Archaeology in Oceania

Archaeology in Oceania, Vol. •• (2014): ••–••
DOI: 10.1002/arco.5034

Wealth in old Hawai‘i: good-year economics and the rise of pristine states

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ABSTRACT

The journals of Captain Cook and his crew contradict bad-year economic theories that posit that traditional Hawaiian farmers were living at the margin. Recognising that pig herds were wealth-assets in old Hawai‘i, an alternative good-year economic theory is developed that interprets the introduction of sweet potato and the development of the rain-fed agricultural systems in which it was cultivated as processes in the creation and management of wealth. The wealth produced in this way was probably used, in part, to promote marriage alliances among elite families. According to the good-year economic theory, fluctuations in the products of the rain-fed agricultural facilities introduced variability into the supply of wealth-assets, which complicated the maintenance of alliances and were one cause of the wars that played a crucial role in the emergence of primary states in traditional Hawai‘i.

Keywords: Bayesian radiocarbon calibration, Hawai‘i, primary state, rain-fed agriculture, sweet potato, wealth.

RÉSUMÉ

Les journaux du « Captain » Cook et de son équipage viennent en contradiction des théories économiques « de mauvaise année », qui placent les horticulteurs hawaiiens traditionnels en limite de productivité d'autosuffisance. Reconnaissant que les troupeaux de cochons constituaient des produits de richesses dans l'ancien Hawai'i, une théorie économique alternative « de bonne année » est proposée, interprétant l'introduction de la patate douce et la multiplication de systèmes agricoles pluviaux permettant sa production, comme des processus de création et de gestion de richesses. La richesse ainsi produite était utilisée, en partie, pour la promotion d'alliances de mariages entre les élites. Suivant la théorie économique « de bonne année », la fluctuation dans les productions issues des modes agricoles pluviaux engendrait des variations dans l'accès aux biens formant les richesses, compliquant le maintien d'alliances et devenant une des causes des guerres qui jouèrent un rôle crucial dans l'émergence d'états primitifs au sein du Hawai'i traditionnel.

Mots Clés: Calibration radiocarbone Baysienne, Hawai'i, état primitif, agriculture pluviale, patate douce, richesses.

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INTRODUCTION

Were the Hawaiian *maka‘āinana* – the common people[1] – living at the margin, frequently in danger of going hungry during bad years? This is a question raised by two recent book-length investigations of traditional Hawaiian society (Hommon 2013; Kirch 2010a), each carried out in a neo-evolutionary framework that stresses imbalances between population and resources as a dominant locus of social change. They posit a history of Hawai‘i where the agricultural system yields small or no surpluses and minimal subsistence requirements of farmers are in danger of not being met, a region of the production function model that can be labelled "bad year" (Figure 1). This density-dependent, bad-year economics approach to Hawaiian history has its basis in one of two neo-evolutionary models proposed a half century ago when social scientists began to synthesise information on Polynesian societies with the goal of explaining the differential development of social complexity in Polynesia. The model that links social change to density-dependent mechanisms had its genesis in the early work of Sahlins (1958), while the other approach, more closely aligned with structuralism, theorised density-independent mechanisms of change situated in the pan-Polynesian cultural theme of status rivalry (Goldman 1970).

Although the investigations of Kirch (2010a) and Hommon (2013) both depend on bad-year economics, they characterise bad years somewhat differently. Hommon develops a "hard times hypothesis" (Hommon 2013: 241) based on a description of social change on the Polynesian outlier of Tikopia following a hurricane in 1952 that destroyed crops and led to severe food shortages (Firth 1959). In the face of escalating thefts from gardens,

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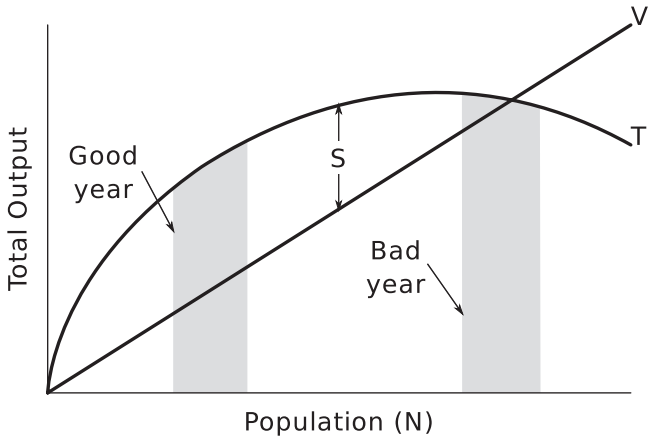
Figure 1. A model of the relationship of population and agricultural system output: T, agricultural production function; V, minimal subsistence requirements to maintain the working population; S, surplus production.
Source: Kirch (2010a: 194).



Tikopia chiefs assumed authority to regulate access to garden lands and imposed increasingly harsh punishments on thieves. Hommon argues that, in a similar fashion, “large-scale food stress” (Hommon 2013: 158) on Hawai‘i Island and perhaps elsewhere provided the context for Hawaiian chiefs to augment their power in ways that transgressed sociopolitical norms and thus spurred the development of social complexity. Kirch takes a different tack, relying on Malthusian theory with a strong focus on the relation of agricultural yield to population size (Hommon 2013: 217 ff.; Kirch 2010a: 177 ff.; Kirch & Zimmerer 2010; Kirch *et al*. 2012; Lee & Tuljapurkar 2008, 2010; Puleston & Tuljapurkar 2008; Tuljapurkar *et al*. 2007) to argue that Hawaiian population growth began to slow in the sixteenth century because production was reaching practical limits, a situation that increasingly diminished a chief’s prospects of laying claim to surpluses.

In both accounts, a primary cause of bad years was the variable yield of rain-fed agricultural systems (Hommon 1986, 2010, 2013; Kirch 1984, 1994, 2010a,b). This idea is based on an analytical model that distinguishes predictable and high-yield irrigated agricultural systems from less predictable and poorer yielding rain-fed agricultural systems (Barrau 1965). The analytical model was historicised and humanised for Hawai‘i when Kirch (1994: 251–68) associated the development of rain-fed agricultural systems with the rise of a powerful line of Hawai‘i Island *ali‘i*, or chiefs, leading to the great king Kalani‘ōpu‘u and his successor, Kamehameha, who famously succeeded in uniting the Hawaiian Islands under his rule in the late eighteenth century. In the density-dependent theory, variable yields from rain-fed agricultural systems on Hawai‘i and Maui Islands at times dipped below the level needed to feed agricultural workers and reduced the growth in tribute that could be collected by chiefs, leading them to wage wars of conquest with their more affluent irrigated agricultural neighbours. According to theory, the organisational skills gained during warfare and later when managing conquered lands initiated a dynamic that spurred the rise of social complexity, culminating in the emergence of state institutions (Hommon 1976, 1986, 2013; Kirch 2010a,b).

A density-independent approach was elaborated by Earle (1978), who argued that development of agricultural facilities would benefit “both the elites and their dependent population” (Earle 1978: 183) and thus that agricultural intensification “was an outcome of political competition and *not* of population pressure” (Earle 1978: 183). This paper attempts to build on the density-independent line of reasoning by considering how social complexity might be achieved during good years. It argues that the production function model used by bad-year economists is too simple for the Hawaiian situation, and that the broad category of surplus production must distinguish between *consumables*, which contribute to well-being and quality of life, and *wealth*, which is “foundational to the power of the dominant gender, social caste or class in every society” (Bell 2004: 12).

In particular, cross-cultural characteristics of wealth-assets (Bell 2004: 12–17) make it possible to identify pig herds as a powerful wealth-asset in old Hawai‘i, the only island group in Polynesia where pigs were “numerous enough to be classed as a capital reserve available for exchange” (Goldman 1970: 476). Because “exchanges are the code through which status information is communicated” (Goldman 1970: 496), development of wealth-assets is tied to status rivalry. In this view, the pursuit of status, in part through the exchange of pigs, spurred the development of social complexity.

These two explanatory approaches – a density-dependent approach based on bad-year economics, and a density-independent approach based on good-year economics – are not mutually exclusive. Hawaiians certainly knew good years when vegetable foods were plentiful and pig herds were healthy and growing, and bad years when vegetable crops and pig herds produced less than expected. Experiences of both kinds would have shaped habits and activities, contributing to the production and reproduction of the social forms encountered by Europeans in the eighteenth century. From a theoretical point of view, both approaches would seem to be required for completeness. For the archaeologist, the choice of one approach over the other, or the degree to which they each contribute to an analysis, ought to be a practical matter having to do with how well the approach structures observations of archaeological materials and how convincingly it relates those observations to a scientific law or a historical explanation based on human intention and motivation (Djindjian 2001).

A central argument of this paper is that the density-dependent approach articulates poorly with the archaeological record of traditional Hawai‘i. Density-dependent interpretations rely on Hawaiian

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tradition and other synchronic sources for evidence of what happened in old Hawai‘i (Hommon 2013: 212–16; Kirch 2010a: 77–123); the problem of equifinality leaves these synchronic data open to a variety of plausible interpretations.

This paper attempts to demonstrate that a density-independent approach provides the structure needed to interpret model-based chronologies recently proposed for the development of agricultural facilities in the rain-fed agricultural system of leeward Kohala district on Hawai‘i Island, and for the construction of mostly small, family-sized temples nearby.

Model-based chronology building using Bayesian calibration of ^{14}C age determinations, which is able to incorporate a wide range of chronological information, produces precise results and yields age estimates for events previously considered undatable, has the potential to revolutionise archaeological interpretation (e.g. Whittle *et al*. 2011). Here, it produces archaeological sequences of change that are interpreted as the direct material remains of wealth creation and management integral to the development of social complexity in old Hawai‘i.

A MOST EXTRAORDINARY HOG ISLAND

Bad-year economics paints a dour picture of old Hawai‘i. It interprets: (i) archaeological evidence as indicating a sufficiently large population to put pressure on agricultural production (Hommon 2013: 234–5; Kirch 2010a: 138–40, 216); (ii) the development of facilities to intensify production in rain-fed agricultural systems as a process of agricultural involution (Geertz 1963), where additional labour inputs are rewarded with proportionally smaller increases in yield (Hommon 2013: 233–4; Kirch 2010a: 147–8); (iii) measurably lower soil nutrient levels within rain-fed agricultural soils as indications of potential crop failure and/or declining yields (Hommon 2013: 232–3; Kirch 2010a: 149–50); (iv) Hawaiian traditions as documents of famines that provoked social unrest and conflict (Hommon 2013: 238; Kirch 2010a: 199–200); (v) ethnographic observations that Hawaiian women worked in rain-fed agricultural fields on Maui and Hawai‘i Islands, an unusual practice in Polynesia, as evidence of labour shortage in the face of declining yields (Hommon 2013: 61; Kirch 2010a: 139, 196, 216); (vi) variable yields of rain-fed agricultural systems in Hawai‘i as a cause of warfare that played a central role in the development of social complexity (Hommon 1986, 2010, 2013; Kirch 1984, 1994, 2010a,b); (vii) the relationship of *ali‘i* to *maka‘āinana* as a source of social tension (Hommon 2013: 217–56; Kirch 2010a: 190–201); (viii) the motives of chiefs in the Kamehameha line as “hostile and expansionistic” (Kurashima & Kirch 2011: 3673); and (ix) archaeological evidence as indicating a decline and abandonment of the rain-fed agricultural fields soon after European contact (Kirch 2010a: 153) and their rapid replacement in the landscape by “early historic ranching enclosures, homesteads, and other features of the postcontact era” (Ladefoged & Graves 2010: 89).

Given this, one might expect the initial accounts of Hawaiian society to describe a parlous state of affairs and widespread poverty, but this expectation is convincingly disappointed by the historical accounts of Captain James Cook and his crew during their sojourn in the islands in 1778 and 1779, which describe Hawai‘i prior to the massive changes eventually brought on by contact with outsiders. This is the testimony of men – there were no European women on board *Resolution* or *Discovery* – who had spent several years in the Pacific and were intimately familiar with islands and islanders throughout the Polynesian triangle. Lieutenant King remarked that sweet potato, the pre-eminent rain-fed agricultural crop in Hawai‘i, “thrives prodigiously, indeed it is such Plenty that the poorest natives would throw them into our Ships for Nothing” (Beaglehole 1967: 618). The sweet potatoes themselves were described by James Trevenen, a midshipman on *Resolution* and later *Discovery*, who judged them “infinitely superior to any others we ever met with at the Society, or Friendly Islands . . . they are bigger than a Man’s head, sweet, and mealy when dressed, & when raw taste something like a chestnut” (Beaglehole 1967: 618, n. 1). The testimony of King and Trevenen indicates that a reasonable surplus was indeed available.

Of course, 1778 and 1779 might coincidentally have been good years for the sweet potato crop, and a deviation from the usual situation, but if this were so, it would be difficult to explain the large size of the pig herds raised and managed by Hawaiians.

Captain Clerke referred to Kaua‘i Island as “the most extraordinary Hog Island we ever met with, take them for Number and size” (Beaglehole 1967: 575). Lieutenant King’s corroborating assessment included a direct comparison with Tahiti and the Society Islands:

> Notwithstanding the much greater quantities of roots & hogs that we destroyd, & of the latter salted down, than at Otaheite or the Society Isles; yet here we never perceivd this had any effect upon the great plenty still on shore; . . .
> Whereas at Otaheite the last time, when things were found in the greatest plenty, Otoo was obliged to take some pains in Supplying us. (Beaglehole 1967: 619)

This testimony to the large size of pig herds is bolstered by native historians who refer to the sacrifice of large numbers of pigs during rituals, as many as 800 or 2000 at a time (Handy & Handy 1972: 252; Malo 1996: 252 ff.).

The link between the rain-fed agricultural fields and the large pig herds described by Cook’s crewmen is clear – sweet potato was the pre-eminent pig fodder throughout Polynesia and Oceania:

> It is in the singularly important role of conversion of vegetable material into rich sources of protein and fat –

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> animal husbandry – that the sweet potato attains ascendancy over any other single plant species. Not only is its ability in this direction recognized, but also, in the feeding of pigs, there is a marked preference over other cultigens. (Yen 1974: 52)

Sweet potato can be fed to pigs in several ways (Yen 1974: 52): leftover meal scraps; substandard roots from harvesting; leaves and stems harvested especially for fodder; after cooking; or by allowing pigs to forage fields, either after final harvest, or, in Hawai‘i, before harvest, a prerogative accorded pigs as "bodies" of the god, Kamapua‘a (Handy & Handy 1972: 253). At Kaupō, Maui, rapid growing varieties of sweet potato were used expressly as pig fodder (Handy 1940: 151).

In fact, the link between the rain-fed agricultural fields and pig herds was so strong that an authority on sweet potato agriculture in Oceania believed that the relationship to pig husbandry defined the place of sweet potato in the traditional Hawaiian agricultural system: "[i]t is in the analogies of implied surplus in both sweet potato and pig, in its ceremonial use, gift exchanges, and finally, trade, which allows for a thesis of agricultural development whose course was accentual within the diversified use of the environment" (Yen 1974: 312). The large Hawaiian pig herds described in 1778–1779 were probably the result of growth over a substantial period of time, a testament to successful management of the variable fruits of rain-fed agricultural fields.

THE ABSENCE OF ARCHAEOLOGICAL EVIDENCE FOR BAD YEARS

Bad-year economics theorises Hawaiian society as living on the margin, producing just enough to feed farmers and finance *ali‘i* projects in good years and often falling below this level so that *ali‘i* and *maka‘āinana* compete for scarce resources (Figure 1). Population is a key variable in this formulation, as it is for Malthusian theories in general, because the situation where population outstrips resources is theorised as an important engine for social change; population size must be sufficient to put pressure on resources so that bad years are frequent. Both Kirch (2010a) and Hommon (2013) take this bad-year economics stance, and they both rely on population pressure to promote social change, though they differ on the nature and timing of that pressure. This is important because population is difficult to measure with archaeological materials. In practice, what this means is that bad-year economics accounts are based on rather weak archaeological inferences about population history, which they supplement with other evidence that is argued to be a proxy for the relationship between population and resources.

Archaeological inferences about population size are weak because of the nature of the evidence. Although Hawaiian archaeologists have repeatedly tried to estimate population histories, they have yet to control adequately for potentially confounding variables. The two approaches that they have employed have been critically reviewed (Clark 1988; Dye 2010b: 142–4; Kirch 2010a: 128–40). The first was the "house-count" method, which assigns ages to habitation structures and counts how many houses are present at different periods of time. The other, sometimes referred to as the "Dye–Komori" method after the authors of a paper that applied it to ^{14}C data from Hawai‘i (Dye & Komori 1992), is based on an idea developed by Rick (1987), which treats the corpus of ^{14}C ages from habitation deposits as a sample from a charcoal population that is argued to be isomorphic with population size over time. In the years since these approaches were developed, some fundamental problems with their implementation in Hawai‘i have been identified. First was the realisation that a failure to identify the charcoal selected for dating meant that Hawaiian archaeologists were often dating old wood. An early investigation in Hawai‘i determined that old driftwood might add several hundred years to the age of a dated sample (Emory & Sinoto 1969); an attempt to measure its effect on archaeological materials found an average error of about a century in a sample of 40 age determinations (Dye 2000).

Subsequently, old wood was identified as a probable cause of a 600-year error in age estimates for the early O18 site (Dye & Pantaleo 2010). Old wood has a similar negative effect on both the house-count and the Dye–Komori approach to estimating population, in both cases inflating estimates for early periods and deflating estimates for later periods, yielding population curves that overestimate the rate of early population growth and magnify any slowdown of population growth later in the sequence.

A second problem, identified somewhat later, has to do with the failure of Hawaiian archaeologists to establish a stratigraphic association between the dated event and the target event. This creates a problem for the house-count approach when a dated event is not associated with the stratigraphic position of a house foundation. A review of a systematic excavation program of 219 test pits through surface architectural features at Kahikinui, Maui (Dixon *et al*. 2000) found that most houses there had been built on top of earlier cultural deposits containing charcoal. In the median case, more than 80% of the cultural deposit predated the excavated structure (Dye 2004). If, as is commonly the case in Hawaiian archaeology, material for ^{14}C dates was collected without regard to the stratigraphic position of the surface architecture, then it probably derived from pre-construction deposits, thereby inflating the age estimate for the house by an unknown number of years (Dye 2004). The effect of the association problem on population estimates derived with the house-count method would be similar to the effect of old wood – an overestimate of population in early periods and an underestimate for later periods. The association problem has a different negative effect on the Dye–Komori method. Here, the problem has to do with identification of charcoal as deriving from a habitation deposit based

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on characteristics of the surface architecture at a charcoal collection locality; the lack of a demonstrated association of the surface architecture with the dated material means that there is no explicit reason to believe that the dated material derived from a household context – the house might just have well been constructed on an agricultural or other non-habitation deposit that yielded charcoal for dating. Because the dates used in the Dye–Komori method must be confidently associated with habitation activities, it is not possible to argue convincingly that the age distribution of charcoal not associated with surface architecture is isomorphic to population size.

Despite these potentially grave and largely unresolved problems with archaeological population estimates in Hawai‘i, Kirch (2010a: 131–40) argues that when the various population history estimates are taken into account, a change in population growth around AD 1500 can be detected. In this view, a demographic transition from high to low rates of growth at the beginning of the sixteenth century was brought about because resources could no longer support a growing population, a situation with "enormous implications for sociopolitical change" (Kirch 2010a: 139). Yet, the evidence for a change in population growth at this time is problematic. The argument relies on two early population history estimates (Dye & Komori 1992; Hommon 1976), both of which show a marked change to lower growth rates in the early sixteenth century, but it ignores the fact that both of the estimates are based primarily on unidentified charcoal dates from contexts without a demonstrated association with the surface architecture. Here, the errors probably introduced by the failure to control for the problems of old wood and stratigraphic association both depress late-period population estimates, which would accentuate or create the impression that population growth rates had changed. Next, a plot of the number of conventional ^{14}C age determinations by century for the islands of Hawai‘i, Maui, Moloka‘i and Lana‘i, which in total show a slight decline in the rate of growth beginning in the sixteenth century (Kirch 2010a: 135), is offered as evidence for a change to lower growth rates in the early sixteenth century. However, this population estimation procedure is problematic. No theoretical justification is given for the implicit claim that the temporal distribution of ^{14}C dates from all contexts – habitation, agriculture, ritual and others – is isomorphic to population size. There is also a technical problem; the graphic purports to plot conventional ^{14}C ages on the calendar timescale (Stuiver & Polach 1977). If true, both the graphic and inferences from it about events on the calendar timescale are nonsense – ^{14}C years are not calendar years and it is not possible to get from one to the other without calibration. There can be no assurance that the evidence cited for lower growth rates in the sixteenth century is anything other than a reflection of the uncontrolled effects of potentially confounding errors.

Hommon recognises the limitations of archaeological estimates of Hawaiian population (Hommon 2013: 205–12) and turns instead to a measure of population growth that does not involve counting houses or tallying residential charcoal samples. In their place, he argues that the development of rain-fed agricultural features in the Leeward Kohala Field System (LKFS) is, itself, evidence for either a growing population or for "rising tax levies" (Hommon 2013: 253). The features themselves do not indicate how the increase in production they might have effected was actually used, whether it kept the children of farmers from going to bed with an empty stomach, or was instead collected by *ali‘i* to finance projects. An increase in production would move the state of the system to the left on the production function graph (Figure 1), but the starting position for the move is not known; it might be near the bad-year section or the good-year section of the function.

In this situation, archaeological evidence for common bad years might indicate that development of rain-fed agricultural features took place when population and resources were out of balance. Elsewhere in the Pacific, archaeological evidence for common bad years includes development of food storage facilities to buffer periodic resource shortages.

One type of food storage facility is a pit, likened to an "underground silo" (Kirch 1984: 133), lined with leaves and used for the semi-anaerobic fermentation of breadfruit and other starches, which is reportedly capable of storing food reserves for decades. Pit-storage of starch staples is known in both Western and Eastern Polynesia (Kirch 1984: 133), primarily on small islands where drought or hurricanes periodically destroy crops and create famine conditions. A few pits possibly used for this purpose have been reported in Hawai‘i (Nakkim 1970), but decades of archaeological investigation have not found other features like them and it seems likely that they were either extremely rare or absent.

Similarly, hilltop fortifications in New Zealand, called *pā*, sometimes appear to have been designed to protect food storage pits from raids by neighbours during bad years (e.g. Fox 1976: 28–9; Kirch 1984: 209–11; Law & Green 1972). Fortified food storage facilities such as these have not been reported from Hawai‘i, whose archaeological record includes only rudimentary forms of fortification. Thus, the archaeological record does not indicate that bad years were common in Hawai‘i, nor that rain-fed agricultural features were developed in response to resource shortages. The alternative hypothesis, that the rain-fed agricultural features were developed in the context of status rivalry, is equally plausible.

EQUIFINALITY AND INTERPRETATION OF SYNCHRONIC DATA

One of archaeology's strengths is its ability to yield diachronic data capable of being interpreted as change in human behaviour. The lack of archaeological evidence for bad-year economics in Hawai‘i means that both Kirch and

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Hommon have to place great weight on a variety of synchronic non-archaeological evidence, and that they interpret this evidence as indicating an imbalance between population and resources. A reliance on synchronic data opens their arguments to the problem of equifinality, where different historical processes plausibly result in the same outcome (Tuggle 2010).

Kirch and Hommon interpret elements of Hawaiian culture and tradition as yielding evidence for purported resource shortages in old Hawai‘i: Hawaiian culture classifies certain vegetable foods as “famine foods”, which has led to the conclusion that famines were well known and relatively frequent (Hommon 2010) and oral traditions are interpreted to “refer more than once to periods of drought, and to resulting famine, social disruption, and warfare” (Kirch 2010a: 199; see also Hommon 2013: 238). Both of these interpretations beg the question of how well the Hawaiian word *wī* translates as the English word “famine”; that is, whether the range of historical events classified as *wā wī*, or “times of famine”, included the mortality and social disruption that accompanied famines in Europe, such as the great famine in Ireland in 1845–1849, in which an estimated 1,000,000 people starved to death when potato crops failed. A review of Hawaiian oral traditions suggests that the meanings of the two words overlap only partially.

The Hawaii Legends Index has 16 entries for “famine”. These entries refer to four traditions, including the legends of Kiha-a-Pi‘ilani, Hua, Kahuoi and Hina Kuluua. The four traditions yielded 16 entries in the index because some of them were retold widely in versions that refer to different places and times. None of the famine traditions mentions mortality, or relates resource scarcity to social disruption or warfare. These characteristics appear to be a product of their interpretation. In fact, a common theme of the legends is decidedly non-Malthusian – access to food was a right of person (Macpherson 1985) made possible by a socially prescribed food-sharing ethic. This ethic was expressed by “the accepted greeting, even to comparative strangers”, which is translated literally as “come in and eat, eat all you want” (Pukui *et al*. 1972), combined with prohibitions against stinginess (Handy & Pukui 1972: 191). The legend of Kiha-a-Pi‘ilani (cited by Kirch 2010a: 199) tells how, after clearing a garden patch during a famine, “Kiha-a-Pi‘ilani went to [the neighbouring lands of] Hamakuapoko and Hali‘imaile to ask for potato slips. The natives gave him whole patches of them wherever he went. ‘Take a big load of the slips and the potatoes too if you want them’ [they said]” (Kamakau 1992: 24).

The other legend cited as indicating famine is set in the time of La‘amaikahiki, dated genealogically to the twelfth century, a century or two after Polynesian settlement and presumably well before the population could conceivably have grown large enough to approach carrying capacity. This legend, too, refers to the sharing ethic. It is set

> during a spell of great drought, when a great famine was experienced over all the lands from Hawaii to Kauai, all the wetlands were parched and the crops were dried up on account of the drought, so that nothing even remained in the mountains. Waipio was the only land where the water had not dried up, and it was the only land where food was in abundance; and the people from all parts of Hawaii and as far as Maui came to this place for food. Because of this drought, all the lands from Hawaii to Kauai were without food and the people were forced to subsist on mosses [seaweed] and other such things. But all through the drought and famine Waipio never went without food. During this famine the people from Hawaii, Maui and other islands came to get food at Waipio. (Fornander 1916–1919: IV, 136)

The other two famine traditions also refer to this food-sharing ethic. It seems perverse to interpret Hawaiian oral traditions as indicating that resource shortages led to social disruption and war, when in fact they celebrate a right of person that would have counteracted these very tendencies.

Similarly, the bad-year economics interpretation of a famine tradition from Kona during the reign of Kalani‘ōpu‘u situates a cause of warfare in the tension created by *ali‘i* demand for tribute and uncertainty in producers’ ability to meet those demands (Hommon 2013: 238). This tension is indeed central to a peripatetic court, which moves from place to place, eating from the fat of the land (*‘āina momona*). But just because this tension is central to the relationship, social mores had developed to ensure that it did not break out of the realm of provisioning the king with food and supplies, and into direct conflict between the people and the king’s court. This would appear to be the message of the tradition about Kalani‘ōpu‘u. In Kamakau’s telling of the tradition, Kalani‘ōpu‘u is directly criticised as “senile with age” (Kamakau 1992: 106) for his exorbitant demands, which made life in Kona “uncomfortable” (Kamakau 1992: 105). Here, Kamakau’s criticism of Kalani‘ōpu‘u refers implicitly to social mores concerning acceptable levels of tribute demands with which his readers were expected to be familiar. Although the tradition indicates that the Kona people “wept bitterly” (Kamakau 1992: 105) at their fate, the rebellion against Kalani‘ōpu‘u that followed did not arise in Kona, but in the distant district of Puna, where a disaffected *ali‘i* usurped tribute that would customarily have been claimed by Kalani‘ōpu‘u. In any event, Kalani‘ōpu‘u effectively resolved the uncomfortable Kona famine by moving his court to North Kohala, rather than staying in Kona, where he risked being killed, as had other *ali‘i* in the past who, for unspecified reasons, chose instead to stay beyond their welcome. In North Kohala, a Ka‘ū chief named Nu‘uanupa‘ahu, who was travelling with the court, joined the rebellion. He was found out by Kalani‘ōpu‘u and died soon after from a shark bite suffered while surfing. This incident conveys supernatural support for the reign of Kalani‘ōpu‘u and reinforces the message of the tradition that the locus of rebellion was the

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relationship of the king to his nobles, who viewed signs of his apparent senility as a weakness that might be exploited. The tradition offers no support for the view that warfare was sparked by strains in the relationship of a king with his *maka'āinana*. It clearly refers to the status rivalry characteristic of Hawai'i and other Polynesian societies (Goldman 1970), in this case, rivalries that sprang up soon after Cook's death in 1779.

Two other pieces of synchronic evidence have been interpreted to bolster the case for bad-year economics. Evidence for depletion of soil nutrients in the sweet potato plots of Hawai'i and Maui Islands (Hartshorn *et al*. 2006; Meyer *et al*. 2007; Vitousek *et al*. 2004) has been interpreted by archaeologists as indicative of declining yields associated with agricultural involution (Hommon 2013: 232–3; Kirch 2010a: 149–50). Soil scientists interpret this evidence with a bit more circumspection; 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 has determined that agricultural practices might have lowered yields at the upper edge of the LKFS, where high rainfall had already leached nutrients from the soil (Meyer *et al*. 2007), but soil nutrient levels within the main part of the field system, although measurably lowered by traditional Hawaiian cultivation, were still relatively high (Chadwick *et al*. 2013). Hawaiian farmers made considerable use of mulch (Malo 1951: 205; Menzies 1920: 76), which would have worked to reverse deleterious effects of nutrient depletion. In addition to ongoing investigations of mulching in traditional Hawaiian agriculture (Lincoln 2013), soil nutrient studies might model the effect of pig husbandry on sweet potato yields. Pigs till, weed and fertilise, and the presence of a large herd in the field system might well have had a positive effect on yields. At this point in the investigation of the effects of traditional agriculture on soil fertility and crop yields, it is premature to reject either a history that posits declining yields or one that posits growing yields, both of which should be considered as alternative hypotheses to be tested by further research.

Similarly, women's labour in the rain-fed agricultural fields of Maui and Hawai'i has been interpreted as a response to labour shortage in the face of declining yields (Hommon 2013: 61; Kirch 2010a: 139, 196, 216), but it could just as well be interpreted as an integral part of the intensification process, unrelated to agricultural involution. Polynesian women traditionally played a role in the production of valuables and wealth-assets (Weiner 1992), and this appears to have been the case in Hawai'i, as well (Hommon 2013: 237–8; Linnekin 1988, 1990). The labour of women in the rain-fed agricultural fields was probably tied, at least to some degree, to the care of pig herds (Hommon 2013: 78; Kirch & Sahlins 1992: 28), and thus might be interpreted as an expansion of women's traditional role as producers of valuables and wealth-assets, rather than an indication that something had gone wrong.

GOOD-YEAR ECONOMICS

Bad-year economics has developed increasingly complex models that relate population and productivity under various constraints (Lee & Tuljapurkar 2008, 2010; Lee *et al*. 2006; Puleston & Tuljapurkar 2008; Tuljapurkar *et al*. 2007). This potentially useful and interesting work is at an early stage of development. The models developed so far are necessarily simple ones that stint "cultural, institutional, and political factors in population dynamics . . . [even though] such factors are important, always and everywhere, and presumably become even more important as political complexity increases" (Wood 1998: 100). One characteristic of these simple models is that the relationship between population and resources has one stable equilibrium state (Kirch 2010a: 194), which is in the bad-year region of the production function where T and V intersect (see Figure 1). Such models describe simple societies, whose functioning must be different than a politically complex society, like Hawai'i, that is remarkable for its elaboration of the Polynesian cultural theme of status rivalry, invention of state institutions and creation of an endogamous *ali'i* political class. Theory with these limitations cannot expect to cover the full range of possibilities explored by traditional Hawaiian society.

One particular limitation of bad-year economics theory is that it divides production in two, distinguishing a basic level of production to meet the minimum subsistence requirements of the cultivators from surplus that can be used to feed livestock, support non-producers or exchange for one purpose or another (see Figure 1). Good-year economics theory requires that the surplus category be further divided into *consumables*, which contribute to well-being and quality of life, and *wealth-assets*, which are the foundation of power (Bell 2004: 12). In the case of the rain-fed agricultural systems of old Hawai'i, the division of surplus into consumables and wealth-assets means distinguishing between individual pigs and herds of pigs. Individual pigs are, of course, consumables the fatty flesh of which was widely prized throughout Polynesia. But herds of pigs are different.

When managed correctly, they satisfy the four criteria that distinguish wealth-assets cross-culturally (Bell 2004): (i) a capacity for growth; (ii) the ability to "generate a flow of consumption benefits to *a set of individuals who have rights to them*" (Bell 2004: 13, italics in original); (iii) scarcity in the sense that increases in size or value must be socially recognised and approved; and (iv) their exploitability over an indefinite time horizon.

Hawaiian pig herds were capable of growing in size, theoretically limited only by the high natural growth rate of the pig population. In practice, the growth rate of pig herds would have been slower than this theoretical rate, but robust rates of growth in the pig population appear to have been achieved with some regularity, as the testimonies of Captain Clerke and Lieutenant King about the abundance of hogs, cited earlier, attest.

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Second, pig herds generated a flow of consumption benefits, primarily to male elite, whose successful claims of rights to control their distribution culminated in an ideological project of unusual scope in Polynesia. Considered a body of the god, Lono, pigs were sacrificed in Lono ceremonies, but were also appropriate offerings to other gods (Valeri 1985a: 45). They were the most important sacrificial animal (Valeri 1985a: 47), and as such, were forbidden to women and were rarely consumed by low-status men. Pigs were “eaten most often by high-ranking men in temples to which male commoners were not admitted any more than women” (Valeri 1985a: 127). These temple activities have been described as feasts and “a distinctly chiefly form of consumption [where] . . . foods, which in other Polynesian societies might be reserved for special feast occasions, had become the daily fare of *ali‘i* households” (Kirch 2001: 178).

Third, pigs appear to have been distributed as a highly valued gift. When Cook stopped at Kaua‘i in 1778, before the English had impressed on Hawaiians what they wanted by way of provisions, the first canoes to come from shore to the ships brought piglets as gifts.

David Samwell, surgeon aboard *Resolution*, learned that pigs “are what they present to Strangers as a token of Friendship at the first Meeting” (Beaglehole 1967: 1082), a custom that Cook later specified was practiced by “chiefs or people of Note” (Beaglehole 1967: 491).

Pigs were given to Cook and his crew many times during their two stays in Hawai‘i, often during state ceremonies that indicate the high value accorded to gifts of pigs. One example is the ceremony performed by Kalani‘ōpu‘u, king of Hawai‘i Island, when he returned from Maui to meet Cook at Kealakekua Bay. Three canoes came off to the ships, and the third, with 18 paddlers, “was filled with hogs and various sorts of vegetables” (Cook & King 1784: III, 17). A bit later, on shore, after Kalani‘ōpu‘u had given Cook five or six feather cloaks, his people brought in “four very large hogs, with sugar-canes, cocoa-nuts, and bread-fruit” (Cook & King 1784: III, 17). Another example was an important act in the reconciliation following Cook’s death, when Kalani‘ōpu‘u gave Captain Clerke 13 hogs along with various vegetables following the return to the English of Cook’s remains (Beaglehole 1967: 547). Gifts of pigs appear to have been common outside these state ceremonies as well. According to Clerke, “they had no idea of making a present of a hog or two, but would frequently beg your acceptance of ten or a dozen” (Beaglehole 1967: 595) and he recounts a courtesy gift of “thirty Hogs and as much Fruit as my people could destroy in a week” (Beaglehole 1967: 595) after an informal dinner with Kalani‘ōpu‘u.

These observations indicate the high marginal utility of pigs in old Hawai‘i – an increase in the size of the herd would make possible more and larger gifts, which would have had a positive valuation in Hawaiian society.

Fourth, the historical record makes it clear that pig herds were exploitable over an indefinite time horizon. When Captain George Vancouver revisited Kealakekua Bay in 1793, after Kalani‘ōpu‘u had died, the new King Kamehameha made a point of indicating to Vancouver, whom he had met when Vancouver was a midshipman on *Discovery* during Cook’s third voyage, that he had inherited the pig herds controlled by Kalani‘ōpu‘u. He presented 90 “very large hogs” to Vancouver while wearing “a printed linen gown, that Captain Cook had given to Terreoboo [Kalani‘ōpu‘u]; and the most elegant feathered cloak that I had yet seen, composed principally of beautiful bright yellow feathers, and reaching from his shoulders to the ground on which it trailed” (Vancouver 1798: II, 126). In late eighteenth century England, a “printed linen gown” referred to a loose, typically full-length (Ribeiro 1984: 26) robe made of patterned material in oriental fashion. It was often worn informally by gentlemen instead of a coat, and was a favourite of portrait artists at least since the time of Samuel Pepys, who a century earlier had sat for a portrait in a gown that signified that “he was a gentleman with a cultivated and leisurely lifestyle” (Ashelford 1996: 102). Kamehameha might have fancied the gown for much the same reason, and he undoubtedly understood that clothing in England, as in Hawai‘i, was a common sign of status (Tcherkézoff 2003:56). However, he would have worn the feather cloak during this state ceremony as “a political statement . . . that his relationship to others was of superiority of power” (Kaeppler & de Rooij 2010: 39), in this case a superiority that legitimised claims to the consumption benefits of the large pig herds of the island that had been held by Kalani‘ōpu‘u when Vancouver first visited the islands. Recent archaeological research in the LKFS has yielded information on how and when pig herds became wealth-assets in old Hawai‘i.

SWEET POTATO AND THE LEEWARD KOHALA FIELD SYSTEM

Direct evidence places the introduction of sweet potato to Hawai‘i some three to five centuries after Polynesian settlement (Dye 2011a: 135). The plant underwent a remarkable radiation after its late introduction. By the early twentieth century, some 230 named varieties were known (Handy 1940: 32–4), the products primarily of rain-fed fields on the geologically younger islands (Ladefoged *et al*. 2010) and colluvial sediments on the geologically older islands (Kurashima & Kirch 2011; Vitousek *et al*. 2010). The agronomic characteristics of the plant opened up vast areas of the geologically younger islands of Hawai‘i and Maui for cultivation (Figure 2) – one estimate is that the agricultural potential of Hawai‘i Island more than tripled and that of Maui Island almost doubled with the introduction of sweet potato (Graves *et al*. 2010: 157). A similar doubling of agricultural potential was found on the geologically older island of Moloka‘i, where the bulk of sweet potato was probably cultivated on colluvial soils (Kurashima & Kirch 2011: 3664).

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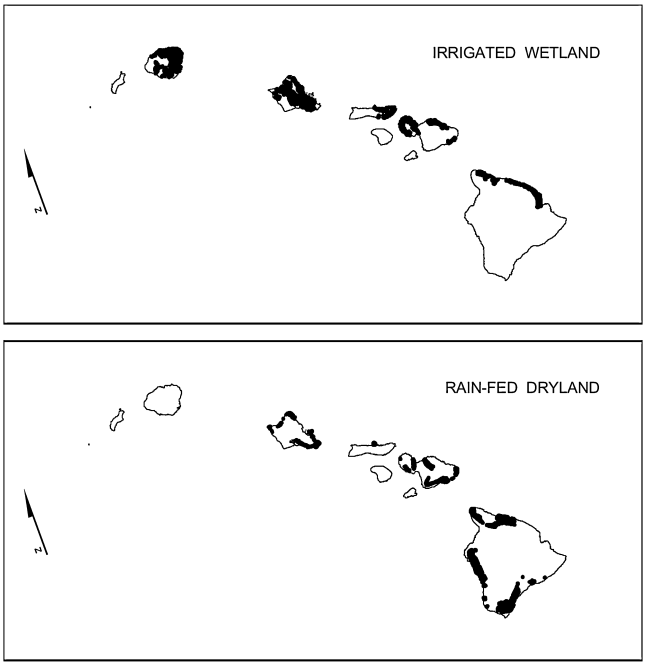
Figure 2. The distribution of irrigated wetland and rain-fed dryland agriculture in the Hawaiian Islands. Note the large areas of the Maui and Hawai‘i Islands suited to rain-fed dryland agriculture.



A sophisticated program of ^{14}C dating carried out at agricultural features (Ladefoged & Graves 2008) and temples (McCoy *et al*. 2011) within the LKFS (Figure 3) makes it possible to track field system development and the tempo of temple construction using Bayesian calibration methods (Buck *et al*. 1996). These methods, which incorporate both stratigraphic information and ^{14}C age determinations in the calibration, typically yield results that are more precise than can be achieved with *ad hoc* interpretative frameworks. In the case of the LKFS, this precision exposes patterns of change that can be interpreted as indicative of good-year economics. Additionally, the gains in precision are sufficient that the archaeological data can be related meaningfully to the line of *ali‘i* leading to Kalani‘ōpu‘u and Kamehameha, starting with ‘Umi a Līloa, an *ali‘i* who figures prominently in the traditions of Kamehameha (Valeri 1985b), and is believed to have ruled in the early seventeenth century.

Dating of agricultural activities in the LKFS indicates that the area was cultivated as early as the fifteenth century (Ladefoged & Graves 2008). Bayesian calibration of dating samples collected beneath agricultural walls indicates that facilities designed to increase production were built in the less desirable southern part of the field system beginning in the late seventeenth or early eighteenth century, and that they were developed primarily in the eighteenth and early nineteenth centuries (Dye 2011b) (Figure 4).

These results of the Bayesian calibration differ from the interpretation based on *ad hoc* dating methods in at least two ways. First, they yield estimates of facility construction dates the precision of which is specified by the posterior distribution. In contrast, the *ad hoc* interpretation of these data yields posterior distributions for the various dated events (Ladefoged & Graves 2008: 782), but the age estimates of the target construction events are vaguely specified – “[t]he majority of the agricultural walls were probably constructed after AD 1660, when four phases . . . of building development occurred” (Ladefoged & Graves 2008: 779).

Second, the Bayesian estimates indicate that development of the field system continued after European contact. This finding has been disputed as an “incorrect conclusion that the leeward Kohala field system was abuzz with activity in the early nineteenth century” (McCoy *et al*. 2012: 1209). Yet, contrary to the bald assertion that the Bayesian finding is “remarkable given the overall trend of post-contact rural depopulation due to introduced disease and migration to nineteenth-century port towns” (McCoy *et al*. 2012: 1209), extant historical and archaeological records indicate that the field system was inhabited well into the early historical period. A missionary census in 1832, more than 50 years after European contact, makes it clear that people were still choosing to live in and cultivate the field system.

The census counted 8014 people in North Kohala and noted that “[m]any of these live along the western shore where there is a good fishing ground, a still greater number along the line of cultivation which commences two or three miles inland” (Adams & Athens 1994). Similarly, a review of the history and cultural resources of North Kohala characterised the period 1841–1863 as one “in which settlements were consolidating on the windward side and in the leeward uplands as the declining Hawaiian population was withdrawing to more habitable and attractive areas” (Tomonari-Tuggle 1988). The transition to the historical period in leeward North Kohala is marked archaeologically by stone-walled exclosures at habitation sites, which Land Commission records suggest were built in the 1840s to protect thatched houses and kitchen gardens from feral cattle (Barrère 1983: 33). Habitation exclosures marked by deposits of mid-nineteenth century historical artefacts of non-traditional materials such as metal, ceramic and glass were recognised in the field system in the late 1960s (Rosendahl 1994), and have been identified subsequently by numerous surveys throughout the region (Clark & Rechtman 2004; Clark *et al*. 2010; Graves 1992; Graves & Franklin 1998; Hammatt & Borthwick 1986; Loubser & Rechtman 2007; Wulzen & Goodfellow 1995). This appears to be a situation where a ruling theory that “the dryland field systems [in Hawai‘i] were all rapidly abandoned within a few decades following European contact” (Kirch 2010a) is almost certainly incorrect for the LKFS and where model-based chronology building yields a result consistent with the local historical record (*pace* McCoy *et al*. 2012: 1209).

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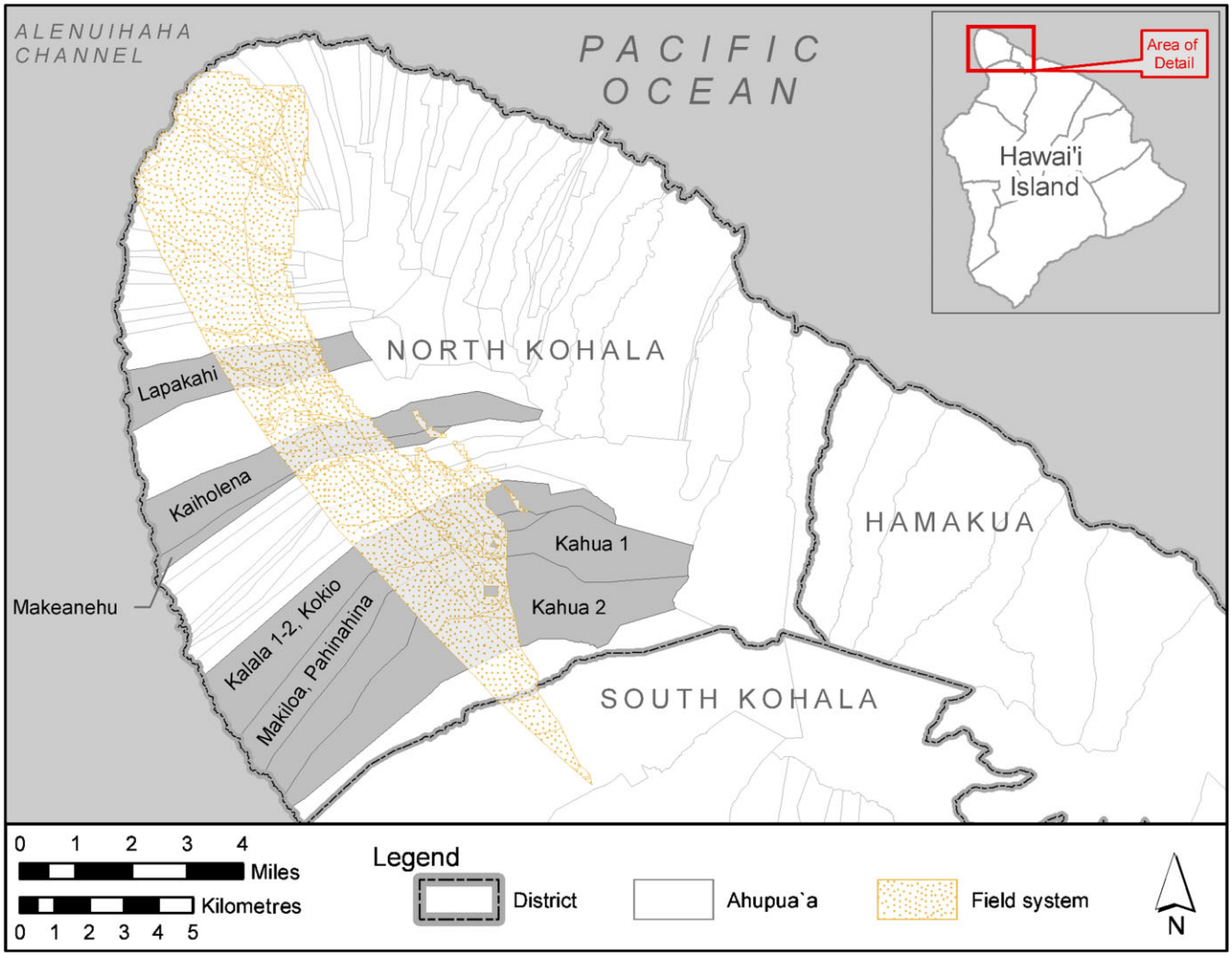
Figure 3. The location of the Leeward Kohala Field System. The shaded and labelled land units are where most of the archaeological investigations of the field system have been carried out.



The posterior distributions of field system facility construction events yielded by the Bayesian calibration are sufficiently precise to be plotted usefully within a genealogical framework (Figure 4). Facility development probably started in the field system sometime after ‘Umi a Līloa. The long left tail of the posterior probability distribution for Group 1 walls, which overlaps the time of ‘Umi a Līloa, is due to their being the earliest facilities established in the field system; estimates of their construction dates are not constrained by earlier dated events. The peak of the posterior distribution is several generations later than the genealogically reckoned time of ‘Umi a Līloa. The Group 2 trails are the earliest extant trails in the field system. They run over the Group 1 field system walls and, in the detailed study area (Ladefoged & Graves 2008), constrained the layout of two subsequent phases of wall construction. Bayesian calibration places Group 2 trail construction around the turn of the eighteenth century, about midway between the time of ‘Umi a Līloa and Kalani‘ōpu‘u. The pace of construction began to increase by the second half of the eighteenth century and it is likely that the Group 3 walls and perhaps the Group 4 trails were established during Kalani‘ōpu‘u’s lifetime. Facility development in the detailed study area drew to a close when the Group 5a and 5b walls were constructed an estimated 50–159 years (67% highest posterior density region) after the Group 2 trails.

This combination of data and model indicates a rapid development of facilities in the generations before Kalani‘ōpu‘u and continuing through to the early nineteenth century and the time of Kamehameha.

A second line of chronological evidence comes from estimates of temple construction dates in the uplands of leeward Kohala near the agricultural fields. Although the temple excavations are not fully reported, an initial publication estimated the chronology of temple development as having fairly great antiquity (McCoy *et al.* 2011), often in the face of relatively late ^{14}C dates from contexts that predate temple construction (Dye 2012). When the dating results are analysed using model-based

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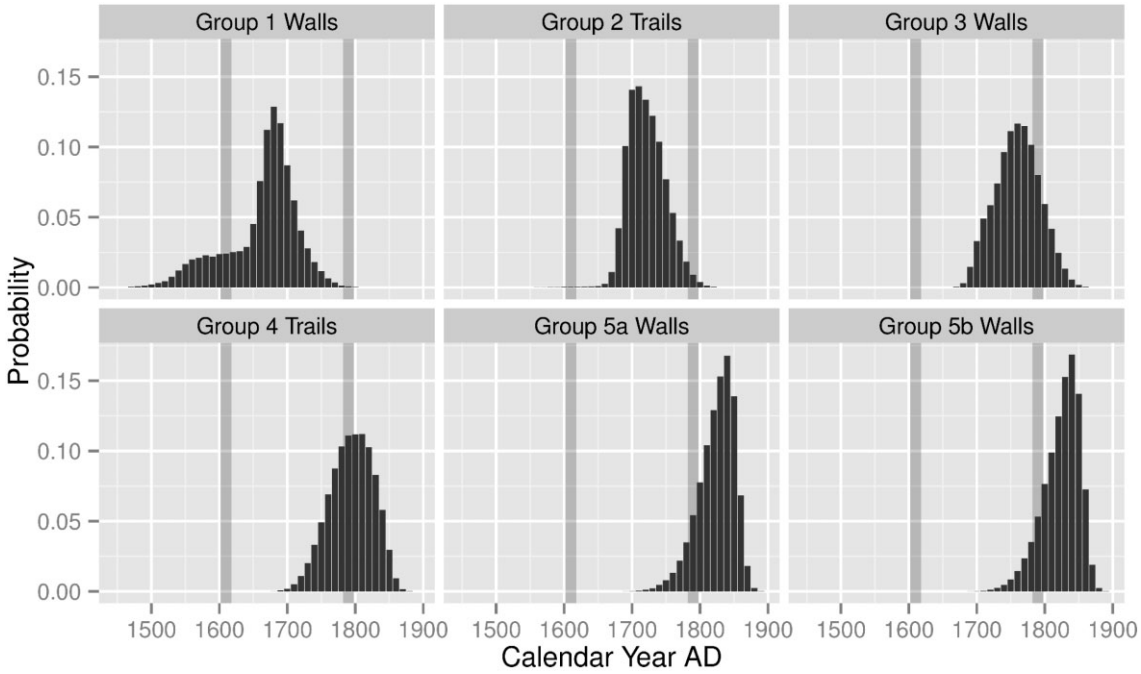
Figure 4. The chronology of dated features in the Leeward Kohala Field System detailed study area. The vertical lines indicate the approximate reign of ʻUmi a Līloa in the early seventeenth century and the time of Kalaniōpuʻu, who was *aliʻi nui* of Hawaiʻi Island when Cook visited.
Source: Dye (2011b).



chronology building, it is apparent that a series of mostly small temples was constructed throughout the field system beginning perhaps as early as the sixteenth century, but primarily in the seventeenth and eighteenth centuries, when seven or eight of the 11 dated temples were probably constructed (Figure 5). The boom in temple construction is more closely associated with the time of Kalaniʻōpuʻu than it is the time of ʻUmi a Līloa, although the dating precision here is not as great as within the field system, where the wealth of stratigraphic information yielded by the intersection of field walls and trails improves the calibration markedly. The contemporaneity of facility development in the field system and temple construction identified by the Bayesian calibration is a pattern that was obscured by the *ad hoc* chronology (McCoy *et al*. 2011), which showed a gradual increase of construction activity over a longer period of time.

The small size of many of the temples identifies them as family shrines, rather than community or polity-level temples (Kolb 2006). Their construction appears to indicate that *makaʻāinana* families had taken responsibility for intensification of production in the field system and that to some degree they subscribed to the ideological project that institutionalised *aliʻi* claims to the consumption benefits of the pig herds raised there. If it is true that pigs were typically eaten in temples late in traditional Hawaiian times (Kirch 2001; Valeri 1985a: 127), then the temple construction sequence represents a material result of social changes in the distribution and consumption of pigs. The temple construction history illuminated by the Bayesian calibration appears to indicate that pigs were distributed for consumption ever more widely during the eighteenth century, at least among males, when the number of temple structures in which pig was consumed grew dramatically.

If development of the LKFS does represent an effort to create and maintain pig herds as wealth-assets, then it is reasonable to ask if this development had any effect on Hawaiian social organisation. The answer to this question appears to be yes. Hawaiian tradition indicates that one way in which *aliʻi* might invest pig herd wealth-assets to expand their influence and elevate their status (Goldman 1970) was to finance alliances with powerful *aliʻi* families on Maui Island and elsewhere (Abad 2000: 513–27, 570–2).

Accounts indicate that the frequency of marriage alliances between Hawaiʻi and Maui *aliʻi* begins to rise a few generations before ʻUmi a Līloa and continued through the genealogical sequence, reaching a peak in the early historical period during the time of Kamehameha (Figure 6). Given the ceremonies of state reported by Cook and his crew, in which large numbers of pigs were exchanged as gifts, it is easy to imagine that marriage arrangements among *aliʻi* families were made in the context of gift-giving that included pigs. To the extent that this was the case, the increased frequency of inter-island marriage alliances attested by tradition might be seen as both a cause and a consequence of the contemporaneous development of large pig herds as wealth-assets in the LKFS, as a source of power for the line of *aliʻi* leading to

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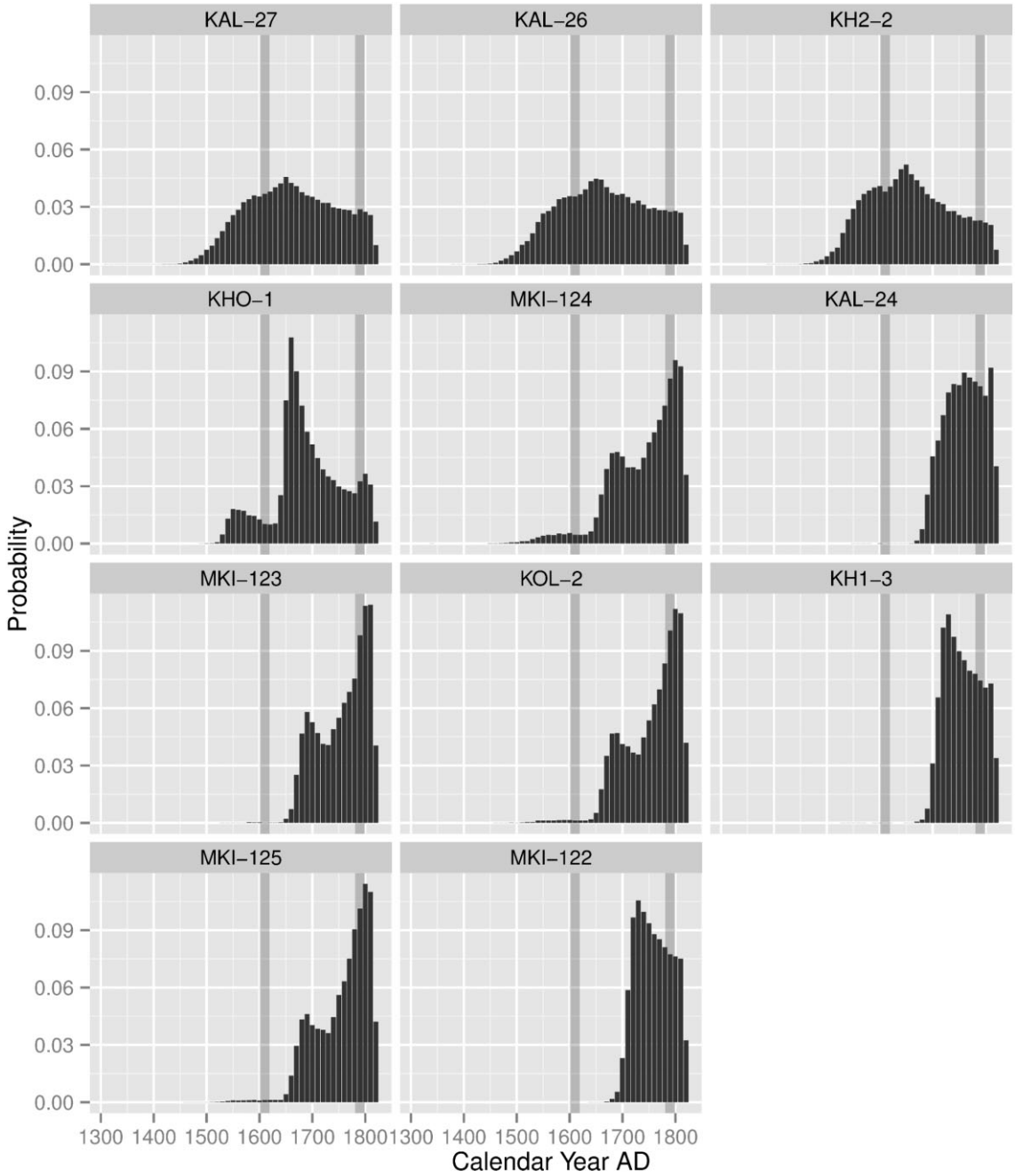
Figure 5. The tempo of temple construction in leeward North Kohala during the post-settlement period. The vertical lines indicate the approximate reign of ‘Umi a Līloa in the early seventeenth century and the time of Kalaniōpu‘u, who was *ali‘i nui* of Hawai‘i Island when Cook visited.
Source: Dye (2012).



Kamehameha and one of the means they used to enhance their status and expand their influence in traditional society.

CONCLUSIONS

Bayesian chronologies of agricultural facility development and temple construction in the LKFS, together with traditions of *ali‘i* alliances kept by the Kamehameha line, indicate that Hawaiian society was changing rapidly in the generations before Cook arrived. Intensification of production in the field system and the concomitant growth of pig herds appears to have been accomplished by families. The boom in construction of family-sized temples in the seventeenth and eighteenth centuries indicates that a family’s consumption benefit claims against the herds of pigs they managed were increasingly honoured in the context of an evolving ideology that increasingly

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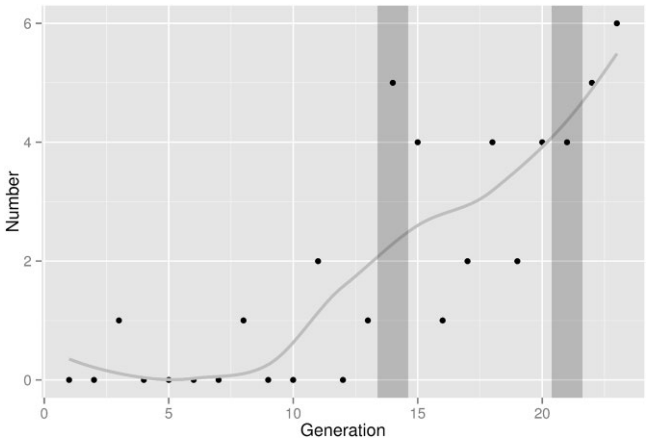
Figure 6. The generational frequency of inter-island marriage alliances, Hawai‘i and Maui. The vertical lines indicate the approximate reign of ‘Umi a Līloa in the early seventeenth century and the time of Kalaniōpu‘u, who was *ali‘i nui* of Hawai‘i Island when Cook visited.
Source: Graves, Cachola-Abad & Ladefoged (2010).



legitimated claims differentiated according to gender and status. These patterns of change indicate a society with a growing ability to produce wealth-assets. They contradict bad-year economic theories (Hommon 2013; Kirch 2010a) that posit a process of agricultural involution leading to resource shortages and social unrest. Instead, they point to a society in which *maka‘āinana* spent most of their lives within the good-year region of the production function.

The idea that variable yields of rain-fed agricultural systems in Hawai‘i were a cause of warfare that played a central role in the development of social complexity has been entertained by archaeologists for almost 30 years. The theories developed from this idea have focused on the possibility that bad yields not only reduced the amount of surplus production, but also declined to levels that threatened the subsistence needs of farmers. In this view, tension caused by *ali‘i* demands for tribute from farmers working at the edge of hunger led *ali‘i* to wage wars of conquest against their affluent irrigated agricultural neighbours. Wide-ranging efforts to find evidence for bad years in comparative ethnography, Hawaiian tradition and the results of multidisciplinary fieldwork have yielded little.

The reason for this difficult search might be that agricultural yields rarely, if ever, declined to levels that threatened the subsistence needs of farmers. Bad years were infrequent because Hawaiian society was operating well within the good-year region of the production function model (see Figure 1). However, good-year economics does not contradict the idea that variability in agricultural yield might have promoted warfare. Instead, it suggests that fluctuation of rain-fed agricultural yields led to warfare because social relations among *ali‘i* families were difficult to maintain with unpredictable swings in wealth (Goldman 1970: 204–12). Leeward *ali‘i* on Hawai‘i and Maui, able to carry out wealth-intensive social strategies during good years when pig herds were large, found themselves unable to do so when crop yields and herd sizes dropped. These fluctuations, which must have been a source of social friction in an alliance maintained, in part, by the exchange of gifts, would at times have led to wars, as one side or both came to see an alliance as less desirable than expected and felt sufficiently aggrieved to use force. This motivation for warfare might arise even in times of relative plenty, and so would go some distance to reconciling the Hawaiian traditions of warfare with the Cook-era descriptions of a thriving and bounteous society. In this view, Hawaiian *ali‘i* were not “hostile and expansionistic” (Kurashima & Kirch 2011: 3673); they were wealthy and influential.

The density-independent approach suggests two archaeological projects for the LKFS. The first builds upon the work begun by McCoy *et al*. (2011) at the family-sized temples. According to the preliminary description of this work, excavations were confined to small pits designed to get under the temple foundation to recover material for ^{14}C dating; these excavations provided data for the important problem of when the temples were built, but did not attempt to collect information with which to characterise activities carried out at the temples. If it is true, as the density-independent account holds, that the temples were constructed as places where pigs were cooked and eaten, then the question is raised whether the remains of those meals can be found by archaeological investigation. A research design for the temples would include a search for earth ovens and trash pits outside of the temple structures, perhaps accomplished by using a backhoe to strip off pasture grass around one or more temples to expose a broad area in which the tops of subsurface features might be identified.

The theory predicts that trash pits associated with temples will contain a high proportion of pig bone in relation to other types of food remains. Comparative data might come from earth ovens and trash pits associated with nearby habitation sites, identified, recovered and analysed in a comparable way. These excavations will directly investigate *maka‘āinana* activities that contributed to the development of pig herds as wealth-assets, and might be interpreted as indicating the degree to which *maka‘āinana* behaviour changed to support this *ali‘i* project.

A second project to investigate the history of walled habitation sites and the decline of the LKFS in the nineteenth century incorporates both historical and archaeological data. Land Commission records suggest that the walls were put up around habitation sites by people determined to stay on the land, but who were eventually overwhelmed by the encroachment of feral cattle and the growth of ranching, which transformed the entire field system to pasture in the last half of the nineteenth century.

First-hand accounts of the difficulties faced by farmers in North Kohala were probably published in

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Hawaiian-language newspapers of the day, and these might be reviewed relatively easily because the work of digitising Hawaiian-language newspapers of the era has made substantial progress.[2] The historical record created by this work can be supplemented by areal excavations at walled habitation sites to chart changes over time in their structure and the organisation of activities within them. Theoretical and methodological issues in studying the abandonment of settlements and regions have been worked out by archaeologists (Cameron 1991; Cameron & Tomka 1993; Smith & Gazin-Schwartz 2008) and the insights they offer might be applied productively to the situation in North Kohala. Excavations at habitation sites can document how daily routines were shaped by the introduction of cattle and the extent to which the lives of farmers changed before a traditional Hawaiian lifestyle became impossible to sustain in the LKFS.

ACKNOWLEDGEMENTS

Figure 3 was drafted by Carl Sholin and Figure 2 by Eric Komori, both based on data provided by Thegn Ladefoged. Valuable comments on earlier versions of this paper were kindly made by Jim Bayman, Geoff Boden, Caitlin Buck, Tim Earle, Rob Hommon, Thegn Ladefoged and Tim Rieth. An abbreviated version of the paper was presented at a session honouring Pat Kirch at the 2013 Society for American Archaeology conference in Honolulu.

The author is responsible for errors of fact and interpretation.

COMMENT AND RESPONSE

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Tom Dye provocatively advances several arguments: (1) that a “bad-year” economic model fails to account for the rise of Hawaiian pristine states; (2) that the Hawaiian capacity to grow pig herds was “limited only by the high natural growth rate of the pig population”; (3) that Hawaiian population was not limited by agricultural production capacity; (4) that pigs were the primary form of consumable wealth utilised by *ali’i* families in their power strategies; and (5) that commoner families “spent most of their lives within the good-year region of the production function”. I will comment – all too briefly, given the space allotted by the editor – on each of these points.

1 Dye paints a caricature of the “density-dependent, bad-year” economic model. In Figure 1, he positions bad-year economics at the extreme end of the production function (the equilibrium intersection of variables *V* and *T*). I never claimed (nor do I believe today) that the Hawaiian economy ever reached this dire inflection point. When I first presented the production-function model, I theorised that in late prehistory Hawai’i had entered a phase in which *the ability to produce surplus was declining in absolute terms* (Kirch 1984: 191, figures 50, 61). This is very different from saying that people were starving every year. Dye also erroneously pegs me as a strict Malthusian, whereas in my recent book on the rise of archaic states in Hawai’I, I am at pains to apply the more nuanced “MaB rachet” model developed by Ron Lee (1986), which integrates Malthus with Boserup (Kirch 2010a: 190–201). Hawaiian farmers were not helpless victims of a Malthusian crunch; they continually innovated (and intensified), as so much archaeological research has demonstrated.

2 Dye accuses me and others of depending on “synchronic data”, yet the evidence he adduces for the capacity of Hawaiian farmers to produce hogs relies on limited accounts of European explorers. Captain Cook certainly received unusually generous quantities of hogs during his visit. But Dye fails to temper this evidence with the unique circumstances of Cook’s encounter: (1) that he was perceived, or at least promoted by the priests, to be the arriving god Lono (Sahlins 1995), patron deity of sweet potato and the dryland field systems; and (2) that his interactions occurred at Kealakekua Bay, a royal centre inhabited by the Hawai’i Island king and chiefs, who could draw upon vast resources to assemble an unusual quantity of pigs. Absent zooarchaeological data on pig production or consumption in the hinterlands, there is no way to know if Cook’s *ad hoc* observations have any relevance to “normal” levels of pig production.

3 Dye claims that archaeological evidence for “bad years” is lacking. If he means that there is no evidence for his caricaturised view that people were starving every year, I agree. But he dismisses too cavalierly, in my view, the dramatic declines in soil fertility in both the Kohala and Kahikinui agricultural systems documented by the Hawai’i Biocomplexity Project (Hartshorn *et al*. 2006; Meyer *et al*. 2007). These are strong empirical indications that nutrient draw-down negatively affected yields. And, to suggest that pigs would “fertilise” and have a “positive effect on yields” in the dryland field systems is nonsense, given that the pigs themselves were being fed on the uptake from the same fields – this was a closed system in that sense. Dye also fails to cite the most recent demographic models of Field *et al*. (2011), which point to the core of the Leeward Kohala Field System (LKFS) having reached a density-dependent state in late prehistory, while population in the southern margins continued to grow.

4 Pigs were, without doubt, a significant form of consumable wealth in Hawai’i, primarily for the *ali’i*. Indeed, Sahlins (1992) abundantly documented the ceaseless demands for hogs made by the chiefs upon commoners in Waialua, O’ahu in the early post-contact

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era. But pigs were still a component of the "staple economy" (since they had to be fed on agricultural yields). Far more important to chiefly power strategies, in my view, was the emerging "wealth economy" based on birds' feathers and featherwork (Kirch 2010a: 220–1), and quite possibly also on the control of other durable goods, such as high-quality stone adzes. Dye does not comment on this durable wealth of old Hawai'i.

5 Dye would have us believe that late pre-contact Hawaiian society possessed "a growing ability to produce wealth-assets". I agree that the highly intensified dryland production systems of Kohala, Kona, Kaupō and other regions exhibited a remarkable capacity to produce "wealth-assets" in the form of staples, pigs, birds' feathers and other commodities, which were regularly collected and utilised by the chiefly elites to further their political aspirations. I disagree that this capacity was actually *growing*, since much evidence suggests that the systems were struggling to keep up with demand and that absolute surplus may have been declining from earlier peaks. Here, I concur with Dye that we need more direct archaeological evidence (such as quantified zooarchaeological assemblages from both residential and temple sites) to test these alternative propositions.

But we must also ask: For whom were these "wealth-assets" being created? Who benefited from the long hours of back-breaking work that went into the highly intensified field systems of Hawai'i and Maui? For the *ali'i*, no doubt, it was always a good year. As Kamakau informs us, either the *maka'āinana* ponied up the requisite quantity of tribute for their *ahupua'a* during the annual Makahiki collection, or the chiefs' warriors would plunder the territory (Kamakau 1964: 20–1). Without doubt, good-year economic theory is useful for understanding chiefly power strategies. Whether it applied equally to the "99%" of commoners is another question. The "trickle down" of some pig meat at occasional temple feasts does not mean that the *maka'āinana* were "living high on the hog".

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Comment Abstract

While in this paper Dye advocates good-year economics as a superior explanatory approach to bad-year economics in the study of the indigenous Hawaiian economy, he also notes that the two approaches are not necessarily mutually exclusive. My comment focuses on the latter point, first by suggesting that the Leeward Kohala Field System could have supported large populations of both humans and pigs, and second by showing that advocacy for either approach may generate confirmation bias.

Comment

Here, Dye presents another of his insightful contributions to the theory and practice of archaeology in Hawai'i, joining advances in chronometry, the nature of surface architecture and the application of Bayesian statistics.

While his primary aim is to advocate good-year economics as a superior explanatory approach to bad-year economics, Dye points out that both approaches "would seem to be required for completeness", a view that I agree will enhance our understanding of Hawai'i's past. For example, population growth and the development of large pig herds need not and, on current evidence, cannot be shown to be mutually exclusive explanations for the development of the field systems of leeward Kohala District and elsewhere in Hawai'i.

By applying models employing data from studies of various traditional Pacific agricultural systems and other sources (Hommon 2013: 58–63, 78–9), I suggest that the Leeward Kohala Field System (LKFS) was probably capable of supporting both a sizeable human population and, at relatively little apparent cost to that population, an abundance of pigs consistent with that evidenced in the traditional histories and late eighteenth century visitors' accounts.

For example, by estimates calculated by applying values in these models, the yield of the sweet potato fields of the 6000-hectare LKFS, lying fallow 60% of the time, could have sustained 22147 members of the district's *maka'āinana* and 500 members of its *ali'i* (chiefly) class, as well as 5344 pigs, an average of about 1.5 pigs annually for every six-member *maka'āinana* household. The pigs raised in the system could have provided 167 pork-consuming male members of chiefly families with 30 kg of pork per month throughout the 8 months of the year when pork was normally consumed. Management of the pigs would have required the equivalent of 668 full-time pig-herders, or 3% of the *maka'āinana* population.

The estimates generated here are, of course, presented not as conclusions but, rather, to demonstrate a useful way of exploring some of the variables relevant to the field system's production. In my view, such thought experiments, as long as assumptions such as fallow periods, caloric content, crop yield and labour are culturally relevant and specified, can help guide our thinking about how to study the system's use and development.

The statement by Earle (1978: 183), cited here by Dye and elsewhere by various authors over the decades, that agricultural intensification "was an outcome of political competition and *not* of population pressure" was based on Earle's research in the archaeology and ethnohistory of Halele'a District, Kaua'i. He observed that "only the prime areas of alluvial soils were farmed intensively aboriginally. Even the alluvial soils were greatly underutilized, as shown by the later, tremendous expansion in irrigation agriculture for rice. The

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inattention to *kula* [rain-fed] farming and the restriction of irrigation farming cannot be explained on technological grounds" (Earle 1978: 163).*

Earle evidently assumed that had agricultural development been generated by population pressure, the residents of Halele'a would have extended cultivation beyond the "prime" alluvial pondfields by building dryland field systems similar to those of Hawai'i Island and Maui to bring adjacent rain-fed areas under cultivation.

"Since this expansion was technologically possible, it seems reasonable to conclude that it was not necessary. For Halelea and most areas with irrigation, Hawaiian agriculture does not show the intensive development normally associated with population pressure"* (Earle 1978: 163–4).

Recent research by the Hawai'i Biocomplexity Group has provided a more plausible explanation for the absence of extensive traditional Hawaiian rain-fed field systems other than those on the geologically young slopes of eastern Maui and Hawai'i and certain rejuvenated environments. Slopes of the older islands, including Kaua'i, having lost soil nutrients through leaching, were too infertile for traditional Hawaiian cultivation (Hartshorn *et al*. 2006; Vitousek *et al*. 2004). Earle (1978: 106–7) himself presents what may be a practical explanation for the lack of development of steeper alluvial lands in Halele'a valleys. The Halelean farmers may have calculated that terracing these slopes yielded insufficient return on investment.

"For a pondfield of constant size, the volume of earth moved in construction increases geometrically with the natural slope of the land. By decreasing the average size of a terrace on steeper slopes, however, the volume of earth moved for a given area may be held constant. As the size of a pondfield decreases, however, the percentage of land area devoted to bunds dramatically increases"* (Earle 1978: 106–7).

The farmers of Halele'a may have decided that the potential benefits of bringing relatively minor additions into the field system would not justify the costs of construction and maintenance.

In somewhat the same way that this account of Earle's advocacy of the density-independent view highlights the value of seeking alternative explanations for observations (in this case the absence of evidence of intensification beyond the boundaries of pondfields on prime alluvial land), Dye's discussion of the terms "famine" and "famine foods" cautions us to exercise care in translations and definitions of terms that may seem to support the density-dependent view. While the term "famine" usually refers to severe and long-term lack of food resulting in high rates of death, it seems clear that in Hawai'i before Western contact, while certain normally unimportant food plants were exploited during times of shortage of the major staple crops, such events were usually limited in severity, duration and geographical extent (Hommon 2010: 22).

*Corrections added on 11 April 2014, after first publication online: Quotation marks have been added to text in the section above to attribute the quotes to the referenced author.

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Dye's analysis of social and political complexity in Old Hawai'i exemplifies the turn to an increasingly well-founded evolutionary archaeological science of Pacific Island populations (e.g. Allen 1996; Field 2004; Ladefoged, Lee & Graves, 2008; Lipo *et al*. 2010). Although his paper is not a programmatic call, the manner in which Dye has carefully analysed data-generation procedures, his concern with the links between models and observations, and his fundamental reliance on concepts of competition for resources (status, pigs, arable land) indicates that he is concerned with both science and evolutionary change. Here, I want to briefly discuss two fundamental characteristics of evolutionary science (indeed any science), both of which play a role in Dye's analysis, and argue for their continued development and use in the archaeological study of sociopolitical complexity in the Pacific Islands, and Pacific archaeology generally. Undoubtedly, others will be more concerned with substantive observations that challenge Dye's account of sociopolitical change. These debates about data are important and necessary, but they are insufficient without the theoretically justified and empirically relevant framework that evolutionary science provides.

Two uncontroversial, and therefore fundamental, characteristics of science are (1) the use of universal concepts and mechanisms (theory) to explain empirical phenomena, and (2) that observations of phenomena are made with units linked to theory, and unambiguously applicable to the empirical world. The best scientific explanations match empirical observations to expectations derived from theory or cannot be falsified. Although this is an exceedingly simple characterisation, many will note that it is different from the typical social science approach, where observations may have no necessary link to theory (they often come from our common sense) and empirical observations are explained by subsuming them under an existing empirical generalisation. Willer and Willer (1973) call this approach to research "systematic empiricism".

Empirical generalisations, however, are often a necessary starting point, as they tell us about possibly important patterns in the world. Dye begins his paper by describing the empirical generalisations used by Kirch (2010b) and Hommon (2013) in their recent analyses of Hawaiian sociopolitical complexity. These authors each assemble a series of archaeological, ethnohistorical,

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palaeoenvironmental and other observations to correlate changing Hawaiian population levels with the amount of subsistence resources. For Hommon and Kirch, the occasional imbalance between relatively high population levels and inadequate subsistence resources is a key driver of sociopolitical change in what Dye terms a density-dependent or bad-year economics approach.

Dye's critical review of the observations that are the foundation of the population-resource generalisation underscores the importance of how we construct observational units in an archaeological science. Dye notes that "inferences" of ancient population size are problematic, as there is no unit by which we can unambiguously observe population size in the archaeological record. Neither archaeological house counts nor radiocarbon date frequencies (the Dye–Komori method), as typically generated, allow us to observe population size in a manner sufficient for evaluating potential explanations in a scientific framework, regardless of the use of these units to make general, relative comparisons. As an alternative to Kirch and Hommon, Dye constructs an empirical generalisation that correlates changes in dryland farming intensity, *ali'i* genealogies and temple construction, primarily family shrines. As he suggests, the observational units that Dye employs are arguably more directly linked to the archaeological record than units used to calculate population size. They are therefore better from a scientific standpoint, but there is still room for refinement. For example, what are the necessary and sufficient archaeologically observable criteria for identifying family shrines within the field of surface stone remains? Theoretically informed artefact classifications are necessary to address questions such as this.

In addition to building appropriate classifications or observational units, universal concepts and mechanisms are used to explain phenomena in a scientific archaeology. In contrast, the explanations typically offered for changes in ancient Hawaiian sociopolitical complexity are empirical generalisations, founded on principles of competition for resources, but lacking a clear mechanism. For example, Hommon (2013) proposes that sociopolitical change in ancient Hawai'i is explained by chiefs asserting greater control over subsistence practices during times of hardship. This explanation is a product, in part, of subsuming the observations of the Hawaiian archaeological and ethnohistorical record under another empirical generalisation correlating twentieth century Tikopian chiefly practices and historically documented environmental stress. We may ask, though, why are these apparently status-linked behaviours differentially transmitted and why do they persist in multiple times and places? What is the mechanism? Here, human intention and agency is often thought to offer a mechanism, but these are problematic as explanations in evolutionary science (Mesoudi 2008; Ormerod 2005). Although Dye's good-year economics model articulates better with observations of the archaeological and ethnohistorical records, it relies in part on determining the intentions of individuals in the past to explain the course of history; for example, that "*ali'i* might invest in pig herd wealth-assets *to expand their influence and elevate their status*" (second italics not in original).

The explanations proposed by all of these authors are founded on concepts of competition for resources, concepts that are already part of evolutionary theory, and it is from here that we should derive our explanatory mechanisms. For example, Ladefoged *et al*. (2008) discuss the probable intentions of chiefs in the development of the Leeward Kohala Field System, but the universal explanatory mechanism that shapes the distribution of chiefly behaviours is selection, irrespective of an individual chief's intentions.

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Tom Dye's paper provides provocative insight into the importance of pigs in the political economy of protohistoric Hawai'i. The historical sources summarised by Dye stress the abundance of pigs probably as the result of extensive dryland sweet-potato farming there. His paper has been chosen for commentary because the significance of this observation was seen as controversial. My commentary builds on the history of Oceanic archaeology dealing with agriculture to suggest why the paper might make some scholars mad, but how in fact it suggests a reconciliation between two materialist traditions that have often been seen in conflict.

Dye's paper refocuses our attention on one of the great themes in Oceanic anthropology and archaeology – the relationships between agricultural systems and the development of complex societies. He describes two approaches – density-dependent (what he calls bad-year economics) versus density-independent (good-year economics) models. These two approaches can perhaps best be understood in terms of the history of materialist studies in anthropology. The "density-dependent" approaches derive from ecological anthropology, as articulated by Julian Steward in the 1950s and represented in archaeology through settlement pattern studies introduced by Roger Green to Pacific archaeology in the 1960s. Settlement pattern studies stress the long-term adjustment of societal organisation to changing subsistence challenges involving growth in population and agricultural intensification. The "density-independent" approach derives from political economy approaches articulated by V. Gordon Childe and ultimately from Karl Marx. These emphasise surplus mobilisation and use in power strategies in the creation of social inequality.

Historically, these two approaches have been set against each other, largely because of Marx's hatred for Malthus' blame-the-victim argument that poverty was caused by

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over-breeding by the lower classes. More recently, Friedman (1974) labelled those working with ecological models as “vulgar” Marxists, creating a modern schism in materialist approaches, with resentment among those using an ecological approach. Dye’s paper suggests a means of rejoining the two approaches as they refer to the role of agricultural production in protohistoric Hawai’i.

In *Social Stratification in Polynesia*, Marshall Sahlins (1958) originally framed eclectically the relationship between agricultural productivity and social complexity. Sahlins sought to explain the extent of social complexity as a result of adaptive radiation to diverse island environments (from atolls to large volcanic islands), and he combined the theoretical approaches of his two professors Steward (at Columbia) and Leslie White (a maverick Marxist at Michigan). Because of his creativity, Sahlins forged a corrective synthesis between the opposed positions of ecological and political approaches represented by his professors. Subsequently, however, he shifted first towards a more ecological approach articulated by his collaborator Elman Service and then towards a more structural Marxist approach. His extremely articulate (but shiftless) work has caused a widening in the schism of materialist approaches based on internal contradictions in his arguments.

Subsequent to Sahlins’ seminal book, and with the introduction of settlement approaches à la Steward, the ecological approach has become foremost in Pacific archaeology. The finest exponent has been Patrick Kirch. In terms of agricultural studies in the Pacific, Barrau distinguished between wet (irrigated) and dry farming. Contrasting hydrologic conditions available on islands channelled these alternative farming strategies with probable outcomes for social evolution. In *The Wet and the Dry*, Kirch (1994) then argued that the relative unstable conditions of dryland farming created intense warfare as population growth caused increased competition over limited and risky resources. In collaboration with Kirch, Ladefoged *et al*. (2010) have conducted the most influential study of the varying pathways (irrigated versus dryland farming) that supported the emergence of Hawaiian chiefdoms and subsequent states. They demonstrate that across the Hawaiian Islands, a remarkable contrast existed between the western islands where wet (irrigated taro) farming dominated and the eastern islands where dry (mostly sweet potato) farming dominated. Their modelling showed that the dryland farming was both less productive and higher risk than irrigation farming. To Kirch (2010a), the increased risk of dryland farming on the eastern islands helped explain why pristine states emerged there and not on the more productive zones of irrigation that characterised the western islands. This ecological approach emphasises how polities emerged to solve challenges (bad-year economics).

The “alternative” political economy approach is represented in the Pacific most clearly by Matthew Spriggs and myself. Sahlins recruited me to show that Wittfogel’s functionalist argument was wrong for the Hawaiian case (Earle 1978, 1980, 2012). This was easy: the Hawaiian irrigation systems on Kaua’i were not large scale (a necessary condition for Wittfogel) and so did not require central management. Rather, the engineered landscapes of irrigation were highly productive (capable of producing a surplus), sustainable and easily controlled by an emergent elite. For Vanuatu, Spriggs (1985, 1986) emphasised that erosion caused by dryland farming created the build-up of alluvium that allowed for intensification with irrigation. Thus underlying ecological conditions created the potential for controlled surplus production that fuelled political elaboration (good-year economics).

Are the two materialist approaches in opposition? I do not think so, and Dye makes the implicit argument that they are complementary. To a large measure, he agrees with the significant scholarship on the dryland field systems conducted by Kirch’s collaborative group, but the value added by Dye’s analysis brings in an explicit political economy perspective. The introduction of sweet potato to the Hawaiian Islands provided an added means of intensifying agricultural production to support both an expanding population and an expanding political economy. Although less productive and higher risk than irrigated taro farming, sweet potatoes provided the ideal food for pigs that were a moveable wealth for political marriage and sacred economies. Although lower in productivity and higher in risk, the sweet potato fields thus supported the production of a key moveable wealth; unlike taro, pigs could be moved regionally to support political strategies at some distance.

No topic could be as timely, given the recent high-profile publications about Hawaiian prehistory (Bayman & Dye 2013; Hommon 2013; Kirch 2010a). Dye’s paper demonstrates the importance of pigs as wealth in the political economy, which has been largely missed in previous studies. He provides a clear example of how ecological approaches can be linked to political economy approaches to give a powerful understanding of social evolution. Why did the irrigation-based chiefdoms on the western islands not develop into states? Probably, it related to limits on scale for the smaller islands. The development of moveable wealth based on the raising of sweet potatoes appears to have allowed the spatially much larger polities based on military conquest and ideological elaboration (Kirch 2010a). In simple terms, the two approaches of bad- and good-year economies are complementary. The old nineteenth century controversies can be overcome to create integrative, materialist theories for social evolution.

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The pre-European formation of the Hawaiian kingdoms or archaic states has been of archaeological interest since the

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1970s, when Tim Earle, Rob Hommon and myself were doing our theses on this topic (Cordy 1974a,b, 1978, 1981; Earle 1973, 1978; Hommon 1972, 1976). There has been work since then by Hommon, Earle, myself, Pat Kirch, Jane Allen, Michael Kolb, Boyd Dixon, Dennis Gosser, Kehau Cachola-Abad and others. Kirch and his colleagues have recently studied aspects of this topic, focusing on rain-fed agricultural fields in leeward Kohala. There are the recent books by Kirch (2010a, 2012) and Hommon (2013), and also some key papers by my colleague Tom Dye (2010a, 2011b), to which we can now add this paper.

This is a topic that focuses on changes in political organisation within polities over time, with the rise of elite control. Over the years, different hypotheses have been generated on what changed in political organisation and when, and on what associated cultural variables may have caused these changes. There is still disagreement on when political organisation changes occurred (see Cordy 2012, 2013), and I would argue that no causal hypotheses have been successful evaluated.

Tom's paper brings up some important points. He disagrees with the causal hypothesis of Kirch and Hommon that population increased to levels approaching production limits in rainfall-field systems of Maui and Hawai'i, threatening available surpluses for those chiefs, leading the chiefs to undertake conquest warfare, resulting in social complexity. Tom points out that evidence cannot yet be marshalled to support this hypothesis. He emphasises that there is no sound archaeological evidence (a) for population levels approaching production limits, as population histories have not yet been accurately reconstructed for any point in time prior to contact, and (b) for indications of bad field years. He suggests that oral historical evidence for famine is rare (listing cases) and does not show resulting mortality, social disruption or war. He clearly doubts that bad agricultural years were frequent, or threatened subsistence needs. Also, his Bayesian dating for Kohala field intensification suggests that it occurred after key political changes (also Dye 2011b: 28–31). These are telling points that others must consider.

Tom proposes status rivalry as the causal factor (independent of population growth), with wealth being accumulated by chiefs for use in status rivalry, this wealth including chiefly pig herds. He suggests that fluctuations in this wealth prevented chiefs from maintaining alliances, and led to conquest and social complexity. He gives a few examples for contact-era consumption of large numbers of pigs at chiefly feasts and of unusual amounts of pigs supplied to Cook's expedition. While I agree that pigs were important wealth items used by chiefs in the era of concern, I believe that there is little understanding of pig use at contact and even less evidence from pre-contact archaeological reconstructions. Pigs were probably differentially consumed in the diet of commoners, low chiefs, high chiefs and rulers; used in differing numbers in special feasts by these strata; and offered up in differing numbers at the religious places of these strata. They were also used to provision armies. None of this information has been carefully compiled from the historical sources of the contact era, nor how pigs were used in status rivalry (no evidence has yet been shown that they were used in marriage alliances), nor the nature of chiefly pig herds (herds are rarely noted, and it is unclear how many of the pigs supplied to foreign ships were from chiefly pig herds or from demands on commoner households). Even if we improve this contact-era picture, archaeology will have to determine amounts of pigs at different points in time that were used by chiefs for feasts and rituals; and this will not be an easy task, as excavations in houses and religious sites are usually limited, and recovered pig bone is often a few fragments (e.g. Kolb, Conte & Cordy 1997; but see Kolb 1999). Also, Tom's claim that proxy archaeological evidence of increasing dryland fields and small family temples directly reflect increasing pig herds used for status rivalry seems shaky. Tom cites Kaua'i as a major pig island based on Cook expedition journals, and my work long ago showed that O'ahu became the major pig-producing island of the early 1800s (Cordy 1972). Both islands had irrigated taro farming as their dominant cultivation approach, suggesting that pig numbers' relationship to the proposed proxy variable of rainfall-fields is not so straightforward. Also, commoner temples do not seem to have a tie to pig wealth of chiefs; rather, actual pig offerings at major temples of high chiefs and rulers seem more relevant. Kolb's study of Maui national temples, which suggested two periods of major pig bone offerings (1500–1650 and 1650–1820), is a more relevant finding (Kolb 1991, 1994, 1999).

Tom's points about the inability to measure variables in Kirch's and Hommon's hypotheses are very important ones, and they seem to apply to Tom's hypothesis too – and to nearly all our hypotheses offered to date. We can generate interesting hypotheses about what changed and why, but we need to diachronically measure key cultural variables of the hypotheses to evaluate them. If one is using archaeological data, such measurement is extremely difficult. It is true for reconstructing population at different points in time, as Tom notes (other issues on measuring population exist; e.g. Cordy 2007; Kirch 2007). It is also difficult to archaeologically reconstruct levels of agricultural production and surplus (as Tom notes) and levels of pig production and surplus. It is true for reconstructing changes in political organisation (number of social strata, number of administrative levels of chiefs, the end of kin-controlled local chiefs and land, etc.), and for associated religious patterns (changes in the size and offerings at national temples). Many of us have made contributions to archaeologically reconstructing these cultural variables – myself and Tainter on identifying political hierarchies or strata (Cordy 1981, 2001; Tainter & Cordy 1977), Dixon and Gosser on the possible end of kin control in local leadership (Dixon *et al*. 2008; see also Cordy 2004), Tom himself on the end of kin control of resources (Dye 2010a), Kolb (1994, 1999) on changes in

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national *heiau* size and associated offerings, Allen on irrigated agriculture (Allen 2000), and Kirch's folks on dryland fields and soil depletion (cited by Tom). But we are a long way from archaeologically measuring these variables at different points in time – identifying key political organisation changes, measuring associated cultural variables (population size, pig production, temple sizes, crop production levels, warfare etc.) and determining if key changes in these variables precede the political changes, and are perhaps causal, or if they follow the changes.

One of our biggest problems is fine chronological dating, as Tom and others have noted. I believe that Tom's use of Bayesian dating will prove useful in refining our chronologies. So too will the uranium–thorium coral dating of some religious structures that Kirch has introduced. Resolving the problems of hydration dating could also help. Until we have more accurate dating, we have problems studying this research question.

Yet another problem exists. In this research question, the community (*ahupua‘a*) or region (*moku* such as Kohala, Wai‘anae) are not the focal area. The changes are in political organisation of a polity. Our reconstructions of variables must not lose sight of what was the polity pattern. We have only recently started to look at regional or *moku* patterns (e.g. in Wai‘anae, Kula, Kahikinui and Kohala), and the diachronic picture of key variables is far from complete for any of these *moku*. Yet patterns of one *moku* do not necessarily equal the patterns of its polity. Our regional studies so far are in lands that generally only had resident commoners and local chiefs. Research has not focused on lands that were political centres (where rulers and high chiefs lived). We have bits of information for these areas from contract archaeology and some research work at best.

Indigenous oral history is another major data source that can address this problem. I disagree with Tom; oral history is diachronic. It provides details for pre-European times on polity sizes over time, variables of political organisation, warfare patterns and so on. However, it too has complexities as a data source that have to be carefully dealt with. To note a few, some oral histories were probably modified in pre-European times. Others recorded in the 1800s may tell their stories with elements of contact culture that did not exist in the past. Also, oral histories recorded from contact up into the 1860s are more accurate accounts at European contact; more recent oral histories blend the earlier accounts and alter them, and must be used much more carefully. Also, oral history goes back accurately only to about AD 1300, and in the 1300s it is spotty. Additionally, one has to be very careful not to over-interpret some of the material. I have been critical about weaknesses in the oral history presentations of Kirch (Cordy 2012, 2013). Nonetheless, I have long found this data source an extremely powerful tool to study this research problem and measure key variables.

In sum, I find Tom's points on pigs as wealth important to consider in the development of Hawaiian political organisation, as I find Kirch's group's Kohala work on dryland fields. However, until we can diachronically measure the variables of these hypotheses, they are only fascinating ideas.

Last, I agree that status rivalry is involved in these changes, or the desire for more mana. However, status rivalry existed in the smallest of Polynesian polities (e.g. Marquesas, Maori). For Hawai‘i, we need to determine what associated factors led to the formation of bigger polities and elite control. This clearly needs much more work, and creative ideas such as this paper of Tom's.

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Dye presents a theoretical dichotomy between bad-year and good-year economic approaches.

He characterises density-dependent bad-year economics as stressing variable yields in rain-fed agricultural systems leading to food shortages and reduced tribute, with a result being the stimulus of conquest warfare with more affluent irrigated agricultural areas. This characterisation is a caricature of the approach, and misses several important elements, including the insight of Kirch (1994) that it is not just rain-fed variable production that is the stimulus but, rather, the differential in reaching the inflection point of the intensification curve in rain-fed versus wetland areas. As an alternative, Dye suggests that Hawaiian society was generally based on a good-year economy, with agricultural yields rarely, if ever, dropping below the subsistence needs of farmers. In this view, everyone had enough food but variation in production would have impacted on the elite's ability to engage in wealth-intensive social strategies such as gifting to reinforce marriage alliances. It was apparently this tension and the frustration over the ability to give and receive gifts that led to warfare. Dye maintains that there is little archaeological evidence to support density-dependent approaches, whereas there is ample evidence for a well-fed, wealthy elite-led, Hawaiian society engaged in periodic warfare. As specified, Dye's good-year economic explanation is unsubstantiated and far too simplistic. In an effort to distinguish it from bad-year economic approaches, Dye creates a dichotomy between the two that is overdrawn. Elements of structuralism and praxis are currently incorporated into the bad-year economic approaches of historical ecology and ecodynamics. The empirical foundations of these approaches, including the archaeological (see Kirch *et al*. 2012 for a summary), geochemical (see Vitousek and Chadwick 2013 for a summary), ethnohistorical (Abad 2000; Graves *et al*. 2010) and experimental (Kagawa & Vitousek 2012) data, are currently much firmer than those of Dye's suggested alternative.

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Dye's conclusions about the timing of agricultural and religious development within the Leeward Kohala Field System (LKFS) are based on his Bayesian analyses (Dye 2011b, 2012) of previously published data (Ladefoged & Graves 2008, 2010; McCoy *et al*. 2011). Dye claims that Bayesian calibrations have the potential to revolutionise our archaeological interpretations, yet because his Bayesian results are so sensitive to the assumptions of his model, there are limitations. For example, Dye (2011a: 28) makes the *ad hoc* assumption that the temporal boundary for the last period of agricultural construction in the LKFS should be based on a "normal curve with a ten year standard deviation centred at AD 1850". As noted by McCoy *et al*. (2012: 1208–9), "the effect of choosing AD 1850 . . . [is that] . . . it forces the posterior probability to ramp up, making it appear as if all activity in the field system occurred in the years immediately prior [to AD 1850]". Unfortunately, the fundamental assumption in Dye's Bayesian model that agricultural development continued until AD 1850 skews the results of his model towards the historical period. Furthermore, the results of Dye's (2011b) Bayesian analysis are heavily influenced by a strict adherence to the original field recording and analysis of Ladefoged and Graves (2008). If the original recording of the spatial relationship of a single wall (in this case, wall "c" intersecting trail "A", as labelled by Dye 2011b: Figure 2) was erroneous, it would significantly undermine Dye's conclusion that much of the intensification of the LKFS is a post-contact phenomenon. Given the inherent ambiguity of some field decisions about whether a wall intersects or abuts a trail, it is inappropriate to create a dating model and derive conclusions about the intensification of the entire LKFS based on a method that can be significantly influenced by the misidentification of a single wall. While Bayesian analysis can contribute to our understanding of past behaviours (e.g. Field *et al*. 2011), the scale of the data and the assumptions of any model must be closely monitored to achieve anything close to revolutionary insights.

Dye places a considerable emphasis on pig husbandry, yet he is not the first to identify the importance of this activity and the generation of surplus wealth in the LKFS. Lockwood's (2009) detailed analysis of lipid biomarkers and XRF of elemental nutrients in soils collected from various architectural structures in Lapakahi identified spatial and temporal trends in pig husbandry. Lockwood (2009: 242) concludes "that growth in the scale of surplus production (as measured by pig husbandry) lagged behind growth in the scale of agriculture and subsistence requirements". While acknowledging the limitations of his analysis, Lockwood suggests that greater growth in pig husbandry occurred during the earlier periods of occupation as opposed to during the late prehistoric and early historical eras. Dye's citation of synchronic traditional and historical accounts noting the importance of pig production for wealth-assets is useful and limited, and Lockwood's analysis is an excellent first step in establishing diachronic trends within the LKFS.

An emphasis on good-year economics is an interesting alternative for understanding Hawaiian sociopolitical development in general, and developments within the LKFS specifically. However, it would seem that in his enthusiasm to distinguish the perspective, Dye has overstepped the data. On the basis of his Bayesian analysis, Dye proposes that the southern part of the field system was primarily developed in the eighteenth and early nineteenth centuries, with much of the development throughout the field system being a post-contact phenomenon (Dye 2011b: 29). While it is probable that people continued to live in and to a limited extent, develop the LKFS during the early historical era, despite Dye's assertions there is no strong archaeological evidence that significant amounts of agricultural development occurred during the historical era. Ranching in the LKFS did begin in the nineteenth century, but the archaeological remains of these activities overlay the gardening features. Dye concludes that his Bayesian chronology for LKFS development is sufficiently precise to correlate it with known historical figures. Yet this is hardly the case, as he only specifies three people (ʻUmi a Līloa, Kalaniʻōpuʻu and Kamehameha) who lived within a time period that spanned some 200–250 years. His association of these individuals with the field system is hardly novel. His suggestion that developments within the LKFS represent an effort to create and maintain pig herds as wealth-assets as opposed to other subsistence- and surplus-related activities, while intriguing, is not well substantiated. Dye is correct that we should avoid *ad hoc* interpretations and ground our explanations in empirical data, and future research should test the assumptions and validity of both good-year and bad-year economic approaches.

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The radiocarbon revolution in archaeology is in its third phase (Bayliss 2009). The initial phase in the 1950s and 1960s saw archaeologists in Hawaiʻi use the imprecise results of the new method to explore the time depth of Hawaiian prehistory, an area of inquiry that was previously the sole province of genealogists. The development of calibration in the 1970s, along with the increasingly precise results yielded by technical improvements at the dating laboratories, made it possible for archaeologists to evaluate and compare local processes of change. The move away from diffusionism during this second phase of the revolution led in Hawaiʻi to a concern with how the state institutions of traditional society developed *in situ*, a project that has borne considerable fruit (e.g. Hommon 2013; Kirch 2010b). The third phase, which was introduced by the development of Bayesian calibration, bridged the gap between the chronological data yielded by

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radiocarbon dating and other sources of relative and absolute chronological information typically available to archaeologists, most notably the stratigraphic information recovered by careful archaeological excavation. Combining these disparate sources of information makes it possible for archaeologists to achieve undreamt of levels of chronological precision. In the United Kingdom, where application of Bayesian methods is most advanced, archaeologists have radically revised their understanding of the early Neolithic period.

Detailed chronologies of change, which at some early Neolithic sites approach generational timescales, have shown archaeologists there how "a very different kind of history can emerge" (Whittle *et al*. 2011: 1), one that brings into focus "the social context in which agency, change and the choices of individuals and communities occurred" (Whittle *et al*. 2011: 4).

The fine-grained chronologies of the development of agricultural infrastructure and construction of family-sized temples in the Leeward Kohala Field System (LKFS) – the centrepiece of my paper – are products of this third phase of the radiocarbon revolution. Cochrane's comment notices the positive effect that fine-grained chronologies of change have on interpretation, the only comment to do so. This might be taken as an indication that the third phase of the radiocarbon revolution is just beginning to penetrate the thinking of Hawaiian archaeologists. Another indication is Ladefoged's criticism of one of the prior probabilities used in the Bayesian model for the agricultural infrastructure. Model-building is central to the Bayesian project because it provides the structure needed to integrate radiocarbon with other sources of chronological information. Thus, it is important to understand that the prior probabilities of the model are based on specific pieces of historical and stratigraphic information and not on *ad hoc* assumptions. In fact, the historical basis for modelling the end of field system development in the mid-nineteenth century includes census data and the archaeological record of enclosed habitation sites with historical-era artefact deposits, and is set out in detail in my paper. Ladefoged's comment suggests that the prior probability should be changed, but does not indicate how it should be changed. To be clear, treating this prior probability as an assumption that can be altered so that the posterior probability takes on particular characteristics – for example, so that it does not "ramp up" (McCoy *et al*. 2012: 1208–9) in the nineteenth century – would be antithetical to scientific inquiry. Similarly, dropping a "strict adherence" to data is not an option open to a scientist. One possible way forward is to marshal historical information on when field system development stopped, revise the model accordingly and demonstrate the difference it makes. The tools for this kind of analysis are well developed and freely available. It is just a matter of whether or not the historical data can be found. In any event, I certainly agree with the larger point behind Ladefoged's criticism, namely that the demise of the traditional Hawaiian field system is ripe for additional investigation. My ideas on how this might be carried out are presented in the paper, but other approaches are certainly possible.

I am extremely grateful for Earle's claim that my argument for the importance of pigs in the political economy of protohistoric Hawai‘i "suggests a reconciliation between two materialist traditions that have often been seen in conflict". I believe the key here is Bell's cross-cultural concept of wealth-assets, which makes it possible to distinguish the large pig herds encountered by Cook and others as wealth-assets, rather than consumables.

The idea that traditional Hawaiian pig herds were created and managed as wealth-assets is resisted by Kirch, who (i) argues that Cook was mistaken about the size of the pig herds because his observations were made over a short period of time at the royal centre of Kealakekua, where he was treated as a god, and (ii) espouses an approach that treats pigs as "a significant form of consumable wealth". However, the historical record for large pig herds is robust. My paper cites three English sources – Clerke, King and Vancouver – and notes native testimony of large pig sacrifices. Abundant pigs were reported throughout the group and not just at Kealakekua. As Cordy notes in his comment, Clerke singled out Kaua‘i Island for its large pig herds, and Hawaiian tradition claims that Captain Brown got 400 pigs at O‘ahu (Kamakau 1992: 169). The surfeit of pigs at Kealakekua was not a one-time aberration. Abundant pigs were noted by Cook and his crew at Kaua‘i in 1778 and by Vancouver when he visited the islands 15 years later in 1793. The Brown incident at O‘ahu took place in 1794. Cook participated in a wide range of pig exchanges, not just ones that might be interpreted as gifts to a god, and was well aware of differences among them (Dye 2011a). He risked mutiny to create conditions in which he hoped to barter and not exchange gifts. Once he achieved this, he found that Hawaiians drove a keen bargain and were willing to take their pigs and other foods home "rather than despose of them for less than they demand" (Beaglehole 1967: 483). This is not the behaviour one expects of a person making offerings to a god.

It is important to understand that treating pigs as consumables limits an analysis of their role in traditional Hawaiian society. This can be shown by comparing Kirch's concept of "consumable wealth" with Bell's concept of wealth-assets. Bell calls the four criteria for the cross-cultural identification of wealth-assets the growth criterion, the consumption criterion, the marginal value criterion and the indefinite life criterion. A valued item that fails to meet one or more of these criteria is a consumable. Kirch's comment mentions the growth criterion – consonant with his longstanding interest in the relationship of pig production to population size – and the consumption criterion when he characterises the distribution of pigs in traditional Hawai‘i. However, his comment stops here, making it clear that his concept of "consumable wealth" does not include the marginal value or indefinite life criteria. Restricting the definition of

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wealth in this way conflates consumables with wealth-assets, and makes it impossible to see the constructive ways in which Hawaiian chiefs created and managed pig herds as wealth-assets. In addition, pigs and the agricultural fields that supported animal husbandry are both visible in the archaeological record, unlike other traditional Hawaiian wealth-assets such as feather cloaks and large canoes, which are mostly invisible to archaeologists. Furthermore, exclusive concern with the growth and consumption criteria draws a simple opposition between commoners as producers and chiefs as consumers. The political dynamic generated by rivalry for status among the chiefly nobility, indicated so clearly in the comparative structural analysis of Polynesian societies (Goldman 1970), is lost in this view. It is difficult to imagine that there is something to gain by ignoring this part of Hawaiian history.

Conflating consumables and wealth-assets also places restrictions on archaeology's contribution to regional comparisons. A structural model of social stratification in Oceania (Friedman 1981) contrasts West Polynesian societies – in which prestige-good systems were characterised by generalised marriage exchange, an elite monopoly on foreign prestige goods, a tendency to establish wife-taking and wife-giving relationships between senior and junior lines of the conical clan, and an asymmetric political dualism that opposed sacred and secular chiefs, land and sea people, men and women, and centre and periphery – with Eastern Polynesian societies where long distances between island groups greatly increased the difficulty of establishing and maintaining supplies of foreign prestige goods and removed the material basis on which a prestige-goods system could develop (Hage & Harary 1996: 116–24). In this view, the decline of prestige-goods systems in East Polynesia led to increased competition among elites that was expressed in warfare and in feasting, which had its basis in agricultural intensification. Here, the theoretical elaboration of pig herds as wealth-assets and the connection between sweet potato agriculture and animal husbandry makes it possible to recognise that the late intensification of infrastructure and construction of family-sized temples within the LKFS indicates the pace at which this characteristic feature of East Polynesian social stratification developed in Hawai'i.

It is too late in Hawai'i to carry out the detailed ethnographic description that served as the basis for identifying prestige-goods systems in Tonga (e.g. Bott 1982; Kaeppler 1978), or that led to the development of conjectural histories of agricultural production and pig husbandry as a way of explaining variations in warfare, leadership, social structure, male–female relationships and ceremonial exchange among highland Papua New Guinea societies (Feil 1987). However, the fine-grained chronologies of change yielded by the Bayesian calibrations, coupled with a suitable theoretical framework, open the door to (i) recognising the constructive role played by chiefs in the development of social stratification during good years and (ii) regional comparisons of developmental histories.

Like our colleagues in the United Kingdom, the third phase of the radiocarbon revolution has put Hawaiian archaeologists in a position to write a "very different kind of history". I am confident that fine-grained chronologies of change produced by Bayesian analyses are the bright future of Hawaiian archaeology, and that interpretations based on them will someday replace the equifinality-prone arguments based on synchronic data that characterize the field today.

NOTES

1. The Hawaiian word *maka'āinana* is typically glossed as "commoner" in that word's general sense of "one of the common people . . . below the rank of a peer" (OED). A more specific sense of "commoner" is "one who has a joint right in common lands; one who enjoys a right of common" (OED). Elsewhere, I have tried to show that Hawaiian *maka'āianana* have an historically variable set of rights with respect to common lands (Dye 2010b).
2. See http://nupepa.org/cgi-bin/nupepa (accessed 19 March 2014).

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