

Review of Gillespie et al. Research at the interface between Indigenous knowledge and soil science; weaving knowledges to understand horticultural land use in Aotearoa New Zealand (<https://doi.org/10.5194/egusphere-2024-3546> Preprint).

(Note “L” refers to line number in the manuscript)

This manuscript (ms.) presents a transdisciplinary study of a traditional agricultural system in New Zealand, combining Māori indigenous knowledge with soil science. The ms. is well-written and organized, and contains important, interesting findings and views that are well within the scope of SOIL, and will help advance the interface between indigenous knowledge (IK) and ethnopedology. The present study can be published after some comments and questions relating to IK and traditional agriculture, and technical soils components, are addressed.

However, reflecting the authors’ recognition of the need for more empirical studies to better quantitatively and scientifically support their transdisciplinary approach (TDR), I strongly encourage the authors to consider a more substantial scientific soil study going forward in their future continuation of this research. This would involve more robust sampling and data collection and analyses of traditional fields and soils, and comparison of the agricultural soils with reference (control) natural soils, and perhaps comparisons of a range of Māori field systems. A more quantitative (increased sample size and use of controls) would help to more rigorously test initial findings and inferences that the authors present in their current stage of research. This would not only help support Māori agriculture and food sovereignty development, but would also help advance soil health and food security internationally.

Main Comments and Questions

These comments mainly fall into two areas: 1) the authors’ TDR approach regarding Māori traditional knowledge and goals regarding agriculture, and 2) technical aspects relating to the data collection, analysis, and interpretation of the agricultural fields, soils, phytoliths, charcoal/ash, and stable C and N isotopes.

1. The authors’ TDR and collaborative research framework demonstrates how far ethnopedology studies have advanced from the early days: from recognizing the importance of indigenous knowledge in its own right, to now much more active engagement with indigenous communities in planning and conducting the research to incorporate those communities’ desired outcomes. In this case, those outcomes involve traditional agriculture in relation to food sovereignty and food security. Another component the authors could address going forward is how to encourage community members, especially younger people, to obtain more education and training, and scientific expertise, so that they can more actively conduct, co-lead, and take the lead in the research, while also incorporating and conserving their heritage of traditional agricultural knowledge, language, and culture. Research areas for community members could be soil science, plant and crop sciences (e.g., phytolith and stable isotope analyses such as done in this study, and genetics), agricultural management, among others.

In terms of writing, the use of TDR language and the conceptual framework regarding the importance of indigenous knowledge and active participation and collaboration with indigenous communities is appropriate and understandable. The He Awa Whiria/two streams framework is useful, and Figure 11 is very good. However, the TDR wording can become excessive for scientists or the public unfamiliar with this approach, so please also keep in mind the need to

communicate clearly and concisely with simpler and non-repetitive language, depending on the audience. Also, generalizing about Western science as narrow (e.g., L593-4, 626) to promote TDR, can come across as unnecessary stereotyping.

2. The multiple kinds of data (soils, phytoliths, stable C and N isotopes, charcoal and ash) explored in this study are excellent, but an expanded sample design is essential as the studies hopefully go forward. The current data and findings are likely sufficient for this paper, but I encourage the authors to point to the future of this study, especially the need for an expanded and more robust sampling design, data collection, and subsequent analyses to test their current finding and inferences. Currently, it appears that only one specific field of earthen raised mound fields was sampled, whereas more sampling would allow for statistically valid comparisons to test current data hypotheses and inferences. With one field studied so far with two soil pits, more work is needed to test if current findings are representative. I realize that perhaps only one field was identified in the immediate study area, but maybe more could be found, or the soil study expanded to other areas where traditional fields have been identified, as indicated in L346-7 etc.

This research would also greatly benefit from incorporation of reference or control soil sampling; i.e., uncultivated/natural soils with similar comparable geomorphic and pedogenic settings, if these are available. This would help to test the anthropogenic soil change and phytolith and isotope signatures inferred by the authors. The lack of reference soils outside of the field systems for comparison raises questions about the validity of some of the current interpretations, and renders them more speculative. Also, scientific-based comparisons among raised mound fields of different age or settings, or different kinds of Māori traditional field systems would add information that would benefit the scientific scope and interpretations overall. A more quantitative use of control natural soils, and expanded sample design for fields and their soils, would support the need for more empirical studies rightly called for by the authors.

While the current findings are interesting and compelling, they also should be considered more preliminary in my view because of the minimal sampling and lack of control natural areas for comparison. The phytolith data about sweet potato (*kūmara*) is convincing, but it seems like some of the conclusions are less certain than currently conveyed, especially those regarding the soils and their management (e.g., L529-30, 538-9, 553-4, 658-9). I think some of the conclusions should be less bold and more toned down, and future work needed to test initial findings based on few samples should be acknowledged more.

Other specific comments and questions

The incorporation of Māori terms is important, and most of these terms are reasonably defined when first stated. However, I suggest that the authors add a table of Māori terms (at least the key ones used more than once) to refer to easily – it may be challenging for many readers to remember the word meanings when going through the whole paper, so being able to access the terms in one place (i.e., table or mini-glossary) would be helpful.

State more explicitly in the Methodology section (e.g., first paragraph of Section 3.2 starting with L275, and maybe around L306 and beginning of Findings L314) that you were sampling an inferred traditional earthen raised mound field system. A little more than just stating features as “mounds” or “earth rows” would be clearer to readers.

Questions about Tables 1 and 2, and Figure 5, and need to give more information:

Soil morphology and horizon designations (some symbols and terms in the New Zealand system may not be familiar to all readers): color (all moist colors? What are the 2nd colors given in parentheses?); texture (explain the abbreviations, especially “Z”); structure (is polyhedral same as granular or ?); Size (state in column label or caption that this is coarse fragment size; does % abundance mean volume %?); horizon designations in Tables and Fig. 5: does A/B mean same as AB or discrete A and B parts within the horizon?); meaning of (f) and (g) in parentheses for Bw? In Tables, add a column with the specific depth intervals for each horizon. For Figure 5, state the scale units (e.g., numbers are 10 cm intervals). Also, I am not seeing the arrows for “beach gravel additions” stated in the Figure 5 caption. You indicate an “Ap2” in the Figure 5b caption, but that’s not shown in the photo – did you mean “2Ap”?

Regarding the landslide: is this a natural landslide or is there possible anthropogenic influence from the agriculture – e.g., could the field construction and use have induced the landslide? Is this landslide an isolated case, or are these landslides common. Need more context here, and this also shows the need to sample more fields.

Good multiple analyses in soil chemistry (especially Mn in Figure 9) and C and N isotopes in relation to possible soil amendment inputs for nutrient management such as penguin guano and seaweed. Again, all of these analyses (soil chemistry, stable isotopes, charcoal/ash, gravels) and interpretations regarding soil modification would benefit from comparison with some kind of baseline data from control (nonagricultural soils that match the agricultural soils in natural pedogenesis and ecological and geomorphic setting), if they are available. With Mn for example, you indicate increases in inferred modified horizons but just for Pit 2 (what about Pit 1?). Incorporating more fields for soils analyses, and comparison with surface horizons etc. in natural soils, could help better characterize Mn distribution, variability, and test whether Mn is diagnostic of amendment inputs. The greywacke gravel input inference in relation to IK seems valid, but greater sample size and comparison with similar natural horizons in control soils could allow you to be more definitive and certain that this gravel could only be from deliberate input for management (e.g., are you certain that the geologic occurrence and distribution of greywacke isn’t more complex?). Monitoring natural control soils along with the agricultural soils could also allow you to test and quantify drainage and soil warming benefits of gravel inputs.

L404 – explain a bit more about manure. Are you saying that use of manure is totally prohibited by Māori?

L621: define FLN in this ms. (Food-Landscape Network).

Why isn’t “ethnopedology” mentioned in the text (only indirectly in one reference). Topics covered in this ms. seem closely related to the subdiscipline of ethnopedology, and seems like it should be mentioned if not highlighted.

References: good citation of literature on traditional Māori agriculture. It might also be useful to also consider citing some literature on traditional agricultural systems and soils research elsewhere in Polynesia and Oceania that may be relevant to your study and findings. Examples:

Lincoln, N. et al. (2014). Indicators of soil fertility and opportunities for precontact agriculture in Kona, Hawai'i. *Ecosphere*, 5(4), art42. doi:10.1890/es13-00328.1

Ladefoged, T. N. et al. (2018). Soil nutrients and pre-European contact agriculture in the leeward Kohala field system, Island of Hawai'i. *Archaeology in Oceania*, 53(1), 28-40. doi:10.1002/arco.5138

Sherwood, S. C. et al. (2019). New excavations in Easter Island's statue quarry: Soil fertility, site formation and chronology. *Journal of Archaeological Science*, 111, 104994. doi: <https://doi.org/10.1016/j.jas.2019.104994>

Autufuga, D. et al. (2023). Distribution of soil nutrients and ancient agriculture on young volcanic soils of Ta'ū, American Samoa. *Soil Systems*, 7(2), 52. doi:10.3390/soilsystems7020052

Also consider other stable isotope research on possible guano use in ancient agriculture:

Santana-Sagredo, F. et al. (2021). White Gold in the Atacama Desert: Isotopic evidence for early seabird guano use in South America. *Nature Plants*, 7, 152-158. <https://doi.org/10.1038/s41477-020-00835-4>

Szpak, P. et al. (2012). Stable Isotope Biogeochemistry of Seabird Guano Fertilization: Results from Growth Chamber Studies with Maize (*Zea Mays*). *PLOS ONE*, 7(3). doi:10.1371/journal.pone.0033741