ABSTRACT

Title of dissertation: UNDERSTANDING THE MECHANISM OF PANEL ATTRITION

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Nonresponse is of particular concern in longitudinal surveys (panels) for several reasons. Cumulative nonresponse over several waves can substantially reduce the proportion of the original sample that remains in the panel. Reduced sample size increases the variance of the estimates and reduces the possibility for subgroup analysis. Also, the higher the attrition, the greater the concern that error (bias) will arise in the survey estimates.

The fundamental purpose of most panel surveys is to allow analysts to estimate dynamic behavior. However, current research on attrition in panel surveys focuses on the characteristics of respondents at wave 1 to explain attrition in later waves, essentially ignoring the role of life events as determinants of panel attrition. If the dynamic behaviors that panel surveys are designed to examine are also prompting attrition, estimates of those behaviors and correlates of those behaviors may be biased. Also, current research on panel attrition generally does not differentiate between attrition through non-contacts and attrition through refusals. As these two source of nonresponse have been shown to have different determinants,

they can also be expected to have different impacts on data quality. The goal of this research is to examine these issues.

Data for this research comes from the Panel Survey of Income Dynamics (PSID) conducted by the University of Michigan. The PSID is an ongoing longitudinal survey that began in 1968 and with a focus on the core topics of income, employment, and health.

# UNDERSTANDING THE MECHANISM OF PANEL ATTRITION

by

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# Chapter 1

# Executive Summary

Sample survey data are used by researchers in many social sciences to study human behavior. Cross-sectional surveys are useful to describe populations and explore associations between variables. However, such cross-sectional designs are less appropriate for the exploration of causal relationships as they do not allow to tease out the temporal ordering of variables, an essential requirement of causal analysis. Increasingly, social and behavioral scientists rely on a special type of sample survey that follows the same sample units over a certain period of time. Keeping track of the temporal ordering is one of the key strengths of longitudinal surveys or panels. The collection of high-quality panel survey data is therefore crucial for the advancement of social sciences.

An important aspect of survey quality is the ability to secure participation from all sample units. For valid inference to be possible, statistical inference theory requires one of the following conditions: a one hundred percent response rate, data missing completely at random (MCAR) (Little and Rubin, 2002), or the elaboration of a statistical model that accounts for the exclusion of certain units in a non-random fashion. Neither of these requirements can easily be met and therefore the consequences of nonresponse are often a concern for survey researchers and data users. The main concern with nonresponse is the potential biasing impact it can have on survey estimates. When nonresponse is selective, that is when nonrespondents are systematically different than respondents, bias in the survey estimates will occur. Another concern is that the reduction in sample size will also increase the variance of survey estimates and result in a loss of statistical power which will then reduce the possibility for subgroup analysis.

Panel surveys are not immune to the nonresponse problem. In fact, nonresponse is even more of a concern in panel surveys than in cross-sectional surveys for two reasons. First, the cumulation of nonresponse over several waves can lower the panel size substantially. As in the cross-sectional setting, this loss of sample will result in an increase of variance. However, the consequence of this sample loss is far greater in panel surveys than in cross-sectional surveys. Because such great value is placed on the repeated measures collected, sample members who drop out or attrite from the survey cannot be easily replaced. This loss of panel members or attrition can therefore inflict great damage to panel surveys.

Second, if determinants of attrition are related to the topic of the survey, it will introduce bias in the estimates, just as it would in cross-sectional surveys. However, as panel studies often collect information on a broad range of topics, they pose a risk that some nonresponse determinant will be correlated with some of the survey estimates. Moreover, the fundamental purpose of most panel surveys is to allow analysts to estimate dynamic behavior (Binder, 1998; Kalton and Citro, 1993). If the dynamic behaviors that panel surveys are designed to examine are also prompting attrition, estimates of those behaviors and correlates of those behaviors may be biased.

This dissertation is composed of three papers that look at various aspect of attrition in the Panel Survey of Income Dynamics (PSID).

□ The first paper (chapter 2) is organized around the replication, critique and extension of previous results obtained by Fitzgerald et al. (1998) in their study of attrition in the PSID. Overall, the independent replication of their study has revealed results that closely matched those published in Fitzgerald et al. (1998).

Not surprisingly, the first paper also reveals that attrition has continued to erode the PSID sample after 1989 (wave 22), the last year covered by the Fitzgerald et al. (1998) study. By 2005 (wave 34), less than a third of the 18,191 original sample members that participated in the first wave remained in the sample. A substantial proportion of the attrition we observe is due to death, which is expected given the aging sample. However, other potentially more disturbing sources of attrition such as non-contacts and refusals continue to occur.

Current research on attrition in panel surveys often focuses on the characteristics of respondents measured at wave 1 to explain attrition in later waves, essentially ignoring the role of life events as determinants of panel attrition. From a substantive point of view, it is not very interesting, either for the purpose of understanding the mechanism of attrition or to get insights into ways to prevent attrition, to know that a given characteristic, measured decades earlier, is related to attrition. The first paper shows evidence that attrition modeling strategies relying on predictors measured at wave 1 can lead to puzzling results and thus calls for the use of more immediate measures of the circumstances of panel members that might be informative of attrition.

☐ The second paper (chapter 3) looks at how recent life events relate to attrition. This paper provides some evidence that individuals living in households with less stable trajectories are more likely to attrite. For example, individuals living in households without young children, and individuals living in households that do not own their home have been shown to be more likely to attrite. Likewise, households who own their home are likely to have stronger residential stability which makes wave on wave contact easier. Individuals who have joined the panel as non-adult sample members are also more likely to attrite, a reflection of the instability that these young sample members likely experience as they transition to adulthood. Finally, individuals who have lived in institutions are also more likely to attrite.

However, in order to unequivocally conclude that change produces attrition, which in turns undermines the estimates of change derived from panel studies, one would need information that is available independently from the sample members' participation in the panel. Data of this nature would allow us to establish whether or not events or status changes occurring *after* the last occurrence of participation but *before* the moment where nonresponse is recorded are actually related to attrition. Data of this sort would typically take the form of administrative data that are available for all sample members. This could be achieved by building a panel of program beneficiaries. An alternative, perhaps more widely applicable to a variety of existing panels, would be data linkage to external sources. Depending on the quality and variety of this external data, linkage would allow for the computation of estimates of change independent from the panel. This could provide "truer" determinants of attrition as well as some measures of bias.

The second paper also confirms the importance of distinguishing between noncontact and refusal in attrition models, as previously demonstrated by Lepkowski and Couper (2002). As these two sources of nonresponse have been shown to have different determinants, they can also be expected to have different impacts on data quality. To my knowledge, this is the first time that such a distinction has been made when studying attrition in the PSID. Prior attrition studies using the PSID have looked at attrition for any cause excluding death. As shown above, not distinguishing between the different sources of attrition often obscures the complex relationship between the correlates of attrition and the various sources of attrition. For example, home ownership has been shown to a have a positive effect on retention in the panel when looking at all sources of attrition combined. However, the competing-risk model that distinguishes between the different sources has revealed that this effect happens because homeowners are easier to contact not because they are more likely to agree to participate.

☐ Finally, the third paper (chapter 4) is focused on identifying potentially interesting groups of panel members that show distinct hazard curves. These distinct curves are thought to be an indication that multiple mechanisms of attrition concurrently play a role in the production of the overall hazard curve that is normally modeled in attrition studies such as in chapter 3.

Difficulties in estimation of the latent-class models make the results presented in this paper inconclusive. While there are some promising indications that the population is a mixture of different classes with respect to the processes of non-contact and refusal, the two-class models that I was able to successfully estimate fall short of expectations in a number of ways. The theoretical framework calls for three classes — stayers, early attriters and late attriters. Difficulties in estimation of the two-class models makes it unlikely for a three-class model to work although no attempt was made in this respect. Also, failure to successfully estimate a model where the effect of time-varying covariates is allowed to vary across classes makes it impossible to distinguish between the different mechanisms developed for this paper. This task required the inclusion of class-varying effects. The time-varying covariates — life events — were expected to have more impact on some classes than others depending on what mechanism is a better description of the data.

# Chapter 2

# Attrition in the Panel Study of Income Dynamics: Replication, Critique and Extension of an Influential Study[1]

## 2.1 Background

Panel survey data are used by researchers in many social sciences to test theories about human behavior and inform policy development (Burkhauser and Smeeding, 2001; Rose, 2000; Lazarsfeld, 1948). Panel surveys are a special type of sample survey designed to measure dynamic processes.[2] Ambitious projects are currently in preparation such as the UK Household Longitudinal Study (UKHLS)[3], an expanded version of the British Household Panel Study (BHPS) with the target of following 40,000 households, and the US National Children's Study (NCS)[4], a panel of 100,000 children who will be followed from their conception until age 21. Many more panels already exist[5] and have made possible a substantial number of

[1]This work would not have been possible without the exceptional collaboration of Dr. John Fitzgerald of Bowdoin College, who generously provided all data extraction and model estimation computer programs that were used in the Fitzgerald et al. (1998) study.

[2]Many design variations exist; see Kalton and Citro (1993); Binder (1998); Duncan and Kalton (1987); Kalton and Citro (2000) for an overview of the possibilities.

[3]http://www.iser.essex.ac.uk/ukhls

[4]http://www.nationalchildrensstudy.gov

[5]An extensive albeit non-exhaustive list of panel studies worldwide can be found here: http://www.iser.essex.ac.uk/keeptrack/index.php

contributions to the social sciences[6].

The collection of high-quality panel data is crucial for the advancement of knowledge in the social sciences. However, several factors such as measurement error and nonresponse error can threaten the quality of survey data (Groves, 1989). Nonresponse is the failure to measure all sample members. In panel surveys, failure to obtain an interview from all units in the initial wave is an important problem, but attrition, that is the cumulative and irreversible loss of sample members, is even more of a concern. Panels usually experience higher levels of attrition in the first few waves but attrition continues, albeit at a lower rate, throughout the duration of the panel (Wooden, 2001). This attrition is in addition to nonresponse in the initial wave. As in the cross-sectional setting, the loss of sample will result in an increase of variance, a decrease in power for subgroup analysis, as well as inflated costs of data collection. However, because such great value is placed on the repeated measures collected, sample members who drop out of the survey cannot be easily replaced. This loss of panel members or attrition[7] can therefore inflict great damage to panel surveys. Furthermore, if determinants of attrition are related to the estimates of interests, it will introduce bias in the estimates. This has been well documented in both cross-sectional (Groves, 2006; Abraham, Helms, and Presser, 2009) and longitudinal surveys (Little, 1995). However, as panel studies often col-

[6] A survey of the literature in the social, behavioral, and medical fields done in 2003 has returned 9799 published research articles using some form of panel data (Bernard, Lemay, and Vézina, 2004).

[7] The term attrition generally refers to "terminal nonresponse", i.e. a situation in which the sample member is permanently lost to follow-up or refuses to participate. It is to be distinguished from wave nonresponse, a situation in which a sample member skips one or several waves but then resumes participation in the study (Kalton, 1986; Kalton and Miller, 1986). Death and other forms of ineligibility are sometimes also referred to as attrition.

lect information on a broad range of topics, they pose a risk that some nonresponse determinant will be correlated with some of the survey estimates.

The attrition literature provides some evidence that younger people, African-Americans, males, renters and low income people have a lower probability to stay in the panel (Zabel, 1998; Rizzo, Kalton, and Brick, 1996; Lepkowski and Couper, 2002). Indicators of a negative survey experience as measured by the amount of item missing data, the time spent by the interviewer editing the form (Zabel, 1998), and reports by interviewers (Lepkowski and Couper, 2002) are correlated with the probability to attrite from the panel. Survey design features such as interview length, mode change, and interviewer change have all been studied as potential predictors of attrition but the results vary depending on whether random assignment of interviewers to cases was used or not. Interviewer change from one wave to another has been shown to have a negative impact on survey participation (Zabel, 1998), but when random assignment of interviewers is used (or when the analysis takes into account the fact that interviewer changes do not happen by mere accident as in Zabel (1998)), the relationship disappears (Campanelli and O'Muircheartaigh, 1999). Surprisingly, interview length has been shown to have a positive impact (Branden, Gritz, and Pergamit, 1995) on survey participation but the relationship seems to be the result of interviewers spending more time with people interested in the survey rather than the pure effect of interview length as an indicator of burden. Length was shown to have a negative impact on retention in the panel in Zabel (1998).

Attrition studies often seem to be based on the assumption that the process leading to attrition is essentially identical to the process leading to non-response in cross-sectional surveys. One manifestation of this is the use of characteristics of respondents at wave 1 to predict attrition later on in the survey. Examples of this approach can be found in several studies such as Campanelli and O'Muircheartaigh (1999), Rizzo et al. (1996), Lepkowski and Couper (2002) and, to some degree, Fitzgerald et al. (1998). While the information available in the first wave of a longitudinal survey is considerably richer than the frame data, thereby allowing richer analysis, the wave 1 approach falls short of accounting for the specificity and complexity of longitudinal surveys in a number of ways.

One important way longitudinal surveys differ from cross-sectional surveys is with respect to the nature of a sample member's relationship to the survey. Sample members are usually informed in the first wave of a panel that the survey they are asked to participate in is longitudinal and that their participation will be required again at a later time. The level of commitment asked from respondents is much higher in longitudinal surveys than in cross-sectional surveys and this might affect participation.

In addition, panel members have more information to evaluate the survey request when asked for participation in subsequent waves than they did in the first wave. They may remember whether they enjoyed the interview and they may even have prior experience with the interviewer. This is rather different than the request for participation in a cross-sectional survey where sample members have very few clues as to the real nature of the request other than the name of the organization conducting the survey and the survey topic.

Finally, in contrast to cross-sectional surveys, participation in a panel relies on consistency. Cross-sectional surveys typically require a one-time commitment from respondent whereas panel surveys will require much more. However, this requirement for consistency is at odds with the changing nature of individuals that panels are designed to measure. Very few individual characteristics and circumstances remain constant over time, with the notable exception of date of birth and, arguably, gender and ethnicity. The decision to cooperate in a panel is likely to be as dynamic and subject to a multitude of factors not known to the respondent or the researcher at the onset of the survey.

## 2.2 Fitzgerald et al. (1998): a case study

The present chapter is organized around the replication, critique and extension of one of the analyses conducted by Fitzgerald et al. (1998) in their study of attrition in the Panel Study of Income Dynamics (PSID), an ongoing panel survey with a focus on income, employment, and health dynamics. I had several reasons to focus on this part of the Fitzgerald et al. (1998) paper as a starting point. First of all, it is typical of the literature on panel surveys outlined above. Secondly, a substantial number of waves — seven yearly interviews between 1990 and 1996 and five biennial interviews between 1997 and 2005, for a total of 12 — have been released since 1989 (wave 22), the last year covered by the Fitzgerald et al. (1998) study. The Fitzgerald et al. (1998) study was, with Zabel (1998), among the last published studies of attrition in the PSID. Previously, Becketti, Gould, Lillard, and

Welch (1988), Lillard and Panis (1998) and Lillard (1989) had focused on attrition by wave 14 and had also found little evidence of bias. How the situation of attrition evolved since wave 23 is currently unknown. Lastly, the Fitzgerald et al. (1998) paper is often cited as evidence that attrition has not had a biasing impact in the PSID despite being related to a number of observable respondent characteristics (see Timberlake (2007); Wilhelm, Rooney, and Tempel (2007); Willson, Shuey, and Elder (2007); Hungerford (2007); Conley, Pfeiffer, and Velez (2007); Shin and Moon (2006); Hofferth (2006); van Hook, Brown, and Bean (2006) for recent examples), a rather intriguing finding.

The first step in extending the first analysis presented by Fitzgerald et al. (1998) is to replicate their results. Section 2.4 presents an overview of the process and the outcome of that replication. I will then use this baseline model to take into account the most recent wave of data available, as of June 2009. The results of the update are presented in section 2.5. I will then show, in section 2.6, the paradox introduced by the use of wave 1 covariates in modeling attrition. The chapter will conclude by a discussion of the findings.

## 2.3 PSID overview

In the mid-1960's, about 5000 US families were selected to be part of the core sample of the PSID. About half of these families came from an equal probability sample of households from the 48 contiguous states drawn by the Survey Research Center (SRC) at the University of Michigan and about half came from the Survey of Economic Opportunities (SEO), a survey of low-income families conducted by the US Census Bureau (Hill, 1992). The response rate to the initial wave was 77% for the SRC sample and 50.8% for the SEO sample (66.5% overall). The initial response rate translated into 18,191 core sample members participating in the first wave. These 18,191 individuals were distributed among 4802 distinct families.

Consistent with what has been observed in other panels (Wooden, 2001), the core sample of the PSID has experienced a higher rate of attrition between wave 1 and wave 2 than at any other time point. The first column of the first panel of table 2.1 shows, for each wave, the number of sample members whose participation has never lapsed. Larger numbers of core sample members are actually participating at each wave than what is displayed in table 2.1. By design, PSID allows panel members who have missed one or more waves to resume their participation in the panel, a phenomenon I chose to ignore for simplicity and to be consistent with Fitzgerald et al. (1998). To see the number of core sample members who were participating at each wave as well as the number returning from nonresponse, please see appendix A. The next columns in the first panel break down the number of participants according to whether they lived in a family unit (FU) or in an institution.

The first column of the second panel shows, for each wave, the number of sample members whose participation has lapsed. The next two columns breaks these number by reasons, i.e. death vs. any other. Overall, table 2.1 depicts a slow attrition process in the PSID. The cumulation of this relatively weak attrition over several waves has taken a toll on the core sample, however. As Fitzgerald et al. (1998) mentioned: "The PSID has suffered a large volume of attrition since it began

| | Always in | | | Ever out | | | |
|---|---|---|---|---|---|---|---|
| Wave | Total | In FU | In inst. | Total | Deaths | Other reasons | Dropped |
| 2 | 16,028 | 15,660 | 368 | 2163 | 84 | 2079 | 0 |
| 3 | 15,430 | 15,099 | 331 | 598 | 74 | 524 | 0 |
| 4 | 15,029 | 14,707 | 322 | 401 | 97 | 304 | 0 |
| 5 | 14,608 | 14,313 | 295 | 421 | 113 | 308 | 0 |
| 6 | 14,168 | 13,863 | 305 | 440 | 100 | 340 | 0 |
| 7 | 13,767 | 13,464 | 303 | 401 | 91 | 310 | 0 |
| 8 | 13,387 | 13,090 | 297 | 380 | 98 | 282 | 0 |
| 9 | 12,916 | 12,627 | 289 | 471 | 87 | 384 | 0 |
| 10 | 12,523 | 12,216 | 307 | 393 | 90 | 303 | 0 |
| 11 | 12,207 | 11,888 | 319 | 316 | 63 | 253 | 0 |
| 12 | 11,834 | 11,522 | 312 | 373 | 72 | 301 | 0 |
| 13 | 11,443 | 11,142 | 301 | 391 | 91 | 300 | 0 |
| 14 | 11,125 | 10,790 | 335 | 318 | 75 | 243 | 0 |
| 15 | 10,858 | 10,537 | 321 | 267 | 86 | 181 | 0 |
| 16 | 10,544 | 10,226 | 318 | 314 | 81 | 233 | 0 |
| 17 | 10,214 | 9,901 | 313 | 330 | 93 | 237 | 0 |
| 18 | 9,861 | 9,592 | 269 | 353 | 94 | 259 | 0 |
| 19 | 9,492 | 9,206 | 286 | 369 | 83 | 286 | 0 |
| 20 | 9,167 | 8,920 | 247 | 325 | 95 | 230 | 0 |
| 21 | 8,878 | 8,684 | 194 | 289 | 96 | 193 | 0 |
| 22 | 8,583 | 8,424 | 159 | 295 | 78 | 217 | 0 |
| 23 | 8,359 | 8,249 | 110 | 224 | 75 | 149 | 0 |
| 24 | 8,126 | 8,036 | 90 | 233 | 90 | 143 | 0 |
| 25 | 7,873 | 7,808 | 65 | 253 | 83 | 170 | 0 |
| 26 | 7,487 | 7,434 | 53 | 386 | 90 | 296 | 0 |
| 27 | 7,152 | 7,106 | 46 | 335 | 99 | 236 | 0 |
| 28 | 6,917 | 6,881 | 36 | 235 | 77 | 158 | 0 |
| 29 | 6,726 | 6,683 | 43 | 191 | 75 | 116 | 0 |
| 30 | 4,861 | 4,839 | 22 | 198 | 69 | 129 | 1667 |
| 31 | 4,581 | 4,557 | 24 | 280 | 154 | 126 | 0 |
| 32 | 4,332 | 4,313 | 19 | 249 | 152 | 97 | 0 |
| 33 | 4,112 | 4,093 | 19 | 220 | 138 | 82 | 0 |
| 34 | 3,940 | 3,916 | 24 | 172 | 106 | 66 | 0 |

Table 2.1: Number of original core sample members whose participation has never lapsed, overall, by residency status (family unit vs. institution), and type of nonresponse.

in 1968 [...] By [wave 22] the [PSID] had experienced approximately 50 percent sample loss from cumulative attrition from its initial [...] membership." The decline of the core PSID sample has continued after wave 22.

Despite of this relatively important attrition, 22% (n=3940) of the original core sample members have never let their participation in the study lapse as of wave 34, the most recent wave included in the present study. Moreover, a substantial proportion of the 14,251 core sample members that have ceased participation have not done so due to attrition, natural or otherwise. At wave 30, a subset of 1,667 low income members (9% of the original core sample) was dropped due to budget constraints. This is reflected in the sudden decrease in sample observable in the first column of table 2.1.[8] The way I have handled these special cases in the analyses is explained below.

The sample cut performed at wave 30 is only one of many design amendments that were adopted. Another element relevant to the description of the sample dynamics in the PSID is the recontact effort of all attriters that was initiated in wave 25. This recontact effort coincided with a change in the PSID follow-up rules which required that recontact efforts be stopped only in case of a strong refusal or the confirmation of the sample member's passing. Prior to this, no attempts were made to secure participation of panel members once they had missed two consecutive waves. This endeavor has led to an increase in sample size after wave 25 as sample mem-

[8]More people have actually been dropped. This is the number who have been dropped *among those who had never missed a wave* by wave 30. Some sample members were dropped *after* they had returned from a spell of nonresponse; these individuals are considered to be attriters in table 2.1. For a version of this table that accounts for sample members who return from nonresponse, please see appendix A.

bers who had stopped participating were convince to take part again. However, this increase is not reflected in table 2.1 as people who resume participation are still considered attriters according to the attrition definition used in this chapter.

The core sample members who have participated in the PSID in the most recent wave are only a fraction of the people actually taking part in the study. The PSID is an indefinite-life panel survey which means that a sample renewal procedure is in place to ensure the perenniality of the survey and help keep the sample representative of the population over time. By 2005, the PSID had collected information on some 68,000 individuals, an increase of more than 50,000 over the size of the original wave 1 core sample. According to the following rules, only a fraction of these 50,000 new "PSID individuals" are true new sample members, however.

This chapter is only concerned with core sample members (SEO and SRC samples) that were respondents in the initial wave (1968). Individuals who refused to take part in the initial wave are not included in the analyses. All individuals added to the PSID sample after wave 1 (this includes descendants of the core sample born after wave 1) are also excluded from the analyses. Further restrictions in the sample warranted by specific analysis will be described when appropriate. All analyses were conducted using the probit command in Stata® (StataCorp, 2007).

## 2.4 Replication of the Fitzgerald et al. (1998) Model

Consistent with the information provided in the Fitzgerald et al. (1998) paper, a distinction was made between four groups of adult core sample members (25 to

64 years old at wave 1): male heads of household, wives, female heads of household and other (which includes, among others, males that are not head of household and all household members aged less than 25). These distinctions were motivated, according to Fitzgerald et al. (1998), by the different level of detail in the information recorded for sample members belonging these various categories. For example, in the early waves of the PSID, male heads had more information recorded about them than wives. As a consequence, Fitzgerald et al. (1998) presented slightly different versions of their attrition models for each of these groups. For the purpose of this chapter, I will only focus on the model for the group of male heads of households aged 25-64 at wave 1 and ignore the others.

For this group, as well as the others, Fitzgerald et al. (1998) made a decision to exclude cases with missing data on wave 1 variables as well as cases at the top and bottom 1% of the income distribution. This trimming of the income variable was done, according to Fitzgerald et al. (1998), to avoid top-coding problems as well as distortion from outliers (Fitzgerald et al., 1998, page 267). Sample members who died between wave 2 and wave 22 were also excluded from the analysis. This selection process leaves Fitzgerald et al. (1998) with a total of 2,253 male heads. An application of the same criteria for the purpose of the current replication yielded a slightly smaller number of male heads (n=2,115). This difference is likely due to retrospective corrections to the official release made by the PSID team after data used by Fitzgerald et al. (1998) were extracted. A figure summarizing the selection process just described, starting with all members of the core sample that participated in the initial wave, is provided in appendix B.

The Fitzgerald et al. (1998) model is a person-level model in which attrition is defined as the first occurrence of wave nonresponse. The dependent variable in the model is coded 1 if the sample member has ever been out (nonrespondent) between wave 2 and wave 22 and 0 otherwise. The probability of attrition is calculated using a probit transformation that uses the inverse of the normal distribution instead of the log of the odds commonly used in a logit model (Kohler and Kreuter, 2005). The predictors are related to labor income (linear and quadratic term, whether or not received labor income), sociodemographics (age — linear and squared, race, educational attainment, presence of young children and number of children), and location/mobility (region of the country, living in rural area, probability of moving as assessed by the head of household, tenure) and were all measured during the wave 1 interview. Also included in the model was a variable indicating which sub-sample (SEO or SRC) the individual belongs to as well as a measure of relative income adequacy (Orshansky income to need ratio[9]). All predictors, aside from income, age and the need-income ratio were dichotomized.

Initial attempts to replicate the Fitzgerald et al. (1998) results were based only on the information available in the published paper. Examination of the data extraction and statistical estimation computer code used by Fitzgerald et al. (1998) provided much more information than the paper and allowed for the closest replication of the published results. However, it has also revealed a few discrepancies between what is described in the paper and what was actually done. These discrepancies are related to the handling of the indicators for race, age, and the presence

[9] Variable v325 in PSID public use file.

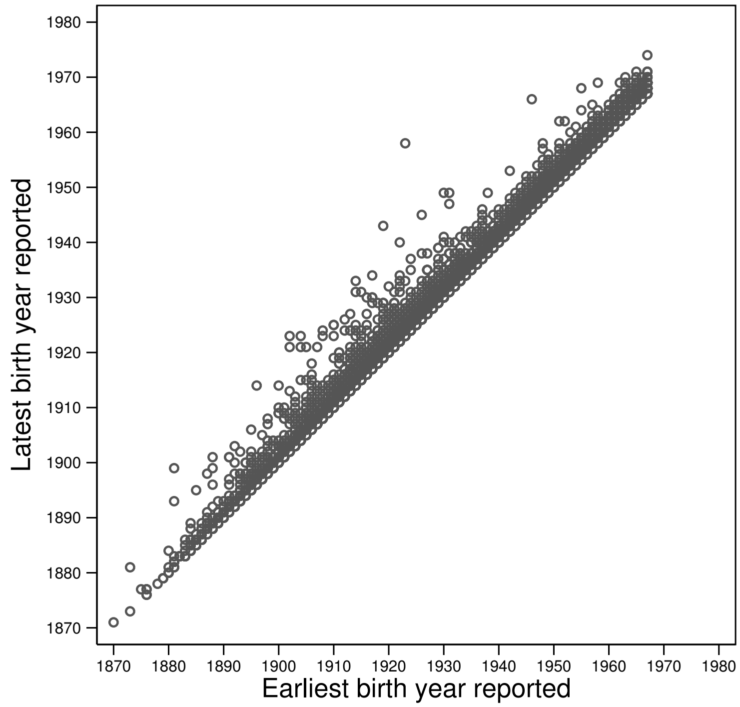



Figure 2.1: Scatterplot of the earliest possible birth year reported against the latest possible birth year reported indicating the presence of substantial measurement error in birth year reporting in the PSID.

of children as well as the method used to compute the standard errors.

In the PSID, age and race are both measured at each wave, which makes measurement error in these variables salient. Illustration of this problem for age can be found in figure 2.1. A similar issue with race reporting is revealed in table 2.2. Both this table and graph show a fair amount of inconsistencies in age and race reporting over time.

There are two possible explanations for these inconsistencies. One possibility is that the inconsistencies could be an artefact of the variability of the interview date for a given wave with respect to the respondent's birthdate. If the PSID interviews were conducted on the exact same date each wave, all respondents should be

| | | Most often reported | | | | |
|---|---|---|---|---|---|---|
| | | White | Black | Other | Not reported | Total |
| Reported at wave 1 | White | 9,888 | 27 | 213 | 17 | 10,145 |
| | Black | 22 | 7,328 | 0 | 4 | 7,354 |
| | Other | 9 | 11 | 599 | 2 | 621 |
| | Not reported | 15 | 29 | 0 | 7 | 51 |
| | Total | 9,934 | 7,395 | 812 | 30 | 18,171 |

Table 2.2: Cross-tabulation of race reported at wave 1 and most often reported race between wave 1 and wave 22.

expected to report an age that is exactly one year higher than at the previous wave (assuming perfect reporting without measurement error). However, due to practical fieldwork consideration, there are some slight wave-on-wave variations in the timing of the interview. For example, respondent X can be interviewed immediately after his or her birthday at wave 5. At wave 6, this same respondent could be interviewed *prior* to his or her birthday. If this respondent were to accurately report his or her age, he or she would report the same age at wave 6 as he or she did at wave 5. On the other hand, if respondent X had been interviewed prior to his or her birthday at wave 5 and after his or her birthday at wave 6, the accurate respondent would report an age two years higher in wave 6 than he or she did in wave 5.

Another possibility is that the inconsistencies in age reporting reflect reporting error: the respondent reports, knowingly or unknowingly, the wrong age. Given the deterministic increase of age as a function of time just described, any wave-to-wave change greater than two years per annual wave can be assumed to be measurement error. The wave-to-wave discrepancies recorded were between 1 and 35 years, with 26.6% of the sample showing discrepancies greater or equal to 2 years.

This indicates a substantial measurement error problem in age reporting. The authors' solution to this problem was to use the most-often reported age as the "true" value.[10] I have also implemented this solution throughout this paper, that is, in the replication of the original Fitzgerald et al. (1998) model presented in the current section as well as in all subsequent models presented in sections 2.5 and 2.6.

A similar solution was applied to race, albeit more controversially as the fixed nature of race is a matter of increasing debate (Eschbach, Supple, and Snipp, 1998; Quintana, Aboud, Chao, Contreras-Grau, Cross, Hudley, Hughes, Liben, Gall, and Vietze, 2006). The real problem lies in the fact that, in the PSID, race is a family-level variable that measures the race of the head of household. However, the head of household can change following mortality or disability. Any change in race recorded could result from a different adult, such as a spouse, of a different race being interviewed. For this reason, I will only implement this solution in the replication of the original Fitzgerald et al. (1998) model to ensure a more faithful replication. However, models presented in section 2.5 and 2.6 will use the race of the head of household at wave 1.

Regarding the variables on presence of children in the family unit, the category *presence of children less than 6* and *no children* were grouped in the same category. However, the paper clearly shows that the intent was to assign the code 1 to *presence of children less than 6* and to code all other responses, including *no children*, as 0.

[10]The implementation of this solution to correct age became clear only upon reviewing the documentation provided by the authors.

Finally, close examination of the estimation programs revealed that the survey design was not taken into account in the computation of standard errors. The PSID sample is stratified by region of the country and clustered by primary sampling units (PSU). It also makes use of weighting to account for the differential selection probabilities and nonresponse to the initial wave. This violates the *iid* assumption and needs to be factored in the computation of standard errors.

The replicated results using all of the information available (published paper as well as what could be gleaned from the estimation programs) are presented first. This replication makes use of the measurement error correction for age and race described above, it includes the erroneous coding for the indicator of *presence of young children*. Finally, the standard errors do not account for the clustering of observations, stratification or weighting. These results are therefore the closest, most faithful replication of the published results presented in table 5 on pages 272-273 of the Fitzgerald et al. (1998) paper.

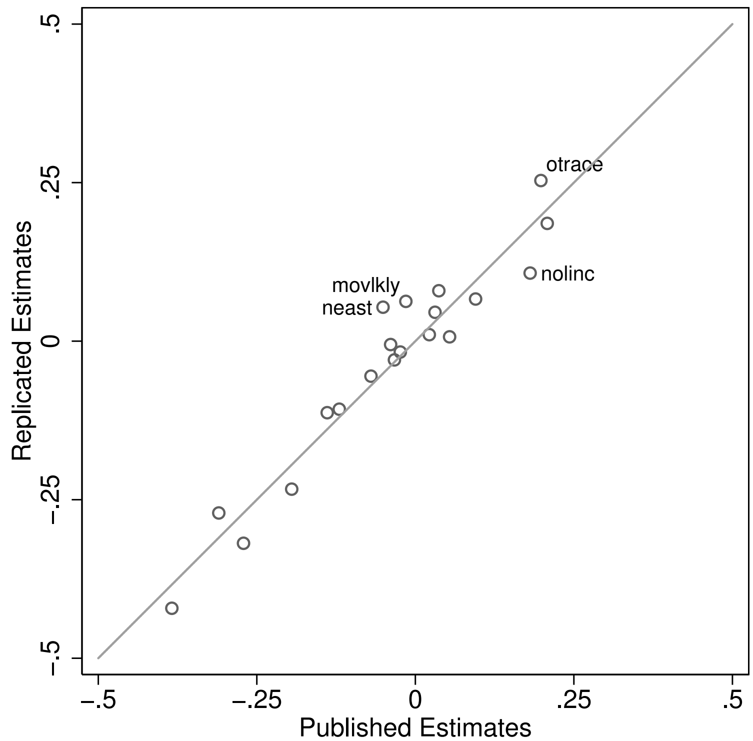
Figure 2.2(a) shows a plot of the replicated probit coefficients against the published probit coefficients. If all coefficients matched perfectly in sign and magnitude, the points would all lie on the diagonal. Most points indeed cluster around the diagonal, however there are some, such as the indicator *living in the Northeast region (neast)*, *likely to move (movlkly)*, *other race (otrace)*, and *no labor income (nolinc)*, which substantially deviate from the diagonal. Figure 2.2(b) shows the standard errors obtained in the replication plotted against the standard errors reported in Fitzgerald et al. (1998). Only the standard error for *other race* and *living in the Northeast region* differ substantially. On close inspection, the standard error for *living in the Northeast region* is exactly ten times smaller than what is reported in the paper, making a typographic error the most likely explanation for this discrepancy.

These results are rather satisfying considering that this was an independent replication, done several years after the original study. Moreover, it is PSID policy to update the data on an ongoing basis which means that the version of the PSID data used in this chapter might differ in some way from the version that Fitzgerald et al. (1998) used.

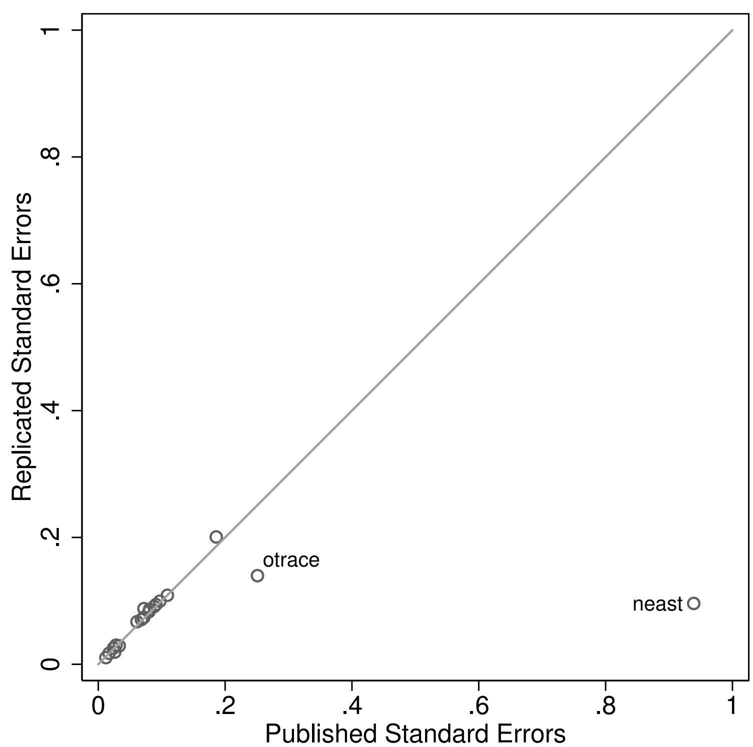
Three modifications were then made to the Fitzgerald et al. (1998) model to resolve the problems highlighted above. In all but one case, these modifications had little effect on the actual results.

First, the coding of the indicator relating to the presence of young children was modified to exclude families that had no children. Correcting this variable so that it conforms to what is described in the paper reverses the sign of the variable. The sign of the *presence of children less than 6 years old* coefficient goes from positive (i.e. the presence of children favors attrition), which is inconsistent with the literature, to negative (i.e. the presence of children favors retention in the panel), which is consistent with the literature (Groves and Couper, 1998).

Secondly, the correction for measurement error in race described above was removed. While this solution makes sense when applied to age, it is more controversial when applied to race. The main problem is that race is a family-level covariate in the PSID. In other words, only the race of the head of household is recorded. However, the head of household can change from wave-to-wave so that




(a)




(b)

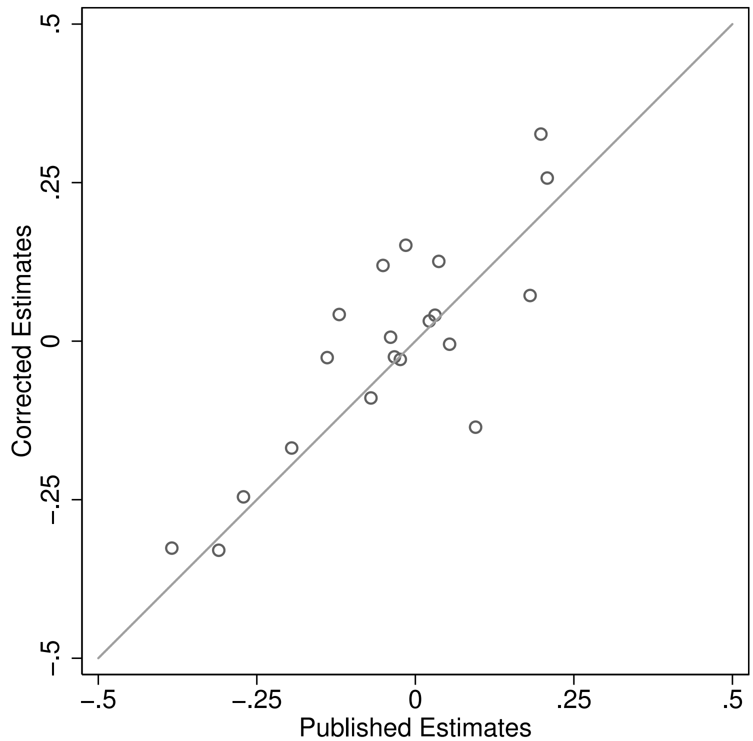
Figure 2.2: Scatterplot of the published Fitzgerald et al. (1998) logit model coefficients and standard errors for male heads of household aged 25 to 64 at wave 1 [table 5, pages 272-273] vs. the equivalent coefficients and standard errors replicated for the purpose of the present study (intercept not displayed).

wave-to-wave change in race is not necessarily spurious. Rather, it could be the reflection of the change in head that could occur in mixed-race families. Assimilating this to measurement error is not warranted. Not using this correction has little impact: the coefficient is slightly higher after correction but the sign remains the same.

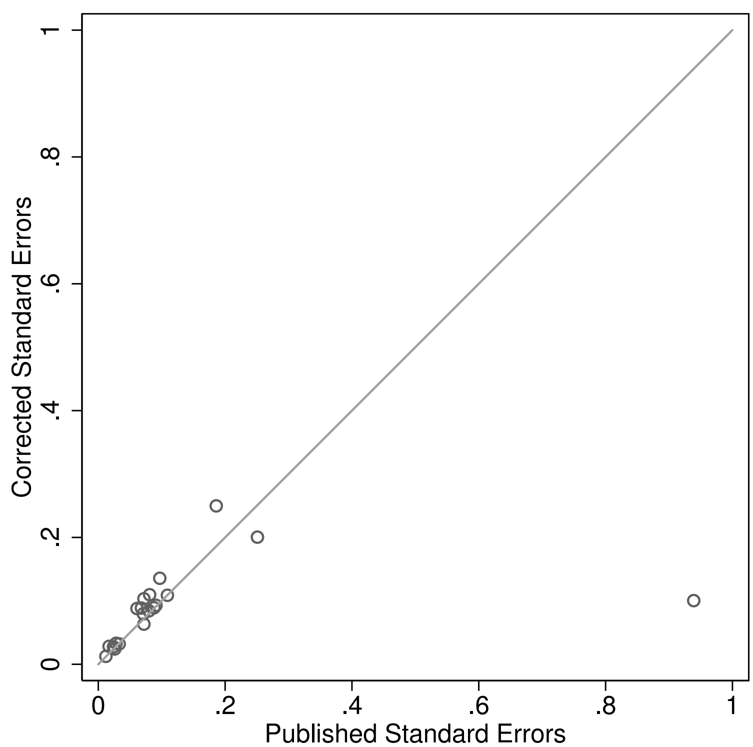
Finally, taking the increase in variance due to clustering, stratification, and weighting into account[11] has very little effect on the standard errors reported by Fitzgerald et al. (1998) (see figure 2.3(b)). However, weighting by the inverse of the selection probability changes the coefficient slightly, as can be concluded from the greater distance from the diagonal of most points in figure 2.3(a).

Table 2.3 shows the actual values of coefficients and standard errors of the published, replicated, and corrected models. The number of individuals experiencing attrition, exclusive of death, as well as the number of observations present at the onset of the panel (n) are displayed at the bottom of the table. The published model refers to the coefficients exactly as they appear in the published paper while the replicated model constitutes the closest independent replication of the published coefficients that I could get. Finally, the corrected model is one in which the amendments described above regarding children, race and standard errors were applied. The corrected model will be used as the basis for all subsequent analysis.

[11] *er31996* is used as the stratum identifier and *er31997* as the sampling error computing unit (SECU) identifier, following a note on the PSID website: http://psidonline.isr.umich.edu/Guide/FAQ




(a)




(b)

Figure 2.3: Scatterplot of the published Fitzgerald et al. (1998) logit model coefficients and standard errors for male heads of household aged 25 to 64 at wave 1 [table 5, pages 272-273] vs. the equivalent coefficients and standard errors corrected for the purpose of the present study (intercept not displayed).

| | Published | | Replicated | | Corrected | |
|---|---|---|---|---|---|---|
| | b (SE) | | b (SE) | | b (SE) | |
| Intercept | 1.130 | + | .298 | | .119 | |
| | (.518) | | (.537) | | (.588) | |
| Labor income | -.0237 | + | -.017 | + | -.029 | * |
| | (.012) | | (.010) | | (.013) | |
| No labor income | .181 | | .107 | | .072 | |
| | (.186) | | (.201) | | (.250) | |
| Labor income squared | .022 | | .010 | | .032 | |
| | (.026) | | (.019) | | (.024) | |
| Race (ref: white) | | | | | | |
| Black | .037 | | .079 | | .126 | |
| | (.081) | | (.087) | | (.110) | |
| Other race | .198 | | .253 | + | .327 | |
| | (.251) | | (.140) | | (.200) | |
| Age | -.039 | | -.005 | | .006 | |
| | (.024) | | (.026) | | (.027) | |
| Age squared | .054 | + | .007 | | -.005 | |
| | (.028) | | (.030) | | (.033) | |
| Education (ref: highschool) | | | | | | |
| Education < 12 yrs | .208 | + | .186 | * | .257 | ** |
| | (.071) | | (.073) | | (.080) | |
| Some college | -.195 | + | -.233 | * | -.169 | |
| | (.097) | | (.099) | | (.136) | |
| College degree | -.384 | + | -.421 | ** | -.326 | ** |
| | (.109) | | (.109) | | (.109) | |
| Region (ref: west) | | | | | | |
| Northeast | -.051 | | .054 | | .119 | |
| | (.939) | | (.096) | | (.100) | |
| North central | -.139 | | -.113 | | -.026 | |
| | (.091) | | (.094) | | (.093) | |
| South | -.120 | | -.107 | | .042 | |
| | (.088) | | (.091) | | (.089) | |
| In SEO sample | -.070 | | -.055 | | -.090 | |
| | (.080) | | (.083) | | (.084) | |
| Lives in rural area | -.271 | + | -.319 | ** | -.246 | * |
| | (.072) | | (.088) | | (.103) | |
| Number of children | -.033 | + | -.030 | + | -.025 | |
| | (.017) | | (.017) | | (.028) | |
| Presence of child <6 | .095 | | .066 | | -.136 | |
| | (.061) | | (.067) | | (.088) | |
| Owns house | -.310 | + | -.271 | ** | -.330 | ** |
| | (.068) | | (.070) | | (.089) | |
| Might move in future | -.015 | | .063 | | .151 | * |
| | (.072) | | (.074) | | (.063) | |
| Income/need ratio | .031 | | .046 | | .041 | |
| | (.033) | | (.029) | | (.032) | |
| Attriters | 1074 | | 909 | | 908 | |
| Observations (n) | 2253 | | 2115 | | 2110 | |

+ p<0.10, * p<0.05, ** p<0.01

Table 2.3: Summary of published, replicated, and corrected models, male heads 25-64 years old.

## 2.5 Update of the Fitzgerald et al. (1998) Model to wave 34

Several years have passed since the Fitzgerald et al. (1998) study was conducted and several new waves of the PSID have been released. Section 2.3 has shown that the attrition trend has continued after wave 22, the last wave covered by the Fitzgerald et al. (1998) study. Also, several design changes have potentially had an impact on attrition: introduction of computer-assisted telephone interviewing (CATI) at wave 26, introduction of event-history calendar (Belli, Shay, and Stafford, 2001) at wave 29 and biannual data collection at wave 30. This raises the question of what would have come out of the Fitzgerald et al. (1998) analysis had it been conducted at wave 34. In other words, would the results obtained by Fitzgerald et al. (1998) have changed if they had modeled the probability of ever having been out by wave 34 as a function of wave 1 characteristics instead of the probability of ever having been out by wave 22?

Due to the fact that a substantial proportion of cases present at wave 22 were dropped at wave 30, it is not possible to exactly replicate the "Corrected Model", that is the model presented in column 3 of table 2.3 in the previous section. In results presented in table 2.3, all cases were used, including those who would eventually be dropped at wave 30. However, in models that involve the comparison of the sample prior to wave 30 to the sample on or after wave 30, the dropped cases have to be excluded from both models so that both models are estimated on the same case base.[12] The number of cases dropped is equal to 220, which leaves 2,098 members

[12] Survival models used in the next chapter will allow to consider these cases as censored at that time point (wave 30), therefore eliminating the need to remove them.

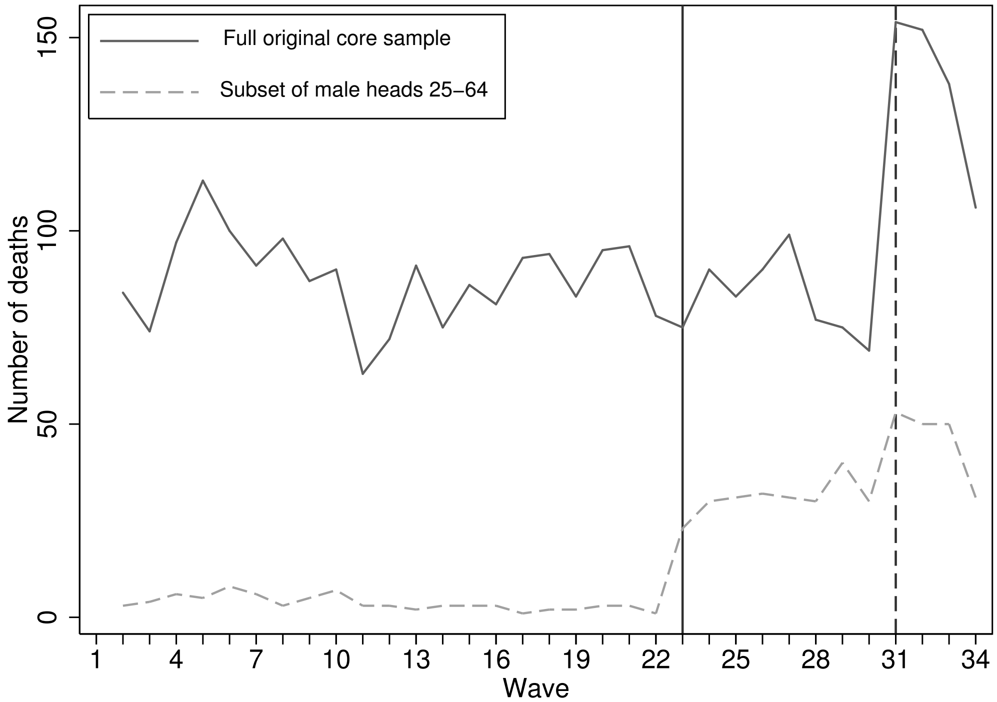



Figure 2.4: Number of deaths per wave for the full core sample compared to the number of deaths per wave for the subset of male heads aged between 25 and 64.

of the sub-sample of interest for both the wave 22 and wave 34 models (see appendix B). This means that the wave 22 model presented in this section, although identical in specification to the corrected wave 22 model presented in the previous section, is based on a slightly different case base.

Table 2.4 displays the results of the wave 22 and wave 34 models. As above, the number of attriters as well as the number of observations (n) are displayed at the bottom of the table.

There is an important point to be made regarding the large difference in sample size between the wave 22 and wave 34 model. Just like in all models presented thus far, cases that attrited through death are excluded from both the wave 22 and

| | Wave 22 Model | | Wave 34 Model | |
|---|---|---|---|---|
| | b (SE) | | b (SE) | |
| Intercept | .024 | | 1.859 | * |
| | (.600) | | (.853) | |
| Labor income | -.031 | * | -.051 | ** |
| | (.013) | | (.018) | |
| No labor income | .132 | | .172 | |
| | (.296) | | (.419) | |
| Labor income squared | .032 | | .048 | + |
| | (.025) | | (.027) | |
| Race (ref: white) | | | | |
| Black | -.057 | | -.158 | |
| | (.118) | | (.175) | |
| Other race | .326 | | .213 | |
| | (.230) | | (.299) | |
| Age | .015 | | -.089 | * |
| | (.027) | | (.042) | |
| Age squared | -.015 | | .148 | ** |
| | (.033) | | (.053) | |
| Education (ref: highschool) | | | | |
| Education < 12 yrs | .245 | ** | .310 | ** |
| | (.082) | | (.100) | |
| Some college | -.142 | | -.020 | |
| | (.141) | | (.164) | |
| College degree | -.293 | * | -.335 | * |
| | (.113) | | (.131) | |
| Region (ref: west) | | | | |
| Northeast | .129 | | .107 | |
| | (.104) | | (.134) | |
| North central | -.009 | | .051 | |
| | (.099) | | (.129) | |
| South | .037 | | .103 | |
| | (.090) | | (.101) | |
| In SEO sample | .517 | ** | 1.068 | ** |
| | (.102) | | (.142) | |
| Lives in rural area | -.251 | * | -.286 | * |
| | (.105) | | (.125) | |
| Number of children | -.026 | | .043 | |
| | (.030) | | (.031) | |
| Presence of child <6 | -.162 | + | -.091 | |
| | (.093) | | (.111) | |
| Owns house | -.373 | ** | -.411 | ** |
| | (.097) | | (.128) | |
| Might move in future | .144 | + | .128 | |
| | (.071) | | (.098) | |
| Income/need ratio | .043 | | .133 | ** |
| | (.033) | | (.038) | |
| Attriters | 885 | | 861 | |
| Observations (n) | 1890 | | 1315 | |

+ p<0.10, * p<0.05, ** p<0.01

Table 2.4: Ever-out probit models for wave 22 and 34, male heads 25-64 years old. Sample members dropped at wave 30 are excluded in both models. The number of attriters differ between the two models due to a few attriters in model 22 subsequently dying. Such individuals were excluded from model 34.

wave 34 models. The subset of cases under study in these models — male heads of households who were between the ages of 25 and 64 in wave 1 — are now quite old (on average, 73.1 years old in wave 34, up from 41.3 years old in wave 1). This aging process is likely responsible for an increase in the number of deaths for that subset of sample members. This would in turn be responsible for the sharp decrease in the number of observations for the wave 34 model in comparison to the wave 22 model. However, examination of figure 2.4, which shows the number of death per wave for the full core sample compared to the subset of male heads aged between 25-64, suggests another explanation. Figure 2.4 clearly shows an increase — indicated by the vertical solid line — in the number of deaths only for the subset of male heads, not for the full core sample (which included the subset of male heads)[13]. Furthermore, this increase occurred at wave 23, just around the beginning of the systematic recontact effort of past attriters mentioned above. Rather that being the result of the aging of the sample, this surge might simply reflect the deaths newly discovered as part of the massive recontact effort which involved verifications in the Social Security Administration database to find out whether or not past attriters were still alive. In other words, some male heads aged 25-64 who were, prior to the recontact effort, considered nonrespondent, were found to be dead as part of the recontact effort. These findings seem to indicate that the recontact effort was particularly successful at identifying deaths in the subset of male heads aged 25 to 64.

Table 2.4 reveals a change in coefficients between the wave 22 model and

[13]The increase in the number of deaths indicated by the vertical dashed line is due to the interview schedule switching from yearly to biannual interviews. The number of deaths recorded at each wave after this design change represents two years worth of deaths, hence the surge.

the wave 34 model but standard errors are mostly unchanged. For example, the coefficients for *age*, *membership in the SEO sample* and the *need/income ratio* have increased while the coefficients for *likelihood to move* has decreased. The coefficient for *number of children* has changed sign, going from negative in wave 22 to positive in wave 34. Is this an indication of a trend in the coefficients or an indication of bad model fit? The next section will provide some indication.

## 2.6 Generalization of the Fitzgerald et al. (1998) Model to Each Wave

The analysis that follows is a generalization of the analysis presented in the previous section. In much the same way that section 2.5 compared a model of the probability of having been ever-out (first occurrence of wave nonresponse for reasons other than death) by wave 22 to a model of the probability of having been ever-out by wave 34, the current analysis generalizes this strategy to every wave of the PSID. Thirty-three ever-out probit models will be estimated, that is one for each wave between wave 2 and wave 34.[14]

For example, the wave 2 ever-out model is one in which the dependent variable, *to have ever been out by wave 2 (yes=1, no=0)*, is regressed on the characteristics measured in wave 1. Likewise, the wave 3 ever-out model would be a regression of the dependent variable *to have ever been out by wave 3 (yes=1, no=0)* on the wave 1 characteristics. This produces a set of thirty-three probit coefficients for each of the twenty-one parameters in the models (including the intercept) for a total of 693

[14] Thirty-six years have elapsed between wave 2 (1969) and wave 34 (2005) which would amount to 36 waves. However, starting at wave 30 (1997), the PSID has been conducted on a biannual basis which bring the total number of waves down to thirty-three.

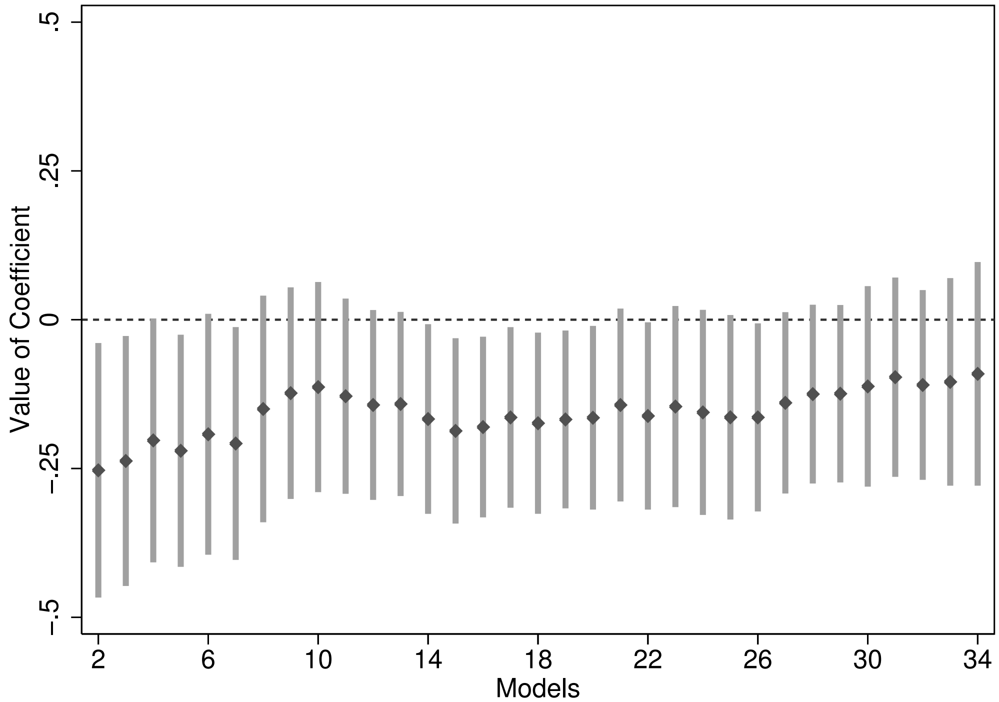



Figure 2.5: Unstandardized probit coefficients for *presence of children less than 6 years old in the family in unit* and 90% confidence interval, male heads 25-64 years old.

coefficients.

As in previous sections, I focus exclusively on the sub-sample of male heads of household between the ages of 25 and 64 in wave 1. As explained in section 2.5, cases that were dropped for cost reasons in wave 30 are removed from all models to allow consistent comparison of models before and after wave 30.

The 693 coefficients produced by this analysis can be plotted in order to facilitate the identification of patterns or trends. For example, figure 2.5 shows the probit coefficients for the *presence of children less than 6* predictor for each of the 33 models, holding all other predictors constant at zero[15], along with the 90 % lin-

[15]Variables are not centered at their mean.

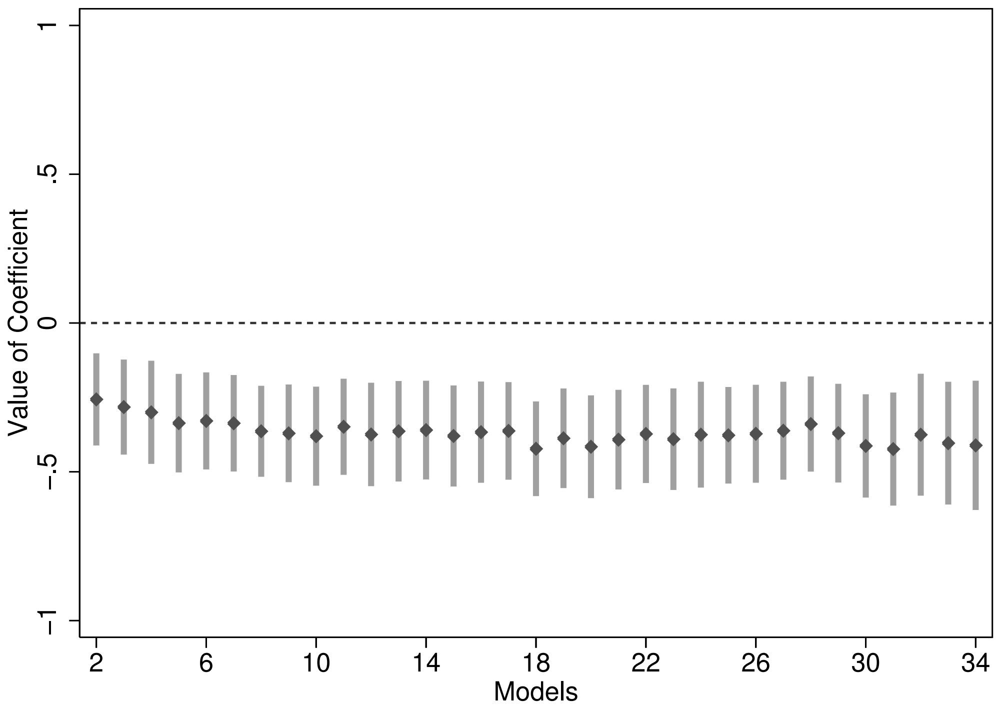



Figure 2.6: Unstandardized probit coefficients for *home ownership* and 90% confidence interval, male heads 25-64 years old.

earized confidence intervals bands[16]. The predictor in question starts out with a statistically significant negative impact on the probability of attrition in models for wave 2 through wave 20 (aside from wave 6 and the period from wave 8 to wave 13) and then becomes not significantly different than 0 in wave 21 and beyond. In other words, the presence of a child aged less than 6 in wave 1 favors retention in the panel, at least in the first half of the panel. It progressively becomes less and less important in later models. In contrast, figure 2.6 shows the evolution of the coefficients for home ownership. As evidenced by the graph, the values of the coefficients remain significantly different than zero for all models which means that home ownership in wave 1 is consistently associated with better retention in the panel across models.

Examination of the development of the intercept provides context for the interpretation of the coefficients. Figure 2.7 shows that the intercept follows a much more regular development through time than the coefficient. It starts out negative and steadily increases through the years in a quasi-linear fashion. The evolution of the intercept appears to reflect the gradual increase in the prevalence of attrition through the years.

These results make sense if we consider the substantive meaning of these two predictors and if we keep in mind that the current analysis excludes any new entrants into the sample (that is, the sample under consideration is getting older). Home ownership is a rather stable trait: most homeowners remain so for an extensive portion of their lives. Consequently, people do not generally change as fast in

[16] This is the significance level used by Fitzgerald et al. (1998) for their published wave 22 model.

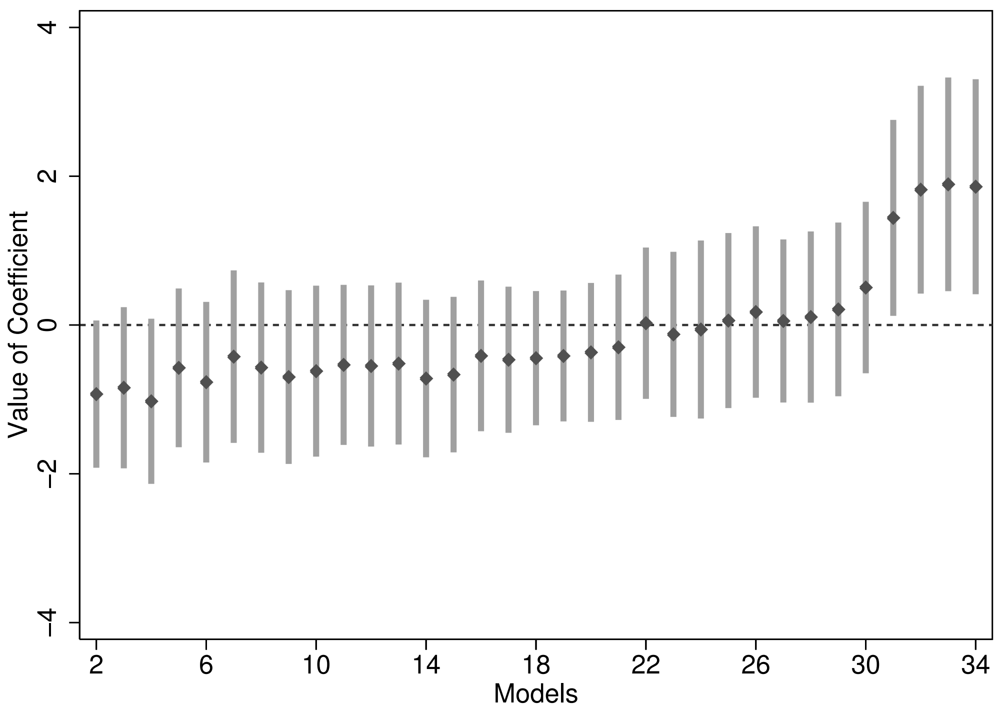



Figure 2.7: Intercept and 90% confidence interval for the ever-out model, male heads 25-64 years old.

that respect as with other dimensions of their lives. On the other hand, having children aged less than 6 living in the home is a substantially more transient attribute than home ownership. Most births within a family tend to be clustered around the same time which means that the period a family has any children aged less than 6 living at home is substantially more constrained in time than home ownership for example. Consistent with the results presented above, the presence of children is a predictor of retention in the panel for the period in which the characteristic is indeed present. However, as the time span increases, and as children move out on their own, this variable no longer differentiates between attriters and non attriters. People who had children then become as likely to attrite as those without children, which makes sense since their children are likely all grown and have moved out of the home. It is the progressive transition of the sample to later stages of the life course that makes wave 1 characteristics become obsolete predictors of attrition.

Although most coefficients in the model change value and sign, only the intercept shows a change in magnitude large enough so that the 90% confidence interval does not overlap. However, it is nonetheless interesting to look for trends in all of the coefficients in the model.

Instead of showing a plot for each coefficient, I provide a table which summarizes the development of coefficients across the thirty-three probit models. In table 2.5 one can see that the intercept starts out negative, becomes non-significantly different than zero and then ends up positive. Indicators for *living in the Northeast*, *other race*, and *number of children* follow a similar pattern. *Age*, the *square of age*, the *need to income ratio*, *living in a rural area*, *holding a college degree*, *membership*

| | Pattern |
|---|---|
| Intercept | $- \rightarrow 0 \rightarrow +$ |
| Northeast | $0 \rightarrow + \rightarrow 0$ |
| Other race | $0 \rightarrow + \rightarrow 0$ |
| No. of child. | $0 \rightarrow - \rightarrow 0$ |
| Rural | $0 \rightarrow -$ |
| College degree | $0 \rightarrow -$ |
| Age | $0 \rightarrow -$ |
| SEO | $0 \rightarrow +$ |
| Age squared | $0 \rightarrow +$ |
| Education < 12 | $0 \rightarrow +$ |
| Income/need ratio | $0 \rightarrow +$ |
| Might move | $+ \rightarrow 0$ |
| Labor income sq. | $+ \rightarrow 0$ |
| Child < 6 | $- \rightarrow 0$ |
| Labor income | $-$ |
| Own house | $-$ |
| Some college | 0 |
| North Central | 0 |
| No labor income | 0 |
| Black | 0 |
| South | 0 |

Table 2.5: Summary description of the development of the probit coefficients across models.

*in the SEO sample*, and *not having completed high school* start out with a value of 0 in early models and end up with a value significantly different than 0 in later models.

It is difficult to identify common features between the coefficients displaying similar patterns. A few coefficients show patterns that are consistent to figure 2.5 and figure 2.6 above. The indicators for *likelihood to move*, and *labor income squared*, start out different than zero and attenuate toward 0 with time. This development is similar to the one displayed by the indicator for the presence of children aged less than 6 years in the family unit (figure 2.5). Other coefficients behave similarly to the indicator for *home ownership* in figure 2.6 such as the coefficients for *some college*, *living in the North Central region*, the *South region*, *having no labor income*, *labor income* and being *African-American*: their value remain negative or equal to 0 for all models.

These variations in coefficients could be taken as an indication that the attrition models become increasingly less predictive of attrition as time increases. In other words, they could be an indication that later attrition models are inadequate. A look at residuals as well as various other measures that summarize how well a model fits the data (Long and Freese, 2006) will provide some indication.

Pearson residuals are one possible indicator of model fit (Long and Freese, 2006; Agresti, 2007). They represent the difference between the observed value $y_i$ and the fitted value $\hat{\mu}_i$ for each observation divided by the standard deviation of the observed values:

$$e_i = \frac{(y_i - \hat{\mu}_i)}{\sqrt{\hat{\mu}_i(1-\hat{\mu}_i)}} \tag{2.1}$$

It is generally recommended to use a modified version of these residuals called the standardized Pearson residuals which take into account the leverage of each observation (Long and Freese, 2006, page 147). Standardized Pearson residuals are equal to the Pearson residuals divided by the square root of 1 minus the leverage of the observation, a measure of the potential of an observation to influence the fit of the model (Agresti, 2007). Standardized Pearson residuals are more variable than regular Pearson residuals, which makes them easier to display, but provide results similar to the regular Pearson Residuals (Long and Freese, 2006, page 147).

Figure 2.8 shows the distribution of standardized Pearson residuals for each of the 33 ever-out probit models. The boxes represent the interquartile range (IQR) of the residuals distribution with the median displayed by the line in the middle. The lines extending on either side of the boxes represent residuals that are within a distance equal to 1.5 times the IQR from the median. Finally, the dots represent the outlier values, defined here as any values outside the 1.5 IQR mark. As can be seen from this graph, both the median value and the variance of the residuals increases over time. For example, the median of the standardized Pearson residuals for the wave 2 model is below 0.2 and the IQR is about equal to 1. On the other hand, the median value of the Pearson residuals for the wave 34 model is well above 0.6 and the IQR is about equal to 0.4. This overall increase in the magnitude of the standardized Pearson residuals in later models means that the number of

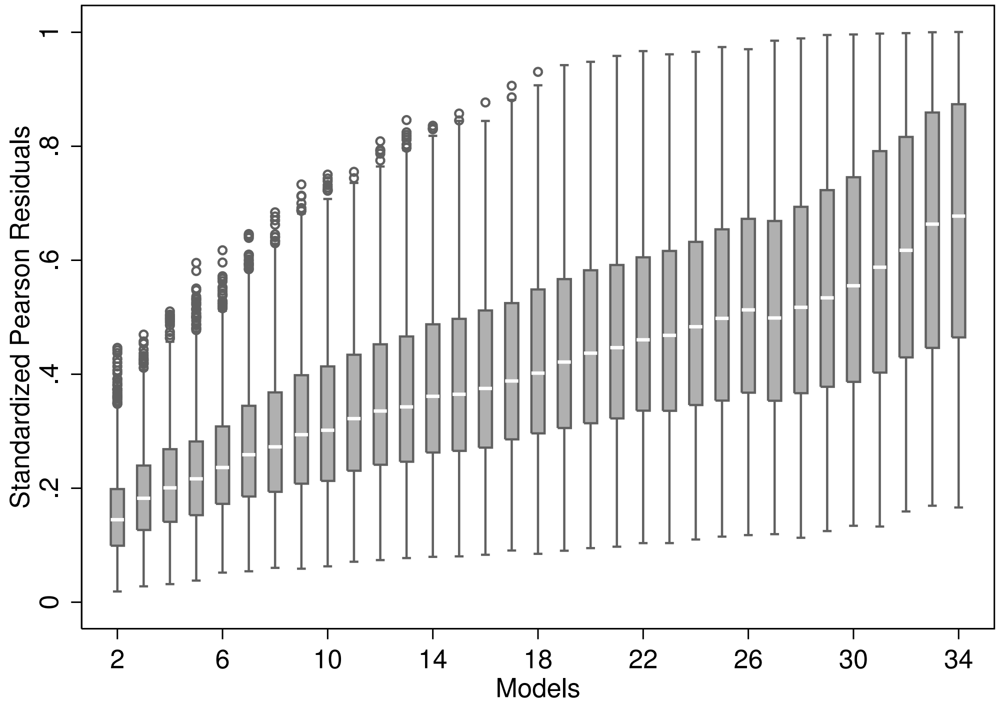



Figure 2.8: Distribution of the standardized Pearson residuals for the thirty-three ever-out probit models.

observations with predicted probabilities that differ markedly from their observed outcome is higher in later than earlier models. This suggests that later models fare more poorly than earlier models in predicting attrition which I interpret as an indication that characteristics of respondents in wave 1 become less suitable predictors of attrition as the time span increases.

Figure 2.9 shows the proportion of false negative, false positive and correctly predicted cases for each model, assuming a predicted probability cut-point of 0.5. The proportion of false negatives refers to cases that are predicted by the model not to attrite when they actually do, whereas the proportion of false positives refers to cases that are predicted by the model to attrite when they actually do not. The proportion correctly predicted, or adjusted count $R^2$, is a combination of the false negative and false positive statistics, adjusted for the proportion that would have been correctly predicted by chance (Long and Freese, 2006).

$$R^2_{AdjCount} = \frac{\sum_j n_{jj} - max(n_{r+})}{N - max(n_{r+})} \tag{2.2}$$

The adjusted count $R^2$ indicates by how much the model improves prediction over chance. Figure 2.9 shows that early models are not very good at identifying attriters as the adjusted count $R^2$ value is quite low. The low count $R^2$ is consistent with the high rate of false negatives produced by such early models. As the proportion of false negatives goes down in later models, the rate of false positives increases. The false positive and false negative rates converge at wave 22 which indicates that the wave 22 model is as likely to falsely predict attriters than it is to falsely predict non-attriters. This would seem to imply that the wave 22 model is the best of the thirty-three probit models presented as it minimizes the proportion of false negatives and false positives. Indeed, this model allows me to correctly predict as attriters a substantial proportion of cases (minimizing the proportion of false negatives) while keeping the number of cases incorrectly predicted to be attriters reasonably low (minimizing proportion of false positive). However, the measures of model fit, particularly the adjusted count $R^2$, are sensitive to the prevalence of the phenomenon of interest. In figure 2.9, the maximum value for the adjusted count $R^2$ coincides with wave 22, the wave at which the 50% attrition threshold was reached. This apparent superiority of the wave 22 model might simply be a

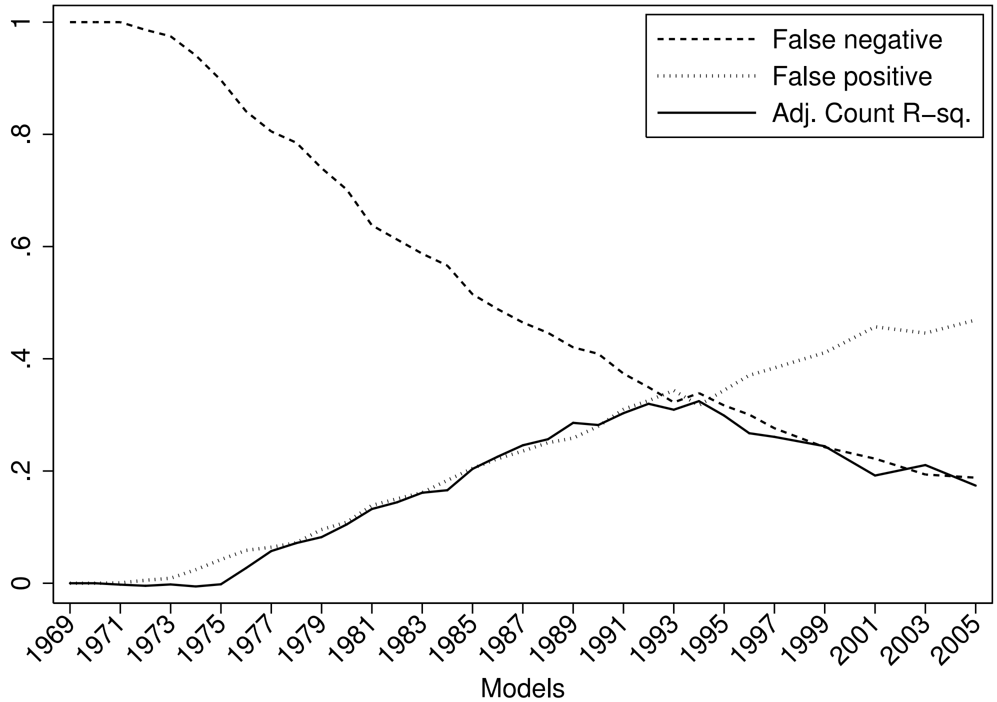



Figure 2.9: Proportion of false positive, false negative and adjusted count $R^2$ statistics for the 33 ever-out attrition models.

reflection of this fact.

## 2.7 Discussion

The current study was based on the replication and extension of previous results published in Fitzgerald et al. (1998). The process of replicating the study has revealed a few minor issues with some of the decisions made by the authors regarding age and race as well as the variable indicating the presence of children. Although quite extensive, I found that the explanation of the model provided in the paper was not sufficient to replicate the results. Other sources of information were necessary which were graciously provided by the authors. Overall, this replication exercise has revealed results that closely matched the results published by Fitzgerald et al. (1998).

I have also shown evidence of how an attrition modeling strategy that relies on wave 1 predictors can lead to contradictory results if periodic assessments of attrition are conducted. Using wave 1 predictors is common in panel attrition studies although other strategies have been tried before. For example, Fitzgerald et al. (1998), in a part of their analysis that I have not replicated, went a step further than their wave 1 models by presenting dynamic models that take into account change between wave $t-2$ and wave $t-1$ as predictor of attrition at wave $t$. Although a considerable improvement over other studies, this kind of strategy is not without issues either. Chief among them is the time lag between the indicators of change and the moment of attrition.

I believe that the real drivers of attrition are events that occur after $t-1$ but before $t$, events that are of course unmeasured for all attriters. What makes these events interesting is that they are probably much stronger predictors of attrition than change between wave $t-1$ and $t-2$. If this is indeed the case, the impossibility to measure the occurrence of such events for attriters would likely introduce bias, especially in panels that are specifically designed to measure life events and transitions such as the PSID. What is needed are more immediate measures of the circumstances of panel members that might be informative of attrition. Ideally, the availability of this information should not be conditional on the sample member's participation in the panel. Record-linkage with external sources of data could provide such information.

Finally, attrition in the PSID has continued to erode the core PSID sample after wave 22, the last wave covered by the Fitzgerald et al. (1998) study. By wave 34, less than a third (or 4966) of the 18,191 original sample members that participated in the first wave remained in the sample. As the original core sample ages, it is possible that a substantial proportion of attrition through non-contact is actually due to death. Death as a source of attrition is of no real concern to panel survey methodologists as long as it is not underreported. However, death can be confounded with attrition due to non-contact — a problem most likely to occur for sample members who live alone or are otherwise socially isolated as no one can report on them. At the same time, non-contact can also be the consequence of experiencing a disrupting life event that forces a move (such as divorce). This uncertainty is problematic because it confounds very different types of attrition and is difficult to resolve without external data (record-linkage). Refusals do not suffer from the same uncertainty. Therefore, careful consideration need to be given to the type of attrition and what it means for attrition bias.

# Chapter 3

# Effect of Life Events on Panel Attrition Due to Non-contacts and Refusals: Evidence from the Panel Study of Income Dynamics

## 3.1 Background

Social and behavioral scientists often rely on panel surveys to test theories about human behavior and inform policy development (Burkhauser and Smeeding, 2001; Rose, 2000; Lazarsfeld, 1948). Panel surveys are a special type of sample survey designed to track micro-level change by following the same sample units over a certain period of time. Despite their specific purpose, panel surveys are faced with the same problem as all sample surveys. Measurement error and nonresponse error can threaten the quality of survey data (Groves, 1989). Nonresponse is the failure to measure all sample members. To be valid, statistical inference theory requires data to meet either one of the following conditions: a one hundred percent response rate, data missing completely at random (MCAR) (Little and Rubin, 2002) or the elaboration of a statistical model that accounts for the exclusion of certain units in a non-random fashion. There has been an increased concern in the survey community about falling response rates in recent years (DeLeeuw and DeHeer, 2002). The main concern is the potential biasing impact of nonresponse on the survey estimates. When nonresponse is selective, that is when nonrespondents are systematically different than respondents, bias in the survey estimates will occur.

In panel surveys, failure to obtain participation from all sample members in the initial wave is an important problem as in any survey. Even more of a concern is attrition, the progressive loss of panel members over time. The term attrition generally refers to a situation in which the sample member is permanently lost to follow-up or refuses to participate. It is to be distinguished from wave nonresponse, a situation in which a sample member skips one or several waves but then eventually resumes participation in the study (Kalton, 1986; Kalton and Miller, 1986).[1] Panels usually experience higher levels of attrition in the first few waves but attrition continues, albeit at a lower rate, throughout the duration of the panel (Wooden, 2001). This loss of sample members due to attrition is in addition to nonresponse in the initial wave. As in the cross-sectional setting, the consequence of a loss of sample is an increase in variance, a decrease in power for subgroup analysis, as well as inflated costs of data collection. Bias in the estimates will be introduced if determinants of attrition are related to the estimates of interest. This has been well documented in both cross-sectional (Groves, 2006) and longitudinal surveys (Little, 1995). However, as panel studies often collect information on a broad range of topics, there is a risk that some nonresponse determinants will be correlated with some of the survey estimates. Finally, because such great value is placed on the repeated measures collected, sample members who attrite from the survey cannot

[1] Death and other forms of ineligibility are sometimes also referred to as natural attrition.

be easily replaced. This loss of panel members can therefore inflict great damage to panel surveys. Thus it is important to find ways to prevent attrition and correct its impact.

Several studies have grappled with the modeling of panel attrition. Findings suggest that younger people, African-Americans, males, renters and low income people all have a lower probability to stay in panels (Zabel, 1998; Rizzo et al., 1996; Lepkowski and Couper, 2002). Some more theoretically-informed models include indicators of community attachment and willingness-to-be found measures; some of these measures have been shown to be related to the probabilities of contacting panel members as well as the probability of securing their participation in the survey given that they were successfully contacted (Lepkowski and Couper, 2002). So-called situational factors such as employment status, financial problems and satisfaction with health show inconsistent results, sometimes being positively related to contact propensity (satisfaction with health, employment status) but not to response propensity, and sometimes being related to both (moving in the last 3 years) (Lepkowski and Couper, 2002).

Zabel (1998) and especially Lepkowski and Couper (2002) acknowledge the importance of the quality of the survey experience in trying to predict response propensity at later waves. Negative survey experience in a previous wave as measured by the amount of item missing data, the time spent by the interviewer editing the form (Zabel, 1998), as well as by various subjective interviewer ratings (Lepkowski and Couper, 2002), is correlated with the probability to attrite from the panel. Survey design features such as interview length, mode change, and interviewer change have all been studied as potential predictors but the results vary depending on whether random assignment of interviewers to cases was used or not. Interviewer change from one wave to another has been shown to have a negative impact on survey participation (Zabel, 1998), but when random assignment of interviewers is used (Campanelli and O'Muircheartaigh, 1999) — or other procedures are used to take into account the fact that interviewer changes do not happen by mere accident, as in Zabel (1998) — the relationship disappears. Interview length has been shown to have no impact or even a positive impact (Branden et al., 1995) on survey participation but the relationship seems to be the result of interviewers spending more time with people interested in the survey rather than the pure effect of interview length as an indicator of burden. Length was shown to have a negative impact on survey participation in Zabel (1998).

A common although not universal approach used in these studies is to use variables measured at wave 1 and test whether they are related to attrition later in the panel (Campanelli and O'Muircheartaigh (1999); Lepkowski and Couper (2002); Rizzo et al. (1996), and, to some extent, Fitzgerald et al. (1998)). This approach is analogous to the practice of using frame variables to model nonresponse in cross-sectional surveys, the only difference being that wave 1 data provide much richer information than sampling frames, which leads to much richer analysis. However, there are three problems with this strategy.

First, these studies assume that fixed attributes are the real determinants of panel participation. However, current theories suggest that survey participation decisions are based at least partially on circumstantial considerations and that there are few, if any, individuals who would accept or decline all requests to participate (Groves, 2006; Groves, Singer, and Corning, 2000). In the panel setting, this implies that fixed characteristics — either truly fixed characteristic such as date of birth or characteristics that are deemed fixed for simplicity such as presence of young children in the household at wave 1 — should be poor predictors of survey participation. On the other hand, current circumstances and recent life events are likely to say more about the decision to participate (Short and Mcarthur, 1986).

Secondly, these studies further assume that the effects of various factors associated with attrition remain constant through time. However, it is reasonable to assume that different factors might have more or less impact on attrition depending on how long an individual has been participating in the survey. For example, continued participation is a sign of commitment to the survey, a factor that could mediate the impact of other factors (Laurie, Smith, and Scott, 1999).

Thirdly, these studies consider attrition to be a dichotomous process, ignoring the time dimension underlying it. By focusing on whether or not attrition has occurred by some arbitrary time point (typically, the most recent wave in ongoing panels, or the ultimate wave in inactive panels), they leave aside the important question of when attrition is most likely to occur. In the process, a lot of information gets discarded: all attriters are considered the same, regardless of the length of their participation in the panel. This kind of analysis tends to produce contradictory results depending on the time point chosen for dichotomization (Singer and Willett, 2003). I have previously illustrated empirically the contradictory results produced by this approach in chapter 2. For example, I have found that the presence of young children at wave 1 had a negative effect on attrition in the early waves of the Panel Study of Income Dynamics (PSID) but the same predictor had no effect later on.

The present study addresses these shortcomings by looking at how recent life events relate to attrition. Some evidence that life events such as household change, job change, change in residence, or change in income have some potential value in explaining attrition can be found in the literature (Short and Mcarthur, 1986). Because these events could influence attrition either by making sample members more difficult to trace and contact, and/or by making them less receptive to the survey request, separate models for contact and response will be developed (Lepkowski and Couper, 2002).[2] However, Groves and Couper (1998) and Lepkowski and Couper (2002) have stressed the importance of considering these components as distinct phenomena reflecting different mechanisms.

There are at least two distinct ways in which life events may be associated with attrition. A "sociodemographic" analysis would suggest that some categories of individuals are inherently less likely to participate in the survey because of their social isolation. As the sociodemographic status of panel members change following the occurrence of life events, panel members switch to a social context less conducive to participation by making them more difficult to contact and less likely to agree to an interview. This sociodemographic interpretation focuses on the status of individuals rather than the disrupting effect of the transitional event itself. On

[2]The inability to locate panel members is another source of attrition theoretically distinct from the inability to contact or to secure participation. In practice, however, it is almost impossible to distinguish a failure to locate from a failure to contact as a contact with the panel member (or an informant, such as a household member) is generally required to establish whether the right panel member was located.

the other hand, a "psychosocial" analysis would suggest that respondents who experience life changes are influenced by the event itself (the "shock") rather than by the new status per se. Being focused on how to deal with the shock, panel members would be less likely to agree to participate in a survey interview. Additionally, as a way to adapt to the shock, individuals might be more likely to move to a different address, either to take employment or to find living quarters better suited to they new situation, thus making them more difficult to contact which would result in attrition.

## 3.2 Data

The Panel Study of Income Dynamics (PSID) is a longitudinal study of a representative sample of the United States population. Like similar surveys worldwide (GSOEP, HILDA, BHPS), it emphasizes the dynamic aspects of economic and demographic behavior, but it also covers a wide range of other topics such as housing and neighborhood characteristics, health status and expenditure, child care and development, time use, kinship, achievement motivation, philanthropic giving, etc. The choice of a longitudinal survey suitable for this study was guided by several factors. The richness and variety of data collected as well as the longevity of the survey makes the PSID an interesting case to study the long term attrition process. Furthermore, the PSID is a very prominent, widely used survey in the social sciences on which hundreds of articles are based. Finally, the last published assessment of PSID attrition dates back to wave 22 (Fitzgerald et al., 1998). Since then, 12 more waves of data have been released and a number of major design changes have taken place that might have altered both the rate of attrition and the correlates of attrition.

In the mid-1960's, about 5000 US families were selected to be part of the core sample of the PSID. About half of these families came from an equal probability sample of households from the 48 contiguous states drawn by the Survey Research Center (SRC) at the University of Michigan and about half came from the Survey of Economic Opportunities (SEO), a survey of low-income families conducted by the US Census Bureau (Hill, 1992). The response rate to the initial wave was 77% for the SRC sample and 50.8% for the SEO sample (66.5% overall). The initial response rate translated into 18,191 individual sample members participating in the first wave. These 18,191 individuals were attached to 4802 distinct families.

The PSID is an indefinite-life panel survey which means that a sample renewal procedure is in place to ensure the perenniality of the survey and help keep the sample representative of the population over time. The renewal procedure generally aims to mimic natural demographic processes. Therefore, in the PSID, all descendants of the core sample members selected for wave 1 are being automatically added to the sample and followed until their death. This procedure results in an increase of the total sample over time. The renewal procedure also aims to mimic social demographic process. Sample members can remarry or change life partners following a divorce or the death of a spouse; they can also move to establish a family of their own. The individuals who are brought into the lives of sample members following such a change in living circumstances serve to provide context to sample members and are of interest to substantive researchers. However, according to PSID follow-up rule, these "cohabitants" do not become part of the sample and do not contribute to an increase in sample size. Therefore, cohabitants stop being followed when they move out of the sample member's household.[3]

However, the natural replenishment of the sample just described cannot account for segments of the US population that were not present in the country prior to the initial wave. Therefore, in an effort to keep the PSID representative of the current US population, the PSID has added refresher samples throughout its recent in history. A sample of 2,000 Latino households, that included families originally from Mexico, Puerto Rico, and Cuba, was added in 1990. While this sample did represent these three major groups of immigrants, it missed out on the full range of post-1968 immigration and was dropped in 1996. It was replaced in 1997 by a sample of 441 immigrant families that are still being followed.

This chapter is concerned with core sample members (SEO and SRC samples) that were respondents at wave 1. All individuals added to the PSID sample after wave 1 (including descendants of the core sample born after wave 1) are therefore excluded from the analyses.

[3]For an extensive description of the PSID design and history, the reader is referred to Hill (1992).

## 3.3 Operational definition of attrition

### 3.3.1 Occurrence of attrition

In its simplest form, attrition can be thought of as a dichotomous, irreversible process: either a sample member is a respondent or not and once he/she becomes a non-respondent, he/she remains so permanently.[4] The outcome of interest would in such a case be a dichotomous indicator taking a value of 0 for waves prior to the point of nonresponse and 1 thereafter. However, the reality is often more complex due to the use of proxy reporting, wave nonresponse and the existence of several different types of attrition. Each of these considerations need to be addressed in the definition of an outcome of interest.

In the PSID, one adult member of the household — generally, the "head of household", referred to as the "head" for short, is asked to provide information about the household, himself/herself, and another adult member of the household, typically the spouse. The individual providing information for the household can change over the course of the panel. In order to minimize nonresponse, the study design allows for the collection of information from an adult other than the head in case of incapacity or refusal of the head. This systematic and extensive use of proxy interviewing is in sharp contrast to the practice in other comparable panels such as the BHPS and the GSOEP in which each sample member within a household is interviewed separately. The extensive use of proxy reporting in the PSID contributes to blurring the distinction between respondents and non-respondents. Contrary to

[4] Absorbing state is a term sometimes used to describe such process.

| | Wave 1 | Wave 2 | Wave 3 | Wave 4 |
|---|---|---|---|---|
| Case 1 | R | R | NR | NR |
| Case 2 | R | NR | NR | NR |
| Case 3 | R | R | NR | R |
| Case 4 | R | NR | R | R |
| case 5 | R | NR | NR | R |
| Case n | R | R | R | R |

(a) Data

| | Wave 1 | Wave 2 | Wave 3 | Wave 4 |
|---|---|---|---|---|
| Case 1 | 0 | 0 | 1 | 1 |
| Case 2 | 0 | 1 | 1 | 1 |
| Case 3 | 0 | 0 | 0 | 0 |
| Case 4 | 0 | 0 | 0 | 0 |
| Case 5 | 0 | 0 | 0 | 0 |
| Case n | 0 | 0 | 0 | 0 |

(b) Forever-out

| | Wave 1 | Wave 2 | Wave 3 | Wave 4 |
|---|---|---|---|---|
| Case 1 | 0 | 0 | 1 | 1 |
| Case 2 | 0 | 1 | 1 | 1 |
| Case 3 | 0 | 0 | 1 | 1 |
| Case 4 | 0 | 1 | 1 | 1 |
| Case 5 | 0 | 1 | 1 | 1 |
| Case n | 0 | 0 | 0 | 0 |

(c) Ever-out

| | Wave 1 | Wave 2 | Wave 3 | Wave 4 |
|---|---|---|---|---|
| Case 1 | 0 | 0 | 1 | 1 |
| Case 2 | 0 | 1 | 1 | 1 |
| Case 3 | 0 | 0 | 1 | 0 |
| Case 4 | 0 | 1 | 0 | 0 |
| Case 5 | 0 | 1 | 1 | 0 |
| Case n | 0 | 0 | 0 | 0 |

(d) Multiple nonresponse spells

Figure 3.1: Schematic representation of alternate attrition definitions. Each row represents the individuals and each column represents the final disposition codes for each wave in a fictitious example panel.

most surveys, the assessment of participation cannot be based on whether or not a sample member granted an interview because all sample members do not have an active role in the participation decision. However, all sample members, including non-adult sample members are essential to the long-term representativeness of the PSID sample and as such they cannot be ignored in a model of attrition.

In this chapter, I will perform the analysis at the individual level and assume that a sample member has been followed-up successfully if they were part of a responding family at wave t. By "responding family", I mean a family for which it was possible to get an interview, regardless of who provided the interview. This means that some panel members will be considered to be "respondents" even when they are not providing information about themselves directly to the interviewer. For clarity, I will refer to these individuals as a "participant".

Another issue to take into account in the elaboration of an operational definition of attrition is wave nonresponse (Kalton, 1986; Kalton and Miller, 1986). It is possible for panel members and panel families to display a non-monotonic pattern of participation. For example, panel members do sometimes resume participation in the panel after a lapse of one or move waves as shown for cases 3, 4 and 5 in figure 3.1(a) where response is represented by the letter R and nonresponse by the letters NR. Although relatively rare in the PSID, recent design changes have contributed to an increase in the prevalence of such cases. In the period prior to wave 25, systematic tracking of non-respondents was limited to one wave after initial nonresponse. As a consequence, the number of attriters who resumed participation was quite low — 43 cases per wave on average for the sample of 18,191 cases under study. At wave 25, this limit was lifted and an intensive recontact effort of past attriters was undertaken which resulted in a surge of sample members coming back into the sample after spells of nonresponse of various lengths.

However marginal, the phenomenon of wave nonresponse needs to be addressed in the definition of attrition. There are several ways to approach the problem, each with its shortcomings. A first approach is to define attrition as a form of "right censoring": only the rightmost uninterrupted spell of nonresponse is considered attrition (see figure 3.1(b)) where attrition is represented by 1's). In other words, if a sample unit does not come back by the end of data collection, it is considered to have attrited. If permanent exit is our definition of attrition, it makes sense to focus on this scheme. However, the "end of data collection" is a moving target in an active panel: cases that appear to have attrited might resurface at the next wave of data collection. A way to address this issue is to define attrition as the first exit (figure 3.1(c)). In other words, as soon as a sample unit misses one wave ("ever-out"), it is considered to be part of the attriters regardless of whether or not it comes back the next wave. This is the solution chosen by Fitzgerald et al. (1998). It assumes that the mechanism of attrition is not different than the mechanism of wave nonresponse which may be a more or less tenable assumption.

A more complex alternative which addresses the shortcoming of the two previous scheme is to allow for multiple spells of non response (figure 3.1(d)). This scheme is appealing because it is the most general: it can accommodate wave nonresponse as well as attrition, which is then defined as wave nonresponse from which there is no return. The main issue with this scheme is that it is plagued by a missing data problem whenever one tries to include time-varying covariates in the attrition model, which is one of the goals of the present chapter. In such a context, there is no information available on returning sample members for the wave prior their return. A fourth and final option is to model the count of wave nonresponse (not displayed). This option is not highly desirable in itself because it leads to a loss of information about the timing of attrition and/or the various spells of wave nonresponse. However, it could be an interesting choice when combined with the ever-out strategy discussed above (figure 3.1(c)).

In this chapter, I will use the ever-out strategy outlined above, which means that the focus will be on modeling the first occurrence of wave nonresponse, conditional on having responded in the first wave.

At first look, this seems like a strict definition of attrition. However, I would argue that it rightly puts the emphasis on the first occurrence of damage to the panel. While it is possible to fix wave nonresponse through weighting and, more conveniently, multiple imputation, missing waves complicate panel analysis and compromise the measurement of change that panels are designed to provide. Secondly, this strict definition does not suffer from the logical issues associated with the forever-out strategy or the loss of information associated with the count strategy. Finally, the number of cases that are unjustifiably classified as attriters after missing only a wave or two is relatively low as shown in table 3.1. The vast majority of sample members display simple participation patterns: 30.8% are always in the panel and 56.9% are what I call pure attriters (they become nonrespondents and never come back), which means that the ever-out coding scheme exactly reflects the participation pattern of 87.8% of the sample. The remaining 12.2% (2,227 cases) experience a variety of patterns ranging from a unique spell of nonresponse that lasts a single wave to multiple spells of various lengths. The 336 cases that experienced a single one-wave spell of nonresponse are the most inadequately represented by the ever-out strategy but they constitute a very small fraction of the sample (1.8%). Individuals experiencing a single two-wave spell also constitute a very small fraction (67 cases or .4% of the total). There is a substantial number (1824 or 10%) experiencing a single spell longer than two waves or even multiple spells but in these cases, it seems reasonable to assume that they do not come back into the sample.

In an attempt to provide further empirical justification for the use of the ever-out definition of attrition, I have created three models with slightly different outcome definitions. The first model uses the ever-out definition, while the second and third model define attrition as having been out for at least two or three consecutive

| | n | % |
|---|---|---|
| Stayers *(did not miss a wave)* | 5,607 | 30.8 |
| Pure Attriters *(became nonresponse and never came back)* | 10,357 | 56.9 |
| Returners *(missed at least a wave but came back)* | 2,227 | 12.2 |
| Experienced a single nonresponse spell *(various lengths)* | 1,189 | 6.5 |
| Spell duration: 1 wave | 336 | 1.8 |
| Spell duration: 2 waves | 67 | .4 |
| Spell duration: 3 waves | 48 | .3 |
| Spell duration: 4 waves or more | 738 | 4.1 |
| Experienced two nonresponse spells *(various lengths)* | 931 | 5.1 |
| Experienced three nonresponse spells *(various lengths)* | 99 | .5 |
| Experienced four nonresponse spells *(various lengths)* | 8 | – |
| Total | 18,191 | 100 |

Table 3.1: Distribution of the initial SRC & SEO sample according to the sample members' participation pattern in the PSID for waves 1 through 34.

waves, respectively. While these models, displayed in appendix C, only use wave 1 predictors (to circumvent the missing data problem explained above), they are a good indication that using a definition of attrition other than ever-out matters little. Indeed, the change in the coefficients value is minimal, with no clear pattern as to the direction of the change: about equal numbers of coefficients increase and decrease in value. However, the BIC and AIC statistics increase which indicates that the two-wave-out and three-wave-out models do not fit the data as well as the one-wave-out (ever-out) model. Such models could be improved by the inclusion of one or more factor(s) that capture the difference between panel members who miss two or three waves and those who become permanent nonrespondents.

### 3.3.2 Type of attrition

Deciding when attrition occurs is only part of the process of the definition of attrition. Three phases can be identified in the survey participation process: the location, the contact, and the request for participation. Nonresponse can occur at each of these phases: addresses can fail to be located; located addresses may fail to be contacted even if they were successfully located; and sample members can fail to be convinced to participate even though they were successfully located and contacted (Groves and Couper, 1998). In panel surveys, death is yet another way for an individual to attrite.

Lepkowski and Couper (2002), following the general framework laid out by Groves and Couper (1998), argue that attrition for failure to contact and attrition for failure to secure participation are two distinct phenomena, each with their specific sets of determinants. On the other hand, authors such as (Behr, Bellgardt, and Rendtel, 2005) argue that the two phenomena might be correlated and that modeling should allow for this possibility. Their theoretical argument is that some non-contact might actually be disguised refusal and therefore the factors predicting refusal might also be correlated with non-contact.

Authors interested in attrition have either used an approach in which failures to contact and refusals are considered in separate, conditional models (Lepkowski and Couper, 2002) or an approach in which a dichotomous response/non-response outcome eliminates the distinction between non-contacts and refusals (Zabel, 1998; Fitzgerald et al., 1998). Without contact data, the former strategy is difficult to implement as it is impossible to know for sure the true sequence of events. On the other hand, the solution adopted in Zabel (1998) and Fitzgerald et al. (1998) is overly simplistic. A third possibility is to model each type of attrition as competing events. Doing so resolves both issues: it allows the creation of two separate models for failure to contact and for refusal while allowing the same predictors to be associated with both phenomena.

Studying mortality as a source of attrition poses a similar challenge. For most panel members, death cannot be ruled out as a possible explanation for the failure to contact, especially for panel members who were living alone prior to their death or were otherwise socially isolated (Groves and Couper, 1998). In such circumstances, no one can report the death of the panel member to field personnel. Unless some external sources are used in an attempt to sort out the true non-contacts (individuals who are still living but cannot be located/contacted) from the actual deaths, panel surveys will tend to underestimate the true death rate of the sample.

Unlike the other sources of attrition, death is generally less of a concern in studies of panel attrition. This is probably because death is considered to be a natural phenomenon that cannot be avoided, contrary to other forms of attrition. Also, in indefinite-life panels such as the PSID, procedures are in place to renew the sample, typically by making the descendants of original panel members part of the panel, which makes them panel members in their own right, even if they were born after the inception of the panel. The logic underlying such a strategy is that births that occur in the panel represent births that occur in the population.

However, death still has the potential to introduce bias in the estimates derived from panel surveys. If factors that are related to mortality such as socioeconomic status (Kitagawa, 1973; Murray, Kulkarni, Michaud, Tomijima, Bulzacchelli, Iandiorio, and Ezzati, 2006; Harper, Lynch, Meersman, Breen, Davis, and Reichman, 2008) are also related to events of interest (for example, illness), mortality will introduce a bias in the estimate of illness. It is therefore important to distinguish the different types of attrition insofar as they might have different determinants and therefore distinct potential to introduce bias in some estimates. Throughout the rest of the chapter, I will make a distinction between attrition (defined as "first occurrence of wave nonresponse") through non-contact, refusal, ineligibility, and death.

Table 3.2 shows the relative importance of the different types of attrition in the original core sample of the PSID. The first column of the first panel of table 3.2 shows, for each wave, the number of sample members who have not attrited, that is, the number of people who have never missed a single wave, according to the definition of attrition I have chosen[5]. The next two columns break down these numbers according to whether these people were living in a family unit or in an institution. The first column of the second panel shows, for each wave, the number of sample members who attrited. The next four columns break down these numbers by reasons, i.e. death, non-contact, refusal, and ineligibility. The number of sample members who were dropped due to budget constraints is displayed in the last column of the table.[6] Analyses presented in this chapter encompass the four types of attrition (death, non-contact, refusal, and ineligibility). The dropped cases need

[5]Larger numbers of core sample members are actually participating at each wave than what is displayed in table 3.2. By design, PSID allows panel members who have missed one or more waves to resume their participation in the panel, a phenomenon I chose to ignore for simplicity. To see the number of core sample members who were participating at each wave as well as the number returning from nonresponse, please see appendix A

[6]More people have actually been dropped. This is the number who have been dropped *among those who had never missed a wave* by wave 30. Some sample members were dropped *after* they had returned from a spell of nonresponse; these individuals are considered to be attriters in table 3.2. For a version of this table that accounts for sample members who return from nonresponse, please see appendix A.

| | Always in | | | Ever out | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Wave | Total | In FU | In inst. | Total | Deaths | Lost/NC | Refusal | Ineligible | Dropped |
| 2 | 16,028 | 15,660 | 368 | 2163 | 84 | 933 | 948 | 198 | 0 |
| 3 | 15,430 | 15,099 | 331 | 598 | 74 | 235 | 205 | 84 | 0 |
| 4 | 15,029 | 14,707 | 322 | 401 | 97 | 94 | 147 | 63 | 0 |
| 5 | 14,608 | 14,313 | 295 | 421 | 113 | 150 | 85 | 73 | 0 |
| 6 | 14,168 | 13,863 | 305 | 440 | 100 | 125 | 146 | 69 | 0 |
| 7 | 13,767 | 13,464 | 303 | 401 | 91 | 129 | 132 | 49 | 0 |
| 8 | 13,387 | 13,090 | 297 | 380 | 98 | 109 | 128 | 45 | 0 |
| 9 | 12,916 | 12,627 | 289 | 471 | 87 | 136 | 191 | 57 | 0 |
| 10 | 12,523 | 12,216 | 307 | 393 | 90 | 109 | 152 | 42 | 0 |
| 11 | 12,207 | 11,888 | 319 | 316 | 63 | 84 | 123 | 46 | 0 |
| 12 | 11,834 | 11,522 | 312 | 373 | 72 | 104 | 155 | 42 | 0 |
| 13 | 11,443 | 11,142 | 301 | 391 | 91 | 130 | 115 | 55 | 0 |
| 14 | 11,125 | 10,790 | 335 | 318 | 75 | 95 | 126 | 22 | 0 |
| 15 | 10,858 | 10,537 | 321 | 267 | 86 | 48 | 106 | 27 | 0 |
| 16 | 10,544 | 10,226 | 318 | 314 | 81 | 60 | 149 | 24 | 0 |
| 17 | 10,214 | 9,901 | 313 | 330 | 93 | 64 | 158 | 15 | 0 |
| 18 | 9,861 | 9,592 | 269 | 353 | 94 | 61 | 176 | 22 | 0 |
| 19 | 9,492 | 9,206 | 286 | 369 | 83 | 96 | 164 | 26 | 0 |
| 20 | 9,167 | 8,920 | 247 | 325 | 95 | 84 | 127 | 19 | 0 |
| 21 | 8,878 | 8,684 | 194 | 289 | 96 | 65 | 106 | 22 | 0 |
| 22 | 8,583 | 8,424 | 159 | 295 | 78 | 55 | 148 | 14 | 0 |
| 23 | 8,359 | 8,249 | 110 | 224 | 75 | 48 | 92 | 9 | 0 |
| 24 | 8,126 | 8,036 | 90 | 233 | 90 | 45 | 95 | 3 | 0 |
| 25 | 7,873 | 7,808 | 65 | 253 | 83 | 64 | 93 | 13 | 0 |
| 26 | 7,487 | 7,434 | 53 | 386 | 90 | 159 | 135 | 2 | 0 |
| 27 | 7,152 | 7,106 | 46 | 335 | 99 | 115 | 117 | 4 | 0 |
| 28 | 6,917 | 6,881 | 36 | 235 | 77 | 60 | 94 | 4 | 0 |
| 29 | 6,726 | 6,683 | 43 | 191 | 75 | 48 | 60 | 8 | 0 |
| 30 | 4,861 | 4,839 | 22 | 198 | 69 | 46 | 66 | 17 | 1667 |
| 31 | 4,581 | 4,557 | 24 | 280 | 154 | 43 | 80 | 3 | 0 |
| 32 | 4,332 | 4,313 | 19 | 249 | 152 | 22 | 66 | 9 | 0 |
| 33 | 4,112 | 4,093 | 19 | 220 | 138 | 13 | 66 | 3 | 0 |
| 34 | 3,940 | 3,916 | 24 | 172 | 106 | 5 | 58 | 3 | 0 |

Table 3.2: Number of original core sample members whose participation has never lapsed, overall, by residency status (family unit vs. institution), and type of nonresponse.

special attention, as I will described below. Discussion of the results will mostly focus on attrition through non-contact and attrition through refusal as they offer the most potential for intervention by researchers.

## 3.4 Modeling attrition

Discrete-time hazard models are particularly well suited to the modeling of panel attrition. As the name implies, these models are extensions of survival regression (Cox, 1972) applicable to survival data for which time is not measured continuously (Allison, 1982, 1984; Singer and Willett, 1993, 2003). Several aspects of these models make them particularly appropriate given my research question and the nature of the PSID data. These models (1) allow for the inclusion of time-varying covariates, (2) allow for the effect of covariates to be time-dependent and (3) take into account the timing of events. In addition, (4) discrete-time hazard models can be set up to accommodate the occurrence of several different kinds of events. Such models are called competing-risk discrete-time hazard models and will be used throughout the rest of this chapter. All analyses were conducted using the logit command in Stata® (StataCorp, 2007).

### 3.4.1 Finding an appropriate time specification

The first step in modeling discrete-time data is to find an appropriate specification of the relationship of hazard with time (Singer and Willett, 2003). The hazard is the conditional probability that individual $i$ will experience the target event in time period $j$ given that he or she did not experience it in any earlier time period, which can be expressed algebraically by:

$$h(t_{ij}) = Pr\{T_i = j | T_i \geq j\} \tag{3.1}$$

where $T_i = j$ represents the moment at which the event occurs and $T_i \geq j$ represents the time elapsed prior to the occurrence of the event.

In other words, the hazard is the proportion of sample members experiencing the event at any given time, conditional on being at risk of experiencing the event (i.e. not having experienced the event prior to that point). In the present chapter, I will model the different sources of attrition separately as they have been shown to have different determinants (Groves and Couper, 1998; Lepkowski and Couper, 2002). In such a context, the hazard is defined as the proportion of sample members experiencing the event of interest at any given time, conditional on not having experienced any other competing event at this time point or any time point prior. This conditional probability, when calculated from the data, is commonly referred to as the empirical hazard.

Cases that were dropped due to budget constraints at wave 30 need careful handling when calculating the hazard. They are considered censored at wave 30 for the purpose of the discrete-time hazard model which means that they are no longer considered at risk of experiencing the events of interest after wave 30. They are therefore excluded from the computation of the hazard after this time point.

Figure 3.2 displays the empirical hazard of attrition by reason of non-contact,

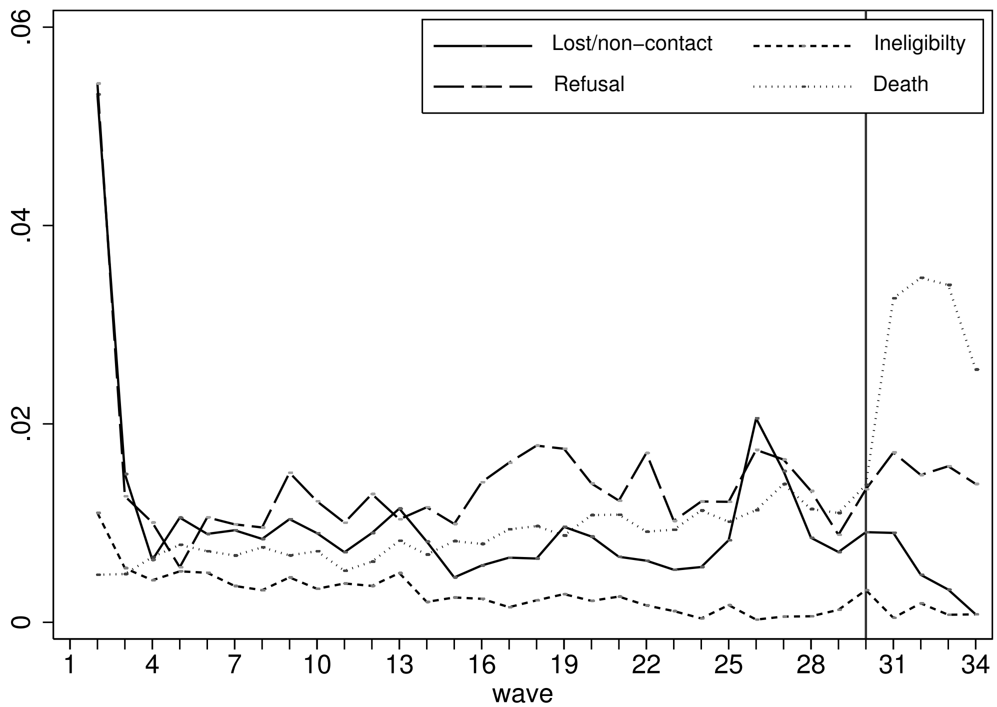



Figure 3.2: Empirical logit of competing-risk discrete-time hazards.

refusal, ineligibility, or death. At first sight, the empirical logit hazard displayed in figure 3.2 appears to follow a rather constant function aside from a sharp drop between wave 1 and wave 2. However, a close inspection reveals quite a bit of irregularity throughout but especially in the most recent waves.

Finding the best specification of the relationship of hazard with time is done through the fitting of a basic model in which the logit of hazard is regressed on time. Time can be expressed in many different ways that each represent different hypotheses on how the hazard is related to time in the panel. Due to the irregular pattern displayed by the logit hazard in figure 3.2, it seems implausible that a smooth function could fit well with these empirical logit hazards. Therefore, a model of the form:

$$\operatorname{logit} h(t_J) = \alpha_2 D_2 + \alpha_3 D_3 + \ldots + \alpha_{34} D_{34} \tag{3.2}$$

where $\operatorname{logit} h(t_J)$ represents the fitted logit of hazard and $D_1 through D_{34}$ represent the dummy indicators for each of the wave, seems like a good solution. The use of the logit scale in modeling will prevent the model from producing implausible values of hazard (i.e., values greater than 1 or smaller than 0). Hence, the logit transformation plays here the same role it does in logistic regression (Kohler and Kreuter, 2005, pages 246-249).

Not surprisingly, the fitted logit derived from this model fit the data very well. However, it is better to try out various specifications and test how the goodness-of-fit statistics fare in comparison to the general specification with one dummy variable per wave. This process is especially important for studies in which there are several time points such as the PSID. In such cases, failure to find a more parsimonious specification of time will limit the ability to allow the main effect of predictors to vary with time (Singer and Willett, 2003). The ability to specify time-varying effects is one of the key advantages of survival analysis.

Alternate models should have deviance statistics comparable in magnitude to the deviance associated with the general model. The difference between the two deviances is to be evaluated against the critical value from the $\chi^2$ distribution with $J-k$ degree of freedom where J = number of parameters in the general specification and $k$ = number of parameters of the model being tested. Failure to reject the null hypothesis is an indication that the reduced model is an acceptable alternative. In other words, more complex (less parsimonious) models should improve on simpler models enough to justify the increase in degrees of freedom. To be acceptable, the goodness-of-fit of this model should also not be significantly different than that of the general specification. In the present case, the irregular aspect of the hazard poses a particular challenge to finding a simple specification of time with as few parameters as possible. As a matter of fact, when compared to the gold standard, i.e., a model that includes a dummy variable for each wave, none of a series of simpler specifications provide a deviance that is as low (non-significantly different) than that of the gold standard specification.

Regarding the time specification retained, it is important to note that each time dummy indicator represents a wave, not an interview year. This choice effectively ignores an important change to the PSID design. In the late 1990's, the PSID switched from an annual to a biennial interview schedule. Therefore, the last annual interviews were conducted in 1997 (wave number 30, represented by the vertical line in the second half of figure 3.2) and biennial interviews were conducted thereafter in 1999 (wave 31), 2001 (wave 32), 2003 (wave 33), 2005 (wave 34), and 2007 (wave 35)[7]. As can be seen in figure 3.2, there is no major change in hazard for non-contact as a result of this design change, which is contrary to expectations. I would have expected the switch to a biennial interview schedule to increase the lost/non-contact rate. As a result, it seems reasonable to ignore the switch to a different schedule in the time specification.

Fitting a hazard model that includes an appropriate specification of time but

[7] Wave 35 is not part of the present analysis.

no substantive predictors yields the baseline hazard, that is the function representing the evolution of the hazard rate through time. When the general specification is used, the baseline hazard is exactly equal to the empirical hazard calculated from the data. Once a proper specification of time has been found, the next modeling step is to include the predictors in the model. The addition of predictors will result in the baseline hazard curve to "move" up or down depending on the whether the effect of the predictor increases or decreases hazard.

### 3.4.2 Accounting for non-independence of observations

The PSID data does not meet the independent and identically-distributed (*iid*) assumption on which most standard statistical procedures rely. Failure to address the violation of this assumption can lead to an underestimation of the magnitude of standard errors in the attrition models. In the PSID, the *iid* assumption is violated through the use of clustering and stratification in the design of the sample. These features are meant to increase the efficiency of the sample design. However, they also induce unequal probabilities of selection which must normally be corrected through the use of weights, themselves a source of increased variance.

Clustering often leads to observations within a cluster being correlated with each other because observations close to each other are more likely to be similar. This correlation, called intra-class correlation (ICC), can either be treated as a nuisance or as a substantively interesting phenomenon that can be modeled. Treating the non-independence of observations as a nuisance can be done through the use of the Huber-White variance estimator (Williams, 2000; Froot, 1989). This strategy will allow the computation of the correct standard errors but will not allow the partition of variance between the different clustering levels. Treating the clustering of observations as a substantive phenomenon can be done through the estimation of a random effect model which allows for the partition and modeling of variances at each level (Raudenbush and Bryk, 2002).

Regardless of the route taken, the question of what level of clustering to include in the model must be resolved. In the PSID, a three-level structure can be identified in which individuals are nested in families[8] which are themselves nested in primary sampling units (PSU). In principle, there would normally be an additional level of clustering induced by the repeated observations of each individual. However, the special set up used in discrete-time hazard models effectively addresses the presence of multiple observations per individual, so this level of clustering can safely be ignored. As a matter of fact, including this lowest level of clustering in the model in combination with an unstructured specification of time would lead to an unidentifiable model (Allison, 1982, pages 81-84).

In the absence of theoretical arguments in favor of one level over the other, it could be argued that both PSU's and families play an equal role in the attrition process and that both should be included in the model. However, fitting a three-level model can be particularly challenging as well as computationally demanding. One way to get around this problem is to include only one level of clustering in

[8]Addresses, not families, are really the units sampled at the second stage. On rare occasions, multiple families can live at the same address which results in a cluster of families. The PSID included all families found at an address in the sample but such families were excluded in the present analysis.

the model. From a theoretical point of view, the family unit seems like a obvious choice, especially given its importance in the participation decision. As mentioned above, the PSID makes extensive use of proxy reporting to collect information about household members. Participation in the interview is typically sought from the head of household only, essentially making the participation decision in the PSID a household-level phenomenon.[9]

This choice is confirmed by an examination of the intra-class correlations, *rho* ($\rho$), which is a measure of the homogeneity of the clusters. The *rho* values presented in table 3.3 overwhelmingly point toward family as the most homogeneous factor. Indeed, family shows consistently higher values of intra-class correlations than PSU for all outcomes. However, homogeneity measures such as *rho* do not take into account the size of the clusters. Doing so, for example by calculating an average design effect, $deff = 1 + \rho(n-1)$, where *rho* is the intra-class correlation and n is the average size of the clusters, would lead to a different choice. Because design effect takes into account the size of the clusters and PSU's are on average much bigger than family units, looking at *deff* would suggest the use of PSU as the clustering unit. However, the homogeneity of the cluster is the real focus and I based my decision on that criterion.

The way to deal with stratification is, to some extent, related to the decision on clustering. As shown above, PSU's are weakly correlated with attrition. Such a finding is not surprising considering the long history of the PSID. In the four

[9]Other members of the family unit will, under limited circumstances, be solicited for interview in lieu of the head of household but the fact remains that the PSID interview is a household-level interview.

| | Wave 1 PSU | | Wave 1 FU | |
|---|---|---|---|---|
| Model | rho | deff | rho | deff |
| Non-contact | 0.029 | 8.9 | 0.317 | 1.8 |
| Refusal | 0.039 | 11.6 | 0.336 | 1.9 |
| Ineligibility | 0.086 | 24.2 | 0.347 | 1.9 |
| Death | 0.026 | 7.9 | 0.308 | 1.8 |
| All causes | 0.017 | 5.6 | 0.380 | 2.0 |

Table 3.3: Comparison of primary sampling unit (PSU) and family unit (FU) intra-class correlations and design effects for the all-type, non-contact, refusal, ineligibility and death hazards.

decades since the PSID sample was drawn, a considerable proportion of the sample members are likely to have moved out of their original PSU's, resulting in a weakening of the intra-correlation on a range of attributes including attrition.

A similar argument can be made with geographical stratification. There is probably little relationship between strata membership in 1968 and strata membership in more recent years of the panel, leading to strata that have little meaning and, in conjunction with geographical clustering, can introduce much complication into the analysis. Indeed, taking both PSU's and strata into account would have the consequence of severely limiting the number of degrees of freedom in the model. The rule of thumb for determining the maximum number of degrees of freedom in such a context is to subtract the number of strata from the number of primary sampling units (PSU) (Korn and Graubard, 1999, pages 193-203). The PSID design comprises 64 PSU's allocated in 32 strata (2 PSU's per strata) which leaves a maximum of 32 degrees of freedom (64-32=32). This number could be even lower in situations where the analysis is restricted to a particular subset of the data (Burns, Morris, Liu, and Byron, 2003). Given that the unstructured specification of time already requires the inclusion of 33 dummy variables, taking up one degree of freedom each, no degrees of freedom would be left for the inclusion of substantive predictors in the model.

One last design feature that needs careful consideration is weighting. Varying probabilities of selection between the SRC and SEO sub-sample would normally require the use of weights (Hill, 1992). However, the individual weights available in the PSID are not pure selection weights. In addition to the varying probabilities of selection, these weights also correct for nonresponse to the initial interview. In order to assess the impact of not including the weights, all the analyses were performed weighted and unweighted and separate models were created for the SRC and SEO samples. As can be seen in appendix D, using these weights in the attrition model has very little effect on the coefficients in the model. The differences between the SRC and SEO models are also minor and will be discussed below.

All analyses presented in this chapter account for the clustering of observations within families through the use of the Huber-White Sandwich estimator (Rogers, 1993; Williams, 2000). Stratification of the sample will not be taken into account. Finally, unless otherwise noted, all models are unweighted.

### 3.4.3 Adding predictors of attrition to the model

Attrition can be viewed as being a function of four sets of determinants. In reverse chronological order from the moment attrition is recorded, there is the fieldwork parameters and other design features under the control of the researcher (1).

Then comes the life events experienced by the sample members between the most recent interview and the moment of attrition (2), followed by the quality of the respondent's experience during the prior interview (3). Fixed sociodemographic characteristics such as race and gender come last (4). Given the availability of data, I will restrict my investigation to life events, which are the main focus of the present study, some survey design features, and sociodemographic characteristics.

Some predictors are individual-level covariates but the majority are family-level covariates. From a modeling standpoint, two types of predictors can be distinguished: the fixed attributes, which remain constant over time for each respondent, and the time-varying covariate which value may change from wave to wave. The life events and shocks are by definition time-varying covariates while the sociodemographics are fixed attributes.[10] Survey design features are also time-varying covariates. They refer to attributes of certain individuals who are treated differently than the majority of the sample through a design decision potentially relevant for attrition (e.g. the decision not to interview people living in institutions).

#### 3.4.3.1 Life events

As mentioned above, life events are thought to be the main drivers of attrition. However, studying them ideally requires the availability of information about what happens to the sample members between waves, particularly the period between $t$, the moment at which attrition is recorded, and $t-1$, the prior interview.

[10] The only exception being age, a time-varying covariate. However, age evolves in a deterministic fashion i.e. one year is added each wave.

By definition, since the focus of this research is on studying attrition, information for the reference period immediately preceding the point of attrition, $t$, is missing. Consequently, the next best source of information about these sample members who attrite is their status at the previous reference period and that is what will be used in the present study. The following life events have been included in the model: *home ownership*, *presence of young children*, and *informant other than head*.

*Home ownership* is a time-varying family-level covariate taking a value of 1 if the family owned the dwelling it occupied at the previous wave and 0 otherwise. Renters, individuals who do not own their dwelling, have been found to be more difficult to locate than homeowners (Zabel, 1998; Lepkowski and Couper, 2002; Abraham, Maitland, and Bianchi, 2006). Some studies have found lower levels of survey cooperation among renters but did not make a distinction between non-contact and refusals (Rizzo et al., 1996; Zabel, 1998; Fitzgerald et al., 1998). As renters have less residential stability (Schachter, 2004), I would expect renters to show higher attrition through non-contact but I would not expect any effect on attrition through refusal.

*Presence of young children* is a time-varying family-level covariate taking a value of 1 if at least one child aged less than 6 lived in the family at the previous wave and 0 otherwise. The presence of young children in a family increases the proportion of time spent at home which then leads to easier contacts (Groves and Couper, 1998). Despite being presumably busy with child care, families with young children also tend to be more likely to participate in surveys (Groves and Couper, 1998; Zabel, 1998), although one study has found no effect of the presence of young children on response propensity (Abraham et al., 2006). Given the preponderance of evidence in favor of children increasing cooperation (Groves and Couper, 1998, page 138-139), I would expect it to decrease the probability of both attrition through non-contact and attrition through refusal at the next wave.

*Informant other than head* is a time-varying family-level covariate taking a value of 1 if an adult other than the head of household was interviewed at the previous wave and 0 if the head of household was interviewed at the previous wave. As mentioned above, the PSID favors interviews with the head of household. However, when this person cannot be interviewed because of incapacity or declines to be interviewed, an attempt to obtain information through another adult member of the family will be made, typically the spouse of the head. Such a switch in respondent may be an indication that a traumatic health event occurred to the head. Such an event might spur other changes, such as a move, which might make it more difficult to contact the family. I therefore expect that such a switch would result in more attrition through non-contact. However, a switch in respondent can also be viewed as a breach in commitment that the head of household may have toward the panel (Laurie et al., 1999). Such commitment may not be transferable to other household members which would then result in more attrition through refusals for individuals living in households where such a switch occurred.

### 3.4.3.2 Survey design features

Although design features are usually fixed for a given survey, changes can sometimes be introduced as part of a carefully planned schedule (Groves and Heeringa, 2006) or due to changing circumstances. Changes made to the design of the PSID fall in the latter category. In most cases, these changes were applied to the entire sample at once which means that their impact on attrition cannot be assessed.

However, some design features have remained constant through the years. Such features are interesting because of the specific treatment they reserve for specific groups of panel members and the potential impact such treatments may have had on the attrition of these groups. Three such design features have been included in the attrition models: *living in an institution*, *being a minor*, and *being part of the SEO sample*.

*Living in an institution* is a time-varying individual-level covariate derived from the final disposition code at previous wave. It takes the value 1 if the individual in question was living in an institutional setting at previous wave (prison, hospital, military, or religious order) and 0 otherwise. In household surveys such as the PSID, individuals living in institutions are by definition not eligible to be interviewed. This rule is applied slightly differently in the PSID. Panel members are not interviewed for as long as they live in institutions but they are still members of the panel which means that if they ever return to living in a household, interviewing will resume if they can be located. During the time away in institution, panel members are kept track of mostly through information provided by family members. The involvement of a third party is not optimal and might lead to increased attrition for panel members who experience a spell in institution.

*Being a minor* is a time-varying individual-level covariate taking on a value of 1 if the individual in question is less than 18 years old and 0 otherwise. By design, individuals less than 18 years old are not eligible to be interviewed in the PSID. As long as they live in the same household as an adult panel member, information is collected about them through that adult panel member, usually the head of household. In other words, up to their $18^{th}$ birthday the participation of young panel members is limited to being part of the sample. Whether or not information is collected about them depends entirely on the head of household. Nonetheless, they are an integral part of the sample and they play an important role in the perenniality of a lifetime panel such as the PSID. In theory, following rules ensure that these individuals are followed and interviewed when they establish a household of their own. However, this transition involves some level of disruption that might complicate the tracking of these individuals. Setting the cut-point at 18 years old is of course not ideal as transitions to adulthood do not all occur at the same time across individuals and across birth cohorts (Winsborough, 1979; Furstenberg, 2000; Blatterer, 2007). However, the 18 year old mark coincides with the moment at which an individual becomes eligible to be interviewed in the event that they establish a household of their their own, and as such, it is an important milestone according to the PSID following rules[11].

[11] Individuals who leave their household before turning 18 or individuals who become orphan before turning 18 become ineligible to be part of the PSID.

*Being part of the SEO sample* is a time-invariant individual-level covariate. It takes the value of 1 if the individual was part of the Survey of Economic Opportunity (SEO) sample and 0 if the individual was part of the Survey Research Center (SRC) sample. As mentioned above, the core sample of the PSID is composed of two sub-samples with distinct histories. The SRC sample was a fresh probability sample of the general population while the SEO sample had been interviewed as part of the Survey of Economic Opportunity, prior to the inception of the PSID. To take into account this difference, separate models for the SEO and SRC samples have been created. This strategy allows the effect of predictors to be different for each sample. In models combining both samples, the SEO indicator was only included as main effect.

#### 3.4.3.3 Sociodemographic characteristics

Sociodemographic characteristics have been shown to be related to nonresponse in surveys (Groves and Couper, 1998). However, given that a lot of emphasis has been put on dynamic processes as potential determinants of attrition in the theoretical background, sociodemographic characteristics are not the primary focus of the current research. As a consequence, they are expected to have a marginal effect on attrition after the inclusion of the predictors described above.

Some sociodemographics such as educational attainment are dynamic in nature — educational attainment almost certainly changes over the life course, but the frequency with which this construct is measured in the PSID makes it all but impossible to use as a truly dynamic variable.[12] Other attributes such as gender and race are more easily thought of as fixed characteristics, although it is increasingly subject to debate, at least in the case of race (Eschbach et al., 1998; Quintana et al., 2006). Despite these caveats, the following sociodemographic characteristics have nonetheless been included in the attrition models:

*Age*, a time-varying individual-level covariate. The interest of age as a potential predictor of attrition lies in the fact that it can be construed as a proxy indicator for the transition between the different life stages (Elder, 1975), periods that are by definition rich in life events. As mentioned above, life events are thought to be the main factors driving attrition. Age is an imperfect indicator of life transitions however. Using age in such a way implies a normative definition of the life course that might hold for all individuals, especially for individuals born at different time periods, as is the case in the core sample of the PSID. The delayed entry into adulthood that has been observed in recent decades (Furstenberg, 2000; Blatterer, 2007) means that cohorts born more recently experience their transition to adulthood at a later age, weakening the usefulness of age a a proxy for age life transition.

The literature shows mixed results of the effect of age on response rate and refusal rates. In general, older people tend to be easier to contact because they spend more time at home. At the same time, they are more likely to refuse, potentially due to social disengagement (Groves and Couper, 1998). When looking at the effect of age on panel attrition, some studies have found a positive curvilinear

[12]Educational attainment has only been measured at wave 1 and wave 18 for the original core sample members. In addition, educational attainment is measured whenever someone becomes a new head of household.

relation of attrition with age which means that the younger and older individuals are both likely to attrite (Fitzgerald et al., 1998; Zabel, 1998). This is to some extent consistent with the life transitions that these age groups experience (i.e. transition to adulthood and transition to retirement). For this reason, both a linear and a quadratic term are included in the models.

*Gender*, a time-constant individual covariate taking a value of 1 for men and 0 for women. Studies are divided on the effect of gender: they either show no effect (Zabel, 1998) or a tendency for male to be less likely to cooperate in cross-sectional surveys (Smith, 1983) and more likely to attrite in panels (Lepkowski and Couper, 2002). The PSID, with its respondent rule favoring heads of household (mostly men) as respondents, could be particularly vulnerable if indeed men are less likely to cooperate in panel surveys as well (Groves and Couper, 1998).

*Race of head*, a time-varying family-level covariate with three categories: white (reference category), black, and other. There is little evidence in the cross-sectional nonresponse literature that non-Whites have lower survey cooperation rates than Whites (Groves and Couper, 1998). O'Neil (1979) and Hawkins (1975) have presented evidence that blacks are more likely to respond to surveys but Weaver, Holmes, and Glenn (1975) have found a greater problem of non-contact among blacks. Smith (1983) has found no effect of race on cooperation. Previous studies of attrition are also inconsistent. No effect of race on attrition was found in Zabel (1998) but Fitzgerald et al. (1998) have found non-Whites to have a higher probability of attrition. When a distinction was made between non-contacts and refusals, this effect has been shown to be due to blacks being more difficult to locate

(Lepkowski and Couper, 2002). However, there is no reason to believe that race per se should have an effect on attrition once all life events are taken into account in the model. I believe that non-Whites experience more unstable trajectories which make them more likely to attrite through non-contact. If one were to include perfect measures of life events in the model, race would no longer have an effect on attrition. This is somewhat of an unattainable goal and race is included here to capture any residual effect not accounted for by the life events included in the model.

*Educational attainment of head*, a time-constant family-level covariate with 4 categories: high school not completed, high school completed (reference category), some college, and college degree. Research shows that lower education groups participate at a lower rate in surveys (Dohrenwend and Dohrenwend, 1968; O'Neil, 1979; Wilcox, 1977) and attrite at a higher rate (Fitzgerald et al., 1998). However,there is some evidence that lower education groups participate at a higher rate (Groves and Couper, 1998) but this result was only marginally significant.

## 3.5 Results

Predictors were first included in bivariate hazard models to test their potential as predictors of attrition. Models are evaluated by comparing their deviance, a fit statistic equal to the log-likelihood of the model multiplied by -2 (Singer and Willett, 2003). The difference in deviance follows an approximate chi-squared distribution with k-degrees of freedom. Only predictors that significantly improved the deviance statistics of the full model compared to the reduced model that included only time dummies were retained.

Models were created separately for each competing risk: attrition through refusal, attrition through non-contact, attrition through ineligibility and attrition through death. As mentioned above, I will focus on the models for non-contact and refusal but will occasionally refer to the ineligibility and death model to provide context to the results. An overall comparison model that makes no distinction regarding the reason for attrition —an "all-type" model— was also created. For each of these models, predictors were added in groups: sociodemographics were added first, followed by the design features and then life events. The different histories of the SEO and SRC samples suggest that this variable might be an important predictor that could capture some of the residual variance unexplained by previously included predictors so it was added separately to the models. The results of those model can be found in appendix E through I.

Not surprisingly, each group of predictors significantly contributed to the model. Most notable, however, are the results for the SEO sample indicator: it significantly improves the model for refusals ($\chi^2$ = 5.95, $df$=1, p<.05) but not the model for non-contact, ineligibility or death. This predictor also does not significantly improve the all-type model. This seems to indicate that the SRC/SEO distinction is an important one, at least for the refusal process, so I created separate models for the 2 samples as well as an overall model. These results can be found in appendix J.

In general, the direction and magnitude of the effects are similar across the SRC, SEO, and overall model combining both samples. However, there are a few notable exceptions such as race, age, gender, education, and the presence of young children in the family.

For example, in the non-contact model, living in a family in which young children were present reduced the odds of becoming non-contact by 32.6% ($t=-3.440$, $p<0.000$) compared to living in a family without young children. However, the same predictor had no effect on the odds of non-contact in the SEO sample. Also, each additional year of age decreased the odds of attrition through non-contact by 5.8% in the SRC sample but had no effect in the SEO sample. Finally, the magnitude of the effect of gender and race is different across the two sub-samples but the effects are in the same direction.

For the refusal model, holding a college degree, as opposed to a high school degree, lowered the odds of attrition through refusal by 25.1% in the SRC sample and by 56.2% in the SEO sample. Also, age increases the odds of attrition through refusal in the SRC sample but has no effect in the SEO sample. However, being less than 18 increases the odds of attrition in the SEO sample but not in the SRC sample. Most other coefficients show little difference across models. This seems to indicate that the SRC/SEO distinction is not very relevant to the refusal process, a finding that is in contradiction with the improvement in deviance brought about by the inclusion of the SEO indicator in the model. Given this lack of difference between the two sub-samples, further analysis will be conducted on the combined samples. However, a main effect of being part of the SEO sample was included in the overall model described next.

Table 3.4 displays the model results for the pooled SRC and SEO samples. The number of individuals experiencing the various types of attrition (*Experienced*

| | All-type | Non-contact | Refusal | Ineligible | Death |
|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) |
| Head's Race (ref: white) | | | | | |
| Black | 1.248 ** | 1.775 ** | .977 | 1.043 | 1.262 ** |
| | (.057) | (.155) | (.077) | (.147) | (.078) |
| Other race | 1.912 ** | 2.993 ** | 1.691 ** | 1.526 | .942 |
| | (.167) | (.427) | (.239) | (.394) | (.152) |
| Age | .935 ** | .967 ** | 1.009 + | .956 ** | 1.056 ** |
| | (.003) | (.007) | (.005) | (.010) | (.009) |
| Age squared | 1.001 ** | 1.000 | 1.000 ** | 1.001 ** | 1.000 ** |
| | (.000) | (.000) | (.000) | (.000) | (.000) |
| Men | 1.264 ** | 1.477 ** | .997 | .930 | 1.735 ** |
| | (.022) | (.050) | (.027) | (.055) | (.068) |
| Education (ref: high school degree) | | | | | |
| No high school degree | 1.108 ** | 1.121 | 1.040 | 1.139 | 1.241 ** |
| | (.043) | (.081) | (.069) | (.127) | (.065) |
| Some college | .910 | 1.038 | .827 + | .675 + | 1.054 |
| | (.053) | (.121) | (.082) | (.145) | (.076) |
| College degree | .675 ** | .611 ** | .672 ** | .540 ** | .804 ** |
| | (.044) | (.081) | (.072) | (.111) | (.066) |
| In Institution | 1.621 ** | 1.575 ** | 1.082 | 3.680 ** | 3.771 ** |
| | (.094) | (.133) | (.111) | (.808) | (.537) |
| Aged < 18 | .449 ** | .234 ** | .691 ** | 7.730 ** | 1.094 |
| | (.023) | (.024) | (.053) | (1.698) | (.243) |
| Home owner | .659 ** | .432 ** | .989 | .615 ** | .642 ** |
| | (.021) | (.029) | (.058) | (.058) | (.028) |
| Non-head informant | 1.334 ** | 1.114 | 1.459 ** | 1.079 | 1.362 ** |
| | (.050) | (.091) | (.095) | (.140) | (.066) |
| Presence of young children | .781 ** | .838 ** | .702 ** | .891 | .875 + |
| | (.032) | (.054) | (.047) | (.107) | (.069) |
| In SEO sample | .982 | 1.005 | .906 | 1.097 | 1.137 + |
| | (.044) | (.083) | (.066) | (.162) | (.075) |
| Wave Effects | Yes | Yes | Yes | Yes | Yes |
| Person-Waves | 316,651 | 316,651 | 316,651 | 316,651 | 316,651 |
| Experienced event | 12,200 | 3,508 | 4,667 | 1,073 | 2,952 |
| Observations (n) | 17,324 | 17,324 | 17,324 | 17,324 | 17,324 |
| df | 47 | 47 | 47 | 47 | 47 |
| Deviance | 97,198.35 | 33,856.92 | 46,665.41 | 12,656.45 | 26,413.15 |
| AIC | 97,294.35 | 33,952.92 | 46,761.41 | 12,752.45 | 26,509.15 |
| BIC | 97,666.82 | 34,325.39 | 47,133.88 | 13,124.92 | 26,881.62 |

+ p<0.10, * p<0.05, ** p<0.01

Table 3.4: Odds ratios and standard errors for the competing-risk discrete-time hazard model of attrition through non-contact, refusal, ineligibility and death; SRC and SEO samples combined. A model that does not make a distinction between these different types of attrition is provided in the first column as reference.

*event*) and the number of observations present at the onset of the panel (*Observations*) are displayed at the bottom of the table. Various model fit statistics such as the deviance, BIC and AIC (Long and Freese, 2006) are also included at the bottom of the table.

The first column of table 3.4 shows the results for an attrition model in which no distinction is made regarding the type of exit i.e. the "all-type" model introduced above. This model is displayed here for reference only, the real focus of this table being the competing-risk model displayed in the four last columns of the table. What is obvious from this table is how the picture of attrition is altered by looking at the various types of attrition separately.

Among time-varying covariates, home ownership at previous wave reduces the odds of attrition overall. However, the effect is very different when looking at non-contact and refusal separately. While home ownership is still associated with a reduction in the odds of attrition through non-contact (OR=0.432, t=-12.441, p<0.000), it has no effect on the odds of attrition through refusal. This means that while homeowners are easier to contact than renters, they are not easier to get to participate. Another time-varying covariate, the switch to a non-head informant, shows a similar pattern. Living in a household in which an adult other than the head of household granted the interview at previous wave increases the odds of attrition overall (OR=1.334, t=7.623, p<0.000). However, this effect seems to be driven mostly by attrition through refusal. Indeed, a non-head informant increase the odds of attrition through refusal by 45.9% at the following wave compare to head informants. Being less than 18 years of age has a negative effect on attrition overall as well as on non-contact and refusals. This factor decreases the odds of exiting the panel through non-contact by 76.6% (OR=0.234, t=-14.303, p<0.001) and by 30.9% (OR=0.691, t=-4.845, p<0.000) for refusals.

Among time-constant covariates, gender also shows an interesting pattern. Overall, men have higher odds of exiting the panel than women. However, most of this effect is due to non-contact: the odds of attrition through non-contact for men are 47.7% (OR=1.477, t=11.542, p<0.000) higher than for women. However, men and women are not different when it comes to refusal.

Some covariates, however, do not show such striking differential association between non-contact and refusal. For example, the presence of young children in the family unit decreases the odds of both non-contact and refusal by 16.2% (OR=0.838, t=-2.760, p<0.05) and 29.8% (OR=0.702, t=-5.243, p<0.000) respectively. Having been in an institution at the previous wave increases the odds of attrition through non-contact but has no effect on the odds of attrition through refusal. The odds of attrition trough non-contact for those who were institutionalized are 57.5% higher (OR=1.575, t=5.377, p<0.000) than their counterpart (individuals who were not institutionalized at previous wave).

Among time-constant covariates, holding a college degree (as opposed to a high school diploma) decreases both the odds of exiting through non-contact and refusal by 38.9% (OR=0.611, t=-3.739, p<0.000) and 32.8% (OR=0.672, t=-3.699, p<0.000) respectively.

## 3.6 Discussion

This chapter provides some evidence that individuals living in households with more stable lives are less likely to attrite. For example, households who own their home are likely to have stronger residential stability (Schachter, 2004) which makes wave on wave contact easier. As expected, no evidence of an effect of home ownership on refusal was found.

I have also found that individuals living in households with young children are less likely to attrite through either non-contact or refusal. Such a result is consistent with Zabel (1998), although no distinction was made between non-contact and refusals. Individuals with young children are likely to spend more time at home and consequently be easier to contact (Groves and Couper, 1998), leading to a lower attrition rate. However, these households are also more likely to be busy, leaving in theory less time to participate in the panel, which should in turn increase the probability to refuse. This, however, is in contradiction with my finding that household with young children are *less* likely to attrite through refusal. A way to reconcile these findings is to consider that having more stable lives, however busy, also makes people more stable in the decisions they make, which would translate into a decreased probability to attrite through refusal.

As expected, I have found a positive curvilinear relationship of age with attrition (Fitzgerald et al., 1998; Zabel, 1998). This relationship indicates that younger as well as older people are more likely to attrite, an effect that is due to these categories refusing at a higher rate. However, the very young people, i.e. individuals who have joined the panel as non-adult sample members, were also found to be less likely to attrite. This result is inconsistent with the instability that young people experience as they transition to adulthood and establish their own household; such events should make these young people ultimately harder to contact. For such young people, the main source of attrition seems to be ineligibility, as evidenced by the odds ratio of 7.7. Ineligibility occurs when the adult sample member(s) who are the legal guardian(s) of the child die or become disabled. Given that the PSID does not seek interviews from minors, such individuals become ineligible by design.

Finally, individuals who have lived in institutions are also more likely to attrite through non-contact, indicating that the challenge with individuals who have lived in an institution is to keep track of them rather than to convince them to maintain their participation. While this factor captures some of the instability in individual trajectories — living in institutional setting almost automatically involves a move of some sort, and may be a sign of some incapacity to be interviewed — it also captures the effect of a design decision made by the PSID not to interview people living in institution. According to the PSID follow-up rules, individuals living in institutions are kept track of but are not interviewed. If these individuals ever come back to living in household, they will be interviewed again. However, since no attempt is made by the PSID to contact these individual at their institution while they live there, the PSID is almost completely reliant on reports by family members and relatives to track these individuals. However, the ties between institutionalized individuals and their family might not always be optimal, which may complicate the tracking of such individuals.

Separate analyses for the SRC and SEO sample have shown that the direction and magnitude of the effects of the predictors examined are similar across the SRC and SEO samples which is why I focused on models pooling the two samples.

This chapter also confirms the importance of distinguishing between non-contact and refusal in attrition models, as previously demonstrated by Lepkowski and Couper (2002). To my knowledge, this is the first time that such a distinction has been made in the PSID. Prior attrition studies using the PSID have looked at attrition for any cause excluding death (Fitzgerald et al., 1998). As shown above, not distinguishing between the different sources of attrition often obscures the complex relationship between the correlates of attrition and the various sources of attrition. For example, home ownership has been shown to a have a positive effect on retention in the panel when looking at all sources of attrition combined. However, the competing-risk model that distinguishes between the different sources has revealed that this effect happens because homeowners are easier to contact not because they are more likely to agree to participate. A similar situation applies to living in institutions.

The distinction between attrition through non-contact and attrition through refusal has more than aesthetic implications as these two sources of attrition likely have different consequences for nonresponse error. Given that evidence seems to indicate that instability is more predictive of non-contact that refusals, focusing on the reduction of non-contact might be more important in terms of reducing bias as loosing people who experience instability likely introduces a bias in the estimate of change. However, in order to unequivocally conclude that change produces attrition, which in turns undermines the estimates of change derived from panel studies, one would need information that is available independently from the sample members' participation in the panel. Data of this nature would allow one to establish whether or not events or status changes occurring *after* the last occurrence of participation but *before* the moment where nonresponse is recorded are actually related to attrition. Data of this sort would typically take the form of administrative data that are available for all sample members. This could be achieved by building a panel of program beneficiaries. An alternative, perhaps more widely applicable to a variety of existing panels, would be data linkage to external sources. Depending on the quality and variety of this external data, linkage would allow for the independent computation of estimates of change, independent from the panel. This could provide "truer" determinants of attrition as well as some measures of bias.

# Chapter 4

# Using Latent-Class Models to Uncover Groups of Attriters with Specific Hazard Curves

## 4.1 Background

Several studies have focused on the modeling of panel attrition. Findings suggest that younger people, African-Americans, males, renters and low income people all have a lower probability of staying in panels (Zabel, 1998; Rizzo et al., 1996; Lepkowski and Couper, 2002; Fitzgerald et al., 1998). Results presented in the previous chapter are consistent with these previous findings. In addition, evidence presented in the previous chapter suggests that individuals living in households with less stable trajectories are more likely to attrite.

One of the weaknesses of attrition models is that they do not specifically address the issue of unobserved heterogeneity, i.e. residual variance in the dependent variable that cannot be modeled by any of the variables measured. This is not a problem specific to attrition models: all models based on observational data run the risk of being affected by this problem. Discrete-time hazard models, the type of model used in the previous chapter, address a number of difficulties associated with the study of survival data but leave the problem of unobserved heterogeneity intact (Singer and Willett, 2003). Models of attrition based on discrete-time hazard models may therefore suffer from bias in the estimated coefficients due to the presence of unobserved heterogeneity.

Random effects, also called *shared frailties* in the context of survival models (Jenkins, 2008), can be used as a way to get unbiased estimates of the coefficients in a model. Such a technique can generally be implemented without the need for a theoretical argument as to the nature of the heterogeneity. Random effect models are therefore invaluable tools in the analysis of observational data that can be implemented whenever repeated measures are available for each sample member. While this technique takes into account the idiosyncrasies of typical members of the population under study, it does not tell us what makes these individuals interesting with respect to the behavior of interest. Moreover, such models rely on the assumption that the random effect is normally distributed, which might not always be the case.

Latent-class models are another way to address the unobserved heterogeneity problems in hazard models. In contrast to random-effect models, they do not rely on the assumption of a normally distributed random effect. Rather, the residual variance in logit hazard probability is estimated nonparametrically and does not rely on the univariate normal distribution assumption (Masyn, 2003; Muthén and Masyn, 2005). This innovation is interesting because it means "different individuals can belong to different subpopulations without the subpopulation membership being observed but instead inferred from the data" (Muthén and Masyn, 2005, page

37). In other words, different groups of sample members (the latent classes that cannot be captured by any of the measured variables) are allowed different hazard functions under the model.

In the context of panel attrition, such latent-class models could be interesting for both substantive and practical reasons. From a substantive point of view, latent-class models could allow for the identification of potentially interesting groups of panel members that show distinct attrition behavior. Combined with a theory elaborating on the potential meaning of these groups, i.e. what they might represent, such models could assist in gaining a better understanding of the mechanism of attrition. From a practical point of view, latent-class survival models could eventually be used as a "diagnostic tool" at the onset of a panel as a first step in the implementation of attrition-reduction strategies tailored to various groups of panel members before they become attriters.

## 4.2 Theoretical mechanisms of attrition

One way to look at the problem of attrition is to consider the existence of several distinct attrition-producing mechanisms. Four such mechanisms can be contrasted, each focusing on a distinct key explanatory factor. These mechanisms represent specific prototypical developments that each play concurrent roles in the overall attrition process. Each mechanism is a purposeful simplification of the reality that describes the development of a specific type of panel member with respect to attrition. Latent-class models could prove useful in finding empirical evidence of the existence of these types of panel members.

In the aggregate, all these mechanisms have the same consequence: the cumulative proportion of attriters gradually increases over time, albeit not at a constant rate over time and not at the same rate for all mechanisms. Implicit in the following description of the various mechanism is the idea that the overall shape of the hazard function (Singer and Willett, 2003) is indicative of the underlying mechanism of attrition that generated it.

The first of these mechanisms focuses on *fatigue* as the driving force behind attrition: if panel members who all agreed to participate in the first wave eventually discontinue their participation, it is because they get tired of participating (Laurie et al., 1999). According to this hypothesis, panel attrition is the result of the cumulative burden (assumed to increase at the same rate for all respondents) imposed on the respondent over time. As the cost of participation increases with each additional wave, participants are likely to get weary. Attrition occurs when the cost of participation exceeds the effort that participants were initially willing to commit or the perceived advantage they derive from their participation.

This fatigue threshold is not the same for all panel members and its variation across individuals results in a decline of the number of panel members that spans several waves. Assuming a linear relationship of attrition with time and a uniform distribution of threshold throughout the sample would produce a constant attrition rate over time, as illustrated in figure 4.1(a). This process would result in a constant decline of the sample over time. Moreover, I would not expect any specific time point to be particularly conducive to attrition.

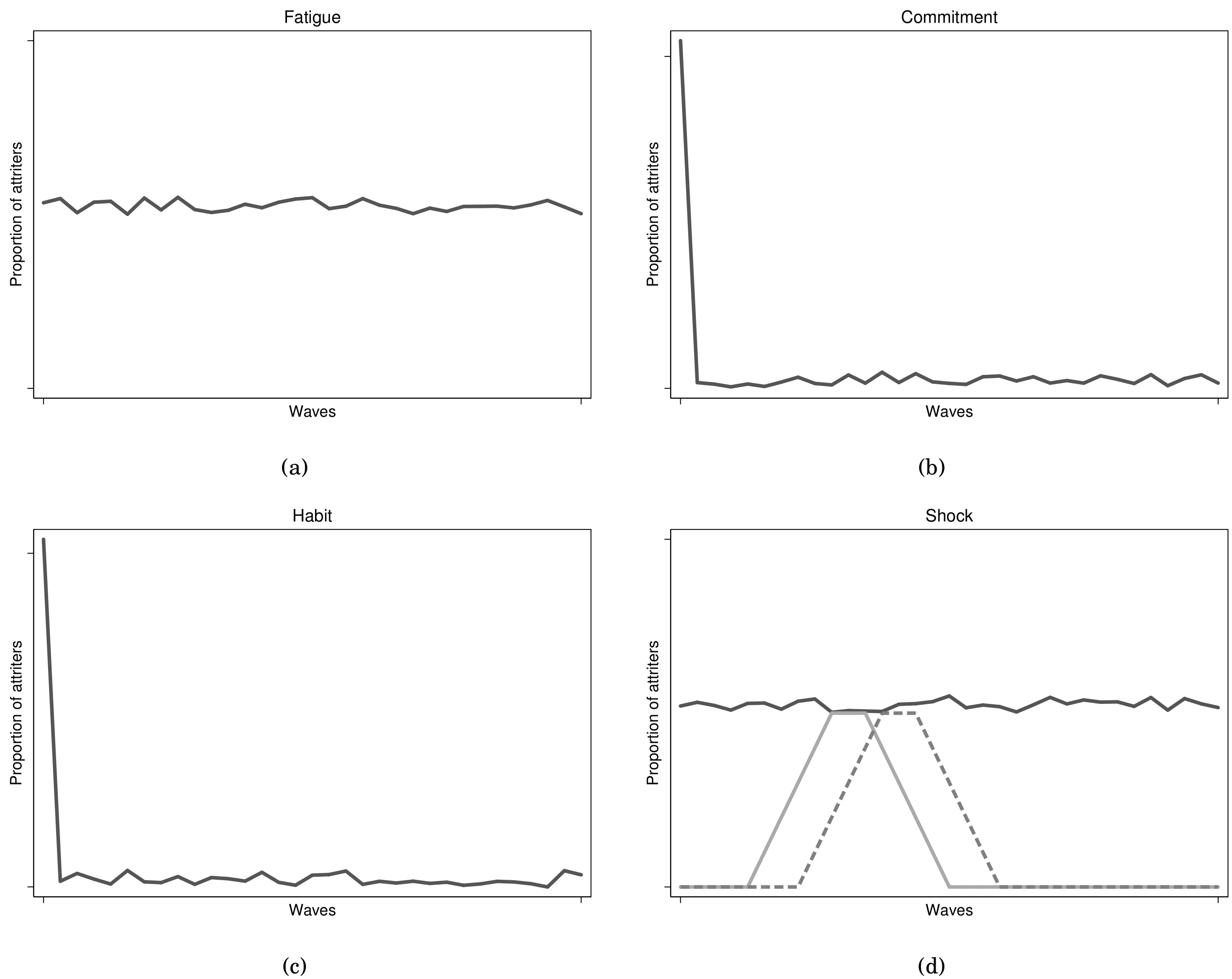



Figure 4.1: Graphical representation of the expected development through time of the proportion of attriters under four attrition mechanisms.

However, we know from experience in multiple panels that the relationship of attrition with time is not linear, a finding that also applies to the core sample of the PSID as shown in chapter 3. Attrition follows a decelerating function over time with the highest attrition rate observable in the first few waves of the panel (Wooden, 2001). This suggests that the fatigue mechanism as described above cannot solely explain attrition in the PSID and that some other mechanism applies, either to the whole sample or to a fraction thereof. Moreover, this mechanism implies that most attrition occurs through refusal. Indeed, under this mechanism, panel members participate for as long as they please but make a more or less conscious decision to not participate when they have had enough. Such decision is most likely conveyed through a refusal to participate rather than a non-contact. However, there is a need for mechanisms that explain both attrition through non-contact and attrition through refusals. The following two mechanisms address these two points.

The second mechanism focuses on the *absence of commitment* (Laurie et al., 1999) to explain attrition: if panel members attrite from the survey, it is because they were not fully committed to participating in the first place. Likewise, if they stay, it is because they are committed to the survey. The plausibility of this mechanism rests on the timing of the commitment decision. Do panel members make the decision to participate in the panel at wave 1 or do they wait to see how the interview will go before they pledge further commitment? Assuming that respondents are made fully aware prior to the wave 1 interview that they are being solicited to participate in a repeated survey[1], it seems plausible that people who refuse to

[1]Current research subject protection regulations require that panel members be informed that

commit would not even participate in the initial wave. Why would they participate at all if they do not intend to participate for as long as requested? It is important to note that such a scenario would deny the possibility of attrition: all nonresponse in panel surveys would be limited to the initial interview. At this moment, either panel members commit to the panel and participate in the first and all subsequent interviews or they do not. Obviously, such a scenario is unlikely to apply to a large proportion of the sample.

However, it is possible that panel members wait until wave 2 before committing to the panel. Prior to the wave 1 interview, panel members have little information on which to base their participation decision. At wave 2, they can use the information previously gathered about the survey to help them in their commitment decision. Alternatively, it could be that panel members use participation at wave 1 as an easy way out of the interviewer request, meaning that their decision to participate then should not have been interpreted as an agreement to participate beyond that first wave. Regardless of which alternative is true, the observed result would be the same: a higher rate of attrition between the first and second wave than for the rest of the panel as the fraction of non-committed panel members exit the survey early on (see figure 4.1(b)). Those who do participate in wave 2 can be thought to have committed while the reverse is true for those who do not participate. This more realistic version of the commitment scenario allows for attrition in the first wave of a panel but still does not allow attrition later on.

A third mechanism focuses on the *absence of participation habit*. According

they are being solicited to participate in a recurring survey.

to this hypothesis, panel participation is a process driven by habit, that is the repetition of past behavior given a stable context (Davidov, 2007; Ouellette and Wood, 1998). In practice, it means that once a panel member has agreed to participate in a panel, this decision is likely to be followed by continuous participation thereafter. From a rational choice point of view, decision-making is a costly process both in terms of the time and effort required for the collection and integration of information. Under this premise, it makes sense for the actor to minimize the expense of resources entailed in decision making by simply abiding by prior decision (Stigler and Becker, 1977). In our case, wave 2 would be the moment when habit is formed, that is, the moment when the behavior expressed at the prior wave is repeated. Those who do participate can be thought to have developed the habit while those who do not can be thought to have not developed the habit.

Although they are very different theoretical mechanisms, commitment and habit would produce similar attrition rate patterns over time. In their purest form, both the commitment and habit mechanisms do not allow for attrition at any other time than between wave 1 and wave 2. If either of these mechanisms were true, I would expect the proportion of attriters to be substantially different than zero between wave 1 and wave 2, and to fall to zero between wave 2 and wave 3 and for all time periods thereafter (see figure 4.1(b) and 4.1(c)).[2] The main difference between the two mechanisms resides in the fact that habits are more susceptible to disruption by change that occurs in the life of panel members than are commitments. Therefore, I would expect life events to have more of an impact on attrition

[2]The jagged line used after wave 2 represents non-significant variations in hazard over time.

if the habit scenario were true than if the commitment mechanism were true.

A fourth mechanism focuses on the *occurrence of life events* as the main factor behind attrition. Under this mechanism, panel members interrupt their participation because they experience one or more life events that disrupt their habit. In the habit mechanism presented above, the repetition of past behavior given a stable context was the factor driving participation in the panel. Here, the disruption of habits induced by life events (shocks) leads to attrition. In essence, this scenario is the counterfactual to the *participation habit* mechanism presented above.

If this scenario were true I would expect attrition rates to be at their maximum during stages in the life course that are particularly rich in life events or transitions (such as early adulthood or the end of active life). In practice, however, these peaks will tend to be obscured by the fact that different cohorts will experience each stage at different chronological times. Indefinite life-panel and other general purpose panels generally include individuals ranging the entire span of the life course[3]. Each of these cohorts will experience most of their transition at the same period in the life course but these periods will occur at different chronological times. As an illustration, consider figure 4.1(d). The peaks represent the attrition rate of two different cohorts each experiencing the same transition to adulthood but at different times. The repetition of this pattern for all cohorts would tend to maintain a rather constant attrition rate over time. The resulting development of attrition rates over time will be similar to the one expected in the fatigue sce-

[3]This is true even if you consider only the original sample members as I do here: lifetime-indefinite panels such as the PSID and the BHPS begin with a sample of the general population from newborns to the eldest.

nario. However, life events are expected to have an impact on attrition under the shock scenario whereas they are not expected to have an impact under the fatigue scenario.

When combined, these four mechanism allow for the distinction of four theoretical groups of panel members. These groups should not be conceived as deterministic. Rather they should be considered to be stable groups conditional on the design used in the survey and as such, these groups can expand or shrink depending on the survey design chosen. The group of *one-time participants / early attriters* will be present in the first wave but not in the subsequent waves. They are somewhat analogous to the group of participants in a cross sectional survey. The group of *repeated participants / late attriters* will participate in multiple waves but will eventually attrite either because of fatigue or because of the occurrence of disrupting life events. That leaves the group of *stayers* who will remain in the panel until their death.[4]

## 4.3 Analysis

Analyses presented in this section aim at identifying empirical evidence of the mechanism described above using discrete-time hazard mixture analysis (Masyn, 2003; Muthén and Masyn, 2005; Masyn, 2009). All analyses described were conducted with Mplus®, a statistical analysis software specifically designed for latent

[4]A group of *non-participants* can also be identified. Panel members that are part of this group would not participate in any wave, not even the first. They are analogous to the to the group of non-participants in a cross sectional survey. They will not be examined in the present study as the sample consists only of panel members who participated in the first wave.

variable modeling (Muthén and Muthén, 2007). My overall analytical strategy for this chapter is to build on a simplified version of the final discrete-time hazard model presented in table 3.4 of chapter 3.[5]

There are two distinct approaches that can be used to build a competing-risk discrete-time hazard models in *Mplus* (Masyn and Kreuter, 2004). The first approach relies on the specification of parallel processes in which each attrition type is modeled by a distinct and independent process. For example, the model presented in chapter 3 requires four distinct processes: one for attrition through non-contact, a second for attrition through refusal, a third for attrition through ineligibility and a fourth for attrition through death. This strategy is equivalent to fitting four logit models, one for each attrition type, on a properly formatted data set. A second approach relies on the specification of two distinct processes. The first process models the occurrence of attrition through any cause while a second process describes what type of attrition occurred (non-contact, refusal, ineligibility or death). This strategy is equivalent to fitting a single multinomial logit on a properly formatted data set.

These two strategies yield largely equivalent results (Jenkins, 2008). The small differences are due to additional constraints that are imposed in the multinomial model (Long and Freese, 2006). However, since the multiple logit approach is closer to the strategy used in chapter 3, I decided to also use it here for consistency.

I draw on Muthén and Masyn (2005) and Masyn (2009) for the notation used

[5]Prior to beginning the analysis specific to this chapter, I made sure I was able to exactly replicate the competing-risk discrete-time hazard model presented in chapter 3. This effectively guaranteed that I was on solid ground before moving on to more complex models with an unfamiliar software. The way to specify discrete-time hazard models in *Mplus* is indeed quite different than conventional statistical packages and I will get back to this point in the discussion.

in model equations described here. Model 0 is a one-class discrete-time hazard model that includes only time-invariant covariates. This model is similar to models presented in chapter 3 except that it does not include time-varying covariates. The logit of the hazard of individual $i$ at time period $j$, $h_{ij}$, is a function of the baseline hazard at time $j$, represented by $\beta_j$, and individual-level time invariant covariates $\mathbf{x}_i$:

$$\text{logit}(h_{ij}) = \beta_j + \kappa' \mathbf{x}_i \tag{4.1}$$

The baseline hazard, $\beta_j$, is assumed to be the same for all individuals, as shown by the absence of the subscript $i$ in equation 4.1. However, the baseline hazard is allowed to vary across time points (unstructured specification). This is represented in equation 4.1 by the presence of the subscript $j$. The three individual-level time invariant covariates included in model 0 are: *being part of the SEO sample*, an individual-level covariate taking a value of 1 if the individual was part of the Survey of Economic Opportunity sample and 0 if the individual was part of the Survey Research Center sample; *gender*, an individual covariate taking a value of 1 for men and 0 for women; and *educational attainment of head*, a family-level covariate with 4 categories: high school not completed (reference category), high school completed, some college, and college degree. These covariates are represented by the matrix $\mathbf{x}_i$.

Figure 4.2 displays model 0 graphically. For simplicity, only one covariate is represented in figure 4.2. The representation of this and more complex models fol-

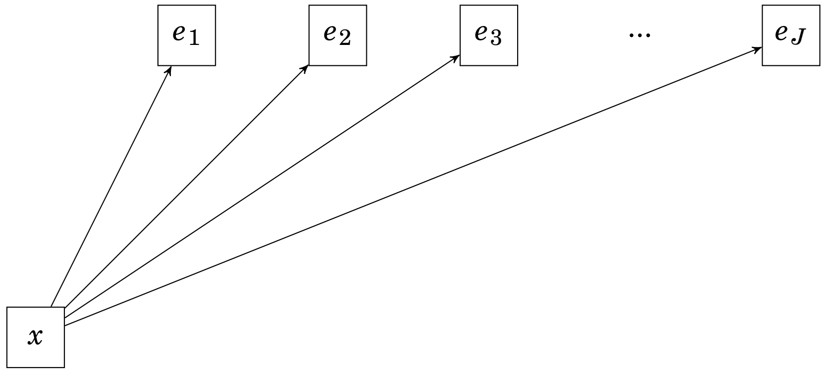



Figure 4.2: Path diagram of a single-class discrete-time hazard model with time-invariant covariates (Model 0). A single covariate, $x$, is shown for simplicity.

lows the general conventions of path analysis such that boxes represent observed variables, circles represent latent (unobserved) variables and the lines represent the relationship between the variables, either observed or unobserved (Loehlin, 2004). Following these principles, boxes $e_1$ through $e_J$ represent the observed outcome for each wave (for example, figure 4.2 shows the process of attrition through non-contact, $e_1 ... e_J$) and box $x$ represents a single time-invariant covariate (gender, for example) that can have an effect on the outcome. The indicators $e_1$ through $e_J$ take a value of 0 for as long as the sample member does not experience the event of interest, a value of 1 at the time of the event, and is set to missing thereafter.

Model 1 builds on model 0 through the addition of a continuous, $\eta$, and a categorical, $c$, latent variable that represents differences between individuals with respect to the logit hazard probability. This continuous latent variable is allowed to vary between classes $k$, as shown in equation 4.3 (Muthén and Masyn, 2005). However, the effect of time-invariant covariates $\mathbf{x}_i$ is constrained to be equal across classes, hence the absence of subscript $k$ for the $\kappa$.

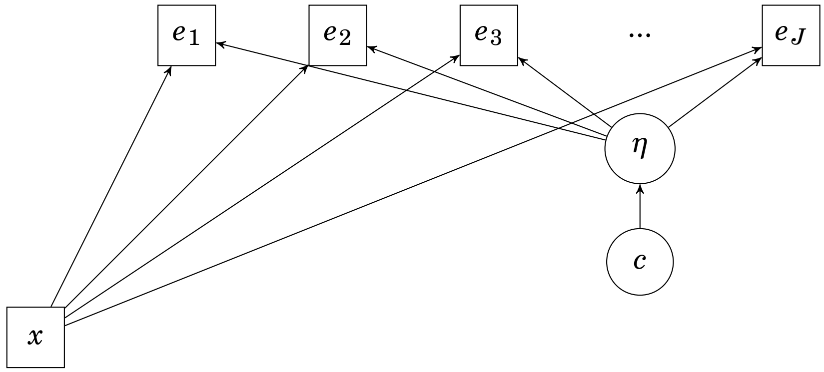



Figure 4.3: Path diagram of a multiple-class discrete-time hazard model with continuous $\eta$ and categorical $c$ latent variable (Model 1). A single covariate, $x$, is shown for simplicity.

$$\text{logit}(h_{ijk}) = \beta_j + \kappa' \mathbf{x}_i + \eta_i \tag{4.2}$$

$$\eta_i = \alpha_k \tag{4.3}$$

In this simple two-class model displayed in figure 4.3, $\eta$ is constrained to be equal to 0 in the first class and is free to take on any value in the second class.[6] This allows for the possibility of 2 classes, for example, one class of panel members that are not very likely to attrite, the "survivor class" ($\alpha_{k=1} = 0$), and another class that is very likely to attrite ($\alpha_{k=2} \neq 0$). To draw a parallel between model 1 and the mechanism presented above, the class referred to as the survivor class is made up of the fraction of people who are "habituated" or "committed" while the other class represents panel members who would quickly attrite from the panel.

Model 2 builds on model 1 by allowing the effect of time-invariant predictors $\mathbf{x}_i$ to vary across classes as shown by the subscript $k$ on the $\kappa'$ in equation 4.4.

[6]However, $\eta$ is not allowed to vary within classes.

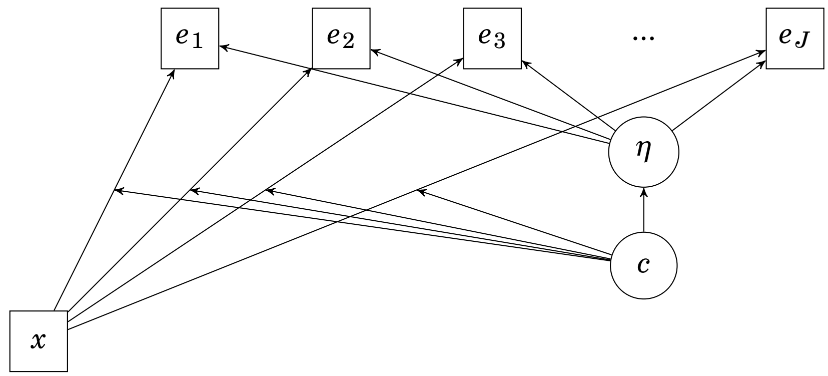



Figure 4.4: Path diagram of a multiple-class discrete-time hazard model with a class-varying effect of the time-invariant predictor $x$ (Model 2).

$$\text{logit}(h_{ijk}) = \beta_j + \kappa_k' \mathbf{x}_i + \eta_i \tag{4.4}$$

$$\eta_i = \alpha_k \tag{4.5}$$

This is shown in figure 4.4 by the arrows pointing from the categorical latent variable $c$ to the arrows representing the effect of $x$ on $e_1$ through $e_J$.

Model 3 is where time-varying covariates are introduced. Inclusion of the time-varying covariate in a two-class model is key for the purpose of teasing out the various attrition mechanisms. As stated above, life events (i.e time-varying covariates), are postulated to have less of an effect on attrition in the non-survivor class if the commitment mechanism were true than if the habit mechanism were true.

Consequently, the effect of time-varying covariates $z$, are allowed to vary across classes as shown by the $k$ subscript in equation 4.6. This is represented in figure 4.5 by the arrows pointing from the categorical latent variable $c$ to the

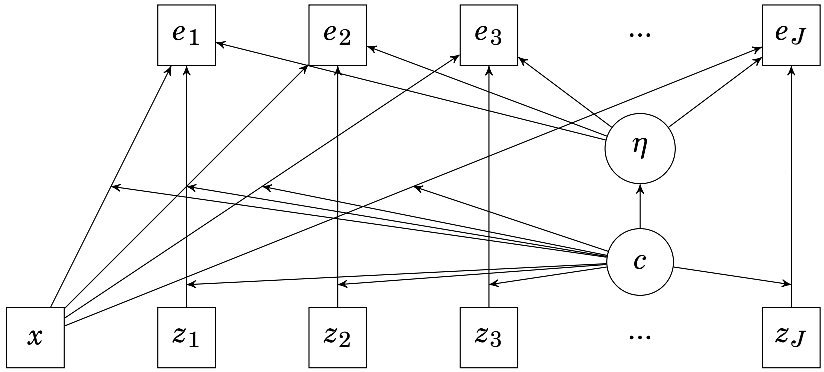



Figure 4.5: Path diagram of a multiple-class discrete-time hazard model with class-varying effects of both the time-invariant predictor $x$ and the time-variant predictor $z$ (Model 3).

arrows representing the effect of $z_1$ through $z_J$ on $e_1$ through $e_J$ respectively.

$$\text{logit}(h_{ijk}) = \beta_j + \kappa_k'\mathbf{x}_i + \xi_k'\mathbf{z}_{ij} + \eta_i \tag{4.6}$$

$$\eta_i = \alpha_k \tag{4.7}$$

Larger effects of the life event indicators in the non-survivor class would provide evidence that the shock scenario is true. Indeed, this would indicate that the group of panel members most likely to experience life transitions does attrite more, which is a defining characteristic of the shock scenario.

The seven time-varying covariates included in model 3 are: *home ownership*, a family-level covariate taking a value of 1 if the family owned the dwelling it occupied at the previous wave and 0 otherwise; *presence of young children*, a family-level covariate taking the value 1 if at least one child aged less than 6 lived in the family at the previous wave and 0 otherwise; *informant other than head* a family-level covariate taking a value of 1 if an adult other than the head of household was interviewed at the previous wave and 0 if the head of household was interviewed at previous wave; *being in an institution* an individual-level covariate taking the value of 1 if the individual in question was living in a institutional setting at previous wave (prison, hospital, military, or religious order) and 0 otherwise; *age* and *age squared*, individual-level covariates; and *race of head*, a family-level covariate with 3 categories: white (reference), black, and other.

## 4.4 Results

Table 4.1 displays a summary of the runs I have attempted. The first column identifies the type of model and refers to the path models presented above. As a reminder, model 0 is the one-class model with time-invariant covariates only, model 1 is the model with a continuous latent variable that differs in value across the classes, model 2 allows the effect of the time-invariant covariates to vary across classes, and finally, model 3 introduces time-varying covariates and allows their effects to vary across classes.

All models 0 execute without problem which is not surprising given that they are conventional one-class discrete-time hazard models. However, problems in estimation begin with the introduction of more than one class into the models. For example, model $1_{ineligibility}$ and model $2_{ineligibility}$ both lead to a non-positive definite Fisher information matrix which indicates that the model is not identified. As a consequence, several parameters were fixed and the standard errors could not be estimated for these two models. Furthermore, the best likelihood failed to be

| Model | Number of classes | Number of parameters | Starts | Result | Deviance | BIC |
|---|---|---|---|---|---|---|
| | | | *Attrition through non-contact only* | | | |
| Model $0_{non-contact}$ | 1 | 38 | 50/5 | Ended normally | 35,850.5 | 36,221.4 |
| Model $1_{non-contact}$ | 2 | 41 | 800/40 | Ended normally | 35,560.6 | 35,960.7 |
| Model $2_{non-contact}$ | 2 | 46 | 800/40 | Ended normally | 35,516.0 | 35,964.9 |
| Model $3_{non-contact}$ | 2 | 55 | 800/40 | Failed to converge | n/a | n/a |
| | | | *Attrition through refusal only* | | | |
| Model $0_{refusal}$ | 1 | 38 | 50/5 | Ended normally | 47,111.6 | 47,482.4 |
| Model $1_{refusal}$ | 2 | 41 | 1500/75 | Best LL not replicated | 47,035.8 | 47,436.0 |
| Model $2_{refusal}$ | 2 | 46 | 800/40 | Ended normally | 46,960.8 | 47,409.7 |
| Model $3_{refusal}$ | 2 | 55 | 800/40 | Failed to converge | n/a | n/a |
| | | | *Attrition through ineligibility only* | | | |
| Model $0_{ineligibility}$ | 1 | 38 | 50/5 | Ended normally | 13,773.5 | 14,144.4 |
| Model $1_{ineligibility}$ | 2 | 41 | 800/40 | Non-positive definite | n/a | n/a |
| Model $2_{ineligibility}$ | 2 | 46 | 800/40 | Non-positive definite | n/a | n/a |
| Model $3_{ineligibility}$ | 2 | 55 | 800/40 | Failed to converge | n/a | n/a |
| | | | *Attrition through death only* | | | |
| Model $0_{death}$ | 1 | 38 | 50/5 | Ended normally | 32,206.0 | 32,576.9 |
| Model $1_{death}$ | 2 | 41 | 800/40 | Ended normally | 32,172.1 | 32,572.2 |
| Model $2_{death}$ | 2 | 46 | 1500/75 | Best LL not replicated | 32,122.0 | 32,570.9 |
| Model $3_{death}$ | 2 | 55 | 800/40 | Failed to converge | n/a | n/a |

Table 4.1: Summary of mixture discrete-time hazard model runs.

replicated for model $1_{refusal}$ which means that the result might not be trustworthy. Consequently, the results of these models should be interpreted with caution. Results are more encouraging when looking at model $2_{refusal}$ as well as models 1 through 2 for death: they all executed normally without any evidence of identification problems. Model $1_{non-contact}$ and model $2_{non-contact}$ also executed normally although a few parameters were fixed at 0.

As shown in table 4.1, the deviance and BIC statistics for models 0 through 2 almost consistently suggest that the two-class models are a better representation of the mechanism that has generated the data than the one-class model (Muthén and Masyn, 2005). The lower the deviance and BIC statistics, the better. A lower deviance and lower BIC statistics mean that the more complex model improved the fit to the data over the simpler model. The only exception is the BIC statistic for the model $2_{non-contact}$ that is higher than that of model $1_{non-contact}$. This suggests that allowing the effect of the time-invariant predictors to vary across class does not improve the model. However, the deviance suggests otherwise.

I focus on model $2_{non-contact}$ and model $2_{refusal}$ and comparing them to their one-class counterpart (model $0_{non-contact}$ and model $0_{refusal}$) to see what effect allowing coefficients to vary across class has on the estimates. Table 4.2 shows the beta coefficients of model 0 and 2 for attrition through non-contact and attrition though refusal. According to model $0_{non-contact}$, being a man, not having a high school degree and being part of the SEO sample increases the chance of attrition through non-contact. Class one of model 2 shows, for the same predictors, effects that are similar in direction and magnitude while the second class of model 2 shows

| | Non-contact | | | | Refusals | | | |
|---|---|---|---|---|---|---|---|---|
| | Model 0 | | Model 2 | | Model 0 | | Model 2 | |
| | Est. | SE | Est. | SE | Est. | SE | Est. | SE |
| Class 1 | | | | | | | | |
| Men | 0.429 | 0.032 | 0.525 | 0.038 | 0.015 | 0.026 | 0.022 | 0.030 |
| No high school degree | 0.199 | 0.071 | 0.266 | 0.078 | 0.019 | 0.064 | 0.062 | 0.071 |
| Some college | -0.014 | 0.118 | -0.029 | 0.128 | -0.200 | 0.098 | -0.219 | 0.101 |
| College degree | -0.611 | 0.132 | -0.623 | 0.148 | -0.395 | 0.107 | -0.423 | 0.118 |
| In SEO sample | 0.648 | 0.065 | 1.056 | 0.073 | -0.072 | 0.058 | 0.142 | 0.067 |
| Class 2 | | | | | | | | |
| Men | | | -0.920 | 0.773 | | | -0.285 | 0.415 |
| No high school degree | | | -5.722 | 0.942 | | | -2.434 | 2.599 |
| Some college | | | 4415.032 | 0.000 | | | 0.550 | 1.459 |
| College degree | | | 382.515 | 0.000 | | | -2.889 | 2.705 |
| In SEO sample | | | -35.124 | 0.000 | | | -5.473 | 2.590 |

Table 4.2: Coefficients and standard errors of model 0 (one class) and model 2 (two classes) for attrition through non-contact and attrition through refusal

effects that are in the opposite direction to class one. However, the effect of the higher education indicator is suspiciously large which might be an indication of a problem with the model, potentially the presence of very large outliers that have lots of leverage in the second class.

As was the case with non-contact, higher education also decreases the probability of refusals in model 0. Being in the SEO sample is not associated with refusal. However, when moving to the two-class model, being in the SEO sample becomes positively associated with refusal in class 1 while at the same time being negatively associated with refusal in the second class. This seem to indicate that the SEO indicator plays a role in the definition of these two classes. The first class contains the SEO sample members who are more likely to refuse while the second class contains the SEO sample least likely to attrite. None of the other predictors have an effect on refusal in the second class.

## 4.5 Discussion

As with any model based on observational data, discrete-time hazard models do not specifically address the issue of unobserved heterogeneity (Singer and Willett, 2003), that is the residual variance in the dependent variable that cannot be modeled by any of the variables measured. Latent-class models can be used to model the unobserved heterogeneity in discrete-time hazard models (Masyn, 2003). Their advantage resides in the fact that the residual variance is assumed to be composed of a mixture of mass points instead of assuming that it follows a univariate normal distribution. This leads to the identification of "groups", the latent classes, which are interesting for both substantive and practical reasons.

Difficulties in estimation of the latent-class models make the results presented inconclusive. While there are some promising indications that the population is a mixture of different classes with respect to the processes of non-contact and refusal, the two-class models that I was able to successfully estimate fall short of expectations. Indeed, failure to successfully estimate a model where the effect of time-varying covariates is allowed to vary across classes (model 3) makes it impossible to distinguish between the different theoretical mechanisms presented above. The time-varying covariates — life events — were expected to have more impact on some classes than others depending on what mechanism is a better description of the data. The theoretical framework also calls for three classes — stayers, early attriters and late attriters. Difficulties in estimation of the two-class models make it unlikely for a three-class model to work although no attempt was made in this respect.

Difficulties encountered in the model estimation could be due to a number of reasons. One possibility is that there are not enough cases to fit these complex models. This is likely what happened for the two-class ineligibility models. Indeed, the number of individuals exiting the panel through ineligibility (1,073) is much lower than the number exiting through non-contact (3,508), refusal (4,667), or death (2,952). In a seemingly related fashion, the ineligibility models also caused more serious difficulties than all the other models. The smaller number of event occurrences in the ineligibility outcome probably lead to empty cells which may have complicated the estimation of the model. In order to investigate this possibility, I have also created a series of models 0 through 4 in which the outcome is redefined to combine all of the types of attrition i.e. non-contact, refusal, ineligibility, and death. With the exception of model 3, all executed normally which seems to give credence to this hypothesis (see bottom of table 4.1).

Another possibility is that the likelihood function has no clear maximum. This could happen if the function is flat or very irregular. In both cases, the EM algorithm would struggle to find the point of maximum likelihood. The high number of random starts necessary for the best likelihood value to be replicated even for the simplest two-class models is an indication of this potential problem. None of the the two-class models would replicate the best likelihood unless the number of random starts was set at 800. Two models (model $1_{refusal}$ and model $2_{death}$ failed to replicate the best likelihood value even after testing 1500 starts. From a substantive point of view, this means that the classes may not be clearly defined, at least for the data under consideration. A different situation might prevail in other panels.

Despite the difficulties, latent-class models could to be a promising tool in attrition research. Combined with a theory as to what these groups might represent, such models could allow for the distinction of potentially interesting groups of panel members showing distinct attrition behavior. From a practical point of view, the identification of such groups could also allow for the implementation of tailored fieldwork strategies with the goal of minimizing attrition. More research is needed.

# Appendix A

## Nonresponse in the Core Sample (SRC & SEO Samples Combined)

| | Remaining in sample | | | Nonresponse | | | | |
|---|---|---|---|---|---|---|---|---|
| Wave | Total | In family unit | In institution | Total | Died | Other nonresponse | Dropped (by design) | In from nonresponse |
| 1 | 18191 | 17807 | 384 | 0 | 0 | 0 | 0 | 0 |
| 2 | 16028 | 15660 | 368 | 2163 | 84 | 2079 | 0 | 0 |
| 3 | 15461 | 15130 | 331 | 598 | 74 | 524 | 0 | 31 |
| 4 | 15092 | 14770 | 322 | 404 | 97 | 307 | 0 | 35 |
| 5 | 14698 | 14403 | 295 | 427 | 114 | 313 | 0 | 33 |
| 6 | 14280 | 13972 | 308 | 449 | 100 | 349 | 0 | 31 |
| 7 | 13891 | 13584 | 307 | 410 | 91 | 319 | 0 | 21 |
| 8 | 13529 | 13228 | 301 | 389 | 98 | 291 | 0 | 27 |
| 9 | 13078 | 12787 | 291 | 486 | 87 | 399 | 0 | 35 |
| 10 | 12689 | 12380 | 309 | 413 | 90 | 323 | 0 | 24 |
| 11 | 12401 | 12081 | 320 | 330 | 64 | 266 | 0 | 42 |
| 12 | 12036 | 11719 | 317 | 388 | 74 | 314 | 0 | 23 |
| 13 | 11664 | 11358 | 306 | 403 | 91 | 312 | 0 | 32 |
| 14 | 11365 | 11025 | 340 | 336 | 77 | 259 | 0 | 37 |
| 15 | 11108 | 10782 | 326 | 286 | 88 | 198 | 0 | 30 |
| 16 | 10813 | 10491 | 322 | 336 | 83 | 253 | 0 | 41 |
| 17 | 10498 | 10179 | 319 | 351 | 94 | 257 | 0 | 36 |
| 18 | 10168 | 9893 | 275 | 371 | 96 | 275 | 0 | 40 |
| 19 | 9810 | 9518 | 292 | 391 | 84 | 307 | 0 | 33 |
| 20 | 9488 | 9231 | 257 | 358 | 96 | 262 | 0 | 35 |
| 21 | 9209 | 9003 | 206 | 310 | 96 | 214 | 0 | 31 |
| 22 | 8914 | 8744 | 170 | 323 | 80 | 243 | 0 | 28 |
| 23 | 8760 | 8643 | 117 | 285 | 126 | 159 | 0 | 82 |
| 24 | 8504 | 8410 | 94 | 272 | 99 | 173 | 0 | 16 |
| 25 | 8452 | 8385 | 67 | 308 | 110 | 198 | 0 | 233 |
| 26 | 8216 | 8158 | 58 | 497 | 120 | 377 | 0 | 228 |
| 27 | 8623 | 8568 | 55 | 773 | 438 | 335 | 0 | 836 |
| 28 | 8274 | 8230 | 44 | 362 | 91 | 271 | 0 | 13 |
| 29 | 8017 | 7968 | 49 | 287 | 85 | 202 | 0 | 30 |
| 30 | 5702 | 5676 | 26 | 2448 | 82 | 183 | 2183 | 50 |
| 31 | 5468 | 5435 | 33 | 387 | 189 | 198 | 0 | 133 |
| 32 | 5281 | 5253 | 28 | 329 | 176 | 153 | 0 | 134 |
| 33 | 5107 | 5077 | 30 | 298 | 166 | 132 | 0 | 113 |
| 34 | 4966 | 4930 | 36 | 264 | 141 | 123 | 0 | 112 |

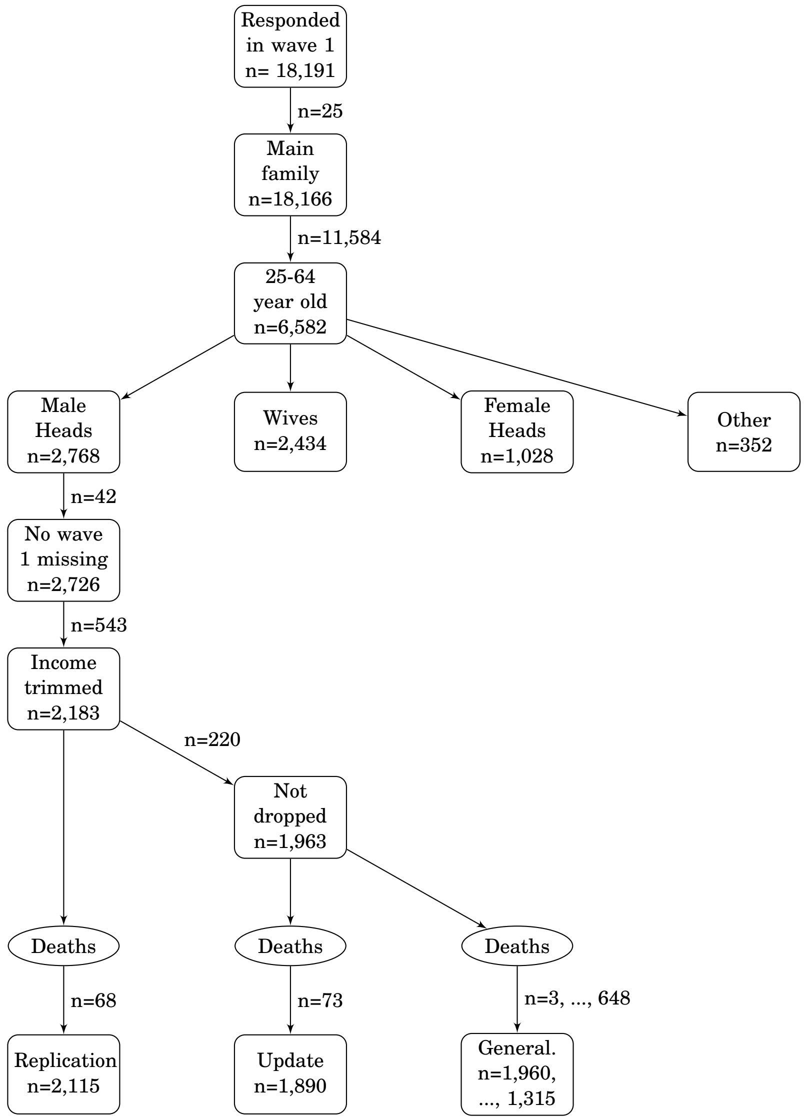
Table A.1: Number of core sample members remaining in sample, nonrespondent, and returning from nonresponse at each wave.

Appendix B

Sample Selection Decision Tree

This decision tree illustrates the sub-sample selection process carried out by Fitzgerald et al. (1998) in preparation for their attrition model of male heads of household aged 25-64 in 1968. The values of $n$ displayed in the decision tree represent the numbers I got when I followed the procedure described in Fitzgerald et al. (1998).

I began with 18,191 core sample members who participated in wave 1. I then excluded 25 individuals who were part of secondary families in multi-family households and 11,584 individuals who were less than 25 or more than 64 years old at wave 1 (1968). Again following Fitzgerald et al. (1998), the 6,582 sample members left were divided into four groups: male heads, wives, female heads and others. Fitzgerald et al. (1998) created models of attrition in each of these groups but I focused only on the group of male heads. Male heads who had missing data on wave 1 variables (n=42) and those at the top and bottom 1% of the income distribution (n=543) were also excluded. For the replication analysis presented in section 2.4, I was left with a total of 2,115 male heads after excluding those who died between wave 1 and wave 22 (n=68). For the update and generalization analysis presented in section 2.5 and 2.6, cases that were dropped at wave 30 (n=220) had to be excluded which leaves 1,963 male heads, from which 73 deaths had to be subtracted to get the number of cases for the update analysis (n=1,830). Finally, in the generalization analysis, a varying number of deaths were excluded depending on which of 33 models is considered. This number varies between 3 for the wave 2 model to 648 for the wave 34 model which leave a sample size between 1,960 and 1,315 for model 2 and model 34 respectively.


Responded in wave 1 n= 18,191
n=25
Main family n=18,166
n=11,584
25-64 year old n=6,582
Male Heads n=2,768
Wives n=2,434
Female Heads n=1,028
Other n=352
n=42
No wave 1 missing n=2,726
n=543
Income trimmed n=2,183
n=220
Not dropped n=1,963
Deaths
Deaths
Deaths
n=68
n=73
n=3, ..., 648
Replication n=2,115
Update n=1,890
General. n=1,960, ..., 1,315

# Appendix C

## Challenging the Ever-out Definition of Attrition

| | One wave out | | Two vave out | | Three wave out | |
|---|---|---|---|---|---|---|
| | OR(SE) | | OR(SE) | | OR(SE) | |
| L.Labor income | .984 | ** | .984 | ** | .985 | ** |
| | (.004) | | (.004) | | (.004) | |
| L.No labor income | 1.302 | ** | 1.306 | ** | 1.349 | ** |
| | (.120) | | (.116) | | (.120) | |
| L.Labor income squared | 1.009 | ** | 1.009 | ** | 1.009 | ** |
| | (.003) | | (.003) | | (.003) | |
| L.Black | .730 | ** | .683 | ** | .674 | ** |
| | (.051) | | (.047) | | (.046) | |
| L.Other race | 2.827 | ** | 2.867 | ** | 2.785 | ** |
| | (.476) | | (.473) | | (.453) | |
| L.Age | .949 | ** | .959 | ** | .963 | ** |
| | (.005) | | (.005) | | (.005) | |
| L.Age squared | 1.110 | ** | 1.099 | ** | 1.094 | ** |
| | (.010) | | (.010) | | (.010) | |
| L.Education < 12 yrs | 1.297 | ** | 1.324 | ** | 1.339 | ** |
| | (.070) | | (.071) | | (.071) | |
| L.Some college | .875 | + | .864 | + | .871 | + |
| | (.066) | | (.065) | | (.066) | |
| L.College degree | .568 | ** | .559 | ** | .553 | ** |
| | (.046) | | (.046) | | (.045) | |
| L.Northeast | 1.111 | | 1.031 | | .999 | |
| | (.084) | | (.077) | | (.074) | |
| L.North central | .865 | * | .803 | ** | .775 | ** |
| | (.061) | | (.056) | | (.054) | |
| L.South | .998 | | .900 | | .873 | + |
| | (.071) | | (.063) | | (.061) | |
| L.In SEO sample | 3.061 | ** | 3.363 | ** | 3.382 | ** |
| | (.209) | | (.226) | | (.226) | |
| L.Lives in rural area | .593 | ** | .599 | ** | .591 | ** |
| | (.038) | | (.038) | | (.037) | |
| L.Number of children | .956 | ** | .950 | ** | .951 | ** |
| | (.012) | | (.012) | | (.012) | |
| L.Presence of child <6 | 1.067 | | 1.074 | | 1.059 | |
| | (.050) | | (.050) | | (.049) | |
| L.Owns house | .757 | ** | .740 | ** | .742 | ** |
| | (.040) | | (.039) | | (.039) | |
| L.Might move in future | 1.127 | * | 1.124 | * | 1.138 | * |
| | (.062) | | (.061) | | (.061) | |
| L.Income/need ratio | 1.018 | | 1.012 | | 1.010 | |
| | (.026) | | (.025) | | (.025) | |
| Intercept | 3.388 | ** | 2.736 | ** | 2.574 | ** |
| | (.420) | | (.333) | | (.312) | |
| Person-Waves | 11,619 | | 11,619 | | 11,619 | |
| N. obs. | 18,191 | | 18,191 | | 18,191 | |
| df | 20 | | 20 | | 20 | |
| Deviance | 12,997.59 | | 13,324.53 | | 13,405.54 | |
| AIC | 13,039.59 | | 13,366.53 | | 13,447.54 | |
| BIC | 13,203.57 | | 13,530.52 | | 13,611.53 | |

+ p<0.10, * p<0.05, ** p<0.01

Table C.1: Comparison of attrition models using three variations in the definition of attrition: a "strict" ever-out model and two "relaxed" ever-out model in which attrition is defined as being out for at least two and three consecutive waves.

Appendix D

Comparison of Unweighted & Weighted Models

| | Unweighted | | Weighted | |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | |
| Black | 1.248 | ** | 1.314 | ** |
| | (.057) | | (.070) | |
| Other race | 1.912 | ** | 1.824 | ** |
| | (.167) | | (.187) | |
| Age | .935 | ** | .933 | ** |
| | (.003) | | (.003) | |
| Age squared | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | |
| Men | 1.264 | ** | 1.247 | ** |
| | (.022) | | (.026) | |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.108 | ** | 1.184 | ** |
| | (.043) | | (.055) | |
| Some college | .910 | | .907 | |
| | (.053) | | (.057) | |
| College degree | .675 | ** | .710 | ** |
| | (.044) | | (.049) | |
| In Institution | 1.621 | ** | 1.623 | ** |
| | (.094) | | (.128) | |
| Aged < 18 | .449 | ** | .492 | ** |
| | (.023) | | (.033) | |
| Home owner | .659 | ** | .633 | ** |
| | (.021) | | (.023) | |
| Non-head informant | 1.334 | ** | 1.414 | ** |
| | (.050) | | (.062) | |
| Presence of young children | .781 | ** | .724 | ** |
| | (.032) | | (.041) | |
| In SEO sample | .982 | | .979 | |
| | (.044) | | (.051) | |
| Wave Effects | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | |
| Experienced event | 12,200 | | 12,200 | |
| Observations (n) | 17,324 | | 17,324 | |
| df | 47 | | 47 | |
| Deviance | 97,198.35 | | 89,793.30 | |
| AIC | 97,294.35 | | 89,889.30 | |
| BIC | 97,666.82 | | 90,261.77 | |

+ p<0.10, * p<0.05, ** p<0.01

Table D.1: Unweighted and weighted competing-risk discrete-time hazard models of exit for any reason, SRC & SEO samples combined.

| | Unweighted | | Weighted | |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | |
| Black | 1.775 | ** | 1.908 | ** |
| | (.155) | | (.191) | |
| Other race | 2.993 | ** | 2.821 | ** |
| | (.427) | | (.467) | |
| Age | .967 | ** | .953 | ** |
| | (.007) | | (.008) | |
| Age squared | 1.000 | | 1.000 | |
| | (.000) | | (.000) | |
| Men | 1.477 | ** | 1.368 | ** |
| | (.050) | | (.063) | |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.121 | | 1.229 | * |
| | (.081) | | (.119) | |
| Some college | 1.038 | | .935 | |
| | (.121) | | (.124) | |
| College degree | .611 | ** | .637 | ** |
| | (.081) | | (.093) | |
| In Institution | 1.575 | ** | 1.597 | ** |
| | (.133) | | (.187) | |
| Aged < 18 | .234 | ** | .218 | ** |
| | (.024) | | (.030) | |
| Home owner | .432 | ** | .383 | ** |
| | (.029) | | (.032) | |
| Non-head informant | 1.114 | | 1.189 | |
| | (.091) | | (.134) | |
| Presence of young children | .838 | ** | .710 | ** |
| | (.054) | | (.064) | |
| In SEO sample | 1.005 | | 1.012 | |
| | (.083) | | (.094) | |
| Wave Effects | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | |
| Experienced event | 3,508 | | 3,508 | |
| Observations (n) | 17,324 | | 17,324 | |
| df | 47 | | 47 | |
| Deviance | 33,856.92 | | 24,033.40 | |
| AIC | 33,952.92 | | 24,129.40 | |
| BIC | 34,325.39 | | 24,501.88 | |

+ p<0.10, * p<0.05, ** p<0.01

Table D.2: Unweighted and weighted competing-risk discrete-time hazard models of non-contact, SRC & SEO samples combined.

| | Unweighted | | Weighted | |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | |
| Black | .977 | | .946 | |
| | (.077) | | (.090) | |
| Other race | 1.691 | ** | 1.749 | ** |
| | (.239) | | (.288) | |
| Age | 1.009 | + | 1.016 | ** |
| | (.005) | | (.006) | |
| Age squared | 1.000 | ** | 1.000 | ** |
| | (.000) | | (.000) | |
| Men | .997 | | 1.007 | |
| | (.027) | | (.030) | |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.040 | | 1.122 | |
| | (.069) | | (.086) | |
| Some college | .827 | + | .858 | |
| | (.082) | | (.090) | |
| College degree | .672 | ** | .730 | ** |
| | (.072) | | (.084) | |
| In Institution | 1.082 | | .977 | |
| | (.111) | | (.140) | |
| Aged < 18 | .691 | ** | .809 | * |
| | (.053) | | (.080) | |
| Home owner | .989 | | .912 | |
| | (.058) | | (.060) | |
| Non-head informant | 1.459 | ** | 1.505 | ** |
| | (.095) | | (.111) | |
| Presence of young children | .702 | ** | .673 | ** |
| | (.047) | | (.056) | |
| In SEO sample | .906 | | .888 | |
| | (.066) | | (.075) | |
| Wave Effects | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | |
| Experienced event | 4,667 | | 4,667 | |
| Observations (n) | 17,324 | | 17,324 | |
| df | 47 | | 47 | |
| Deviance | 46,665.41 | | 45,786.82 | |
| AIC | 46,761.41 | | 45,882.82 | |
| BIC | 47,133.88 | | 46,255.29 | |

+ p<0.10, * p<0.05, ** p<0.01

Table D.3: Unweighted and weighted competing-risk discrete-time hazard models of refusal, SRC & SEO samples combined.

| | Unweighted | | Weighted | |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | |
| Black | 1.043 | | 1.303 | |
| | (.147) | | (.218) | |
| Other race | 1.526 | | 1.155 | |
| | (.394) | | (.312) | |
| Age | .956 | ** | .957 | ** |
| | (.010) | | (.013) | |
| Age squared | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | |
| Men | .930 | | .852 | * |
| | (.055) | | (.062) | |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.139 | | 1.249 | |
| | (.127) | | (.170) | |
| Some college | .675 | + | .678 | |
| | (.145) | | (.172) | |
| College degree | .540 | ** | .483 | ** |
| | (.111) | | (.108) | |
| In Institution | 3.680 | ** | 2.764 | ** |
| | (.808) | | (.903) | |
| Aged < 18 | 7.730 | ** | 10.392 | ** |
| | (1.698) | | (2.992) | |
| Home owner | .615 | ** | .625 | ** |
| | (.058) | | (.073) | |
| Non-head informant | 1.079 | | 1.204 | |
| | (.140) | | (.170) | |
| Presence of young children | .891 | | .918 | |
| | (.107) | | (.169) | |
| In SEO sample | 1.097 | | 1.090 | |
| | (.162) | | (.176) | |
| Wave Effects | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | |
| Experienced event | 1,073 | | 1,073 | |
| Observations (n) | 17,324 | | 17,324 | |
| df | 47 | | 47 | |
| Deviance | 12,656.45 | | 10,890.68 | |
| AIC | 12,752.45 | | 10,986.68 | |
| BIC | 13,124.92 | | 11,359.15 | |

+ p<0.10, * p<0.05, ** p<0.01

Table D.4: Unweighted and weighted competing-risk discrete-time hazard models of ineligibility, SRC & SEO samples combined.

| | Unweighted | | Weighted | |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | |
| Black | 1.262 | ** | 1.281 | ** |
| | (.078) | | (.108) | |
| Other race | .942 | | .866 | |
| | (.152) | | (.143) | |
| Age | 1.056 | ** | 1.057 | ** |
| | (.009) | | (.011) | |
| Age squared | 1.000 | ** | 1.000 | ** |
| | (.000) | | (.000) | |
| Men | 1.735 | ** | 1.801 | ** |
| | (.068) | | (.081) | |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.241 | ** | 1.285 | ** |
| | (.065) | | (.076) | |
| Some college | 1.054 | | 1.067 | |
| | (.076) | | (.082) | |
| College degree | .804 | ** | .817 | * |
| | (.066) | | (.071) | |
| In Institution | 3.771 | ** | 4.903 | ** |
| | (.537) | | (.878) | |
| Aged < 18 | 1.094 | | 1.298 | |
| | (.243) | | (.381) | |
| Home owner | .642 | ** | .633 | ** |
| | (.028) | | (.033) | |
| Non-head informant | 1.362 | ** | 1.381 | ** |
| | (.066) | | (.077) | |
| Presence of young children | .875 | + | .869 | |
| | (.069) | | (.101) | |
| In SEO sample | 1.137 | + | 1.139 | + |
| | (.075) | | (.089) | |
| Wave Effects | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | |
| Experienced event | 2,952 | | 2,952 | |
| Observations (n) | 17,324 | | 17,324 | |
| df | 47 | | 47 | |
| Deviance | 26,413.15 | | 28,638.73 | |
| AIC | 26,509.15 | | 28,734.73 | |
| BIC | 26,881.62 | | 29,107.20 | |

+ p<0.10, * p<0.05, ** p<0.01

Table D.5: Unweighted and weighted competing-risk discrete-time hazard models of death, SRC & SEO samples combined.

Appendix E

Discrete-time Hazard Models of First Exit through any Reason

| | Model 1 | | Model 2 | | Model 3 | | Model 4 | |
|---|---|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | | | |
| Black | 1.325 | ** | 1.332 | ** | 1.234 | ** | 1.248 | ** |
| | (.047) | | (.048) | | (.045) | | (.057) | |
| Other race | 1.997 | ** | 2.031 | ** | 1.899 | ** | 1.912 | ** |
| | (.169) | | (.173) | | (.161) | | (.167) | |
| Age | .968 | ** | .932 | ** | .935 | ** | .935 | ** |
| | (.002) | | (.003) | | (.003) | | (.003) | |
| Age squared | 1.001 | ** | 1.001 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | | (.000) | |
| Men | 1.267 | ** | 1.251 | ** | 1.264 | ** | 1.264 | ** |
| | (.022) | | (.022) | | (.022) | | (.022) | |
| Head's Education (ref: high school degree) | | | | | | | | |
| No high school degree | 1.152 | ** | 1.141 | ** | 1.106 | ** | 1.108 | ** |
| | (.044) | | (.044) | | (.043) | | (.043) | |
| Some college | .907 | + | .898 | + | .910 | | .910 | |
| | (.053) | | (.053) | | (.053) | | (.053) | |
| College degree | .655 | ** | .650 | ** | .676 | ** | .675 | ** |
| | (.043) | | (.043) | | (.044) | | (.044) | |
| In Institution | | | 1.589 | ** | 1.622 | ** | 1.621 | ** |
| | | | (.090) | | (.094) | | (.094) | |
| Aged < 18 | | | .416 | ** | .448 | ** | .449 | ** |
| | | | (.022) | | (.023) | | (.023) | |
| Home owner | | | | | .660 | ** | .659 | ** |
| | | | | | (.021) | | (.021) | |
| Non-head informant | | | | | 1.334 | ** | 1.334 | ** |
| | | | | | (.050) | | (.050) | |
| Presence of young children | | | | | .781 | ** | .781 | ** |
| | | | | | (.032) | | (.032) | |
| In SEO sample | | | | | | | .982 | |
| | | | | | | | (.044) | |
| Wave Effects | Yes | | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | | 316,651 | | 316,651 | |
| Experienced event | 12,200 | | 12,200 | | 12,200 | | 12,200 | |
| Observations (n) | 17,324 | | 17,324 | | 17,324 | | 17,324 | |
| df | 41 | | 43 | | 46 | | 47 | |
| Deviance | 98,354.46 | | 97,793.63 | | 97,198.82 | | 97,198.35 | |
| AIC | 98,438.46 | | 97,881.63 | | 97,292.82 | | 97,294.35 | |
| BIC | 98,764.38 | | 98,223.07 | | 97,657.53 | | 97,666.82 | |

+ p<0.10, * p<0.05, ** p<0.01

Table E.1: Discrete-time hazard models of first exit for any reason, SRC and SEO samples combined.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.424 | ** | 1.419 | ** | 1.328 | ** |
| | (.090) | | (.091) | | (.087) | |
| Other race | 1.957 | ** | 1.989 | ** | 1.900 | ** |
| | (.254) | | (.260) | | (.248) | |
| Age | .956 | ** | .927 | ** | .931 | ** |
| | (.002) | | (.003) | | (.004) | |
| Age squared | 1.001 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.232 | ** | 1.225 | ** | 1.246 | ** |
| | (.028) | | (.028) | | (.029) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.270 | ** | 1.261 | ** | 1.193 | ** |
| | (.065) | | (.065) | | (.061) | |
| Some college | .923 | | .912 | | .915 | |
| | (.062) | | (.062) | | (.062) | |
| College degree | .705 | ** | .700 | ** | .714 | ** |
| | (.051) | | (.051) | | (.051) | |
| In Institution | | | 1.599 | ** | 1.623 | ** |
| | | | (.136) | | (.145) | |
| Aged < 18 | | | .440 | ** | .501 | ** |
| | | | (.035) | | (.040) | |
| Home owner | | | | | .641 | ** |
| | | | | | (.027) | |
| Non-head informant | | | | | 1.425 | ** |
| | | | | | (.071) | |
| Presence of young children | | | | | .751 | ** |
| | | | | | (.050) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 172,808 | | 172,808 | | 172,808 | |
| Experienced event | 6,296 | | 6,296 | | 6,296 | |
| Observations (n) | 9,048 | | 9,048 | | 9,048 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 49,731.23 | | 49,512.94 | | 49,149.95 | |
| AIC | 49,815.23 | | 49,600.94 | | 49,243.95 | |
| BIC | 50,113.87 | | 49,913.80 | | 49,578.14 | |

+ p<0.10, * p<0.05, ** p<0.01

Table E.2: Discrete-time hazard models of first exit for any reason, SRC sample only.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.253 | ** | 1.241 | ** | 1.188 | ** |
| | (.082) | | (.081) | | (.078) | |
| Other race | 1.909 | ** | 1.911 | ** | 1.835 | ** |
| | (.233) | | (.235) | | (.224) | |
| Age | .989 | ** | .950 | ** | .951 | ** |
| | (.003) | | (.005) | | (.005) | |
| Age squared | 1.000 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.320 | ** | 1.290 | ** | 1.294 | ** |
| | (.035) | | (.034) | | (.035) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.002 | | .995 | | .995 | |
| | (.058) | | (.058) | | (.058) | |
| Some college | .962 | | .961 | | .979 | |
| | (.118) | | (.118) | | (.117) | |
| College degree | .607 | ** | .602 | ** | .646 | ** |
| | (.103) | | (.103) | | (.106) | |
| In Institution | | | 1.554 | ** | 1.603 | ** |
| | | | (.119) | | (.124) | |
| Aged < 18 | | | .477 | ** | .498 | ** |
| | | | (.033) | | (.035) | |
| Home owner | | | | | .674 | ** |
| | | | | | (.034) | |
| Non-head informant | | | | | 1.223 | ** |
| | | | | | (.071) | |
| Presence of young children | | | | | .836 | ** |
| | | | | | (.043) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 143,843 | | 143,843 | | 143,843 | |
| Experienced event | 5,904 | | 5,904 | | 5,904 | |
| Observations (n) | 8,276 | | 8,276 | | 8,276 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 48,090.26 | | 47,867.04 | | 47,636.20 | |
| AIC | 48,174.26 | | 47,955.04 | | 47,730.20 | |
| BIC | 48,469.15 | | 48,263.97 | | 48,060.19 | |

+ p<0.10, * p<0.05, ** p<0.01

Table E.3: Discrete-time hazard models of first exit for any reason, SEO sample only.

Appendix F

Discrete-time Hazard Models of First Exit through Non-contact

| | Model 1 | | Model 2 | | Model 3 | | Model 4 | |
|---|---|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | | | |
| Black | 2.145 | ** | 2.165 | ** | 1.780 | ** | 1.775 | ** |
| | (.151) | | (.152) | | (.126) | | (.155) | |
| Other race | 3.337 | ** | 3.472 | ** | 2.999 | ** | 2.993 | ** |
| | (.457) | | (.479) | | (.407) | | (.427) | |
| Age | 1.068 | ** | .964 | ** | .967 | ** | .967 | ** |
| | (.006) | | (.007) | | (.007) | | (.007) | |
| Age squared | .999 | ** | 1.000 | | 1.000 | | 1.000 | |
| | (.000) | | (.000) | | (.000) | | (.000) | |
| Men | 1.500 | ** | 1.469 | ** | 1.477 | ** | 1.477 | ** |
| | (.050) | | (.049) | | (.050) | | (.050) | |
| Education (ref: high school degree) | | | | | | | | |
| No high school degree | 1.169 | * | 1.157 | * | 1.121 | | 1.121 | |
| | (.086) | | (.086) | | (.082) | | (.081) | |
| Some college | 1.055 | | 1.039 | | 1.038 | | 1.038 | |
| | (.123) | | (.121) | | (.121) | | (.121) | |
| College degree | .571 | ** | .571 | ** | .610 | ** | .611 | ** |
| | (.075) | | (.075) | | (.080) | | (.081) | |
| In Institution | | | 1.406 | ** | 1.575 | ** | 1.575 | ** |
| | | | (.115) | | (.133) | | (.133) | |
| Aged < 18 | | | .208 | ** | .234 | ** | .234 | ** |
| | | | (.021) | | (.024) | | (.024) | |
| Home owner | | | | | .432 | ** | .432 | ** |
| | | | | | (.029) | | (.029) | |
| Non-head informant | | | | | 1.114 | | 1.114 | |
| | | | | | (.091) | | (.091) | |
| Presence of young children | | | | | .838 | ** | .838 | ** |
| | | | | | (.054) | | (.054) | |
| In SEO sample | | | | | | | 1.005 | |
| | | | | | | | (.083) | |
| Wave Effects | Yes | | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | | 316,651 | | 316,651 | |
| Experienced event | 3,508 | | 3,508 | | 3,508 | | 3,508 | |
| Observations (n) | 17,324 | | 17,324 | | 17,324 | | 17,324 | |
| df | 41 | | 43 | | 46 | | 47 | |
| Deviance | 34,838.74 | | 34,350.74 | | 33,856.93 | | 33,856.92 | |
| AIC | 34,922.74 | | 34,438.74 | | 33,950.93 | | 33,952.92 | |
| BIC | 35,248.65 | | 34,780.18 | | 34,315.64 | | 34,325.39 | |

+ p<0.10, * p<0.05, ** p<0.01

Table F.1: Competing-risk discrete-time hazard models of non-contact, SRC and SEO samples.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 2.419 | ** | 2.435 | ** | 2.008 | ** |
| | (.291) | | (.294) | | (.254) | |
| Other race | 3.430 | ** | 3.594 | ** | 3.228 | ** |
| | (.783) | | (.829) | | (.728) | |
| Age | 1.037 | ** | .935 | ** | .942 | ** |
| | (.008) | | (.010) | | (.009) | |
| Age squared | .999 | ** | 1.000 | * | 1.000 | * |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.321 | ** | 1.308 | ** | 1.310 | ** |
| | (.069) | | (.069) | | (.070) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.341 | * | 1.336 | * | 1.213 | |
| | (.159) | | (.160) | | (.143) | |
| Some college | .983 | | .945 | | .893 | |
| | (.152) | | (.146) | | (.141) | |
| College degree | .565 | ** | .575 | ** | .583 | ** |
| | (.093) | | (.095) | | (.096) | |
| In Institution | | | 1.208 | | 1.359 | * |
| | | | (.168) | | (.200) | |
| Aged < 18 | | | .166 | ** | .209 | ** |
| | | | (.029) | | (.036) | |
| Home owner | | | | | .384 | ** |
| | | | | | (.037) | |
| Non-head informant | | | | | 1.206 | |
| | | | | | (.164) | |
| Presence of young children | | | | | .674 | ** |
| | | | | | (.076) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 172,808 | | 172,808 | | 172,808 | |
| Experienced event | 1,260 | | 1,260 | | 1,260 | |
| Observations (n) | 9,048 | | 9,048 | | 9,048 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 12,573.87 | | 12,360.95 | | 12,093.13 | |
| AIC | 12,657.87 | | 12,448.95 | | 12,187.13 | |
| BIC | 12,956.50 | | 12,761.80 | | 12,521.32 | |

+ p<0.10, * p<0.05, ** p<0.01

Table F.2: Competing-risk discrete-time hazard models of non-contact, SRC sample only.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.769 | ** | 1.752 | ** | 1.552 | ** |
| | (.198) | | (.196) | | (.172) | |
| Other race | 2.736 | ** | 2.778 | ** | 2.544 | ** |
| | (.511) | | (.522) | | (.469) | |
| Age | 1.093 | ** | .989 | | .990 | |
| | (.009) | | (.010) | | (.009) | |
| Age squared | .998 | ** | 1.000 | ** | 1.000 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.622 | ** | 1.576 | ** | 1.595 | ** |
| | (.070) | | (.069) | | (.070) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.056 | | 1.046 | | 1.056 | |
| | (.097) | | (.096) | | (.096) | |
| Some college | 1.287 | | 1.289 | | 1.310 | |
| | (.234) | | (.235) | | (.234) | |
| College degree | .782 | | .778 | | .852 | |
| | (.194) | | (.190) | | (.201) | |
| In Institution | | | 1.467 | ** | 1.628 | ** |
| | | | (.150) | | (.169) | |
| Aged < 18 | | | .259 | ** | .281 | ** |
| | | | (.031) | | (.034) | |
| Home owner | | | | | .461 | ** |
| | | | | | (.041) | |
| Non-head informant | | | | | 1.035 | |
| | | | | | (.105) | |
| Presence of young children | | | | | .971 | |
| | | | | | (.075) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 142,985 | | 142,985 | | 142,985 | |
| Experienced event | 2,248 | | 2,248 | | 2,248 | |
| Observations (n) | 8,253 | | 8,247 | | 8,249 | |
| df | 40 | | 42 | | 45 | |
| Deviance | 21,835.65 | | 21,597.74 | | 21,345.37 | |
| AIC | 21,917.65 | | 21,683.74 | | 21,437.37 | |
| BIC | 22,205.40 | | 21,985.50 | | 21,760.19 | |

+ p<0.10, * p<0.05, ** p<0.01

Table F.3: Competing-risk discrete-time hazard models of non-contact, SEO sample only.

# Appendix G

## Discrete-time Hazard Models of First Exit through Refusal

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) |
| Head's Race (ref: white) | | | | |
| Black | .888 + | .889 + | .921 | .977 |
| | (.056) | (.056) | (.060) | (.077) |
| Other race | 1.608 ** | 1.617 ** | 1.629 ** | 1.691 ** |
| | (.222) | (.223) | (.223) | (.239) |
| Age | 1.032 ** | 1.016 ** | 1.008 + | 1.009 + |
| | (.004) | (.005) | (.005) | (.005) |
| Age squared | 1.000 ** | 1.000 ** | 1.000 ** | 1.000 ** |
| | (.000) | (.000) | (.000) | (.000) |
| Men | 1.001 | .996 | .998 | .997 |
| | (.027) | (.027) | (.027) | (.027) |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.048 | 1.045 | 1.033 | 1.040 |
| | (.069) | (.069) | (.068) | (.069) |
| Some college | .823 * | .821 * | .831 + | .827 + |
| | (.082) | (.081) | (.082) | (.082) |
| College degree | .671 ** | .670 ** | .679 ** | .672 ** |
| | (.072) | (.072) | (.073) | (.072) |
| In Institution | | 1.196 + | 1.085 | 1.082 |
| | | (.123) | (.112) | (.111) |
| Aged < 18 | | .738 ** | .688 ** | .691 ** |
| | | (.058) | (.052) | (.053) |
| Home owner | | | .997 | .989 |
| | | | (.058) | (.058) |
| Non-head informant | | | 1.458 ** | 1.459 ** |
| | | | (.095) | (.095) |
| Presence of young children | | | .701 ** | .702 ** |
| | | | (.047) | (.047) |
| In SEO sample | | | | .906 |
| | | | | (.066) |
| Wave Effects | Yes | Yes | Yes | Yes |
| Person-Waves | 316,651 | 316,651 | 316,651 | 316,651 |
| Experienced event | 4,667 | 4,667 | 4,667 | 4,667 |
| Observations (n) | 17,324 | 17,324 | 17,324 | 17,324 |
| df | 41 | 43 | 46 | 47 |
| Deviance | 46,879.50 | 46,852.81 | 46,671.36 | 46,665.41 |
| AIC | 46,963.50 | 46,940.81 | 46,765.36 | 46,761.41 |
| BIC | 47,289.42 | 47,282.24 | 47,130.07 | 47,133.88 |

+ p<0.10, * p<0.05, ** p<0.01

Table G.1: Competing-risk discrete-time hazard models of refusal, SRC and SEO samples

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | .959 | | .959 | | .966 | |
| | (.108) | | (.108) | | (.110) | |
| Other race | 1.782 | ** | 1.788 | ** | 1.788 | ** |
| | (.360) | | (.361) | | (.368) | |
| Age | 1.031 | ** | 1.028 | ** | 1.021 | ** |
| | (.005) | | (.007) | | (.007) | |
| Age squared | 1.000 | ** | 1.000 | ** | 1.000 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | .988 | | .985 | | .998 | |
| | (.032) | | (.032) | | (.033) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.170 | + | 1.169 | + | 1.143 | |
| | (.098) | | (.098) | | (.096) | |
| Some college | .863 | | .862 | | .870 | |
| | (.097) | | (.097) | | (.098) | |
| College degree | .745 | * | .743 | * | .749 | * |
| | (.087) | | (.087) | | (.088) | |
| In Institution | | | 1.185 | | 1.072 | |
| | | | (.178) | | (.163) | |
| Aged < 18 | | | .944 | | .877 | |
| | | | (.109) | | (.101) | |
| Home owner | | | | | .956 | |
| | | | | | (.071) | |
| Non-head informant | | | | | 1.465 | ** |
| | | | | | (.120) | |
| Presence of young children | | | | | .732 | ** |
| | | | | | (.069) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 172,808 | | 172,808 | | 172,808 | |
| Experienced event | 2,555 | | 2,555 | | 2,555 | |
| Observations (n) | 9,048 | | 9,048 | | 9,048 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 25,189.42 | | 25,187.11 | | 25,103.27 | |
| AIC | 25,273.42 | | 25,275.11 | | 25,197.27 | |
| BIC | 25,572.06 | | 25,587.96 | | 25,531.45 | |

+ p<0.10, * p<0.05, ** p<0.01

Table G.2: Competing-risk discrete-time hazard models of refusal, SRC sample only

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | .928 | | .924 | | .958 | |
| | (.101) | | (.101) | | (.106) | |
| Other race | 1.555 | * | 1.557 | * | 1.570 | * |
| | (.317) | | (.318) | | (.314) | |
| Age | 1.035 | ** | 1.009 | | 1.003 | |
| | (.006) | | (.008) | | (.008) | |
| Age squared | .999 | ** | 1.000 | ** | 1.000 | * |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.010 | | 1.000 | | .988 | |
| | (.044) | | (.044) | | (.044) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | .906 | | .903 | | .901 | |
| | (.093) | | (.092) | | (.092) | |
| Some college | .787 | | .785 | | .793 | |
| | (.165) | | (.165) | | (.167) | |
| College degree | .424 | ** | .423 | ** | .438 | ** |
| | (.118) | | (.118) | | (.121) | |
| In Institution | | | 1.193 | | 1.118 | |
| | | | (.167) | | (.156) | |
| Aged < 18 | | | .666 | ** | .632 | ** |
| | | | (.070) | | (.064) | |
| Home owner | | | | | .987 | |
| | | | | | (.089) | |
| Non-head informant | | | | | 1.425 | ** |
| | | | | | (.147) | |
| Presence of young children | | | | | .709 | ** |
| | | | | | (.067) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 143,843 | | 143,843 | | 143,843 | |
| Experienced event | 2,112 | | 2,112 | | 2,112 | |
| Observations (n) | 8,276 | | 8,276 | | 8,276 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 21,418.34 | | 21,397.53 | | 21,317.60 | |
| AIC | 21,502.34 | | 21,485.53 | | 21,411.60 | |
| BIC | 21,797.22 | | 21,794.46 | | 21,741.60 | |

+ p<0.10, * p<0.05, ** p<0.01

Table G.3: Competing-risk discrete-time hazard models of refusal, SEO sample only

Appendix H

Discrete-time Hazard Models of First Exit through Ineligibility

| | Model 1 | | Model 2 | | Model 3 | | Model 4 | |
|---|---|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | | | |
| Black | 1.239 | * | 1.236 | * | 1.096 | | 1.043 | |
| | (.131) | | (.129) | | (.112) | | (.147) | |
| Other race | 1.767 | * | 1.747 | * | 1.581 | + | 1.526 | |
| | (.445) | | (.439) | | (.404) | | (.394) | |
| Age | .874 | ** | .951 | ** | .956 | ** | .956 | ** |
| | (.005) | | (.009) | | (.010) | | (.010) | |
| Age squared | 1.002 | ** | 1.001 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | | (.000) | |
| Men | .934 | | .918 | | .930 | | .930 | |
| | (.056) | | (.054) | | (.055) | | (.055) | |
| Education (ref: high school degree) | | | | | | | | |
| No high school degree | 1.172 | | 1.175 | | 1.143 | | 1.139 | |
| | (.132) | | (.131) | | (.128) | | (.127) | |
| Some college | .641 | * | .655 | + | .672 | + | .675 | + |
| | (.139) | | (.142) | | (.146) | | (.145) | |
| College degree | .506 | ** | .501 | ** | .535 | ** | .540 | ** |
| | (.103) | | (.102) | | (.109) | | (.111) | |
| In Institution | | | 3.701 | ** | 3.668 | ** | 3.680 | ** |
| | | | (.798) | | (.805) | | (.808) | |
| Aged < 18 | | | 7.344 | ** | 7.776 | ** | 7.730 | ** |
| | | | (1.569) | | (1.713) | | (1.698) | |
| Home owner | | | | | .610 | ** | .615 | ** |
| | | | | | (.057) | | (.058) | |
| Non-head informant | | | | | 1.079 | | 1.079 | |
| | | | | | (.141) | | (.140) | |
| Presence of young children | | | | | .894 | | .891 | |
| | | | | | (.107) | | (.107) | |
| In SEO sample | | | | | | | 1.097 | |
| | | | | | | | (.162) | |
| Wave Effects | Yes | | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 316,651 | | 316,651 | | 316,651 | |
| Experienced event | 1,073 | | 1,073 | | 1,073 | | 1,073 | |
| Observations (n) | 17,324 | | 17,324 | | 17,324 | | 17,324 | |
| df | 41 | | 43 | | 46 | | 47 | |
| Deviance | 12,909.59 | | 12,714.44 | | 12,657.57 | | 12,656.45 | |
| AIC | 12,993.59 | | 12,802.44 | | 12,751.57 | | 12,752.45 | |
| BIC | 13,319.50 | | 13,143.88 | | 13,116.29 | | 13,124.92 | |

+ p<0.10, * p<0.05, ** p<0.01

Table H.1: Competing-risk discrete-time hazard models of ineligibility, SRC and SEO samples.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.433 | | 1.446 | + | 1.332 | |
| | (.319) | | (.308) | | (.280) | |
| Other race | 1.023 | | 1.041 | | .970 | |
| | (.515) | | (.528) | | (.492) | |
| Age | .872 | ** | .945 | ** | .954 | ** |
| | (.008) | | (.015) | | (.016) | |
| Age squared | 1.002 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | .827 | * | .822 | * | .833 | * |
| | (.070) | | (.069) | | (.070) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.274 | | 1.277 | | 1.209 | |
| | (.197) | | (.197) | | (.189) | |
| Some college | .715 | | .735 | | .746 | |
| | (.196) | | (.202) | | (.204) | |
| College degree | .522 | ** | .520 | ** | .539 | ** |
| | (.119) | | (.118) | | (.123) | |
| In Institution | | | 2.690 | ** | 2.586 | * |
| | | | (.983) | | (.988) | |
| Aged < 18 | | | 7.922 | ** | 8.847 | ** |
| | | | (3.010) | | (3.456) | |
| Home owner | | | | | .622 | ** |
| | | | | | (.086) | |
| Non-head informant | | | | | 1.278 | |
| | | | | | (.227) | |
| Presence of young children | | | | | .937 | |
| | | | | | (.230) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 168,887 | | 168,887 | | 168,887 | |
| Experienced event | 506 | | 506 | | 506 | |
| Observations (n) | 8,931 | | 8,932 | | 8,945 | |
| df | 40 | | 42 | | 45 | |
| Deviance | 5,931.29 | | 5,861.98 | | 5,834.77 | |
| AIC | 6,013.29 | | 5,947.98 | | 5,926.77 | |
| BIC | 6,304.28 | | 6,253.17 | | 6,253.31 | |

+ p<0.10, * p<0.05, ** p<0.01

Table H.2: Competing-risk discrete-time hazard models of ineligibility, SRC sample only.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | .967 | | .965 | | .883 | |
| | (.163) | | (.162) | | (.146) | |
| Other race | 1.714 | + | 1.671 | + | 1.556 | |
| | (.522) | | (.509) | | (.485) | |
| Age | .888 | ** | .984 | | .986 | |
| | (.008) | | (.016) | | (.016) | |
| Age squared | 1.001 | ** | 1.000 | ** | 1.000 | * |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.044 | | 1.011 | | 1.017 | |
| | (.087) | | (.085) | | (.086) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.029 | | 1.022 | | 1.029 | |
| | (.161) | | (.160) | | (.161) | |
| Some college | .585 | + | .592 | + | .605 | |
| | (.184) | | (.186) | | (.189) | |
| College degree | .714 | | .713 | | .782 | |
| | (.333) | | (.333) | | (.367) | |
| In Institution | | | 4.331 | ** | 4.405 | ** |
| | | | (1.137) | | (1.155) | |
| Aged < 18 | | | 7.990 | ** | 8.373 | ** |
| | | | (2.098) | | (2.282) | |
| Home owner | | | | | .610 | ** |
| | | | | | (.081) | |
| Non-head informant | | | | | .892 | |
| | | | | | (.172) | |
| Presence of young children | | | | | .916 | |
| | | | | | (.119) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 139,645 | | 139,645 | | 139,645 | |
| Experienced event | 567 | | 567 | | 567 | |
| Observations (n) | 8,131 | | 8,141 | | 8,133 | |
| df | 39 | | 41 | | 44 | |
| Deviance | 6,872.72 | | 6,733.64 | | 6,702.37 | |
| AIC | 6,952.72 | | 6,817.64 | | 6,792.37 | |
| BIC | 7,232.86 | | 7,111.84 | | 7,107.54 | |

+ p<0.10, * p<0.05, ** p<0.01

Table H.3: Competing-risk discrete-time hazard models of ineligibility, SEO sample only.

Appendix I

Discrete-time Hazard Models of First Exit through Death

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) | Odds Ratios (S.E.) |
| Head's Race (ref: white) | | | | |
| Black | 1.477 ** | 1.470 ** | 1.359 ** | 1.262 ** |
| | (.074) | (.073) | (.069) | (.078) |
| Other race | 1.074 | 1.070 | .986 | .942 |
| | (.169) | (.170) | (.159) | (.152) |
| Age | 1.041 ** | 1.046 ** | 1.057 ** | 1.056 ** |
| | (.007) | (.009) | (.009) | (.009) |
| Age squared | 1.000 ** | 1.000 ** | 1.000 ** | 1.000 ** |
| | (.000) | (.000) | (.000) | (.000) |
| Men | 1.698 ** | 1.689 ** | 1.732 ** | 1.735 ** |
| | (.066) | (.066) | (.068) | (.068) |
| Education (ref: high school degree) | | | | |
| No high school degree | 1.308 ** | 1.310 ** | 1.251 ** | 1.241 ** |
| | (.067) | (.068) | (.065) | (.065) |
| Some college | 1.029 | 1.027 | 1.050 | 1.054 |
| | (.074) | (.074) | (.076) | (.076) |
| College degree | .765 ** | .767 ** | .799 ** | .804 ** |
| | (.063) | (.063) | (.065) | (.066) |
| In Institution | | 3.636 ** | 3.763 ** | 3.771 ** |
| | | (.495) | (.538) | (.537) |
| Aged < 18 | | .962 | 1.113 | 1.094 |
| | | (.212) | (.247) | (.243) |
| Home owner | | | .634 ** | .642 ** |
| | | | (.027) | (.028) |
| Non-head informant | | | 1.364 ** | 1.362 ** |
| | | | (.066) | (.066) |
| Presence of young children | | | .880 | .875 + |
| | | | (.069) | (.069) |
| In SEO sample | | | | 1.137 + |
| | | | | (.075) |
| Wave Effects | Yes | Yes | Yes | Yes |
| Person-Waves | 316,651 | 316,651 | 316,651 | 316,651 |
| Experienced event | 2,952 | 2,952 | 2,952 | 2,952 |
| Observations (n) | 17,324 | 17,324 | 17,324 | 17,324 |
| df | 41 | 43 | 46 | 47 |
| Deviance | 26,639.99 | 26,571.60 | 26,417.94 | 26,413.15 |
| AIC | 26,723.99 | 26,659.60 | 26,511.94 | 26,509.15 |
| BIC | 27,049.91 | 27,001.04 | 26,876.65 | 26,881.62 |

+ p<0.10, * p<0.05, ** p<0.01

Table I.1: Competing-risk discrete-time hazard models of death, SRC and SEO samples.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.303 | ** | 1.291 | ** | 1.242 | * |
| | (.111) | | (.110) | | (.107) | |
| Other race | .994 | | .979 | | .937 | |
| | (.181) | | (.182) | | (.172) | |
| Age | 1.035 | ** | 1.047 | ** | 1.064 | ** |
| | (.009) | | (.011) | | (.012) | |
| Age squared | 1.000 | ** | 1.000 | ** | 1.000 | + |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.788 | ** | 1.788 | ** | 1.845 | ** |
| | (.084) | | (.083) | | (.088) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.398 | ** | 1.405 | ** | 1.331 | ** |
| | (.084) | | (.085) | | (.082) | |
| Some college | 1.072 | | 1.073 | | 1.092 | |
| | (.085) | | (.085) | | (.086) | |
| College degree | .806 | * | .809 | * | .836 | * |
| | (.071) | | (.071) | | (.074) | |
| In Institution | | | 5.310 | ** | 5.305 | ** |
| | | | (.932) | | (1.030) | |
| Aged < 18 | | | 1.126 | | 1.419 | |
| | | | (.397) | | (.504) | |
| Home owner | | | | | .610 | ** |
| | | | | | (.033) | |
| Non-head informant | | | | | 1.413 | ** |
| | | | | | (.082) | |
| Presence of young children | | | | | .907 | |
| | | | | | (.124) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 172,808 | | 172,808 | | 172,808 | |
| Experienced event | 1,975 | | 1,975 | | 1,975 | |
| Observations (n) | 9,048 | | 9,048 | | 9,048 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 16,787.07 | | 16,712.28 | | 16,597.63 | |
| AIC | 16,871.07 | | 16,800.28 | | 16,691.63 | |
| BIC | 17,169.70 | | 17,113.13 | | 17,025.82 | |

+ p<0.10, * p<0.05, ** p<0.01

Table I.2: Competing-risk discrete-time hazard models of death, SRC sample.

| | Model 1 | | Model 2 | | Model 3 | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.355 | ** | 1.351 | ** | 1.328 | ** |
| | (.121) | | (.121) | | (.120) | |
| Other race | 1.021 | | 1.022 | | .985 | |
| | (.260) | | (.261) | | (.257) | |
| Age | 1.056 | ** | 1.056 | ** | 1.059 | ** |
| | (.014) | | (.020) | | (.020) | |
| Age squared | 1.000 | | 1.000 | | 1.000 | |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.593 | ** | 1.582 | ** | 1.592 | ** |
| | (.106) | | (.106) | | (.107) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.072 | | 1.071 | | 1.060 | |
| | (.105) | | (.105) | | (.105) | |
| Some college | .961 | | .959 | | .984 | |
| | (.177) | | (.176) | | (.184) | |
| College degree | .709 | | .710 | | .769 | |
| | (.180) | | (.180) | | (.190) | |
| In Institution | | | 1.979 | ** | 2.082 | ** |
| | | | (.507) | | (.534) | |
| Aged < 18 | | | .914 | | .964 | |
| | | | (.295) | | (.314) | |
| Home owner | | | | | .725 | ** |
| | | | | | (.053) | |
| Non-head informant | | | | | 1.264 | ** |
| | | | | | (.110) | |
| Presence of young children | | | | | .846 | + |
| | | | | | (.080) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 143,843 | | 143,843 | | 143,843 | |
| Experienced event | 977 | | 977 | | 977 | |
| Observations (n) | 8,276 | | 8,276 | | 8,276 | |
| df | 41 | | 43 | | 46 | |
| Deviance | 9,789.91 | | 9,783.95 | | 9,753.99 | |
| AIC | 9,873.91 | | 9,871.95 | | 9,847.99 | |
| BIC | 10,168.79 | | 10,180.88 | | 10,177.99 | |

+ p<0.10, * p<0.05, ** p<0.01

Table I.3: Competing-risk discrete-time hazard models of death, SEO sample.

# Appendix J

## Model Comparison for SEO and SRC Samples

| | All | | SRC | | SEO | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.234 | ** | 1.328 | ** | 1.188 | ** |
| | (.045) | | (.087) | | (.078) | |
| Other race | 1.899 | ** | 1.900 | ** | 1.835 | ** |
| | (.161) | | (.248) | | (.224) | |
| Age | .935 | ** | .931 | ** | .951 | ** |
| | (.003) | | (.004) | | (.005) | |
| Age squared | 1.001 | ** | 1.001 | ** | 1.001 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.264 | ** | 1.246 | ** | 1.294 | ** |
| | (.022) | | (.029) | | (.035) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.106 | ** | 1.193 | ** | .995 | |
| | (.043) | | (.061) | | (.058) | |
| Some college | .910 | | .915 | | .979 | |
| | (.053) | | (.062) | | (.117) | |
| College degree | .676 | ** | .714 | ** | .646 | ** |
| | (.044) | | (.051) | | (.106) | |
| In Institution | 1.622 | ** | 1.623 | ** | 1.603 | ** |
| | (.094) | | (.145) | | (.124) | |
| Aged < 18 | .448 | ** | .501 | ** | .498 | ** |
| | (.023) | | (.040) | | (.035) | |
| Home owner | .660 | ** | .641 | ** | .674 | ** |
| | (.021) | | (.027) | | (.034) | |
| Non-head informant | 1.334 | ** | 1.425 | ** | 1.223 | ** |
| | (.050) | | (.071) | | (.071) | |
| Presence of young children | .781 | ** | .751 | ** | .836 | ** |
| | (.032) | | (.050) | | (.043) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 172,808 | | 143,843 | |
| Experienced event | 12,200 | | 6,296 | | 5,904 | |
| Observations (n) | 17,324 | | 9,048 | | 8,276 | |
| df | 46 | | 46 | | 46 | |
| Deviance | 97,198.82 | | 49,149.95 | | 47,636.20 | |
| AIC | 97,292.82 | | 49,243.95 | | 47,730.20 | |
| BIC | 97,657.53 | | 49,578.14 | | 48,060.19 | |

+ p<0.10, * p<0.05, ** p<0.01

Table J.1: Discrete-time hazard models of first exit for any reason.

| | All | | SRC | | SEO | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.780 | ** | 2.008 | ** | 1.552 | ** |
| | (.126) | | (.254) | | (.172) | |
| Other race | 2.999 | ** | 3.228 | ** | 2.544 | ** |
| | (.407) | | (.728) | | (.469) | |
| Age | .967 | ** | .942 | ** | .990 | |
| | (.007) | | (.009) | | (.009) | |
| Age squared | 1.000 | | 1.000 | * | 1.000 | ** |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.477 | ** | 1.310 | ** | 1.595 | ** |
| | (.050) | | (.070) | | (.070) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.121 | | 1.213 | | 1.056 | |
| | (.082) | | (.143) | | (.096) | |
| Some college | 1.038 | | .893 | | 1.310 | |
| | (.121) | | (.141) | | (.234) | |
| College degree | .610 | ** | .583 | ** | .852 | |
| | (.080) | | (.096) | | (.201) | |
| In Institution | 1.575 | ** | 1.359 | * | 1.628 | ** |
| | (.133) | | (.200) | | (.169) | |
| Aged < 18 | .234 | ** | .209 | ** | .281 | ** |
| | (.024) | | (.036) | | (.034) | |
| Home owner | .432 | ** | .384 | ** | .461 | ** |
| | (.029) | | (.037) | | (.041) | |
| Non-head informant | 1.114 | | 1.206 | | 1.035 | |
| | (.091) | | (.164) | | (.105) | |
| Presence of young children | .838 | ** | .674 | ** | .971 | |
| | (.054) | | (.076) | | (.075) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 172,808 | | 142,985 | |
| Experienced event | 3,508 | | 1,260 | | 2,248 | |
| Observations (n) | 17,324 | | 9,048 | | 8,249 | |
| df | 46 | | 46 | | 45 | |
| Deviance | 33,856.93 | | 12,093.13 | | 21,345.37 | |
| AIC | 33,950.93 | | 12,187.13 | | 21,437.37 | |
| BIC | 34,315.64 | | 12,521.32 | | 21,760.19 | |

+ p<0.10, * p<0.05, ** p<0.01

Table J.2: Competing-risk discrete-time hazard models of non-contact.

| | All | | SRC | | SEO | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | .921 | | .966 | | .958 | |
| | (.060) | | (.110) | | (.106) | |
| Other race | 1.629 | ** | 1.788 | ** | 1.570 | * |
| | (.223) | | (.368) | | (.314) | |
| Age | 1.008 | + | 1.021 | ** | 1.003 | |
| | (.005) | | (.007) | | (.008) | |
| Age squared | 1.000 | ** | 1.000 | ** | 1.000 | * |
| | (.000) | | (.000) | | (.000) | |
| Men | .998 | | .998 | | .988 | |
| | (.027) | | (.033) | | (.044) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.033 | | 1.143 | | .901 | |
| | (.068) | | (.096) | | (.092) | |
| Some college | .831 | + | .870 | | .793 | |
| | (.082) | | (.098) | | (.167) | |
| College degree | .679 | ** | .749 | * | .438 | ** |
| | (.073) | | (.088) | | (.121) | |
| In Institution | 1.085 | | 1.072 | | 1.118 | |
| | (.112) | | (.163) | | (.156) | |
| Aged < 18 | .688 | ** | .877 | | .632 | ** |
| | (.052) | | (.101) | | (.064) | |
| Home owner | .997 | | .956 | | .987 | |
| | (.058) | | (.071) | | (.089) | |
| Non-head informant | 1.458 | ** | 1.465 | ** | 1.425 | ** |
| | (.095) | | (.120) | | (.147) | |
| Presence of young children | .701 | ** | .732 | ** | .709 | ** |
| | (.047) | | (.069) | | (.067) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 172,808 | | 143,843 | |
| Experienced event | 4,667 | | 2,555 | | 2,112 | |
| Observations (n) | 17,324 | | 9,048 | | 8,276 | |
| df | 46 | | 46 | | 46 | |
| Deviance | 46,671.36 | | 25,103.27 | | 21,317.60 | |
| AIC | 46,765.36 | | 25,197.27 | | 21,411.60 | |
| BIC | 47,130.07 | | 25,531.45 | | 21,741.60 | |

+ p<0.10, * p<0.05, ** p<0.01

Table J.3: Competing-risk discrete-time hazard models of refusal.

| | All | | SRC | | SEO | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.359 | ** | 1.242 | * | 1.328 | ** |
| | (.069) | | (.107) | | (.120) | |
| Other race | .986 | | .937 | | .985 | |
| | (.159) | | (.172) | | (.257) | |
| Age | 1.057 | ** | 1.064 | ** | 1.059 | ** |
| | (.009) | | (.012) | | (.020) | |
| Age squared | 1.000 | ** | 1.000 | + | 1.000 | |
| | (.000) | | (.000) | | (.000) | |
| Men | 1.732 | ** | 1.845 | ** | 1.592 | ** |
| | (.068) | | (.088) | | (.107) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.251 | ** | 1.331 | ** | 1.060 | |
| | (.065) | | (.082) | | (.105) | |
| Some college | 1.050 | | 1.092 | | .984 | |
| | (.076) | | (.086) | | (.184) | |
| College degree | .799 | ** | .836 | * | .769 | |
| | (.065) | | (.074) | | (.190) | |
| In Institution | 3.763 | ** | 5.305 | ** | 2.082 | ** |
| | (.538) | | (1.030) | | (.534) | |
| Aged < 18 | 1.113 | | 1.419 | | .964 | |
| | (.247) | | (.504) | | (.314) | |
| Home owner | .634 | ** | .610 | ** | .725 | ** |
| | (.027) | | (.033) | | (.053) | |
| Non-head informant | 1.364 | ** | 1.413 | ** | 1.264 | ** |
| | (.066) | | (.082) | | (.110) | |
| Presence of young children | .880 | | .907 | | .846 | + |
| | (.069) | | (.124) | | (.080) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 172,808 | | 143,843 | |
| Experienced event | 2,952 | | 1,975 | | 977 | |
| Observations (n) | 17,324 | | 9,048 | | 8,276 | |
| df | 46 | | 46 | | 46 | |
| Deviance | 26,417.94 | | 16,597.63 | | 9,753.99 | |
| AIC | 26,511.94 | | 16,691.63 | | 9,847.99 | |
| BIC | 26,876.65 | | 17,025.82 | | 10,177.99 | |

+ p<0.10, * p<0.05, ** p<0.01

Table J.4: Competing-risk discrete-time hazard models of death.

| | All | | SRC | | SEO | |
|---|---|---|---|---|---|---|
| | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | | Odds Ratios (S.E.) | |
| Head's Race (ref: white) | | | | | | |
| Black | 1.096 | | 1.332 | | .883 | |
| | (.112) | | (.280) | | (.146) | |
| Other race | 1.581 | + | .970 | | 1.556 | |
| | (.404) | | (.492) | | (.485) | |
| Age | .956 | ** | .954 | ** | .986 | |
| | (.010) | | (.016) | | (.016) | |
| Age squared | 1.001 | ** | 1.001 | ** | 1.000 | * |
| | (.000) | | (.000) | | (.000) | |
| Men | .930 | | .833 | * | 1.017 | |
| | (.055) | | (.070) | | (.086) | |
| Education (ref: high school degree) | | | | | | |
| No high school degree | 1.143 | | 1.209 | | 1.029 | |
| | (.128) | | (.189) | | (.161) | |
| Some college | .672 | + | .746 | | .605 | |
| | (.146) | | (.204) | | (.189) | |
| College degree | .535 | ** | .539 | ** | .782 | |
| | (.109) | | (.123) | | (.367) | |
| In Institution | 3.668 | ** | 2.586 | * | 4.405 | ** |
| | (.805) | | (.988) | | (1.155) | |
| Aged < 18 | 7.776 | ** | 8.847 | ** | 8.373 | ** |
| | (1.713) | | (3.456) | | (2.282) | |
| Home owner | .610 | ** | .622 | ** | .610 | ** |
| | (.057) | | (.086) | | (.081) | |
| Non-head informant | 1.079 | | 1.278 | | .892 | |
| | (.141) | | (.227) | | (.172) | |
| Presence of young children | .894 | | .937 | | .916 | |
| | (.107) | | (.230) | | (.119) | |
| Wave Effects | Yes | | Yes | | Yes | |
| Person-Waves | 316,651 | | 168,887 | | 139,645 | |
| Experienced event | 1,073 | | 506 | | 567 | |
| Observations (n) | 17,324 | | 8,945 | | 8,133 | |
| df | 46 | | 45 | | 44 | |
| Deviance | 12,657.57 | | 5,834.77 | | 6,702.37 | |
| AIC | 12,751.57 | | 5,926.77 | | 6,792.37 | |
| BIC | 13,116.29 | | 6,253.31 | | 7,107.54 | |

+ p<0.10, * p<0.05, ** p<0.01

Table J.5: Competing-risk discrete-time hazard models of ineligibility.

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