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ABSTRACT

Effects of Attrition and Non-Response in the Health and Retirement Study

We study the effect of attrition and other forms of non-response on the representativity over time of the Health and Retirement Study (HRS) sample born 1931-1941; the sample was initially drawn in 1992. Although some baseline characteristics of respondents do appear correlated with non-response over time, the 2002 sample of respondents does not appear to suffer significantly from selection on observables, except for race and ethnicity; for these two observables, longitudinal weights based on the Current Population Survey (CPS) can be used and are provided with the data set. We attribute this lack of selection to the fact that attritors who differ most eventually come back to the survey in waves prior to 2002. Although this allows cross-sections to remain fairly representative in later waves, it suggests that longitudinal analysis should use the unbalanced sample rather than the balanced sample of those interviewed in all waves. Individuals who attrit but who are recruited back into the survey are very different from those who are permanent attritors to the HRS. Finally, we investigate the selective nature of the decision of respondents to grant HRS permission to access their Social Security records and of the non-response introduced by employers of pension policyholders not providing HRS with worker's Summary Plan Descriptions. We find that subsamples for which such information is available are selective on a number of dimensions, such as education and other socioeconomic status (SES) outcomes.

JEL Classification: C42, C80, I10, J26

Keywords: attrition, non-response, administrative records, economics of aging

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1. Introduction

Longitudinal surveys such as the Health and Retirement Study (HRS) provide a rich source of information to study the evolution of various socioeconomic and health outcomes of a population of interest. However, as in any panel survey, a major potential weakness is that some respondents drop out over time, and when their characteristics are different from those in the retention sample, the representativity of the sample deteriorates, which may invalidate inferences drawn for the population of interest.

The original cohort entering the HRS study in 1992 was composed of individuals born between 1931 and 1941 and their spouses, irrespective of their age. The next year, a much older cohort was interviewed, the Study of Assets and Health Dynamics among the Oldest Old (AHEAD) cohort, which was born before 1923. Both these cohorts have been followed every two years up through 2006.[1] In 1998, two new cohorts were added and blended into the original sample, the so-called Child of the Depression Age Cohort (CODA), born between 1924 and 1930, and the War Babies cohort, born between 1942 and 1947. Every wave sees some respondents leave the study. Hence, it is important to know if the sample in recent waves remains a random sample from the population age 55+ in the United States.

We are not aware of studies that have looked closely at how problems related to attrition affect the representativity of the HRS. In the case where attrition is a problem, it is important to develop tools that researchers can use to deal with such problems and get a sense of which analyses are more likely than others to be affected by attrition. In this study, we look at the effects of attrition from 1992 to 2002 for the original cohort born 1931–1941. We choose to work with this cohort because it has been in the study for the longest time and is, therefore, the most likely cohort, along with AHEAD, to have suffered from non-random attrition.

We also investigate the selective nature of two other aspects of the HRS. First, HRS seeks consent from respondents to merge their Social Security record from SSA. Although the consent rate is high, there remains the possibility that those refusing the match may be different from those giving consent.[2] The same holds true for Summary Plan Descriptions (SPD) of pension plan holders that HRS requests from employers. The information contained in such records is widely used by researchers. Hence, we also investigate the selective nature of restricting the sample to respondents with a match.

We should make clear that attrition can have effects on some analyses and not on others. This will depend on several dimensions of the analysis, such as whether the analysis intends to look at a cross-section in a given year or intends to follow respondents over time to learn about some pattern of behavior, the outcome of interest being estimated, and the specific model (particularly conditioning variables) one is considering. Effects of attrition can result from selection on observable or unobservable characteristics. Our analysis will focus mostly on the effect of attrition from selection on observables on cross-sectional comparisons. As for longitudinal analysis, we will investigate the dynamic nature over waves of participation and mortality in the HRS. This will be informative about likely effects on longitudinal analysis.

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[1]The AHEAD cohort is interviewed in 1993, 1995 and then merged with the main study for 1998 onwards.
[2]Olson (1999) and Haider and Solon (2000) also investigate the representativity of the SSA record match.

Section 2 presents data about participation and vital status in the original HRS cohort over waves. Section 3 presents a statistical model of attrition and an analysis of the determinants of attrition and mortality. Section 4 investigates whether the match outcome for Social Security records and Summary Plan Descriptions for pension holders is random. Section 5 presents the impact of non-random attrition on some cross-sectional tabulations of interest using inverse probability weighting based on estimates from Section 3. Finally, Section 6 concludes.

2. The HRS Cohort 1931–1941

The target population of the original HRS cohort is households where at least one member was born between 1931 and 1941. The sample is drawn using a multistage area probability sample of households and an interview is attempted with all age-eligible respondents and their spouses. Only non-institutionalized individuals are considered at baseline, although the HRS follows respondents entering nursing homes in later waves. The Institute for Social Research (ISR) in Michigan conducts the survey.

The HRS has three supplements that over-sample respondents from certain groups. These supplements consist of a 2:1 over-sample of African Americans, Hispanics, and Floridians. Of the 15,497 interviews attempted, 12,654 were realized. This yields an overall individual response rate of 81.6% at baseline. The response rate is very similar for the main study and the African American (81.1%) and Floridian (82.2%) samples, but the rate is lower for the Hispanic supplement (77%).

Table 2.1 Baseline Non-Response

Sample Component		Eligible	Interviewed	Response Rate
Complete Sample		15,497	12,654	81.6%
Subsets	Core (not Florida)	12,052	9,872	81.9%
	African American Supplement	2,211	1,794	81.1%
	Hispanic Supplement	509	392	77.0%
	Florida Sample	725	596	82.2%

Notes: Taken from HRS website: http://hrsonline.isr.umich.edu/ docs/sho_refs.php?hfyle=design&xtyp=2. These numbers refer to all respondents and their spouses who can be born before 1931 or after 1941.

We focus our analysis on respondents born between 1931 and 1941. These respondents are age 51 to 61 in 1992 and 61 to 71 in 2002. These sample selection criteria imply that the sample in 1992 is composed of 10,069 respondents.

An issue with any survey, longitudinal or not, is whether respondents who decided to participate at baseline are representative of the population of interest. Answering this question is inherently difficult because characteristics of those who did not participate are unknown. This is unlike attrition, where at least baseline characteristics are available for the group who will not respond in later waves.

Hence, we must resort to a comparison with an external dataset to measure the extent of non-randomness from non-response. Other studies, such as the Current Population Survey (CPS,) also suffer from non-response.[3] This implies that even if the composition of the CPS and HRS sample is different, it may be complicated to interpret different statistics across surveys. One example is household wealth, where it has been established that neither CPS or HRS is representative of the high end of the wealth distribution (Juster, Smith, and Stafford, 1999). Because the HRS does not capture the top few percent of the wealth distribution, it, like all household surveys, will not match mean population wealth well.

The HRS is different from the CPS along certain recognized dimensions. The HRS dataset contains weights that match cell frequencies in the CPS defined by race and ethnicity, birth cohort, and gender at baseline. This corrects for the HRS over-sampling design and for differential unit non-response across these groups. Beyond these characteristics, it is still difficult to compare the CPS and the HRS at baseline because differences in computed statistics from each dataset can and sometimes are the result of question wording differences. One important example is that because of changes in question wording over time, CPS and HRS incomes no longer match well. (see Banks, Marmot, Oldfield, and Smith, 2006).

Nevertheless, some evidence is presented in Table 2.2 showing that the samples are quite similar in terms of some measures that are comparable across the two surveys (HRS and CPS) for respondents age 51-61: educational attainment, marital status, and labor force participation.

Table 2.2 Baseline Comparison of HRS and CPS in 1992

Characteristics	HRS 1992 (weighted)	CPS 1992
Education		
% less than high school	23	23.7
% high school or GED	38.8	38
% with at least some college education	38.2	38.3
Marital Status		
% married	74.4	73.8
% widow(ed)	5.9	6.3
% divorced/separated	13.8	14.5
% never married	6.0	5.4
Labor Force		
(%) Main activity is working	66.3	65.9

Notes: CPS statistics from March Supplement of 1992. 1992 CPS weights are used for HRS statistics, while we used the weights provided for march supplement in the CPS. Respondents are age 51–61.

Because the HRS is a study of a near-elderly population, it emphasizes tracking the vital status of respondents over waves, shown in Table 2.3. Deaths are reported by relatives contacted by an interviewer. Respondents are presumed alive when they cannot

[3] The non-response rate for the March Supplement to the CPS is lower (5-10%) than that for the baseline HRS.

be reached but some information identifies them as alive. Finally, when no vital status information can be obtained, vital status is coded as unknown.

Table 2.3 Vital Status in Waves 1992-2002

Vital Status	1992	1994	1996	1998	2000	2002
Alive	10069	9831	9522	9090	8616	8035
presumed alive	0	16	55	63	93	221
death reported in wave	0	168	211	213	272	345
Mortality rate		1.7%	2.1%	2.2%	3.0%	4.0%
death reported prior wave	0	0	168	379	592	864
Cumulative mortality rate		1.7%	3.8%	5.9%	8.6%	12.0%
vital status unknown	0	54	113	324	496	604

Notes: A respondent is presumed alive if the interviewer cannot reach a respondent but has access to some information that the respondent might be alive. If no such information can be obtained, the respondent's vital status is classified as unknown. Mortality rate is calculated as a fraction of alive or presumed alive respondents at previous wave. The cumulative mortality rate is calculated as the fraction of 1992 respondents known to be alive.

As shown in the table, the mortality rate grows from 1.7% in the second wave to 4% in 2002 as the cohort ages. The unweighted cumulative mortality rate over all waves is 12%. A back-of-the-envelope calculation using SSA life table mortality rates for an individual age 55 (the mid-point age in 1992) yields a cumulative mortality rate of 11.8% (gender weighted using fraction of males in the sample) over this 10-year period. If we weight HRS deaths to correct for the over-sampling of African Americans and Hispanics (as well as Floridians), we obtain a cumulative mortality rate of 11.3%. Therefore, the HRS tracks the deceased reasonably well.

The ISR attempts two types of interviews depending on the vital status of the respondent. In the event where death is reported, an exit interview rather than a core interview is attempted. The exit interview is most often accomplished with the widow(er) or with another close relative of the deceased respondent. It is in general shorter than the core interview.

Table 2.4 reports that in 1992, 152 core interviews are missing and 156 are not in the sample for that wave. The first 152 are absent age-eligible spouses who do not provide an interview at baseline. These last 156 respondents are future spouses of age-eligible HRS respondents.[4] In general, the response rate on core interviews is higher in all subsequent waves, although there is a small downward time trend (90.5% in 1994 versus 87.3% in 2002, see Table 2.4).

The response rate on exit interviews is generally lower than on core interviews, perhaps because relatives are less inclined to participate in a survey asking about the deceased. Once exit interviews are completed, a respondent is classified as out-of sample. Other reasons for exclusion from the sample include an explicit request by the respondent to be removed from the study. By 2002, 11.5% of original respondents are considered out-of-sample. Table A.1 in the appendix gives a breakdown of all interview/sample membership and vital status found in the HRS.

[4] We calculated the overall response rate in 1992 as the product of the response rate for age-eligible respondents and their spouse (81.6%), and the response rate is conditional on being considered in sample in 1992 (98.5%). 15,497 interviews were attempted in 1992, of which 12,654 were completed. But this includes the spouses of age-eligible respondents who were born before 1931 or after 1941. Hence, we do not know the number of attempted interviews on individuals born between 1931 and 1941.

Table 2.4 Interview Status in Waves 1992–2002

	Wave					
Interview status	**1992**	**1994**	**1996**	**1998**	**2000**	**2002**
Core interview attempted						
core interview obtained	9,761	8,845	8,469	8,097	7,644	7,379
core interview missing	152	925	1124	1151	1246	1072
response rate	80.4%	90.5%	88.3%	87.6%	86.0%	87.3%
exit interview attempted						
exit interview obtained	0	129	172	231	319	383
exit interview missing	0	39	41	49	76	80
response rate		76.8%	80.8%	82.5%	80.8%	82.7%
out of sample	156	131	263	541	784	1,155
% out of sample	1.5%	1.3%	2.6%	5.4%	7.8%	11.5%
total	10069	10069	10069	10069	10069	10069

Notes: Calculated from frequencies in Table A.1. Exit interviews include post-exit interviews, while out-of- sample includes non-eligible spouses that become eligible at a later wave, those who are permanently dropped from the sample (at their request or from HRS decision,) and respondents that are dead and for whom an exit or post-exit interview was completed. For 1992, the response rate does not take account of the initial round of non-response, as shown in Table 2.1. Since we do not include spouses born before 1931 or after 1941 in the analysis, the number of respondents interviewed at baseline will differ from what is shown in Table 2.1

Interviewers re-contact every respondent who did not provide a core interview in the previous wave but is considered to be part of the sample. (This excludes baseline non-respondents.) Each participant gets $20 for an interview.[5] As a result of re-contacts, participation patterns in the HRS are considerably diverse. Figure 2.1 shows the various flows of entry and exit across years. The traditional exit routes are mortality and what is commonly defined as attrition, or non-response, in a given wave conditional on providing an interview in a previous wave. For example, of the 9761 respondents who provided core interviews in 1992, 167 (1.7%) were reported dead the following wave, and 788 (8.1%) were missing because they could not be reached or they refused to provide an interview. In 2002, only 5% of respondents take this last route. This pattern most likely reflects heterogeneity in terms of general willingness to participate in surveys, since those with a lower relative willingness to participate will have left in earlier waves.

There is also entry of previously interviewed respondents who may have skipped an interview. Starting in 1996, between 26.8% and 44.3% of respondents with missing interviews come back to the panel to provide a core interview. This last feature of participation sequences can be of major importance for the representativity of the HRS sample over time. It may help to keep cumulative attrition down compared to other surveys that might not attempt to re-contact respondents missing in a given wave. If these respondents coming back are also those with different characteristics, this can also attenuate the attrition bias in inferences on cross-sections. It also shows that an analysis of attrition in the HRS should not consider non-response as an absorbing state.

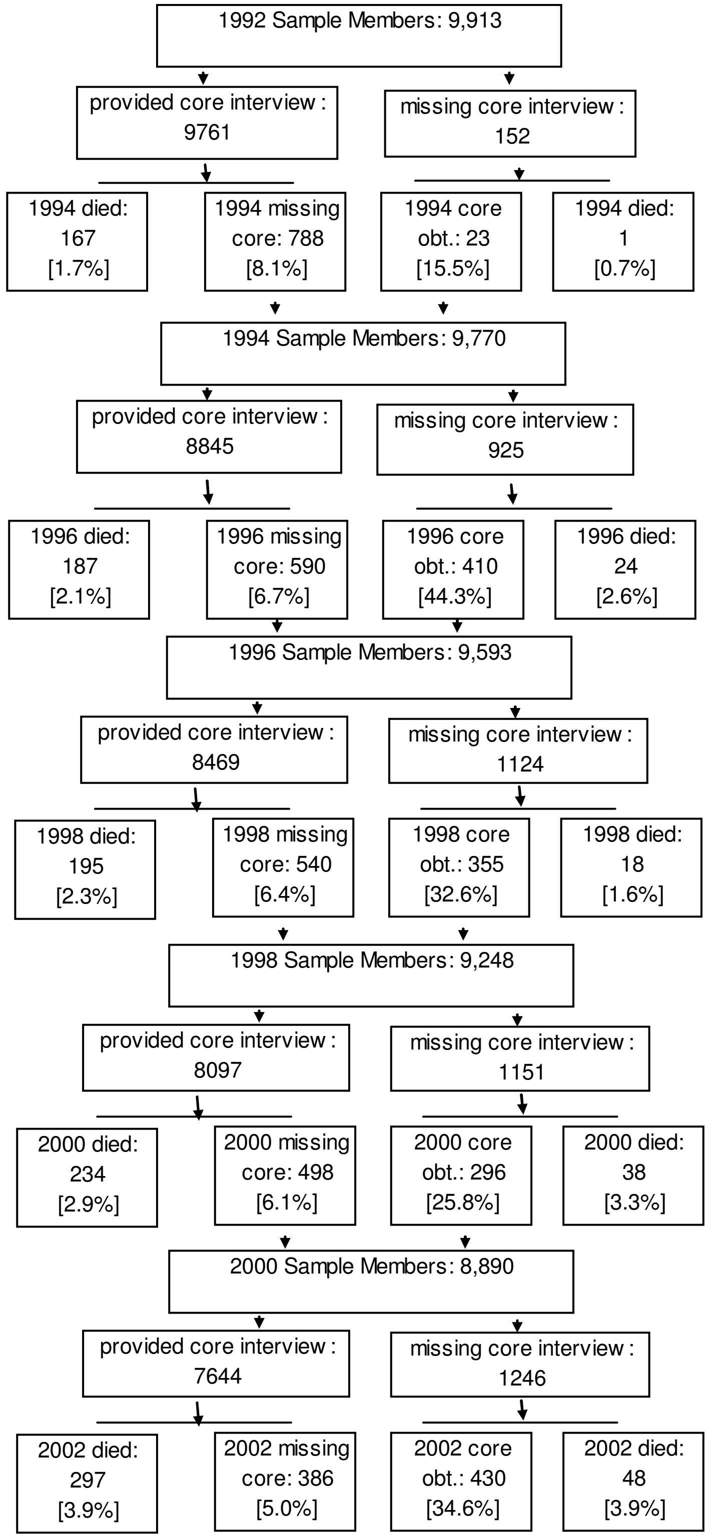
Given that a fraction of respondents are not re-interviewed in later waves, one might ask if the remaining sample from the population remains representative over time.

[5]IRS also experimented with randomized "end games" for a subset of respondents classified "hard refusal." The reward for participation in such games can reach up to $100 and is reported in Hill and Willis (2001) to have some effect on participation. A similar experiment was accomplished in 2000. Rodgers (2006) reports strong participation effects for re-contacts of participates who did not provide an interview at the last wave.

If those leaving the panel in the left branch of Figure 2.1 have systematically different characteristics, measured and unmeasured, this will distort the view of the population of interest that one draws from the sample of HRS respondents.

Using baseline weights that match the CPS on birth cohort, race, ethnicity and gender will restore the representativity of the sample if attrition is totally random. If attrition is only determined by the same factors that go in the construction of weights that match the CPS in later years (race and ethnicity, household composition, birth cohort, and gender), yearly CPS weights will also restore the representativity of the sample so that inferences on each cross-section should not suffer from any bias from attrition. But the issue of importance is to see if attrition is determined by additional factors not captured in the CPS weights. In such a case, the representativity of the HRS would deteriorate over the years and HRS-provided weights would not correct entirely for the loss of representativity. This could lead to biased inference on the population of interest. We formalize these concepts in the next section and investigate what are the factors that may drive attrition.

Figure 2.1 Exits and Entry between 1992 and 2002



3. Attrition and Mortality

To adopt a precise definition of attrition and the context in which it may matter, we consider a simplified two-period longitudinal survey. In the first period, assume a representative sample of size *N* is drawn from the population of interest. The information gathered consists of (y_{i0}, z_i) where y_{i0} is some outcome of interest and z_i are other characteristics of respondents. This information is gathered for every respondent at baseline. Assume that every respondent survives to the second period.

In the second period, denote by $s_i = (0,1)$ an indicator for whether or not the respondent provides information on y_{i1}. Some respondents will not provide an interview in the second period and are coded as attritors ($s_i = 0$). However, they remain part of the population of interest. The question is whether the selected sample for which y_{i1} is observed ($s_i = 1$) remains representative for the population of interest and, if not, how this affects inference on some feature (e.g. mean) of the population distribution of (y_{i1}, z_i).

3.1.1 Selection on Observables and Weighting

Suppose we are interested in estimating the mean of y_i in the second period, $E(y_{i1})$. Since we can only use the retention sample, what we can estimate consistently is $E(y_{i1} \mid s_i = 1)$. A sufficient assumption for $E(y_{i1}) = E(y_{i1} \mid s_i = 1)$ is that y_{i1} is independent of s_i. This assumption may be too strong. For example, some characteristics z_i may affect y_{i1} as well as s_i. They may drive the costs and benefits a respondent could derive from an interview, thus affecting the decision to participate (Hill and Willis, 2001). This assumption is known as *missing completely at random* (MCAR, see Rubin and Little, 1987). If correct, it allows to consistently estimate the quantity of interest from the retention sample without any corrections. A less restrictive assumption is the assumption *missing at random* (MAR, see Little and Rubin, 1987) or *selection on observables* (Fitzgerald et al., 1998):

$$\text{MAR}_{\text{x}} : y_{i1} \perp s_i \mid x_i \tag{1}$$

The symbol $\perp$ denotes independence (conditional on x_i). Here x_i may contain time invariant characteristics z_i observed at baseline but may also contain period 1 outcomes ($x_i = (z_i, y_{i0})$). Denote the retention probability (the probability that $s_i = 1$) conditional on x_i as $p(s_i \mid x_i)$. Thus individuals with different characteristics and baseline outcomes can have different probabilities of staying in the survey in the second period. Under the missing at random assumption it may well be the case that $E(y_{i1}) \neq E(y_{i1} \mid s_i = 1)$. It can be easily shown, however, that if MAR holds

$$E(\frac{s_i}{p(s_i \mid x_i)} y_{i1}) = E(y_{i1}). \tag{2}$$

Hence weighting can be used to get consistent estimates, with the weight for each respondent given by the inverse of the retention probability $p(s_i \mid x_i)$.

Two types of weights can be constructed: frequency weights and inverse probability weights. Frequency weights are provided with the HRS dataset for each wave. We refer to these weights as the HRS weights. These weights use the ratio of the sample size in a given year for the CPS (a cross-section) and HRS in cells defined by gender, race, household composition and birth cohort. Hence weights are a function $w(q_i)$ where q_i has gender, race, household composition and birth cohort. These weights are adjusted each wave (using the CPS March Supplement of the corresponding year) and hence account for attrition under the assumptions that attrition or selection play no role in the CPS and that attrition in the HRS is MAR with $x_i = q_i$:

$$MAR_q : y_{i1} \perp s_i \mid q_i . \qquad (3)$$

This assumption is still rather restrictive since there may well be other variables not included in q_i that affect both response behavior and the variable of interest.

Alternatively, a set of weights can be constructed from (2) using an expanded set of characteristics x_i (containing q_i) observed in one or more earlier waves of the HRS. Estimating $p(s_i \mid x_i)$ is straightforward using, for example, a logit or probit model. Under the assumption MAR_x, "inverse probability weights" (IPW) can be constructed (Horvitz and Thomson, 1952; Horowitz and Manski, 1998; Wooldridge, 2003), given by

$$\mathrm{IPW} : w(x_i) = \frac{1}{p(s_i \mid x_i)} \qquad (4)$$

HRS weights and IPW weights should have the same effect on the estimates of quantities of interest if the MAR assumption also holds conditional on q_i.

Our strategy will be to first investigate how $p(s_i \mid x_i)$ depends on a large set of time invariant characteristics and baseline outcomes x_i. Indeed, we can check if $p(s_i \mid x_i) = p(s_i \mid q_i)$. If it does, i.e., observed baseline characteristics other than race, ethnicity, household composition, birth cohort and gender do not explain attrition, then HRS weights are enough to correct for selection (under the maintained assumption MAR_x). If baseline characteristics other than those in q_i predict attrition, we will analyze how using inverse probability weights based upon a full set of characteristics x_i gives different tabulations than when using HRS weights based upon q_i. The difference between the two tabulations will provide a measure of how selection on observables not in q_i affects tabulations of interest.

3.1.2 Selection on Unobservables

The MAR assumption will be violated if there are unobserved variables that are correlated with both y_{i1} and s_i conditional on x_i. Many selection models have been developed in the literature to deal with this case. A seminal example is Hausman and

Wise (1979).[6] All these models require exclusion restrictions for nonparametric identification – variables that drive attrition but have no effect on the variable of interest. Fitzgerald et al. (1998) point out that in the case of attrition it is virtually impossible to find plausible exclusion restrictions. Both respondent and interviewer characteristics are likely to be correlated with the outcome of interest.[7] In general the two assumptions (MAR and HW) are undistinguishable unless additional information is used. Hirano et al. (2001) propose using refreshment samples to distinguish between the two assumptions. They find support for the MAR assumption in a Dutch dataset on travel behavior. Because of the identification problems and the experience in the recent panel data literature, we do not explore selection on unobservables in this study and focus on correcting for selection on a rich set of observables.

3.2 Baseline Determinants of Attrition

We first study the determinants of attrition in the HRS by looking at how baseline (1992) characteristics of respondents correlate with whether they are still alive and provided an interview in 2002. This is informative if one intends to analyze the evolution of the 1931–1941 birth cohort from 1992 until 2002. We define four types of participation sequences from 1992 to 2002.

First, 64.3% of the 1992 respondents provide core interviews in all six waves from 1992 to 2002 (the *always in* group). Second, as seen in Figure 2.1, there is a sizeable fraction of respondents (8.9%) who respond in both 1992 and 2002 but who do not respond in at least one intermediate wave. We refer to these as *ever out*. The last two groups are respondents who are not interviewed in 2002. 12.1% of respondents die prior to interview in 2002. Finally, 14.7% of the 1992 respondents are not interviewed in 2002 for other reasons than death. We refer to them as *attritors*. These would fit under the traditional definition of attrition (although some of them may come back in 2004 or later).

Mortality does not affect the representativity of a sample over time in a cross-sectional sense, since the relevant population in a given year is that of the survivors. However, longitudinal analysis at the individual level may be contaminated by composition effects from mortality. For example, mortality might bias the estimates of the aggregate age trend of wealth holdings because of the well-known positive correlation between health and wealth (see, e.g. Attanasio and Hoynes, 2000). This makes it important to consider mortality as an alternative exit route in the analysis. However, our main focus is on non-response and its effect on inferences from the retention sample, particularly in 2002.

In Table 3.1, we present baseline characteristics of respondents who gave an interview in 1992 by type of participation over the 1992–2002 period. First, it is obvious that characteristics associated with mortality reflect the well-known relation between health and socioeconomic status (SES). Older individuals, African-Americans, Hispanics, unhealthy, and less educated respondents are more likely to die over the 10-year period. It turns out that the attritors group has an overrepresentation of individuals born outside the

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[6] An example of an application of a selection model to attrition in the PSID is given by Lillard and Panis (1998).

[7] Good interviewers are often assigned to "hard refusal" respondents based on observed characteristics.

United States, Hispanics, and to a lesser-extent African-Americans. However, along most attributes, the attritors are not very different from respondents interviewed in all waves in terms of education, marital status, and labor force participation status.

Rather, it is respondents who are ever out but present in 2002 who are quite different from respondents who answer in all waves. They are more likely to have experienced a divorce (37.3% versus 30.4%), and far more likely to be African-American (14.9 versus 8.7%) or Hispanic (13.4 versus 5.4%). They are generally less educated (16.6 versus 20.5% with college or above) and less likely to be retired or disabled (10.7 versus 15.5%). One can think of this sub-sample as the temporary attritors, and this group appears to be highly selective on observables.

Table 3.1 Baseline Characteristics by Type of Participation Sequence 1992–2002 (Weighted using Baseline HRS Weights)

		Status 2002			
Characteristics	**Always in**	**Ever out**	**Died**	**Attritors**	**total**
Demographics in 1992					
age (yrs)	55.5	55.0	56.4	56.4	55.6
female (%)	54.6%	48.5%	42.8%	41.5%	52.4%
born outside U.S. (%)	8.9%	13.7%	7.0%	6.9%	9.8%
African American (%)	8.7%	14.9%	26.2%	16.6%	10.3%
hispanic (%)	5.4%	13.4%	8.0%	5.5%	6.4%
married (%)	78.4%	72.7%	65.8%	67.5%	76.7%
widow(er) (%)	5.7%	5.2%	9.6%	8.8%	5.9%
divorced (%)	12.4%	18.7%	20.1%	19.4%	13.8%
once divorced (%)	30.4%	37.3%	38.2%	38.1%	31.9%
single (%)	3.4%	3.4%	4.5%	4.3%	3.6%
household size (#)	2.61	2.73	2.57	2.52	2.61
Health Status in 1992					
health good (%)	25.9%	28.2%	25.6%	30.3%	26.7%
health fair/poor (%)	16.1%	19.7%	46.7%	17.8%	20.1%
ever had severe cond. (%)	16.5%	14.9%	44.5%	17.3%	19.7%
ever had mild cond. (%)	36.9%	39.9%	61.1%	39.7%	40.3%
at least one ADL (%)	3.5%	5.2%	13.6%	2.9%	4.7%
SES and Employment Status in 1992					
high school (%)	39.1%	36.3%	37.4%	39.9%	38.8%
some college (%)	20.3%	19.2%	17.9%	19.1%	19.8%
college and above (%)	20.5%	16.6%	11.6%	15.6%	18.5%
own house (%)	83.9%	73.0%	71.7%	81.9%	81.3%
working (%)	68.4%	71.9%	48.5%	67.6%	66.3%
retired or disabled (%)	15.5%	10.7%	29.7%	16.1%	16.8%
not labor force (%)	13.4%	12.9%	12.1%	13.7%	13.3%
N	6,273	866	1,184	1,438	9,761
%	64.30%	8.90%	12.10%	14.70%	100%

Notes: See appendix for variable definitions. "Always in": respondents who provide core interviews in all 6 waves between 1992 and 2002. "Ever out": respondents who provide core interviews in 1992 and 2002 but have skipped one or more interviews in intermediate waves. "Died": Respondents in 1992 who died before 2002. Attritors: respondents not in the HRS in 2002. HRS 1992 weights used.

These statistics suggest that respondents "ever out" are far more different from those "always in" than they are from the "attritors." In fact, attritors are closer to respondents answering in all waves then to those ever out. We find qualitatively similar but quantitatively larger differences between the four groups when looking at the distribution of household wealth, income, and individual earnings in 1992 (Table 3.2).

Table 3.2 Baseline Wealth, Income and Earnings Distribution by Type of Response 1992–2002 (Weighted Using Baseline HRS Weights)

Household Wealth in 1992	**Mean**	**10th pctile**	**25th pctile**	**Median**	**75th pctile**	**90th pctile**
Always in	$ 264,791	$ 6,412	$ 53,220	$ 143,484	$ 307,777	$ 587,899
Ever out but in for 2002	$ 277,287	$ -	$ 16,671	$ 94,898	$ 246,222	$ 698,910
Died prior to 2002	$ 178,119	$ -	$ 10,067	$ 77,329	$ 190,565	$ 393,826
Attritor	$ 243,056	$ 3,462	$ 50,655	$ 141,064	$ 300,083	$ 582,596
Total	$ 252,801	$ 2,565	$ 43,602	$ 128,882	$ 288,541	$ 569,778
Household Income in 1992						
Always in	$ 66,430	$ 13,209	$ 28,405	$ 52,579	$ 83,613	$ 127,116
Ever out but in for 2002	$ 66,711	$ 10,259	$ 23,083	$ 47,705	$ 77,193	$ 119,264
Died prior to 2002	$ 44,598	$ 7,079	$ 15,048	$ 30,778	$ 58,380	$ 91,692
Attritor	$ 64,550	$ 12,824	$ 27,883	$ 50,270	$ 76,944	$ 116,699
Total	$ 63,706	$ 11,542	$ 25,648	$ 49,193	$ 79,419	$ 121,444
Earnings (conditional on positive earnings) in 1992						
Always in	$ 38,405	$ 6,412	$ 15,389	$ 30,778	$ 50,014	$ 73,097
Ever out but in for 2002	$ 44,655	$ 8,977	$ 17,954	$ 30,008	$ 49,501	$ 74,379
Died prior to 2002	$ 32,201	$ 5,130	$ 12,824	$ 25,648	$ 44,884	$ 64,120
Attritor	$ 40,823	$ 7,694	$ 16,415	$ 30,778	$ 48,731	$ 70,532
Total	$ 38,747	$ 6,412	$ 15,389	$ 30,422	$ 48,731	$ 71,815

Notes: All figures in 2002 $USD and unweighted. See the appendix for the definition of each variable. HRS 1992 weights used.

For those who eventually die, the full extent of the SES-health gradient can be seen. Median wealth in 1992 is half that of those always in ($77,329 versus $143,484). Again, respondents ever out but present in 2002 are far more different from those always in than from attritors. For example, median wealth is $94,898 for those ever out compared to $143,484 for those always in and $141,064 for attritors. Differences in wealth for ever out are partly (but not all) explained by lower home ownership (73% versus 83.9% for those always in, own a house). In relative terms, differences are larger at the bottom of the distribution than at the top (90th percentile), suggesting that the selection effect originates from the lower end of the distributions. Finally, differences in earnings (conditional on positive earnings) are far smaller than those in household income or wealth.

Since none of these comparisons takes account of the correlation among characteristics, we estimate a multinomial logit explaining the type of response behavior from baseline characteristics of respondents interviewed in 1992. Define indicators s_{ij} $(j = a, e, d, o)$ denoting whether respondent *i* has been *a*lways in, *e*ver out, *d*ied or was *o*ut in 2002 (i.e. attritors) . The probability of each outcome is modeled as

$$P(s_{ij} = 1 \mid x_{i0}) = \frac{\exp(x_{i0}\beta_j)}{\sum_{j'} \exp(x_{i0}\beta_{j'})} \qquad (6)$$

The vector x_{i0} stands for the vector of baseline characteristics and the βs are parameters to be estimated. Tables 3.3 and 3.4 present parameter estimates of these multinomial logits (with *always in* as the reference category, i.e., β_a=0) for females and males separately. We include basic demographics, health indicators, and quintile indicators for the distributions of wealth, household income, and earnings. By doing the latter, we allow for non-linearities in the effect of these variables on attrition and mortality. Table A.2 in the appendix gives more details on the construction of the explanatory variables.

Table 3.3 Determinants of Panel Status in 2002 - Females

Reference: always in	Parameter Estimates - Status 2002		
Covariates	Ever out	Died	Attritor
age 50-55 spline	-0.022	0.066	-0.043
age 56-60 spline	-0.074**	0.070**	-0.005
born outside U.S.	0.117	-0.539**	0.394**
African American	0.273	0.102	0.047
Hispanic	0.839**	0.087	0.417**
widow(er)	-0.356*	0.297*	-0.132
Divorced	-0.284	0.028	-0.167
once divorced	0.182	0.136	0.000
Single	-0.791**	0.001	-0.257
Household size	0.026	0.005	-0.075**
high school	-0.028	-0.098	-0.212*
some college	-0.016	-0.076	-0.421*
college and above	-0.318	-0.129	-0.455**
own house	-0.299*	-0.235	-0.160
Retired	-0.290	0.201	-0.193
Disabled	-0.196	0.323	-0.595**
not labor force	-0.174	-0.021	-0.168
1st wealth quintile	0.275	-0.281	-0.255
2nd wealth quintile	-0.007	-0.370**	-0.096
4th wealth quintile	-0.140	-0.117	0.000
5th wealth quintile	0.140	-0.564**	0.136
1st earnings quintile	0.054	0.329*	0.155
2nd earnings quintile	-0.293*	0.163	-0.117
4th earnings quintile	0.177	0.010	0.153
5th earnings quintile	0.368	-0.159	0.092
1st hld income quin.	0.314	0.121	-0.075
2nd hld income quin.	0.090	0.125	0.050
4th hld income quin.	0.031	-0.362*	0.018
5th hld income quin.	0.067	-0.161	-0.262*
health reported good	0.259**	0.274*	0.125
health fair/poor	0.091	0.719**	0.102*
ever had severe cond.	0.141	0.840**	0.068
ever had mild cond.	0.079	0.389**	0.192**
at least one ADL	-0.148	0.431**	-0.382**
Constant	-0.913	-6.042**	1.545
Observations	5165	Chi-Sq. Died eq.	
LogLike	-4719.66	SES (df=12)	23.610**
Pseudo-R2	0.071	Chi Tests: Attrition Eq.	

SES (df=12)	13.61
Region (df=8)	41.40**

Notes: Multinomial logit point estimates. ** p-value<0.01, * p-value<0.05. The dependent variable is the type of participation. Covariates refer to baseline characteristics of respondents in 1992. The reference category is *always in*; *ever out* refers to respondents with core interviews in 1992 and 2002 but not in at least one wave between 1992 and 2002. Census division dummies are included in the estimation, but these estimates are not reported.

The effect of race and schooling on the probability a respondent is ever out or is not present in 2002 is significant for both males and females. Hispanics, in particular, appear to have higher non-response probabilities. As for African Americans, only males appear to have a higher propensity to quit the panel. Respondents who were not born in the United States are also more likely to quit the panel. One plausible conjecture is that this results from return migration. We have also included census division indicators to control for unexplained differences across regions of the country (coefficients not reported). These are jointly significant for both males and females in the attrition equation (females Chi-sq (8) = 41.4, males Chi-sq (8) = 16.4). These differences can either be explained by regional factors affecting response probabilities, such as the fact that residency in some states is seasonal (e.g., "snow-birds" in Florida), or by survey administration differences across regions. This last possibility is mitigated by the fact that the survey is centrally coordinated in Michigan. Census divisions such as South-Atlantic, which includes Florida, have higher fractions of attritors and ever out respondents than northern and western regions. This is in line with the "snow-bird hypothesis."

Respondents in the lowest wealth quintile appear to have a greater probability of leaving the panel and coming back. However, likelihood ratio tests of the null hypothesis of no effect of income, wealth, and earnings in the attritor equation cannot reject this joint hypothesis at any conventional significance level. The lack of a link between attrition and baseline wealth, income, and earnings is in line with results from the PSID reported in Fitzgerald et al. (1998).[8] Unconditional differences in Table 3.2 appear to be largely the result of other differences, most notably race and ethnicity. However, the link with income and wealth is much stronger for mortality, even conditional on a rich set of controls for baseline health. Joint tests looking at the null that there are no SES effects on mortality reject this hypothesis for both males and females.

Table 3.4 2002 Panel Status Explained from Baseline Characteristics - Males

reference: always in	Parameter Estimates – 2002 Panel Status		
covariates	Ever out	died	Attritor
age 50-55 spline	0.005	0.057	-0.014
age 56-60 spline	-0.053*	0.066**	-0.012
born outside U.S.	0.078	-0.188	0.485**
African American	0.693**	0.366**	0.238*
hispanic	0.631**	-0.100	0.218
widow(er)	0.130	0.171	-0.653

[8] Fitzgerald et al. (1998) report differences in terms of labor income, which are usually only statistically different from zero at the 10% level. They use a quadratic function of labor income, which may be more sensitive to outliers than our quintile indicators.

divorced	0.539*	0.322*	0.040
once divorced	0.122	0.201*	0.067
single	0.194	0.150	0.412*
household size	0.065	-0.019	-0.050
high school	0.023	0.015	-0.108
some college	-0.068	0.153	-0.013
college and above	-0.171	-0.191	-0.384**
own house	-0.195	-0.020	-0.103
retired	-0.629**	0.260*	0.266*
disabled	-0.128	0.344	0.123
not labor force	-0.066	0.337	0.081
1st wealth quintile	0.362*	0.592**	-0.006
2nd wealth quintile	0.171	0.044	-0.004
4th wealth quintile	-0.012	0.110	0.118
5th wealth quintile	0.194	-0.004	0.170
1st earnings quintile	0.021	0.296*	0.012
2nd earnings quintile	-0.200	0.008	-0.104
4th earnings quintile	-0.186	0.061	0.215
5th earnings quintile	-0.203	0.183	0.186
1st hld income quin.	-0.120	0.011	0.100
2nd hld income quin.	-0.147	0.230	0.150
4th hld income quin.	0.013	-0.160	0.105
5th hld income quin.	-0.095	-0.349	-0.023
health reported good	-0.071	0.353**	0.104
health fair/poor	-0.053	0.692**	-0.022
ever had severe cond.	-0.230	0.908**	0.053
ever had mild cond.	0.079	0.517**	-0.031
at least one ADL	0.506*	0.468**	-0.211
constant	-2.308	-6.042**	-0.569
Observations	4596	Chi-Sq. Dead eq.	
LogLike	-4590.71	SES (df=12)	27.810**
Pseudo-R2	0.083	Chi Sq.: Attritor Eq.	
		SES (df=12)	7.06
		LR test for Region effects (df=8)	16.35**

Notes: Multinomial logit estimates ; ** p-value<0.01, * p-value<0.05. The dependent variable is the type of response behavior. Covariates refer to characteristics of respondents in 1992. The reference category is *always in*; *ever out* refers to respondents providing core interviews in 1992 and 2002 but not in at least one wave between 1992 and 2002. Census division dummies are included, but these estimates are not reported.

3.3 Wave-by-Wave Determinants of Attrition

Attrition can also be driven by events that occur after the 1992 interview. Baseline controls are imperfect proxies of the likelihood of such events. (An unhealthy person at baseline is more likely to suffer a major health event that may cause non-response.) One advantage of moving toward a longitudinal model to explain attrition is that this may better capture events between waves that actually trigger attrition.

We therefore perform a multinomial logit regression of current wave status (provided interview, reported dead, or attrited) conditional on providing a core interview in the previous wave and on previous wave, as well as baseline (1992), characteristics.

The multinomial logit thus becomes a discrete time competing risk model. The two risks are attrition and mortality.

Given the features of participation transitions observed in Figure 2.1, it would in principle be possible to model reentry into the survey. However, this would be complicated by the fact that previous wave characteristics are not observed when a respondent reenters. Instead, we consider only the pairs of observations at wave *t*–1 and wave *t* where the respondent responds in wave *t-1*. Thus, a respondent who does not answer in 1994 but answers in every other wave will be included in the estimation for the years *t*=1994, *t*=1998, *t*=2000 and *t*=2002, but not for *t*=1996.

Let the indicators $s_{it,j}$ $(j = p, d, n)$ denote respondents who *p*articipate in the survey in wave *t*, died between wave *t-1* and *t*, or did *n*ot respond in year *t,* respectively. The probability of each state is assumed to be given by the multinomial logit expression

$$P(s_{it,j} = 1 \mid x_{it-1,} x_{i0}, s_{it-1,p} = 1) = \frac{\exp(x_{i0}\beta_j + x_{it-1}\gamma_j + \lambda_{tj})}{\sum_{j'} \exp(x_{i0}\beta_{j'} + x_{it-1}\gamma_{j'} + \lambda_{tj'})} \qquad (7)$$

We correct standard errors for the clustering introduced by considering multiple occurrences of the same respondent in the likelihood. Also, year effects λ_{tj} are introduced to capture duration dependence or calendar time effects. Baseline characteristics are included in addition to previous wave characteristics to control for time-invariant heterogeneity. The included previous wave characteristics are indicators for whether the individual was divorced or widowed, various health indicators, an indicator for whether the respondent was working for pay, and wealth, earnings and household income. We consider participation in the survey as the reference outcome. Respondents who do not respond in a wave are denoted as having "attrited," although they might come back to the survey in later waves.

Table 3.5 Determinants of Current Wave Status

Ref: participation	**Parameter Estimates (current wave status)**			
	Females		**Males**	
covariates	**died**	**attrited**	**died**	**attrited**
age 50-55 spline	-0.006	0.009	0.005	0.028
age 55-60 spline	0.048	-0.042**	0.046	-0.019
age 60-65 spline	0.065*	-0.013	0.065**	-0.026
age 65+ spline	0.136*	0.043	0.002	0.033
Baseline Characteristics				
born outside U.S.	-0.828**	0.310**	-0.219	0.360**
African American	0.026	0.095	0.253**	0.344**
hispanic	0.095	0.424**	-0.099	0.367**
widow(er)	-0.004	-0.090	-0.315	-0.377
divorced	-0.259	-0.202	-0.250	-0.206
once divorced	0.059	0.015	0.135	0.059
single	0.092	-0.469**	0.013	0.353**
household size	0.023	-0.020	-0.015	0.019
high school	0.017	-0.095	0.034	0.007
some college	0.200	-0.238**	0.209	0.017

college and above	0.322	-0.367**	0.060	-0.255**
own house	-0.164	-0.093	-0.057	-0.123
health reported good	-0.237	0.156*	0.069	-0.002
health reported fair/poor	-0.370**	0.070	0.042	-0.044
ever had severe cond.	0.642**	0.002	0.658**	-0.116
ever had mild cond.	0.182	0.084	0.352**	-0.035
at least one ADL	0.141	-0.013	0.151	0.064
retired	-0.003	-0.171	0.049	-0.014
disabled	0.100	-0.236	0.074	0.093
out of labor force	-0.144	-0.132	0.149	-0.004
1^{st} wealth quintile	-0.405**	0.067	0.357**	0.081
2nd wealth quintile	-0.455**	-0.003	0.010	0.033
4^{th} wealth quintile	-0.068	-0.035	0.257*	0.040
5^{th} wealth quintile	-0.216	0.108	0.273	0.104
1^{st} hld income quin.	0.204	0.025	0.011	0.041
2nd hld income quin.	0.118	0.009	0.144	0.002
4^{th} hld income quin.	-0.235	0.005	-0.184	0.019
5^{th} hld income quin.	-0.003	-0.177	-0.207	-0.078
1^{st} earnings quintile	0.121	0.138	0.094	-0.013
2nd earnings quintile	0.116	-0.161	-0.070	-0.078
4^{th} earnings quintile	-0.112	0.188*	0.064	-0.002
5^{th} earnings quintile	-0.499	0.158	0.120	-0.008
Previous Wave Characteristics				
widow (t-1)	0.286	-0.050	0.481*	0.090
divorced (t-1)	0.309	0.064	0.431**	0.528**
health good (t-1)	0.503**	0.070	0.358**	0.062
health fair/poor (t-1)	1.495**	0.073	1.097**	-0.021
Onset severe hlt cond. (t-1)	0.833**	0.139	1.110**	0.250**
Onset mild hlt cond. (t-1)	0.142	0.140	0.164	0.196**
ADL>0 (t-1)	0.442**	-0.436**	0.254**	0.047
work for pay (t-1)	-0.676**	0.042	-0.463**	0.045
wealth (t-1) /100K	-0.059**	0.003	-0.016	0.000
hld. Income (t-1) /100K	-0.172	0.010	0.000	0.032
Earnings (t-1) /100K	0.575	0.121	-0.160	-0.036
constant	-4.758	-2.950	-5.881**	-4.073**
Observations/LogLike	22668	-6834.26	19452	-6952.14
Pseudo-R2	0.065		0.075	
Chi sq. Time-varying characteristics				
Died Eq. (11)	204.38**		251.44**	
Unknown Eq. (11)		23.46**		23.67**

Notes: Multinomial logit point estimates, ** p-value<0.01, * p-value<0.05. The dependent variable is the state in a given wave (provided core interview, died or unknown status). Estimation is done conditional on providing a core interview at the last wave. Both baseline and time-changing characteristics are included in the estimation. A respondent can appear more than once in the estimation. Standard errors are corrected for clustering at the respondent level. Census division and year dummies are included in the estimation but the estimates are not reported.

Estimation results in Table 3.5 show that previous wave information is informative about current wave status for both mortality and attrition. As expected, mortality is closely related to previous wave health events. Interestingly, for males, it is also related to previous wave wealth, confirming differential mortality across SES groups as found by, for example, Attanasio and Hoynes, 2000.

As for attrition, the onset of “mild” health problems (such as diabetes, hypertension, or mental health problems) and severe problems (heart and lung disease, stroke) are significantly positively associated with attrition for males. For females, the effects are in the same direction, but are not statistically significant, possibly because of the lower number of observations for females. For females, the onset of a limitation in performing daily activities is found to have a negative effect on attrition. If attrition is positively related to mobility, limitations with activities of daily living (ADLs) may indicate restricted mobility, increasing the chances that interviewers can contact the respondent.

Divorce status in the previous wave is positively associated with attrition for males but not for females. A similar result was found in the PSID (Lillard and Panis, 1998). We found earlier that divorced males are more often in the category “ever out” but not in “attritors,” suggesting the effect of being divorced on participation in the survey may be temporary.

We can summarize our main findings so far as follows:

1. Those who are “attritors” as of 2002 have different baseline characteristics in terms of race and ethnicity, immigration status, education, health, and high household income for females.
2. Those “ever out” are respondents who, in terms of baseline characteristics, are the most different from those who remain always in the survey or those that are not present as of 2002.
3. Those who are dead by 2002 had systematically lower SES than those who survived, confirming evidence of differential mortality.

In terms of the first finding, since other characteristics than race and ethnicity, gender, household composition and age are associated with attrition as of 2002, it is likely that the MAR assumption implicit in using HRS weights is violated. The potential effects of selection on observables on inference from the 2002 cross-section in HRS will depend on the strength of the relationships of these characteristics with attrition and how they correlate with outcomes of interest. We will use the estimates above to construct IPW.

The second finding is important for at least two reasons. First, it implies that using the balanced sample with only those “always in” is likely to suffer from even stronger selection on observables than when using the unbalanced sample. Second, it reveals how important it might be for a longitudinal survey to re-contact non-respondents in latter waves. A sizeable fraction of non-respondents does return and our analysis suggests that these may have the type of characteristics that would otherwise make the attritors much more susceptible to introducing serious selection on observables. Hence, it would appear to be a desirable strategy for the overall representativity of a study to design mechanisms that increase re-contact rates.[9]

Finally, our third result shows that it is important to acknowledge the possibility that differential mortality distorts our view of the evolution of certain outcomes, such as

[9]Rodgers (2006) shows that the increased incentives experiment in the 2000 wave of HRS had the strongest participation effects among those who did not respond in previous waves. The response rate for those who received $20 was 37.4%, while it was 45.2% for those who were offered $50.

wealth or income over the life-cycle. However, investigating effects of differential mortality on inferences from the HRS is beyond the scope of this analysis.

Before attempting to gauge the effects of selection on observables on tabulations of outcome of interest in 2002 using inverse probability weighting, the next section considers other non-response problems that may bias the representativity of the HRS cohort sample, particularly when studying pensions and Social Security entitlements and receipt of benefits. A significant fraction of respondents did not give their consent to use records from the SSA. The same holds true for pensions. HRS tries to obtain summary pension plan descriptions from employers of respondents with a pension on the current job. About one third of workers with a pension on the current job do not have such a description, which is partly the result of missing information on employers provided by respondents and non-consent by employers to provide such plans.

4. Linked Pension Summary Plan Description and Social Security Administration Records

In 1992, HRS asked consent from respondents to request their earnings record and benefit entitlement from the SSA. HRS also asked to get summary plan descriptions (SPD) from employers of respondents who said they had a pension and reported the name and address of their employer. These data are often used to study the effect of pension incentives on retirement and other outcomes or to study the accuracy of self-reported pension and social security wealth.

Table 4.1 reveals that nearly three quarters of respondents in 1992 gave consent for HRS to obtain their Social Security records from SSA.[10] Females are slightly more likely to give consent than men (76.3% versus 73.7%). SPD matches can be from the current job or from past jobs. We focus our analysis on current job pension matches. In 1992, 56.3% (52.5%) of male (female) workers declared to HRS that they had a pension on their current job. HRS was able to collect SPDs for two thirds of these. This is slightly lower than for the Social Security match. Permission to link to Social Security data and obtaining an SPD match (for workers who self-report they have a pension on the job) are significantly correlated for males (Chi-square = 4.83, p-value=0.028) but not for females. For example, among males, 68.3% of workers who gave for permission the SSA match also have a SPD. For those who did not give consent for the SSA match, 62.6% have a SPD.

Table 4.1 SSA Record and SPD Match

	Male	Female	Total
# with core Interview 1992	4,596	5,165	9,761
Match with SS earnings history	3,386	3,945	7,331
	73.7%	76.3%	100%
Reported Pension on	2,015	1,621	3,636
current Job 1992 (% of workers)	56.3%	52.5%	54.5%
Match with Employer SPD	1,350	1,110	2,460
Provided Pension	67.0%	68.5%	67.7%
Plan Information			
Chi-square Independence Test	4.83	0.485	
SSA and Pension SPD	p=0.028	p=0.487	

[10] Although there are a few consents for whom the record could not be retrieved (e.g., a problem with matching the self-reported SSN with a record), we use consent throughout to define the outcome of the match. We focus on the match with the earnings record file. SSA & HRS released three files for researchers to use. The first one is the earnings record file, which collects earnings of respondents from 1951 to 1991, along with quarters of coverage for eligibility Social Security benefits. The second was constructed by researchers (O. Mitchell, J. Olson and T. Steinmeier), and the third refers to wages and self-employment income in non-covered jobs. We define our match indicator from the match with the first file, the earnings record, since it is the most widely used source and, in fact, is the source of calculations for the second file.

Notes: RAND HRS files with earnings histories of respondents who gave their consent. For SPDs on pensions, the variable created in the tracker file, version 2002, was used, and only respondents who had a match in 1992 were selected. Self-report on pension policy holding on current job is used as the denominator in calculating the match rate. The chi-square independence test is done for the subset of respondents who report having a pension at their current job.

The outcome of the match may not be random. For example, it could proxy general willingness to participate in surveys and/or an aversion to have private information disclosed to a third party.[11] One way to check this is to perform a logit of the match outcome (1=match, 0=no match) on baseline characteristics of respondents. Table 4.2 present logit results for the Social Security match.

Table 4.2 Baseline Determinants of SSA Record Match

Baseline 1992 Characteristics	Males	Females
age 50-55 spline	-0.021	0.007
age 56-60 spline	0.029	0.000
born outside U.S.	0.010	-0.047
African American	-0.295**	-0.452***
Hispanic	-0.305**	-0.377**
widow(er)	-0.235	0.101
Divorced	-0.046	0.018
once divorced	0.112	0.098
Single	-0.163	0.314
household size	0.000	0.033
high school	0.025	-0.034
some college	-0.102	-0.282**
college and above	-0.123	-0.107
own house	0.018	0.103
Retired	0.316**	0.257**
Disabled	-0.004	0.533**
not in labor force	0.365**	0.086
1st wealth quintile	0.405**	0.243*
2nd wealth quintile	0.349**	0.374**
4th wealth quintile	0.095	-0.024
5th wealth quintile	-0.221**	-0.326**
1st hld income quin.	-0.206	-0.311*
2nd hld income quin.	0.091	-0.049
4th hld income quin.	0.079	0.283*
5th hld income quin.	0.168	0.457**
1st earnings quintile	-0.227*	-0.143
2nd earnings quintile	-0.010	0.082
4th earnings quintile	0.066	-0.138
5th earnings quintile	0.145	-0.309**
Health reported good	-0.148*	-0.059
Health fair/poor	-0.153	-0.255*
ever had severe condition	0.188**	0.165*
ever had mild condition	0.051	0.082
at least one ADL	-0.125	-0.033
Constant	1.927	0.754

[11] Note that in the case of SPDs, the consent decision is not only that of the respondent (giving the (correct) address of his employer) but also of the employer (to provide SPDs).

observed probability	0.737	0.768
Observations	4596	5165
Pseudo R squared	0.025	0.024

Notes: Logit estimates of the probability that a matched SSA record is obtained. Point estimates reported, ** p-value <0.01, * p-value<0.05. Variable definitions in appendix.

Both African-Americans and Hispanics are less likely to give consent to HRS to obtain their earnings record at SSA. Interestingly, retired individuals are more likely to give consent than workers. The probability of a match decreases with wealth and increases with household income. The probability of a match is nearly 20% lower at the top of the wealth distribution than at the bottom (holding other characteristics constant). Along with the earnings record, HRS provides adjusted baseline weights for those respondents with a match. However, these only take into account selection based on the same characteristics as the HRS weights. Hence, from the results presented, they appear insufficient to correct for selection on observables.

We perform the same exercise for the SPD match, looking at the probability of a match in the sample of 1992 respondents who report having a pension on their current job. Results in Table 4.3 reveal that there are important effects of education for both males and females. Contrary to the SSA match, racial differences are small. African-Americans born in the United States who report a pension on their job actually have higher match rates than white and Hispanic respondents. The difference in terms of education can be a proxy for firm size, since better-educated respondents tend to work in larger firms, which tend to be better organized to provide SPDs. Similar to the SSA match, the pseudo R-square is rather low (less than 0.05).

Table 4.3 Baseline Correlates of SPD match

Baseline 1992 Characteristics	**Males**	**Females**
Age 50-55 spline	0.020	-0.004
Age 56-60 spline	0.004	0.029
born outside U.S.	-0.304	-0.392*
African American	0.191	0.362**
Hispanic	-0.173	0.020
widow(er)	0.362	-0.186
Divorced	0.440**	-0.120
once divorced	-0.039	-0.234
Single	0.710**	-0.461
household size	0.040	-0.013
high school	0.289**	0.412**
some college	0.380**	0.368*
college and above	0.622**	1.080**
own house	0.210	-0.124
Retired	0.175	-0.067
1st wealth quintile	-0.349	0.215
2nd wealth quintile	-0.076	0.035
4th wealth quintile	-0.208	-0.103
5th wealth quintile	-0.547**	0.278
1st hld income quin.	0.080	-0.383
2nd hld income quin.	-0.149	-0.311*

4th hld income quin.	-0.037	-0.172
5th hld income quin.	0.016	-0.393**
1st earnings quintile	-1.903**	-0.258
2nd earnings quintile	0.080	0.301
4th earnings quintile	-0.044	0.099
5th earnings quintile	0.123	0.388*
health reported good	-0.005	-0.120
health fair/poor	-0.318*	-0.284
ever had severe cond.	0.396**	-0.008
ever had mild cond.	-0.019	0.096
at least one ADL	0.384	-0.680
Constant	-0.916	0.861
observed probability	0.670	0.685
Observations	2015	1621
Pseudo R squared	0.044	0.049

Notes: Logit estimates of the probability that a matched employer provided pension record is obtained. The sample consists of all respondents reporting having a pension on their current job. Point estimates reported; ** p-value <0.01, * p-value<0.05. Variable definitions in appendix.

One might think that the match indicators proxy the general willingness to participate in surveys or the level of tolerance the respondent has to divulging private information (from the employer or SSA) to a third-party organization. One way to check this possibility is to see if there is a correlation between the decision to give consent to SSA (or pension) match and participation in later waves of the survey.

The top panel of Table 4.4 gives the distribution of participation sequences over the period 1992–2002 conditional on the outcome of an SPD match and in the second panel conditional on the outcome of an SSA match. Conditional on having a pension at baseline, there is no difference between participation in the survey and the SPD match outcome. This is in sharp contrast to the SSA consent decision, where large differences in participation patterns are observed. Among those who refuse to have their record matched, 25.3% will eventually leave the panel and not come back by 2002 compared to 11.2% among those who gave consent in 1992. A Chi-square independence test clearly rejects the null of independence of the two outcomes. One possible reason for the low association of the SPD match with panel participation is that the crucial factor for the match is the employer rather than the employee.

To control for other characteristics associated with both decisions before assessing whether the SSA match decision correlates with participation sequences, we re-ran the multinomial logits in Tables 3.3 and 3.4, explaining panel participation behavior from 1992 and 2002, conditional on baseline controls and the match variables. The results (in panel 3 of Table 4.4) show a strong negative association between the probability of observing someone leave the panel and the decision to give consent to merge their SSA record. The marginal effect is of similar magnitude as the raw difference in attrition rates among the two groups (-15.9 and -14.2 percentage points for males and females, respectively). This means that little of the relationship between the match and future participation in the survey is explained by background characteristics.

The simplest explanation for the association is that the match outcome proxies for unobservables related to the willingness both to participate in a survey and to have sensitive information transferred from SSA to a third party. Respondents who have a low willingness to participate in surveys may be less likely to give consent for an SSA match.[12] This will be less clear for the SPD match, since both respondents and employers can decide not to provide the relevant information.

Table 4.4 Relationship Between Type of Interview Sequence and Match with SSA Record and SPD)

	Sequence Type 1992-2002				
SPD match	**Always in**	**Ever out**	**Died**	**Attritor**	**Total**
conditional on having a pension					
no match (%)	68.28	9.78	6.21	15.73	100
match (%)	68.74	8.86	7.24	15.16	100
Chi-square	2.14p = 0.54				
SSA match	**Always in**	**Ever out**	**Died**	**Attritor**	**Total**
no match (%)	48.19	13.54	13	25.27	100
match (%)	69.59	7.33	11.84	11.24	100
Chi-square	455.4p<0.001				

Marginal "Effect" On Probability of of attrition in 2002 (controls as in Table 3.3)

		Males	Females	
SPD match		0.011	-0.002	(conditional on having pension)
	p-value	0.553	0.901	
SSA match		-0.159	-0.142	
	p-value	<0.001	<0.001	

Notes: The first two panels report the distribution of sequence types by whether a match could be obtained from their SPD or from the SSA for earnings record. The last panel repeats multinomial logits in Table 3.3 and 3.4, adding match status variables as controls. This exercise is performed for the pension match variable, conditional on having a pension in 1992, and for the SSA match variable on the whole sample of 1992 respondents. These marginal effect are on the probability of attrition (as of 2002).

The results for the non-randomness of the SSA match are largely in line with those reported in Olson (1999) and Haider and Solon (2000). However, they indicate selection on observable effects that are hard to quantify. Hence, it is important to look at how retirement outcomes in the subsamples with a match differ from those without a match.

We look at how these selection on observable effects may explain differences in expectations about retirement. These individual expectations are well-known to be strongly correlated with future retirement outcomes and hence provide a good check on how retirement patterns between the two groups will differ.

The HRS has expectation questions about when respondents, currently working, expect to retire and when they expect to claim Social Security benefits. In addition, the HRS collects data on the probability that the respondent will work past ages 62 and 65. Table 4.5 shows that the distributions of these expectations for the groups with and

[12] Another interpretation is that asking for SSN discourages future participation in the survey. It is not possible to tell the two explanations apart.

without an SSA match or SPD match. For questions asking about expected age, there is a considerable amount of missing data. Non-response to these questions is correlated with each match variable, which may affect the comparisons we are about to make. However, there is much less non-response for the subjective probabilities to work past ages 62 and 65. Conditional on providing a non-missing response, the distribution of expected ages of retirement is quite similar among those who do not give consent for HRS to link their information with an earnings record and those who do give consent. Things are less clear-cut for the expected age to claim Social Security benefits. In 1992, those that give consent expect to claim in a larger fraction at age 65 than at age 62. They report to be as likely to work past 62 compared to those not giving consent but less likely to work past age 65.

For the SPD match, differences are much larger. Those with a SPD match tend to expect to retire earlier than those who do not have a match. They expect to claim Social Security benefits sooner than those without a match as well. Finally, they report that they are much more unlikely to work past 62 and even less likely to work after age 65 (43.8% versus 55.2%).

Table 4.5 Retirement Expectations in 1992, SSA and SPD match

Age expect to retire				
	SSA match		SPD match	
%	no match	match	no match	Match
Missing	*41.06*	*27.19*	*38.28*	*19.95*
50-59	10.8	10.25	6.9	14.1
60-61	11.73	9.9	8.23	12.48
62	22.22	23.9	23.82	23.26
63-64	15.84	14.06	15.17	13.65
65	21.09	22.31	23.86	20.12
66/67	10.6	12.31	12.79	11.05
67/70	4.53	4.14	5.18	3.19
71+	3.19	3.13	4.05	2.16
Age expect to claim SS benefits				
	SSA match		SPD Match	
%	no match	match	no match	Match
missing	*51.36*	*38*	*48.59*	*31.16*
min-61	1.75	2.38	2.81	1.67
62	56.98	51.96	50.38	55.72
63-64	2.12	2.83	2.66	2.72
65	34.79	39.21	39.94	36.61
66-67	1.37	1.19	1.2	1.25
68-70	2.87	2.22	2.81	1.88
71-max	0.12	0.19	0.2	0.16
Probability that work past				
	SSA match		SPD Match	
	no match	match	no match	Match
age 62				
missing	*1.52*	*0.74*	*1.13*	*0.65*
positive	72.66	71.69	74.17	68.81
age 65				
missing	*1.82*	*1*	*1.34*	*1.01*

positive	53.68	49.42	55.24	43.83

Notes: 1992 distribution of expectations. A respondent must be working to answer these questions, not have claimed Social Security benefits. The employer pension merge tabulations are done on the sample of workers who report having a pension on the current job in 1992. The % of missings for each variable is reported. The other fractions are reported conditional on providing a non-missing value

To see if these differences result from selection on observables, we can use match probabilities predicted from the logits in Table 4.2 and 4.3 to construct a set of weights based on the same idea as the inverse probability weighting scheme developed in Section 3. If selection on observables is responsible for differences, particularly for the SPD match where large differences occur, weighting should bring the two distributions (across those with a match and those without) much closer. Table 4.6 presents the same distributions as in Table 4.5, but weighted using predicted match probabilities.

Table 4.6 Retirement Expectations in 1992, SSA and SPD Match, Weighted Using the Inverse of Predicted Match Probabilities

Age expect to retire				
	SSA match		SPD match	
%	no match	match	no match	Match
missing	*41.2*	*27.27*	*23.26*	*16.61*
50-59	11.02	10.33	8.53	14.37
60-61	11.73	9.92	7.99	12.38
62	22.17	23.98	26.84	23.5
63-64	15.59	14.02	17.63	13.74
65	21.25	22.15	21.2	19.94
66/67	10.45	12.29	11.86	11.66
67/70	4.56	4.17	3.24	2.79
71+	3.23	3.15	2.7	1.64
Age expect to claim SS benefits				
	SSA match		SPD Match	
%	no match	match	no match	Match
missing	*51.53*	*38.16*	*35.41*	*28.36*
min-61	1.8	2.44	2.29	2.03
62	56.84	52.18	54.65	55.46
63-64	2.15	2.78	3.29	2.81
65	34.77	39.02	36.14	36.2
66-67	1.46	1.18	1.18	1.46
68-70	2.86	2.2	2.26	1.85
71-max	0.11	0.2	0.2	0.18
Probability that work past				
	SSA match		SPD Match	
	no match	match	no match	Match
age 62				
missing	*1.56*	*0.78*	*0.46*	*0.55*
positive	72.6	71.79	71.23	69.74
age 65				
missing	*1.86*	*1.05*	*0.53*	*0.96*
positive	53.82	49.51	44.75	43.27

Notes: 1992 distribution of expectations. A respondent must be working to answer these questions, not have claimed Social Security benefits. The employer pension merge tabulations are done on the sample of workers who report having a pension on the current job in 1992. The % of missings for each variable is reported. The other fractions are reported conditional on providing a non-missing value. logit weights used.

Differences among those who have an SSA match and those who don't do not disappear after weighting for known determinants of match probabilities. Things are quite different, however, for the SPD match. Both for expected age for claiming Social Security benefits and for the probability to work past age 62 and 65, the weighing realigns both distributions. This suggests that selection on observables was behind these observed differences for the SPD match. High education and high household income for females were positively associated with an SPD match. However, differences in terms of expected retirement age do not vanish. Differences appear particularly important at the bottom of the age distribution (ages 50–61).

These findings seem to suggest that there is cause for concern when using the sample of respondents with a match. Many characteristics of respondents are associated with both SSA and SPD match. We find important selection on observable effects on the distribution of retirement expectations for the SPD match, which implies that the sample of respondents with a match is non-random. Weights help make the groups (with and without a match) more similar, but some differences remain in the distribution of expected retirement age. In the case of the SSA match, differences in expectations do not appear to be as large as for the SPD match.

Ideally, to investigate the selective nature of the consent decision to a SSA match, we would compare benefits received by those who gave consent and those who did not. The problem is that very few respondents receive benefits in 1992, since they are age 51–61 and the earliest age at which a person can claim an old-age Social Security benefit (on his/her own account) is age 62. However, looking at later waves implies that we have to exclude attritors, who, as we have seen, in higher proportion than others, did not consent to have their SSA record matched. For example, out of those giving an interview in 2002, only 20% did not have a match compared to 25% in 1992. This reflects the association found in Table 4.4.

Another avenue would be to get a random sample of earnings records from SSA in 1992 and compare it to the sample with a match in the HRS. Unfortunately, these data are not publicly available. What is available is a 1% random sample of benefit records in 2001. The 2001 SSA Public Use MicroData File (SSA-PUF) consists of a 1% sample from the Master Record of actual benefits paid to individuals in December 2001. Hence, we complement our analysis of the representativity of the sample with a match in 2002 by not only comparing the distribution of benefits received between those with a match and those without but by also comparing it to the distribution in the SSA-PUF database for recipients from the same age cohort. We convert the monthly benefit to an annual benefit (multiplying by 12) and exclude from both HRS and SSA-PUF all respondents who have started claiming benefits after 2000. We do this to deal with respondents who received benefits for part of the year.[13] Because we are comparing to a external random sample that does not over-sample certain groups, we weight HRS statistics using 2002 HRS weights to deal with the African American, Hispanic, and Floridian supplements and with attrition from race and ethnicity, age, and gender. Hence, we essentially impose the MAR assumption conditional on race, ethnicity, and gender and postpone using other weighting schemes to the next section.

Table 4.7 presents various deciles of the distribution of annual benefits for all HRS respondents (conditional on having claimed before 2000), for those with or without a match with earnings record and finally for the sample selected from the SSA-PUF.

Table 4.7 Comparison of the Social Security Benefit Distributions in HRS and the SSA Public Use MicroData File

	Quantile of Annual Social Security Benefits Received						
Males	mean	p10	p25	p50	p75	P90	% receive
HRS, all	$ 11,380	$ 5,540	$ 8,631	$ 11,943	$ 13,591	$ 16,006	62.2%
HRS, without SSA match	$ 11,472	$ 5,540	$ 8,914	$ 11,450	$ 13,543	$ 16,572	63.1%
HRS, with SSA match	$ 11,350	$ 5,577	$ 8,618	$ 12,053	$ 13,666	$ 15,772	61.9%
SSA Public Use File	$ 11,580	$ 5,820	$ 9,180	$ 12,360	$ 13,920	$ 15,960	
Females							
HRS, all	$ 8,169	$ 4,186	$ 5,614	$ 7,387	$ 10,551	$ 12,743	65.4%
HRS, without SSA match	$ 8,099	$ 4,309	$ 5,540	$ 7,166	$ 10,465	$ 12,364	64.7%
HRS, with SSA match	$ 8,188	$ 4,186	$ 5,651	$ 7,412	$ 10,588	$ 12,817	65.5%
SSA Public Use File	$ 7,943	$ 4,200	$ 5,760	$ 7,200	$ 10,140	$ 12,660	

Notes: For the SSA Public Use MicroData File OASD 2001, we selected individuals born 1931–1941 and we only used in the calculations benefits received from own earnings, from the spouse earnings, or from a deceased spouse's earnings. 97441 records were used. The SSA Public Use files reports the monthly benefit payable for December 2001 and applies the annual Cost-of-Living Adjustment (COLA of 2.6%) payable January 1st to this amount. We multiply this benefit by 12 and apply the COLA to the HRS self-report for the year 2001. We select only respondents receiving benefits as of 2000 to avoid incomplete years of benefit receipt. We applied 2002 HRS weights for HRS figures.

Remarkably, there appear to be no systematic differences in the various distributions of annual Social Security benefits. The proportion of individuals receiving benefits does not vary with the consent decision. The mean differs by no more than $200 across the three distributions (HRS with match, no match and SSA-PUF), both for males and females. Differences across the distribution are also remarkably small and do not appear to be systematic. Hence, although differences in terms of expectations persist, even after weighting, differences in benefits actually received are very small and the distributions compare well with the actual distribution from the 1% sample from the 2001 Master Record from SSA. This last comparison, however, suffers from the caveat that attrition may have blurred some of the differences by 2002 because of the negative association between match probabilities and attrition probabilities.

[13] Since we extrapolate the monthly information in the SSA-PUF to a yearly benefit, we would otherwise overestimate benefits for respondents who have claimed benefits recently and reported these "incomplete" benefit streams to HRS.

5. Implications of Selection on Observables for 2002 Tabulations

Results from Section 3 showed that a significant number of baseline characteristics (as well as wave-by-wave characteristics) are associated with attrition. In this section, we use inverse probability weighting to measure the effects of selection on observables on unconditional tabulations that could be done on the 2002 cross-section of respondents. We measure this effect by comparing two weighting strategies that impose different missing at random assumptions. HRS weights impose that outcomes and attrition are random, conditional on race, ethnicity, household composition, gender, and age. In Section 3, we showed that we could relax that assumption by adding to the conditioning set a large set of baseline characteristics. This could be implemented by using inverse probability weighting.

To implement inverse probability weighting, we need to construct retention probabilities from the estimates in Section 3. We choose to construct those probabilities from the baseline determinants of attrition (Table 3.3 and 3.4). The population of interest in 2002 is that of individuals born between1931-1941 who are age 61 to age 71 in that year. For those present in the sample in 1992, this means that their retention probability is the probability to be "always in" or "ever out," conditional on being alive in 2002. Since we have individual predictions for each of these probabilities, we can calculate the retention probability as

$$p(s_i \mid x_{i0}) = \frac{p(s_{i,a} \mid x_{i0}) + p(s_{i,e} \mid x_{i0})}{1 - p(s_{i,d} \mid x_{i0})}. \tag{8}$$

where a refers to "always in", e to "ever out" and d to "died". Hence, given baseline characteristics of each respondent and estimated parameters, we compute the predicted retention probability for each respondent.

HRS weights in 2002 not only correct for selection on race, ethnicity, gender, household composition and age, but also for over-sampling and other baseline non-response differences in HRS compared to the CPS. Hence, to make the IPW weights comparable to the HRS weights, we need to adjust the IPW for over-sampling and other differences in baseline non-response. Hence, we construct the inverse probability weights as

$$w_i(x) = \frac{w_{1992}(q_i)}{p(s_i \mid x_{i0})} \tag{9}$$

where $w_{1992}(q_i)$ is the HRS weight for respondent with characteristics q_i (race, ethnicity, household composition, gender, and age). In the case where $p(s_i \mid x_{i0}) = p(s_i \mid q_i)$, and our interest is to estimate the mean of some outcome y in 2002 both weights will provide the same estimate, In fact,

$$E(s_i w(q_i) y_{i2002}) = E(s_i w_{2002}(q_i) y_{i2002}). \tag{10}$$

since $w_{2002}(q_i) = \frac{w_{1992}(q_i)}{p(s_i \mid q_i)} = w(q_i)$. Hence, the difference between estimates using the HRS weights (for 2002) and the IPW weights give us an indication of the bias introduced by the fact that characteristics other than those in the HRS weights enter retention probabilities.

Tables 5.1–5.4 show the effect of weighting on cross-sectional distribution of a large number of socioeconomic outcomes of respondents in 2002. We show four estimates. The first one is the unweighted statistic, while the second corrects for baseline non-response and over-sampling using HRS 1992 weights. In most instances, making that correction has a considerable effect. This correction tends to correct for the over-sampling of groups with low SES or worst health outcomes, such as African Americans and Hispanics. The third column shows the effect of adding to the baseline correction the inverse probability weighting correction in eq. (8). In general, the effect of IPW corrections are very small. When there is a change compared to the statistic in preceding column, it tends to revise SES statistics downward and health statistics upward, which is in line with the correlation between baseline characteristics and attrition. Finally, the most important finding is that weighting using IPW makes hardly any difference compared to using HRS weights from 2002. This tends to support that selection on observables is present but that, conditional on characteristics that enter HRS weights, it is random for virtually all outcomes we looked at.

Table 5.1 Labor Market & Private Pension Outcomes Using Different Weights

2002 characteristics of Original 1992 respondents	No WGT	Weighting (WGT) scheme WGT HRS-92	WGT IPW	WGT HRS-02
Male				
(%) work for pay	44.4	44.5	44.3	44.6
(%) with pension on job	36.4	37.7	37.7	37.6
(%) receive pension (not working)	49.0	50.1	50.2	49.6
(mean) Hours worked	36.7	36.7	36.7	36.6
(mean) earnings	$ 39,599	$ 42,160	$ 42,022	$ 41,504
Female				
(%) work for pay	31.8	32.4	32.1	32.5
(%) with pension on job	39.6	39.2	39.3	39.5
(%) receive pension (not working)	27.2	27.9	27.8	27.9
(mean) hours worked	31.4	31.5	31.5	31.6
(mean) earnings	$ 23,911	$ 24,302	$ 24,272	$ 24,384

Notes: The sample consists of initial 1992 respondents providing core interviews in 2002. Statistics are reported using different weights (WGT). HRS provided weights HRS-92 and HRS-02, matching HRS frequencies, in 1992 and 2002 respectively, on the March Supplement of the CPS) by cells defined by birth cohort, gender, race, and ethnicity. The weights derived for attrition (IPW) are constructed as shown in the text based on multinomial logit predicted probabilities in Tables 3.3 and 3.4. Variable definitions are found in Table A.2

Table 5.2 2002 SES (Education, Income, Wealth ,and Poverty) Using Different Weights

		Weighting (WGT) scheme		

2002 characteristics of Original 1992 respondents	No WGT	WGT HRS-92	WGT IPW	WGT HRS-02
Male				
education				
(%) lt. high school	23.5	20.1	20.2	20.4
(%) high school or GED	35.0	35.1	34.9	34.9
(%) college & more	41.5	44.8	44.9	44.8
Household Income and Wealth				
(median) income	$ 42,069	$ 44,616	$ 44,258	$ 44,200
(median) wealth	$ 173,000	$ 197,500	$ 197,000	$ 193,500
(%) living in poverty Census definition	6.61	5.88	5.94	6.14
Female				
education				
(%) lt. high school	25.4	21.7	22.0	22.0
(%) high school or GED	40.7	41.9	41.8	41.7
(%) college & more	33.9	36.4	36.2	36.3
Household Income and Wealth				
(median) income	$ 29,295	$ 32,170	$ 32,004	$ 32,000
(median) wealth	$ 129,000	$ 154,000	$ 153,000	$ 152,000
(%) living in poverty Census definition	12.18	9.67	9.74	9.82

Notes: The sample consists of initial 1992 respondents providing core interviews in 2002. Statistics are reported using different weights (WGT). HRS provided weights HRS-92 and HRS-02, matching HRS frequencies, in 1992 and 2002 respectively, on the March Supplement of the CPS by cells defined by birth cohort, gender, race, and ethnicity. The weights derived for attrition (IPW) are constructed as shown in the text based on multinomial logit predicted probabilities in Tables 3.3 and 3.4. Variable definitions are found in Table A.2

Table 5.3 2002 Health Outcomes Using Different Weights

Health Outcomes

2002 characteristics of Original 1992 respondents	No WGT	Weighting (WGT) scheme WGT HRS-92	WGT IPW	WGT HRS-02
Male				
(%) Health limits work	26.1	25.5	25.6	25.7
(%) at least one ADL	10.3	9.6	9.7	9.6
(%) ever had severe condition	42.0	42.0	42.3	42.1
(%) ever had mild condition	62.7	61.6	61.6	61.4
(%) Medicaid recipient	5.9	5.3	5.3	5.5
Female				
(%) Health limits work	30.3	29.3	29.4	29.5
(%) at least one ADL	14.4	13.1	13.1	13.1
(%) ever had severe condition	36.7	36.3	36.4	36.2
(%) ever had mild condition	63.9	61.5	62.0	61.5
(%) Medicaid recipient	9.5	7.6	7.7	7.8

Notes: The sample consists of initial 1992 respondents providing core interviews in 2002. Statistics are reported using different weights (WGT). HRS provided weights HRS-92 and HRS-02, matching HRS frequencies, in 1992 and 2002 respectively, on the March Supplement of the Current Population Survey (CPS) by cells defined by birth cohort, gender, race and ethnicity. The weights derived for attrition (IPW) are constructed as shown in the text based on multinomial logit predicted probabilities in Table 3.3 and 3.4. Variable definitions are found in Table A.2

Table 5.4 Living Arrangements, Marital Status and Family Members Alive using Different Weights

2002 characteristics of Original 1992 respondents	No WGT	WGT HRS-92	WGT IPW	WGT HRS-02
		Weighting (WGT) scheme		
Male				
Marital Status (%)				
married	82.6	81.8	81.5	80.8
widower	5.0	4.7	4.7	5.0
divorced/separ.	9.5	10.1	10.1	10.6
never married	2.9	3.4	3.7	3.7
Living arrangement & Family				
living with others (other than spouse)	26.5	24.7	24.7	25.0
(mean) number of siblings	2.6	2.4	2.4	2.4
(mean) number of children	3.6	3.5	3.5	3.5
(%) mom alive	17.9	17.9	17.8	17.5
(%) dad alive	3.5	3.4	3.3	3.3
Female				
Marital Status (%)				
married	59.3	62.0	62.2	61.1
widower	21.6	20.4	20.4	20.5
divorced/separ.	15.6	14.4	14.3	15.0
never married	3.5	3.2	3.2	3.4
Living arrangement & Family				
living with others (other than spouse)	29.4	26.6	26.7	26.9
(mean) number of siblings	3.0	2.8	2.8	2.8
(mean) number of children	3.6	3.6	3.6	3.6
(%) mom alive	18.0	18.4	18.2	18.3
(%) dad alive	4.6	4.6	4.6	4.6

Notes: The sample consists of initial 1992 respondents providing core interviews in 2002. Statistics are reported using different weights (WGT). HRS provided weights HRS-92 and HRS-02, matching HRS frequencies, in 1992 and 2002 respectively, on the March Supplement of the CPS by cells defined by birth cohort, gender, race, and ethnicity. The weights derived for attrition (IPW) are constructed as shown in the text based on multinomial logit predicted probabilities in Tables 3.3 and 3.4. Variable definitions are found in Table A.2

Two main reasons may explain the lack of effect from selection on observables. The first is that the group of attritors as of 2002 is quite small. It has to be very different in terms of characteristics to cause shifts in the sample statistics we have computed. Although it may not have effects on these tabulations, it could be that other analyses could by affected, particularly those that use the wave-by-wave transitions in the data. The second explanation has to do with the fact that the group that differs the most in terms of baseline characteristics makes it back to the survey. As we have already discussed, it might be that the group that may cause the most damage on the representativity of the sample is captured back in the study.

To check that possibility, we perform the following exercise. We assume respondents “ever out” do not make it back to the study for the 2002 interview. Hence, if we were to assess the effect of selection on observables on statistics computed on respondents in 2002, the only group interviewed would be the "always in" group or what is commonly known as the balanced sample from the panel.

To assess the effect of selection on observable on that sample, we would do the same test with IPW and HRS weights in 2002, except that retention probabilities would be given by

$$p(s_i \mid x_{i0}) = \frac{p(s_{i,a} \mid x_{i0})}{1 - p(s_{i,d} \mid x_{i0})}$$

since those "ever out" would not be in the study in 2002. Hence, we redo some tabulations on that sample using IPW and HRS weights. We perform those tabulations for total wealth and household income, two outcomes that are likely to be sensitive to selection on observables. The evidence in Table 5.3 showed a quite small selection effect for wealth for males while not for females and no difference for household income. Tables 5.5 and 5.6 present the statistics for the "always in" sample only and for the sample that includes the "ever out" respondents (as statistics in Table 5.4). Results show strong selection on observable effects for household income and wealth when using the balanced sample. For example, the difference in total wealth using the IPW weights and HRS weights on the balanced sample is nearly $15,000 at the median. This difference is negligible when including those "ever out." There is, therefore, a strong case to make for using the unbalanced sample (using those "ever out") rather than the balanced sample.

Table 5.5 Effects of Weighting on Household Income: Sample Excluding Ever Out Sequences

	Statistic (percentile)				
	10th	25th	median	75th	90th
Household Income in 2002					
Only "always in" (attrition weights correct for "ever out" and "attritors")					
Unweighted	$ 9,636	$ 19,068	$ 35,912	$ 62,760	$ 110,530
HRS-92	$ 10,720	$ 21,048	$ 38,436	$ 66,336	$ 119,964
IPW (both ever out and attritors)	$ 9,960	$ 19,630	$ 36,728	$ 64,424	$ 116,674
HRS-02	$ 10,558	$ 20,812	$ 38,040	$ 66,052	$ 120,100
"Always in" and "Ever out" sample (attrition weights correct for "attritors" only)					
Unweighted	$ 9,100	$ 18,432	$ 34,900	$ 61,740	$ 110,675
HRS-92	$ 10,200	$ 20,520	$ 37,348	$ 65,812	$ 120,000
IPW (only attritors)	$ 10,164	$ 20,504	$ 37,255	$ 65,657	$ 119,500
HRS-02	$ 10,081	$ 20,412	$ 37,200	$ 65,672	$ 120,136

Notes: In the Top panel, only "always in" respondents (interviews in all years from 1992 to 2002) are retained in the sample. Weights for attrition (includes "ever out" and "attritors") are constructed from the multinomial logit estimates in Tables 3.3 and 3.4 . In the bottom panel "always in" and "ever out" respondents are retained. IPW Weights are derived again from the multinomial logit estimates and are the same as those used in Tables 5.1–5.3.

Table 5.6 Effects of Weighting on Household Wealth: Sample excluding Ever Out Sequences

	Statistic (percentile)

	10th	25th	median	75th	90th
Household Wealth in 2002					
Only "always in" (attrition weights correct for "ever out" and "attritors")					
Unweighted	$ 2,100	$ 48,525	$ 155,300	$ 381,000	$ 795,000
HRS-92	$ 5,400	$ 62,000	$ 182,800	$ 441,000	$ 870,000
IPW (both ever out and attritors)	$ 2,650	$ 52,000	$ 165,675	$ 413,100	$ 843,000
HRS-02	$ 5,100	$ 60,500	$ 180,000	$ 432,000	$ 862,000
"Always in" and "Ever out" sample (attrition weights correct for "attritors" only)					
Unweighted	$ 1,400	$ 42,000	$ 146,000	$ 364,500	$ 778,000
HRS-92	$ 4,000	$ 56,000	$ 173,000	$ 426,000	$ 873,000
IPW (only attritors)	$ 4,000	$ 55,640	$ 173,000	$ 426,000	$ 870,000
HRS-02	$ 4,000	$ 54,700	$ 172,500	$ 424,001	$ 862,500

Notes: In the Top panel, only "always in" respondents (interviews in all years from 1992 to 2002) are retained in the sample. Weights for attrition (includes "ever out" and "attritors") are constructed from the multinomial logit estimates in Table 3.3 and 3.4 . In the bottom panel "always in" and "ever out" respondents are retained. IPW Weights are derived again from the multinomial logit estimates and are the same as those used in Tables 5.1-5.3.

6. Conclusions

In this study, we have investigated the effects that attrition and missing links with administrative data might have on inferences in the HRS. Our analysis focused on the HRS cohort born 1931–1941 that was interviewed every two years since 1992. We have restricted our analysis to biases introduced by selection on observables on cross-sectional inferences on the sample of initial respondents interviewed in 2002. Our main strategy has been to investigate the determinants of attrition and non-response and analyze how weighting schemes that are easy to implement could be use to address potential biases.

Our main conclusion is that for traditional cross-sectional tabulations made in 2002 for this age range, there is very little evidence of attrition bias from selection on observables that would warrant the use of more complicated weighting schemes than the weights provided by HRS. These match along certain characteristics the representativity of the sample to that of the CPS. There is selection on observables, but this is mostly from race, ethnicity, gender, and age, factors that enter the HRS weights. This lack of additional selection effect appears to result from the finding that respondents who differ the most eventually make it back to the study and that the group of attritors, as of 2002, remains small (less than 15%) in comparison to other past studies that might not have re-contacted non-respondents.

We have also looked at the non-randomness of the match of respondents' records with their SSA earnings record and SPD information provided by employers. We found that various baseline characteristics of respondents were correlated with the probability of a match, in addition to race, ethnicity, gender, and age. For the SSA record match, this is in line with findings by Olson (1999) and Haider and Solon (2000). Adjusted HRS weights provided with the earnings record are insufficient to address selection on observables. We then looked at expectations about retirement outcomes in groups defined by whether a match was done or not. Expectations were different for both match (SSA and SPD), although differences for the SPD match appeared larger. We found that in the case of SPD, the non-randomness was significantly—but not entirely—explained by selection on observables, particularly from selection on education. More educated workers who report they have a pension on the current job were more likely to have a match. In the case of the SSA match, weighting did little to correct the smaller differences we observed between the groups with and without a match. An interesting finding is that SSA match probabilities were found to be negatively associated with future attrition probabilities from the study, but that when we looked at the distribution of Social Security benefits of those with and without a match in 2000, no difference was observed between the groups.

These finding has some implications for practitioners. We have found that the balanced sample—the sample that excludes those who come back to the study—suffers from significant selection on observables when looking at financial outcomes in 2002. Hence, we would argue in favor of using the unbalanced sample in longitudinal analysis. However, this creates important methodological challenges, since respondents who skip waves are hard to incorporate in most panel data models. Another implication is that users of matched data should be concerned about the representativity of the matched HRS sample with administrative records (either from SSA or employers).

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Appendix A: Construction of Analytic File

Choice of Sample

We used the RAND HRS release E, based on the final release of waves 1992, 1994, 1996, 1998, 2000, and 2002, along with the tracker file 2002. The tracker file was first used to select respondents of the HRS cohort (born between 1931 and 1941) and to select a number of variables such as interview status, sample status, and vital status, as well as the pension match indicator. The Social Security match indicator is taken from merging the tracker file with the earnings record provided by SSA. We omitted from the sample respondents later assigned to the AHEAD cohort, known as AHEAD/HRS overlap cases. We merged data from the RAND HRS for each wave. We selected respondents who were born between 1931 and 1941. This exclusion led to the elimination of spouses of HRS cohort members not born in those years. In total 9,824 core interviews are completed in 1992, including 65 AHEAD/HRS overlap cases. Omitting those gives the figure in Table A.1 of 9,761 core interviews completed in 1992.

Table A.1 Vital, Sample and Interview Status over Waves

	Vital Status, Sample Status and Interview Status / Interview Type	Survey wave 1992	1994	1996	1998	2000	2002
	Alive						
	in sample						
1	core obtained	9,761	8,845	8,469	8,097	7,644	7,379
2	core missing	152	855	961	921	936	655
3	*total core*	9,913	9,700	9,430	9,018	8,580	8,034
4	not yet interviewed (not eligibible, future spouse)	156	131	92	64	36	
5	exit requested (not eligible)				8		
6	permanently dropped other reason (core missing)						1
7	**total alive**	10,069	9,831	9,522	9,090	8,616	8,035
	presumed alive						
8	in sample (core missing)		16	55	63	92	221
9	permanently dropped other reason (not eligible)					1	
10	**total presumed alive**	0	16	55	63	93	221
	death reported						
	in sample						
11	exit obtained		129	171	188	225	289
12	exit missing		39	40	25	47	56
13	**total death reported**	0	168	211	213	272	345
	death reported prior wave						
	in sample						
14	exit obtained				7	11	23
15	exit missing				14	12	23
16	post-exit obtained				26	66	70
17	post-exit missing			1	10	17	1
18	*total exit and post exit attempted*			1	57	106	117
19	completed exit (not eligible)			128	263	401	646
20	no one eligible for exit int prev.wave (not eligible)			39	58	77	89
21	permanently dropped other reason (not eligible)				1	8	12
22	**total death reported prior wave**	0		168	379	592	864
	vital status unknown						
23	in sample (core missing)		54	108	167	218	196
24	exit requested (not eligible)				105	125	245
25	permanently dropped other reason (not eligible)			5	52	153	163
	total vital status unknown	0	54	113	324	496	604
	Statistics						
26	# eligible sample respondents sum(3,6,8,13,18,23)	9,913	9,938	9,805	9,518	9,268	8,914
	Eligibility rate (26 as fraction of all cells)	98.5%	98.7%	97.4%	94.5%	92.0%	88.5%
	Fraction of interviews obtained (% of 26)						
	% core (1)	98.5%	89.0%	86.4%	85.1%	82.5%	82.8%
	% exit or post-exit sum(11,14,16)	0.0%	1.3%	1.7%	2.3%	3.3%	4.3%
	% missing sum(2,8,12,15,17,23)	1.5%	9.7%	11.9%	12.5%	14.1%	12.7%
	Total	100%	100%	100%	100%	100%	100%
	Response rate						
	on core (1 as fraction of sum(3,6,8,23))	98.5%	90.5%	88.3%	87.6%	86.0%	87.3%
	exit/post exit (sum(11,14,16) /sum(13,18))		76.8%	80.7%	81.9%	79.9%	82.7%

Definition of Variables

We used two panel status variables in this analysis. The first one tracks the status a respondent's record in 2002, conditional on being interviewed in 1992. We defined four states: (1) continuously interviewed between 1992 and 2002; (2) missed some interviews but interviewed in 1992 and 2002; (3) died prior to 2002; and (4) not interviewed in 2002 for reasons other than death. The variables used for this construction are x*iwwave* and x*alive* from the tracker file. Someone who was reported dead in 2002 is defined as (3) (died), even though an exit interview was collected in 2002. Someone presumed alive by the interviewer is defined as alive. The other status variable is a wave-specific variable that uses the same information as the cumulative status variable but tracks the status at each wave (1 = core interview provided, 2 = dead, 3 = no interview provided, known alive). Table A.2 documents the variables we use in the analysis.

Table A.2 Variable Definitions

Demographics	Type	Definition	RAND HRS vars
age	years	age of respondent	ragey_b
female	0/1	gender of respondent	ragender
born outside U.S.	0/1	respondent born outside U.S.	rabplace(11)
African American	0/1	race is African American	raracem(2)
hispanic	0/1	race is hispanic	hispan
married	0/1	respondent married/partnered	rmstat(1,2,3)
widow(er)	0/1	widow or widower	rmstat(7)
divorced	0/1	currently divorced	rmstat(4,5,6)
once divorced	0/1	once divorced but now married	rmdiv>0
single	0/1	never married	rmstat(8)
household size	number	number of household members	hhhres
Census Division	1/9	Census division of primary residence in 1992	rcendiv
number of siblings	number	number of siblings alive	rlivsib
number of children	number	number of children alive	hchild
dad alive	0/1	father alive	rdadliv
mom alive	0/1	mother alive	rmomliv
Health Status			
health good	0/1	health reported good	rshlt(=3)
health fair/poor	0/1	health reported fair/poor	rshlt(=4,5)
ever had severe cond.	0/1	ever had cancer/lung/heart/stroke	rcancre rhearte rstroke rlunge
ever had mild cond.	0/1	ever had psychic/diabetes/blood pressure	rdiabe rhibpe rpsyche
at least one ADL	0/1	at least one limitation in activities of daily living	radla>0
SES and Employment Status			
high school	0/1	high school education (completed or not)	raeduc(2,3)
some college	0/1	some college education (not completed)	raeduc(4)
college and above	0/1	completed college education or higher degree	raeduc(5)
own house	0/1	own primary residence	hafhous!=6
working	0/1	working for pay	rlbrf(1,2)
retired or partly retired	0/1	self-reported retired/partly retired	rlbrf(4,5)
disabled	0/1	self-reported disabled	rlbrf(6)
not labor force	0/1	not in labor force or unemployed	rlbrf(3,7)
have pension current job	0/1	conditional on working	rjcpen
receive pension income	0/1	receive any income from a pension	rpeninc
Income and Wealth $USD 2002 (BLS CPI used)			
total wealth	$USD2002	IRAs+Stocks+Bonds+Savings+Certificate&Deposits +Primary residence value + other assets - Debt - Mortgage	haira hastck habond hachck hacd hadebt hamln hahous hamort harles hatran haothr
hld income	$USD2002	Household annual gross income	hitot
individual earnings	$USD2002	Individual annual gross earnings	riearn
poverty threshold	0/1	based on CPS poverty definition for household income, does not include institutionalized family members	hinpov