Aggression and Hypoglycemia in the Andes: Another Look at the Evidence[1]

by Ted C. Lewellen

IN AN AWARD-WINNING AND MUCH-QUOTED ARTICLE titled "Aggression and Hypoglycemia among the Qolla: A Study in Psychobiological Anthropology" (1973), Ralph Bolton offered an impressive array of quantitative evidence in support of the hypothesis that low blood sugar is a primary cause of the notorious levels of conflict among the natives of the Lake Titicaca Basin in Peru and Bolivia. Since this was the first field study to correlate blood-glucose levels with aggression and one of the earliest and most significant anthropological investigations on the interrelation of biological and psychological factors in human behavior, its significance extends far beyond the small Andean community where the research was done. In this paper I will reevaluate the evidence, in relation both to "Qolla" aggression and to hypoglycemia, in the light of my own research in Peru and the numerous medical studies of hypoglycemia published since Bolton's article.

No Indians called the "Qolla" exist; Bolton used this ancient word to include two language groups under a single term. This extension of research done among Quechua-speaking peasants to Aymara-speakers was justified in that the two groups seem to comprise a single subculture in this area. Also, while Bolton studied a Quechua community, his data confirmed the work of other anthropologists, who have long characterized the Aymara in a particularly uncomplimentary fashion. While I shall show that the Aymara are neither pathologically aggressive nor hypoglycemic, Bolton was certainly in the best of company with his severely negative appraisal of these people. The Aymara are probably the single most maligned people in the anthropological literature. La Barre (1966:113) summarizes the opinions of several researchers:

> I found the Aymara truculent, hostile, silent, suspicious, treacherous, and vindictive; masters of indirect aggression when they did not express it directly. Forbes called them "intensely suspicious and distrustful . . . [with] the most deeprooted and inveterate hatred of their white oppressors"; Grandidier considered the Aymara "cruel"; Wall, "hard, vindictive, bellicose, rebellious, egotistical, and jealous of his liberty . . . lacking in will except to hate." . . . Hewitt wrote that "They are the most difficult of all Andean peoples to cultivate; in fact, it is well-nigh impossible to establish friendly relationships with them." . . . These forceful and uncompromising statements, it must be remembered, are not the projected "racial" stereotypes, say, of a Bolivian white class that notoriously exploits the Aymara; they are the considered judgments of professionally trained ethnographers. . . .

Tschopik (1951:172), the best-known of these professionally trained ethnographers, concurs:

> In the Aymara case, the multitude of determinants stemming from the combined socio-cultural and physical environments have molded a personality type so distinctive and, from the Western European point of view, so aberrant that it has been described in remarkably consistent terms by several independent observers.

Under the influence of then-fashionable modal personality theory, Tschopik characterizes their "aberrant" personality as "masochistic . . . with extreme personal dependency and constant avowals of helplessness and unworthiness" (p. 183) and chooses as significant modal traits anxiety, hostility, irresponsibility, submissiveness, and disorderliness. Only a few years ago, the Aymara were unflatteringly portrayed in *Science News* (Trotter 1973:76):

> The meanest people in the world are probably the Qolla, an Andean subculture inhabiting the area around Lake Titicaca between Peru and Bolivia. Anthropological literature has described the members of this group as the meanest and most unlikeable people on earth—the classic example of an extreme personality type dominated by excessive hostility and aggression.

Bolton was the first to attempt to operationalize this supposed Aymara pathology. His study, published in a tightly focused and precisely argued 365-page doctoral dissertation (1972) and condensed in his 1973 article, is certainly one of cultural anthropology's outstanding examples of hypothesis-testing field research. Having collected a minutely detailed file on aggressive behavior, based on both interview and direct

[1] An earlier, somewhat different, version of this paper, titled "The Meanest People in the World: Anthropological Stereotyping and the Aymara Indians," was presented at the annual meeting of the American Anthropological Association in Cincinnati in 1979. Field research upon which this paper is to some extent based was partially funded by the Instituto de Estudios Aymaras, Chucuito, Peru.

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observation (he and his wife were so violently threatened that he was forced to keep a gun for protection), Bolton found that the homicide rate for the community of Incawatana was higher than any recorded national rate: 55 murders per 100,000 population, as compared with national rates ranging from .3 to 34 per 100,000. This meant that over 30% of all households contained an adult member whose parent, child, spouse, or sibling had been involved as an offender in a homicide case. Bolton hypothesized (p. 242):

> There is a curvilinear relationship between the amount of drop in blood glucose during the administration of a Glucose Tolerance Test and the level of aggressiveness of an individual, with high levels of aggressiveness occurring in the range of mild hypoglycemia, i.e., a drop in glucose by 10 to 25 mg. per 100 ml. below fasting level. Moreover, it is suggested that the relationship is causal in nature, with moderate glucose deprivation causing aggressivity.

To test this low-blood-sugar—or hypoglycemia—hypothesis, Bolton compared ratings for aggression from three Incawatana natives (one source, which did not coincide with the others, was disregarded) with a "conflict file" on aggression within the community; this confirmed that those rated as high aggressors by informants were indeed involved in more acts of aggression than others. Then 66 subjects were selected, roughly at random, to undergo a glucose tolerance test (GTT). Prior to taking this test, the subject fasts for 12 hours, usually overnight. A blood sample is taken to determine the fasting level of glucose in the blood; then the subject drinks a solution containing 50 g of sugar, and blood samples are taken at half-hour intervals for the first hour and every hour for four hours after that. For each blood sample, sugar concentrations are established in terms of milligrams per hundred milliliters of blood. As anticipated, Bolton found a statistically significant relationship between mild or moderate hypoglycemia and high aggressiveness (severe hypoglycemics could be assumed to be too fatigued to be aggressive). This relationship was further confirmed by a correlation between mild hypoglycemia and high levels of hostility as expressed in fantasy (Bolton 1976).

It should be emphasized that at no time does Bolton postulate a unicausal theory of Qolla aggression; rather, he views hypoglycemia as part of a complex of biological and social factors. However, he does consider low blood sugar to be a primary or more direct cause insofar as other "stress factors contribute to the causation of aggression through their effects on the glycemic condition of the person" (Bolton 1972:341).

The hypothesis consists of two separate postulates requiring quantifiable evidence—(1) that the "Qolla" as a group possess a pathological personality type, as described by previous researchers, and (2) that hypoglycemia is a primary cause of their aggressive behavior. I will attempt to show that evidence for both these conclusions is minimal at best. Rather, the negative characterization of the Aymara is simply a pseudoscientific stereotype (no one, as far as I know, has characterized the Quechua so negatively); these people are no more aggressive, and certainly no more homicidal, than other peasants; they are not hypoglycemic, and, in any case, there is little evidence that hypoglycemia causes aggression.

AYMARA PERSONALITY: BEYOND THE STEREOTYPE

With the exception of *Science News,* all of the sources cited earlier on the unlovable Aymara personality are based on field research done more than 30 years ago. The consensus would be somewhat different today. In fact, virtually all of the more recent ethnographers of the Aymara would appear to disagree with at least the more extreme of these characterizations. Hickman (1971:xvi) notes that during his and his wife's stay of well over a year in Chinchera, Peru, very near where Tschopik worked, they were the recipients of no aggression, nor were they at any time robbed, despite the most minimal precautions. Carter (1968:258) found many of the negative characteristics of the Aymara unsubstantiated by his research. Far from being crushed beneath the burden of self-abnegation and fatalism, popular schoolboys had positive self-images and preferred rational solutions to magical ones (Carter 1966: 372). He also notes (p. 258) that "one of the first social lessons children are taught is that of hospitality. Even more than accumulated wealth, it is hospitality that gives high status. . . . Indeed, if the Aymara could be said to have one unforgiveable sin, that sin would be miserliness."[2] In contrast to characterizations of the Aymara as pathologically fatalistic, Miracle (1976:11) observed that nearly all the games played by young people of the Bolivian community where he conducted his research stressed skill, and Hickman (1971:204) found that only 31% of his questionnaire respondents felt they had little control over their destiny. Paul Brown (personal communication, 1978) and Winifred Mitchell, who spent 15 months living in a remote community, found the traditional characterizations of the Aymara absurd. Virtually all the U.S. Catholic missionaries in the Peruvian altiplano, some of whom have lived in the area as long as 20 years, know of the negative anthropological characterizations of the Aymara and dismiss them as a sort of perverse joke. (The standard missionary response when the hypoglycemia hypothesis is mentioned is, "When attacked, throw them candy bars!")[3]

My own opinion of the Aymara after about 15 months among them, including 4 months living with a family in a remote peasant community, is that they are suspicious of strangers, but courteous, friendly among themselves, quick to joke and equally quick to laugh; they are extremely hard-working and conscientious if they can see the point of the job, and with proper leadership they can work together remarkably well for the public good; they—especially the women—have raised the dance to an art form; they love a fiesta and like to get drunk, but this normally occurs only a few times a year; being at the bottom of the socioeconomic order, they are often frustrated in their ambitions, and there are often tensions over land boundaries which may lead to interfamilial verbal feuding and occasionally to fisticuffs. Their children are well-behaved, generally happy, and ready to take advantage of a spare moment for a game. In short, the Aymara are just like the rest of us, not one whit better or worse than they have to be.

If it is true that the Aymara are no more pathologically aggressive, or pathologically anything else, than other peoples, how did this scientifically certified character assassination develop, and how has it been so long maintained? Plummer (1966), in a rare defense of the Aymara, noted that early anthropologists, such as La Barre and Tschopik, identified with the hated mestizo class and often employed mestizo interpreters, thus bringing out a justified hostility toward an oppressor (see also Carter 1966:367). This problem has been to a great extent alleviated as more and more Aymara have become bilingual and approachable directly through the Spanish language; non-Aymara-speaking anthropologists require no interpreter or mediator.

Several other reasons might be considered. First, a certain

[2] Carter is the only recent fieldworker quoted by Bolton (1972: 334–37; 1973: 190) in support of the traditional negative characterization of the Aymara. This is rather selective, since Carter has been one of their more outspoken defenders.

[3] The Maryknoll missionaries, through the Instituto de Estudios Aymaras, partially funded my research. During my three-month preliminary visit to the area I taught them anthropology. Later, during 12 months of fieldwork, I got to know all of them, many quite well, and found them an invaluable source of information and contacts.

sullenness is a culturally standardized means of dealing with outsiders who are perceived as threatening, and this can easily be interpreted by an approaching researcher as silent aggression or hostility. Second, as Paul Brown (personal communication, 1978) has pointed out, the Aymara tend to perpetuate their own myth by boasting of aggression, for example, by claiming to have killed someone who is very much alive. Third, until about 15 years ago, most studies of the Aymara took place in towns where normal rural sanctions had broken down. Tschopik (1951:173) himself recognizes this problem, noting, in relation to his modal-personality designations, that "both La Barre and I have found it necessary to make qualifications, and to point out that such characterizations apply more specifically to the Aymara of the towns and haciendas than to the independent, ayllu-dwelling Indians." Osborne (1952:15) puts its succinctly: "The traditional picture of the drunken, vicious, deceitful and apathetic Indian is not a true picture of the Indian in his natural and traditional state. When integrated into his own communities, he is sober, hard-working, honest and meticulous." Fourth, and perhaps most important, Aymara culture is based on a complex network of reciprocal exchanges: anthropologists who are perceived as taking (i.e., information) without giving something in return receive exactly the same hostility a native would in similar circumstances. Those anthropologists who have involved themselves in providing health or transportation services, such as the Hickmans, teaching adult classes, such as the Browns, or teaching in their public schools, such as myself, have not been the victims of overt aggression. Finally, once a stereotype is established "scientifically"—that is, by the intuitions of professional anthropologists—it is self-maintaining, as the perceptions and expectations of later researchers are funneled by the written opinions of their precursors.

However, given the painstaking thoroughness of Bolton's research, there can be little doubt of the high aggression rate in Incawatana. Aggression per se is too broad a concept to be quantitatively compared, but there are comparative data on Bolton's measure of aggression, the homicide rate. While his figures are indeed high, the question is whether the Incawatana murder rate is representative of that of the Aymara in general. The answer is no.

It should be noted that Bolton's sample is *one*—one of literally hundreds if we are speaking of communities surrounding Lake Titicaca, one of thousands if we are including the entire Aymara language group. No statistician would generalize from such a sample unless many others confirmed the data. But do they? We have seen that La Barre, Tschopik, and other early ethnographers of the Aymara characterized these people negatively, but aggression was only a part, and often a minor part, of these characterizations. The crucial element, however, is the murder rate. There is, of course, a problem in finding comparative data relevant to murder, since it should not be expected that all murders will be reported to the authorities. In any event, there is scant support for generalizing the Incawatana murder rate. La Barre (1948:158), in his only mention of the subject, refers to "the rare cases of native murder," and Tschopik (1951:170) notes that "the murder of adults . . . appears rare, and only five cases were remembered." According to my informants, in the community where I lived there had been no murders in the last five years; were I to generalize this finding, it would appear that the Aymara were the least aggressive people in the world. The truth seems to lie somewhere in between. Perhaps the best non-native source for such information is Father Jim Madden, a Maryknoll priest who speaks fluent Aymara and who lived in the countryside, in a typical peasant hut, for six years as part of an experimental project. During this time he served on foot ten communities, a total of more than 5,000 people. Knowing many of these people intimately, he was in a unique position to hear of every murder, prosecuted or unprosecuted, rumored or real. During the first five years of his stay, there were two reported murders and one probable unreported murder in these communities, giving a statistical average of roughly 12—or, if only reported murders are included, 8—murders per 100,000 per year. In 1976 the U.S. rate was 8.8 per 100,000, but for the six largest cities the rate was 23.5 (U.S. Department of Justice 1979:433–51). In 1977, the rate for Cleveland was 39 per 100,000 and for Detroit 36.5 (U.S. Department of Commerce 1978:179).

Nor is the Incawatana rate quite as exceptional as it may first appear. Bolton himself quotes homicide rates as high as 63 per 100,000 for some districts in Colombia and up to 200 for certain areas in Mexico. Significantly, in a listing of 25 nations, those with the highest homicide rates (Colombia, Mexico, and Nicaragua), ranging from 22.8 to 34 per 100,000, are all essentially impoverished agricultural countries, suggesting a correlation between aggression and the whole complex that makes up "underdevelopment" (Wolfgang and Ferracuti 1967, quoted in Bolton 1972:195–98). These statistics represent homicides reported to the authorities, so the truly comparative figure for Incawatana would be 20 per 100,000—slightly more than half the national rate of Colombia and only one-tenth the rate for the village of Acan in Mexico.

While such considerations are important in absolving the Aymara of a long-standing anthropological stereotype, Bolton is not attempting to establish that the "Qolla" as a group are exceptionally aggressive; indeed, this pathology is assumed as the background for his hypothesis. Rather, he is interested in demonstrating a causal relationship between hypoglycemia and aggression, and here he certainly does show a remarkable correlation. Or does he?

THE HYPOGLYCEMIA HYPOTHESIS: A REEVALUATION

Although hypoglycemia has inspired an endless succession of popular books (Abrahamson and Pezet 1971, Airola 1977, Barmakian 1976, Fredericks and Goodman 1969, Martin 1976, Yudkin 1972, to name just a few), the medical establishment has tended to view it as a "fad disease," a "nondisease" (Yager and Young 1974), or a "cult illness" (Levine 1974); its diagnosis for an endless list of symptoms has been branded "hypoglycemic quackery" (Marks 1976*a*:127). This faddish approach to a real illness stimulated the American Diabetes Association, the American Endocrine Society, and the American Medical Association (1973) to issue a joint statement disavowing the overdiagnosis of hypoglycemia. Marks (1976*a*:128) vehemently objects to the idea put forth in popular books that hypoglycemia is "at the root of such diverse conditions as alcoholism, drug addiction, psychotic disorders of all types, juvenile delinquency, multiple sclerosis, obesity and migraine, as well as almost every other physical, psychiatric or social ailment for which no specific etiological agent has been discovered." Despite its pop-culture appeal, documented cases of hypoglycemia are relatively rare (Cahill and Soeldner 1974).

This would certainly not seem to be the case with the "Qolla"; Bolton found that fully 55.5% of his subjects suffered from hypoglycemia. Actually, however, his research showed only that his population reacted normally to the glucose tolerance test. If we are to believe his published glucose levels (which seem unreasonably *high:* I will suggest why later), then by the most common criterion used in the medical profession today not a single one of his subjects was hypoglycemic.

There are two types of hypoglycemia, requiring different kinds of diagnosis. *Fasting* hypoglycemia, which is extremely rare (accounting for only about 1% of patients), occurs when blood-sugar level drops below normal values after a long period without food. Bolton (1972:260) considered fasting glucose levels but found no significant relation with aggression. The more common type of hypoglycemia, usually caused by a malfunction of the liver, pancreas, pituitary, or thyroid, is called *reactive* and appears after eating or after the administration of a glucose concentrate (Barnes and Barnes 1978:11; Danowski 1978:11). There is considerable variation in the medical literature as to what blood-sugar level justifies a diagnosis of reactive hypoglycemia. According to a recent article in *Time* (1980), "Depending on the specialist, the lowest 'normal' can be any point between 35 mg and 60 mg of glucose in 100 ml of blood. . . . One study found values as low as 22 mg per 100 ml in apparently healthy women." According to "Whipple's Triad," which dates to 1944, a determination of hypoglycemia may be justified if symptoms (weakness, sweating, palpitations, etc.) appear at the same time as glucose levels of 50 mg or below and if the symptoms disappear immediately upon administration of glucose (Marks 1976*b*:1). While this "triad" is still widely used today, the majority of American doctors would seem to use 40 mg glucose per 100 ml of blood as the base-level criterion for diagnosis (Danowski 1978:144; Krupp and Chafton 1974:691; Marks 1976*b*; Marks and Rose 1965:70; Pinckney and Pinckney 1978:85), though all would use other criteria as well. Bolton does not give absolute values for his glucose levels in his article, but they are included as an appendix in his dissertation (1972: 344–47). Even four hours following oral administration of a glucose concentrate, none of his subjects showed a glucose level below 45 mg per 100 ml of blood. In other words, even by the criterion of the glucose tolerance test alone (Bolton uses no other criteria), most doctors would not have diagnosed any of his subjects as hypoglycemic.

As I have said, Bolton's criterion for mild hypoglycemia was a drop in blood-sugar level by 10 to 25 mg per 100 ml from fasting level. Anyone with a drop of more than 25 mg was considered severely hypoglycemic. For example, if a person had a fasting level of 92 mg glucose that dropped to 81 after the first hour and to 72 after three hours (this is my own test reading), he would be considered moderately, bordering on severely, hypoglycemic. Yet the lowest reading, 72 mg, is 32 mg higher than the base level which most doctors would consider hypoglycemic. Since normal fasting levels can be 100 mg or higher, Bolton would consider severely hypoglycemic persons with nearly twice the blood-sugar concentrations of those the American medical establishment would consider even minimally hypoglycemic. Bolton provides no source for his criterion for hypoglycemia, though it would seem to be Fredericks and Goodman (1968:112), one of the earliest of the "pop" paperbacks on hypoglycemia and certainly one of the works the American Medical Association had in mind in its repudiation of the overdiagnosis of hypoglycemia. I have encountered nothing even approaching Fredericks and Goodman's criteria in any medical journal or diagnostic manual. It should be added that there is a general agreement, well expressed in Whipple's Triad, that hypoglycemia cannot, in any case, be diagnosed on the basis of the glucose tolerance test alone. While anyone who has worked with peasants in remote areas must sympathize with Bolton's use of a single criterion, one wonders if he could have maintained his diagnosis if other criteria had been employed, such as determining symptoms at the blood-glucose nadir or performing "modern hormonal assays and tests" (Levine 1974).

During the last ten years an enormous body of medical research has been published on the subject of hypoglycemia, and much of it points in the same direction: the range of *normal* variation in reaction to the glucose tolerance test is much wider than was formerly believed. Leggett and Favazza (1978) cite several studies, including one which found 42% of *normal asymptomatic* subjects to have glucose levels below 50 mg, while another study found that in 48% of normal subjects blood-sugar levels dropped this low, many even below 40 mg, during the test. If lowered gradually, the authors note, blood glucose can reach as low as 5 to 10 mg without symptoms. Danowski (1978:148), in a statistical test of several hundred healthy, nonoverweight participants, found that 20% to 30% had glucose readings from 45 to 59 mg without symptoms—"Indeed, such persons felt perfectly well. . . ." Chandler (1977) found that up to 30% of a normal population showed glucose curves that dropped below 50 mg during the glucose tolerance test. In an extensive test of 207 normal subjects from Kristianstad, Sweden, Nilsson and others (1964) found that for men over forty the *average* drop in blood glucose below fasting level three hours after being administered a glucose solution was 21.9 mg; this is more than double the drop in blood-glucose levels which Bolton would consider mildly hypoglycemic, and almost severely hypoglycemic by his criterion. In another test of normal subjects, Boyden and Shen (1975) found that in 14% of subjects blood-glucose levels fell below 40 mg and in 32% they fell below 45 mg four hours into the test.

None of Bolton's subjects showed a drop in blood-glucose levels below 45 mg. While a somewhat high 52% did reach this level, this would hardly seem surprising considering the well-established fact that normal fasting blood-glucose levels for high-altitude peoples are considerably lower than for sea-level peoples. Bolton (1972:241) quotes Monge's (1968; Monge and Monge 1966) readings showing average fasting blood-glucose levels of as low as 64 mg at high altitudes (the average at low altitudes is 90 to 100 or more mg). Bolton's Incawatana sample, which lives at an altitude of 3,800 m, averaged a fasting level of 70 mg. Picón-Reátegui (1962, 1963) confirmed earlier studies by Forbes in showing that there was a greater drop in blood-glucose levels in high-altitude peoples during the administration of a glucose tolerance test, to as low as 21 mg, but *without any symptoms of hypoglycemia.* He hypothesized that high-altitude glucose levels could be related to higher carbohydrate intake and thus might be "an adaptive mechanism which promotes more efficient glucose utilization" (1963:1260).

Given Monge's and Picón-Reátegui's high-altitude blood-glucose readings, which were not accompanied by hypoglycemic symptoms, Bolton's lowest readings of 45 mg, far from demonstrating widespread hypoglycemia in Incawatana, seem curiously high. Since, as we have seen, 45 mg is not unusual even for a sampling of a normal, asymptomatic sea-level population, we would expect the blood-glucose levels in Bolton's population to drop much lower, at least for a significant percentage of subjects. The reason for this discrepancy from expectation would seem to be associated with Bolton's method of measurement. Because of the difficulties of testing large numbers of people in a remote environment, Bolton could not use the usual complex laboratory measurement of blood glucose and was forced to rely on Dextrostix, described by its manufacturer as "a semi-quantitative test for glucose in whole blood." A drop of blood is applied to a "reagent strip" that changes color according to how much glucose is in the blood, and this color is compared with a standardized color chart. Color values are calibrated for 0, 25, 45, 90, 130, 175, and 250 mg per 100 ml blood. If a reading is, for example, below 45 but not far enough below to be interpolated between the 25 and 45 color blocks, it is read as 45, even though it may be significantly lower (Miles Laboratories 1979). As one diagnostic manual notes, "The test [Dextrostix] is designed primarily for screening and is not intended to replace more pre-

cise analytical procedures" (Frankel, Reitman, and Sonnenwirth 1970:1882).

As we have seen, blood-glucose readings for normal subjects can be quite low without any evidence of hypoglycemia. The explanation now seems fairly clear: the glucose tolerance test *creates* low blood sugar in healthy individuals. As Dr. Leonard Madison, quoted in *Time* (1980:71), observes: "Hypoglycemia is a normal response to the glucose tolerance test. Man was not built to take an overload of glucose like that. Look at it this way: if you run up a flight of stairs and find yourself short of breath, it does not mean you have heart disease." This sentiment has been expressed by several other researchers, who note that the ingestion of a high dose of almost pure glucose is a wholly artificial stress that tells one little about the individual's response to everyday conditions (Barnes and Barnes 1978:22; Field 1975:65; Marks 1976*b*:5).

Finally, experiments which have attempted to show a correlation between low blood sugar and various symptoms have not produced positive results. One such experiment by Marks (1976*b*:4) with 65 hospital patients revealed "no obvious correlation between the lowest venous blood glucose concentrations observed at any time during the test and the development of symptoms. Nor was any relationship discernible between the presence either of symptoms or of venous hypoglycemia and the occurrence or absence of symptoms possibly attributable to spontaneous hypoglycemia." In another test (Ford, Bray, and Swerdoloff 1976), 30 hospital patients were compared, 18 with diagnosed hypoglycemia and the rest without. The authors concluded that "there was no demonstrable relationship between personality type and GTT diagnosis" and that their data "do not support the proposition that the emotional distress experienced by hypoglycemic patients is due to hypoglycemia" (p. 29).

CAUSE AND CORRELATION

The sample population of Incawatana does not, as a whole, reveal abnormal reactions to the glucose tolerance test, nor does any individual within that population show a reaction that would be clinically diagnosable as hypoglycemia according to contemporary medical practice. However, since Bolton *does* show a statistically significant correlation—although "only low or moderate" (1972:340)—between blood-sugar levels and aggression, it might be argued that my objections are entirely semantic. If we get rid of the ill-defined term "hypoglycemia" and substitute simply "blood-glucose levels," then Bolton would indeed seem to have supported his hypothesis that those with a middle-range drop in blood glucose of 10 to 25 mg during the glucose tolerance test are more aggressive.

A basic law of statistics must be kept in mind here: *correlation is not cause*. There is little attempt, either in Bolton's dissertation or in his article, to *isolate* blood sugar as a primary cause of aggression. An enormous body of data on the biological bases of aggression exists. A recent bibliography of aggressive behavior listing 3,856 books and articles on aggression includes only 14 sources related to hypoglycemia, and these were published mainly during the '30s and '40s (Crabtree and Moyer 1977). Moyer (1968, 1971), perhaps the leading researcher on the physiological bases of aggression, specifies a number of blood-chemistry influences but puts the emphasis on hormonal imbalances. In a comprehensive review of the literature, Brain (1977) provides a bibliography of more than 500 articles and books relating hormones to aggression, but this factor was not considered in the Incawatana study. If aggression among the people of Incawatana is part of a complex system, as Bolton himself argues, then we might reasonably expect that many correlations with aggression would be possible. Actually, Bolton (1972:277–332) does report positive correlations between aggression and diet, population density, disease, coca and alcohol consumption, and hypoxia (symptoms relating to low oxygen at high altitudes). The correlation with "hypoglycemia" is stronger, but, given what has been said about contemporary medical opinion on this subject, this does not seem to justify the assumption that low blood glucose is a primary cause. What Bolton does show, and shows quite clearly, is that aggression is caused by a multitude of interlocking stressors—many of which are by no means common to all "Qolla" communities. Incawatana would seem to be not only exceptional in terms of aggression, but also exceptionally backward, disorganized, and poor in diet.

Using Bolton's data, an alternative "primary-cause" hypothesis easily suggests itself: that *both* aggression and low blood sugar are caused by alcohol ingestion. In 1967 Mancilla asserted that Peru had for the past 11 years held top position in a world ranking of drinkers (quoted in Bolton 1972:323), while consumption among the "Qolla" is 154% of the national average. Bolton (1972:324) cites a 1955 study to the effect that hypoglycemia causes alcoholism by creating a craving for the pick-me-up of a drink. It is now known that this view, once widely held, is incorrect, as is the corresponding view, largely derived from animal studies, that alcohol raises blood-sugar levels (Marks 1975:377). Just the opposite is the case: alcoholism causes a drop in blood sugar. Neane and Joubert (1961) studied 23 South Africans with spontaneous hypoglycemia after excessive alcohol ingestion. By 1965 over 100 cases of alcohol-induced hypoglycemia had been reported in the literature—most, predictably, from underfed areas of Africa, South America, and the southern U.S.A. (Marks and Rose 1965:258–61)—and today it is "known from hundreds of documented cases that alcohol-induced hypoglycemia is not rare—indeed, it should be included among the commonest types of fasting hypoglycemia" (Marks 1975:378).

Reactive hypoglycemia can also be directly attributed to alcohol ingestion, especially for people who have a poor diet. A study of nine alcoholics admitted to the Boston hospital showed that glucose impairment persisted for days after the subjects were put on normal diets and that this impairment showed up on the glucose tolerance test. Though there was a gradual improvement in glucose tolerance with a hospital diet, after several days a retesting of six of the subjects showed a decline in glucose levels below 50 mg three to five hours after the administration of oral glucose. The authors of this study suggest that the drop in blood-sugar levels was caused by impairment of carbohydrate metabolism due to alcoholism (Freinkel et al. 1963). In another test, in which insulin injections were given with alcohol, there was a drop 50% below fasting level after only 20 minutes, and the test group took much longer than the control group to return to normal glucose levels. When the experiment was repeated, changing control group to test group and vice versa, the results were virtually identical. This experiment showed clearly that alcohol directly causes a drop in blood glucose, while the opposite, that hypoglycemia causes alcoholism, is highly unlikely (Arky, Veverbrants, and Abrahamson 1968:575).

Such experiments are in line with Bolton's finding for Incawatana that "the higher the afternoon glucose level the less involvement in drinking; and . . . the subjects who maintain high glucose levels through the fourth and fifth readings of the GTT are less likely to be heavy drinkers" (1972:325–26). Bolton also notes (p. 324), not surprisingly, that "a high proportion of Qolla aggressive acts occur when people have been drinking." If we consider alcohol intake as the *prior* cause, this makes considerable sense of Bolton's finding that those with a 10 to 25 mg drop in blood-glucose levels were more

aggressive than those with lesser or greater nadirs on the glucose tolerance test. Thus an alternative hypothesis, entirely consistent with the Incawatana data, would be that those who regularly imbibe moderate levels of alcohol display higher rates of overt aggression than those who refrain from drinking or who drink themselves comatose. Or, in plain English, moderate drunks tend to be nastier than either sober people or falling-down drunks—quite possibly true, but hardly earth-shaking.

I do not mean to suggest a unicausal explanation for "Qolla" aggression, though I would find such a hypothesis highly plausible for at least some cases. Actually, there are probably numerous factors—social, psychological, and biological—involved, but there appears to be no particular reason to single out blood-glucose levels.

CONCLUSIONS

It should be emphasized that at no point have I challenged Bolton's data. Indeed, the magnitude and meticulousness of the Incawatana research testifies to exemplary fieldwork. Questions arise only in the generalization of a high aggression rate found in a single community and in the interpretation of the aggression/hypoglycemia correlation. Much of the information on the diagnosis of hypoglycemia and on normal glucose tolerance test responses was not available in 1972, when the Incawatana study was written, but it is now obvious that Bolton applied a diagnostic criterion that would have rendered any normal population hypoglycemic. If the glucose levels for the community of Incawatana were within the range of normality, it is difficult to see how this could be the primary cause of the abnormal aggression rate; one does not expect the ordinary to cause the extraordinary. And, as I have tried to show, the aggression rate—at least as measured by homicides—is indeed extraordinary compared with that for the Aymara who make up the vast majority of the Indians Bolton refers to as "Qolla."

My intention in this article is not to "refute" the hypoglycemia hypothesis—which would hardly be possible with a single study—but rather to open the doors for debate. Yet I hope this article will have a greater significance, namely, to dispel some of the mythology surrounding the Aymara personality. While the question of the psychobiological determinants of behavior may be of broad medical and anthropological interest, this particular case also has a strong ethical component. The negative anthropological stereotyping of the Aymara developed from early traveler prejudices, was codified through the modal-personality theories popular when Tschopik and La Barre did their studies, and then was quantified by Bolton.

Bolton certainly had no intention of vilifying the people among whom he worked both as Peace Corps volunteer and as participant observer. Indeed, he expressed the hope that his findings may be used toward "the solution of the problems which afflict the people of Incawatana" (1972:viii). The negative views of earlier fieldworkers were introduced only as a sort of "field" upon which he could work out the wider applications of his research. Yet whatever Bolton's intentions, the effect has been to provide a quantitative, "hard-science" foundation for a damaging stereotype that had previously rested on nothing more than intuition. The most immediate and tangible result was the *Science News* story (Trotter 1973) characterizing the "Qolla" as "the meanest people in the world" and "the most unlikeable people on earth." If, as I have contended here, this characterization is without substance, then a culture has been wrongly maligned.

As was once the case with the Kwakiutl and Dobu, the Aymara have been neatly epitomized by a handful of negative modal-personality traits, and the anthropologist who wishes to broaden this view is put in the absurd position of arguing what should be taken for granted—that these people, like anybody else, have a sense of humor and play, love their children and even each other, are often profoundly moral and religious, refrain from killing each other with any degree of regularity, and somehow muddle through like human beings everywhere. However, the dramatic and simplistic is far more durable than the ordinary and complex, so the negative "scientific" stereotype will undoubtedly persist. Unfortunately, such notions can no longer be confined within a closed academic community; they have a tendency to get back to the people themselves. Many Aymara, both educated and uneducated, are fully aware of their characterization in the anthropological literature and are deeply, and justifiably, offended.

Who, then, are the Aymara? If they are not the bizarre people they have been made out to be, neither are they noble savages. As Carter (1968:259) observes, noting their similarities to peasants in other societies, their harsh environment, and their history, they are exactly "what one would normally expect."

Comments

by JACK ADAMS
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It is ironic that this paper debates hypoglycemia and aggressive behavior among the natives of a remote South American country, when over 50,000,000 Americans have hypoglycemia in varying degrees of severity. Hurdle (1970) quotes a statement made in 1957 by S. P. Gyland at a meeting of the American Medical Association: "There is probably no illness today which causes such widespread suffering, so much inefficiency and loss of time, so many accidents, so many family break-ups, and so many suicides, as that of hypoglycemia." Hurdle goes on to say, "Ironically and tragically, over 25 years later we now have an American hypoglycemic epidemic!"

Hypoglycemia—an abnormality of metabolism that results in a precipitous drop or flat curve in normal blood-sugar levels—was discovered in 1924 by Seale Harris, who 25 years later received the Distinguished Service Medal for his research from the AMA. Unfortunately, it is still difficult to find a physician who has a thorough and comprehensive understanding of this problem, its symptoms, diagnosis, and treatment.

Nathan Pritikin, Director of the Longevity Research Institute in Santa Barbara, Calif., has suggested that hypoglycemia is intimately related to an individual's diet and, more specifically, to his or her overconsumption of refined and/or processed carbohydrates. In addition, Philpott (1977) has clinically observed that hypoglycemia is caused by specific allergic-like reactions to specific substances and not just restricted to carbohydrates. Unfortunately, neither Lewellen nor Bolton makes any reference to the diet of the subjects used in their studies. Such information would help to clarify the real issues in the controversy.

Lewellen appears to regurgitate the arguments of the American Medical Association and fails to mention opposing viewpoints from the voluminous literature published by orthomolecular-oriented physicians and researchers. Readers should judge for themselves after consulting, for example, Cheraskin et al. (1974), who cite studies demonstrating a relationship between hypoglycemia and juvenile delinquency and argue that in adults faulty or insufficient nutrition may impair mental functions, manifesting itself in tendencies toward aggressiveness, destructiveness, denial, and contradiction.

by PAUL T. BAKER
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As a biological anthropologist who has worked extensively with the native populations in the altiplano of the southern Andes, I should celebrate the hope that Lewellen's article may help correct the unfortunate myth of the unusually aggressive Aymara. Even more, I should be content that the dangerous concept that a population-level aggressiveness can be tied to low blood-sugar levels has been challenged. Instead, I am disturbed about what both the acceptance and the refutation of these myths connote for anthropology as a scientific discipline.

With diligence, many anthropologists, including Bolton and myself, have attempted to perform the anthropologist's role of overviewer of the human condition, including both the biological and cultural components. The controversy reviewed by Lewellen reflects our frequent failure to fulfill these roles with scientific competence. In a recent review of our research in the southern Andes (Baker and Little 1976), Bolton (1979) noted the lack of adequate sociocultural studies. I agree, but I must add that we tried to involve more cultural anthropologists without success.

The hypothesis that hypoglycemia could produce unusual aggressive behavior in a whole population was rejected at an early stage by most well-trained human biologists, and I am somewhat surprised that it is still considered useful to publish a refutation of it in a major journal such as CURRENT ANTHROPOLOGY. The focus of this problem for anthropology as a discipline is shown in Lewellen's article, in which he cites the original and scientifically acceptable articles on Aymara behavior but is willing to use scientifically unacceptable sources such as *Time* and "most doctors' opinions" for the biological information. Is anthropology to be characterized by competence in social science but total incompetence in human biology? I hope not. If the biology of anthropology is to be reestablished, there must be better collaboration than that apparent so far between sociocultural anthropologists and anthropologists trained in human biology.

by JOSEPH W. BASTIEN
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My experience with the Aymaras of Bolivia from 1963 until 1969 and with the Qollahuayas (a cultural subgroup of the Aymara) from 1972 until 1980 has been very positive (see Bastien 1978, Bastien and Donahue 1981). I found these Andeans sensitive, intelligent, hard-working, witty, and persistent and not nearly as aggressive as many Texans I know.

Aggression in both instances is often caused by excessive drinking. Among the Aymaras, and other Andeans as well, violence often occurs during fiestas, when there are ritual obligations to toast each other and offer libations to Mother Earth. Traditionally, the ritual drink was chicha, fermented corn; now it is raw alcohol, which is upwards of 150 proof. Consequently, many participants become intoxicated early in the three-day fiesta, and a lot of accidents occur; people have hands blown off from delayed reactions in setting off dynamite, fall asleep on the path and freeze to death, slip off ledges, and suffer severe burns. Sober Andeans seldom suffer such accidents. Fights do occur during fiestas, but most result in deep head wounds and broken collarbones; seldom is someone killed. This brings a curse to the fiesta.

There is indeed a certain amount of tension in the Andes, and I attribute it to a real basic conflict between traditional symbolic systems and external economic and political forces. In Kaata, Bolivia, for example, Andeans fought for 200 years to have Niñokorin, the lower part of their mountain, restored to their ayllu, which they understood according to a mountain-body metaphor (see Bastien 1979). Colonizers had confiscated Niñokorin for a hacienda, and this caused a discrepancy between the body metaphor and the mountain metaphor. The struggle was an attempt to eliminate the discrepancy. This provides a cultural explanation for the "violence" in the Andes that Aymara ethnologists have overemphasized. Moreover, it places the cause of the "aggression" on foreigners, who have divided the land of Andeans without regard to basic Andean patterns.

by AMITABHA BASU
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The search for biological determinants of behavioural traits has always been a favourite endeavour of scientists and laymen alike. Some behavioural traits do indeed have biological causes, the classic example being that of mental retardation associated with trisomy of Chromosome 21 in Down's syndrome. But extrapolation from the Down's-syndrome (or a similar) example to assign every behavioural trait a biological cause is conceptually unwarranted. Equally unwarranted, methodologically, is the effort to prove or disprove a hypothesis without appropriate methodological rigour. Both Bolton's hypothesis about the control of a behavioural trait (aggression) by a biological determinant (hypoglycaemia) and Lewellen's refutation of the hypothesis suffer from conceptual and methodological problems.

In my opinion, four separate questions have become confounded in this debate: (1) Are the Qolla aggressive? (2) Are the Qolla hypoglycaemic? (3) Is there a causal relationship between aggressiveness and hypoglycaemia? and (4) Is it ethically desirable to ascribe an aggressive (or any other) stereotype to a people when such ascription may be utilized to denigrate them?

On the first question, Bolton presumes that the Qolla are aggressive, while Lewellen contests this. Indeed, aggression, defined, in general, in terms of causing harm to others and, more specifically in the case of humans, in terms of *the motivation for* causing harm to others, is not adequately measured by homicide rate, subjective rating by local raters, and/or involvement in litigation—the traits used by Bolton. Further, as Lewellen points out, the village of Incawatana may not be representative of the Qolla people in general. (Generalizing from data on a single village, not particularly chosen for representativeness, is a common methodological error of anthropological studies.)

On the second question, Lewellen has convincingly demonstrated, citing the relevant literature, the methodological weaknesses of Bolton's characterization of the Qolla as hypoglycaemic.

On the third and methodologically the most important question, both Bolton and Lewellen leave much to be desired. Specifically, even though the Qolla of a particular village, i.e., Incawatana, may be both aggressive and hypoglycaemic, as claimed by Bolton, or nonaggressive and nonhypoglycaemic, as claimed by Lewellen, the two traits may not be statistically associated/correlated, much less causally related. The cooccurrence of aggressiveness and hypoglycaemia may well be due to the relationship of each to a third trait, e g., alcoholism, as is suggested by Lewellen. (Interpreting cooccurrence in a single village in terms of statistical association/correlation and interpreting the latter in terms of cause-effect relationship is another common methodological error of anthropological studies.) Indeed, in proposing his "model of the bio-aggression system of the Qolla" Bolton (1973:fig. 2) himself implicitly denies a straightforward causal relationship between the two traits. To test Bolton's hypothesis it is necessary to (1) examine

whether other allegedly aggressive/criminal people are hypoglycaemic, (2) measure aggressiveness and hypoglycaemia using more sophisticated methods than Bolton's, and (3) enquire into the cause-effect relationships that Bolton proposes but does not probe.

On the fourth and conceptually the most important question, the negative stereotyping of the Qolla personality (even though their alleged aggressiveness is yet unproved) and the suggestion of a biochemical cause for it (even though this causation is yet undemonstrated) may be misused not only to damage the Qolla image to the outside world, but also by implication to offer some "hard-science" rationalization for the negative stereotyping of other communities as "fierce people" (the Yanomama of Latin America) "wayward servants" (some Bushmen and other hunters of southern Africa), "criminal tribes" (the Lodha of eastern India), etc., which is ethically unjustified.

Lewellen submits that his intention is "not to 'refute' the hypoglycemia hypothesis . . . but rather to open the doors for debate." In my opinion there is precious little room for debate; Bolton's hypothesis is too flimsy to deserve further attention.

by PAUL F. BROWN
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While Lewellen's rebuttal of Bolton's aggression-and-hypoglycemia hypothesis is of interest to Andean specialists, its significance goes beyond regional interests by underscoring the rather befuddled state of current anthropological and other knowledge concerning the relationship between culture, biology, and behavior. Lewellen argues, quite correctly, that in order to conclude that a biological characteristic or condition (in this case, hypoglycemia) causes a particular behavior (violent aggression), one must demonstrate, clearly and independently, three different propositions: (1) that a population exhibits a particular behavior, (2) that a population has a certain biological condition, and (3) that this biological condition, rather than other factors, is the principal *cause* of the behavior.

In the case of Incawatana, there seems to be little doubt that the population behaves in an unusually violent and aggressive manner compared with other Quechua and Aymara communities. Although I find reported murders problematic as a measurement of aggression for other altiplano communities, I do not question Bolton's data for Incawatana. The second and third propositions are much more difficult to prove. Are the Aymara hypoglycemic? Lewellen shows clearly the problems involved in the diagnosis of this condition. Even if we dispose of the term hypoglycemia, as Lewellen suggests, and look only at blood-glucose levels, problems of interpretation remain. This brings us to the third proposition, that low blood-glucose levels cause the people of Incawatana to behave aggressively. Lewellen presents an impressive array of research results which show that no causal link exists between blood-glucose levels and aggression, but he fails to consider the problem of comparability in his use of these findings. In all of the studies he describes, reference is made to "normal subjects." However, what may be normal for one population may not be normal for another in terms of nutrition, habitat, energy expenditures, indicators of health status, measurements of growth and development, and, perhaps, blood-glucose levels. That no relationship has been found between blood-glucose levels and a set of symptoms in *Western* populations is no assurance that such a relationship does not exist in others. Given the vast array of differences in culture and environment between the people of Incawatana and the subjects of most published medical studies, it is not unreasonable to hypothesize differences in physiology which may result in symptoms or behaviors not encountered in Western populations.

Even if it can be shown that no causal relationship exists between blood-glucose levels and aggression in Incawatana, we are still left with the problem of explaining the unusually aggressive behavior. Lewellen's alcoholism hypothesis suffers the same shortcomings as Bolton's: while the ingestion of alcohol may result in lowered blood sugar, we do not know that it *causes* aggressive behavior of the sort responsible for murder. Again, even if it were true, why is it more frequent in Incawatana than in other altiplano communities? Perhaps, as Fals-Borda (1955) argues for peasants in Colombia, involvement in the world of the mestizos has resulted in the breakdown of sanctions in Incawatana which normally guard against violent outbursts. In one of the altiplano communities I studied (Brown 1978), traditional Aymara practices of reciprocity and mutual aid were abandoned in favor of more individualistic activities as the community became more involved in the cash economy of Peru. However, while the people did express less trust in their neighbors than people in other communities, no greater incidence of violence was reported.

Lewellen does not supply us with an explanation for aggression in Incawatana, but he does point out the pitfalls awaiting the anthropologist who insists on searching for "primary-cause" explanations at the expense of other, related variables. Until we know more about how the history, environment, culture, and biology of the Qolla result in "what one would normally expect," we are not in a position to explain adequately the unexpected.

by HANS C. BUECHLER
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Lewellen's article is a welcome contribution to the rectification of the dismal image prevalent particularly in the earlier literature on the Aymara and, to a lesser extent, the Quechua. As Carter (1966) points out, this image may have been adopted by early anthropologists from their fieldwork assistants, who were frequently mestizos. It goes without saying that class antagonisms breed negative stereotypes. That such characterizations of entire societies should still be taken seriously by social scientists is unfortunate indeed. Equally unwarranted is the generalization of findings in one community with an extraordinarily high rate of homicide to all Quechua and Aymara. Like Lewellen and many other modern researchers, Judith-Maria Buechler and I did not find the Aymara any more aggressive than any other society. Indeed, the Aymara generally treat one another with respect and an often highly formalized and elaborate etiquette, and they tend to avoid confrontations in daily living as much as possible. Judith-Maria Buechler found confirming evidence in her observation of children's play, which one would expect to reflect adult personality traits. These games were remarkably free of aggressive and, to a large extent, even competitive content.

Lewellen's hypothesis that the seeming connection between hypoglycemia and aggression in Incawatana may result from differences in alcohol consumption bears further investigation. Aggression among the Bolivian Aymara of Lake Titicaca with whom I am familiar is much more frequent during fiestas, when it erupts under the influence of alcohol. In fact, fiestas are the only context in which aggressive behavior—although always disapproved—is expected to occur and even deemed excusable. The involvement of sober individuals in a brawl of this kind is considered highly reprehensible, even under extreme provocation by an intoxicated person.

While intoxication clearly serves as an outlet for aggression, the public nature of traditional Aymara drinking patterns provides built-in limits. The Aymara take considerable care to

reduce aggressive behavior as much as possible. Quarrels are immediately broken up by kin when they occur, and the protagonists are restrained or even removed from the scene. The brawls I observed between 1961 and 1976 during dozens of fiestas and rites of passage (cf. Buechler 1980) usually lasted only a few minutes even when they represented the culmination of interpersonal tensions built up over years. Fiestas may thus provide catharsis while reducing the danger of serious harm.

Aggression during fiestas must be seen in the wider context of the functions of drinking in Aymara rituals. Alcohol is also the symbol of positive human relationships. As the Aymara see it, a drink represents the affection of the person who proffers it. To refuse it amounts to a denial of an interpersonal tie. Fiestas, then, are rites of intensification designed to bring out both positive and negative interpersonal feelings and put them on display.

I hope that Lewellen's article will put an end to the maligning of two societies and serve as a warning of the pitfalls in attempting to correlate physiological variables and personality traits, whether fictitious or real.

by CURTIS R. CADORETTE
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This article is both timely and articulate. The prejudices created by certain anthropologists were long overdue to be debunked and the Aymara to be described as normal human beings. The black legend of the "treacherous and vindictive" Aymara is simply disguised racism. Lewellen is to be congratulated for laying to rest the unsubstantiated opinions fed to anthropologists by mestizo informants, whose bias against the Aymara is all too easily documented.

As Lewellen points out, many researchers have had little or no understanding of the Aymara language. This implies little knowledge of Aymara social structure or mores, both of which are highly evolved but truly accessible only through the language. It is no coincidence that researchers who have lived in villages and attempted to use Aymara have reached strikingly different conclusions from those who have lived in towns, with their mestizo values. One need only the most minimal experience of the altiplano to realize that town (*pueblo*) and country (*campo*) represent two visions of reality which have been pitted against each other for some four centuries. The Spanish-speaking mestizo has little use for the Aymara-speaking *campesino* and vice versa. The former have been exploiters and the latter their victims. The use of mestizo informants has directly introduced their prejudices into the supposedly impartial work of researchers, whose goodwill perhaps should not be doubted but whose methodology should. The question of hypoglycemia is nothing less than the red herring Lewellen shows it to be.

The article alludes to the social exploitation and the pattern of passive-aggressive behavior which admittedly exist here. It is unfortunate that the socioeconomic etiology of these phenomena is not scrutinized more closely. It seems that far more insight would be gained into the Aymara world if the social mechanisms of the altiplano were more adequately understood. Knowing a bit more about the concrete effects of racism, economic feudalism, and linguistic discrimination would be more helpful in understanding the Aymara than determining their glucose tolerance.

The previous research on the Aymara, at least in certain cases, is best laid aside as otiose and methodologically incorrect, as this article intimates. The Aymara are as articulate and developed as any people when one allows them to speak on their own terms in their own language. Their peculiarities as a people are due to a special history and geographic location and are better perceived as assets than as liabilities.

by WILLIAM E. CARTER
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The debate that Lewellen opens here underscores two problems that have plagued anthropology since its inception: (1) the bias of the discipline in favor of the bizarre and (2) the ease with which unicausal explanations are proposed and accepted.

By now the negative stereotypes of the Aymara should have disappeared from the literature. Practically everyone who has worked with that ethnic group over the past 20 years has spoken to the issue, and most have agreed that, if the Aymara have any antisocial complexes, these can be attributed to their having been the victims of prejudice and exploitation for more than 500 years and are much more apparent in their dealings with outsiders than in their dealings with other Aymara. If this article has significance, then, it is not that it dispels "some of the mythology surrounding the Aymara personality."

One of the problems in relating Lewellen's comments to Bolton's work is the fact that Bolton worked with a Quechua- rather than with an Aymara-speaking group and obscured that fact by calling the group "Qolla." Having worked in Bolivia with both Aymara- and Quechua-speakers, I am convinced that, in spite of the fact that most of their cultural traits are shared, there are some basic differences. I found that Quechua-speakers, for example, were much more likely to engage in everyday social or solitary drinking than were Aymara-speakers, whose drinking patterns tended to be far more ritualized and festive in nature. Since alcohol is a depressant and since it is known to lower inhibitions, its repeated consumption, rather than the presence of hypoglycemia, could well be seized upon as the "primary cause" of abnormal aggression that Bolton is prone to speak of.

Lewellen has done all of us a service in raising statistical and factual questions with regard to the homicide rates reported by Bolton, and he is correct in questioning whether homicide rates can be taken as a direct index of overall aggression, in that the latter is such a broad concept that it cannot be quantitatively compared. However, his reliance on unquantified, verbal ethnographic reports in arguing that the homicide statistics provided by Bolton for Incawatana are atypical must be called into question. In my several years of residence and work on the Bolivian altiplano, I only rarely heard of a homicide. Yet there could have been many that I never heard of, for I never specifically explored the issue. Until we have carefully controlled data on homicide from a number of randomly chosen sites around Lake Titicaca, we can never know whether the figures presented by Bolton are representative. In this sense, the burden of proof is on those who wish to detract from or refute Bolton's arguments, not on Bolton himself.

I congratulate Lewellen on reviewing recent writings regarding hypoglycemia and on questioning Bolton's overdependence on the findings of a single diagnostic tool, the glucose tolerance test. Lewellen fails to take into consideration, however, the recent work of Burchard that indicates that coca can be a glucose regulator and that it therefore can play an extremely important role in controlling the hypoglycemic tendencies of high-altitude populations.

Overall, Lewellen raises far more questions than he answers. He demonstrates how easy it is to fall into the traps of overgeneralization, misuse of statistics, premature conclusions, and unicausal hypotheses. The challenge now facing those who work in the high Andes is to go beyond such crudity, carefully to gather statistical, cultural, economic, psychological, and biomedical data, and only then to attempt an understanding of the complex facts and variables involved. Fully to explore the questions raised here will require close collaboration with specialists from a variety of fields, but especially with those

working in bio-medicine. The day has passed when anthropologists can tackle such complex issues alone.

by RANJAN GUPTA
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Whilst Lewellen's paper is thought-provoking and worth incorporating into the literature, it is not logically satisfying. I am skeptical about his claim to "another look," because the mere fact that the Aymara are nonaggressive and nonhypoglycemic neither proves nor disproves that these two factors are unrelated. Since we know that high-altitude hypoxia may disturb glucose metabolism, which in turn may evoke hunger, irritability, etc., the possibility of a hypoglycemia-aggression causal relationship cannot be ruled out without experimental verification. It is important to examine as many different populations as possible with respect to the two traits under consideration, using standardized techniques and a strict experimental design.

An important issue is raised in this paper that has been bothering me for quite some time—the issue of the ethical implications of a study of this kind. I fully agree with Lewellen that the time has come to expunge the "negative anthropological stereotyping" of the Qolla, initially made by early travellers and amateur ethnographers and later supported by anthropologists. The same applies to the negative stereotyping of other communities, e.g., Yanomama (fierce), Bhotia (cunning and treacherous), and Lepchas (timid and submissive). The Lepchas—an indigenous population of Sikkim and Darjeeling district, India—are very much disturbed by the early ethnographers' imputation to them of timidity and submissiveness. It is unfortunate that present-day anthropologists still employ these negative stereotypes, for in doing so they help to maintain a bad image that had no rational justification in the first place. The Lepchas are like any other people on the globe, neither better nor worse. Anthropologists should be friendly to the people with whom they are working and responsive to their problems rather than helping to perpetuate misconceptions and negative stereotypes. Notwithstanding my reservations about Lewellen's methodology, he is to be congratulated for bringing this ethical issue to the surface.

by WILLIAM J. HUDSPETH
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Both Bolton (1973) and Lewellen have been misled by significant empirical and conceptual problems inherent in research pertaining to reactive hypoglycemia.

Persons who experience neuropsychological (sensory, motor, psychological) or sympathetic (sweating, headache, fatigue) disturbances after eating carbohydrates (as during a glucose tolerance test) may be considered candidates for the following diagnoses: (*a*) reactive hypoglycemia, (*b*) transient brain dysfunction, or (*c*) emotional disorder. Basic principles of diagnosis would presume that measures relating to all of these options be obtained during the glucose tolerance test. As surprising as it may seem, direct evidence for transient brain dysfunction has not previously been considered in research dealing with reactive hypoglycemia.

The relationship between blood-sugar levels and neuropsychological disturbances is precisely as Lewellen reports: nonexistent. Nevertheless, the types of symptoms to which Bolton (1973) refers do occur during glucose-tolerance evaluations (Clemmer 1977). Thus, when the test fails to assess brain functioning, the limited diagnostic options which remain (*a* or *c* above) can only be expected to support the contradictory opinions characterized by the Bolton and Lewellen papers. Therefore, it seems probable that a mechanism other than hypoglycemia may account for the documented symptoms arising during a "hypoglycemia" episode.

In recent investigations, we expanded the scope of glucose tolerance evaluation to include all possible diagnostic options (Hudspeth 1980, Hudspeth and Peterson 1981). Serial recordings of electroencephalograms (brain electrical activity) and symptomatic disturbances were used as the objective measures of transient brain dysfunction. Abnormal EEG's (epileptiform activities) occurred in 27% of our subjects at the times they experienced neuropsychological disturbances. This evidence of transient brain dysfunction was unrelated to the test criteria typically used to diagnose reactive hypoglycemia. Furthermore, several subjects exhibited two-stage reactions. First-stage disturbances always occurred when blood-glucose levels were near peak values; second-stage disturbances followed the lowest (even nonhypoglycemic) blood-glucose samples. Depending upon the type of test reaction seen in each subject, two-stage reactions appeared precisely at the periods of peak insulin release and inhibition respectively (Hofeldt, Dippe, and Forsham 1972, Hofeldt et al. 1974). After replicating these observations with insulin measures, we now believe that excessive insulin levels (in response to ingested glucose) are directly responsible for increases in brain-cell electrolyte and water content (e.g., hyperosmolality) leading to seizure activities and brain edema (Arieff et al. 1974). These changes give rise to the neuropsychological disturbances traditionally but erroneously attributed to hypoglycemia.

Bolton's observations and conclusions may well be correct as they pertain to the Qolla community. However, there may also be procedural limitations upon his conclusions, as is aptly noted by Lewellen. In light of the progress we have made in this area, studies of the sort reported by Bolton are of the utmost importance. We, however, would like to see such work carried out in industrial societies, where dietary, aggressive, and criminal behaviors suggest sizable risk factors.

by WESTON LA BARRE
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Lewellen makes an exemplary contribution, with which I largely agree. Bolton studied a Quechua-speaking community, which in ethnographic space and historical time can only dubiously be equated with the colonial Colla; and the alleged psychological qualities at issue are those of the Aymara, not of the Quechua, who have not been so described.

It is an irony that a would-be biological explanation should in fact be so unbiologistic. If, as we can suppose, the environmental conditions have persisted for some time, it would be quite surprising if over millennia there had been no *biological adaptation* to ecological conditions. In parallel conjecture, one might just as well attribute Aymara ethos to anoxemia as to hypoglycemia, yet Monge has argued a massive *adaptation*, in Andean man's enormous chest and lungs; and even a healthy young Caucasian male's red blood cells characteristically increase from some 5,000,000 to 7,500,000 per cc in a mere few weeks on the Titicaca plateau, perhaps with the traditional stopover at Tia Bates's *pension* in Arequipa halfway up.

When Lewellen quotes Tschopik to the effect that the Aymara have been "described in remarkably consistent terms by several independent observers," it should be emphasized that a near-dozen men—Scot, French, Belgian, and American—over a span of 85 years (1861–1946) described them thus, and Tschopik himself joined me in this characterization. What needs to be explained is the fact of this striking consensus—now in direct disagreement with the reports of modern Aymarists such as Carter, Hickman, Osborne, and Plummer. I find no reason to discredit the earlier multinational consensus; nor can we

question the competence of the younger ethnographers (in whose consensus Lewellen joins).

Whereas the holistic anthropology I have long advocated takes more and more factors into account in explaining a complex cultural issue, the new "psychobiology" lops off the first half of its term and drastically shrinks the significance of the second. For "psychobiology" we have learned to read "genetic" or "animal-analogy." Yet a complete reversal in Aymara ethos in less than a century hardly argues a genetic cause. Ethos is evidently a complex psychocultural phenomenon, not to be contained by a *simpliste* reductionist "blood-rhubarb-level" explanation (a medical-student friend's locution). We have here a classic case of Alfred North Whitehead's "fallacy of misplaced concreteness," however meticulous the methodology. The change in ethos, I have argued (La Barre 1966), is not a change in genes or climate but a change in socio-politico-economic conditions following the Bolivian revolution—an evidently authentic series of social changes and not, as in most Latin American revolutions, a mere circulation of personnel in an unchanged system.

Furthermore, in taking off from the earlier consensus rather than the contemporary one, Bolton's 1972 study would seem to be reckoning without one's host ethnographically. Further still, as a measure of "aggressiveness" the moderate murder rate calculated from Father Madden's data agrees better with Tschopik's and La Barre's moderate estimates than with Bolton's. The figures are also moderate in comparison with Lewellen's Latin American rates.

Lewellen is quite correct in differentiating urban and *comunidad* populations, as I had earlier noted (La Barre 1948:39). I agreed already in 1948 (p. 40) with Lewellen that the traits attributed to the Aymara might better apply to the Bolivian mestizo, and I suspect that he is right that earlier ethnography may have been influenced by the use of mestizo interpreters, whereas modern fieldworkers have bilingual Aymara available.

About 1935, Sapir made the to me startling statement that pop medicine and dietetics were "branches of contemporary folklore." Bolton's hypoglycemia is evidently another case in point, along with cholesterol-phobia and the rest. Thus I would heartily endorse Lewellen's general conclusion that "there are probably numerous factors—social, psychological, and biological—involved, but there appears to be no particular reason to single out blood-glucose levels."

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The conclusion to Lewellen's critique leaves me a bit puzzled. Indirectly, it seems to corroborate the very position that Bolton has taken; much as Bolton (1973, 1976) has suggested, Lewellen argues that multiple socio-environmental and biological factors acting in concert make aggression more likely in certain villages—especially those which have had less economic and social contact with the outside. It has been proposed that Aymara may have lived in their exceedingly harsh environments possibly for as long as 12,000 years (MacNeish 1970), during which dietary deficiencies, cold, low oxygen pressure, and low food availability have been persistent selective pressures. Perhaps these facts alone validate the otherwise meaningless statement by Carter ("they are exactly 'what one would normally expect' ") with which Lewellen ends his article.

Lewellen's essay lacks the evidence he needs to back up his statements; no attempt has been made to replicate Bolton's findings. Clearly, it is more, rather than less, hypothesis testing that is required to sort out the many separable issues and points of contention in this debate. In Lewellen's essay, missing data are often replaced with innuendo. Some remarks appear to be aimed at discrediting Bolton as much as at criticizing his work. For example, Lewellen implies that if Bolton had been providing services to the village (in addition to his research) he would not have been threatened with violence. It is really irrelevant that Bolton was, in fact, assisting villagers whenever possible, because the hypoglycemia-aggression model emerged not from an analysis of the number of threats he or his family received, but from a precise research design (as Lewellen himself describes it) which produced quantitative data in need of explanation. If, as Lewellen claims, an enormous amount of medical information now exists on hypoglycemia, I cannot help but wonder why a physician's comment from a *Time* magazine article is used to support one of his most important counterarguments. And why are not more of the presumably innumerable "recent ethnographers" who disagree with the earlier assessment of Aymara personality cited?

Lewellen charges that the scores used by Bolton to identify severe hypoglycemia do not correspond even to the minimum reading used by North American physicians in diagnosing hypoglycemia among North Americans. It seems important to point out that the readings relevant in properly diagnosing the degree of hypoglycemia suffered by Incawatana peoples would not necessarily be the same as those described for North Americans by North American physicians. It would be interesting to determine if behavioral measures such as Bolton's could be used to adjust or refine the diagnoses of North Americans; I am certain that North American physicians do not scrutinize and study the behavior of their patients to the degree that Bolton studied his subjects. In any case, there are major physiological and anatomical differences, due to natural selection, that, in addition to acclimatization processes, indicate differences in genetic structure between Aymara and local North American groups. Among these are greater ventilatory rates and greater residual blood volume, depressed growth rates (see Hoff 1972), greater ventilation-perfusion ratios, stocky body builds and short stature, and larger thoracic cavities among the high-altitude Peruvians. The differences in blood chemistry due to hypoxic stress and its relationship to basic metabolism surely must complicate and make imprecise comparisons of the test results of high-altitude Peruvians and North Americans (see Baker and Little 1976). For argument's sake, I would like to suggest that, rather than being excessive in labeling individuals mildly or extremely hypoglycemic, Bolton may in fact have been too conservative. Since for at least 12,000 years the Aymara have lived in an exceedingly stressful environment (from a biological point of view; see Thomas 1972), in which food and sometimes water are in short supply and the intense cold and low oxygen pressure impose additional metabolic costs, natural selection certainly has had time to select for certain attributes (see Baker 1969). Theoretically, selection should favor individuals with a high reserve of glycogen, which is stored in the liver and released by glycogon, secreted by the pancreas. Glycogen is metabolized into glucose and released into the blood when the energy requirements of the organism demand it. Experiments reported by Clegg (1978) demonstrate that this store of energy is greater in high-altitude-living rats than in others and is especially important during the first two days of life, before lactation by mothers has been fully established. He argues that, especially for the neonate, a deficiency of glycogen can prove fatal. Thus, individuals are born with high levels which serve to keep normal blood-sugar levels high and, in this environment, improve the chances of survival and reproduction. If this information is at all applicable to humans who live at comparable altitudes and in environments of great resource fluctuation and cold (and we are not yet sure that it is), then for such

peoples higher standing (i.e., normal) blood-sugar levels might be characteristic. In other words—and this must remain speculative—selection might favor individuals with higher glycogen storage levels which would make it possible on a normal basis to maintain higher glucose levels in the blood. This could both accommodate the higher metabolic demands imposed on a daily basis by low oxygen pressure, which requires greater cardiovascular efficiency (see Monge and Monge 1966), and make it more likely that seasonal energy deprivation would not prove fatal. From this perspective, one could argue that the low blood-sugar scores among the Qolla are relatively much lower and much more serious or dangerous than those same low scores when they are found among North Americans. The range of tolerance of blood-sugar-level shifts might be much more restricted in stressed than in nonstressed peoples, and the range of normalcy might be much higher in the first place.

The fact that this hypothesis can at least be entertained and would appear to be as valid as Lewellen's underscores the fact that only more physiological work in this area can resolve some of the aspects of this debate. For example, do women exhibit comparable fasting levels of the glucose tolerance test, and, if not, what cultural or biological mechanisms affect their fitness—and how much of a role do they play in contributing to, or diminishing the effect of, the hypoglycemia exhibited by some of the men?

by MARTIN K. NICKELS
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Lewellen's paper is an exemplary follow-up of another scholar's work. Too often these days, reinvestigations of previous studies are not held in as high regard as "original" research. It is especially refreshing to see that Lewellen does not engage in ad hominem attacks. His critique is conveyed in most honorable fashion; he has conducted himself admirably, and our discipline is the better for it.

When, as a graduate student, I read Bolton's paper on Qolla aggression and hypoglycemia, I first wondered where he had obtained his criteria for defining mild and severe hypoglycemia and, secondly, questioned the validity of his ranking individuals as to their aggressiveness on the basis of their neighbors' opinions. Lewellen seriously examines the first of these concerns but does not really explore the second.

His reevaluation of Bolton's diagnoses of hypoglycemia in the 66 Qolla males tested is justified. The medical literature now available on hypoglycemia seems clearly to support his conclusion that, while the Qolla subjects did show drops in their blood-glucose levels, they were not clinically hypoglycemic. But is that the real issue? Bolton attempts to show that the drops are related to aggressive behavior, not that they have to be of sufficient magnitude to be so related. One interpretation Bolton offers for the apparent association in his data between levels of aggressiveness and reactions on the glucose tolerance test in his Qolla sample has to do with glucose homeostasis. In this interpretation, the individual whose blood-glucose level is falling may be able to raise it for a short time by engaging in emotionally charged behavior. It seems to me that it may not be necessary for glucose levels to drop to the point where they become clinically definable as hypoglycemic for this possible homeostatic response to be initiated. Thus, regardless of the accuracy of Bolton's test data or Lewellen's assertion that the very administration of the glucose tolerance test itself induces low blood sugar in healthy subjects, I think the model of glucose homeostasis should be investigated for its possible contribution to our understanding of the physiology of aggression.

My concern regarding Bolton's ranking of Qolla males in terms of their aggressiveness has given way in the past few years to concern over a problem that plagues any such study as his: the problem of defining "aggression" in the abstract and then classifying specific human behaviors as aggressive. As Bolton's study illustrates, it is difficult to analyze the biological bases of complex human behavior. This difficulty has been especially cited by critics of sociobiology and its application to humans. Therefore, while I applaud his effort, and especially his presentation of some stimulating possible explanations of the physiology of human aggression, I fear that a great deal of experimental laboratory work will be required before we can investigate seriously the biological bases of human aggression.

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13 I 81

Lewellen offers interesting and valuable criticisms of Bolton's well-known work on the biochemical basis of aggressive behavior. He indicates the weakness of Bolton's argument that the Qolla are aggressive because they are hypoglycemic by showing that (*a*) they are not aggressive (Bolton relies on early sources that misrepresent the Aymara and uses comparative homicide statistics incorrectly; I might add that his presentation of quotations and anecdotes is highly selective); (*b*) they are not hypoglycemic (Bolton's methodology was weak, as was his interpretation of the results of the tests he performed); (*c*) they are not even really Qolla (I would agree that the term *qolla* is not widely used, although small cheeses from the Department of Puno are known in some parts of the Department of Cuzco as *qolla kisu*, "cheese from the altiplano," and in some places on the shores of Lake Titicaca there are dancers called *qolla* who represent agricultural areas, in contrast to pastoral zones, urban settlements, and the lake itself [Michael Painter and Jane Collins, personal communication]). Lewellen not only shows that the Qolla are unexceptional when compared with other groups, but also attacks Bolton's examination of intrapopulation variation. He offers an alternative to Bolton's explanation that, up to a certain point, increasing hypoglycemia leads to increasing aggressivity (extremely hypoglycemic individuals being too weak to fight), suggesting that, in comparison with light drinkers, heavy drinkers are more aggressive and also more likely to test as hypoglycemic.

Lewellen also argues that the audience for whom these issues hold importance is much wider than a small circle of anthropologists. He believes that it is unethical to portray the Aymara as pathologically aggressive, particularly when it is not the case and when many Aymara-speakers take offense. He indicates that closer links between anthropologists and Aymara-speakers are the basis for correcting the stereotypes. I would add that this closeness is due not only to Aymara-speakers' having learned Spanish, as Lewellen says, but, among other factors, to some anthropologists' having learned Aymara.

Bolton is not quite as unicausal as Lewellen suggests. He has a double-headed arrow linking the terms "hypoglycemia" and "aggression" in a chart, for instance (1973:251), and he makes an effort to sort out direct and indirect causes of aggression. Nonetheless, his intention—to demonstrate that hypoglycemia is the principal cause of aggression among the Qolla—is clear from his three introductory paragraphs and his principal statement of his hypothesis (1973:243–44). Similarly, Lewellen may be correct in faulting Bolton for having an understanding of hypoglycemia that is at variance with that of the majority of the medical profession, but (as he mentions only once in passing) he cannot justly criticize Bolton for failing to take into account works published since he wrote.

Lewellen also fails to address the complexity of the processes which Bolton examines. In seeking to link biochemistry with social interaction, Bolton studies phenomena on very different levels of organization. Although many social scientists would be willing to recognize that social behavior has biochemical and neurophysiological underpinnings, they would generally be reluctant to explain the former by the latter because of the weight of a traditional academic division of labor, because of a resistance to reductionism, and because of the difficulty of examining the proximate mechanisms which transform physical and chemical processes into social ones. The efforts to link biological and social sciences and to move into new intellectual territory are admirable, but the difficulties of the enterprise should be acknowledged. Even if the Qolla did demonstrate high levels of hypoglycemia and aggression, it would still be an enormous challenge to clarify the relations of biochemistry and social interaction.

Bolton seems to assume that he does not suffer from these epistemological problems because of his focus on observable behavior, a reliance on replicable methods of data gathering, and the use of statistical manipulation of quantitative data (although it remains unclear to me how he calculated some of his percentages [1973:247]). Much anthropological research lacks clarity and precision; Bolton's efforts to introduce them are commendable, although there are means to this end other than his narrowly behaviorist ones. Furthermore, Bolton retains much confusion. Defining aggression as "behavior whose goal is the injury of some object" (1973:230) raises problems of intentionality and causality, as in the definition of insults, intrusions on personal space, and the like. Aggression is often ambiguous and culturally defined, as murder trials in our own society demonstrate. Bolton also includes other terms which are even more difficult to pin down, such as "moral code" (1973:229).

Lewellen's criticisms appear largely justified. His correction of the false stereotype of the Aymara personality reflects the greater presence of Aymara-speakers in political arenas, particularly in the regions in which they form the majority; different relations between anthropologists and the populations they study, notably less reliance on intermediaries and interpreters; and a greater concern for the social and political consequences of research—all factors which I believe are positive. However, the importance as well as the difficulty of linking biology and social science should have been addressed.

by D. TYAGI
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Psychobiological anthropological studies are needed, and Bolton's study was one of the earliest significant ones. Lewellen proposes to show that evidence that the "Qolla" possess a pathological personality type and that hypoglycemia is a primary cause of their aggressive behavior is 'minimal at best." He raises two basic issues: whether Aymara are aggressive and whether Bolton's method of typing people as hypoglycemic was correct. On the basis of his experience with these people and (recent) literature he suggests that Aymara are neither pathologically aggressive nor hypoglycemic. He demonstrates that these people are not aggressive *now*, but it is possible that they were when Bolton collected his data. On the second issue, the diagnosis of hypoglycemia, one may agree with Lewellen that the use of a drop in blood-sugar level from fasting level and the Dextrostix method is not a very accurate way of typing people as hypoglycemic. Now, if we agree with Lewellen's view that these people were not aggressive, this second issue is a nonissue. However, before we reject Bolton's hypoglycemic hypothesis we should collect more data from various communities using *similar* methodologies.

by EDWARD T. UYENO
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Lewellen points out that Bolton's criteria for diagnosing hypoglycemia are quite different from those employed by contemporary doctors and that Bolton's conclusion that there is an association between hypoglycemia and aggressiveness among the Qolla Indians of Peru is therefore questionable. He further argues that Bolton's subjects were selected from only one Qolla village and consequently his findings should not be generalized to Qolla culture as a whole. Although Lewellen does not comment on the conditions under which the glucose tolerance test was administered, it seems that more rigorously controlled conditions would be necessary to obtain reliable and valid data. Both Bolton and Lewellen discuss some of the important practical problems (for example, the implementation of the test schedule) and ethical questions (for example, the effects of a negative pseudo-scientific characterization) that an anthropologist must consider as he expands the scope of his interests to include the biological and physiological bases of personality and cultural behavior.

[Ralph Bolton reports that he is preparing a detailed response for publication in a later issue.—EDITOR.]

Reply

by TED C. LEWELLEN
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The commentators can be divided rather neatly into two groups: (1) ethnologists who have done fieldwork among the Aymara or Quechua and (2) a mixed group of biologists, medically trained anthropologists, and a psychiatrist who are knowledgeable about hypoglycemia but (Baker excepted) have not done research in the altiplano.

Of the first group (Bastien, Brown, Buechler, Cadorette, Carter, La Barre, and Orlove), *none found the Aymara to be exceptionally aggressive.* Since these anthropologists represent much of the finest research that has been done in southern Peru and Bolivia, I hope that this consensus will put to rest the hoary stereotype of the pathological Aymara personality. Beyond this, there is little agreement but much excellent criticism. Some points I found particularly useful are the following:

1. My rebuttal of the earlier view on Aymara personality did not take into account the possibility, suggested by La Barre, that they *were* hostile in the past but have become more benign because of social changes over the last 30–40 years. While I would need some convincing if this were applied to basic personality changes, it does seem reasonable that their attitude toward outsiders has become much less hostile and suspicious as their relatively closed social system has opened up with wage-labor migration to the coast and generally more interaction with the mestizo world. However, this is balanced by Brown's suggestion that they may be more liable to fight among themselves today because earlier sanctions have broken down.

2. I hesitated to make a point of Bolton's merging of Quechua and Aymara under the term "Qolla" because I was unaware of good comparative data on the two. Carter's note that he has studied both and observed differences is significant.

3. The question of homicide rates remains open, as several commentators note. While I believe my rate for Soqa is accurate and Jim Madden provides good broad estimates, these data

can hardly be extended to the altiplano as a whole. The fact remains, however, that there is *no* evidence supporting the generalization of Bolton's homicide figures to the Aymara and at least some quantitative evidence that they cannot be so generalized.

The second group of responses predictably focuses less on Aymara personality and more on the relation between hypoglycemia and aggression. What is surprising is the variety of criticism, ranging from total rejection of my argument (Adams) to total rejection of Bolton's study (Basu). This variety reflects the spectrum of opinion on hypoglycemia within the field of medicine and the contradictory results and interpretations of laboratory studies.

McKenna argues that, because of physiological adaptation to high altitudes, the Aymara store relatively more glycogen to be metabolized into glucose, with the result that the nadirs of their glucose tolerance tests, as recorded by Bolton, would be effectively lower than the same nadirs for an American population. This would be true if, as he suggests, the "Qolla" had "higher standing normal blood-sugar levels." Yet, in reality, these people have much *lower* normal blood-glucose levels than their low-altitude counterparts. Am I reading this right?

Also in reference to McKenna, I did not mean to criticize Bolton by innuendo in arguing that a resaercher's lack of community involvement might bring out peasant hostility. This was a direct reference to the early ethnographers whom Bolton quotes on Aymara aggression. Indeed, Bolton and his wife both supplied numerous services for the people of Incawatana.

The most severe criticism of my article comes from Adams, representing the orthomolecular school of medical research, which stresses the curative properties of megavitamins and diet. This sometimes radical movement—witness Adams's claim that there are 50,000,000 hypoglycemic Americans—is often in conflict with the more conservative opinions of the American Medical Association. It may ultimately turn out that the orthomolecular movement is the more correct, but this is by no means established, and I will stand by my statement that "most" doctors would tend to favor the definition of hypoglycemia I employ in my article. However, in fairness, it should be emphasized that the data I use are taken exclusively from mainstream medical journals and diagnostic manuals and thus represent mainstream, or conservative, medical opinion. Ongoing discussion in this journal will perhaps provide a forum for debate between these two opposed medical views on hypoglycemia.

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