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It is a rare pleasure to have the opportunity to respond to such thoughtful, serious reviews. I shall reply to them in the order in which they appear.

Hunt's review provides the most detailed summary of *Before the Horror,* a summary with which I have only the most minimal points of contention. He incorrectly notes, for example, that my initial increase of King's population estimate to a range of 478,000 to 658,000 is founded on my acceptance of a higher Kealakekua house count and household-size estimate than that put forward by King. That is true only of the high end of the range; the lower figure of 478,000 accepts all of King's conditions and corrects only for the actual coastal mileage of the then-inhabited islands (p. 28). Similarly, on another small point, Hunt contends that my discussion of the relatively arid nature of Waimea on Kaua'i and Kealakekua on Hawai'i "oversimplifies" certain facts; what he fails to note is that his "correction" of this alleged oversimplification was anticipated and discussed by me in the book (p. 17).

These, however, are points of almost no significance to the overall search for the best estimate of Hawai'i's pre-1778 population size. Among the issues raised by Hunt that *are* of significance, both to my estimate and to future work on the question, is, first, his assertion that the "plausibility" of a thesis is insufficient for it to be accepted, and, second, his contention that the best future estimates will have to be founded on archaeological research and "the empirical nature of the archaeological record." Hunt, of course, is an archaeologist--as is Ramenofsky, who makes much the same point, and as is Patrick Kirch, who concludes his own review of *Before the Horror* on a similar note (Kirch 1990).

Taking the second of these matters first, I disagree with its implied disciplinary chauvinism. While archaeological work no doubt is essential to research in this field, it is far from sufficient. Analysis of historical material also is vitally important, not only for what its "empirical record" tells us of detailed human observations at the moment of Western impact and in the immediately succeeding decades, but also for its documentation of later nonarchaeological observations that can generate retrospective hypotheses. For example, although I mention several times in *Before the Horror* the likely impact of disease-induced infertility as a factor in the post-1778 depopulation of Hawaiʻi--the projected rate of depopulation itself, as Hunt acknowledges, being an important component in any estimate of the pre-1778 population--it is only in subsequent work in historical archives that I have been able to demonstrate empirically that infertility and subfecundity were the *key* elements in that depopulation. This has significant methodological implications not only for Hawaiʻi and Pacific population estimates but also for estimates of the pre-Columbian populations of the Americas (Stannard 1989). And archaeology was not a part of this research.

History, moreover, is but one of the additional disciplines that must be combined with archaeology if we are to continue to advance in this field. As Francis Black's review in this forum shows, no good estimate can be expected from scholarship that ignores research in epidemiology. In addition, comparative demographic analysis is crucial, as is work in a variety of other allied disciplines.

As for Hunt's (and Ramenofsky's) comment that "plausibility" is insufficient for a retrospective population estimate to be generally accepted by the scholarly community, I would of course agree--while disagreeing with the implication that my account is *only* plausible. On the contrary, it is the most plausible *and* the most empirically grounded study of the subject conducted to date. What needs to be recognized here is that research in this field requires the widest possible effort at data collection from a number of disciplines along with the application of both deduction and induction, and that it is on the basis of induction that our ultimate generalizations will have to be formed. Given the severely limited body of direct and unambiguous evidence with which we have to work, we need to be very careful to maximize our scrutiny of what philosopher of science Rudolph Carnap has called "total evidence" at the same time that we must be highly sensitive to the plausibility of our final argument. To ignore the plausibility criterion--Ramenofsky goes so far as to say that "plausibility is not the business of science"--is to place us in the position of someone discovering a large footprint in the snow of an Oregon forest and declaring that--in the absence of any other data--the footprint was just as likely to have been caused by Bigfoot as by a bear. A comparable implausibility, although one not commonly recognized, is the conventional idea that Hawai'i, with the most hierarchically structured polity in East Polynesia, could also have had (excepting New Zealand) the lowest population density in the region-- as it would have had with a population of half a million or less.

Ironically, moreover, it is precisely the ignoring of plausibility (along with consideration of only a severely restricted body of data) that has led to so many beautiful archaeological hypotheses dying in the grip of an ugly fact or a homely bit of logic. An example of this that is particularly germane to the present discussion is the notorious (in Hawai'i) *pre-*1778 population decline thesis advanced in the early 1980s by archaeologists Robert Hommon and Patrick Kirch (see Hommon 1980 and Kirch 1982). As I demonstrate in *Before the Horror* (pp. 66-69), this idea (which now appears in the guise of conventional wisdom in a number of textbooks--for example, Bellwood 1987:98) is shockingly at odds with a wealth of comparative data from such disciplines as history, physical anthropology, cultural anthropology, and nutritional science. As such, it is flagrantly implausible to the point of impossibility--and now, it appears, belatedly is being backed away from, even by its principal proponents (for example, Kirch 1985:288). An earlier concern for collateral evidence along with the plausibility criterion would have prevented it from entering the pages of the introductory texts that presently are misinforming a generation of students.

Indeed, even as I write, another narrowly constructed and blatantly implausible population growth theory for pre-1778 Hawai'i is making the archaeological and public rounds. Adopting the "cold fusion"' technique of announcing research results without accompanying detailed documentation, two archaeologists from Honolulu's Bishop Museum and Hawai'i Pacific College, Tom Dye and Eric Komori, have garnered local headlines with their reported discovery that the population of Hawai'i at the time of Western contact was only 100,000 to 150,000 (Borg 1989). Although a detailed critique of Dye and Komori's work is impossible since, even today--more than six months after their announcement of findings in mid-October of 1989--they have not produced the evidence underpinning their analysis, it appears that they have constructed a 1,500-year population growth model based entirely on a study of habitation-site radiocarbon dates on file at the Bishop Museum. The resulting 0.27 percent annual growth rate (based on their systematic analysis of these unsystematically gathered data) is so low that the founding population probably would not have survived the first years of settlement (cf. McArthur, Saunders, and Tweedie 1976:317-318), and if it did, they freely acknowledge (personal communication) it would have grown to an absurdly small size by 1778.

Using their estimated date of first settlement and conventional estimates for the size of that settlement, Dye and Komori's growth rate produces fewer than 6,000 Hawaiians at the time of Western contact--less than half the number that were counted swimming around Cook's ships at Kealakekua Bay alone, and about the same number of people per square mile as currently inhabit the vast, frozen tundra of Alaska. Of course, one way to make such a wildly low growth rate result in a more realistic end-point population size is to *begin* with a large population. So, Dye and Komori have invented out of thin air, with not one piece of supporting evidence, a massive, indeed invasion-like Polynesian settlement of Hawai'i, with continued back-and-forth sailing--in waters that effectively were unnavigable between Hawai'i and the Marquesas (Finney 1967: 155-161)--during the earliest centuries of colonization. Even then, their resulting 1778 population estimate is as low or lower than it is known to have been half a century later--which is, in light of all the historical evidence of a massive post-1778 population collapse, simply impossible.

Clearly, there are major problems with Dye and Komori's habitation-site data and with their applied methodology, as is evident in part from the utter implausibility of their conclusions. Rather than facing these facts, however, they have preferred to invent auxiliary hypotheses that have no empirical or logical underpinning in a futile effort to shore up the ramshackle edifice that is collapsing all around them. Unfortunately, this is a procedure that is all too typical.

In sum, while I share Hunt and Ramenofsky's advice that plausibility is an *insufficient* criterion in judging a thesis, I would submit that it is a *necessary* criterion--and that their own discipline provides good evidence to that effect, littered as it is with the carcasses of once--bright ideas that ignored the plausibility question. My point here is not to single out archaeology for criticism (other disciplines have similar problems and archaeological research certainly is essential to progress in the field under discussion), but I raise the issue merely to illustrate the need for an interdisciplinary thickening of analyses as we proceed into the future with this very difficult but very important subject.

Finally, regarding Hunt's review, he makes the noteworthy point of introducing Liebeg's Law of the Minimum: that population size is constrained by the lowest, not the average, availability of critical resources. This will indeed have significance for estimating the pre-1778 population of Hawai'i when and if we ever have complete, detailed, and credible data on the islands' minimum and average levels of pre-1778 resource availability. Such data will not be available in the near future, however, because (as I point out in *Before the Horror,* p. 38) the necessary compilation and analysis will require a thorough survey of climates; soils; topography; types of agriculture, aquaculture, and fishing; caloric requirements of the population--and more. As of now we do nut even have an adequate survey of the amount of land that was being cultivated prior to 1778, so I was limited in my carrying-capacity discussion to population-density comparisons of hypothesized populations in Hawai'i with known densities in other, less productive and less intensely cultivated, environments. Thus, while Hunt's point here is relevant to analyses that may take place in the distant future, it has no material bearing on the present discussion. His remark that the existence of Liebeg's law "perhaps confounds" my argument is, therefore, simply incorrect.

In addition to raising Hunt's "plausible" versus "empirical" dichotomy (which, in this case, I obviously think is a bit of a red herring), Ramenofsky chides me in her review for what she believes is my "tautological reasoning." Unless I am missing something, her support for this allegation is threefold: first, she claims that I began my project with a "bias" in favor of a conclusion that the pre-1778 population was higher than previously believed; second, my several lines of varying methodological inquiry supposedly converge in agreement with my original "bias"; and third, those several lines of inquiry are each said to contain certain assumptions for which there is little empirical support. We need to take these one at a time.

First, let me confess that I did indeed begin this endeavor with a hypothesis, not a bias--rather, a then-unverified suspicion--that the pre-1778 population of Hawai'i was higher than what the conventional wisdom claimed. I would have had to have been an ignoramus in this general field of inquiry to have begun with any other hypothesis, since there is hardly a case on record in which a modern analysis of an indigenous people's population magnitude at the time of Western contact did *not* conclude that it was higher than previously believed. Indeed, Ramenofsky's own superb research lends powerful support to the idea that such conclusions are the rule of contemporary work in this area (Ramenofsky 1987). For that matter, her own words in the present review nicely distill the matter: "Although anthropologists readily admit that native peoples died from introduced disease, they either underestimate the magnitude of the decline or they assume that the disaster postdated initial documentation and settlement." This does not mean that higher population estimates are an absolute and invariable rule but merely that their dominant pattern provides the ground for the best-supported initial hypothesis. Certainly there is nothing "tautological" in this procedure.

Perhaps, then, it is the claimed convergence of my different lines of inquiry--the historical record, the potential carrying capacity of the islands, the likely population growth and decline rates, and so on--that troubles Ramenofsky. What makes for tautology, however, is not whether independent lines of inquiry converge in mutual support of a hypothesis (that is called confirmation), but whether those lines of inquiry are logically tainted by a preliminary design and framing of the inquiries that guarantees in advance ultimate confirmation among them.

Is this what I did in *Before the Horror?* Clearly the answer is no. In the first place, every independent line of inquiry was pursued--independently--to its most conservative possible conclusion, as Black in part confirms in his review, and as Ramenofsky at one point acknowledges in correctly noting that the pre-1778 population growth model I used was "the worst case scenario developed by McArthur, Saunders, and Tweedie (1976) in a simulation study of Pacific island peopling and growth." In the second place, the conclusions of these separate lines of inquiry do not converge: they point to a range of population from about 800,000 to about 1,500,000--from which I selected the most conservative overall number to put forward as the most likely and most supportable estimate. (Not surprisingly, some anthropologists and historians specializing in demographic reconstruction have since written to me to say they find my conclusions too conservative.)

Finally, there is the fact that all these independent lines of inquiry contain some elements that are empirically unverified or unverifiable, such as the specific pre-1778 population growth scenario--which, of course, can never be known with precision. Surely, though, Ramenofsky must realize that it is routine in many areas of science (although I do not consider this work, or most of archaeology for that matter, to be science) to conduct analyses of empirically undetectable phenomena. Quarks, gluons, and other elements of quantum chromodynamic theory, for example, are unobservable themselves, but--like all subatomic particles--their existence acquires empirical significance from the fact that they, along with other subsidiary assumptions, can *generate* empirically verifiable theories.

My connecting this realm of science with the problem presently under discussion is only metaphorical, to be sure, but the point is that it is perfectly respectable--and hardly tautological--to construct separate, though mutually relevant, lines of inquiry, each of which is founded on a combination of empirical data and both deductive and inductive logic. Indeed, it is a thoroughly unoriginal truism to observe that the combination of deduction and induction is the heart of the scientific method. Without deduction and induction--because the empirical data on such matters as the pre-1778 population growth rate in Hawaiʻi are so thin--we would have to stand foolishly mute on a subject about which much other evidence, of various sorts, abounds.

Where, then, is the tautology? Could it be buried somewhere in my challenge to prospective critics, if they wish to be taken seriously, "to demonstrate-- *in specific scholarly detail* " (pp. 80, 142) precisely how their final estimate is superior to mine? I cannot imagine that it is located here, since that makes no sense at all, although clearly this too seems to trouble Ramenofsky. Certainly, however, as someone who evidently is concerned with the correctness of scientific procedure, she recognizes here a simple assertion of the scientific axiom that it requires a superior theory--not merely potshots at subsidiary portions of an existing theory--to overturn a fully advanced theoretical argument. (Not incidentally, I must add that I inserted this remark in the book only because of my awareness of the abysmally poor quality of previous population estimates for pre-1778 Hawaiʻi--the same estimates on which archaeologists and other scholars, as well as the general public, have routinely relied--and in anticipation of the sort of intellectually anarchic critique that can be expected from certain of those quarters.)

Well, I give up. I just cannot locate in Ramenofsky's review or in my work evidence supportive of her general points of criticism. And it is impossible to discuss in detail a general critique that contains no substantive core. That then takes us to her narrower and more specific points of focus.

1. Like Hunt, Ramenofsky misstates my handling of the differing house counts at Kealakekua Bay in 1779, although, as noted earlier with Hunt, this is a trivial matter either way.

2. Ramenofsky makes much of my supposed "employment" of Henry Dobyns's "principle of military parity" in discussing my findings regarding differential population densities in Hawai'i's leeward and windward political districts, concluding that Dobyns's principle remains untested, and thereby presumably undermining my argument at that point. The reader of these reviews, who has not also read the book, cannot tell, however, that I do not "employ" Dobyns's thesis, but rather *mention* it in passing in a single phrase that is one part of a single sentence (p. 22). Although I think Dobyns has a point of some interest here, it is a point that is thoroughly incidental to my overall argument regarding the matter in question.

3. On this same question of population densities varying from district to district, Ramenofsky asks: "If boundaries between leeward and windward districts were established after the decimation that Stannard describes, do they pertain to 1778?" Good question--or at least it would be a good question if it were not known to every archaeologist, anthropologist, and historian working on Hawaiian materials that the districts under discussion were *not* established after 1778, but that they long predated Western contact.

4. In *Before the Horror* I argue that King's guess that a quarter of all Hawai'i's island coastlines were uninhabited was a gross (though understandable) exaggeration (pp. 23-25). In fact, it appears that almost no coastal locales were uninhabited--that, according to both archaeological and historical data, even the most inhospitable coastline areas contained village populations. Still, to be conservative, I built into my estimate an assumption that perhaps 10 percent of the coastlines were uninhabited, thereby leaving about 90 percent inhabited. Ramenofsky's comment is: "Why not 87 percent or 95 percent?" Why not indeed? With a modest estimated inland population of 10 percent of the total (a figure that Hunt, for instance, agrees is conservative), an 87 percent instead of 90 percent level of coastal habitation would reduce my overall archipelagic population estimate by about 2.9 percent--an insignificant amount in the context of the round numbers we are forced to work with because of the absence of detailed data. Moreover, to repeat, it is probable that more, rather than less, than 90 percent of the coastal areas were inhabited. In sum, since precision is clearly impossible on matters of this sort, Ramenofsky's rejoinder here is no more than a quibble.

I do not wish to appear unduly harsh in this reply, either to Hunt's or to Ramenofsky's critique, particularly since I am an admirer of their work and I appreciate their generally favorable comments on my study.

On the other hand, I believe it is quite apparent, under scrutiny, that those specific points of theirs that I have discussed in the preceding pages are either ill-considered or off the mark. Moreover, when all the dust has settled on such matters as the structure of question framing and empiricism versus plausibility, it will be instructive to notice that neither Runt nor Ramenofsky challenges my population estimate in any *substantive* way.

This then takes us to Black's review. Interestingly, approaching the problem "from multiple directions"-- precisely what Ramenofsky seems to have identified as "tautological reasoning" on my part--Black sees as "several lines of reasoning converg[ing] to form a surprisingly strong impetus for revision" in that "the overall probability is the product, not the sum, of the parts." I couldn't have said it better myself.

While there is almost nothing in Black's review with which I flatly disagree--including his remark that my description of the Cayapo example is over-simplistic; it was, after all, only a single-sentence reference in a straightforward list of two dozen examples of introduced disease disasters--there is a great deal in his contribution that deserves discussion. His comment that influenza could not have been introduced to Hawai'i by Cook's crews because of its relatively short cycle requires particular attention.

First, we must begin with the fact that Cook's ships apparently introduced *some* new respiratory disease to Hawai'i that, in the words of assistant surgeon William Ellis, caused a general outbreak of "coughs and colds" and at least some death from "a violent griping or colic" (Ellis 1782: 151). Since, as I note in the book (pp. 77-78), there is no good evidence of tuberculosis existing in Hawai'i prior to Western contact--and since Cook's ships, like most of England at the time, were infested with the disease--my primary assessment was that the symptoms described by Ellis were the beginnings of a tuberculosis outbreak. The British did not remain in Hawai'i long enough to witness the major consequences of the diseases they had carried to the islands, but tuberculosis was certainly one of them, and it has long been known to cause raging epidemics with 50 to 60 percent mortality rates in virgin soil populations (see, for example, Dubos 1965:173; Cook 1973:500). The question, then, was not whether tuberculosis was loosed upon the Hawaiians in 1778 and 1779--clearly it was, and with catastrophic effect --but whether *other* respiratory infections also were introduced.

In addition to tuberculosis, another possible culprit that I did not mention but that Black does is diphtheria, which caused major loss of life among American Indians on a number of occasions in the seventeenth and eighteenth centuries (Dobyns 1983: 19-20) and that William McNeill classes with influenza, smallpox, measles, and bubonic plague in its shocking demographic impact in past centuries (McNeill 1976: 145). Dengue is still another possibility that might occur to some because its initial recorded appearance in several locations throughout the world coincided chronologically with Cook's arrival in Hawai'i; however, dengue is transmitted by specific mosquito vectors that would have to have been present in the ships' water supplies, and mosquitoes of any type are not recorded as being present in Hawai'i until the 1820s (Culliney 1988:271-272). So, in the absence of new evidence, dengue would have to be ruled out.

That leaves influenza. Clearly, conventional theory regarding the spread of the influenza virus as a person-to-person transferral via the respiratory route, with a twenty-four- to seventy-two-hour incubation period, supports Black's conclusion that it could not have been carried to Hawai'i by Cook's ships. However, conventional theory has a very hard time explaining certain anomalies that have existed in the medical literature for over a century. Among these are isolated outbreaks of influenza, particularly among ships that have been at sea for extended periods. August Hirsch, in his classic *Handbook of Geographical and Historical Pathology,* described the phenomenon.

> Among isolated outbreaks of the disease, the often observed epidemics on board ship are especially interesting. In several cases of the kind [enumerated by Hirsch, but excised here in the interests of space] the crews were attacked, and that too just as suddenly and without warning as when influenza appears on land, while the ships were lying in port or cruising off the coast, no trace of the disease having shown itself either before or after in the same region ashore. . . . In other and still more interesting cases, the disease has appeared, at a time when it was generally prevalent on land, among the crews of ships on the high seas *which had not previously communicated with an infected shore;* and those outbreaks befell at the same time as the outbreaks of influenza on the coasts nearest to the position of the ships. (Hirsch 1883:19-20; emphasis added)

There are various possible explanations for these occurrences. They range from the idea that the sailors (and those in other isolated areas) may have been "pre-seeded" with a low-level and perhaps unnoticed virus at an earlier date that burst forth when triggered by a weather change (Pyle and Patterson 1984: 182-83) to the notion that the disease was carried by animals on board the ships (Guerra 1985, 1988). Neither of these seems likely in the Hawai'i case, but a third hypothesis does at least deserve scrutiny. As I note in *Before the Horror* (pp. 74-75), recent research by highly regarded British and Soviet epidemiologists suggests that most cases of influenza are spread by symptomless individuals who contract influenza a year or more earlier and become carriers of the disease; an unknown stimulus, probably associated with climate change during the so-called flu seasons, causes the virus to emerge from the carriers and spread to those in contact with them (Hope-Simpson and Golubev 1987).

Although at this time the Hope-Simpson/Golubev thesis remains a distinctly minority view, the research is continuing and bears watching if only because of an intriguing--albeit possibly coincidental--combination of facts: first, a flu epidemic was in progress in England during the months immediately preceding Cook's departure on the voyage that would take him to Hawai'i (Creighton 1894: 359-361); and second, Cook's ships arrived in Hawai'i and deposited some serious respiratory illness or illnesses, in both 1778 and 1779, during what would come to be Hawai'i's flu season--thus, at just the time when contagion among his ships' crew members would have been active.

Space does not permit full discussion here of several other important matters raised by Black, including the psychological impact of mass death, with its consequent undermining of what is colloquially called the will to live. Black cites J. V. Neel and others on this phenomenon among twentieth-century Yanamama, but very similar descriptions exist regarding early nineteenth-century Hawaiians: as one traveler observed of Hawai'i's native people in 1837, "When they get ill, they immediately give themselves up, and in those cases seldom recovered" (Hinds 1968: 123). Further, Black's comment on the literal homogeneity of the Hawaiian population at the time of Western contact and during most of the era of the great population collapse, with its likely effect on societywide susceptibility to introduced disease, is extremely important and requires much more detailed exploration. On the one hand, like many long-isolated indigenous peoples Hawaiians do show evidence of homogeneity in their limited range of blood-types (Morton et al. 1967: 24-34; Mourant 1983:105-107). On the other hand, the very existence of genetic bottlenecks remains a highly controversial subject on several levels, particularly when founding populations number in the scores or hundreds, as was likely the case with the first successful Polynesian settlers in Hawai'i.

On the above two issues, moreover, I would offer some words of caution: we need to tread very carefully here to avoid the appearance of blaming the victim. In his brief discussion of the genetic question, Black quite prudently and rightly stresses that homogeneity does not suggest inferiority--any more, I would add, than the unusual genetic susceptibility of East European Jews to Tay-Sachs disease, or blacks to sickle-cell anemia, or melanin-deficient Caucasians to skin cancer (among many examples) suggests inferiority among those groups. Removed from their scientific context and placed in the popular realm, however, such subjects potentially lend themselves to racist exploitation.

To summarize my response to Black's review: first, I agree with almost all of it, largely, I confess, because he is in every particular supportive of my overall population estimate; and second, I appreciate it because it goes beyond critical discussion of historical demography and initiates important lines of inquiry relevant to a larger medical history of the Hawaiian people and other peoples of the Pacific. More than a review, then, it is itself a positive contribution to a rich, complex, and--to Hawaiians and to those concerned with their ongoing historical experience--essential research project that has only recently begun.

The review by Cruz and English raises a sensitive issue beyond its contents, an issue that needs to be addressed directly. Both Cruz and English are native Hawaiians, and both are also students at the University of Hawai'i; the other three reviewers are non-Hawaiian, and all of them are highly accomplished scholars in their various professional fields, Two unfortunate impressions are created by this situation: first, that there are no native Hawaiian scholars with sufficient professional credentials or competence to review *Before the Horror;* and second, that Cruz and English were selected by the editor of this forum in a gesture of ethnic tokenism, I can assure readers that the first of these possible impressions is wrong: there are any number of highly qualified Hawaiians in both history and medicine (some of whom provided crucial information and advice to me in the writing of the book) who could have been called upon to produce a critical review of the work. If it was the desire of the editor that a Hawaiian voice should be heard in this forum, why were none of the available professionals asked to provide it? As to the issue of tokenism, the forum editor assures me that such was not his intent. But what *would* you call it if, to borrow a parallel example, a book review editor was to select as five forum reviewers for a book on, say, slavery in America, three accomplished white scholars and two black graduate students, ignoring in the process a host of accomplished black scholars? Come to think of it, tokenism may be the wrong word after all.

In any case, although Cruz and English do not address any of the technical aspects of *Before the Horror* in their review, their comments do connect with the book's closing words on the larger political context of discussion on this subject. Some of their observations, however, seem obvious or problematic or undeveloped. For example, they say: "For Hawaiians, history is not simply a fact of the past but an ongoing process in the present--a point Borofsky (1987) emphasizes is true for other Polynesian islands as well." They (and Borofsky) might better have replaced "other Polynesian islands" with "all humanity." As John Dewey observed more than fifty years ago: "History cannot escape its own process. It will, therefore, always be rewritten. As the new present arises, the past is the past of a different present" (Dewey 1938:239). Dewey, of course, largely was repackaging here Benedetto Croce's famous dictum, written two decades earlier, that all history is contemporary history. And the notion was hardly original with Croce.

Later, Cruz and English ask rhetorically: "Can Hawaiian history written by non-Hawaiians ever be a completely 'true' reflection of the Hawaiian past, when it has no relevancy to present-day Hawaiians?" As with all rhetorical interrogatories, the authors seem to think the answer obvious, so they pursue it no further. But while the question undeniably is important (and its relativism quite distinct from the matter of temporal perspective mentioned in the preceding paragraph), it is far from answered merely by the asking. In fact, although apparently unrecognized by Cruz and English, their singular rhetorical query here contains several complex and difficult epistemological issues. The meaning of the word "true" with regard to history, for instance, is not apparent on its face; and the privileged cultural perspective of the native historian may well be a reality, at least in certain cases, but it is not a self-evident reality that can be blithely assumed. Neither is the notion unchallengeable that the only "true" histories (whatever that may mean) are "relevant" (another ambiguous word) to the current concerns of the people being studied. Nor, still further, is it necessarily true, as these reviewers imply, that "Hawaiian history written by non-Hawaiians . . . has no relevancy to present-day Hawaiians." The fact that such histories may sometimes be dumb or hateful or racist does not, as a matter of course, make them "irrelevant" to contemporary Hawaiians; on the contrary, unfortunately, dumb and hateful and racist behavior by non-Hawaiians that is directed at Hawaiians is all *too* relevant a reality today, both in and out of the world of scholarship. In sum, there are intricate and formidable scholarly concerns (including the matter of whether relevancy is relevant} beneath what Cruz and English in their review have reduced to a slogan, concerns that need to be analyzed and argued, not merely asserted.

Finally, after gratuitously saying that my "motives" for writing *Before the Horror* may be open to "question" (a statement that has no mooring in anything else they say, and thus is incomprehensible as to either intent or meaning), Cruz and English express excitement that works such as mine "free" Hawaiians "in the Western sense" (1) to "choose" their history. I certainly hope my book has no such effect, which is why I so insistently asserted in its pages (to the annoyance of Ramenofsky and no doubt others) that those holding a contrary view of this subject have an obligation "to demonstrate-- *in specific scholarly detail* " the supporting evidence *for* their contrary view. Of course, in a trivial sense anyone is free to "choose" to believe anything; but as a responsible participant in the world of scholarship, one is *not* free to decide, to take an exaggerated example, that creatures from another galaxy built the pyramids of Egypt--just because someone may have written a book (even one with footnotes) arguing that such was the case. Indeed, it was disagreement over much more serious epistemological and evidentiary matters that was the central issue in my exchange earlier with Hunt and especially Ramenofsky. Whatever level of resolution ultimately results from such encounters, it will be founded, as in those exchanges, on disciplined debate and careful judgment, not on "freedom of choice."

As graduate students during a time when it has become fashionable for scholars in some disciplines, especially anthropology, to delight in exercises about people "inventing" their histories, Cruz and English appear to have fallen victim to the most superficial understanding of this form of analysis. Moreover, they end up parading themselves as examples of the most insidious rendition of such "invention"--the rendition that trivializes native views of the past as unsubstantiated and "freely chosen" fictions that eventually are unmasked by omniscient Western scholars (for example, Linnekin 1983; Keesing 1989). This version of what was once, in an earlier and philosophically more serious form, known as "constructionist" history (Meiland 1965) is, in a literal sense, a perversion--a turning to error--of the equally serious "invention of culture" idea advanced by Roy Wagner (1975), and applied by others with equivalent emphasis to Western cultural traditions (for example, Hobsbawm and Ranger 1983). Hawaiians like Cruz and

English will not liberate themselves from the clutches of neocolonial historiography simply by declaring themselves "free to choose." Such a statement merely supports the allegations of those who claim that native views of the past are pipe dreams.

There are other problems with this review, such as the authors' inversion of causality in their assertion that the "Spanish imposition of alien structures on Andean society . . . led to its disintegration," when it is well known--and even cited in *Before the Horror* (p. 46)--that Andean society lost about 93 percent of its population within the century following Western contact. Thus, the "imposition of alien structures" was possible only long *after* that population collapse had been set in motion.

These matters aside, Cruz and English do at least have a keen if undisciplined sense that much that passes for history in many parts of the globe, but particularly in areas that still feel the weight of a colonial past and perhaps a neocolonial present, is little more than political myth. And, as I contend in a piece that Cruz and English cite in their bibliography but not in their text, the minimizing of precolonial indigenous population size in locales that have fallen under outside domination is almost always the first building block in the construction of the colonizers' self-justifying political mythology (Stannard 1988). As one noted historian has put it, low population estimates in such circumstances often serve as historical "rationalization for the invasion and conquest of unoffending peoples" by acting to "smother retroactive moral scruples" that might otherwise emerge (Jennings 1976:15).

Once again, then, it is worth reminding ourselves of the highly charged political atmosphere that surrounds this subject. Nearly twenty-five years ago, when many anthropologists and historians still believed that the pre-Columbian population of the entire western hemisphere totaled less than 9 million persons, Henry F. Dobyns began his famous reassessment of that estimate--a reassessment that concluded with a new population estimate of between 90 and 112 million--by noting that "the idea that social scientists hold of the size of the aboriginal population of the Americas directly affects their interpretation of New World civilizations and cultures" (1966:395). A decade later, Francis Jennings observed that the reverse of Dobyns's comment was equally true.

> The idea that scholars hold of New World cultures directly affects their interpretation of the size of aboriginal populations. Proponents of the concept of savagery stipulate, among other things, that large populations are impossible in savage societies.

> It follows that if aboriginal populations can be shown to have been large, they could not have been savage. A logical approach may thus be made into the whole question of the nature of aboriginal society and culture through the gate of numbers. (Jennings 1976:16)

Today, no informed scholars any longer believe that the population of the Americas in 1492 was less than 9 million. Debate continues as to the best possible estimate, but it is debate largely between those who now suggest a figure well above 112 million (Dobyns 1983) and those who put the number at between 50 and 75 million (Denevan 1976:289-292; Thornton 1987:25). In short, the range of debate is between estimates that are six to sixteen times the conventional estimate of only twenty-five years ago. By comparison, my estimate for Hawai'i's pre-1778 population is only two to three times what has long been the popular belief. Future scholars, I suspect, will find my estimate to be conservative, but not before a good deal of academic blood has been spilled in politically-motivated efforts to preserve the conventional wisdom. It is thus a credit to this journal and to its selected reviewers that this lengthy exchange has been conducted at so serious and thoughtful a level and without descent into the world of diatribe that so often characterizes discussion on this subject.

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