Because the authors note that a hotter initial state extends the dynamo lifetime, the results depend critically on the adopted starting temperatures.

I recommend the addition of a dedicated methods section that derives plausible initial core and mantle temperatures from an energy budget, and evaluates how circularization timescales (10-100 Myr) influence whether such heating would actually produce or preserve a "hot start," given concurrent cooling. It would be useful to state explicitly whether the assumption of a well-mixed, fully liquid core is physically consistent with the derived temperature range and core composition. While I recognize that the models are aimed at estimating dynamo longevity rather than reconstructing exact initial conditions, the adopted starting temperatures are presented without clear physical justification, and a quantitative discussion of their origin and plausibility is necessary to substantiate the main conclusions.

I acknowledge that Figure 4 presents a useful sensitivity analysis, but the physical motivation for the adopted initial temperatures and compositions remains unclear. The parameter ranges appear somewhat arbitrary, and without a derivation from an energy budget, it is difficult to assess whether the tested values are representative of realistic post-capture conditions.

As requested, we added a new Methods section ("Internal Heating from Formation, Capture, and Differentiation", lines 488-557) to quantify the maximum expected heating from accretion, capture, and differentiation. There is more than enough energy available to reach the temperatures initialized in our models. As the reviewer states, coupled interior/tidal models are required to better reconstruct exact initial conditions. We added a new citation to point readers towards recent work that presents the development of those kinds of models.

2. Core composition

The three end-member crystallization modes (FeS layer, iron snow, and central Fe solidification) provide a useful framework, but represent discrete points within a continuous Fe-S compositional space. The models use cores containing 33 wt% S or 12.5 wt% S: how were these values selected? Given the sensitivity of chemical buoyancy and electrical conductivity to sulfur content, clarifying the chosen Fe/S ratios and their motivation (especially when extrapolating from Ganymede to Triton) would strengthen the analysis.

The three crystallization regimes are adapted from Ganymede-based models, yet Ganymede's formation environment and thermal history differ substantially from Triton's. While Ganymede provides a valuable comparative example because it maintains an active dynamo, this alone may not justify adopting its interior regimes as direct analogues. Transitional Fe-S compositions, rather than the Ganymede-style end members, could be more realistic for Triton given its capture history, mixed volatile inventory, and likely higher oxidation state. If feasible, a qualitative exploration or scaling across intermediate compositions would show whether the dynamo outcome is robust across plausible Fe/S values or depends on a narrow range.

In Figure 4, we now present a sensitivity test for a broader range of compositions (i.e., 30–36wt% S for the S-rich models and 0–25wt% S for the S-poor models, lines 641-654). The values in the example models (e.g., 33 and 12.5 wt% S) were selected for illustrative purposes in the middle of those ranges. The dynamo outcome is robust across plausible Fe/S values and is minimized only if the crystallizing solid would have the same composition as the residual fluid (i.e., if the core starts as pure Fe, pure FeS, or at the eutectic composition). Finally, Ganymede, like Triton, might have a transitional Fe-S composition, so "the Ganymede-style end members" discussed in prior studies are also illustrative examples of possible compositions and dynamical regimes for the metallic core.

Minor comments

- A brief comparison with Pluto (Triton's closest analogue in size and composition) would strengthen the discussion. If capture-induced tidal heating and differentiation are responsible for a long-lived metallic core and dynamo at Triton, then Pluto's apparent lack of such features provides a valuable control case. Discussing how compositional or volatile differences might explain their divergent outcomes would place the results in a broader planetary-formation context.

As requested, we added a brief comparison to the new Methods section about the sources of internal heating. Indeed, Pluto is missing what we consider the largest potential energy source and thus it is perhaps not too surprising that it is missing a metallic core and dynamo (lines 527-557).

- The study's motivation centers on the detectability of Triton's magnetic field, yet no quantitative field strengths are reported. I encourage the authors to estimate surface and external magnetic field strengths for each end-member model using established dynamo scaling laws. The quantities already available (core size, convective power, density, rotation rate) should allow approximate dipole field estimates at the core/mantle boundary, Triton's surface, and at Voyager 2's closest approach. Comparing these values to instrument detection limits would clarify the observational implications. A brief discussion of how such intrinsic fields could be distinguished from induction responses to Neptune's magnetosphere would further enhance the paper's relevance to future mission planning.

We report the quantitative field strengths at the surface in subpanel e of Figure 3 (lines 610-622) and Extended Data Figures 1 and 2 (lines 671-679). They are of order μT while the dynamo is operating. Lines 56–63 discuss how such a field would have been undetectable during the Voyager 2 flyby.

We also added Extended Data Figure 4 (lines 707-710) to show how the dynamo field compares to the predicted induction response of the putative ocean (it is much larger).

- Very minor: It may also be helpful to define the Rossby number alongside the magnetic Reynolds number, since both are discussed as critical dynamo criteria. This would improve clarity for readers outside the immediate subfield.

We now mention this criterion around lines 308-317 and include the Rossby number in the new table (Extended Data Table 1, lines 727-729) along with the nondimensional numbers suggested by the other reviewer.

Recommendation

The paper presents an interesting and potentially impactful advance in our understanding of Triton's interior evolution. The modeling framework itself appears sound, and the results offer genuinely novel insights into Triton's possible magnetic history. With the inclusion of a physically motivated derivation of initial thermal conditions and a clearer treatment of compositional sensitivity, it would be suitable for publication in a high-impact journal. At present, however, the conclusions rely on assumptions that require further quantitative justification.

Reviewer #2 (Comments for the Author):

This manuscript argues that Triton's "hot start" and subsequent core crystallization pathways can drive and sustain a present-day core dynamo, yielding surface fields of the order of a few microtesla and complicating ocean-induction responses. The paper is timely and potentially impactful: a live dynamo at Triton would reshape priorities for the Neptune–Triton system and for interpreting magnetometer data. The modeling framework is clear and the three crystallization regimes (FeS top-down layer, iron snow, Earth-like inner core) cover the most important parts.

However, in its current form, the claim of a dynamo on Triton feels a little bit weak on several core aspects (critical magnetic Reynolds number, type/morphology of the dynamo, and explicit observational discriminants versus induction). Addressing the major comments below would, in my view, lift this to Nature Astronomy level.

Overall recommendation: Major revision

Major comments

1. Analysis of your produced field (a few microtesla)

You state that a roughly ~1 μT surface field could overwhelm an induction signal and would have been undetectable by Voyager 2 at its closest approach (~0.04 nT at the spacecraft), but there is no quantitative comparison between plausible induced responses and your intrinsic field predictions. Perhaps you could add synthetic magnetometer time series or, at minimum, comment on the expected amplitude of an inductive response for a range of ocean salinities/conductivities. Your work provides valuable predictions for the dynamo field, and it would be useful to understand how it can be quantitatively compared to the ocean's induction response. This would clarify where/when an intrinsic field biases induction inversions.

We added Extended Data Figure 4 (lines 707-710) to make the requested comparison. The predicted dynamo fields are much larger than the ocean's induction response, although the shape of the two curves is different.

2. Onset of the dynamo

You adopt a critical Rm of nearly 40 (with a stated plausible range 10–100) and implement an on/off threshold. I understand that the cited literature goes along with these values. However, this typical value must be taken cautiously: it is well known that the onset of dynamos can significantly increase with turbulence and with small magnetic Prandtl number (here Pm~ 10^{-6} in metallic cores). In the range reported by Christensen, Pm in numerical simulations remains relatively large (around 0.1–10) and no clear increase of the onset is observed. But there is evidence that the onset increases rapidly as Pm decreases (see, for instance, Schekochihin et al. 2004, Phys. Rev. Lett. 92, 054502). Interestingly, all experimental turbulent dynamos (with the

exception of the VKS dynamo, due to ferromagnetic impellers) have failed to observe dynamos up to Rm \sim 200, possibly due to the effect of turbulence. This matter on the possible higher value of the critical Rm should be discussed.

We conducted new sensitivity tests and added a new Extended Data Figure 3 (lines 683-687) to show the predicted dynamo lifetimes if the critical Rm is 100, 150, or 200. We now also discuss these issues and cite Schekochihin et al. (2004) around lines 308-317.

3. Scaling laws and proximity to onset

Similarly, you could discuss the fact that the scaling laws used for predicting the field strength might differ depending on whether we are dealing with a thermal-convection dynamo, an iron-snow dynamo, or other mechanisms. Please also discuss the applicability of these scalings depending on the parameters (rotation, etc). Similarly, given that your solutions sit quite close to dynamo onset, scaling laws may change compared to the one used here. A clarification would help in understanding the consequences of the work reported here.

We now discuss these caveats around lines 326-329. Indeed, we would not be surprised if future dynamo models revealed that we should be using different scaling laws for field strength given the possibility that Triton's dynamo could be dominantly multi-polar and driven by crystallization at the top of the core, instead of by an Earth-like inner core.

4. Spectral imprint of a stratified top layer

Please discuss how a stratified top layer (thermal, FeS, or snow) would imprint the spectrum via a skin effect that damps high-degree harmonics. You briefly allude to this in the Discussion, but the paper would benefit from expanding this quantitatively for Triton's parameters. Again, this might be interesting for analyzing future magnetometer data.

The last paragraph of the Methods (lines 569–579) now discusses the skin effect in more detail, including the expectation from prior studies that the timescale of variation is inversely proportional to the harmonic degree of the field. We are hesitant to make more specific quantitative predictions at this stage, because (as the reviewer mentioned above) the scaling laws used to predict the dynamo power spectrum are rather uncertain.

5. Narrative of Fig. 3

I think you should clarify the four evolutionary stages when discussion Fig.3. In Fig. 3 you alternate between super-adiabatic and sub-adiabatic phases. Please make explicit that it is the super-adiabatic heat flux (not absolute T) that sets buoyancy power and thus Rm; hence why early rapid cooling boosts the dynamo before mantle heating temporarily suppresses it. A short, self-contained explanation near Fig. 3a–b would help non-specialists.

We added this clarification to lines 154–155.

6. Discontinuities in Rm and B near ~2.5 Gyr

I am not sure I understand the origin of the strong jump in Rm at t=2.5Gyr.: why does the onset of chemical buoyancy produce such a large change in Rm? As the FeS layer is small at first and gradually increases, I would expect a similarly gradual effect on Rm. Please clarify whether the jump is physical, numerical (e.g., step-function thresholding), or both.

It's physical. The crystallization of the FeS layer instantaneously adds chemical buoyancy, which depends on the rate at which the layer grows (not the total thickness of the layer). Since the growth rate instantaneously jumps from zero to a positive number, the chemical buoyancy flux jumps as well. We now clarify this point in the caption to Figure 3 (lines 619-622).

7. Dimensionless numbers for modeling (near-onset case)

People doing numerical simulations of astrophysical dynamos might be interested in having an example of a dynamo operating very close to its onset. You might consider giving additional estimates for Triton, such as the Rossby number and Ekman number, in order to facilitate modeling of this problem (along with Re, Pm, and, if relevant, the Elsasser number).

Good idea. We added Extended Data Table 1 (lines 727-729) to summarize some typical values for nondimensional numbers of interest.

8. Double diffusion vs. iron snow

"Iron snow" in your models is not, per se, classical double-diffusive convection. Please clarify whether additional double-diffusive layering is expected, and whether such layers could delay convective restarts or modulate the chemical buoyancy flux in your FeS and inner-core regimes. A brief scope note would be sufficient.

We added a brief scope note (lines 412-421) as a short paragraph below the first paragraph in the "Iron Snow in the Metallic Core" subsection.

We also added a few new sentences (lines 465-467) to the end of the "Growth of an Inner Core" section.

9. Connect model outputs to observations

You should relate your model outputs more closely to observations: details of the magnetic environment near Triton; Neptune's rotating, non-axisymmetric field as a driver of induction; mission durations and instrument sensitivity; and how these tie to your outputs B_surf, expected topology given small Rm, secular variation, etc.). This would make the measurement implications more concrete.

As also requested by the other reviewer, we added Extended Data Figure 4 (lines 707-710) to compare the strength of a dynamo field to the predicted induction response from an ocean during a notional flyby. Previous studies have shown that an induction response is potentially detectable even given Neptune's rotating, non-axisymmetric field, etc. Because the dynamo field is so much stronger, we are confident that it would be detectable also. How to detect an ocean in the presence of a dynamo field would be a great topic for a follow-up study. As we state at the end of the main text, the JUICE mission to Ganymede will showcase exactly this procedure.

10. Time-evolving onset conditions

I was wondering whether your data on the size and composition of the core can be used to estimate more accurately how the critical superadiabaticity for dynamo action changes over time. In other words, as the interior evolves, the structure may change sufficiently for the size and conductivity of the core to change; therefore, the onset may not be just a fixed line in the heat-flow plots? Maybe this effect is negligible ?

This effect is somewhat included in our models. For example, the adiabatic heat flow shown in Figure 3b (lines 610, 614-616) is updated as the core cools and changes in composition. This effect is indeed small compared to the other uncertainties in our model.

Summary: The manuscript links Triton's hot start and subsequent crystallization pathways to a present-day core dynamo, predicting surface fields of a few microtesla and clear implications for ocean-induction measurements.

Originality & significance: The work is timely and valuable, integrating three crystallization regimes into mission-relevant magnetic predictions; its novelty lies in connecting capture-heating, interior evolution, and dynamo observables for Triton, which is significant even if individual ingredients (thermal evolution, dynamo scalings) are known.

Data & methodology: The 1-D interior evolution plus buoyancy-flux/dynamo scaling is appropriate and clearly implemented, but would benefit from a compact table of parameters and dimensionless numbers (Rm, Re, Pm, Rossby, Elsasser) and a more prominent k_c/\eta sensitivity.

Statistics & uncertainties: Treatment of uncertainty is currently limited around the critical Rm, small-Pm regimes, and near-threshold behavior; quantify these (and the effect of a stratified top layer) and compare intrinsic vs. induced signals with synthetic tracks.

Conclusions: The FeS-layer scenario looks promising, but robustness remains conditional on Rm_c, Pm, k_c, and the onset model; claims of an active dynamo are plausible yet not definitive without the expanded uncertainty analysis.

Suggested improvements: (i) add synthetic magnetometer predictions contrasting intrinsic and induced fields;

Done (Extended Data Figure 4, lines 707-710).

(ii) report dimensionless numbers/regime;

Done (Extended Data Table 1, lines 726-729).

(iii) explore higher Rm_c and arguments on near-threshold scaling;

Done (Extended Data Table 1, lines 726-729).

(iv) quantify spectral damping from a stable top layer;

Done (last paragraph of the methods, lines 569-579).

(v) clarify Fig. 3's staging and the ~2.5 Gyr discontinuities;

Done (Figure 3 caption, lines 619-620).

(vi) briefly discuss double-diffusive layering.

Done (Methods, brief scope note, lines 412-421, and a few new sentences, lines 465-467).

References: Generally appropriate; consider citing low-Pm onset/experimental constraints (e.g., small-scale dynamo and VKS context).

We added the reference mentioned by the reviewer (ref. 63, lines 310-311 and 388-389).

Clarity & context: The paper is well written; the abstract and introduction are clear, but the Results/Discussion should more explicitly narrate the dynamo phases, spell out observation-facing diagnostics, and foreground the limitations/assumptions that control the main claim of a dynamo still active now on Triton.

First Round Reviews

Thanks to both reviewers for the very helpful comments! The reviews are copied below verbatim and our point-by-point responses follow in purple text. In general, we expanded the text and added new figures and a table to provide the context and elaboration desired by the reviewers. Our top-level conclusions remain the same. That is, Triton could (but is not guaranteed to) host a dynamo that produces magnetic fields much stronger than the induction response from a putative ocean.

Reviewer #1 (Comments for the Author):

This manuscript presents an investigation into the potential for a present-day dynamo within Triton, motivated by how its capture history could have initiated early internal differentiation. By modeling the evolution of Triton's interior, the authors seek to calculate when a dynamo may exist and assess its maximum lifetime. They explore three plausible core crystallization regimes and demonstrate the conditions under which Triton may still have an active dynamo. They find that a nominal FeS-layer (S-rich) model could sustain a partially molten core for more than twice the age of the Solar System.

The topic is highly relevant: Triton is a prime target for future outer solar system missions, and constraining its internal structure and magnetic potential provides new context for understanding captured and differentiated icy satellites. The manuscript is clearly written, and the modeling framework is thoughtfully constructed.

However, several key assumptions, particularly regarding the initial thermal state following capture, require fuller justification and quantitative grounding. The dynamo lifetime, which is the primary result, appears highly sensitive to the assumed initial conditions. I strongly encourage the authors to expand their discussion and analysis of these parameters.

Major comments

1. Initial thermal state

The results hinge on the adopted initial core and mantle temperatures (1600-2400 K), yet these values are assumed, not derived. Because the authors note that a hotter initial state extends the dynamo lifetime, the results depend critically on the adopted starting temperatures.

I recommend the addition of a dedicated methods section that derives plausible initial core and mantle temperatures from an energy budget, and evaluates how circularization timescales (10-100 Myr) influence whether such heating would actually produce or preserve a "hot start," given concurrent cooling. It would be useful to state explicitly whether the assumption of a well-mixed, fully liquid core is physically consistent with the derived temperature range and core composition. While I recognize that the models are aimed at estimating dynamo longevity rather than reconstructing exact initial conditions, the adopted

starting temperatures are presented without clear physical justification, and a quantitative discussion of their origin and plausibility is necessary to substantiate the main conclusions.

I acknowledge that Figure 4 presents a useful sensitivity analysis, but the physical motivation for the adopted initial temperatures and compositions remains unclear. The parameter ranges appear somewhat arbitrary, and without a derivation from an energy budget, it is difficult to assess whether the tested values are representative of realistic post-capture conditions.

As requested, we added a new Methods section ("Internal Heating from Formation, Capture, and Differentiation", lines 488-557) to quantify the maximum expected heating from accretion, capture, and differentiation. There is more than enough energy available to reach the temperatures initialized in our models. As the reviewer states, coupled interior/tidal models are required to better reconstruct exact initial conditions. We added a new citation to point readers towards recent work that presents the development of those kinds of models.

2. Core composition

The three end-member crystallization modes (FeS layer, iron snow, and central Fe solidification) provide a useful framework, but represent discrete points within a continuous Fe-S compositional space. The models use cores containing 33 wt% S or 12.5 wt% S: how were these values selected? Given the sensitivity of chemical buoyancy and electrical conductivity to sulfur content, clarifying the chosen Fe/S ratios and their motivation (especially when extrapolating from Ganymede to Triton) would strengthen the analysis.

The three crystallization regimes are adapted from Ganymede-based models, yet Ganymede's formation environment and thermal history differ substantially from Triton's. While Ganymede provides a valuable comparative example because it maintains an active dynamo, this alone may not justify adopting its interior regimes as direct analogues. Transitional Fe-S compositions, rather than the Ganymede-style end members, could be more realistic for Triton given its capture history, mixed volatile inventory, and likely higher oxidation state. If feasible, a qualitative exploration or scaling across intermediate compositions would show whether the dynamo outcome is robust across plausible Fe/S values or depends on a narrow range.

In Figure 4, we now present a sensitivity test for a broader range of compositions (i.e., 30–36wt% S for the S-rich models and 0–25wt% S for the S-poor models, lines 641-654). The values in the example models (e.g., 33 and 12.5 wt% S) were selected for illustrative purposes in the middle of those ranges. The dynamo outcome is robust across plausible Fe/S values and is minimized only if the crystallizing solid would have the same composition as the residual fluid (i.e., if the core starts as pure Fe, pure FeS, or at the eutectic composition). Finally, Ganymede, like Triton, might have a transitional Fe-S composition, so "the Ganymede-style end members" discussed in prior studies are also illustrative examples of possible compositions and dynamical regimes for the metallic core.

Minor comments

- A brief comparison with Pluto (Triton's closest analogue in size and composition) would strengthen the discussion. If capture-induced tidal heating and differentiation are responsible for a long-lived metallic core and dynamo at Triton, then Pluto's apparent lack of such features provides a valuable control case. Discussing how compositional or volatile differences might explain their divergent outcomes would place the results in a broader planetary-formation context.

As requested, we added a brief comparison to the new Methods section about the sources of internal heating. Indeed, Pluto is missing what we consider the largest potential energy source and thus it is perhaps not too surprising that it is missing a metallic core and dynamo (lines 527-557).

- The study's motivation centers on the detectability of Triton's magnetic field, yet no quantitative field strengths are reported. I encourage the authors to estimate surface and external magnetic field strengths for each end-member model using established dynamo scaling laws. The quantities already available (core size, convective power, density, rotation rate) should allow approximate dipole field estimates at the core/mantle boundary, Triton's surface, and at Voyager 2's closest approach. Comparing these values to instrument detection limits would clarify the observational implications. A brief discussion of how such intrinsic fields could be distinguished from induction responses to Neptune's magnetosphere would further enhance the paper's relevance to future mission planning.

We report the quantitative field strengths at the surface in subpanel e of Figure 3 (lines 610-622) and Extended Data Figures 1 and 2 (lines 671-679). They are of order μT while the dynamo is operating. Lines 56–63 discuss how such a field would have been undetectable during the Voyager 2 flyby.

We also added Extended Data Figure 4 (lines 707-710) to show how the dynamo field compares to the predicted induction response of the putative ocean (it is much larger).

- Very minor: It may also be helpful to define the Rossby number alongside the magnetic Reynolds number, since both are discussed as critical dynamo criteria. This would improve clarity for readers outside the immediate subfield.

We now mention this criterion around lines 308-317 and include the Rossby number in the new table (Extended Data Table 1, lines 727-729) along with the nondimensional numbers suggested by the other reviewer.

Recommendation

The paper presents an interesting and potentially impactful advance in our understanding of Triton's interior evolution. The modeling framework itself appears sound, and the results offer genuinely novel insights into Triton's possible magnetic history. With the inclusion of a physically motivated derivation of initial thermal conditions and a clearer treatment of compositional sensitivity, it would be suitable for publication in a high-impact journal. At present, however, the conclusions rely on assumptions that require further quantitative justification.

Reviewer #2 (Comments for the Author):

This manuscript argues that Triton's "hot start" and subsequent core crystallization pathways can drive and sustain a present-day core dynamo, yielding surface fields of the order of a few microtesla and complicating ocean-induction responses. The paper is timely and potentially impactful: a live dynamo at Triton would reshape priorities for the Neptune–Triton system and for interpreting magnetometer data. The modeling framework is clear and the three crystallization regimes (FeS top-down layer, iron snow, Earth-like inner core) cover the most important parts.

However, in its current form, the claim of a dynamo on Triton feels a little bit weak on several core aspects (critical magnetic Reynolds number, type/morphology of the dynamo, and explicit observational discriminants versus induction). Addressing the major comments below would, in my view, lift this to Nature Astronomy level.

Overall recommendation: Major revision

Major comments

1. Analysis of your produced field (a few microtesla)

You state that a roughly ~1 μT surface field could overwhelm an induction signal and would have been undetectable by Voyager 2 at its closest approach (~0.04 nT at the spacecraft), but there is no quantitative comparison between plausible induced responses and your intrinsic field predictions. Perhaps you could add synthetic magnetometer time series or, at minimum, comment on the expected amplitude of an inductive response for a range of ocean salinities/conductivities. Your work provides valuable predictions for the dynamo field, and it would be useful to understand how it can be quantitatively compared to the ocean's induction response. This would clarify where/when an intrinsic field biases induction inversions.

We added Extended Data Figure 4 (lines 707-710) to make the requested comparison. The predicted dynamo fields are much larger than the ocean's induction response, although the shape of the two curves is different.

2. Onset of the dynamo

You adopt a critical Rm of nearly 40 (with a stated plausible range 10–100) and implement an on/off threshold. I understand that the cited literature goes along with these values. However, this typical value must be taken cautiously: it is well known that the onset of dynamos can significantly increase with turbulence and with small magnetic Prandtl number (here Pm~ 10^{-6} in metallic cores). In the range reported by Christensen, Pm in numerical simulations remains relatively large (around 0.1–10) and no clear increase of the onset is observed. But there is evidence that the onset increases rapidly as Pm decreases (see, for instance, Schekochihin et al. 2004, Phys. Rev. Lett. 92, 054502). Interestingly, all experimental turbulent dynamos (with the exception of the VKS dynamo, due to ferromagnetic impellers) have failed to observe dynamos up to Rm \sim 200, possibly due to the effect of turbulence. This matter on the possible higher value of the critical Rm should be discussed.

We conducted new sensitivity tests and added a new Extended Data Figure 3 (lines 683-687) to show the predicted dynamo lifetimes if the critical Rm is 100, 150, or 200. We now also discuss these issues and cite Schekochihin et al. (2004) around lines 308-317.

3. Scaling laws and proximity to onset

Similarly, you could discuss the fact that the scaling laws used for predicting the field strength might differ depending on whether we are dealing with a thermal-convection dynamo, an iron-snow dynamo, or other mechanisms. Please also discuss the applicability of these scalings depending on the parameters (rotation, etc). Similarly, given that your solutions sit quite close to dynamo onset, scaling laws may change compared to the one used here. A clarification would help in understanding the consequences of the work reported here.

We now discuss these caveats around lines 326-329. Indeed, we would not be surprised if future dynamo models revealed that we should be using different scaling laws for field strength given the possibility that Triton's dynamo could be dominantly multi-polar and driven by crystallization at the top of the core, instead of by an Earth-like inner core.

4. Spectral imprint of a stratified top layer

Please discuss how a stratified top layer (thermal, FeS, or snow) would imprint the spectrum via a skin effect that damps high-degree harmonics. You briefly allude to this in the Discussion, but the paper would benefit from expanding this quantitatively for Triton's parameters. Again, this might be interesting for analyzing future magnetometer data.

The last paragraph of the Methods (lines 569–579) now discusses the skin effect in more detail, including the expectation from prior studies that the timescale of variation is inversely proportional to the harmonic degree of the field. We are hesitant to make more specific quantitative predictions at this stage, because (as the reviewer mentioned above) the scaling laws used to predict the dynamo power spectrum are rather uncertain.

5. Narrative of Fig. 3

I think you should clarify the four evolutionary stages when discussion Fig.3. In Fig. 3 you alternate between super-adiabatic and sub-adiabatic phases. Please make explicit that it is the super-adiabatic heat flux (not absolute T) that sets buoyancy power and thus Rm; hence why early rapid cooling boosts the dynamo before mantle heating temporarily suppresses it. A short, self-contained explanation near Fig. 3a–b would help non-specialists.

We added this clarification to lines 154–155.

6. Discontinuities in Rm and B near ~2.5 Gyr

I am not sure I understand the origin of the strong jump in Rm at t=2.5Gyr.: why does the onset of chemical buoyancy produce such a large change in Rm? As the FeS layer is small at first and gradually increases, I would expect a similarly gradual effect on Rm. Please clarify whether the jump is physical, numerical (e.g., step-function thresholding), or both.

It's physical. The crystallization of the FeS layer instantaneously adds chemical buoyancy, which depends on the rate at which the layer grows (not the total thickness of the layer). Since the growth rate instantaneously jumps from zero to a positive number, the chemical buoyancy flux jumps as well. We now clarify this point in the caption to Figure 3 (lines 619-622).

7. Dimensionless numbers for modeling (near-onset case)

People doing numerical simulations of astrophysical dynamos might be interested in having an example of a dynamo operating very close to its onset. You might consider giving additional estimates for Triton, such as the Rossby number and Ekman number, in order to facilitate modeling of this problem (along with Re, Pm, and, if relevant, the Elsasser number).

Good idea. We added Extended Data Table 1 (lines 727-729) to summarize some typical values for nondimensional numbers of interest.

8. Double diffusion vs. iron snow

"Iron snow" in your models is not, per se, classical double-diffusive convection. Please clarify whether additional double-diffusive layering is expected, and whether such layers could delay convective restarts or modulate the chemical buoyancy flux in your FeS and inner-core regimes. A brief scope note would be sufficient.

We added a brief scope note (lines 412-421) as a short paragraph below the first paragraph in the "Iron Snow in the Metallic Core" subsection.

We also added a few new sentences (lines 465-467) to the end of the "Growth of an Inner Core" section.

9. Connect model outputs to observations

You should relate your model outputs more closely to observations: details of the magnetic environment near Triton; Neptune's rotating, non-axisymmetric field as a driver of induction; mission durations and instrument sensitivity; and how these tie to your outputs B_surf, expected topology given small Rm, secular variation, etc.). This would make the measurement implications more concrete.

As also requested by the other reviewer, we added Extended Data Figure 4 (lines 707-710) to compare the strength of a dynamo field to the predicted induction response from an ocean during a notional flyby. Previous studies have shown that an induction response is potentially detectable even given Neptune's rotating, non-axisymmetric field, etc. Because the dynamo field is so much stronger, we are confident that it would be detectable also. How to detect an ocean in the presence of a dynamo field would be a great topic for a follow-up study. As we state at the end of the main text, the JUICE mission to Ganymede will showcase exactly this procedure.

10. Time-evolving onset conditions

I was wondering whether your data on the size and composition of the core can be used to estimate more accurately how the critical superadiabaticity for dynamo action changes over time. In other words, as the interior evolves, the structure may change sufficiently for the size and conductivity of the core to change; therefore, the onset may not be just a fixed line in the heat-flow plots? Maybe this effect is negligible ?

This effect is somewhat included in our models. For example, the adiabatic heat flow shown in Figure 3b (lines 610, 614-616) is updated as the core cools and changes in composition. This effect is indeed small compared to the other uncertainties in our model.

Summary: The manuscript links Triton's hot start and subsequent crystallization pathways to a present-day core dynamo, predicting surface fields of a few microtesla and clear implications for ocean-induction measurements.

Originality & significance: The work is timely and valuable, integrating three crystallization regimes into mission-relevant magnetic predictions; its novelty lies in connecting

capture-heating, interior evolution, and dynamo observables for Triton, which is significant even if individual ingredients (thermal evolution, dynamo scalings) are known.

Data & methodology: The 1-D interior evolution plus buoyancy-flux/dynamo scaling is appropriate and clearly implemented, but would benefit from a compact table of parameters and dimensionless numbers (Rm, Re, Pm, Rossby, Elsasser) and a more prominent k_c/\eta sensitivity.

Statistics & uncertainties: Treatment of uncertainty is currently limited around the critical Rm, small-Pm regimes, and near-threshold behavior; quantify these (and the effect of a stratified top layer) and compare intrinsic vs. induced signals with synthetic tracks.

Conclusions: The FeS-layer scenario looks promising, but robustness remains conditional on Rm_c, Pm, k_c, and the onset model; claims of an active dynamo are plausible yet not definitive without the expanded uncertainty analysis.

Suggested improvements: (i) add synthetic magnetometer predictions contrasting intrinsic and induced fields;

Done (Extended Data Figure 4, lines 707-710).

(ii) report dimensionless numbers/regime;

Done (Extended Data Table 1, lines 726-729).

(iii) explore higher Rm_c and arguments on near-threshold scaling;

Done (Extended Data Table 1, lines 726-729).

(iv) quantify spectral damping from a stable top layer;

Done (last paragraph of the methods, lines 569-579).

(v) clarify Fig. 3's staging and the ~2.5 Gyr discontinuities;

Done (Figure 3 caption, lines 619-620).

(vi) briefly discuss double-diffusive layering.

Done (Methods, brief scope note, lines 412-421, and a few new sentences, lines 465-467).

References: Generally appropriate; consider citing low-Pm onset/experimental constraints (e.g., small-scale dynamo and VKS context).

We added the reference mentioned by the reviewer (ref. 63, lines 310-311 and 388-389).

Clarity & context: The paper is well written; the abstract and introduction are clear, but the Results/Discussion should more explicitly narrate the dynamo phases, spell out observation-facing diagnostics, and foreground the limitations/assumptions that control the main claim of a dynamo still active now on Triton.

Second Round Reviews

We once again thank the reviewers for their helpful feedback! The second round reviews are copied below verbatim and our point-by-point responses follow in purple text.

In addition to changes directly requested by the reviewers, we clarified the modeled surface magnetic field strength and corresponding strength at Voyager 2's closest approach in lines 73-74. Previously, we stated Triton had a magnetic field strength of ~1 μT and a Voyager 2 strength of 0.04 nT. This is accurate, as we were stating orders of magnitude, but in order to have a bit more consistency with other sections of the manuscript, we updated this to ~2 μT and 0.08 nT. Results of "1 μT" are now explicitly only given for an order of magnitude.

Further, during the proofreading of our manuscript, we discovered a small error of unit conversion in our code. We have updated our figures with the correct calculations, which hardly changed. Text has been slightly modified to reflect the minor changes (on line 202, 2400 K has been changed to 2600 K, and the caption for Figure 4 has been amended). We also move the mention of Extended Data Figure 4 from the caption of Figure 3 to the main text (lines 182-184).

Reviewer #1 (Comments for the Author):

I have reviewed the first revision of "A Modern Dynamo as a Consequence of Triton's Capture at Neptune" submitted to Nature Astronomy. The authors have clearly taken my previous comments into account, and I thank them for the care taken in both the revisions and the detailed response provided.

The newly added Methods section, Internal Heating from Formation, Capture, and Differentiation, directly addresses my concerns regarding Triton's initial heat budget. The section is well constructed and provides clear physical justification for the adopted initial temperatures. I also appreciate the discussion of similarities between Triton and Pluto in this context, which helps anchor the assumptions in a broader planetary framework.

I am glad to see that the authors explored a broader range of core compositions, both overall and in Figure 4. That said, I found this figure difficult to interpret, as the differences between models are not visually clear to me.

We updated Figure 4 to include more clear definitions for each marker symbol.

Relatedly, the response file notes that Ganymede may represent a useful analogue for Triton due to the possibility of a transitional Fe-S core composition. I believe this is a valuable clarification and would suggest that this motivation be stated explicitly in the manuscript itself. Even a single sentence added after "We use three end-member models of core crystallization (Figure 2),

motivated by other models that have been developed for application to Ganymede" would help make the rationale for using Ganymede-style end members clearer to readers.

Good idea. As requested, we added a brief sentence clarifying the potential similarity between Ganymede and Triton's interior composition to lines 119-120.

I also wish to note that I appreciate the new Extended Data Figure 4.

Overall, I am satisfied with the modifications the authors have made and consider that my main scientific concerns have been addressed. I will therefore follow the editor's recommendation regarding the disposition of the manuscript.

Reviewer #2 (Comments for the Author):

The authors have satisfactorily addressed all my comments and questions. I therefore consider that the manuscript is now suitable for publication in Nature Astronomy.
Christophe Gissinger

Referee Report on
A Modern Dynamo as a Consequence of Triton's Capture at Neptune
By Lana J. Tilke, Kevin T. Trinh, Joseph G. O'Rourke

Summary

This manuscript argues that Triton's "hot start" and subsequent core crystallization pathways can drive and sustain a present-day core dynamo, yielding surface fields of the order of a few microtesla and complicating ocean-induction responses. The paper is timely and potentially impactful: a live dynamo at Triton would reshape priorities for the Neptune–Triton system and for interpreting magnetometer data. The modeling framework is clear and the three crystallization regimes (FeS top-down layer, iron snow, Earth-like inner core) cover the most important parts.

However, in its current form, the claim of a dynamo on Triton feels a little bit weak on several core aspects (critical magnetic Reynolds number, type/morphology of the dynamo, and explicit observational discriminants versus induction). Addressing the major comments below would, in my view, lift this to Nature Astronomy level.

Overall recommendation: Major revision

Major comments

1) Analysis of your produced field (a few microtesla)

You state that a roughly ~1 μT surface field could overwhelm an induction signal and would have been undetectable by Voyager 2 at its closest approach (~0.04 nT at the spacecraft), but there is no quantitative comparison between plausible induced responses and your intrinsic field predictions. Perhaps you could add synthetic magnetometer time series or, at minimum, comment on the expected amplitude of an inductive response for a range of ocean salinities/conductivities. Your work provides valuable predictions for the dynamo field, and it would be useful to understand how it can be quantitatively compared to the ocean's induction response. This would clarify where/when an intrinsic field biases induction inversions.

2) Onset of the dynamo

You adopt a critical Rm of nearly 40 (with a stated plausible range 10–100) and implement an on/off threshold. I understand that the cited literature goes along with these values. However, this typical value must be taken cautiously: it is well known that the onset of dynamos can significantly increase with turbulence and with small magnetic Prandtl number (here Pm~ 10^{-6} in metallic cores). In the range reported by Christensen, Pm in numerical simulations remains relatively large (around 0.1–10) and no clear increase of the onset is observed. But there is evidence that the onset increases rapidly as Pm decreases (see, for instance, Schekochihin et al. 2004, *Phys. Rev. Lett.* 92, 054502). Interestingly, all experimental turbulent dynamos (with the exception of the VKS dynamo, due to ferromagnetic impellers) have failed to observe dynamos up to Rm \sim 200, possibly due to the effect of turbulence. This matter on the possible higher value of the critical Rm should be discussed.

3) Scaling laws and proximity to onset

Similarly, you could discuss the fact that the scaling laws used for predicting the field strength might differ depending on whether we are dealing with a thermal-convection dynamo, an iron-snow dynamo, or other mechanisms. Please also discuss the applicability of these scalings depending on the parameters (rotation, etc). Similarly, given that your solutions sit quite close to dynamo onset, scaling laws may change compared to the one used here. A clarification would help in understanding the consequences of the work reported here.

4) Spectral imprint of a stratified top layer

Please discuss how a stratified top layer (thermal, FeS, or snow) would imprint the spectrum via a skin effect that damps high-degree harmonics. You briefly allude to this in the Discussion, but the paper would benefit from expanding this quantitatively for Triton's parameters. Again, this might be interesting for analyzing future magnetometer data.

5) Narrative of Fig. 3

I think you should clarify the four evolutionary stages when discussion Fig.3. In Fig. 3 you alternate between super-adiabatic and sub-adiabatic phases. Please make explicit that it is the super-adiabatic heat flux (not absolute T) that sets buoyancy power and thus Rm; hence why early rapid cooling boosts the dynamo before mantle heating temporarily suppresses it. A short, self-contained explanation near Fig. 3a–b would help non-specialists.

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You should relate your model outputs more closely to observations: details of the magnetic environment near Triton; Neptune's rotating, non-axisymmetric field as a driver of induction; mission durations and instrument sensitivity; and how these tie to your outputs B_surf, expected topology given small Rm, secular variation, etc.). This would make the measurement implications more concrete.

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Data & methodology: The 1-D interior evolution plus buoyancy-flux/dynamo scaling is appropriate and clearly implemented, but would benefit from a compact table of parameters and dimensionless numbers (Rm, Re, Pm, Rossby, Elsasser) and a more prominent k_c/\eta sensitivity.
Statistics & uncertainties: Treatment of uncertainty is currently limited around the critical Rm, small-Pm regimes, and near-threshold behavior; quantify these (and the effect of a stratified top layer) and compare intrinsic vs. induced signals with synthetic tracks.
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Suggested improvements: (i) add synthetic magnetometer predictions contrasting intrinsic and induced fields; (ii) report dimensionless numbers/regime; (iii) explore higher Rm_c and arguments on near-threshold scaling; (iv) quantify spectral damping from a stable top layer; (v) clarify Fig. 3's staging and the ~2.5 Gyr discontinuities; (vi) briefly discuss double-diffusive layering.
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