THE TWO FACES OF PHYSICAL ANTHROPOLOGY

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Friends of Juan Comas:

Like the Roman God Janus, physical anthropology —the study of human biological variation— looks in two directions at the same time.

We can do no more to honor the memory of Professor Juan Comas and we would do no less than to be votaries of physical anthropology —to follow it where it leads along roads sometimes only roughly laid out during Comas' lifetime and only vaguely seen and followed in our own. It is not the results of specific studies that Comas would have valued most highly, but the definition of problems, the development and systematization of appropriate methods, and the accumulation of data to build up a body of knowledge on the subject.

One might think that my reference to the two-faced Roman idol is an allusion to looking at the past and looking at the future. Indeed it is true that physical anthropology often turns to the past —whether to the long span of time of human evolution or to the relatively more recent peoples who developed these valleys, hoed their milpas on the slopes, built their pueblos where they found water and left their bones where the skilled archeologists of Mexico have found them.

And anthropology looks to the future. The knowledge of past human adaptation, how *Homo sapiens* developed and spread over the habitable earth, shows us what we can expect or at least hope for as peoples meet and mix. Maladaptation, too, evidenced in human misery and disease and crowded slums has both biological and social consequences for the future that we wish to avert. In the unearthed skeletons and obsidian arrow and atlatl points we see the results of battles and human sacrifice. They barely hint at the destructiveness of the armaments of warfare that mankind has invented and that now can be unleashed.

The eyes to the past and the eyes to the future are not the two faces of which I wish to speak today, however. Our idol, Janus,

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faces in two other directions. They are exemplified in two of the many topics that focused Juan Comas' scholarly interest. On the one side he looked at the inborn, the genetic: the peoples of this country and others, their innate differences and similarities and the anthropometric measurement of the variation. On the other side he faced the social problem of racism, recognizing the labile nature of people, and their ability to respond to opportunities. If the inherent biology of people varies, how can we say that all are equal? It is an expression of faith. Individuals will demonstrate their special virtues only if opportunity arises and equality of opportunity is the best route for the whole species to find and utilize its strength. This is not a hollow motto. Many many studies demonstrate the range of variation of biological traits in every population studied. There are differences in innocent characteristics such as the form of the hair —straight or curly— or the blood groups of the red cells, A, B, O, or AB. Some of these differences are also true of whole groups of people, the usually straight hair and high frequency of blood-group O of some Amerindian populations versus the curly or woolley hair and different distribution of blood group frequencies in Africa, for instance. Furthermore, there are also functional differences among individuals and perhaps between groups. Cold adaptation in the arctic may be best achieved by peoples whose ancestors as well as themselves have lived there. Adaptation to high altitudes in the Andes seems to be better achieved by the highland Quechua and Aymara than by those who have come there recently. The problem is complex because the environment itself can stimulate the very biological traits that yield the adaptations (sweating mechanisms in response to heat and a large respiratory capacity in response to high altitudes, for example). To what extent is the capacity for adaptation achieved in response to the environmental challenge during growth and to what extent is it inborn? For instance, would those of us born near sea level have adapted better to high altitude had we been raised there from an early age? Or, with our inheritance, is the response of a few weeks residence in the mountains all that can be had? It is difficult to study what might have been had one's life been different. Nevertheless there are appropriate strategies by which these questions about human adaptation can be approached. Basically, to study the significance of genetic differences one needs to examine genetically different individuals in the same situations. To study environmental influences one needs to study genetically similar individuals in different situations. These clear-cut strategies rarely can be followed in pure form, however, and even when they are, they leave the question of the interaction of different kinds of individuals in different kinds of environments to be understood by more complex study designs.

The career of Professor Comas manifests the dichotomy; his physical anthropology had the two faces. On the one side Comas studied the inborn aspects of human beings. He examined anthropometric (largely genetic) differences among peoples of different places. He systematized and standardized these methods so that anthropometric comparisons would be meaningful. His *Manual of Physical Anthropology* (Comas, 1957) and his compilations of findings of many Mexican and other investigators of studies of the indigenous populations of this and other American nations are the face of Janus that looks in on the innate differences among people. At the same time Professor Comas was always concerned with those differences among races that resulted from the environmental differences —especially disadvantageous environments forced on people of one race by those of another. This face of Janus sees the underlying similarities of peoples with the overlaid differences that result from the ecological setting— especially the cultural factors that vary among us. He emphasized that the species is one but that our diversity of cultures leads us to behave in a variety of ways.

If I may, I should like to recount some of my own experiences with Janus, our two-faced god of physical anthropology. I hope I shall not be considered too immodest for selecting examples from my own work rather than from better studies by others elsewhere. My purpose is only to show the variety of hypotheses that can readily be approached even in a modest research effort such as ours in Paracho, Michoacán. My mother was a biochemist and my first research was with her and dealt with biochemical genetics —the mode of inheritance of an inborn error of metabolism, pentosuria (Lasker et al. 1936). It is inherited as a Mendelian recessive. My father was a social scientist, however, so even the two faces within my family tended to look in the two directions, and my second published effort was a book review of a book my father gave me for that purpose, Otto Klineberg's (1935) "Race differences". Klineberg is a social psychologist. When he was a student, anthropology and psychology were housed in the same building at Columbia University and Klineberg came under the influence of Franz Boas. The Klineberg book was important to me because of its well-reasoned answers to works purporting to show the biological superiority of one race over another. Klineberg used the strategy of similar people in different environments (American Blacks in the South and Blacks from the South in the North to show that intellectual development varied according to the sociocultural environment. Although there were no genetic differences between them, at that time Black students in New York City schools achieved higher scores on intelligence tests than did Black

children in the cities of the South from which they had migrated. I was strongly influenced by Klineberg and I suspect that Professor Comas knew and exchanged ideas with Klineberg since they published together a work for UNESCO (1956) that combatted racism through clarifying its social causes and lack of biological basis.

My wife, Dr. Kaplan, and I first came to Mexico in 1948. I had spent two years in China and always assumed that my anthropological research would be in the Far East. Dr. Kaplan had been planning to work in Puerto Rico. Fortunately we had an opportunity to come here. The late Professor Norman Humphrey at Wayne State University had worked in Jalisco and enjoyed it immensely (as I think most foreign anthropologists who have worked in Mexico and gotten to know the Mexican people do) and he encouraged us. Also my colleague in the Anatomy Department Dr. F. Gaynor Evans, had conducted zoological field studies in this country and wanted to return and he accompanied us on that trip.

When Dr. Evans and his wife and Dr. Kaplan (then still a graduate student) and I arrived in this city in April 1948 we stayed at a little hotel near the railway station and we called on the few physical anthropologists then active here. We found Juan Comas at the Instituto Indigenista, I think it was on Londres or Liverpool. He was very helpful and tolerant of our ignorance and we spent several hours with him. He had several suggestions for our work and he told us about his. We remained friends from that day until his last. We also met Daniel Rubin de la Borbolla and he eventually introduced us to Licenciado Arriaga, at that time director of the musem in Morelia. Licenciado Arriaga in turn introduced us to Pablo Velasquez who in turn took us to his compadres in Paracho, Michoacán, Don Cesario Sosa, a longtime resident of the United States and his wife, now widow, Sra. María Caro de Sosa. Over the years their house has been home to many anthropologists and we lived there for five months at that time. Dr. Mauricio Swadesh, late of this department, your distinguished linguist, was married in that house while he was studying Tarascan.

Our chief motive in the study we conducted at that time was to examine the external environmental face of our god Janus. We wanted to pursue the problem of Marcus Goldstein's famous monograph "Demographic and bodily changes in descendants of Mexican immigrants." We wanted to see how much effect residence in the United States might have had on Parachexos who had lived and worked in the north and then returned to Paracho. We found that those who went to the U.S. as adults did not differ from sedentes who had never left Mexico; those who went while young tended to be taller and also larger in other respects, the younger

they were when they left and the longer they resided in the U.S. (Lasker, 1952). That is, the anthropometric differences between migrants and sedentes could not be ascribed to self-selection of a particular kind of person to migrate, but must be ascribed to growth changes that occurred differently in the two environments.

We also asked about what race the people thought their parents belonged to (Kapan and Lasker, 1953). It turned out that the chief difference between those who called their parents Indigenas or Naturales and those who called their parents Mestizos or Blancos was that of the age of the subject. Young adults who were, perhaps, more socially mobile, were more likely to think of themselves and therefore to describe their parents as "Mestizos". Those over 45 years old, however, perhaps because they tended to be content with their lot, thought of themselves and of their parents as natives. We later found the same thing true on the north coast of Peru and published that finding in the Festschrift for Juan Comas' 65th birthday (Lasker and Kaplan, 1965).

At the same time and on subsequent field trips to Paracho and elsewhere in this country we looked with the other face of physical anthropology. For instance, BAIB is an amino acid; there are innate differences in the amounts people excrete. Europeans generally have 7 to 10% high excretors. Several Amerindian groups range between 50 and 60% high excretors. We found that all values in Mexico were intermediate. There were more high excretors in Oaxaca than in Michoacán. Oaxaca, the proportion of high excretors tends to be greater in the inhabitants of the more Indian towns than in those of the more Mestizo ones. For instance, in the Tarascan area there were 30% high excretors in Cheran and 14% in Paracho (Lasker et al., 1969). Thus hereditary racial differences apparently still persist.

The store of information we collected in Paracho in 1948 and on several subsequent occasions has served us in good stead for numerous subsequent studies:

In one study, to find out if migration is selective, we asked which of the people measured in 1948 had migrated to the United States in the subsequent four years (Lasker, 1954a). There were virtually no physical differences between those who went and those who did not. Thus the evidence was against a physical selection of migrants. However the migrants did have wider hands which may have been a purely chance finding by may have represented a tendency for selection of manual workers for temporary labor migration.

In another study we examined the possibilities for random genetic drift. Some Mexican towns are small and isolated and random genetic drift would be a factor. In Paracho, however, the

effective size of the breeding population was estimated as 967 and the rate of admixture was about 20%. The product of these values is well above 50 and towns such as Paracho have too high a rate of exogamous marriages and too large a breeding population to be likely to diverge in gene frequencies from surronding areas by random genetic drift (Lasker, 1954b).

A cross comparison of dental caries and ability to taste PTC suggests that there may be an effect of one on the other, but different studies of this question are not in agreement and the mechanism is still unknown. Except for a small study by my sons which showed that some youths who were tasters became non-tasters and vice-versa in a period of two years, no one has studied PTC tasting longitudinally to show what changes occur with age in this largely hereditary trait.

A study of skin color by photoreflectance of students in Paracho schools showed little differences between ages but the exposed skin was darker in the boys. Boys in the Indian boarding school were darker than town boys in the day school (Lasker, 1954c). Mestizo populations would be ideal for further studies to understand the nature and reasons for differences in skin pigmentation.

Two demographic studies based on data we collected in Paracho demonstrate the use of the same data to look in both directions.

On the genetic, inborn, side it can be assumed that some part of the variation in anthropometric measurements is genetic. Therefore the whole human species and most of its populations are about the size they are, because Darwinian natural selection has selected genes for that size. One hypothesizes that the average size found is a favorable size for survival. Other things being equal, individuals of higher Darwinian fitness are hypothesized to be less variable in anthropometric dimensions than those of lower Darwinian fitness. We used four measures which approach fitness: 1. The number of live-born offspring, 2. The number of offspring per year of married life, 3. The number of siblings ever born, and 4. The number of living siblings. In general we found little tendency of balancing selection in the data from Paracho (Lasker and Thomas, 1976). In most anthropometric measurements there was little difference in anthropometric variation among those who left more descendants (or whose parents left more descendants) and those who left fewer.

In collaboration with Dr. Willian Mueller of the University of Texas (Houston) we are now looking at data Dr. Evans collected in Uruapan, Michoacán to test for confirmation of this. The purpose of these studies is to see if we find a genetic effect: selective survival of more average individuals. The men who went to the U.S. to work started out with lower fertility, but wound up at older ages with more offspring. When allowance is made for migration as well as

for age and sex there is no evidence that the more fertile differ from the less fertile on the average, but there is a suggestion that the less fertile are more variable in one respect, lower facial height, in both sexes suggesting stabilizing selection. There is also one instance of the reverse. It will apparently require additional studies accumulating substantial numbers of cases in order to asses the influence of selection on human beings in this way.

Some of the same data are now being looked at with the other face of physical anthropology. A student of mine, Mr. Gregory Fuller, is recompiling our data on offspring who survived infancy and on those who died in infancy to see if the parents differ in physical and social variables. Our hypothesis involves a purely environmental mechanism: that the measurements of the parents of infants who die will themselves have been less well nourished and will show this in smaller, slighter physique. We hypothesize also that the socio-economic data which Dr. Kaplan collected in 1952 will show that the lost infants will more often have had poor parents who owned less property and lived in smaller houses or log trojes. The same measurements of the same parents and the same survival (or not) of their infants can, in theory, be used to measure the population genetic process of balancing or directional natural selection, as we have tried to do, and also can be used to estimate predominantly environmental factors by associating physical status and social conditions of parents with deaths of their children in infancy. That is, we expect to show to what extent small size or other characteristics of parents and infant mortality of their offspring can be traced or imputed to the socio-economic conditions of the family extending from one generation to the next.

As you can see from the somewhat mixed studies, a comparative science such as physical anthropology does not yield an ideal model for simultaneously studying and separately weighing the genetic factor and the influences of diet and other environmental components. That is what the search should be about, but the oportunities must be exploted as they are found.

Strategies comparing multiple samples, some controlling for environments, some for genetics are needed. One must not push exclusively for one factor or another.

The environmental influences will predominate in some instances, the genetic influences in others. For instance when it comes to human mental capacities, genetic differences exist among individuals but essentially all the differences *between* populations can be accounted for by the impact of education, formal and informal. The children with the most opportunity to interact with adults are the most likely to score well in intelligence tests. The present tenddency to small families may therefore tend to increase the intelli-

gence test scores of the population —without any selection or change in gene frequencies. On the other hand, variations in shovel-shaped incisors, for instance, seem to be inherited (except insofar as they are disguised by tooth wear from a rough diet or enamel hypoplasia from childhood stresses) despite the fact that the mode of inheritance has not yet been demonstrated.

My most recent work has been on a quasi-genetic subject. Surnames are inherited much like genes (in the English system we inherit half of our parents' surnames; in the Spanish system half of grandparents'). Since surnames can be abstracted from historical as well as contemporary records and since it is possible easily to survey very large numbers of people in this way, their behavior, under certain assumptions, can be used as a model of what happens to genes (Lasker, 1977). The assumptions are: 1. that the surnames are passed from parent to child, 2. that two persons of the same surname have an ancestor in common from whom they derived it, and 3. that the lines of descent marked by the surnames are representative of all lines of descent.

Our studies in Peru, Italy and England and the studies of others in Brazil, Switzerland, Japan, the U.S.A. and elsewhere show that the assumptions are not fully met (Lasker, 1980). In Peru we found more commonality of surnames between different communities than the rates of migration between then would explain (Lasker, 1978). In England we found that surnames of frequent occurence are much less localized in distribution than rare surnames; thus the rare ones serve better than the frequent ones to mark common ancestry. This concurs with the findings of Ellis and Starmer, among others, that some surnames are polyphyletic. Inbreeding estimates from isonymy therefore tend to be too high by comparison with results from pedigree analysis. For this reason I have not published the results from Paracho or elsewhere in Mexico. Most surnames here were given to Indians and Mestizos by priests and surnames from the names of the same few saints (such as Sanchez, Perez, Hernandez and Martinez, for instance) were independently given to unrelated individuals even in distant places. Although surnames in México only date from after the conquest, whereas English surnames generally go back to the Middle Ages, the same kinds of allowances could be made for violations of the assumptions in both areas. One can use a research strategy which analyzes different kinds of surnames separately (common surnames versus rare ones, names of Indian derivation versus Spanish ones). A shortcoming with this kind of study which remains, however is that whereas surnames may model genetic traits, they are subject to influences and explanations that are peculiar to the history of surnames. Changes in surnames have little in common with biological muta-

tion. Poor Janus can see the surname analysis only with his genetic set of eyes. But even this can help the total view, differences between biological and surname inheritance studies can show one something about the difference, the part of the biological "inheritance" not explained by common origin or the part of surname "inheritance" explained by the history of surnames).

The full development of human biology in Mexico requires attention to both sets of factors. The time is ripe for multiple studies comparing genetic and environmental factors at the same time such as a study of human growth in rural sedentes of both Indian villages and Mestizo ranchos and of migrants to cities from both places. Mexican physical anthropologists trained by Juan Comas and others, and foreign colleagues sympathetic to the practical needs of Mexico at this time, but nevertheless dedicated to basic research problems worked out here but applicable to humankind everywhere, can now proceed. They will start where Juan Comas left off, but he would have wanted them to go further, and in his spirit they will achieve levels of understanding that have escaped us.

In experimental biology the accepted strategy is to select a specific relationship (preferably a relationship in which a previous condition is hypothesized as the cause of a subsequent event). A suitable (usually genetically homogeneous) organism is selected, all extraneous conditions are standardized by making them identical in experimental and control samples, and the one relationship is demonstrated to exist or not under the specific conditions.

In human biology as practiced by physical anthropologists the preferred strategy is almost diametrically opposite. What we should like to do in this comparative science is compare each of all the kinds of human beings in each of every kind of possible situation. In this way we can focus on the interaction between different factors. Experimental studies emphasize the relationship between specific variables. It is often more efficient, however, to start with many primary variables. We can then recognize the complex nature of the problems and select primary variables among anthropometric, physiological, demographic and biochemical traits that bear on important questions of scientific and/or practical significance. Our own work, like that of others, is grossly deficient in meeting these standards but we hope we have inched forward and always acknowledged our shortcomings in a way that will increase the prospects for others to improve and refine the search.

I thank the *Purepechi* of the *Capital de la Sierra Tarasca* for the opportunities I have had for research in Paracho and I thank the organizers of this colloquium for the honor of allowing me to share these notions. Dr. Francis E. Johnston and Dr. Paul T. Baker kindly

criticised a draft. Whether physical anthropology has one face, or two, or many it has, we think, enough eyes to see relationships of intrisic and extrinsic factors in human biology that will be helpful to the wider worlds of public health, population policy, and civil rights. We trust it will see things as they are and help put an end to the false image of the superiority of one race over another and also to the simplistic false notion that we are, and are only, what we eat.

Thank you.

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