Hafted stone and shell tools: responses to reviewers

I thank both reviewers for their useful comments. Their comments on my text are pasted below in italics, with my responses underneath.

Page numbers refer to pages in the original preprint.

Christopher Buckley (author) 13th March 2023.

Reviewer 1 (anonymous)

General Comments:
The literature review in this paper mixes observations on stone, shell and metal tools, and discusses (deep) archaeological data/phenomena. The author might want to modify their abstract accordingly and maybe their title as well.
Agreed. I have modified title and abstract.

The paper presents a global typology for hafted tools in the Asia-Pacific region, which is interesting and useful. One comment that relates to other aspects mentioned in this review: the author generalises based on ethnographic data but then applies their haft typology to the past, which is problematic on a methodological and theoretical level. For example, how can one assume that mortise and tenon hafting (M types in the author's typology) were not used for adzes in the past?
My discussion on haft typology is based equally upon ethnographic examples from New Guinea, and archaeological examples from the European Neolithic. The European Neolithic examples of hafted tools with handles of wood and bone, from Alpine lake sites and other regions, is extensive, important and deserves to be better known. I refer to this literature and discuss it in my article.

Regarding M-adzes (ie a blade hafted horizontally, mortise-and-tenon fashion, into a wooden handle), in my text I note first of all that there are no ethnographic or archaeological examples of such tools. I go on to explain that this is because this hafting pattern would render a stone blade uniquely vulnerable to fracture along its weakest axis at the point where the blade enters the handle. This method of hafting is occasionally encountered with metal blades, which are tougher and more resistant to this kind of fracture.

I've added some words to my text to clarify these points.

On a similar note, the author uses ethnographic data and very limited archaeological examples to demonstrate 3 waves of stone tool innovations in the Papua/New-Guinea region, which is not convincing in my opinion. The hypothesis is interesting, but without a thorough and wide-scale archaeological investigation, claiming this as a model to explain the distribution of ethnographic stone tools is just not robust enough.
The archaeological and ethnographic evidence is actually quite compelling. I agree that my first draft did not include enough discussion of this evidence, so I have added more discussion and references. This additional perspective is now in the sections headed 'Archaeology of the New Guinea Highlands' and 'The New Guinea interior: evidence for change and succession in stone tools."

A ‘thorough and wide-scale archaeological investigation’ that the reviewer refers to would certainly be desirable, but it may be a long time coming. The New Guinea environment is also a uniquely difficult one for archaeology: in the highland region investigations are limited to flat areas in the valley floors and a few cave/ overhang sites. Organic remains are rare, and the highland practice of cremating rather than burying the dead mean that there are no collections of artifacts from grave sites (a distinction vs Remote Oceania). Most highland societies were small in scale and left few traces, their substantial field systems being the main impact on the landscape. For these reasons, archaeology alone is unlikely to answer all of the important questions. The most promising approach (I think) is the integration of archaeological, ethnographic and linguistic data. This is the approach that I follow here.

One of the references suggested by the reviewer is a great example of how ethnographic and archaeological data can be combined in a constructive manner:
Hermann, A., Molle, G., Maury, R., Liorzou, C. and Mcalister, A. 2019. Geochemical sourcing of volcanic materials imported into Teti'aroa Atoll shows multiple long-distance interactions in the Windward Society Islands, French Polynesia. Archaeology in Oceania, 54(3): 184–199.

The model that I propose is an extension of existing thinking on the connection between farming and language. It accounts for the known patterns of archaeological and ethnographic stone tools in the New Guinea highlands and makes predictions about what may be found in future investigations. This is as much as one can hope for from a hypothesis, and sufficient reason, therefore, to propose it.

I’ve added more discussion of these points in the section headed ‘Implications’.

The final and concluding discussion about potential correlations between material culture and language is not announced in the abstract or in the "Objectives" section of the paper.
Agreed and corrected.

General lack of bibliographical references in multiple instances (noted in the pdf).
Agreed and corrected.

My overall recommendation for this paper is that the paper should be published after major revisions addressing the comment made in my review and in the ms document.

Page 2
“These ranged from casual purchases by tourists to systematic collecting expeditions
such as that conducted by A. B. Lewis on behalf of the Field Museum in Chicago.”
This might be an under-utilized source but as is this seems over simplistic, as many institutions across the globe have participated in such an endeavour.
My point is a simple one: there have been relatively few publications that deal with ethnographic examples of hafted tools in the AP region. I have removed the words ‘under-utlized’ to avoid offending anyone. But see my comment below on the relative lack of citations of Crosby’s work.

“Stone tools were made and used across the region from the time of its earliest settlement, and there was continuity between archaeological, historical, and ethnographic evidence of stone tool use, as well as the peoples who used them.”
Unfortunately not always

Agreed, continuity is not present in every case. That said, stone tools were in daily use at the time the first colonial-era voyagers entered the region, and there is no reason to suppose these tools were invented or imported just prior to 1750, so in the first instance we must look for their antecedents in the Asia Pacific region. More complex (and therefore less parsimonious) scenarios involving loss, replacement, importation, or reinvention should only be invoked if there is evidence to support them. I have added some words in the revised text on this point.

"The most important compendium of information for the ethnographic tools of the near-Pacific is the PhD thesis written by Eleanor Crosby (1973)".
Again, this is overlooking a lot of the literature. While this resource is extremely useful, it is not the most important compilation of information. Many scholarly publications have been released on that topic in the last 5 decades.
Moreover, Crosby's compilation focuses [sentence incomplete]
In the revised version I cite and discuss all the other works (that I am aware of) that deal with ethnographic tools (the Petrequin's work, Steensberg, Hampton, Sillitoe, Hughes etc). These works deal with specific areas: Crosby is the only author to have attempted a regional survey (hence, 'compendium'). According to Google Scholar, Crosby's work has picked up only 20 citations in 50 years, so there hasn't been a great deal of interest in the topic.

"near-Pacific"
Scholars have agreed on the terms of Near and Remote Oceania (see Green 1991), but this expression is a little odd. I suggest that it is changed or would need to be clarified.
I agree – I have changed the wording throughout to clarify.

"Crosby surveyed a large number of hafted stone tools from Melanesia and Micronesia, mainly in antipodean collections."
Warning! Crosby compiled mixed data from stone and shell tools. Stone and shell technologies are very different and are not comparable in terms of technical behaviour/traditions as they relate to different modes of actions.
This should be more explicit in the manuscript and the title of the paper should be modified accordingly or shell adzes should be removed from the study.
I agree - I have modified the title of the paper to reflect the fact that both stone and shell tools are surveyed and discussed.

Page 3
"The complete map and raw data can be viewed online ..."
This is very impressive!

Page 4
(map)
Warning: the distribution of Polynesian outliers is much larger than the one outlined here ; Rapa Nui / Easter Island (within the Polynesian triangle) is located outside of the map so triangle needs to be open and arrow added ; Australasia refers here to Australia (Australasia comprises Australia, New Zealand and sometimes islands of the western Pacific). This needs modifications.
Agreed – I have updated the maps accordingly.

Page 5
"I have no objection to pursuing ethnoarchaeological objectives, but in this survey my primary

focus is the AP region so I will formulate no 'ethnoarchaeological models'. Comparisons that I make between regions, a topic I will return to near the end of this chapter, will also be more compelling (I think) if the interpretations have been arrived at using independent data and reasoning."

What is the angle then? How is the approach justified theoretically and methodologically? Knowing that using ethnographic data to address historical questions is problematic since societies and material cultures evolve.

Good question. I have expanded Section 1.2 to explain my approach in detail.

Page 5

"In contrast to this approach, a hafted tool with a ground edge embodies two innovations: the additional leverage and reduced shock on the hand and wrist afforded by a haft, and the possibility of resharpening by anyone (including non-experts) in possession of a grinding stone."

This is certainly not true in the Pacific islands, where the resharpening of stone adzes always entails "refreshing" the cutting edge by flaking before resharpening, as well as reshaping the entire blade as well. The lack of skill is generally responsible for the early discard of tools with accidents due to unadapted percussion technique.

I acknowledge the reviewer's experience relating to Pacific Islands. I agree that seriously damaged tools can only be resharpened by knapping, and lack of skill in this respect is certainly a reason for early discard. My statement regarding the importance of grinding is based on ethnographic work in the highland regions of New Guinea by Sillitoe, Hampton and others. I have added details and references in Section 1.3.

The perspectives from ethnography and archaeology also tend to be different on this point. Since deliberate burial as grave goods was not a factor in the New Guinea highlands, one might expect an archaeological investigation to yield mainly damaged tools and fragments. Even in Remote Oceania, intact adzes were rarely discarded and hence rarely found in archaeological contexts. In contrast, an ethnographer examining tools that are in-use will mostly be looking at tools in serviceable condition. This is another example of why it is important (I think) to integrate the two types of data.

Page 7

"Typologies of hafted forms"

What about the typology of Petrequin and Petrequin 1993, 2020?

The axe/adze typology employed by the Petrequins is based upon hafting method and is similar to those employed by other authors. They do not tabulate their typology, so I have not attempted to do so.

Page 20 (map)

Several comments:

- Not all adze blades in Polynesia are tanged

- The information on "basalt trade" in Polynesia is not up-to-date and very simplified here. Not sure if necessary to outline since it is mainly a pre-ethnographic-period phenomenon. For more recent syntheses see Hermann 2015 and Weisler & Walter 2017.

Thanks for the comments. Yes, not all Polynesian blades are tanged! I have reviewed all the references that I can find on the basalt trade and updated the map, this is now a separate figure (Fig 20). This trade is the key to understanding why ethnographers found similar basalt blades across a very wide region of the Pacific.

Page 21

“As noted, the earliest fully ground blades on the Asian mainland were blades with oval or teardrop shapes in plan-view, and lenticular or oval cross-sections. Ground stone blades with complex shapes (quadrangular and shouldered forms) first appeared on the southeast Asian mainland around 8kya. From around 5kya onwards they began to appear in ISEA, where they are associated with the expansion of Austronesian language speakers, fishing and farming peoples who originated on the mainland. These blades were almost certainly hafted as T1-adzes, and were used for agriculture and woodworking, including boat-building.”
References needed
Added

“As mentioned, such tools had mostly disappeared across this region before colonial times, However, early iron tools preserved in Dutch Museums are hafted in similar ways to ethnographic stone tools. The majority are attached to the top of a T or L-shaped haft by means of lashed cane or plaiting work. The blades, which are mostly flat quadrangular blades with a tang, were presumably locally made and pre-date imported European forms (Fig 7). Most were oriented as adzes, a few as axes. These tools, considered together with the archaeological data, suggest that the T1-adze has been the dominant form in ISEA for at least the last two millennia.”
References needed
I have provided an illustration of one of these iron tools. There are quite a number in Dutch museums. The inference that the T1 adze was the dominant form in ISEA is my own.

Page 22
“Australia and Near Oceania were first settled by people speaking the forerunners of Australo-Papuan languages, between around 60kya and 25kya. Until around 9kya Australia and New Guinea were part of a single continent (Sahul), the two becoming separate as a result of sea level rises following the end of the last ice age. Early settlers crossed from the continent of Sundaland through the island archipelago of Wallacea to reach Sahul, and possessed seaworthy boats. They brought flaked tool technologies with them, and probably other technologies, such as string and cord usage, bows and arrows, and bamboo knives, though little trace remains of such organic materials.”
References needed
Added

“Some axe heads are also partly covered in spinifex gum in order to make a more secure attachment to the handle.”
References needed
Added

“3.5 Melanesia and Micronesia: offshore islands”
This is confusing, which islands are offshore, and offshore from what? Does the author mean "remote" ?
I have clarified this in the revised text.

“Despite the appearance of innovative tools, the replacement of older types by new forms in the highland region was slow, partly due to cultural conservatism and partly to physical factors. Objects were exchanged through local trade, but technologies such as tool production moved more slowly. Ian Hughes’s study (1977) records an extensive trade in axe blades in the Central Highlands in pre-contact era, but even a few kilometers from axe quarries users had no knowledge of where the sources were or how the axes were made. The extreme slowness of

technological change in the New Guinea highland means that tool distributions in the recent past continued to reflect (in part) ancient phases of agricultural intensification. Putting these various elements together suggests that distribution of tool types in the highland region reflected three waves corresponding to the emergence and intensification of agriculture
(Fig 18). The three waves are as follows:"
I must be missing something : I don't see how ethnographic data can be used to demonstrate historical processes or phenomena.
I disagree. Present-day material culture distributions are extremely important for delineating historical processes. Any model for (say) the historical development of hafted tools must explain the observed ethnographic distribution of those tools. This distribution severely constrains the models that one might pose. That does not mean ignoring the archaeological and linguistic data: the integration of these three aspects is the key.

Aside from this general point, there is specific ethnographic data in New Guinea that points to a gradual westward expansion of adze using peoples at the expense of axe-users (Petrequin & Petrequin 2020: 99, 180). I have added some references and discussion of this in my text.

I note that the Petrequins, probably the foremost living experts on the stone tools of Papua, are not shy about posing historical and evolutionary hypotheses based upon their ethnographic observations, in fact this is a major theme of their work. Their inferences seem very reasonable to me.

Page 41
"Wave 1: M-axes
The first wave began between 9000 and 6000ya and was associated with the appearance of permanent settlements in the highlands, forest-clearance and low-intensity agriculture, probably originating in the Central Highland region. This wave was associated with the dispersal of lenticular blades hafted as M-axes, and TNG-language speaking peoples."
Where is the data supporting that?
The key points are summarized by Golson (2005, 2017), who led the work on the emergence of agriculture in the Highland region. I quote from his 2017 review: "The transition from waisted axes to ground and polished stone axes in Papua New Guinea is linked with tool design, hafting technology and forest clearance for cultivation in the Holocene". See also Gaffney et al 2015.

I have added references to archaeological data for successions of tool forms in the Highland region in the paragraphs preceding the description of the model. I don't insert references in the description of the model itself, since I want to make it clear that this part of my paper is a hypothesis, not a review.

"and the handle was similar to forms found in the southwestern province of Papua."
Reference?
Added

Page 42
"Wave 2: T1a and T1r adzes
The second wave was associated with the intensification of agriculture and the spread of the adze, a superior tool for general woodworking and vegetation management that extended the life of valuable stone blades. Adze use was associated with two centers of innovation: T1a adzes in the west."
Where is the data supporting that?

This model explains the present-day distribution of adzes. I've added a new paragraph ('Implications') directly after the section describing the model, that explains this.

"Wave 3: T1f axes and wealth stones
The third wave was associated with further intensification, probably associated with the arrival of the sweet potato during the last millennium."
Where is the demonstration?
This model explains why such objects appeared and were circulated in some areas and not others (see 'Implications').

"Prestige TSf tools replaced older T1r adzes in the Central Highlands, and wealth stones were produced and circulated in the Baliem Valley and Massim regions."
Is the author addressing (pre)historical processes or recent historical facts?
Both. See the revised paragraph headed: 'The New Guinea interior: evidence for change and succession in stone tools'.

"3.7 Polynesia
This region was explored and settled by peoples speaking Austronesian languages (Lapita voyagers). The expansion of this group is thought to have originated in the Bismarck Archipelago, moving into Western Polynesia (Tonga, Samoa, Futuna) and the Fijian archipelago around 3000ya, with a second wave of sea voyaging that resulted in the settlement of the most remote islands around 1000-800ya."
References?
Added

"Despite the complexity of hafted tools found in the Bismarck Archipelago, and evidence of contact and interchange between incoming and indigenous populations (Shaw et al 2022), a single tool type came to predominate in Polynesia."
This doesn't make much sense: the Polynesians develop in the central Pacific area, far away from the PNG region.
Polynesians are the successors to Lapita colonizers. My text doesn't say that Polynesians came from the Bismarck Archipelago.

Page 43
"Adzes are not common in Lapita archaeological sites, but a site in Tonga dating from 2700-2500ya, a relatively early phase in the settlement of Western Polynesia, yielded a variety of complete and fragmentary stone adzes. These adzes were mainly plano-convex forms lacking obvious tangs (Reepmeyer et al 2021). The assemblage is not obviously different from blades found in New Guinea (for example), implying that distinctive Polynesian forms developed after this primary colonization event."
No, we know Polynesian material culture develop after the Lapita period, the cultural sequence is clearly established.
The reviewer and I are in perfect agreement on this point. I have reworked this section slightly to make this clear (I hope).

"The Polynesian Outliers group was settled around 1000ya, at around the same time as Eastern Polynesia (Leppard et al 2022)."
The Polynesian outliers don't form a group.

This is inaccurate and certainly not applicable for most of the 18 outliers.
For the Polynesian Outliers a more appropriate citation would be the collective book published by Feinberg & Scaglion in 2012.
Thanks, I have revised this section and added the Feinberg and Scaglion reference, which is indeed a better source.

Page 44
"This trade lasted from around 1000ya to around 400ya, which is presumably the period in which Polynesian adzes acquired their final forms and which explains the close similarities between tools from far-flung regions."
See also Best et al 1992 for western Polynesia-rest of Pn connections, Hermann et al 2017 for the Marquesas-Austral connection, Quintus et al 2022 and Hermann et al 2019 and for Tonga-Society connection.
Thanks for the comments and references. As noted, I've revised this section and expanded it.

Page 45
"Convergence in blade forms
The 'default' shape of a fully-ground stone blade..."
Is it really the case by default? Globally there are many axes and adzes that were ground on the active part only.
Note that I am discussing fully-ground blades here.

Page 51
"At the largest scale there is little or no direct interaction between communities because they are isolated by distance. This lack of interaction leads to distinctions that gradually increase, through the forces of invention and cultural drift, and persist over millennia."
no lack of interaction in the Pacific region : more "relative isolation"
I agree, I have modified this statement. Even in the Pacific, islands developed distinctive, local cultures that gradually diverged after initial colonization.

Reviewer 2: Adrian L. Burke, Département d'anthropologie, Université de Montréal
I very much enjoyed reading this chapter. I have to say at the outset that I am not an expert on the archaeology of the Asia-Pacific region. I work primarily on the Indigenous pre-contact archaeology of North America. I have however, like many anthropologically trained archaeologists, read the classic ethnographies of the region. I am also quite familiar with Pétrequin and Pétrequin's work in Irian-Jaya and their work on the axe-adze manufacturing communities, an "incontournable" for anyone interested in the manufacture of axes-adzes in the past. I can therefore not really speak to whether the survey aspect of this chapter is exhaustive since I am not familiar with the data available.

The text is well-written and easy to read. The objectives are laid out clearly and the organization of the paper is logical. This is not a journal article but rather a book chapter and as such it is longer than most articles which also allows the author to conduct a thorough survey and provide more descriptive data than usual. I found very few grammatical errors. I have communicated my minor stylistic suggestions directly to the author and they are not pertinent here. The figures in the chapter are excellent. The maps are very useful and help to summarize the large corpus of data. There is also an online Google Maps version of the maps and the distribution of the axe-adze data that is really very helpful and user-

friendly. The photographs of ethnographic axes-adzes are a plus and provide the necessary visuals to show how varied and sophisticated this technology is in the Asia-Pacific region.

This chapter is primarily a survey of the ethnographic data available for the Asia-Pacific region concerning axes and adzes, and more specifically the various ways that these tools were hafted in the recent, ethnographic past. This survey of the data is then compiled and used by the author to address questions that are more archaeological in nature. The distribution of different axe/adze forms and their specific hafting methods are compared to ethnic or linguistic groups in the region to look at interactions and relationships between these groups, as well as the population history of different groups that occupy the region today ("waves" of migration). Buckley also addresses the issue of convergent evolution with regards to axe/adze forms in the Asia-Pacific region and then compares this to the European Neolithic. I will return to these two major questions below after a few comments on the text.
The author says that he will not formulate any ethnoarchaeological models (p. 5). This is too bad but I think that it is understandable and one sees the purpose of his text towards the end and it is clearly more archaeological than ethnoarchaeological.
Both reviewers have commented on this, so I have expanded this discussion (Section 1.2) to try to explain my approach more clearly. I certainly have no objection to combining ethnographic and archaeological data, in fact it is the theme of the present work. However, for the purposes of my discussion on convergence, some care is required when comparing datasets on New Guinea ethnographic tools with hafted tools from the European Neolithic, to avoid the risk of circular reasoning. I have tried to explain this more clearly in my revision of Section 1.2.

As I noted in my replies to Reviewer 1, I think that the unique circumstances of the Pacific region, and the New Guinea highlands in particular, make it essential to combine archaeological, ethnographic and linguistic data when building historical models.

Buckley mentions in two places the fact that polished axes/adzes appear before agriculture in other parts of the world (p. 6 & 27). It is interesting to note that in many parts of North America, flaked-pecked-ground-polished stone tools of a large calibre such as axes, adzes and gouges predate horticulture by thousands of years. Moreover, they are present in areas where horticulture was never possible and was never adopted such as the Boreal Forest. Even more intriguing is the fact that they are also present in areas and at times when trees were absent or very small suggesting that they should not always be automatically associated with felling of trees and woodworking.
Thanks – I have added a brief note to reflect this.

The last paragraph of section 1 (p. 6) is not really clear and I am not sure it is necessary.
Agreed – I have deleted this.

In addition, on the same page in the second paragraph of section 2, the author states "These sophisticated ground tools were mainly, but not exclusively, associated with migrations of Austronesian-language speaking peoples." which seems to contradict what he just wrote in the last paragraph of section 1.
Yes, I should have said 'shouldered adzes' rather than 'ground tools'. I've deleted this sentence as it's not essential for my discussion at this point.

If there is one weak link in this chapter it is the apparent conflation of ethnicity and language. I repeat, I am not an expert in the Asia-Pacific region but it is always best not to presume that an "ethnic" group as defined by outsiders is the equivalent of a linguistic group. I think that it would be useful if the author

added one or two paragraphs to address this rather thorny issue in anthropology if only to make his assumptions clear to the reader from the outset. Defining "ethnic" groups is hard in anthropology and ethnoarchaeology. Trying to match these amorphous and dynamic ethnic groups on the ground with linguistic groupings is tricky at the best of times. This in turn has a direct impact on how we read the author's interpretations of the axe/adze distribution data in the last part of the chapter that are more archaeological.

As a student of material culture, I agree whole-heartedly with this point. The problem we face is that, when dealing with ethnographic material, we must start somewhere, and we find that linguists are the ones who have done the work and defined the groupings. There is much less research on material culture, and what exists is patchy and difficult to make comparisons with. In the case of the Baliem Valley in New Guinea, for example, the first researcher in the Valley was Myron Bromley, a missionary linguist. His work defined the ethnic groupings in the Valley and still does to this day. This work is gradually being updated and modified, but there is currently more interest (understandably) in addressing issues of self-identification than there is in addressing questions of material culture.

I have added some thoughts on this point to my text, though I certainly would not claim to have resolved these issues.

In section 5 Correlations between material culture and language, Buckley returns to this issue and takes a critical look at it, which I appreciated. The author discusses the example of some coastal ethnographic materials compared to language groups showing how it is in fact very hard to correlate the two. He asks: "why are correspondences between material culture and ethnicity so difficult to discern (requiring cutting-edge statistical analysis) in the Lewis dataset?" I suspect that the answer is because there is often no direct or obvious correlation (see for example Hodder 1982, Symbols in action: ethnoarchaeological studies of material culture)! I am not sure that I am convinced, or that my misgivings are assuaged, by Buckley's arguments in this section, and I leave it up to each reader to decide for him or herself.

I agree that Hodder's excellent 'Symbols in Action' is one of the few texts that seriously addresses this question. From what I can see, it is best addressed on a case-by-case basis. In the case of the Baliem Valley in New Guinea, there are (or were) clear associations between language group and stone tool use, as described in detail by Hampton, the Petrequins and others. I have modified my text to reflect this point.

Regarding the question of how an ethnographic distribution of axe/adze hafting styles can be used to address issue of migrations and peopling of a region, I find that the author's interpretations and hypotheses are reasonable and based on the data at hand. I assume that it will be up to the specialists in the region to fight it out over whether they accept these interpretations or not. At the very least, they provide hypotheses and models that can be tested against other archaeological data or DNA data. The proposed model for the adoption of agriculture in interior highland New Guinea, and the proposal of three waves of development of the tools associated to agriculture is fascinating. It is not entirely clear however if the author implies that these waves were accompanied by people or not.

Yes, I believe they were accompanied by the expansion of farming peoples, an extension of the farming-language hypothesis. I have added a few words to clarify this.

For the larger Asia-Pacific region the author proposes models that are more clearly migration models for the islands and these are based again on the hafting styles of the axes and adzes. As Buckley states, this kind of research trying to match material culture to specific groups is not very popular any more (old-fashioned culture-history?), but this chapter shows that a careful survey of the ethnographic data can at

least generate some very compelling models for the peopling of a region and also for the adoption and intensification of agriculture – two things that interest most archaeologists.
'Old-fashioned' is correct. In archaeology and anthropology approaches are often taken up and then dropped, sometimes even renounced, according to prevailing fashion.

Finally, concerning the issue of convergent evolution in technology and forms of tools for example, the use of morphospace to compare hafting methods between the Asia-Pacific region and the European Neolithic is a good way to present the data visually and show the high number of parallel forms. I am not sure if I agree with the author that this is a "remarkable" example of convergent evolution (p. 49), but it is certainly interesting and compelling.
My enthusiasm got the better of me. I have toned this down a bit.

It is worth more investigation and more cross-cultural comparison, another approach that seems to have fallen out of favour with archaeologists. I think that the author may want to eventually try statistical methods to test some of these ideas as is done in the book by O'Brien, Buchanan, and Eren that he cites.
Thanks – this book was a source of inspiration for how to think about tool morphospaces. I will look again at these essays with the question of statistical testing in mind.

At the end of section 4 Buckley discusses alpine jade axes and their association with high status, comparing them to axes in New Guinea that were seen as prestige items. It would be good to provide some references here, possibly to Pétrequin and Pétrequin's recent JADE project, or for example to the Irish Axe Project (see references below).
Thanks, I've added the Petrequin reference.

To conclude, I think that this chapter should be published and it will be a useful contribution to the field. The specific study of hafting remains understudied in archaeology and merits much more serious attention and research. Buckley points to the European studies of well-preserved hafts and stone tools, and I would add that there is also significant research being done on use-wear of hafted segments of stone tools. I think that this study will help to promote such studies and to continue the dialogue between the recent ethnographic past and the more ancient archaeological past.

The book by Hampton on the Dani manufacture and use of stone axes/adzes is not cited which surprised me. It is a very interesting book, full of ethnographic information on this technology. Perhaps the author should cite/consult it.
Hampton, O.W. "Bud" 1999 Culture of Stone: Sacred and Profane Uses of Stone among the Dani. College Station: Texas A&M University Press.
I am glad the reviewer has mentioned this book. Though it was perhaps not clear from the first draft, my chapter is intended to be part of a book that describes Hampton's collection of stone tools and other artifacts from the Baliem Valley region of Papua, putting them in context. His work will be more thoroughly discussed in other parts of the book.

Hampton passed away in 2017. His collection was recently donated by his family to the small non-profit organization that I work for (Tracing Patterns Foundation). We have conserved, photographed and re-housed the collection, and we are in the process of preparing a catalog, of which this essay will form part. We recently signed an MOU with the Museum Lokabudaya at Cenderawasih University, who have agreed to accession the collection, and we hope to transfer the collection to the Museum during 2023.

They have an extensive collection of Asmat material and a limited amount of Baliem Valley material culture, so it should complement their existing collections quite well.

Other references that may be useful.

Pétrequin, Pierre, Gauthier, Estelle et Pétrequin, Anne-Marie 2017 Jade : objets-signes et interprétations sociales des jades alpins dans l'Europe néolithique. Besançon: Presses universitaires de Franche-Comté, Centre de recherche archéologique de la vallée de l'Ain.

Cooney, Gabriel, and Stephen Mandal 1998 The Irish Stone Axe Project. Dublin.

Sheridan, Alison, Gabriel Cooney, and Eoin Grogan 2014 Stone Axe Studies in Ireland. Proceedings of the Prehistoric Society 58(1):389-416.