The Tasaday Environment: Seventeen Years On

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Philippine Studies vol. 50, no. 1 (2002): 76–92

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In 1989, a version of this article was presented at a session on the Tasaday as part of the annual general meeting of the American Anthropological Association. It was withdrawn from publication (Headland 1992) due to irreconcilable differences between the editor's view of the Tasaday environment and mine. My withdrawal gave rein to even more imaginative analysis by someone who had never seen the Tasaday or their environment, as well as a withering attack on my field methods.

This article would not have seen print, were it not for the revival of what seemed to be, even in 1989 if attendance at the above-mentioned meeting was symptomatic, of decidedly waning interest, academically and generally. However, with the possibility of two books on the fate of small forest group likely to appear in the near future, it seemed that this account, made available on separate request to the two authors, should finally appear to balance if possible, some of the published conjectural versions of the Tasaday environment as it was at early contact.

I thank Father Joseph Galdon for his sympathetic reception of this essay; and I acknowledge the memory of Father Frank Lynch, under whose supervision I conducted fieldwork..

In 1972 I conducted ethnobotanical fieldwork among the Tasaday of Mindanao in two periods—late July to early September and for nine days in December—totaling only forty-one days. In a survey of subsistence agriculture in the Pacific Islands, such a period is not too short to achieve at least a qualitative understanding of the generally rather familiar production systems.

Working with a group of forest foragers however, I knew that I did not have the same advantage. For one, the collection of plants, the beginning for ethnobotanical research, was anticipated to be a large task, knowing in general terms the species richness of the tropical rain forests and, from others' previous research, the dissected landform of the Tasaday domain. Thus, with the time constraint known, I asked for (of Panamin) and received the assistance of two botanists—Hermes Gutierrez, Head of the Botany Division of the National Museum of the Philippines, his assistant Ernesto Reynoso, and Richard Elkins of the Summer Institute of Linguistics, an expert in Manobo languages[1] (by this time Tasaday had been identified as a Manobo language).

Unfortunately, Elkins was only with us for four days before he fell ill and had to leave. But in that short time, in addition to pursuing his own language interests, he helped to set up the form of enquiry on the uses of plants that could be easily followed, especially by my fourteen-year-old Tboli "minder" Dad Tuan from Panamin,[2] and not surprisingly, by Gutierrez and Reynoso, Filipino Austronesian speakers.

In the first fortnight then, we four collected over 200 plant forms and elicited from our Tasaday hosts the vernacular names of those plants, with information on their uses. Early in the exercise, we found that we could record parallel plant data in Tboli and Blit Manobo, since we had native speakers of both languages in the visitor's camp, as well as Sindi, Balayam's Blit Manobo wife. Since we were at our camp on the path to the caves in the afternoon, this allowed me to estimate the amount of food brought back by the Tasaday.

Our daily collection forays in the forest were organized into ecological zone coverage with at least one Tasaday informant; this gave us opportunities to check on indigenous naming and utilization information on plants previously collected. We were quick to learn the dangers of solo or unaccompanied walks in the forest, for many of the tracks away from the caves we could not read, but were shown the presence of pit traps, noose snares and the *balatik* spear traps whose lethal characters were intensified even by the fact that some were poorly constructed.

After 14 August, when the botanists left to begin the systematic identifications at the National Museum, the NBC-TV team arrived, and I have to admit that the trepidation that I had forcefully expressed about this clash of original schedules was mostly unwarranted. With Dad Tuan, the shape of our forest fieldwork underwent a reversal, in that now instead of the Tasaday following us from plant to plant for collection, we followed them on trips for plant food acquisition or the inspection of traps, and this was made easier because, unlike the previous week, when there were absences from the caves among the group (Yen 1976a, 168), everyone remained in the vicinity.

It was not difficult to see why: curiosity at the activities of a large number of strange but sympathetic people; the now continual presence of Manda Elizalde; and the impending birth of a child to Itut. The adverse effects for me—as I was to realize—were that Tasaday travel in the subsistence quest was limited during this period to short forays of not more than five hours, with or without Dad Tuan, and some rice was being given to our hosts, possibly distorting our dietary estimates.

Apart from the first phase of fieldwork of plant collection, the most profitable part of my study was in December, with Carol Molony as linguist, Dad Tuan, and another Tboli named Sol Sulan who operated the portable radio in case of emergency. The constraints on Tasaday subsistence behavior were this time not a perceptible consideration. Now we were to see what perhaps Carlos Fernandez and David Baradas may have seen in their unreported study of May 1972; that Tasaday life was not quite the original ideational construction, that the quest for subsistence had a somewhat harsher reality.

The combination of Molony and Tuan in their linguistic enquiries was to be a key to mine in regard to Tasaday subsistence, for there is no better check of plant use, animal use and acquisition methods than the spontaneous references to what we call "production system" made in conversation that can become part of published linguistic texts (Molony with Tuan 1976).

Referring to a diagrammatic representation of the approach to ethnobotanical research in the Tasaday domain, what we achieved ethnobotanically was certainly modest enough. The fifty-odd food plants consumed in forms of starch, as leaves or leaf-buds, flowers or fruit, fungi or tree exudates (Yen and Gutierrez 1976), even excluding the Dafal-introduced *Caryota* sago process, signaled a fairly wide-spectrum diet. In ethnoscientific terms, our field research was only a scratching of the surface.

With regard to the subsistence system, we were able to obtain a portrait of the Tasaday in 1972 that we called transitional, and to reconstruct the "historical" period under the influence of Dafal as a state of flux (Yen 1976a, 182). We even offered a prediction for the nature of transition that had to include the population factor, and the necessity for exogamy. But mistakenly we thought that, if left alone, the Tasaday mating pattern would follow their stated earlier custom of marriage with other forest groups. Perhaps we should have weighted more heavily the Tasaday statements that they could not find the Tasafang and Sanduka, and the effect of Elizalde's action in bringing the Manobo woman Sindi into the group from the village of Blit. Here I have reintroduced population, the socio-environmental factor that has to influence any consideration of the survival of the group in terms of the nature of its subsistence system.

The 1972 Subsistence System in Transition

The transitional character of Tasaday subsistence in 1972 may best be portrayed in two ways: the internal impact of outside influences and the involuntary effects of external environmental change. By that time it was well known that rain forests of Cotabato, like those of the rest of the Philippines and Southeast Asia, the Pacific Islands and the world, were undergoing increasing pressure through hardwood logging. At the same time there was an older trend, commercial agriculture which absorbed the rain forests of most of the riverine plains, bringing with it quite massive immigrations from other parts of the Philippines with their own subsistence requirements—and obviously, the entrepreneurs.

Then there were the subsistence farmers (always with a quotient of hunting-gathering in their economies), the owners of the land and probably descendants of the original Filipino colonizers of Mindanao who faced not only land alienation, but competition from new swiddeners. The much maligned swidden farming in its pure form is the least ecologically destructive of all three modes of production, as the account of Conklin (1957) of traditional Hanunoo agriculture illustrates with its cyclic climax forest-agriculture-fallow-forest sequences. In concert however, the three provide a virtually irrepressible strain on the last pristine environments whose only modifiers were the human groups who made their lives there.

Such is also the lot of the remaining subsistence farmers in the world's rain forests—even those of Mindanao in the domino effect that we have noted, and the Philippines as a whole. Neugebauer (1988, 29) in describing the struggle of contemporary Mayan swiddeners of the rain forest, stresses a major cause of food shortage as "a result of many-sided sociocultural conflicts," applicable to the Tasaday foragers of 1972 and indicative that, of itself, agriculture is not always a subsistence sinecure in situations of social stress.

The map of western Mindanao shows the nature of the terrain, an outline of the Tasaday reserve and the vegetational cover of the region. It shows the topographic nature of the Tasaday region and the outline of the reserve established in 1972. The same area shows the loss of forest cover due to logging and swiddening by new immigrants to the region.

I have been unable to find aerial photographs of this area from World War II that might convey something of the longer ecological sequence. But if it is accepted that the less-than-fifty-percent canopy cover consists of some of the effects of logging and swiddening, it is possible to suggest, with the huge expanses of plantation cropping, something of the pressure on the Tasaday domain. Perhaps even more drastic was the effect on the Sanduka and Tasafang, the other groups with whom the Tasaday intermarried.

From one direction, west of the Tasaday caves, Helen Mabandos (unpublished manuscript) traced the colonizing movement of the Blit Manobo during fieldwork in 1971 under Robert Fox. From outside of the reserve boundary, the Blit Manobo swiddeners migrated from settlements on the Tafal Creek system occupied in the 1940s, crossing to a northern creek system in the reserve to settle around 1951; then south to a connected system sometime in the mid-1960s. This series of small waterways was connected in the east to the Lawa River and Lamuton Creek near the Tasaday caves. It is perhaps an interesting coincidence that my own enquiries (Yen 1976a, 176) of Dafal placed his advent among the Tasaday at "as early as 1960"(rather than 1966–1967 as some publications have stated) when a hunter such as he would naturally use such passages in his pursuit.

By 1970 Blit had moved further southeast to the site that we knew in 1972. Already now close to the Tasaday, they were to move even closer eastwards along the Blit Creek in the next sixteen years according to unpublished field enquiries by Mabandos and Rogel-Rara (personal communication), and furthermore, the recollections of the older informants indicated that the pre-1940s movements began from much further to the northwest. This may give some indication of the pressure from one direction that we sensed in 1972, and perhaps the threats from the swiddeners, planters and loggers to the north and east were at least as severe.

The foregoing attempts to describe some of the background for the Tasaday fieldwork that we presented (Yen and Nance 1976) without excluding any of the data, secure or insecure. The main conclusion drawn on the Tasaday subsistence system was that the foraging range had to be bigger than formerly described (Fernandez and Lynch 1972). It followed that greater distances had to be covered in the food quest, and thus the neat food sharing that was observed at the caves was at most times a partial rather than consistently full congress of the group in the absence of various members at any given time.

I venture to suggest that full assemblage was more frequent during the Panamin period of Tasaday history. In itself, this is not a portrait of long-term internal adjustment; the intensification or concentration of foraging activity closer to the caves was a reactive response to the advent of Panamin and its perceived benefits—and one showing something of its fragility by the time of our fieldwork. The earlier technological influence of Dafal may be viewed as precipitating a more lasting internal adjustment to external factors. Given time and increasing contact, this dynamic situation could have led to the adoption of swidden agriculture. But in 1972 there was no sign of local production of crops, their transportation or consumption (other than Panamin rice) by the Tasaday.

Hunter-gatherers are not passive parts of the species roster of their environments; they tend to influence biotic composition in sometimes detectable parallels with agriculturalists (Yen 1989). Our observations of wild yam (*Dioscorea* spp.) foraging directed to the form the Tasaday called *biking*, as the principal but not only tuberous food, indicated an overexploitation of stands near the cave. The same pertains to the sago-producing *Caryota cumingii*, for the two palms not yet in fruit (the early stage of which is the best time for starch extraction) that were harvested during our visits produced only an estimated 12 and 20 kg of *natek* starch.

While the expected yields of the various sago palm genera recorded by Ruddle et al (1978, 62) cannot be directly applied to the Tasaday, some idea of more respectable production levels are indicated by *Caryota urens* from India ranging from 102 to 159 kg compared with *Arenga pinnata* of India, Indonesia and the Philippines from 25 to 75 kg and the commonest sago palm *Metroxylon* spp. from 28 to 272kg. By their own admission, the Tasaday said that higher yields could be expected with more distant palms; and during a helicopter survey in August, we observed taller specimens and thicker stands in the denser forests to the south and east.

The species of Caryota of the Tasaday forest does not share the alternative reproductive habit of adventitious growth of many palm species that provides replacement palms (or vegetative planting material for agriculturalists) after harvest. The introduction of *natek* processing by Dafal had its cost, for there was a trend towards the depletion of seedling stocks of palms providing the heaviest source of palm buds (*ubud*). Thus there was a further impetus towards the more extensive exploitation of the environment in which palms formerly enacted their full life cycle to die after seed production.

Of further importance is that the naturally-spent, fallen palm is also the major source of *libertad,* the sought-after grub in the diet. The Tasaday had apparently not developed a cyclic method of production—a kind of agronomy that could act to conserve for reproduction and provide for the variety of products that the palm can provide, including digging sticks.[3] The concentration of visitors, researchers and film crew in 1972 (Fernandez and Lynch 1972, 280), including our own expedition, made it difficult to detect such a trend if it existed. Certainly in December, natek was not made near the caves, but three foraging parties returning within a few days of our arrival brought only about 5 kg among them. It was unlikely that this reflected the total product harvested; rather, as did partial returns of meat to the caves from more distant forays (Yen 1976a, 173), it pointed to significant amounts of food being consumed by smaller groups away from their cave base.

With regard to the consumption of animals, it is interesting that in 1972, it was not a Tasaday perception that animals or birds near the caves had become scarcer. We were left with the impression that this was the consequence of rather poor application of Dafal's hunting lessons and a less than enthusiastic adoption of such food.

The estimation of Tasaday diet over five days in August 1972 included both natek and deer meat. Robson and Yen (1976) reported that the sampling indicated the daily protein and energy requirements of the group were satisfied by only 78 percent and 27 percent respectively of the dietary standards available at the time. Incidental ingestion of raw stream animals, fruit and flowers could not be quantified.

On newer standards issued by FAO/WHO/UNU in 1985, and taking into more detailed account the absences recorded in the sampling period in 1972 (Yen 1976a, 168) and using more accurate food values then were earlier available, Fankhauser (1992) has revised our figures for protein to 150 percent, and 42 percent for energy. Using figures for energy requirement more recently applied for Africa, the available energy for the Tasaday becomes a still-inadequate 57 percent from hunting-gathering during the sampling period. Notwithstanding the effects of applying different dietary standards, the gifts of rice by our party's Tboli and Blit Manobo support personnel (Nance 1975, 311; Yen 1976, 163) militated against any real accuracy of our determinations of the indigenous diet but, perversely, does reflect the Tasaday diet in transition. A further imponderable that was indicated in the original dietary reports was the incidental consumption during short-term foraging, including not only flowers and fruit, but also raw Dioscorea tubers, palm hearts, palm grubs and tree-trunk exudates.

Subsistence in Tasaday "Prehistory"

Much as we would have liked to construct a pre-Panamin, pre-Dafal Tasaday economy, and especially with excavational archaeology, we could not do so.[4] The chances of success are unenhanced by reflection over the time-gap since fieldwork, but we may reconsider some of the observations from 1972, prompted by the less personal comments that have been made by the hoax promulgators.

Nutritional Accounting

If our food sampling of five days at the cave is considered without the rice supplement, and accepting the circumstance that the normal range for foraging was narrowed for the Tasaday who stayed at base, the caloric and protein intake without the products of the Dafal-introduced hunting and Caryota starch extraction would leave even the population adjusted for absence and age of consumers at ridiculously low levels. The energy deficit would need to be made up with yam and leaf bud to five times the amount recorded, but protein without deer meat would require nearly eleven times the quantity of stream animals and weevil grubs (Fankhauser 1992, Table 3). While this is unlikely to dictate five or eleven times of foraging area, the implication of a wider range is inevitable (Yen 1976a, 171) to accommodate the requirements of a "prehistoric" diet.

Foraging Range

The foraging range in prehistory may never have been significantly greater in magnitude than it was in 1972 (even though we could only suggest that it was greater than 25 square kilometers) unless the size of the group was larger. This might have been the case periodically within Tasaday population dynamics over time. In both periods of our fieldwork, the "breaking out" from the seemingly sedentary group was observed, to the extent that at our unexpected arrival in December, all the Tasaday were absent from the main caves. While their re-appearance there in small groups over three days may be construed as a return from their unsuspected and unseen agricultural activities, their own, seemingly innocent explanations of hearing the helicopter while foraging, and expecting to see Manda Elizalde, could be acceptable in view of the natek and deer meat some carried.

The small quantities of food brought back to the caves might have been an indication of the carrying distance, but perhaps more significant in respect of the subsistence system, was the secular efficiency of smaller separate groups for capture and consumption in a wider ranging system.

The caves assumed a more cultural value as a central place which, despite its varying occupancy (Yen 1976a), expressed the social cohesion of the group. My surmise on the pattern of foraging as sectorial rather than concentric (Yen 1976a, 172), if correct, could have been influenced by the Tasaday-perceived encroachment on their earlier "unlimited" domain.

Yams and the Rain Forest

All of the above can be discarded on an emerging "universal" for human subsistence, presently identified by its vying authors as question or hypothesis. It is best expressed by Bailey *et al* (1989, 59) as "that humans do not nor ever have existed independently of agriculture in the rain forest."

Thus in pre-agricultural times, no one "existed" in any rain forest in the world. Headland (1987, 463) brings negative causal specificity to the contention with "wild starch foods such as yams were so scarce and so hard to extract that human foragers could not have lived in such biomes without recourse to cultivated foods."

Thus despite protestation to the contrary, at this time the Tasaday could not have existed in the rain forest of Cotabato—without agriculture. Of course this doubting of the hunter-gatherer estate in ethnographic populations is not new; Levi-Strauss (1963) addressed the issue of pseudo-archaism versus "real archaism," allowing however for the existence of the latter form. Bailey et al (ibid., 71) appear to make some concession to archaeological evidence "of numerous sites in rain forest environments in Malaysia," but to them perhaps, these may be exceptions proving a rule. They may have to watch, with more care than their six-line dismissal (65) conveys, the existence of the Australian rain forest dwellers of Queensland, whose subsistence systems (on a continent where no indigenous agriculture was practiced for 40,000 years) have been reconstructed by Harris (1978) from ethnohistorical documents soon after European contact with these people in the late nineteenth century. Horsfall (1987), an archaeologist, has uncovered occupation sites from the prehistoric Holocene in the same region.

There are some rules for hunter-gatherers disqualifying them as rain forest foragers that Bailey and his colleagues appear to be formulating: if they trade with non-rain forest dwellers (some Queensland rain forest people did, some did not); if they live near the sea (perhaps some of the Queensland Aborigines referred to, whose rain forest domains reached the seashore, may be excused for using marine resources); if they use fish from inland lakes; if they use the forest fringe.

Bailey et al (1989, 62) also join Headland in the yam question, in a physiological argument that is apparently supposed to demonstrate that yams cannot grow naturally in rain forest because they are "highly light dependent" and thus "once existed only in treefalls or along streams." Heliotropy is, of course, a characteristic of all Dioscorea, cultivated or feral, and is a strong feature of wild species, often forming joint canopies with the highest tree components in climax vegetation associations—at least in New Guinea and the Tasaday forest.

Headland (1987, 1992), in developing the null hypothesis in reference to the Tasaday, is at pains to conform with the latterly popular academic procedures for evaluation, citation counting. I have not had time to follow his exhortation to consult the extensive bibliographies of four papers on the question, but only two. And they partially. I stopped at the use and misuse of data more familiar to me.

Examples are the palaeoenvironmental reconstructions for New Guinea (Bailey et al 1989, 70) in which the indications of recession and spread of rain forests from the Pleistocene do not convey their impossibility as human habitats; one citation, Powell (1976, 177) even indicates their potential of exploitable species and ultimate domestication. Headland (1987, 473) quotes comments of Yen (1977) on the general absence of material evidence for root-producing plants in archaeology and the quite equivocal evidence for 10,000 year old agriculture at Spirit Cave in Thailand as support for "the wild yam hypothesis." The absence of such root plant evidence of course, does not mean such plants were absent or present in any site, nor indeed that humans can or cannot subsist in such environments.

The real issue here is the potential and actual human carrying capacities of rain forests—plural because there must surely be variability within the category—just as we agriculturally-based peoples can perceive as *difficult* , the lot of hunter-gatherers in the Arctic, the tundra, the deserts and the rain forests. The sweeping generalizations for the latters' inhospitality for human occupation ignore the fact of their variability, 30 to 40 rain forest sub-varieties according to Neuberger (1988, 7) quoting recent surveys by Walter (1973) and the World Resources Institute (1986), as well as the intravarietal variations (e.g. Whitmore 1975) that provide diversity of natural resource sets for human ecosystems.

The Bailey-Headland hypothesis seems to indicate that the human carrying capacity of rain forests is zero, the cause being the paucity of calorie-producing biota. Thus for them it is unnecessary to consider population, and foraging range whose effects are both quantitative and qualitative. But with the Tasaday example, the foraging domain is the question to which our 1972 data could only provide unquantified indications.

Headland (1987, 478) converts my assessment of Tasaday diet as low-level to marginal, cf. "certainly we have not returned to an application of a marginal or precarious mode"(Yen 1976a, 182). He also (Headland 1987, 1992) converts the gifts of rice by Panamin personnel into a regularly made-up deficit that allowed the group to live in the forest since 1971, and infers an agricultural source of dietary supplementation in the 1960s "wherever they were living."[5] Among the sources canvassed for such supplement is the growing by the Tasaday of root crops in small, secret gardens invisible to naïve visitors. Overall, I think that I prefer the paid actors or the School for Tasaday scenarios to this coy environmental construction as a bow to the hoax theory!

Population

Fernandez and Lynch (1972) recorded the population number of Tasaday at twenty-five in March 1972. Unay, a woman of child-bearing age had died apparently before the discovery of the group in 1971, while it was not until August 1972 that a review of the photographic records revealed that Ukan, a son of Unay and Lefonok, had also died sometime before March.

In April 1972, Sindi became the wife of Balayam and in September, Itet and Bilangin produced a fifth son. Until 1977, my information from Dad Tuan and John Nance was that these changes to the population had occurred: Bilangan and Itet had produced two more children, Udelen and Dul one, but the elderly deaf-mute couple Tekaf and Ginon had died, as had three of the children I had known, and another born after I had left.

I do not know the exact sequences of births and deaths, but the possible maximum number for the group during the 1972—1977 period was 30, with a minimum of 24 at the end.

Thus over a five-year span at least, variation in group number did not indicate the necessity for significant change in subsistence system. But was 30 close to marginal for the environment to maintain as a coherent group? Could future reductions indicate the measures that are hypothesized as population control among indigenous groups—partial emigration or division of the group, infanticide, invalidicide, senilicide?

When this account of population fluctuation reached me, I could not help but graphically recall the incident of 11 December 1972 and my insistent interference with the plan to take the sick Lobo away from the caves to die (Yen 1976a, 180). Perhaps Headland was right in applying marginality to the group of 25–27, but without the necessity to resort to agriculture for maintenance of that number. But it is subjective judgment on my part that he would be absolutely right now, if the reported number of 61 (Nance, in *Asiaweek* 1986) attempt to live in the 1972 Tasaday domain. Not only could the territory be expanded too far to maintain the solidarity of the group by the pattern of reassemblage at the caves, but the external modern pressures that we have earlier considered would come into even more immediate play with the shrinkage of the Cotabato forest.

Another factor of population is the genetic one. The hoax theory obviously eliminates the other forest groups, the Sanduka and the Tasafang, as part of an interbreeding unit. But if we accept these proveniences of the married women in the Tasaday group (excepting Sindi), and if we believe they could not be found in recent times, perhaps even as victims of the increasingly intensive pressure on the resources and people of the Cotabato forests and their margins, the Tasaday may be seen in a different and fateful light.

In 1972, there were, in biological parlance, only three breeding units or pairs in the "population," four when Unay was alive (her husband Lefonok being the brother of Udelen who with the Tasafang woman Dul formed one of the remaining pairs). Our understanding was that incest was not allowed, but even if it were, the sex ratio of all single persons as well as young children with its shortage of females (Fernandez and Lynch 1972) could only produce internally an unusual mating pattern.

The computer simulation model for Pacific island population isolates by McArthur et al. (1978) cannot be directly applied in its entirety to the Tasaday situation, but their results for populations with three parental sets, with the incest rule in place, are indicative of the dangers of extinction. In 120 computer runs, 92 were "headed for extinction" with only 18 "presumed successes," while the minimum probabilities for extinction of the group, with the same pair number and the ages of the women 18 to 29, ranged between 65 percent and 90 percent.

It may be thus suggested that the Tasaday, perhaps always in hazard with the limited gene pool in the intermarriage pattern with Sanduka and Tasafang,[5] may even have recognized the danger of extinction in 1972, expressed as firstly the pressure on Elizalde to replicate his action in bringing Sindi in from Blit, and later to seek to perpetuate an exogamous mating system along the track to the outside world that had been established by Dafal. The loss of intermarrying neighbor groups as a comparatively recent (but pre-1972) effect of the shrinkage of the forest may have had a greater negative influence on Tasaday survival than reduction of natural resources.

Subsistence and Tasaday Origins

Our exercise in the naming of plants (Yen 1976b) included a reduction from the collected species through plant forms to plant parts. With Blit Manobo and Tboli nomenclature recorded, it did seem that the basic plant vocabularies had more in common (with a reduced number of possible words) than that exhibited in species names. Independently of the linguists I found the published account of Cotabato Manobo subsistence by Lopez (1968) that not only gave an apparent cognate for the Tasaday word for replacement of harvested Dioscorea tuber tops in the ground as "planting," but in plant parts the correspondence of ten out of twelve words was higher than the Tasaday comparisons with Blit Manobo or Tboli.

While this was of minor significance, I was not surprised at the linguists' consideration of Cotabato Manobo in the prehistory and perhaps origin of Tasaday (Molony with Tuan 1972; Molony 1988). Such a Manobo connection is interesting in terms of the Tasaday subsistence system, for like the hunter Dafal and his restive connection with the

Blit Manobo swiddeners, the preference for hunting of some groups of the Cotabato Manobo despite the necessity for agriculture is reported by Kerr (1988, 144).

Can this preference be projected backwards in time? Could representatives of such groups be separated to become exclusively foragers? If the admittedly inexact lexicostatistical calculation of linguistic separation has validity for approximating the separation of Tasaday from its parental language, the estimates for AD twelfth to fourteenth century (Llamzon 1971; Molony with Tuan 1976) bring this "event" squarely into the period of Islamic trading in the Sulu region, and the prelude to colonization of Mindanao.

The account of this Islamic period by Patanne (1972, 306) and the coming of Sharif Kabungsawan in AD 1475, of forced religious conversion and the use of firearms, offers "an actual case history of an external influence finding accommodation on the local scene" (309). If this accommodation also produced refugees ahead of inland Moslem expansion, it could have been precursory in the process of dispersal and redistribution of indigenous groups. With different identities and pressures, this continuing process in the tumultuous history of Mindanao was to eventually affect the lives of isolated populations like the Tasaday, after having created them.[5]

Obviously this writer shares the view that the Tasaday forbears were agriculturalists—or more strictly, farmer-foragers. This is really not from any evidence gained in the short ethnobotanical study other than the lexical item for the loosely defined term for planting, *famula*. Rather is it that unless separation of Tasaday from their parental linguistic group is significantly earlier than presently supposed, the latter group must have practiced slash-and-burn agriculture; for even 700 years ago most Filipinists would agree that cultivation was well established in the archipelago.

Thus in the remoter past or in the immediate future, Headland's hypothesis of Tasaday agriculture is more acceptable, and the Tasaday subsistence in the interim may be addressed in Levi-Strauss' term of pseudo-archaism.

Conclusion

This should be taken as a concluding rather than a conclusive statement on the subsistence system of the Tasaday. Perhaps those of us who conducted research should have refused to publish the results of such short-term fieldwork, for although we thought we were exercising caution in our presentations and our conclusions, we provided further fuel for the construction of the hoax theory, now the regrettable feature of the Tasaday environment.

Last to receive a battering was the environmental-subsistence work, with such a question repeated at least twice as "why Tasaday would choose to hike for three days to a valley where they could dig up a few wild yams, rather than take a three-hour walk to a village where they could secure rice, corn, sweet potato, and cassava" (Headland 1987, 1992). Does this simplification really require pondering, parsing anthropologically? Lest this reference is an out-of-context distortion, I should like to address the context as a whole.

I thought the hard-won gains in human ecology of subsistence systems were the recognition of the necessity to examine case by case before comparisons were made; that systems involved not only the nature of resources, but quantification if possible, and the setting up of equations that involved populations of consumers and the physical limits, the territorial imperatives of the real and the potential for production and, not as an afterthought, the cultural governances of that production. Obviously we did not achieve such definitions of the Tasaday system, but we were not forced into synthesis in the way that we are now.

But in Headland's portrayal of what we did there is a deeper implication. That in our partially completed research, we should not have tried to apply the ethnographer's healthy skepticism of relating what we saw to the information we received from our informants, and test as far as we were able the consistency of that information—whether we got it right. What we should have done was disbelieve what was said to us, to the degree that even if the Tasaday answers to questions that pertained to cultivation conveyed ignorance, and even if I could not see "root crops in small gardens," that I should have relied on the version of the "wild yam hypothesis" credited to me. "Nobody's fool..." Indeed?

Notes

1. By this time, Tasaday had been identified as a Manobo language.

2. The youthful Dad Tuan was the only possible Panamin supervisor of researchers so often mentioned by proponents of the Tasaday hoax theory during the December 1972 fieldwork.

3. The inner bark of Caryota was the material from which the hunting bows of the Tasaday, brought in by Dafal from Blit, were made. The toy bows of the Tasaday

children were indeed made of local bamboo, but no example of Tasaday claiming to make the hunting weapon was recorded up to 1972.

4. Archaeology was the subject of discussion in Panamin planning of research.

5. Physical abnormalities noted in some Tasaday children such as albinism, spinal deformation, partially webbed foot gave rise to our amateur speculation on possible inbreeding outcomes. More informed opinion might have resulted if Panamin plans for a physical anthropologist researcher had eventuated.

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