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Methods for collecting panel data:

What does cultural anthropology have to learn from other disciplines?

Clarence C. Gravlee/a/, David P. Kennedy/b/, Ricardo Godoy/c/, William R. Leonard/d/

/a/ Clarence C. Gravlee, Department of Anthropology, Florida State University, Tallahassee, FL 32306-7772. E-mail: cgravlee@fsu.edu. Telephone: 850-644-4754. Fax: 850-645-0032

/b/ David P. Kennedy, Carolina Population Center, CB#8120, University Square, 123 West Franklin Street, Chapel Hill, North Carolina, 27516-2524. E-mail: dkennedy@unc.edu. Telephone: 919-966-5987. Fax: 919-966-7019

/c/ Ricardo Godoy, Heller School for Social Policy and Management, Brandeis University, Waltham, MA 02454-9110. E-mail: rgodoy@brandeis.edu. Telephone: 781-736-2784. Fax. 781-736-2774

/d/ William R. Leonard, Department of Anthropology, Northwestern University, 1810 Hinman Avenue. Evanston, IL 60208. E-mail: w-leonard1@northwestern.edu. Telephone: 847-491-4839. Fax: 847-467-1778

Address correspondence to:

Clarence C. Gravlee
Department of Anthropology
Florida State University
Tallahassee, FL 32306-7772, USA
E-mail: cgravlee@fsu.edu
Tel. 1-850-644-4754; Fax: 1-850-645-0032

Abstract

In this article we exploit the growth of panel studies in the behavioral, public health, and medical sciences over the last decades to review how they have dealt with problems unique to the collection of quantitative data and to draw lessons for cultural anthropology. We assess the benefits and drawbacks of panel studies for cultural anthropology and identify when they likely pay off. Cultural anthropologists have long valued panel studies because they have methodological, analytical, and practical advantages over cross-sectional studies. Repeated measures of the same unit at different times permit accurate estimates of continuity, maturation, s evolution, adaptation, intergenerational transmissions, and life-cycle dynamics because one can use the unit of observation as its own controls. By following the same unit over time one can estimate change not only for individuals, but also for groups. With repeated measures of the same person, one can shorten the recall period for eliciting data and track events as they unfold, thereby reducing measurement errors from faulty recall; one does not need to ask people to recall events far back in time. As rapport between researcher and subject grows, panel studies allow researchers to probe sensitive topics harder to tap in cross-sectional studies. Panel studies allow pre-post evaluations of projects and estimates of structural, dynamic behavioral models. Panel studies provide an ideal venue to test approaches to correct measurement errors of cross-sectional studies. But offsetting the advantages of panel data, one also finds disadvantages, unique to panel studies, including biases from attrition and conditioning. With panel data the adverse consequence of measurement errors increase. Panel studies also require long-term continuity in management, archiving, documentation, financing, and leadership. Despite the limitations, cultural anthropology can profit from looking at what neighboring disciplines have done and are doing in panel studies.

Key words: panel, longitudinal, cross-sectional, accuracy, surveys

Over the last 50 years, longitudinal studies have grown in both number and importance across the social, medical, and public health sciences. One might expect that cultural anthropologists would have seized on this trend. Cultural anthropologists widely value long-term field research, because it facilitates the analysis of continuity and change and potentially improves the depth and quality of anthropological data (Foster et al. 1979, Kemper and Royce 2002). However, relatively few cultural anthropologists conduct longitudinal research that incorporates panel data. Panel studies, which involve repeated measures from the same units of analysis at different points in time, have the potential to improve our understanding of many processes central to anthropological theory, including enculturation, adaptation, maturation, life-cycle development, modernization, and intergenerational transmission of culture.

In this paper, we argue for the increased use of panel designs in cultural anthropology. Our argument is organized in four parts. We first define panel studies in relation to other types of research design. We then describe the history of panel studies in anthropology and in neighboring disciplines, noting the rapid growth of panel studies across the social and medical sciences over the last 50 years. Next, we discuss the advantages and disadvantages of panel studies and argue that their unique properties make them essential in studies that focus on change over time and causality. Finally, we review how successful panel studies in various disciplines have dealt with the unique challenges of panel studies and provide guidelines for maximizing the advantages of collecting panel data in cultural anthropology.

For brevity, we emphasize methods of collecting quantitative data using structured, face-to-face interviews. This emphasis does not reflect the limits of panel designs. Like cross-sectional research, panel studies can include the full range of qualitative and quantitative methods in social science. As Menard points out, “both longitudinal and cross-sectional research may involve case studies, ethnographies, experiments, sample surveys, census, or archival data collection" (Menard 2002:34). We focus on structured face-to-face surveys to make the discussion manageable, but much of what we say applies across data-collection modes and types of data (James and Sørensen 2000, Mednick, Harway, and Finello 1984, Saldaña 2003, Schulsinger, Mednick, and Knopf 1981).

Our focus is on research design, rather than on statistical methods for analyzing panel data. We recognize that data analysis is, in fact, an integral part of design, since researchers who are familiar with a broad range of analytic tools are more likely to collect the right kinds of data. But the analysis of panel data is a broad subject worthy of book-length treatment (e.g., Diggle et al. 2002, Hsiao 2003, Singer and Willett 2003). Here we emphasize methods for collecting valid and reliable panel data—a prerequisite for meaningful data analysis. We also leave aside the use of multiple survey modes (Dillman and Christian 2003) and do not discuss problems common to both cross-sectional and panel designs.

Our review draws on literature published across the social, medical, and public health sciences. To provide selected examples from anthropology, we draw on a panel study (1999-present) among the Tsimane', a foraging-horticultural society in the Bolivian Amazon (citation?). Some of the ideas presented here come from a multidisciplinary workshop funded by the National Science Foundation entitled "Stability of Methods for Collecting, Analyzing, and Managing Panel Data" held at the American Academy of Arts and Sciences, Cambridge, Massachusetts, during March 26-28, 2003. Some of the papers presented at the workshop will appear in *Field Methods*.

Definitions

Our goal of learning from the experience of neighboring disciplines is complicated by cross-disciplinary differences in the vocabulary of research design. The family of methods we describe are known in various fields as panel methods, longitudinal designs, prospective cohort studies, or repeated observations. In cultural anthropology, "longitudinal" is often taken to mean "long-term" fieldwork. The first task, then, is to define terms.

Figure 1 illustrates our understanding of the relationships among types of research design. We follow others in regarding longitudinal methods as a broad class of methods, rather than as a specific suite of techniques (Menard 2002; Wall and Williams 1970). Longitudinal methods are perhaps easiest to define in contrast to cross-sectional research, in which data are collected for each unit of analysis at one point in time. Although data collection typically lasts for months or even years, a standard assumption in cross-sectional research is that data for all cases—be they people, households, villages, or countries—can be regarded as contemporaneous (Menard 2002:2). Longitudinal research, on the other hand, requires repeated measures on the same variable for at least two points in time. This minimal definition implies that each wave of data collection uses standardized measurement procedures to ensure the comparability of data from different time periods and to facilitate the study of change over time (Baltes and Nesselroade 1979, Menard 2002).

Several types of research may be regarded as longitudinal, according to this definition. As illustrated in Figure 1, we first distinguish between panel and repeated cross-sectional research. Repeated cross-sectional studies collect data from independent samples at two or more points in time. When samples are comparable (e.g., two probability samples from the same population), it is possible to make inferences about aggregate changes over time by comparing cross-sections. However, because repeated cross-sectional studies do not follow units of observation through time, it is not possible to study individual trajectories of change with such studies. To illustrate this point, we may think of a national periodic census as a repeated cross-section. Census data may show, for example, how the proportion of people living in poverty changes from one decade to the next, but it does not indicate whether any specific person experiences a change in poverty status over time.

Such statements about individual-level change require panel data. In panel studies, researchers obtain information from the same units of observation for two or more periods of time (Heckman and Robb 1985). Figure 1 shows three varieties of panel designs. In *prospective panels*, researchers collect data from the same initial cohort at two or more points in time. In *retrospective panels*, researchers doing a cross-sectional study collect information about the past and present, and treat information about different points in time as if it had been collected at those times. In *rotating panels*, researchers drop part of the initial sample after multiple waves of data collection and replace it with a fresh look-alike sample to lower respondent fatigue and to ensure that the panel continues to be representative of the target population (Menard 2002). Panel studies often combine elements of these three designs.

The distinction between panel and repeated cross-sectional studies depends on how one defines the unit of analysis. Census data, for example, may be regarded as panel rather than repeated cross-sectional data, if nation-states rather than individual people are defined as the unit of analysis. If we were conducting, say, an international study of changing poverty rates around the world over several decades, then census data would reveal something about the trajectories of change for individual nation-states. In this sense, repeated censuses from different nation-states would exhibit the defining feature of panel studies: repeated measurements for the same units of analysis over two or more points in time.

Our focus for the remainder of this paper is on methods for the design and management of panel studies. One rationale for this focus, as opposed to a discussion of longitudinal methods more generally, is to highlight the distinction between long-term ethnographic fieldwork and longitudinal research. This distinction is often not made clear. For example, in Kemper and Royce's (2002) *Chronicling Cultures*, the index entry for "longitudinal research" directs readers to "*see also* long-term research." Foster (2002:256) likewise equates long-term and longitudinal research, though he distinguishes both from restudies of a field site. Similarly, Saldaña (2003:3) suggests that "a qualitative study becomes longitudinal when its fieldwork progresses over a lonnnnnnng time." Yet, according to our definition, not all long-term fieldwork is longitudinal, nor is all longitudinal research long-term (cf. Johansen and White 2002).

The chief criterion of panel studies—repeated measurements on the same units of analysis over time—does not imply that studies last a long time. The duration of panel studies and the time elapsed between waves of data collection may be long or short, depending on the research questions. For example, Scudder and Colson's (2002) ethnographic censuses in the Gwembe District of Zambia, which may be regarded as a panel design, have systematically collected demographic information on roughly 10,000 Tonga for nearly 50 years. By contrast, Espy and colleagues (2000) followed a panel of 40 newborn infants for just two weeks to determine the effect of prenatal cocaine exposure on infant's physical and cognitive development. Despite radically different time scales, these studies both demonstrate the key feature of panel designs: repeated measurements of the same units of analysis in the same way over time. By this criterion, relatively few long-term studies in cultural anthropology may be regarded as true panel designs.

Panel Studies in Anthropology and Neighboring Disciplines

The history of panel research is generally traced to 1759, when the French Count Philibert Guéneau du Montbeillard began recording his son's stature at six month intervals from birth to age 18 (Baltes and Nesselroade 1979). Montbeillard's record-keeping bears little in common with contemporary panel studies, aside from the systematic nature of his repeated observations. The current concept of panel research was not established until the 1920s and 1930s, when several classic studies of human growth and development began—many involving anthropologists (Bogin 1999).

Since the mid-twentieth century, panel studies have proliferated across the social sciences. Figure 2 illustrates this explosive growth. Based on the *Inventory of Longitudinal Studies in the Social Sciences* (Young, Savola, and Phelps 1991), it shows the number of studies initiated annually from 1911 to 1980 (three-year moving average). This figure makes clear the rapid expansion of panel research from the mid-1950s through the late 1970s. Other sources confirm this trend and suggest that it continues into the present. For example, Alderman et al. (2001) note that the journal *Demography* published only 26 articles based on panel data during 1980-1989, but that it published 65 such articles between 1990 and 2000. Goodman and Blum (1996) point to a similar trend in management research, and Ferraro and Kelley-Moore (2003) demonstrate the sharp growth of panel methods in the *Journal of Gerontology: Social Sciences*. In the last decade, at least 40 percent of all studies published annually in that journal have used panel data.

Several factors have contributed to the growth of panel research in the social sciences. Much growth in the 1960s and 1970s came in response to increased federal funding for panel studies in the United States. For example, policy concerns regarding work and economic behavior led to funding for ongoing studies like the National Longitudinal Surveys (NLS) in 1966 and the Panel Study of Income Dynamics (PSID) in 1968 (Giele and Elder 1998). Technological and computing advances also facilitated data collection at a national scale and increased researchers' capacity for managing, analyzing, and sharing large, complex data sets

(Phelps and Colby 2002). In addition, advances in statistical methods for analyzing longitudinal data have encouraged researchers not only to collect panel data but also to ask new questions of existing data sets (Ferraro and Kelley-Moore 2003). The growth of panel studies is evident in other industrialized countries too (Young et al. 1991, Schneider and Edelstein 1990, Fisher et al. 2002).

Yet not all social sciences have contributed equally to this growth, and anthropology is among the least prolific disciplines in panel research. Figure 3 depicts the number of longitudinal studies published in 1990-2003 across five disciplines, according to the Social Sciences Citation Index (SSCI): anthropology, sociology, economics, epidemiology, and sociology. The SSCI is an imperfect measure, since it excludes monographs and may reflect cross-disciplinary differences in keyword indexing. For example, indexing of longitudinal studies may be more reliable for psychologists than for anthropologists, since the former generally publish more explicit descriptions of research methods. Still, we think the SSCI provides a fair estimate of broad trends in the most widely read social science literature.[1] During 1990-2003, 430 longitudinal studies were published in epidemiology, 330 in psychology, 213 in economics, and 65 in sociology. In anthropology, by contrast, the SSCI identifies only 16 longitudinal studies.

A closer look at SSCI data reveals that the comparative dearth of anthropological panel studies is greatest in cultural anthropology, as compared to other subfields. In biological anthropology, there is an established tradition of panel research, beginning with Franz Boas's 1894 study of Worcester school-children—the first longitudinal study of human growth in the

[1] For each discipline, we searched the SSCI for "(longitudinal or panel) and" the name of that discipline (e.g., anthropology, economics, etc.). The approximate shape of the distribution across disciplines holds after adjusting for the number of total publications in that discipline (data not shown). Our results are also confirmed by searches of other databases, including WorldCat, ArticleFirst, and the WilsonWeb Social Sciences Full Text Index.

United States (Tanner 1981). SSCI data suggest that this tradition continues. A search for the keyword "longitudinal or panel" from 1990 to 2003 yields 64 articles in the *American Journal of Physical Anthropology* and 121 articles in the *American Journal of Human Biology*. Corresponding searches in *Cultural Anthropology* and the *American Ethnologist* return no publications, while *Cross-Cultural Research* returns just one. This pattern indicates a significant shortcoming, given cultural anthropologists' broad theoretical interest in dynamic processes of cultural continuity and change over time.

For another view of cross-disciplinary differences in the design of panel studies, we compare exemplary studies in anthropology against the gold standards of neighboring disciplines. Table 1 summarizes key features of two classic ethnographic projects and five noteworthy panel studies from public health, epidemiology, sociology, economics, and psychology. Although other ethnographic studies warrant inclusion in this comparison, we focus on the Gwembe Tonga and Tzintzuntzán projects, because they provide ample documentation and are moving towards electronic dissemination of their data.

Table 1 draws out several important contrasts. First, unlike exemplary studies in neighboring disciplines, the Gwembe Tonga and Tzintzuntzán projects were not conceived as panel studies (Foster 2002:255, Scudder and Colson 2002:198). Instead, as long-term ethnographic fieldwork unfolded, each project developed an institutional framework that accomodates the collection of panel data, but does not require it. Consequently, the range of variables for which panel data have been collected in Gwembe and Tzintzuntzán is more limited in scope than is the case in the other studies in Table 1. In both Gwembe and Tzintzuntzán, the core panel data consists of periodic ethnographic censuses that Scudder and Colson (2002) and

Foster (2002) began at the outset of their studies. The detail and systematic nature of these data permits true panel analysis (Clark et al. 1995, Kemper 2002:290).

By contrast, the other studies in Table 1 were explicitly designed as panels to maximize the benefits of tracking the same individuals over time for studying processes of change: human physical and cognitive growth, the development of heart disease and stroke, the causes and consequences of adolescent health-related behaviors, the impact of government policy on poverty and economic behavior, and the lifecourse development of youthful aspirations, social relations, family structure, employment, and health. As a result, these studies emphasized the comparability of measures on the same cases over time for a broad range of variables relevant to key research questions. They also have sampling strategies and tracking rules guided by theory, and the periodicity of data collection is shaped by the research questions. These contrasts point to the need for explicit panel designs in anthropology, with appropriate sampling strategies, suitable periodicity, and systematic data collection procedures to ensure comparability over time (Royce and Kemper 2002, see also Moles 1977).

Another striking contrast in Table 1 is the number of publications resulting from each study. Although there are disciplinary differences in the expectations and format of scholarly publishing, the wide gap in the number of publications based on panel studies in anthropology and in neighboring disciplines also points to a more fundamental difference. The ethnographic projects in Gwembe and Tzintzuntzan were primarily the work of the original researchers and their students; the panel studies in other disciplines were conceived in part as resources for the entire research community. For that reason, all but the Fels study have made provisions for public access to the raw data. There are currently efforts underway to make data from Gwembe and Tzintzuntzan more widely available (Cliggett 2002; Kemper 2002; Scudder and Colson

2002), but these exemplary anthropological studies still lag behind other disciplines in the archiving and dissemination of data. As we argue below, data sharing maximizes the contribution of panel studies to the growth of empirical knowledge and to the training of future researchers. Indeed, in the last five years alone, more than 70 Ph.D. dissertations were based on PSID data.

A related pattern evident in Table 1 is that most panel studies involve significant interdisciplinary collaboration. In part, this pattern may reflect a common theoretical emphasis on individual development in sociocultural context, which intersects with many disciplinary perspectives. Yet it may also be that interdisciplinary collaboration provides a better return on investment in the infrastructure required for panel studies. In anthropology, it is notable that several successful panel studies cross subdisciplinary boundaries, especially those between cultural and biological anthropology[CG1] (Hill and Hurtado 1996, Scudder and Colson 2002).

Finally, Table 1 hints at the challenges of financing a panel study of long duration. All seven studies have relied on a variety of funding agencies to sustain their efforts. However, whereas the panel studies in neighboring disciplines have relied primarily on one or a few agencies (notably the National Institutes of Health) for core funding, the ethnographic studies have had to seek support from multiple foundations and funding agencies. This pattern poses a challenge for the continuity of panel studies in anthropology, and it points to the importance of establishing long-term relationships with funding agencies. We return to this topic below.

Advantages of Panel Studies

The most important advantage of panel studies is the ability to identify and describe patterns of change *within units of analysis*. Singer and Willett (2003) point out that this problem entails two levels of analysis: (1) describing *within-individual* trajectories of change and (2) examining differences *among individuals* in their trajectories of change (cf. Baltes and

Nesselroade 1979). Both levels of analysis are relevant to questions of long-standing interest to anthropologists.

Consider the comparative study of folk racial classification in Brazil and other parts of Latin America. A recurrent theme in the ethnographic record is that racial identity is more fluid and ambiguous in Brazil than it is in the United States (e.g., Degler 1971, Harris 1964, Twine 1998). This view is commonly expressed by the idea that "money whitens"—as one achieves greater prestige in terms of social class, ascribed racial identity favors lighter categories. The evidence for "whitening" comes almost entirely from cross-sectional ethnographic studies, despite the fact that it implies a dynamic process of individual-level change. Following Singer and Willett, there are two pertinent questions: (1) How do individuals' ascribed identities change over time? and (2) do men and women, children and adults, rich and poor, or light- and dark-skinned Brazilians change in the same or different ways over time? Panel data are uniquely suited to address these questions and, in a comparative frame, would shed new light on the relative fluidity of racial identity in Brazil, in the United States, and in other societies. For example, a recent study using data from the National Longitudinal Study of Adolescent Health discovered that 12 percent of American adolescents reported inconsistent racial identities across different interview contexts (Harris 2002).

Zarger and Stepp (2004) provide another example of anthropological relevance. They ask how ethnobotanical knowledge acquisition among Tzeltal Maya children might have changed over the last 30 years, given rapid sociocultural, economic, and environmental change. Their design may be regarded as a repeated cross-section, since they carefully replicated Stross's (1970) work in the same community. Zarger and Stepp found that Tzeltal children between the ages of 4 and 12 in 1999 could identify correctly the same proportion of plants as could children of the same ages in 1968. They conclude that little or no change occurred in children's plant-naming ability over the intervening 30 years.

Given the repeated cross-sectional design, we can have confidence in this conclusion about the nature of change at the aggregate community level. However, Zarger and Stepp's broader interest in the "process of acquiring environmental knowledge" (p. 414) would be well served by tracking the same sample of children over time. Using panel data, we might ask: (1) How does each child's knowledge of ethnobotanical terminology change with age? and (2) what accounts for whether children acquire new knowledge at the same or different rates? It is possible, as Zarger and Stepp propose (p. 416), that cross-sectional differences among children of different ages reflect developmental regularities in the acquisition of ethnobotanical knowledge. It is also possible, however, that children of varying ages in 1999 would have been exposed to different processes of enculturation, because they came of age at different times. Panel data with children of multiple ages would help to differentiate these possibilities and to illuminate the *process* of enculturation.

This example illustrates the importance of panel data for understanding developmental processes, because cross-sectional studies alone can never distinguish between age *differences* and age *changes* (Schaie 1967). Figure 4 depicts the problem. Suppose that we conducted a cross-sectional study in 1980 and observed a cross-sectional difference in, say, political attitudes among three generations of a community. This difference could be interpreted as part of the aging process: Perhaps people become more conservative with age. However, generational differences could also stem from the fact that people born in 1925 grew up in different sociocultural environments than did people born in 1945, who in turn grew up in different environments than did people born in 1965. Cross-sectional studies cannot rule out this alternative, because they confound the effects of aging and cohorts (Palmore 1978, cf. Handwerker 2001:114).

Because they track change within units of analysis, panel studies can distinguish between age and cohort effects. For example, if we were to follow-up with our 1980 sample in 1990 and in 2000, we could determine how each individual's political attitudes changed with age and thereby distinguish maturational changes from differences among cohorts. Following a single cohort over time would present another problem, however, since differences between waves of data collection could be due to changes in the sociocultural environment, rather than to consistent maturational changes associated with the aging process. Panel studies with multiple cohorts, as shown in Figure 4, improve our ability to disentangle the effects of age, period, and cohort (Palmore 1978)[DPK2][CG3][CG4].

Panel studies also offer benefits for establishing the direction and magnitude of causal relationships (Finkel 1995, Menard 2002). There are generally four criteria of causal inference in the social sciences: (1) variables must covary, (2) their relationship must not be spurious, (3) there must be a logical time order, and (4) there must be a theory about the causal mechanisms involved (Bernard 2002, Blalock 1964, Davis 1985). Cross-sectional studies are adequate for demonstrating covariation, but they generally cannot rule out the possibility of a spurious relationship, nor can they establish temporal order. Panel data inform causal analysis by determining the temporal sequence of presumed cause and effect (Miller 1999), and they permit better tests of spuriousness (Finkel 1995). Experimental designs, which combine panel data with randomization to treatment and control groups, permit the strongest inference about causal relationships but are not always feasible.

A less widely recognized advantage of panel studies is their potential to improve the reliability and accuracy of estimates. This advantage accrues in two ways. First, panel studies make it possible to control for so-called personal fixed effects—attributes that do not change over time, but that may bias observed relationships in cross-sectional studies (e.g., genotype). Second, prospective panels reduce recall bias—a known threat to the reliability of estimates (Bernard et al. 1984)—by shortening the time elapsed between the event of interest and the date of data collection. For example, a recent analysis from the Framingham Heart Study used prospective panel data on parents and offspring to estimate the importance of parents' history of heart disease for offspring cardiovascular risk (Lloyd-Jones et al. 2004). The study confirmed the importance of parental history but revised previous estimates, which were based primarily on retrospective data. The associated recall bias in previous studies had often produced inflated estimates of the risk associated with parental heart disease.

Yet panel designs are not always appropriate. When the primary goal of research is to describe the distribution or pattern of association among variables at one point in time, cross-sectional studies are apt. Repeated cross-sectional studies like that of Zarger and Stepp (2004) may be better suited to studying change at the aggregate level. However, panel studies are uniquely suited to address anthropological questions about patterns and processes of change within units of analysis. They also offer advantages for causal inference and can improve the reliability and accuracy of estimates. The balance of this paper deals with the challenges of panel designs—including measurement error, attrition, costs, data storage and dissemination, and sustainability. We offer specific advice to minimize the adverse consequences of threats to the validity of panel studies.

Dealing with Challenges of Panel Studies

Measurement Error

Measurement error refers to the difference between the true value of a variable and the measured variable. Although all studies are vulnerable to some form of measurement error, panel studies introduce a unique threat of error due to respondents' repeated exposure to the same questions or stimuli. We distinguish between two types of measurement error: classical and systematic. Classical measurement errors are random and uncorrelated over time. Systematic measurement errors display a pattern. Classical or random measurement errors arise when respondents guess in answering questions, when researchers make mistakes coding or transcribing data, or when they mismatch files of respondents from different survey rounds.

Classical measurement error. Our study [DPK5]among the Tsimane' (n=4,657) illustrates classical measurement error. In 2001, we asked people for their age and that of their children. We returned a year later at about the same time and asked the same questions, even though we knew respondents' age had increased by only one year (±1-2 months to account for variation in the re-interview date). Only 20 percent of respondents reported their age correctly. A quarter of the sample reported an age that represented an increase of more than 1.5 years, and 55 percent reported an age that represented a decrease of more than six months. Such errors not only create problems in interpreting the age data but also also introduce error into any new variables derived from age or other variables with random measurement error. For example, using age to construct age-standardized anthropometric indices of nutritional status would introduce random measurement error into those variables as well.

In addition to errors from inaccurate responses and from mistakes in coding or transcribing data, classical measurement errors in panel studies also arise from changes in how data are collected. In particular, random measurement errors arise from changes in (1) wording or order of questions (Kalton and Schuman 1982, Fowler 2001); (2) data-collection modes (e.g., from telephone to mail survey), even if question wording remains the same (Kalton, Kasprzyk, and McMillen 1994, Shettle and Mooney 1999); (3) number and types of questions (Polivka 1996, Angrist and Krueger 1999); (4) visual layout in self-administered surveys (Christian and Dillman 2003); (5) codes and surveyors (Epstein 2002); (6) respondents' interpretation of questions as the study progresses, even if the survey mode and wording of questions remain constant (Seldin, Friedman, and Martin 2002); and (7) proxy respondents (people who answer for others; for instance, parents who answer questions about small children, spouses who answer questions for an absent spouse, etc.).

Measurement error from changes in respondents' interpretation of questions deserves a brief discussion, because it has received relatively little attention. Even if researchers pose a question unambiguously, the meaning of that question to respondents might change as the norms of a society change. Changes in the meaning of a given question may or may not be informative. Mott (2002) gives the example of shifting categorization over time of children from biological to step or vice versa. Instead of being unreliable, changing answers more likely reflect shifting intensity in the relation between the parent, step-parent, and the child. In such instances, inconsistency yields insight into changing meanings over time. However, with panels spanning several generations, it may be hard to know how participants in early waves interpreted questions (Seldin, Friedman, and Martin 2002).

<u>Systematic time-in-sample errors.</u> Unlike classical measurement errors, systematic measurement errors are persistent and patterned, and arise when participants under- or over-report answers. Time-in-sample systematic errors are unique to panel studies, and refer to changes in participants' response due to repeated exposure to the same questions or stimuli

(Bailar 1994, Duncan 2000, Kalton and Citro 2000, Rose 2000) . Time-in-sample errors are also known as panel conditioning, contamination, or re-interviewer effect.

Time-in-sample errors can increase or decrease measurement error. Improvements in accuracy occur for several reasons. Respondents may become motivated to provide more accurate information as they get to know better the interviewer and the purpose of the study. Sudman and Bradburn (1982:82) note that “confidence and perceived threat both level off fairly rapidly after two or three … interviews.” In an anthropological context, Vogt (2002:145) notes that long-term engagement with a community helps build rapport and improves data quality. Other anthropologists also articulate this benefit of repeated field visits (Foster 2002:275, Royce 2002:10).

In experimental studies, participants may provide more accurate answers in later phases because they learn how to do the tasks (e.g., take tests)—provided that elapsed time does not impair cognitive functioning (Levin et al. 2000). In an experimental field study on patience among Tsimane’, we offered 154 adults 15-80 years of age the choice of having money or candy now or having a larger reward in the future. We did the experiment during four consecutive quarters and found that measures of patience became more reliable as the study unfolded. Within-subject correlation coefficients of patience between the first and second quarter were 0.004 for money and 0.12 for candy, and in neither case were results statistically significant. Within-subject correlation coefficients for patience between the third and fourth quarters were 0.32 for money and 0.46 for candy, and in both cases results were statistically significant at the 99% confidence level or higher (Godoy et al. 2004). The measure for the first quarter was unreliable either because respondents did not understand the task, did not trust the researcher to deliver the delayed reward, or both. Reyes-García et al. (2003) provide another anthropological example of increased reliability over time as study participants repeatedly answered questions on ethnobotanical knowledge.

But repeated exposure to the survey can also erode the accuracy of data because of fatigue or from learning. Surveyors may get tired in later interviews and not provide as much stimulus to respondents as in earlier interviews. Experienced surveyors may start to amplify or provide leads when asking questions. O'Muilcheartaigh (1994) shows that in the Current Population Survey of the United States, respondents mechanically repeated information supplied in earlier interviews. With repeated surveys, respondents learn that some questions may lead to further questions, and may give rote responses or refuse to answer to minimize the time spent with the surveyor (Bailar 1994, Kalton and Citro 2000, Scudder and Colson 2002). Repeated exposure to questions on certain topics, such as political participation and health, can affect responses by making people more politically active or conscious of their health (Campbell and Stanley 1966).

Consequently, panel conditioning is more pronounced in certain categories of behavior, such as political attitudes, health, or sexual behavior, than in others, such as job search or employment (Veroff, Hatchett, and Douvan 1992, Ashenfelter, Deaton, and Solon 1986). It is also more pronounced with a shorter time interval between surveys because of more frequent exposure to the surveyor. However, Sudman and Ferber (1979) suggest that time-in-sample bias tends to decrease after the first wave. Waterton and Lievesley (1994) draw on three annual waves (1983-1986) of the Social Attitudes Survey from Great Britain and compare changes in the panel with data from a national cross-sectional survey of political attitudes. They find that participants "become politicized by the interview, that they report more honestly, and that they become less likely to answer 'don't know'", but they also find that the magnitude of conditioning was small and that conditioning was "not a major hazard" (pp. 335-336). Halpern et al. (1994) show that repeated questioning of adolescent males about sex had no significant effect on their sexual behavior.

Still, we know relatively little about the severity of problems from panel conditioning (Duncan 2000, Rose 2000), probably because we lack a reliable method to detect it. One strategy to detect panel conditioning is to combine a panel with an independent cross-section and to interview both samples in parallel (O'Muircheartaigh 1994). In panel studies of opinions, researchers have estimated the extent of conditioning by comparing (a) the change in the mean of a variable from the start to the end of the panel and (b) the mean of the same variable for respondents in the last wave of the panel with the mean of the variable for a cross-sectional random sample that represents the same population from which the panel was drawn. The first comparison (a) allows one to detect gross change from exposure to the survey, and the second (b) allows one to determine whether the measure at the end of the panel diverges in significant ways from the cross-sectional control group. This procedure provides a rough estimate of conditioning, because changes in (a) could arise from other factors besides panel conditioning.

<u>Consequences of measurement error in panel studies</u>. With cross-sectional data, classical measurement errors of the dependent variable increase the standard error but do not bias estimates of parameters. Classical measurement errors of explanatory variables produce an attenuation bias, or movement of the estimated parameter toward zero. When classical measurement errors affect the dependent variable, the explanatory variable, or both, the likelihood of accepting the null hypothesis of no effect increases (Duncan and Hill 1985, Bound and Krueger 1991). The adverse consequences of random measurement errors get magnified

with panel data because random measurement errors increase the ratio of noise to signal (Angrist and Krueger 1999, Ashenfelter, Deaton, and Solon 1986, Griliches and Hausman 1986).

To see why, consider a cross-sectional study at time *t*, where variable *X* contains a true signal (x_t) and some noise or random error in measurement (ε_t). One can write the difference (Δ) in the variable *X* between two time periods, *t* and *t*+1:

[1]. $\Delta = X_{t+1} - X_t = (X_{t+1} - X_t) + (\varepsilon_{t+1} - \varepsilon_{t}$[CG6])

where $(X_{t+1} - X_t)$ is the signal and $(\varepsilon_{t+1} - \varepsilon_t)$ is the noise or error surrounding the signal. If error terms are serially correlated, then expression $(\varepsilon_{t+1} - \varepsilon_t)$ will approach zero. If *Xs* are serially correlated over time, then expression $(X_{t+1} - X_t)$ will also approach zero. In typical panel data one expects X_{t+1} and X_t to display high serial correlation. For instance, people with high income today will likely have high income tomorrow. As a result, if the error terms have low serial correlation, then the ratio of signal to noise will be lower with panel data than with cross-sectional data. Classical measurement errors therefore produce greater attenuation bias with panel than with cross-sectional data (Angrist and Krueger 1999). On the other hand, panel data is better at removing errors that do not change over time since those errors get swept away by differencing.

Correcting measurement errors. There are three strategies that help to abate classical measurement error when collecting panel data. The first is to delete poorly measured rounds of data collection. Early rounds may contain greater random measurement error because of panel conditioning, distrust of researchers, or because researchers have not yet learned the local language and how to operate well in the local culture. As a result, researchers doing panel studies often[DPK8] spend the early rounds refining methods before starting formal data collection. Even after finalizing data-collection modes, researchers often drop one or more round of data due to measurement error. The Consumer Expenditure Survey of the United States uses the first of five rounds of surveys to bound questions for subsequent rounds because researchers assume consumers must learn how to report purchases (Silberstein and Jacobs 1994). The designers of the Survey of Income and Program Participation in the United States took a decade to develop and pilot test the survey (Hauser 2003).

There are also examples from panel studies in anthropology. Hill and Hurtado (1996) deleted demographic data collected early in their research among the Aché Amerindians of Paraguay, because they recognized the prevalence of measurement error and lying during the early rounds. In a 10-quarter panel study among Tawahka Amerindians in Honduras and in a six-quarter panel study among Tsimane' Amerindians in Bolivia, researchers deleted the first 1-2 quarters of data because of low reliability among observers (Byron 2003, Reyes-García 2001, Demmer and Overman 2001).

A second strategy to abate measurement error is to improve consistency in data collection formats. Keeping constant the data collection format lowers random measurement error because it ensures that the data elicited comes from participants' response to the same stimuli and not from changes in the way researchers collected data. As we elaborate below, changes in the design or administration of a survey can also increase bias due to attrition, or loss of participants in later phases of a study.

The use of laptop or handheld computers in the field allows for consistency checks for respondents across different surveys rounds (Gravlee 2002, Couper and Nicholls 1998). If a reported value diverges significantly from past values or falls outside the range of acceptable responses, the surveyor knows about it immediately and can explore the reason for the discrepancy during the interview. The Fels Longitudinal Study has two different people enter the same data as a check. Keeping constant the day of the week and month, or season of the year matters if seasonality affects the parameters of interest (cite?). The Current Population Survey collects data during the week of the 19th every month and uses laptop computers with mostly pre-recorded answers to standardize coding and data-entry procedures (cite?). To avoid burdening subjects, surveyors sometimes bring past data to the interview and simply confirm it with the subject, or use it as a benchmark to assess changes since the last interview.

The third strategy to abate measurement error is to collect data on so-called instrumental variables. Whereas the previous two strategies correct for measurement error during data collection, instrument-variable estimation is a statistical technique to reduce biases from error in the measurement of an explanatory variable after data has been collected. Instrumental variables are also used to reduce biases from endogeneity, omitted variables, or reverse causality, but we focus only on the use of instrumental variables to lower biases from measurement errors.

A convincing instrumental variable is a variable that correlates highly with the poorly measured explanatory variable of interest “for reasons the researcher can verify and explain” (Angrist and Krueger 2001), but that does not correlate with the error term or with the outcome, except through the poorly measured variable. For instance, researchers have used weather variability as an instrumental variable for income in rural areas of developing nations (Paxson 1993). Instrument-variable estimation is a two-step process. The first step is to obtain a predicted value for the poorly measured variable using the instrumental variable(s) on the right side (along with other covariates) of a regression model. The second is to use the predicted value of the poorly measured variable as an explanatory variable, with the outcome of substantive interest as a dependent variable. As Angrist and Krueger (2001) note, finding a convincing instrumental variable requires understanding the relation one is trying to estimate. There are several recent reviews of instrumental-variable estimation for poorly measured or endogenous variables (Angrist and Krueger 2001, Rosenzweig and Wolpin 2000, Meyer 1995).

Attrition

Attrition, or respondents' ceasing participation in a study, has been called the "Achilles heel" of panel studies (Thomas, Frankenberg, and Smith 2000). Our discussion here focuses on how to monitor and redress attrition; statistical tests to assess whether attrition biases conclusions are covered elsewhere (cite?).

Attrition can be temporary or permanent. Temporary or intermittent attriters leave the sample but return or decline to participate in some rounds of a study but not in others (Burkam and Lee 2000, Rendtel 2002). Unit non-response occurs when respondents turn down requests for an interview or drop out of the sample; item non-response occurs when respondents do not answer only some questions. Permanent attriters migrate out of the study site after early rounds and never return, or they remain in the site but refuse to participate or are hard to find (Thomas, Frankenberg, and Smith 2000).

If attrition is random and uncorrelated with observed variables, then sample size and the efficiency of parameter estimates will suffer, but estimated coefficients will remain unbiased (Sherman 2000). In this case, observations are said to be missing completely at random (MCAR), and one can ignore and not model the causes of attrition (Little and Rubin 1987). Besides loss of efficiency, the only cost associated with this type of attrition is the investment in initial waves of data collection and perhaps in replacing attriters with a new sample.

Non-random or informative attrition refers to units leaving the sample for systematic reasons, due to some observed or unobserved shared attributes. This type of attrition can (a) produce inconsistent parameter estimates (Heckman 1979, Hausman and Wise 1979, Nijman and Verbeek 1992, Little and Rubin 1987, Schafer 1997), (b) leave the researcher with an unrepresentative sample, (c) affect the univariate distribution of variables (Hauser 2003), and (d) give the false impression of stability, since those who divorce, find new jobs, die, get sick, or change lifestyles are more likely to move and leave the sample (Rendtel 2002, Goldberg et al. 2003, Ridder 1990). Informative attrition cannot be ignored and must be modeled simultaneously with the outcome of substantive interest. The extent to which nonrandom attrition biases parameter estimates is an empirical issue.

We know of no panel study explicitly designed to study the causes of attrition. Studies on attrition are an afterthought, done after or while researchers collect panel data. As a result, our understanding of the causes of attrition comes from indirect, fragmentary evidence (Alderman et al. 2000). One approach to determine whether attriters differ systematically from those who do not leave the sample is to compare the mean and variace of study variables for the two groups prior to attrition.

[Insert Table 2 about here]

In a two-year panel study among the Tsimane[DPK9]', the sample of households fell by 17.7 percent (378 to 311) from 2001 to 2002. Table 2 reports bivariate analyses to assess whether attriters and nonattriters differed along observed dimensions. These results suggest that the two groups resembled each other, except in food consumption. The next step would be to assess the multivariate associations between the variables in Table 2 and the probability of attrition, using a probit or logit model. Variables that appeared significant in bivariate analyses may lose statistical significance once the relationships among covariates are taken into account (Zabel 1998). Of course, correlates of attrition are not necessarily causes. The strongest inferences about the causes of attrition are based on exogenous variables that respondents cannot control or that researchers assign at random, such as randomly assigned interviewers or survey formats[CG10][DPK11].

Recent reviews suggest that the magnitude of attrition varies widely across surveys and sociocultural contexts (Glewwe and Jacoby 2000, Thomas, Frankenberg, and Smith 2000). Alderman et al. (2000) review attrition rates from 10 studies in developing countries and find that rates varied from as low as 1.5 percent per year in Indonesia (1993 to 1997) to 19.4 percent per year in Bolivia (1995/6 to 1998). The Wisconsin Longitudinal Study lost only 2.8 percent or the sample from 1957 to 1975, but the Panel Study of Income Dynamics lost 40 percent of its sample from 1968 to 1981 and half of its sample by 1989 (Becketti et al. 1988, Fitzgerald, Gottschalk, and Moffitt 1998). Ridder (1990) reviews case studies from Europe showing attrition rates of over 50 percent. Attrition rates among school-age populations may be even higher, as a study with high-school students in Adelaide lost 80 percent of the sample by the end of the seventh year (Simkin et al. 2000).

Despite the potential magnitude of attrition, empirical studies suggest that attrition rates do not significantly bias parameter estimates in either developed or developing nations (Rendtel 2002). However, studies of attrition bias tend to be based on large, nationally representative samples, which are unlike most anthropological studies. Attrition could significantly bias estimates if samples are small, if attrition is not random, and if attriters account for a large share of the sample (Angrist and Krueger 1999). Consequently, to improve the validity of panel data, anthropologists should take steps to reduce attrition before it occurs.

Hauser (2003) describes key aspects of project management that have facilitated a low attrition rate in the Wisconsin Longitudinal Study. First, the primary researchers, who had a vested interest in data quality, did the work themselves rather than subcontracting data collection. Second, before each wave of data collection, they sent letters, made telephone calls, and in more recent years, advertised in regional media and on the Internet to remind people about the upcoming survey. Third, they followed attriters through telephone interviews and tracked them by asking neighbors, parents, and siblings. If all leads failed, they started afresh. (what does this mean? They chose a new sample?) Other panel studies have used similar techniques (Badawi et al. 1999, Thomas, Frankenberg, and Smith 2000).

Other standard retetion strategies include: (a) obtaining at baseline a list of close contacts likely to know the participant's future address should the participant move, (b) keeping continuity in the relationship between interviewers and respondents, (c) changing survey modes to fit the participants' needs, (d) sending more skilled interviewers after people who decline interviews (Falaris and Peters 1998), (e) sending surveyors to track households that move out of the study site (Maluccio, Thomas, and Haddad 1999), (f) sending "personalized persuasion letters" to reluctant participants (Hill 1992), (g) matching participants with interviewers of the same ethnic identity, (h) scheduling interviews so they do not conflict with participants' other activities (Frank et al. 2003), (i) paying participants for the interview and for returning postcards with change of address information, (j) intensive training of staff, and (k) instilling *esprit de corps* among surveyors and pride among respondents for taking part in the study (Giele 2002).

To emphasize the value of respondents' contribution, many panel studies use personal hand-written reminders (Robles, Flaherty, and Day 1994), birthday cards (with address correction requested), frequent telephone calls (Hartsough, Babinski, and Lambert 1996), gifts (e.g., coffee mugs or telephone magnets with logo of the study), and reports summarizing research results. The Framingham Heart Study gives participants the results of their medical exams. Gifts and material incentives are typically small, symbolic, and have often come late in the study. The strongest impetus—at least in developed nations—comes from participants' feeling part of a larger research project, contributing to the public good, and feeling at ease with researchers. (I'm concerned that many anthropologists who work outside the industrialized world would not see how the examples in the last three paragraphs apply to them[DPK12].)

Costs

We know of only one study that compares the costs of collecting panel and cross-sectional data (Duncan, Juster, and Morgan 1984). This study finds that panel studies have lower costs per unit of observation than do cross-sectional studies. However, as Heckman and Robb (1985) note, Duncan and colleagues do not provide details of how they estimated costs and ignore the extra costs of correcting for non-random attrition, particularly for the costs of finding hard attriters.

Panel studies probably enjoy lower fixed costs for training and recruitment than do cross-sectional studies, but panel studies incur costs in retaining participants. Both the costs and benefits of retention strategies depend on the amount of effort invested in retaining participants. To lower costs of panel studies, researchers in developed nations often collect initial waves of information through personal interviews and then switch to a mix of cheaper survey modes, such as telephone, web, or mail (Dillman 2002, Hauser 2003). Following participants who split from original households, rather than selecting new samples, also lowers the costs of panel studies (Ashenfelter, Deaton, and Solon 1986). On the other hand, the natural growth of the initial cohort and its descendants can increase the costs of a panel study, if one follows all descendants.

Because the PSID tracked descendants of the initial cohort, its sample size increased from about 5,000 to 8, 700 families, and its costs grew as well. To offset these costs, the PSID does not interview every participant in every round. Rather, subgroups of the sample participate with varying frequency.

Consistency in the face of change

Despite the benefits of using the same survey mode for reducing measurement error, changes in panel data collection instruments are almost always necessary. Panel studies allow researchers to learn from their mistakes in early rounds of data collection. Researchers often want to add or modify questions because unanticipated questions and new goals surface, because participants find it hard to understand or interpret questions, or because more efficient technologies or more accurate methods of data collection become available. Also, to lower the burden of respondent participation, some questions asked in early rounds need not be asked as frequently or at all in later rounds when the answers are unlikely to change across survey rounds.

Changes to panel data collection instruments might be inevitable, but researchers must consider the possible effects of changes and decide if the costs outweigh the benefits. Dillman and Christian (2003) review evidence suggesting that survey modes themselves—face-to-face interviews, telephone interviews, web surveys, interactive telephone surveys, and mail surveys—shape the way researchers phrase questions and present information to subjects. They suggest that researchers doing panel studies who switch survey modes might need to change the wording of questions and the presentation of information to ensure that the meaning of the question remains the same. In addition to changes in mode of administration, simply deleting, modifying, or adding new questions can create a mode effect on other sections of the data collection instrument (Rose 2000). One way to track the effect of changes when adding new questions or modifying exiting questions is running both old and new questions in parallel during some rounds to assess their correlations within respondents. Some studies that have tracked the effect of changes found that changes resulted in only minimal improvements in the accuracy of findings (Polivka 1996)(Medin and Wilson 1994).

Data storage, documentation, and dissemination

Effective documentation, archiving, and dissemination of data are essential to the progress of empirical research. Among the many benefits of sharing data are the following: (1) It reinforces the norms of open scientific inquiry, (2) facilitates replication and refinement of prior results, (3) encourages diverse analytic viewpoints, (4) advances the development of new methods of data collection and analysis, (5) creates resources for training new researchers, (6) promotes the exploration of questions not anticipated by the original researchers, (7) generates comparative and synthetic research, and (8) eases the creation of new data sets by pooling results from multiple studies (Fienberg 1994, Rockwell and Abeles 1998). In short, as the National Research Council advises, “sharing data should be a regular practice” (National Research Council 1985:25).

Most funding agencies and professional organizations echo this point. The National Institutes of Health “believes that data sharing is essential” and requires all proposals seeking at least $500,000 in direct costs to develop a plan for data sharing or to explain why data sharing is not possible (U.S. Department of Health and Human Services 2003). The National Science Foundation’s Division of Behavioral and Cognitive Sciences, which oversees all three programs in anthropology, is “committed to the principle that . . . data collected with public funds belong in the public domain,” and it encourages grantees to “develop and submit specific plans to share materials collected with NSF support,” where possible and appropriate (National Science

Foundation 2003). Similar data sharing policies are in effect at other funding agencies (Rockwell and Abeles 1998). In addition, the ethical codes of leading professional organizations in the social sciences—including the American Sociological Association, the American Psychological Association, and the American Anthropological Association—encourage data sharing as an integral part of research design and publication.

The special strengths and challenges of panel studies make data documentation and dissemination particularly important. First, panel studies are almost always team efforts, often involving researchers from vastly different disciplines. Effective collaboration requires clear documentation of design decisions and methods for data collection and analysis. It also increases the need for early development of procedures for data sharing among team members (Fienberg 1994). In anthropology, Vogt (2002:145) notes that fieldworkers in the Chiapas Project agreed early on to deposit their data in a central archive that was accessible to other researchers on the project. Scudder and Colson (2002:206) decided from the beginning to share data from their Gwembe Tonga research with one another. They have now extended that policy to a new generation of team members and are working to provide wider access to their data in an electronic format (Cliggett 2002:247, Scudder and Colson 2002:227). This example suggests that policies for internal data sharing can be extended to provide data access to a broad community of scholars.

Second, high-quality documentation is key to tracking consistency and change in data collection procedures over time and to reconstructing the meaning of data collected in earlier phases of a study. High-quality documentation is also necessary for any third party unfamiliar with the original data collection to be able to use the data. Taylor (2000:162) identifies the necessary elements of data documentation for panel studies and warns that "without it, the data are, at best, meaningless, and, at worst, subject to serious misinterpretation." This warning seems prescient when reading Lamphere's (2002) account of long-term ethnographic study among the Navajo. She tells of difficulties in trying to make sense of "masses of accumulated data" in Clyde Kluckhohn's Ramah Project files: "It was as if the 'key' to the Ramah Files had died with Kluckhohn" (Lamphere 2002:119). Systematic data collection procedures and documentation of the project's background, research design, data structure, and field protocols would have provided an enduring "key" to secure the value of Kluckhohn's materials for later generations of researchers.

Third, panel studies typically generate far more data than the original researchers can analyze, making panel data sets potentially valuable to the entire research community. This point is amply illustrated by the thousands of publications based on publicly available panel data like the PSID (see Table 1). But the potential for secondary analysis of anthropological material is also clear. Foster (2002:263) reflects on the amount of data he has amassed in Tzintzuntzán and notes that much analysis remains to be done, "almost certainly by someone else." Scudder and Colson (2002:224) likewise comment on the challenges of maintaining—much less analyzing—the enormous database they have created over 50 years of research. Their collaboration with colleagues more expert in database management and quantitative data analysis demonstrates the value of secondary analysis (Clark et al. 1995). White's collaboration with these and other researchers also illustrates that making data available to other researchers enables the next generation of researchers to pose new questions and apply novel analytic approaches as new methods are developed (e.g., Johansen and White 2002). Also, anthropological data made available to the public can be incorporated in the classroom for training, regardless of whether the data result in additional publications.

Anthropologists must also be aware that there are obstacles to data sharing. First, although networked computing and the Internet have made it easier for researchers to store and share data electronically, there remain significant costs associated with cleaning, coding, and documenting data for distribution (Rockwell and Abeles 1998). Fienberg (1994:3) suggests that these costs are especially high when no advanced provisions have been made. Thus, panel researchers should anticipate the costs required for data documentation and dissemination in the initial budget proposal. Funding agencies may regard such expenses as cost-effective, if it appears likely that a community of researchers will benefit from having access to panel data (Rockwell and Abeles 1998).

A second potential barrier to data sharing is principal investigators' self-interest in publishing primary analyses of the data. This concern has been addressed in neighboring disciplines. Rockwell and Abeles (1998:S6) note that data-sharing policies acknowledge initial investigators' "right of *first and continuing* use, but not of *prolonged exclusive* use." A common standard across disciplines is that data should be made available by the time that the initial major analyses are published (Fienberg 1994). This standard generally provides original researchers with an extensive period of first use. Principal researchers typically begin thinking about potential analyses for at least a year before data collection begins and for another year while data is being collected. It generally takes at least another two to three years for the original researchers to complete and publish initial analyses, and several more years for secondary analysts to obtain the data, learn to use it, and complete and publish their analyses (Rockwell and Abeles 1998).

A third potential barrier to data dissemination is the privacy and confidentiality of research participants. As both a legal and an ethical imperative, researchers have an obligation to ensure that research participants cannot be identified, and this responsibility is heightened when data are made publicly available (National Research Council 1985). Researchers in the biomedical sciences have considered this issue carefully, since the data they collect is often highly sensitive. They have demonstrated that it is generally possible to protect confidentiality by removing direct identifiers (e.g., names, phone numbers, addresses), by reducing the precision of variables (e.g., assigning age categories, collapsing occupational codes, recoding extreme values), or by restricting access to the highly sensitive data (Fienberg 1994, National Research Council 1985). Guidelines are also available for protecting confidentiality when preparing small, qualitative data sets for data archiving (Corti, Day, and Backhouse 2000). Thus, concerns about confidentiality are not unique to anthropology, and established practices to protect participants' confidentiality in neighboring disciplines permit the sharing of most anthropological data. However, not all data sets are suitable for the public domain (Rockwell and Abeles 1998). It is important for individual researchers to weigh the benefits of data sharing against the potential harm to research participants if confidentiality cannot be maintained.

In most instances, the benefits are likely to outweigh the costs. Indeed, the benefits of data sharing are so widely recognized in neighboring disciplines that researchers now have many well documented data sets to choose from. If anthropology is to remain competitive for public funding and support, it is important to make available its data so the public at large can use them. We suggest that anthropologists learn more about data documentation and dissemination practices in neighboring disciplines by seeking out key resources like the Inter-University Consortium for Political and Social Research (ICPSR) and the UK Economic and Social Data Service (ESDS). ICPSR offers advice regarding data documentation in its *Guide to Social Science Data Preparation and Archiving* (http://www.icpsr.umich.edu/access/dpm.html), and the

ESDS provides comprehensive advice on documenting both qualitative and quantitative data sets (http://www.esds.ac.uk/aandp/create/). There is also growing support for the Data Documentation Initiative (DDI), an international effort to establish a standard format for documenting social science data (http://www.icpsr.umich.edu/DDI/). Although the DDI has been used primarily with large quantitative data sets, Kuula (2000) reports that it is appropriate for documenting qualitative data, too.

Sustaining a panel study

Because successful panel studies can outlast their original principal investigators, they must recruit new leaders. To a young potential leader, the costs of a longitudinal study take place now but the benefits occur in the distant future. As a result, young potential leaders face weak incentives to lead a panel study, and might even view such a role as an obstacles to their career (Entwisle, Alexander, and Olson 2002). Many panel studies have been fortunate to have potential successors in the right place at the right time, but the procedure for recruiting new leaders remains unresolved (Giele 2002).

Many anthropologists attest to their good fortune of having taken part in a panel study early in their career (Kemper and Royce 2002). In the edited volume *Looking at Lives*, senior researchers from different behavioral sciences attribute their professional success to the foundations they gained while taking part in panel studies (Phelps, Furstenberg, and Colby 2002). A common theme is the need to balance the demands of long-term research with the demands of producing results in the short run. To reap tangible short-run benefits from a panel study, young researchers can treat the early rounds of data as a stand-alone, cross-sectional study (Duncan 2000). Researchers publish preliminary findings from early rounds, but wait until the project ends to produce more definitive publications. For the strategy to work, researchers must be able to admit that conclusions from their intermediate outputs may be wrong (Furstenberg 2002).

Most federal agencies and private foundations finance research projects for no more than five years, after which researchers must apply for new money for another cycle. The funding cycle requires researchers to seek funding continuously, either from the original funding agency or from new agencies. Researchers doing panel studies try to develop a long-term relation with a primary funding source to provide core, long-term funding support (Mott 2002). Shifting from funding agency to funding agency, each with its own priority, makes it hard to maintain the original focus of a panel study. Funding agencies may only care about some types of data, and may not support additional follow-up studies with all or part of the original sample (Hauser 2003). To supplement core funding, panel studies charge third parties for including new questions on some rounds, use panel data and the research sites to offer training, and search for new funding opportunities even if the new topic bears a tenuous link with the goals of the original panel.

<u>Conclusions: lessons for anthropology from successful, long-term panel studies</u>.

- Pay offs
- When are they are likely to be critical and when not

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Table 1. Design features of selected large-scale, longitudinal studies across academic disciplines.

	Gwembe Tonga Research Project	Tzintzuntzan, Mexico	Fels Longitudinal Study	Framingham Heart Study	National Longitudinal Study of Adolescent Health (Add Health)	Panel Study of Income Dynamics (PSID)	Wisconsin Longitudinal Study
Discipline	Anthropology	Anthropology	Public Health, Human Biology	Epidemiology	Sociology, Psychology, Public Health	Economics	Sociology
Principal investigators	Thayer Scudder, Elizabeth Colson	George M. Foster, Robert V. Kemper, Stanley Brandes, Peter Cahn	Roger M. Siervogel	Daniel Levy, Philip A. Wolf	J. Richard Udry	Frank P. Stafford, Robert F. Schoeni	Robert M. Hauser
Institutional affiliation	Cal Tech, UC-Berkeley	UC-Berkeley, Southern Methodist U	Wright State U	National Heart, Lung, and Blood Institute, Boston U	U North Carolina-Chapel Hill	U Michigan	U Wisconsin-Madison
Years covered	1956-present	1945-present	1929-present	1948-present	1994-present	1968-present	1957-present
Original target population	Gwembe Tonga, Zambia	mestizo-*Purépecha*, Lake Pátzcuaro region	SW Ohio children	Framingham, MA adults age 30-62	U.S. adolescents grades 7-12 in 1994	U.S. adult population	Wisconsin high school graduates in 1957
Sample design	Census	Census	Purposive; track offspring; continue to recruit	Purposive; track offspring	Probability; track sample's parents, administrators, peers, friends, and romantic partners	Probability; track all members of sampled family units over time	Probability; track parents, siblings, spouses, widows
Sample size	10,000[a]	5,213[b]; original population =1,231; current population = 3,610 plus >2,000 emigrants and their descendants	4,970[c]	Original cohort: 5,209; Offspring cohort: 5,124 Generation III being recruited: goal of 3,500	Wave I: 20,745 adolescents, 17,700 parents; Wave II: 14,738 adolescents; Wave II: 15,197 young adults	Original design: 3,000; data for > 65,000 individuals by 2003	10,317
Units of analysis	Village, family, individual	Village, household, family, individual	Individual	Individual, family	Individual, family, school, neighborhood	Individual, family	Individual

Table 1. Continued.

Periodicity and mode of data collection	Major fieldwork in 1956-57, 1962-63, 1972-73, 1981-82 1987-88, 1992, 1994-96, 1997, 1998; ethnographic census updated continuously in some villages; subprojects at irregular intervals	Ethnographic censuses in 1945, 1960, 1970, 1980, 1990, 2000; biennial household surveys 1974-present; subprojects at irregular intervals	5 exams from birth to 1 year; semiannual exams from age 5 through puberty; annual to age 20; biennally after 21	Regular exams every 2 years	Waves I and II surveys, 1994-96 Wave III, 2001-02	Annual survey interviews 1968-97; biennial interviews 1997-present	Surveys with original cohort or parents in 1957, 1964, 1975, 1992; selected siblings in 1997, 1994; original cohort, siblings, spouses, widows 2003-04
Core content of panel data	Demographic data; for some years: education, residence, labor migration, economic indicators	Demographic data, socioeconomic data, parish church archival data (late 1700s-present); civil registry data, (1930-1990s)	Demographic data, health history, physical activity, smoking, alcohol, maturity, mentruation, anthropometry, body composition, blood pressure	Demographic data, health history, extensive physical exam, laboratory tests	Demographic data; health-related behaviors; social relationships; family, school, community contexts; anthropometry; biomarkers	Demographic data, economic indicators, family structure, housing, labor market work, housework time, geographic mobility, health	Demographic data, youthful aspirations, schooling, family formation, work history, social participation, residence, mental ability, health
Funding agencies	Institute for African Studies at U Zambia, SSRC/ACLS, NSF, John Simon Guggehnheim Mem, MacArthur, Fulbright, Cal Tech, UC-Berkeley	Institute of Social Anthropology, Smithsonian Institution, NSF, NIGMS, Wenner-Gren Foundation; Fulbright, UC-Berkeley, SMU	Fels Fund of Philadelphia, NIH, NSF	NHLBI	NICHD plus 17 other federal agencies	OEO, NSF, NICHD, NIA, HHS, HUD.	NIMH, NIA, Soc Security Admin, UW-Madison, Russell Sage, NSF, MacArthur, Spencer, and other foundations
Number of publications	Ted Can you venture a guess? >200	>100	>1,500	> 1,200	> 1,000	> 2,000	> 160
Data publicly available?	No	In process	No	Yes, with approved proposal	Yes	Yes	Yes

Table 1. Continued.

[a]Approximate total population as of 1992 (Clark et al. 1995:95). [b]Master database as of 2000 (see Tzintzuntzan documentation site). [c]Total database, including 4,099 living participants as of 2003 (Remsberg and Siervogel 2003:250) NHBLI = National Heart, Lung, and Blood Institute, NIA = National Institute on Aging, NICHD = National Institute of Child Health and Development, NIH = National Institutes of Health; NIMH = National Institute of Mental Health, NSF = National Science Foundation; OEO = Office of Economic Opportunity in U.S. Dept. of Commerce, SSRC/ACLS = Social Science Research Council/American Council of Learned Societies, HHS = U.S. Dept. of Health and Human Services, HUD = U.S. Dept. of Housing and Urban Development.

Table 2. Comparison of mean and standard deviation of attributes of subjects and households that left and that did not leave the sample using only pre-intervention survey, 2001, among Tsimane' Amerindians, Bolivia

Variable:	Left sample		Did not leave sample		Difference
	N	***Mean and SD***	***N***	***Mean and SD***	***Mean and SE***
ADULTS:					
Anthropometrics:					
BMI	67	23.24 (2.23)	307	23.11 (2.51)	.12 (.31)
ZAM	69	-.77 (.93)	309	-.73 (.86)	-.03 (.09)
ZSF	67	-.62 (.48)	311	-.71 (.51)	.08 (.08)
ZWT	66	-.91 (.46)	309	-.97 (.55)	.05 (.05)
Economic:					
Income	68	53.85 (89.23)	309	59.04 (94.67)	-5.18 (10.71)
Earnings	68	39.87 (70.86)	309	37.73 (85.74)	2.14 (8.29)
Consumption	68	5.46 (6.27)	309	11.86 (9.03)	-6.40 (1.18)***
Barter	68	8.50 (20.52)	309	9.43 (20.14)	-.92 (2.80)
Wealth	68	387 (302)	309	405 (376)	-18 (45)
Culture and defense of property rights:					
Encroachers	58	1.34 (1.03)	253	1.20 (.97)	.13 (.12)
Farming:					
Area rice	70	6.25 (4.38)	307	6.99 (5.14)	-.74 (.81)
Area maize	70	2.08 (2.54)	306	2.16 (2.85)	-.78 (.80)
Fallow forest	71	3.87 (4.4)	306	4.68 (5.24)	-.80 (.58)
Old-growth forest	70	5.37 (4.12)	307	5.48 (4.79)	-.10 (.75)
Human capital:					
Schooling	58	1.81 (1.90)	253	1.50 (2.22)	.30 (.41)
Perceived health:					
Diarrhea	67	.05 (0.23)	311	.07 (.26)	-.01 (.03)
Days ill	67	1.98 (3.28)	311	2.21 (3.36)	-.22 (.33)
Days in bed	67	.73 (1.57)	311	1.07 (2.37)	-.34 (.24)
Demographic:					
Male	67	.56 (.49)	311	.57 (.49)	-.005 (.05)
Age	67	30.80 (10.71)	311	32.85 (12.32)	-2.04 (1.46)
Household size	67	5.74 (2.40)	311	6.07 (2.59)	-.32 (.37)

Notes: Standard deviation in parenthesis; in column "difference" number in parenthesis is standard error (SE) from OLS regression of outcome variable against binary dummy variable for attrition (1=attrition; 0= no attrition) with constant and clustering by village. Anthropometric measures of children exclude children below one year of age. *, **, and *** significant at ≤10%, ≤5%, ≤1%.

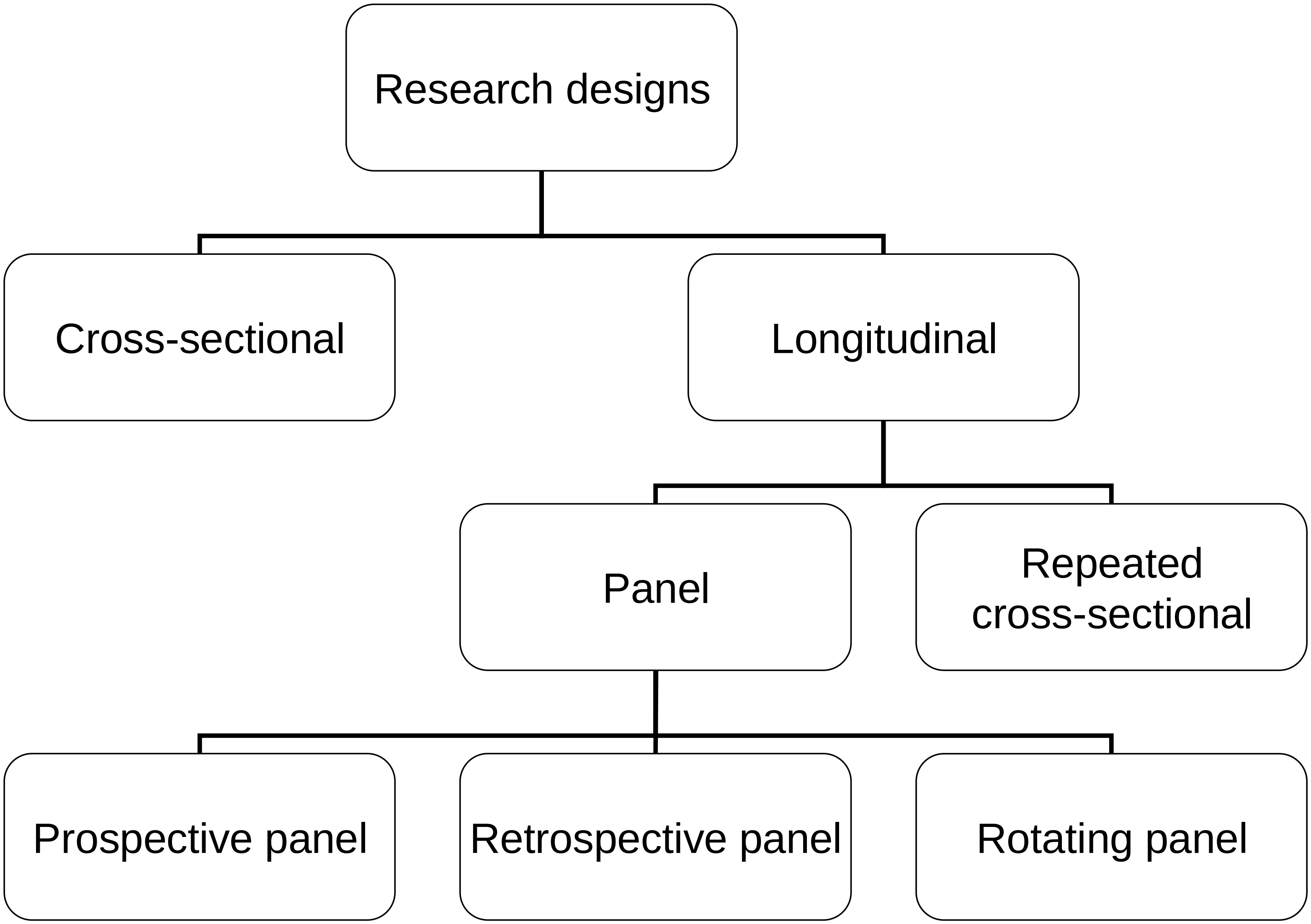

Figure 1. Cross-sectional and longitudinal research designs.

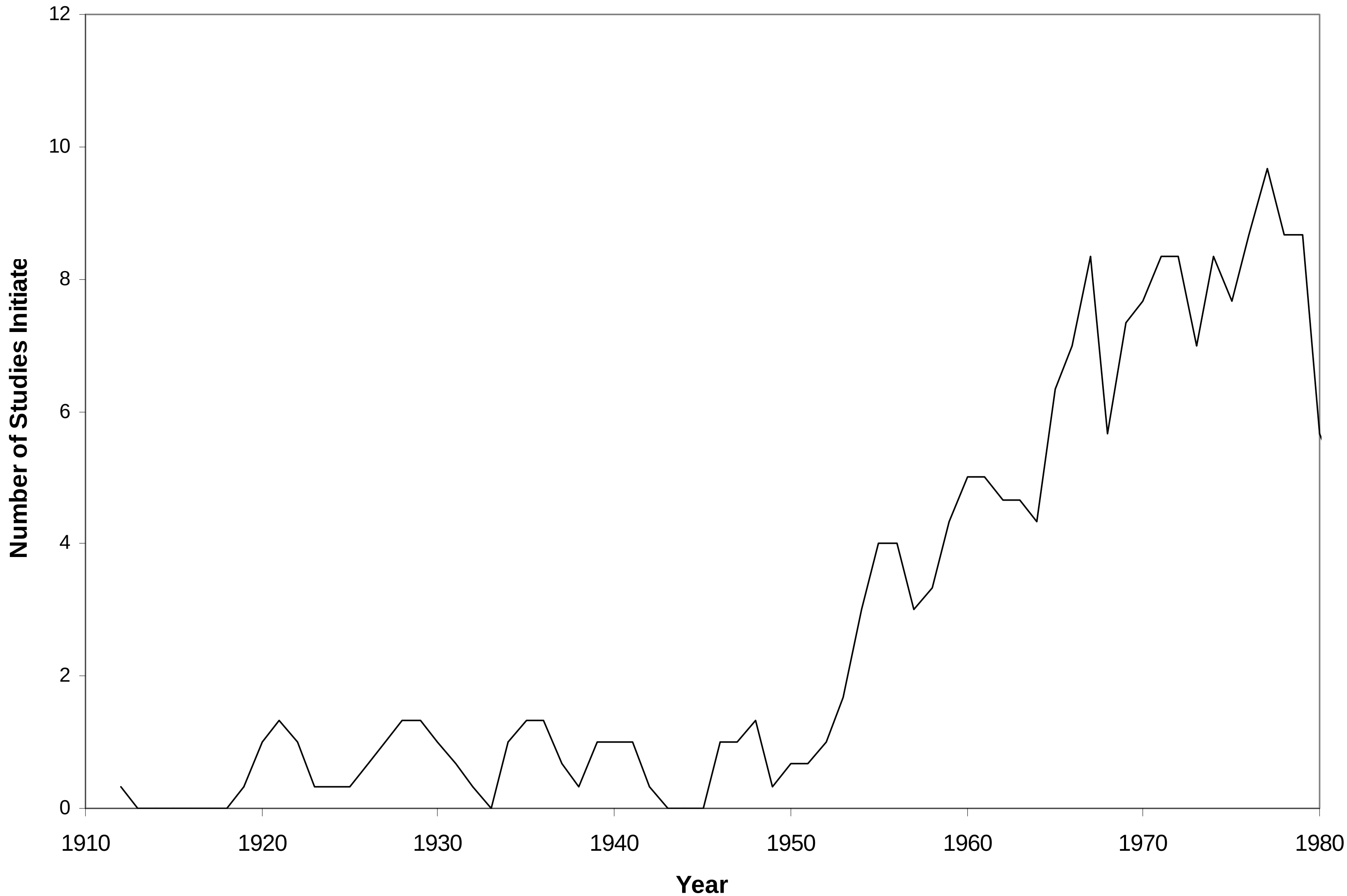

Figure 2. Number of studies in the *Inventory of Longitudinal Studies in the Social Sciences*, 1911-1980 (three-year moving average). Source: Young et al. 1991.

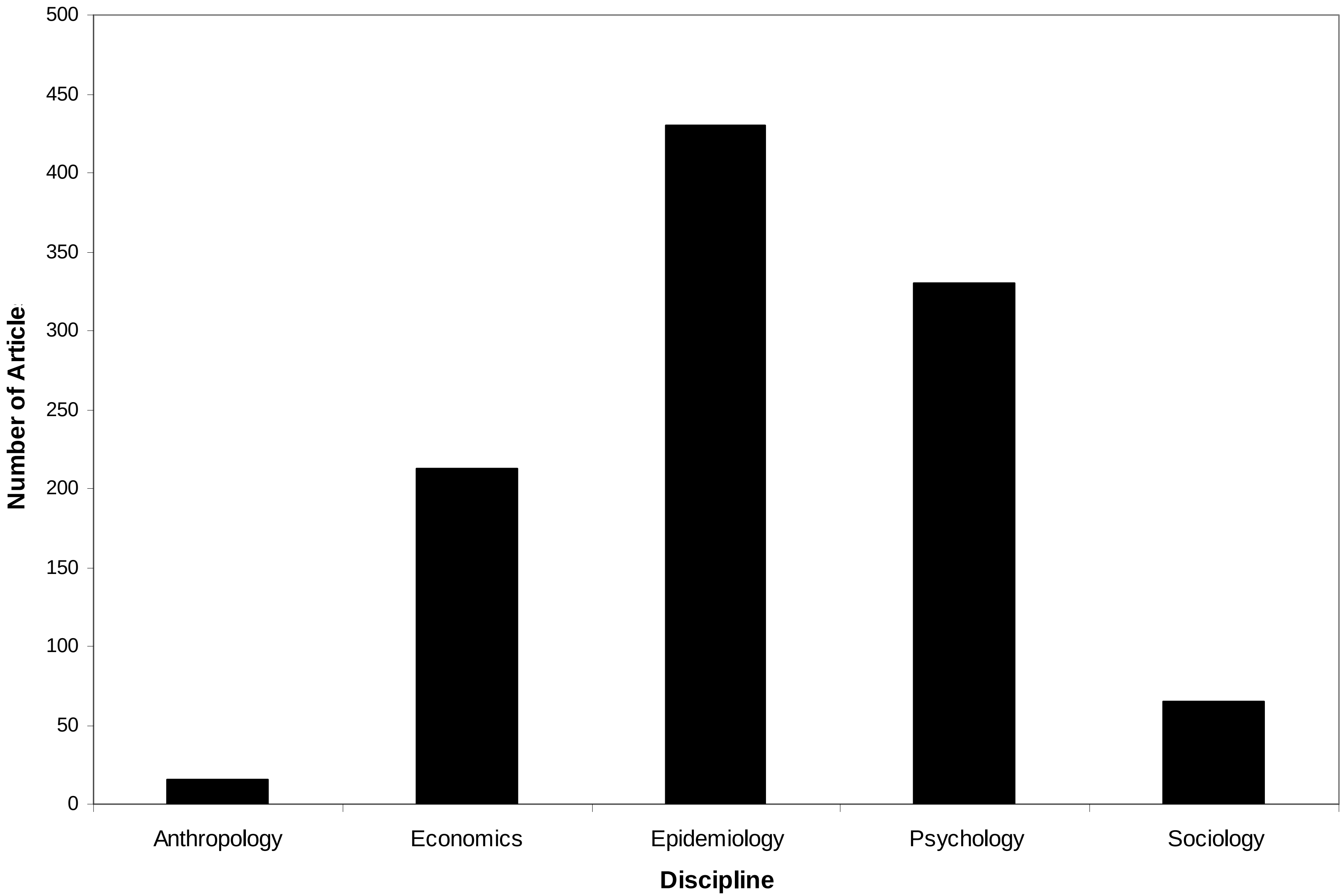

Figure 3. Frequency of longitudinal studies in the Social Science Citation Index by discipline, 1990-2003

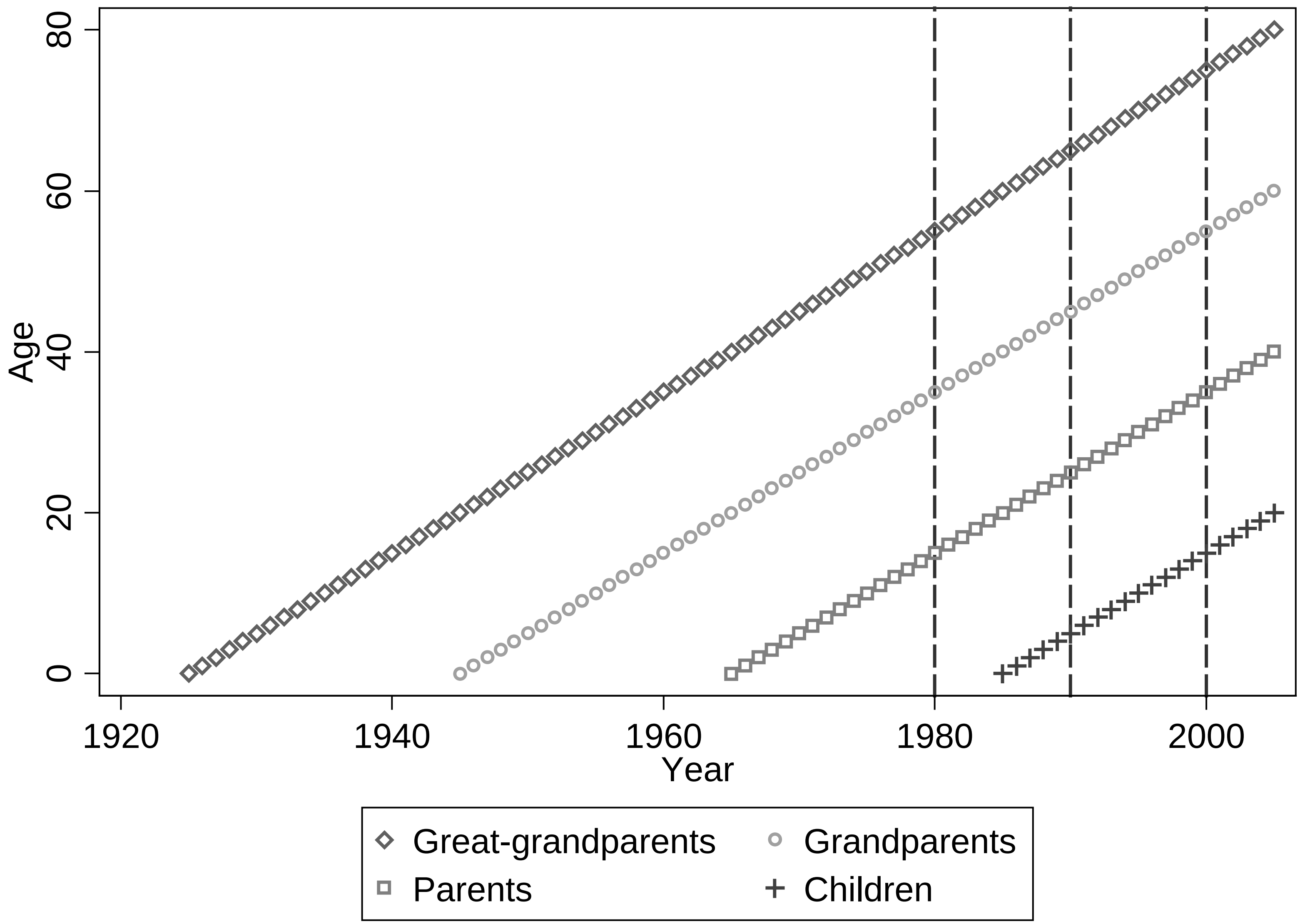

Figure 4. Lexis diagram of age, period (year) and cohort effects. Dashed lines represent hypothetical times of data collection.