Letters

Psychotherapy: Assessing Methods

Eliot Marshall deserves kudos for a perceptive analysis of the heated debate between psychotherapists and the federal government (News and Comment, 4 Jan., p. 35). Although the issues may be resolved in terms of political considerations rather than on the basis of scientific evidence, a few observations seem pertinent.

1) Contrary to Constantine's statement that "there are virtually no controlled clinical studies, conducted and evaluated in accordance with generally accepted scientific principles, which confirm the efficacy, safety and appropriateness of psychotherapy as it is conducted today," there have been a number of studies that shed important light on these issues. Moreover, the burgeoning literature has been carefully assessed by respected researchers. Perhaps the most authoritative recent review (*1*) characterizes the overall results of outcome studies as "clearly positive." The authors further note: "Our review of the empirical assessment of the broad range of verbal psychotherapies leads us to conclude that these methods are worthwhile when practiced by wise and stable therapists" (*1*, p. 180). They do admit that "often persons are not helped or are even hurt by inept applications of the very treatments that are intended to benefit them." In other words, as is true in other fields, the methods are no better than the person using them. This is particularly true in the case of psychotherapy and constitutes a weighty argument against attempts to "certify" methods of psychotherapy as one might certify a drug.

2) Research on the effective ingredients in psychotherapy has undergone impressive development during the last 25 years. Needless to say, there remain many unanswered questions. Unfortunately, too, available knowledge does not readily translate into cut-and-dried answers now demanded by the government. Instead, the bulk of the scientific evidence points to the overriding importance of the *collaborative relationship* that develops between a patient and a therapist. This relationship in turn is determined by such variables as the patient's motivation for and ability to profit from psychotherapy as well as the therapist's human qualities, commitment to the therapeutic task, and clinical skill. On the whole, there is scant evidence that therapeutic methods per se determine therapeutic outcomes.

For these reasons, the government's current insistence on "clinical trials" and the imposition of the medical model on psychotherapy is not in keeping with available scientific knowledge. To be sure, we need more concentrated research on the kinds of individuals whose problems can be significantly helped by psychotherapy; the human and technical qualifications of therapists; and a host of related issues that have a bearing on the potential benefits as well as the limitations of psychotherapy. These questions cannot be answered by clinical trials, nor can a group of experts resolve them by fiat. If immediate action must be taken it seems wiser to establish and enforce standards of competence for individual practitioners instead of running "horse races" between different methods of psychotherapy.

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Amazon Warfare

Regarding the report "Protein deficiency and tribal warfare in Amazonia: New data" (2 Mar. 1979, p. 910), we wish to call attention to Chagnon and Hames' misrepresentation of the position on Amazon warfare taken by cultural ecologists and their apparent misunderstanding of the relevance of their data.

They note that "recent data on . . . Amazonian tribes . . . fails to indicate a correlation between protein intake and intensity of warfare patterns." Yet, they admit that the Jívaro, Yąnomamö, and Barí, "considered by most anthropologists to be very warlike tribes . . . consume more meat than the more peaceful tribes." Certainly this *is* a correlation—and the one that we expect. Although the tropical forest is generally game-poor (*1–3*), warfare, by redistributing human population and creating "no-man's-lands" between hostile communities, produces conditions that reduce the likelihood of overpredation and increase hunting potential (*1, 4, 5*). (Such a process has been observed as well in temperate zones, and even among nonhuman predators) (*6*). This, of course, does not mean that all Yąnomamö villages will be equally productive, a point on which Chagnon and Hames are mistaken because of their idiographic emphasis upon individual villages instead of viewing the larger cultural-ecological system, and because they confuse protein deficiency in the diet with our position that protein resources are a limiting factor in the environment (*1, 2*).

They also ignore a critical link between diet composition and intervillage competition. Among the Yąnomamö, whose villages—on which horticulture has a strong centripetal effect—may reach 150 to 200 persons, Chagnon himself has observed that "game animals are not abundant and an area is rapidly hunted out" (*7*). Such circumstances compel an eclectic diet and a (centrifugal) strategy of deep-forest hunting in which large species such as tapir and white-lipped peccary are major targets (*2*). Among the Yąnomamö, these two species—both highly mobile and problematical—may constitute as much as 62 percent of total game by weight (*8*); but, because they are unpredictable (and tapir especially susceptible to overkill), they only represent about 14 percent by frequency (*8*). For this reason, Chagnon and Hames' report of an average of 75 grams per adult per day and Lizot's comparable figure of 77 (*9*) obscure the fact that, outside of a few days of overabundance (when excess protein is excreted), consumption is probably closer to an average of 30 grams (*5*)—and would probably be much less in the absence of war-created game reserves. Moreover, these data from small villages on major Ori-

noco tributaries (*9*), are probably *atypical*, since Yąnomamö traditionally dwell inland (*9*), where game supplies are least secure. The interior village from which Lizot reports an average of 36 grams (*9*) is probably more representative and suggests that the Yąnomamö protein harvest—when the distribution of tapir and white-lipped peccary are taken into account—may frequently become marginal, especially for large settlements and where population pressure—evidenced by high rates of female infanticide (*7, 9*)—has critically reduced strategic game supplies. This puts severe strains on intravillage game distribution, leading to fission and ensuing hostilities (*5, 7*). Nevertheless, as Chagnon and Hames note, the most intense and chronic war is between more distant villages. We attribute this in good measure to competition engendered by deep-forest hunting of certain critical but problematic species.

Nothing in Chagnon and Hames' report seriously discredits the ecological interpretation of Amazon warfare (*10*) in general or of Yąnomamö war in particular. They have yet to propose a compelling alternative.

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10. For a more detailed discussion of these issues, see (*5*); also J. Ross, in *Beyond the Myths of Culture: Essays in Cultural Materialism*, E. Ross, Ed. (Academic Press, New York, in press).

The response by Ross and Ross to our report on tribal warfare in Amazonia is punctuated with so many undocumented assertions and misrepresentations of the existing literature that it would take an article-length response to address them. Coincidentally, however, we have already submitted a two-part expanded version of our report to another sci-

entific journal. Most of the topics mentioned by the Rosses are dealt with more extensively in the expanded version than was possible in our original *Science* report, and we believe we adequately answer the questions raised by the Rosses.

One overriding major issue, however, should be kept fully in mind in this debate. In the sciences there must be a meaningful relation between empirical evidence, analytical methods, and general theoretical propositions. Theories must be falsifiable, and a peculiar attribute of the "Protein School" is that the various members consistently modify their positions to remove their theory further and further away from the possibility that it could be falsified. Thus, in 1974, one of the senior spokesmen of the Protein School, Marvin Harris of Columbia University, summarized the general position accordingly: When asked, ". . . how do you explain warfare among the Yąnomamö?" he replied "I think there may be a shortage of protein there . . ." (*1*), a contention prompted by the then-valid claim that the first author of the *Science* report had not presented quantified data on protein consumption during the course of his previous field studies. Our *Science* report was an attempt to provide such data, and the field research conducted by the second author was specifically designed to answer that criticism. Astonishingly, after our report in *Science* was published and clearly showed that there is considerable reason to doubt that a protein shortage exists, Harris argued, ". . . it is not surprising that the small settlements studied by Chagnon and Hames enjoy high per capita fish- and game-protein levels" (*2*). This new position is found in the above criticism of our report by the Rosses, adumbrated also in a recent publication by E. Ross (*3*) that describes efforts to reconcile scientific disagreements by recourse to evidence as ". . . vacuous empiricism. . . ." How does one falsify the "scientific" claim that a *shortage* of animal protein in native Amazonian diets leads to tribal warfare when *high* per capita protein consumption also leads to the same effect? We would indeed, using the logic of the Protein theorists, find it difficult to provide a compelling alternative to this kind of preemptive theorizing!

As for an alternative approach to the relation of material resources to human biocultural evolution, we believe the second part of our forthcoming article will adequately address this issue. The general bodies of theory relevant to this issue have been laid out in Chagnon and Irons (*4*) (reviewed in *Science*, 14 December 1979, p. 1294) and in the general field of evolutionary ecology, summarized in such texts as Krebs and Davies (*5*) (reviewed in *Science*, 24 August 1979, p. 781). Ecology, finally, derives from the field of biology—whether or not it is modified with the adjective cultural—and the "cultural ecology" of the Protein School seriously violates many principles of ecological theory as these are widely understood by biologists.

We sincerely hope that the theory to be presented in the forthcoming essay by J. Ross (cited in reference 10 of the Rosses' critique) on Amazon warfare unambiguously presents propositions that can be verified or falsified by empirical data.

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5. J. Krebs and N. Davies, Eds., *Behavioural Ecology* (Sinauer, Sunderland, Mass., 1978).

Erratum: In the caption to the picture accompanying the review of *Solar System Plasma Physics* by Michael C. Kelley (18 Jan., p. 297) the date of the launching of Explorer I should be January 1958.

Erratum: In the list of recent recipients of the National Medal of Science (News and Comment, 25 Jan., p. 387), Lyman Spitzer, Jr., should have been identified as professor of *astronomy* at Princeton University.

Erratum: In the report "Aborginal Indian residence patterns preserved in censuses and allotments" by John H. Moore (11 Jan., p. 201), Table 1 was inadvertently omitted:

Table 1. Distances from mothers' to married children's allotments for first-generation descendents of Sand Creek family heads.

Choices	Distance (miles)	
	Daughters (N = 23)	Sons (N = 14)
Near	0.5	0.5
	0.5	1.0
	0.5	3.1
	1.0	
	1.0	
	1.3	
	1.6	
	1.9	
	2.2	
	2.4	
	2.4	
	2.8	
	3.0	
	3.7	
	4.5	**4.5**
Far	5.5	18.0
	5.5	18.8
	9.0	21.2
	15.1	21.3
	16.6	25.0
	19.3	25.2
	35.0	31.5
	36.6	32.9
	37.1	41.4
		46.9
		47.5