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ATTRITION IN THE KWAZULU NATAL INCOME DYNAMICS STUDY, 1993-1998

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ABSTRACT

Panel (or longitudinal) data often provide an understanding of the dynamic behavior of individual households not possible with cross-sectional or time-series information alone. However, a disturbing feature of this type of survey in both developed and developing countries is that there is often substantial, nonrandom attrition. Therefore, an important concern is the extent to which attrition inhibits inferences made using the data. This note examines attrition in the KwaZulu-Natal Income Dynamics Study (1993–1998) and assesses the extent of attrition bias for a specific empirical example. The analysis shows that 1993 first round nonresponse is largely unrelated to observable characteristics of the communities other than indicators of migration activity. Multivariate regressions are then used to describe the characteristics of the households attriting in 1998, revealing the importance of distinguishing between two types of attriting households, those that moved and those that apparently moved but left no trace. For example, increased household size reduced the probability of either type of attrition, whereas measures of higher quality of fieldwork in the 1993 survey only reduced the probability that a household left no trace. While observable differences between attritors and non-attritors indicate attrition is nonrandom, it does not necessarily follow that estimated relationships based on the non-attriting sample suffer from attrition bias.

To more directly explore attrition bias, which is by its nature model specific, this analysis estimates household-level expenditure functions correcting for attrition bias using standard Heckman selection procedures and a quality of 1993 interview variables as identifying instruments. There is positive selection, and although many of the other parameter estimates are quite similar, a Hausman test rejects the equality of coefficients between the corrected and uncorrected models. Therefore, this study concludes, at least for this simple case, that attrition does appear to bias the "behavioral" coefficients. These results are in contrast to other work using these data that suggests little attrition bias for different estimated models, highlighting that attrition is indeed model specific. Large levels of attrition do not always lead to attrition bias; however, sometimes they do. Since it is typically difficult to determine the bias for a particular analysis a priori, it behooves researchers using panel data not to avoid using panel data when there is attrition, but to always evaluate the effect of such bias on the analysis at hand.

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1. INTRODUCTION

The analysis of panel (or longitudinal) data, where the *same* individuals or households are interviewed multiple times, contributes substantially to the understanding of a variety of phenomena. For example, while two cross-sectional surveys of different households at two points in time might reveal a constant poverty rate, they are silent as to whether this reflects chronic poverty, i.e., the same households in poverty in each period, or transitory poverty with an equal proportion of households exiting and entering poverty between surveys. If appropriate policy action depends on the chronic or transitory nature of poverty, it is critical to be able to distinguish between the two, something that panel data allow. Thus, panel data often permit an understanding of the dynamic behavior of individual households not possible with cross-sectional or time-series information alone.

A second advantage of panel data is that they enable us to resolve, or at least reduce concern about, a key econometric problem: omitted variable (or unobserved heterogeneity) bias. For example, rarely do surveys observe or measure a family's preferences and priorities for educating its children. It is quite likely that families that put a high priority on education will perform additional work to obtain income needed to pay school fees. If we use cross-sectional data alone to determine the effect of family income on education, we risk making incorrect inferences, i.e., families with the highest income may also be those that prioritize education the most. In other words, omitted preferences for education are correlated with included income measures. Estimates derived from such data will tend to overstate the impact that an income transfer would have on educational decisions of families that give only an average priority to education. In contrast, with panel data, econometric methods can be used to control for these sorts of time-invariant preferences and family characteristics, allowing unbiased estimates of the effect of income on education.[1]

Panel data are not a panacea, however. In practice, one must balance the potentially substantial benefits against the many real difficulties encountered in survey work that lead to, in particular, errors of measurement and sample attrition.[2] Either of these can introduce different sources of bias, inhibiting anew the capacity to make correct inferences from the data.

Many of the estimators that control for unobserved heterogeneity are sensitive to bias from measurement error; indeed, in cases with large (random) measurement errors it may even be preferable to eschew standard panel data econometric techniques and not control for the omitted variables (Hsiao 1986). While exact results rely on the data and the form that measurement error takes, the commonly used signal-to-noise ratio provides an intuition for the underlying problem. For example, since fixed-effects estimators rely on variation over time for identification, in settings where there is little such variation, fixed-effects estimators may actually lower the signal-to-noise ratio relative to the alternative of ordinary least squares. Conversely, in settings where there is rapid change over time, as is arguably the case in South Africa, fixed-effects estimators may increase

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[1] Hsiao (1986) notes two other econometric advantages of panel data: (1) increased number of observations or degrees of freedom, and (2) increased variability of regressors since they are measured over both space and time.

[2] One should also keep in mind that unless refresher samples are added in later rounds, the current period representativeness of the panel sample deteriorates over time, and this may occur more quickly in rapidly changing societies. Thus, many analyses appropriate for a representative cross-sectional survey are not appropriate for individual rounds of a panel survey.

the signal-to-noise ratio. In practice, it is not usually possible to ascertain the extent or nature of measurement error, however, although panel data often provide partial means to do so, e.g., through repeated measurements.

Similarly, unless it is random, sample attrition (i.e., when some targeted households are not successfully interviewed in all rounds) may introduce biases into analyses based only on the non-attriting sample. Of course, the potential problem of selection bias due to nonresponse exists in cross-sectional surveys as well, but it is typically exacerbated in panel data due to the difficulties inherent in interviewing the same individuals or households multiple times. To put greater confidence in the results obtained using panel data, we must assess the magnitude of these potential problems.

This analysis focuses on the latter problem of sample attrition, and the possible ensuing selectivity, with special reference to a recently collected South African panel survey of African and Indian households, the KwaZulu-Natal Income Dynamics Study (KIDS) (May et al. 2000). The study examines attrition in KIDS in detail in order to (1) document the procedures and outcomes of the survey as a resource, both for those using this publicly available data and for those embarking on their own survey work; (2) describe the characteristics of households that attrited in the second round sample and explore their correlates in a multivariate framework; and (3) propose a simple methodology to assess, and correct for, attrition bias using information reflecting the quality of the fieldwork in the first round as identifying instruments. Because KIDS is a comprehensive survey and can be used for a variety of analyses, however, it will not be possible to make global statements about attrition bias. As Becketti et al. (1988) point out, the hypothesis that attrition is correlated with some (possibly unobserved) variable of interest is quite broad. Rather, the results presented here should be treated as methods to be replicated by other analysts using the data.

This note proceeds as follows. The next section briefly reviews the literature on attrition in panel data. Section 3 describes the 1993 first round sample and initial levels of nonresponse. Section 4 begins with a detailed account of the1998 second round survey protocols and the extent and nature of attrition experienced. While observable differences between attritors and non-attritors (as well as within the former group) indicate attrition is nonrandom, this does not necessarily imply that estimated relationships based on the non-attriting sample suffer from attrition bias. To more directly explore attrition bias, which is by its nature model specific, Section 5 uses a simple application to estimate household level expenditure functions and correct for attrition bias using standard Heckman selection procedures and quality of 1993 interview variables as identifying instruments.

2. ATTRITION IN PANEL DATA: A REVIEW OF THE LITERATURE

Among the earliest large-scale (e.g., 1,000+ household) panel surveys, are those begun in the United States in the 1960s, including the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey of Labor Market Experience (NLS). While initially designed to study the nature and causes of poverty in the United States, these surveys have subsequently been used to examine a range of topics, including labor supply, earnings, family composition changes, and residential mobility (Baltagi 1995).

For the most part, large-scale panel studies began much later in developing countries,[3] facilitated by the LSMS project of the World Bank in the late 1980s (Deaton 1997). Prior to that, however, various institutions, including the Institute for Crop Research in the Semi-Arid Tropics (ICRISAT) and the International Food Policy Research Institute (IFPRI), conducted several small-scale surveys.

As a result, both the level of attrition and analysis of its impact are more advanced for the U.S.-based surveys. For example, in the PSID, over half the original 1968 sample had attrited by 1989 (Fitzgerald, Gottschalk, and Moffitt 1998a). Such high levels of attrition have spawned research examining the reliability of inferences drawn from these data. It turns out that the *level* of attrition alone need not necessarily distort the results; one must examine the processes underlying it (see “Attrition in Longitudinal Surveys,” *The Journal of Human Resources* 33 (2)). On the whole, research in this area has not found large biases due to sample attrition for some commonly estimated labor market models. While encouraging to those working with panel data, these results may not extend to other models or settings and data sets where the processes underlying the attrition may differ.

There are a number of reasons why one might expect selection due to attrition to be more severe in developing than developed countries (Ashenfelter, Deaton, and Solon 1986; Thomas, Frankenberg, and Smith 1999). In particular, better information and capability for tracking clearly exist in developed countries; respondents are often just a

[3] One exception is the Indian National Council of Applied Economic Research Additional Rural Incomes. Survey (NCAER-ARIS) initiated in 1968 on a national sample of over 4,100 households and followed for three years and then reinterviewed again in the early 1980s.

phone call away. Furthermore, the high levels of mobility and long distance migration associated with development are likely to complicate survey work in developing countries. Focusing on South Africa, the distrustful legacy of apartheid and the dramatic change in the country since 1994 are unlikely to make it an easy place in which to do a panel study. Partly offsetting these concerns, however, are the much lower refusal rates typical of developing countries, perhaps reflecting lower opportunity costs of time and possibly different cultural attitudes toward the interviewing process.

There is a small literature on attrition in developing country datasets. The Indonesian Family Life Survey demonstrates that with careful planning it is possible to collect panel data in developing countries with similar or even *lower* attrition than in typical developed country surveys (Thomas, Frankenberg, and Smith 1999). In Indonesia, a key element to the high reinterview rate was the decision to track movers, something not typically done in developing country surveys. Tracking reduced attrition by more than half. Nevertheless, the attrition that remains is still nonrandom and is associated with migration, as well as with community and household characteristics. After controlling for community-level wealth, attrition is negatively associated with household size and positively associated with logarithmic per capita expenditures, but only for those below the 25th percentile of the per capita expenditure distribution within the community. Thus, it appears that households at the lower end of the distribution were more likely to move and not be successfully tracked.

While documenting the existence of nonrandom attrition is important, it is not the end of the story. What is of ultimate concern is whether, and to what extent, the attrition invalidates inferences made using the data. For example, consider a canonical (one period) selection model as described in equations (1) and (2).

$$y_i = x_i \boldsymbol{b_1} + \boldsymbol{e}_i \qquad y_i \text{ observed only if } A_i^* < 0 \tag{1}$$

$$A_i^* = x_i \boldsymbol{b}_2 + z_i \boldsymbol{g} + \boldsymbol{u}_i \tag{2}$$

Equation (1) is the model of interest. The outcome variable, y_i, is observed only for a subset of the entire sample, those for whom the latent index, A_i^*, is less than zero. Equation (2) represents a selection function depending on (possibly) the same independent variables in (1) as well as some additional ones. In practice, we do not typically observe (2) but only an indicator of whether an observation is selected or not, i.e., $A_i = 1$ ($A_i^* < 0$) if selected and $A_i = 0$ ($A_i^* \geq 0$) if not. If there is correlation between the error terms $\boldsymbol{e}_i$ and $\boldsymbol{u}_i$, estimation of (1) ignoring (2) leads to inconsistent parameter estimates of $\boldsymbol{b}_1$; this is commonly referred to as selection bias.

If we now treat y_i as an outcome variable from the second period of a two-period panel data set (but leaving x_i as first period measures, perhaps because they are time invariant) and recast (2) as an attrition function, we have the equivalent result for attrition in a panel survey. If there is correlation between the error terms, estimation of (1) ignoring (2), i.e., estimation on the non-attriting sample alone, leads to inconsistent parameter estimates and thus incorrect inferences from the data.

The stylized model presented here makes it clear that any evaluation of attrition bias must be model specific. As the outcome modeled in (1) changes from labor supply, to fertility, to child health, $\boldsymbol{e}_i$ changes, reintroducing the possibility that there is correlation between the error terms and therefore attrition bias.

Building on the methodology of Becketti et al. (1988), Fitzgerald, Gottschalk, and Moffitt (1998a) suggest a simple way to test for attrition bias in panel data using first-round information supplemented by knowledge of A_i, i.e., whether the household attrited at a later date. The procedure is to estimate (1) using the entire set of first-round information with x_i and a set of interactions between x_i and the attrition indicator A_i as independent variables. The aim is to determine whether those who subsequently leave the sample differ in their initial behavioral relationships. If the interactions are significant, then it should be taken as a warning sign for attrition bias.

If attrition bias is found, one possible solution, estimation of a selection-corrected model (Heckman 1979), lies with z_i provided it is correlated with attrition but not correlated with e_i. A selection (or in this case attrition) function is estimated first that includes all the exogenous variables and the identifying instruments z_i and then a selection correction factor is introduced into the second-stage estimation of (1).

Fitzgerald, Gottschalk, and Moffitt (1998a, 1998b) also suggest an alternative solution for a slightly different form of attrition selection. They first distinguish between two cases: 1) selection on unobservables, essentially the model discussed above, and 2) selection on observables, where z_i and e_i are correlated but e_i and u_i are not. A convenient interpretation for the second formulation is that z_i are outcome variables measured in round 1, perhaps even including y_i itself, which might be considered endogenous in the (structural) model (1). Their proposed solution involves estimating an attrition function using the (endogenous) z_i and then estimating model (1) by weighted least squares where the weights are constructed from the first-stage attrition function (see Fitzgerald, Gottschalk, and Moffit 1998b for details).

Since the South African data provide quality of 1993 interview variables that are plausibly exogenous to a variety of outcomes, this analysis adopts the Heckman selection approach to correct for attrition bias. A further advantage to this approach, compared with the weighted least-squares methodology described above, is that it is robust to attrition selected on both observables and unobservables.

3. 1993 BASELINE SURVEY

The first South African national household survey, the Project for Statistics on Living Standards and Development (PSLSD), was undertaken in the last half of 1993 by a consortium of South African survey groups and universities under the leadership of the South African Labour and Development Research Unit (SALDRU) at the University of Cape Town, with financial and technical support from the World Bank and the governments of Denmark, The Netherlands, and Norway (PSLSD 1994).[4] Similar to a living standards measurement survey (Grosh and Muñoz 1996; Deaton 1997), the main instrument was a comprehensive household survey that collected a broad array of information on the socioeconomic condition of households. Among other things, it included sections on household demographics, household environment, education, food and nonfood expenditures, remittances, employment and income, agricultural activities,

[4] PSLSD has also been referred to as the SALDRU survey, the South African Integrated Household Survey (SAIHS), and the South African Living Standards Measurement Survey (LSMS).

health, and anthropometry (weights and heights of children aged 6 and under). In addition to the household questionnaire, a community questionnaire was administered in each cluster of the sample to collect information common to households in an area such as school availability, health care facilities, and prices for various commodities.

An important component of the survey design, as with any household survey, was the definition of a household. To account for the complexity of the South African situation with its history of residential restrictions and migrant labor, a two-tiered definition for household members, resident or nonresident, was formulated based on time spent in residence. Only limited information was collected from nonresident household members.[5]

The sample was selected using a two-stage, self-weighting design. In the first stage, clusters were chosen with probability proportional to size from census enumerator districts (ESD) or approximate equivalents where an ESD was not available. This step was also designed, via stratification, to provide representativeness at the province and homeland area levels as they were demarcated in 1993. In the second stage, a census of all physical dwellings or "stands" in each chosen cluster was completed, the dwellings were numbered, and a list of those to be interviewed was randomly generated. In addition, a second list of "replacements" or "substitutes" was randomly generated from

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[5] PSLSD (1994) provides the details: *resident household members* were defined as (i) those who had lived under this roof for more than 15 of the last 30 days; (ii) when they are together they share food from a common source (i.e., they cook and eat together); and (iii) contribute to or share in, a common resource pool (i.e., they contribute to the household through wages and salaries or other cash and in-kind income or they may be benefiting from this income but not contributing to it, e.g., children, and other non-economically active people in the household" (p. iv). The household was also defined to include *nonresident members*, i.e., those that satisfied conditions ii and iii but who needed only to have lived under the same roof at least 15 days out of the past year.

the remaining dwellings; when it was not possible to interview a predesignated, first-choice random sample household, the team was instructed to interview a household from the replacement list.[6] Households were then constructed based on the set of people who lived in the selected dwellings, making it possible for more than one household, as defined in the survey, to reside in a single dwelling (see PSLSD 1994 for further details).

Before turning to the 1998 second round survey, I document the level of non-response in the 1993 round for the 1998 target sample: African and Indian households living in KwaZulu-Natal Province. While it is true that households on the replacement list are randomly selected, it seems unlikely that the process by which first-choice households were dropped is completely random. For example, they may have been households that refused to be interviewed or that migrated (temporarily or permanently) between the time of the census and the survey (although this was less than one month in nearly all cases). On the other hand, more replacements in a cluster might be reflective of less careful fieldwork if interview teams rushed to complete their work and did not carefully search for respondents. Since little or no information was collected on the first-choice households not interviewed, I follow Thomas, Frankenberg, and Smith (1999) by examining the characteristics of the survey communities and their association with the

[6] In addition to replacements at the household level, a small number of first-choice random sample clusters were also replaced because they were considered too dangerous to carry out fieldwork. These were replaced with communities displaying otherwise similar characteristics (PSLSD, 1994). The author is not aware of any source detailing where this occurred.

frequency of replacement interviews in order to explore possible effects on the final sample interviewed.[7]

Overall, 90.3 percent of the predesignated first-choice random sample of 1,393 households[8] was interviewed in the original 1993 fieldwork, but this average completion rate conceals substantial variation among clusters. One-third of the clusters indicated *all* first-choice households were interviewed while in the remaining two-thirds, 1993 average completion rates vary, dipping as low as 48 percent for one rural community in the former KwaZulu homeland area. On average, rural areas had slightly lower completion rates than urban areas (90 versus 92 percent); within urban areas Indian communities had slightly lower completion rates than African communities (90 versus 93 percent).

Completion rates are largely unrelated to an array of observed characteristics of the community and average characteristics of households interviewed in the community, with only a few exceptions, including indicators of previous migration to the area. Using the fraction completed in each community as the dependent variable, the most robust results from regressions treating each of 69 communities as an observation are negative and significant associations between completion rates and indicators of in-migration to

[7] The ensuing analysis treats 32 households for which the replacement variable (*visit*) is missing as having been first-choice random sample draws. The results are qualitatively unchanged if these are treated as replacements instead.

[8] There were 1,393 (1,178 African, 215 Indian) households in the 1993 KwaZulu and Natal sample targeted for reinterview in 1998. This excludes 112 white and 53 colored households, groups that were not targeted in 1998.

the cluster (see Table 1).[9] For example, specification (3) in the third column of Table 1 indicates that in clusters where the fraction of households interviewed that migrated to the location in the past five years was higher, completion rates were significantly lower. A similar negative relationship obtains for a dummy variable indicating communities where more people had arrived than left in the 12 months prior to the 1993 survey, i.e., where there was net in-migration. Communities with lower completion rates appear to have been either growing or experiencing high mobility. Therefore, in addition to being a possible proxy measure for survey quality, the completion rate may also be a proxy measure for mobility within communities. For both these reasons the completion rate might be a useful predictor of attrition in later rounds. Of course, these results raise the possibility that even the first-round sample may be somewhat selective toward households that were more firmly rooted in the community and less likely to move. I return to the role of migration for second round attrition below.

4. 1998 RESURVEY

South Africa has undergone dramatic political, social, and economic change since the first democratic national elections in 1994. With the aim of addressing policy research questions concerning how these changes are affecting South Africans, the households surveyed in PSLSD in KwaZulu-Natal province were resurveyed from March to June

[9] The lack of association for other variables is not due to the large number of controls. Except for presence of a secondary school and the migration indicators, none of the other controls are significantly correlated with completion rates in unconditional bivariate comparisons. A variety of nonlinear specifications for expenditures were also considered.

1998 for the KIDS. A research consortium including the University of Natal, the University of Wisconsin, and IFPRI directed the survey. The choice of KwaZulu-Natal was in part the result of practical considerations, including a confluence of research interests, resources, and the feasibility of locating households that had been interviewed in 1993 (since the original survey was not designed to be a panel). The data, as well as a sampling of the policy questions it can be used to inform, are described elsewhere in more detail (May et al. 2000).

One of the administrative changes made after the 1994 elections by the South African government was the designation of new provinces and provincial boundaries. The former KwaZulu homeland area and Natal province were combined to create KwaZulu-Natal province. Unlike some of the other new South African provinces, however, the pre-1994 borders of Natal (which circumscribed the KwaZulu homeland area) were not altered; thus the 1993 sample remains representative at the newly formed provincial level.

In theory, three factors underlie the level of attrition in a survey: 1) the mobility of the target population, 2) the success with which those who move are followed and reinterviewed, and 3) the number of refusals. Thus, attrition is often closely linked to migration behavior. In practice, there is also the possibility of problems or errors in fieldwork (both in earlier and later rounds).

ATTRITION IN 1998

The 1993 (and thus 1998 target) portion of the PSLSD sample included 1,393 households (215 Indian and 1,178 African, see Table 2). Interview teams first went to the location of a 1993 household. If it was learned that the household had moved, the team was instructed to get new location information using a household identification form. The teams sought address or other contact information from other family members, neighbors, and local facilities, e.g., clinics and schools. If a new address was found, and was sufficiently detailed, the household was (later) tracked to its new location.

Of the target sample, 1,171 households (84.1 percent) were successfully reinterviewed (success defined as having reinterviewed at least one member from the 1993 household) (see Table 2). Sixty-three households were tracked to new locations and successfully reinterviewed using the tracking protocol described above.[10] Many surveys in developing countries do not attempt to track movers; had this strategy been followed only 79.6 percent of the target households would have been reinterviewed. Put another way, the tracking procedures yielded nearly a 25 percent reduction in the level of attrition between the 1993 and 1998 surveys. In most surveys of this type in developing countries, refusal rates are low (Deaton 1997; Thomas, Frankenberg, and Smith 1999). This is also true for KIDS: only nine recontacted households refused an interview. Finally, in four households (three single-person and one two-person), all of the 1993 members had died by 1998.

[10] A small number of households moved out of KwaZulu-Natal; while a few of these were followed, only one was successfully interviewed.

For Africans, reinterview rates were the same in rural versus urban areas. Within urban areas, however, reinterview rates for Africans were higher (89.6 percent) in the metropolitan areas, which are characterized by more permanent housing structures and street addresses (not shown). Indian households proved more difficult to reinterview, in part because over 15 percent had moved between the survey rounds.

An alternative way to categorize the households is according to whether they lived in the former KwaZulu homeland area or in the former Natal province (not shown). Among Africans, reinterview rates were much higher in rural areas of the former KwaZulu homeland area compared with rural areas of former Natal province (87.0 percent versus 62.5 percent). Residential restrictions were stricter and property rights more limited for non-whites, and especially blacks, in former Natal province than in the former KwaZulu homeland. Furthermore, two of the communities surveyed in former Natal province consisted of black farmworkers on large, commercial white-owned farms where there appears to have been high turnover, including one farm that went bankrupt, dispersing nearly all of its residents. Finally, the low reinterview rates may also be related to a spate of expulsions from large farms after the 1994 national elections, apparently in anticipation of land reform.

CHARACTERISTICS OF ATTRITING HOUSEHOLDS

For more than one-third of the households not reinterviewed, information collected verified that the household had moved but without enough detail to allow tracking to a new residence. For the remaining households, however, there was simply no trace, i.e., no one approached in the community recognized the name of any household members when presented with the 1993 household roster (see Table 2). These two groups, those who are known to have moved and those who seemingly left no trace, are somewhat different, and it is instructive to consider them separately in the attrition analysis.[11]

To compare the characteristics of 1) those reinterviewed with 2) those not reinterviewed but known to have moved and 3) those not reinterviewed leaving no trace, I estimate a multinomial logit distinguishing the three mutually exclusive categories. The specification conditions on explanatory variables that parallel the baseline completion rate regressions discussed above with two important differences. First, in order to keep them more "reduced form" in nature, they exclude the direct migration indicators. Second, they include two proxy measures of 1993 survey quality, one of which is the cluster average completion rate modeled in the previous section.

Table 3 presents the derivatives (P/ X), at the overall mean of the regressors for each independent variable.[12] (Robust standard errors allowing for correlation within clusters are used to calculate the asymptotic t-statistics in parentheses.) For example, in the first row, first column, 0.057 indicates that households in former Natal province were 5.7 percent more likely to fall into the category not reinterviewed but known to have moved (hereafter "movers") relative to households that were successfully reinterviewed (the omitted category). Similarly, households in former Natal province were 1.8 percent

[11] Table A1 presents similar results for binomial logit specifications on attrition, ignoring the moved versus no-trace distinction for the entire sample of 1,393 households.

[12] The multinomial analysis presented in Table 3 excludes 13 households (nine refusals and four deaths).

(first row, second column, not significant) less likely to fall into the category not reinterviewed and leaving no trace, hereafter “no trace,” relative to households that were successfully reinterviewed.

After conditioning on a host of community-level characteristics, the indicator for the former Natal province is significantly different from zero for the mover category, indicating that households in Natal were 5.7 percent more likely to move relative to being reinterviewed, consistent with the factors relating to instability described above. Households living in wealthier communities, as measured by the logarithm of community average per capita expenditures, appear less likely to move, thereby facilitating reinterview. Community-level wealth is not associated with the probability of being in the no trace category, however. Instead, households in urban communities are less likely to be in the no trace group, probably reflecting the street addresses that aided in tracking. Households in communities with clinics were also less likely to be in the no trace group; clinics often provided an important source of information for the interview teams when tracking. Offsetting these, however, households in communities with tarred roads (approximately half the urban areas) were more likely to be in the no trace category; these communities may be less closely knit.

A number of household-level characteristics are also associated with attrition. Conditional on community average per-capita expenditures, households with more resources, as measured by per-capita expenditures, were more likely to be movers relative to those reinterviewed, but less likely to be in the no trace group. In comparison to the Indonesian case, where households at the lower end of the distribution were more likely to move and not be successfully tracked, in South Africa it was wealthier households that were more likely to move and not be successfully tracked but poorer households that were more likely to leave no trace. Conditional on household size, wealthier households were more likely to move; it is likely that wealthier households had a higher profile in the community, which made it easier for the survey team to learn their whereabouts.

Unsurprisingly, the KIDS survey was more likely to reinterview larger households, especially relative to the no trace group, though additional members increased the likelihood less and less. Various linear splines for household size were also considered and verify that the relationship is nonlinear with diminishing probability for additional members (not shown). This suggests larger households were not as likely to move, consistent with their associated higher moving costs, and, among those that did move, the teams were more likely to find some trace of larger households, which presumably had more contacts within the community.

Identifying and reinterviewing households in a panel survey relies heavily on the accuracy of the original fieldwork. Measures of quality for the original interview, then, may help predict the success of reinterview (Zabel 1998). Two such measures of quality of the 1993 interview are considered next. The first is an indicator of whether the questionnaire was verified (signed) by the team supervisor. Verification was indicated as having been done for all Indian households but only 22 percent of African ones. The hypothesis is that properly verified questionnaires were more likely to have been accurately and fully completed (correct names, address, etc.), making recontact more likely, other things equal.

The second measure recasts the 1993 average completion rate for first-choice, random sample households modeled in the previous section as an indicator of survey quality in communities. More replacements may be indicative of less careful fieldwork if it signals an effort to complete the fieldwork quickly without thoroughly searching for respondents. This interpretation must be balanced with the evidence provided above, however, that the completion rate also appears to be marginally associated with net migration into communities. I include this measure as well since there is no variation in the verification indicator for the Indian subsample.[13]

Both indicators are significant for the no trace group only, and in the hypothesized direction: verification increases the likelihood of reinterview as do higher community average completion rates. While this is consistent with their interpretation as quality variables, the earlier findings regarding the completion rate cannot be disregarded. This measure is probably an indicator of *both* migration activity and quality of the earlier fieldwork. However, since it is not significantly related to the movers, the quality of interview interpretation appears to dominate. In either case, the processes underlying nonresponse in the first round appears to have been compounded in the second round.

In summary, the evidence presented here indicates the following. A large percentage (84 percent) of the original sample was successfully reinterviewed after nearly five years, and the ability to follow those who had moved contributed a substantial portion of the overall success rate. However, attrition in the KIDS survey is nonrandom and varies with, among other things, household size and community and household-level

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[13] We know from 1998 field experience that there were quality variations in the Indian subsample; for example, some communities had a large number of incorrect addresses.

resources. Furthermore, attrition is closely linked to migration. The characteristics of the households that were not reinterviewed but left no trace differ from the other movers, which suggests that the processes underlying their attrition may have been different. Indicators of quality of the interview in 1993 were identified that significantly influence the likelihood of being in the no trace group and might be used to correct for sample selection based on unobservables.

5. ATTRITION SELECTION CORRECTED EXPENDITURE FUNCTIONS

Nonrandom attrition does not imply that estimated relationships based on the non-attriting sample necessarily suffer from attrition bias, but only that they might. Whether they do depends on the existence of correlation between the error terms in equations (1) and (2) shown above. For example, if attrition is selective on observable right-hand-side covariates, and the model is well specified, it may be possible to condition on those variables allowing consistent estimation of (1). This is not possible, however, if there is selection on unobservables. In that case, one possible solution is a standard selection correction methodology (Heckman 1979; Maddala 1986). If there are *z* variables that are correlated with attrition but not with the error term in the equation of interest, a selection equation can be estimated and a correction factor added to the second-stage equation. This is the strategy employed below using the verification indicator and the average 1993 completion rate as identifying instruments.

One of the main goals of the KIDS was to assess the changes in income and expenditure of households five years after South Africa’s first national elections. As such, the data will be used in efforts to understand the underlying mechanisms for households that were able to escape poverty (Carter and May 2000). A simple framework to begin analyzing this is the household-level expenditure function. Below I present estimates of such a function to assess whether attrition will influence estimates based only on the non-attriting sample. To some extent, this is a loaded example since we have already learned above that attrition was associated with per capita expenditures; nonetheless, it is illustrative of a methodology to both test for, and correct, attrition bias.

Following Fitzgerald, Gottschalk, and Moffitt (1998a), I first estimate an expenditure function using all the 1993 first round data and a complete set of interactions with an attrition dummy variable for those households that attrited in 1998, using ordinary least squares (robust standard errors allowing for correlation within clusters are used to calculate the t-statistics in parentheses). The results indicate that households that later attrite are somewhat different in terms of the 1993 relationship between the logarithm of per capita expenditures and a standard set of conditioning variables (Table 4, column 1). There appears to be some attrition bias for this relationship.

Next, I consider two formulations of a “permanent” expenditure function estimating 1998 logarithmic per capita expenditures using initial, i.e., 1993, values of the right hand side variables.[14] Column 2 presents this specification of the expenditure function estimated using ordinary least squares. The results are plausible with over 50 percent of the variation in logarithmic per capita expenditures explained. They indicate

[14] This formulation, using 1993 explanatory variables, is necessary in order to have the complete set of observations for estimation of the selection corrected estimates that follow.

higher per capita expenditures for urban areas, Indian households, smaller households, male-headed households, and households with more educated heads.

The second specification of the 1998 expenditure function is a maximum likelihood, selection-corrected version using the verification indicator and 1993 average completion rate as identifying variables for the first-stage probit predicting selection into the sample, i.e., non-attrition. Column 3 shows the results from the selection (non-attrition) probit. Verification significantly predicts attrition above and beyond the other conditioning variables in the expenditure function. Finally, Column 4 presents the maximum likelihood results from a Heckman selection corrected model (Heckman 1979; Maddala 1986).[15] The selection term is positive and significant, indicating there was positive selection into the sample of non-attritors. With the exception of the constant and an indicator for the former Natal province, however, most of the point estimates are very similar to the uncorrected estimates in column 2. Nonetheless, a Hausman test rejects the hypothesis that the slope coefficients in the uncorrected (column 2) and corrected (column 4) expenditure functions are equal (p-value=0.0267). For this example, then, I conclude that the "behavioral" coefficients in the expenditure functions are indeed biased by attrition in the sample, although this bias is largely confined to a few coefficients.

[15] The results are nearly identical if the verification indicator alone is used as the identifying instrument.

6. CONCLUSIONS

Panel data often provide an understanding of the dynamic behavior of individual households not possible with cross-sectional or time-series information alone. However, a disturbing feature of such surveys in both developed and developing countries is that there is often substantial, nonrandom attrition. In developing countries, attrition is closely linked to migration, which is the result of household-level decisions and is likely to be selective. For these reasons, an important concern is the extent to which attrition inhibits inferences made using the data. This paper examines attrition in the KwaZulu-Natal Income Dynamics Study (1993-1998) and assesses the extent of attrition bias in the context of a specific empirical example.

The analysis shows that 1993 first-round nonresponse is largely unrelated to observable characteristics of the communities other than indicators of migration activity. Multivariate regressions are then used to describe the characteristics of the households attriting in 1998. It is instructive to distinguish between two types of attriting households, those that moved and those that moved but left no trace. For example, increased household size reduced the probability of either type of attrition, whereas measures of quality of fieldwork in the 1993 survey only reduced the probability that a household left no trace.

While observable differences between attritors and non-attritors (as well as within the former group) indicate that attrition is nonrandom, this does not necessarily imply that estimated relationships based on the non-attriting sample suffer from attrition bias. To more directly explore attrition bias, which is by its nature model-specific, I estimate household-level expenditure functions correcting for attrition bias using standard Heckman selection procedures and quality of 1993 interview variables as identifying instruments. The results suggest that, at least for this simple case, attrition does appear to be biasing the “behavioral” coefficients.

In a related paper focused on attrition on unobservables, Alderman et al. (2000) use some of the above techniques to explore attrition bias for three developing country data sets, including the KIDS data examined in this paper. They also document that a variety of family background characteristics are significant predictors of attrition, indicating it is indeed nonrandom. Nevertheless, for a majority of the outcome variables considered across the different countries, coefficient estimates for the influence of those same family background characteristics are not significantly affected by attrition. In particular, for the KIDS sample, estimates of a variety of child anthropometric outcomes indicate attrition bias in only a few of them.

These examples clearly demonstrate that attrition bias for models estimated on panel data is indeed model-specific. Large levels of attrition do not always lead to attrition bias; however, sometimes they do. Since it is typically difficult to determine the bias for a particular analysis a priori, it behooves researchers using panel data not to avoid using panel data when there is attrition, but to always evaluate the effect of such bias on the analysis at hand.

TABLES

Table 1. Baseline nonresponse in the 1993 KwaZulu-Natal sample

Dependent variable: Completion fraction in community (N=69)	(1)	(2)	(3)
Community characteristics			
(1) if former Natal province	0.016	0.040	-0.016
	(0.3)	(0.6)	(0.2)
(1) if urban	0.015	0.044	0.047
	(0.4)	(0.9)	(1.0)
(1) if African	0.059	0.020	-0.070
	(0.8)	(0.2)	(0.6)
(1) if secondary school	0.004		-0.017
	(0.1)		(0.4)
(1) if clinic	-0.057*		-0.062*
	(1.8)		(1.7)
(1) if tarred roads	0.047		0.013
	(0.8)		(0.2)
(1) if net in-migration past year	-0.090***		-0.087**
	(2.9)		(2.5)
Mean characteristics of households in community			
Log per capita expenditures		-0.004	-0.060
		(0.1)	(0.8)
Log household size		0.103	0.062
		(1.1)	(0.7)
Fraction with male household head		0.050	0.117
		(0.4)	(0.9)
Education of household head		0.001	0.006
		(0.0)	(0.4)
Age of household head		-0.045	-0.018
		(1.4)	(0.6)
Age of household head squared X1000		0.461	0.223
		(1.4)	(0.7)
Fraction owning house		-0.183*	-0.162
		(1.7)	(1.6)
Fraction in-migrating past 5 years		-0.490**	-0.468**
		(2.1)	(2.1)
Constant	0.905***	1.919*	1.659*
	(10.3)	(1.9)	(1.8)
R^2	0.22	0.19	0.36
F(all covariates)	2.5	1.2	2.0
p-value	[0.0261]	[0.2866]	[0.0320]

Notes: Ordinary least squares estimates. Absolute value of t-statistics is in parentheses; * indicates significance at 10 percent, ** at 5 percent, and *** at 1 percent.

Table 2. Attrition in the 1998 KwaZulu-Natal sample

Reinterviewed?	African Rural	African Urban	Indian Urban	Total
Yes, same location	719	240	149	1108
Column percent	(81.3)	(81.6)	(69.3)	(79.6)
Yes, different location	23	21	19	63
Column percent	(2.6)	(7.1)	(8.8)	(4.5)
No, known to have moved	54	13	14	81
Column percent	(6.1)	(4.4)	(6.5)	(5.8)
No, no trace	81	18	29	128
Column percent	(9.2)	(6.1)	(13.5)	(9.2)
Refusal	4	1	4	9
Column percent	(0.5)	(0.4)	(1.9)	(0.6)
Death	3	1	0	4
Column percent	(0.3)	(0.4)	(0.0)	(0.3)
Total	884	294	215	1393
Row percent	(63.5)	(21.1)	(15.4)	(100.0)

Table 3. Multinomial logit attrition regressions in the 1998 KwaZulu-Natal sample

Omitted category: Reinterviewed in 1998		
(N=1,380)	Movers	No trace
Community characteristics		
(1) if former Natal province	0.057**	-0.018
	(2.0)	(0.4)
(1) if urban	0.010	-0.079**
	(0.3)	(2.0)
(1) if African	-0.059	0.057
	(1.5)	(0.9)
(1) if secondary school	0.018	-0.003
	(1.0)	(0.1)
(1) if clinic	-0.018	-0.054**
	(0.9)	(2.0)
(1) if tarred roads	-0.033	0.122***
	(1.4)	(3.7)
Community average log pce	-0.070**	0.024
	(2.3)	(0.8)
1993 completion fraction	-0.053	-0.160*
	(0.7)	(1.7)
Mean characteristics of households in community		
Log per capita expenditures	0.018**	-0.027*
	(2.1)	(1.9)
Log household size	-0.025**	-0.042***
	(2.4)	(2.7)
(1) if male household head	0.022	-0.008
	(1.4)	(0.6)
Education of household head	-0.001	0.003*
	(0.3)	(1.9)
Age of household head	-0.001	0.000
	(0.4)	(0.0)
Age of household head squared X1000	0.010	-0.006
	(0.5)	(0.2)
(1) if own house	0.007	-0.030
	(0.4)	(1.2)
(1) if questionnaire verified	0.022	-0.082**
	(1.1)	(2.3)
Constant	0.237	0.229
	(1.2)	(0.7)
Pseudo R2	0.10	
Chi-square (all covariates)	153.4	
p-value	[0.0000]	

Notes: Multinomial logit estimates, derivatives (P/ X), at the overall mean of the regressors for each independent variable are shown. Absolute value of asymptotic t-statistics in parentheses is calculated using robust standard errors, allowing for within cluster correlation. * indicates significance at 10 percent, ** at 5 percent, and *** at 1 percent.

Table 4. Reduced form household expenditure functions

Dependent variable: Ln per capita household expenditures	(1) 1993 Ln per capita expenditure	(2) 1998 Ln per capita expenditure	(3) Selection Probit	(4) 1998 Ln per capita expenditure
Right-side variables 1993				
Community characteristics				
(1) if former Natal province	-0.589 ***	-0.234 ***	-0.323 **	-0.318 ***
	(3.8)	(3.9)	(1.9)	(3.3)
(1) if urban	0.303 ***	0.415 ***	0.349 ***	0.461 ***
	(2.6)	(9.1)	(2.9)	(5.3)
(1) if African	-0.982 ***	-1.071 ***	-0.101	-1.088 ***
	(7.0)	(13.6)	(0.6)	(9.5)
Household characteristics				
Household size	-0.094 ***	-0.060 ***	0.080	-0.052 ***
	(10.1)	(10.9)	(5.1) ***	(7.5)
(1) if male household head	0.097 **	0.055	-0.056	0.050
	(2.1)	(1.3)	(0.6)	(1.3)
Education of household head	0.054 ***	0.070 ***	0.001	0.070 ***
	(6.4)	(10.9)	(0.1)	(8.2)
Age of household head	0.010	0.004	0.006	0.006
	(1.1)	(0.6)	(0.3)	(0.8)
Age of household head squared X1000	-0.040	0.044	-0.026	0.032
	(0.5)	(0.6)	(0.2)	(0.4)
Attrition indicator	0.032	-	-	
	(0.1)			
Attrition indicator interactions	Yes	No	No	No
Inverse Mills ratio (lambda)	N/A	N/A	N/A	0.364 ***
				(2.9)
(1) if questionnaire verified	-	-	0.331 **	-
			(2.2)	
Average 1993 completion rate	-	-	0.536	-
			(1.4)	
Constant	6.223 ***	6.178 ***	-0.174	5.99 ***
	(24.8)	(26.3)	(0.3)	(20.7)
F(attrition interactions)	1.95 *			
p-value	[0.0664]			
N	1,393	1,171	1,393	1,171
R2 (Pseudo R2 in column 3)	0.57	0.55	0.07	
Chi-square (all covariates)			90.4	
p-value			[0.0000]	

Notes: Absolute value of t-statistics in parentheses is calculated using robust standard errors. allowing for within cluster correlation. * indicates significance at 10 percent, ** at 5 percent, and *** at 1 percent.

Table 5. Attrition correlates in the 1998 KwaZulu-Natal sample

Dependent variable: (1) if household attrited in 1998 (N=1,393)	**(1)**	**(2)**	**(3)**
Community characteristics			
(1) if former Natal province	0.163***	0.037	0.030
	(2.6)	(0.5)	(0.4)
(1) if urban	-0.067	-0.088*	-0.066
	(1.1)	(1.8)	(1.1)
(1) if African	0.048	-0.019	-0.008
	(0.6)	(0.3)	(0.1)
(1) if secondary school	-0.023		0.015
	(0.5)		(0.4)
(1) if clinic	-0.057		-0.074*
	(1.4)		(1.7)
(1) if tarred roads	0.079*		0.083**
	(1.9)		(2.1)
Community average log pce	-0.001		-0.050
	(0.0)		(1.0)
1993 completion fraction	-0.249*		-0.232*
	(1.7)		(1.8)
Mean characteristics of households in community			
Log per capita expenditures		-0.010	-0.009
		(0.5)	(0.4)
Log household size		-0.085***	-0.087***
		(3.7)	(4.1)
(1) if male household head		0.015	0.015
		(0.5)	(0.7)
Education of household head		0.002	0.003
		(0.7)	(1.1)
Age of household head		-0.000	-0.001
		(0.0)	(0.2)
Age of household head squared X1000		0.003	0.002
		(0.1)	(0.1)
(1) if own house		-0.015	-0.024
		(0.4)	(0.7)
(1) if questionnaire verified		-0.078	-0.083
		(1.2)	(1.4)
Constant	-0.019	0.156	0.641
	(0.0)	(0.6)	(1.4)
Pseudo R2	.05	0.07	0.09
Chi-square (all covariates)	20.9	33.1	56.0
p-value	[0.0074]	[0.0005]	[0.0000]

Notes: Binomial logit estimates, derivatives (P/ X), at the overall mean of the regressors for each independent variable are shown. Absolute value of asymptotic t-statistics in parentheses is calculated using robust standard errors allowing for within cluster correlation. * indicates significance at 10 percent, ** at 5 percent, and *** at 1 percent.

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