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Stefan Dercon and Joseph S. Shapiro

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Lessons on poverty mobility from longitudinal data

Stefan Dercon[a] and Joseph S. Shapiro[b]
[a]University of Oxford and [b]London School of Economics

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This paper gives a selected review of some of the work on poverty mobility, largely based on recent research. The literature is vast and many interesting suggestive findings have emerged. Most evidence points to the importance of household endowments, in terms of education or assets, as well as community characteristics, in terms of roads and other infrastructure, to allow movement out of poverty. For urban areas, location and access to particular types of jobs appears to matter. The paper revisits this evidence. However, in analyzing poverty mobility, it is not self-evident to move from describing the correlates of poverty mobility to understanding the causal linkages in standard data sets. A related problem involves the methods used, not least the tracking rules and the high attrition rates in most panel data surveys. We illustrate both the evidence as well as some of the problems related to the evidence using panel data from Ethiopia and Tanzania.

Key words: poverty, mobility, poverty dynamics, panel data, longitudinal data

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Corresponding author: stefan.dercon@economics.ox.ac.uk

1. Introduction

Most studies on economic development offer statements on the evolution of poverty over time. It is surprising, then, that only recently have more systematic efforts started to use quantitative data to document this process of changing poverty in developing countries. This paper surveys what has been learned from these efforts, focusing on the evidence gathered using longitudinal data in developing countries.

Extensive resources have been invested in carrying out large representative surveys with detailed data on different welfare indicators. A notable example is the World Bank's Living Standard Measurement Surveys (LSMS) program. While much insight can be gained from these datasets, in virtually all cases these surveys are cross-sectional (or, occasionally, consist of two-wave panel datasets). The problem is that by effectively interviewing different people in each period, surveys provide only limited information on the individual and household trajectories of poverty or living standards over time. While pseudo-panel techniques are possible and can be used effectively (e.g., Deaton and Paxson 1994), the attraction and use of panel data for illuminating the dynamic processes of poverty has become apparent in recent years.

Many conceptual issues related to measuring poverty trends remain unresolved. In this paper, we stay away from these issues and limit ourselves to examining the type of analysis that is generally common in this literature: reporting poverty levels in different periods and providing some analysis of the evolution of these indicators over time, at the individual or household level. Such studies analyze the factors that are correlated with welfare outcomes and usually offer a causal explanation. We refer to this literature as the poverty mobility literature, even though no specific measures of mobility (such as income mobility measures) are usually used (Fields 2002).

Much research since 2000 has explored the short- and long-term dynamics of poverty. The Chronic Poverty Research Centre, a UK-funded partnership of universities, research institutes, and nongovernmental organizations, has sponsored several journal issues and numerous working papers on chronic poverty in various countries. The Pathways From Poverty study has also published initial reports from a number of countries.[1] The Indonesia Family Life Survey (IFLS), South Africa's KwaZulu-Natal Income Dynamics Study (KIDS), and the Ethiopia Rural Household Survey (ERHS) have each inspired numerous papers. An increasing number of studies use instrumental variables or pseudo-panels to estimate the bias that measurement error or panel attrition introduce.

In assessing the types of data that have been available for this kind of work, we summarize post-2000 research on income mobility that uses consumption or income data from a panel of individuals, households, or cohorts. We exclude studies that present region-level poverty indexes at two points in time, and we emphasize empirical rather than theoretical results.[2] This review did not revisit the raw data of these studies, but rather relies on the evidence reported in other papers.

The paper is structured around three key questions. First, what are the findings on the extent to which people move in and out of poverty, and who escapes poverty and who stays behind?

Although studies show a large degree of poverty mobility, there are marked differences across settings. There are also concerns about measurement error. Thus it is difficult to draw general conclusions on this point.

Second, what correlates are identified as linked with changes in poverty? We focus particularly on data from rural Ethiopia to discuss the typical emerging evidence. The main finding is that particular household and community endowments matter, although the context appears to determine the specific factors that are most influential. Further evidence is presented on the role of risk in keeping people poor or allowing them to fall into poverty.

Third, what are some of the problems with existing evidence? A number of issues are discussed, including the nature of the samples, attrition bias, and the problems with measurement error. In some cases problems with specific studies are highlighted, but this review is not meant to be mainly critical. The type of evidence that can be gathered from this work is discussed using examples from ongoing work in Ethiopia and Tanzania.

A number of recent overview articles have pursued related objectives, including Baulch and Hoddinott (2000) and McKay and Lawson (2002).[3] While updating these reviews, we also emphasize some of the remaining challenges related to finding robust evidence for poverty mobility as well as issues such as attrition. In particular, it is our sense that crucial methodological lessons have to be drawn from many of the current studies on mobility. Much of the quantitative work on poverty mobility appears to miss opportunities. For one thing, these studies have not paid enough attention to the underlying problems in these datasets, specifically the methods used to sustain the panel and casual interpretations of attrition. Furthermore, poverty mobility is often also correlated with spatial mobility, yet most panel datasets underlying poverty mobility work limit themselves to tracing households residing in the original localities, missing a potentially crucial part of the analysis of poverty mobility.

In light of these problems, and drawing especially on ongoing work Tanzania, we call for an increasing focus on individual mobility rather than just household mobility. It is an approach that implies intricate and time-consuming tracking and data collection but one that will pay off, we believe, in a deeper understanding of how and why some people escape poverty.

2. Selected Evidence from Recent Panel Data Surveys: Descriptive Statistics

The paper annex presents a brief overview and update on useful panel data surveys and their coverage, based on rounds and versions of these surveys used in recent studies (table 10). The evidence from nearly 50 panel data surveys was collated. Five of these surveys are rotating panels. Two-thirds of the panel data surveys are short, covering five years or less.

Rotating panels or relatively short panels are notoriously difficult to use for clear inference on poverty dynamics. In particular, they make it hard to disentangle phenomena such as poverty fluctuations (many people facing variable living standards, as measured by income or consumption) and measurement error (variables that are difficult to measure accurately for making statements on the standard of living) from genuine poverty mobility. In the latter, people

move out of poverty into a persistently better standard of living or fall into a persistent state of poverty.

It would be an impossible task to summarize all the studies that used these panel data surveys, not least from the point of view of poverty mobility. Many studies present basic descriptive statistics and then continue to the analysis of specific questions. Only a subset of studies undertook an analysis of changes in poverty over time. Furthermore, methods for basic poverty analysis vary widely, and in many cases it would be hard to argue that the state of the art in basic poverty analysis has been followed. As a consequence, this paper must generalize about the evidence brought forward. As do most studies on poverty mobility, we focus on monetary measures of poverty, that is, measures based on consumption or income aggregates, so other dimensions of poverty are ignored. Probably most problematic for careful analysis, we generally base our discussion on head count figures—in line with much of the reported analysis but deeply flawed as a full portrayal of poverty.

2.1 How Likely Are People to Fall into or Escape Poverty?

We can simply describe poverty persistence according to the portion of households that are always, sometimes, or never poor across a survey's waves. Loosely speaking, this tends to be interpreted as suggestive evidence for people who are facing "transient" poverty processes, while other people are "always" and therefore "chronically" poor. This dichotomy is close to concepts of chronic and transient poverty as introduced (more carefully) by Jalan and Ravallion (2002), but most of the empirical work tends to focus on poverty status only, that is, on the head count. Nevertheless, one should be cautious in interpreting these concepts (for a critique based on the analysis of risk and poverty, see Dercon 2005).

Different surveys have different numbers of waves, and different welfare measures (including sometimes income and at other times consumption-based measures); they cover different populations and use different poverty lines, so their results are imperfectly comparable. Even given different methodologies, table 1 allows the generalization that in most countries over half the people a cross-section may identify as poor are in fact transiently poor, and only a minority are chronically poor. A transiently poor person in this context is someone who is not poor in all periods but only in some periods considered, while a chronically poor person is poor throughout the data period. The estimates vary by country and study: two studies using Ugandan data find that the ratio of households poor in only some of the waves to households poor in any wave is 68 and70 percent, two very similar estimates. But for South Africa's KIDS study, estimates of this ratio are in the range of 27 to66 percent.

While this updates the evidence from Baulch and Hoddinott (2000), it does not find different answers to their questions. Just as in their paper, caution is required in interpreting these results. First, the results will depend on the number of rounds: the more data rounds are used, the more likely someone is to be classified as "sometimes" poor, that is, transiently poor. For example, in the Ethiopian rural data presented by one of the studies in table 1, when all six rounds are used, we find that 80 percent of households have a consumption level below the poverty line at least one but not more than five of the six rounds. When only the first and last rounds are compared,

however, transient poverty is only 43 percent. Similarly, it will matter how far apart different rounds are.

While such poverty indexes reflect absolute mobility, transition matrices offer a more detailed picture of relative mobility (table 2). Across nine papers that include transition matrices, between 32 percent (Free State, South Africa) and 59 percent (Chile) of the poorest quintile of a country stays as the poorest quintile over two time periods. Less mobility appears for other quintiles: of all households, between 12 percent (Ethiopia) and 63 percent (Tehran, Iran) of households remain in the same income or consumption quintile across two time periods.

Table 1   Proportion of Households That Are Always, Sometimes, or Never Poor
percent

Country and period	Always poor	Sometimes poor	Never poor	Sometimes poor as % of ever poor[a]	Source
Income or consumption panel data					
Argentina 1995–2002	14.0	29.0	57.1	67	Cruces and Wodon 2003b[a]
Bangladesh 1987–2000	31.4	43.5	25.1	58	Sen 2003
Bangladesh 1994–2001	11.7	30.6	57.8	72	Kabeer 2004
Chile 1996–2001	10.1	21.1	68.8	68	Contreras et al. 2004
China (Sichuan) 1991–95	5.8	38.2	56.0	87	McCulloch and Calandrino 2003
Nicaragua	27.0	22.0	52.0	45	Davis and Stampini 2002
Egypt 1997–99	19.0	20.4	60.6	52	Haddad and Ahmed 2003
Ethiopia (urban)	21.5	36.2	42.2	63	Kedir and McKay 2005
Ethiopia 1994–97 (rural)	7.0	63.0	30.0	90	Bigsten et al. 2003
Ethiopia 1994–2004 (rural, 6 rounds)	6.0	79.0	15.0	93	Authors' calculations
Ethiopia 1994–2004 (rural, 2 rounds)	20.0	43.0	37.0	68	Authors' calculations
Ethiopia 1994–97 (urban)	13.0	46.0	41.0	78	Bigsten et al. 2003
Indonesia 1998–99	17.5	40.3	42.2	70	Suryahadi, Widyanti, and Sumarto 2003
Iran 1992–95	4.9	43.3	51.8	90	Salehi-Isfahani 2003
Kenya and Madagascar 1993–1995	70.8	22.5	6.8	24	Barrett et al. 2006
Poland 1993–96	5.9	31.6	62.5	84	Luttmer 2001
Russia 1994–98	3.4	41.9	54.7	92	Luttmer 2001
South Africa 1997–2001	35.9	58.3	5.8	62	Stevens 2003
South Africa KIDS	17.7	34.0	48.3	66	Carter and May 2001
South Africa KIDS	62.2	23.3	14.5	27	Cichello, Fields, and Liebbrandt 2005
South Africa KIDS	22.4	31.6	46.1	59	May et al. 2000

Uganda 1992–99	18.9	39.4	41.7	68	Lawson 2004
Uganda 1992–99	12.8	29.9	57.3	70	Lawson, Okidi and McKay 2003
Vietnam 1992–98	6.1	18.6	75.3	75	Baulch and Masset 2003
Nutrition and education panel data					
Vietnam 1992–98 (stunting)	35.9	28.8	35.3	45	Baulch and Masset 2003
Vietnam 1992–98 (malnutrition)	23.5	18.0	58.6	43	Baulch and Masset 2003
Vietnam 1992–98 (primary education)[c]	3.1	11.1	86.8	78	Baulch and Masset 2003
Vietnam 1992–98 (lower secondary education)[c]	29.0	31.0	40.0	52	Baulch and Masset 2003

Note: For papers that report both expenditure and income poverty statistics, this table reports expenditure only; for papers that report only one welfare aggregate, this table reports that aggregate. Similarly, for papers that report both equivilized and per capita welfare aggregates, this table reports the equivilized aggregate, but for papers that report only one of these statistics, this table reports that number.

a. The data from Cruces and Wodon 2003 are based on the mean across quarterly data.

b. Sometimes poor as a % of ever poor is the ratio of those poor in some waves only and those poor in any wave (including those always poor)

c. The data refer children of relevant age groups that are 'never enrolled', 'sometimes enrolled' and 'always enrolled'.

Table 2   Estimates of Poverty Movements in Transition Matrices
percent

	Percentage of all households			Percentage of households in bottom quintile			
Country	*Remain on diagonal*	*Move up by one quintile*	*Move up by more than one quintile*	*Remain on diagonal*	*Move up by one quintile*	*Move up by more than one quintile*	*Source*
Argentina[a]	46.5	19.9	9.6	45.8	25.3	28.8	Albornoz and Menéndez 2003
Chile	23.6	15.0	23.8	59.4	25.7	14.9	Contreras et al. 2004
Ethiopia	11.9	9.8	31.4	34.5	45.0	21.5	Block and Webb 2001
Indonesia[b]	39.6	19.4	10.3	49.7	26.5	23.8	Suryahadi, Widyanti, and Sumarto 2003
Iran (Tehran)	62.8	19.8	12.4	45.0	32.2	22.8	Salehi-Isfahani 2003 (Tehran)
South Africa	31.3	18.0	15.4	36.5	26.0	37.5	Woolard and Klasen 2005
South Africa (Free State)	20.0	19.9	14.4	32.4	29.4	38.2	Booysen 2003
South Africa KIDS	26.0	17.1	23.0	43.0	31.5	24.5	Keswell 2001
Vietnam	41.1	39.9	10.5	50.7	26.6	23.2	Glewwe and Nguyen 2002

Note: Based on income or consumption panel data.

a. Argentina is the 1991–95 quarterly mean.

b. Indonesia is August 1998–December 1998 comparison.

The overwhelming finding from these studies is that there is a considerable degree of poverty mobility. Many people move in and out of poverty. There are noticeable differences in country-by-country experience, but given that many of the surveys are not representative surveys of the country, interpreting these differences is difficult. Furthermore, there is a concern about measurement error. Poverty indicators, by documenting what happens to the lower end of the consumption or income distribution, do not explicitly distinguish measurement error but consider all data as an accurate representation of reality. If there is substantial (even if random) measurement error, then poverty mobility is likely to appear higher than it really is. Consumption and income data are always bound to be tainted by measurement error, but this error is also not likely to be the same across different settings. As a result, it is very difficult to learn from these patterns across different settings about moving out of poverty and falling into poverty, even when such movements are transitory. This issue is discussed further below.

Finally, some research has also expanded on nonmonetary measures of poverty, and a few researchers measure mobility in terms of education and nutrition (Table 1). Baulch and Masset (2003) present mobility estimates from Vietnam's LSMS panel using education and malnutrition, and find different transition matrices. They find that monetary poverty is less persistent than poverty by most other measures—an intuitive result given the irreversibility of education and the long-run effects of malnutrition. Consumption and income poverty data effectively measure flows, while education and stunting are stock variables that change more slowly than flow variables.

2.2 Why Do Some Households Escape Poverty?

The standard approach to investigating the correlates and causes of poverty tends to start with a descriptive profile of poverty dynamics. What are the characteristics of those who moved out of poverty relative to other groups? Subsequently, many studies use simple regression analysis with per capita real household consumption or its natural logarithm as an outcome and explanatory household and community variables. Most also estimate a logit or probit for the probability of escaping or falling into poverty over a time period, rarely addressing Ravallion's (1996) critique of some of the implied assumptions. A number of studies estimate multinomial logits with different outcomes for the time periods during which a household was poor. Alternatively, some have looked at hazard models (e.g., Baulch and McCulloch 2003) or some form of ordered logits and probits, although they all suffer from Ravallion's critique that they reduce a useful continuous variable into a discrete variable.

Studies of this nature are not easily interpreted. For example, although the contexts and data differ, a common finding is that education helps people escape poverty. But interpretation is very difficult. Is it education that makes people move out of poverty, or is it that families who manage to offer education to their children are also able to offer their children other opportunities—ones that may be unobservable to the researcher but that are important in climbing out of poverty? Or, to put it differently, in a long-term dynamic view of poverty changes, education may have to be seen as endogenous to long-term wealth and poverty changes as well. This illustrates a more fundamental problem in this literature. Most of the analysis of the determinants of changing poverty over time have used frameworks that effectively only give (bivariate or multivariate) correlates of poverty changes, and "causal" analysis is limited. Nevertheless, we discuss below

some of the findings in the literature, using data from Ethiopia as a specific example of moving beyond correlations to causal analysis.

Identifying the correlates linked to poverty mobility: An example

The standard approach starts with a profile of the characteristics linked to poverty mobility. An example is given in Table 3, based on an analysis of Ethiopian rural data (see Dercon 2002). This is within the context of a subsample of the Ethiopia Rural Household Survey, for which data were available for 1989, as well as the regular rounds subsequently. This was a period of decreasing poverty in these villages, with poverty falling from about 61 percent to 51 percent. There was also substantial mobility: 35 percent of households were poor in both periods, but 27 percent moved out of poverty and 16 percent moved into poverty. It was also a period of liberalization in food markets, so that large relative price changes occurred; depending on whether rural households were living in surplus or deficit areas, relative prices moved in opposite directions. These changes may also have had different effects depending on whether the household was a surplus farm household or not, as was subsequently confirmed in the analysis. For more on this see Dercon (2002, 2006).

Table 3   Household Characteristics by Poverty Transition in Ethiopia

Category	Characteristic	Always poor	Fell into poverty (nonpoor in 1989, poor in 1994)	Moved out of poverty (poor in 1989, nonpoor in 1994)	Always nonpoor	Overall mean
Livestock	Value livestock per adult in 1989 in Birr	155.32	550.92	344.72	828.89	418.60
Land	Land per adult (1989) (in hectares)	0.34	0.55	0.42	0.66	0.46
Export crops	Chat grown now	0.07	0.08	0.16	0.26	0.14
	Coffee grown now	0.35	0.15	0.02	0.05	0.17
Fertilizer	Fertilizer used in 1994	0.57	0.53	0.48	0.60	0.55
	Using more modern inputs in 1994 than in 1989	0.11	0.19	0.27	0.29	0.20
Demographics	Male adults in 1989 (above 15 years)	1.34	1.25	1.41	1.32	1.34
	Adult equivalent units in household 1989	5.56	4.65	5.42	4.29	5.08
	Male-headed household	0.83	0.83	0.88	0.81	0.84
Education	Head completed primary school	0.02	0.00	0.07	0.02	0.03
	Average years education of male adults (1994)	0.33	0.43	0.55	0.32	0.40
Location	Distance to nearest town by road (kilometers)	15.40	13.84	12.46	12.46	13.71
	All-weather road through village?	0.05	0.27	0.36	0.62	0.29
Prices	Percentage change in real producer prices for crops 1989–1994	19.86	28.27	37.70	23.26	26.69
Shocks	Any serious adult illness episodes between 1989 and 1994	0.71	0.70	0.51	0.55	0.62
	Number of adult illness episodes per adult in family	0.34	0.27	0.21	0.32	0.29
	Short-run rainfall experience (1994 minus 1989)[a]	-0.28	-0.20	-0.11	-0.08	-0.18
	Long-run rainfall experience (1994 minus 1989)[b]	-0.02	-0.02	0.06	0.02	0.01

Source: Authors' analysis of Ethiopia Rural Household Survey.

Note: n = 354. Values are in 1994 prices. Birr is the local currency. 6 Birr in 1994 was approximately 1 US dollar.

a. Difference in percentage deviation from mean in 1994 and 1989. Deviation relative to long-term mean for main season in area. Measure of how good the last main season preceding the 1994 survey was relative to the last mean season preceding the 1989 survey round.

b. Difference in percentage deviation from long-term mean in 1994 and 1989. Rainfall of last five years relative to long-term mean. Measure of how good the last five years were relative to the previous five years.

It is noticeable that the characteristic profiles of those moving in and out of poverty are different from those of the other groups, at least in terms of descriptive statistics. These differences are also statistically significant in many cases. For example, relative to those who stayed poor (the "always poor" category), those who moved out of poverty had significantly better endowments in terms of land and livestock in 1989. They also had significantly more educated heads of household, but the levels of education in 1989 were still surprisingly low. They also had better roads and better rainfall, and they benefited in terms of better producer prices for the crops they were selling. Relative to those who fell into poverty, they also had a somewhat better household male labor supply. In sum, these data illustrate a more general finding: those moving out of poverty tend to be able to rely on good endowments, in terms of assets such as land and livestock, human capital, and infrastructure. However, one should not forget that these data were collected in a period of changing incentives in the economy that rewarded those whose assets gave them productive potential: in other words, economic liberalization is likely to have increased returns to these endowments.

Multivariate analysis can improve on this type of analysis, since it can help to establish the quantitative contribution of each factor to the observed changes in poverty, as well as establishing the marginal contribution of each factor to potential changes in consumption or poverty. Table 4 shows this for the same data presented in Table 3, drawing on Dercon (2006), where details on the specification can be found. The left-hand side is the log of food consumption (total consumption data were not available for both periods, but in 1994 food still constitutes about 80 percent of total consumption). The broad patterns discussed before show up here as well: increases in food consumption are correlated with having more land to start with (and gaining some land—on this see Dercon 2006). The other significant factor is the change in producer prices faced by the household: the effect is positive for all, but larger for surplus farmers. Finally, good weather and access to nearby towns and good roads matter significantly as well. Table 4 uses food consumption as the dependent variable. Other studies (e.g., Lawson, McKay, and Okidi 2003) use poverty status as the dependent variable. While there are good methodological reasons for preferring to use consumption as the left-hand side variable (see Ravallion 1996), in this particular case the results are very similar.

Finding significant correlates and sizeable marginal effects is not quite the same as showing which factors matter most for the observed changes. For example, in Table 4, an increased return to land size was shown, but given the relatively small land holdings, the contribution of this increase is only small.Dercon (2006) simulated the contribution of different factors to the observed changes in consumption by poverty status, in particular for those moving out of poverty in this period. It was found that the combined effects of increases in relative producer price, returns to roads, and returns to location explained virtually the entire change in consumption for those moving out of poverty, with all the other factors only marginally affecting the overall change. To be clear, this only means that these factors were most relevant in this period, and not that this will always be the case.

Table 4 Linear Regression: Explaining Changes in Consumption in Ethiopia

	Coefficient	t-value
Constant	0.185	(1.43)
Ln (land in hectares in 1989 + 0.1)	0.211	(2.07)
Δ ln (land in hectares + 0.1)	0.239	(3.24)
Ln (adults in 1989)	-0.090	(-1.23)
Δ ln (adults)	0.287	(1.18)
Ln (years adult education +1)	0.016	(0.07)
Ln (number of adult serious illnesses +1)	-0.205	(-0.92)
Δ (% real price changes)	0.371	(3.67)
Δ (% real price changes) squared	0.642	(3.28)
Surplus farmer *Δ (% real price changes)	0.664	(3.50)
Δ ln (rain last season)	0.826	(4.12)
Ln (distance to town)[a]	-0.223	(-2.18)
Road infrastructure?[b]	0.205	(2.36)
Adjusted *R*-squared		0.09

Note: n = 354. Dependent variable: change in log food consumption between 1989 and 1994 (mean 0.3733). Ordinary least squares (OLS) regression with robust standard errors corrected for village cluster effects.
a. Road infrastructure is a dummy that is one if the the road linking the village to the nearest town is an road accessible in all seasons.
b. The distance variable is the distance in kilometers to the nearest town scaled relative to the mean distance in the sample.

Using the full sample, Dercon et al. (2006) have shown that the development of rural roads in the survey area of the Ethiopia Rural Household Survey has had considerable effects on consumption and poverty in later years as well, up to 2004. Indeed, from a policy point of view, it has been found to be a very robust and strong impact. In particular, the extensive work on the econometric specification and robustness of these results in this recent paper gives some confidence in the causal link between infrastructure and poverty in rural Ethiopia (see also, Dercon, 2004a).

Identifying the correlates linked to poverty mobility: Other evidence

Several other studies are reviewed below, but it is not possible to fully assess the statistical validity of the analysis in each case. In particular, most studies do not provide the detailed statistical analysis that would allow one to move from a descriptive set of correlates to a more causal analysis. Nevertheless, the results are interesting.

Table 5 gives some details of particular studies; the brief discussion cannot do full justice to them.[4] Education and/or the nature of jobs one has access to (which may at least correlate with education) appears to matter regularly, especially in the more urban settings. In rural settings in South Africa and Egypt, possession of livestock and land facilitates moving out of poverty. Location variables appear to matter as well. For example, in Ethiopia, residence in the capital

city is a factor in escaping poverty. It is hard to generalize from these different studies—in fact, the two studies using urban Ethiopian data are not entirely consistent in their conclusions. In any case, not *all* standard characteristics reflecting better assets and human capital show up as significant factors, but typically a subset do. In other words, it is too simple to state that all endowments matter equally, but endowments do matter nevertheless. Which endowments matter most is likely to depend on the context, including the overall economic and policy climate, since this will largely determine the opportunities available to poor people. The rural Ethiopia data reported before are striking in this respect: roads clearly became more important in allowing people to escape poverty in the 1990s than they were in the 1980s. This may be because economic liberalization in the 1990s created trading opportunities that could be accessed by road, while civil war in the 1980s meant that access to roads was not necessarily beneficial for the poor, as war restricted the movement of goods and people considerably.

Table 5   Estimated Determinants of Escaping or Falling into Poverty from Selected Studies

Source	Locality	Estimation method	Factors significant for escaping poverty	Factors significant for falling into poverty
Contreras et al. 2004	Chile	Logit for escaping poverty, logit for falling into poverty	Owning own home; household head working in formal sector; technical education	Young household head; household head had health problems
Fields et al. 2003b	Indonesia	OLS for change in per capita income	Region; household head getting a job, particularly in formal sector	
Haddad and Ahmed 2003	Egypt	Quantile regression (0.7) for staying poor and changing poverty status separately	Significant negative association with chronic poverty: value of livestock assets; owning more land; head schooling; work in manufacturing; community and recreation, and other nonfarm sectors; old head	Significant positive association with chronic poverty: large household
Kedir and McKay 2005	Ethiopia	OLS for change in consumption	Location in capital city; household head working as wage employee	
Bigsten et al. 2003	Urban Ethiopia	(1) Probit for falling into poverty; (2) probit for escaping poverty; (3) multinomial logit for always poor, sometimes poor, or never poor	Urban areas: education of household head and spouse; self-employed workers; living in regional capital	
Woolard and Klasen 2005	South Africa	OLS for change in expenditure, income, and predicted income, separately rural/urban	Owning more physical assets (including land, livestock, and other assets)	Large household; female-headed household; low initial employment access; poor initial asset endowment; low education
Herrera 1999	Lima, Peru,	OLS for change in expenditure	Head of household with better education; female-headed household	Household with more children

Risk and limited poverty mobility

One factor that has received relatively limited attention in most studies is the role of risk in causing poverty mobility. For example, in the list above, only Contreras et al. (2004) find some evidence for this, in particular that health problems are correlated with falling into poverty. The effect is also well documented in the Ethiopian rural data reported earlier, including in Table 3. Dercon and Krishnan (2000) show that the risk factor is an important reason for the poverty fluctuations observed in 1994–95 in these data. In most other studies, its omission is largely due to the fact that no data are available on shocks experienced by households, even though they turn up in many narratives of poverty mobility in more qualitative studies.

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However, the relevance of risk goes beyond causing short-term fluctuations in outcomes. There is increasing evidence that it is an important factor in the lack of upward mobility of some households—that is, risk keeps them poor. For a detailed discussion, see Dercon (2005). This process is relatively well documented for nutritional shocks that lead to stunting in young children. Alderman et al. (2006)document this for Zimbabwe; they found that young children of about 1–2 years were seriously affected by a drought episode, never quite recovered, and stayed stunted. This led not just to effects on body height but also to lower educational attainment and lower lifetime earnings.

In this respect, HIV/AIDS represents a slow but devastating shock, and Booysen (2003) reports that it deepens and lengthens poverty spells. He uses panel data from the Free State of South Africa to compare the mobility of AIDS-affected households to that of a nonaffected group. Affected households are more likely to fall into poverty and to remain in chronic poverty. Predictably, morbidity and mortality worsen the disease's socioeconomic effects. The study's comparison group includes neighbors of AIDS-affected households that in the baseline period did not have individuals knowingly infected with HIV or suffering from related diseases like tuberculosis or pneumonia that serve as markers for likely HIV infection. The paper's estimates may depend heavily on the significant differences in characteristics between the HIV/AIDS and comparison groups rather than on the effects of the disease itself. Nonetheless, in the absence of good instruments or experimental evidence, this gives reason to believe that AIDS depresses mobility.

More evidence on this can be found in Beegle, De Weerdt, and Dercon (2006a), linking adult mortality in a region in Tanzania to consumption growth over 10 years. This area is one of the most severely affected by HIV-AIDS in the Lake Region of Central Africa. The authors find that losing an adult in the household has a strong negative impact, even though the effect does not persist beyond about four to five years. They find this effect by exploiting variation across split-offs over time, thus accounting for any initial fixed effects stemming from the original household's characteristics and, to a considerable extent, controlling for the unobserved heterogeneity. In another paper, Beegle, De Weerdt, and Dercon (2005) find that adult mortality shocks have long-term implications for the children losing a parent, resulting in stunting and lower educational attainment at adulthood.

Several theoretical studies argue that a one-time negative shock to income may permanently decrease income, and Lokshin and Ravallion (2004) test this proposition using six- and four-observation panels from Hungary and Russia. They assume that different households adjust similarly but to different equilibria. The data come from the mid-1990s in two transition economies, settings in which many households have faced recent shocks and hence possibly moved away from their long-run equilibrium incomes. The 50 percent attrition of observations over a short four-to-six-year observation period should inspire concern as to the extent of a poverty trap that these data could capture. The researchers estimate that following loss of half a household's income, in both Hungary and Russia, household income returns to 80 percent of its original level within three years. In both countries, however, shocks have longer-lasting effects for poor people. In Hungary, a household at the 25th percentile of the distribution will only recover 75 percent of its original income within three years, while a household at the 75th

percentile will recover 90 percent of its original income within three years. While these results show the long effect of a shock, they do not suggest the existence of shock-induced poverty traps.

Dercon (2004a) finds even longer-lasting impact of shocks. This paper uses data from rural Ethiopia and finds that Ethiopia's 1984–85 famine had measurable effects on household consumption a decade later, in 1995. His main results come from fixed effects regressions, so that unobserved fixed heterogeneity in growth is accounted for, though he also estimates instrumental variables estimates resembling those of Jalan and Ravallion (2002). Two of the factors with clearest positive effect on consumption growth are rainfall and roads: having nearby roads increased consumption growth by 15 percent. Elbers, Gunning, and Kinsey (2002) build a model of the long-run effects of shocks and use generation-long panel data from a sample of resettled Zimbabwean households to monitor the persistence of shocks. They find that shocks changed income and capital accumulation and had surprisingly long-lasting effects.

An interesting dataset from rural Ethiopia, part direct measurement, part recall, confirms the importance of rainfall shocks but measures resistance to droughts as a function of herd size. Lybbert et al. (2004) use a 17-year panel of 55 pastoral households covering a time period with two major droughts. They suggest that the self-insurance of having a large herd of 45–75 cattle at the beginning of a drought helps smooth consumption and ensure a reasonable herd size after a drought, but that having fewer than 45 cattle may constitute a poverty trap wherein households cannot diversify income sources but remain vulnerable to shocks. McPeak and Barrett (2001) use the same data and compare with ongoing panel surveys of herders and farmers in Kenya. They emphasize the distressing frequency of income shocks in rural East Africa and note that increasing security in rangeland may improve mobility: at present much good land goes undeveloped because landowners fear threats from armed raiders.

Conclusions regarding the correlates of poverty mobility

In general, the intuitive notion that household and community endowments affect people's ability to escape poverty appears to be confirmed by most evidence. But it is clear that which endowments matter depends on the particular setting that is investigated. The general context, including the economic circumstances and opportunities, may well help to determine whether particular endowments matter. Furthermore, risk and shocks are a factor that hinders people from escaping poverty or even pushes them into poverty. The evidence on the assets and characteristics determining poverty mobility is nevertheless not always entirely convincing, since observing correlations is not the same as providing causal evidence. Much more work is clearly needed.

3. Some Problems with the Evidence

Beyond the caution regarding causal interpretation, there are further and possibly even more serious problems with most of the evidence. We focus on two issues. First, do we trust the data? Or, more precisely, does the fact that we know poverty is measured with error give us any reason to be concerned about inference regarding poverty mobility? Second, and most importantly, is attrition in panel datasets a serious issue for inference? This will lead us to a brief final section,

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largely focusing on evidence from one particular study that may provide a lesson for how to do this kind of work in the future.

How does measurement error affect estimates of mobility?

A first estimation problem arises when welfare aggregates inaccurately measure true income or consumption. Error can take four forms: surveys can elicit inaccurate or incomplete measures of consumption or income; price deflation over time and space can inaccurately reflect real prices; adult equivalency scales can inappropriately estimate per capita welfare; and survey cleaning can mismatch households in different survey waves (Baulch and Hoddinott 2000; Kamanou and Morduch 2002). Most literature focuses on the first type of error—survey responses that differ from a respondent's true income or consumption. In practice, the second and third types of error have a similar result: they increase the variance of welfare measures without increasing the variance of welfare, thereby erroneously inflating estimates of mobility. Causes of misreported consumption or income can include recall error, nonresponse, and inaccurate imputation of missing values (McKay and Lawson 2002). Misestimated consumption aggregates can arise from failure to address any of the issues outlined in Deaton and Zaidi (2002), particularly treatment of lumpy purchases like durables or bulk commodities—a point that Salehi-Isfahani (2003) emphasizes for grain purchases in rural Iran. Studies use two responses: quantifying measurement error and trying to eliminate it. In the rest of this subsection, we offer an (unavoidably more technical) discussion of some approaches in the literature.

Simulating measurement errors offers a direct way to approximate the effects of measurement error on mobility inference. Ligon and Schechter (2004) compare the bias and precision of various mobility indices in the presence of measurement error and find that some indices perform better than others. Chesher and Schluter (2002) derive methods to simulate the effect of measurement error on various poverty and inequality indices. They use a cross-section of data from Indonesia's 1993 National Socioeconomic Survey (SUSENAS) and show that larger errors generate increasingly imprecise poverty measures. Muller (2003) applies this technique to 1982–83 quarterly panel data from Rwanda to argue that only very large measurement error would affect his results.

Another simulation comes from comparing consumption, income, and income predicted from an earnings function. Woolard and Klasen (2005) report all results for consumption and income, since surveys tend to measure consumption more accurately than they measure income (Deaton 1997). These researchers find that the difference between the levels and change of income and consumption was greater in urban than in rural areas, but that estimates of inequality and mobility have similar magnitude for income and consumption. But it is difficult to discern the extent to which any differences in income and consumption measures reflect measurement error and the extent to which they reflect poor households' purposeful smoothing of consumption (Skoufias and Quisumbing 2005). Luttmer (2001) also compares income and consumption, as do Baulch and McCulloch (2000). Woolard and Klasen (2004), and Fields et al. (2003b) use predicted income in addition to reported income.

Others compare several welfare measures, an approach that links to multidimensional poverty literature. Scott (2001) compared transitory income of Chilean farmers to changes in the

household's harvest, livestock production, and wealth. Albornoz and Menéndez (2003) use income predicted by household assets with the idea that it may have less error than reported income would. Gong, Van Soest, and Villagomez (2004) use Mexico's urban employment rotating panel to measure mobility between formal employment, informal employment, and unemployment. They argue that Mexico has far higher mobility between these sectors than most industrial countries do. In two papers, Munshi and Rosenzweig (2005, 2006) argue that caste continues to limit mobility in India, especially through mechanisms of school, marriage, and informal within-caste lending.[5]

Proxy reports may be another source of measurement error. Rosenzweig (2003) reports a correlation of 0.86 between self-reported and family-reported levels of schooling, a level near the 0.9 correlation for twins living apart in the United States (Behrman, Rosenzweig, and Taubman 1994). But for landholding, he finds correlation between self- and family reports of only 0.4. Although he offers no corroboration of income data, the individual interviewed in surveys may cause additional error. As solutions for measurement error, dropping outliers from earnings functions, as do Woolard and Klaasen (2005), may exclude the most egregious measurement errors but may also bias the remaining sample. Muller (2003), following Jalan and Ravallion (2002), also uses quantile regressions, partly to avoid sensitivity to income outliers.

Kamanou and Morduch (2002) emphasize that per capita consumption discontinuously changes when a family has a new baby. They estimate that up to a fourth of the variance in per capita consumption arises from changes in household size. Baulch and Masset (2003) show that equivalized and per capita expenditure give different pictures of mobility for Vietnam. Fields et al. (2003b) show that changes in log-income rather than household size account for at least half of the total income change they measure for 84 percent of households in Indonesia, 73 percent in South Africa, and 88 percent in Venezuela.

Instrumental variables offer a more common solution. If the instrument predicts true consumption but error in measuring the instrument is uncorrelated with error in measuring consumption, then the instrument can give reliable inference on mobility. Studies use rainfall, nutrition or weight, education, household size, assets, once- or twice-lagged income and their squares, dwelling materials, and subjective living standards ("How would you rate the general material situation of your household?") as instruments for current consumption or income (Luttmer 2001; Glewwe and Nguyen 2002; Fields et al. 2003b; Dercon 2004a ; Lokshin and Ravallion 2004; Antman and McKenzie 2005; Newhouse 2005). Glewwe and Nguyen (2002) estimate that half of mobility estimated by simple means is simply measurement error. Luttmer (2001) finds that measurement error may account for 30–60 percent of cross-sectional variance in consumption and 55–80 percent of expenditure shocks. But these instruments may weakly correlate with consumption, and more importantly, errors in measuring the instruments may correlate with errors in measuring consumption.

Antman and McKenzie (2005) again use pseudo-panel data to argue that OLS with a panel substantially overstates mobility. They use 58 quarters of data from 1987 to 2001 in Mexico's National Survey of Urban Employment (ENEU) and construct cohorts according to the birth year and education level of the household head. The OLS slope coefficient of income on its lagged value using panel data is 0.668 or 0.598 including cohort dummies. But their pseudo-panel

estimates of this slope coefficient are 0.988 or 0.832 including individual effects. Over five years, their pseudo-panel indicates a coefficient of 0.950. While these results may vary with different cohort definitions, any of these estimates implies a considerable overstatement of mobility.

For our purposes one conclusion should be clear: analysis ignoring these issues is likely to overstate poverty mobility, especially when very few time periods are used in the analysis. While the fact that any study observing substantial poverty mobility in the data finds significant correlates with different household and community characteristics suggests that a significant part of the observed mobility is genuine, the presence of substantial measurement error cannot be discounted.

How does attrition affect estimates of mobility?

The disappearance of households between baseline and follow-up data—attrition—may bias estimates of mobility and its determinants. Thomas, Frankenberg, and Smith (2001) label this problem the Achilles heel of panel surveys. Purely random attrition would have no effect other than decreasing precision, since random attrition would merely shrink the survey's sample size. But households disappear due to marriage, migration, violence, business success or failure, or political instability, and these causes that lead households to disappear from the data may also affect households' income mobility. Hence, attrition may cause analysis to underestimate upward or downward mobility.

Respondents' selective refusal to participate in surveys accounts for much attrition in industrial countries but less in developing countries, where the inability to track households plays a more important role (Jasso, Rosenzweig, and Smith 2000; Thomas, Frankenberg, and Smith 2001). One can easily understand why: telephones, Internet, land titling, and other fixed contact methods work far better in industrial than in developing countries. Furthermore, in more agrarian societies, the gradual movement out of agriculture of large parts of the population is an essential part of development.

Evidence from a few panels shows the varied importance of attrition due to household movement in developing-country datasets. Nearly 60 percent of attrition in Nicaragua's three-year LSMS was due to households leaving their dwellings (Davis and Stampini 2002). In Indonesia's Family Life Survey, which made extensive effort to track households, 82 percent of 1993–97 attrition was due to the inability to find households. IFLS in 1997 asked respondents to list a phone number of a neighbor, friend, family member, employer, or other individual who could contact them, and only 20 percent could provide such information (Thomas, Frankenberg, and Smith 2001). In a small Lesotho survey with a first wave that was not intended as the baseline for a panel, 83 percent of attrition was due to households that could not be tracked (Wason and Hall 2004). Elbers, Gunning, and Kinsey (2002) use a Zimbabwean panel with only 10 percent attrition between 1983 and 1997, but they define a household as the resident of a particular piece of land, so their panel ignores migration and considers only complete disintegration or eviction of a household as causes of attrition. While this approach achieves low attrition, it does not guarantee that a household observed in one wave of a panel is the same household that was observed in a different wave of the panel.

Table 6 presents attrition rates, follow-up methods, and analysis of attrition for all datasets from Table 9 for which attrition information was easily available.[6] Attrition rates range widely, from 0 percent in the Bangladesh Nutrition Survey (BNS) to nearly 50 percent in a survey in Hungary.Selection of survey method is heavily endogenous with investment in the survey, duration of the survey, location of the survey, and other factors, so the numbers in Table 6 do not accurately measure a causal effect of survey method. Nonetheless, the difference in mean attrition rates is striking: surveys of dwellings achieve a mean attrition rate of 33 percent, while surveys including tracking of households outside their dwellings but only in their local communities achieve a mean rate of 14 percent, and surveys with extensive tracking achieve a median rate of 5.6 percent.

Table 6   Attrition Rates for Different Follow-up Methods for Selected Panel Data Surveys

Dwellings		*Local tracking*		*Extensive tracking*	
Survey	*Rate*	*Survey*	*Rate*	*Survey*	*Rate*
Bolivia PIDI	35.0	Egypt EIHS	7.2	Bangladesh BNS	0
Mexico ENEU	35.0	India REDS	17.5	Indonesia IFLS	5.6
Nicaragua LSMS	31.1	Philippines IFPRI	21.4	South Africa KIDS	16.0
Peru LSMS	15.0	Vietnam VLSS	11.0		
Venezuela EHM	50.0	Ethiopia ERHS	7.0		
Mean, median	33.0, 35.0	Mean, median	14.0, 14.0	Mean, median	7.2, 5.6

Source: See surveys in the annex, Table 10. Attrition rate is the number of households that disappear between baseline and last wave divided by number in baseline. Table includes only surveys for which information could be obtained on both attrition rate and follow-up method. Note that these are household-level attrition rates.

Table 7 summarizes the evidence on the nature of attrition in those studies for which more information was available. A review of the tracking rules for each survey shows large variability in the methods used. Similarly, the way in which attrition is discussed and analyzed also varies. From these studies it appears that, generally, households more likely to disappear have younger heads, and live in urban areas, and have higher per capita income and more unmarried individuals. These results cohere with intuition—the households most likely to leave their dwellings are the households with characteristics that naturally associate with spatial mobility. Most studies recognize these differences but comment that the magnitude is sufficiently small as to weakly affect estimates of poverty or related income mobility.

Table 7 Attrition in Selected Panel Surveys

Country and data	Attrition rate (%)	Rules for tracking households	Description and analysis	References
Argentina EPH	1995–2002: 48	Drop a household if any member refuses to answer income questions.	Graph comparing complete and panel sample at baseline: lower income in sample, different poverty rates.	Cruces and Wodon 2003b
Bangladesh BNS	0	For people who moved, village residents who cohabited with departed members were questioned; for people who remained in the village, the original interviewee was also interviewed.	Compare means, analysis of households that would have attrited under different sampling rules.	Rosenzweig 2003
Bangladesh HIES	No information	From village census, baseline sampled 1,245 households in 62 villages, stratified into 8 landowning groups. The follow-up in 32 villages stratified by wealth (rather than land), then followed up original households. So of the 965 households interviewed in follow-up, 695 were interviewed in baseline.	No information	Hossain et al. 2002
Bolivia PIDI	35	Interviewers did not track and follow migrants.	Compare means, probit for attrition. Statistical tests.	Alderman et al. 2001
Chile CASEN	11.8	No information	No information	Contreras et al. 2004
China (rural Sichuan) RHS	No information	No information	They simulate bias if households with different identifying information in the baseline and follow-up had been dropped.	McCulloch and Calandrino 2003
Côte d'Ivoire	5–15	No information	Regression showing smaller and younger households attrit.	Kamanou and Morduch 2002

Country and data	Attrition rate (%)	Rules for tracking households	Description and analysis	References
CILSS				
Egypt EIHS	7.2	Interviewed household if it remained in the same primary sampling unit. So loss of households was due to households moving, being absent at time of visit, or refusing interview.	Compare means and determinants of consumption in 1997 for entire sample and panel sample. Rural attritors were poorer, urban attritors were wealthier; neither difference significant.	Haddad and Ahmed 2003
Ethiopia ERHS	1989–94: 7 1994–97: 2	Identify household head or household that considers itself as the successor household if head has died. Restricted to village.	No specific analysis reported.	Dercon and Krishnan 2002
Ethiopia EUHS	Kedir and McKay: 30.3 Bigsten et al.: 7	Identify household head or household that considers itself as the successor household if head has died. Restricted to particular urban area.	No information	Bigsten et al. 2003; Kedir and McKay 2005
Hungary HHP	Each round: 15 1992–97: 48	No information	Estimate income equations simultaneously with equation for attrition. Smaller households and households outside Budapest less likely to attrit. They provide estimates corrected and uncorrected for attrition.	Lokshin and Ravallion 2004
India NCAER	No information	Kept if (1) household head in 1970/71 remained alive in 1981/82 and household was still intact; or (2) head remained alive but household members had not stayed together; or (3) household head had died but rest of household stayed intact. They do not use weights, so estimates reflect the sample and not India's rural population.	None	Bhide and Mehta 2004
India REDS	17.5	Lost information on children who	Use *jati* fixed effects in estimation.	Munshi and

Country and data	Attrition rate (%)	Rules for tracking households	Description and analysis	References
		moved out of Dadar or who dropped out.		Rosenzweig 2005
Indonesia IFLS	1993–97: 5.6	Follow households that change dwellings. Median interview time: 6 hours per interview on 17 islands in 20 languages. Main visit: interview households in same dwelling and within generally 30 minutes by public transportation (local move).	Movers were more similar to nonmovers at baseline than at follow-up. Lowest reinterview in capital city.	Thomas, Frankenberg and Smith 2001
Iran	33.8	No information	Compare means in baseline.	Salehi-Isfahani 2003
Kenya and Madagascar USAID	No information	No information	Probit for attrition; variables jointly insignificant	Barrett, Bezuneh, and Aboud 2001
Kenya (Nyanza province) KICS	28 women, 33 men	Common reasons for attrition: mortality, migration, not found after three visits, busy, or sick.	Compare means, probit for attrition, BGLW test.	Alderman et al. 2001
Lesotho	34.4	No information	Probit for attrition. Larger households with more pigs more likely to remain.	Wason and Hall 2004
Madagascar 1–2–3	1997–98: 22 1998–99: 21	No information	Compare means. Attritors younger, better educated, smaller households, less often own their dwellings. But no difference in poverty status.	Herrera and Roubaud 2005
Mali	7.9	No information	Compare means. No significant difference at baseline.	Christiaensen and Boisvert 2000
Mexico ENEU	35	Follow dwellings only; if household moves it is dropped.	Extensive analysis and compare panels with pseudo-panels.	Antman and McKenzie 2005; Gong, Van Soest, and Villagomez 2004; Wodon 2001
Nicaragua	31.1	Visit same dwelling; if household has	Compare means, probit for attrition,	Davis and Stampini

Country and data	Attrition rate (%)	Rules for tracking households	Description and analysis	References
LSMS		moved, do not follow them.	compare regression coefficients, Heckman regression for regressing consumption and attrition on covariates. Attrition not significant problem.	2002
Peru	1997–98: 12 1998–99: 3	No information	Compare means. Attritors younger, better educated, smaller households, less often own their dwellings, early 17% more than non-attritors. But no difference in poverty status.	Herrera and Aboud 2005
Philippines IFPRI	1962 baseline 1985–94: 21.4	Follow up households in the same village.	No information	2007
Poland HBS	No information	No information	Wealthier and urban households disappear.	Luttmer 2001;
Russia RLMS	No mean information	Follow dwellings only.	No information	Heeringa 1997
South Africa Free State	7.6	No information	No information	Booysen 2003
South Africa KIDS	15	Interviewers tracked, followed, and re-interviewed migrants and mobile households.	Compare means, probit for attrition.	Alderman et al. 2001; May et al. 2000
Uganda IHS and UNHS	28	No information	Compare means, probit for attrition, compare baseline results with and without attrited households. Insignificant differences between panel and nonpanel, though rural and larger households more likely to remain.	Lawson, Okidi and McKay 2003; Lawson 2004; Deininger and Okidi 2003
Venezuela EHM	50	Keep only households in same dwelling.	Households that remain have larger size and smaller real income per	Freije 2003

Country and data	Attrition rate (%)	Rules for tracking households	Description and analysis	References
			capita, fewer unmarried individuals and couples without children.	
Vietnam VLSS	28 individuals, 11 households	Households in the same commune were followed, but households that moved out of the commune were not followed.	Newly formed households underreported; more urban than rural attrition. Also both surveys have same sample frame (Vietnam 1989 census), but rural-to-urban migration since then means the later data less represent the country. They use weights in the first round but not the second round.	Baulch and Masset 2003; Glewwe and Nguyen 2002
Zimbabwe	1982–97: 10	They follow land and define a household as the residents of a piece of land.	10% dropped due to surveying errors, household disintegration, or government eviction.	Elbers, Gunning, and Kinsey 2002

Note: Attrition rate = number of households lost / total baseline households. It is unweighted, so it is for the sample, not the population.

For example, Alderman et al. (2001) conducted a careful analysis on this issue for Bolivia's Integrated Child Development Project (PIDI) and Kenya's Ideational Change Survey (KICS), where interviewers did not follow migrants, and for South Africa's KIDS survey, where interviewers did follow migrants. They show that in all three countries, household characteristics in the baseline could not effectively predict attrition, and attrition did not affect baseline regression estimates of the particular relationships of interest to them, for example between family background and education or health outcomes. In other words, relative to baseline characteristics, there are no obvious patterns to the attrition. These results cohere with analysis of attrition in the U.S. Panel Study of Income Dynamics (Fitzgerald, Gottschalk, and Moffit1998).

This is a difficult issue and this type of analysis cannot entirely settle it. Such studies can only rely on baseline data on the households that have disappeared from the survey to assess whether these households are "different" or not. For example, it could be that the entrepreneurial people left the community, so that inference on moving out of poverty and its determinants may be biased even though there are no observable differences. In a related vein, it could be that negative shocks, such as illness or crop losses experienced after the baseline round, forced some people to leave (or contributed to their death). Baseline characteristics would not be able to reveal this, and the underlying causes of mobility would not be identified from the data. In sum, the evidence that attrition may not matter much is potentially seriously flawed.

A number of studies have taken the attrition issue more seriously. By trying to trace more people than what would be implied by a typical simple revisit rule, they also provide evidence on the relevance of attrition for analysis in general, and for inference on poverty and mobility. These studies have shown that attrition, particularly in the case of dwelling-based follow-up, will significantly bias results. Indonesia's Family Life Survey achieved only 5.6 percent attrition after four years by a detailed protocol that Thomas, Frankenberg, and Smith (2001) carefully document. Follow-up in the IFLS second round, with interviews in 20 languages on 17 islands requiring an average of six hours each, presented some challenge. One entire enumeration area in Jakarta had been bulldozed and replaced with a shopping and apartment complex, and by extensive tracking the researchers followed 18 of the 20 households originally from that enumeration area. While households that moved locally were generally similar to households that did not move, households that moved far away differed greatly from households that did not move in a variety of characteristics, including simple welfare outcomes. In mere numbers, these authors report that dwelling-based sampling would have given a follow-up rate of 77–84 percent rather than the 94 percent that they achieved.[7]

Rosenzweig (2003), using an 18-year Bangladeshi panel with a 0 percent attrition rate, argues against the dwelling-based follow-up that the LSMS program suggests (Glewwe and Jacoby 2000) or the intact household (or same head) approach that India's ICRISAT and the REDS surveys used. To achieve this zero attrition, previous co-residents of those not present in the village were interviewed to collect some basic characteristics of those not present. Rosenzweig reports that using the LSMS criteria would have dropped more than half of males less than age 30 from his data. The impact on assessing mobility over time is shown to be substantial.

The way forward: Long-term panel data with full tracking of individuals

Few if any studies take this issue of attrition a logical step further. When studying mobility, one needs to study welfare outcomes over long periods. But households change over time: children are born, adults leave and set up new households, couples get married, people die. Households split; people move out or move in. To put it simply, there is no such thing as a stable household unit that can be followed over long periods of time. The unit that is followed is based on some judgment that may affect the results. For example, ICRISAT used to follow a household as long as it had the same head, and later, it followed the person who held the land asset of the previous head. In both cases these are choices that are likely to affect the results of the analysis.

While the aforementioned work, linking poverty changes to household and other characteristics and circumstances, makes substantial progress, the results are bound to be problematic unless one thinks carefully about attrition. Poverty mobility is the evolution of the poverty outcomes of individuals and households that try actively to shape their own futures, given what they have, in response to their own circumstances and the general environment they face, including the policy environment. Specifically, households split, move out of communities, and migrate as part of their poverty mobility strategies. Even more regular life-cycle events, such as marriage, birth, and death, are shaped and influenced by the process of trying to move up or failing to do so. In short, attrition is bound to be a crucial and endogenous part of mobility work, even if work that only looks at the impact of attrition using baseline observable characteristics may not find that it is a problem. If panel surveys only look for "original" households in original dwellings or even in the original community, then one crucial part of poverty mobility, spatial mobility—moving elsewhere to try to do better, in response to opportunities or pressures of destitution—is totally ignored. In fact, if development economics theory has anything to say about how poverty reduction takes place, it is that fewer people remain involved in agriculture as jobs are created in other sectors. Thus spatial mobility is likely to be central.

When taking seriously the various split-offs and changes to the household in panel data, one reaches another methodological conclusion as well: following a household does not make sense, since a household changes rapidly over time. Defining what is the "same" household is highly complicated because individuals move in and out. The only feasible solution is to base the sampling and tracking strategy on *individuals*. This is what a number of recent and current studies aim to do. The Bangladesh study reported by Rosenzweig (2003) and discussed above started from a small sample of 50 households initially, and tracked all split-off households to result in a sample of 308 households.

In Tanzania, the Kagera Health and Development Study (KHDS) does this systematically on a relatively large scale compared to other current studies (see, for example, Beegle, De Weerdt, and Dercon 2006b). The KHDS began in 1991 and conducted a detailed panel data survey in four rounds between 1991 and 1994. It was implemented in Kagera, a region near Lake Victoria with high HIV/AIDS prevalence. It initially surveyed 900 households. In 2004 a new survey round was launched, aiming to collect data to study the long-term implications of adult mortality and related impacts from HIV/AIDS. It was clear from the start that much had happened to change these initial households, and a decision was made to focus on all *individuals* ever surveyed in the earlier rounds.[8] This led to an effort to trace more than 6,000 people.

At the level of original households, the tracing was successful: 93 percent of original households were reinterviewed, comparable to the best "short-term" panel data survey and highly successful given the 10-year period since the previous interviews. The result was a survey of 2,774 households, linked to 832 recontacted households. At the individual level, about 87 percent of respondents were reinterviewed as part of the survey, but the implications of spatial mobility are very striking. Only 49 percent of the individuals interviewed in 1991–94 had stayed in the same village. Nineteen percent moved to a nearby village, and 20 percent moved further away to another village or town in Kagera region; 10 percent moved outside the region, but still within Tanzania, and 2 percent moved to other countries, including Uganda and Rwanda.[9] In sum, without tracking there would be no way to assess mobility in any spatial sense, and if tracking had been done using "dwelling" or "same village" rules more than half the individuals would have been lost.

Beegle, De Weerdt, and Dercon (2006b), using some simple descriptive statistics, contend that inference about poverty and poverty mobility would be very misleading without this tracking. Table 8 shows that poverty decreased in the KHDS sample, but to a very different extent across the different locations to which people moved. Those who remained in the village saw the poverty head count fall from 36 to 32 percent, a decline of 4 percentage points. But those who moved to a nearby village had an 11 percentage point decline, those moving elsewhere in Kagera experienced a 13 percentage point decline, and those moving outside Kagera saw poverty decline by 23 percentage points. If sampling had occurred using a "same location" rule, then poverty mobility would have been considered to be substantially lower than what actually took place. Surely, this does not look like "random attrition" according to standard rules. In fact, Beegle and colleagues found that initial observable variables are not at the root of these differences in poverty mobility; if anything, unobservable characteristics drive these differences, as when more able or entrepreneurial individuals move farther away.

Table 8 Poverty and Spatial Mobility in Kagera, Tanzania
Comparing the Poverty Head Count between 1991 and 2004 by initial locality

2004 location	Mean 1991	Mean 2004	Difference means	N
Within village	0.36	0.32	-0.04***	2,611
Nearby village	0.33	0.22	-0.11***	566
Elsewhere in Kagera	0.37	0.24	-0.13***	571
Out of Kagera	0.30	0.07	-0.23***	327
Full sample	0.35	0.27	-0.08***	4,075

Source: Beegle, De Weerdt, and Dercon 2006b.
***=significant at 1 percent

Of course, this type of work imposes considerable costs. One alternative is to fill in the "missing" individuals and households by using key-informant interviews. This is effectively what Rosenzweig (2003) did in some survey work. For each individual not present in the village, a simple key-informant interview was conducted, trying to glean a few indicators. But the indicators have to remain limited to a few basic assets such as land or education. Such

information can help assess the potential bias entailed by attrition—but again on the basis of indirect observable characteristics related to welfare outcomes.

In the "new" ICRISAT surveys, which involve tracking all the individuals ever interviewed in 1975–84, a similar instrument is used, but it is pursued further. Key informants and family are asked not just about assets but also about their perceptions of the welfare situation of the missing individuals, using a seven-step scale from very rich to very poor. But there is still the problem of discrepancies between community or key-informant perceptions of other people's well-being and those people's own self-assessment. In other words, while both perceptions are information about the person, it would be wrong to simply assume both are measures of the *same*, particularly since measurement error may have different properties for self-reported data than for reports on a family member.

Another alternative is to turn the idea of a panel data survey upside down. Rather than starting with a baseline, one could start with interviews for a full sample drawn from a current population and then use recall to establish a baseline for these people. This is effectively what was done by Krishna (2004) and to some extent in the herd histories in Ethiopia. While such studies provide interesting insights and information, as argued earlier, there are caveats in using these methods to assess poverty mobility. For example, in the Ethiopia ERHS, subjective welfare perceptions were elicited in different rounds over a 10-year period. In 2004, the same questions were asked using 10-year recall. The answers were revealing: for example, when asked in 2004, 29 percent said that they had been rich or very rich in 1994. In 1994, only 7 percent of respondents provided this answer.

In short, while recall provides interesting information for assessing welfare outcomes and poverty mobility, one must be careful. Recall is unlikely to give the same information as one would have obtained if the questions had been asked in the past, and this in turn affects inference on poverty mobility. Furthermore, cognitive dissonance may lead individuals to remember events and circumstances that cohere with their life situation but forget or misreport those past events that conflict with their current circumstances. Finally, retrospective studies cause further selectivity bias since only the surviving people that stayed in a particular location are being interviewed. As an indication of the nature of change in terms of poverty in a particular locality, this is particularly fraught. Finding good alternatives to tracing all individuals is clearly a challenge.[10]

4. Conclusions and Directions for Future Research

In the last few years, many more panel datasets have become available from developing countries. A number have been used for the analysis of poverty mobility and its correlates. Most research has found that household and community endowments, such as assets and infrastructure, matter for allowing people to move out of poverty, while shocks and risk make and keep people poor. Nevertheless, it is difficult to generalize on which factors matter most in different contexts. Furthermore, many studies are not able to provide evidence that goes beyond correlates; rarely if ever has any causality been established convincingly. Better statistical analysis, combined with a careful contextual understanding of the economic and other circumstances constraining poverty mobility, will be essential to move this research forward.

Heavy attrition appears to be present in most panel databases. Following dwellings rather than tracking households worsens attrition. Investing in tracking households substantially lessens at least attrition rates. Good research practice should involve reporting the rate of attrition, comparing means and determinants of attrition, and discussing reasons for attrition.

But just following households is not enough. With longer panel datasets, the future for understanding long-term mobility must lie in methodological innovations. These include an increasing focus on individual mobility and not just household mobility. It is clear that there are no simple shortcuts for this work, whether in quantitative or qualitative studies: some of the people of interest will inevitably move, and this will impose costs in terms of data collection and other logistical requirements. Simple recall and key-informant interviews are unlikely to solve these issues.

Annex.   Data Sources for Poverty Mobility Work

The datasets used in recent studies (Table 9 and 10) constitute distinguishable categories of surveys. Several LSMS-type household surveys, such as those in Peru and Vietnam, included a multiyear panel designed into the survey. Quarterly or annual rotating panels in Argentina, Mexico, and Venezuela allow study of short-term income dynamics or analysis of attrition and measurement bias. Several data sources designed for evaluation of a specific development intervention—the Bangladesh Household Income and Expenditure Survey (HIES), the Integrated Child Development Project in Bolivia, Progresa/Oportunidades in Mexico, and others—provide insight into mobility as a useful side benefit of impact evaluation.

Only five of the 44 are rotating panels, that is, repeated cross-sections where some portion of the respondents remain in the survey for a set period. Mexico's ENEU, for example, surveys a household for five quarters before replacing it. Some panels came from following up a baseline survey that was not originally designed to be part of a panel. Lesotho's 1993 survey, for example, was designed as a rapid assessment of the effects of a drought affecting much of Sub-Saharan Africa, and the original survey did not anticipate a follow-up in 2002 (Wason and Hall 2004). The Ethiopia Rural Household Survey, with six rounds since 1994, has at its core a smaller sample of households interviewed by the International Food Policy Research Institute in 1989 on the effects of the 1984–85 famine. Still other databases, such as the 55-household herd history in Ethiopia, were completed for specific individual papers, largely based on recall data. Only one data source—Kenya's Welfare Monitoring Survey (WMS)—had no panel of households and involved repeated cross-sections used to form a panel of communities.

Most surveys used in research last only a few years, but some panels have longer duration. Of the 44 panel data sources categorized here, nine (20 percent) span six to 10 years and another 10 (23 percent) last more than 10 years. A few long panels cluster: Bangladesh has three panel data sources that span more than five years, while India and Ethiopia each have two (and each has one with a subsample spanning an even longer period). Many of these surveys will have additional waves. At the same time, the panels with longer duration often have samples of fewer than 500 households, and attrition may affect longer panels more severely. Also, despite the increasing presence of long panels, two-thirds of panels still cover a time span of five or fewer years.

Table 9 Duration of Panel Surveys Used in Recent Research

Panel duration	Surveys
1 to 5 years	Argentina EPH, Bolivia PIDI, Chile CASEN, China RHS, Côte d'Ivoire CILSS, Côte d'Ivoire FMHS, Egypt EIHS, Hungary, Indonesia UNICEF/BPS, Indonesia IFLS, Indonesia 100 village, Iran, Kenya WMS, Kenya/Madagascar USAID, Kenya KICS, Madagascar 1–2–3, Mali, Mexico ENEU, Mexico Progresa, Nicaragua LSMS, Peru ENAHO, Poland, Rwanda, Russia, South Africa Gauteng, South Africa Free State, Venezuela
5 to 10 years	Bangladesh BNS, Bangladesh Greeley, Bangladesh HIES, Ethiopia ERHS (full), Ethiopia EUHS, India ICRISAT (old), Kenya TAMPA, Uganda, Vietnam
More than 10 years	Ethiopia ERHS (subsample), Ethiopia herd histories, India ICRISAT (new), India NCAER, India REDS, Tanzania KHDS, Lesotho, Philippines, South Africa KIDS, Zimbabwe

Note: Duration defined according to waves used in articles profiled in this review. The rotating panels in Argentina EPH and Mexico ENEU offer many overlapping one-year panels.

Table 10   Datasets Used in Recent Research

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
Argentina (Greater Buenos Aires)	Permanent Household Survey (EPH)	Rotating panel	May and October 1990–2002	12 cohorts, average 453 households	All urban Argentina	Fields et al. 2006; Wodon 2001; Garrido and Marina 2002; Cruces and Wodon 2003a; Albornoz and Menéndez 2003
Bangladesh	Greeley survey	Panel	1994, 2001	1,184 households	Comilla and Tangail districts	Kabeer 2004
Bangladesh	Bangladesh Nutrition Survey (BNS)	Panel	1981/82, 2000	50 households initially, 308 households in follow-up	15 villages	Rosenzweig 2003
Bangladesh	Household Income and Expenditure Survey (HIES), also known as BIDS panel	Panel	1987/88, 2000	379 households	21 villages	Sen 2003
Bolivia	Integrated Child Development Project (PIDI) evaluation data	Panel	1995/96, 1998	2,047	Poor urban areas	Alderman et al. 2001
Chile	National Socioeconomic Characterization (CASEN)	Panel	1996, 2001	4,700 households	National	Contreras et al. 2004
China	Rural Household Survey (RHS)	Panel	1991, 1995	3,311 households	Rural Sichuan	McCulloch 2003
Côte d'Ivoire	Côte d'Ivoire Living Standards Survey (CILSS)	Rotating panel	1985, 1986, 1987, 1988	800 households	National	Kamanou and Morduch 2002
Côte d'Ivoire	Farm Management and	Panel	1993, 1994,	120 rice-farming	Three humid to	Barrett, Bezuneh, and

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
	Household Survey (FMHS)		1995	households	subhumid agro-ecological zones of Côte d'Ivoire	Aboud 2001
Egypt	Egypt Integrated Household Survey (EIHS)	Panel	1997, 1999	347 households	National	Haddad and Ahmed 2003
Ethiopia (Borana Plateau)	Herd histories	Panel	1980, 1997	55 households	Selected pastoralists from southern Ethiopia	McPeak and Barrett 2001; Lybbert et al. 2004
Ethiopia	Ethiopia Rural Household Survey (ERHS)	Panel	1994 (2), 1995, 1997, 1999, 2004; also 1989	1,477 households (354 households since 1989)	Rural areas	Dercon and Krishnan 2000, 2002; Bigsten et al. 2003; Dercon and Krishnan 2002; Dercon 2004a; Dercon et al. 2004
Ethiopia	Ethiopia Urban Household Survey (EUHS)	Panel	1994, 1995, 1997, 1999, 2004	1,500 households (urban)	Urban areas	Bigsten et al. 2003; Kedir and McKay 2005
Hungary	Hungarian Household Panel (HHP)	Panel	6 waves, 1992–97	1,385 in panel from 2,668 in original	National	Lokshin and Ravallion 2004
India	Rural Economic Development Survey (REDS); National Council of Applied Economic Research (NCAER) panel	Panel	1970/71, 1981/82, 1999	3,319–4,979 households in 2,509 villages	Areas within Mumbai	Bhide and Mehta 2004; Munshi and Rosenzweig 2005
India	ICRISAT (Maharashtra, Andhra Pradesh, Madhya Pradesh, Gujarat)	Panel	1975–84	400 households; much research on smaller dataset	Rural south India	Gaiha and Imai 2002; 50+ research papers, main papers summarized in

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
						Morduch 2004
India	ICRISAT "new" (Maharashtra, Andhra Pradesh)	Panel	2001–6	All households still residing in villages from "old" ICRISAT survey, plus from 2004 tracking of all split-offs including migrants	Rural south India	In progress
Indonesia (rural)	100-village survey by UNICEF and Statistics Indonesia (BPS)	Panel	1998, 1999 (4 interviews)	10,640 households	Villages from 8 provinces	Suryahadi, Widyanti, and Sumarto 2003
Indonesia	Indonesia Family Life Survey (IFLS)	Panel	1993, 1997, 2000 (and part of sample, 1999)	2,132 households	Rural areas from 13 provinces	Fields et al 2003a, 2003b; Strauss et al. 2004; Newhouse 2005; see also IFLS Web site
Iran	Data from Statistical Center of Iran	Panel	1992, 1993, 1994, 1995	3,371 households	National	Salehi-Isfahani 2003
Kenya	Tegemeo Agricultural Monitoring and Policy Analysis Project (TAMPA)	Panel	1997, 2000, 2004	1,500 households	Rural areas	Muyanga, Ayieko, and Gamba 2005
Kenya	Welfare Monitoring Survey (WMS)	Cross-sections/ pseudo-panel	1994, 1997	Panel of 981 clusters, 10 different households in each cluster	Rural areas	Christiaensen and Subbarao 2005
Kenya	Quarterly survey of six sites	Panel	Ongoing		Rural northern	McPeak and Barrett 2001

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
					Kenya	
Kenya and Madagascar	U.S. Agency for International Development (USAID) panel	Panel	1993–95	301 households	Rural areas	Barrett, Bezuneh, and Aboud 2001
Kenya	Kenyan Ideational Change Survey (KICS) for HIV/AIDS knowledge	Panel	1994/95–1996/97	900 women and their husbands	Nyanza province	Alderman et al. 2001
Lesotho	Lesotho panel survey	Panel	1993, 2002	328 households	Unclear	Wason and Hall 2004
Madagascar	1–2–3 Survey	Rotating panel	1997–98 and 1998–99	1997–98: 1,151 1998–99: 2,371 1997–99: 1,249	Antananarivo (capital city)	Herrera and Roubaud 2005
Mali	Rural Survey	Panel	1997–98	274 households	Zone Lacustre	Christiaensen and Boisvert 2000
Mexico	Progresa/Oportunidades evaluation survey	Panel	1997–2000 every 6 months	41,000 households	Rural areas	Skoufias 2001
Mexico	National Survey of Urban Employment (ENEU)	Rotating panel	Quarterly, 1987–2001		Urban areas	Wodon 2001; Gong, Van Soest, and Villagomez 2004; Antman and McKenzie 2005
Nepal	Nepal Living Standards Survey	Panel	1995/96, 2003/4	962 households	National	Bhatta and Sharma 2006
Nicaragua	LSMS	Panel	1998, 2001	3,015	Urban and rural	Davis and Stampini 2002
Pakistan	Peshawar panel	Panel	1996, 1999	299	Villages in Peshawar district	Kurosaki 2006a, 2006b
Peru	National Household Survey (ENAHO)	Panel	1997–99	1997–98: 2,709 1998–99: 1,872 households	Urban areas	Herrera and Rouboud 2005

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
Peru	LSMS Instituto Cuanto	Panel	1990, 1994, 1996	421 households	Urban areas	Herrera 1999
Philippines	International Food Policy Research Institute (IFPRI) and Research Institute for Mindanao Culture (RIMCU) data	Panel	4 rounds in 1984/85, then 1 round in 1992, 2004	352 households	One village in Pangasinan province, Luzon island	Fuwa 2007
Poland	Household Budget Survey (HBS)	Panel	1993–1996	4,919 households	Rural and urban	Luttmer 2001
Rwanda	National budget-consumption survey	Panel	Quarterly (4 rounds), 1982–83	270 households	National	Muller 2003
Russia	Russian Longitudinal Monitoring Survey (RLMS)	Panel	1994, 1995, 1996, 1998	3,596 households, 1,970 in panel	National	Luttmer 2001; Lokshin and Ravallion 2004
South Africa (Gauteng province)	Community Agency for Social Enquiry study of urban informal settlements	Panel	1997, 2001	200 households	Urban, Gauteng province	Stevens 2003
South Africa	USAID survey	Panel	May/June 2001, Nov/Dec 2001, July/Aug 2002	355 households in 2 communities	Free State	Booysen 2003
South Africa	KwaZulu-Natal Income Dynamics Study (KIDS)	Panel	1993, 1998, 2004	1,171 households	KwaZulu-Natal	Alderman et al. 2001; Carter and May 2001; Keswell 2001; Fields et al. 2003a, 2003b; Cichello, Fields, and Liebbrandt

Country	Survey name	Type	Waves[a]	n	Coverage	Research using these data
						2005
Tanzania	Kagera Health and Development Survey (KHDS)	Panel	1991, 1992, 1993, 1994, 2004,	900 households in 1991–94, all households and split-offs in 2004 (2,774 households)	Kagera	Beegle, De Weerdt, and Dercon 2005
Uganda	Integrated Household Survey (IHS) and Uganda National Household Survey (UNHS)	Panel	1992, 1999	1992–96: 818 households; 1992–99: 1,005 households	National	Lawson, McKay, and Okidi 2003; Lawson 2004
Venezuela	Household Sample Survey (EHM)	Rotating panel	1997 and 1998	7,744 households	National	Freije 2003; Fields et al. 2003a, 2003b ; Fields et al. 2006
Vietnam	Vietnam Living Standards Survey (VLSS)	Panel	1992/93– 1997/98	4,272 households	National	Glewwe and Nguyen 2002; Baulch and Masset 2003
Zimbabwe	Bill Kinsey data	Panel	1983/84, 1987, annually 1992–2002	400 households		Elbers, Gunning, and Kinsey 2002

Note: In some cases different papers using the same data use different numbers of households. "National" coverage generally refers to nationally representative surveys, while "rural and urban" refers to surveys that include households in both rural and urban areas but do not necessarily constitute a nationally representative sample.

a. The dates follow the convention that for example 1985/86 refers to a round or wave of survey covering 1985 and 1986, while 1985-86 refers to two rounds of the survey, one in 1985 and one in 1986.

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[1] On the work of the Chronic Poverty Research Centre see, for example, *European Journal of Development Research* 17 (1); *International Planning Studies* 10 (1); *Journal of Human Development* 5 (2); and *World Development* 31 (3). Also see the organization's Web site at http://www.chronicpoverty.org. On Pathways Out of Poverty, see http://www.basis.wisc.edu/global_pathways.html.

[2] For studies using a panel of geographic areas, see Fofack, Monga, and Tuluy (2001) on Burkina Faso regions and Chomitz et al. (2005) on Brazilian municipalities. Recent theoretical discussions of estimating mobility or comparisons of different measures appear in Gottschalk and Spolaore (2002), Schluter and Trede (2003), and Ligon and Schechter (2004). Discussion of studies in the United States appears in the summer 2002 issue of the *Journal of Economic Perspectives* (see Bowles and Gintis 2002).

[3] Related reviews are Hickey (2001) and Lawson, McKay, and Moore (2003).

[4] A number of other studies are reviewed in McKay and Lawson (2002), yielding broadly the same conclusions.

[5] For the United States and Scandinavian countries, Aaberge et al. (2002) replace survey estimates of income with payroll record estimates, which may measure income with less error. But such data are less available in developing countries and may exclude the more common informal sector income.

[6] Often survey documents other than mobility analyses discuss these details. Some such documents appear in the far-right column of table 7, while others, such as the local mimeographs mentioned in Barrett, Bezuneh, and Aboud (2001) and Lokshin and Ravallion (2004), are not used here.

[7] But note that this is household-level attrition, that is, a household is considered traced if at least one member is traced.

[8] The survey instrument used was still largely a household survey instrument, but to be applied to the household to which the tracked individuals now belonged.

[9] One individual, currently residing in Sweden, remains to be interviewed.

[10] A "statistical" solution is to drop the analysis of panels altogether and rely on pseudo-panels, or the tracking of a cohort (age, demographic group, other identifiable group) through various cross-sections. For Mexico, Antman and McKenzie (2005) find that disappearing households are younger, less often married, and with smaller households and larger incomes than households that remain in the sample; the effects are significant given the large sample but are only small in size.