Peer Community In Ecology

Are Moas ancient Lazarus species?

Werner Ulrich *based on peer reviews by* ***Richard Holdaway*** *and* ***Tim Coulson***

Floe Foxon (2023) The Moa the Merrier: Resolving When the Dinornithiformes Went Extinct. bioRxiv, ver. 2, peer-reviewed and recommended by Peer Community in Ecology.
https://doi.org/10.1101/2023.08.07.552261

Submitted: 09 August 2023, Recommended: 23 October 2023

Cite this recommendation as:
Ulrich, W. (2023) Are Moas ancient Lazarus species?. *Peer Community in Ecology*, 100582. 10.24072/pci.ecology.100582

Published: 23 October 2023

Ancient human colonisation often had catastrophic consequences for native fauna. The North American Megafauna went extinct shortly after humans entered the scene and Madagascar suffered twice, before 1500 CE and around 1700 CE after the Malayan and European colonisation. Maoris colonised New Zealand by about 1300 and a century later the giant Moa birds (Dinornithiformes) sharply declined. But did they went extinct or are they an ancient example of Lazarus species, species thought to be extinct but still alive? Scattered anecdotes of late sightings of living Moas even up to the 20th century seem to suggest the latter. The quest for later survival has also a criminal aspect. Who did it, the Maoris or the white colonisers in the late 18th century?

The present work by Floe Foxon (2023) tries to settle this question. It uses a survival modelling approach and an assessment of the reliability of nearly 100 alleged sightings. The model favours the so-called overkill hypothesis, that Moas probably went extinct in the 15th century shortly after Maori colonisation. A small but still remarkable probability remained for survival up to 1770. Later sightings turned out to be highly unreliable.

The paper is important as it does not rely on subjective discussions of late sightings but on a probabilistic modelling approach with sensitivity testing prior applied to marsupials. As common in probabilistic approaches, the study does not finally settle the case. A probability of as much as 20% remained for late survival after 1450 CE. This is not improbable as New Zealand was sufficiently unexplored in those days to harbour a few refuges for late survivors. However, in this respect, it is a bit unfortunate that at the end of the discussion, the paper cites Heuvelmans, the founder of cryptozoology, and it mentions the ivory-billed woodpecker, which has recently been redetected. No Moa remains were found after 1450.

References:

Foxon F (2023) The Moa the Merrier: Resolving When the Dinornithiformes Went Extinct. bioRxiv, 2023.08.07.552261, ver. 2 peer-reviewed and recommended by Peer Community in Ecology. https://doi.org/10.1101/2023.08.07.552261

Reviews

Evaluation round #2

Reviewed by Tim Coulson, 07 October 2023

I have read the revised manuscript and found the description of the mehtods very helpful. The modelling is very well done. I also liked the jackknife analysis, which adds nicely to the paper. Although not an expert on the moa, i very much enjoyed reading this manuscript. It is clearly written, conveys an interesting case study, and i expect it to be well cited.

Reviewed by Richard Holdaway, 19 October 2023

The author has made significant changes that meet most if not all of my concerns and I congratulate them on their thoroughness - and indeed their promptness in generating a revision. I am sorry that my review of the revised preprint has been delayed: health issues which are now mostly resolved.

My initial review was critical because there is an ”industry” in propagating stories of moa survival. And a deep feeling in some people that such beasts should not and could not have gone extinct: a hope and indeed belief in a better world, where such results as moa extinction are just not possible.

I used the term ”obviously” because a 95% confidence interval is just that... there is a 1 in 20 chance that it does not include the actual extinction date.

As a former member of the international Extinction Committee, I am well aware of the care necessary before a taxon is pronounced dead. There have been many Lazarus species, and there continue to be.

However, being somewhat closer to the matter here, I possibly better understand the social climate that, particularly in the 19th century, led to claims such as those included at least initially in the study. Which is why I believe that most if not all are irrelevant to the actual timing of moa extinction (remembering too that there were at least 9 species of moa, with different ecologies and geographic distributions and it is highly unlikely that all went extinct in the same year.

The best example of such a ”record” is that of Paddy Freaney and his friends. Bruce Spittle is confident that they were genuine. I, and most people involved at the time, do not share that belief. Freaney at that time was supported by the New Zealand Accident Compensation Corporation as a result of a back injury. The Corporation was very interested when they found through the media that he had been hunting in the rugged landscape of the Canterbury high country when apparently incapable of such activity. His business, that of the Bealey Hotel, was in decline at the time but many were attracted to the hostelry in response to the publicity and the marketing of such items as ”Moa poo”. The business survived and prospered on the increased trade.

I am happy for the preprint MS to go to formal review with a journal, but please do not include the quotation from my first review.

Regards

Richard Holdaway

Evaluation round #1

DOI or URL of the preprint: https://doi.org/10.1101/2023.08.07.552261

Version of the preprint: 1

Authors' reply, 02 October 2023

Dear Drs Werner Ulrich, Richard N. Holdaway, and Tim Coulson,

I am extremely grateful for the opportunity to revise my manuscript. Thank you all for providing thoughtful feedback. In revising my article following your comments, I believe that the manuscript has been improved greatly. Accordingly, I have added your names to the Acknowledgements section of the article in recognition of your contributions. Please find below my detailed responses and descriptions of revisions.

Review by Dr Tim Coulson

1. "As with any extinct species, reports of sightings can often be unreliable, and assigning confidence in each alleged sighting can be a challenge. What I would suggest is removing each sighting in turn or altering confidence in the sighting, and see how this impacts results. It will give an alternative estimate of confidence in the likelihood of extinction in each year."

Response: Thanks, the jackknife resampling you describe (i.e. removing each sighting in turn) is an excellent suggestion. I've updated the Methods and Results sections of the manuscript to implement this method, among other sensitivity tests that were recommended by Dr Richard N. Holdaway in their review below.

2. "A second comment is that a few more details on the methods could be given so that readers do not need to read Brooke."

Response: Sorry for this; I was aiming for brevity, but I realise I should have sympathised with the reader a bit more in describing the methods. In this revision, I've completely restructured the Methods section into three distinct subsections (Data; Model; Conservative Measures and Sensitivity Tests), and I have provided a mathematical description of the model that was implemented, including equations. I hope this is much more clear now. Sorry again.

Review by Dr Richard N. Holdaway

1. "In the Introduction, the author develops the case for reanalysis of moa extinction with a brief survey of moa characteristics and biology. But they rely on secondary, peripheral, and obviously incorrect literature. The best example of the last is the "3.6 metre tall moa" of Widrig & Field 2022). Where those authors obtained that number is puzzling: the largest female giant moa (Dinornis) were 2 m high at the rump and their necks would have allowed them to reach perhaps 2.5 m. And the same reference is derivative with respect to moa biology, for which there is an extensive and easily located literature. Scofield & Ashwell (2009) is hardly the most appropriate reference for the interactions between Haast's eagle and moa: there are more authoritative publications on the subject… on reproduction rates and age of maturity when there are primary literature references to growth rates, age at maturity, and reproductive rates, such as Turvey & Holdaway (2005) Postnatal ontogeny, population structure and extinction of the giant moa Dinornis. Journal of morphology 265: 70-86 and Turvey et al. (2005) Cortical growth marks reveal extended juvenile development in New Zealand moa. Nature 435: 940-943"

Response: I am both grateful and lucky to have had my manuscript assessed by Dr Holdaway, who I recognise is perhaps the world's foremost expert on the Moa. Dr Holdaway's experience with this literature vastly exceeds my own, and so I appreciate these suggestions. Accordingly, I have:

- Deleted the comment on the height of Dinornis robustus and reference to Widrig & Field from the second paragraph of the Introduction.
- Replaced the reference for Moa as megaherbivorous browsers in the second paragraph of the Introduction with Wood et al. (2020), which represents a recent synthesis of current knowledge of Moa feeding habits and contains many comments on Moa browsing.
- Replaced the reference for Moa as Haast's eagle prey in the second paragraph of the Introduction with Brathwaite (1992) and Worthy & Holdaway (2002).
- Replaced the reference for reproduction rates and sexual maturity in the third paragraph of the Introduction with Turvey & Holdaway (2005) and Turvey et al. (2005).
- Replaced other citations with Worthy & Holdaway (2002).

2. “While acknowledging the rapid extinction model in Holdaway et al. (2014), the author then cites Perry et al. (2014) as reporting high quality radiocarbon ages. The earlier cited paper and Allentoft et al. (2014) reported much larger, and just as precise, suites of ages from throughout the South Island, and assessed the chronology of extinction.”

Response: Thanks, I have added citations to Holdaway et al. (2014) and Allentoft et al. (2014) to this sentence in the third paragraph of the Introduction, as requested. I’ve also added Holdaway and Jacomb (2000) in acknowledging the rapid extinction model.

3. “In discussing alternatives to the overkill, the author cites Diamond (2000). Diamond (pers. comm. 2002) pointed that his view had changed after he had seen how even sporadic hunting trips by the indigenous inhabitants severely limit cassowary populations in terrain far more challenging than anything inhabited by moa in New Zealand.”

Response: Because I did not have this personal communication with Diamond, I don’t think it’s appropriate for me to cite Diamond directly, however, I have revised this fourth paragraph in the Introduction to refer to the past tense, i.e. changing “is not a view held by all” to “was not a view held by all”; “critics point” to “critics pointed”; and “critics opt” to “critics opted” to reflect a change in opinion. I have also added to the end of the paragraph “Some (previous) critics now support the overkills hypothesis (Holdaway 2023)” where the latter citation is private communication with the reviewer (i.e., the above comment number 3.).

4. “With reference to the obvious possibility of moa survival after 1450 CE (which the analyses in Holdaway et al. (2014) do not preclude), it is interesting that a “sighting” in 1993 should be referenced to a 1973 publication.”

Response: I am slightly surprised to hear the reviewer say that the possibility of moa survival after 1450 CE is “obvious”; although the upper 95% HPD for moa extinction in the South Island is given as 1480 CE in Holdaway et al. (2014), the abstract of this article states that the Moa were “exterminated” in “a brief (<150 years) period”, which would coincide with 1400-1450 CE if the first settlers arrived circa 1250-1300 CE, and this wording suggested to me that the authors of that study did not believe later survival was likely (or indeed “obvious”). Regardless, I have added “which the model by Holdaway et al. (2014) does not completely preclude” to the fifth paragraph of the Introduction. On the 1973 publication, that citation was meant for the latter comment on reports having originated from Maori and European peoples, rather than the former comment on purported sightings having been as recent as the 1990s. Sorry this wasn’t clear. I’ve now cited Spittle (2010) after the 90s sighting comment. Hope this helps.

5. “whether or not Mackal (1983) devoted a chapter to moa is irrelevant: people’s interest in a subject is not evidence for or against the object of the interest.”

Response: Sorry. My intention here by citing Heuvelmans and Mackal was simply to convey that this study (Foxon 2023) is not the first written piece on the possibility of late Moa survival. These citations were not indended to provide evidence one way or another. Accordingly, I have re-written this part of the fifth paragraph of the Introduction to say more precisely that “Other works have discussed this possibility, including Heuvelmans (1986) which lists “A surviving species of the Moa family… known to some as roa-roa” in an ‘Annotated Checklist of Apparently Unknown Animals,’ as well as Mackal’s (1983) book on ‘hidden animals’.”

6. “In addition to the perhaps unwarranted extension of the method in time and including accounts at second and often greater hand, there are issues in the data included. I feel that the events the author has harvested from a compendium of such reports must be far more critically assessed before they are accepted for analysis. For example, many of the records included are by apparent eye witnesses. “Eye witnesses” are regarded as being less reliable in general than other forms of evidence in courts of law because too many conscious and unconscious factors can bias the observation, but obviously not all can be rejected solely on that basis. Having said that, the author includes in the category of eye witness accounts those occurring several generations earlier than the actual report. That approaches the category of hearsay, to which many of the other events can be assigned. The author is well aware of that, given the annotations to the records in the database, but the events were included in the analysis nonetheless.”

Response: First, I have added an acknowledgement of the “unwarranted extension of the method in time”

in the revised manuscript, citing the referee (see final paragraph of the Model subsection of the Methods section). Second, to address the referee's requests for 'separation of wheat from chaff', I have added an additional sensitivity test removing all 'second and often greater hand' sightings from the model (see Methods and Results). Predictably, the probability of Moa persistence remains low in this sensitivity test, comparable to the pessimistic model with all sightings.

7. "most if not all the observations of bones on the surface or in caves and swamps, can be dismissed as undateable regardless of the observer's views of the possibilities of their recent deposition. All field experience shows that such remains can appear "recent" after centuries of exposure."

Response: I agree with this point, which is why I previously made it in the 'Notes' column for each surface bone 'sighting' in the original NMSRD dataset I provided with the submission (I previously stated in the 'Notes' column of the dataset for such records that "bones may have been deceptively old and well preserved"). Whereas I tried to take the 'ambivalent' approach (Brook et al.'s wording) of assigning a very low but non-zero probability to this type of sighting, the reviewer would prefer that I took the 'extreme pessimisim' approach (also Brook et al.'s wording) by removing them from the dataset altogether (i.e., assigning probability zero). Accordingly, just like with the 'second and often greater hand' category of sightings addressed in the response above, I have added another sensitivity test to the manuscript in which all bone 'sightings' are removed to estimate their impact on the model. Once again, the probability of Moa persistence remains low in this sensitivity test, as in the main model, as would necessarily be the case when removing a number of sightings from a model of this kind.

8. "There are far more recent and higher precision ages on moa eggshell than were av" [sic]

Response: Unfortunately, it appears that the rest of this comment (after "av") was cut off in the version of the reviewer report I received. I think the gist of the comment is that, in the original NMSRD dataset I collected, the first few records correspond to carbon-dated Moa remains. I think the reviewer was hoping for "more recent and higher precision" carbon-dated Moa remains than those from Higham (1994), which is the source I used previously. Accordingly, in this revision I consulted Supplemental Table 1 of Holdaway et al. (2014), which contains the conventional radiocarbon ages of 96 Moa eggshells in archaeological contexts. I replaced the data from Higham (1994) with these 96 more recent and higher precision estimates, and I have updated the manuscript to reflect this upgrade. The impact of swapping the original data from Higham (1994) with the more recent and numerous data from Holdaway et al. (2014) was negligible, for example the cumulative persistence probability was 0.21 for 1770 CE in the main model both for the Higham data (in the previous version of this article) and the Holdaway et al. data (in this revision), and to be clear, that's without making any of the other changes to the data described elsewhere in this document. The reason for this is because all that really matters is the last (most recent) eggshell, which was 1390 CE in Higham and 1408 CE in Holdaway. So while I assure you that I have made the requested change (and I agree that these newer data are more appropriate), the dates for the last eggshells (1390 CE and 1408 CE) are so similar that there's no impact whatever on the results or conclusion. I do appreciate this suggestion, however.

9. "The bases for other "sightings" seem not to have been thought through. A few examples will suffice… [the referee then provides a number of examples]."

Response: I sympathise (and agree with) this comment by the referee. My question, however, is that given the results of the model already provide very low probabilities of recent Moa persistence even when giving proponents the 'benefit of the doubt' and including all sightings in the model, regardless of how dubious they may be, what exactly is the point of applying any additional filter on the data? The referee asserts that removing this selection of sightings "must have implications for the results of the analysis". This is true, and the only possible impact on the model from removing a selection of sightings would be to decrease the probability of persistence. If we were in a situtation in which the main or even pessimistic models were providing 'borderline' persistence probabilities of ~0.5, it would be informative to see if this extra filtering of the data had any impact on which side of the fence the model results land (i.e., more likely than not that Moa persisted, or more likely than not that they were extinct). But the fact is, we are already in a situation with the complete sighting record

that the results are pretty firmly in the extinction camp. Given that in addressing previous comments as part of this revision I have already added sensitivity tests for (1) jackknife resampling; (2) removing all surface bone accounts; and (3) removing all second or greater hand accounts, and given the results of these and the main model(s) favour extinction, I think it is sufficient simply to remove the sentence "but give probabilities of Moa survival post-1450 CE that cannot be dismissed entirely" from the Abstract of the article, and to add an additional paragraph to the Discussion section of the article acknowledging the uncertainty of the sightings highlighted in the referee's comment above, and stating what I have said in this response. Again, I agree with the referee- it's just not clear to me what more can be added.

10. "While the author is to be applauded for trying to take anecdotes from subjectivity to objectivity, the manuscript in its present form does not achieve that objective in my view. At the end of the process, the results echo the situation before the analysis. Are we further ahead? The Bayesian extinction analyses reported elsewhere have long tails extending to the present, but with vanishingly small probabilities after 1750 CE."

Response: I think there's a point to be made here about the fact that the results of the sighting model support and supplement the results of the Bayesian extinction analyses. I don't see the model in the present study as replacing or surpassing the previous Bayesian model (that was certainly not my intention!), but as complementing it using different kinds of method and data. I've added a new paragraph to the Discussion section of the manuscript stating the above.

11. "Given that on present evidence moa had large territories and most species physically could not negotiate steep terrain is beyond possibility that such viable populations of moa survived into the 20th century. All potential surviving habitat (which excludes the precipitous rain-soaked valleys of Fiordland) has been traversed continuously first by gold prospectors (who had visited most valleys by 1880) and since by hunters, hikers, and scientists."

Response: I like this quotation so much that I have included it in the Discussion section of the manuscript with citation to you. I hope you don't mind...

Thanks again for your consideration of my manuscript. I hope that these revisions are satisfactory.
Kind regards,
Floe Foxon - Folk Zoology Society

Decision by Werner Ulrich, posted 26 September 2023, validated 26 September 2023

Full revision

Dear Dr Foxon

Finally, I have received 2 reviews from experts in the field. As you see both vary in conclusion and argument. The second referee is very critical and questions the sightings you mention. The first referee is more general and wants to see methodological clarification and better statistics. In my view you should be more neutral when testing the early and late extinction hypotheses. As it stands you seem to select arguments in favour but not against your hypothesis. The second referee makes good points against your sighting reports and consequently against your major conclusion. For a non-expert these are convincing. You need to provide conclusive arguments to all his comments.

Therefore I invite you to revise your manuscript and to provide detailed answers to all of the points raised by the referees

Kind regards
Werner Ulrich

Reviewed by Richard Holdaway, 20 September 2023

In this contribution, the author attempts to apply a new statistical protocol – developed to objectively assess "eyewitness" and other observations to determine the likelihood of continued existence, or date of extinction, of apparently extinct taxa – to the fate(s) of the large flightless New Zealand birds of the order Dinornithiformes, known collectively now as moa.

The algorithms were developed to critically assess reports of an Australian marsupial predator, the thylacine Thylacinus cynocephalus. The reports for that species all apply to a timescale from the middle of the twentieth century to near the present. Both in timescale and the nature of the purported records of the thylacine differ in kind from those employed in the present MS for moa.

In the Introduction, the author develops the case for reanalysis of moa extinction with a brief survey of moa characteristics and biology. But they rely on secondary, peripheral, and obviously incorrect literature. The best example of the last is the "3.6 metre tall moa" of Widrig & Field 2022). Where those authors obtained that number is puzzling: the largest female giant moa (Dinornis) were 2 m high at the rump and their necks would have allowed them to reach perhaps 2.5 m. And the same reference is derivative with respect to moa biology, for which there is an extensive and easily located literature. Scofield & Ashwell (2009) is hardly the most appropriate reference for the interactions between Haast's eagle and moa: there are more authoritative publications on the subject. The author seems to have only a limited knowledge of the literature on the Dinornithiformes.

While acknowledging the rapid extinction model in Holdaway et al. (2014), the author then cites Perry et al. (2014) as reporting high quality radiocarbon ages. The earlier cited paper and Allentoft et al. (2014) reported much larger, and just as precise, suites of ages from throughout the South Island, and assessed the chronology of extinction. The author then proceeds to cite a 40-year-old popular book as the basis for statements on reproduction rates and age of maturity when there are primary literature references to growth rates, age at maturity, and reproductive rates, such as Turvey & Holdaway (2005) Postnatal ontogeny, population structure and extinction of the giant moa Dinornis. Journal of morphology 265: 70-86 and Turvey et al. (2005) Cortical growth marks reveal extended juvenile development in New Zealand moa. Nature 435: 940-943, as well as Worthy & Holdaway (2002) Lost world of the moa. Indiana University Press. That book covers most aspects of moa biology and also the interactions between eagle and moa yet is not cited at all.

In discussing alternatives to the overkill, the author cites Diamond (2000). Diamond (pers. comm. 2002) pointed that his view had changed after he had seen how even sporadic hunting trips by the indigenous inhabitants severely limit cassowary populations in terrain far more challenging than anything inhabited by moa in New Zealand. With reference to the obvious possibility of moa survival after 1450 CE (which the analyses in Holdaway et al. (2014) do not preclude), it is interesting that a "sighting" in 1993 should be referenced to a 1973 publication. Finally, whether or not Mackal (1983) devoted a chapter to moa is irrelevant: people's interest in a subject is not evidence for or against the object of the interest.Data

In addition to the perhaps unwarranted extension of the method in time and including accounts at second and often greater hand, there are issues in the data included. I feel that the events the author has harvested from a compendium of such reports must be far more critically assessed before they are accepted for analysis.

For example, many of the records included are by apparent eye witnesses. "Eye witnesses" are regarded as being less reliable in general than other forms of evidence in courts of law because too many conscious and unconscious factors can bias the observation, but obviously not all can be rejected solely on that basis. Having said that, the author includes in the category of eye witness accounts those occurring several generations earlier than the actual report. That approaches the category of hearsay, to which many of the other events can be assigned. The author is well aware of that, given the annotations to the records in the database, but the events were included in the analysis nonetheless.

The bases for other "sightings" seem not to have been thought through. A few examples will suffice. There are far more recent and higher precision ages on moa eggshell than were av

"Sighting 14" in the spreadsheet, assigned rating of 2, reports that a Maori elder, Haumatangi, "70 years old"

interviewed in 1844 remembered seeing both James Cook during one of his visits in the 1770s and "the last" moa. There is no indication how the elder's age was established or confirmed but a 70-year-old in 1844 would have been born in or around 1774. No location for either his contact with Cook or the moa were provided. Cook's most extensive contacts with the New Zealand coast occurred in late 1769 through 1770 during his first voyage. On his second voyage he spent six weeks in Fiordland in the southern autumn of 1773, where he made contact with a few local Maori, and at Queen Charlotte Sound during the winter. His fortnight's visit in February 1777 was confined to Queen Charlotte Sound, where he again engaged with local Maori.

For Haumatangi to have seen – and remembered – Cook during his visits, he would have had to been significantly older than 70. At best, a birth year of 1770 would have made him around seven during Cook's short stay in 1777. At worst, to have been in 1770of an age where some memory can be trusted in most of us, Haumatangi would have been born closer to 1763-1765 and been 80 in 1844. The only data provided in the "sighting" is his age of "70 years". In the absence of any justification for that, and of any other information such as the locations (beyond "Wellington region"), and given the date of 1844, just a few years after Richard Owen's pamphlet asking for more bones was widely circulated in the infant colony, it is at least conceivable that the story was concocted to enhance the speaker's standing with the Europeans.

"Sighting 10" Moa eggs in a nest is extremely unlikely. Recent observations of nest scoops and analysis of the breeding system suggest that a typical clutch was a single egg.

"Sighting 20" Regardless of Hector's opinion, such tracks are well known now to have been made and maintained by the large flightless nocturnal parrot, the kakapo (Strigops habroptila). Hence, number 20 must be discarded.

"Sighting 85" Whether or not Geoffrey believed the story is irrelevant. Atholl Anderson has argued convincingly that Alice McKenszie's birds were vagrant king penguins from the subantarctic islands.

"Sighting 100" See above. There is no evidence that moa had more than one, or very rarely, two chicks. If they were indeed bipedal, they may have possibly been Cape Barren geese, which are known to have arrived from Australia and bred. Size is difficult to judge from an aircraft.

Lastly, "Sighting 106" The "sighting" was made in an area where experienced ornithologists had studied the bird fauna for several decades, without seeing any evidence of any such large bird. What is missing from this, as well as many of the others The photograph was subjected to image analysis at the University of Canterbury: the only conclusion from the enhancement was that the camera was stationary although it had been claimed that the poor focus had been caused by the image had been taken while the photographer was moving towards the animal. Having personally seen the image and the enhancement at the time, it is clear that it was the rear end of a red deer. Paddy Freaney's Bealey Hotel had a major increase in clientele after the publicity and was still selling "moa droppings" some years later. Opinions voiced at the time by people who knew the participants suggested that practical joking was part of their ethos. Whatever the truth or otherwise of those views, the image was not of a moa. Whether or not Spittle himself believed the trio were sincere is, as with such opinions in the records of other "sightings", irrelevant. Removal of this 1993 "sighting" must have implications for the results of the analysis.

Whereas, the thylacine study was based on mostly explicitly dated "sightings", many of those included in the Spittle database can barely be assigned to century. Before the manuscript is submitted for consideration for formal publication there needs to be much deeper critical assessment of the "sightings", and those that can be seen as hearsay rigorously discarded. It is not sufficient in this context to base inclusion on one person's "belief in the veracity of the observer" against all the other evidence for potential hoax. In addition, most if not all the observations of bones on the surface or in caves and swamps, can be dismissed as undateable regardless of the observer's views of the possibilities of their recent deposition. All field experience shows that such remains can appear "recent" after centuries of exposure. Conclusion

While the author is to be applauded for trying to take anecdotes from subjectivity to objectivity, the manuscript in its present form does not achieve that objective in my view. At the end of the process, the results echo the situation before the analysis. Are we further ahead? The Bayesian extinction analyses reported

elsewhere have long tails extending to the present, but with vanishingly small probabilities after 1750 CE.

Sadly, the focus is still in the sightings on the “last moa”, when before that there must have been a self-sustaining population until at least a decade or two before any final bird. Given that on present evidence moa had large territories and most species physically could not negotiate steep terrain is beyond possibility that such viable populations of moa survived into the 20th century. All potential surviving habitat (which excludes the precipitous rain-soaked valleys of Fiordland) has been traversed continuously first by gold prospectors (who had visited most valleys by 1880) and since by hunters, hikers, and scientists.

The manuscript in its present form, without drastic separation of wheat from chaff in the “sightings” list, just maintains the dichotomy between evidence for extinction versus belief without evidence in later survival.

Richard N. Holdaway

Reviewed by Tim Coulson, 21 September 2023

The paper takes a method developed by Brooke to assess the distribution of extinction times for the thylacine and applies it to the New Zealand moa. The method itself is robust and can provide new insight into the timing of extinction. However, any method is only as good as the data used in analyses. As with any extinct species, reports of sightings can often be unreliable, and assigning confidence in each alleged sighting can be a challenge. What I would suggest is removing each sighting in turn or altering confidence in the sighting, and see how this impacts results. It will give an alternative estimate of confidence in the likelihood of extinction in each year.

A second comment is that a few more details on the methods could be given so that readers do not need to read Brooke.