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Comments and Reply]

Author(s): C. Loring Brace, Robert J. Hinton, Tasman Brown, R. C. Green, Edward F.  
Harris, Alex Jacobson, Christopher Meiklejohn, Yuji Mizoguchi, Shao Xiang-Qing, Patricia  
Smith, Richard J. Smith, J. Specht, John Terrell and J. Peter White

Source: *Current Anthropology*, Vol. 22, No. 5 (Oct., 1981), pp. 549-569

Published by: The University of Chicago Press on behalf of Wenner-Gren Foundation for  
Anthropological Research

Stable URL: <http://www.jstor.org/stable/2742288>

Accessed: 20-02-2018 23:10 UTC

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# Oceanic Tooth-Size Variation as a Reflection of Biological and Cultural Mixing<sup>1</sup>

by C. Loring Brace and Robert J. Hinton

The Pacific sea . . . encouraged maritime enterprise. Instead of being like the forest, marshes and mountains of a continent or great island, a barrier to communication, it was a highway which favoured intercourse and migration. [Crawford 1852: ccliv]

A light-colored people of unitary language colonized the island world of the Pacific Ocean many generations ago. Where they encountered dark-colored people, they influenced their culture and language, and they also took over much from their culture and language. [Dempwolff 1937: 193, translation ours]

ONCE UPON A TIME, the student of human biological form assumed that an understanding of "racial" variation could best be

<sup>1</sup> The work on which this paper is based was accomplished in spite of denial of support by the National Science Foundation. Partial funding was made available by the Horace H. Rackham School of

C. LORING BRACE is Professor of Anthropology at the University of Michigan and Curator of Physical Anthropology at that university's Museum of Anthropology (Ann Arbor, Mich. 48109, U.S.A.). Born in 1930, he was educated at Williams College (B.A., 1952) and at Harvard University (M.A., 1958; Ph.D., 1962). He has taught at the University of Wisconsin-Milwaukee (1960-61), the University of California, Santa Barbara (1961-67), and the University of Auckland (1973). His research interests are human evolution, dental anthropology, "race," and the history of biological anthropology. His publications include "Structural Reduction in Evolution" (*American Naturalist* 97: 39-49); "The Fate of the 'Classic' Neanderthals: A Consideration of Hominid Catastrophism" (*CA* 5: 3-43); "Environment, Tooth Form, and Size in the Pleistocene" (*Journal of Dental Research* 46: 809-16); with Ashley Montagu, *Human Evolution* (2d edition, New York: Macmillan, 1977); with Harry Nelson, Noel Korn, and Mary L. Brace, *Atlas of Human Evolution* (2d edition, New York: Holt, Rinehart and Winston, 1979); and "Australian Tooth-Size Clines and the Death of a Stereotype" (*CA* 21: 141-64).

ROBERT J. HINTON is an NIDR Postdoctoral Fellow of the Center for Human Growth and Development and the Department of Anatomy of the University of Michigan. He was born in 1946 and educated at the University of Tennessee (B.S., 1968), and the University of Michigan (M.A., 1975; Ph.D., 1979). He was a postdoctoral scholar and lecturer in the Department of Anthropology at the University of Tennessee in 1979-80. His research interest is the functional morphology of the human masticatory apparatus. Among his publications are, with D. S. Carlson, "Temporal Changes in Human Temporomandibular Joint Size and Shape" (*American Journal of Physical Anthropology* 50: 325-34); with M. O. Smith and F. H. Smith, "Tooth Size Changes in Prehistoric Tennessee Indians" (*Human Biology* 52: 229-45); "Form and Function in the Temporomandibular Joint," in *Craniofacial Biology*, edited by D. S. Carlson (Center for Human Growth and Development Craniofacial Growth Series Monograph 10); and "Changes in Articular Eminence Morphology with Dental Function" and "Form and Patterning of Anterior Tooth Wear among Aboriginal Human Groups" (*American Journal of Physical Anthropology* 54: 439-55, 555-64).

The present paper was submitted in final form 30 v 80.

gained by positing the existence of a limited number of original "stocks," or *souches*, to use Broca's (1860:605) term. Some of these, obedient to the push of some ill-defined inner urge, moved with single-minded determination over vast distances before enigmatically coming to rest where they are currently to be found. Inevitably during these protracted and heroic migrations, the isolation which was assumed to be a condition of the original existence of the various "stocks" was disrupted. Interracial contact, with varying degrees of mixing and amalgamating, was the consequence, and the unsatisfyingly imprecise boundaries and gradations visible in the world today were the result (Dixon 1923, Hooton 1931).

The addition of data and techniques from the realm of genetics, initially offered in the name of modern science, raised hopes that the impasse encountered by the biometric anthropologists could be transcended and the spectrum of human variation could be objectively classified into a convenient set of categories (Boyd 1940). More than a generation has elapsed since that hope was articulated, and in spite of enormous efforts expended in the acquisition of gene-frequency data on a plethora of populations (e.g., from Boyd 1950 to Mourant, Kopec, and Domaniewska-Sobczak 1976) the millennium is just as elusive as ever.

The problem lies in the fact that the expectations that have been guiding the work of many human geneticists are exactly the same as those of old-fashioned physical anthropologists going right back to the out-and-out polygenists of a century ago (cf. Broca 1860, Pouchet 1864, Topinard 1879). The present unsatisfactory picture has been assumed to be the result of recent mixtures blurring the distinctions that existed during initial isolation (Boyd 1940: 352, 361). The solution advocated has been the study of objectively measured traits under the control of one or a small number of genes. If this differs from the old anthropometry in focus, it resembles it in the insistence

Graduate Studies at the University of Michigan; by the Department of Anthropology, University of Auckland, Auckland, New Zealand; by the generosity of Prof. and Mrs. Gerald W. Brace; and through loans provided by the American Express Company, the National Bank of Detroit, and the Ann Arbor Bank.

Permission and help in examining collections was provided by S. T. Brooks, Thomas M. Brown, Lucas Chin, James P. Garlick, Roger Hardley, Jean-Louis Heim, Philip Houghton, Teuku Jacob, the late Stanley Larnach, F. P. Lisowski, the late N. W. G. Macintosh, Roy Maeda, David R. Moore, Stewart Parks, Graeme Pretty, Michael Quinnell, George J. Romanes, Sood Sangvichien, Yoshiaki Sinoto, James R. Specht, Peter Stanbury, Robert Stone, W-H. Sung, Alan West, H-M. Yang, and Douglas Yen.

We are grateful to Jack Golson and Karl L. Hutterer for their extensive comments on and criticisms of earlier versions of this paper. The errors that remain, however, are entirely our own.

that the traits be stable (which in this instance means low mutation rates), relatively uninfluenced by the environment, and, most telling of all, "nonadaptive." Given these strictures, especially the last, it should be obvious why these studies have produced so little insight into the course of human evolution. Furthermore, geneticists, lacking familiarity with the relevant data from archaeology, linguistics, and cultural adaptation and history, have rushed off to study primitive isolates that turn out to be neither isolated nor "primitive" (e.g., Neel 1970).

The Pacific has proven to be a particularly attractive arena for the pursuit of such studies (e.g., Morton 1973), since isolation is so visibly obvious—finite dots of land surrounded by thousands of miles of uninhabitable water. With a concentration on simple inherited traits of no known adaptive significance in an area settled by small founding populations "with restricted subsequent introgression," it is a foregone conclusion that "most gene frequency variation in the Western Pacific is neutral" (Morton and Keats 1976:395). The situation dictates the conclusion that genetic drift alone is the agency by which difference occurs, but, in the words of the geneticist who proposed the mechanism in the first place, this will "rarely if ever contribute to evolutionary advance" (Wright 1951:452). And even if such studies do have some short-term uses for the study of local history (Harpending 1974), it simply does not follow that "this was probably typical of pre-agricultural and swidden Man, and of Europe before the Roman empire" (Morton and Keats 1976:395).

Another approach has been to subject a battery of measurements—usually of the skull—to multivariate analysis in the hope that the resulting mathematical vectors will tell us something about population relationships (Howells 1970, 1973*a*, *b*, 1977, 1979). Despite Howells's valiant attempts, the results have been less than fully satisfying. It is just possible that the reason for this is related to the reason the study of gene-frequency variation in the Pacific has not given us a picture of much more than trivial differentiation. That is, the individual dimensions whose measures serve to generate the covariance matrices are themselves of unknown adaptive significance or only partially and tangentially related to those aspects of morphology which really are under the control of selection. As a result, no amount of manipulation can bring us much closer to an understanding of the evolutionary dynamics of the populations in question.

In contrast, by focusing on traits of obvious adaptive value, one ought to be able to make systematic sense out of their different manifestations if long-standing differences in the intensity of selective-force operation are taken into account. Archaeological and linguistic evidence for population movements and encounters should also square with the observed biological picture—and, indeed, this seems to be the case for the distribution of immunoglobulin haplotypes (Curtain, Van Loghem, and Schanfield 1976). Further, if the trait considered is under polygenic control and hence less at the mercy of genetic drift, then the agreement should be clearer still.

This paper is an attempt to demonstrate that this can indeed be done. Using dental data, it can be shown that long-standing regional differences in selective-force intensity resulted in major differences in tooth size (Brace 1978). Subsequently the technology that led to population expansion and long-distance water-borne transportation accomplished the settlement of areas that had previously been uninhabited (Finney 1977, 1979) and, in the process, brought disparate populations into contact with each other. We show that simple tooth dimensions can provide an index of the extent to which the encountering populations did and did not mix. To some extent, these results conform to the kinds of expectations that were in the minds of anthropologists 50 and 100 years ago. The difference, we would maintain, is in our commitment to evolutionary mechanics. Finally, we suggest that this picture, based on initial difference and varying degrees of subsequent mixture, is applicable only

to the Western Pacific and cannot, without major qualification, serve as a model for deciphering the course of human evolution in other parts of the world.

## THEORETICAL EXPECTATIONS

The study of tooth size has proven useful in analyzing the events of hominid evolution in the Pliocene, the Early and Middle Pleistocene, the Late Pleistocene, and the Post-Pleistocene (Brace 1967, 1979*a*; Brace and Mahler 1971; Brace, Mahler, and Rosen 1973), and it has even cast light on relatively recent changes from mainland Asia all the way to Australia (Brace 1978, 1980*a*). In essence, the present paper is an expansion of the demonstration (Brace 1980*b*) that tooth size in Melanesia rises above the Polynesian-Taiwanese mean towards the Highland New Guinea-Australian means to an extent that parallels the linguistic divergence from hypothetical Proto-Austronesian.

Basically the human dentition is a food-processing device, and if there are major dental differences between past and present populations these should indicate differences in food-processing requirements. Some have taken this to point to differences in diet, but this is sharply at variance with the fact that major changes in the dentition have occurred in the absence of any indications of dietary change. For example, the teeth of the Classic Neanderthals of western Europe are 15% smaller than those of their predecessors at the end of the last interglacial in Yugoslavia (Brace 1979*b*), yet there is no evidence that the diet had changed at all. Teeth reduced by another 5% between the Classic Neanderthals and the early Upper Paleolithic and another 6% between the early and the late Upper Paleolithic, again with no evidence for dietary change. What we do have evidence for is the development of a food-preparation technology—a proliferation of cutting tools and, most important of all, suggestions that a heated-stone cookery was systematically in use.

In assessing the adaptive value of teeth and the significance of population differences in tooth size, the important thing, as has been said (Brace 1977:199), is not so much the food itself as what was done to it before it was eaten. If technology can take the place of teeth for a major part of the normally required amount of food processing, then the selective pressures formerly maintaining a large dentition are relaxed and conditions are set for the operation of the probable mutation effect (Brace 1963).<sup>2</sup> The expectable consequence is dental reduction. Furthermore, the extent to which the dentition of a modern population has been reduced below the Middle Pleistocene size-plateau should be an indication of how long the ancestors of that population have enjoyed the benefits of an elaborated food-preparation technology.

For various reasons (Brace 1978, 1979*a*, *b*), the long-term residents of the northern portions of the Old World—from Europe to China—developed elaborate food-preparation techniques before anyone else and consequently show the greatest degree of dento-facial reduction. Eventually these techniques spread south, and by the time written histories began to be recorded they had become the property of virtually all human populations. However, the large faces and teeth of highland New Guinea and Murray Basin Australia may just indicate the relative recency of the arrival of such culinary refinement. In this view, then, the modern spectrum of human tooth- and

<sup>2</sup> For theoretical reasons that we intend to avoid in the present paper, the probable mutation effect has been rejected by many geneticists who are unfamiliar with the data and implications of molecular biology. The most recent critique surveys previous ones and attempts to bring them up to date, but it continues to assume that for a trait controlled by  $n$  loci each will have only  $1/n$  effect (Williams 1978:179). An elementary application of the molecular perspective via developmental biology, however, should lead one to the realization that the effect will be closer to  $n/1$  than to  $1/n$ .

face-size differences is a product of the different cultural histories of the past 100,000 years.

When Europeans developed an effective oceangoing means of transportation in the Renaissance, it brought them suddenly into contact with parts of the world where the histories of culturally influenced selective forces had run different courses for differing lengths of time. One of the consequences was the development of the unfortunate and biologically indefensible concept of "race" (cf. Brace and Montagu 1977: chap. 11). Another was the generation of mixed populations in areas of long-standing contact—the "Cape Coloured" of South Africa and the American "black," for example. While it has been recognized that population mixing of this nature depended upon the development of long-distance oceanic transportation—and hence differed in kind from the way in which change normally came about in prehistoric times—it has largely gone unrecognized that a similar but much older and more extensive situation has literally shaped the faces of Oceania (Green 1978). The European colonization of South Africa and the Western Hemisphere was accomplished in less than 500 years by hundreds of transoceanic voyages and recorded in writing; the peopling of the Pacific was the result of many thousands of voyages over several thousands of years by people who lacked a tradition of literacy (cf. Howells 1973, Bellwood 1979, Kirch 1980). In the process, people of essentially continental Asian origin (Howells 1979) were brought into contact with people who had inhabited the large landmasses of the Western Pacific—New Guinea, New Britain, and Bougainville at least—for as much as 40,000 years and possibly longer. The time depth for the differential operation of selective forces in New Guinea as opposed to mainland Asia was great enough that the appearance of their inhabitants had become quite different by the time they met on the shores of Melanesia. One of the predictable consequences of such an encounter is the exchange, borrowing, and sharing of cultural elements, and the other obviously is the production of mixed offspring. From the evidence available, it is clear that both processes occurred. We shall attempt to demonstrate how tooth-size data can be used to suggest not only the proportions of the mixture in specific instances, but also the route the latecomers took in the peopling of the Pacific and the production of the mixtures of modern Melanesia.

## TOOTH SIZE

The indicator of tooth size that we use for our comparisons is the "summary tooth-size" figure (Brace 1979b, 1980a), the sum of the mean cross-sectional area ( $MD \times BL$ ) for each tooth category, upper and lower. In notation, this is written  $TS = \sum \bar{X}_j$ , where  $\bar{X} = (\sum [MD \times BL]) / N_j$ ,  $j = I^1, I^2, \dots, M^3, I_1, I_2 \dots, M_3$ , and  $N_j$  = total number of measured teeth in each category.

For most of the groups examined, it was not possible to make an accurate determination of the sex of individual specimens. Consequently, we lumped all of the individuals assignable to a given group and came up with a single figure. This procedure has yielded clear and interpretable results when applied to Asia, precontact North America, Europe, and Australia (Brace 1978, 1979b, 1980a). Questions have been raised (e.g., by Pietruszewsky 1980 and Smith 1980), but the patterns remain quite in accordance with prediction. Furthermore, where reliably sexed samples are known (for example, Australians from the Western Desert and the Murray Basin, Hong Kong Chinese, Hawaiians, Taiwanese, Thai, and Mesolithic and Upper Paleolithic Europeans [Brace and Ryan 1980]), the uncorrected lumped means differ by no more than measurement error from those based on separately calculated male and female subgroups which are subsequently averaged.

There are two drawbacks to the use of the summary tooth-

size figure. First, it lumps the cross-sectional areas of all the teeth, and if the groups being compared differ in the relative contribution made by the front as opposed to the back teeth this will not be picked up. Second, unless the analysis is based on data from complete dental arches only, there is no way to compute a variance for summary tooth-size. For the various groups for which data on complete dentitions do allow variance figures to be calculated, the standard deviation ranges from 32 to 135. The largest variance is found in Australia and must be considered reliable because the sample size in the two largest groups measured is over 130 individuals. In all but one of the other Australian groups for which more than a dozen complete dental arches were measured, the standard deviation was over 130 (Brace 1980a). Although Australian mean tooth-size is also large, the variance is relatively larger than for groups with a smaller average tooth-size. In Australia, the coefficient of variation runs from just under 9 to more than 10. In our Hawaiian sample it is 7.7, and for our mainland Asians it is less than 7. With these figures in mind, we argue, as in a previous study, that "as a kind of informal index . . . a summary tooth-size difference of 50 mm<sup>2</sup> between groups compared is probably meaningful, and a difference of 100 mm<sup>2</sup> almost certainly has some basic biological meaning" (Brace 1980a:144).

Table 1 lists the Asian and Oceanic groups examined and their summary tooth-size figures. The final column shows the average size of  $N$  and the smallest and largest tooth category sample for each group. Since single-rooted teeth are frequently lost postmortem, the smaller  $N$  usually refers to one of the incisor categories. Where the population is represented by especially small samples—as in Tonga and Samoa—it is more than likely that the group mean may be significantly altered by the disproportionate representation of one sex. Just by chance, then, such samples may produce figures larger or smaller than our hypothesis would predict.

## TOOTH SIZE IN THE PAST

As we begin to make our comparisons, we are going to take the position that, by the end of the Pleistocene in the Orient, there was a marked north-south tooth-size differential. We can guess that in the tropical portion, from mainland Southeast Asia through Indonesia and out to New Guinea and Australia, human appearance was characterized by dark pigmentation, marked hair curl, and large faces and teeth. Of course, we cannot prove what hair form and skin color really looked like in prehistoric people, and hair and skin appearances are not genetically linked to face and tooth size. Yet there is such an association in the living peoples of the Western Pacific, and it seems reasonable to suspect that the selective forces that conspired to produce this configuration in the living worked in similar fashion in the past. Thus where we find notably large faces and teeth in the past in the tropics of the Orient, we feel justified at least in *suspecting* the nature of the appearance of skin and hair.

Prehistoric skeletal material is scarce in the area, and the provenience for some of the more suggestive material is questionable. The Niah skull, for example, is often mentioned with a date of nearly 40,000 years (Harrisson 1976:127). But the date was not made on material associated with the skull, and, since the site was dug by "levels" that bore no relation to the actual stratigraphic profile, there will always be a question concerning its true antiquity (Solheim 1977, Kress 1979). It could easily be Neolithic, as so much of the material in the cave really is (R. H. Brooks, personal communication). That it looks like the Lake Mungo material in Australia (Thorne 1977:197) or Tasmanian or Melanesian (Howells 1973b:178) has been variously suggested (Kennedy 1979). Unfortunately for our

TABLE 1  
SUMMARY TOOTH-SIZE FIGURES FOR  
ASIAN AND OCEANIC POPULATIONS

GROUP	TS	AVERAGE <i>N</i> AND RANGE OF <i>N</i>
Bali Bronze Age.....	1,287	11(9-13)
Bismarck Archipelago.....	1,259	7(3-12)
Borneo.....	1,190	66(10-160)
Borneo Neolithic.....	1,312	15(6-23)
Bougainville Nasioi.....	1,359	138(64-180)
Celebes Mesolithic.....	1,288	154(57-253)
China.....	1,157	32(12-40)
China Bronze Age.....	1,191	234(134-319)
Fiji.....	1,338	12(3-23)
Flores Mesolithic.....	1,358	9(2-14)
Guam.....	1,309	34(24-43)
Hawaii.....	1,200	135(94-150)
Japan.....	1,200	178(102-211)
Java.....	1,240	36(20-48)
Malay Peninsula Mesolithic.....	1,370	22(8-32)
Marquesas.....	1,204	35(29-41)
Moriori.....	1,181	13(6-19)
New Britain.....	1,334	25(12-46)
New Caledonia.....	1,256	10(3-24)
New Guinea, Eastern Highlands....	1,395	51(32-57)
New Guinea, North Coast.....	1,286	32(11-45)
New Guinea, Sepik River.....	1,321	6(1-20)
New Hebrides.....	1,328	32(19-44)
New Hebrides, Malekula.....	1,295	38(11-75)
New Ireland.....	1,266	8(3-21)
Philippines, Visayas.....	1,288	109(53-201)
Samoa.....	1,311	3(2-4)
Taiwan, Prehistoric.....	1,205	4(1-6)
Thai.....	1,233	53(44-59)
Thai Bronze Age.....	1,224	40(23-52)
Thai Neolithic.....	1,252	30(16-44)
Tonga.....	1,371	14(6-19)

SOURCES: The Bronze Age Bali, Celebes Mesolithic, Flores Mesolithic, and Malay Peninsula Mesolithic were calculated from the data in Jacob (1967); the Bougainville figure was calculated from Bailit, Dewitt, and Leigh (1968); the Japanese figure was combined from Miyabara (1916) and Yamada (1932); and the highland New Guinea figure was calculated from Doran and Friedman (1974), with correction constants taken from sexed Australian material. The remaining figures are based on measurements made by the senior author on specimens in collections at Adelaide, Ann Arbor, Bangkok, Brisbane, Canberra, Edinburgh, Hong Kong, Honolulu, Jogjakarta, Kuching, Las Vegas, Melbourne, Paris, Otago, Sydney, and Taipei.

present concerns, there are not enough teeth to produce a summary tooth-size figure, so their contribution to our expectations is suggestive rather than substantive. However, the sum of the cross-sectional areas of the three maxillary molars calculated from the data in Brothwell (1960), 407 mm<sup>2</sup>, is indeed more like that of the Tasmanians (411 mm<sup>2</sup> as reported by Brace 1978a) than like that of those whose remains were recovered from the surface of Niah Cave and measured by the senior author in 1974 at Kuching (354 mm<sup>2</sup>). If nothing else, the Niah skeleton indicates that large-toothed people did live in northwestern Borneo at some time in the not too distant past. If the "deep skull" at Niah did belong to a group with fully Tasmanian-sized teeth—1,429—then clearly a dramatic reduction had occurred by the Neolithic to produce the 1,312 figure. The reduction from the Neolithic to modern size—1,312 to 1,190—at Niah is quite comparable to the changes that took place over the same period of years in England, France, and China (Brace 1978, 1979b).

There are two Southeast Asian Mesolithic groups, located effectively at opposite ends of the Indonesian archipelago, for which summary tooth-size figures can be produced. At the eastern end, the Mesolithic skeletal material from Flores yields

a summary tooth-size figure of 1,358, and from the Hoabinhian material at Gua Kepah on the west coast of the Malay Peninsula the summary tooth-size is 1,370 (data from Jacob 1967:65, 103)—comfortably similar to that for the Walbiri of Yuendumu settlement in the middle of Australia, 1,350 (Brace 1980a). Over 100 years ago, Huxley (1863:265) remarked of the form of skeletal material from Gua Kepah that "the face must have had as prognathous a character as that of an ordinary Australian." The available metric data do nothing to detract from that judgment.

In between Flores and the Malay Peninsula there are some suggestive if incomplete pieces of evidence. The Australian affinities of the famous Wadjak specimens, given to Dubois before he moved his operations from Sumatra to Java in 1889, have been variously commented upon. Weidenreich (1945:21), for example, clearly and explicitly noted the similarities between Wadjak and the Keilor skull found in 1940 not far north of Melbourne; Keilor, he remarked, "is a duplicate of the Wadjak skull." Of course, there is no way to reconstruct the exact provenience of Wadjak beyond noting that the fauna recovered was similar to the "subrecent Indo-Malayan" fauna at the Javanese Mesolithic site of Sampung with the additional presence of *Tapirus* (Jacob 1967:56). Although a complete summary tooth-size figure for Wadjak cannot be produced because the upper incisors are lacking, the sum of the cross-sectional areas of all the rest of the teeth adds up to a figure that is considerably larger than that for Kow Swamp, at the very top of the Australian range of variation. If Kow Swamp upper-incisor dimensions are used to complete the Wadjak data, the summary tooth size is 1,619—larger than the 1,561 figure for Kow Swamp, larger than the 1,578 figure for the *erectus* material at Choukoutien, and nearly as large as the 1,631 figure for the Krapina Neanderthals from Yugoslavia (data from Jacob 1967:49; Thorne 1976:108; 1977:197; and Brace 1979b, 1980a).

Finally, there is skeletal material from the Mesolithic site of Guo Lowo, near Sampung in eastern Java. Again the data are not complete—no measurements for an upper first premolar are present—and there is no indication of how many individuals are represented. But if the P<sup>1</sup> area from Gua Kepah is inserted, Sampung yields a summary tooth-size figure of 1,452, which is comfortably in the range of the large-toothed Australians, between the 1,429 figure for Tasmania and the 1,486 figure for the Murray Basin (data from Jacob 1967:49 and Brace 1980a).

On the basis of these data, we suggest that a case can be made for the existence of an indigenous population in peninsular and island Southeast Asia with faces and teeth that were essentially the same as those seen in Australia at the onset of European colonization. Figure 1 provides an outline using real data for such a view. The Walbiri → Broadbeach spectrum nearly encompasses the Australian range of variation, and Mesolithic Flores and Gua Kepah are clearly more aligned with the Australian spectrum than with that of modern Southeast Asia. Using these data as a point of departure, we suggest that the people of Australasia prior to the advent of food-producing subsistence strategies and technologies possessed Australian Aboriginal-sized faces and teeth. (Presumably also they were dark of skin and curly of hair, but that is not really central to the scheme we are offering.) The hypothetical extent of these preagricultural people is illustrated in figure 2. There is one Mesolithic group with a tooth-size figure that is decidedly below the trend of the others mentioned; the figure from the Upper Toalan site of Leang Tjadang in the southern Celebes, 1,288 mm<sup>2</sup>, is just below the smallest Australian figure (1,296 for Cape York) and exactly the same as that of the Bronze Age population at Gilimanuk in Bali (1,287). Although this is only the smallest of hints, it might just indicate that the path of entry for the late-Mesolithic-to-Neolithic change in subsistence strategies in Indonesia was via the Philippines and Celebes and

not via the Malay Peninsula and Sumatra. As will be mentioned later, there are some hints from archaeology and linguistics that bolster such a view.

### FIRST MOVEMENT INTO THE PACIFIC

Most scholars agree that the initial populating of the south-eastern half of the shaded area depicted in figure 2 took place more than 40,000 years ago, at a time when the technology for accomplishing ocean voyages was rudimentary. Only the shortest water gaps could be traversed, and the obvious route was via Sundaland (Birdsell 1977). Indonesia today stands like an arrow pointing to the Pacific, and many who would interpret the peopling of the realm assume that the logic of geography would always dictate this as the principal route. Our guess for the direction of late-Pleistocene movements, first of people and later of technological items, is depicted in figure 3.

For purposes of appraising the selective forces that influence human tooth and face form, whether a group is regarded as horticulturalist or intensive collector is immaterial. The lack of garden fences and permanent settlements that keeps northern Australians from being called horticulturalists (White 1979) does not constitute a distinction in the selective forces affecting the dentition that would make their condition different from that encountered among the Papuans just across the Torres Strait. Thus the fact that Australia, at the point of European contact 200 years ago, could be regarded as "Mesolithic" (Brace 1980a) while New Guinea was essentially "Neolithic" and may have been so for as much as 9,000 years (Golson and Hughes 1976) is without any importance to an appraisal of the selective forces influencing the maintenance of tooth size. The two areas shared a focus on the utilization of plant foods that involved techniques of grinding, pounding, and cooking that were essentially the same. Whether or not the Kartan indicates the extension of Hoabinhian influence as far as southern Australia and whether or not the waisted tools of Kiowa, Kosipe, and other sites in highland New Guinea are similar indications of diffusion, most would agree that an intensive post-Pleistocene concentration on the preparation and use of plant foods

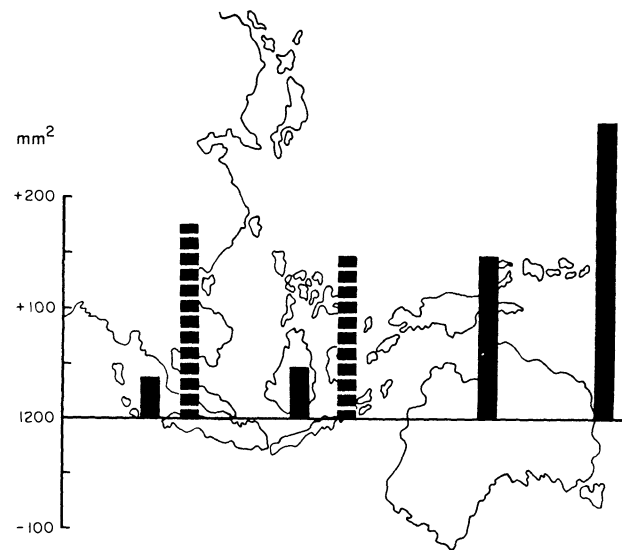


FIG. 1. Summary tooth-size variation above a 1,200-mm<sup>2</sup> base line for living (solid bars) and Mesolithic (broken bars) groups ranging from peninsular Southeast Asia to Australia. The contemporary populations are (left to right) modern Thai, modern Javanese, the Walbiri of Central Australia, and a contact-dated group of Australian burials from Broadbeach in southeastern Queensland. The Mesolithic ones are Flores and Gua Kepah on the Malay Peninsula.

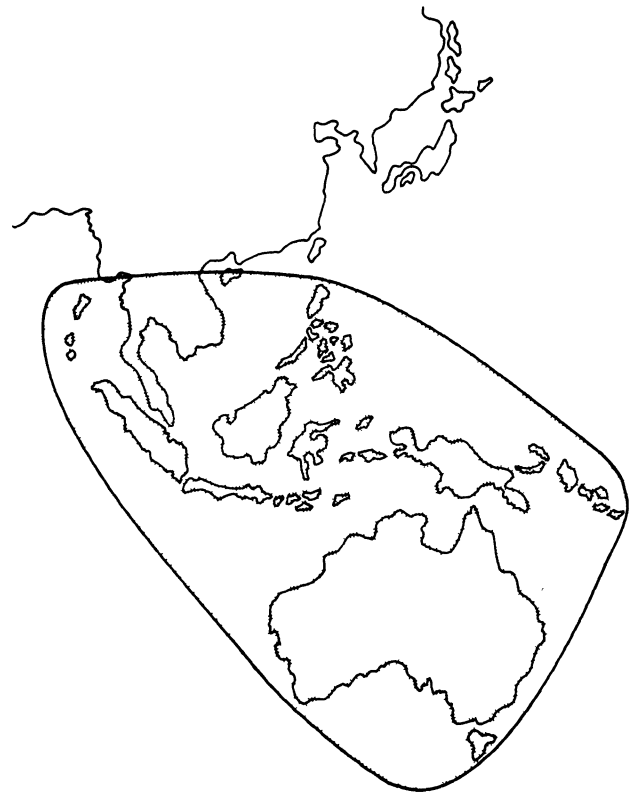


FIG. 2. The hypothetical extent of large-toothed (and presumably curly-haired and heavily pigmented) preagricultural people in Australasia late in the Pleistocene.

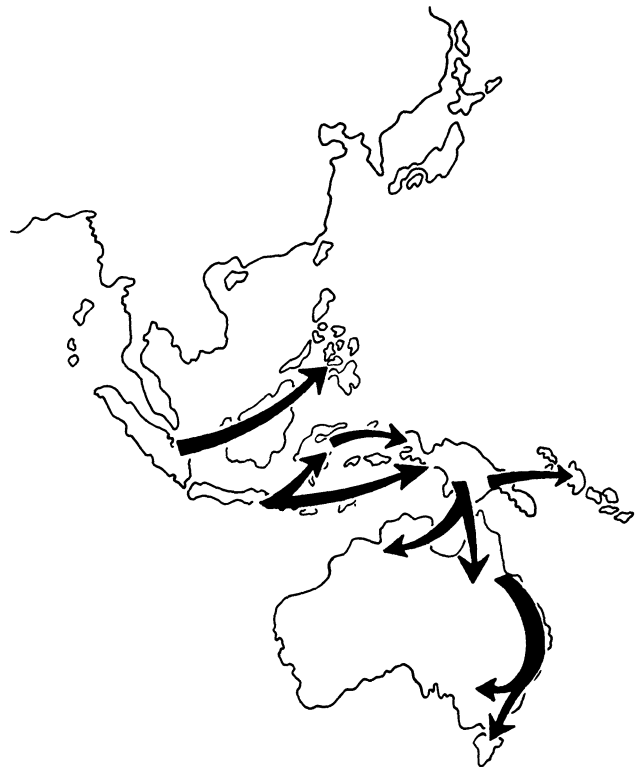


FIG. 3. Hypothetical routes for the initial late-Pleistocene movement of people across the water gaps and into previously unoccupied regions such as New Guinea and Australia.

spread from continental Southeast Asia out through Indonesia and into New Guinea and Australia very much as indicated in figure 3.

But if this was the route for the initial spread, it would seem that later immigration chose another avenue. First of all, once an area is occupied, the descendants of the indigenes simply remain unless threatened with invasion by newcomers in great number with clear-cut technological advantages. No such situation occurred in Australasia. Rather, increasingly sophisticated elements of Mesolithic technology spread slowly but continuously through the region after the end of the Pleistocene—a process that was still going on in Australia at the time of first contact with Europeans (Brace 1980*a*). The pieces of food-producing lifeways spread slowly by the same route (Golson 1971, 1974, 1977; White and Allen 1980). Presumably this was accompanied by a minor amount of gene flow and a slight alteration in selective forces, although nothing of such magnitude as would drastically alter the appearance of the inhabitants. By the time people of markedly different appearance with the technological capacity for mounting anything like an “invasion” arrived on the scene, the descendants of the first immigration were gardening with sufficient skill that all the major available tracts of land were already utilized to full capacity. The only unutilized bits remaining were the islets to the north of the previously settled area and ranging east throughout the Pacific—the area now known as Micronesia and Polynesia. Throughout this area today there is a superficially evident community of physical appearance (Howells 1973*b*, 1979). Skin color tends to be much lighter and hair less curled than in the previously occupied areas to the south and west, and face form displays little of the prognathism of the earlier peoples. In addition, there are obvious similarities in language from Taiwan to Indonesia and from the Philippines to Hawaii, Easter Island, and New Zealand (Crawford 1852, Capell 1962, Clark 1979). Finally, archaeology has provided a substantive basis for the suspicion raised by the other branches of anthropology that the populating of the small-island Pacific was accomplished by people who had developed a sophisticated seagoing technology and possessed the horticultural capacity to wring a subsistence out of the previously uninhabited islands (Green 1978, Bellwood 1979).

## THE FINAL MOVEMENT INTO THE PACIFIC

It is now generally agreed that this final populating of the Pacific was by Austronesian-speaking peoples, but there has been considerable dispute concerning where they came from and what this portends for making sense out of island Melanesia. One attempt at a solution from a linguistic point of view assumed, on the basis of obvious geography, that Indonesia was the source for the Austronesian spread (Crawford 1852: cclxxxiii). Another attempt, based on the assumption that diversity was the consequence of long-term in situ differentiation, suggested that the place of origin must therefore be in island Melanesia (Dyen 1962*a, b*). Finally, there are those who suggest a source in Taiwan (Shutler and Marck 1975) and ultimately on the Asian mainland “roughly in the South China region” (Benedict 1966:257; 1975:31).

The archaeological evidence for the settlement of small-island Oceania coincides nicely with the linguistic estimate of “a date of 4,000 B.C. or earlier for the initial dispersal of Austronesian languages” (Pawley and Green 1975:54). At that time, Proto-Austronesian society is assumed to have possessed agriculture, domestic animals, pottery, and the capability of making long ocean voyages in outrigger canoes (Green 1978:2). The spread of these people towards the eastern Pacific can be traced, at least in its beginning, by the dispersion of pottery called Lapita, after a site on the west coast of New Caledonia (Gifford and Shutler 1956), and now dated at Ile des Pins

back to more than 2000 B.C. (Shutler and Shutler 1975:59). The archaeologist most closely identified with studies on the Lapita-Austronesian spread considers the most plausible “homeland” to have been in the Bismarck Archipelago 6,000 years ago, although he concludes, “I do not know and refuse to speculate on Lapita’s ultimate origins” (Green 1978:6, 17). Although others have expressed support for this cautious view (White and Allen 1980), some suspect a Lapita origin west of Wallace’s line. These authors observe that the eastern extent shows a history of material-culture loss (Blust 1976) and note the similarity to Lapita pottery of the red-slipped ware from the Sulu Islands of the southern Philippines, where early Lapita-like shell adzes and gouges occur at 4500 B.C. (Spoehr 1973:190–91; Solheim 1975:24). In a circumspect reversal of his earlier refusal to speculate, Green (1979:45) has offered some encouragement for such views by his observation that “an eastern area of Island Southeast Asia is certainly a very likely site on the grounds of language, food plants, domestic animals, technology, watercraft, and voyaging capability, as well as other aspects of the cultural content.”

As much as anything else, the biological characteristics of the people of small-island Oceania suggest the locus of their origin (Howells 1979). Their relative depigmentation can only be explained by the shaping effects of selective forces that operate well north of the strictly equatorial. The genetic frequency of the immunoglobulin haplotype *Gm<sup>a1b</sup>* of the Austronesian-speakers in the Pacific is similar to that in Thailand and Taiwan and markedly different from that among the non-Austronesian-speakers of Melanesia, New Guinea, and Australia (Curtain, Van Loghem, and Schanfield 1976, Schanfield 1977, Schanfield and Fudenberg 1975, Schanfield and Gershowitz 1973). And finally, the tooth-size reduction that occurred during the Late Pleistocene of the temperate zone all the way from Europe to China (Brace 1978) is also clearly apparent from Taiwan to Hawaii and New Zealand.

Putting all of these together, it seems most likely that the original source for the people who constituted the second and final major migration into the Pacific was the Southeast Asian coast somewhere in the neighborhood of South China. The probable course of this movement of peoples is suggested in figure 4. If the first extension of human populations east of Wallace’s line was a slow affair characterized by population increase of initially sparse hunter-gatherers into adjacent terrestrial areas after the overcoming of a few short water gaps, this final movement was a relatively rapid one by technologically sophisticated horticulturalist-fishermen. We have no direct evidence for the length of time required for the initial spread, but the final one had gotten from the Asian mainland to eastern Melanesia in a space of 2,000 years and to the rest of Polynesia in just 2,000 more years, leaving most of the habitable islands of the Pacific occupied not long after the beginning of the Christian era.

## MELANESIA

The final problem to which we turn our attention is framed by Hocart’s classic rhetorical question, “Who are the Melaneseans?” (Hocart 1923, echoing Sergi 1903:339). Huxley (1870:404) had casually referred to the area as “the Negrito Islands,” and, while in using that term he did not imply any judgment concerning stature, the suggestive power of the word and the existence of short-statured dark-skinned people in such separated places as the Andaman Islands, the Malay Peninsula, the Philippines, New Guinea, and the New Hebrides has created the idea that these must be the remnants of an ancient configuration of great typological significance. Since we suspect that there is a grain of truth hidden here, we cannot dismiss this out of hand. Indeed, it seems likely that the dark-skinned people in each of these localities do represent the descendants

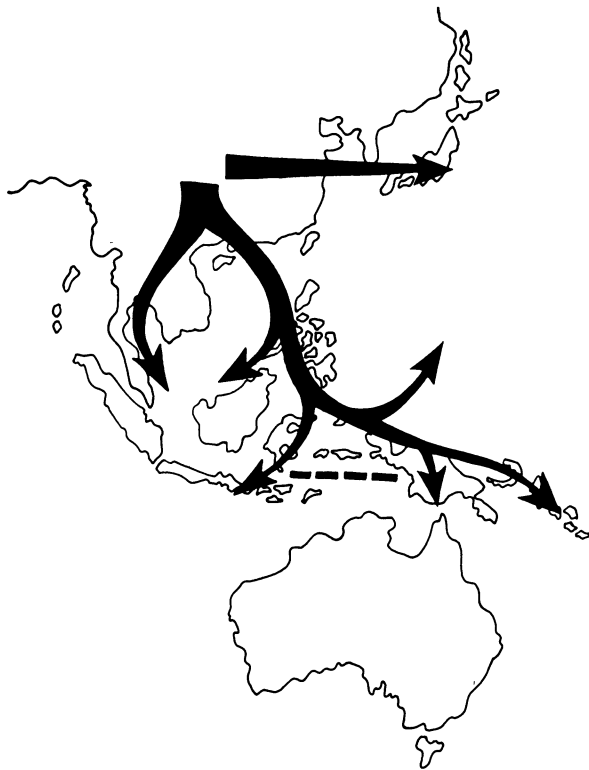


FIG. 4. Probable route for the final major spread of peoples into the Pacific.

of the indigenes prior to the movement of the final wave of settlers suggested in figure 4, but it seems unlikely that their size represents anything more than *in situ* local adaptation. Indeed, in the Philippines (Howells 1977:180), Melanesia, and New Guinea (Simmons 1973:23; Simmons and Graydon 1970), the so-called pygmies grade without break, both morphologically and serologically, into the normal-sized adjacent populations.

The African affinities of the Melanesians, once widely assumed (cf. Sergi 1912:209, 213), have now been denied by most serious students of the matter (e.g., Howells 1973b:48, 162), but no satisfying interpretation has been substituted in acceptable form. One recent effort has attempted to identify the ancestral populations that underlie the assumed Melanesian entity, noting that these are comprised of Australoids, Melanesians (the "ordinary negroid-looking kind"), Papuans (the "narrow-nosed high-skulled types"), and Negritos ("a true pygmy black") (Howells 1973b:36). The author then concludes, enigmatically, that "Melanesians are a single basic population rather than a composite" (Howells 1973b:48). The "great genetic diversity" of this population is then mentioned, and most modern workers agree (Curtain 1976). But the idea of a "single population" remains even among those who have explicitly warned against typologizing. For example, the very person who warned of the typological impetus behind seeing a single essential entity in Australia turns around and quotes with approval the view that "all the populations of New Guinea and the Melanesian islands so far tested are of the same basic stock" (Allbrook 1976:216).

Allbrook's warning that, under most circumstances, the urge to label populations as "single essential entities" or the results of the hybridization of several such is a manifestation of old-fashioned typological thinking is a warning well stated. As we have noted, the history of anthropology contains a depressing number of accounts that purport to explain the observed picture of human variation by invoking long-distance migrations and subsequent mixtures of "stocks" that had originally arisen

in isolation. It would be just as arbitrary, however, to deny that this had ever occurred. Melanesia may be one of the few instances to which such an account can be applied. If, as we suggest, the large islands of Melanesia were first settled late in the Pleistocene by descendants of essentially Australoid Indonesians who had been developing *in situ* since the beginning of the Middle Pleistocene, and if, further, the Austronesians moved out into the small-island Pacific within the last 4,000 to 6,000 years, then we ought to be able to get some measurable index of what happened when these two differing groups met.

Once again, we suggest that tooth size can give us the index we need. Figure 5 presents a detailed suggestion of the eastern extent of the hypothetical large-toothed Australasian populations late in the Pleistocene. Admittedly, there are no prehistoric teeth from New Guinea and island Melanesia, but there is some archaeological evidence that the area was occupied by people at "a pre-pottery, pre-agricultural stage" (Shutler and Shutler 1975:97). There are surface finds of stone tools on New Britain (Chowning and Goodale 1966), although these may not be preagricultural; a prepottery flake-tool industry on New Ireland may be as early as 6000 B.C. (White 1972; 1979:356-57), similar artifacts have been found on Bougainville, and there is a non-Lapita level at the Fotoruma cave site on Guadalcanal that is of the same age as the Lapita sites in the Reef/Santa Cruz Islands (Green 1978:6-7; 1979:47). If the report of radiocarbon dates from New Caledonia of around 10,000 B.P. (Shutler and Shutler 1975:97) can be confirmed and if Mangaasi pottery really does represent the "founding cultural complex" of the New Hebrides (Green 1979:47), then this would be one more piece of evidence for the existence of pre-Lapita people in island Melanesia. In any case, it has been suggested that the chipped-stone tools at prepottery levels on New Britain, New Ireland, and Bougainville "may be connected with the spread of Papuan speakers to these islands of western Melanesia" (Shutler and Shutler 1975:73).

It is of interest to note, then, that the teeth of known non-Austronesian-speaking peoples in Melanesia are fully as large as those of Australian Aborigines. The summary tooth size for the Nasioi of Bougainville is 1,359 mm<sup>2</sup>, which is no different from the 1,350 for the Walbiri, and the figure of 1,395 from Lufa and Goroka in the eastern highlands of New Guinea is the same as the 1,393 figure for the southwestern Australian Aborigines in the Western Australian Museum at Perth. The figure for New Britain, 1,334, one of the largest in western Melanesia, is close to the 1,332 figure for the coast of northern

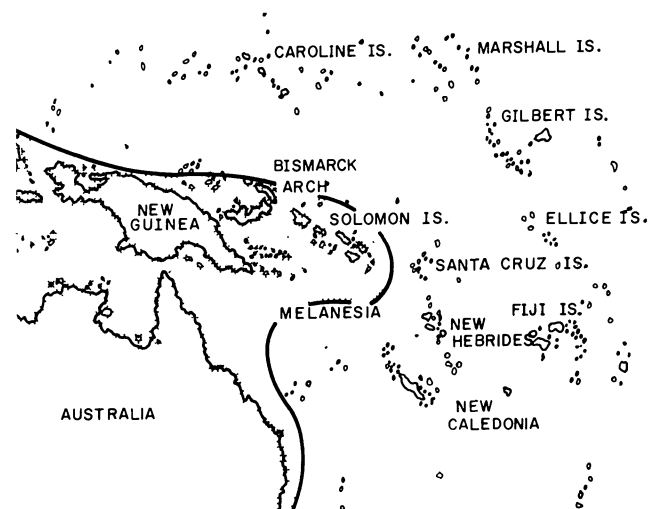


FIG. 5. The hypothetical eastern extent of large-toothed preagricultural people in Melanesia.



Queensland and the 1,328 figure for the New Hebrides. It also compares with the 1,313 figure calculated from the large New Britain series ( $N = 159$  [74–239]) published by Janzer (1927: 317). The New Britain skulls were collected by various amateurs, mostly residents of Rabaul, and eventually became parts of collections in Australian museums. For most of them, it is not even possible to tell exactly where they came from, let alone what language the individual spoke, but it is likely that it was northern New Britain, where Rabaul itself is located. It is just there that the greatest concentration of non-Austronesian-speakers was to be found (Wurm 1971:645).

Figure 6 arranges the named groups from left to right roughly in order of their distance from the Asian mainland. With just a couple of exceptions, the extent to which the solid bars rise above the 1,200 mm<sup>2</sup> base line is also a good indicator of the extent to which the language spoken in that area departs from Proto-Austronesian. This picture is precisely what one would expect if small-toothed Austronesian-speaking people had moved into the Pacific via Taiwan and had mixed to varying degrees with large-toothed non-Austronesian-speaking precursors resident on the larger landmasses of Melanesia. Essentially this view is expressed by a linguist whose informal observation of human physical appearance has created “the impression that the highest Austronesian word content in Melanesian languages is found in some areas in which the admixture of the Polynesian geographical race in the present-day Melanesian . . . is rather marked” (Wurm 1967:34). A fully developed attempt to deal with the situation in Oceanic linguistics from this perspective was eloquently presented by Capell in 1962, but it was not greeted with much enthusiasm at the time (cf. Dyen 1962*a* and other comments), and, except for similar sentiments expressed by Wurm (1967, 1971, and elsewhere), the thrust of its general implications has largely been rejected (Pawley and Green 1975). However, if one modifies Capell’s assumption of Indonesian origins to coincide with an Austronesian spread in the direction depicted in figure 4, then the “mixing” (pidginization and creolization) in Melanesia accounts for the linguistic diversity found there just as it does for biological variation as shown by differences in tooth size.

We suggest, then, that the movement of small-toothed Aus-

tronesian-speakers into Melanesia occurred by sea along a route similar to that depicted in figure 7. Where initial contact between the earlier big-toothed non-Austronesian-speaking people and the seagoing newcomers took place, both biological and cultural (as exemplified in the linguistic situation) mixing took place. This is depicted by the diagonal-line shading in figure 8. Subsequently, groups in which the mixing had taken place moved eastwards, accounting for the fact that eastern Melanesia is now populated by people who are noticeably darker of skin and larger of tooth than the initial Austronesian settlers of the small-island Pacific. This would also account for the presence of so many non-Polynesian items of material culture, diet, and custom in island Melanesia, and it would also explain why so many of the languages that are formally classified as Austronesian are considered “aberrant” or “atypical.”

Such a procedure would be expected to produce a distribution of summary tooth-size figures like the one recorded in figure 6. The admittedly small sample of pre-Chinese inhabitants of Taiwan, whose descendants are the modern “Malayopolynesian”-speakers of that island (Chang 1970), was excavated from the Fantzuyuan shell-mound site halfway down the west coast (Sung 1962) and possessed teeth almost exactly the same size as those visible in Hawaii, the Marquesas, and New Zealand. Predicting the probable departure from this base line using the data from linguistics, material culture, and a subjective assessment of phenotype, the remaining bars in figure 6 are all reasonable except for those for Samoa and Tonga. Both these groups are properly within Polynesia, albeit not that far from the eastern extent of Melanesia. Both speak languages descended from Proto-Polynesian (Pawley and Green 1975:44). Given the fact that “the region comprising Fiji and the Polynesian Triangle is linguistically and archaeologically rather homogeneous” (Pawley and Green 1975:53), having been settled by related people in a relatively short span of time, we would expect tooth size and other aspects of biological appearance to be similarly “rather homogeneous.”

Fiji, of course, was subject to the influence of elements from eastern Melanesia long after it was initially settled (Pawley and Green 1975:47). Phenotypically this is superficially visible in the greater degree of hair curl and pigmentation than is

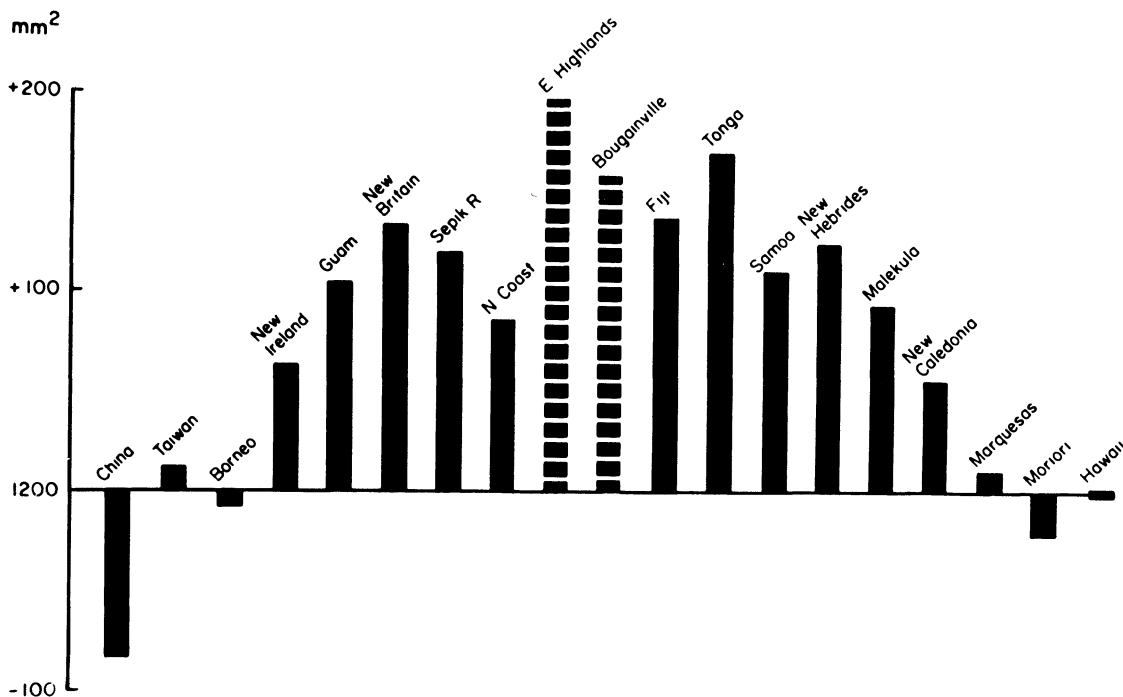


FIG. 6. Summary tooth-size deviations above and below a 1,200-mm<sup>2</sup> base line for a series of populations ranging from South China out through Melanesia and Polynesia. The broken bars are for non-Austronesian-speaking groups.

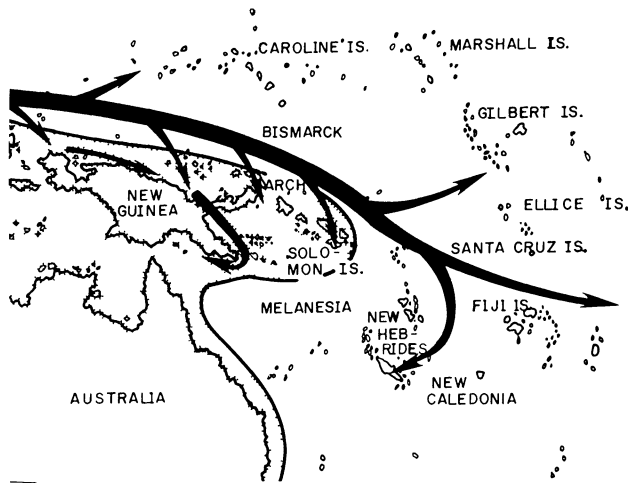


FIG. 7. The hypothetical route of small-toothed Austronesian-speakers into Melanesia and out into the Pacific.

typical for Polynesia. This also is supported by the presence of the haplotype  $Gm^{201b}$  (Curtain, Van Loghem, and Schanfield 1976). Tonga also suggests to the observer that a mixed Melanesian influence has had more impact on the appearance of the population than elsewhere in the Polynesian triangle, but this certainly does not lead us to anticipate teeth that are of fully Australian Aboriginal size and just a bit larger than those of the non-Austronesian inhabitants of Bougainville. The only explanation that we can offer is that our figures have been affected by the use of very small samples. To be sure, other groups (e.g., Prehistoric Taiwan, Sepik River, New Ireland, Flores Mesolithic, New Caledonia) also are composed of very few individuals but still conform to expectations. For Tonga, the number of individuals contributing to each separate cross-sectional area varied between 6 and 19. The average  $N$  was less than 15. For Fiji, the average  $N$  was less than 12, and for Samoa it was only 3. It is just possible that the burial caves from which much of the material is reported to have come contained individuals that were of elite status and were disproportionately large and/or male. In any case, the figure for Tonga especially is disconcertingly large. As with some of the others reported, samples of this size are not large enough to produce reliable summary tooth-size figures, but since they are all we have to work with we report them anyway. Although the results deviate somewhat from our expectations, we do not feel that they seriously undermine the general model supported by the rest of the data presented.

## SUMMARY AND CONCLUSIONS

We have chosen to look at the people of Oceania from the point of view of a single trait that is of evident adaptive significance and has responded to the different selective-force intensities in different geographic areas. We suggest that the pursuit of such a course of study is more likely to produce insight into the history and dynamics of the populating of an area than is multivariate analysis or gene-frequency investigation of traits of little or no known adaptive value. The trait we have focussed upon is the dentition, specifically the sum of the average cross-sectional areas of all the separate categories of teeth. This summary tooth-size figure is the best single representative of the amount of crushing surface which the teeth can bring to bear upon whatever is inserted into the mouth. It is also the best measure we have of the capacity to continue effective food processing despite the appreciable amount of wear that is the expectable lot of peoples that lack technologically produced

means of reducing the otherwise necessary amount of mastication.

During the Late Pleistocene, tooth size in the populations inhabiting the northern portions of the Old World underwent a reduction of between 40% and 50%. These Late Pleistocene dental reductions and the culinary innovations which allowed them to occur were much slower in developing in the southern reaches of human habitation, particularly in the Far East. Thus, by the beginning of the Holocene, 10,000 years ago, there was a sharp tooth-size differential between people living in the latitude of China and those on the equator. It is quite possible that there was an unbroken gradation from the small dentitions of the north to the unreduced ones in the south, but subsequent events led to the confrontation of peoples with long-term northern and southern evolutionary histories by effectively short-circuiting the peoples in the intervening inhabited areas.

The first peopling of the Pacific was by preagricultural peoples 40,000 or more years ago. The route taken was from Indonesia—Sundaland during the time of lowered Pleistocene sea levels—across the short water gaps to New Guinea and possibly some of the larger Bismarck and Solomon islands and, of course, into Australia. In the absence of any concrete evidence, we assume these to have been big-toothed people like the Late Pleistocene inhabitants of the Murray Basin in Australia and the post-Pleistocene Mesolithic peoples of Indonesia. Subsequently domesticated plants and animals spread through all but the Australian part of this area, and the Mesolithic way of life was converted into swidden agriculture, but the alteration in selective-force intensity has not been of long enough duration to create more than a beginning of a trend towards dental reduction.

The final peopling of the Pacific was by people who had come ultimately from a part of the Asian continent where tooth-size reduction had been taking place throughout the Late Pleistocene. They moved into Oceania with the aid of a sophisticated seagoing technology which enabled them to bypass the settled lands of South Asia and move more or less directly into the island Pacific. Their agricultural and marine resource utilization capabilities enabled them to colonize the previously unoccupied small islands of Micronesia and Polynesia, but inevitably they met and mingled with the descendants of the earlier settlers of large-island Oceania on the shores of Melanesia.

Tooth size provides a good index of the extent of that mingling. Among the non-Austronesian-speaking people of Bougainville

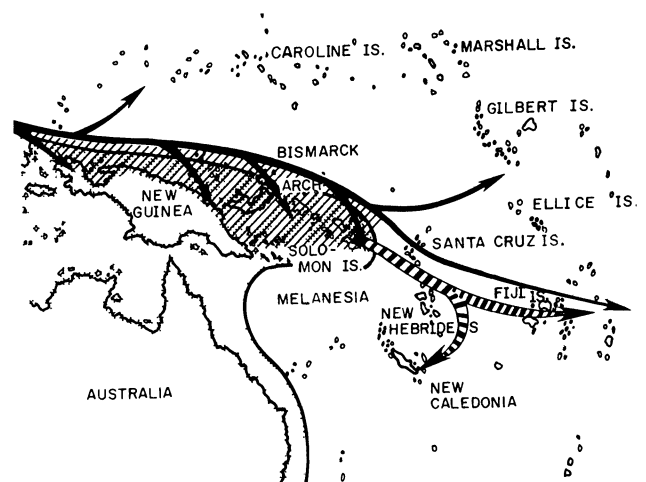


FIG. 8. The area of mixing of biological and cultural elements in Melanesia (diagonal-line shading) and subsequent movement of mixed populations into eastern Melanesia (broken arrows).

and eastern highland New Guinea, teeth are fully as large as they are in central Australia, and we suggest that no appreciable mixing had yet occurred. Tooth size in eastern Polynesia from Hawaii to New Zealand is exactly the same as it was in pre-Chinese Taiwan, and, again, we suggest that no more than a minimal amount of mixing had occurred during the Polynesian movement eastwards into the Pacific. Tooth size on coastal New Guinea and island Melanesia, however, falls in between the large and the small to an extent that represents the ancestral contribution of each—a situation that is nicely paralleled by the cultural and linguistic evidence for the mingling of Austronesian and non-Austronesian and New Guinea and non-New Guinea elements.

But if the Pacific can be understood in terms of two separate waves of populating and their consequent meeting and amalgamating, the situation is one that could only occur given a post-Pleistocene level of technological sophistication—both agriculture, with its implications for maintaining dense populations, and the capacity to travel long distances over uninhabitable ocean. Consequently, research on the peoples of the Pacific is unlikely to tell us much about the processes that shaped human form during our Pleistocene past. It can only tell us about what has happened in the Pacific itself and its adjacent source lands—a problem that is quite interesting enough in itself to justify the efforts expended in its pursuit.

## Comments

by TASMAN BROWN

*Dental School, University of Adelaide, Adelaide, S.A. 5001, Australia. 31 III 81*

In this and previous papers Brace and his colleagues have presented an impressive array of tooth-size data relating to Australian, Southeast Asian, and Oceanic populations. Through their efforts we now have a clearer picture of variability in tooth diameters over a considerable geographic and time span. The approach adopted by these authors is a refreshing alternative to the multivariate analysis of craniometric data, which often gives rise to considerable difficulties in interpretation depending on the input variables and covariance matrices. In the present case we have a single variable, tooth size, which appears to be a conceptually simple representation of crushing surface and, by inference, masticatory efficiency. However, the use of summary tooth-size values to “provide an index of the extent to which the encountering populations did and did not mix” is probably not as straightforward as the authors’ analysis suggests.

Sampling problems encountered with small numbers of museum specimens surely create difficulties in the subsequent analysis of data and interpretation of results. The extent to which small samples, in some instances ten or fewer, can be accepted as representative of a population is doubtful, particularly in view of the relatively large variances for summary tooth size (32 to 135 where these could be calculated). The pooling of data from males and females is another source of difficulty referred to by the authors. Their observation that uncorrected lumped means were almost identical to those derived by averaging male and female means calculated separately is only what would be expected with almost equal representation of males and females. The assumption that this situation also applies to unsexed material is only valid if male and female specimens are present in approximately equal numbers. There is no evidence that this condition is met.

While it is true that tooth size is under reasonably strong polygenic control, there is accumulating evidence that a variety of environmental agencies can exert influences on the developing dentition. For example, the maternal effect contributes to

tooth-size variability in both deciduous and permanent dentitions, particularly the former (Townsend 1980). Developmental interactions between neighbouring teeth also affect tooth diameters, more so the mesiodistal, as they contest for available space within the growing jaw. Although nongenetic influences on tooth-size variability are not fully understood, it is likely that further work in this field will clarify the interplay of factors affecting dental morphogenesis and lead to a reassessment of the adaptive significance of tooth dimensions.

Notwithstanding the above comments, Brace and Hinton have provided valuable data on tooth-size variability. Their interpretation of gradients in summary tooth size in terms of migrations into and around the oceanic regions is interesting, but in view of limitations in the data it must remain tentative at this stage.

by R. C. GREEN

*Department of Anthropology, University of Auckland, Private Bag, Auckland, New Zealand. 13 IV 81*

I have one general comment on the Brace and Hinton thesis of just two separate waves of genetically different people who subsequently mixed in Island Melanesia as being solely responsible for the peopling of the Pacific: much as one might wish things in Pacific prehistory were that simple, I fear they are probably not. I am a little alarmed by what Brace and Hinton have done with my views on this matter to arrive at an interpretation of the archaeological information that is said to support their position. Basically they have conflated a *hypothetical* initial dispersal of Austronesian languages from Island Southeast Asia into Oceania around 4000 B.C. with a suggested Lapita expansion around 1600 B.C., allowing them to see these two events as essentially one migration, the second of their two-population scheme.

Linguistic diversity in the Oceanic subgroup of languages suggests that Austronesian penetration of Oceania centred somewhere in the Bismarck Archipelago. Despite Brace and Hinton’s claim “that the archaeological evidence for the settlement of small-island Oceania coincides nicely with the linguistic estimate of a ‘date of 4000 B.C. or earlier for the initial dispersal of Austronesian languages,’” there is no such archaeological evidence. The hypothesis is a linguistically based one for which good supporting archaeological evidence is as yet meagre if not almost nonexistent. The spread of these initial Austronesian settlers in Oceania towards the eastern Pacific therefore cannot be traced, *and especially not in its beginning*, as Brace and Hinton assert, by the dispersion of pottery called Lapita.

I have placed the most plausible Lapita “homeland” in the Bismarck Archipelago also, but not 6,000 years ago as they indicate. Rather I would place it between 3,600 and no more than 4,000 years ago, and I believe this distinctive cultural complex may have developed there before its spread eastwards. I have been careful to state that while in the eastern Lapita area of Fiji, Tonga, and Samoa we can make a good case for associating this cultural complex with an Austronesian language subgroup, Proto-Central Pacific, this is not easily done for the Lapita sites farther west in Island Melanesia, where such correlations with other higher-level Oceanic subgroups become highly speculative. In ending a discussion of this question I “warn against any attempt to assign a linguistic identity to people responsible for the western Lapita sites” (Green 1979:48). Brace and Hinton cite “the archaeologist most closely identified with studies on the Lapita-Austronesian spread”; it cannot be I. Finally, in their section expressing this conflation of my views I find them mentioning a date for a Lapita site on the Ile des Pins of more than 2000 B.C. and citing Shutler and Shutler (1975:59); yet I discussed this date in detail (Green 1979:32) and gave good reasons for rejecting it.

In short, to support their particular interpretation of man’s settlement of the Pacific to fit the tooth-size data, Brace and

Hinton have grossly simplified matters and attributed to me positions I do not hold. My belief is that the "big-toothed" Fijians, Tongans, and Samoans may well prove to be not a result of sampling error at all, but the very kind of evidence that allows us to dismiss this particular two-migration-and-"mixing" explanation of the peopling of the Pacific as unsatisfactory even for tooth size in Oceania.

by EDWARD F. HARRIS

*Department of Orthodontics, College of Dentistry, 875 Union Ave., University of Tennessee, Memphis, Tenn. 38163, U.S.A.*  
2 IV 81

Brace and Hinton perform a useful task by integrating archeological and linguistic data with a physical record of the people themselves. Far too often the anthropological subdisciplines fail to bring the several kinds of anthropological information to bear on a specific problem. The present work does, however, contain assumptions and interpretations that I question:

1. The authors begin by disparaging the typologic goals of early researchers, even though these are now only of historical interest. Yet the gist of their argument is simply that big-toothed peoples were the first to migrate into Oceania, followed comparatively recently by proto-Polynesians. This is certainly not a new scenario, except that it had not previously been shown that summed tooth area (*TS*) coincides (rather roughly, I think) with this two-wave scheme.

2. They dismiss the extensive biochemical data available for the Pacific as of unknown adaptive significance, thereby confusing "unknown" with "no" adaptive importance. If they choose to focus on a single datum (*TS*) this may be defensible, but perhaps they do protest too much about the relevance of other biological information.

3. They also eschew the multivariate approach as "less than fully satisfying," even though interpretations of their own data are in concert with the comprehensive anthropometric study by Howells (1970). One prime exception is that they fail (by commission?) to discuss Micronesia in their interpretation of the protracted and heroic Polynesian migrations. Their one Micronesian sample (Guam, *TS* = 1,309) fits very poorly with the single-minded west-to-east movement of proto-Polynesians they claim (figs. 7 and 8).

4. They state that *TS* is of "obvious adaptive value," but this is unsubstantiated. "Obvious" and "commonsense" claims have been proved wrong too often to give them much credence. What is the evidence in hominids that tooth size enhances longevity, contribution to subsequent generations, access to mate(s), or other features demonstrably linked to evolutionary success? The authors may claim a "commitment to evolutionary mechanics," but I cannot find any hint of evolutionary process in their analysis or conclusions.

5. The fossil evidence does strongly suggest that earlier inhabitants of Southeast Asia had larger teeth than more recent (Austronesian) immigrants. It should be noted that von Koenigswald's (1952) description of this difference anticipated Brace and Hinton's by some three decades (also see Haddon 1923, Weidenreich 1945). It also warrants emphasizing that the temporal differences in tooth size (fig. 1) are not, as implied, evolutionary changes toward size reduction. Instead, they reflect the physical displacement (replacement?) of large-toothed Mesolithic peoples by genetically and culturally different, comparatively recent groups with smaller teeth (e.g., von Koenigswald 1952; Coon 1962, 1965; Swindler 1962).

6. The preferred use of just one variable (*TS*) contrasts sharply with current trends in synchronic and diachronic lineage reconstructions. There are innumerable ways two groups can differ in the size and proportionality of individual tooth types while exhibiting the same *TS*. Proportionality (shape) has repeatedly been shown to be a more pertinent measure of group

relationship than size (e.g., Penrose 1954, Spielman 1973, Corruccini 1973).

The converse is also true: a difference in *TS* does not necessarily indicate a relevant phyletic difference. As just one example, the use of *TS* by itself ignores developmental (nutritional) and physiological considerations of body size. As was pointed out some time ago (Flower 1885), not only are the teeth of Tasmanians, Australians, and Melanesians absolutely large, they are *disproportionately* large relative to a measure of skull size.

*TS* cannot distinguish between groups possessing large teeth because they are large overall and groups that have disproportionately large teeth. Stature and other measures of body size vary considerably in Southeast Asia and Oceania, and the obvious consideration here is the authors' invocation of "inadequate sample size" to explain away the large teeth of Tongans (*TS* = 1,371) and of Samoans (1,311). Data on stature (Howells 1970) show that Tongans and Samoans—along with adjacent Polynesian groups in the Cook and Society Islands—are among the tallest (largest) peoples in the Pacific (ca. 171 cm). It is, then, wholly predictable that they would have larger teeth, no matter what damage this inflicts on the authors' tenuous east-west *TS* cline (fig. 6). Moreover, this problem highlights a major weakness in the authors' reconstruction, namely, their disregard of within-group variability and of in situ adaptations through time.

by ALEX JACOBSON

*Department of Orthodontics, University of Alabama School of Dentistry, 1919 7th Ave. South, Birmingham, Ala. 35294, U.S.A.* 11 III 81

The study of tooth size has proven useful in analyzing hominid evolution and is a recognized method of differentiating between populations. Reduction in tooth size has been used in this study to trace the migration of people settling in the larger and smaller islands of Oceania. Tooth size, according to the writers, provides a good index of the extent of the mingling. They are careful to mention that tooth-size figures are suggestive rather than substantive.

Part of the tooth-size discrepancy is attributed to the development of food-preparation technology, proliferation of cutting tools, and, most important, heated-stone cookery rather than dietary change. It is maintained that relaxation of the selective pressures maintaining a large dentition created the conditions for the operation of the probable mutation effect of dental size reduction. In principle, this deduction is reasonable, but time relative to additional dietary information may need to be considered.

Cooking has long been practiced by many primitive populations. Possibly a distinction should be made between primitive and refined diets. There are numerous examples of isolated populations that have access to cutting tools and cooking utensils but are still required to use their teeth to masticate food which is unrefined. Such groups generally have large, well-formed jaws and robust teeth. Where such groups are introduced to "modern" diets comprising refined sugars and carbohydrates, a notable almost immediate change is a high incidence of caries and periodontal disease. Within a single generation, individuals subsisting on such a diet exhibit marked reduction in jaw size and generalized overcrowding of the dental arches because of the lack of functional demands upon the jaws. Tooth dimensions, on the other hand, do not respond as rapidly to lack of functional demands as does skeletal morphology.

The article is fascinating and represents a tremendous endeavour on the part of the investigators to disentangle, by deductive reasoning, the mingling of the populations of that area. Deciphering the course of evolution of any area is often a

Herculean task. What an advance it would be if the combined input of physical and social anthropologists, linguists, archaeologists, historians, and geneticists could be synthesized to formulate a single hypothesis in such studies!

by CHRISTOPHER MEIKLEJOHN

*Department of Anthropology, University of Winnipeg, Winnipeg, Man., Canada R3B 2E9. 14 iv 81*

This is a fascinating follow-up to Brace's (1980) article and deals with an equally formidable problem. The model is one against which further data can be tested, and this is most positive. Though I claim no expertise in the prehistory of the Pacific basin and therefore cannot comment on the full implications of the historical reconstruction, I would like to make some comments on the general approach.

While the discussion of original "stocks" at the beginning of the paper seems to me essentially a straw man, more might have been said about the myth of primitive isolates, especially with reference to models spanning more than a few generations. There has been a tendency in the literature to view small-scale preagricultural societies as true isolates. This conflicts with the evidence that there is a clear minimum size below which populations must maintain links with other groups in order to obtain marriage partners of suitable social categories (MacCluer and Dyke 1976, Wobst 1976). The result is that small groups are perforce subject to strong gene flow. Effective isolation by distance is possible only in reasonably dense population systems. It might therefore be argued that the cline of variation depicted in fig. 8 between the postulated areas of earlier and later immigration results from recent contact. Longer-term contact would have extended the zone of differentiation considerably farther, given the efficacy of gene flow (Brues 1972). This would add weight to the general conclusions of the article. On the other hand, I am not convinced that the authors have answered the general problem I raised in regard to Brace's earlier paper (Meiklejohn 1980). In essence, the tooth-size data are interpreted in terms of cultural and biological mixture. They could, however, be interpreted as a strong local cline mediated by selection alone. While I believe that the archaeological and linguistic evidence as currently understood would support Brace and Hinton, I question whether their model can stand only on the biological data offered here. This is a matter which deserves further work.

by YUJI MIZOGUCHI

*Department of Anthropology, National Science Museum, 3-23-1 Hyakunin-cho, Shinjuku-ku, Tokyo 160, Japan. 2 iv 81*

Part of the hypothesis proposed here on the origins of the diverse Oceanic peoples may be consistent with the suggestion by Yamaguchi (1967), on the basis of osteological data, that the Australian Aborigines were derived from a generalized Upper Paleolithic or Mesolithic population in Asia or with the report of Riesenfeld (1956) that a Mongoloid cline from Indonesia through Micronesia to Polynesia is confirmed by a west-to-east cline in the frequency of shovel-shaped incisors and reduced lateral incisors. However, there are some weaknesses in this article, especially in data management.

The authors themselves note the risks associated with the use of small samples and samples not segregated by sex. However, there are two further questions about their data processing. One is that it is unclear what kind of materials they used. If the specimens were plaster casts or the like obtained from the living, I wonder whether they were able to measure the buccolingual diameters of tooth crowns, especially in anterior teeth, which in many cases cannot be measured because of the supporting gums. The other question concerns the data on third molars. For both Mongoloids and Australoids it has been re-

ported that there is a tendency for the third molars to be congenitally absent (Coon 1962). If so, mean values estimated from erupted third molars alone would not be good estimates of the population means. Either third molars should be excluded from the analysis or the results should be corrected taking the frequency of third-molar agenesis into account.

The authors point out two drawbacks of their summary tooth-size figure concerning the relative contributions to it of front and back teeth and its variance. The former is surely serious in assessing differences among populations. The difference between two summary tooth-size figures has essentially the same meaning as Penrose's (1954) size distance, which is empirically known to be less powerful in classifying populations than Penrose's shape distance. It seems to me that they should use a distance or similarity coefficient such as Penrose's shape distance for detecting the difference in proportions of various traits between two populations. Using the summary tooth-size figure alone robs the teeth of some of their "evident" adaptive significance.

The concept of mixture of peoples appears to play an important role in the present attempt to explain the diversity of the peoples of Oceania today. In Japan, there is evidence that various traits, especially of the facial skeleton, have changed remarkably over the last 1,000 years, some, for example, clearly decreasing in size and then increasing again (Suzuki 1969). Such phenomena may have occurred also in Oceania. If so, the problem of mixture should be more carefully dealt with on the basis of many more prehistoric data for each place.

Finally, I disagree with the authors about Brace's (1963) probable mutation effect. To be sure, a certain character might be reduced by the probable mutation effect if it were completely independent of all other characters in an organism, but, in fact, it seems most likely that many characters are manifested by interactive factors through the ontogenetic process. In this process, some characters can probably become an environment partially controlling the formation of others (Stern 1960). Under these circumstances, it seems unlikely that the probable mutation effect simply causes structural reduction, such as dental reduction, which may be explained by a series of changes in all the relevant characters originally derived from mutation and the subsequent selection.

by SHAO XIANG-QING

*Section of Anthropology, Fu-Dan University, Shanghai, People's Republic of China. 31 iii 81*

Anthropology's subdivisions—physical anthropology, cultural anthropology, archaeology, etc.—are seemingly not very closely linked with one another, forming more or less independent courses in the university curriculum. Brace and Hinton's work, however, is especially interesting in that physical anthropology and cultural anthropology are ingeniously connected; it is valuable in bridging the gap between physical and cultural anthropology.

Cultural factors evidently influence physical traits. It is quite certain that cooking technique improvement resulted in the reduction of the size of the teeth. Since the Neolithic stage, dental reduction has been more significant in southern China because of the consistent development of cooking techniques in this region.

Teeth are the hardest tissue of the human body, readily preserved and highly resistant to chemical degradation. They sensitively reflect personal, sex, and racial characters (as evidenced by shovel-shaped incisors). In the long human-evolutionary process, teeth are the most important organ for elucidating evolutionary laws, as for example in the cingulum on the neck of the teeth and the wrinkles on the occlusive surface; thus the teeth are certainly the ideal material for anthropological research.

However, I wish to make the following comments on Brace and Hinton's paper:

The authors have combined the male and female data and drawn mean values from these pooled data, but strictly speaking the male and female data shown belong to two different populations.

Since the data on China were obtained only from Chinese residing in Hong Kong, I would not think this a random sample taken from all over China; therefore the data cannot be representative of all China.

I think that, in table 1, figures for the average, standard deviation, standard error, minimum, maximum, range, and coefficient of variation of tooth size would be useful.

Finally, it seems to me that in measuring the crown height of human teeth, it would be better to use the volume of the tooth rather than the cross-sectional area if the tooth material is available.

by PATRICIA SMITH

*Department of Anatomy and Embryology, Hebrew University-Hadassah Medical School, P.O.B. 1172, Jerusalem, Israel.*  
31 III 81

I agree with Brace and Hinton that tooth size is related to selective pressures and decreases rapidly when conditions permit, but I would argue that the rate of reduction observed suggests that this may be due to positive selection for small teeth rather than chance reduction through the probable mutation effect. Tooth size in all mammals shows a very close relation to functional needs, and the "need" for adequate tooth mass may be finely balanced against the heavy calcium demands made by the developing foetus on its parent.

I disagree with the assertion that tooth-size reduction from the Middle Pleistocene on is attributable to changes in the toolkit, with concomitant changes in food resources utilised. Do Brace and Hinton seriously consider that the presence of Middle-Pleistocene-sized teeth in recent populations as diverse as Amazonian Indians and Australian Aborigines means that they have Middle Pleistocene technologies? They further quote Freyer's (1977) data showing Upper Paleolithic tooth-size reduction related to changes in tool technology but ignore the point that Freyer went to considerable lengths to explain—that the changes in the toolkit made available a wide range of new food resources obtainable by netting, spearing, etc.

Since tooth size does reflect functional demands on the dentition and these may vary with local adaptations, tooth sizes in populations that are "biologically" closely related may differ, while populations living in different parts of the globe, differing in skin color, hair texture, and body and facial proportions, have teeth of similar dimensions (Smith 1979). I would therefore question the rationale for using tooth size alone as a measure of "biological and cultural mixing" over a large area. More specifically, I fail to understand how Brace and Hinton can use similarities in tooth size between Mesolithic Indonesians and Recent Australians as a basis for inferring similarities of "facial contours, skin color, and even hair texture." This argument, weak in itself, omits any mention of Mesolithic Australians (Roonka, Cohuna, Mossiel, Lake Nitchie), who presumably were more likely to be related to Recent Australians than were Mesolithic Indonesians but differed from them even in tooth size (Smith 1979).

The validity of the measure of difference in tooth size, considered "biologically significant," has been queried before in CURRENT ANTHROPOLOGY on the grounds that the measure used is less than the measure of intrapopulation variation; I shall not repeat these arguments here (Smith 1980, Brown and Townsend 1980). I do, however, feel it necessary to question Brace and Hinton's explanation that teeth in the Tongan sample were large because they came from individuals who were exception-

ally large. To the best of my knowledge there is no evidence of a correlation between tooth size and body size in *Homo sapiens* (Garn, Lewis, and Kerewsky 1968, Anderson, Thompson, and Popovich 1975).

Finally, Morton's explanation of differences in Pacific Islanders as due to genetic drift is ridiculed, but his basis for doing so—namely, the relatively short period of occupation of these islands, by closely related groups of similar technical status and life-style, permitting the assumption that differences in selective pressures were minimal—is omitted.

In conclusion, many of the statements in this article should be viewed as representing a rather one-sided point of view.

by RICHARD J. SMITH

*Department of Orthodontics, Dental School, University of Maryland, Baltimore, Md. 21201, U.S.A.* 30 III 81

Brace and Hinton's interpretation of Oceanic prehistory can be no better than the data around which it is built, measurements of tooth size. In a previous article, Brace (1980) used the same type of tooth measurements. Comments by Brown and Townsend (1980), Koritzer (1980), Preston (1980), and St. Hoyme (1980) at that time pointed out a number of problems, also relevant to the present study, which I believe seriously compromise the type of analysis attempted here.

In addition to the concerns raised earlier, there are a few additional problems to be discussed. After arguing that the cross-sectional area of teeth is a reflection of biological function and has adaptive significance, Brace and Hinton then go on to treat their summary measurement as if similarities and differences between populations could be explained *exclusively* by dietary function (including food-preparation techniques). But whether or not genetic drift or founder effects are important, it is clear that some variation in human tooth size is related to variation in body size (Garn, Lewis, and Kerewsky 1968, Henderson and Corruccini 1976, Lavelle 1977) and that selection for tooth size may result from pleiotropic effects on selection for body size (Leamy and Bader 1970, Leamy 1978). Lande (1979) has demonstrated that a trait of probably even greater adaptive significance than tooth size, namely, brain size, may vary among subspecies predominantly as a result of selection for body size.

An equally important weakness of this study stems from the use of a single surface-area measurement to summarize the entire dentition. Although acknowledging some of the simplifying assumptions involved (and Brace has worked on these problems elsewhere), in practice Brace and Hinton proceed by suggesting that their measurement is a very good one indeed, with "obvious adaptive value" and better than, for example, Howells's multivariate vectors for the skull. What is known of dental function, however, including theoretical (Lucas 1979), experimental (Yurkstas and Manly 1949, Crompton and Hiiemae 1969, Walker and Murray 1975, Sheine and Kay 1977), and comparative (Kay 1978) studies, demonstrates that complex occlusal features, rather than overall tooth size, are related to masticatory efficiency.

The combination of anterior teeth and posterior teeth in a single measure is also difficult to justify. Among primate species, anterior and posterior teeth seem to vary almost independently and are influenced by very different features of the diet. Almost without exception, recent workers interested in dental function have treated incisors, canines, and the posterior teeth as separate units (e.g., Gould 1975, Hylander 1975, Pirie 1978, Goldstein, Post, and Melnick 1978).

In summary, then, I could provide no more accurate assessment of the measurements used in this study than Brace and Hinton's own view of Howells's vectors: that they are "of un-

known adaptive significance or only partially and tangentially related to those aspects of morphology which really are under the control of selection." The fact that an ad hoc scenario for Oceanic colonization can be made compatible with these data should not be taken as providing confirming evidence for the validity of the results. Ad hoc explanations, as we should all know, can "make sense out of anything" (Merton 1968).

by J. SPECHT

*Australian Museum, P.O. Box A285, Sydney South, N.S.W. 2000, Australia. 28 III 81*

The authors use summary tooth size (*TS*) as an index of mixing between populations of two migrations into the Pacific Islands: the first, with high *TS* values, in the Pleistocene, the second, with low *TS* values, in the mid-Recent. The Pleistocene settlement of New Guinea is well attested, but for island Melanesia the authors have had to rely on circumstantial evidence and a dubious set of  $C^{14}$  determinations from New Caledonia (White 1979). They can now cite a firm  $C^{14}$  date of  $11,300 \pm 1,200 - 1,100$  years B.P. (SUA-1490) from a cave site in western New Britain (Specht, Lilley, and Normu n.d.). They should also note that the Balof shelter on New Ireland is dated  $\geq 6,800 \pm$  and not 6,000 B.P. (White, Downie, and Ambrose 1978). A greater time depth is likely, but there is no evidence to support the 40,000 years mentioned in the paper. In some respects, a shortened time depth would support the Brace-Hinton suggestion that the reason high *TS* values have been retained is, in part at least, too little time for selection pressures to operate. This, however, raises a distinct problem for the New Guinea highlands.

If I understand the tooth-size reduction theory correctly, reduction of selective pressures for large total tooth areas was consequent on changes of tooth functions, particularly dietary and food-preparation changes. I do not see what changes in either aspect took place so much earlier in Asia than in New Guinea as to permit the reduction of *TS* values. Dietary change associated with the development of cultivation was probably as much in the frequency and volume in which a food was consumed as it was in the nature of that food. Moreover, there is no evidence that formal gardening began later in New Guinea than on the Asian mainland; instead, the earliest evidence from the New Guinea highlands dates to the immediate post-Pleistocene (Golson and Hughes 1976). As for food-preparation techniques, inadequacy of evidence prevents detailed comments, though the first presence of pottery in Japan seems to be much earlier than is yet known from New Guinea. The earth oven, however, has a wide currency in the Pacific, and food prepared in this way is in no way more difficult to consume than that cooked in pots.

As presented, the Brace-Hinton hypothesis on "mixing" of populations and languages in Melanesia assumes initial settlement by non-Austronesian (NAN)-speakers followed by Austronesian (AN)-speakers at a later date. The mixed populations are on New Guinea and in island Melanesia. Even Capell (1962) accepted most of these as normal AN languages and not as "aberrant." There may indeed be mixed AN-NAN languages, but they remain to be demonstrated (e.g., Lincoln 1976); "aberrant" languages are as easily explained in terms of great time depth.

The Brace-Hinton hypothesis is clearly linked with Lapita pottery and, by implication, with Eastern Oceanic languages, yet some EO speakers (e.g., Fiji, Tonga, Samoa) have high *TS* values said to result from "Melanesian influence" following initial settlement of those islands. If the Hawaiian, Marquesan, and Moriori *TS* values are correct (all 1,204 or less), similar values should be found in western Polynesia. Instead, we are asked, in effect, to accept that "Melanesian influence" caused

*TS* to rise dramatically in 2,000–3,000 years; the implication is that high *TS*, dark skin, and curly hair are dominant over low *TS*, lighter skin, and straight hair. Alternatively, we accept the Brace-Hinton suggestion that the high values for western Polynesia result from biased samples; all exceptionally large males of elite status. There is no evidence that elite status and *TS* are in any way linked.

There is also no evidence to support the claim that the first settlers of Melanesia (by implication, NAN-speakers) were utilizing "all the major tracts of land" to "full capacity" for gardening, forcing later AN immigrants to settle "islets."

The evidence cited from island Melanesia for pre-Lapita (pre-AN?) settlement is dubious. Balof shelter could have been settled by AN-speakers (admittedly not Lapita); if the dubious New Caledonian tumuli dates are discounted, none of the evidence cited by Brace and Hinton clearly pre-dates Lapita. I am not against the idea of pre-Lapita settlement of island Melanesia, but at present, south of the Bismarck Archipelago, the archaeological evidence is weak and certainly does not support the idea of NAN-speakers' preceding AN-speakers.

The authors rightly note the impossibility of identifying the languages formerly spoken by those whose skulls now grace many a museum shelf. This immediately weakens the value of the New Britain sample, which, Brace and Hinton admit, is poorly provenanced. If these skulls do indeed come mainly from the Rabaul area, they could be of AN-speaking Tolai, of NAN-speaking Baining, of one or more other NAN groups of the Gazelle Peninsula, or from any combination of these; they are certainly not firm evidence for "mixing." A similar problem surrounds the "New Guinea North Coast" sample.

Finally, there is a problem of categories and typologies. The distinction between high and low *TS*, apparently linked with other phenotypic differences, is a variant on the idea of discrete "stocks" or "races," admittedly improved by the addition of "evolutionary mechanics." It does not follow that there were similar dichotomies in language and material culture. I see no a priori grounds for assuming formerly distinct cultural and/or phenotypic categories whose boundaries were subsequently blurred by "mixing." Is there no room for some degree of initial cultural diversity, for local development and change? I prefer Groube's (1971) masterly appreciation of the "problem" of Polynesian origins to the restricted, almost static views of Brace and Hinton.

by JOHN TERRELL

*Field Museum of Natural History, Chicago, Ill. 60605, U.S.A. 9 III 81*

Brace and Hinton seem embarrassed that their ideas are so old-fashioned, in spite of their "commitment to evolutionary mechanics." Their paper contains little fact, but this deficiency evidently is unimportant; when evidence (e.g., for Samoa and Tonga) disagrees with their ideas, they brush aside the discrepancy with the well-worn broom called "sample size." Simply that "most scholars agree" with some of the ingredients Brace and Hinton mix together in their scenario for the settlement history of the Pacific does not mean that the interpretations they select are necessarily unfalsifiable or true.

Some observations: (1) By circular reasoning Brace and Hinton assert but do not demonstrate that innovations in food-preparation technology have slackened the selective pressures formerly maintaining a large human dentition. In their argument, the initial idea that "the large faces and teeth of highland New Guinea and Murray Basin Australia may just indicate the relative recency of the arrival of such culinary refinement" thereafter becomes accepted fact. When convenient to their argument, they wave the magic wand called "the differential operation of selective forces," and lo, supposition

is transmuted into fact. (2) Whatever Lapita pottery has to tell us, it doesn't tell us about the "beginning" of the alleged spread of "Proto-Austronesian society" towards the eastern Pacific. (3) The biological characteristics of the people of small-island Oceania have so far told us nothing much about their "locus of origin." The best that anyone can say, for instance, is that Polynesians look "too different" to be related to Melanesians by common origin rather than admixture; however, such statements are not scientific arguments, no matter who makes them. (4) Even if we did know enough about the selective forces contributing to skin color, in hauling out long-distance migration and subsequent mixing of "stocks" Brace and Hinton contradict themselves when they claim that "relative depigmentation can only be explained by the shaping effects of selective forces. . . ." (5) That Shanfield, Curtain, and others have said *Gm* frequencies can be used to differentiate Austronesian-speakers in the Pacific from non-Austronesian-speakers does not mean no one has cried "Balderdash!" (see Howells 1976, Terrell and Fagan 1975, Terrell n.d.).

I suspect that the majority of linguists aren't going to be happy about Brace and Hinton's language-mixing ideas: this is talk that gives physical anthropologists a bad name in linguistic circles. More to the point, however, if one examines what linguists working in the Pacific actually can say, one finds the following: (1) Contrary to the common assertion by linguists and others, the antiquity of the so-called Austronesian and so-called non-Austronesian languages is unknown; what little evidence there is (e.g., Kruskal, Dyen, and Black 1971) suggests that the Austronesian languages could be anywhere from 5,000–6,000 to possibly 35,000 or more years old. (2) The Austronesian and non-Austronesian language classes are polythetic and nonrigorous. (3) The languages within each class are very diverse. (4) As the history of work on the Indo-European family shows, all languages probably do change over time, but they do not necessarily diverge from each other. (5) Hence it is hardly irrelevant that languages in both classes are spoken by speech communities differing in size, demographic parameters, comparative isolation from each other, etc.

Without being able to go into detail here, I suggest that these observations may have the following consequences: (1) That non-Austronesian and Austronesian languages differ need not mean they have separate origins (at present, a language is called Austronesian if it is judged similar enough to languages already called Austronesian, i.e., *the set of unique linguistic features allowing us to assign a language unambiguously to one class or the other is unknown*). (2) If it is true that the Austronesian and non-Austronesian languages have a common origin, then the hypothesis that the diversity within Pacific languages (that is, the "typical Austronesian," "atypical Austronesian," and "non-Austronesian" subclasses) has developed within the area of their present distribution is more parsimonious than hypotheses—such as the one favored by Brace and Hinton—calling for long-distance migrations and subsequent large-scale genetic intermixing.

In any science worthy of the name, one must consider (even if the facts are too few to falsify) the null hypothesis that the differences thought to exist—judged in the present instance by a "kind of informal index"—are not real, i.e., are not due to the cause or causes hypothesized. Brace and Hinton should have considered the null hypothesis not only because doing so is good science, but also because in this case the null hypothesis may happen to be right: the so-called Austronesian and non-Austronesian "peoples" may be "nothing more than fictions created by linguistic taxonomy" (Terrell and Fagan 1975:8). And if this is so, then the division of the settlement history of the Pacific into two linguistic periods or "movements"—an early period sometimes called "pre-Austronesian" and a later period marked by the incursion of Austronesian-speaking peoples—is meaningless.

by J. PETER WHITE

*Department of Anthropology, University of Sydney, Sydney, N.S.W. 2006, Australia. 5 III 81*

I have a number of problems with this article.

1. It shows a limited understanding of Oceanic culture history. Bellwood's (1979) dual-migration model is not the most commonly accepted one (cf., e.g., Green 1976, 1979; White and Allen 1980), and the suggestion that the peopling of the Pacific required "many thousands of voyages" would make most prehistorians reach for their calculators. West of the New Hebrides, the correlation between Lapita pottery makers and Austronesian-language-speakers is clearly unacceptable (Green 1979:47), while to bring the Pacific islands' inhabitants from Asia is to rely on no direct evidence whatever.

2. Brace and Hinton say that "long-standing selective pressures" in the northern Old World led to systematic geographical variation in tooth size. They provide no evidence at all for either tooth-size variation (cf. Brace 1978:214) or selective pressures deriving from "elaborate" (or any other) food processing in eastern Asia over the last 100,000 years. The argument they use looks suspiciously circular.

3. They ignore completely the data that "elaborate food processing" in the form of seed grinding existed in Australia from at least 17,000 years ago (Kamminga and Allen 1973; Allen 1974:315) and that by their model there should be some distinction in tooth size between teeth much older and much younger than that date. At present the major reduction in sizes of mandibles and teeth occurs in Australian populations around 6,000 years ago (P. Brown, personal communication) and cannot be linked with any such change in technology as proposed by Brace and Hinton.

4. A commitment to "evolutionary mechanics" is claimed, yet the paper does not consider the possibility that selective pressures other than food processing, which is surely not the only one affecting tooth size, may have altered tooth sizes in Oceania over the last 4,000 years. Brace and Hinton are forced into a special-pleading position to explain tooth-size indices from Tonga and Samoa. They explain the large index from Fiji in terms of mixing but do not explain why even more "mixed" populations of island Melanesia have smaller indices. Only three of five samples derived from original Austronesian-speaking populations support their proposition—always assuming that samples are not contaminated by post-European contact mixture.

5. If one reorders the Asian mainland and Island data in table 1 into approximate chronological order, there is a decline in *TS* of about 150 between the oldest and the most recent. Since these data span at the most 10,000 years, we need to have an explanation for this "short-term" variation before accepting the much shakier long-term construct.

This paper seems to me to be just as "old-fashioned" as the ones it derides (cf. Terrell and Fagan 1975), and one must agree with the wisdom of the NSF in refusing to support such research.

## Reply

by C. L. BRACE and R. J. HINTON

*Ann Arbor, Mich., U.S.A. 15 v 81*

Our critics appear to be out in force, although for rather different reasons. Some, represented especially by Green, have registered varying degrees of alarm at our efforts to simplify what they prefer to regard as a much more complicated picture. Others, especially Terrell, seem to feel that an even



simpler model is to be preferred and that our efforts would only make things needlessly complex. Both of these reactions, and some of the other criticisms as well, reflect limitations in perspective due, on the one hand, to the commendable if restricting refusal to deal with material from beyond the area of their own personally conducted fieldwork and, on the other hand, to a kind of intellectual intransigence that seems more sui generis than related to an effort to produce plausible interpretations from real data.

The most thoughtful general critique is Meiklejohn's, expanding his comment on the interpretation of tooth-size clines in Australia (Meiklejohn 1980). We do not understand his suggestion that our discussion of "stocks" is a straw man. As we noted, the traditional explanations of the appearance of modern regional populations in terms of the mixing of different original stocks are unjustified in most instances; the case of the peopling of the islands of Oceania, however, is one of the very few in which such a model is the most useful way of looking at things. The argument in question, then, was not set up to be demolished, but quite the opposite. It does not work for Australia, and this is why the Australian and Oceanic cases were the subject of separate papers. Since our most outspoken critics deny that we are dealing with facts when it would appear that they simply have neglected to check them, we shall recapitulate in an effort to reconcile both instances with the larger framework.

By the end of the last interglacial, human populations everywhere had achieved a specific identity as *Homo sapiens*, although none presented an appearance that we would characterize as "modern." Middle Pleistocene, i.e., *erectus*, standards of skeletal robustness and muscularity still prevailed while brain size had attained its modern level. The first such representative to have been found was at the Neanderthal in 1856, and some have felt that it would be convenient to use that name to refer to all archaic *H. sapiens* where a Middle Pleistocene physique was retained but the brain was modern in size. This was what was meant by referring to a Neanderthal stage or grade of human evolution (Brace 1964a, 1979c, 1981, n.d.). Presumably the skeleto-muscular robustness was a reflection of the selective forces engendered by the hunting of a Pleistocene fauna with a relatively simple technology. The import of this has been treated in previous accounts (Brace 1979a; 1980a:160).

The original Neanderthal skeleton lacked jaws and teeth, and so did its counterparts in Asia, Solo in Java and Mapa and Dali in China (Weidenreich 1951, Wu 1981). The most extensive early Neanderthal material known is from Krapina in Yugoslavia. This has been considered in admirable detail (Gorjanović-Kramberger 1906, Smith 1976, Malez 1970, Wolpoff 1979), and it has been shown that the dentition as a whole was metrically not significantly different from that of *H. erectus* (Brace 1979b). The anterior teeth, however, are significantly ( $P \leq .001$ ) larger than those of any other hominid population, and this is what provides the basis for the concern expressed by Mizoguchi and R. Smith for the loss of information that may occur by lumping all the teeth of the dental arch into a single figure. As tooth size reduces in the Late Pleistocene, however, the discrepant emphasis on either anterior or posterior teeth largely disappears (Brace 1979b).

Since the Asian Neanderthal counterparts lack jaws and teeth and all we have to suggest that these were robust is the preservation of Middle Pleistocene levels of cranial vault thickness and browridge and neck-muscle attachment size, we must use Dali and Mapa as the terminus ante quem for the survival of Middle Pleistocene dento-facial robustness. If the *TS* figure for Zhou Kou Dian (Choukoutien), 1,578, really does apply, then reduction had to proceed at a rate of between 50 and 100 mm<sup>2</sup> per 10,000 years to produce what is visible in South China today. This of course is the parallel of what happened in Europe over the same period of time and, it has been suggested, for the same reasons (Brace 1978, 1979b).

By the end of the Pleistocene, summary tooth size in Europe had reduced to 1,237 mm<sup>2</sup> (from Frayer 1978), while in China it was evidently comparable. Weidenreich (1938-39) did not record tooth measurements for the individuals from the Upper Cave at Zhou Kou Dian, and the specimens were lost during the Japanese invasion of China in 1941 (Plumb 1952, Shapiro 1974). Excellent casts are preserved in the Institute of Vertebrate Paleontology and Paleoanthropology in Beijing, where, through the courtesy of Woo Ru-kang (Woo Ju-kang), the senior author was able to measure the complete male and two incomplete female dental arches in 1980. The pooled figure of 1,305 mm<sup>2</sup> suggests a reduction from Middle Pleistocene size quite comparable to that found in Europe.

Reduction did not stop with the end of the Pleistocene in the northern portions of the Old World, as is shown by the figure of 1,201 mm<sup>2</sup> for the English Neolithic and 1,120 mm<sup>2</sup> for the 17th-century inhabitants of London (Brace 1979b:543). The figure we cite for modern Hong Kong, 1,157 mm<sup>2</sup>, shows that a comparable amount of reduction had occurred in China. Shao Xiang-qing is quite right in noting, however, that Hong Kong cannot stand for all of China, and, as a result of his hospitality and assistance, the senior author was able to collect substantial numbers of modern and Neolithic Chinese tooth measurements in Shanghai in 1980. To these were added further modern and Neolithic Chinese measurements from the collections in the care of Woo Ru-kang in Beijing. A full treatment is inappropriate here, but we can at least note that our preliminary figures show that the modern Shanghai-Beijing gradient of 1,204 ( $N = 104-293$ ) to 1,263 ( $N = 12-35$ ) parallels in reduced form the Long Shan-Yang Shao gradient of 1,222 ( $N = 24-69$ ) to 1,278 ( $N = 2-14$ ).

These figures may change slightly when we finish analyzing the material collected, and the same will probably be true when our Japanese data are completely analyzed. Our preliminary figure for the late-to-latest Jomon, 1,149 mm<sup>2</sup> ( $N = 5-28$ ) (measured with the assistance of J. Ikeda in Kyoto and of H. Koike, K. Hanihara, Y. Mizoguchi, and B. Yamaguchi in Tokyo), shows that the beneficiaries of the oldest pottery tradition in the world show, as expected, a particularly marked degree of dental reduction. As Mizoguchi comments, there is evidence for a post-Jomon increase in Japanese face size. Our partially analyzed dental metrics concur with these observations, although, in contrast to the explanation favored by Suzuki (1969), our data suggest that this was not the result of in situ change but was caused by the influx of people with somewhat larger jaws and teeth as the Yayoi brought rice cultivation from the mainland. This, like the Oceanic situation, is one of those few instances in which recent human form has been materially influenced by migration and the subsequent mixing of populations with slightly different adaptive histories.

Even though there are meaningful gradients of tooth-size difference in China and Japan dating back to the time of the Chinese Neolithic, all the populations of mainland Asia after the end of the Pleistocene are characterized by summary tooth sizes that average less than 1,300 mm<sup>2</sup>. For the past 2,000 years, the coastal populations have averaged 1,200 mm<sup>2</sup> and even less. This is in dramatic contrast to the situation in Australia, where summary tooth size at the end of the Pleistocene averaged well over 1,500 mm<sup>2</sup> to nearly 1,600 mm<sup>2</sup>, exactly the same as that for *H. erectus* 500,000 years earlier (Brace 1980a:147). This, we maintain, is evidence that Middle Pleistocene kinds of selective forces continued to operate on the jaws and teeth of Australians until 10,000 years ago.

Subsequently the rules of the game changed completely. The largest recent dentitions in Australia were those of the people in the upper Murray River Basin, where tooth size averaged 50 to 100 mm<sup>2</sup> smaller than that of their predecessors in the same area 10,000 years earlier. It is reasonable to interpret this as change in situ because it represents precisely the same rate of change as that observed in situ in Europe since the rules of

the game evidently changed there at the onset of the last glaciation. Farther north in Australia, however, a degree of reduction is found that would have required more than twice the rate known from anywhere else in the world. The explanation offered is that this was accomplished in part by gene flow from the north, where reduction had previously been taking place.

This of course raises the questions of why reduction should occur and what causes it. The suggestion has been offered that this is the result of mutations that occur after the relaxation or suspension of the selective forces that had previously operated to maintain an adaptive state (Brace 1963). As one biologist put it, "The five billion nucleotides of a human cell are continuously being degraded by mutation, spontaneous or other. In a state of nature, natural selection just as continuously acts as a proof-reader, keeping the genetic message reasonably close to its proper meaning" (Hardin 1967:798). Perhaps the most striking consequence of developments in the cultural milieu since the Middle Pleistocene, however, is the increasing extent to which *H. sapiens* does not live simply "in a state of nature." Human ingenuity has increasingly set up barriers between the human phenotype and the selective forces that once impinged upon it (Post 1971). In his basic work establishing the theory of evolution, Darwin (1859:chap. 14) realized that the suspension of natural selection would lead to reduction, and various notables from Darwin himself (1871:161-62) to Muller (1960:433) and Julian Huxley (Whitman 1975) have warned in rather moralistic tones of the biological consequences to humankind of continued cultural development.

The consequences of selection relaxation in the non-value-laden realm of field and experimental zoology (Wilkins 1971) and molecular evolution (Kimura 1979) were anticipated in the development of the concept of the probable mutation effect. Mizoguchi has questioned this, using a variant of Wright's (1964) pleiotropy argument, and there are both theoretical (Brace 1964b, Wolpoff 1969) and practical (Garn 1977:61) answers to his concern. P. Smith has tried to view the observed reductions as consequences of positive selection created by the calcium demand of the developing fetus, but the crowns of the permanent teeth do not even start to calcify until a year and a half after birth. In fact, most of the dentition calcifies long after weaning (Moorrees, Fanning, and Hunt 1963). Many others have also objected, but no plausible alternative has been proposed short of invoking factors yet unknown (and perhaps unknowable). For the moment, then, we shall continue to note the operation of the probable mutation effect and take comfort from the fact that observers such as Jacobson accept it "in principle."

However much the dentition may be used as an ancillary tool, its principal function is as a food-processing device. Thus, as soon as technology takes over some of the role of processing food, there is a reduction in the selection maintaining the formerly necessary levels of dental development. R. Smith has suggested that food-processing capabilities are represented by occlusal features and not dental size, and this may very well be the case for insectivores, prosimians, and various other mammals; until recently, however, the dental occlusal surfaces of most hominids were worn away by adulthood, and the majority of the reproductive life-span was spent with a dentition that had a flat and featureless occlusal plane (Brace 1977). At this point selection relates solely to the amount of usable tooth substance, and this is what is recorded in the summary tooth-size figure. Ideally, as Shao has mentioned, crown height would improve our assessment of usable tooth substance, but the practical difficulties in the path of providing this and the other statistics he suggests are such that this simply cannot be done for the material available (Brace 1980a:143).

It has been suggested that the relaxation of the intensity of selection maintaining usable tooth size began with the adoption of earth-oven cookery (Brace 1978:214; 1979b:545-46; 1979c:

89-90; 1980a:150; Brace and Montagu 1977:335-36). As Specht remarks, food prepared in an earth oven does indeed approach that cooked in pottery for ease of consumption, although the soups and gruels that can sustain the edentulous cannot be so matched. We suggest that it is no accident that there are Jomon and Chinese Neolithic individuals in some number who had survived the complete loss of their teeth for so many years that alveolar resorption had modified the appearance of their mandibles to the point where all recognizable anatomical landmarks had disappeared. Nothing like this ever occurs in populations without the extensive use of container cookery, although in populations where earth-oven cookery was a regular practice individuals could survive for many years with varying degrees of tooth loss. The classic example is the European Neanderthal from La Chapelle-aux-Saints (Brace 1979b:545-46).

This brings up another point. Even if Specht is right in stressing the 9,000 B.P. antiquity of the propagation and preparation of plants in highland New Guinea, pottery did not get beyond the eastern edges of the highlands, and that only within the last 1,000 years (White 1971:50; White and Allen 1980:731). Furthermore, until the penetration of the sweet potato into the highlands within the last 400 years (Golson 1977:628), whether or not that signalled pig provisioning and rapid population growth (Watson 1977:60; White and Allen 1980:731), the probable subsistence crop was *Pueraria*, which requires a lot of cooking and even then tends to be "hard and woody" (Watson 1964:2). Such conditions do not indicate a marked relaxation in the selective forces maintaining large teeth. Certainly nothing indicates an antiquity for selective-force relaxation that goes tens of thousands of years back into the Pleistocene as is the case for the northern portions of the Old World.

From this, we would expect the amount of in situ dental reduction to be no more than 100 mm<sup>2</sup>, putting indigenous dental arches in the 1,400-1,500 mm<sup>2</sup> size range at least. Our figure for the eastern highlands, 1,395, very nearly qualifies, and, for the moment, we shall treat it as though it did. Elsewhere in New Guinea, however, tooth size becomes smaller as one approaches the coast, particularly in areas where Austro-nesian languages predominate. Of course, one could argue for the long-term reduction of selective forces in these areas, but even given the maximum allowable rate of 100 mm<sup>2</sup> per 10,000 years this would have taken between 30,000 and 40,000 years to accomplish, a span of time which does not fit with either the linguistic or the serological evidence and for which there is no archaeological evidence whatsoever.

We did support a date of more than 40,000 years for the initial spread of people into New Guinea and, by inference, the nearer large islands, but, contrary to Specht's reading, it was based on evidence from Australia (see Brace 1980a:147). (We do thank Specht, however, for catching our typographical error concerning the date of the Balof shelter on New Ireland.) Largely as a result of the absence of evidence to the contrary, we assume that these people were hunter-gatherers who lacked the kind of food-preparing technology that would have relaxed the selection maintaining large jaws and teeth, allowing these to reduce to their current size. Even though Specht prefers Fiji (Groube 1971) and Green prefers the Bismarcks as the locus for the shaping of the Polynesians, we suspect that neither would accept the view that the proto-Polynesians stayed put in their preferred cradle for 30,000 to 40,000 years while the requisite biological changes were accomplished.

It is the absence of any local evidence for the antiquity of different forms of food-preparation technology that leads us to reject Meiklejohn's suggestion of strong local clines maintained by selection alone. This is also what leads us to seek the source of modern Oceanic levels of dental reduction in areas where we

have evidence to support the view that conditions did operate in situ for a long enough period of time to produce the reductions observed.

The time framework we are talking about, the one needed to produce the differences between Murray Basin Australia or highland New Guinea and the condition represented by Hawaii and Japan, is a minimum of 30,000 years and in all probability double or more that amount of time. It is the perspective of this amount of time that led us to "conflate" Green's linguistic and archaeological steps. In an exchange of letters some years ago, Green conceded (July 9, 1976) that this approach could easily encompass his efforts while his more limited areal objectives could not encompass ours. This remains true, although in itself this does not increase the probability that we are right.

The influence of small sample size and unknown sex ratios remains a source of uncertainty in assessing the reliability of a summary tooth-size figure. For example, P. Smith (1980) noted the preponderance of males in the Australians from Broadbeach in southern Queensland. Recalculating the summary tooth size correcting for sex (Brace 1980c) brought the figure down to the level of the Murray Basin Aborigines, where initial expectations suggested that it belonged (Brace 1980a: 146, 151-52). When nine of the groups used in the present study were recalculated with corrections made for sex, we found that the uncorrected summary tooth-size figure averaged 20 mm<sup>2</sup> higher than the corrected mean. The overestimate ranged from 2 to 5 mm<sup>2</sup> for our Chinese groups to nearly 60 mm<sup>2</sup> for the Murray Basin Aborigines, where sexual dimorphism and tooth size were both at maximum levels (Brace and Ryan 1980). Factors such as this lead us to question the significance of figures that differ by less than about 100 mm<sup>2</sup>. Where Austronesian groups with an *N* of over a dozen approach or exceed the 1,300 mm<sup>2</sup> level, however, we suspect the influence of something more than just uncorrected sex ratios. Body size may indeed have something to do with it, although it is curious to see R. Smith make this suggestion citing the same source that P. Smith uses to defend the lack of relation between tooth and body size. The most definitive work on the subject concludes that "it is clear that taller parents do have children with systematically larger mesiodistal and buccolingual dental crown dimensions. This was true for all classes of teeth and for both jaws" (Garn, Lewis, and Walenga 1968:1197). As Harris notes, a consideration of stature might well bolster our case, but most of our figures were compiled from the teeth of crania which did not have associated long bones. Even if we were to take the available estimates into account, this would not explain the discrepancy between Tonga and Hawaii. Tonga remains the one major contradiction to our general model, but we included it in order to show that there is still much to be accounted for. Is this the exception that proves the rule, or does it really disprove it?

Finally, we have to consider the phenomenon of Terrell attempting to play Tweedledum to his own Tweedledee. It would seem that he has agreed to have a battle even though he does not really know whether or not the available data have spoiled his nice new null hypothesis. He does suggest that our approach is the kind of thing that gives physical anthropologists a bad name in linguistic circles. We were quite conscious of violating Howells's (1973:95) caution that "a physical anthropologist walks softly among linguists," and this is why the first version was subjected to a trial run at a conference of Austronesian linguists (Brace 1980b). Terrell, however, in his prolonged and thunderous "Balderdash!" (Terrell and Fagan 1975), engages in the kind of talk that would give archaeologists a bad name among physical anthropologists if there were others who behaved in such a fashion. There is a great deal of information on immunoglobulin haplotype frequencies and their distributions, and the statistical significance of their associations cannot simply be dismissed with an angry

epithet. Furthermore, there is evidence that variation at the immunoglobulin locus, unlike some other aspects of human genetic variation studied in Oceania, is under intense selection in certain areas (Schanfield 1975, Schanfield, Wells, and Fudenberg 1979). The situation is not clarified by the mere wielding of abusive verbiage.

There is another test of our views that shows considerable promise of providing independent confirmation. This involves the quantitative treatment of various nonmetric aspects of dental morphology (Turner and Swindler 1978, Levy 1981). This, however, merely indicates one of the directions in which further work can be pursued. As Brown has aptly indicated, our conclusions must remain tentative. At least we have laid out a hypothesis, even if it is hardly a new one, and taken some first steps towards providing the data and the rationale with which it can be tested. There are many other dimensions that have not even been mentioned, and we hope that others will be stimulated by this to rise to the challenge.

Although it may not meet with White's approval, the Committee on Scholarly Cooperation with the People's Republic of China of the U.S. National Academy of Sciences and the Chinese Academy of Science contravened some of the wisdom of the National Science Foundation by providing support for the collection of some of the additional data reported here.

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