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– ARTICLE –

States Without Archaeological Correlates? A report from Hawai'i

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ABSTRACT

Two recent archaeological narratives of ancient Hawaiian society apply a neo-evolutionary approach to political development to argue that a primary state evolved prior to contact with Europeans in the late 18th century. Our analysis demonstrates that this finding is based on interpretations of indigenous oral traditions and contact-period historical accounts but lacks archaeological warrant. The Hawaiian archaeological record does not yield the conventional neo-evolutionary correlates of statehood. Moreover, archaeological evidence for the neo-evolutionary model of ladder-like transformation is also lacking. A chronological analysis of Hawaiian political development inferred from the archaeological record reveals that it was a seamless process, with no evidence of a disjuncture when a statehood event might have occurred. We advocate a historical approach to investigating political development in Hawai'i that articulates directly with the archaeological record, and is sufficiently developed and general to be applicable elsewhere in the world.

Keywords: Hawai'i, archaeology, theory, method, states

INTRODUCTION

Archaeology's decades-long project to investigate traditional Hawaiian political development within a generalized neo-evolutionary framework (sensu Jennings 2016: 41–44) culminated in two significant and well-received book-length treatments: Kirch's (2010) *How Chiefs Became Kings* and Hommon's (2013) *The Ancient Hawaiian State*. The two books are similar in many ways. They each augment data taken from the Hawaiian archaeological record with a variety of other source material. Interpretations of indigenous oral traditions, contact-era observations made by visitors to the islands, historical accounts written by native Hawaiians and others, and ethnological comparisons with societies elsewhere in Polynesia are conspicuous parts of both narratives. Neither narrative is primarily archaeological. Rather, they each develop and amplify the comparative anthropological foundations laid in the mid-twentieth century by Sahlins (1958) and Goldman (1955) primarily with interpretations of traditional histories derived from Hawaiian oral traditions. True to their neo-evolutionary roots, both narratives echo Carneiro (1970) and many others (e.g., Fried 1967; Haas 1982; Spencer 2003) in assigning population pressure and environmental circumscription a central role in ancient political development. Indicative of their reliance on traditional histories, both stress the decisive role played by warfare. Moreover, both Kirch (2010) and Hommon (2013) conclude that the Hawaiian case is an example of primary state formation, bolstering the arguments of scholars who classify Hawai'i as a state (e.g., Allen 1991; Hommon 1976, 1986; Seaton 1978; Trigger 2003: 94) and arguing against what they both see as the dominant narrative produced by archaeologists who claim that Hawaiian political development culminated in chiefdoms (e.g., Cordy 1981; Kirch 1984, 1985; Sahlins 1958; Service 1962). Their arguments also run counter to interpretations of historical accounts that posit contact with Europeans and Americans in the late eighteenth century instigated characteristics of statehood (e.g., Andrade 2008: 69; MacKenzie 1991; Osorio 2002: 83). Finally, Kirch and Hommon develop an estimate of when Hawaiian statehood occurred. Their arguments have since been accepted by many regional researchers working from a variety of theoretical perspectives (e.g., Earle 2012; Earle and Spriggs 2015; Field *et al.* 2011; Gill *et al.* 2015; McCoy 2014, 2018).

These narratives have profound implications for anthropological archaeology: states in Hawai'i have allegedly been documented, yet the archaeological correlates that are construed to typify ancient states worldwide are inferred by ethnographic analogy to flesh out a fragmentary material record (Yoffee 2005: 23). Archaeological evidence of urbanism and elite control of craft production by attached specialists are absent in Hawai'i (e.g., Bayman 2018: 3–5, 7–8; DiVito *et. al.* 2019; Jennings & Earle 2016: 482–483; Kirch 2010: 167; Lass 1994: 65; P. McCoy *et al.* 2015: 4). The

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widespread distribution of lithic material from the Mauna Kea adze quarry on Hawai'i Island implies that it was a common-pool-resource (*sensu* Bayman & Sullivan 2008) that was shared by elites and non-elites throughout the islands (Lass 1998: 25; cf. Kirch *et al.* 2012: 1060). Moreover, the intensity and geographic scale of durable goods (e.g., lithic material) circulation in ancient Hawai'i was surpassed by societies in the American Southwest (e.g., Bayman 2018: 4–5, on Hohokam) and the US Southeast (e.g., Sassaman 2005, on Poverty Point). In such regions, raw materials (e.g., marine shell, minerals, obsidian) and crafted goods (e.g., turquoise beads, copper objects) were transported great distances and across hundreds of kilometers.

Because archaeological correlates of the institutional characteristics of early, archaic states – endogamous class stratification and divine kingship, craft specialization and wealth finance, state cults and ritual cycles, standing armies and conquest warfare (see discussion below), and royal residences and palaces – are equivocal or altogether lacking in the Hawaiian archaeological record (e.g., Lass 1994: 66–67; McCoy 2018: 242, 264; Mills *et al.* 2011; Wolforth 2005), they are frequently invoked with oral traditions (e.g., Hommon 2013; Kirch 2010). Where such correlates are apparent in the archaeological record of Hawai'i, such as those associated with monumentality (e.g., see Kirch *et al.* 2012; Kolb 1994) or differential mortuary treatment (e.g., Donham 2000: 818–819), they are incommensurate with the kind, or magnitude, that would distinguish them from chiefdoms or other non-state societies (as confessed by Hommon [2013: 260]). For example, some monumental constructions among societies elsewhere in the Pacific (e.g., Nan Madol, Pohnpei) and North America (e.g., Mesa Grande, Arizona; Cahokia, Illinois) are more massive than Hawaiian temples (Athens 1983; Bayman 1998: 5–6; Fowler 1975; Hommon 2013: 260). Archaeological evidence of differential mortuary treatment in ancient Hawai'i is also enigmatic: grave good assemblages in Hawai'i typically reflect economic activities (e.g., fishing, woodworking) rather than social stratification, and there is a dearth of high-status males in the best-studied mortuary assemblage on Maui (Donham 2000), an island where an ancient state allegedly developed (i.e., Kirch 2010; Hommon 2013). This pattern conflicts with interpretations of ancient Hawaiian states that were ruled by powerful males. Accordingly, Donham (2000: 8.19) concludes that rigid class formation (i.e., elites versus commoners) is not supported by the archaeological record.

In the absence of conventional archaeological correlates of statehood, archaeologists in Hawai'i have struggled to ascertain states in the material record. Their use of oral traditions to fill this gap, which was necessary in the absence of suitable archaeological data, masks the inability of archaeology to estimate when statehood occurred and it highlights the need for advances in theory and method. We argue that the struggle to ascertain states in the archaeological record of Hawai'i has not yet been successful, and that the pursuit of archaeological evidence of a statehood event encouraged by neo-evolutionary theory is misguided.

Neo-evolutionary archaeologists attach importance to the question of *when* Hawai'i became a state, framing statehood itself as an event that archaeologists might investigate. This direct approach ignores the fact that the state, however theorized, is an abstract notion (Abrams 1988) that leaves no direct trace in the archaeological record. The complaint that neo-evolutionists (and some of their staunchest critics) identify ladder-like stages of political development defined by bundles of characteristics, such as states, cities, and civilizations that did not appear synchronously is germane to our discussion (Jennings 2016). We concur with Jennings (2016) and argue instead that events must be situated in the archaeological record where the processes that belong to them are open to inquiry with archaeological methods and materials. Because 'the state is always in a state of becoming' (Pauketat 2007: 40), attempts by archaeologists to identify a statehood event in Hawai'i and elsewhere in the world will prove unproductive (Spriggs 1988: 57). The proper archaeological study of political development is dynamic; it eschews the question of *when* Hawai'i became a state to instead focus on the process and tempo of political development that generated the complex multi-island polities witnessed by Europeans in the late eighteenth century and ensuing decades of the early contact-period (see Earle 2012: 97).

ARCHAEOLOGICAL PROCESS AND EVENTS

We argue that events must be situated in the archaeological record so the processes that belong to them are available to inquiry with archaeological methods. Bayesian modeling operationalizes event and process by building on an understanding of archaeological events that was developed, in part, because the 'archaeological study of 'process' – processes of technological and sociocultural stability, change, and evolution, processes of adaptation to changing environmental conditions, and so on – is predicated on the accurate measurement of change through time' (Dean 1978: 223).

This contrasts with an understanding of events adopted by some archaeologists from the historian's distinctions among short-term history of events, the medium-term history of economic cycles, and the long-term history of population and subsistence forms. For example, in Hawai'i the historian's distinctions are manifest as a contrast between 'long-term, deep time processes' interpreted as ultimate causes and 'short-time, historically contingent events and actions' interpreted as proximate causes by Kirch (2010: 178). This approach based on the historian's narrative device posits events an archaeologist might wish to reconstitute, but neglects to link them to the archaeological record. An alternative is offered by Bayesian chronological modeling, a method that allows estimation of the timing of historical events using archaeological data.

In practice, the Bayesian calibration software applications commonly used by archaeologists, such as BCal (Buck *et al.* 1999) and OxCal (Bronk Ramsey 2001), yield age estimates for events. The events reported by a Bayesian calibration typically include each dated event, the events marking the start and end boundaries of each phase, and events specified as floating parameters in the chronological model. Any of these can estimate an event the archaeologist wishes to reconstitute depending on the nature of the dated materials and the structure of the chronological model. By and large, archaeologists have used Bayesian calibration to yield more precise estimates for events than was previously possible and to gain estimates for events that could be modeled but not directly dated (e.g., the onset of deposition in a particular area that represents past occupation). In cases where archaeologists are able to assign events in a Bayesian calibration to a common past process, then an alternative view of the calibration results called a tempo plot can be constructed (Dye 2016; DiNapoli *et al.* 2020). The tempo plot shifts the focus from estimation of an event to estimation of activity at a point in time, using the calibration results to estimate how many events occurred by that time, regardless of which events had occurred. The tempo plot is constructed by carrying out this estimation for very many points in time, typically each year of an archaeological sequence, to yield an estimate of the tempo of change that makes no reference to particular events. It describes a process as it is 'going on,' rather than how it looks after it is 'gone over' (Stout 2003).

Our review and reinterpretation of the archaeological evidence that might be cited in support of Hommon (2013) and Kirch's (2010) arguments for Hawaiian statehood situates events in the archaeological record and interprets Bayesian calibration results as ongoing processes with a series of tempo plots. Such plots reveal that Hawaiian political development was a seamless process, with no evidence for when in the process of political development a statehood event might have occurred.

CRITIQUING NARRATIVES OF THE HAWAIIAN STATE

Within the context of their neo-evolutionary similarities, Hommon (2013) and Kirch's (2010) narratives differ from one another in interesting ways. The argument for statehood put forward by Hommon (2013) represents the culmination of a distinguished career spent developing and refining that argument (Hommon 1986). The book is divided in four parts, the first three of which i) describes Hawaiian society in the early contact period using primarily ethnographic and historical records, and is sometimes illustrated by archaeological materials, ii) reviews political theory to make the argument that the ethnographic material of the first part satisfies a reasonable definition of statehood, and iii) compares early contact period Hawai'i with ethnographic data from other Polynesian societies. The fourth part, The Model of Hawaiian State Emergence, 'is based primarily on archaeological evidence of Hawai'i Island's agricultural development and an account of Hawaiian political history drawn from traditional sources' (Hommon 2013: 203). Here, archaeological materials provide a chronological framework within which Hawaiian traditions are interpreted. The archaeological materials are coupled loosely with the interpretations of traditions to yield a plausible narrative. The relationship of the plausible narrative to the archaeological record is expressed in a series of conjectures and untested hypotheses rather than a more structured test that might expose the narrative to the possibility of refutation.

Hommon (2013) defines 'state' generically, to include primary and secondary states, as well as archaic and modern states. His definition is logically intensional and practical, pointing to a legitimate government's practices of 'the collection of taxes, the conduct of state rituals, the promulgation and enforcement of laws, the development of public works, the maintenance of intrapolity order, and the management of extrapolity relations by means of trade, diplomacy, and war' (Hommon 2013: 121).

The statehood event is described in the context of traditions, without reference to realized or potential archaeological correlates: i) a sixteenth century O'ahu Island chief 'temporarily transcended the limits of his office by leading the defense of his island' (Hommon 2013: 259), ii) early seventeenth century accounts from Hawai'i Island provide evidence for 'institutionalization of coercive rule' (Hommon 2013: 259), and iii) 'explicit examples of delegation of central political power' (Hommon 2013: 259) date to the early eighteenth century on O'ahu Island. The traditions of delegating central political power, which were estimated to date to around AD 1720, 'appear to be the earliest definitive indications of a Hawaiian polity organized as a state' (Hommon 2013: 259).

In contrast, Kirch (2010: 6) reverses his earlier assessment to assert that Hawaiian political development that he once believed culminated in chiefdoms (e.g., Kirch 1984) instead instigated the rise of a particular kind of state, an 'archaic state,' which is defined by six traits: i) an endogamous ruling class; ii) a divine king; iii) a centralized political economy; iv) a legitimizing state cult and formalized temple system coordinated by full-time priests; v) a monopoly of force via a standing army; and vi) elite residential quarters (palaces) for a king and his court, and the enjoyment of sumptuary privileges that were supplied by full-time craft specialists. His statehood narrative identifies archaeological evidence 'revelatory to the emergence of archaic states in pre-contact Hawai'i: population and demographic change, the development and intensification of agroecosystems, and the archaeological record of monumental architecture' (Kirch 2010: 126). These data anchor an influential cultural sequence that was first set out in the context of chiefdom development (Kirch 1985: 298–308) and modified to accommodate new data and advances

in interpretation, particularly of ^{14}C dates. The cultural sequence posits rapid changes in population, along with investments in agriculture and monumental architecture in the periods spanning AD 1200–1650, followed by a Protohistoric Period from AD 1650–1778 characterized by a stable population, secondary intensification of established agricultural systems, and endemic warfare (Table 1). In contrast to the Expansion Period(s) when changes were widespread and ongoing, the Protohistoric Period is relatively static: absent a growing population, agricultural development is limited to 'secondary intensification' and investment in monumental architecture declines to insignificance. In this sequence, the Hawaiian 'archaic state' emerged at the beginning of the Protohistoric Period around AD 1650.

We consider the archaeological data for the four primary drivers identified by Kirch (2010:216): population and demographic change, warfare, development and intensification of agroecosystems, and monumental architecture.

Population and demographic change

Population growth is considered an ultimate cause and 'one of the fundamental changes underlying the historical transformation of Hawaiian society' (Kirch 2010:128). Initially, demographic history was inferred from counts of dated houses. Nevertheless, mounting evidence suggests that the Hawaiian settlement pattern is one in which houses with a limited use life were abandoned and the stones from their dry laid masonry foundations re-used for building projects elsewhere (Dye 2010:140–142). The implication of this recycling behavior for the house count method is clear – houses from all periods are not available for investigation. A view of house construction events dated with ^{14}C collected from beneath their foundations (Dye 2010:110–119) shows this problem. The oldest well-dated house was constructed at least five centuries after the islands were initially colonized and most of the houses recorded by archaeologists were built in the century or two before the countryside was largely abandoned following European contact (Figure 1). The absence of charcoal

Table 1. *Hawaiian cultural sequence proposed by Kirch (2010:128).*

Period	Date	Description
Foundation	AD 1000–1200	Small founding populations in salubrious cores
Early Expansion	AD 1200–1400	Exponential increase in population, development of irrigated agriculture
Late Expansion	AD 1400–1650	Population growth peaks and stabilizes, development of rain-fed agriculture, investment in monumental architecture
Protohistoric	AD 1650–1778	Stable population, secondary intensification of rain-fed agriculture, endemic warfare

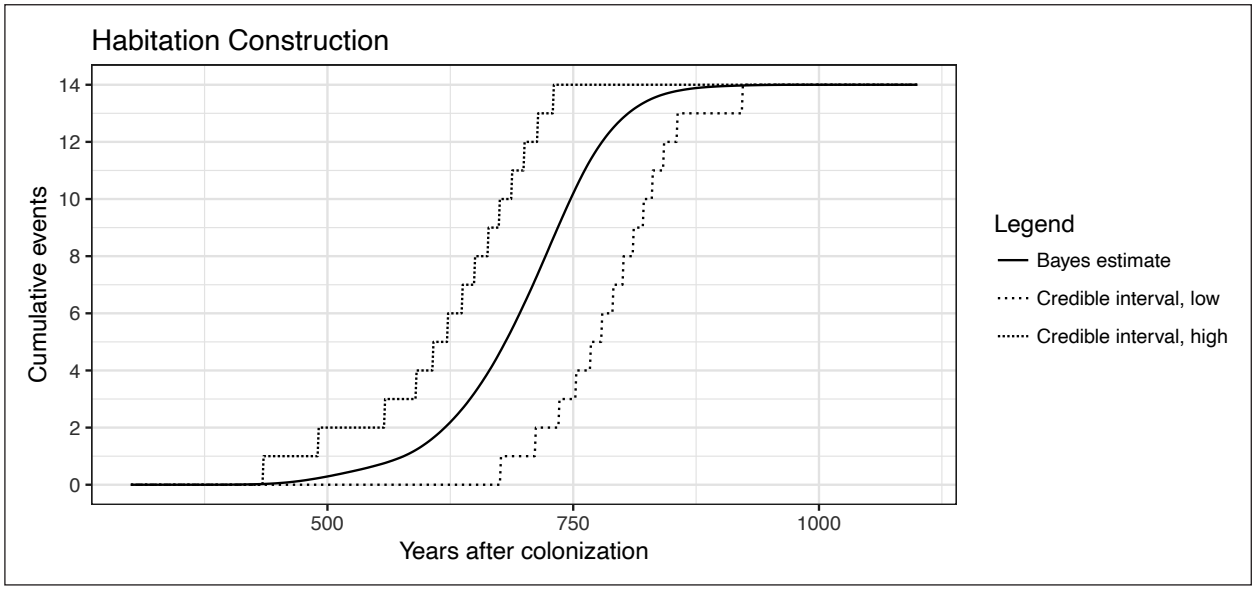

Figure 1. Tempo of dated habitation construction events plotted along an axis beginning with Polynesian colonization of the Hawaiian archipelago. Sources: Dye (2010a), Rieth and Filimoehala (2015), and Dixon et al. (2004). The Bayesian estimate and 95% credible intervals were drawn with the ArchaeoPhases software (Philippe and Vibet 2017).

samples from older dismantled houses creates an event horizon for the house count method in the seventeenth century. The house count method, conscientiously applied, might provide population information for a brief period near the end of the Hawaiian sequence. Claims that it tracks population through the entire Hawaiian sequence lack evidential support.

By the 1990s archaeologists in Hawai'i were attempting to investigate demographic history with a method that uses 'dates as data', rather than counting houses. The conceptual basis of the dates as data method is captured by the assertion that 'if archaeologists recovered and dated a random, known percentage of the carbon from a perfectly preserved carbon deposit to which each person-year of occupation contributed an equal and known amount, they could estimate the number of people who inhabited a region during a given period' (Rick 1987: 56). Deviations from the rather strict requirements of the method are recognized as investigation bias, which violates the requirement of a random sample, preservation bias, which violates the perfect preservation requirement, and creation bias, which violates the requirement that each person-year contribute an equal amount of charcoal to the deposit.

The first application of the dates as data method in Hawai'i summarized 598 mostly unidentified wood charcoal dates from putative domestic contexts, which were chosen in an effort to control for the various biases associated with the method and to ensure isomorphism with population (Dye & Komori 1992). Care was taken to exclude charcoal samples associated with agriculture 'because there is a high probability that more charcoal was produced during the initial clearing of virgin forest than during subsequent clearings of second growth' (Dye & Komori 1992: 117). The summed calibrated probability distribution showed a peak in the mid-fifteenth century followed by a population decline into the historic period.

Subsequently, two crippling criticisms have undermined this population history estimate, both of which concern uncontrolled creation biases (Dye 2010: 142–144). The population estimate identified domestic contexts with excavations at habitation sites, on the false assumption that the excavated deposits were associated with the domestic architecture. The best evidence that the assumption is false comes from 219 test pits excavated at Kahikinui on Maui Island, which yielded an unparalleled view of the stratigraphic position of surface architecture across the archaeological landscape (Dixon *et al.* 2000). In all but seven instances, the test pits proved that surface architecture was constructed on an existing cultural deposit. In the vast majority of cases, most of the cultural material had been deposited by the time the surface architecture was constructed. In fact, in the median case 86 percent of the cultural deposit was beneath the base of the surface architecture. Charcoal that pre-dates the domestic architecture cannot be associated with domestic activities and thus cannot be used to control for creation bias. Indeed, many of these pre-architectural deposits are likely associated with earlier episodes of forest clearing, which deposited large amounts of charcoal in a short time and represent an uncontrollable creation bias that discredits the population estimate.

The second crippling criticism is that the ^{14}C determinations are mostly on pieces of unidentified wood charcoal that were processed without controlling for the potential effects of in-built age. This inbuilt-age problem is widely recognized as having had deleterious effects on Hawaiian chronology (e.g., Athens *et al.* 2014; Dye 2000; Hommon 2013; Rieth & Athens 2013; Rieth *et al.* 2011); there is no reason why population estimates based on ^{14}C dates with potential in-built age would not be similarly affected.

Nevertheless, Kirch ignores the house count event horizon and the criticisms of the application of dates as data in Hawai'i, suggesting that the demographic history it yields supports the house count histories, at least in 'key respects' (Kirch 2007a: 62). The dates as data approach was subsequently extended to ^{14}C dates uncontrolled for creation bias collected from residential, agricultural, and ceremonial contexts at Kahikinui, Maui (Kirch 2007b: 94–98). Most recently, three other collections of ^{14}C dates on mostly unidentified wood charcoal – two published collections from Kaua'i and Moloka'i islands, and an unpublished collection from Hawai'i Island – have been interpreted as evidence for demographic history using the dates as data approach (Kirch 2010: 135–136). In each case, the claim that the temporal frequency distribution of dates is isomorphic with population size is undermined by uncertainty about the nature of events, either by an inability to identify the dated event or by a failure to distinguish a dated event from a reference event or a target event (Table 2).

Absent a reasoned argument why the various date distributions should be isomorphic with population and unresponsive to the criticisms leveled against the application of both the house count and dates as data approaches in Hawai'i, the claim that the 'population of the islands first underwent an exponential phase of expansion, between about AD 1100 and 1500 … followed by a period in which the rate of archipelago-wide population growth declined, and may have leveled off' (Kirch 2010: 138–139) cannot be sustained. The date distributions it describes might as well indicate the pace of forest clearing. In the future, archaeologists might investigate the population history of Hawai'i using the dates as data method, which yields plausible results in some instances (e.g., Edinborough *et al.* 2017) and can be carried out with sophisticated statistical software (Bevan & Crema 2020). The success of that investigation will be determined by a demonstrated ability to control for investigation, preservation, and creation biases.

Warfare

Neo-evolutionary explanations of endemic warfare in late pre-contact and early post-contact Hawai'i are directly tied to the presumed limitations of rain-fed agriculture

Table 2. *Sources of archaeological materials used by Kirch (2010) to infer demography.*

Reference	Scope	Dating Technique	Method	Ed to Er	Er to Et	Bias Control
Hommon (1976)	Kaho'olawe	Hydration	House count	n/a	No	n/a
Kirch (1984)	W. Hawai'i Island	^{14}C	House count	No	No	n/a
Cordy (1981)	W. Hawai'i Island	Hydration	House count	n/a	No	n/a
Kirch (2010)	Kahikinui, Maui	^{14}C	House count	No	No	n/a
Dye and Komori (1992)	Hawaiian Islands	^{14}C	Dates as data	No	No	Yes
Carson (2005)	Kaua'i	^{14}C	Dates as data	No	No	No
Kirch (2010)	Hawai'i Island	^{14}C	Dates as data	No	No	No
McCoy (2007)	Moloka'i	^{14}C	Dates as data	No	No	No

on the leeward islands of Maui and Hawai'i Island (Hommon 2010: 25, 2013: 253–256; Kirch 2010: 198–199). For Kirch (2010: 198), 'approaching limits to intensification drove the pattern of 'expansionistic warfare and conquest of adjacent political (and economic) territories.' Though aggression, violence, raiding, and (generally limited) warfare are recognized as having a deep history in Hawai'i, and Polynesia more broadly (Hommon 2013: 239), Kirch (2010: 209) suggests a qualitative change in violence as recorded in Hawaiian oral traditions beginning in the late sixteenth century. Beginning around this time, for most wars, territorial expansion was the goal. The ability to wage large-scale war and the gains of successful conquests are seen as partial drivers of state development. Hommon (2013: 255) finds that 'the delegation of political power, rather than warfare, is the underlying organizational principle of state formation,' but that administration of military units may have been a precursor of state bureaucracy.

Archaeological data for warfare play no role in either narrative and both are based on oral and written traditional histories, Euro-American documentation, and ethnography (Bayman & Dye 2013: 91–92). This reflects the fact that archaeological evidence of Hawaiian warfare is limited or ambiguous (Kolb & Dixon 2002; Stokes 1937; Wolforth 2005). Kolb & Dixon (2002) and Wolforth (2005) are the most recent treatments focused on Hawaiian warfare, and both studies attempt to incorporate archaeological data while relying predominantly on ethnohistoric and historical references.

No battlefields have been identified by their physical attributes (e.g., Stokes 1937). Documentation of fortifications includes two notched ridges at Nu'uanu on O'ahu (Emory 1924: 35; McAllister 1933: 88) and a defensible ridge at Kawela on Moloka'i known through oral history as a place of refuge (Weisler & Kirch 1985: 150) (though the fortification of this ridge has not been described and once again oral history seems to be the basis for interpretation). Likewise, archaeologists in Hawai'i often refer to 'refuge caves' used during times of war (e.g., Kennedy & Brady 1997), but have failed to distinguish them from caves used for other ethnographically known functions (Tuggle 2010). The array of wooden, stone, and shark teeth weapons known from ethnographic collections (Buck 1957: 417–585) are largely nonexistent in archaeological contexts (a few exceptions are Emory & Sinoto 1961: 67; Hendren 1975: 147; Soehren & Tuohy 1987: 199–200). Slingstones have been more commonly recovered by archaeologists, though still at low frequencies, and were potentially used in warfare. Yet their use for bird hunting is also indicated in ethnohistoric sources (e.g., Malo 1951: 39), thus their presence is not necessarily indicative of conflict. Osteological evidence of interpersonal violence and perimortem trauma is rare. For a Maui skeletal series with 712 individuals, there was no evidence for ritualized violence or combat (Pietrusewsky *et al.* 1991: 38). Han *et al.* (1986) report a low incidence of trauma among 355 burials from Hawai'i Island, and Suzuki (1993) found 37 cases of some form of trauma among 349 individuals from Mōkapu Peninsula, O'ahu, only a few of which are likely from violence. An exception is a mass grave found on O'ahu that contained the partial skeletal remains of 24 to 34 young adult to adult males, many of which exhibit perimortem trauma in the form of cut marks, chop marks, fractures, and missing elements (Carlson *et al.* 1994). This feature is not securely dated, but it may relate to Kamehameha's post-contact conquest of the island in AD 1795.

Late eighteenth century warfare in Hawai'i is well-attested by traditional and documentary sources. Warfare's antiquity is less certain, throwing into question its role in social change in the centuries prior to contact.

Development and intensification of agroecosystems

The idea that 'contrastive agroecosystems' (Kirch 2010: 140) have differential effects on political development is based on an analytical model that distinguishes predictable and high-yield irrigated agricultural systems from less predictable and poorer yielding rain-fed systems. The model was historicized and humanized for Hawai'i when the development of rain-fed agricultural systems were associated with the eventual rise of king Kalani'ōpu'u and his successor, Kamehameha, who famously united the Hawaiian Islands after European contact in the late eighteenth cen-

tury (Kirch 1984:251–268). In theory, variable yields from rain-fed agricultural systems at times dropped below the level needed to feed the dependent population of farmers, as could happen due to drought (Allen 2004; DiNapoli & Morrison 2017). These circumstances reduced the tribute that elites could collect, leading them to wage wars of conquest with their more affluent irrigation-farming neighbors. According to the theory, the organizational skills gained during warfare and the management of conquered lands spurred political development.

Archaeology's contribution to the theory identified distinctive 'temporal pathways of intensification' (Kirch 2010:143) for irrigated and rain-fed agricultural systems. The early development of irrigated agriculture contrasted with a later development of rain-fed agriculture on the southern islands of Maui and Hawai'i. Summarizing dating programs in rain-fed agricultural fields in Kohala (e.g., Ladefoged & Graves 2008), Kona (e.g., Allen 1992), and Waimea (e.g., Clark & Kirch, eds. 1983) on Hawai'i Island, Kahikinui (Coil and Kirch 2005) on Maui Island, and Kalaupapa (McCoy 2005) on Moloka'i Island, Kirch concluded that 'in all cases the onset of major dryland [rain-fed] cultivation began around AD 1400. Following two centuries of initial development, a final phase of intensification, typically marked by formalized garden plots and territorial boundaries, commenced about AD 1600 to 1650, and continued until the early postcontact period' (Kirch 2010:153). Evidence for depletion of soil nutrients in the sweet potato plots of Hawai'i and Maui Islands (e.g., Hartshorn *et al.* 2006; Vitousek *et al.* 2004) has been interpreted by Hommon (2013:232–233) and Kirch (2010:149–150) as indicative of declining yields and agricultural involution (Geertz 1963).

The argument for agricultural involution has not received much support. It ignores observations made by the first European explorers to Hawai'i that the pig herds raised on the rain-fed agricultural fields were the largest in the Pacific (Dye 2014a). Also, soil scientists interpret soil nutrient depletion more conservatively. One study concluded that rain-fed agricultural soils on Maui produced crops sufficient for local demands over very long time frames (Hartshorn *et al.* 2006). On Hawai'i Island, a similar study determined that agricultural practices might have lowered yields at the upper edge of the leeward Kohala Field System, where high rainfall had already leached nutrients from the soil (Meyer *et al.* 2007). However, soil nutrients within the field system, although measurably lowered by Hawaiian cultivation, were still relatively high and did not entail 'declines below critical levels in overall agricultural potential in the period before European contact' (Ladefoged *et al.* 2018:38). In short, the hypothesis of agricultural involution appears to be a requirement of the neo-evolutionary models developed for Hawai'i that are contradicted by the historical and soil nutrient data.

Monumental Architecture

The characteristic that changed Kirch's mind about the neo-evolutionary stage of Hawaiian political development had to do with 'the emergence of divine kingship' (Kirch 2010:5), a common characteristic of 'some well-known states where texts are available' (Marcus & Feinman 1998:5) and believed to belong to an early, archaic form of statehood. On this account, archaic states are 'detectable through their manifestations of monumentality' (Kirch 2010:128, 175). The temporal development of the Hawaiian temple system in Kirch (2010:157–165) was extrapolated from investigations on Maui Island, which used ^{14}C dates and pioneered the use of ^{230}Th dating in Hawaiian archaeology (Kirch & Sharp 2005). These data were later augmented with a much larger collection of ^{230}Th dates (Kirch *et al.* 2015:166).

The analysis of ^{14}C and ^{230}Th dates from the Kahikinui temples is problematic. First, many of the ^{14}C dates are on unidentified wood charcoal and the adverse effects of inbuilt-age are presumed to affect the chronology of temples. This problem was inadvertently obscured when Kirch (2010) grouped the various ^{14}C dates according to their ages rather than their depositional contexts in the comparison with the ^{230}Th dates. This procedure has the unintended consequence of making it appear that the ^{14}C dates and the ^{230}Th dates corroborate one another, when a closer inspection by context reveals discrepancies (Dye 2016).

Second, the ^{230}Th dates offer an analytical challenge comparable to inbuilt-age for charcoal. The question of a 'storage-age' – a delay between the death of a coral and its deposition in an archaeological context – is overlooked by the assumption that only freshly broken coral was used as an offering in ceremonial contexts. However, the data show several inversions that indicate deposition of old corals (Dye 2016).

Finally, in several instances temple construction dates were estimated by the ages of coral pieces deposited on top of the temple. Because old corals are known to have been deposited at the Kahikinui temples, and a depositional event on the temple surface must be later than the construction event, it is not possible to determine the relationship of the age of the coral to the temple construction event.

These issues call into question the interpretation that the dating evidence documents 'a major phase of temple construction in Kahikinui beginning ca. AD 1550 and continuing until ca. AD 1700' (Kirch *et al.* 2015:166), a period of 150 years (Figure 2). This result, despite its greater range, was interpreted as confirming and supporting the earlier result, reported as AD 1580–1640 and AD 1570–1610 (Kirch *et al.* 2015:166, 174). When the problematic aspects of the temple dating program were controlled in a Bayesian model it was evident that the tempo of temple construction at Kahikinui, Maui, is coincident with the tempo of temple construction in the rain-fed agricultural fields of leeward

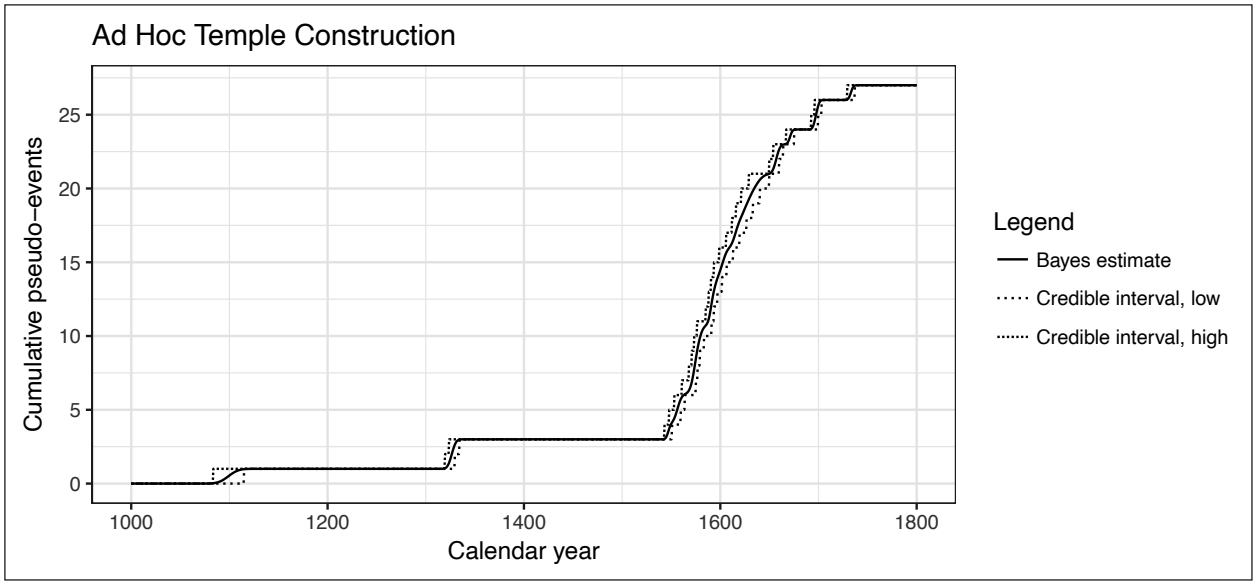

Figure 2. Tempo plot of temple construction pseudo-events in the rain-fed agricultural district of Kahikinui, Maui. Compare the tempo plot to Kirch, Mertz-Kraus, and Sharp (2015: Figure 4). The Bayesian estimate and 95% credible intervals were calculated with the ArchaeoPhases software (Philippe and Vibet 2017).

Kohala, Hawai'i Island (Dye 2016: 6). The tempo of temple construction events yields no indication of when a statehood event might have taken place (Figure 3). Instead, the tempo plot reveals a seamless process during which temples were constructed at a fairly regular pace over a period of two to three centuries. The prediction of the cultural sequence that investment in monumental architecture declined during the Protohistoric Period from AD 1650 to 1778 (see Table 1) is counter-indicated by these results.

DISCUSSION AND CONCLUSIONS

The generalized neo-evolutionary approach in Hawai'i has not advanced archaeological inquiry on state development. The archaeological correlates of states, or ancient states, are not documented in narratives of the Hawaiian state that integrate archaeological records with traditions and historical accounts. The idea of a statehood event, inspired by neo-evolution's ladder-like stages of political evolu-

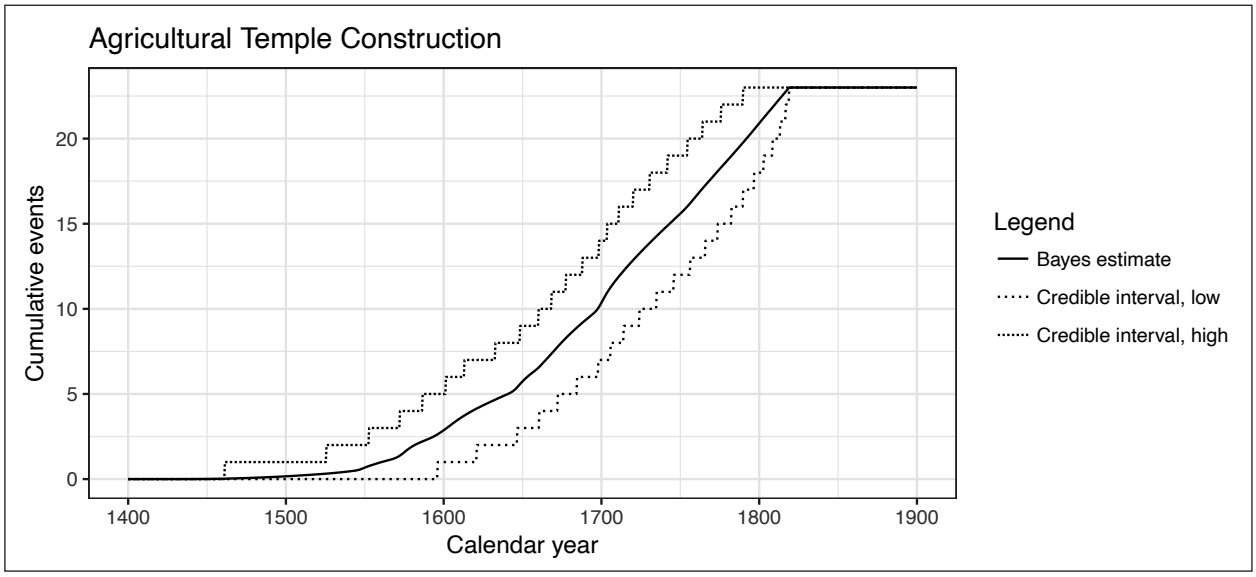

Figure 3. Tempo plot of temple construction events in the rain-fed agricultural districts of Kahikinui, Maui and leeward Kohala, Hawai'i. The Bayesian estimate and 95% credible intervals were calculated with the ArchaeoPhases software (Philippe and Vibet 2017).

tion, has not been established with Hawaii's archaeological materials. Instead, archaeological materials indicate that the tempo of political development was seamless, without the kind of transformative discontinuity implied by neo-evolutionary stages of temporally-synchronous bundled characteristics.

In other respects, neo-evolutionary theory had beneficial effects on archaeological inquiry in Hawai'i, and Hommon and Kirch have made important contributions in this regard Starting in the late 1960s and early 1970s as part of a reaction to a culture historical approach whose primary goal was artifact classification and identification of temporally sensitive artifact types (Dye 1989; Kirch 1985:1–21), the neo-evolutionary project dramatically broadened the scope of archaeological inquiry in Hawai'i. The neo-evolutionary focus urged archaeologists to record the full range of site types and to view the pattern of human settlement (Green 1984). It developed research questions and practices to collect and interpret a wide range of paleoenvironmental data that established the context to identify and chronicle landscape changes that followed human settlement. One early and significant accomplishment of this broadened inquiry was the hypothesis of distinct paths of irrigated agricultural development on the older islands with permanent streams and rain-fed agricultural development on the younger, relatively undissected islands (Kirch 1984).

However, these advances were not matched by successes in drawing historical inferences from the archaeological record. For example, it adopted absolute dating techniques, though unfortunately these were applied without taking into account either the potential effects of old wood or the relationship of the dated events to the events the archaeologist wished to reconstitute (issues that were only realized decades later). This practice led to decades of mistaken inference that Polynesians discovered and settled Hawai'i early in the first millennium AD, many centuries earlier than the current consensus settlement date in the eleventh century AD (Athens *et al.* 2014).

Perhaps the best example of the limitations of neo-evolutionary theory in Hawai'i involves the statehood event, where for many years Hommon (1976, 1986, 2013) argued that state institutions developed in spite of the absence or weak development of the usual archaeological correlates of statehood, while Kirch (1984, 1985) argued that political development in Hawai'i resulted in chiefdoms. In our view, this focus on the statehood event was unfortunate because the state is an abstract notion that leaves no direct trace in the archaeological record. Interest in the statehood event, despite its lack of fit with archaeological inquiry, was seen as important because the classic neo-evolutionary formulation classified chiefdoms as a form of 'primitive' social organization and the state as the lone example of 'civilization' (Service 1962:174).

The difficulty of drawing historical inferences restricted use of the archaeological record to conveying a 'pessimistic and even tragic version of cultural evolution that sees population growth and other factors constraining cultural change to take place along lines that most people do not regard as desirable' (Trigger 1989:367). In Hawai'i, this pessimistic turn associated population growth with resource depletion, agricultural involution, susceptibility to environmental disaster, and other 'hard times' leading to endemic warfare. Yet, as we have seen, archaeologists in Hawai'i have unsuccessfully investigated population history, over-interpreted equivocal evidence of resource depletion, and can offer no convincing archaeological evidence for the warfare hypothesized to have driven political development.

Current estimates of settlement sometime in the eleventh century AD reduce the plausibility of the pessimistic version of cultural evolution. The 700–800 year sequence is about half what it was believed to be when the pessimistic version of cultural evolution was developed (e.g., Kirch 1984), leaving little time for population to reach carrying capacity in one of the largest island groups in eastern Polynesia. Without population pressure on resources, an ultimate cause of 'the transformation of a classic Polynesian chiefdom into an emergent archaic state' (Kirch 2010:178) is removed. The lack of strong evidence for resource depletion posited by neo-evolutionary theory can be explained as a circumstance that did not develop in the short time Hawai'i was settled, rather than a failure of archaeological inquiry.

The question of warfare is more complex. This requirement of some models of neo-evolutionary theory is well-attested in Hawaiian tradition, but has left little trace in the Hawaiian archaeological record (Kirch 1990:339; Stokes 1937), which lacks the fortifications and mass burials that characterize most ancient states (Keeley 1996:55). Indeed, cross-cultural analyses confirm the lack of a correlation between the frequency of warfare and density of human population (Keeley 1996:118). One possibility for this discrepancy between Hawaiian tradition and the archaeological record is that the nature of warfare in pre-contact Hawai'i potentially differed from post-contact warfare in the islands. In either case, it is notable that unlike Hawai'i, warfare is strikingly visible in the archaeological records of non-state societies such as pre-contact New Zealand (e.g., McCoy & Ladefoged 2019).

Finally, completion of the neo-evolutionary project in Hawai'i leaves archaeologists with the question of how best to study the past without relying on ladder-like stages of social and political complexity. Two hypotheses on the endpoint of processual development indicate the range of possibilities. On one hand, Hawaiian society is argued to be one among many in which 'rudimentary classes were superimposed on hierarchically structured kinship networks or ethnic groups, with kinship remaining the basis of sociopolitical organization' (Trigger 2003:47). On the other, the assertion that collective rights of land control were completely eroded in Hawai'i rests on the claim that at some point in time 'all land became the property of the

conquering ruler' (Jennings & Earle 2016: 483).

We argue that successful inquiry of this developmental spectrum requires a strong culture historical base, where objects and events identified in the archaeological record are located accurately in space and time. This sort of approach can be applied just as easily to the archaeological objects and events deposited by ancient commoners, such as house foundations, fire pits, garden features, etc., as the objects and events deposited at the direction of elites such as temples or taboo food remains. Visual display tools that impart a processual view, such as tempo plots, can provide evidence of continuity and change for interpretations that are couched in terms of social processes that articulate directly with Hawai'i's archaeological record. In this way, archaeology can enhance its contribution to a study of the past that considers but does not foreground interpretations of tradition, comparative ethnology, linguistics, and historical accounts.

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Supplemental Material

The supplementary materials are available online at *ScholarSpace* at the University of Hawai'i-Mānoa; the materials can be accessed using the following unique identifier: http://hdl.handle.net/10125/62038.

– FORUM –

Invited Comments on: *States Without Archaeological Correlates? A report from Hawai'i*

Robert J. Hommon[1]

Bayman, Dye, and Rieth assert that their 'chronological analysis of Hawaiian political development inferred from the archaeological record reveals that it was a seamless process, with no evidence of a disjuncture when a statehood event might have occurred.'

Their conclusion is based on a belief that a 'statehood event' between AD 1400 and 1800 would have disrupted the lives of Hawaiians to a degree sufficient to have been reflected generally in the archaeological record. An article by Beck *et al.* (2007) discusses the selective visibility of such events, in which social transformations would be reflected not generally but only in the specific archaeological contexts involved in the event. The present article, however, focuses on the lack of observable discontinuity in the archaeological record of the construction of 11 temples in Kohala District, Hawai'i Island, all or most of which likely served as community *heiau* rather than polity *heiau*. (Hommon 2013: 90–98, 248–249; McCoy *et al.* 2011; Mulrooney & Ladefoged 2005; Stokes 1991: 165–181).

It appears unlikely that the authors' analysis is capable of reliably distinguishing between archeological records with statehood events and those without, because the data that they analyse are not appropriate to the task. To recognise such an event, focus must be on those elements, such as the major *heiau* and primary central places that would have been transformed.

Their analysis appears to depend on several misleading assumptions:

- first, that a statehood event would have resulted in a general disruption in community activities;
- second, that such a disruption would be expressed in ways that can be detected as a disjuncture in the archaeological record that they examined (Kohala temple construction data); and
- third, that an absence of a disjuncture in evaluated data demonstrates the absence of a statehood event.

I suggest that rather than demonstrating the absence of a statehood event in Hawai'i, there are other plausible ways to interpret the failure of the authors' analysis to detect evidence of disjuncture in the Hawaiian archaeological record. First, the data chosen for analysis are insufficient to detect the degree or form of disjuncture generated by a statehood event. Here, I suggest that a careful consideration of the historical record would aid in the identification of the specific markers of the event requiring investigation. Ignoring the rich trove of accounts by participants in and observers of Hawai'i's primary states seems to be a major flaw in their argument. Second, regardless of the level of sensitivity of the analysis, a statehood event did occur but, because of the resiliency of Hawaiian communities, it resulted in insignificant disruption and therefore does not appear as a discernible disjuncture.

I suggest that the failure to identify a disjuncture in the Hawaiian archaeological record is based on the scalar effects of such a transformation. In other words, changes would have been additions to rather than replacements of institutional operation. For example, despite remarkable changes (and high visibility in state centers and religious monuments) of Inca imperial conquest in the Andes, the changes in community life were remarkably minor (D'Altroy & Hastorf 2001). Hawaiian communities and households may well have been resilient in the face of whatever disruption the statehood event caused, resulting in little discernible disjuncture in the archaeological record.

If the emergence of the state took place much as I describe in *The Ancient Hawaiian State,* then the lives of the great majority of the population may have continued relatively unchanged. The most significant changes were probably experienced by the elites, during the transition from chiefly hierarchy to bureaucracy

I suggest that failure to consider relevant information in the detailed Hawaiian historical record increases the likelihood that application of the authors' analysis could yield both false positive results (findings of statehood events where none occurred) and false negative results (findings of no statehood events where they actually occurred, as in the case of the Kohala temples discussed in the article).

The study of early states in other regions continues to benefit from the vast body of the traditional and ethnohistoric historic resources of the Hawaiian primary states. Yet the authors' desire to develop an approach that is solely limited to the archaeological record is understandable since, of all the known instances of emergence of primary states – those that arose spontaneously, uninfluenced by preexisting states – only Hawai'i and Tonga were described in participants' and observers' accounts. Rather, I suggest

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that the application of historical data should help us understand how archaeological research must be framed to investigate events related to institutional change.

Bayman *et al.* close their discussion with the observation that archaeology, surely among the most effective of fields in applying multidisciplinary approaches, 'can enhance its contribution to a study of the past that considers but does not foreground interpretations of tradition, comparative ethnology, linguistics, and historical accounts.' While I agree with their view that archaeology is the best way to investigate primary states worldwide, our most valuable resource in support of this aim is the large, diverse body of eye-witness accounts by participants in and observers of the Hawaiian (as well as Tongan) primary states, unique in the world. These sources offer the models for how archaeological research can be designed to study such events.

I close with two observations on the study of primary states. First, it is common to find various authors, no matter their view on primary states, favoring diverse lists of identifying elements of such states. My list for primary state societies, for example, emphasises centralised government, delegation of power, taxation, stratified bureaucracy, and conquest warfare, all of which are common in states, but rare or absent in non-states. States are not all the same; their historical variation should always be considered. My second point is that in all examples except those in Polynesia, descriptions of the earliest emergence of primary states depend largely on backwards projections from later, fully literate societies (Jennings & Earle 2016: 475, 485).

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Patrick V. Kirch[2]

A core premise underlying Bayman *et al.*'s critique of *How Chiefs Became Kings* is that the only valid methodological approach, and the only acceptable evidence, for the emergence of archaic states in ancient Hawai'i is that provided by archaeology, i.e., material remains in dated contexts, preferably calibrated using Bayesian modeling. As they state, 'We argue that events must be situated in the archaeological record so the processes that belong to them are available to inquiry with archaeological methods' (p. 48). For them, any events that occurred in history must be documented archaeologically to be accepted as part of the historical record. Traditional oral narratives (*mo'olelo*) of the Hawaiians themselves are evidently not acceptable to Bayman *et al.*, for their critique in large part centers on the use of these non-Western forms of historical documentation by myself and by Robert Hommon. Similarly, arguments grounded in detailed comparative analysis of Hawaiian with other Polynesian societies carry no weight with Bayman *et al.*, even though in my view such comparative study is essential to understand precisely how Hawai'i departs from other Polynesian cultural patterns.

I make no apologies for the *holistic* theoretical and methodological approach that I bring to historical anthropology in Hawai'i and to Polynesia more generally. While I am an archaeologist, and archaeological data are always in the forefront of my work, I have consistently sought to employ evidence from sister branches of anthropology when these are relevant to my research problems: historical linguistics, comparative ethnography, oral traditions and folklore, even bioanthropology may contribute to the resolution of research questions I have addressed. My theoretical and methodological approach is spelled out in great detail in the work that I co-authored some years ago with Roger Green, *Hawaiki, Ancestral Polynesia: An Essay in Historical Anthropology* (Kirch and Green 2001). A core tenet is the use of 'triangulation,' adducing evidence from multiple subfields of anthropology and allied disciplines to refine our understanding of history.

I reject Bayman *et al.*'s claim that I have failed to document the transformation of Hawaiian chiefdoms into archaic states, simply because I have not met *their* stated standards of strictly archaeological evidence. I never said that archaeological evidence alone demonstrates the process of socio-political transformation in Hawai'i. It was precisely because tracing this transformation requires the use of non-archaeological evidence, in particular that of indigenous Hawaiian oral narratives, that in *How Chiefs Become Kings* I devote an entire chapter to a careful analysis of the oral narratives. The dating of events documented in

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the *mo'olelo* is not the same as with archaeological remains (primarily ^{14}C dating); the former are tied to chiefly genealogies that yield a relative sequence of events. Nevertheless, as I show in some detail in a later work, *A Shark Going Inland Is My Chief* (Kirch 2012), the sequence of individuals and the chronology of events recorded in the *mo'olelo* pertaining to the past 20 or so chiefly generations can be correlated with the archaeological record in a sometimes surprisingly clear manner. I also find it curious that Bayman *et al.* refer to a 'statehood event', a term I do not use; my view is that the sociopolitical transformation that took place in Hawai'i was a *process*.

Bayman *et al.* try to pigeonhole me in the 'neo-evolutionary' paradigm of 20th century anthropology, an assertion that I likewise reject. That claim might be made with respect to my writings of 35 years ago, such as *The Evolution of the Polynesian Chiefdoms* (Kirch 1984), but not of *How Chiefs Became Kings*. Neo-evolutionists argue for the primacy of processual factors such as environmental adaptation or population pressure as 'prime movers' of cultural change. While I still regard population growth and agricultural intensification as 'necessary conditions' to state emergence, I was at pains in *How Chiefs Became Kings* to stress that any explanation of historical transformation in ancient Hawai'i requires equal attention to social dynamics and *individual agency*. Hence, once again, the importance of drawing upon the indigenous oral narratives, as these offer an important emic or 'insider' perspective on history, one that balances an etic, processual perspective. Pi'ilani and Kiha-a-Pi'ilani on Maui, Līloa and 'Umi on Hawai'i, made Hawaiian history what it is. They acted within the constraints imposed by the times they lived in, but they also changed the trajectory of Hawaiian history. That we know their names and something of what they did is owed to the *mo'olelo*, not to Bayesian-calibrated tempo plots.

Bayman *et al.* erroneously suggest that it is the early, neo-evolutionary Sahlins of *Social Stratification in Polynesia* (1958) that influences my perspective on Hawaiian history. On the contrary, it is Sahlins the structuralist-historical anthropologist, the author of *Islands of History*, and most particularly the Sahlins with whom I collaborated in the Anahulu Valley project (Kirch and Sahlins 1992), who inspired me to write *How Chiefs Became Kings*. It was in particular what I called 'Marshall Sahlins's challenge' (Kirch 2010:11–13) that sent me on my quest to understand the emergence of divine kingship and archaic states in precontact Hawai'i. Hardly neo-evolutionary!

There are many specific assertions or claims in Bayman *et al.* that I take issue with, but the limitations posed on this brief commentary preclude discussing them here. I will comment only on the dating of monumental architecture – specifically of temple or *heiau* sites – on the island of Maui. In my 2010 book, I presented a corpus of 41 radiocarbon dates from 31 *heiau* in Kahikinui district (Kirch 2010, fig. 4.9), arguing that an early set of 16 dates 'demonstrate the rapid imposition of the Kahikinui temple system' between *ca.* AD 1450 and 1600, followed by another set of 22 dates that 'indicate continued construction, rebuilding, and use of temples from about AD 1600 until the early postcontact era' (2010:162). As Bayman *et al.* point out (p. 52), there was a potential problem with this data set, which includes charcoal samples obtained by Michael Kolb that were not identified to taxon and thus may include some dates with an unknown amount of in-built age. It is also important to note that most of these samples are from temple *use* contexts, not from contexts that can be unambiguously associated with initial temple *construction*.

More recently, in my monograph on the *heiau* of Kahikinui and Kaupō districts, I present a refined set of 35 radiocarbon dates from 23 *heiau* (Kirch and Ruggles 2019, fig. 3.13). This set excludes all of the Kolb dates, including only samples excavated by myself or Alex Baer, all botanically identified and run on short-lived taxa. One very early date is from a small shrine, and one other date from a basal context pre-dating *heiau* construction can be eliminated. The remaining 33 dates demonstrate even more clearly than my 2010 sample that *heiau* construction and use in southeastern Maui begins no earlier than the mid-15th century, with a rapid pulse in *heiau* use between *ca.* AD 1450–1600.

Even more striking are the results of ^{230}Th dating on branch corals from 26 *heiau* sites in Kahikinui (Kirch *et al.* 2015; Kirch and Ruggles 2019, fig. 3.16). Contrary to what Bayman *et al.* assert, there is absolutely no evidence for a 'storage age' delay in the deposition of these corals; moreover, fully half of the samples were obtained from 'architecturally integral' contexts (such as within wall fill) that directly relate to *heiau* construction. Whereas most of the radiocarbon dates are from *heiau use* contexts, the branch coral dates are more firmly linked to initial temple *construction*. Both sets of architecturally integral and surface coral offerings exhibit virtually identical temporal patterns (Kirch and Ruggles 2019, fig. 3.17), strongly indicative of a relatively short and intense pulse of temple construction between AD 1550–1700. This is the time period during which, I argue, Hawaiian sociopolitical structures were transformed from chiefdoms to archaic states. Not as an 'event,' but as a process over several generations of powerful leaders who increasingly promoted the idea of divine kingship.

Bayesian modeling and tempo plots can be useful tools for the analysis of archaeological data. But in and of themselves they will never explain how archaic states emerged in the most isolated archipelago of the Pacific. For that, we need a far more comprehensive and holistic anthropological approach. And one, moreover, that does not dismiss indigenous history.

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In 2015, Smith offered recommendations for those seeking to improve their archaeological explanations. Like others before him (e.g., Dunnell 1982; Gould 1978; Haber 1999), Smith argues that scholarly inquiries require addressing the question: *how would you know if you are wrong?* Falsifiability has long been recognized as a fundamental feature of science (e.g., Popper 1959) and requires framing questions in ways that can be empirically assessed. For archaeology, Smith (2015) stresses that we avoid *post hoc* arguments and explicitly include the evaluation of alternatives and potential complications. In brief, we should challenge claims we make about the archaeological record by ensuring that we have not simply restated our starting assumptions and that the current conclusion is sufficiently better than alternatives.

Overall, our understanding of Pacific history benefits whenever researchers have engaged in such critical evaluation of existing knowledge (e.g., Allen 2006; Anderson 2008; Fitzpatrick 2010; Rieth and Athens 2019). In the case of Rapa Nui, for example, the process of raising alternative hypotheses, challenging the assumptions made in long-standing claims, generating new evidence, and critically evaluating data has led to a relatively radical rethinking of the island's past (e.g., Boersema 2015; Cauwe 2011; DiNapoli *et al.* 2020a, 2020b; Hunt and Lipo 2006; Lipo *et al.* 2013; Mulrooney 2013; Mulrooney *et al.* 2010; Rainbird 2002; Stevenson *et al.* 2015) Certainly, such revisions were not always well-received by those most entrenched in previous accounts (e.g., Bahn and Flenley 2011; Diamond 2007, 2010; Flenley and Bahn 2007a, 2007b, 2011), but over time our knowledge has changed as old narratives have been falsified or shown to be built on false premises. Such is the healthy process of science.

In *States without Archaeological Correlates? A Report from Hawai'i,* Bayman and colleagues raise a number of criticisms of historical narratives of two of the most widely-acknowledged authorities in Hawaiian archaeology, Patrick Kirch and Robert Hommond. Like the case of Rapa Nui, their probing into the empirical basis of long-standing historical frameworks for the islands is certainly part of a similar kind of process in which our knowledge is being challenged, questioned, and probed. Of course, only time will tell whether these challenges stand up to inquiry, but their work suggests that critical evaluation of assumptions using recently developed quantitative tools will lead to new knowledge. Given the role that our understanding of the details of Hawaiian history has played in shaping our understanding of the emergence of large-scale social and economic entities, careful evaluation and critical analyses are well-warranted.

Bayman and colleagues evaluate the conventional narrative for Hawai'i using the suggestions made by Smith (2015) in three ways. First, Bayman *et al.* describe a notable disconnect between assumptions traditionally made about the pre-contact history of the islands as 'archaic states' and the archaeological record. The record shows no unambiguous evidence of elite control over key resources such as basalt, no strong evidence of elites in burials, no craft specialization, and no standing armies. At the same time, evidence that is often cited as linked to the presence of a pre-contact Hawaiian 'archaic state' such as the rapid imposition of elite ritual control or large-scale, group-level combat, may have been a gradual process or post-dates the arrival of Europeans.

Bayman *et al.*'s suggestion that evidence for the state is lacking also points to the alternative that Hawaiian archaeological features such as the monumental architecture may be explained by factors other than the top-down imposition of elite and 'state control' often presumed. Ostrom (1990), for example, won the Nobel prize in Economics by demonstrating the conditions under which collective action among individuals can occur over the use of common-pool resources. Group-level collective action can and does produce complex organization, offering potential new areas of inquiry for explaining many features of the Hawaiian archaeological record. The lack of evidence supporting an 'archaic state' coupled with possible alternative accounts points to a situation similar to what led to a radical reevaluation of Rapa Nui's so-called 'collapse' narrative

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and ultimately a revised account.

Second, and perhaps most controversially, Bayman *et al.* raise questions about the extent to which information from oral traditions and ethnohistoric accounts reflect practices in the past. This argument might appear to be eliminating a potentially rich source of information or even devaluating local knowledge in a colonial tradition. Of course, their argument does nothing of the sort. Oral traditions are indeed valuable sources of information that reflect important aspects of heritage. But it is fair to ask questions about the origins of traditions and how they relate to other dimensions of the historical record. If we learned anything from the 'garbology' work of Rathje (Rathje and Murphy 1992), we know that people create accounts for themselves and their behavior different and independent from the empirical record. As in any historical account (e.g., Wolf 1982; Zinn 1980), 'facts' often reflect particular points of view generated at specific times to serve the needs of a subset of individuals. To complicate matters, oral traditions are often manifold and contain conflicting accounts. Following the suggestions of Smith (2015), we should not pick and choose those that best meet our preconceptions. Similar to the work by Cachola-Abad (2000), these traditions are sources of information that must be explained in the same way as the archaeological record. In the case of Rapa Nui, for example, oral traditions reflect a complex history that combines deep ancestral accounts with elements that were introduced as a consequence of post-European events (DiNapoli *et al.* 2020a; Mulrooney *et al.* 2009). The traditions are significant sources of information though they require placing them into historical and cultural contexts.

Perhaps the most promising and significant contribution of Bayman *et al.* is their demonstration of how it is possible to pose hypotheses, establish empirical expectations, and then quantitatively evaluate the available evidence. Bayesian tempo plots reveal a seamless process of large-scale construction activity that contradicts assertions of rapid changes predicted by models of state emergence (see DiNapoli *et al.* 2020 for similar work on Rapa Nui). These data cast into doubt many other aspects of conventional narratives assuming population pressure and resource depletion driving organizational changes associated with warfare, elite coercion, and other markers of organizations assumed to be examples of the 'state.' Such synthetic, quantitative analyses offer new ways of assessing our knowledge about large-scale processes and are beginning to be applied through the Pacific (e.g., DiNapoli *et al.* 2020b; Dye 2016; Mulrooney 2013). Their analyses point to a direction that archaeology is rapidly moving: we can produce empirical expectations for models of cultural change and assess the degree to which we can isolate the factors that might account for that change.

In the end, Bayman *et al.* demonstrate how we can, and should, go beyond qualitative assumptions and arguments made on the basis of simple authority. In the context of the emergence of social complexity in Hawai'i, one path forward is to generate model-based hypotheses amenable to falsification and that account for the limitations of the archaeological record (Perreault 2019). In a discipline that seeks open exchange of knowledge and seeks to reject forms of veracity determined solely by arguments of authority (e.g., Lake 2012; Marwick *et al.* 2017), however, raising questions is vital to advancing the discipline. It is through the process of questioning assumptions that we can improve our understanding of the past. This process happens by iteratively challenging the foundations of our understanding regardless of who makes the claim.

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Mark D. McCoy[5] & Thegn N. Ladefoged[6]

Bayman *et al.*'s simplified caricature of Kirch (2010) and Hommon (2013) employs an underdefined and poorly referenced neo-evolutionary strawman to question the existence of the Hawaiian archaic state. Whereas earlier scholarship could be critiqued for an over reliance on neo-evolutionary systems theory (e.g., Kirch 1984), Kirch (2010) and Hommon's (2013) more recent works on the archaic state are far more nuanced. Kirch (2010) explicitly incorporates core elements of ecodynamics: notions of agency, historical contingency and non-linear processes in the co-evolution of social and natural subsystems through mutual interaction and bi-directional influences (see Fitzhugh *et al.* 2019 for a review of ecodynamics). Both Kirch (2010) and Hommon (2013) leverage findings of the Hawaiian Biocomplexity Project to help understand how the 'intertwined linkages between land, population, agriculture, and surplus provide one set of dynamic, long-term causal factors that are essential to explaining the emergence of Hawaiian archaic states' (Kirch 2010:200).

Bayman *et al.*'s suggestion that Hawaiian political development occurred seamlessly does not account for punctuated changes resulting from non-linear relationships between heterarchical (sensu Crumely 1995; Brumfiel 1995) societal elements. As noted by Kirch (2010) and Hommon (2013), investments in monumentality and increases in agricultural production are not linearly correlated to the spatial extent of political integration or the degree of social inequalities. Bayman *et al.* argue against 'temporally-synchronous bundled characteristics', but their own analysis does little to enlighten on how different facets of society were, or were not, heterarchically related. The

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lack of a smooth progression in the political development is what one would theoretically expect and indeed what Kirch (2010) and Hommon (2013) have documented and helped explain.

The gap between the positions taken by Kirch (2010) and Hommon (2013) versus Bayman *et al.* is less about the archaic state and more about the application of controlled comparison. Controlled comparisons are foundational to the sciences and are powerful analytical tools for understanding the long-term history of human societies. The use of controlled comparison is not without its critics, and has come under attack by quantitative researchers as insufficiently rigorous (Slater and Ziblatt 2013:1303). In their defense of controlled comparison in political science, Slater and Ziblatt (2013:1322) highlight several principles, specifically recommending 'comparisons that operationalize their chief subject of concern in terms of *general variables or mechanisms*, that seek out *representative variation* that attempts to mirror a broader population, and that engage with theory to select cases that *maximize control*' (emphasis in original).

All three of these principles are seen in both Kirch's (2010) and Hommon's (2013) books where they argue for re-classifying Hawaiian society in the centuries prior to European contact as an archaic state. For archaeology, to maximize control means to, as best as we can, control for environmental, cultural, and social conditions when constructing comparisons. We want to compare apples-with-apples (i.e., people living in closely related, but not identical, conditions). Kirch (2010:8–9) is explicit in his commitment to controlled comparison, framing Hawai'i as, 'a model system for understanding a particular stage in the evolution of sociopolitical formations, the transition from chiefdoms to archaic states'. He, and Hommon, do not stop at re-classification but offer similar, but not identical, explanations for social transformation (i.e., *general variables or mechanisms*).

Bayman *et al.*'s critique centers on one aspect of controlled comparison: archaeological metrics of social change (i.e., *representative variation*). Unfortunately, they take such an extreme position that it leads to logical fallacies. They argue that the state is an abstract notion that 'leaves no direct trace in the archaeological record.' However, much of what archaeologists study—economy, religion, subsistence—are abstract. Indeed, Baymen *et al.*'s suggestion that the Mauna Kea quarry was a common pooled resource requires abstract notions of property (see also Dye 2014). Their separation of state from other aspects of society as legitimate foci of archaeological study is arbitrary.

What is perhaps more troubling is that Bayman *et al.* reject the principles of maximizing control and identifying explanatory variables or mechanisms. This leads to blind spots and questionable conclusions. They observe that 1) the circulation of artifacts and raw material in the American Southwest was geographically larger than in the Hawaiian Islands, and 2) in Aotearoa we find much more physical evidence of warfare. If they had maximized control in the environmental, cultural, and social dimensions, they might have also noted that in Aotearoa large social networks, as reconstructed through obsidian circulation, became smaller over time as warfare became more frequent (Ladefoged *et al.* 2019; McCoy and Ladefoged 2019). That fact, combined with oral histories that provide a rich account of the purpose, frequency, and consequences of inter-group violence—for both Aotearoa and Hawaii—makes the infrequency of inter-island artifact movement in Hawaiian Islands comprehensible as the consequence of warfare. Specifically, warfare between Hawaiian island kingdoms may have discouraged geographically broad social networks. This was not absolute, and when we do find stone that has traveled across boundaries between kingdoms it tends to be associated with the highly ranked. Southwestern social networks tell a different story. They, in part, allowed agricultural groups without a central hierarchy to thrive in an unpredictable environment. These cross-cultural insights are why the principle of maximizing control cannot be abandoned.

Bayman *et al.* do not offer any alternative explanations for why Hawaiian society developed along the trajectory it did. This is in contrast to the vibrant research that is being carried out using the transformation to an archaic state as our new working hypothesis. A limited selection of the research directions following the publication of Kirch (2010) and Hommon (2013) include advances in how households and settlement patterns can unlock key aspects of hierarchy (Field *et al.* 2010, 2011; McCoy and Codlin 2016; Vacca 2019), investigations into collective action and resistance in hinterlands (Kahn *et al.* 2016; Ladefoged *et al.* 2020; Hommon 2020), the role of religious ritual in the rise of the state (Baer 2015, 2016; Kikiloi 2012; McCoy 2018), and the dynamic relationship between the environment and society as seen through the lens of food production (Lincoln and Ladefoged 2014; Kagawa-Viviani *et al.* 2018; Quintus and Lincoln 2018; Lincoln 2020). We would encourage scholars to productively engage with previous research and collect new data and information to derive theoretically informed insights.

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Bayman *et al.* make a strong case that when evaluating claims about the existence of statehood in Hawaii we should place more emphasis on diachronic patterning in the archaeological record than has generally been the case. When we do so, they suggest, there is no obvious qualitative break in the trajectory of key developments that would support the conclusion that a statehood level of organisation emerged at a certain point. They also argue that there has been a significant misunderstanding of the nature and timing of population growth as result of misreading relevant archaeological indicators like evidence of habitation frequencies. I am not in a position to comment on their reading of the Hawaii archaeological evidence so my comments address more general considerations.

As their discussion indicates, their work is part of a long tradition of criticising neo-evolutionary typologies and their categories and the attempt to find archaeological correlates of those categories, especially 'the state', so it is worth looking more widely at how this topic has been treated. In the 1970s Wright and Johnson's proposal (1975) was that states had a greater number of decision-making levels than chiefdoms and that this would be recognisable in the existence of multi-level site hierarchies, as evidenced,

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they suggested, in ancient Mesopotamia. This emphasis on the managerial aspect of social evolution has provided the main framework for work on identifying early states in the Anglo-American tradition. Indeed, it is interesting that the neo-evolutionary approach to the development of complex societies in the Pacific has gained a new lease of life in recent years by the application of the comparative method in cultural evolution, based on the phylogenetic tree of the Austronesian languages. This has been used to test models of the development of social complexity in SE Asia and the Pacific, using the coded data on 'jurisdictional hierarchy beyond local community' from Murdock's Ethnographic Atlas (Currie *et al.* 2010) without using any archaeological data at all (and Hawaii is coded as a complex chiefdom).

However, as Chapman (2008) shows, other traditions have different views. He points to studies of the Iberian Argaric Bronze Age by Spanish Marxist archaeologists who saw the evidence as indicating the existence of a state form of organisation because, they claimed, there was evidence of an economically dominant class controlling surplus production and maintained by social coercion, the key features of the state from their Marxist perspective. The issue is not so much whether Marxist archaeologists like Lull and Risch (1995) were correct or not in their assessment of the Argaric but that characterisations of the state have a context in specific theoretical traditions and the one dominant in Anglo-American archaeology/anthropology is not the only one.

As Chapman goes on to point out, an alternative option, which has been argued by many, is to do away with definitional approaches altogether and focus on specific political histories rather than assigning specific configurations to established categories. Bayman *et al.* also share this view and I am very much in favour of their emphasis on the importance of tracing the time-space specifics of developments and not attaching category labels. Long ago, albeit in a different context, Marvin Harris (1968) advised archaeologists to 'shrive themselves of anthropological categories', and Binford complained that archaeologists have remained wedded to 'unrealistic identification approaches' to the archaeological record, based on checklists of archaeological characteristics believed to be associated with supposed evolutionary stages. I later suggested (Shennan 1993) that claiming the existence of social institutions can never simply be a matter of constructing an appropriate indicator variable, because the institutions cited are invariably abstractions of a complex nature which may themselves be contested in a different theoretical tradition. I argued for a focus on the local level of social and cultural reproduction and the importance of reconstructing specific social practices, rather than the generalized social institutions of neo-evolutionary stages.

Accordingly, I am very much in favour of Bayman *et al.*'s emphasis on the importance of tracing the time and space specifics of developments, and also of their use of current techniques of radiocarbon date analysis to extract novel information about rates of change, like their cumulative event plots based on the construction of dated event sequences. As they say, the use of Bayesian methods of radiocarbon analysis integrating all chronologically relevant information makes it possible to date these with a much higher precision than previously and therefore make them much more useful. This is very much part of the current trend of using 'dates as data', though the authors are sceptical of attempts to use dates as data to infer population trends in Hawaii, on the grounds that they have failed to take into account taphonomic processes affecting the formation of the settlement record, and in particular the relative invisibility of early settlement which is now coming to light. Presumably new work in the future will be able to take this into account.

On the other hand, Bayman *et al.* are perhaps on less safe ground in believing that the late date for the establishment of human settlement on Hawaii, giving a timespan of ~700 years, would have been too short to result in sufficient population growth to put pressure on resources. A growth rate of 2% p.a., by no means unusual in populations colonising previously unoccupied areas, would have seen a more than 50-fold population increase over that time. Brown and Crema's (2019) analysis suggests that in the horticultural zone of northern New Zealand population reached carrying capacity in less than 300 years, and even allowing that Hawaii's carrying capacity may have been significantly higher because of its lower latitude position that would make little difference when doubling times are fast. At 2% p.a. growth the population doubling time is 35 years; even if it is only half that the corresponding figure is still only 70 years.

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A Reply from the Authors

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March 16, 2021

We would like to thank our valued colleagues for their comments on our arguments that 1) Hawai'i fails to yield the traditional archaeological correlates of statehood, and 2) neo-evolutionary hypotheses have proven difficult to operationalize and test with the Hawaiian material remains studied by archaeologists. Our evolutionary archaeology colleagues, Lipo and Hunt, and Shennan believe we have made our case and recognize that arguments similar to ours have been taken to heart by archaeologists in other parts of the world. In contrast, our neo-evolutionary archaeology colleagues, Hommon, Kirch, McCoy, and Ladefoged, often misrepresent and dismiss our arguments without responding to them directly.

Why this difference?

We argue here that the difference stems from confusion among the neo-evolutionists over 1) the nature of archaeological inquiry, 2) the role played by material remains in the study of Hawaiian history, and 3) the limitations of neo-evolutionism. We welcome the opportunity to identify and describe these confusions in the brief space of this reply because we believe they stand in the way of productive archaeological inquiry in Hawai'i and that they will continue to do so if left unattended.

A pragmatic account of archaeological inquiry recognizes that it is fueled by doubts, with the aim of resolving them to beliefs through observation. It comprises three types of inference common to all forms of inquiry (Misak 2004). Formulating hypotheses is guided by abductive inference that imagines a state of affairs in the past that would render otherwise surprising observations as expectations. This is the creative aspect of inquiry, which is highly developed by archaeologists under the auspices of explanation. Operationalizing hypotheses is guided by deductive inference that, in archaeology, classifies material remains in a way that makes it possible to track them in space and time (Dunnell 1971). Testing hypotheses is guided by inductive inference that attempts to establish whether the observed space/time distribution of artifacts can be used to predict future observations or is merely due to chance. Our argument that neo-evolutionary hypotheses have proven difficult to operationalize and test focuses on deductive and inductive inference, and it takes issue with specific hypotheses or explanations only insofar as they have resisted deduction and induction.

Our neo-evolutionary colleagues appear to be confused by our pragmatic doubts. Our paper does not question the existence of complex hierarchical polities in Hawai'i. It does point out that 1) arguments for designating these polities as 'states' rest primarily on interpretations of tradition and ethnology, rather than archaeology; 2) archaeological correlates of the statehood hypothesis, as commonly applied in the discipline, are lacking in Hawai'i (Bayman and Dye 2013: 96–98), and the statehood hypothesis is not yet archaeologically testable; and 3) a statehood event is unlikely to be represented in the archaeological record. These are all points about the logic and entailments of scholarly arguments and the nature of the Hawaiian archaeological record. They make no claims about political development in Hawai'i *per se*.

We admire that the study of ethnology and interpretation of tradition 'refine our understanding of history,' as Kirch puts it. But is it not possible that with archaeology, the various sources contribute to this understanding in different ways? Our desire for a theoretical framework productive of archaeological inquiry is interpreted unfairly by the neo-evolutionists as a repudiation of Hawaiian tradition and ethnology. From the pragmatic archaeologist's point of view, ethnology and interpretation of tradition are potential sources of hypotheses leading to archaeological inquiry. Using ethnology and interpretation of tradition in this way indicates a concern for the value of the material record of Hawai'i's history studied by archaeologists. It does not diminish ethnology or interpretation of tradition as guides to understanding. Indeed, traditional histories and ethnology offer springboards for generating hypotheses ahead of archaeological inquiry, a view we share with Lipo and Hunt. We would be working at cross purposes if we repudiated these sources and limited the scope of archaeological inquiry.

Rather, we advocate archaeological inquiry because the independent dating evidence it offers is capable of making a unique contribution to Hawaiian history; it is the only possible way that new sequences of events, the subject matter of history, can be identified and established. By comparison, Hawaiian tradition is limited by its focus on events important to aristocrats, and to Hawai'i Island aristocrats in particular. It has relatively little to say about the history of everyday commoner life. Likewise, ethnology is a social science concerned with statuses, roles, institutions, and the like. It has little interest in sequences of particular events. For much of what transpired in old Hawai'i, archaeology is, practically speaking, the sole source of new information on sequences of events. This is the root of our concern over the disconnection between neo-evolutionism and the archaeo-

logical record.

We find things to like in what we understand to be McCoy and Ladefoged's assessment of weaknesses in the correlates approach to neo-evolutionism. Our own view is that Hawai'i could be a poster child for the failure of the correlates approach. Shennan kindly refers to critiques of the correlates approach that might augment McCoy and Ladefoged's negative assessment. We also have no truck with Hommon's explanation why the statehood event posited by neo-evolutionary theorists – variously estimated to have taken place in the late 1400s (La Croix 2019), around AD 1650 (Kirch 2010), and around AD 1720 (Hommon 2013, 259) – might be difficult to identify archaeologically. Indeed, the reasons he lists – that 1) community resilience dampened disruption, 2) changes might have been additive to institutions rather than replacements, and 3) changes might have been limited to specific archaeological contexts associated with the event – augment our argument that neo-evolutionary hypotheses have proven difficult to operationalize and test with the material remains studied by archaeologists. Our argument on this score focuses on four primary drivers identified by (Kirch 2010): population and demographic change, warfare, development and intensification of agroecosystems, and monumental architecture. Here, the commentators offer little resistance and it appears that our argument has won the day.

The commentators do not dispute our characterization of the difficulties faced by archaeologists hoping to study Hawaiian population history. This is an important topic and a fresh approach is needed. Shennan notes that the new short chronology (Athens, Rieth, and Dye 2014) still leaves time to grow a large population with an annual rate around 2 percent, and La Croix (2019) works out the calculation in some detail, proposing an annual growth rate of 2.8 percent. This kind of reasoning toward a hypothesis provides an opportunity and challenge for archaeological inquiry.

The commentators do not disagree with us that archaeologists are unlikely to find direct evidence of warfare. McCoy and Ladefoged suggest an indirect approach based on their research in New Zealand that links changes in the distribution of volcanic glass to the rise of Māori warfare. They hypothesize that 'warfare between Hawaiian island kingdoms may have discouraged geographically broad social networks … and when we do find stone that has traveled across boundaries between kingdoms it tends to be associated with the highly ranked.' The hypothesis seems implausible to us, in part because of what is already known about the distribution of volcanic glass in Hawai'i. Volcanic glass in Hawai'i moved regularly across political boundaries, followed a theoretically expected distribution away from the source both within and between islands, and showed none of the characteristics Renfrew identified as indicators of elite control (DiVito *et al.* 2020). Perhaps McCoy and Ladefoged are referring to the lesser-studied adzes, instead? This will be clarified when they operationalize and test their hypothesis, but until that happens and results comparable to those from New Zealand are reported, archaeologists in Hawai'i will have no clear path to study the history of warfare.

None of the commentators dispute our argument that the neo-evolutionary hypothesis of agricultural involution is contradicted by a growing body of data that indicates nutrient levels sufficient to sustain production. The history of agricultural development is increasingly being modeled with Bayesian techniques and interpreted as a mix of regional and local processes (Quintus and Lincoln 2020). Work in this area appears to be building the evidential base required to discriminate between the 'bad year' hypotheses proposed by the neo-evolutionists and the 'good year' alternative (Dye 2014).

The commentators seem to agree that the chronology of temples is related to political development. Our paper recites three problems with the chronology of temple development in Kirch (2010): in-built age of unidentified wood charcoal dates, stratigraphic relationships between dated and target events, and storage age of coral dates, evidence for which consists of older corals deposited on the surface of temples that yielded younger corals in the construction fill (Dye 2016). Kirch claims to have solved the first two problems in a recent monograph, but in the comments simply asserts that storage age of coral dates is not a problem. This assertion strikes us as an authorization to suspend the law of stratigraphic superposition. We encourage our colleagues to compare our model-based chronology that attempts to take full account of stratigraphic, direct dating, and historical information with the ad hoc chronologies proposed in Kirch (2010) and the recent monograph he cites. Where the ad hoc chronologies conform to a cultural sequence that posits little change in the century or so prior to Cook's visit in 1778–1779, the model-based chronology indicates this was a period of rapid and accelerating change. We believe the dates from the temples are strong evidence that contradicts Kirch's cultural sequence.

We heartily agree with McCoy and Ladefoged's assessment of the vibrant research conducted during the past decade. What encourages us about this research is its movement away from strictly neo-evolutionary concerns with the development of the aristocracy to matters that would have been important to commoners stewarding 'ili 'āina. A case in point is the open-source software developed to investigate seasonality of sweet potato production in the rain-fed agricultural fields of Hawai'i Island (Kagawa-Viviani *et al.* 2018). Seasonality estimates, such as the ones generated by the software, would have factored into decisions made by aristocratic land managers on temporary assignment to the ahupua'a, but would have been integral to the practices mastered by the local haku 'āina tasked with producing bountiful harvests year after year (Handy and Pukui 1972).

This brings us to the question posed by Hommon, Kirch, and McCoy and Ladefoged as to why we don't advocate an alternative explanatory framework to neo-evolutionism.

Our answer harks back to the idea of abduction as the creative part of archaeological inquiry. We envision a discipline where several explanatory frameworks generate hypotheses and where investigations of commoner family histories complement existing studies of the aristocracy. Hawaiian history is more than an intellectual problem in political development that might be solved; it is a many-faceted mystery that will hopefully yield some of its secrets to the collaborative inquiry of a diverse and democratic research community that includes archaeologists committed to identifying sequences of events and analyzing them along the lines set out in ourpaper.

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