were brought about by Indonesian strategies to forcibly incorporate Papua into the Republic. The results of this study further our understanding of social change in Dani society and provide a model by which to understand the processes of interregional interactions and nutritional transitions in other times and regions.

Acknowledgments

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Comments

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Somerville et al. suggest that their study provides "a model to understand processes of nutritional transitions in other times and regions." As an archaeologist with an interest in the prehistory of the wider Australasian region, I am intrigued by the logistics of prehistoric food production, human decisions, and resulting social and ecological changes. Somerville et al.'s analysis of the differential adoption of foods by communities that were spatially and social connected to the source of the new foods (the missionary airstrip) offers an example of a social process that I could only roughly outline using archaeological evidence. Their study makes several suggestions for New Guinea's prehistory that should be evaluated in light of the long human occupation of the region.

The island of New Guinea was colonized by modern humans approximately 40,000 years ago, when it was still part of the supercontinent of Sahul, which encompassed New Guinea, Australia, and Tasmania. Although archaeological evidence from the earliest period is scarce, human colonists arrived by boat and subsisted on Sahul's abundant flora and fauna. Archaeological evidence suggests that over the millennia and across the transition from the Pleistocene to the Holocene, the earliest New Guinean human diets retained their hunter-gatherer elements. Bones, stones, and nutshells from excavations have confirmed that humans consumed the fruits and nuts of pandanus (*Pandanus julianettii*) and canarium (*Canarium salomonense*; Yen 1991), which grew throughout the region, and a range of small animals, including birds, possums, bats, and snakes (Bourke 2009). At the onset of the Holocene, human diets in some parts of New Guinea transitioned to include more carbohydrate-rich foods (Denham 2011). Wild stands of native plants, such as taro (*Colocasia esculenta*), sugarcane (*Saccharum officinarum*), and bananas (*Musa* species), became the focus of human manipulation and transfer. By 7,000 years ago, there is evidence for major ecological change in New Guinea; swamps were drained and modified to provide suitable habitats and surfaces for producing food (Denham 2011; Denham, Golson, and Hughes 2004; Golson 1991), and some plants show signs of domestication. From a few archaeological sites, we can produce a model of the early Holocene New Guinea subsistence system: low-level food producers who focused on the production of carbohydrate-rich fruits, roots, and stalks, supplemented with wild-collected fruits, nuts, animals, and birds. In some areas, wild foods provided a greater proportion of the diet (Bourke 2009).

Somerville et al. describe the traditional subsistence system of the Mulia Dani as being focused on the cultivation of sweet potato (*Ipomoea batatas*), red pandanus fruit (*Pandanus conoideus*), taro, sugarcane, and bananas. At the time of data collection in the 1980s, the sweet potato was the most important crop and constituted 85%–90% of the human diet. The authors note that the adoption of sweet potato farming is an important historical question in New Guinea; this is an understatement but nevertheless true. The work of Yen (1974), Ballard et al. (2005), and others have revealed that the origins, histories, and social processes that led to the dominance of sweet potato in New Guinea are varied and complex. Pigs were also involved, and many have implicated them as the engine behind the production and consumption of sweet potatoes in antiquity (Golson 1982; Watson 1977), but this process was equally varied and complex. Somerville et al. suggest that some of the spatial and temporal aspects of the transmission and spread of sweet potato consumption in humans in antiquity (but not pigs) could be traced via the detection of low bone $\delta^{15}N$ values. I find this to be an interesting proposition and one deserving of comment.

First, testing this hypothesis would require the isotopic analysis of human bone from dated archaeological contexts, preferably many samples from many places. If that hurdle could be cleared, the use of low bone $\delta^{15}N$ values alone would be problematic. Other terrestrial plants that were part of the old (i.e., before the introduction of sweet potato) New Guinea food production system, including taro, sago palm (*Metroxylon* species), pandanus, banana, and other food plants, have similarly low levels of nitrogen (Ambrose et al. 1997; Kinaston et al. 2014). Discerning a sweet potato consumer from among a lineup of similar carbohydrate consumers would be difficult, perhaps impossible, even if the isotopic food web of all of the potential food choices were reconstructed.

The authors also describe the results of their study as having implications for understanding the process of subsistence transition. After examining variables of sex, profession, age, and village location, the authors conclude that individuals from villages that were closer to the source of new foods and who also abandoned their subsistence practices for another (in this case, becoming a wage laborer rather than a sweet potato farmer) showed the most divergence in diet and food preference. In the 1980s, some groups of Mulia Dani relished and consumed meat in higher frequencies, while remote groups craved and subsisted on traditional foods, dominated by sweet potato. This proposed model could be illuminating for understanding prehistory, both before and after the introduction of sweet potato. We would expect that populations closer to the source would transition first and most completely (with children leading the way) and that more distant populations would retain their cravings for the foods most readily available to them. This leads to the question, What was the frontline of culinary desire in prehistoric New Guinea? Could it have been sweet potato when it was the new introduction in New Guinea? Or, was it pork, which required copious amounts of sweet potato foddering in order to be produced? That question aside, either food would have expanded through the anthropogenic niches that had been carved into New Guinea when the early Holocene food production system was established (Golson 1982). Only the most distant ecological limits of food production would have preserved the oldest dietary choices of hunter-gatherers. If we use Somerville et al.'s model as an indicator for the past, it is at the sources, or central nodes of social networks and ecological zones, that we should look for the first signatures of change.

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This study illustrates the potential for integrating ethnographic, human biology, and biogeochemical data sets to empirically test and refine anthropological models and theories of diet change in the context of interregional interactions. Somerville and colleagues' interdisciplinary approach combines diverse kinds of data (demographic, spatial, food preference, oral health, and dietary isotopic analyses) to explore how new foods and food ideas were appropriated among the Mulia Dani of highland western New Guinea during recent historical changes in the community's social organization and level of contact with foreign peoples. While the hypotheses presented about how different variables would respond to dynamic factors such as the degree of contact with missionaries or relative biological age were often correct, there were also a number of instances where the integration of different data sets provided new evidence that did not support expectations based on trends seen in other ethnographical contexts. These findings, therefore, provide an important example for the value of real-world hypothesis testing using multiple methodologies for developing more nuanced anthropological models for the dynamics of culture change.

From an archaeological perspective, as the authors point out, one of the most interesting contributions from this study is that it explores links between data that can be archaeologically testable (i.e., isotopic analyses of human materials and biological data on dentition, age, and sex) and data that is usually archaeologically absent (i.e., known food preferences, occupation, and detailed residence patterns) in a well-documented context of changing diet and social organization. The linking of these two types of data opens up the possibility of using insights gained through the study of a modern population, by way of ethnographical analogy, for interpreting diet changes in more ancient times.

As in other areas of archaeology, ethnographical analogies (for review, see Ascher 1961; Orme 1974; Wylie 2002:136–153) could become an important comparative source for developing anthropologically informed interpretive frameworks for biogeochemical analyses of the human past. Isotopic data, in particular, has become a major line of evidence for understanding past dietary changes and associated social, economic, and environmental processes. A common issue for interpreting isotope data is that relevant archaeological contexts often lack detailed socioeconomic information (i.e., the kinds of data that are available to ethnographical researchers), and this creates a challenging environment for assessing the relative merit of social as opposed to economic/environmental explanations that may seem equally viable in the absence of detailed information about socioeconomic context. Presently, there exist relatively few ethnographically contextualized isotopic data sets that could be used to frame interpretations of changes in precontact archaeological isotopic data (although some recent exceptions exist, particularly from health research; e.g., Hedges et al. 2009; Nash et al. 2012; Seabert et al. 2013; Wilkinson, Yai, and O'Brien 2007). In this context, Somerville and colleagues' study provides a strong example of how this kind of work can help to develop more sophisticated and nuanced anthropological models and theories for interpreting social and economic variables in archaeological dietary data.

It is also worth pointing out that there may be similar analogical potential in analyses of materials from historical archaeological sites and museum-archived ethnological and biological collections from the recent past. Historical contexts where archaeological or ethnological museum materials are well connected with detailed historical documentation can also provide a comparatively rich interpretive context for exploring connections between isotopic and other kinds of data, some of which are usually absent from archaeological sites dating to earlier time periods (e.g., Katzenberg et al. 2002; Klippel 2001). This kind of hypothesis testing is becoming more common in isotopic investigations of human and animal remains in historical archaeological contexts.

Although Somerville and colleagues' integrative approach provides a robust data set for their interpretations, gaps in certain areas limit some interpretive potential by preventing comparisons between key variables. For instance, interpretations could have been further advanced if demographic information (e.g., age, sex, profession) and oral health data were gathered more uniformly or if foods available in the area had been sampled for an isotopic baseline. It would also, for instance, be interesting to use isotopic analyses of hair samples from the foreign persons contemporaneously residing in the area as a comparative baseline for Westerner diets. In addition, the potential for tracking dietary change could have been significantly improved if the study had gathered data representative of multiple points in time in order to assess diachronic (i.e., longitudinal) change. Nonetheless, given the field conditions under which data were collected, these shortfalls are understandable. Moreover, these critiques are based partly on the benefit of hindsight, which would not have been available to the authors during the period of data collection almost 30 years ago, at which time the field of anthropological isotopic analyses was very new. Setting aside these minor critiques, the authors have done a marvelous job of assembling and interpreting an outstanding data set and situating their findings within broader theoretical and methodological debates in anthropology and archaeology.

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This analysis of culture contact and ensuing changes in food preference, diet, and oral health is a welcome addition to the literature. The depth of historic and ethnographic background provides a strong foundation for assessing the differential impact of missionary influence on villagers' food choices and changes in diet that have an effect on oral health status. Villages were classified as low, middle, and high contact based on proximity and degree of interaction with missionaries. Hypotheses regarding the impact of differences in missionary contact and changes in food preference, diet, and dental health are tested, though no dental health data were available for the high-contact village.

The introduction provides a useful summary of prior studies of interregional biocultural interaction between societies of different power structures and between archaic states and their peripheries. Notable by its absence from this review is the rich literature on hunter-gatherers, whose oscillating interaction with their sedentary agricultural neighbors, especially in South Asia, has relevance to the Dani of Mulia (Bird-David 1992*a*, 1992*b*; Gardiner 1985; Stiles 1993). The biocultural interaction between contemporary foragers and farmers of South Asia has direct implications for understanding intergroup trade in food that could have further illuminated Dani-missionary contact and has enhanced understanding group differences in oral health in prehistory (Lukacs 1990, 2002).

I found this study innovative in that it combines well-grounded ethnographic detail regarding traditional dietary patterns with systematically collected data on food preferences, dental status, and stable isotope analysis of hair. Though similar in some respects to Walker and Hewlett's (1990) study of behavior, subsistence, and oral health in Cental African foragers and farmers, this analysis of Dani-missionary contact has several shortcomings. Some limitations are acknowledged by the authors and compromise the analysis of changes in diet and dental health. For example, hair samples were mixed after collection, only bulk samples were analyzed for a subsample of study participants (~25%), dental health data were unavailable for the high-contact villager sample, and sample size was variable and possibly too small to detect significant variation in dental conditions. Some results were contrary to expectation; for example, greater access to imported food among high-contact villagers did not result in clear declines in oral health, and dental diseases (caries and missing teeth) were not more frequent in females than in males, as expected.

When considering food preferences, culturally mediated differences in taste are theoretically contextualized using Bourdieu's (1984) notion of taste, a perspective that views taste as culturally constrained by socioeconomic options that result in preference being given foods that are functional or practical. An important but overlooked variable influencing variation in taste is the genetic influence on taste, especially genes responsible for detecting sweetness and bitterness (Feeney et al. 2011; Meyers and Brewer 2008; Wendell et al. 2010). It is very possible that genetic variation is an important contributor to individual taste and food preferences, though including it in the analysis may have been beyond the scope of this study.

While dietary carbohydrate may have been a critical component in providing caloric energy for encephalization during the course of human evolution (Hardy et al. 2015), the traditional diet of the Dani is heavily dependent on carbohydrate-rich sweet potatoes. However, the Dani diet is well documented to be low in protein, and the primary staple of the diet is not considered as a contributing factor to variation in oral health. Studies of experimental animals (mice and rats) and humans suggest that the intake of a protein-depleted diet or low-quality dietary protein may result in a significant decrease in total immunoglobulin A (IgA) in saliva. One critical function of IgA antibodies in saliva is inhibiting attachment and adherence of oral bacterial to enamel surfaces, thus promoting resistance to dental caries. The high-carbohydrate, low-protein diet should result in a higher frequency of caries and missing teeth than was found in this study. Not all aspects of the influence of diet on cariogenesis and oral health generally were given due consideration in this analysis.

A life-history view of nutritional status was not considered though changes in oral ecology may occur with the aging process, a phenomenon known as allostasis (Zaura and ten Cate 2015). These changes in oral environment may have a direct effect on oral health and, especially, caries prevalence. For example, childhood malnutrition has been linked to increased caries later in life in Lima, Peru (Alvarez, 1995), and in the Caribbean (Psoter, Reid, and Katz 2005); experientially controlled fasting leads to reduced saliva flow rate, increased plaque formation, and reduced buffer capacity (Johansson, Ericson, and Steen 1984; Johansson, Lenanderlumikari, and Saellstrom 1994); and early childhood malnutrition causes salivary gland hypofunction that persists into adolescence (Psoter et al. 2008). If occasional periods of undernutrition were experienced by the Dani, this would have an impact on oral health during childhood and in adults.

The authors are commended for including the issue of sex differences in oral health in their analysis, and I agree that every study of oral health should be critically examined for all factors impacting sex differences in dental disease. This study found no significant differences in oral health by sex, though higher rates of missing teeth and dental caries in women were expected. The authors consider several reasons that could explain why sex differences were not found, yet because sex differences in caries prevalence involves a complexly intertwined set of etiological factors, not all factors are addressed. It has been shown that the frequency of eating and the types of food consumed are often different by sex in some groups (Berbesque, Marlowe, and Crittenden 2011). Additionally, women in some societies may engage in ritual fasting or practice specific dietary restrictions during pregnancy, known in India as "eating down," to ensure a small fetus and easy delivery (Lukacs 2011*a*, 2011*b*). Fertility and women's reproductive biology (menses, pregnancy) also contribute in part to variation in oral ecology and higher rates of dental disease in women (Lukacs 2008). The ubiquitous finding of poorer oral health in women than men is further supported by global evidence of a reversal of caries rates from childhood through adolescence. Males and females often have equal caries rates in childhood, or boys have higher rates than girls. During and after puberty, females nearly universally have higher caries prevalence than males, a pattern that is widely found around the world regardless of variation in diet, food processing, or preference (Lukacs, forthcoming).

Finally, I fully concur with the authors' admonition to bioarchaeologists that caution is essential in inferring sexual division of labor from dental data alone and that the full range of local cultural, environmental, and economic variables should be considered when explaining sex differences in oral health. This is especially important when the data do not agree with expectation, as in this study.

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The question of why people do what they do is a central theme in social science, and anthropologists take advantage of broad time depths and comparative perspectives to address it. Somerville and colleagues are addressing this question using middle-range theory (e.g., Binford 1977), applying measurements that typify (bio)archaeological studies of the past to the present, or more precisely, the Mulia Dani of Papua New Guinea undergoing a nutrition transition circa 1987, when data for this paper were collected. Bioarchaeologists need these kinds of checks to show us limitations of our data for studying the past and to attenuate those limitations by developing models linking data to interpretations (e.g., Bender et al. 2011; Floyd and Littleton 2006; Goodman, Martinez, and Chavez 1991; Lukacs and Largaespada 2006; Reitsema and McIlvaine 2014; Vercellotti et al. 2014; Walker and Hewlett 1990; Walker, Sugiyama, and Chacon 1998; Wood et al. 1992). In this comment, I restrict my focus to isotopic and food preference data.

Foods people eat from among available options (i.e., diet) reflect access, tradition, and choice, and because access, traditions, and the ability to choose are related to power (Mintz 1995; Wolf 1990), food and power are inextricably linked. When it comes to diet change and variation, a central anthropological question is, Is this a choice, or are people constrained or coerced into these differences? Somerville and colleagues tackle how and why diet changed following Christian missionization at Mulia starting in 1958. Oral health, stable isotope, and food preference data from villages near and distant from a mission settlement suggest that meat preference and consumption increase with increasing proximity to the mission airstrip. The contribution of this paper is not detection of dietary variation during a nutrition transition per se, but rather its theoretical engagement and incorporation of other data sets to explain, and not merely describe, diet change.

In the context of diet change, what comes first, the new food or the desire for the new food? Do people adjust according to changes external in the environment or actively drive change when pursuing new desired resources? Somerville and colleagues report that by 1987, meat was (1) a desirable food in areas where it was more accessible (food preference surveys) and (2) eaten where desirable and more accessible (stable isotope data from hair; although it should be noted that nitrogen isotope values of all villagers excepting those from Gallo's camp are no higher than those of vegetarians reported elsewhere; O'Connell and Hedges 1999; Petzke, Boeing, and Metges 2005). This in and of itself does not resolve the chicken-and-egg issue of which came first, meat or its desirability. However, theoretical engagement with Bordieu's concept of "foods of necessity" does. "Foods of necessity" supposes that "people without a wide range of food choices tend to express preference for foods to which they have the most access, whether due to class-related economic constraints or to logistical material constraints," rather than striving for inaccessible or nutritionally low-value foods. Interpreted with this heuristic, Mulia data suggest that the Dani did not seek out new foodways to accommodate their desires; rather, the foods people desired changed in light of what was available. Incorporating this guiding framework breaks down the binary of choice and constraint: people neither made meat available because they preferred it nor passively ate whatever was available. The Mulia Dani preferred and ate meat when it was more available.

One of this paper's premises is that people immersed in new ideas more readily will adopt new foodways—in this case, children and people whose occupations thrust them into contact with the mission. Bioarchaeological perspectives on childhood increasingly view subadults not as static phases nor as mere immature adults but as individuals with the capacity to act and think (Halcrow and Tayles 2011; Littleton 2011; Sofaer-Derevenski 2000), "human beings in their own right, albeit ones on small feet" (Lillehammer 2000:24). Viewed in this light, subadults are important units of social change, fusing tradition and innovation to become a blueprint for cultural transference and change in the next generation (Sofaer 2007). Somerville and colleagues present data suggesting that children more readily add meat to their dietary regimes. Although the authors conclude that children (6- to 17-year-olds) are one of the "segments of society most likely to exhibit dietary changes," figure 8 raises a question of where the age-related variation lies. Trend lines in figures 7 and 8 include 0- to 5-year-olds, whose isotopic values are higher because of breast-feeding. When only children and young adults are included (6- to 39-year-olds), R^2 drops to 0.006 for δ^{15}N, with no change discernible among children. Nevertheless, individuals aged 40+ years are significantly different from everyone else (compared to 6- to 39-year-olds; Kruskal-Wallis, δ^{13}N, $P = 0.003$; δ^{15}N, $P = 0.001$; compared to just 18- to 39-year-olds, δ^{13}N, $P = 0.008$; δ^{15}N, $P = 0.009$). Therefore, it is not children in 1987 who pioneered a new protein diet—it is the children of 1958 who did. People who were 11–29 years old in 1958 when the mission arrived retain signs of a traditional diet in 1987. The data suggest a threshold age in childhood beyond which people resist changing their meat consumption when faced with new constraints and opportunities, a lag time in age-related variation in diet after a nutrition transition, and a barrier between some parent-offspring generation(s) that traditional foodways did not cross.

The authors find that profession is not a predictor of diet change. To bioarchaeologists, this serves as another reminder that social or occupational differentiation can exist without dietary and skeletal stress correlates. But here is a weakness in bioarchaeology today: the tendency to prioritize, in study and reporting, cases of change and variation over cases of stasis and no variation. This tendency stems from our dependence on evolutionary theories (including life-history theory) to explain skeletal data. To better explain situations where variation and change in the human skeletal record are not detected by bioarchaeological means, applications of social theory will lead the way (Agarwal and Glencross 2011; Klaus and Tam 2009; Sofaer 2006; Somerville, Fauvelle, and Froehle 2013; Zuckerman, Kamnikar, and Mathena 2014).

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I work as a bioarchaeologist in contact-period contexts in the southeastern United States. Although much of this work has focused on microevolutionary processes, more recently I have become interested in a broader assessment of the biocultural changes that came with colonization. The paper by Somerville et al. gives bioarchaeologists much to consider, and here I focus on four points.

First, I note that some of the analytical categories and data types are familiar and readily available to bioarchaeologists, while others are not. For example, those working with skeletal remains can access information on age and sex and can record information on caries, ante mortem tooth loss, and isotopic variation. Other sources of data are not available (surveys on food preferences), while others still occupy a gray area, such as whether occupation could ever really be inferred based on skeletal indicators. Still, given how visible paleodietary research is within bioarchaeology, notions of taste and preference are an important consideration, especially given the dominant focus of biocultural explanations that may not account for such emic and idiosyncratic explanations. We cannot interview the dead, but doing so may explain some of the variation we see in our data sets. Somerville et al.'s paper provides a reminder that we are always seeing people only in part.

The second point relates to notions of community definition and the realities of how we construct analytical categories. Here we see one group of people (so named), and Somerville et al. define relevant subsets of this community based on age, sex, occupation, and physical location. Sex, a bioarchaeological "first order of business" provides little of use. Age, however, suggests an important dimension related to generational turnover and the concept of memory. It is the children (specifically the children's interaction among peers and with external institutions) that drive changes observable in the bioarchaeological record (diet). The archaeology of childhood—and, subsequently, the bioarchaeology of children—has certainly become more visible in the past decade (Thompson, Alfonso-Durruty, and Crandall 2014). Somerville et al. remind us why. Although the fragile bones of children may always be less accessible, targeting the formative years of life is possible through dental (and petrous) remains. Physical location also provided an important vector of variation for the Dani data set—proximity to centers of contact. Our ability to engage similar lines of evidence depends on archaeological site visibility, previous surveys and resources, to a large extent. Somerville et al. show us that even within a single population, dietary and health variation does exist, and this variation reflects both political and economic landscape and a specific community's positioning within it. The Dani are not monolithic, and Somerville et al. remind us that the archaeological populations we work with likely were not either. Furthermore, their paper shows us that analysis of health differentials and disparities within communities can be some of the most interesting aspects of our work. This point was made in *Current Anthropology* decades ago by Wood et al. (1992); the field has been slow to respond (but see DeWitte et al. 2016).

The third point relates to time. There are two issues to consider. First, as noted above, is the relationship between the rate of change and prevailing notions of taste and preference (or, more broadly, simply being and doing) that have a generational component to them. Even today, we see those younger than us as the makers of change (often in a begrudging manner; e.g., "those millennials"), unencumbered by tradition and "the way things should be." Any parent can attest to the challenges the peer group can create. Counteracting this, however, is just how quickly the changes documented by Somerville et al. were occurring. Those of us working in archaeological contexts are surely missing some of the nuance they are able to capture here. For example, in Spanish Florida, there is probably a 75-year gap between European contact and the first mission samples we have access to. Somerville et al. perform their analyses within a relative moment of Dani history, while archaeological work happens (usually) over much coarser periods of time. Seeing the variation across space and within a different generational set of the population should cause those of us working in the past to ask, Just what are we missing?

The fourth and final point relates to the period of time they are studying—the twentieth century. Despite the recent context, the actors and institutions—missions, missionaries, indigenous non-Europeans—are familiar to bioarchaeologists working in 500-year-old contexts. Somerville et al.'s paper reminds us that the bioarchaeology of colonialism is not just about a more distant (and quaint) history. Rather, these colonial encounters continued well into the twentieth century, with the same actors, institutions, responses, and effects still in place. In a postcolonialist sense, then, this should cause reflection in the bioarchaeological community about the implications of our work for contemporary stakeholders. The same types of analyses and analytical frames we use to study the past can also be applied to the near present. Doing so may help bridge the study of ancient and contemporary health disparities, but it also (again, in a postcolonialist sense) causes us to reflect on our part in the unfolding post-Columbian history that we all (in the globalized world) now live.

Reply

We thank the commentators for their close reading of the manuscript and for their constructive remarks. Due to the multimethodological nature of the research, we are grateful to have the comments and insights of these scholars with diverse backgrounds and specializations in dental anthropology, the archaeology of the region, the consequences of colonialism, and stable isotope analysis for dietary reconstruction. Although multiple important issues were raised in their comments, we will limit our reply to address four of the particularly critical points. These include the complexity of factors influencing oral health, the use of isotope analysis as a proxy for sweet potato introduction, the important role of children in driving social change, and the gaps in sample.

The comments by Lukacs focused primarily on our treatment of the patterns of oral health among the Dani. He noted that our findings of no significant differences in oral health between the sexes and of relatively lower-than-expected frequency of caries and missing teeth are in contrast with studies from other regions, especially low-protein, high-carbohydrate–consuming populations. While this is true, and while we agree that a greater discussion of the complexity of factors that influence these variables could have strengthened the paper, we will also note that some of Walker's previous studies among the Bantu horticulturalists of the Democratic Republic of the Congo (Walker and Hewlett 1990) and the Yora and Shiwiar hunter-horticulturalists of South America (Walker 1998) similarly found no significant sex differences in oral health. We agree with Lukacs that our study would be improved by more detailed examination of the frequency of food types consumed by sex, specific cultural practices of food consumption, reproductive biology, and age-related trends. Regrettably, however, our interpretive power was limited by the time elapse between data collection and analysis and our lack of knowledge of some of these factors. Nevertheless, future research projects involving the Dani's oral health data will attempt to untangle the complexity of factors influencing the observed trends. In particular, we will be producing a more detailed examination of the food preference surveys, which will be combined with the oral health data to explore in finer resolution the relationship between food preference, consumption, and oral health among the Mulia Dani.

Field notes important methodological considerations relevant to our suggestion that stable nitrogen isotope values ($\delta^{15}N$) in human tissues could be used to detect the antiquity of sweet potato farming in the region. She observes that sweet potatoes are not the only terrestrial plants with low $\delta^{15}N$ in Melanesia. It is true that determining sweet potato consumption from other low-$\delta^{15}N$ plants such as taro or sago palm would be extremely difficult. However, we contend that such studies could still have merit when other lines of archaeological data are included for context (as ethnographic and other records for the Dani at the time of data collection suggested). Sweet potatoes enable intensive farming practices upon which relatively large sedentary societies developed, particularly within the Dani region (Heider 1970, 1997). Other populations within Melanesia incorporate ^{15}N-depleted foods, but these do not seem to have the same degree of scalability as sweet potatoes, and sampled populations consuming these plants do not have values as low as the sweet potato–dominated subsistence strategy of the Dani (Ambrose et al. 1997; Kinaston et al. 2013; Umezaki et al. 2016; Yoshinaga et al. 1996). In theory, if a large and diachronic sample of human skeletal material was recovered and analyzed, an observed decrease in bone δ^{15}N values could be used as one line of evidence for the initial adoption of intensive sweet potato horticulture. Nevertheless, it is true that the resolution of δ^{15}N in human tissues would not be able to discern small amounts of sweet potato consumption from other low-δ^{15}N plants, particularly if they were not daily dietary items.

Field, Reitsema, and Stojanowski all remark on the significance of the data on children among the sampled Dani villages. Although often overlooked in anthropological and bioarchaeological studies, children can act as primary drivers of change within a community. The commentators note that a growing literature is supporting this notion (e.g., Bluebond-Langner and Korbin 2007; Lewis 2007; Perry 2005). Children fill an interesting space in their societies because, on the one hand, they are particularly vulnerable in terms of health, violence, and influence, but on the other hand, they are themselves active agents for social change. We suggest that the concept of resilience (Gunderson and Holling 2002; Holling 1973), which refers to external or internal perturbations and responses that maintain normal operating conditions (e.g., identity, health, disposition), can be helpful in conceptualizing the role of children in social change. Over time, adults in a society have developed means of enhancing their individual resilience, such as established traditions, relationships, and personal dispositions, but children lack the history and experience—and possibly even the motivations—to maintain preexisting ways of life. In this sense, the lower levels of resilience among children in fact enable them to act as the engines of social change.

Lukacs also notes that aspects of adult health, particularly oral health, may in part reflect events that happened in childhood and that a life-history perspective can add much to biocultural studies such as ours. This observation underscores the point that children are important subjects of anthropological research, not only for their role in driving social change but also because of the lasting consequences that childhood experiences can have on adulthood. While we unfortunately did not have longitudinal dental, ethnographic, or demographic data to address such issues in our research, future studies incorporating analysis of enamel hypoplasia could be useful to explore the consequences of childhood health on adult bodies.

The comments by Reitsema regarding children are particularly constructive. Responding to our stable isotope results, she notes that the most meaningful differences in diet are between individuals aged 18–39 and those aged 40+, with the latter exhibiting the values most suggestive of the precontact way of life. She concludes that it was actually the individuals who were children during the time of first European contact (the 18–39 age group of the study) who were the pioneers of new dietary traditions. Her reinterpretation of the data is very well received, and we appreciate this insightful observation, which remains in agreement with our overall interpretations of the data.

The research project began as a collaborative effort between one of us, Douglas Hayward, and Phillip Walker, who passed away in 2009. Upon Walker's death, we decided to aggregate the data, which had been on the academic back burner for many years. Indeed, as the commentators noted, the paper follows closely in the vein of Walker's previous studies on the biological consequences of culture contact (e.g., Walker 1998; Walker and Hewlett 1990). We agree with the commentators that our conclusions are limited by a relatively small and incomplete sample. Unfortunately, no food preference or oral health data were available for Gallo's Camp, which was the only high-contact village in Mulia. Also, hair samples were not collected in a manner amenable to intrahair diachronic sampling. As Guiry observes, some of the hair sampling issues were a factor of the era in which they were collected, a time before hair sampling and isotopic analysis reached the popularity and sophistication that they have today. Guiry also suggests that it would have been helpful to sample Westerners living in Mulia as a comparative baseline for the Western diet and that samples from multiple points in time would have been useful to assess longitudinal changes. To this, we further suggest that future projects involving hair take care to sample from the vertex region on the back of the head, as this area appears to have the most consistent hair growth patterns (Bost 1993) and to label the scalp and distal ends for transportation and storage. Biocultural studies involving hormones such as cortisol are becoming increasingly popular in anthropological research, and because these can also be analyzed in hair samples, it is important to collect samples in a manner that makes them usable for multiple lines of analysis. Despite some holes in the data set and the span of time since the original sample collection, we are pleased to have the data published to contribute to the legacy of Walker's work on studies of health and culture contact. We thank our colleagues for their insightful comments on the article and for the constructive discussions regarding the multimethodological data set.

—Andrew D. Somerville, Melanie A. Martin, Lee P. Hayes, Douglas Hayward, Phillip L. Walker, and Margaret J. Schoeninger

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