Response to Feedback from Associate Editor

Your manuscript has been reviewed by the three original reviewers. While they all reckon that you dealt with most of the issues that had previously been raised, there remain still some concerns that you need to address before the manuscript can be accepted for publication.

AUTHORS' RESPONSE: Thank you for considering a revised resubmission. We appreciate your constructive feedback and the opportunity to address reviewer comments.

Specifically, you need to provide (1) details on sampling storage and discuss the implication of storage time between sampling and freezing on available nitrogen concentrations;

AUTHORS' RESPONSE: We have updated the manuscript to provide additional details on sample storage time and conditions (lines 153-162). We do not have specific times that each sample was frozen, and we would be surprised if anyone had such detailed freeze-thaw dates on most of their samples as some studies do not even report how their soil samples were handled. It would be highly unlikely that we had frozen and thawed the samples in a way that created a bias in the data that would create the clear patterns with distance that we show (as the one reviewer notes). We have provided comparisons with sample storage in other studies in our response to Reviewer 3.

(2) compelling explanation on the contrast between strong nitrogen limitation based on foliar data and high nitrogen availability based on KCl extractions;

AUTHORS' RESPONSE: We have provided a thorough response to Reviewer 2's concern about the high soil nitrate values relative to foliar N, including demonstrating papers showing similar results using KCl extractions. One such study (Seymour et al. 2014) used KCl extractions to demonstrate that termite mounds, a well-studied hotspot of nutrient heterogeneity on southern African savanna landscapes, have soil nitrate concentrations of up to 974 mg/kg, a concentration that is comparable with our study. That same study found only weakly elevated foliar N on termite mounds relative to control sites. The uptake of nutrients by plants is controlled by many different mechanisms, which are beyond the scope of our study. While one could expect higher levels of N in the grasses given the concentration of nutrients in the soil, the response we show is not uncommon. But given that our paper is not about the physiological mechanisms with which plants take up nutrients, we do not discuss this discrepancy in detail. We have updated the manuscript discussion to include comparison between our results and this study (lines 453-458).

(3) evidence for cation limitation of plant growth in natural communities on African savannas;

AUTHORS' RESPONSE: We have rephrased that portion of the introduction (lines 91-94) to focus on the importance of micronutrients more generally (both cations and anions), as this is what we actually measured in the study. We also updated the citations to ones that experimentally demonstrated increased plant productivity with the addition of micronutrients (sodium and potassium). However, we limited this section to just one sentence in the introduction, as we do not actually measure or report plant productivity in the study, and we found that attempts to expand this section were distracting from the main focus of the manuscript. Micronutrient limitation clearly exists in plants (Epron et al. 2012; Chen et al. 2024), and we show that elephant carcasses are significant sources of micronutrients. We simply draw this parallel to suggest the potential importance of these carcasses. Any further discussion would be conjecture on our part, and we chose not to do that.

Epron, D., Laclau, J-P., Almeida, J. C. R., Gonçalves, J. L. M., Ponton, S., Sette, Jr., C. R., Delgado-Rojas, J. S., Bouillet, J-P. & Nouvellon, Y. Do changes in carbon allocation account for the growth response to potassium and sodium applications in tropical Eucalyptus plantations? *Tree Physiology*, 32, 667-679, https://doi.org/10.1093/treephys/tpr107, 2012.

Chen, B., Fang, J., Piao, S., Ciais, P., Black, T. A., Wang, F., Niu, S., Zeng, Z. & Luo, Y. A meta-analysis highlights globally widespread potassium limitation in terrestrial ecosystems. *New Phytologist,* 241, 154-165, https://doi.org/10.1111/nph.19294, 2024.

(4) detailed description on how geochemical data were obtained and convert all soil chemistry values to mg/kg (soil dry weight) to guarantee comparability of your results with other studies.

AUTHORS' RESPONSE: As noted above, we updated the methods to include further methodological details, including sample storage information. We also provided a detailed explanation for Reviewer 2 on soil conversions, providing the stoichiometry for our calculations and a Sigma-Aldrich protocol confirming them. All of our soil chemistry values are in mg/kg.

Finally, revise your Results and Discussion based on the data obtained after conversion soil chemistry values.

AUTHORS' RESPONSE: We have updated our discussion based on reviewer feedback, particularly the paragraphs discussing the impacts of elephant carcasses on soil ammonium, nitrate, and phosphate concentrations. We also expanded the discussion to include better comparisons with the literature on smaller carrion (lines 446-467), as recommended by Reviewer 3. We believe that these changes greatly strengthen the quality of the discussion, and we appreciate the feedback.

Response to Feedback from Reviewer 1

The authors have completed a comprehensive revision which includes new and revised analyses, greater details in the methods, and improvements to the narrative. The ms is now greatly improved - well done!

AUTHORS' RESPONSE: Thank you!

Response to Feedback from Reviewer 2

General comments/major concerns:
The authors addressed some of the concerns raised in the initial review, but there are still areas that need some significant clarification and correction related to the conversion of concentrations measured in soil chemistry with the conversion to mg/kg (dry weight?). The authors describe (line 175) that water soluble nitrate and phosphate were extracted using 100 ml soil and 200 ml deionized water. I am unfamiliar with measuring soil in ml rather than mass, and I am concerned that this is not a standardized approach and does not provide a meaningful measure of those ions. This extends to the ammonium extraction—if this was also conducted as a 1:2 ratio (100 ml soil to 200 ml water), that is also a non-standardized approach. Is this a typo and this should be 100 mg of soil? This confusion extends further to the calculations for converting mg/L, which as described, that math would end up with the units of mL/mL of soil, which is not a unit I've seen before with soil chemistry. The concentrations should be based on a gram dry weight basis, not volume of soil. There should be some correction for dry mass of the soil.

AUTHORS' RESPONSE: This is a typo; thanks for catching it! We have fixed this line to read "100 mg soil and 200 mL deionized water". The units for soil ions (ammonium, nitrate, and phosphate) were mg/L, which we then converted to mg/kg by multiplying by 2 based on the 1:2 soil:water extraction ratio. The stoichiometry behind this conversion is as follows:

mg/L (1 L / 1000 ml) (200 ml / 100 mg) (1000 mg / 1 kg) = mg/L x 2

Multiplying by 2 is also the conversion recommended by Sigma-Aldrich: https://www.sigmaaldrich.com/US/en/technical-documents/protocol/environmental-testing-and-industrial-hygiene/soil-solid-waste-and-groundwater-testing/ammonium-in-soils#calculation

Further, despite describing this conversion process to present ammonium concentrations in soil in a standardized way (mg/some gram dry weight basis), the authors later (lines 370-372) describe ammonium in soil as mg/L. The figures of ammonium show data as mg/kg. The values presented (assuming there was a typo and the data are actually mg/kg gram dry weight basis) are also significantly smaller than other studies with smaller carcasses (e.g., Quagiotto et al., 2019), which is the opposite of the discussed results and conclusions stated. Here, the maximum concentration of ammonium reported (based on your figure 2C) is ~150 mg/kg. The max concentration of ammonium in Quagiotto et al. (2019) is ~800 mg/kg. The opposite is true of nitrate, which your results suggest is 500-1500 mg/kg whereas in Quagiotto et al. it is ~2 mg/kg. This is exceptionally high nitrate, even compared to some of the prior data generated from Kruger and included in the author's responses to reviewer #3. In sum, it is really difficult to figure out how the data were generated and then handled to get to the results presented for the soil geochemistry.

There is something strange going on with the soil nitrogen data (excluding the %N and nitrogen isotopes) that does not make sense from the perspective of what should be expected (high ammonium, low nitrate), what is typical of ecosystems (mentioned by reviewer #3 in the first review), and compared to other studies at Kruger.

AUTHORS' RESPONSE: In lines 368-372, we gave ammonium concentrations in mg/L to allow easy comparison with Britto & Kronzucker (2002) (cited in line 369), showing how soil ammonium levels compare with those typically considered toxic to plants. We understand that this was confusing and have updated the paragraph as follows to present all units in mg/kg:

"The mean ammonium level at the center of carcass sites (34.8 mg/kg) was higher than the level generally considered toxic to plants (Britto & Kronzucker, 2002). Yet, we found living grass—typically *U. trichopus*—in the center of the carcass site at seven out of ten of our sites (ammonium range 10-172 mg/kg) and at the 2.5m distance for all sites (ammonium range 0-72 mg/kg). The three sites without vegetation in the center had the highest ammonium levels (70-144 mg/kg), suggesting that *U. trichopus* has a higher degree of ammonium tolerance than some sympatric grass species but may still be limited by the high ammonium levels at the centers of these three relatively fresh carcass sites."

We believe that the difference between our results and the results in Quaggiato et al. (2019) is a matter of time scale. The Quaggiato paper measured soil ammonium and nitrate under rabbit carcasses for 20 days postmortem, finding that soil ammonium at carcass sites was 20x higher than controls at day 4, while nitrate did not significantly differ between carcass and control sites across the 20-day period. However, studies that continue measurements for longer time periods consistently exhibit the same trend that we did – soil ammonium concentrations peak earlier in the decay period than nitrate, with a transition from high soil ammonium to high soil nitrate over time (Yong et al. 2019; Keenan et al. 2018). Moreover, elephant body size may be coming into play with these results. Parmenter and MacMahon (2009) compared the impacts of carrion body size on soil nutrient concentrations and found that soil nitrogen peaks occurred earlier for smaller carrion (e.g. deer mice, chipmunks) relative to larger carrion (e.g., dog, mule deer) (Parmenter &

MacMahon, 2009, Table 5). Thus, we would expect that peak soil nitrogen concentrations at elephant carcass sites would take longer than the rabbits in Quaggiato et al. (2019).

As shown in previous responses to reviewers, the nitrate concentrations at our control 15m distances are consistent with those found in the Kruger literature. The contribution that our research makes to the literature is demonstrating the impacts of elephant carcasses on soil nitrate, something that has previously not been tested and therefore cannot be directly compared with other studies. However, we are not the first researchers to record these high nitrate concentrations in southern African savanna soils. Soil nitrate concentrations of up to 974 mg/kg have been recorded on termite mounds (using the KCl extraction method), another major nutrient hotspot on savanna landscapes (Seymour et al. 2014).

Finally, with regards to ammonium, our freshest elephant carcass was 24 days postmortem, and it had the highest soil ammonium concentration. However, the mean site age was older – 350 days postmortem (range 24-811 days). Because of this age distribution, our data may be capturing the high soil nitrate concentrations at later stages of decomposition after nitrification has occurred but may not be capturing the full impact of ammonium in those early stages. Indeed, this trend of conversion from ammonium to nitrate is visible in Figure 5, where we see that soil ammonium (Fig. 5A) and respiration potential (Fig. 5C) both spike early then decrease rapidly, while soil nitrate (Fig. 5D) does not follow that pattern.

Seymour, C. L., Milewski, A. V., Mills, A. J., Joseph, G. S., Cumming, G. S., Cumming, D. H. M. & Mahlangu, Z. Do the large termite mounds of *Macrotermes* concentrate micronutrients in addition to macronutrients in nutrient-poor African savannas? *Soil Biology and Biogeochemistry*, 68, 95-105, https://doi.org/10.1016/j.soilbio.2013.09.022, 2014.

Additionally, the phosphate seems really low, which is also maybe a conversion issue?

AUTHORS' RESPONSE: Our maximum soil phosphate concentration was 58 mg/kg, which is indeed lower than Quaggiatto et al. (2019), which recorded a max of ~275 mg/kg soil phosphate at rabbit carcass sites. However, our results are consistent with Parmenter & MacMahon (2009), which found that mule deer carcasses increase soil P by only 13g after 2 years and 41g after 5 years postmortem. Our phosphorus results are also greater than Mlambo et al. 2007, which found that intercanopy P in southern African savanna soils was only 8-16 mg/kg. These differing results across studies highlight that the impacts of carcasses on soil nutrient dynamics are complex and differ with factors such as species, scavenger densities, soil type, precipitation, etc. Indeed, one reason why phosphate levels at carcass sites are lower than expected may be due to bone dispersal by scavengers, which we address in lines 414-426:

"The elevated plant-available P at the center of carcass sites likely came primarily from phosphate released from decomposing tissue (Yong et al. 2019). Bone decomposition, which is also likely a major source of P from animal carcasses (Subalusky et al. 2020), occurs over long time scales (Coe, 1978; Subalusky et al. 2020) and therefore should result in the slow release of P and a gradual decrease in the N:P ratio (Parmenter & MacMahon, 2009; Quaggiotto et al. 2019). Indeed, initial inorganic N influxes to the Mara River in Kenya from mass wildebeest die-offs are 10-fold greater than concurrent increases in P, which instead releases slowly over about seven years of bone decomposition (Subalusky et al. 2017). Research following megacarcasses over longer timeframes postmortem is needed to clarify when P from enriched soil is absorbed by plants and at what stage megacarcass bones begin contributing to soil P dynamics. It is also possible that bone dispersal by scavengers may result in less P leaching from bones close to where the elephant died and more P being distributed across the landscape at distances far from the carcass site."

The concerns related to the soil geochemical data are significant and have impacts on the discussion and conclusions presented. Until these data issues are resolved, aspects of the discussion are not supported by the current results.

AUTHORS' RESPONSE: We have updated and expanded the discussion in response to specific comments below, particularly the sections on soil nitrate and ammonium.

Additionally, one suggested change is to replace the words "post-death" with either "post-mortem" or "postmortem." While "post-death" is not technically incorrect, the convention and terminology used more broadly is "postmortem."

AUTHORS' RESPONSE: We have changed all instances of the word "post-death" to "postmortem".

Specific comments:

- Lines 172-174: It may be better to cite an EPA or USGS protocol rather than these papers.

AUTHORS' RESPONSE: We have added citations for specific protocols for P Bray-1 and the 1:2 soil:water extraction analyses. We have kept the Croghan & Egeghy (2003) citation because it refers to the use of half the detection value for measurements under the detection limit.

- Line 280: I think it's great to evaluate change in soil chemistry as a function of carcass time since death, but could the authors explain why an exponential decay would be expected?

AUTHORS' RESPONSE: Thanks! We chose to use the exponential decay function because it models a non-constant rate of decrease that asymptotes without reaching zero, which is what we could expect for soil nutrients.

- Line 182: The authors state, “To determine whether soil anions were distinct…” but then list cations. At the end of this same sentence, you don’t measure cations or anions “using microwave assisted digestion.”

AUTHORS’ RESPONSE: We have edited this sentence as follows for clarity (lines 183-185): “To determine whether soil micronutrients were distinct and elevated at the center of carcass sites relative to soil further from the center, we measured concentrations of sodium (Na), magnesium (Mg), iron (Fe), calcium (Ca), potassium (K), and phosphorus (P).”

- Line 331: Can you clarify what is meant by the term “variance.” Is this change in mean?

AUTHORS’ RESPONSE: We mean variance in the standard statistical definition – a measure indicating the average squared deviation of each point from the mean. A higher variance means the points are spread out further from the mean, while a lower variance means they are clustered closer to the mean.

- Line 366: The added text, “but much greater in magnitude in these much larger carcasses” is not supported by the data presented

AUTHORS’ RESPONSE: We have updated this discussion paragraph as follows to more fully explore the relationships amongst carcass age, ammonium inputs, and plant responses (lines 366-381):

“The initial influx of ammonium from elephant carcasses may have time-dependent impacts on plant abundance at elephant carcass sites. The mean ammonium level at the center of carcass sites (34.8 mg/kg) was higher than the level generally considered toxic to plants (Britto & Kronzucker, 2002). Yet, we found living grass—typically *U. trichopus*—in the center of the carcass site at seven out of ten of our sites (ammonium range 10-172 mg/kg) and at the 2.5m distance for all sites (ammonium range 0-72 mg/kg). The three sites without vegetation in the center had the highest ammonium levels (70-144 mg/kg), suggesting that *U. trichopus* has a higher degree of ammonium tolerance than some sympatric grass species but may still be limited by the high ammonium levels at the centers of these three relatively fresh carcass sites. However, the recentness of the disturbance from the carcass likely also plays a role in determining plant abundance near the center of the carcass. Because of the elephant carcass site age distribution, (mean 350 days postmortem; range 24-811 days), this study may not have captured the full impact of ammonium release from carcasses during the early stages of decomposition. Soil ammonium spiked early and decreased rapidly (Figure 5A), and future research on carcasses within the first few weeks postmortem would enhance our understanding of these early nutrient dynamics.”

- Lines 367-372: As discussed above, why is mg/L being used here. This part of the discussion is not supported by the results presented.

AUTHORS' RESPONSE: As described above, in lines 368-372, we gave ammonium concentrations in mg/L to allow easy comparison with Britto & Kronzucker (2002) (cited in line 369), showing how soil ammonium levels compare with those typically considered toxic to plants. We see now that this was unclear and have updated the paragraph to show all units in mg/kg:

"The mean ammonium level at the center of carcass sites (34.8 mg/kg) was higher than the level generally considered toxic to plants (Britto & Kronzucker, 2002). Yet, we found living grass—typically *U. trichopus*—in the center of the carcass site at seven out of ten of our sites (ammonium range 10-172 mg/kg) and at the 2.5m distance for all sites (ammonium range 0-72 mg/kg). The three sites without vegetation in the center had the highest ammonium levels (70-144 mg/kg), suggesting that *U. trichopus* has a higher degree of ammonium tolerance than some sympatric grass species but may still be limited by the high ammonium levels at the centers of these three relatively fresh carcass sites."

- Line 379: The way this sentence reads suggests that nitrate is sourced from heterotrophic microbes. Consider revising this sentence to clarify that the carbon story relates to heterotrophic microbes, but the nitrate does not necessarily inform heterotrophic microbial activity.

AUTHORS' RESPONSE: We have revised this sentence as follows (lines 382-384): "Soil nitrate (Figure 2B) and soil respiration potential (Figure 3) were also elevated near the center of carcass sites, indicating higher activity rates for nitrifying bacteria and heterotrophic microbes (Prosser, 2011)."

- Line 386-387: The units here for respiration rates are not presented on a gram dry weight basis—is this just a typo?

AUTHORS' RESPONSE: We have updated the units to "µg CO_2 per hour per gram of dry soil".

- Lines 442-444: Because of some of the confusing regarding the data as presented, this sentence is not supported clearly by the data. The discussion should have some more specific comparisons to smaller carcasses and their impacts on ecosystems to support this sentence (and whole paragraph).

AUTHORS' RESPONSE: We have updated this paragraph to include comparisons with smaller carrion and other nutrient sources (e.g., termite mounds) (lines 446-467):

“The magnitude of nitrogen inputs from megacarcasses, as well as the substantial size and duration of their impact zones, means their impacts on ecosystem processes may be functionally distinct from smaller carrion. Soil nitrate concentrations at elephant carcass sites are orders of magnitude higher than at carcass sites of smaller carrion (e.g., rabbits, white-tailed deer, kangaroo) (Quaggiato et al. 2019; Bump et al. 2009; Barton et al. 2016). Even for large ungulates such as moose, total soil inorganic nitrogen (ammonium + nitrate) at carcass sites is a mean 300 mg/kg (Bump, Peterson, & Vucetich, 2009), substantially lower than the mean total soil inorganic nitrogen at elephant carcass sites (2.5m distance; 473 mg/kg). Termite mounds, another long-lasting source of savanna nutrient heterogeneity, have mean soil nitrate concentrations (197 mg/kg) lower than elephant carcass sites, but maximum nitrate concentrations that are on par with them (974 mg/kg) (Seymour et al. 2014), again indicating that elephant carcasses are one of the strongest known individual contributors of soil nitrogen in African savanna ecosystems, which may have important implications for savanna ecology. Indeed, there is evidence that carcass size strongly impacts scavenger food web structure (Moleón et al. 2015; Morris et al. 2023). Moreover, the attraction of animals to carcasses via scavenging, predation, or mourning (Goldenberg & Wittemyer, 2020) could have positive feedbacks on nutrient cycling (Bump, Peterson, & Vucetich, 2009; Monk et al. 2024), which may be magnified by carcass size. Thus, the impacts of megacarcasses on savanna ecosystem processes may be dissimilar to the effects of small carrion and more similar to other more persistent contributors to savanna ecosystem processes, such as termite mounds (Davies et al. 2016), cattle bomas (Augustine, 2003), and even mass animal mortality events (Subalusky et al. 2017, 2020).”

- Line 457: I would be careful with listing “nitrate” here as being sourced from the carcasses. Ammonium is sourced from the carcasses, but nitrate forms as microbes transform the ammonium into nitrate through nitrification. The increase in nitrate only occurs later in decomposition once the system returns to being oxygenated (not examined here). The carcass itself is not releasing/introducing nitrate as the nitrogen pool. I would delete the reference to nitrate here.

AUTHORS’ RESPONSE: We have changed the phrase “pulses of ammonium, nitrate, and phosphate” to “pulses of nitrogen and phosphate”.

- Line 462: While I agree that this type of work could help us understand the impacts of megaherbivore declines on ecosystems, I would remove the term “Anthropocene.” This is not an officially recognized term (it was rejected this year by the geological community). I would just replace that word with “at present” or “in modern ecosystems” or something similar.

AUTHORS' RESPONSE: We have removed the word Anthropocene and rephrased this as "in modern ecosystems".

- In all figures, use appropriate superscript or subscript on axis labels

AUTHORS' RESPONSE: We have corrected figures so that all axis labels show appropriate superscripts and/or subscripts.

Response to Feedback from Reviewer 3

The authors provide careful revisions to my comments, which are very much appreciated. I have a couple of remaining comments.

AUTHORS' RESPONSE: Thanks for your constructive feedback.

On the available N, the authors make a convincing case that freezing didn't affect the results greatly. However, this leaves the potential influence of storage time between sampling and freezing, which can lead to considerable changes in nitrate and ammonium concentrations. How long between sampling soil and freezing it? This is not stated in the revised description. Comparison with literature values is only helpful if those data were collected with minimal storage time, but I expect that almost all studies mentioned involve some storage time before extraction, freezing, etc. Often samples are stored for many days (fridge etc) before extraction, leading to 100+ pp concentrations of available N. This is a well-known artifact that is still not widely recognized in ecological studies. Studies that extract immediately - e.g. using in-field KCl extraction or rapidly upon return to the laboratory - tend to find very low extractable N concentrations. Anyway, as the authors mention, the samples were all treated in the same way, so the patterns with distance are presumably realistic. However, the stark contrast between the apparent strong N limitation based on foliar data and extremely high N availability based on KCl extractions is a conundrum. If someone can do in-field KCl extractions sometime at Kruger (especially seasonally?) it would be very helpful in interpreting the nutrient situation there. If such studies have already been done, it would be useful to compare them to the results presented here.

AUTHORS' RESPONSE: We stored our soil samples in commercial freezers (-20°C) the same day they were collected. Soil freezing times varied depending on the date the soils were collected, with the earliest samples frozen for ~1 month longer than the latest samples. We have updated the methods as follows (lines 156-158): "The rest of each sample was placed in a plastic bag on the day of collection and stored in a -20°C freezer for up to 1 month; samples were stored in coolers with ice blocks during the transition from the freezer at the field site to the lab for analysis."

Unfortunately, many savanna nutrient studies do not provide sample storage times or conditions. We performed a search on Google Scholar for peer-reviewed manuscripts from 2000 onward using the search time "soil nitrate African savanna". Of the first 20 papers listed, 8 performed lab analyses measuring soil nitrate on untreated African savanna soils. Five of the eight studies used KCl extractions, while the other three used soil:water extractions. Three of the eight relevant manuscripts stored samples in the freezer at -20°C, two stored them in refrigerators, and two did not state how samples were stored. None of the studies provided information on sample storage time. We agree with the reviewer that this systemic problem in reporting methods makes cross-study comparisons difficult, and we appreciate the suggestion that we add our own sample storage times to the manuscript, which we have done.

Seymour et al. (2014) measured soil nitrate concentrations in Zimbabwe using KCl extractions and found that mean soil nitrate in control sites was 3.72 mg/kg (range 2-7 mg/kg) but was substantially higher (mean 197 mg/kg, range 3-974 mg/kg) in soil taken from termite mounds. However, they found only a weak difference in foliar nitrogen concentrations on vs. off termite mounds. These results are consistent with what we found – extremely high nitrate concentrations at the center of a hypothesized nutrient hotspot, with a much smaller impact on foliar nitrate.

Seymour, C. L., Milewski, A. V., Mills, A. J., Joseph, G. S., Cumming, G. S., Cumming, D. H. M. & Mahlangu, Z. Do the large termite mounds of *Macrotermes* concentrate micronutrients in addition to macronutrients in nutrient-poor African savannas? Soil Biology and Biogeochemistry, 68, 95-105, https://doi.org/10.1016/j.soilbio.2013.09.022, 2014.

In response to my question about evidence for Ca limitation in savannas the authors state that evidence does exist. They provide citations and they have amended their manuscript to include this. However, one citation is a review from 1986 that provides no evidence for cation limitation in natural plant communities anywhere (in fact, they state that "There is virtually no direct evidence that native terrestrial ecosystems are nutrient limited"), another citation is a classic paper on uplift of nutrients by plants but does not provide direct evidence of cation limitation. A third citation concludes that productivity in woodland savanna is co-limited by N and P. Another review deals with N and P and also does not provide any evidence of cation limitation. The final paper deals with potassium dynamics in cropping systems where K is an issue because N and P are supplied as fertilizer. So, I ask again - is there any evidence for cation limitation of plant growth in natural communities on African savannas? Appropriate evidence would be an increase in productivity following addition of calcium or potassium to a savanna plant community.

AUTHORS' RESPONSE: This manuscript tests the impacts of elephant carcasses on soil and plant nutrient concentrations, with potential implications for herbivore foraging activity. Thus, whether or not African savanna plants are cation limited is outside the scope of what we are testing. We have updated this section of the introduction to better reflect the scope and focus of our manuscript as follows (lines 91-96):

"Aboveground, plant growth in African savannas is limited by nutrient availability, most commonly N and P (Ries & Shugart, 2008; Pellegrini, 2016), and micronutrients such as sodium (Na) potassium (K) may co-limit plant productivity as well (Epron et al. 2012; Chen et al. 2024). Thus, the large influx of nutrients released from megacarcasses might increase the mobilization of nutrients by plants, potentially increasing nutrient accessibility for vertebrate and invertebrate herbivores (Yang, 2008; Grant & Scholes, 2006; Anderson et al. 2010; Joern et al. 2012)."

Throughout the manuscript, we have gone back to our original use of the word "micronutrient" to refer to soil cations and anions, as we include both in our analyses.

Please note that orders in Soil Taxonomy are capitalized (Alfisols, Andisols, etc).

AUTHORS' RESPONSE: Thanks for catching this. We have capitalized Inceptisols, Versitols, and Andisols (line 124).