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# Sampling Strategies for the Collection of Cultural Data: An Extension of Boas's Answer to Galton's Problem

Danielle F. Wozniak

College of New Rochelle, [dwozniak@cnr.edu](mailto:dwozniak@cnr.edu)

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## Sampling Strategies for the Collection of Cultural Data: An Extension of Boas's Answer to Galton's Problem<sup>1</sup>

W. PENN HANDWERKER AND DANIELLE F. WOZNIAK

*Department of Anthropology (U-176), University of Connecticut, Storrs, Conn. 06269-2176, U.S.A. I v 97*

Social and behavioral scientists conventionally judge the reliability, validity, and generalizability of findings by reference to criteria set by classical statistical theory. However, much social and behavioral research entails the study of cultural phenomena and, thus, what anthropologists call ethnography. "Ethnography," as we use the term, refers to the processes and the products of research that seeks to document what people believe, feel, and do in a way that situates those phenomena at specific times in the history of their life experiences, including pertinent global events and processes. To achieve this goal, ethnography requires distinctive methodological tools. First and foremost, one must interact intensively and create personal relationships with the people one wants to understand. In contrast to research traditions in which "experts" study "subjects," ethnography requires one to ask other people to help dispell the ethnographer's ignorance of them. Ethnography therefore rests on the collection of cultural data. "Culture," as we use the term, refers to the systems of mental constructs people use to interpret themselves and the world around them (e.g., Barnett 1953). Culture thus can be thought of as *meaning* and as consisting of a series of labeled (e.g., "mother") definitions

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of phenomena together with the myriad emotional and intellectual associations individuals acquire and attach to specific labels in the process of living.

The culture theory that prevails in contemporary anthropology (e.g., Geertz 1973, 1994) comes to us from the seminal work of Cooley and Mead, and many of the presumptions of what, in sociology, came to be called symbolic interactionism constitute fundamental tenets. The *process of living* through which people construct their symbolic world consists of interactions that individuals experience over the course of their lives both with others and with aspects of the nonsentient world. We thus take for granted that people live in a *culturally constructed* world and that that world is *socially constructed*.

#### THE PROBLEM

This conclusion poses an important challenge to research carried out by reference to the criteria of classical statistical theory. Violations of the assumption that data observations occur independently and that prediction errors associated with any one case remain uncorrelated with errors made for other cases (for example, cross-sectional heteroskedasticity or spatial or temporal autocorrelation) invalidate the usual statistical tests. Questions like "How old are you?" or "What is your race?" elicit datum points—labels, names—that reflect elements of the unique life history that makes each of us an independent being and therefore may yield data consistent with the assumption of observation and error independence. Sophisticated diagnostic techniques now allow us to evaluate whether or not our data violate these assumptions, and equally sophisticated techniques (for example, measurement unit transforms, weighted least squares, and differencing procedures) give us means to avoid the analytical confusion such violations create.

Cultural data, however, come from questions like "How do you know you are a mother, and how did you learn it?" or "Who belongs to your family, and what distinguishes family members from other people?" Whereas the answer any one person gives to a question like "How old are you?" has no necessary relationship to the answer any other person gives to the same question, culture theory posits that no one can respond to a question like "How do you know you are a mother?" without talking about the social interaction necessary to formulate the answers. Cultural data thus should reflect the social (interactive) processes by which we construct our knowledge of each other and the way these social processes work. Cultural datum points should exist only insofar as they reflect the lack of independence between sources. Cultural data thus appear to invalidate the usual statistical techniques and make the classical sampling criterion of case independence impossible to attain. How, then, should we select cases, what analytical techniques are appropriate, and to whom can we legitimately generalize?

If there are substantive differences between life-experience markers (names, labels) and cultural data (the definitions and intellectual and emotional associations we attach to names and labels), an unbiased ("random") sample of life-experience data should yield accurate parameter estimates whereas a convenience sample may or may not (since convenience samples may, by chance, be unbiased samples). By contrast, if cultural data are socially constructed, unbiased and convenience samples of people whose lives encompass equivalent experiential variability should yield identical findings. Differences in cultural data ("boundaries" between one cultural consensus and another [after Keesing 1994]) should reflect life-experience differences (e.g., racism, income constraints on opportunities) that influence how individuals construct the meanings they use to conduct their lives.

#### METHODS

We tested these hypotheses with data on the construction of family relations collected from a population defined as the list of 1,990 foster mothers maintained by the Department of Children and Families (DCF), State of Connecticut, to which Danielle Wozniak had been granted access for purposes of research on the social and self-construction of identity among foster mothers. We selected two sets of informants independently. A simple random sample of 45 women was selected using Excel's uniform random-number generator. We selected a convenience sample by asking key informants to participate and to ask people they knew who fostered children to participate. We collected data by means of mailed questionnaires. The simple random sample yielded a 60% return rate ( $n = 27$ ). The convenience sample included 42 key informants and their friends. We maintained standard human-subjects protection procedures.

Life-experience data collected included (1) the number of years a woman had been a foster mother (range 0–30 years, mean = 5.9 years, s.d. = 6.5), (2) how many children she had fostered (range from 0 for women newly certified as foster mothers to an estimated 250, mean = 23.0, s.d. = 46.3), (3) how many of these children she had adopted (range 0–8, mean = .8, s.d. = 1.4), (4) how many children she was currently fostering (range 0–5, mean = 1.8, s.d. = 1.2), (5) how many of these children were relatives (range 0–3, mean = .1, s.d. = .5), (6) how many persons currently lived in her home (range 1–10, mean = 5.1, s.d. = 1.9), (7) how old she was (range 28–78, mean = 44.5, s.d. = 9.2), (8) her race (44.93% "white," 47.8% "black," 4.35% "Hispanic," and 1.45% other), and (9) annual household income (excluding DCF payments), from "under \$15,000" (4.35%) through "under \$30,000" (24.65%) and "under \$60,000" (49.28%) to "\$60,000 and over" (18.84%). These data thus included variables for which the parameter was known (race, age) and variables that measured life-experience differences (e.g., racism, income constraints on opportunities) that might influence the meanings that individuals constructed. We constructed three addi-

tional variables: (1) a measure of fostering experience, as an interaction of age and number of years a woman had been a foster mother, (2) the natural logarithm of the experience variable, and (3) a dummy variable measure of lower-class status, coded "1" for households with under \$15,000 annual income and households with under \$30,000 annual income if household size exceeded 5 and "0" otherwise.

Cultural data collected included four sets of questions bearing on family relations, the significance of which had emerged in texts (conversations and transcribed interviews) bearing on the nature of families collected over the course of two years of intensive participant-observation. The first set asked each woman to evaluate the importance of nine criteria she might (or might not) use to identify family "members": that the person (1) be loved, (2) be born into the family, (3) be legally adopted, (4) contribute work or income, (5) be taken care of by her, (6) live with her for a long time, (7) no longer live with her, (8) feel like a family member to her, and (9) feel like a family member him or herself. These criteria thus encompass conventionally recognized legal, biological, and instrumental criteria as well as self-defined affective criteria. We randomized the order of presentation. Informants rated the importance of each criterion on a four-point scale from "not at all important" to "very important."

The second set of questions that elicited cultural data asked each woman to evaluate the extent to which she thought of people identified by specific social labels as belonging to her family. We used labels representing conventionally recognized legal and biological distinctions and self-defined affective distinctions that had emerged in earlier ethnographic research and randomized the order of presentation. These included (1) foster daughter, (2) adopted son, (3) cousin, (4) daughter-in-law, (5) stepdaughter, (6) son-in-law, (7) son, (8) stepson, (9) best friend, (10) daughter, (11) uncle, (12) mother (13), father, (14) foster son, (15) husband, (16) adopted daughter, (17) daughter's boyfriend, (18) aunt, (19) boyfriend, (20) grandchild, and (21) close male friend. The third set of cultural data asked each woman to evaluate the degree to which these people (sans "uncle," which we inadvertently left off the questionnaire) were permanent family members. Informants rated each item on a four-point scale from "not at all a member/a permanent member" to "very much a member/very permanent."

The fourth set of questions that elicited cultural data asked each woman to assess the importance of nine criteria she might (or might not) use to identify herself as a "mother": that a child (1) be born to her, (2) be born to a family member, (3) contribute work or income, (4) be loved by her, (5) be taken care of by her, (6) live with her for a long time, (7) be adopted, and (8) be thought of by her as her child, and (9) think of her as her or his mother. Like the first set of cultural data, these criteria encompass conventionally recognized legal, biological, and instrumental ties and self-defined affective criteria, and we randomized the order of presentation. Informants rated the importance of each criterion on a four-

point scale from "not at all important" to "very important."

SYSTAT (Wilkinson et al. 1997) and ANTHROPAC (Borgatti 1992) software generated the output reported here. We use simple point estimates and 95% confidence limits to test the hypothesis that the simple random sample accurately estimates population parameters and the convenience sample does not. We use consensus analysis (e.g., Romney, Weller, and Batchelder 1986, Weller and Romney 1988) to test explicitly for the presence or absence of cultural boundaries and, thus, to test the hypothesis that findings from the simple random sample and the convenience sample do not differ. For each set of cultural data, consensus analysis conducts a minimal residual factor analysis of the cases adjusted for random error, thus asking if the responses of each woman constitute just one measurement of an unobserved consensus about meaning. The following analysis treats responses as qualitative assignments of meaning rather than rankings and measures similarities among individuals as simple matching coefficients. A large proportion of the variance among individuals explained by a single factor the eigenvalue of which is three or more times larger than the next largest (a dramatic scree fall) warrants (contingent on diagnostic analysis) the inference of cultural consensus. The correlation between individuals and the first factor evaluates the degree of variability in meanings represented in the sample; their average provides a means of measuring the reliability and validity of the identified consensus. The consensus analysis procedure identifies the existence of significant agreement differences by the presence of more than a single major factor. Initial analyses will test the null hypothesis that there are no differences in meaning between responses from the simple random sample and the convenience sample. Cultural consensus among informants selected randomly and informants selected by convenience constitute grounds for accepting the null hypothesis.

This study uses multidimensional scaling and PROFIT analysis to examine the data further. Multidimensional scaling works directly on matrices of similarity coefficients and transforms those coefficients into spatial distance: short distances imply great similarity and great distances imply great dissimilarity. The similarity matrices among informant cases analyzed by consensus procedures may exhibit one of at least four different patterns. A very high degree of consensus—such as would exist if informants worked with a single set of "correct" answers—would appear as a tight centralized cluster of cases with increasing degrees of random scatter as one moved away from the answers of the "experts." A weaker consensus might appear as a more uniformly distributed but still clearly clustered scatter of cases. By contrast, the absence of consensus would appear as a random scatter of cases. The existence of two or more distinct consensuses—such as would exist, for example, if "white" and "black" women thought about fostering and the construction of family relations in different ways—would appear as two or more foci in the

TABLE 1  
Parameter Estimate Accuracy of the Random and Convenience Samples

Parameter	Parameter Value	Random Sample Point Estimate (95% CI)	Convenience Sample Point Estimate (95% CI)
Percentage of "white" foster mothers	58	67 (42-84)	31 (16-49)
Percentage of "black" foster mothers	37	26 (9-48)	62 (43-78)
Average age of foster mothers	47.2	47.0 (42.9-51.1)	43.0 (40.4-45.6)

multidimensional scaling map. However, it is possible that the mapped agreements among women reflect more subtle differences. Thus, this study conducts a PROperty-FITting (PROFIT) analysis (Kruskal and Wish 1978) of the mapped agreements among women to evaluate the extent to which variables such as race, income, or the number of children adopted influence the manner in which foster mothers construct family relations. The probability values for the vector  $R^2$  are generated from 1,000 random permutations based on Fisher's method of randomization. We conducted the PROFIT analysis with all 12 of the variables constructed with individual data plus a variable that distinguished the simple random sample from the convenience sample as a further check on consensus analysis findings. This exploratory PROFIT analysis of 13 variables run five times capitalizes on chance findings of low probabilities; we corrected a conventional  $\alpha$  of .05 with the Bonferroni procedure.

#### FINDINGS

Table 1 shows, as expected from classical statistical theory, that the simple random sample accurately estimates known population parameters (albeit not very precisely, owing to the small sample size) and that the convenience sample does not. All 95% confidence limits for the simple random sample encompass the parameter; no 95% confidence limits for the convenience sample encompass the parameter.

Table 2 shows, as expected from culture theory, that the simple random sample and the convenience sample yield identical findings. Initial factor eigenvalue ratios are three or more times higher (and account for 65% to nearly 90% of the variance in individual responses) for cultural data sets bearing on the criteria for judging someone as a "family member," who belongs to the "family," the permanency of "family membership," and the affective criteria for judging oneself a "mother," thus identifying clear consensus about the meanings of these issues. For someone to be considered a "family member," it is "very important" that the person be loved, be taken care of by the woman, be felt to be a

family member by her, or feel like a family member him- or herself. It is "not at all important" that the person be born into the family, be legally adopted, contribute work or income, live with the woman a long time, or no longer live with the woman.

A person with any of the following social labels is "very much" a "family member": foster daughter, adopted son, cousin, daughter-in-law, stepdaughter, son-in-law, son, stepson, best friend, daughter, uncle, mother, father, foster son, husband, adopted daughter, aunt, and grandchild. A person with any of the following labels, by contrast, is only "somewhat" a member of the family: daughter's boyfriend, boyfriend, and close male friend. Everyone is a "very permanent" family member except foster daughter, son's girlfriend, boyfriend, and close male friend, who are only "somewhat permanent" family members, and daughter's boyfriend, who is only "somewhat" and "not very permanent" a family member.

There exists a clear consensus that to be a "mother" it is "very important" to love a child, take care of the child, think of oneself as a mother, and be thought of as her or his mother by the child. The consensus about the affective meaning of being a "mother" breaks down entirely for the conventional biological, legal, and instrumental criteria of "motherhood." For these latter criteria, the ratio of the first to the next largest eigenvalue is only 1.3 and the average level of agreement among informants only .01. Whereas the multidimensional scaling maps of consensus agreements exhibit clear, single foci, the map of the agreements over the importance of biological, legal, and instrumental criteria of "motherhood" exhibits random scatter (see fig. 1). PROFIT analyses reveal that the patterns of agreement among informants, whether or not they exhibit cultural consensus, show no patterning by the number of years a woman has been a foster mother, how many children she has fostered, how many of these children she has adopted, how many children she currently fosters, how many of these children are relatives, how many persons currently live in the home, how old she is, her race, annual household income, fostering experience, the natural logarithm of the experience variable, lower-class sta-

TABLE 2  
Consensus and PROFIT Analysis Findings

Cultural Data Set	Ratio of 1st Eigenvalue to Next Largest (Percentage of Explained Variation)	Average Agreement among Informants	Correlation between Informant Responses and the Identified Cultural Consensus	Significant Vectors Identified by PROFIT Analysis (Bonferroni-Adjusted for $\alpha = .05$ )
Criteria for judging someone a family member	3.0 (64.7)	0.54	0.98	none
Who belongs to the family	11.6 (87.0)	0.82	0.99	none
Permanency of family membership	6.9 (78.8)	0.71	0.99	none
Criteria for judging self a mother (affective)	7.4 (83.5)	0.76	0.99	none
Criteria for judging self a mother (legal, biological, instrumental)	1.3 (42.9)	0.01	(no identifiable consensus)	none

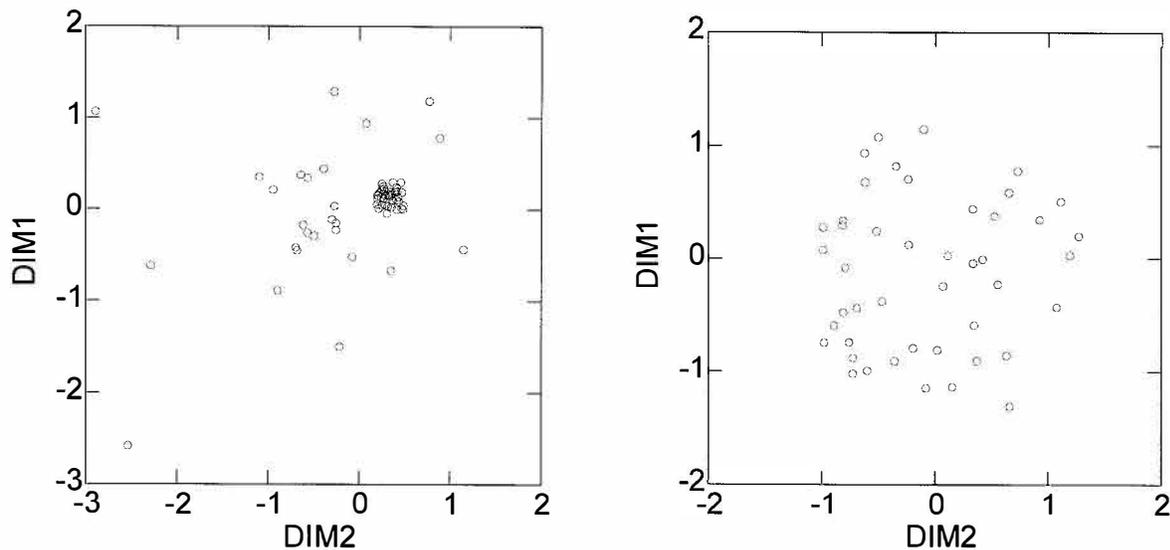


FIG. 1. Multidimensional scaling maps of informant agreements about criteria for judging oneself a “mother”. Left, affective criteria (stress = .0517), right, biological, legal, and instrumental criteria (stress = .1237).

tus, or the differences between the simple random sample and the convenience sample. Only 3 of 65 PROFIT runs yielded probabilities at or below .05, which we could expect merely by chance. No probability fell at or below the Bonferroni adjusted probability of (.05/65 = .0008).

IMPLICATIONS

These findings support the theoretical distinction between “life-experience” data and “cultural” data and validate the hypothesis that individuals construct “cultural” data through their social interaction with other

people and aspects of the nonsentient world.<sup>2</sup> They therefore validate the practice of sampling knowledgeable informants—informants we carefully select (Johnson 1990)—to learn about cultural domains with clear rules or correct answers. These findings also validate the practice of selecting informants because they are available for cultural domains—such as those studied—for which there exist no set of clear rules and for which everyone is an “expert.”

2. Our findings about how foster mothers construct family relations challenge American kinship models based on conventional biological, legal, and instrumental criteria. We will deal separately with these issues of power and the construction of culture.

Conceptualizing life stories in ways that coherently incorporate the implications of being born at a specific time and place, the effects of maturation, and the experience of specific historical events and processes at specific ages constitutes a major theoretical challenge. Studying them requires distinctive methodologies. Indeed, this problem arose the first time an anthropologist used numerical methods—when Edward B. Tylor attempted to explain the origins and distribution of kin avoidances with a hodgepodge of psychological, functional, and evolutionary hypotheses (1889). One of the originators of the numerical tools that we use today, Sir Francis Galton, asked Tylor how he knew that his cases were independent, since the similarities he tried to explain by reference to individual needs and social functions might merely reflect social interaction mediated either by a common history or geographical proximity. Tylor had no answer.

In the second use of numerical methods by an anthropologist, Boas (1894) addressed this problem with a cluster analysis of regional and historical social relationships bearing on the distribution of myths and stories found among tribes of the Northwest Coast, the adjacent Plateau, and three widely scattered communities (interior Athapaskan, Ponca, and Micmac). In the process, Boas adduced evidence for the regional and historical influences to which Galton alluded in his question about observation independence (spatial and temporal autocorrelation). Boas found that neighboring communities shared more than distant communities (e.g., Northwest Coast tribes were more similar to each other than to tribes on the Plateau, but Northwest Coast and Plateau tribes shared more than either did with the interior Athapaskan, Ponca, and Micmac) and that people who might share an ancestral community (who spoke languages from the same language family) shared more than people who spoke languages from different language families.

Where Boas demonstrated case-dependence in cultural data collected in and taken to characterize different communities, our study extends Boas's findings to cultural data collected from and taken to characterize different individuals. The socially constructed nature of cultural phenomena makes the classical sampling criterion of independent case selection not only impossible to attain but also undesirable. Indeed, research on cultural phenomena that ignores the historically and regionally specific social interaction through which we create cultural phenomena simply misses the point (e.g., see Wolf 1984). This does not make sampling fluctuations go away, but it means that classical tests will not give us the information we need. As we reorient theory and research in ways that transform ethnography into ethnology by a focus on cultural variability between individuals rather than between reified and essentialized groups (cf. Bidney 1944, Barnett 1953, Murdock 1971, Vayda 1994, Keesing 1994), this also means that we take seriously the question of who agrees with whom about what and to what degree and search for the

TABLE 3  
*Cultural Data Reliability and Validity Estimates for Different Sample Sizes and Levels of Agreement Among Informants*

Number of Informants	Average Level of Agreement	Reliability	Validity
9	0.5	0.9	0.95
18	0.5	0.95	0.97
36	0.5	0.97	0.99
6	0.6	0.9	0.95
12	0.6	0.95	0.97
24	0.6	0.97	0.99
4	0.7	0.9	0.95
8	0.7	0.95	0.97
16	0.7	0.97	0.99
3	0.8	0.92	0.96
6	0.8	0.96	0.98
18	0.8	0.99	0.99
3	0.9	0.96	0.98
6	0.9	0.98	0.99
18	0.9	0.99	~1.00

variability in experience that might explain cultural differences, if and when we identify them.

Over the past decade, research on ethnographic methods has produced tools that allow us to demonstrate the evidentiary value of cultural data.<sup>3</sup> Weller (1987) has shown that the Spearman-Brown prophecy formula can be applied to informants rather than items, allowing us to measure the reliability and validity of the cultural data we report. Indeed, table 3 shows that so long as the average level of agreement among informants is .5 or higher, ethnographic conclusions based on information from small numbers of informants (3–36, depending on the level of agreement) exhibit exceptional reliability (.9–.99) and validity (.95–~1). To assess internal validity, consensus, multidimensional scaling, and PROFIT analytical tools, among others, allow explicit tests for the existence of cultural consensus and the presence and basis of cultural boundaries and, thus, valid generalization to populations defined by the life experiences studied.

Efficient research designs should differentiate carefully between life-experience and cultural data. Life-experience data consist of measurements of names or labels applied to categories of experience and therefore of measurements that may be made independently of each other. "Race," "ethnicity," and "gender" labels collected from any one person have no necessary relationship with the labels collected from any other person. Answers to questions about what a label refers to or about the intellectual and emotional associations individuals attach to the construct (how the label was cre-

3. Concern with measuring the reliability and validity of cultural data explicitly, however, goes back to Harold Driver's "The Reliability of Culture Element Data" (1938).

ated, how one learns which to attach to oneself and under what circumstances, and the emotional and other associations evoked by the label) must reflect social interaction. Understanding of the meanings people use to experience and respond to the world around them comes from examination of the historically and regionally situated social interaction through which they create and change meaning.

Efficient sampling strategies for cultural data should therefore focus on the selection of knowledgeable informants whose lives encompass experiential variability that may influence the studied meanings, track levels of agreement, and expand sample sizes and sampling criteria consistent with levels of agreement and identified cultural boundaries.

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## The Effects of Markets on Neotropical Deforestation: A Comparative Study of Four Amerindian Societies<sup>1</sup>

RICARDO GODOY, DAVID WILKIE, AND JEFFREY FRANKS  
*Department of Anthropology, University of Florida,  
 Gainesville, Fla. 32611, U.S.A. 22 IV 97*

Ever since Gross et al.'s (1979) seminal article on the effects of acculturation on agricultural intensification, scholars have been debating the effects of markets on indigenous people's use of natural resources as part of a larger discourse on how economic development or income affects the environment. Despite at least two decades of debate, scholars still disagree on these effects. In this article we present a model, three hypotheses arising from it, and empirical evidence from four Amerindian societies in the rain forest of Honduras (Tawahka) and Bolivia (Mojeño, Yuracaré, Chimane) to examine the role of markets in the loss of old-growth rain forest. We explain the conditions under which markets hurt or help conservation and thus help to bridge opposing views in the debate.

Our focus on Amerindians does not imply that they are the main culprits in neotropical deforestation; the model and hypotheses apply to any group. Rather, their behavior is of special interest because they are gaining a greater say in the use of their natural resources (Turner 1995). Although we stress the role of markets in the loss of forest, we recognize that macroeconomic and sectoral policies also matter. Finally, we focus on the loss of old-growth rain forest because it generally contains more biological diversity than secondary-growth forest (Frumhoff 1995, Saldarriaga et al. 1985, Lawrence, Leighton, and Peart 1995, Finegan 1996, Silver, Brown, and Lugo 1996, Denevan 1992).

The debate about how markets or economic develop-

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