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Energetic efficiency and political expediency in Titicaca Basin raised field agriculture ☆

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Abstract

Vast tracts of prehistoric raised fields are present in the Titicaca Basin of Bolivia and Peru. Archaeologists at present consider raised field agriculture to have been much more productive and efficient than the rain-fed dryland agriculture currently practiced in the region. However, the recent failure of a number of long-term raised field rehabilitation projects has called this understanding into question. In this paper, I review the production, labor requirements, and energetic efficiency of raised field agriculture. Contrary to the existing literature, I conclude that raised field agriculture was always somewhat less efficient than traditional rain-fed dryland agriculture. Finally, I propose a new model of the political economic role of raised field agriculture. In order to appreciate the role played by raised fields in ancient Andean complex polities it is necessary to abandon the unrealistic model of raised field agriculture currently dominant in the archaeological literature.

Keywords: Bolivia; Peru; Raised fields; Political economy; Tiwanaku; Middle Horizon

The remains of vast tracts of abandoned raised fields on the margins of Lake Titicaca were first reported by Smith et al. (1968). Since that time, they have attracted a great deal of attention and have been the focus of ongoing debate among andeanists (cf. Erickson, 1985, 1987, 1992a, 1993, 1994; Graffam, 1990; Kolata, 1986, 1991; Stanish, 1994). The extent of the remains is truly impressive. Around 1200 km^2 of relict raised fields are present in the Titicaca Basin (Diaz Zeballos and Velásquez Coaquira, 1992, p. 27; Kolata and Ortloff, 1996b, p. 112). The majority of these (around 80%) are located in the vicinity of the Peruvian city of Juliaca, within a roughly triangular area defined by the towns of Lampa, Paucarcolla, and Taraco (altogether some 950 km^2). Significant concentrations are also present near Pomata, Peru (ca. 40 km^2), and in Bolivia's Katari Basin (70 km^2), Tiwanaku Valley (60 km^2), and Machaca-Desaguadero (60 km^2) regions (Fig. 1). Many smaller concentrations also exist (Diaz Zeballos and Velásquez Coaquira, 1992 describe the distribution of raised fields in the department of Puno, Peru). Considered as a whole, the Titicaca Basin contains one of the four largest concentrations of prehispanic raised fields in the western hemisphere (Turner and Denevan, 1985).

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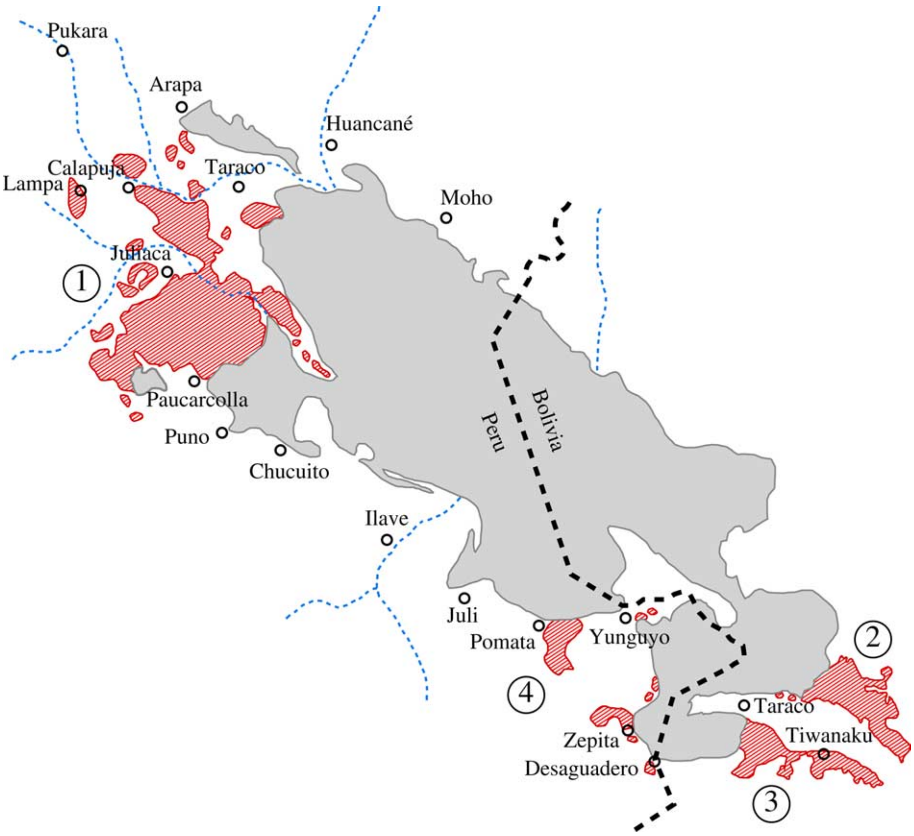

Fig. 1. Map of Titicaca Basin, with major raised field groups indicated. (1) Juliaca group, (2) Katari Basin group, (3) Tiwanaku Valley group, and (4) Pomata group. The Desaguadero field group is not shown, since no reasonably accurate map depicts it. Modified from Diaz Zeballos and Velásquez Coaquira (1992, Map 2), and Kolata and Ortloff (1996a,b, Fig. 5.4).

Raised field agriculture was a lost technology by the time the Inka conquered the Titicaca Basin. Apart from scattered occurrences of possibly related terms in Bertonio's colonial Aymara dictionary (Stanish, 2003), and some evidence for limited Inka period raised field use on the Island of the Sun (Bauer and Stanish, 2001, pp. 161–163), no accounts of it exist in colonial documents or in descriptions of pre-Conquest indigenous culture. However, since the time of their first systematic scholarly description the Titicaca Basin raised fields have come to occupy a central position in accounts of the region's prehistoric past. Unusually, they have also slipped the bonds of academic discourse. For a period in the late 1980s and early 1990s raised field agriculture was widely lauded in Bolivia and Peru as an indigenous technology uniquely suited to the harsh *altiplano* environment (see Swartley, 2000, 2002).

In this paper, I argue that current interpretations of the role played by these agricultural landscapes in prehispanic political economies are premised on a flawed conception of the fields themselves and of their productive capacity. Various investigators, most prominently Erickson (1982, 1985, 1987, 1988a, 1992a,b, 1993), Kolata (1986, 1991), and their various collaborators, have portrayed the Titicaca Basin raised fields as an almost miraculous indigenous technology, making possible high agricultural yields in this very difficult agricultural environment. I call this conception the 'hyperproductivity hypothesis.' I contend that, to the contrary, raised field agriculture is and always has been somewhat less energetically efficient than the traditional rain-fed agricultural systems of the area, and that most archaeological treatments of raised fields have neglected to take into account certain basic principles of Andean potato agriculture. For these reasons I suggest that the importance of raised fields in prehistory is related not to their supposed exceptional productivity—which I argue to be chimerical—but rather to the role they played in the political economy of Titicaca Basin polities.

My concern throughout will be to understand the ways in which raised field agriculture might have functioned within the prehistoric political economies of the Titicaca Basin. I wish to explore the collapse of the modern raised field rehabilitation program because I believe the fact of its failure is a strong indication that we, as archaeologists, have misunderstood in some basic way the nature of prehispanic raised field agriculture in the *altiplano*.

The economic miracle of raised field agriculture

Clark Erickson's initial raised field rehabilitation experiments beginning in 1981 (Erickson, 1985, 1988a) produced spectacular results that were widely discussed and enthusiastically received by the international aid community. His fields produced potato (*Solanum* spp.) yields ranging from 11,000 kg/ha to more than 22,000 kg/ha, between two and four times the average for typical dryland agriculture in the region. In his initial reports Erickson was cautious, emphasizing that these fields were planted in areas that had lain fallow for centuries, and that it was not certain that such high production could be sustained over multiple cropping years (Erickson, 1985, pp. 227–228). However, in later publications he seems to have become comfortable with the idea of continuous potato cultivation on raised fields, without crop rotation or fallow periods (Erickson, 1993; Erickson and Candler, 1989).

Some of the highest yield figures to date were reported by Kolata (1991, pp. 104–105). In an experimental study involving 12 raised field plots during the 1987/88 agricultural season, he documents yields ranging from 19,000 to 26,000 kg/ha, with an average of approximately 20,000 kg/ha. On the basis of these data, Kolata elaborated a scenario of prehispanic raised field use that has since become firmly established in the archaeological literature. He not only made a case for continuous potato cultivation without fallowing, but went considerably further, arguing that raised fields were capable of producing two harvests of potatoes within a single agricultural season. In the context of the Titicaca Basin, these claims are remarkable. As Kolata puts it, "continuous cultivation on fixed, permanent fields, short or no fallow periods, and two episodes of sowing and harvesting within the same agricultural year are inconceivable in the contemporary agrarian landscape of the high plateau" (1991, p. 108). Yet, he continues, "these may have been standard features of Tiwanaku agricultural practice." Janusek and Kolata have recently reiterated some of these claims, and suggest that a "consensus" exists regarding the hyperproductivity of raised field agriculture (Janusek and Kolata, 2004, p. 409).

Kolata's formulation represents the purest expression of the 'hyperproductivity hypothesis.' The hyperproductivity hypothesis refers to the notion that raised field agriculture is capable of very potato high yields—much higher than those of traditional Titicaca Basin rain-fed agriculture—on a continuous, sustainable basis with no fallow interval. This understanding of raised field agriculture emerged from the early work of Erickson, Kolata, and their colleagues. It has since become an almost canonical element of the discourse of Titicaca Basin archaeology.

Raised field agriculture clearly is capable of producing more usable energy per unit area than is traditional Titicaca Basin dryland agriculture. It is therefore a form of agricultural intensification. The most influential modern treatment of agricultural intensification has been that of Boserup (1965), who proposed a general relation between the process of agricultural intensification and increasing population density. In her model increasing production concentration (yields per unit area) is associated with decreased labor efficiency; extensive agricultural systems, in the same natural environment, are more efficient than are more intensive systems. Boserup predicted that intensification would take place when increasing population density made higher production concentration necessary. Her model has been successful in describing sequences of agricultural intensification in many parts of the world.

Stone and Downum note that Boserup's model depends upon the critical assumption that "the labor costs of intensification are both necessary and sufficient to raise production concentration. They are necessary in that higher production requires more work, and sufficient in that the proportionate increase in work succeeds in raising outputs" (Stone and Downum, 1999, p. 114). Viewed in this light, the claims of the hyperproductivity hypothesis are doubly remarkable. In effect, Erickson and Kolata have claimed that raised field agriculture simultaneously allows greater production concentration (production per unit land) and greater labor efficiency (production per unit labor) than does contemporary dryland agriculture. If the hyperproductivity hypothesis is valid, then Titicaca Basin raised field agriculture violates Boserup's assumptions. Intensification in this case would result in heightened labor efficiency, quite the reverse of Boserup's scenario. Raised field agriculture would therefore be considered an example of non-Boserupian intensification (Stone and Downum, 1999, p. 116).

The question of whether raised field agriculture is Boserupian or non-Boserupian is a critical one. If the former, then we would expect the relation between intensification and population density to be much as Boserup predicted. However, if raised field agriculture is non-Boserupian—if the hyperproductivity hypothesis is valid—no such relation will pertain, and the process of intensification will be governed by an entirely different logic. In order to evaluate social models of raised field agricultural production in Titicaca Basin prehistory it is necessary to first resolve this issue. For this reason a considerable portion of this article is devoted to comparing raised field agriculture and traditional Titicaca Basin dryland agriculture in terms of their labor efficiency. I will consider in turn the failure of the recent raised field rehabilitation projects, the agro-ecological properties of raised field systems, and the issue of nematode predation and its relation to fallowing in the Titicaca Basin environment. Using the results of these investigations, I will then model the relative labor efficiency of Titicaca Basin raised field and dryland agriculture. I conclude that the hyperproductivity hypothesis has no foundation in fact, and that raised field agriculture satisfies both assumptions of Boserup's model. Raised field agriculture cannot be thought of as a non-Boserupian form of intensification. Finally, I will review several models of prehistoric raised field agriculture that are consistent with Boserup's assumptions.

Raised field rehabilitation

The experimental results of Erickson and later of Kolata, and their accounts of a remarkable prehispanic agricultural technology, were quickly and energetically received by the international aid communities in both Peru and Bolivia. In the 15 years following Erickson's first experiments millions of dollars of domestic and foreign development funds were invested in the rehabilitation of prehistoric field systems and the construction of new ones following the ancient designs. In the department of Puno, Peru, no fewer than 16 organizations were engaged in raised field rehabilitation projects during the 1980s (Izqueirdo Condori, 1992b, p. 148). Between 1981 and 1994 more than 1000 ha of raised fields were reconstructed in Puno (PIWA, 1994, p. 38). According to Fernández Valdivia and Garcia Chire (2000), by the year 2000 some 2006 ha had been reconstructed in 10 provinces of Puno, involving 3350 families and 420 *campesino* organizations. Efforts in Bolivia were on a smaller scale. The NGO *Fundación Wiñaymarka* reconstructed somewhat more than 50 ha of raised fields between 1986 and 1991 (Kolata et al., 1996, pp. 204–205), working in more than 50 rural communities (Swartley, 2000, p. 131).

The NGOs involved themselves in raised field rehabilitation for a number of reasons. First of all, a scientific literature existed, if initially a scanty and preliminary one, that described a highly productive, non-capital intensive agricultural system currently unknown in the area. This was the foundational literature of the hyperproductivity hypothesis; the early works of Erickson, Kolata, and their colleagues. Second, and perhaps more importantly, raised field agriculture could almost have been made to order for the zeitgeist of the development community in the 1980s. Swartley (2000, 2002) has attributed the remarkable initial success of the raised field rehabilitation project to several interrelated trends, including:

> …government economic interests and development policy, indigenous social movements and ethnic politics, and the rise of development theory predicated on the concept of "sustainability." The raised field rehabilitation project had several discernible characteristics, such as its low economic and technical inputs, its indigenous origin, and "ecological sustainability," and its imagined links to a primordial past that fit with the expectations and agenda of academic researchers and development personnel (Swartley, 2000, p. 74).

And yet the raised field rehabilitation project has undeniably failed. Swartley recounts that in 1994 "many raised fields were beginning to be abandoned," and that by 1996 "all of the raised fields had been abandoned. I did not see any of the fields I had seen in production in 1994, still in production by 1996/1997" (Swartley, 2000, p. 213). To my knowledge, Swartley's work is the only attempt at a post-mortem analysis of the Titicaca Basin raised field rehabilitation projects. The major NGOs involved in rehabilitation, including the *Programa Interinstitucional de Waru Waru* (PIWA) in Puno and the *Fundación Wiñaymarka* in Bolivia, are by now virtually all defunct, and have produced no literature relating to their collapse, nor to the failure of their central project. However, any regular visitor to the region will attest that few if any raised fields remain in production today, and the failure of the NGOs would seem to speak for itself. Raised field agriculture has emphatically *not* been adopted in any significant way by the *campesinos* of the contemporary Titicaca Basin, despite ample funding, international enthusiasm, and over a decade of systematic promotion. This failure suggests that both archaeologists and aid workers have fundamentally misunderstood raised field agriculture in the *altiplano*.

Raised fields as agroecosystems

Raised fields have a very wide distribution in the New World, and are found throughout Bolivia (Denevan, 1966), Peru (Hastorf, 1993; Hastorf and Earle, 1985; Moore, 1988), Ecuador (Batchelor, 1980; Denevan et al., 1985; Knapp, 1984; Knapp and Denevan, 1985), much of lowland South and Central America (Parsons, 1985; Puleston, 1977; Turner, 1983), and as far north as Wisconsin (Gallagher, 1989; Gallagher et al., 1985; Gartner, 2003). Due perhaps to this wide distribution they have attracted a considerable amount of scholarly attention, and numerous projects have attempted to experimentally replicate functioning raised fields in diverse environments. This research has produced a good understanding of the salient properties of these field systems.

According to Erickson and Candler (1989, p. 234), raised fields possess three principal agriculturally significant properties that set them apart from other forms of agriculture. These are: (1) the concentration, production, and recycling of soil nutrients, (2) the improvement of crop microclimates, and (3) water control and conservation. The first of these properties refers to the famed 'canal muck' of *chinampas* agriculture. Aquatic flora and fauna in the canals provide a rich source of fertilizer when the canals are cleaned and the canal-bottom muck is added to the upper surface of the planting platform. This periodic addition of natural fertilizer can compensate for the depletion of soil nutrients (Carney et al., 1996; Erickson, 1985; Erickson and Candler, 1989; Knapp, 1984).

The water in the canals between the planting platforms has been demonstrated to have beneficial effects on raised field microclimates, regulating local temperature by absorbing solar energy in the daytime and radiating it to the surrounding soil and air at night (Kolata and Ortloff, 1989; Ortloff, 1989). In addition, the canals, being at a lower elevation than the planting surfaces, can serve to collect the coldest air, further protecting crops. This can result in temperatures as much as 1 °C above that of the surrounding terrain (Knapp, 1984). In an environment such as the Titicaca Basin, where frosts can occur at any time of year, but in which temperatures rarely drop very far below freezing, even a temperature change this subtle can be of real significance. This property of raised fields is especially important in ameliorating the effects of static frosts, in which cold air settles in a thin layer over the ground surface on very still nights (Riley and Freimuth, 1979). This type of frost causes a great deal of damage in the Titicaca Basin, since it may occur during the growing season.

Erickson and Candler's third property, water control and conservation, has several dimensions. First, the topography of the fields serves to ensure drainage and prevent flooding of planting surfaces. In many cases, this drainage effect may have been the primary reason for the construction of the raised fields: to reclaim seasonally inundated areas or shallow, permanent wetlands for cultivation. In addition, however, fields located in areas with a high water table, or that for some other reason have permanent water in their canals, can be irrigated in case of drought. This practice has been observed by Erickson and Candler (1989), and they conclude that 'splash' irrigation of planting surfaces with water from the canals can be accomplished with little effort. Indeed, in one drought year in Huatta, Peru, raised fields produced good yields while surrounding dryland fields failed completely, due solely to the fact that the raised fields were 'splash' irrigated as necessary.

Given all of these indisputably beneficial properties of raised fields, we may ask again: why did the raised field rehabilitation project fail? The simplest answer is that the raised field rehabilitation project failed because raised field yields, in practice, failed to live up to the fantastic yields projected by the early experiments. Why were the fields less productive in practice than in theory? One possible answer, and the one favored by the formulators of the hyperproductivity hypothesis, is a social or cultural one. Kolata et al. (1996, p. 221), for example, observe that "the principal reason for relatively low yields in participating communities was poor field management..." They argue that, particularly with respect to water management, farmers were either insufficiently educated in the technique, or inadequately assimilated NGO guidance. They also note that "there was a certain degree of cultural resistance to continuous cropping on the part of the indigenous small farmers of the region," and that "overcoming small farmer skepticism surrounding the feasibility of continuous cultivation, which stems from decades of experience with long-fallow systems, is difficult" (1996, p. 210). It is a fair point that there could be difficulties with a new and unfamiliar agricultural technique. However, it is not accurate to portray *campesino* farmers as technically conservative. These same farmers enthusiastically employ agricultural chemicals and farm machinery to the extent that their resources allow, despite the fact that these techniques are not "traditional."

Another explanation for the abandonment of the rehabilitated fields is that the theory of raised field agricultural production employed by archaeologists and NGO personnel alike was fundamentally flawed. The hyperproductivity hypothesis was the result of a specific and highly tentative interpretation of the way in which the properties of raised field agriculture would play out in the context of the *altiplano* environment. In light of the failure of the raised field rehabilitation program it is necessary to reevaluate that initial interpretation.

The key assertion of the hyperproductivity hypothesis is that raised fields obviate the necessity for regular fallowing. The periodic 'mucking' of the canals is thought to renew soil fertility and prevent the nutrient depletion evident in rain-fed agricultural fields. On the one hand, the soil regenerative properties of raised field agriculture seem to be real (Carney et al., 1996). Fundamental to the hyperproductivity hypothesis, however, is the additional assumption that the regeneration of soil fertility is the principal factor determining the fallow interval of modern Titicaca Basin agricultural systems, and that the continual renewal of soil fertility would allow for continuous potato cultivation. I will argue that this assumption is invalid.

Orlove and Godoy (1986), in an enlightening review of traditional highland Andean agricultural systems, conclude that the regeneration of soil fertility is only one of the functions served by fallowing and crop rotation. Another function, at least equally as important, is the control of parasitic nematode populations, which "attack potato roots, reducing the ability of the plants to absorb water and nutrients" (1986, p. 179). These microscopic root parasites are the reason why continuous potato cultivation is not now and probably never has been practiced in the Titicaca Basin, or anywhere in the Andean highlands.

Potato-parasitic nematodes

Potatoes are indigenous to the Andean region of South America. Within this area, they are afflicted by a wide range of pests and pathogens, including insects, bacteria, fungi, viruses, and plant-parasitic nematodes that have no doubt co-evolved with the Andean High-Elevation Crop Complex (Pearsall, 1992). Among the most significant potato pests in high-elevation areas of the Andes are three species of nematode: the golden (*Globodera rostochiensis*) (Spears, 1968) and white (*Globodera pallida*) potato cyst nematodes (CIP, 1983, pp. 68–69; Franco et al., 1993) and the potato rosary nematode (*Nacobbus aberrans*, also known as the false root-knot nematode) (CIP, 1983, pp. 72–73).[1] These microscopic animals attack the roots of potato plants, and in sufficient densities interfere with the ability of the plants to absorb water and soil nutrients, stunting growth and dramatically reducing yields.

Globodera spp. is much better understood than is *N. aberrans.* This is so because the *Globodera* species have a worldwide distribution, affecting yields in more than 50 countries (Evans, 1993, p. 221).[2] Extensive studies of *Globodera* spp. have been carried out by an international research community. *N. aberrans*, by contrast, has a more limited distribution. In addition, *N. aberrans* populations outside of Argentina, Bolivia, Peru, and possibly Ecuador are apparently not parasitic on potatoes (Franco, 1994, p. 184), affecting mainly sugar beets in the United States, and amaranth, chile, tomato, and beans in Mexico (Cristóbal et al., 2001, p. 228). Therefore, systematic research on *N. aberrans* potato parasitism is rare. For these reasons, the following discussion will be concerned exclusively with nematodes of the genus *Globodera*.

Globodera spp. nematodes are capable of causing significant declines in overall potato yields when present in sufficient numbers in the soil. Yield losses attributable to these two species can be as high as 62%, depending upon a variety of factors (Franco, 1994, p. 185). The most significant of these factors is the population density of the animals themselves. The relation between potato yields and nematode population densities has a sigmoid form (Evans, 1993; also Trudgill et al., 1996). At very low population densities, no yield reduction is observed. However, once nematode densities have surpassed the so-called "tolerance threshold" (T), yields begin to drop sharply, until arriving at a minimum value beyond which further increases in nematode population have little additional effect (Fig. 2).

Eradication of these potato-parasitic nematodes is virtually impossible. *Globodera* spp. form durable cysts which remain in the soil for many years. The annual rate of decrease in viable cyst densities in soil seems to vary between the two *Globodera* species. In the absence of a host crop, viable *G. rostochiensis* cysts will decrease by around 33% annually (Evans, 1993, p. 224). The rate for *G. pallida* may be as low as 15–20% annually (Evans, 1993, p. 225). This means that long fallow periods, perhaps a decade for *G. rostochiensis*, and as much as two decades for *G. pallida*, are required in order to eliminate nematode populations from infested fields. Shorter fallow periods, however, or rotation with non-host crops, can reduce the population density to acceptable levels.

Since eradication of these nematodes is not economically feasible, integrated management strategies focus on the maintenance of nematode population levels at or below the tolerance threshold. The tolerance threshold for *Globodera* spp. varies between potato varieties, and probably between nematode pathotypes as well, but broadly speaking seems to fall into the range of 1–3 eggs per gram of soil (Greco, 1993, p. 216). Modern industrial potato cultivation relies heavily on chemical nematicides and soil fumigants to achieve very short fallow periods. However, the methods available to traditional Andean agriculture were of only two kinds: (1) the introduction of resistant potato varieties and (2) fallowing and rotation with non-host crops.

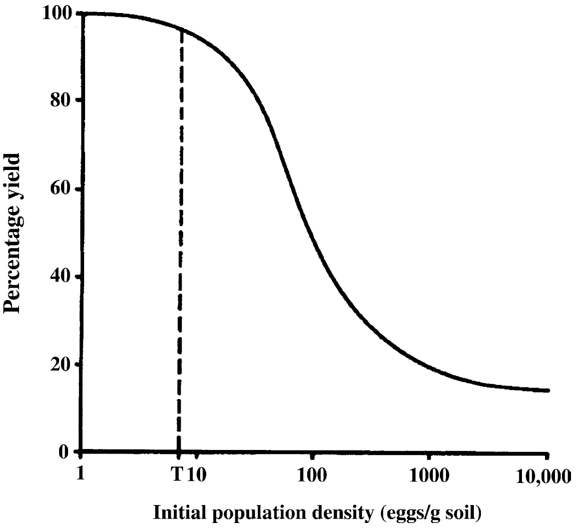

Fig. 2. Relation between initial nematode population density and potato yields. After Evans (1993, Fig. 1).

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[1] This discussion draws extensively from Franco (1994).

[2] Interestingly, in the Andean region *G. rostochiensis* is apparently not found north of the Titicaca Basin—though infestations have been reported from the extreme southern provinces of the Department of Cusco (Delgado de la Flor and Jatala, 1991a)—while around Lake Titicaca mixed populations of *Globodera* spp. are the norm (Jatala et al., 1979, p. 210).

In the Andean region, as one might expect, there exist a great many varieties of potato, and an indeterminate but almost certainly great number of pathotypes or 'races' of *Globodera* spp. These races of potatoes and potato parasites evolved in relation to one another, and the effect of a particular pathotype of *Globodera* spp. varies widely between different potato varieties. For example, a recent study of pathotypes of *G. pallida* in the Department of Cusco has found nematode infestation in 12 of 13 provinces, the 13th being agriculturally marginal and poorly suited for potato agriculture (Delgado de la Flor and Jatala, 1991a). The investigators found five separate races or pathotypes of *G. pallida* in Cusco, which seem be to geographically clustered (Delgado de la Flor and Jatala, 1991b). That is, certain pathotypes of *G. pallida* predominated in certain geographical areas. Samples of seven separate potato varieties were planted and grown with systematically varying concentrations of each of the five *G. pallida* pathotypes. Each pathotype had a dramatically different effect on the various potato varieties that were exposed to it. The investigators measured this effect in terms of the rate of nematode reproduction within a single generation. Rates of reproduction on a single variety varied from a high of 19.14 to a low of 0.42. This clearly demonstrates that not all potato varieties are equally efficient hosts for all *Globodera* spp. pathotypes. There is apparently a great deal of regional variability in both potato and potato cyst nematode populations. The circulation of resistant potato varieties between agricultural regions may have been a significant economic activity in the Andes from the dawn of agriculture. However, while a resistant variety might allow a shortened fallow period it could never make continuous potato cultivation possible. A highly resistant potato variety would over time simply shift the pathotype composition of the local nematode population. In the face of great and continuously evolving nematode diversity, resistant varieties are a temporary solution. Some form of crop rotation and fallow was certainly always necessary.

I should add here that there are some indications that raised fields may in fact possess some anti-nematodal properties. Fernández Valdivia and Garcia Chire (2000) have noted a significant correlation between soil humidity in raised fields and potato yields. This may be significant, since high levels of moisture can often reduce nematode populations. Indeed, inundation is a recommended method for nematode eradication in some circumstances (Dufour et al., 1998, p. 8). On the other hand, the observed association could simply be a result of increased water availability to the cultivated plants. We must assume, therefore, that raised fields are equivalent to dryland fields in terms of nematode reproduction and predation.

It seems likely that the high yields reported by Erickson and Kolata were due, at least in part, to the initial lack of plant-parasitic nematodes in those fields. It has often been noted that the areas in which raised fields were rehabilitated had not been used for agriculture for decades and perhaps for many centuries. This means, first of all, that initial soil fertility would be expected to be high. It also means that nematode density would initially be very low. Given that at densities of less than 1 egg/cm^3 of soil *G. rostochiensis* reproduces on a susceptible cultivar at a rate of between 1000% and 1900% in a single generation (LaMondia and Brodie, 1986, Table 2), it might take 3 or 4 years for a small initial population, introduced in the potato seed or on the agricultural implements or shoes of the farmers, to reach the tolerance threshold. An expected pattern when a field with low or absent nematode population levels is planted continuously with potatoes would be several years of very high yields, after which the threshold density would be surpassed and yields would drop precipitously. To my knowledge, no study of continuous potato cultivation in raised field plots over more than a few years has been published, however. The only evidence available are the accounts given to Swartley by her informants in Wankollo, Bolivia. She summarizes their reports as follows:

> In all cases farmers recounted "normal," "good," or "very good" production in the first year of community level raised field cultivation (1990-91). According to farmers, the fields produced so well that this was stated as the primary reason for expanding... raised fields ... in the following agricultural year (1991-92). Yet in each case, production dropped dramatically in the second year, producing "very little," not very good, or "average" crops of potatoes. ... [B]y the third year all of the raised fields produced very poor harvests ... so that only a handful of residents were prompted to plant a fourth year of potatoes (Swartley, 2000, p. 175).

Swartley contends that this was a general pattern in the Bolivian raised field projects, and that "across the Lake Titicaca Basin, farmers discontinued cultivating individual fields after 2–4 agricultural seasons" (Swartley, 2000, p. 173). Increasing *Globodera* spp. population levels would produce exactly the kind of pattern Swartley's informants describe.

Obviously, a systematic study of raised field production across multiple years under various crop rotation regimes is needed. Even in the absence of such data, however, it is clear the crop rotation of some kind must have been practiced on the Titicaca Basin raised fields (as indeed some raised field researchers have also asserted; for example, Canahua Murillo and Larico Mamani, 1992, pp. 132–134). What form might such a crop cycle take? To answer this question I will turn to the modern fallowing systems of the Titicaca Basin.

Modern Titicaca basin agricultural systems

If continuous potato cultivation on raised fields is not possible, due to the vulnerability of tuber agriculture to nematode predation and to other crop pests, it follows that some system of crop rotation and/or fallowing must have been practiced. The exact form of this system will most likely forever elude us, but we can take as a starting point the sectoral fallowing systems of the modern Titicaca Basin described by Orlove and Godoy (1986; for other Andean systems see Godoy 1991; Kraft 1994; also many papers in Herve et al., 1994). In these systems, in any given year, the total area of land in each crop is more or less equal. The quantity and content of the harvest therefore remains approximately constant over the course of an entire cycle.

Orlove and Godoy (1986) catalog six such systems located in the Titicaca Basin, at altitudes comparable to that of the raised fields (that is, below 3850 m.a.s.l., lake level being approximately 3810 m.a.s.l.). Of these, four are actually located close to the margins of the lake. The remaining two examples are the communities of Irpa Chico and Jesús de Machaca, located near to one another south of the Quimsachata Mountains in the Department of La Paz, Bolivia, not far from the raised fields associated with the Rio Desaguadero (Table 1). This information provides us with some perspective on fallowing and crop cycles in the lower elevations of the Titicaca Basin.

Several regularities are readily apparent in these data. First, and perhaps most obviously, potatoes always occur first in the crop cycle. This is a general pattern in highland Andean agricultural systems, and is by no means unique to the Titicaca Basin cases. Also significant is the fact that potatoes are never planted more than once per cycle. This is also a general pattern in the Andean highlands (Brush, 1977).

Several useful measures can be constructed on the basis of this information. The first is what I call the potato ratio. If n is the number of years in the cycle, and p is the number of potato years, then the potato ratio (P) would be expressed by $P = \frac{p}{n}$. In the case of the modern Titicaca Basin examples in the table above, P ranges from 0.09 to 0.20, and the mean value of P for the six systems is 0.16. The general pattern of one potato year in a 6- to 8-year cycle seems to effectively control *Globodera* spp. population levels in many parts of the world (Blanco, 1994; Esprella et al., 1994). The second measure, the fallow ratio (F), is expressed by $F = \frac{f}{n}$ where f is the total number of fallow years in the cycle. For the six cases in Table 1, F ranges from 0.40 to 0.64, with a mean value of 0.53.

Evans provides two modern, industrial crop cycles for lands infested with *Globodera* spp. nematodes, one for a field infested with *G. rostochiensis* and another for fields infested with *G. pallida* (1993, pp. 225–227) (Table 2). Interestingly, even with the use of chemical nematicides, proven resistant cultivars, and soil fumigation, he only achieves P values of 0.2 and 0.4. These values are higher than the Titicaca Basin values, but if modern industrial agriculture cannot achieve continuous potato cultivation in the face of *Globodera* spp. infesta-

Table 2
Modern crop cycles (after Evans, 1993, Table 3)

Year	*G. rostochiensis*	*G. pallida*
1	Non-host	Non-host
2	Non-host	Non-host
3	Non-host	Non-host
4	Resistant cultivar	Non-host
5	Susceptible cultivar + nematicide	Non-host
6	—	Non-host
7	—	Resistant cultivar
8	—	Susceptible cultivar + nematicide
9	—	Soil fumigation
10	—	Trap crop

Table 1
Sectoral fallowing systems in the lower elevations of the Titicaca Basin

Community	Elevations	Crop years	Fallow years	Crop 1	Crop 2	Crop 3	Crop 4
Chiripa	3855	3	4	Potato	Barley	Faba beans	—
				—	Wheat	Tarwi	—
				—	Quinua	—	—
				—	Cañihua	—	—
Ichu	3830	3	2	Potato	Barley	Habas	—
Acora	3861	2	3	Potato	Quinua	—	—
				—	Barley	—	—
Chanajari	3840	4	7	Potato	Ulluco	Faba beans	Barley
				—	Oca	—	—
Irpa Chico	3800	3	3	Potato	Quinua	Barley	—
Jesús de Machaca	3800	3	3	Potato	Quinua	Cañihua	Barley
				—	Cañihua	—	—

From Orlove and Godoy (1986, Appendix 1).

tion, I think it unlikely that such a feat was accomplished with raised fields.

On the basis of these figures, an ideal Titicaca Basin dryland agricultural cycle can be constructed. This ideal cycle can be expressed in the following form: *pxxfff*, where the sequence of the letters indicates the sequence of stages in the cycle, *p* represents a potato year, *f* represents a fallow year, and *x* represents cultivation of a crop other than potatoes. This ideal cycle has a value of P equal to 0.17 and F equal to 0.50. In terms of these two measures, then, this ideal system falls very near to the mean values for the modern systems for which we have detailed information. An alternative cycle would be *pxxff*, with $P = 0.20$ and $F = 0.40$. These two ideal cycles both fit the modern data, in terms of the P and F measures, with the second maximizing P and minimizing F.

Crops other than potatoes currently grown in the Titicaca Basin include *quinua*, *cañihua* (*Chenopodium pallidicaule*), barley (*Hordeum* spp.), and Faba beans (*Vicia faba*). *Quinua* and *cañihua* are native Andean cereal-like crops which are rich in protein but which produce relatively low yields. In the past, these two crops were most likely even more significant than at present, as some frost-resistant European cereals, most notably barley and some varieties of wheat (*Triticum* spp.), have come to occupy an analogous position in many contemporary Andean agricultural systems. Faba beans are an Old World legume that occupies a role analogous to that of the indigenous *tarwi* in highland agricultural systems.

Indigenous tubers other than potatoes, such as *oca*, *mashwa*, and *ulluco*, were no doubt important in the past, as they are today. However, Orlove and Godoy's data seem to indicate that they are cultivated in relatively small quantities and utilized mainly as dietary supplements in the *altiplano*. It seems certain that potatoes have always been the most important of the Andean tubers.

For modeling prehistoric agricultural cycles, I will assume all non-potato crop years to be years of *quinua* production. This is not a realistic assumption, but it is provisionally acceptable because figures both for annual yields and cal/kg for *quinua* are very similar to those for other highland cereals, such as *cañihua*, and for highland indigenous maize (*Zea mays*), as well (Graffam, 1990, p. 29, Table 3). Following Graffam, therefore, I will use *quinua* yields as proxy data for other cereals. Unfortunately, quantification of the dietary input of other tuber crops is impossible with available published data. Sufficiently detailed yield and labor figures do not exist to allow them to be included in models of raised field and dryland crop cycles. The model that results clearly will be a very preliminary one, and hopefully will be refined to take into consideration more complex crop cycles when data for other crops become available. The ideal dryland agricultural cycles will therefore be rendered as *pqqfff* and *pqqff*, where the *q* stands for a year of *quinua* cultivation.

How could raised field crop cycles differ from this ideal dryland model I have presented? I have argued that in the absence of any convincing demonstration of anti-nematodal properties of raised field systems we must assume that a sustainable potato ratio could not be higher than in dryland systems. However, if we accept the many good arguments for the soil fertility effects of raised fields, it may be possible that the fallow ratio could be reduced to zero. Therefore, continuous potato cultivation (*p*) is not possible, but, under these assumptions, *pqqqq* is possible, as is *pqqqf*, for

Table 3
Sample gross potato yields for (A) raised field and (B) dryland agriculture

Potatoes (kg/ha)	Source
(A)	
8439	Erickson and Candler (1989) and Garaycochea (1987)
6690	Izquierdo Condori (1992a, p. 118) (nine seasons in Puno)
5370	Izquierdo Condori (1992a, p. 132) (community of Cochela)
7402/7544	PIWA (1994, p. 294) (potatoes/bitter potatoes)
7010/5540	Canahua Murillo et al. (1992, p. 27) (potatoes/bitter potatoes 1990/1991)
8640	Kolata et al. (1996, p. 222) (15 communities in Bolivia, 1990/1991, recalculated)
25,000	Kolata (1991, p. 105)
(B)	
3050	Denevan (1982) (5-year average, Department of Puno)
4776	Tschopik (1946) (average for Chucuito, 1940s)
6000	Graffam (1990, p. 29) (5-year average, Department of La Paz)
5000	Tapia Vargas (1994, p. 270) (Bolivian national average)
2400	Kolata et al. (1996, p. 227) (small Pampa Koani control plot)
3300	Swartley (2000, p. 219) (Wankollo, La Paz, Bolivia., 1996-1997)
4680	Canahua Murillo et al. (1992, p. 27) (1990/1991)
4980	Izquierdo Condori (1992b, p. 132)

example. These cycles would not be possible in a dryland context.[3]

The hyperproductivity hypothesis assumes, fundamentally, that raised field agriculture is much more efficient than is traditional dryland agriculture. Given the assumption that raised fields must have been cultivated using cropping cycles, and that continuous potato cultivation was never possible, can raised field agriculture still be thought of as more efficient than dryland agriculture? To answer this question we must consider the yields, labor inputs, and relative efficiency of a range of crop cycles in both raised field and dryland contexts.

Raised field and dryland efficiency compared

In order to determine the relative efficiency of raised field as opposed to dryland agricultural systems under a variety of crop rotation schemes, it is necessary to devise a formula for calculating efficiency for each potential cycle. It is also necessary to choose values for a series of critical variables in the formulas. This section is dedicated to the formulation and discussion of formulas to calculate yields and labor inputs of crop cycles in both raised field and dryland contexts.

In comparing raised field and dryland agriculture, a perennial problem is a confusion between figures that count only the actual planting surface of raised fields and figures that count both the planting surfaces and the canals. Kolata and colleagues (1996, p. 213) have introduced a terminological innovation into the raised field literature. They proposed distinguishing between net and gross measures.[4] Net measures refer only to the planting surface of the fields, while gross yields refer to the combined area of planting surfaces and canals. In order to remain consistent in any discussion of raised field agriculture, it is extremely important to be clear about whether the measures one employs are gross or net measures, and to use one type of measure consistently. In the discussion to follow, all figures for raised field production yields and labor input will be reported and discussed as gross figures, including both planting surfaces and canals.

I have opted to measure yield in terms of calories/ha, as such a measure allows comparison of the differential contributions of potato and *quinua* to the total harvest.[5] I am fully aware that this measure fails to capture the nutritional significance of the crops in question. This study, however, intends to be a gross assessment of the overall productivity of a variety of simple crop cycles in raised field and dryland agricultural contexts. I do not purport nor intend to offer a sophisticated dietary model.

To begin with, then, the annual production of an area A, in terms of calories, will be designated $P(A)$. The following equation presents a first pass at calculating $P(A)$.

$$P(A) = \frac{\sum_{x=1}^{n} Y(A)_x}{n}, \tag{1}$$

where $Y(A)_x$ is the yield of $crop_x$ over area A, in calories, in a given year x and n is the number of stages in the cycle (crop years + fallow years).

$Y(A)_x$ can be further specified, thusly:

$$Y(A)_x = AC_x(k_x(1-p_x)-s_x), \tag{2}$$

where A is the total area of the system in ha, k_x is the yield of $crop_x$ in kg/ha, s_x is the amount of $crop_x$, which must be saved out of the harvest as seed, in kg/ha, p_x is the rate of loss due to spoilage for $crop_x$, and C_x is the cal/kg value for $crop_x$.

Which finally gives us, from Eqs. (1) and (2):

$$P(A) = \frac{A\sum_{x=1}^{n} C_x(k_x(1-p_x)-s_x)}{n}. \tag{3}$$

Eq. (3) allows us to calculate the total annual production, in calories, of a given cycle extending over a given area A. First, however, we must arrive at values for the variables k, p, s, and C for the two crops in which we are, at present, interested. The values I have selected for these various production variables are summarized in Table 4.

One variable that merits a more comprehensive discussion is the productivity (k) of potatoes. For raised fields, most discussions—for example, Graffam (1990) and Kolata (1986)—have employed Erickson and Candler (1989) and Garaycochea's (1987) figure of 8439 kg/ha. The difficulty with this has always been that the figure reflects a short period of only a few years, and therefore does not capture variation in yields from year to year. Fortunately, a great deal of experimental work has been done since Erickson, Candler, and Garaycochea's early work, and more extensive data are now available. Table 3 presents a variety of sources for raised field potato yields. These figures vary widely, and probably reflect a variety of local conditions, including fluc-

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[3] I should again emphasize that I am by no means suggesting that *quinua* was ever cultivated 3 or 4 years in a row. *Quinua*, of course, is afflicted by its own pests and diseases and probably must be rotated also. As I said earlier, I am using *quinua* as a proxy for other Andean crops.

[4] In the Puno literature net figures are often referred to as "area cultivada," while gross figures are, confusingly, "area neta" or "area del sistema."

[5] In a more sophisticated analysis, other indigenous crops would also be considered. The necessary data, however, especially as respects cultivation and yields on raised fields, are at present available only for potatoes and *quinua*.

Table 4
Values of production variables

	Modern dryland		Raised fields	
	Potato	*quinua*	Potato	*quinua*
k	4980 kg/ha	700 kg/ha[a]	6690 kg/ha	1104 kg/ha[b]
p	0.02[c]	0.02[c]	0.02[c]	0.02[c]
s	1080 kg/ha[d]	10 kg/ha[e]	744 kg/ha[f]	10 kg/ha[e]
C	1000 cal/kg[g]	3510 cal/kg[h]	1000 cal/kg[g]	3510 cal/kg[h]

[a] Graffam (1990), a 5-year average for the department of La Paz. Other figures inlcude: 600–800 kg/ha (Tapia Vargas, 1994, p. 267; a Bolivian average), 750 kg/ha (Catacora Ccama and Canahua Murillo, 1992, p. 42; Puno average), and 530 kg/ha (Izquierdo Condori, 1992a, p. 132; Puno average).

[b] PIWA (1994, p. 292). Catacora Ccama and Canahua Murillo (1992, pp. 41–43) report a somewhat higher figure of 1280 kg/ha.

[c] Graffam (1990, p. 70). This figure may be too low (see below).

[d] 900 kg/ha (Izquierdo Condori, 1992b, p. 164), corrected for 20% seed loss in storage. Rhoades et al. (1988, pp. 49–50) have documented storage loss rates for seed potatoes in traditional agricultural contexts of between 16 and 25%. These losses are due mainly to the potato tuber moth (*Phthorimaea operculella*), the larvae of which burrow into tubers in storage.

[e] Izquierdo Condori (1992b, p. 164), for both dryland and raised field systems. In any event, the seed requirements for *quinua* are so low that variation in them has only a negligible effect on the final calculations.

[f] 620 kg/ha (Izquierdo Condori, 1992b, p. 165), corrected for 20% seed loss, as discussed above. Other figures include 600 kg/ha (PIWA, 1994, p. 286, recalculated).

[g] Kolata (1986) and Denevan (1982).

[h] Graffam (1990, p. 29).

tuation of temperature, rainfall, and other variables. Canahua Murillo and colleagues (1992, p. 27) stress that yields vary dramatically with local microclimates and other factors. For the present analysis, I have elected to use a figure reported by Izquierdo Condori (1992a, p. 118) of 6690 kg/ha for nine raised field agricultural seasons in Puno. This figure reflects a large number of widely distributed experimental plots over a number of agricultural seasons. This and other recent figures from the work of PIWA, reflecting longer-term experimentation, seem to indicate that the higher initial yield figures of Erickson, Candler, Garaycochea, and Kolata were unsustainable.

The value of k for dryland production is also difficult to determine, for many of the same reasons. Erickson and Candler (1989) and Garaycochea (1987) use Denevan's (1982) figure of 3050 kg/ha, a 5-year average for the Department of Puno. A variety of other figures are presented in Table 3. However, many of these figures average a wide variety of elevation and climate zones. Raised fields are for the most part located near to the lakeshore at lower elevations. These areas are more productive for potato agriculture than are higher and colder areas further from the lake. What we need therefore, are yield figures for dryland potato agriculture that are from the same locations and the same agricultural seasons as the raised field yield figure we are using. Fortunately, Izquierdo Condori (1992a, pp. 132, 118), in the same table that contains the raised field yield figure I selected above, provides us for comparison a figure of 4980 kg/ha. This is very similar to estimates of dryland potato yield averaging a variety of seasons in the 1980s and early 1990s (Canahua Murillo et al., 1992, p. 27).

Finally, the issue of potato seed requirements is a complicated one. Potatoes, propagated vegetatively, are a seed-intensive crop. By weight, seed material may amount to 20% or more of the final harvest. Thus, accurate reporting of seed use is critical in any calculation of the productive efficiency of potato agriculture. Such an accurate assessment of seed use has for the most part been lacking in archaeological accounts of raised field agriculture. Archaeologists, to the extent that they have considered the question at all, seem to have simply assumed that seed investment in raised fields will be roughly half that in dryland systems. This is because the area of canals is of course not planted, and these authors assume an equal spacing and size of seed in both types of agriculture. For example, Graffam (1990, p. 70) employs a seed requirement figure of 1200 kg/ha for dryland cultivation, and uses half that, 600 kg/ha, for raised field agriculture. While this procedure may appear to be reasonable, it in fact misrepresents the reality of the raised field rehabilitation experiments. Rehabilitated raised fields have typically been sponsored by development organizations which have provided high-quality (i.e., disease- and pest-free) seed free of charge to participating individuals and communities. As a result, larger seeds have been planted closer together in rehabilitated raised fields than is traditional practice in dryland plots. This disparity has been recorded by some of the Puno projects. Gross seed investments of 600 kg/ha (PIWA, 1994, p. 286, recalculated) and 620 kg/ha (Izquierdo Condori, 1992b, p. 165) are reported for raised field potato agriculture, contrasting with 900 kg/ha for traditional dryland potato agriculture in the same communities (Izquierdo Condori, 1992b, p. 164). At least some of the reported productive advantage of raised field agriculture probably reflects the greater quantity and superior quality of the seed employed in the rehabilitation projects.

The average annual labor input necessary to cultivate a given area (A) in the context of any given crop cycle can be calculated in the following manner:

$$L(A) = A\left(\frac{R}{r} + \frac{\sum_{x=1}^{n} N_x}{n}\right), \tag{4}$$

where A is the area in ha, $L(A)$ is the average annual systemic labor input in person-days/ha, R is the average cost of system maintenance or renovation in person-days/ha, r is the frequency of system maintenance or renovation in years, N_x is the average annual cost of cultivation (planting, harvest, etc.) of $crop_x$ in person-days/ha, and n is the total number of years in the cycle.

The variables in this equation are somewhat more difficult to determine than were those of Eq. (4). The values I have selected are summarized in Table 5.

The variable R refers to the labor cost involved in major renovations of the infrastructure underlying the system in question. For example, in irrigated terrace systems, terrace reconstruction, canal cleaning, and so forth would determine the value of R. In the case of raised fields, it consists most prominently of periodic field reconstruction. Following Erickson, I will assume the reconstruction of raised fields to be equivalent in terms of labor cost to their initial construction. It should be noted that the value I employ for R (793 person-days/ha) is substantially lower than similar estimates from raised field experiments in other parts of the world (Denevan et al., 1985; Puleston, 1977; Turner and Harrison, 1981). Erickson (1985, p. 220) convincingly accounts for this discrepancy by pointing to specific labor-saving techniques employed by the people who constructed the fields, specifically the practice of cutting and moving blocks of sod for field construction, rather than loose soil.

Table 5
Values of labor variables

	Modern dryland		Raised fields	
	Potato	*quinua*	Potato	*quinua*
R	0[a]	0[a]	793[a]	793[a]
r	—	—	15[b]	15[b]
N	183[c]	61[d]	250[e]	83[f]

[a] Canahua Murillo and Larico Mamani (1992, pp. 158–159), though they note that actual labor inputs vary dramatically with the type of soil. PIWA (1994, pp. 282–284) provides comparable figures. Others include 760 (Tapia and Banegas, 1990, p. 97), and 200–1000 (Erickson, 1985; Erickson and Candler, 1989; also Denevan, 1982, Garaycochea, 1987). For dryland agriculture $R = 0$, since no infrastructure exists to be reconstructed, assuming, of course, that irrigation is not practiced.

[b] Erickson has suggested that an estimate of raised field reconstruction once every 10 years is a reasonable figure. In the following discussion, I will assume field reconstruction every 15 years, in order to avoid overestimating the labor investment required in raised fields.

[c] Golte (1980, Table 6), for six communities in the Department of Puno. Izqueirdo Condori (1992b) suggests 151. It is reasonable that dryland demands be somewhat less than raised field demands, due to the fact that the yield per hectare is so much lower in dryland systems, greatly reducing the person-days/ha necessary to accomplish the harvest.

[d] Izqueirdo Condori (1992b) notes that dryland *quinua* cultivation labor requirements are approximately one-third those of potato cultivation. I will therefore assume that N for *quinua* is $\frac{1}{3}$ the value for potato cultivation, or, in this case, 61 person-days/ha. Izqueirdo Condori (1992b) provides a comparable value of 52.

[e] Erickson and Candler (1989, p. 240, Table 2). This figure does not count possible splash irrigation in drought years. Izqueirdo Condori (1992a) suggests a higher figure of 312 person-days/ha. However, this is based on observation of a single 0.41 ha plot, and therefore must be regarded with some suspicion.

[f] As with dryland production, I will assume that *quinua* labor input is $\frac{1}{3}$ that for potato production, or 83 person-days/ha annually. This is preferable to Izqueirdo Condori's (1992b) clearly excessive figure of 745.

Given the foregoing, the labor productivity (measured in cal/person-day) of any given crop cycle may be simply calculated using the following equation:

$$E = \frac{P(A)}{L(A)}. \tag{5}$$

This equation yields a measure of efficiency calculated in terms of calories/person-day of labor. In order to calculate the efficiency of various crop rotation cycles for both dryland and raised field agriculture, I wrote a small computer program that identifies every possible unique crop cycle with a total duration of less than 12 years, the length of the longest modern Titicaca Basin cycle given in Orlove and Godoy (1986). The program then calculates the efficiency of each cycle in both raised field and dryland contexts. In the simplified context of cycles consisting only of potato, quinua, and fallow years, and of no more than the specified duration, exactly 278 unique crop cycles are possible. The results are graphically presented in Figs. 3 and 4. In each case, three kinds of crop cycles are distinguished. Those labeled 'possible dryland cycles' are cycles falling into the range of tolerance of modern Titicaca Basin cropping systems, as discussed earlier; that is they have a potato ratio (P) value falling between 0.09 and 0.20, and a fallow ratio (F) between 0.40 and 0.64. Those labeled 'possible raised field cycles' are those deemed to be probably feasible on raised fields, as discussed above; that is, they have a potato ratio value falling between 0.09 and 0.20, but no restriction on their fallow ratio. Thus, all possible dryland cycles are also possible raised field cycles, but not all possible raised field cycles are also possible dryland cycles. The remaining cycles, labeled "impossible cycles," fall outside of these ranges, and are purely hypothetical. They are shown only for reference, and were probably never employed in either dryland or raised field systems.

Fig. 3 presents dryland efficiency (cal/person-day) for all cycles in relation to the potato and fallow ratios. Two things are immediately obvious. First, *quinua* production is much more efficient than is potato cultivation. Thus, the strong negative correlation between efficiency and the potato ratio evident in Fig. 3A. Potatoes produce more calories per hectare, but require a much higher labor investment, and are consequently less efficient than *quinua* in purely energetic terms. Therefore, as the pota-

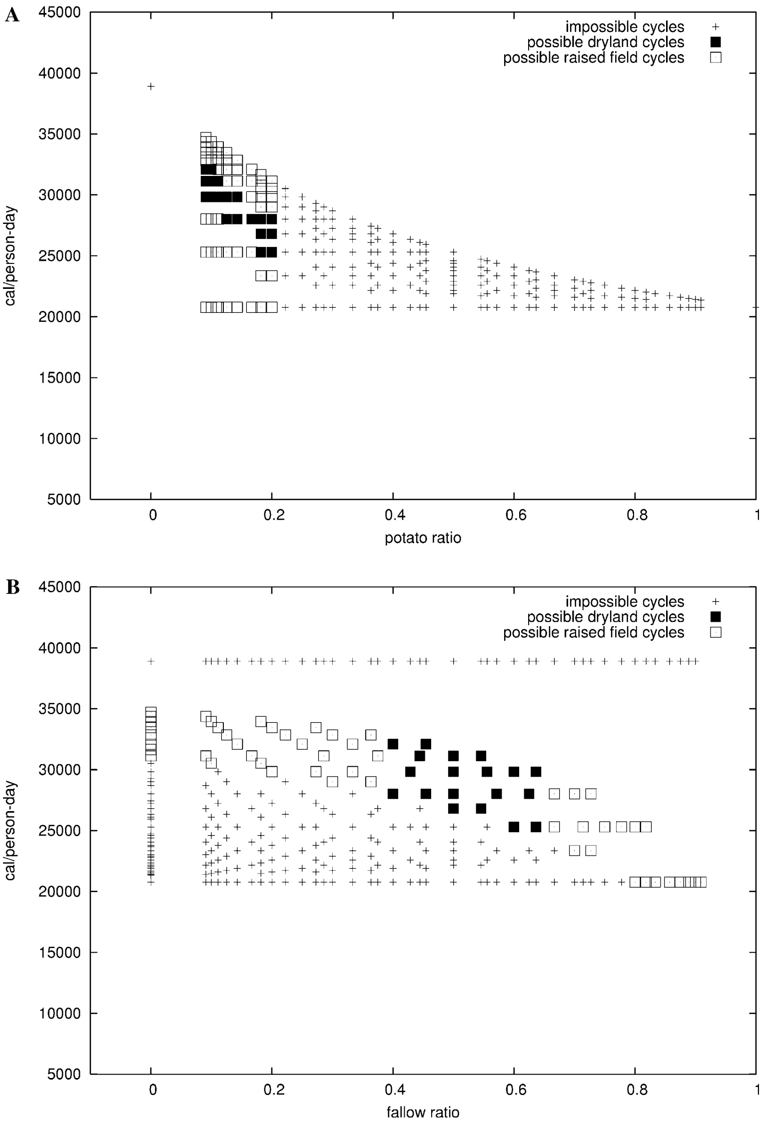

Fig. 3. Relation between dryland labor productivity (cal/person-day) and the (A) potato and (B) fallow ratios.

to ratio increases the efficiency of the crop cycle decreases. Fig. 5 relates the relative efficiency of potato versus quinua agriculture over a wide range of possible and clearly impossible potato yield values. Apparently potato production only becomes comparable to quinua cultivation in terms of energetic efficiency at yields of about 14,000 kg/ha for dryland agriculture, and of over 30,000 kg/ha for raised fields. These yields, though they may have been reached on occasional good years, are well beyond the realm of possibility as average yields in the context of traditional or raised field agricultural techniques.

The second fact worth noting with respect to dryland agriculture is that the fallow period has no impact on crop cycle efficiency in this model (Fig. 3B). This is clearly not realistic, but fallow-related yield variation has not been incorporated into my analysis. The important point, however, is that in dryland systems no labor investment is made during fallow years, so the length of the fallow period does not affect the overall efficiency of a cycle. Therefore *pqqf* is exactly as efficient, in this model, as *pqqfff* and *pqqfffff*. Within the framework of this model, there are therefore two ways in which dryland agriculture can be intensified in the Titicaca Basin context: (1) reducing the fallow ratio and (2) increasing the potato ratio.

The same relation between potato and quinua yields governs the efficiency of raised field agriculture. The difference, of course, is that fallow years do have a labor cost in raised field agriculture, due to the need to period-

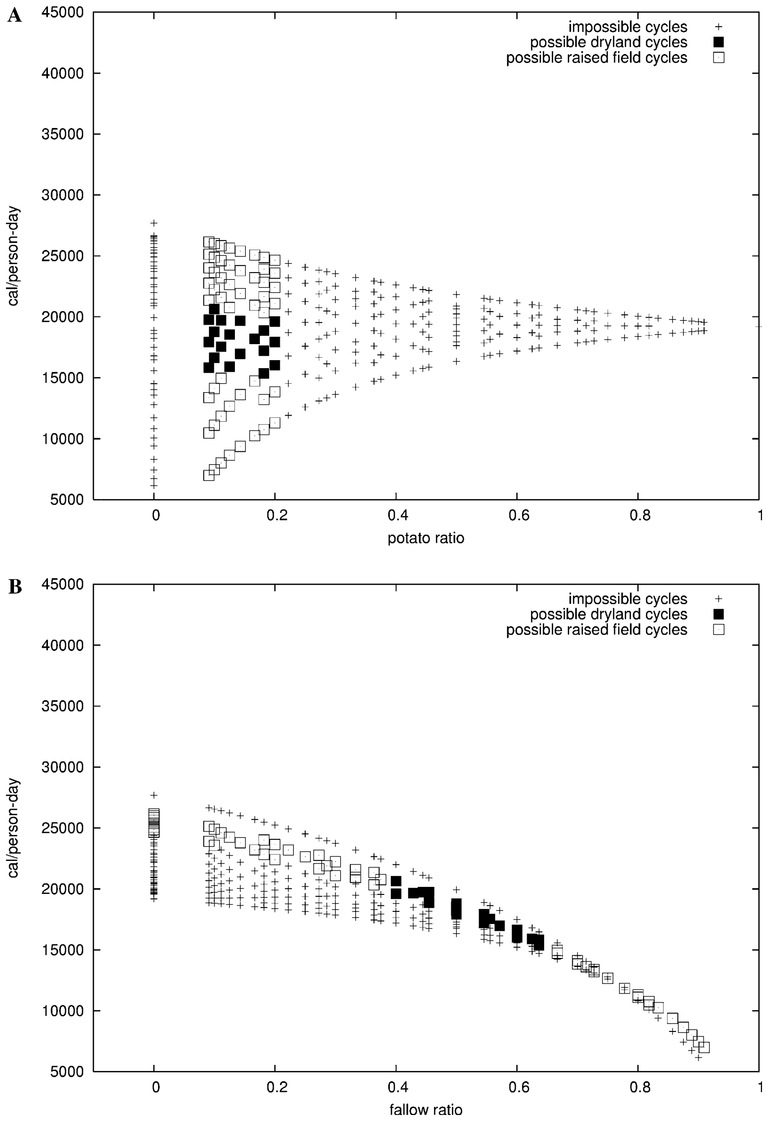

Fig. 4. Relation between raised field labor productivity (cal/person-day) and the (A) potato and (B) fallow ratios.

ically repair and maintain the planting platforms and canals, regardless of whether or not they have been in production or no. Therefore, there is a strong negative correlation between maximum efficiency and the fallow ratio in raised field agriculture (Fig. 4B).

The purpose of this exercise, however, was to determine the relative efficiency of raised field and dryland systems. Fig. 6 charts the relative efficiency of raised field over dryland agriculture in relation to the potato ratio. Clearly, raised fields become relatively more efficient as the potato ratio increases. Also clear, however, and as significant, is that raised fields are less efficient than dryland agriculture for all possible cycles. That is, the ratio of raised field over dryland efficiency is always less than one.

Some may object that a direct comparison of efficiency by crop cycle misrepresents the situation. After all, raised fields are (theoretically) capable of supporting crop cycles that are impossible in dryland contexts. Would it not be more appropriate to compare the most efficient possible raised field cycle with the most efficient possible dryland cycle? In my earlier consideration of modern Titicaca Basin agricultural systems, I suggested that the optimal possible dryland cycle (minimizing F and maximizing P) would be *pqqff*, while the optimal possible raised field cycle would be *pqqqq*. The raised field efficiency of *pqqqq* is 24,649 cal/person-day, under my assumptions, while the dryland efficiency of *pqqff* is 28,019 cal/person-day, for a raised field/dryland ratio of 0.88. Under the assumptions that I outlined earlier,

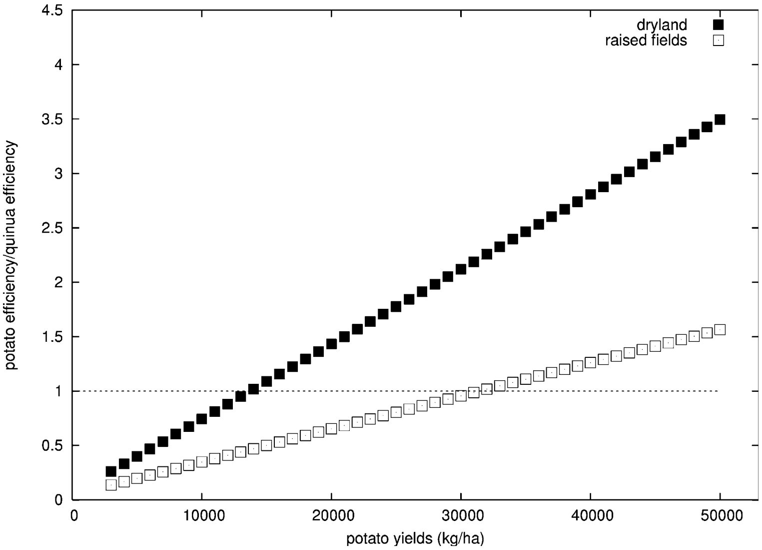

Fig. 5. Potato/*quinua* labor productivity for a range of hypothetical potato yields.

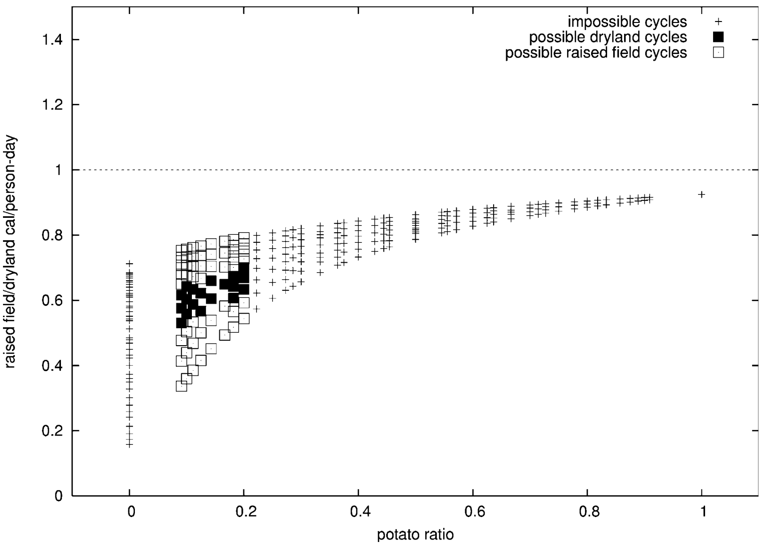

Fig. 6. Raised field/dryland labor productivity for all crop cycles.

then, raised fields are in all cases at least slightly less efficient than dryland agriculture in terms of labor productivity.

The most extensive systematic comparison of dryland and raised field yields of which I am aware reaches a similar conclusion. Fernández Valdivia and Garcia Chire, 2000, in a study of dryland (*pampa*) and raised field production in eight farming communities over five agricultural seasons (1994/1995 to 1998/1999), compare dryland and raised field production using a series of measures. For our purposes, the most significant of these is their *Tasa de Remuneración a la Fuerza de Trabajo Familiar*, an index of return on household labor similar to my own measure of efficiency. In terms of this measure, they found raised fields to be superior to dryland agriculture in two of the eight communities (2000, p. 135). However, in two other communities, dryland agriculture was superior to raised fields. There was no significant difference in the remaining cases. These inconclusive results are mirrored in their analysis by agricultural season. Raised fields were superior to dryland agriculture in only one of the 5 years; in the other years there was no significant difference between the systems (2000, p. 137). Altogether, then, their results indicate that there is no significant difference between raised field and dryland agriculture in terms of efficiency. This is so despite the fact that they were comparing raised fields to flat *pampa* areas. Most highland agriculture takes place on the flanks of hills, which are considerably less prone to frost damage than are the flat *pampas*. Yields of potatoes, in particular, are typically considerably higher in hillside fields than on the *pampa*.

My conclusion, then, is this: raised fields were probably always somewhat less efficient than dryland agriculture, and may have been considerably less efficient. This exercise, though unavoidably limited and schematic, has shown the hyperproductivity hypothesis to be entirely without foundation in fact. Raised field agriculture satisfies the two assumptions of Boserup's model, and therefore cannot be thought of a non-Boserupian intensification. When the necessity for crop rotation on raised fields is recognized, the failure of the raised field rehabilitation projects is entirely understandable. What is more difficult to explain, however, is why, in light of the foregoing, raised fields were invented and used in the first place, and why Titicaca Basin peoples invested such a tremendous quantity of labor in their construction.

Why raised fields?

The hyperproductivity hypothesis has been demonstrated to be untenable. Raised fields were never the economic miracle they have been perceived to be, and they are apparently incompatible with the economic strategies of modern farmers in the region. However, alternative explanations of prehistoric raised field use remain possible. I will consider three such explanations here: (1) the Boserup model (population-driven intensification), (2) the residential preference model, (3) the risk reduction model, and (4) the staggered production cycles model. There is no reason to suppose that a single model can account for raised field use throughout all of Titicaca Basin prehistory. On the contrary there is every reason to suspect that a variety of intensification pathways resulted in raised field use at various times and in various places. All of these models are plausible in certain circumstances, and all may prove useful in explaining raised field cultivation in at least some cases.

The Boserup model

The most obvious of these models is that raised fields are the result of a classical Boserupian intensification process (Boserup, 1965); that is to say that they represent an increase in productive density made necessary by an increase in population density, at the cost of decreased labor efficiency. A possible scenario would be that slow, steady, long-term population growth throughout the Formative Period and Middle Horizon in the Titicaca Basin [documented for the southern Basin by Bandy (2001)] resulted first in the shortening of fallow periods and the maximization of the potato ratio, and then, when the sustainable limits of this strategy had been reached, in the use of raised fields.

In conditions of low population density and ample land availability, such as pertained early in the Formative Period (Bandy, 2001), we can expect *quinua* to have been significantly more important in the diet than it is at present. There are preliminary indications that this was indeed the case (Whitehead, pers. comm. 2003). As population density increased in the Middle and Late Formative, Boserup's model would predict a gradual shortening of the fallow period and a maximization of the potato ratio. Once the limits of this strategy had been reached raised fields became important as a means of further increasing production density. This is a very interesting model, in that it suggests that the status of the potato as the highland agricultural staple may have been a relatively late development. It also suggests that raised field agriculture became important only in the later periods of Titicaca Basin prehistory. This is clearly a subject that merits further research. Fig. 7 graphically depicts the sequence of intensification as predicted by the Boserup model.

Unfortunately, the existing data on prehistoric population distribution in major raised field areas are not sufficient to allow a systematic evaluation of the Boserup model. It is certainly a plausible scenario. Importantly, it is also capable of explaining the abandonment of raised fields with the collapse of the Tiwanaku state. This abandonment could have been the result of the general decrease in population density that followed Tiwanaku state collapse in many areas of the Titicaca Basin, especially in the Tiwanaku heartland (Bandy, 2001, pp. 243-247).

The residential preference model

An intriguing and rather less conventional model was proposed by Erickson to explain the early raised fields he documented in the Huatta area (1988b). Residential preference has long been understood to be related to agricultural intensification in at least some cases (Hastorf and Earle, 1985, p. 591; Leach, 1964). Erickson posits a specifically lacustrine residential preference for early agricultural populations in the Huatta area. Raised field farming would have allowed these people to live adjacent to the lake, a very rich resource zone, while at the same time practicing agriculture. Therefore, raised field agriculture "permitted a dense population of wetland-oriented people to maintain sedentary lives" (1988a, p. 13). It is an entirely reasonable proposal that groups with a lacustrine settlement preference would adopt raised field cultivation in areas, such as Huatta, where the lake margin is separated by a large, marshy *pampa* from zones which would be productive for dryland farming.

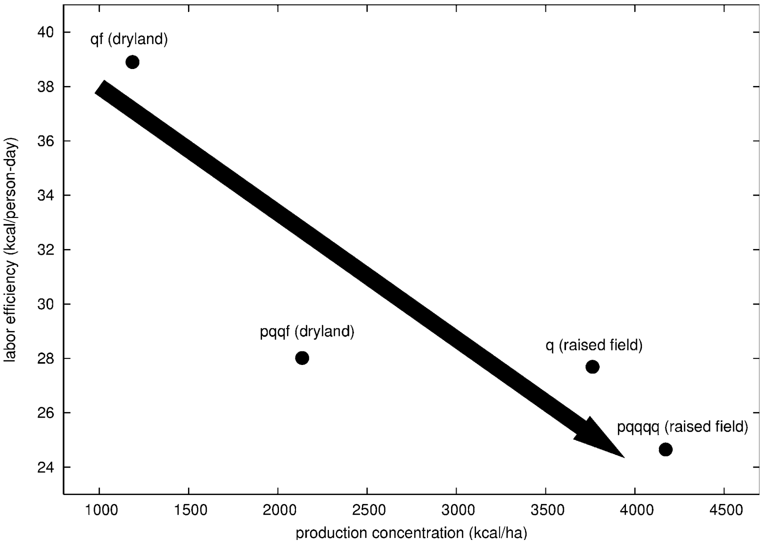

Fig. 7. Several dryland and raised field cycles plotted using labor efficiency (kcal/person-day) and production concentration (kcal/ha). The large arrow indicates the direction of the Boserup model intensification sequence.

Erickson's model could be extended to predict the existence, in areas of extensive lakeshore *pampa*, of separate raised field and dryland farming economies, practiced by separate populations. The first group would have a lacustrine residential focus, and depend significantly on fishing for their subsistence, while the second would settle in inland areas suitable for dryland agriculture and practice an agropastoral lifeway. Such economic specialization could even have given rise to ethnic distinctions, producing a Formative Period equivalent to the late prehistoric Aymara/Uru society (Wachtel, 1986). More research must be done before we can begin to evaluate such a model empirically, but it seems to me that this model is likely to explain the initial innovation of raised field agriculture in the Early or Middle Formative, particularly in the Huatta area. The residential preference model is limited, however, in that it is incapable of explaining raised field use in areas directly adjacent to land well suited to dryland agriculture. Many substantial Titicaca Basin raised field groups, including the relatively late constructions of the Katari Basin and the Tiwanaku Valley (Janusek, 2001; Janusek and Kolata, 2003), are located in such areas. The residential preference model may be capable of explaining certain specific instances of raised field use, but another theory is necessary to account for raised field agriculture in most of the Titicaca Basin, especially in later prehistory.

The risk reduction model

Some scholars studying raised fields in other areas of the world have suggested that they may have served as part of a strategy for reducing subsistence risk (Gallagher, 1989; Gartner, 2003, pp. 24–25). In this model, "predictable yields over the long term apparently compensate for the labor inputs of raised field construction" (Gartner, 2003, p. 25). There may be some validity to this model in the Titicaca Basin case. The Titicaca Basin is certainly a risky environment for agriculture, and Titicaca Basin farmers display a great concern for reducing the risk of subsistence failure. However, as in most of the Andean highlands, risk reduction strategies typically involve the dispersal of small fields over a wide area and across a great variety of microclimatic zones in order to reduce the likelihood of catastrophic subsistence failure (Goland, 1993). This is a very extensive strategy. Therefore, raised fields, which entail a much greater localization of productive activity, would seem to be an unlikely risk reduction strategy in the highland environment. Raised fields remain susceptible to many of the same events that produce agricultural failure in dryland fields, such as hail and severe frosts. Raised field agriculture is certainly not consistent with ethnographically known risk mitigation practices in the Andean highlands, and it seems unlikely that a convincing model of raised field agriculture will be formulated in these terms.

The staggered production cycles model

I would like to propose a fourth alternative model of raised field use: the staggered production cycle model. This model posits that water in the canals between the planting surfaces of raised fields made possible what Erickson and Candler (1989) call 'splash irrigation.' This practice permitted farmers to offset the raised field and dryland agricultural cycles by planting raised fields significantly before the onset of the rains. In this way it was possible to create a raised field agricultural cycle separate and offset from the dryland cycle. It is an example of what Golte (1980) has called a 'polycyclic strategy.'

Golte has done Andeanists the very great favor of pointing out that the fundamental limiting factor for highland agriculture is not so much labor availability as it is the problem of scheduling. I regard his work as absolutely fundamental for understanding ancient as well as contemporary Andean political economies. In his slim volume, Golte (1980) observes that in areas where the growing season is tightly circumscribed, as in the Andean highlands, a labor scheduling problem arises. In such areas, planting and harvest must take place at certain well-defined points in the annual cycle. There is little room for variation, since to plant too early would mean that the crops would fail for want of rain, and to plant too late would not allow sufficient time for crops to mature before the onset of killing frosts. This situation creates an uneven distribution of labor requirements over the course of the annual cycle, and a severe "labor crunch" during the harvest.

Fig. 8 displays the average monthly values for mean minimum nighttime temperature, mean precipitation (percent), and agricultural labor (percent) for the Titicaca Basin. The climatic data are from Graffam (1990). The labor figures are calculated from information on modern potato cultivation in Chucuito, a lakeside community in Puno (Golte, 1980). The figure clearly displays the onset of the agricultural year, with field preparation beginning with the first rains in September and October, planting in November and December, and harvest in late April and May, before the mean nighttime minimum temperature dips below freezing in June. It is also apparent that the harvest season presents the most significant 'crunch' period in the cycle, with the months of April and May accounting for 47.2% of the total annual agricultural labor. It is obvious that the labor requirements of the harvest are the most significant variable constraining the amount of land that a household or community may plant in a given year. Since the most concentrated and intensive labor demands of agricultural production occur at the time of the harvest, a productive unit—a household, for example—may not plant more land than it is capable of harvesting within a short and sharply delimited time period. In such situations groups often develop mechanisms for spreading their subsistence labor more evenly throughout the year. Golte calls these polycyclic strategies.

The most fundamental and probably ancient example of this sort of strategy in the Andes is tuber agropastoralism. In this system, the timing of labor investment in camelid pastoralism is offset from the crunch periods of the agricultural cycle. In this way labor that cannot be invested in agriculture is invested in herding, and vice versa. This results in a more complete and efficient use of labor throughout the year than would be possible in either a purely agricultural economy or in a purely pastoral one.

Another kind of polycyclic strategy is to be found in the agricultural systems of the highland valleys, with which Golte is primarily concerned. In these environments, producers practice a form of microverticality, exploiting the agricultural properties of land at varying elevations. In these different elevations a variety of crops may be planted, each with a distinct growing season and set of labor requirements. In this way a whole series of staggered agricultural cycles are exploited, together with

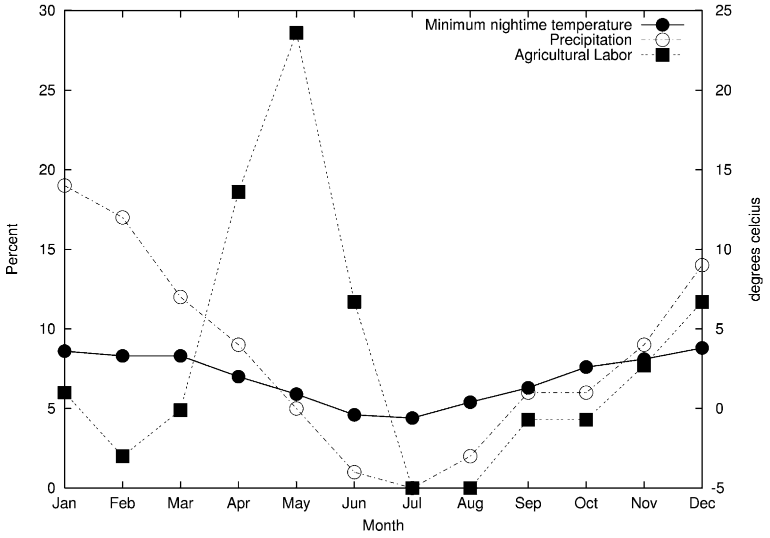

Fig. 8. Climatic and agricultural seasonality in the Titicaca Basin.

pastoralism in high-altitude grasslands, to spread productive labor more evenly throughout the year. This is a technique for maximizing annual productivity at the household level.

In this connection, it is interesting to note that Golte singles out the Titicaca Basin as an area in which the possibilities for polycyclic labor maximization are extremely limited. This is because the circumlacustrine plain lacks the dramatic vertical relief of the valleys and is therefore unsuitable to microverticality-oriented agricultural strategies. Herding and fishing and more recently wage labor can make up for this to some extent. But, Golte points out, opportunities for maximizing the use of agricultural labor throughout the year remain limited in the Titicaca Basin relative to other areas of the Andean highlands. I would like to propose, however, that raised field agriculture represents exactly such a strategy for the staggering of agricultural cycles and the maximization of annual productivity, and one well suited to the Titicaca Basin environment.

The key to this strategy would have been the water in the canals, which, as Erickson and Candler have noted, would permit splash irrigation of the fields (Erickson and Candler, 1989) . This practice has been observed by Erickson, and he concludes that splash irrigation of planting surfaces with water from the canals can be accomplished with very little effort. Indeed, in one drought year in Huatta, Peru, his raised fields produced good yields while surrounding dryland fields failed completely, due solely to the fact that the raised fields were splash irrigated as necessary. This technique could have allowed planting to take place after the danger of frosts had passed, but still well before the onset of the rainy season and the dryland planting period. In this way, a separate raised field agricultural cycle could be established, complementary to the dryland cycle.

It is important to emphasize that not all raised fields could have been used in this way. The use of raised fields in a polycyclic strategy depends upon the presence of permanent standing water in the canals between the planting platforms, and especially during the dry season. Generally speaking, there are three sources for this water: (1) groundwater, in areas with a high water table, (2) springs or rivers, in the case of fields that are fed by canals or aqueducts, and (3) rainfall. Only raised fields fed by groundwater or canals could be employed in the polycyclic fashion I propose, since rain-fed fields would not have water in the canals during the dry season. However, there is considerable evidence that many raised fields fall into these two categories, and that substantial water control features are often associated with raised field groups (Kolata and Ortloff, 1996b; Ortloff, 1996; Ortloff and Kolata, 1989). A systematic study of the distribution and relative importance of these three categories of water sources in the major raised field areas would be an important contribution to this debate.

The use of irrigation to allow early planting and polycyclic production is not unknown in the modern Andes. Hastorf, for example, has observed this practice in the Mantaro Valley of central Peru (1993; Hastorf and Earle, 1985). She describes the use of irrigation canals to plant certain fields 1–2 months in advance of the onset of the rains and the main planting. These fields are then harvested before the harvest of the main body of fields is begun.

Polycyclic irrigation strategies are also practiced in the modern Titicaca Basin. In a review of recent irrigation development projects in Puno, Berastain (1996, p. 114) notes that in 74% of cases irrigation is used to begin the agricultural cycle in August. As can be appreciated in Fig. 8, this is more than a month before the beginning of the dryland cycle. As Berastain observes, the effect of this is to add a "second season" to the year (1996, p. 114). Not only does early planting spread agricultural labor more evenly in time, Berastain notes that it also improves yields by reducing the risk of early frosts that affect the growth and ultimate yields of crop plants (1996, p. 115).

By employing splash irrigation, therefore, a separate raised field agricultural cycle can be established, beginning and ending as much as 2 months in advance of the dryland cycle. Even if this is the case, however, we have yet to explain the massive labor investment in raised fields evident at certain times and places in Titicaca Basin prehistory. After all, modern and late prehistoric farmers seem to have met their subsistence needs within the constraints of a single dryland agricultural cycle, and the fact remains that raised fields were less efficient than dryland farming. Little incentive would have existed for farmers to have invested in this second, more labor-intensive agricultural cycle. A possible explanation, however, will emerge from a consideration of prehistoric Titicaca Basin political economies.

Raised field agriculture and Titicaca Basin political economies

Large, complex political formations employing a system of staple finance (Earle, 1987; D'Altroy and Earle, 1985) depend upon a storable, sizable agricultural surplus. This surplus is used to support retainers ranging from artisans, priests, and scribes to warriors and bureaucrats, to sponsor spectacular displays of generosity such as religious festivals or athletic competitions, to underwrite the construction of public works such as roads and temples, and, ideally, to provide a buffer against periods of agricultural failure. In the absence of an advantageous external arrangement such as an important position on a major trade route, this surplus must be generated by the labor of a subject population. If a polity is to generate an agricultural surplus inter-

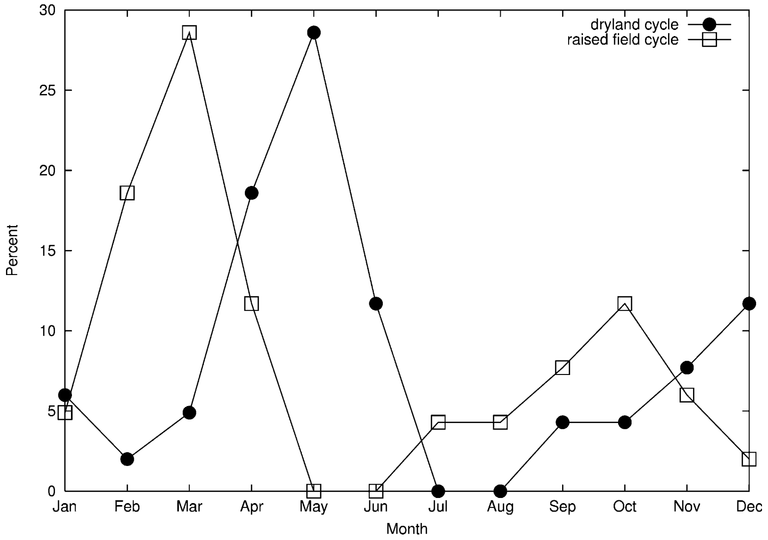

Fig. 9. Monthly labor requirements of hypothetical raised field and dryland agricultural cycles.

nally, at least some social segment is required to intensify agricultural production. That is to say that certain segments of the population must produce more than is necessary for their own subsistence.

In such societies the political economy turns upon the strategies employed by elites or leaders to effect this intensification, and the processes by which the generation and distribution of this surplus are negotiated. In the Titicaca Basin, the problem of generating this agricultural surplus is particularly acute. This is so precisely because, as Golte noted, the dryland agricultural cycle is so tightly constrained and there are so few possibilities of polycyclic production. In order to intensify dryland agricultural production, elites or leaders would inevitably demand labor from a subject population in precisely the busiest times of the agricultural year, particularly at the harvest. That is to say that the agricultural labor demands necessary for surplus generation would have directly conflicted with the critical subsistence activities of the farmers themselves, a circumstance which can be expected to have generated considerable discontent or even open rebellion if pushed too far. This circumstance would have severely limited the annual rate of agricultural surplus production in any Titicaca Basin political economy dependent upon the intensification of dryland agriculture.

The implications of the strategy of staggered production cycles for Titicaca Basin political economies are profound. I have argued that the lower efficiency and labor-intensive nature of raised field agriculture made it unlikely that raised fields would be employed by independent farmers except in a small number of special situations. An example of such a situation would include Erickson's postulation of a lacustrine residential preference, as discussed earlier. Let us therefore assume that the majority of subsistence production took place in dryland fields. If would-be surplus extractors were to somehow finance the construction of a complex of raised fields, and begin the planting of those fields 2 months prior to dryland planting, that is in August and September rather than in October and November, no labor whatsoever would be required of tribute payers in the month of May, the period of the dryland harvest, and the busiest time in the dryland cycle. The raised field harvest would already have been completed in March and April, months when dryland cultivators devote relatively little time to their own fields. This means that surplus-related labor demands would have conflicted virtually *not at all* with the subsistence activities of the population. Fig. 9 depicts these offset cycles of surplus-related agricultural labor demands and commoner subsistence activity in this scenario, assuming that all surplus-related agriculture took place in raised fields and all commoner agriculture in dryland systems.[6]

In light of the foregoing, the unique advantage of raised fields for complex Titicaca Basin polities lay not in their hyperproductivity, as has previously been assumed, but rather in the fact that they presented the opportunity to exact agricultural labor from subjects while minimizing interference with household subsistence activities. At the same time they made possible the mobilization of a vast labor potential that could not otherwise have been applied to agricultural produc-

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[6] This is of course a highly unlikely scenario, and is presented only as an ideal situation. Reality was no doubt more complicated.

tion. This would have simultaneously (1) reduced conflicts between surplus-related labor demands and the subsistence interests of the populace and (2) permitted a significantly higher overall rate of annual agricultural labor extraction and therefore of annual surplus production. Although raised fields were not energetically efficient when compared to dryland agriculture, they were politically expedient in terms of minimizing conflicts between surplus production and subsistence. Raised fields, therefore, are inefficient when viewed from the perspective of the individual farmer, but are highly efficient when viewed from the perspective of a surplus-producing polity.

This model, if valid, has several important implications. The first is that Kolata (Kolata, 1986, 1991; Kolata and Ortloff, 1996b) is correct when he argues that raised field agriculture was indeed the single most critical aspect of the surplus economy of the Tiwanaku state. If a prolonged drought in A.D. 1100 compromised the functioning of the raised field system, this would have undermined the entire political economy and could quite possibly have brought about the collapse of the Tiwanaku state, as Kolata and Ortloff argue (Kolata and Ortloff, 1996a).

If the strategy of staggered production cycles was indeed a central component of the Tiwanaku political economy, we may also begin to view in a new light the structural conditions underlying the state's rather limited and sporadic expansion beyond the confines of the Titicaca Basin. Unlike the Wari state, whose political economy seems to have been 'portable' (at least within a highland context) the Tiwanaku political economy was 'tethered' to areas that were (1) suitable for large-scale raised field agriculture and (2) characterized by a tightly constrained dryland agricultural cycle. That is to say that it was tethered to the circumlacustrine areas of the Titicaca Basin itself. Thus Tiwanaku expansion beyond the Basin took the form of trade relationships or small enclaves as in San Pedro de Atacama, or of isolated colonies founded to produce goods for export to the *altiplano* capitol, as in the Moquegua Valley of Peru (Goldstein, 1989, 1993) rather than the territorial conquest and colonial administration of local populations. Indeed, it seems likely that the Tiwanaku state never did expand beyond the Basin in a true territorial or administrative sense. This fact may have been a result of the fundamental importance of staggered production cycles in the Tiwanaku political economy, and the non-portable nature of this strategy.

Another implication is that, at least as far as the larger Tiwanaku period raised field complexes are concerned, most of the people providing labor for the cultivation of the fields probably did not reside adjacent to the fields themselves. Thus, the so-called 'cities' of Lukurmata (Bermann, 1990, 1994) and Pajchiri, that administered raised field production (Kolata, 1986, 1991, 1993), may not have been truly urban in character. Rather, they may have consisted of a core of permanent inhabitants and a large area that was occupied only seasonally, when large groups of tribute-payers travelled to the area to work on the fields. The outer sectors of these sites, which are at present poorly known, may resemble seasonal work camps or temporary tent cities, as has been suggested for the site of Chen Chen in Moquegua (Bandy et al., 1996), rather than nucleated, urban habitation. This is an important and archaeologically testable implication of the staggered production cycle hypothesis.

If, as I have suggested, the strategy of staggered production cycles was fundamental to the political economy of Titicaca Basin polities, then my model has a further implication. If Titicaca Basin surplus extraction strategies emphasized minimizing conflict between commoner subsistence and polity-related surplus production, then most polities that employed such a strategy were probably characterized by only weakly developed coercive institutions. They would likely have relied upon institutionalized hospitality to convince farmers to work the polity's raised fields. Complex examples of such polities might be called "hospitality states." By the Tiwanaku period, then, we should expect the development of massive systems of institutionalized hospitality. Evidence for large-scale seasonal feasting activity should be abundant, together with the architectural, iconographic, and material assemblage correlates of such activity. In fact, the model of Tiwanaku as a "hospitality state" is in considerable agreement with recent statements by Janusek, who considers "Tiwanaku cultural hegemony" to be "characterized by the distribution of elaborate [ceramic] wares with food and drink in return for services on the part of local groups" (Janusek and Kolata, 2004, p. 416).

Moreover, the model has the potential to explain, in part, the peculiar character of Tiwanaku state collapse. Recent treatments of Tiwanaku collapse have emphasized the importance of a severe drop in lake level and probable drought taking place sometime in the eleventh century (Abbott et al., 1997; Binford et al., 1997). Kolata and Ortloff (1996a) argue that this drought led to an agricultural collapse, since water could no longer be maintained in the raised fields, and consequently to the collapse of the Tiwanaku state. However, it is not possible at present to demonstrate that the eleventh century drought would have made raised fields unusable. An alternative scenario, bearing in mind the proposed polycyclic use of raised fields, is equally feasible (Bandy, 2001, p. 300–302). In this scenario, the eleventh century drought gradually reduced the productivity of dryland agriculture, leading to subsistence stress for the mass of the population of the Tiwanaku core area. These populations, responding to subsistence stress, could have intensified household agricultural production. As I have argued, raised field agriculture was one of the very few avenues of agricultural intensification possible in the Titicaca Basin environment. However, if rural households began to employ raised fields for their own subsistence production, state labor demands would then have come to conflict with household agricultural labor. Over time, an acceleration of this process could gradually have deprived the elite of Tiwanaku of the labor necessary to operate the state raised field complexes. A process such as I have described would lead to a gradual reduction in state surplus production, and a concomitant decline in the scale of hospitality deployable by the Tiwanaku elite. This process would be self-reinforcing, and over time would lead to state collapse. Thus, collapse would be a prolonged process, characterized by the gradual disappearance of state ritual and paraphernalia, and by the gradual depopulation of the capitol as elites lost the ability to support large numbers of retainers, craft specialists, and other dependents. This is exactly the manner in which Janusek (1994) reconstructs Tiwanaku collapse from his excavations in the capitol and in Lukurmata.

A final implication of the staggered production cycle model is that we should expect raised field agriculture to increase in importance with the formation of the first complex multi-community polities at the beginning of the Late Formative Period, around 200 B.C. (Bandy, 2001). Erickson (1988a, 1993; also Graffam 1990) has argued convincingly that a large, formal bureaucracy and centralized authority were not necessary for the effective operation of raised field systems. The strategy of staggered production cycles would have been available to the earliest polities in the Titicaca Basin, and raised field use should be generally correlated with the level of surplus extraction; that is, it should increase as the scale and complexity of political systems increase. Stanish (1994) has presented compelling evidence that this is indeed the case in the Juli-Pomata region of Peru. In Juli-Pomata, settlement patterns indicate increasing importance of raised field areas in the Late Formative and into the Tiwanaku period, with a subsequent decrease in importance in the Late Intermediate Period following Tiwanaku collapse. I have argued that a similar pattern is evident in the Tiwanaku core region (Bandy, 2001, pp. 295–300). In this area a major, long-term movement of population into raised field areas is observable beginning in the Late Formative and culminating in the Tiwanaku period, with areas distant from raised fields experiencing reduced or negative population growth.

Conclusions

In this paper, I have argued that the hyperproductivity hypothesis—the assertion that continuous, high-yield potato cultivation was possible on Titicaca Basin raised fields—is untenable. Continuous potato cultivation was never possible in the Titicaca Basin due to the presence of potato-parasitic nematodes of the genus *Globodera*, as well as to other factors. Taking modern Titicaca Basin agricultural systems as a starting point, I modeled a variety of possible and hypothetical crop cycles on raised fields and in dryland contexts. The results of this exercise indicated that raised field agriculture has never been more efficient than dryland agriculture in energetic terms, and was probably considerably less efficient. Raised field agriculture, in other words, cannot be thought of as an example of non-Boserupian intensification.

If raised fields were less efficient than dryland agriculture, why was so much labor invested in their construction? Throughout this paper I have argued that raised field agriculture conforms to the assumptions of Boserup's model. The implications of this are profound. Debates over Titicaca Basin raised fields usually center on the question of whether the construction and use of the intensive agricultural landscapes was related to the activities of small-scale communities (the "bottom-up" perspective) or to centrally directed complex polities (the "top-down" perspective) (see Janusek and Kolata, 2004, p. 405). The Boserupian character of raised field agriculture as an intensification process means that "bottom-up" explanations must confined to a narrow set of conditions.

Raised fields are less efficient than is traditional dryland agriculture. This being the case, there are two scenarios that are capable of explaining raised field construction and use by small-scale autonomous communities. I have called these the "residential preferences model" and the "Boserup model." In the first case, raised field use by autonomous communities will be restricted to a small number of localities where a lacustrine residential preference precludes the practice of dryland agriculture. In the second, raised field use by autonomous communities will take place only when regional population density has increased to the point that further sustainable intensification of dryland agriculture is not possible; that is, it will take place only in the later periods of Titicaca Basin prehistory. Raised field use by autonomous communities certainly took place in Titicaca Basin prehistory. However, it was probably considerably less common and less important than has been envisioned by proponents of the "bottom-up" perspective.

On the other hand, it would appear that raised field agriculture has certain properties that make it ideally suited to a particular form of complex political economy. I am referring to a model of raised field agriculture as a polycyclic strategy; the "staggered production cycle model." Early planting and harvest of raised fields would make it possible for Titicaca Basin polities to offset surplus production from commoner subsistence production, and therefore would allow polities to minimize the degree to which they interfered in the subsistence activities of their subject populations. Minimizing this conflict would permit a greater overall level of surplus extraction, by tapping labor that ordinarily would be unavailable for agricultural production. My arguments are therefore in considerable agreement with the proponents of the "top-down" perspective; most raised field agriculture in most of Titicaca Basin prehistory may be expected to be related to surplus production by complex polities.

Finally, the "staggered production cycle model" was argued to be capable of explaining some of the idiosyncratic properties of the Tiwanaku state and earlier Titicaca Basin polities, and possibly the character of Tiwanaku state collapse. Titicaca Basin social evolution, though it shares some features with other state formation sequences worldwide, is idiosyncratic, and was indelibly marked by the environment in which it played out. The Titicaca Basin presented difficult challenges for emerging polities, but it also presented unique and compelling opportunities. My analysis suggests that complex polities and states emerged in the Titicaca Basin with only weak institutions of coercive authority. Instead, they relied heavily on a strategy that minimized their economic conflicts with subject populations while still allowing the generation of a sizable agricultural surplus. This strategy, however, was not portable to areas outside of the Titicaca Basin, and it may be for this reason that Tiwanaku never assumed a truly imperial aspect. The marked differences between Tiwanaku and Wari as regional and macro-regional phenomena may be accounted for by a fundamental difference at the core of their political economic systems.

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