# CUTTING THE COSTS OF ATTRITION: RESULTS FROM THE INDONESIA FAMILY LIFE SURVEY

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# Cutting the costs of attrition:
## Results from the Indonesia Family Life Survey

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Abstract

Attrition is the Achilles heel of longitudinal surveys. Drawing on our experience in the Indonesia Family Life Survey, we describe survey design and field strategies that contributed to minimizing attrition over four waves of the survey. The data are used to illustrate the selectivity of respondents who attrit from the survey and, also the selectivity of respondents who move from the place they were interviewed at baseline and are subsequently interviewed in a new location. These results provide insights into the nature of selection that will arise in studies that fail to track and interview movers. Attrition, and types of attrition, are related in complex ways to a broad array of characteristics measured at baseline. Our evidence also suggests attrition may be related to characteristics that are not observed in our baseline. We draw on data from a Survey of Surveyors and describe characteristics of both the interviewers and the interview that predict attrition in later waves. These characteristics point to possible strategies that may reduce levels of attrition and may also reduce the impact of attrition on the interpretation of models estimated with longitudinal data.

Keywords: Attrition, longitudinal studies, survey design, Indonesia

# 1. INTRODUCTION

Over the last quarter century the quantity and quality of socio-economic surveys conducted across the globe has grown spectacularly. Longitudinal surveys, which have been on the leading edge of this movement, have proved to be extremely powerful resources for research in economics and the social sciences. However, non-response has limited the contributions of many of the longitudinal surveys conducted in developing countries. Not only are non-response rates high in some important surveys, the causes and consequences of non-response are poorly understood. Whereas non-response in higher income societies like the United States results largely from refusal, in most developing countries, refusal rates in household surveys are extremely low. In those contexts, failure to relocate and re-interview respondents because they moved after the baseline is the primary cause of non-response.

In fact, in many longitudinal surveys in developing countries the design for follow up surveys explicitly targets *only* the subset of respondents who remain in their baseline location. Other surveys follow respondents who move within the vicinity of that location. These surveys will, in general, have higher rates of attrition than studies that follow migrants. Moreover attrition in those studies will be selected on the same characteristics that are associated with geographic mobility. The importance of this phenomenon for interpreting results of studies that use longitudinal survey data remains controversial in the literature.

The Indonesia Family Life Survey (IFLS) is ideally suited to shed new light on this issue. The baseline was conducted in 1993. Since the first follow-up interview in 1997, IFLS has sought to minimize attrition and track respondents who move. In each of the three follow-up surveys, 19 out of every 20 target households have been re-contacted. Successful follow-up is not a result of low rates of mobility. Of the baseline respondents who were re-interviewed in the fourth wave in 2007, over one-third had moved away from the community in which they were interviewed at baseline. The four waves of the survey span a period of rapid economic growth, dramatic economic and political upheavals in the late 1990s at the time of the Asian Crisis, and the 2004 Indian Ocean tsunami, which was one of the largest natural disasters in recorded history. These events have elevated rates of geographic mobility above an already substantial baseline.

After providing a brief overview of the literature on attrition in longitudinal surveys, we describe the protocols used in IFLS to maximize recontact across the survey waves. We then discuss levels of attrition in the waves, contrasting the actual recontact rates with what would have been achieved had we not followed movers. We further distinguish movers by, roughly speaking, the

distance they move from the baseline location. We present evidence not only that failure to track movers raises rates of attrition but also that the selectivity of those who attrit is linked to the distance they move from the baseline location. This selection appears also to depend on characteristics unlikely to be observed at baseline in most socio-economic and demographic surveys. Exploiting a Survey of Surveyors that we conducted in the first follow-up in 1997, the final section describes the survey and surveyor characteristics that predict successful follow-up in subsequent waves. These results suggest additional strategies that might be adopted to reduce attrition in the field as well as potential indicators that can be measured in surveys and might mitigate the impact of attrition in models of behavior.

## 2. ATTRITION IN LONGITUDINAL STUDIES

Attrition is the Achilles heel of longitudinal studies. Becketti et al (1988) provide an insightful discussion in the context of the Panel Study of Income Dynamics (PSID). Ashenfelter, Deaton and Solon (1986) discuss potential concerns in the context of longitudinal surveys conducted in developing countries. See, also, Dodds, Furlong and Croxford (1989).

One line of inquiry in this literature focuses on the consequences of non-response for interpretation of panel data models of behaviors. In developed country contexts, research on attrition has tended to focus on refusals (but see Abraham, Maitland and Bianchi, 2006, for an exception.)

Fitzgerald, Gottschalk and Moffitt (1998) discuss attrition in the PSID. Twenty years after the baseline, nearly half the original respondents had attrited from the sample and they are more likely to be drawn from among lower socio-economic status respondents. However, the authors conclude that attrition has not seriously distorted the representativeness of the PSID sample. Their comparisons of models of earnings, marital status and welfare participation estimated using the PSID and the Current Population Survey yield similar slope coefficients and they find that income and marriage transitions have little impact on the representativeness of the non-attriting sample. They conclude that attrition in the PSID is mostly noise.

The same conclusions emerge in similar studies of attrition in longitudinal surveys conducted in developing countries. Using data from Bolivia, Kenya and South Africa, Alderman et al (2001) report that attrition rates are high and that those who attrit are selected on socio-economic characteristics (negatively in Bolivia and positively in the other surveys). They find that the impact of attrition on coefficient estimates in multivariate models of child development, fertility and child

anthropometry is seldom significant. They conclude that "attrition apparently is *not* a general and pervasive problem for obtaining consistent estimates [in these models]" (page 114). Falaris (2003) comes to the same conclusion using World Bank data from the Cote d'Ivoire, Peru and Vietnam. He finds that in models of school attainment, labor force participation, self-employment, wages and fertility, it is not possible to differentiate between respondents who were only interviewed at baseline and those that were followed up.

Many longitudinal surveys in developing countries are designed to exclude from follow up waves those respondents who move out of the original community. This design, recommended by the World Bank (Glewwe and Jacoby, 2000), was followed in all the surveys examined by Falaris and in two of the three surveys examined by Alderman et al.

The implication of these studies is that in longitudinal surveys with arbitrary attrition, it is feasible to construct weights based on characteristics of respondents observed at baseline that will adjust for attrition. Put another way, conditional on these weights, attrition can be treated as random and ignorable (Rubin, 1987). This is an extremely important conclusion and suggests that allocating resources to minimize attrition is unlikely to be wise.

As Fitzgerald, Gottschalk and Moffitt note, however, this conclusion is predicated on the assumption that attrition is selected on characteristics that are *observed* at baseline. In general, if unobserved characteristics are correlated both with attrition and with the behavioral outcome being analyzed in a model, then weights based on observed characteristics will not fully adjust for attrition. Thomas, Frankenberg and Smith (1998) argue the ignorability assumption, that attrition is selected on observed characteristics, is unlikely to be generally true in longitudinal surveys in developing countries. In those surveys, relatively few respondents refuse to participate and so attrition is primarily because respondents who move are lost to follow-up. Predicting movers accurately is very hard, even in surveys with rich data, and it is plausible that many hard to measure factors, such as ambition, patience, willingness to embrace uncertainty and take risks, will affect migration decisions and thereby attrition.

Using the first two waves of IFLS, Thomas, Frankenberg and Smith establish that failure to follow movers exacerbates the selective nature of attrition in these surveys. They show that, in many dimensions that are observed at baseline, longer distance movers have more in common with those that are not recontacted in the follow up than respondents who were interviewed in the same location in both waves. They also show that movers are not only mobile across space but also across socio-

economic status. During the four year hiatus between the first and second wave of IFLS, growth in per capita expenditure among longer distance movers is over 75% greater than among respondents who did not move. The latter are the people likely to be re-interviewed in surveys that are designed to not follow movers and those studies will tend to understate improvements in living standards. The same observation is made by Beegle, De Weerdt and Dercon (2008) based on data from Tanzania.

Using data from Bangladesh, Rosenzweig (2003) shows that restricting attention to stayers in models of school attainment and monthly earnings substantially and significantly overstates the magnitude of the effect of origin household income and land on mobility while it understates the impact of the level of schooling in the origin household. He concludes that survey protocols that do not follow split offs from origin households should be avoided. Similar results are suggested by Gritz, MaCurdy and Mroz (1994) who highlight differences in labor force attachment of those who attrit relative to those who do not in the PSID.

A second line of inquiry has focused on survey methods and interview outcomes. An extensive literature in survey research considers the impact of the characteristics of interviews and interviewers on non-response and quality of information collected in cross-section surveys. (See, for example, Groves and Kahn, 1979, Groves, 1989, Groves and Couper, 1998 and Groves, 2006.) In contrast, relatively few studies have explored survey design and field protocols in the context of attrition in longitudinal surveys. An insightful discussion is provided by Olsen (2005) who describes the key role of design features in keeping attrition low in the National Longitudinal Survey of Youth (NLSY). He emphasizes the importance of persistence, following-up respondents who are skipped in one wave and "selling the survey" to both the interviewers and respondents.

A few studies examine the association between interview characteristics in one wave and non-response in a subsequent wave. In addition to Olsen's work on NLSY, Hill and Willis (1998) examine the Health and Retirement Study and Zabel (1998) examines both PSID and the Survey of Income and Program Participation (SIPP).

Whereas Hill and Willis (1998) report that length of interview has no impact on willingness to participate in subsequent waves of HRS, Zabel finds that attrition is lower among respondents who completed an interview that lasted longer in the prior wave in both SIPP and PSID. A similar result is reported by Branden, Gritz and Pergamit (1995) for the NLSY. The effect is significant in PSID and is not explained by people who take longer to complete an interview being more co-operative since Zabel controls for the average length of interview in the first five waves of the survey. Moreover, he

reports that an explicit attempt to reduce the length of the interview in 1973 resulted in higher attrition in later waves.

Attrition is lower in HRS and SIPP when respondents are contacted by the same interviewer across waves (Hill and Willis, 1998; Zabel, 1998). This is interpreted as reflecting rapport between the interviewer and respondent although no such association is reported in the NLSY (Olsen, 2006).

In one of the few studies that explores survey design and attrition in a developing country context, Maluccio (2004) re-examines the South African data used by Alderman et al (2001). He estimates models of household per capita expenditure that explicitly adjust for attrition using indicators of survey quality as instruments.[1] He concludes that attrition in this model is not ignorable and that inferences about long run resources are contaminated by attrition.

We extend these analyses below, drawing on information collected in a Survey of Surveyors that we conducted as part of the second wave of IFLS. We show that in addition to the characteristics of the interviews, the characteristics of the interviewer who conduct the interview is predictive of interview outcomes in later waves. We find that the factors that are important differ depending on whether the case is a subsequent refusal versus a tracking case—which suggests it is important to distinguish among reasons for non-response in longitudinal surveys. Before discussing the results, the next sub-section describes follow-up procedures in IFLS that are designed to minimize attrition.

## 3. FOLLOW UP IN IFLS

The keys to achieving low rates of non-response in IFLS include careful attention to planning and training, persistence and commitment to the study goals by the entire team and the allocation of resources. A critical feature of successful follow-up and re-interviewing of respondents is providing interviewers and trackers with detailed information on a wide range of individual, household and family attributes of respondents. This process necessarily begins with the first wave of data collection. During the interview in IFLS1 (and in each subsequent wave) we collected extensive information that would facilitate future re-contact. Detail about the current location over and above the address is collected. This includes a sketch map with landmarks and a description of how to find the location, along with land and mobile phone numbers and email addresses. Respondents are asked to identify people who will likely know their whereabouts in the future, including both people living in the community as well as people living elsewhere. We explicitly ask whether the respondent might

[1] Survey quality is measured by whether the questionnaire was verified by the supervisor in the baseline and the completion rate in the original enumeration area.

move in the future and, if so, likely destinations and probe for contacts in those destinations. We also ask about non co-resident family members or friends with whom the respondent keeps in touch and are therefore likely to know the whereabouts of the respondent in the future, even if he/she moves. Contact information about each of these people is collected from the respondent.

We supplement the detailed re-contact information with data collected from each respondent during the main interview, such as the location and address of schools and workplaces and names of other non-coresident family members. At the start of each follow-up survey, all the information that is potentially relevant is collected together to generate an electronic recontact database which the field teams draw on as needed.

The interviewing teams begin by returning to the dwelling where the household was last found. If the entire household had moved the interviewers look for informants who can provide new information on the household's location. In addition to people on the contact list provided by the informant in prior waves, the interviewers ask neighbors and, as needed, reach out to local leaders, teachers at the schools that are listed on the contact sheet, visit former workplaces, and also ask the local post office and health centers about the current whereabouts of the respondents. A tracking form is used to record the name and address of the informant providing the information along with as much detail as possible about the addresses, telephone numbers, schools and work places of the household members as well as the name and address of potential informants in the new location who are likely to know the whereabouts of the respondents. In some instances, respondents are unable to provide clear addresses but can describe how to find the household. Whenever possible, we ask the informant to help us draw a sketch map of the destination location. All the information that each informant provides is recorded on a form which is entered into the electronic database. Different informants provide different information, so we patch the pieces together to compile an updated listing for the next step in tracking. As soon as they are collected, these data are uploaded into our centrally-maintained tracking database.

If the dwelling is still inhabited by someone from the target household but one or more individuals identified as targets for tracking (defined according to protocols specified below) has moved, then the interviewers complete an individual tracking form. The content of the form parallels the household tracking form, but in most of these cases more complete information is obtained than when an entire household has moved.

Armed with information on movers drawn from our centralized database, the teams make a choice as to whether the case should be handled as local tracking (destinations within about 45 minutes of the origin area by public transportation) or longer distance tracking. Teams typically search for local tracking cases while still working within the enumeration area. Longer distance tracking cases are referred to staff at central headquarters, who then assign the case to the field team whose route will take them closest to the new location. The team is provided with the most up-to-date information on the whereabouts of the target respondent.

The process is dynamic. The tracking database is updated as new information on movers is collected. In some cases, the information collected from different informants or teams conflicts. These conflicts are resolved by manual examination of the records in conjunction with electronic communication among the interviewers and staff in headquarters.

Each of the 23 teams in IFLS is responsible for the interviews in their work area. At the start of the survey, each team is assigned to about fifteen enumeration areas (eas) covering about 10 kabupaten (or districts). The eas are typically quite far apart: most are in different kecamatans (sub-districts) and only a small number are in the same desa (which are villages in rural areas or neighborhoods in urban areas).[2] After all the assigned eas have been visited a team undertakes a sweep through the entire work area to find and interview respondents thought to reside there but who were missed during the main fieldwork. These include respondents who moved within the area but were not deemed "local" tracking cases when location information was collected, respondents who moved back to a desa after that desa had been visited and respondents who moved into the area but the location information had not been collected at the time the team was working nearby.

When these sweeps have been completed by all the teams, interviewers are collapsed into a smaller number of tracking teams (composed of the best interviewers) to continue tracking hard to find respondents and longer-distance movers. This tracking phase typically lasts several months and ends when all the remaining incomplete cases are judged unlikely to be found in that survey wave.

[2] These geographic distinctions are important. To put them into perspective, in the 15 provinces included in IFLS, there are 228 kabupaten, 3,323 kecamatans and 42,824 desas. According to the 2003 Potensi Desa Survey, a survey of leaders of each desa in the country, the median number of people living in a desa is slightly under 3,000, the median population of a kecamatan is nearly 45,000 people and the median population of a kabupatens is almost 650,000 people.

## 4. RESULTS

With this background, we turn to results from IFLS, beginning with an overview of the study design, follow-up protocols and completion rates in each wave after the 1993 baseline. We then focus on interview outcomes in the 2007 wave and discuss the importance of following respondents who moved from the origin location. Empirical evidence on the selectivity of movers and respondents who were not re-interviewed suggests that, at least in IFLS, attrition is not ignorable. We end with a discussion of interview and interviewer characteristics which might serve as instruments for attrition in behavioral models.

*Completion rates in each wave of IFLS*

A central design issue for any household survey is the question of whom within the household to interview and, in longitudinal surveys, whom to follow if any household member moves. In IFLS, when all the members of a household have changed location, we track that household unless the available information indicates that the entire household has moved outside the IFLS provinces.

The rules for interviewing and tracking individual members within IFLS households vary across the different waves of the IFLS. Table 1 summarizes re-contact rates for each survey wave.

IFLS1 was conducted in 1993. The stratified sample, which consisted of 321 eas in drawn from the 1993 SUSENAS, was representative of about 83% of the Indonesian population at the time. The 1993 survey covered 13 provinces. Outlying areas were excluded. Over 93% of the target households were contacted and the total number of individuals living in those households was 33,081.

For cost reasons IFLS1 implemented a within-household sampling scheme, which involved individual interviews with the household head and his/her spouse, up to two of their children age 0-14 who were randomly selected, and a randomly selected member age 50 or older and his/her spouse. For a randomly selected 25% of the households, an individual age 15 to 49 and his/her spouse were also randomly selected from the remaining members on the household roster. This scheme led to the designation of 22,588 people as eligible for individual interviews. Of those people, 97.5% completed interviews and 2.5% refused. (Frankenberg and Karoly, 1995.)

The first follow up, IFLS2, was in 1997. The goal was to relocate and re-interview all IFLS1 households and individually interview all “target respondents”. The targets include the 22,588 “main” respondents who were individually assessed in IFLS1, as well as all 1993 household

members who were age 25 or older at baseline (i.e. all cohorts born before 1969). Expanding the targets to include the latter group resulted in IFLS being representative of those cohorts without having to take into account the within household sampling strategy adopted in 1993. In addition, we interviewed every individual in the origin household and all new entrants into those households. (See Frankenberg and Thomas, 2000, for a detailed description.) If one or more individuals had moved out of the household by 1997, those movers who were target respondents were tracked. The new households in which movers were found are split-off households.

Once a household was found, the rules for interviewing household members differed for origin and split-off households. In origin households the goal was to interview all members including new entrants since 1993. In split-off households only target respondents, their spouses, and their biological children were to be interviewed. The rationale for this was that it reduced the number of new respondents in the survey who had only a tenuous connection to IFLS1 household members.

Results for IFLS2 are displayed in panel B of Table 1. Of the 33,081 household members in IFLS1, 854 died before IFLS2 leaving 32,227 of the original members (row 3). 26,948 of them were interviewed in IFLS2. Of those, 23,049 were “target respondents” and thus eligible to be tracked (row 05). Over 91% of the “target respondents” were interviewed (row 06), about 1% refused and the rest were lost to follow-up.

Almost 4,000 respondents were interviewed in IFLS2, because they were living in the same household as a target respondent, although they themselves were not eligible for tracking. An additional 5,404 respondents were individually assessed in IFLS2 (row 09). They are new entrants to households of target members and include, for example, new spouses and children born after the 1993 baseline survey. In all, IFLS2 completed interviews with 32,352 respondents (row 10), almost 50% more than in IFLS1.

The sample of people eligible for an interview in IFLS3 includes both IFLS2 respondents and people who were eligible for the survey but not interviewed. This second group includes people lost to follow-up and refusals. This point is an important one. It means that even when a respondent is not found in one wave, the respondent stays on the sample listing and we attempt to find him/her in all subsequent waves. Several studies follow only the people interviewed in the most recent round of data collection, with the result that attrition is cumulative. Because all eligible respondents are kept in the pool, at the end of IFLS2 the potential sample for IFLS3 was 37,631 respondents.

In 1998, a year after IFLS2, households who in 1993 were living in a purposively selected subsample of 25% of enumeration areas were included in a special study to measure the immediate impact of the East Asian financial crisis. That study added 1,970 respondents who were eligible to be interviewed in IFLS3 yielding a total potential sample of almost 40,000 respondents (Panel C, row 1). Among these 790 had died by IFLS3.

In IFLS3 follow-up and interviewing protocols were the same as in IFLS2 except that the definition of a target respondent was expanded to include additional classes of respondents.[3] Of the 38,811 respondents eligible for IFLS3, 32,189 were eligible to be tracked and interviewed. Of these, 91.5% were interviewed and just under 1% refused. The rest were lost to follow up. These rates are very similar to those in IFLS2 in spite of the expansion of the eligibility rules. In IFLS3 there were slightly over 6,000 new entrants, and over 38,000 people were individually assessed in the survey. Almost 45,000 people were eligible for the next wave. (Strauss et al, 2004.)

IFLS4 used essentially the same re-contact and interview protocols as IFLS3. Of those eligible for IFLS4, 2,610 died between the waves leaving 42,305 potential respondents, of whom 32,757 were "target respondents". Of these people, 86.5% were tracked and interviewed with 1% refusing and the remaining 12% lost to follow up. We will discuss these respondents in more detail in the next section. There were over 12,000 new respondents in IFLS4 and nearly 45,000 people were individually assessed in the survey-- double the number of respondents in IFLS1. (Strauss et al, 2009.)

*Follow-up in 2007 wave of IFLS*

Our analysis of the factors that are associated with attrition focuses on the respondents who were eligible to be tracked in 2007. In addition, we focus primarily on respondents who were age 15 and older (born before 1979) at the time of the baseline survey in 1993 because those respondents were individually interviewed by an enumerator and completed an extensive battery of questions. We will draw on that information in our assessment of the selectivity of non-respondents. Before discussing those respondents in detail, panel A of Table 2 presents interview outcomes in 2007 for all

[3] Four additional classes of respondents were eligible for tracking in 2000. They are individuals born since 1993 in original IFLS1 households, individuals born after 1988 and resident in an original IFLS1 household in 1993, IFLS1 household members born between 1968 and 1988 and interviewed in 1997 and a 20% random sub-sample of 1993 household members born between 1968 and 1988 if they were *not* interviewed in 199. The motivation behind expanding the group of individuals eligible for tracking was to be able to follow young children in panel households and a subset of young adults, born between 1968 and 1988. This strategy was designed to keep the sample, once weighted, more closely representative of the original 1993 population in the 13 IFLS provinces. In addition, interview rules were expanded in IFLS3 for splitoff households to include any IFLS1 household member, not just the tracked member, their spouses and biological children.

target respondents who were in IFLS1. Older respondents, born before 1979, are in the left hand panel and younger respondents are in the right hand panel.[4]

Of slightly over 16,500 respondents in the older cohort, 88% were interviewed in the 2007 wave, resulting in an attrition rate for this sample of 12% after 14 years. This amounts to an average loss of less than 1% per year.

One indicator of the costs of not following movers in a longitudinal survey is apparent from row 2a of the table. Among respondents who were eligible to be tracked, 70% were located in the same desa (village or neighborhood) in which they were interviewed in 1993. Had we not followed people who moved, attrition would have been over 30%. This is more than two and half times the actual level of attrition. Some of the people interviewed in the same desa had moved within that desa; we do not attempt to distinguish within-desa movers from those who were re-interviewed in the same house. In the absence of good address information, it is difficult to know whether a person is living in exactly the same housing structure or on the same plot of land in each survey wave. Further, it is not the case that all these people who were interviewed in the same desa had never moved. Many of them had moved to a different desa between the waves and returned to the original desa.

Eighteen percent of target respondents had moved from the desa of residence at baseline, and were tracked to their new desa and interviewed in the new location. Of those, 4% had moved outside the desa but within the kecamatan (or sub-district) which one might interpret as relatively local movers. Another 5% had moved outside the kecamatan but remained within the kabupaten (district or regency). A further 5% moved outside the kabupaten but remained in the province and 4% had moved out of the province.

At baseline, the IFLS sample was clustered in 321 desas in 13 provinces. As we follow movers, the spatial coverage of the survey expands. This is displayed in Figure 1 which identifies the 288 kecamatans included in the baseline in 1993 in the upper panel and the four-fold increase to nearly 1,400 kecamatans in which respondents were interviewed in 2007 in the lower panel.

As in many other surveys in developing countries, refusals account for only a small fraction of non-response in IFLS. We distinguish two groups of people among those who were lost to follow-up. About half of these respondents are people for whom we obtained no information because we had no contact with them or other members of their household. However, for the other half we were able

[4] There are 5,265 respondents who were born to target respondents after 1993 and so are included in the 2007 target sample in Table 1. 91% of these respondents were interviewed in 2007. Since they were not born at baseline, and we use baseline characteristics in the models, they are not included in Table 2.

to gather some information because we interviewed at least one member of the household in which the respondent was interviewed in 1993. In these cases a limited amount of proxy information was collected from the respondents we did interview about the respondents we did not find. On-going research is exploring the quality of this proxy information. For research questions where it is possible to use proxy information, the attrition rate would be almost halved to less than 7%.

The pattern for younger respondents (born between 1979 and 1993) is broadly similar, although the completion rate is lower and only 55% of the respondents would have been re-interviewed if we had not tracked movers. Over 15% of the respondents had moved outside the original kabupaten. No information was collected for nearly three-quarters of the 18% who were not found.

Panel B of Table 2 focuses on respondents born before 1979 and presents interview outcomes by gender, birth cohort and location of residence in 1993. The gender differences are modest. The cohort differences are not. The youngest cohorts (1969-78) are very similar to those born after 1979. The oldest cohorts are the most likely to be interviewed (91%) and the least likely to have moved away from the desa. If we use proxy information collected from other 1993 household members, we would have a completion rate close to 97% of all target respondents in the oldest cohort. The completion rate is highest among people who were living in rural areas (93%), most of whom were interviewed in the same desa (80%). For the majority of cases that were lost to follow-up, the entire household moved and we have failed to contact anyone from the original household. People who lived in urban areas in 1993 have been more mobile and those who were living in a major city even more mobile so that completion rates are lower and the fraction still living in the same desa is much lower.

Birth cohort, location and, to a less extent, gender are predictive of response rates and reasons for non-response. In the next sub-section, we explore whether other characteristics are related to non-response drawing on the extremely rich information about socio-demographic and economic characteristics of each respondent measured at baseline. We focus on the respondents born before 1979 because characteristics at baseline are not likely to be as informative for respondents who were young children at that time. Education of a child at age 10, for example, conveys little information about longer-term socio-economic status. Moreover, many of those who were children at baseline move with their parents, who are included in the analyses reported below.

*Selectivity of non-respondents and movers*

Table 3 draws comparisons between those who were interviewed in 2007 and those who were not. The comparisons are made across multiple dimensions of well-being measured at baseline and so are known for every eligible respondent, including those who were not interviewed in 2007.

The average number of years of completed schooling of respondents, as of 1993, is reported in the first row. Those who were interviewed had, on average, 5.96 years of schooling (row 1 column 1), those who were not interviewed had completed almost 7.97 years of schooling (column 2) on average. The difference, 2 years of schooling, is significant (column 3). Standard errors that take into account clustering of the sample are reported below the means and the difference.

Looking down column 3, relative to those who were interviewed in 2007, respondents who were not interviewed had significantly better educated fathers and mothers, were over 1cm taller, were 12 percentage points less likely to be married, 24 percentage points more likely to have moved by age 12 and 8 percentage points less likely to have been working for pay at the time of the baseline. Among those who were working at baseline, attriters earned significantly more.

Household characteristics are reported in the lower panel of the table. About 40% of those who were interviewed were living in a household that owned a farm business in 1993 but only a quarter of those who were not interviewed lived in a household with a farm business. Attrition is significantly lower among those who owned a farm business. However, owning a non-farm business is not associated with attrition – slightly over one-third of all respondents lived in a household with a non-farm business and there is no difference between those who were interviewed and those not interviewed. The level of household per capita expenditure (PCE) was significantly higher at baseline for those who attrited relative to those who were interviewed. This is not because those who were interviewed came from larger households: in fact there is no difference in average household size between the two groups.

Non-response is not only selected on age, location and gender as shown in Table 2. Table 3 establishes that there are large and significant differences in the socio-economic characteristics, work choices, farm ownership, demographic characteristics and migration experience (in 1993) of those who were subsequently interviewed in 2007 and those who were not re-interviewed.

To explore these differences in more depth, the right hand panel of the table distinguishes those interviewed by the location in which they were found in 2007, following the same geographic

groups used in Table 2. Those who were not interviewed also follow the same distinctions adopted in Table 2.

Column 4 reports the average among respondents who were interviewed in the same desa in 1993 and 2007 (stayers). The average education of this group is 5.44 years. The remaining columns report the *difference* for each group relative to the stayers. Columns 5 through 8 report differences for movers who were interviewed. For example, movers outside the desa but within the kecamatan had 1.74 more years of schooling (column 5) whereas the gap between movers and stayers is 2.67 years for those who moved to a different province (column 8).

Differences between those interviewed in the same desa and those not interviewed are reported in columns 9 through 11. Those who refused had 3.45 more years of schooling and those with whom there was no contact with any original household member had 2.53 more years of schooling.

Each of the differences in education in columns 5 through 11 is significant indicating that the stayers are significantly less educated than any of the classes of movers and classes of non-respondents. Among movers, those who stayed within the original kecamatan are less educated than those who moved further away but, within the latter group, the differences in education are not significant, nor is it possible to distinguish them from those who were not found, at least in terms of own education. This is an example of how stayers are different from longer distance movers who are, in this dimension, more similar to those who attrit. As noted above, this has important implications for the representativeness of samples in longitudinal studies that do not follow movers.

Education levels of the father and the mother of the respondent are reported in the second and third rows, respectively. Respondents who were not interviewed have substantially and significantly better educated parents than those who were interviewed. As with own education, respondents who were interviewed in the original desa have parents with significantly less schooling than any other respondents. Parental education rises with distance from the origin desa, and those who moved beyond the origin kabupaten have better educated parents than those who were not found. Refusals have the best educated mothers.

The fact that parental education is associated with attrition suggests that non-response is selected on background and possibly early life experiences. Evidence on the links between non-response and height in the next row provides corroborating evidence. Height of an adult is indicative of both background (through genetic influences) and early life health and nutrition. People who were

not interviewed are significantly taller than those who were interviewed and refusals are significantly taller than any other group. Those for whom no information is collected are taller than movers who are found and interviewed who are, in turn, taller than stayers re-interviewed in the original desa.

IFLS collects a migration history from each respondent. We use information on moves prior to the baseline survey and restrict attention to moves made before the respondent was age 12, most of which were made with parents. Whereas about half the respondents who were interviewed in 2007 had moved by age 12, over three-quarters of those who were not re-interviewed had moved by age 12. This 25 percentage point difference is significant. Among those who were re-interviewed, longer distance movers are the most likely to have moved by age 12. Respondents about whom no information was collected were the most likely to have moved by age 12.

This evidence indicates that attrition among adults is selected not only on age, education and location at baseline but also on characteristics that are associated with other markers of human capital, background, early life experience and choices by parents made well before the baseline. Moreover, choices made by the respondent by baseline are also predictive of subsequent attrition.

As shown in the next panel of the table, respondents who were married at baseline are more likely to be interviewed, less likely to have moved away from where they were interviewed in 1993 and the least likely to stray far from the original desa. A very similar pattern emerges for respondents who were working for pay at baseline. Whereas workers are less likely to attrit from the survey, among those who are working, higher earnings are associated with higher rates of attrition. Respondents who were earning more at baseline are more likely to subsequently move away from the original desa, more likely to move far away and more likely to not be found, paralleling the results for other indicators of human capital discussed above. Refusals have significantly higher earnings at baseline than any of the other groups in the table suggesting that value of time is likely a factor in a respondent's decision regarding participation in the survey.

The final panel of the table explores the relationship between non-response and characteristics of the respondent's household at baseline. Respondents who were living in households that owned a farm business at baseline are significantly more likely to be re-interviewed. This is primarily driven by the fact that these people are the least likely to move away from the original home and, those that do, keep contact with the farm household making it easier for us to find them in 2007. This is not because entrepreneurs or the self-employed are more likely to be interviewed – owning a business is unrelated to interview outcomes. Rather, like marriage, owning land likely

anchors at least some members of the original household and those who move keep contact with the original household, rendering tracking movers more successful.

Household resources are measured by the (logarithm) of per capita expenditure (PCE) which is a longer-run indicator of resource availability. Respondents who were not interviewed lived in households with significantly higher levels of PCE at baseline. Those who were interviewed in the same desa in 2007 have the lowest level of PCE at baseline and PCE rises with distance from the original location. Respondents who refused and those for whom no information was collected have the highest levels of PCE at baseline. The gap between their PCE and that of the stayers interviewed in the original desa is about twice as large as the gap between stayers and those who moved out of the original kabupaten.

Differences in PCE may reflect differences in household composition rather than differences in resources available to households. In principle, one might adjust for these differences, but it is unclear how such adjustments should be made. The final row presents differences in household size across the classes of interview outcomes. The people for whom no information is collected in 2007 were living in the smallest households at baseline—about 2/3 of a person smaller than the households of respondents who were reinterviewed in their 1993 desa. All other respondents who were interviewed in 2007 had been living in households with about about 1/3 of an additional person at baseline. Those who refused to participate in 2007 were living in the largest households at baseline. The key result is that the highest PCE households and those with the fewest numbers of members at baseline are least likely to have been re-interviewed in 2007.

Table 3 paints a complex picture of selection on a wide variety of socio-economic and demographic characteristics that span family background, resources and human capital choices. Moreover, among those who were re-interviewed, the extent of selection varies with how far the respondent is tracked from the original desa, while among those who were not re-interviewed, selectivity varies with the reason for non response.

An important question revolves around whether differences in the selectivity of those who attrit and those who do not can be fully explained with observed characteristics. The fact that attrition is correlated with so many different observed characteristics suggests that characteristics that we do not observe may also play a role. This issue lies at the heart of the ignorability assumption. We will take this up in more detail below, but some preliminary insights are provided by Appendix Table 1, which repeats the estimates of differences in Table 3 in regression models that simultaneously adjust

for age, gender and location of residence (province and whether the respondent was living in urban area) in 1993.

The first column of the appendix table repeats the unadjusted difference between those who were and those who were not interviewed (Table 3, column 3). The second column displays the adjusted differences. While the magnitudes of the differences are smaller, none of the inferences drawn from Table 3 are affected in terms of signs or significance. Adjusted differences relative to interviewing the respondent in the original desa are displayed in columns 4 through 10 of the appendix table. The patterns follow those in Table 3 and the main conclusion remains: selection is important and varies in complex ways with both the nature of the interview outcome and the characteristics of the respondent.

In sum, in terms of the characteristics measured at baseline and used in these analyses, the target respondents who are most likely to attrit from the survey are better educated, taller, have better educated parents, are more likely to have moved at an early age, come from higher PCE households, are more likely to be young, single and urban. In contrast, it is respondents who had the least human capital, lowest earnings, lowest PCE, were married, lived in a rural household with a farm business and had not moved at any early age are the most likely to be interviewed in the same desa in 2007 as in the baseline. Movers who were interviewed fall between these extremes. In general, the longer the distance of the move from the original desa, the greater the distance in socio-economic and demographic characteristics from respondents who were interviewed in the same desa in the baseline and follow-up. However, in some cases, movers look (in terms of these dimensions of socio-demographic characteristics at baseline) more like those not found and in other cases they look more like the people interviewed in the same desa.

Clearly, had IFLS chosen not to follow movers, the attrition rates would have been higher (as shown in Table 2), *and* attrition would have been even more selected than it is, at least in terms of the baseline characteristics included in Table 3.

*Unobserved differences between "movers" and "stayers"*

It has been argued that if the differences between respondents who are interviewed and those who attrit can be fully captured by characteristics that are observed at baseline, then controlling these characteristics in empirical models of behavior will take into account the differences between those who attrit and those who do not.

If the differences between those who attrit and those who do not are not fully observed at baseline, then an adjustment strategy based on only observed characteristics may not perform well, and will in general lead to statistical biases in estimation. To explore this issue, we examine changes in the relationship between (the logarithm of) monthly earnings and education over time for respondents who were interviewed in the same desa in both the baseline and follow-up and respondents who had moved. Table 4 reports the estimated coefficients on education.[5]

The first row of panel A of the table displays results from the baseline survey for stayers (respondents who were interviewed in the same desa in 2007) in the first column and movers (those interviewed in a different desa in 2007) in the second column. The difference in the estimates is reported in the third column. At baseline, people who did not subsequently move had higher returns to their education than those who subsequently moved. The second row reports the same model using respondents who were interviewed in the 2007 survey. By then, the tables had turned, and for the same respondents, movers had higher returns to their education than stayers, and this difference is also significant. Growth in earnings is reported in the third row. Among stayers income growth is unrelated to education. Among movers, income growth is 3% higher for each year of education. This difference is also significant.

We conclude that not only do returns to education measured at baseline differ for movers and stayers, but changes in those returns after the baseline also differ. While there are several potential explanations for this result, we conjecture that it likely reflects differences between movers and stayers that we are not capturing in the baseline survey (at least with the characteristics in the models). This may reflect differences between movers and stayers that we did not measure at baseline, it may reflect innovations in the lives of those who move or it may reflect innovations that occurred after the baseline which affected the probability an individual moved from the origin desa and did not return.

Given the patterns reported above, we conjecture that the differences between movers and stayers are lower bound estimates of the differences between those not found and respondents interviewed in the original desa. An important implication of these results is that it may not be prudent to draw conclusions about the extent of selectivity among those who attrit based on comparisons of baseline characteristics of respondents who are followed up with those who are not.

[5] The models also control age, gender and location of residence in 1993.

Our results indicate that in the case of IFLS, some of the characteristics that affect attrition are not observed at baseline.

We can explore this further by exploiting an unusual feature of IFLS. The IFLS collects information about the earnings of each person who was living in the original household at the last interview, even if the person is no longer a household member. These people account for about 45% of those not interviewed in 2007, but note that they are not randomly drawn from non-respondents and that there are legitimate questions about the quality of proxy responses about people who are not co-resident. (See, for example, Rosenzweig, 2003.)

With these concerns in mind, we have experimented with expanding the sample to include these proxy reports. Results for the growth regressions for this expanded sample are reported in panel B of Table 4. The return to education in these models is smaller for movers and the difference in the return to education between movers and stayers is also smaller. In both cases, the estimates are significant at a 10% size of test. The proxy reports apparently ameliorate the extent of selectivity of non responses, suggesting the collection of information about former household members by proxy may be useful. However, the proxy reports cannot fully address the problem that those for whom no information was collected are selected on characteristics observed at baseline as well as characteristics that were not and possibly could not have been observed at that time.

The evidence thus far suggests that selection of attriters is unlikely to be ignorable. We turn next to survey design features that may help reduce attrition and possibly ameliorate the impact of attrition on interpretation of behavioral models.

*Survey characteristics and subsequent interview outcomes*

Successful completion of an interview in a survey is the outcome of a complex interaction between enumerators, people who know the respondent and can provide information that leads to contacting the respondent, and ultimately communication with the respondent. Thus far, we have focused on the characteristics of respondents that are predictive of interview outcomes. We turn next to the characteristics of the survey and the enumerators.

Many of the factors likely to be important for success in the field are not amenable to empirical investigation in the context of a single longitudinal survey because the factors are either difficult to measure and/or are effectively fixed across the waves of the survey. These factors include, for example, the design of the survey, the design and implementation of protocols for tracking and follow-up, the quality of leadership overseeing the study, field worker training and supervision, how

well the interviewers are able to sell the study to the respondents, support provided to enumerators to help them complete their work, development and deployment of timely information and tools that aid the field workers locate and interview respondents. Other factors that are hard to measure include building rapport with respondents and others in the community, and the enthusiasm, commitment, persistence and work ethic of the interviewers.

The extent to which survey characteristics that we have measured are predictive of interview outcomes in subsequent surveys is explored in Table 5. We use survey characteristics measured in the first follow-up of IFLS respondents in 1997 because one of our innovations in that wave was a Survey of Surveyors which measured an array of markers of human capital potentially related to success in the field. The Survey of Surveyors was completed by each enumerator when the fieldwork was completed. It was not conducted in 1993. We examine whether survey characteristics in 1997 are predictive of interview outcomes ten years later in the 2007 survey. Respondents who were not interviewed in 1997 are excluded from these analyses.

There are at least two mechanisms through which a good enumerator will have an influence on successfully interviewing the same respondent in subsequent survey waves. First, the best enumerators develop good rapport with each respondent, convey a sense of empathy towards the respondent, and build trust with the respondent. This rapport should facilitate future contacts with the same respondent, including reducing the probability the respondent will refuse to participate in subsequent rounds. Second, the quality of information the enumerator collects is likely to be higher as the quality of the enumerator increases. As discussed above, it is difficult to overstate the importance of detailed and accurate information about potential future locations and people to contact in order to re-locate the respondent.

Table 5a reports results from multivariate regression models that include information about the interviewers and interviews (in panels A and B). Whether the respondent was interviewed in 2007 is the outcome in a logistic model reported in panel A. Odds ratios in column A1 indicate the odds that a person was interviewed relative to not being interviewed for each of the covariates. The second model (panel B) is a multinomial logit with five outcomes: interviewed in the same desa in 1993 and 2007 (the excluded category), interviewed outside the desa (“movers”), those who refused, those who were not found but proxy information was collected from a 1993 household member and those for whom no information was collected. Columns B1-B4 report risk ratios relative to the excluded category of being interviewed in the original desa. The models in panels A and B include

only characteristics of the interviewers and the interviews and provide insights into the characteristics of those enumerators who are likely to be more successful in the field. In order to provide insights in the marginal impact of the enumerators, the same models have been re-estimated with an extended set of covariates that include team fixed effects (to capture location and supervision effects) as well as characteristics of respondents. Those estimates are reported in Columns C and D in Table 5a (for interviewer and interview characteristics) and Table 5b for respondent characteristics.

Markers of the human capital of the enumerators are included in the first block of covariates in Table 5a. Prior survey experience is a powerful predictor of success in subsequent rounds. Respondents who were interviewed by experienced enumerators in 1997 were 65% more likely to be interviewed in 2007 than respondents who had been interviewed by an enumerator with no survey experience in 1997. Looking across the columns in the multinomial logistic model, [B], respondents who were interviewed by more experienced fieldworkers are much less likely to refuse in subsequent rounds. They are also less likely to be lost to follow-up which possibly reflects the benefits of better information recorded on future contacts in prior rounds.

Interviewers are not randomly assigned to teams, field sites and respondents. On the one hand, more experienced interviewers were more likely to work in areas that were more challenging and to be assigned to more difficult cases.[6] On the other hand, teams were balanced so that more experienced interviewers could help their less experienced teammates. In panel II of the table, the models include the same set of respondent characteristics explored in Tables 2 and 3 as well as team fixed effects. The latter capture both the role of supervisors as well as heterogeneity in interview outcomes by geography since each team was assigned to a province or to part of a province.

After controlling these characteristics, survey experience continues to count with lower rates of subsequent refusal and loss of the entire household to follow-up. If the survey was the first job the interviewer ever had, tracking in later waves is less successful although this effect does not persist after controlling respondent and team effects.

All enumerators were paid the same amount in IFLS. In the Survey of Surveyors, we asked each interviewer about the income earned in the prior job (for those who had worked before). Higher income in the prior job is associated with low rates of successful interviews in subsequent surveys. Looking across the models, this is driven by elevated probabilities of refusals in subsequent rounds (which is significant in models without controls) and less successful tracking in 2007 (as indicated by

[6] In some provinces, there was only one team and the interviewers were selected to work there because they spoke the local language.

a lower probability of being interviewed outside the desa and a higher probability of interviewing an original household member but not the target respondent).

About three-quarters of the enumerators had a bachelor's degree prior to working on IFLS. Around 60% of the enumerators planned to return to school. Whereas a bachelor's degree is not predictive of subsequent interview success, respondents interviewed by an enumerator who planned to return to school were less likely to be interviewed in 2007 – because they (and everyone else in their household) was less likely to be found in 2007. This suggests that the quality of the information on re-contact was poorer among those enumerators who saw IFLS as a temporary job between episodes at school rather than a longer-term commitment.

As part of the Survey of Surveyors, each enumerator completed a 30 item test that covered high school and college mathematics skills. According to this marker, enumerators with more human capital are more successful but this advantage disappears when the models include respondent and team controls.

We turn next to "non cognitive" skills or personality traits of interviewers and focus on three traits that are potentially important: being assertive, organized and careful. Each enumerator was asked to think about the statement "I am [trait]" and rate him or herself on a ten point scale ranging from strongly disagree (1) to strongly agree (10). Being assertive has a modest impact on successful tracking in subsequent rounds although this effect is not significant after controlling respondent characteristics. Organized fieldworkers are an advantage although not because of tracking but possibly better information about households that moved. Careful fieldworkers seem to collect information necessary to reduce the probability that an entire household is lost to follow-up.

The last set of survey characteristics included in the model pertains to the characteristics of the respondent and interactions between the respondent and enumerator. First, at the end of every interview, the enumerator rated his or her impression of the accuracy of the respondent's answers. We created an indicator variable for the 8% of respondents whose accuracy was thought to be "good". This is another powerful predictor of subsequent interview success, particularly for respondents who were interviewed outside the original desa in 2007. These respondents are also much less likely to refuse in subsequent surveys (although the effect is statistically significant at only 10%).

IFLS is a complex instrument that involves administering an interview to every adult household member. The average adult interview lasted about an hour. Respondents who completed

longer interviews in 1997 are significantly more likely to be re-interviewed in 2007. Recall that similar results were reported for the NLSY, SIPP and PSID in the U.S.

There are several potential mechanisms that might explain the association between length of interview and subsequent interview success in IFLS. For example, it is possible that older respondents take longer because, for example, they have more events to recall in each of the domains of their life histories they are asked about, they may take longer to recall because they are older, and, as noted above, older people are more likely to be re-interviewed in subsequent rounds. As shown in panel D of the table, this is not the reason. After controlling age and other respondent characteristics along with team effects, a longer interview in 1997 results in a significantly lower probability that no one from the origin household is re-interviewed in 2007. (This result persists if the sample is restricted to older respondents.) Further, there is no association between length of interview and subsequent refusal. We conclude that the evidence suggests that the breadth of information and detail collected in a longer interview assists field workers to track movers in subsequent rounds and this is the primary route through which interview length affects subsequent attrition from the survey.

This is consistent with our experience in the field and underlies the attention we pay to arming each fieldworker with the information they need to successfully track respondents. In addition to contacts on the contact list, family, neighbors and community leaders, we have obtained contact information that has led to successful tracking from other workers at the same place of employment, workers at the post office, health centers used by the respondent and schools that their children have attended.

The final panel in Table 5a reports $\chi^2$ tests for the joint significance of indicators of interviewer human capital (in the first row), those characteristics and the non-cognitive self-assessments (in the second row) and all the interview characteristics (in the third row). In models without respondent and team controls, the interviewer characteristics and the interview characteristics are all jointly significant. When controls are added, interviewer human capital is a significant predictor of whether or not an interview is completed at a 10% size of test. When interview characteristics are added, the covariates are all jointly significant at a 2% size of test. The characteristics can explain a significant fraction of the entire households that are lost to follow-up and also the respondents who are interviewed outside the desa indicating that these characteristics are useful for predicting movers and those that are hard to find. We are unable to explain refusals or those about whom information is only collected from a member of the 1993 household.

In addition to identifying the characteristics of enumerators that are associated with lower attrition in later surveys, the enumerator characteristics are potentially useful instruments for taking attrition and non-response into account in models that seek to explain respondent behavior. These might include, for example, models of education attainment, labor market success or migration decisions. To the extent that the interviewer's assessment of the accuracy of the respondent and the length of the interview in *prior* waves are valid instruments in these behavioral models, this information may help identify impacts of interest.[7]

Table 3 provides a simple way to draw comparisons between respondents interviewed in different locations and also between them and those not interviewed. Table 5b presents a multivariate analogue of the comparison in Table 3 while also controlling the interview and interview characteristics in columns C and D of Table 5a.

We summarize the main results from Table 5. Males are less likely to be interviewed than females because males are more likely to move away from the origin desa. Similarly, better educated respondents and those whose mothers are better educated are more likely to move away and thus less likely to be interviewed. This effect is exacerbated by the fact that these people are also more likely to refuse to participate. Neither fathers's education nor height is significantly related to interview outcomes except that taller people are significantly more likely to refuse. Respondents who moved before age 12 are much less likely to be interviewed – and, if they are interviewed, they are more likely to be tracked outside the desa. Migration before age 12 is the best predictor that an entire household will be lost to follow-up suggesting that these people warrant special attention to reduce attrition. In contrast, married respondents and respondents who were working at baseline are more likely to be re-interviewed because they are the least likely to stray from the original desa. Follow-up rates rise with age, again because migration rates decline with age. (The excluded category is those respondents born between 1975 and 1978.)

Turning to household characteristics, respondents from higher PCE households are less likely to be interviewed not only because the respondent is more likely to move from the original desa but the entire household is more likely to be lost to follow-up. This latter effect is mitigated as household size increases and if the household owns a farm business. Relative to respondents in urban households, those living in rural areas are more likely to be re-interviewed, less like to move and

[7] One would not want to use interview or interviewer characteristics in the same wave as an instrument because they would presumably also affect the quality and range of answers to questions in that wave, the dependent variable in the second stage regression.

their entire households are less likely to be lost to follow up. The differences between urban and city dwellers is small; the latter are more likely to be found outside the desa which largely reflects the fact that desa distinctions are less meaningful in large cities.

Taken together, the respondent characteristics are significant predictors of interview outcomes. Similarly, team fixed effects are powerful predictors of outcomes, particularly refusals and loss of entire households. While, in part, this reflects geographic heterogeneity, it is not the entire story. Restricting attention to the provinces that had more than one team, controlling province effects, the team effects continue to be significant in the multinomial models reflecting variation in follow-up success and refusal rates. We suspect that at least some of these differences can be attributed to team dynamics, team quality and team supervision.

## 4. CONCLUSIONS

Fourteen years after the baseline, attrition in IFLS remains low. It is not, however, ignorable in Rubin's sense. Not only are those who attrit different from those who are re-interviewed, but non-respondents are not all the same. It is important to at least distinguish between those who refuse to participate and those who are lost to follow-up. We have also pointed out differences between respondents for whom proxy information is collected from a member of the household in which the respondent was living at baseline and respondents who were lost to follow-up along with all other members of the baseline household.

Furthermore, there are systematic differences among those who are re-interviewed. Those interviewed in the same location at baseline and in the follow-up are different from those who have moved away from the origin location. Distinguishing movers by, roughly speaking, the distance they move from the baseline location, we present evidence that not only does failure to track movers result in higher rates of attrition but that the selectivity of those who attrit is linked to the distance they move from the baseline location. Movers are not only different in many dimensions that are observed at baseline but they also differ in ways that are not observed in the IFLS baseline, in spite of the richness of that survey. This may be because it is difficult to measure some factors that are likely to be related to migration choices, like ambition and willingness to embrace uncertainty in the future. It is likely that it also reflects the influence of changes in the life of a respondent that occur after the baseline which affect decisions about moving. These influences are likely to become more important

as the hiatus between the baseline and follow-up increases and in studies of respondents who were younger at baseline.

We conclude that the full potential of longitudinal studies in developing countries is unlikely to be realized if surveys are not designed to follow movers. However, even the best surveys suffer from attrition. Our analysis of data from a Survey of Surveyors that we conducted in the first follow-up of IFLS in 1997 establishes that interview outcomes ten years later in 2007 depend not only on observed characteristics of respondents but also the interview in 1997 and characteristics of the interviewers. These results suggest additional strategies that might be adopted to reduce attrition in the field as well as potential instruments that might assist in reducing the impact of attrition in models of behavior.

What are the costs of tracking movers? In general, the answer will depend on the nature of the study. Tracking is less expensive in telephone interviews and web-based interviews relative to face-to-face interviews. The costs of tracking will be larger when the baseline is conducted in a geographically concentrated area but movers migrate in all directions. The costs of tracking can be reduced by integrating tracking and follow-up into the project from the outset. Costs in IFLS were kept manageable by prudent selection and good training of fieldworkers in conjunction with thorough supervision, simultaneously visiting each of the baseline enumeration areas and conducting local tracking, fully exploiting all the information collected in prior waves to reduce the costs of re-locating respondents and following up on leads while they are still hot by exploiting the advantages of maintaining information on tracking cases in real time. It is, therefore, difficult to estimate the marginal costs of tracking and impossible to separate local tracking from re-interviewing respondents who do not move. That said, our best guess is that, on average, longer-distance tracking cases cost about 20% more than the average cost of other cases. In our view, this cost-benefit calculation overwhelmingly favors following movers in longitudinal surveys in developing countries.

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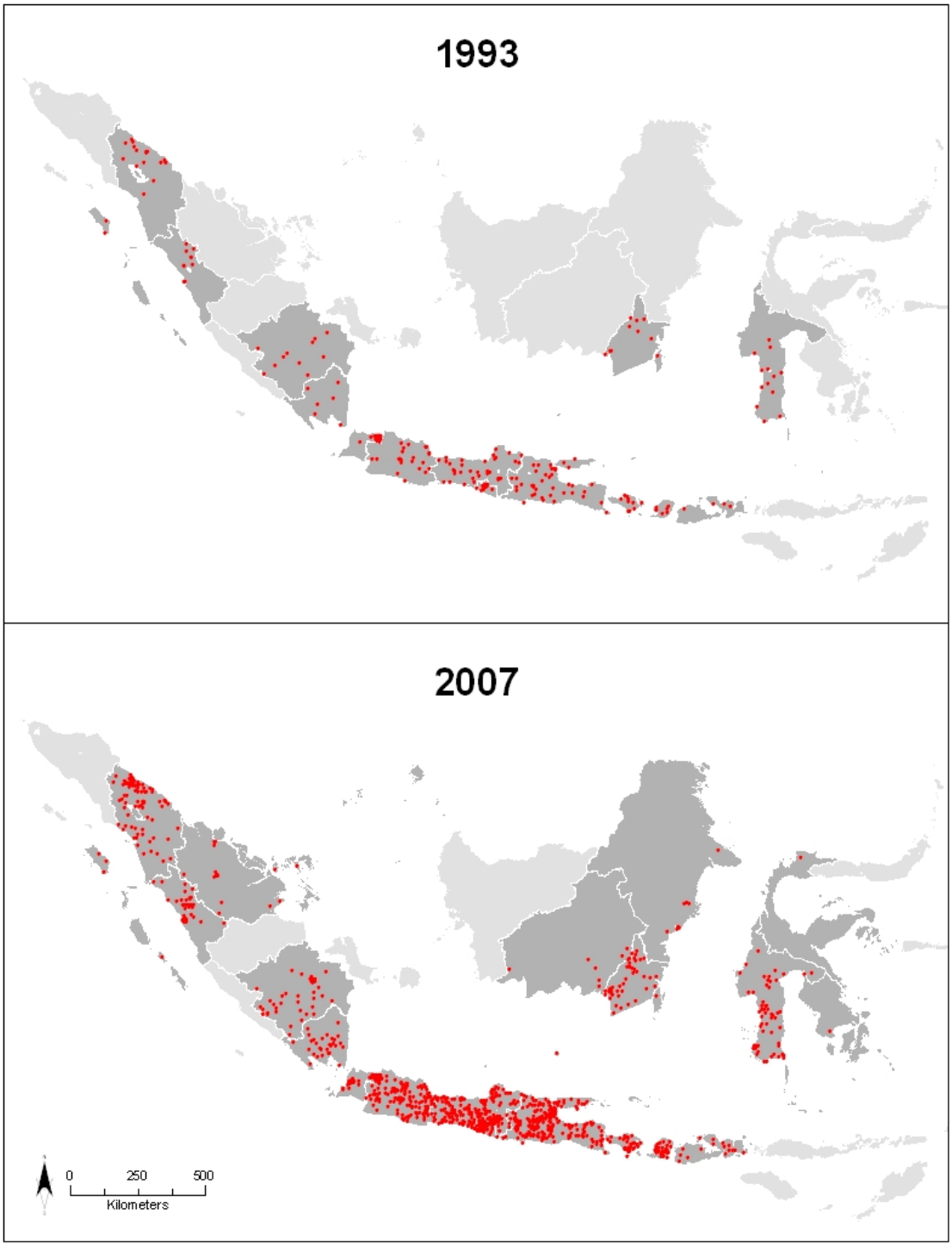
Figure 1 Location of respondents in 1993 and 2007 waves of IFLS




Note: Each dot represents a kecamatan in which at least one respondent was interviewed. The 1993 sample spanned 13 provinces. Provinces not visited are colored lighter grey.

**Table 1**: IFLS Completion rates: Individual respondents

| | A. IFLS1 1993 | | B. IFLS2 1997 | | C. IFLS3 2000 | | D. IFLS4 2007 | |
|---|---|---|---|---|---|---|---|---|
| | # | % | # | % | # | % | # | % |
| 01. Eligible for survey | | | 33,081 | | 39,601 [3] | | 44,915 | |
| 02. Died between the waves | | | 854 | | 790 | | 2,610 | |
| 03. Eligible for survey \| alive at survey date *(row 01-row 02)* | 33,081 [1] | | 32,227 | | 38,811 | | 42,305 | |
| 04. Assessed *(%=row 04/row 03)* | | | 26,948 | *83.6%* | 32,586 | *84.0%* | 32,636 | *77.1%* |
| 05. Eligible to be tracked and contacted of whom | 22,588 [2] | | 23,049 | | 32,189 | | 32,757 | |
| 06. Interview conducted | 22,019 | *97.5%* | 21,073 | *91.4%* | 29,440 | *91.5%* | 28,351 | *86.5%* |
| 07. Refused | 569 | *2.5%* | 244 | *1.1%* | 261 | *0.8%* | 367 | *1.1%* |
| 08. No interview conducted | | | 1,732 | *7.5%* | 2,488 | *7.7%* | 4,039 | *12.3%* |
| 09. New entrants in this wave | - | | 5,404 | | 6,104 | | 12,096 | |
| 10. Total sample interviewed this wave *(row 04+row 09)* | 22,019 | | 32,352 | | 38,690 | | 44,732 | |
| 11. Total potential sample for next wave *(row 03+row 09)* | 33,081 | | 37,631 | | 44,915 | | 54,401 | |

Notes: [1] Respondents included in 1993 baseline. [2] Respondents selected for individual interview in 1993.

[3] There are 1,970 new entrants in 2000 from a special 1998 survey of a sub-sample of respondents

**Table 2**: 2007 interview outcomes for respondents
who were interviewed in 1993 and eligible to be tracked in 2007

| **Panel A: Birth cohort** | Born before 1979 | | | | Born after 1979 | | | |
|---|---|---|---|---|---|---|---|---|
| | # resp | % | # resp | % | # resp | % | # resp | % |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| **1. Eligible to be tracked** | 16,510 | | | | 10,982 | | | |
| **2. Interview conducted** | 14,486 | *88* | | | 8,989 | *82* | | |
| *Location of interview* | | | | | | | | |
| a. same desa as 1993 | | | 11,468 | *69.5* | | | 6,040 | *55.0* |
| b. same kecamatan as 1993 | | | 733 | *4.4* | | | 546 | *5.0* |
| c. same kabupaten as 1993 | | | 812 | *4.9* | | | 722 | *6.6* |
| d. same province as 1993 | | | 828 | *5.0* | | | 903 | *8.2* |
| e. different province | | | 645 | *3.9* | | | 778 | *7.1* |
| **3. No interview conducted** | 2,024 | *12* | | | 1,993 | *18* | | |
| *Reason not interviewed* | | | | | | | | |
| a. refused | | | 226 | *1.4* | | | 103 | *0.9* |
| b. interview other HH mem | | | 899 | *5.4* | | | 419 | *3.8* |
| c. no contact w/ any HH mem | | | 899 | *5.4* | | | 1,471 | *13.4* |

| **Panel B: Born before 1979** | Gender | | Birth cohort | | | 1993 location | | |
|---|---|---|---|---|---|---|---|---|
| **Demographic characteristics** | Male | Female | <1958 | 1959-68 | 1969-78 | Rural | Urban | City |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] |
| *Of those eligible to be tracked %* | | | | | | | | |
| **1. Interview conducted** | *87* | *89* | *91* | *88* | *82* | *93* | *83* | *81* |
| *Location of interview* | | | | | | | | |
| a. same desa as 1993 | *67* | *72* | *80* | *69* | *54* | *80* | *61* | *53* |
| b. same kecamatan as 1993 | *5* | *4* | *3* | *5* | *6* | *4* | *5* | *5* |
| c. same kabupaten as 1993 | *5* | *5* | *3* | *6* | *7* | *4* | *5* | *8* |
| d. same province as 1993 | *5* | *5* | *3* | *4* | *8* | *3* | *6* | *10* |
| e. different province | *4* | *4* | *2* | *4* | *6* | *2* | *5* | *5* |
| **2. No interview conducted** | | | | | | | | |
| *Reason not interviewed* | *13* | *11* | *9* | *12* | *18* | *7* | *17* | *19* |
| a. refused | *1* | *1* | *1* | *1* | *1* | *0* | *2* | *2* |
| b. interview other HH mem | *6* | *5* | *5* | *6* | *5* | *2* | *8* | *10* |
| c. no contact w/ any HH mem | *6* | *5* | *2* | *4* | *12* | *4* | *6* | *7* |

**Table 3:** Characteristics of respondents at 1993 baseline and interview outcomes in 2007 survey wave

| Characteristics of respondents at baseline in 1993 | Interview outcome in 2007 | | | Resp ivw in same desa in 1993 & 2007 | Difference: Relative to interview in same desa (col 4) | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | Interviewed in | | | | Not interviewed because | | |
| | Resp inter-viewed | Resp not ivw | Difference (not ivw-interview) | | Same kecamatan | Same kabuparten | Same province | Other province | Resp refused | Found only oth HH mem | No contact w/ orig HH |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] | [10] | [11] |
| *Individual characteristics* | | | | | | | | | | | |
| Education (yrs) | 5.96 | 7.97 | 2.01 | 5.44 | 1.74 | 2.37 | 2.83 | 2.67 | 3.45 | 2.32 | 2.53 |
| Respondent | [0.13] | [0.20] | [0.17] | [0.12] | [0.20] | [0.19] | [0.22] | [0.23] | [0.50] | [0.15] | [0.29] |
| Father | 2.75 | 4.36 | 1.61 | 2.32 | 1.55 | 2.03 | 2.62 | 2.28 | 2.37 | 2.33 | 1.71 |
| | [0.09] | [0.19] | [0.17] | [0.08] | [0.20] | [0.19] | [0.24] | [0.27] | [0.43] | [0.23] | [0.26] |
| Mother | 1.92 | 3.39 | 1.48 | 1.56 | 1.06 | 1.58 | 2.13 | 2.11 | 2.65 | 1.99 | 1.48 |
| | [0.08] | [0.18] | [0.16] | [0.07] | [0.15] | [0.16] | [0.19] | [0.22] | [0.43] | [0.20] | [0.24] |
| Height (cms) | 153.88 | 155.08 | 1.20 | 153.74 | 0.91 | 0.89 | 0.86 | 0.64 | 3.98 | -0.21 | 1.79 |
| | [0.12] | [0.28] | [0.27] | [0.13] | [0.45] | [0.41] | [0.44] | [0.49] | [0.67] | [0.51] | [0.35] |
| % moved | 51.60 | 75.04 | 23.45 | 47.55 | 22.15 | 23.88 | 28.93 | 27.31 | 23.52 | 12.94 | 35.26 |
| by age 12 | [1.41] | [1.95] | [1.96] | [1.43] | [2.66] | [2.66] | [2.73] | [2.85] | [5.08] | [3.29] | [2.38] |
| % married | 87.74 | 75.36 | -12.38 | 88.82 | -2.74 | -4.94 | -9.76 | -9.52 | -3.82 | -18.59 | -12.64 |
| | [0.40] | [1.75] | [1.69] | [0.40] | [1.90] | [1.82] | [2.21] | [2.81] | [3.41] | [2.63] | [2.32] |
| % working | 71.48 | 63.77 | -7.71 | 72.50 | -4.10 | -5.31 | -6.66 | -9.53 | -8.01 | -7.83 | -9.32 |
| for pay | [0.81] | [1.57] | [1.59] | [0.86] | [2.21] | [2.06] | [2.39] | [2.71] | [4.18] | [2.64] | [2.00] |
| ln(monthly | 10.99 | 11.68 | 0.69 | 10.90 | 0.38 | 0.49 | 0.62 | 0.66 | 1.14 | 0.46 | 0.86 |
| earnings) | [0.05] | [0.08] | [0.08] | [0.05] | [0.11] | [0.10] | [0.13] | [0.13] | [0.17] | [0.11] | [0.10] |
| *Household characteristics* | | | | | | | | | | | |
| % HHs own | 40.76 | 23.53 | -17.23 | 44.69 | -13.16 | -15.59 | -22.83 | -21.38 | -30.84 | -10.64 | -30.28 |
| farm bus | [2.09] | [2.43] | [2.01] | [2.13] | [2.79] | [2.19] | [2.27] | [2.77] | [5.78] | [2.03] | [3.02] |
| % HHs own | 36.03 | 37.39 | 1.35 | 36.11 | -0.46 | 0.03 | -0.09 | -1.08 | 10.21 | 0.99 | -0.62 |
| non farm bus | [1.08] | [1.77] | [1.68] | [1.14] | [2.52] | [2.36] | [2.57] | [2.85] | [7.02] | [1.87] | [2.74] |
| ln(per capita | 10.74 | 11.16 | 0.42 | 10.69 | 0.11 | 0.19 | 0.29 | 0.44 | 0.71 | 0.22 | 0.69 |
| expenditure) | [0.03] | [0.05] | [0.04] | [0.03] | [0.04] | [0.04] | [0.04] | [0.06] | [0.14] | [0.04] | [0.06] |
| Household | 5.39 | 5.36 | -0.03 | 5.34 | 0.30 | 0.23 | 0.31 | 0.18 | 0.96 | 0.45 | -0.68 |
| size | [0.05] | [0.12] | [0.12] | [0.05] | [0.13] | [0.12] | [0.12] | [0.14] | [0.41] | [0.10] | [0.18] |

Notes: [Standard errors] of means take into account clustering of sample. Sample includes target respondents born before 1979.

**Table 4:** Earnings and education of movers and stayers
in 1993, 2007 and change in earnings over this period

Models estimated separately for respondents who were interviewed in same desa in 1993 and 2007 and respondents who had moved from the 1993 desa by 2007.

| Sample/ Dependent variable | Location of interview in 2007 | | |
|---|---|---|---|
| | Same desa | Other desa | Difference |
| | [1] | [2] | [3] |
| A. Respondents interviewed in 1993 and 2007 | | | |
| *ln(monthly earnings)* | | | |
| Measured in 1993 | 11.3 | 9.5 | -1.8 |
| | [0.6] | [0.7] | [0.8] |
| Measured in 2007 | 9.0 | 10.5 | 1.5 |
| | [0.5] | [0.6] | [0.7] |
| *Change in ln (monthly earnings)* | | | |
| (2007-1993) | -0.2 | 3.1 | 3.3 |
| | [0.7] | [1.3] | [1.4] |
| B. Add non-resident HH members for whom income obtained by proxy in 2007 | | | |
| *Change in ln (monthly earnings)* | | | |
| (2007-1993) | -0.2 | 2.1 | 2.3 |
| | [0.7] | [1.1] | [1.3] |

Notes: Dependent variable is ln(monthly earnings) measured in 1993 and 2007 for the same respondents
Estimate of association with years of completed schooling (measured at baseline) reported in table.
Alll models also control age, gender and location of residence in 1993. [Standard errors] in parentheses take into account clustering.

**Table 5a:** Interview outcomes in 2007 and characteristics of interviewer and interview

| Model: | I. No other controls | | | | | II. Controlling respondent characteristics in 1993 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Outcome in 2007 | Interviewed in 2007 | Ivw outside desa | Refuse | Ivw mem of 1993 HH | No info collected | Interviewed in 2007 | Ivw outside desa | Refuse | Ivw mem of 1993 HH | No info collected |
| *Reference outcome:* | *Not ivwd* | *Interviewed in original desa* | | | | *Not ivwd* | *Interviewed in original desa* | | | |
| | [A1] | [B1] | [B2] | [B3] | [B4] | [C1] | [D1] | [D2] | [D3] | [D4] |
| (1) interviewer | | | | | | | | | | |
| has survey experience | 1.65 | 0.84 | 0.26 | 0.69 | 0.59 | 1.24 | 0.87 | 0.42 | 0.95 | 0.69 |
| | [4.7] | [1.7] | [3.0] | [3.1] | [3.0] | [1.9] | [1.6] | [1.9] | [0.4] | [1.9] |
| first job | 1.13 | 0.82 | 0.73 | 0.86 | 0.87 | 1.03 | 0.90 | 0.60 | 1.02 | 0.90 |
| | [1.3] | [2.4] | [0.8] | [1.3] | [0.9] | [0.3] | [1.3] | [1.1] | [0.2] | [0.7] |
| ln(income in last job) | 0.77 | 0.91 | 1.85 | 1.40 | 1.03 | 0.86 | 0.89 | 1.37 | 1.26 | 1.00 |
| (if had job) | [2.9] | [1.5] | [2.4] | [3.6] | [0.2] | [1.7] | [2.0] | [1.2] | [1.9] | [0.0] |
| has bachelor's degree | 1.14 | 0.92 | 0.83 | 1.08 | 0.67 | 1.11 | 1.03 | 0.61 | 1.14 | 0.85 |
| | [1.0] | [0.8] | [0.5] | [0.5] | [1.7] | [0.9] | [0.3] | [1.3] | [0.6] | [0.9] |
| plans to return to school | 0.62 | 1.21 | 1.30 | 1.14 | 2.97 | 0.83 | 1.08 | 0.85 | 1.02 | 1.73 |
| | [4.9] | [2.4] | [0.9] | [1.1] | [6.1] | [1.9] | [1.1] | [0.5] | [0.2] | [3.5] |
| Math test | 1.83 | 0.63 | 0.71 | 0.34 | 0.70 | 0.70 | 1.14 | 2.70 | 1.00 | 1.94 |
| (Fraction correct) | [2.1] | [2.1] | [0.4] | [3.4] | [0.9] | [1.1] | [0.4] | [0.9] | [0.0] | [1.2] |
| Interviewer assessment | | | | | | | | | | |
| Assertive | 0.96 | 1.05 | 1.03 | 1.00 | 1.07 | 1.02 | 1.04 | 0.89 | 1.00 | 0.98 |
| | [1.5] | [2.0] | [0.3] | [0.0] | [1.5] | [0.5] | [1.4] | [1.2] | [0.0] | [0.4] |
| Organized | 1.08 | 0.90 | 0.93 | 1.00 | 0.87 | 1.00 | 0.93 | 1.01 | 1.04 | 0.95 |
| | [2.6] | [4.1] | [1.0] | [0.0] | [3.9] | [0.1] | [3.0] | [0.2] | [1.1] | [1.5] |
| Careful | 1.03 | 0.98 | 0.95 | 1.01 | 0.92 | 1.01 | 1.01 | 0.95 | 1.05 | 0.94 |
| | [1.5] | [1.2] | [0.9] | [0.5] | [2.3] | [0.7] | [0.6] | [0.8] | [2.2] | [1.9] |
| Interview characteristics | | | | | | | | | | |
| (1) respondent accurate | 1.47 | 1.37 | 0.17 | 0.86 | 0.76 | 1.45 | 1.21 | 0.17 | 0.83 | 0.77 |
| | [2.2] | [2.8] | [1.7] | [0.8] | [0.8] | [2.0] | [1.7] | [1.7] | [0.9] | [0.7] |
| Length of interview | 1.73 | 0.77 | 0.61 | 0.47 | 0.63 | 1.30 | 0.91 | 0.66 | 0.84 | 0.67 |
| | [5.8] | [3.7] | [1.4] | [5.7] | [3.5] | [2.7] | [1.2] | [1.1] | [1.3] | [2.6] |
| Joint significance ($\chi^2$) | | | | | | | | | | |
| Interviewer human capital | 60.6 | 24.9 | 11.3 | 37.2 | 60.2 | 11.1 | 10.8 | 6.8 | 4.0 | 22.9 |
| [p value] | [0.00] | [0.00] | [0.08] | [0.00] | [0.00] | [0.09] | [0.09] | [0.34] | [0.67] | [0.00] |
| All interviewer characs | 70.5 | 44.0 | 18.7 | 39.8 | 106.5 | 11.9 | 18.6 | 7.1 | 13.4 | 31.2 |
| [p value] | [0.00] | [0.00] | [0.03] | [0.00] | [0.00] | [0.22] | [0.03] | [0.62] | [0.15] | [0.00] |
| + interview characteristics | 97.5 | 69.1 | 24.4 | 66.9 | 118.8 | 22.9 | 24.2 | 12.0 | 14.9 | 42.4 |
| [p value] | [0.00] | [0.00] | [0.01] | [0.00] | [0.00] | [0.02] | [0.01] | [0.36] | [0.19] | [0.00] |

Notes: Odds ratios relative to not being interviewed from logistic regressions (in models A and C). Risk ratios (relative to being interviewed in original desa from multinomial logistic regressions (in models B and D). [t statistics] and [p values] in parentheses take into account clustering. Sample restricted to respondents who were interviewed in 1997 because interviewer characteristics were not measured in 1993. All other characteristics measured in 1993.

**Table 5b:** Interview outcomes in 2007 and characteristics of respondent
controlling characteristics of interviewer and interview

| Outcome in 2007 | Interviewed in 2007 | Ivw outside desa | Refuse | Ivw mem of 1993 HH | No info collected |
|---|---|---|---|---|---|
| *Reference outcome:* | *Not ivwd* | *Interviewed in original desa* | | | |
| | [C1] | [D1] | [D2] | [D3] | [D4] |
| *Respondent characteristics (in 1993)* | | | | | |
| (1) if male | 0.80 | 1.17 | 0.67 | 1.57 | 1.20 |
| | [2.5] | [2.3] | [1.5] | [3.4] | [1.2] |
| Education (yrs) | 0.96 | 1.04 | 1.11 | 1.06 | 1.01 |
| | [3.7] | [4.0] | [3.2] | [3.7] | [0.7] |
| Father's education | 1.01 | 1.01 | 0.95 | 1.00 | 1.01 |
| | [1.0] | [1.2] | [1.6] | [0.1] | [0.6] |
| Mother's education | 0.96 | 1.02 | 1.15 | 1.04 | 1.04 |
| | [2.7] | [1.6] | [3.0] | [2.1] | [1.5] |
| Height (cm) | 0.99 | 1.00 | 1.07 | 1.00 | 1.01 |
| | [1.5] | [0.0] | [3.4] | [0.5] | [1.2] |
| (1) resp moved before age 12 | 0.59 | 2.45 | 1.39 | 1.74 | 2.33 |
| | [4.9] | [10.5] | [1.1] | [3.4] | [4.7] |
| (1) resp married | 1.65 | 0.67 | 0.71 | 0.47 | 0.60 |
| | [3.8] | [3.7] | [0.8] | [3.8] | [2.7] |
| (1) respondent working | 1.43 | 0.84 | 0.59 | 0.74 | 0.68 |
| | [3.4] | [2.1] | [1.8] | [1.6] | [2.8] |
| (1) Born 1965-74 | 1.29 | 0.62 | 0.73 | 0.59 | 1.01 |
| | [2.1] | [5.0] | [1.0] | [3.9] | [0.1] |
| (1) Born 1955-64 | 1.75 | 0.40 | 1.03 | 0.29 | 0.84 |
| | [3.8] | [7.9] | [0.1] | [6.4] | [0.7] |
| (1) Born 1945-54 | 2.04 | 0.26 | 1.04 | 0.20 | 0.64 |
| | [4.3] | [10.0] | [0.1] | [6.9] | [1.5] |
| (1) Born before 1945 | 1.81 | 0.23 | 1.31 | 0.11 | 0.98 |
| | [3.2] | [10.0] | [0.7] | [8.0] | [0.1] |
| *Household characteristics (in 1993)* | | | | | |
| (1) HH had farm business | 1.25 | 0.64 | 0.54 | 0.91 | 0.53 |
| | [1.6] | [5.0] | [0.8] | [0.6] | [2.3] |
| (1) HH had non farm bus | 0.95 | 0.91 | 1.28 | 0.95 | 1.04 |
| | [0.5] | [1.2] | [0.7] | [0.4] | [0.2] |
| ln(HH per capita expenditure) | 0.77 | 1.18 | 1.78 | 1.16 | 1.47 |
| | [3.0] | [2.9] | [1.9] | [1.3] | [2.7] |
| HH size | 1.04 | 0.99 | 1.09 | 0.99 | 0.86 |
| | [1.4] | [0.9] | [1.3] | [0.2] | [3.0] |
| (1) resident in rural area | 1.38 | 0.66 | 0.28 | 0.83 | 0.54 |
| | [2.4] | [3.8] | [1.7] | [1.2] | [2.7] |
| (1) resident in major city | 0.93 | 1.48 | 1.24 | 1.28 | 1.18 |
| | [0.4] | [2.1] | [0.5] | [1.2] | [0.6] |
| Joint significance ($\chi^2$) | | | | | |
| Respondent characs | 272 | 770 | 164 | 413 | 168 |
| [p value] | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |
| Team fixed effects | 73 | 40 | 18511 | 65 | 7034 |
| [p value] | [0.00] | [0.00] | [0.00] | [0.00] | [0.00] |

Notes: See Table 5a. Odds ratios (model C) and relative risk ratios (model D) and [t statistics] in parentheses.
Joint tests report chi-squared statistics and [p values]. All variance-covariance matrices take clustering into account.
Respondent characteristics measured at baseline in 1993. All models include team fixed effects

**Appendix Table 1:** Adjusted differences in baseline characteristics of respondents by 2007 interview outcome

| Characteristics of respondents at baseline in 1993 | Difference: Interviewed - not ivwd | | Inter-viewed in same desa | Adjusted difference relative to resp interviewedin same desa (column 3) | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | | Interviewed in | | | | Not interviewed because | | |
| | Unadjusted (Tbl 3 Col 3) | Adjusted differences | | Same kecamatan | Same kabuparten | Same province | Other province | Resp refused | Found only oth HH mem | No contact w/ orig HH |
| | [1] | [2] | [3] | [4] | [5] | [6] | [7] | [8] | [9] | [10] |
| *Individual characteristics* | | | | | | | | | | |
| Education (yrs) | 2.01 | 0.75 | 5.44 | 0.64 | 1.04 | 1.26 | 0.85 | 2.04 | 0.75 | 1.10 |
| Respondent | [0.17] | [0.13] | [0.12] | [0.18] | [0.18] | [0.20] | [0.18] | [0.42] | [0.13] | [0.23] |
| Father | 1.61 | 0.46 | 2.32 | 0.40 | 0.72 | 1.09 | 0.54 | 1.26 | 0.56 | 0.62 |
| | [0.17] | [0.13] | [0.08] | [0.18] | [0.16] | [0.20] | [0.21] | [0.40] | [0.19] | [0.20] |
| Mother | 1.48 | 0.57 | 1.56 | 0.19 | 0.54 | 0.92 | 0.72 | 1.79 | 0.60 | 0.64 |
| | [0.16] | [0.12] | [0.07] | [0.14] | [0.14] | [0.15] | [0.18] | [0.41] | [0.16] | [0.20] |
| Height (cms) | 1.20 | 0.63 | 153.74 | 0.15 | 0.51 | 0.43 | 0.35 | 3.17 | -0.41 | 0.97 |
| | [0.27] | [0.25] | [0.13] | [0.36] | [0.31] | [0.40] | [0.46] | [0.63] | [0.45] | [0.32] |
| % married | -12.38 | -8.12 | 88.82 | -1.54 | -2.86 | -4.28 | -5.71 | -5.38 | -9.11 | -9.76 |
| | [1.69] | [1.36] | [0.40] | [1.73] | [1.68] | [1.78] | [2.09] | [2.89] | [2.22] | [1.88] |
| % moved | 23.45 | 14.94 | 47.55 | 16.22 | 19.53 | 23.28 | 17.19 | 10.97 | 13.15 | 24.48 |
| by age 12 | [1.96] | [1.76] | [1.43] | [2.52] | [2.74] | [2.82] | [2.66] | [4.63] | [3.00] | [2.27] |
| % working | -7.71 | -1.50 | 72.50 | -1.94 | -3.16 | -2.24 | -1.81 | -3.26 | -1.91 | -1.91 |
| for pay | [1.59] | [1.41] | [0.86] | [2.08] | [1.91] | [2.04] | [2.44] | [3.78] | [2.47] | [1.82] |
| ln(monthly | 0.69 | 0.27 | 10.90 | 0.18 | 0.27 | 0.30 | 0.09 | 0.60 | 0.23 | 0.31 |
| earnings) | [0.08] | [0.06] | [0.05] | [0.10] | [0.09] | [0.11] | [0.09] | [0.16] | [0.11] | [0.08] |
| *Household characteristics* | | | | | | | | | | |
| % HHs own | -17.23 | -3.71 | 44.69 | -7.98 | -5.26 | -9.40 | -5.85 | -5.70 | -3.03 | -8.95 |
| farm bus | [2.01] | [1.28] | [2.13] | [1.92] | [1.69] | [1.81] | [1.91] | [3.96] | [1.53] | [2.06] |
| % HHs own | 1.35 | 0.65 | 36.11 | -2.13 | -2.44 | -3.20 | -1.32 | 7.47 | 0.13 | -2.04 |
| non farm bus | [1.68] | [1.68] | [1.14] | [2.50] | [2.44] | [2.59] | [2.80] | [6.66] | [1.93] | [2.85] |
| ln(per capita | 0.42 | 0.22 | 10.69 | 0.09 | 0.11 | 0.19 | 0.16 | 0.36 | 0.16 | 0.36 |
| expenditure) | [0.04] | [0.03] | [0.03] | [0.03] | [0.03] | [0.04] | [0.04] | [0.13] | [0.03] | [0.05] |
| Household | -0.03 | -0.40 | 5.34 | -0.07 | -0.09 | -0.12 | -0.34 | 0.55 | -0.07 | -1.11 |
| size | [0.12] | [0.11] | [0.05] | [0.11] | [0.11] | [0.12] | [0.13] | [0.40] | [0.10] | [0.17] |

Notes: [Standard errors] take into account clustering of sample. Cols 4-10 are estimated differences relative to col 3 (ivw in same desa in 1993 and 2007). All models also control age, gender and location of residence in 1993.