nature portfolio

Peer Review File

Dinosaur extinction can explain continental facies shifts at the Cretaceous-Paleogene boundary

Corresponding Author: Dr Lucas Weaver

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Dr Weaver,

Your manuscript titled "Dinosaur extinction can explain continental facies shifts at the K-Pg boundary" has now been seen by 3 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

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In particular, please ensure that the revised manuscript meets the following editorial thresholds:

**Provide firm and robust stratigraphic and paleontological evidence to demonstrate that Dinosaurs were ecosystem engineers in the Late Cretaceous, and that their extinction led to landscape and ecosystem reorganization in the early Paleogene.

**Justify your methodological approach, demonstrating that the iridium sampling is robust, and that the stratigraphic analysis of KPg sections is complete.

**Appropriately discuss your recommendations in the context of the existing literature, and discuss alternative interpretations of your data.

******

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Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor,
Communications Earth & Environment
Consulting Editor, Communications Sustainability

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I think that it is an interesting idea that the removal of large dinosaurs from a landscape would have a visible impact on the deposition of fluvial sediments and the resulting stratigraphy. This hypothesis is worth discussion, and I like the paper's multi-basin approach. I also admire the effort to frame these concepts within a regional context and I applaud the documentation of the new sites in the Bighorn Basin. That having been said, I think that it is worth separating the densely sampled K-Pg sections from the coastal plain (Williston Basin) from the more sparsely sampled montane proximal basins (Raton, Denver Bighorn, Alberta, and Powder). It is in the Williston Basin where Roland Brown's "last dinosaur below the lowest coal" approach has been most commonly applied and where this new hypothesis can best be tested.

I agree that it is important to use the geochemical and physical proxies for the impact when attempting to define the K-Pg impact layer, but I felt the authors were too dismissive of palynostratigraphy which has been the primary tool used to locate and constrain the K-Pg horizon. Ideally, palynology, shock mineral and iridium analysis should all be part of an integrated toolkit to define the K-Pg horizon.

The iridium sampling in this paper seems to have been done in a non-continuous manner. Since most terrestrial Ir spikes are restricted to a few cm, it seems that interval samples might miss the impact horizon altogether. Two of the five plots seem to show more than one iridium peak. It would have been useful if the authors had resampled one of the known K-Pg impact layers to calibrate their technique.
I find the figure with the field photographs too small to be of much use. The authors are making a case that would be much more convincingly made by stratigraphic section connected as panel diagrams. Generalizations made about the thickness and width of ancient channel deposits are not supported by mere photographs. Likewise, the small diagrams on Fig. 4 are too generalized to be of much use. At its core, this is a paper about sedimentology and stratigraphy, but it does not include very much new sedimentologic or stratigraphic data.

I'll focus the rest of my comments on the coastal plain K-Pg sections of the Williston Basin. It is unclear if the "Wet Paleocene" hypothesis was actually ever published as such, or if the term is used here as a straw man. It has been clear since Fastovsky (1987) and Fastovsky and McSweeney (1987) that the Hell Creek Formations in dominated by saturated paleosols and that both the Hell Creek and the Fort Union were wet depositional settings.

The majority of the argument about the Hell Creek-Fort Union contact is based on fieldwork near Jordan, Montana with only the one site from Mill Iron representing the vast eastern exposures of the Hell Creek-Fort Union contact near Glendive MT, Baker MT, Ekalaka MT, Marmarth ND, Buffalo SD, Huff ND, and Faith SD. This is unfortunate because it is this eastern area that has the primary evidence for the marine tongues in the Hell Creek and the Paleocene Cannonball Sea.

A number of statements about the Cannonball Seaway are incorrect or are based on paleogeographic maps or diagrams that themselves are unconstrained by data. The Cannonball Sea is known from exposures of the Cannonball Member of the Fort Union Formation best exposed near Mandan, North Dakota. The outcrop belt of the Cannonball runs roughly N-S between long. 100 and 102 degrees West and it is poorly exposed over most of its mapped area (Cvancara, 1976). The base of these unit is less that 10m above the K-Pg boundary and its total thickness does not exceed 115m. No exposures of the formation are known north or south of North Dakota. Thus, the seaway itself is known only from very localized exposures. Various authors have speculated about the northern or southern extent of the seaway, but these speculations are unconstrained. The absence of Paleocene marine deposits in the northern portion of the Gulf Coast suggests that the Cannonball Seaway might have extended north to the Arctic Ocean and this is probably why Blakely has shown a northward dribbling of the seaway on his map shown in Fig. 1. The bottom line is that we know very little about the extent of the Cannonball Seaway largely due to lack of exposure or post-Paleocene erosion.

Lower Paleocene marine rocks that have been considered correlative to the Cannonball Formation occur in the Cave Hills and Table Mountain in Harding County SD where they overly the Ludlow Member of the Fort Union and are thus likely to have been deposited around 65Ma.

In southwestern North Dakota, two "marine tongues" (the Corbicula and Oyster tongues) occur in very limited outcrops on the banks of the Little Missouri River north of Marmarth, ND and are expressed by the presence of in situ Corbicula and oysters in brown mudstones. These horizons occur ~80M about the Hell Creek-Fort Union contact.

The Hell Creek Formation in central ND contains two marine horizons, the Breien member and the Cantapeta Tongue. Both are evidence of marine transgression in Hell Creek Time.

The Tanis K-Pg site south of Bowman North Dakota appears to be a seche wave deposit caused by the impact shock waves. Curiously, the Tanis site contains well preserved ammonites and baculites as well as marine dinoflagellates. This suggests that a pre-Cannonball K-Pg age marine unit, now lost to erosion must have existed at the time of the K-Pg impact. It also suggests direct marine proximity coincident with the deposition of lignite beds that mark the HC-FU contact.

Since there is a disconformity between the Fox Hills Sandstone and the Hell Creek Formation, the regressive cycle of the Cretaceous seaway was completed after the deposition of the Fox Hills Sandstone and before the deposition of the Hell Creek Formation. The subsequent marine transgression is documented by the two Hell Creek Marine tongues, the Tanis ammonites and dinoflagellates, the Cave Hills nearshore marine deposits, and the two marine tongues in the Ludlow Member of the Fort Union Formation. This there is evidence for marine progradation at five times between 66.7 and 65.0Ma. This is the more complete context for the argument that marine transgression drove the deposition of the lignitic zone at the base of Fort Union Formation.

I think that the authors could have done a more thorough job of inspecting published sections where the K-Pg boundary was picked above the basal lignite of the Fort Union Formation. These have been documented by both Nichols and Bercovici and two of these sections (Pyramid Butte and John's Nose) do have iridium data that supports the palynological picks. Several other sites documented by Nichols and Johnson (2002 GSA Spec Pap. 361, Fig. 36) show up to 3m of lignitic strata below the K-Pg contact in 14 sections (including one section that has an in situ dinosaur above the basal lignite but below the palynological pick for the K-Pg boundary).

While it does seem unlikely that marine transgression in the western Dakotas would impact the coeval HC-FU transition as far to the west in the Fort Peck region without being time-transgressive, I think we need more observations on well-constrained sections before the HC-FU marine transgression hypothesis is rejected.

Reviewer #2 (Remarks to the Author):

General Comments

The manuscript presents a hypothesis of dinosaurs as ecosystem engineers (DEE) in the topmost Cretaceous, with herds of these large organisms promoting an open and patchy vegetation mosaic. In contrast, after the extinction of the non-avian dinosaurs, Paleocene vegetation assumed a denser forested structure, with distinctive coals. Evidence in support of the hypothesis includes a prominent change in fluvial style across the boundary (as defined by an iridium anomaly and other physical evidence) over several basins in a large area of western North America. The hypothesis draws parallels with ecosystem changes after the extinction of Pleistocene megafauna.

I found the account well-argued, intriguing, and likely to be of interest to a wide readership. The manuscript is well written, with good illustrations, a solid account of Methods, and detailed Supplementary Information. There is no doubt that large animals, especially herds of herbivores, profoundly change modern landscapes and, where documented, in the past.

Below are some comments about topics that need fuller documentation and discussion. They include justification in the text for the profound landscape effects of dinosaurs up to the boundary in the study area and for limited landscape effects of mammals thereafter. Inferring changes in fluvial style is a considerable challenge and needs a much fuller account. Additional information could go as text and figures in the Supplementary Information but also needs to be discussed in the main text. I have not uploaded an annotated pdf.

Detailed Comments

1) Documentation of fluvial style. Lines 131-3 note that the DEE hypothesis is based on 1) meander-belt size and stability, and 2) the amount of clastic sediment deposited on the distal floodplain. An example is given in lines 139-146, where Upper Cretaceous fluvial strata comprise small, single-storied channel bodies attributed to short-lived river channels or crevasse splays (interpreted as "meander belts" in Line 133), associated with drab mudstones and hydromorphic paleosols. These deposits are attributed to frequent avulsion of channels linked to dinosaur activity, allowing clastic sediment to be deposited across overbank areas. The overlying Paleocene strata comprise large, multistoried channel bodies with lateral-accretion deposits attributed to meandering channels, associated with coals. These deposits are interpreted as the product of prolonged reworking of meandering channels stabilized within a more densely vegetated landscape, with little deposition of overbank clastics.

An abrupt change in fluvial style above the KPB is the key observation upon which the DEE hypothesis is evaluated. It is thus important to make a robust interpretation. Although there clearly is a change in facies, the photos of fluvial strata (Fig. 2 and Extended Data Figs. 2 and 3) are too small to document the changes effectively. Additionally, the text contains only a brief general account (Lines 130-168), mainly linked to earlier studies in the literature, although some additional description is provided in the Supplementary Information. Overall, the information provided does not allow the reader to make a confident, independent assessment of the fluvial style. Some key issues are discussed below.

Little information is provided about the important question of dimensions (width and thickness) of channel bodies below and above the KPB. I don't think that there is any information about the thickness of the Cretaceous channel bodies, other than being "small" (Line 139), and the Paleocene channel bodies are noted only as 10-30 m thick at one locality. Lines 508, 520-1, and places in the Supplementary Information describe them only as "enormously thick", "huge", or "large". I need some convincing that small, localised channel bodies below the KPG and “enormously thick” channel bodies above the KPG really represent continuity between the same fluvial systems, differing only as to whether dinosaurs were present on the landscape.

There is also a note in Supplementary Information that “minor erosional unconformities” are present at the boundary in some areas. This raises questions about whether the fluvial systems are continuous from below to above the boundary, or whether there may be gaps in time sufficient for fluvial systems to have evolved a range of possible styles.

Paleoflow measurements are mentioned in the Methods, where Lines 578-9 mentions trough cross-beds, ripple marks, and accretion surfaces. However, no paleoflow data are provided. Was paleoflow similar in channel bodies below and above the KPB? Or was the change in dimensions and style accompanied by a significant change in paleoflow patterns, which might represent reorganisation of fluvial systems at the boundary? Or could we be seeing a contrast between transverse systems (megafans) and axial drainage (trunk rivers) above and below the KPB?

Crucially, what is the internal organisation and 3D geometry of the channel bodies below and above the KPB? This gets minimal discussion. All the channel bodies are attributed to meandering systems, but such an inference needs much more documentation. Single-storey bodies below the KPB could be small or short-lived meandering systems, or crevasse channels, as suggested by the authors. If they are crevasse channels, what types of mainstem channels provided the flow? They could also be small fixed-channels (self-constraining: Candel et al. 2019, The Depositional Record) and possibly anastomosing, incised into tough alluvium. Or possibly low-sinuosity channel bodies with alternate or side bars. Were they alluvial channels, in equilibrium with their floodplains, or were they incised?

Above the KPB, some of the inclined surfaces certainly look like lateral accretion (LA) surfaces in meandering channels (Lines 159-160), but some could be counterpoint bars rather than point bars (see Durkin et al., 2020, Sedimentology). Some of the thick sandstone bodies might be more akin to braided-fluvial systems with a mixture of mid-channel and bank-attached bars, some of which might have inclined slipface deposits that show up in outcrops. It’s hard to tell from the photos.

Supplementary Information notes that some of the multistorey channel bodies above the KPB are deeply incised into the associated strata. Could some be large valley fills? If so, were they really forming in conformity with the overbank peats or might they represent later incisional events into a wetland with which they had no previous connection? If the incised channels are considerably younger than the coals, the presence of peats would have little relevance to a hypothesis of strong bank vegetation constraining the channels.

I would be much happier about the interpretation of fluvial style if the text and Supplementary Information were accompanied by a set of detailed tracings of photomosaics and a solid facies analysis, with interpretation of bedforms, barforms, and channel-body geometry. Similarly, detailed tracings and close-up photos of floodplain deposits should be included. If space is too limited for such images and accounts in the main text, they should be included in the Supplementary Information and linked to the main text.

2) Other explanations for changes in fluvial style. Regional tectonic changes were ruled out by the authors because they should operate over a long timescale. Additionally, large channel bodies and coals would be unlikely to appear in separate basins simultaneously. Although these arguments appear reasonable, much depends on exactly what “tectonic changes” are in play. Local fault motion would be unlikely to affect several basins, but a major episode of thrusting might cause widespread changes in linked basins. More broadly, changes in channel discharge, dimensions, and styles can occur for many reasons, including river capture (for example due to headward erosion unrelated to tectonic events) and the autogenic formation of new lobes and channels on alluvial fans and megafans. Although such changes would not likely affect numerous basins simultaneously, much depends on how far apart the localities are, how truly separate the basins were (whether, for example, the same major fault zone bounds several basins), and whether KPB strata were laid down on megafans at basin margins or in axial river systems. These questions need discussion, if tectonics and drainage-basin changes are to be ruled out.

Climatic change above the boundary (the ‘Wet Paleocene’ hypothesis) is discounted by the authors, on the grounds that floodplains were poorly drained both below and above the KPB. However, much depends on a detailed assessment of paleoclimate. Are such assessments available? For example, might a modest change to somewhat more humid conditions account in part for more coal above the boundary?

In relation to paleoclimate, Lines 107 and 299 note a 30 degree range of latitude for the study sites. Based on the use of Ron Blakey’s reconstruction in Figure 1, this might seem to be paleolatitude, rather than simply modern latitude. Can you clarify this? If it is paleolatitude, the observed sedimentological effects covered a wide paleoclimatic range, and this could usefully be emphasised.

3) Stratigraphic distribution of dinosaur and mammalian fossils. A detailed account of fossil evidence is needed for the reader to be convinced that dinosaurs were ecosystem engineers in the Cretaceous but that mammals didn’t carry this function in the Paleocene. What is the evidence for the presence of dinosaurs? -- dinosaur bones, trackways, trample grounds, eggshells? How abundant are the indication, and how far below the KPB are such occurrences present and abundant? Is there any evidence that trackways and trampling were present along or within channels? Similarly, how diverse and abundant are mammalian occurrences in the studied Cretaceous and Paleocene strata?

These issues aren't discussed in the main text, but evidence is provided in Lines 469-548 of the Methods, with some information about the relative position of the fossils with respect to the KPB. However, this important evidence also needs to be summarized explicitly in the main text.

4) Dinosaur extinction patterns. The sense in the text is that dinosaurs were effective landscape agents right up to their extinction at the KPB. Line 123 says “sudden extinction”. However, the extinction pattern is not explored in the text. Did dinosaurs become extinct en masse at the KPB, or might their diversity and abundance have changed (decreased?) prior to the KPB? If particular clades and dinosaurs overall died out gradually, changes in fluvial style would likely be gradual, not abrupt, raising doubt about the viability of the DEE hypothesis.

The literature contains a lengthy and complex discussion about dinosaur diversity and abundance late in the Cretaceous, depending on geographic areas and taxa. For example, Brusatte et al. (2017) inferred that dinosaurs were diverse up to the KPB in Brazil, whereas in China, Han et al. (2022, PNAS) and Ma et al. (2022, Geophysical Research Letters) inferred a Late Cretaceous decline in dinosaur diversity, suggesting a linkage to the long-term deleterious effects of Deccan volcanism. Sakamoto et al. (2016 PNAS) and Condamine et al. (2021, Nature Communications) recorded a decline in dinosaur speciation and diversity well before the asteroid impact. Clearly, dinosaur diversity and abundance are not simple issues, but patterns of dinosaur diversity and abundance need discussion in the main text and Supplementary Information, if the reader is to be convinced that dinosaur extinction caused an abrupt change in fluvial systems.

5) Dinosaur effects on fluvial systems. “Dinoavulsion” is not a new concept, and DEE has earlier manifestations in the literature. Jones and Gustason (2006, Ichnos, v. 13, p. 31-41) inferred that trampling of dinosaurs through riverbanks and channel blockage by dinosaur skeletons promoted avulsion. McCarthy et al. (1998, African Journal of Ecology, v. 36, p. 44-56) noted that hippo trails in the Okovango Delta cut through riverbanks and cause avulsion. These papers could usefully be cited.

6) Impact of mammals on vegetation and rivers. Mammals that survived into the early Paleocene, or evolved rapidly, might conceivably have been large and/or abundant enough to cause some of the effects attributed to the dinosaurs. Lines 54-7 and 238-240 note the effects of large-animal trampling and uprooting on fluvial systems. However, groups of relatively small animals (for example, deer and goats) forage on trees and shrubs, assisting in keeping landscapes open.
As regards the KPB, García-Girón et al. (2022, Science Advances) noted a considerable diversification of mammals prior to the KPB, with increasing trophic impact, and Longrich et al. (2016, J. Evolutionary Biology) documented a rapid recovery of mammals after the KPB (within a few hundred thousand years). Although a major mammalian landscape effect seems unlikely, the diversity and abundance of mammals around the KPB needs to be addressed in the text.

Additionally, for the study sites, we are not told anything about the length of time in the Paleocene over which denser forests and stable meander belts are thought to have persisted. Was it hundreds of thousands, or millions, of years? Did the environmental setting eventually become more varied, and does the duration of strata characterized by coals and large channel bodies fit with what is known about the diversification and size of Paleocene mammals? Did they, in their turn, become ecosystem engineers at a later time?

I hope that these comments will be helpful in bringing an interesting and provocative manuscript to publication.
Martin Gibling

Reviewer #3 (Remarks to the Author):

The paper makes the interesting and novel claim that terrestrial landscapes were altered substantially immediately after the extinction of the dinosaurs. By their trampling and feeding they kept landscapes open, suppressing tree growth, but when they had gone, forests thickened and sediment was retained in higher, meandering river systems and not washed down to the flood plain.

They establish neatly that the facies shift occurred across the KPg boundary, and that modern megaherbivores like elephants maintain open habitats by trampling and feeding. They note that the facies shift is predictable and occurs everywhere across the Western Interior of North America and challenge the current idea that such a shift is coincidental with the extinction boundary, marked by the iridium anomaly, and means nothing. Most famous of the novelties is the Z-coal, and coals pop up everywhere immediately after the asteroid impact, associated with colorful mudstones. These have traditionally been interpreted as pond deposits, but this is convincingly revised here to deposits associated with meandering rivers.

The dinosaur ecosystem engineering (DEE) hypothesis is then presented, based on evidence of the body sizes of the latest Cretaceous dinosaurs, and evidence from the rock record and modern ecology. Such generalized ecosystem engineering of landscapes by dinosaurs has been suggested many times before, and so this is a well-established idea, and widely acceptable.

Linking the two to make the DEE is novel and stimulating. The authors combine thorough evidence from their observations of sedimentology of sections, which is the novel work here. They present ways in which their hypothesis could be refuted, which is gratifying and impressive.

The text is thorough and clear, and beautifully well organised. The illustrations are thorough and comprehensive. The author list includes all the expertise in sedimentology, paleosols, paleobotany, and regional geology one could wish for.

I hunted high and low and failed to find any objectionable sentences, or even typos.

Mike Benton

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Version 1:

Decision Letter:

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Dear Dr Weaver,

Your manuscript titled "Dinosaur extinction can explain continental facies shifts at the K-Pg boundary" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you address the remaining concerns of our reviewers and better acknowledge the study's limitations and the existence of alternative hypotheses, discuss your results in light of the literature, and clarify your definitions.

We therefore invite you to revise your paper one last time. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I have read the authors' response to the reviews and here is my feedback:

I like the hypothesis presented in this paper, but I do not think that the authors have provided enough data to reject alternate hypotheses. The paper covers a vast area reviews several Laramide basins with many of the observations based on literature. The field observations are broad and generalized and the diagrams used to support the hypothesis are either highlighted outcrop photographs (Fig. 1) or highly generalized cartoons (Fig 4).

The primary data presented are new detailed K-Pg sections and broader regional generalizations about the nature of the pre and post K-Pg stratigraphy and sedimentology. Neither data set is neither particularly large nor robust. As a result, the paper reads more like a grant proposal than a publication. In addition, other data that contradict the thesis were not adequately discussed (e.g. the many detailed K-Pg boundary sections in North Dakota where the transition to coal lies below the K-Pg boundary as defined by iridium anomalies.

The core of the paper is really a discussion about the relative location of the formation contact and the K-Pg boundary at many different localities. To do this well, one needs clear definitions of both, and I don't think that the paper has done that particularly well. I recognize and applaud the effort at regional synthesis, but I think the paper itself falls short.

I think that a few key lines of evidence are absent. The first is the hyperabundance of volcanically-derived bentonitic clay in the Hell Creek formation and its absence in the Fort Union Formation Could not the formation change be caused by a cessation of volcanic eruptions in the hinterland of western Montana at or near the K-Pg? This mechanism might do a better job at explaining the east-west extent of the no coal to coal transition near the K-Pg boundary. The Cannonball Sea hypothesis is dismissed with minimal analysis.

I guess that I am fine with it being published because I think that the hypothesis should be in the literature, but I wish that the data were more, and better, and that the figures were less abstracted and more data rich.

Specific comments:
In the figures, both the formation contact, and the K-Pg should be noted. This is most egregious in Figure 1 (which needs sub-labels btw) where the Raton Basin section shows a line for the K-Pg at the base of a sandstone bed while the white tonstein that is the K-Pg boundary is visible in the outcrop on the right side of the photo below the red line.

Also, the authors often use the terms “sandstones” and “mudstones” when they should use sandstone beds (or layers, units etc.)

Kirk Johnson

Reviewer #2 (Remarks to the Author):

I appreciate the substantial and thoughtful revisions to the manuscript, following the reviewers' comments. I also appreciate the frank assessment that the individual study areas need a fuller sedimentological and geochemical assessment. The improved manuscript now provides a credible body of data to support the important hypothesis of dinosaurs as ecosystem engineers in the Western Interior landscapes. Congratulations on a provocative body of research. Martin Gibling

We thank the reviewers for these thoughtful and constructively critical comments. In general, we agree with the points raised, and we have addressed them in the revised version of the manuscript. Along with addressing minor points, we have made the following substantive revisions:
(1) We have added a new figure, Extended Data Figure 4, that includes additional sedimentological details, better demonstrating fluvial-system changes across the K-Pg boundary
(2) We have revised Figure 2 to be larger and thus better illustrate the sedimentological features discussed here
(3) We have added substantial text to the main manuscript and Supplementary Information better describing the fluvial features central to our discussions
(4) We have better justified our iridium sampling approach and rationale for interpreting these iridium anomalies as indicative of the K-Pg boundary
(5) We have added caveats and additional details to explain our interpretations and decisions throughout the manuscript and Supplementary Information.

We believe this revised manuscript is substantially improved and adequately addresses all of the reviewers' concerns. Below, we respond to each reviewer comment individually.

Again, we thank the reviewers and editors for their thoughtful assessment of our paper. It is much better as a result.

Sincerely,

Lucas N. Weaver

Editors:

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

**Provide firm and robust stratigraphic and paleontological evidence to demonstrate that Dinosaurs were ecosystem engineers in the Late Cretaceous, and that their extinction led to landscape and ecosystem reorganization in the early Paleogene.

We have now included additional sedimentological and stratigraphic data to more clearly demonstrate the phenomenon highlighted here. Specifically, we have added additional details in both the main text and Supplementary Information (see responses to Reviewer 2), enlarged Figure 2, and added an additional sed/strat figure (Extended Data Figure 4).

**Justify your methodological approach, demonstrating that the iridium sampling is robust, and that the stratigraphic analysis of KPg sections is complete.

We have now more clearly described our iridium-sampling approach and rationale for interpreting these iridium anomalies as indicative of the K-Pg boundary. See Lines 519–527 and Lines 608–613.

**Appropriately discuss your recommendations in the context of the existing literature, and discuss alternative interpretations of your data.

We have now added caveats where necessary based on the reviewer comments. See specific responses below.

Reviewer #1 (Remarks to the Author):

I think that it is an interesting idea that the removal of large dinosaurs from a landscape would have a visible impact on the deposition of fluvial sediments and the resulting stratigraphy. This hypothesis is worth discussion, and I like the paper's multi-basin approach. I also admire the effort to frame these concepts within a regional context and I applaud the documentation of the new sites in the Bighorn Basin.

We are grateful that the reviewer appreciated our intent and approach here. Thank you!

That having been said, I think that it is worth separating the densely sampled K-Pg sections from the coastal plain (Williston Basin) from the more sparsely sampled montane proximal basins (Raton, Denver Bighorn, Alberta, and Powder). It is in the Williston Basin where Roland Brown's "last dinosaur below the lowest coal" approach has been most commonly applied and where this new hypothesis can best be tested.

We agree that the Williston Basin provides the best record of continental K-Pg boundary (KPB) sections and most obviously documents KPB-coincident facies changes (championed by Roland Brown, among others). It is for that reason that the bulk of our sedimentological and stratigraphic portions of the manuscript focused on the Williston Basin. That said, although not as richly sampled as the Williston Basin, we believe it would be a missed opportunity not to discuss the implications of this hypothesis in other basins in the Western Interior. Indeed, one of the major pieces of evidence supporting our hypothesis is the geographically widespread nature of the KPB-coincident facies change. We emphasize that our hypothesis and the predictions laid out near the end of the manuscript should prompt testing of this idea in all continental KPB sections, and a rigorous analysis of our core hypothesis requires that we examine all available continental records in the Western Interior, not treating some basins separately.

I agree that it is important to use the geochemical and physical proxies for the impact when attempting to define the K-Pg impact layer, but I felt the authors were too dismissive of palynostratigraphy which has been the primary tool used to locate and constrain the K-Pg horizon. Ideally, palynology, shock mineral and iridium analysis should all be part of an integrated toolkit to define the K-Pg horizon.

We agree that palynology is an important tool for identifying the KPB, despite its flaws, and we also agree that we were too quick to dismiss its utility in the main text. As such, we have now emphasized the utility of palynology in the main text, and we more clearly point the reader to the Supplementary Information for a more nuanced discussion of the merits and shortcomings of using pollen to identify the KPB (see Lines 210–214).

The iridium sampling in this paper seems to have been done in a non-continuous manner. Since most terrestrial Ir spikes are restricted to a few cm, it seems that interval samples might miss the impact horizon altogether. Two of the five plots seem to show more than one iridium peak.

We assume by “non-continuous” the reviewer means “not at an extremely fine stratigraphic scale”? It is true that our Bighorn Basin sites were sampled more coarsely than, for example, some of the KPB sites (cm to dm scale, vs. mm to cm scale). However, this was done for several reasons:
(1) Practically, we did not have the budget to pay for iridium analyses on dozens of samples per site. As such, we were more selective in our sampling and focused on the fine-grained intervals yielding the highest potential of an impact dust signature (see e.g. Senel et al., 2023 Nature Geoscience).
(2) Scientifically, our sampling specifically aimed to explore the hypothesis that the KPB aligns with a distinct facies change. As such, we were mostly interested in sampling red claystones immediately underlying the Fort Union Formation, and we took samples above and below to contextualize those samples. Indeed, in all of our sites, the greatest Ir spikes occurred at those red claystones, which did not exceed 5 cm in thickness.
(3) Scientifically, we too were surprised to see other smaller iridium peaks in the Little Polecat Dome and Elk Basin sections. However, we discovered that this is not uncommon—multiple iridium peaks have been reported at other KPB sites (e.g., West Bijou in the Denver Basin). We agree that these deserve further study, as does the mobility of iridium in terrestrial settings, but such a study is beyond the scope of this paper.

To address this concern, we have now bolstered our explanations and justifications of our Ir sampling approach in the main text (see Lines 519–527 and Lines 608–613).

It would have been useful if the authors had resampled one of the known K-Pg impact layers to calibrate their technique.

We agree that it would have been nice to have resampled a previously known KPB for comparison; however, this was not the intent of this project and would have expanded beyond our financial and logistical means. Nonetheless, we conducted this sampling by strictly following the procedures outlined in previous papers (e.g., Nichols and Johnson, 2008, and references therein), and both LNW and TST were trained in sediment sampling for iridium analysis by researchers who conducted those analyses in the northeastern Montana portions of the Williston Basin (Greg Wilson Mantilla, Jason Moore; Moore et al., 2014). Thus, we are confident that our approach is comparable to that conducted in other areas, and we have justified our technique in Lines 519–527 and Lines 608–613.

I find the figure with the field photographs too small to be of much use. The authors are making a case that would be much more convincingly made by stratigraphic section connected as panel diagrams. Generalizations made about the thickness and width of ancient channel deposits are not supported by mere photographs. Likewise, the small diagrams on Fig. 4 are too generalized to be of much use. At its core, this is a paper about sedimentology and stratigraphy, but it does not include very much new sedimentologic or stratigraphic data.

This is a fair critique, and one that is echoed by Reviewer #2. We agree that these interpretations would be bolstered by more clearly displaying the sedimentological and stratigraphic data underlying them. As such, we have done the following: (1) Enlarged the images in Figure 2, (2) Added additional annotated photographs and accompanying sedimentological and stratigraphic logs in a new figure (Extended Data Figure 4), and (3) Included more detailed sed/strat descriptions in the main text and Supplementary Information. See our responses to Reviewer 2.

Re: Fig. 4, we agree it is generalized, but we disagree that it is not of much use. Instead, we feel that this figure accomplishes two things: (1) illustrates the location of key KPB sections in the Western Interior, and (2) gives an overview of the types of facies changes known to occur at the KPB in those locations. Indeed, this was inspired by previous diagrams discussing paleoenvironmental change at the K-Pg boundary: Sweet and Braman (1992:fig. 1) and Sweet and Braman (2001:fig. 5). Although we previously pointed the reader to the Supplemental Information for more details about each of these different localities, we have now emphasized this point and clarified that these are generalized schematics that do not capture every detail of these sections (see revised caption for Fig. 4, Lines 335–226 and 338–340).

I'll focus the rest of my comments on the coastal plain K-Pg sections of the Williston Basin. It is unclear if the "Wet Paleocene" hypothesis was actually ever published as such, or if the term is used here as a straw man.

The term "Wet Paleocene" was created by us to encompass various hypotheses to explain the KPB facies change that proposed something to do with increased wetness (whether that be via increased precipitation, a rising water table, etc.). These are clear and prevalent hypotheses in the literature that we feel deserve to be addressed here since they are central to the topic of this paper. We address the Fastovsky papers below, but this hypothesis has also been invoked by researchers like Wolfe and Upchurch (1987) and Sweet and Braman (1992). To make that purpose clearer, we have now added clarifying text in Lines 161–163.

It has been clear since Fastovsky (1987) and Fastovsky and McSweeney (1987) that the Hell Creek Formations in dominated by saturated paleosols and that both the Hell Creek and the Fort Union were wet depositional settings.

We agree, and these studies are consistent with our interpretations of Hell Creek and Fort Union sedimentology. However, Fastovsky (1987) still invoked a rising water table (via increased precipitation) to explain the facies change at the KPB. From his Summary and Conclusions section (Fastovsky, 1987:p. 292): "A rise in water table, probably attributable to increased rainfall, induced a change in the paleoenvironments of this region at a time approximately coincident with that of the faunal and floral changes that define the K-P boundary." The same explanation is invoked by Fastovsky and McSweeney (1987:pp. 66 and 75).

A key point of this paper is arguing that there is no evidence for widespread ponding in the lowermost Fort Union, and the "pond" deposits of Fastovsky are really point-bar deposits (i.e., lateral accretion beds or inclined heterolithic stratification) associated with large meandering channels. Far from a straw man, this interpretation is central to much of the discourse surrounding KPB facies changes.

The majority of the argument about the Hell Creek-Fort Union contact is based on fieldwork near Jordan, Montana with only the one site from Mill Iron representing the vast eastern exposures of the Hell Creek-Fort Union contact near Glendive MT, Baker MT, Ekalaka MT, Marmarth ND, Buffalo SD, Huff ND, and Faith SD. This is unfortunate because it is this eastern area that has the primary evidence for the marine tongues in the Hell Creek and the Paleocene Cannonball Sea.

This is true, and we agree that future research testing this idea in other portions of the Williston Basin would be a great idea (and something we hope to do in the future). Nonetheless, our findings from the Jordan area are consistent with those reported throughout the Williston Basin and beyond (see our references in the main text and the Supplementary Information) and sufficiently compelling to justify presenting a new hypothesis. Nonetheless, we have added additional text to make clear that our detailed sed/strat data are relatively limited in geographic scope (Lines 638–640), and we believe this study presents hypotheses that could be tested in other localities in the future.

A number of statements about the Cannonball Seaway are incorrect or are based on paleogeographic maps or diagrams that themselves are unconstrained by data.

We are unclear about the specific “incorrect” statements about the Cannonball Seaway about which the reviewer is referring. However, we did not make any interpretations based on paleogeographic maps—we did include a Ron Blakey map approximating the paleogeography of the Western Interior at the KPB in Figure 1, but that was merely for illustrative purposes to show the approximate location of the Western Interior Seaway and mountain uplifts relative to KPB sections. To clarify this point, we’ve added additional language to the Fig. 1 caption. See Lines 90–91.

We are also unclear about diagrams that are “unconstrained by data”. We relied on previous stratigraphic and geochronological data for our interpretations about the timing of, and evidence for, Cannonball Sea transgression. We assume the reviewer is referencing our Extended Data Figure 5 (previously EDFig. 4)? If so, we acknowledge that we did not adequately articulate where those data and subsequent interpretations came from—we have now remedied that with additional text in the Methods (see Lines 653–655) and in the caption for Extended Data Figure 5 (see Lines 770–774).

The Cannonball Sea is known from exposures of the Cannonball Member of the Fort Union Formation best exposed near Mandan, North Dakota. The outcrop belt of the Cannonball runs roughly N-S between long. 100 and 102 degrees West and it is poorly exposed over most of its mapped area (Cvancara, 1976). The base of these unit is less that 10m above the K-Pg boundary and its total thickness does not exceed 115m. No exposures of the formation are known north or south of North Dakota. Thus, the seaway itself is known only from very localized exposures. Various authors have speculated about the northern or southern extent of the seaway, but these speculations are unconstrained. The absence of Paleocene marine deposits in the northern portion of the Gulf Coast suggests that the Cannonball Seaway might have extended north to the Arctic Ocean and this is probably why Blakely has shown a northward dribbling of the seaway on his map shown in Fig. 1. The bottom line is that we know very little about the extent of the Cannonball Seaway largely due to lack of exposure or post-Paleocene erosion.

We thank the reviewer for this perspective on the exposures (or lack thereof) of the Cannonball Member of the Fort Union Formation. We appreciate that more information is needed to understand the geographic extent of this unit, but these shortcomings do not support the interpretation that KPB facies changes were driven by the transgression of the Cannonball Sea. As we describe in the text, our best chronostratigraphic data for that unit support its deposition beginning well-after the KPB (e.g., Peppe et al., 2009, 2011). Nonetheless, we have added additional language making clear the realities of Cannonball exposures and the potential for uncertainty regarding its geographic extent in the Supplementary Information (pg. 11, near the top of the second paragraph) as well as in the caption of EDFig. 5 (Lines 772–774).

Lower Paleocene marine rocks that have been considered correlative to the Cannonball Formation occur in the Cave Hills and Table Mountain in Harding County SD where they overly the Ludlow Member of the Fort Union and are thus likely to have been deposited around 65Ma.

We thank the reviewer for pointing out the occurrence of these exposures, but their presence atop the Ludlow Member at an estimated age of 65 Ma supports our argument that Cannonball transgression far post-dates the KPB (ca. 66 Ma) and thus is an unlikely culprit for KPB facies change.

In southwestern North Dakota, two “marine tongues” (the Corbicula and Oyster tongues) occur in very limited outcrops on the banks of the Little Missouri River north of Marmarth, ND and are expressed by the presence of in situ Corbicula and oysters in brown mudstones. These horizons occur ~80M about the Hell Creek-Fort Union contact.

We assume the reviewer meant “~80M *above* the Hell Creek-Fort Union contact”? If so, then yes, we are aware of these marine tongues in the Fort Union Formation, and we already included them in Extended Data Figure 5 (formerly EDFig. 4)—see especially panel C. Our understanding is that the “Boyce Tongue”, the stratigraphically lowest, is the one recognized by the presence of *Corbicula*, whereas the “Three V Tongue” is rich in oysters (following Hartman, 1993). Importantly, based on the available chronostratigraphic data, the lowest of these tongues (Boyce) occurs near the top of chron29n, approximately 850 ka after the K-Pg boundary, supportive of our assertion that there is no clear evidence of a marine transgression coincident with the K-Pg boundary.

The Hell Creek Formation in central ND contains two marine horizons, the Breien member and the Cantapeta Tongue. Both are evidence of marine transgression in Hell Creek Time.

We are aware of the Breien Member and Cantapeta Tongues in the Hell Creek Formation. These were already included in our Extended Data Figure 5 (see panel A), and we discussed these in the main text (Lines 227–229). We agree that the sed/strat of the Hell Creek Formation is consistent with a continental unit formed during a marine transgression (see Lines 225–229).

The Tanis K-Pg site south of Bowman North Dakota appears to be a seche wave deposit caused by the impact shock waves. Curiously, the Tanis site contains well preserved ammonites and baculites as well as marine dinoflagellates. This suggests that a pre-Cannonball K-Pg age marine unit, now lost to erosion must have existed at the time of the K-Pg impact. It also suggests direct marine proximity coincident with the deposition of lignite beds that mark the HC-FU contact.

The interpretations of the Tanis site as a seiche deposit formed by the K-Pg impactor need more thorough analysis. Nonetheless, we thank the reviewer for highlighting the need to discuss this possibility. We have done so in additional text added to the Supplementary Information (see pg. 11, second to last paragraph).

There was certainly a pre-Cannonball K-Pg age marine unit somewhere in the Western Interior, but the Tanis site does not provide direct estimation of how "close" it was to the site, nor that it was undergoing a transgression or regression or was stagnant. We contend that hanging interpretations of a theoretical marine unit that may or may not have been erased by erosion is less parsimonious than our interpretation. Further, occurrence of marine invertebrates at the Tanis site does not suggest close marine proximity coincident with the deposition of the Fort Union Formation—the Tanis site, as we understand it, is largely an isolated set of exposures of the Hell Creek Formation, and there are no Fort Union exposures in the immediate vicinity.

Since there is a disconformity between the Fox Hills Sandstone and the Hell Creek Formation, the regressive cycle of the Cretaceous seaway was completed after the deposition of the Fox Hills Sandstone and before the deposition of the Hell Creek Formation. The subsequent marine transgression is documented by the two Hell Creek Marine tongues, the Tanis ammonites and dinoflagellates, the Cave Hills nearshore marine deposits, and the two marine tongues in the Ludlow Member of the Fort Union Formation. This there is evidence for marine progradation at five times between 66.7 and 65.0Ma. This is the more complete context for the argument that marine transgression drove the deposition of the lignitic zone at the base of Fort Union Formation.

We do not dispute the interpretation that marine regression occurred between the Fox Hills and Hell Creek, that there were at least two marine transgressive pulses during Hell Creek deposition, nor that there were at least two marine transgressive pulses that occurred during the long period of Fort Union deposition. Indeed, we discuss these in the text (see previous response) and convey them in the EDFig. 5 (panel A). This sort of waxing and waning of the Western Interior Seaway occurred throughout the Late Cretaceous and early Paleocene; yet, never before Fort Union deposition do we see the facies change that characterizes that unit. That latter fact is already discussed in the Supplementary Information (pg. 12, last paragraph of the 'Alternative hypotheses…' section). As we've demonstrated in the main text, Extended Data Figure 5, and the Supplementary Information, there is no evidence that the KPB continental facies change was coincident with any of these transgressions nor time-transgressive as would be expected. Thus, our point stands: There is no evidence of marine transgression at the KPB, nor evidence that it is responsible for the KPB facies change.

I think that the authors could have done a more thorough job of inspecting published sections where the K-Pg boundary was picked above the basal lignite of the Fort Union Formation. These have been documented by both Nichols and Bercovici and two of these sections (Pyramid Butte and John's Nose) do have iridium data that supports the palynological picks. Several other sites documented by Nichols and Johnson (2002 GSA Spec Pap. 361, Fig. 36) show up to 3m of lignitic strata below the K-Pg contact in 14 sections (including one section that has an in situ dinosaur above the basal lignite but below the palynological pick for the K-Pg boundary).

We did inspect these sections and have already addressed them both in the main text (Lines 215–222) and at length in the Supplementary Information (see pgs. 9 and 10, last two paragraphs of the 'All impact-identified KPBs…' section). In short, although the appearance of coals is one indicator of the large-scale lithological shift, it is not the only one; there are also a few coals that occur entirely within the HC. Hence, it is not unexpected that there are a few instances where the KPB occurs above the basal lignite.

While it does seem unlikely that marine transgression in the western Dakotas would impact the coeval HC-FU transition as far to the west in the Fort Peck region without being time-transgressive, I think we need more observations on well-constrained sections before the HC-FU marine transgression hypothesis is rejected.

We appreciate that the reviewer does not think there is sufficient evidence to support the marine-transgression hypothesis, but that it could still be investigated further. We do not disagree—as noted in conclusion, one of our hopes is that this paper stimulates further research on the KPB in continental settings and the mechanisms underlying paleoenvironmental change recorded in those fluvial strata. Nonetheless, as highlighted in the responses above, the bulk of the available data refute the marine transgression hypothesis. As with any hypothesis, this could be shown to be an incorrect interpretation in the future, but we hope the revisions mentioned above help make it clearer why we've made this decision here.

Reviewer #2 (Remarks to the Author):

General Comments

The manuscript presents a hypothesis of dinosaurs as ecosystem engineers (DEE) in the topmost Cretaceous, with herds of these large organisms promoting an open and patchy vegetation mosaic. In contrast, after the extinction of the non-avian dinosaurs, Paleocene vegetation assumed a denser forested structure, with distinctive coals. Evidence in support of the hypothesis includes a prominent change in fluvial style across the boundary (as defined by an iridium anomaly and other physical evidence) over several basins in a large area of western North America. The hypothesis draws parallels with ecosystem changes after the extinction of Pleistocene megafauna.

I found the account well-argued, intriguing, and likely to be of interest to a wide readership. The manuscript is well written, with good illustrations, a solid account of Methods, and detailed Supplementary Information. There is no doubt that large animals, especially herds of herbivores, profoundly change modern landscapes and, where documented, in the past.

We thank the reviewer for their complimentary assessment of our manuscript!

Below are some comments about topics that need fuller documentation and discussion. They include justification in the text for the profound landscape effects of dinosaurs up to the boundary in the study area and for limited landscape effects of mammals thereafter. Inferring changes in fluvial style is a considerable challenge and needs a much fuller account. Additional information could go as text and figures in the Supplementary Information but also needs to be discussed in the main text. I have not uploaded an annotated pdf.

We agree with the reviewer that we could provide clearer support for the changes in fluvial geomorphology that we describe in the paper. We are grateful for this review because it made us recognize that, because we (the authors) have spent so much time around these K-Pg strata and are entrenched in that literature, we took some things for granted that would not be clear to readers outside of this specific study system. We address these sentiments below in our responses to specific comments.

Detailed Comments

1) Documentation of fluvial style. Lines 131-3 note that the DEE hypothesis is based on 1) meander-belt size and stability, and 2) the amount of clastic sediment deposited on the distal floodplain. An example is given in lines 139-146, where Upper Cretaceous fluvial strata comprise small, single-storied channel bodies attributed to short-lived river channels or crevasse splays (interpreted as “meander belts” in Line 133), associated with drab mudstones and hydromorphic paleosols. These deposits are attributed to frequent avulsion of channels linked to dinosaur activity, allowing clastic sediment to be deposited across overbank areas. The overlying Paleocene strata comprise large, multistoried channel bodies with lateral-accretion deposits attributed to meandering channels, associated with coals. These deposits are interpreted as the product of prolonged reworking of meandering channels stabilized within a more densely vegetated landscape, with little deposition of overbank clastics.

An abrupt change in fluvial style above the KPB is the key observation upon which the DEE hypothesis is evaluated. It is thus important to make a robust interpretation. Although there clearly is a change in facies, the photos of fluvial strata (Fig. 2 and Extended Data Figs. 2 and 3) are too small to document the changes effectively. Additionally, the text contains only a brief general account (Lines 130-168), mainly linked to earlier studies in the literature, although some additional description is provided in the Supplementary Information. Overall, the information provided does not allow the reader to make a confident, independent assessment of the fluvial style. Some key issues are discussed below.

Little information is provided about the important question of dimensions (width and thickness) of channel bodies below and above the KPB. I don’t think that there is any information about the thickness of the Cretaceous channel bodies, other than being “small” (Line 139), and the Paleocene channel bodies are noted only as 10-30 m thick at one locality. Lines 508, 520-1, and places in the Supplementary Information describe them only as “enormously thick”, “huge”, or “large”. I need some convincing that small, localised channel bodies below the KPG and “enormously thick” channel bodies above the KPG really represent continuity between the same fluvial systems, differing only as to whether dinosaurs were present on the landscape.

This is a fair critique, and we admit that we made the incorrect assumption that some of these characterizations were “obvious” (a reflection of our disciplinary bias!). We have included more detailed descriptions of channel size (stratigraphic thickness and lateral extent) in Lines 144–149. We have also modified Figure 2 to be larger and more clearly show the sedimentary features to which we refer, and we have included a new figure (Extended Data Figure 4) which includes stratigraphic logs that more clearly demonstrate the relative thicknesses, stacking patterns, and sedimentary features of the latest Cretaceous vs. earliest Paleogene fluvial strata.

There is also a note in Supplementary Information that “minor erosional unconformities” are present at the boundary in some areas. This raises questions about whether the fluvial systems are continuous from below to above the boundary, or whether there may be gaps in time sufficient for fluvial systems to have evolved a range of possible styles.

The “minor erosional unconformities” refer to places in which Fort Union channels have scoured into underlying Hell Creek strata. However, these frequently occur lateral to a continuous section, wherein the K-Pg boundary (KPB) is present with no evidence of missing time or rock below or above. The occurrences of these Paleocene channels scouring into underlying Cretaceous strata have been well-documented in the literature (e.g., Archibald, 1982; Smit and van der Kaars, 1984; Fastovsky and Dott, 1986; Lofgren et al., 1990; Moore et al., 2014; Weaver et al., 2022; Claytor et al., 2023), and there is no evidence for substantial missing time between the cessation of Hell Creek deposition and onset of Fort Union deposition. To avoid confusion, we have added clarifying language following that statement in the Supplemental Information.

Paleoflow measurements are mentioned in the Methods, where Lines 578-9 mentions trough cross-beds, ripple marks, and accretion surfaces. However, no paleoflow data are provided. Was paleoflow similar in channel bodies below and above the KPB? Or was the change in dimensions and style accompanied by a significant change in paleoflow patterns, which might represent reorganisation of fluvial systems at the boundary? Or could we be seeing a contrast between transverse systems (megafans) and axial drainage (trunk rivers) above and below the KPB?

We did not originally include specific paleoflow measurements because paleoflow directions for the Hell Creek and Fort Union formations are already well-documented in the literature. Our reference to paleoflow measurements in the Methods was specifically about documenting that paleoflow, where measurable, is consistently perpendicular to the depositional dip of the lateral accretion deposits in the Fort Union Formation. Nonetheless, we agree that it would be useful and more convincing to include our paleoflow measurements here. We have now done so, and these data are summarized in Lines 175–180. Note that there is no systematic change in paleoflow direction across the KPB (roughly east–northeast); yet, we have measured increased variation in flow directions in the Fort Union Formation, supportive of our interpretation that Fort Union channels are more sinuous and had more long-lived meander belts.

Crucially, what is the internal organisation and 3D geometry of the channel bodies below and above the KPB? This gets minimal discussion. All the channel bodies are attributed to meandering systems, but such an inference needs much more documentation.

We have now provided details about why we consider this a meander river system, drawing from our own observations, which are consistent with the primary literature. See Lines 140–143.

Single-storey bodies below the KPB could be small or short-lived meandering systems, or crevasse channels, as suggested by the authors. If they are crevasse channels, what types of mainstem channels provided the flow? They could also be small fixed-channels (self-constraining: Candel et al. 2019, The Depositional Record) and possibly anastomosing, incised into tough alluvium. Or possibly low-sinuosity channel bodies with alternate or side bars. Were they alluvial channels, in equilibrium with their floodplains, or were they incised?

We admittedly do not have all of the details to constrain the specifics of the meandering system brought up here, but we intend to do so in the future. Our intent of this paper was the highlight the differences in pre- and post-KPB fluvial systems and the potential mechanisms underlying their genesis. We did not, however, conduct an exhaustive study of the fine details of these systems. Although we hope to pursue these questions in the future, we hope the additions noted here help alleviate some of these concerns.

Above the KPB, some of the inclined surfaces certainly look like lateral accretion (LA) surfaces in meandering channels (Lines 159-160), but some could be counterpoint bars rather than point bars (see Durkin et al., 2020, Sedimentology). Some of the thick sandstone bodies might be more akin to braided-fluvial systems with a mixture of mid-channel and bank-attached bars, some of which might have inclined slipface deposits that show up in outcrops. It's hard to tell from the photos.

We thank the reviewer for raising these points. In short, there are details in the meandering systems here that deserve further research, and we are excited to pursue these directions in the future. However, no matter if they are point bars or counterpoint bars, both are indicative of pronounced meandering river systems in Fort Union strata. Durkin et al. (2020), describe these features in Late Cretaceous strata, but those deposits are flanked laterally by typical Late Cretaceous overbank floodplain deposits (their Facies 5), consistent with hydromorphic paleosols (see for example, their Fig. 4C). As such, we contend that the preponderance of evidence supports our interpretation that lower Fort Union strata are dominated by meander-river deposits. Nonetheless, we have addressed these details in the Supplementary Information now (see pg. 10, first paragraph of the 'Alternative hypotheses…' section), leaning on our previous papers describing early Paleocene channel deposits in detail (Weaver et al., 2022; Claytor et al., 2023). We have also added more detailed information about these facies and their outcrop associations in the new Extended Data Figure 4.

Supplementary Information notes that some of the multistorey channel bodies above the KPB are deeply incised into the associated strata. Could some be large valley fills?

This is certainly a possibility. We have yet to put this in print, but the idea that some of these deeply-incised and laterally constrained channel deposits may be incised valley fills (e.g., the

Garbani channel, see Weaver et al. 2022) is something we've discussed. We hesitate to draw those conclusions at this point, but this is an active avenue of future research.

If so, were they really forming in conformity with the overbank peats or might they represent later incisional events into a wetland with which they had no previous connection? If the incised channels are considerably younger than the coals, the presence of peats would have little relevance to a hypothesis of strong bank vegetation constraining the channels.

The characteristic Fort Union channel deposits are undoubtedly coeval with the coals. The Fort Union Formation, and most earliest Paleogene strata in the Western Interior (as we illustrate here), is/are characterized by meandering-channel deposits bracketed above and below by coals. Although it is difficult to find extensive exposures in which the relationships between coal and channel deposits can be traced laterally, these alternating coal-channel-coal units persist for the entirety of Tullock Member deposition (see Sprain et al. 2015 and 2018) and more broadly within the Fort Union Formation, with a typical coal-to-coal interval spanning ~80 ka. Thus, the preponderance of evidence supports the interpretations that peat swamps existed lateral to meander belts, and when those meander-belt-channels eventually avulsed, they went to the lateral peat-swamp lowlands. We've now emphasized this point in Lines 157–158.

I would be much happier about the interpretation of fluvial style if the text and Supplementary Information were accompanied by a set of detailed tracings of photomosaics and a solid facies analysis, with interpretation of bedforms, barforms, and channel-body geometry. Similarly, detailed tracings and close-up photos of floodplain deposits should be included. If space is too limited for such images and accounts in the main text, they should be included in the Supplementary Information and linked to the main text.

We have now increased the size of the images in Figure 2 and created a new figure, Extended Data Figure 4, to better satisfy this request. We have also better described the barforms in the Supplementary Information (see pg. 10, first paragraph of the 'Alternative hypotheses…' section). We recognize that the lighting and scale of some images in Figure 2 (a necessity of the manuscript length and field conditions) obscure some details, however, we have now included more detailed stratigraphic logs of those units in Extended Data Figure 4, and we refer the reader to previous studies that corroborate our interpretations throughout the manuscript (especially Fastovsky and McSweeney, 1987).

2) Other explanations for changes in fluvial style. Regional tectonic changes were ruled out by the authors because they should operate over a long timescale. Additionally, large channel bodies and coals would be unlikely to appear in separate basins simultaneously. Although these arguments appear reasonable, much depends on exactly what "tectonic changes" are in play. Local fault motion would be unlikely to affect several basins, but a major episode of thrusting might cause widespread changes in linked basins. More broadly, changes in channel discharge, dimensions, and styles can occur for many reasons, including river capture (for example due to headward erosion unrelated to tectonic events) and the autogenic formation of new lobes and channels on alluvial fans and megafans. Although such changes would not likely affect numerous basins simultaneously, much depends on how far apart the localities are, how truly separate the basins were (whether, for example, the same major fault zone bounds several

basins), and whether KPB strata were laid down on megafans at basin margins or in axial river systems. These questions need discussion, if tectonics and drainage-basin changes are to be ruled out.

These are fair points, and again reflect our "taking for granted" some background in tectonic activity across the KPB in the Western Interior. We have now made clear the specifics (mechanistically and temporally) of the Laramide Orogeny, emphasizing how such tectonic processes were unlikely to have resulted in an instantaneous change in fluvial geomorphology (see Lines 240–243).

Climatic change above the boundary (the 'Wet Paleocene' hypothesis) is discounted by the authors, on the grounds that floodplains were poorly drained both below and above the KPB. However, much depends on a detailed assessment of paleoclimate. Are such assessments available? For example, might a modest change to somewhat more humid conditions account in part for more coal above the boundary?

We discussed this at length in the Supplement originally, but we have now briefly emphasized this point in the main text (see Lines 182–184). There is no compelling evidence for increased humidity or precipitation at the KPB.

In relation to paleoclimate, Lines 107 and 299 note a 30 degree range of latitude for the study sites. Based on the use of Ron Blakey's reconstruction in Figure 1, this might seem to be paleolatitude, rather than simply modern latitude. Can you clarify this? If it is paleolatitude, the observed sedimentological effects covered a wide paleoclimatic range, and this could usefully be emphasised.

This was in reference to modern latitude, not paleo latitude. Nonetheless, most plate movement in the North America was east-west in the Late Cretaceous–Paleocene, and there is little evidence for pronounced geographic migration of the strata discussed here. We have now clarified that these are modern latitudes in the caption for Figure 4 (Line 335). We have already emphasized the geographic breadth of the KPB facies shift throughout the manuscript and Supplementary Information.

3) Stratigraphic distribution of dinosaur and mammalian fossils. A detailed account of fossil evidence is needed for the reader to be convinced that dinosaurs were ecosystem engineers in the Cretaceous but that mammals didn't carry this function in the Paleocene. What is the evidence for the presence of dinosaurs? -- dinosaur bones, trackways, trample grounds, eggshells? How abundant are the indication, and how far below the KPB are such occurrences present and abundant? Is there any evidence that trackways and trampling were present along or within channels? Similarly, how diverse and abundant are mammalian occurrences in the studied Cretaceous and Paleocene strata?

We have now included more details about the pre- and post-KPB fossil record, which is supportive of a sudden extinction and persistence of dinosaurs up to the KPB (see Lines 273–281).

These issues aren't discussed in the main text, but evidence is provided in Lines 469-548 of the Methods, with some information about the relative position of the fossils with respect to the KPB. However, this important evidence also needs to be summarized explicitly in the main text.

We have now specified that dinosaurs are known from below the KPB and unknown from above the KPB in the main text (Lines 273–281).

4) Dinosaur extinction patterns. The sense in the text is that dinosaurs were effective landscape agents right up to their extinction at the KPB. Line 123 says "sudden extinction". However, the extinction pattern is not explored in the text. Did dinosaurs become extinct en masse at the KPB, or might their diversity and abundance have changed (decreased?) prior to the KPB? If particular clades and dinosaurs overall died out gradually, changes in fluvial style would likely be gradual, not abrupt, raising doubt about the viability of the DEE hypothesis.

There is certainly debate about whether or not dinosaurs were in decline before the KPB. Taxonomically, this could be true; however, dinosaurian megafauna fossils (especially *Triceratops*) persist right up to the KPB. Thus, large herding animals persisted until the end of the Cretaceous. We've added text to clarify this in Lines 273–281.

The literature contains a lengthy and complex discussion about dinosaur diversity and abundance late in the Cretaceous, depending on geographic areas and taxa. For example, Brusatte et al. (2017) inferred that dinosaurs were diverse up to the KPB in Brazil, whereas in China, Han et al. (2022, PNAS) and Ma et al. (2022, Geophysical Research Letters) inferred a Late Cretaceous decline in dinosaur diversity, suggesting a linkage to the long-term deleterious effects of Deccan volcanism. Sakamoto et al. (2016 PNAS) and Condamine et al. (2021, Nature Communications) recorded a decline in dinosaur speciation and diversity well before the asteroid impact. Clearly, dinosaur diversity and abundance are not simple issues, but patterns of dinosaur diversity and abundance need discussion in the main text and Supplementary Information, if the reader is to be convinced that dinosaur extinction caused an abrupt change in fluvial systems.

See response above. We've now discussed this in Lines 273–281.

5) Dinosaur effects on fluvial systems. "Dinoavulsion" is not a new concept, and DEE has earlier manifestations in the literature. Jones and Gustason (2006, Ichnos, v. 13, p. 31-41) inferred that trampling of dinosaurs through riverbanks and channel blockage by dinosaur skeletons promoted avulsion. McCarthy et al. (1998, African Journal of Ecology, v. 36, p. 44-56) noted that hippo trails in the Okovango Delta cut through riverbanks and cause avulsion. These papers could usefully be cited.

Thank you to the reviewer for pointing out these unfamiliar references! We have included a reference to Jones and Gustason (2006) on Line 264 (Ref. 40). Although McCarthy et al. (1998) is certainly pertinent to this paper, we believe our current references already substantially document this phenomenon in modern ecosystems.

6) Impact of mammals on vegetation and rivers. Mammals that survived into the early Paleocene, or evolved rapidly, might conceivably have been large and/or abundant enough to cause some of

the effects attributed to the dinosaurs. Lines 54-7 and 238-240 note the effects of large-animal trampling and uprooting on fluvial systems. However, groups of relatively small animals (for example, deer and goats) forage on trees and shrubs, assisting in keeping landscapes open. As regards the KPB, García-Girón et al. (2022, Science Advances) noted a considerable diversification of mammals prior to the KPB, with increasing trophic impact, and Longrich et al. (2016, J. Evolutionary Biology) documented a rapid recovery of mammals after the KPB (within a few hundred thousand years). Although a major mammalian landscape effect seems unlikely, the diversity and abundance of mammals around the KPB needs to be addressed in the text.

We have now added some details about the diversity dynamics of mammals before and after the KPB (see Lines 281–284). But note that mammals didn't become "big" (even deer or goat sized) until thousands to millions of years after the K-Pg mass extinction. Truly large mammals didn't evolve until the late Paleogene and early Neogene.

Additionally, for the study sites, we are not told anything about the length of time in the Paleocene over which denser forests and stable meander belts are thought to have persisted. Was it hundreds of thousands, or millions, of years? Did the environmental setting eventually become more varied, and does the duration of strata characterized by coals and large channel bodies fit with what is known about the diversification and size of Paleocene mammals? Did they, in their turn, become ecosystem engineers at a later time?

These are all great questions, and for many we do not know the answer. The scope of our study is focused on K-Pg boundary successions, so only a million years or so on either side of the boundary. Sedimentologically, the lower Fort Union Formation (e.g., Tullock and Ludlow members) remains similar for at least 1 Ma. The middle Fort Union, typified by the Lebo Member, exhibits a sedimentological change that is mappable in outcrop, and it is likely caused by the transgression of the Cannonball Sea (see our Extended Data Figure 5). Outside of the Williston Basin, the middle–late Paleocene are characterized by the maturation of the intercontinental 'Tertiary' basins familiar to us today—each of these thus experience pronounced local tectonic controls on basin evolution and fill that are challenging to universalize, at least at this time. Although fun to speculate about, we would be extending far beyond our available data to posit mechanisms driving all Paleogene facies changes in the Western Interior.

I hope that these comments will be helpful in bringing an interesting and provocative manuscript to publication.
Martin Gibling

Thank you, Martin, for these detailed and constructive notes. They were very helpful.

Reviewer #3 (Remarks to the Author):

The paper makes the interesting and novel claim that terrestrial landscapes were altered substantially immediately after the extinction of the dinosaurs. By their trampling and feeding they kept landscapes open, suppressing tree growth, but when they had gone, forests thickened and sediment was retained in higher, meandering river systems and not washed down to the flood plain.

They establish neatly that the facies shift occurred across the KPg boundary, and that modern megaherbivores like elephants maintain open habitats by trampling and feeding. They note that the facies shift is predictable and occurs everywhere across the Western Interior of North America and challenge the current idea that such a shift is coincidental with the extinction boundary, marked by the iridium anomaly, and means nothing. Most famous of the novelties is the Z-coal, and coals pop up everywhere immediately after the asteroid impact, associated with colorful mudstones. These have traditionally been interpreted as pond deposits, but this is convincingly revised here to deposits associated with meandering rivers.

The dinosaur ecosystem engineering (DEE) hypothesis is then presented, based on evidence of the body sizes of the latest Cretaceous dinosaurs, and evidence from the rock record and modern ecology. Such generalized ecosystem engineering of landscapes by dinosaurs has been suggested many times before, and so this is a well-established idea, and widely acceptable.

Linking the two to make the DEE is novel and stimulating. The authors combine thorough evidence from their observations of sedimentology of sections, which is the novel work here. They present ways in which their hypothesis could be refuted, which is gratifying and impressive.

The text is thorough and clear, and beautifully well organised. The illustrations are thorough and comprehensive. The author list includes all the expertise in sedimentology, paleosols, paleobotany, and regional geology one could wish for.

I hunted high and low and failed to find any objectionable sentences, or even typos.

Mike Benton

Thank you, Mike, for this flattering assessment of our manuscript! We are happy you found it compelling and well-written.

Responses to Reviewers

Your manuscript titled "Dinosaur extinction can explain continental facies shifts at the K-Pg boundary" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment, provided you address the remaining concerns of our reviewers and better acknowledge the study's limitations and the existence of alternative hypotheses, discuss your results in light of the literature, and clarify your definitions.

We have now addressed all of Kirk Johnson's points and the specific points highlighted by the senior editor, Alienor Lavergne. We now have specific language highlighting the study's limitations (Lines 296–298), discuss the hypothesis about volcanism that wasn't previously addressed (second to last paragraph of the 'Alternative hypotheses…' section of the Supplementary Discussion) (we already discuss many alternative hypotheses at length), and clarify our definition of facies change (Lines 77–78). Our paper discusses, at length, our results in light of the literature, so we were unsure about what specifically needed addressing there.

We are thrilled by this provisional acceptance, and we hope the editors will find these revisions in line with their expectations for publications. Below, we address the reviewers' comments directly.

Reviewer #1 (Remarks to the Author):

I have read the authors' response to the reviews and here is my feedback:
I like the hypothesis presented in this paper, but I do not think that the authors have provided enough data to reject alternate hypotheses.

We appreciate that Kirk is still skeptical, but we have at least made a case for an alternative hypothesis that, we believe, is more consistent with the available data. Nonetheless, we agree that we may have been too strong with our language in the manuscript about "refuting" alternative hypotheses to ours. As such, we have now modified the text in those two places (Line 148 and Line 202) to "casts doubt on".

The paper covers a vast area reviews several Laramide basins with many of the observations based on literature.

We do not disagree with Kirk here. This paper sprouted from our primary observations and data collection in the Williston Basin, it grew from our fieldwork in the Bighorn Basin (which first clued us into the potentially region-wide phenomenon discussed here), and it matured once our coauthor, Pim Kaskes, confirmed from his field experiences in the Raton Basin that our suspicions from the literature were legitimate. Thus, our "boots on the ground" perspective is, indeed, limited to three basins, due in large part to the early stage of all of our careers. As we will discuss again below, we hope that this paper will inspire future empirical studies, by ourselves and our colleagues, to test this hypothesis explicitly in the field, in places we have not yet been fortunate enough to study ourselves.

The field observations are broad and generalized and the diagrams used to support the hypothesis are either highlighted outcrop photographs (Fig. 1) or highly generalized cartoons (Fig 4).

We are unsure about the critique here, especially given the addition of Extended Data Figure 4 in the previous revision. Annotated outcrop photographs (as in Fig. 1), highlighted units/facies (as in EDFig. 4) are commonplace in the sedimentological and stratigraphic literature. Figure 4, as discussed in the previous response to reviewers, is intentionally broad to convey the regionally widespread pattern we highlight here. Near the bottom of this documented, we respond to explicit critiques on the figures in the "specific comments" section.

Lastly, the field observations were broad and generalized for two reasons: (i) the patterns discussed here, we contend, are indeed broad and generalized (i.e., similar patterns occur over a large geographic area); (ii) most of these sedimentological/stratigraphic patterns have already been well-documented in the primary literature, and those that weren't (the propensity of lateral accretion beds and the general absence of clastic-rich distal-floodplain sediments) we documented in the text, Fig. 1, Extended Data Fig. 4, and in our previous publications (Weaver et al., 2022 in *Palaios*, Claytor et al., 2023 in *J. Vertebrate Paleontology*). As we noted in our response to Dr. Gibling after our previous review, we by no means contend that we have thoroughly documented every detail on these meandering fluvial systems. Instead, our intention was to document a pattern, provide new data consistent with that pattern, and provide an explanation for why that pattern may exist.

All that said, we have now added additional text to make clear that testing this hypothesis thoroughly will require additional, robust studies and datasets that are not currently available (Lines 296–298).

The primary data presented are new detailed K-Pg sections and broader regional generalizations about the nature of the pre and post K-Pg stratigraphy and sedimentology. Neither data set is neither particularly large nor robust. As a result, the paper reads more like a grant proposal than a publication.

As we noted above, our intention here was to document a pattern and explore a potential explanation for that pattern—we by no means consider the job "done". As with any research, there is always more to do, but we feel this is an important first step that will hopefully catalyze additional detailed studies of K-Pg boundary sections across western North America.

In addition, other data that contradict the thesis were not adequately discussed (e.g. the many detailed K-Pg boundary sections in North Dakota where the transition to coal lies below the K-Pg boundary as defined by iridium anomalies.

We are unclear what is meant by adequately discussed, since we indeed mention these in the main text (starting Line 182) and discuss those sections at length in the Supplementary Information (see the last paragraph of pg. 14 and continuing onto the top of pg. 15). We feel that the current main text adequately captures our interpretation, and the Supplement is where we flesh out specifics for interested parties.

The core of the paper is really a discussion about the relative location of the formation contact and the K-Pg boundary at many different localities. To do this well, one needs clear definitions of both, and I don't think that the paper has done that particularly well. I recognize and applaud the effort at regional synthesis, but I think the paper itself falls short.

We begin our manuscript with a definition of the K-Pg boundary, which is the formal definition of the K-Pg boundary (Molina, E. et al. The global boundary stratotype section and point for the base of the Danian Stage [Paleocene, Paleogene]. *Episodes J. Internat. Geosci.* **29**, 263–273 [2006]). Further, we do not only focus on formational contacts—many of the K-Pg facies changes occur within named formations (e.g., beneath the 'barren zone' of the Raton Formation). Nonetheless, we recognize that although we give numerous detailed explanations of what we mean by "facies change", we could more clearly define it in the main text. We have thus added some clarifying language in Lines 77–78.

I think that a few key lines of evidence are absent. The first is the hyperabundance of volcanically-derived bentonitic clay in the Hell Creek formation and its absence in the Fort Union Formation Could not the formation change be caused by a cessation of volcanic eruptions in the hinterland of western Montana at or near the K-Pg? This mechanism might do a better job at explaining the east-west extent of the no coal to coal transition near the K-Pg boundary.

We appreciate this idea, and it is not one we considered given that there is no evidence for volcanic cessations near the K-Pg boundary. Indeed, the lowermost Fort Union is packed with ash beds (mostly preserved in coals), see Sprain et al. (2015) in *GSA Bulletin*. Nonetheless, we have now addressed why we do not think this is a likely mechanism second to last paragraph of the 'Alternative hypotheses…' section of the Supplementary Discussion.

The Cannonball Sea hypothesis is dismissed with minimal analysis.

We are confused by this comment, since we discuss this hypothesis, and why we do not feel it is a likely mechanism, at length in the main text and Supplemental Information. We are unsure what is meant by "minimal analysis".

I guess that I am fine with it being published because I think that the hypothesis should be in the literature, but I wish that the data were more, and better, and that the figures were less abstracted and more data rich.

We are grateful that Kirk is supportive of our paper being published. We too wish the data were more and better, and we hope future work will achieve that goal, hopefully motivated by the data and ideas included in this paper, which were the best available to us now.

Specific comments:

In the figures, both the formation contact, and the K-Pg should be noted. This is most egregious in Figure 1 (which needs sub-labels btw) where the Raton Basin section shows a line for the K-Pg at the base of a sandstone bed while the white tonstein that is the K-Pg boundary is visible in the outcrop on the right side of the photo below the red line.

The Raton Basin photograph traces the exact line provided by our coauthor, Pim Kaskes, where they were working on the impact claystone in the field. And the entire formation is the Raton Formation, not at a formational contact. All other boundaries occur at the formational contact. Nonetheless, we have now added labels to make this clear in the revised Fig. 1.

Also, the authors often use the terms “sandstones” and “mudstones” when they should use sandstone beds (or layers, units etc.)

We have added “beds” to any mention of sandstones, mudstones, claystones, or siltstones throughout the text.

Kirk Johnson

Thank you, Kirk, for thoughtfully reviewing our manuscript.

Reviewer #2 (Remarks to the Author):

I appreciate the substantial and thoughtful revisions to the manuscript, following the reviewers’ comments. I also appreciate the frank assessment that the individual study areas need a fuller sedimentological and geochemical assessment. The improved manuscript now provides a credible body of data to support the important hypothesis of dinosaurs as ecosystem engineers in the Western Interior landscapes. Congratulations on a provocative body of research. Martin Gibling

Thank you, Martin, for this kind assessment of our revisions. Your review was incredibly helpful and greatly improved our paper.