Sociological Methods & Research
40(1) 32–56

DOI: 10.1177/0049124110390765
http://smr.sagepub.com

Nonparametric Tests of Panel Conditioning and Attrition Bias in Panel Surveys

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Abstract

Over the past decades there has been an increasing use of panel surveys at the household or individual level. Panel data have important advantages compared to independent cross sections, but also two potential drawbacks: attrition bias and panel conditioning effects. Attrition bias arises if dropping out of the panel is correlated with a variable of interest. Panel conditioning arises if responses are influenced by participation in the previous wave(s); the experience of the previous interview(s) may affect the answers to questions on the same topic, such that these answers differ systematically from those of respondents interviewed for the first time. In this study the authors discuss how to disentangle attrition and panel conditioning effects and develop tests for panel conditioning allowing for nonrandom attrition. First, the authors consider a nonparametric approach with assumptions on the sample design only, leading to interval identification of the measures for the attrition and panel conditioning effects. Second, the authors introduce additional assumptions concerning the attrition process, which lead to point estimates and standard errors for both the attrition bias and the panel conditioning effect. The authors illustrate their method on a variety of repeated questions in two household panels. The authors find significant panel conditioning effects in

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knowledge questions, but not in other types of questions. The examples show that the bounds can be informative if the attrition rate is not too high. In most but not all of the examples, point estimates of the panel conditioning effect are similar for different additional assumptions on the attrition process.

Keywords

panel conditioning, attrition bias, measurement error, panel surveys

Introduction

An important development in the social sciences over the past decades has been the increasing use of panel surveys at the household or individual level. Panel data have important advantages for research: They help, for example, to analyze changes at the micro level, disentangle permanent from transitory changes, and distinguish between causal effects and individual heterogeneity (see, e.g., Baltagi 2001; Lee 2002). Two potential drawbacks compared to, for example, independent cross-sections are attrition bias and panel conditioning effects (see, e.g., Sharot 1991; Trivellato 1999).

Attrition bias can arise if respondents drop out of the panel nonrandomly, namely, when attrition is correlated to a variable of interest. Panel attrition has been studied extensively, usually without considering panel conditioning effects. Summaries can be found in, for example, Fitzgerald, Gottschalk, and Moffitt (1998); Vella (1998); and Nicoletti (2006). Whether attrition bias plays a large role varies with the type of survey or the sample used. For example, Clinton (2001) finds no attrition bias in demographic characteristics, while Fitzgerald et al. (1998), Van den Berg and Lindeboom (1998), and Zabel (1998) find that attrition can be very selective and had a profound effect on the data. According to Goodman and Blum (1996), attrition may lead to a loss of particular groups of people in subsequent waves, resulting in a biased sample or lack of generalizability. They argue that researchers should systematically assess the effects of attrition on longitudinal data to rule out attrition as a source of bias or to more accurately interpret their findings when an attrition bias cannot be excluded. Existing models attempting to correct for attrition can be divided into models based upon assumptions on the attrition process but not requiring additional data (see, e.g., Das 2004; Hausman and Wise 1979; Little and Rubin 2002; Rubin 1976) and models where assumptions on attrition are avoided but additional data are used (see, e.g., Bhattacharya 2008; Hirano et al. 2001; Nevo 2003; Ridder 1992). For example, Hirano et al. (2001) show how a refreshment sample can be used to relax the assumptions under which the attrition bias can be identified. Their first model makes the assumption that the observations in the second period are missing at random (MAR; Rubin 1976). Their second model is closely related to the model of Hausman and Wise (1979), allowing the probability of attrition to depend on second period variables but not on first period variables. With a refreshment sample, the distinction between these two models can be nonparametrically identified.

Panel conditioning arises if responses in a follow-up wave are influenced by participation in the previous wave(s). The experience of the previous interview(s) may affect the answers in a next interview on the same topic, such that the answers differ systematically from the answers of respondents who are interviewed for the first time. This may be a good thing and reduce measurement error, if respondents learn how to interpret questions and make fewer errors. On the other hand, experienced respondents may become strategic and learn, for example, that answering ''no'' reduces the burden of their task, avoiding follow-up questions (see, e.g., Duan et al. 2007; Meurs et al. 1989). Sturgis, Allum, and Brunton-Smith (2007) expand on the main theory behind panel conditioning: the cognitive stimulus hypothesis. Questions asked about a certain topic may induce respondents to reflect more closely on that topic after the interview, talk about it, or acquire additional information through the media. This will lead to a difference between knowledge or attitudes at the first and second interview. The three cited studies find empirical evidence in favor of panel conditioning effects, but their analysis ignores potential attrition effects or time trends. Brannen (1993) asked explicit questions on the effects of survey participation and found that respondents became more aware of and interested in the research issues (child behavior and parental roles, in this case).

Many other studies in various social sciences have also looked at panel conditioning, with mixed findings, and typically without making a clear distinction between attrition bias and panel conditioning. Williams (1970), Williams and Mallows (1970), and Meurs et al. (1989) found systematic biases due to attrition or panel conditioning. Coombs (1973) found large differences in knowledge between first-time interviews and reinterviews but not in behavior or attitudes. Waterton and Lievesley (1989) found some evidence that especially respondents with low knowledge scores are influenced by reinterviewing. On the other hand, Dennis (2001) and Clinton (2001) found little evidence for attrition or panel conditioning in the Knowledge Networks' panel (an online panel that is representative of the entire U.S. population), and Pennell and Lepkowski (1992) found hardly any evidence of panel conditioning or attrition bias in income sources reported in the Survey of Income and Program Participation. Mathiowetz and Lair (1994) found evidence of panel conditioning in the measurement of functional health limitations, which can be explained by strategic behavior: By not reporting limitations, follow-up questions can be avoided. Similar results for the use of various types of health care services were found by Duan et al. (2007), who concluded that there was underreporting in the later items reported in the same survey. Van der Zouwen and Van Tilburg (2001) showed that most of their evidence of panel conditioning for measurement of personal network size in repeated personal interviews could be attributed to behavior of the interviewers. Sharpe and Gilbert (1998) found that repeated testing (interrupted by a one-week interval) increases the scores on the Beck depression scale and attributed this to social desirability, mood-congruent associative processing, or self-monitoring, triggered by the first interview. Similar effects, called ''testing effects'' in this context, were found by Chan and McDermott (2007).

In practice, it is difficult to separate the effects of panel conditioning from those of other changes between waves (Kalton, Kasprzyk, and McMillen 1989). Many studies on panel effects do not explicitly distinguish between attrition and panel conditioning and only look at the total bias induced by both. For example, Bartels (1999) and Golob (1990) only look at ''panel bias,'' the combined bias due to panel conditioning and attrition. Lohse, Bellman, and Johnson (2000) compare people who stay in the panel and people who drop out and explicitly mention that differences can be attributed to panel conditioning or attrition bias. Some authors try to distinguish between panel conditioning and attrition by making assumptions on the nature of the attrition process. In particular, they assume that attrition is random conditional on given covariates, which are then used to construct weights correcting for the attrition bias. For example, Pennell and Lepkowski (1992) use longitudinal weights to control for attrition effects in their assessment of panel conditioning effects on income sources. Nancarrow and Cartwright (2007) and Dennis (2001) use demographic weights to control for panel attrition when estimating panel conditioning effects based on panel tenure. Kruse et al. (2009) compare a longitudinal sample with three cross-sectional samples and control for attrition with demographic characteristics and survey experience. Wang, Cantor, and Safir (2000) control for socioeconomic and demographic variables to correct for attrition but acknowledge that differences can still be due to compositional differences rather than panel conditioning. In general, it is not possible to say whether the corrections for attrition bias in the studies cited earlier are sufficient to obtain unbiased estimates of panel conditioning.

In this article we aim at disentangling panel conditioning from attrition bias, with the goal of testing for panel conditioning while controlling for attrition bias. We extend the framework of Hirano et al. (2001) incorporating the possibility of panel conditioning effects, emphasizing the usefulness of a refreshment sample. The setup, with an initial sample interviewed once (in case of attrition) or twice (nonattrition) and a refreshment sample interviewed once, is described in the second section. The third section formalizes the total bias and its decomposition into a panel conditioning and attrition effect. We show that without further assumptions, these effects are not point identified but are identified up to an interval, in the sense of Manski (1989, 1995). The fourth section discusses additional assumptions on the attrition process under which we can point identify the attrition and panel conditioning effects. In the fifth section we illustrate our method for several repeated measurements in two large socioeconomic surveys. We find evidence of panel conditioning in knowledge questions, but not in questions on behavior or attitudes. The sixth section describes how our framework can be extended to the harder case of estimating transition probabilities, where even without attrition, the refreshment sample does not point identify the panel conditioning effect. The final section provides our conclusion.

Setup

We consider the same population at two different interview times, Time 1 and Time 2. The variable of interest is Z. For convenience we assume that Z can only have two outcomes, coded as 0 and 1. This can in principle be extended to more outcomes, since the distribution of any outcome of interest can be fully characterized by binary events. For example, if Z is continuous, we can study the binary variables $I[Z > z]$ for each z, where I is the indicator function; see Manski (1995). Similarly, we look at marginal distributions of Z, but the approach also applies to conditional distributions given time invariant covariates X. In practice, this means estimation by subsample with given values of X if X is discrete, while some smoothing technique can be applied if X has continuous components. Our approach does not directly apply if conditioning variables are time varying, for example, when estimating a transition probability, since Time 1 values are not observed in the refreshment sample. We discuss this as an extension in the sixth section.

The variable $Z_1 \in \{0, 1\}$ denotes the answer to the question of interest at Time 1. $Z_2(1) \in \{0, 1\}$ is the answer to this question at Time 2 that the respondent (would) give(s) if the interview at Time 2 is his or her first interview, namely, if he or she is not affected by panel conditioning. Our main parameter of interest is the time change in the probability of outcome 1, $\Pr(Z_2(1) = 1) - \Pr(Z_1 = 1)$.

$Z_2(2) \in \{0, 1\}$ denotes the Time 2 answer that the respondent (would) give(s) if the interview at Time 2 is his or her *second* interview. Compared to the setup of Hirano et al. (2001) we incorporate panel conditioning; namely, $Z_2(1)$ can be different from $Z_2(2)$: The answer to the question at Time 2 can be affected by being interviewed at Time 1.

Finally, the variable W describes attrition: $W = 1$ if the respondent, if interviewed at Time 1, also responds at Time 2 (“panel observation”), and $W = 0$ otherwise (“attrition”). All variables of interest form a vector $(Z_1, Z_2(1), Z_2(2), W)$ of four binary random variables, with a population distribution characterized by 16 joint probabilities.

The survey design is assumed to be as follows. At Time 1 a random sample of size n_1 is drawn from the population of interest, Sample 1. We assume throughout the article that there is no initial (unit or item) nonresponse (or that initial responses are missing at random (MAR; cf. Rubin 1976). We therefore observe all Time 1 answers of the respondents in Sample 1; they are denoted by $Z_{i,1}$, $i = 1, \ldots, n_1$. At Time 2, all members of Sample 1 are approached for a second interview. If i responds, then $W_i = 1$ (i is a panel member) and his or her Time 2 answer $Z_{i,2}(2)$ is observed. If respondent i does not respond, we only observe $W_i = 0$ (attrition) and not $Z_{i,2}(2)$. Hence, $n_P = \sum_{i=1}^{n_1} W_i$ is the number of respondents in Sample 1 that stay in the panel (“panel members”) and $n_A = n_1 - n_P$ is the number that drop out.

At Time 2, a refreshment sample is available. This is a new random sample (“Sample 2”) of size n_R from the population of interest (to be precise: The population excluding the respondents in Sample 1, but this is not important since we assume the population is infinitely large). We assume there is no nonresponse in the refreshment sample (or that nonresponse is MAR). Since the respondents in the refreshment sample are interviewed for the first time, this sample yields observations $Z_{i,2}(1)$, $i = 1, \ldots, n_R$.

In summary, at Time 1, we only have respondents interviewed for the first time (attrition and panel sample, the union of them is Sample 1). At Time 2, we have respondents interviewed for the second time (panel part of Sample 1), respondents who are interviewed for the first time (refreshment sample Sample 2, again a random sample), and respondents who do not respond at Time 2 (attrition part of Sample 1).

Parameters Identified Without Further Assumptions

The sample design implies that 8 functions of the 16 joint probabilities are identified and can be estimated consistently without further assumptions. From Sample 1 we can consistently estimate six probabilities using their sample analogues: $\Pr(Z_1 = z_1, W = 0)$, $z_1 \in \{0, 1\}$, and $\Pr(Z_1 = z_1, Z_2(2) = z_2, W = 1)$, $z_1, z_2 \in \{0, 1\}$. Similarly, the refreshment sample can be used to consistently estimate the probabilities $\Pr(Z_2(1) = z_2)$ and $z_2 \in \{0, 1\}$ using sample analogues.

This is obviously not enough to estimate the complete joint distribution of $(Z_1, Z_2(1), Z_2(2), W)$. For example, we can estimate the marginal distribution of $Z_2(1)$, but we know nothing about how $Z_2(1)$ relates to the other three variables, since $Z_2(1)$ is never observed jointly with any of the other three. Similarly, we know nothing of the distribution of $Z_2(2)$ when $W = 0$. The latter is the familiar problem of identification under selective attrition, as in Hirano et al. (2001). The difference with Hirano et al. (2001) is that we want to allow for panel conditioning effects, implying that we allow first time responses $(Z_2(1))$ and second time responses $(Z_2(2))$ to be different. The refreshment sample is informative about the distribution of $Z_2(1)$ but not on that of $Z_2(2)$; panel observations are informative about $Z_2(2)$ but only for the respondents who do not drop out $(W = 1)$.

To illustrate this setup, we consider two examples, without and with attrition.

Example 1: Without Attrition

In this example, everyone in Sample 1 participates in Wave 1 as well as Wave 2. Assume Sample 1 is very large so that we can ignore sampling error. Say the Sample 1 fraction who answer yes to the (yes/no) question of interest rises from 0.5 in Wave 1 to 0.6 in Wave 2, so $\Pr(Z_1 = 1) = 0.5$ and $\Pr(Z_2(2) = 1) = 0.6$ The change 0.1 can be either a genuine time effect (the fraction of the population also increases by 0.1; namely, $\Pr(Z_2(1) = 1) = 0.6$) or an effect of panel conditioning (participating in Wave 1 has induced 10 percent of all respondents to change their answer in Wave 2 from no to yes, so $\Pr(Z_2(1) = 1) = 0.5$). Without refreshment sample, we cannot distinguish between the time and the panel conditioning effects. In practice, researchers will typically assume that the panel conditioning effect is zero and conclude that the fraction for which the true outcome is yes has increased by 0.1, ascribing the complete change to a time effect. This conclusion is biased if the panel conditioning effect $\Pr(Z_2(2) = 1) - \Pr(Z_2(1) = 1)$ is not equal to zero.

With a randomly drawn refreshment sample we can do much better. The Time 2 answers of the refreshment sample are not subject to panel conditioning. If the fraction of yes answers in the refreshment sample ($\Pr(Z_2(1) = 1)$ is 0.5, then there is no time effect (since this is the same as the fraction in Sample 1 at Time 1), and the complete change of 0.1 in the original sample is due to panel conditioning. If the fraction in the refreshment sample is q, then the panel conditioning effect is $0.6 - q$ and the time effect is $q - 0.5$. In this case, with a random refreshment sample and no attrition in the original sample, both the time effect and the panel conditioning effect are identified, showing the value of having (and using) a refreshment sample.

Example 2: With Attrition

Now consider an example with an attrition rate of 2 percent: 2 percent of Sample 1 (drawn at Time 1) does not participate at Time 2. Let the fractions answering yes to the question of interest (again) be 0.5 in Wave 1 (for Sample 1 as a whole) and 0.6 in Wave 2 for the 98 percent of Sample 1 who participate in Wave 2. And let q be the fraction with $Z = 1$ in the refreshment sample (at Time 2). Irrespective of attrition or panel conditioning, an unbiased estimate of the time effect is again given by $q - 0.5$, exploiting the refreshment sample. Not using the refreshment sample and accounting for neither panel conditioning nor selective attrition, the estimate of the time effect would (again) be $0.6 - 0.5 = 0.1$. The total bias in this estimate is $0.1 - (q - 0.5) = 0.6 - q$, as in Example 1. It can now be due to selective attrition, panel conditioning, or both.

To isolate the panel conditioning effect (or decompose the total bias into panel conditioning and attrition effects), we now have to deal with the incomplete observations. If all those who drop out would have answered yes in Wave 2 had they been forced to participate, then the fraction yes in Wave 2 in the complete Sample 1 would have been $\Pr(Z_2(2) = 1) = 0.98 * \ 0.6 + 0.02 * 1 = 0.608$. The panel conditioning effect would then be $0.608 - q$, and the attrition bias would be the remainder of the total bias, -0.08. On the other hand, if all the dropouts would have answered no had they been forced to participate, the fraction yes in the complete Sample 1 would have been $\Pr(Z_2(2) = 0.98 * 0.6 + 0.02 * 0 = 0.588$; the panel conditioning effect would then be $0.588 - q$, and the attrition bias 0.012. Without assumptions on the nature of the attrition (Are the dropouts yes or no sayers?), we cannot say more than this, and we can identify the panel conditioning effect only up to the (closed) interval $[0.588 - q; 0.608 - q]$ of width 0.02 — the attrition rate. The panel conditioning effect is not point identified but "identified up to a bounding interval" (Manski 1989, 1995).

Panel Conditioning and Attrition Bias

This section formalizes the total bias and its decomposition into panel conditioning and attrition effects and derives the bounds on these effects.

The (true) *time effect* (taking outcome 1 as the benchmark) is given by $TE = \Pr(Z_2(1) = 1) - \Pr(Z_1 = 1)$. The second term can be estimated consistently from Sample 1. Ignoring possible effects of attrition and panel conditioning and not using a refreshment sample, one would estimate the first term using the sample fraction of outcome $Z = 1$ in the panel observations at Time 2 (excluding those who drop out):

$$\begin{aligned}\hat{\Pr}(Z_2(2) = 1|W = 1) &= \frac{\hat{\Pr}(Z_2(2) = 1, W = 1)}{\hat{\Pr}(W = 1)} = \frac{n_1}{n_P}\frac{1}{n_1}\sum_{i=1}^{n_1} Z_{i,2}(2)W_i \\ &= \frac{1}{n_P}\sum_{i=1}^{n_1} Z_{i,2}(2)W_i\end{aligned}$$

This is a consistent estimator of $\Pr(Z_2(2) = 1|W = 1)$, which can differ from $\Pr(Z_2(1) = 1)$ because of attrition and panel conditioning. Using it to estimate TE therefore leads to an asymptotic "total bias" TB in the time effect given by

$$TB = \Pr(Z_2(2) = 1|W = 1) - \Pr(Z_2(1) = 1).$$

With the refreshment sample, $\Pr(Z_2(1) = 1)$ can be estimated consistently as the fraction with outcome $Z = 1$ in the refreshment sample. Thus, TB is identified (without additional assumptions) and can be estimated consistently by

$$\begin{aligned}\hat{T}B &= \hat{\Pr}(Z_2(2) = 1|W = 1) - \hat{\Pr}(Z_2(1) = 1) \\ &= \frac{1}{n_P}\sum_{i=1}^{n_1} Z_{i,2}(2)W_i - \frac{1}{n_R}\sum_{i=1}^{n_R} Z_{i,2}(1).\end{aligned} \tag{1}$$

Inference on TB is straightforward, since Samples 1 and 2 are independent samples. For example, a test for the null hypothesis $H_0 : TB = 0$ can be based upon the difference between two independent sample fractions.

Decomposition

As illustrated in Example 2, the total bias can be decomposed into a panel conditioning effect (PC) and an attrition bias (AB) as follows:

$$\begin{aligned}TB &= PC + AB \\ &= [\Pr(Z_2(2) = 1) - \Pr(Z_2(1) = 1)] \\ &\quad + [\Pr(Z_2(2) = 1|W = 1) - \Pr(Z_2(2) = 1)].\end{aligned}$$

As we saw in Example 2, we cannot identify PC or AB without making additional assumptions, because $\Pr(Z_2(2) = 1)$ is not identified (since we do not observe $Z_2(2)$ if $W = 0$). But writing $\Pr(Z_2(2) = 1) = \Pr(Z_2(2) = 1, W = 1) + \Pr(Z_2(2) = 1 | W = 0) \Pr(W = 0)$ and using that $\Pr(Z_2(2) = 1 | W = 0)$ is between 0 and 1, the following sharp bounds can be derived straightforwardly:

$$
\begin{aligned}
PC \in & [\Pr(Z_2(2) = 1, W = 1) - \Pr(Z_2(1) = 1), \\
& \Pr(Z_2(2) = 1, W = 1) - \Pr(Z_2(1) = 1) + \Pr(W = 0)] \\
AB \in & [\Pr(Z_2(2) = 1 | W = 1) - \Pr(Z_2(2) = 1, W = 1) - \Pr(W = 0), \\
& \Pr(Z_2(2) = 1 | W = 1) - \Pr(Z_2(2) = 1, W = 1)].
\end{aligned}
$$

The bounds can be estimated consistently, replacing probabilities by sample analogues. The distance between the bounds is $\Pr(W = 0)$, the attrition probability.

Note that PC is defined on the complete population, and AB is defined in terms of the answers affected by (potential) panel conditioning. The following alternative decomposition considers panel conditioning among those who do not drop out and the attrition effect in (unobserved) answers not affected by panel conditioning:

$$
\begin{aligned}
TB &= PC^{Alt} + AB^{Alt} \\
&= [\Pr(Z_2(2) = 1 | W = 1) - \Pr(Z_2(1) = 1 | W = 1)] \\
&\quad + [\Pr(Z_2(1) = 1 | W = 1) - \Pr(Z_2(1) = 1)].
\end{aligned}
$$

In the remainder we focus on the first decomposition, since it seems conceptually more attractive to consider the panel conditioning effect in the complete population. Bounds for the alternative decomposition can be derived in a similar way (see online Appendix 1, available at http://smr.sagepub.com/supplemental).

Additional Assumptions

Particularly if the attrition rate is substantial, the bounds are too wide to be informative, and additional assumptions are needed to make useful inferences. In this section we discuss several possible additional assumptions concerning the attrition process and show how they help to obtain point identification of the panel conditioning effect. Which of the additional assumptions is most plausible will depend on the application of interest.

Assumption 1 (completely missing at random [CMAR] after panel conditioning):

$$\Pr(W = 1|Z_2(2) = z) = \Pr(W = 1), \quad z \in \{0, 1\}.$$

This assumption says that Wave 2 nonresponse is completely missing at random (cf. Little and Rubin 2002). It is rather strong, since it does not condition on the Wave 1 answer. In practice it seems better to introduce a weaker version, missing at random conditional on observables, in this case the Time 1 answer Z_1.[1]

Assumption 2 (MAR after panel conditioning):

$$\Pr(W = 1|Z_1 = z_1, Z_2(2) = z_2) = \Pr(W = 1|Z_1 = z_1), \quad z_1, z_2 \in \{0, 1\}.$$

The MAR assumption is one of the most popular ways to correct for attrition or, more generally, sample selection (see e.g., Little and Rubin 2002). This may be a reasonable assumption in many circumstances, particularly if we perform the analysis for a subsample with given sociodemographic characteristics (e.g., low educated low income single females in the age group 20 to 25). For example, if the specific question of interest is part of a large survey with many questions on a variety of topics, attrition can be correlated with Z because both attrition and Z depend on sociodemographic characteristics. But within a narrowly defined socioeconomic subgroup, there is much less concern for a correlation between attrition and the answer Z to the question of interest.

There are other cases, however, where random attrition given sociodemographics is not so plausible. Take the question, ''Do you currently have a permanent job?'' People without a permanent job may tend to move more often than others. Unless movers are perfectly traced down and do not have a larger probability to leave the sample than nonmovers, it seems likely that respondents who do not have a permanent job are more likely to drop out of the sample than the others. In this case, an alternative assumption on the attrition process seems more plausible, for example, the assumption that the relation between attrition and the variable of interest is stable over time, in the sense that the difference between the fraction of yes answers in the complete population and among those who do not drop out is the same in Waves 1 and 2. To make this precise, note that Assumption 2 can also be written as $\Pr(Z_2(2) = z|W = 1) - \Pr(Z_2(2) = z) = 0$. Assuming stationarity of the attrition bias implies the following.

Assumption 3 (Stationary attrition bias after panel conditioning):

$$\Pr(Z_1 = z|W = 1) - \Pr(Z_1 = z) = \\ \Pr(Z_2(2) = z|W = 1) - \Pr(Z_2(2) = z), \quad z \in \{0, 1\}.$$

It should be noted that there are alternative ways of imposing stationarity that lead to slightly different outcomes in most practical examples. For example, stationarity can be imposed upon the attrition probability. This leads to Assumption 4.

Assumption 4 (Attrition probability depends in the same way on Z_1 and $Z_2(2)$:

$$\Pr(W = 1|Z_1 = z) = \Pr(W = 1|Z_2(2) = z), \quad z \in \{0, 1\}.$$

We show in the following that any of these attrition assumptions leads to point identification of the panel conditioning effect. It should be emphasized that the assumptions are not testable and that which of them is more plausible depends on how the nature of the question and the sample design affect attrition, as illustrated in the aforementioned example. In the empirical examples, we compute the panel conditioning effect under various assumptions and investigate whether it matters which assumption is made. The literature on panel conditioning typically makes Assumption 1 or 2. The attrition analysis of Hirano et al. (2001) considers weaker assumptions on attrition that lead to identification (their AN model), but then they assume that there is no panel conditioning.

Note that all the assumptions we have made are nonparametric—they do not impose assumptions on the distribution of error terms, and so on. In this sense, our approach is similar to matching but not to, for example, Heckman type of selection models (Heckman 1979), which make either parametric or semiparametric model assumptions.

Point Estimation Under Additional Assumptions

How can the additional assumptions introduced earlier be used to obtain point estimates? All our point estimates are based on sample analogues of unconditional or conditional probabilities. We consider the Assumptions 1 through 4 in turn.

Assumption 1 implies that W and $Z_2(2)$ are independent, and hence,

$$\Pr(Z_2(2) = 1|W = 1) = \Pr(Z_2(2) = 1),$$

so $AB = 0$ and $PC = TB$, and AB and PC are identified. We can simply ignore attrition.

Assumption 2 implies

$$\Pr(Z_1 = z_1, Z_2(2) = 1) = \frac{\Pr(Z_1 = z_1, Z_2(2) = 1, W = 1)}{\Pr(W = 1|Z_1 = z_1)}, \quad z_1 \in \{0, 1\},$$

and hence,

$$\Pr(Z_2(2) = 1) = \\ \frac{\Pr(Z_1 = 0, Z_2(2) = 1, W = 1)}{\Pr(W = 1|Z_1 = 0)} + \frac{\Pr(Z_1 = 1, Z_2(2) = 1, W = 1)}{\Pr(W = 1|Z_1 = 1)}.$$

The four probabilities on the right hand side can directly be estimated with their sample analogues, so AB and PC are identified. The attrition bias is dealt with by partitioning the sample on the basis of Z_1. The MAR assumption implies that attrition can be ignored within each of the subsamples. Note that Assumption 2 implies that the attrition bias $\Pr(Z_2(2) = 1|W = 1) - \Pr(Z_2(2) = 1)$ is zero if W is independent of Z_1.

Assumption 3 implies

$$\Pr(Z_2(2) = 1) = \Pr(Z_2(2) = 1|W = 1) + \Pr(Z_1 = 1) - \Pr(Z_1 = 1|W = 1),$$

and the probabilities on the right-hand side can be estimated directly by their sample analogues, so that AB and PC are identified.

To illustrate with a numerical example: Suppose half of the people have a permanent job at Time 1 ($\Pr(Z_1 = 1) = 0.5$), attrition is zero among those with a permanent job and 20 percent among those without a permanent job at Time 1. Then the fraction of nondropouts with a permanent job at Time 1 is $0.5/0.9 = 0.556$, so the attrition bias at Time 1 is $\Pr(Z_1 = 1|W = 1) - \Pr(Z_1 = 1) = 0.556 - 0.5 = 0.056$. Assumption 3 says that the attrition bias in reported outcomes at Time 2 is the same, so $\Pr(Z_2(2) = 1) = \Pr(Z_2(2) = 1|W = 1) - 0.056$, where $\Pr(Z_2(2) = 1|W = 1)$ is the fraction answering yes at Time 2 among those who stay in the panel. This shows that $\Pr(Z_2(2) = 1)$ is identified. The refreshment sample identifies $\Pr(Z_2(1) = 1)$, so the panel conditioning effect $\Pr(Z_2(2) = 1) - \Pr(Z_2(1) = 1)$ is identified as well.

Assumption 4 implies

$$\Pr(Z_2(2) = 1) = \Pr(Z_2(2) = 1|W = 1) \Pr(W = 1) / \Pr(W = 1|Z_1 = 1),$$

and the probabilities on the right hand side can be estimated directly by their sample analogues, so that Assumption 4 identifies AB and PC.

Empirical Illustrations

We use the estimated bounds and point estimates of the previous section to compute estimates of panel conditioning effects and attrition bias in several examples. First, we use the CentERpanel, an Internet panel representative of the Dutch population of age 16 and older. Second, we use the first two waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), covering the population of age 50 and older in 11 European countries.

The CentERpanel is administered by CentERdata, Tilburg University. Participating households are a probability sample of all Dutch households except those in institutions. The setup is similar to the one chosen by Knowledge Networks in the United States. Because not everyone owns a personal computer or has access to Internet, CentERdata provides a set-top box for people who do not have a computer, enabling them to complete the questionnaires online. Respondents are asked to fill out a questionnaire every week. We selected various binary variables in several two-wave research projects. This selection was entirely based upon the availability of data. We looked at questions that were fielded more than once and focused on different types (knowledge, actual behavior, actual circumstances, attitudes, opinions, or future expectations). If more questions within one type were available, we made a random choice. Details of the questions and the results are presented in the online Appendix 2 (found at http://smr.sagepub.com/supplemental).[2]

The hypothesis that the *total bias* is equal to zero is rejected for only a few questions, all measuring knowledge. For other question types, no significant total bias was found. The fact that knowledge questions are the most sensitive to panel conditioning is consistent with the literature (cf. first section).

We decomposed the total bias into a panel conditioning and an attrition effect. For the questions with an insignificant total bias, both effects were always insignificant also, irrespective of which of the additional assumptions concerning attrition we used. We therefore focus on the three knowledge questions for which the total bias is significant: ‘‘Do you know what campylobacter is?’’ ‘‘Do you know what cross-infection is?’’ and ‘‘Have you ever heard of a foundation named Stichting Pensioenkijker?’’ The first two stem from a survey module on hygiene knowledge, fielded in November 2003 and November 2005. The third is from a survey module on pensions and pension knowledge administered in February 2004 and February 2005. Stichting Pensioenkijker is a Dutch nonprofit organization that aims at

Table 1. Panel Conditioning in Three Knowledge Questions

	Campylobacter	Cross-infection	Stichting Pensioenkijker
Size Sample 1	1,510	1,510	1,734
Attrition rate (percentage)	18.8	18.8	10.7
Size Sample 2	891	891	701
Total bias (percentage points)	6.17*	6.71*	5.20*
Panel conditioning effect			
Interval estimate	(0.90, 19.70*)	(–6.43, 12.38*)	(3.44*, 14.16*)
Point estimate			
Assumption 2	5.96*	6.31*	5.20*
Assumption 3	5.85*	5.88*	5.20*
Assumption 4	5.72*	5.69*	5.20*

$*p < .05$.

increasing the Dutch population's knowledge about pensions and helping them to prepare financially for retirement. Their main instrument is a Web site (http://www.pensioenkijker.nl).

Table 1 summarizes the results (for details, see the online Appendix 2). Consider the first example—knowledge of campylobacter. At Time 1, 19.3 percent report they know what this is. Among panel observations, this increases to 28.1 percent at Time 2, whereas in the refreshment sample, it only increases to 21.9 percent. The difference is the estimate of the total bias, 6.17 percentage points. Without making further assumptions, the estimates on the lower and upper bound of the panel conditioning component of the total bias are 0.90 and 19.70 percentage points. The lower bound is not significantly different from 0 ($SE = 1.76$). In this example, we can, without making additional assumptions, therefore not conclude that there is a panel conditioning effect.[3] But of course it is possible that this is due to the width of the bounds.

The conclusion changes if additional assumptions are made on the nature of attrition so that point identification is obtained. Under the additional Assumptions 2 through 4, we find that almost all of the total bias can be attributed to panel conditioning, with insignificant attrition bias estimates and significant point estimates of the panel conditioning effect varying from 5.72 to 5.96 percentage points. Which of the assumptions is most plausible is hard to judge without further analysis, but in this example the result is insensitive to the choice of assumption (or to the choice of decomposition, see the online Appendix 2).

In the second example, on knowing the meaning of cross-infection, the results are similar. The estimate of the total bias is 6.71 percentage points, and under the additional assumptions that allow for point estimation and nonzero attrition, most of this is panel conditioning (with estimates varying from 5.69 to 6.31 percentage points, all significantly different from zero). The only difference with the first example is that the point estimates of the lower bound of the panel conditioning effect are negative so that the estimated interval contains zero, making a test whether the lower bound is significantly different from zero unnecessary. The negative lower bound and positive upper bound already imply that without additional assumptions on attrition, the null hypothesis of no panel conditioning cannot be rejected. Again, the lack of information reflected in the width of the bounds may drive this result.

The third example, on having heard of Stichting Pensioenkijker, gives the strongest evidence of panel conditioning. At Time 1, 7.55 percent of respondents have heard of this organization. For panel respondents, this rises to 16.47 percent one year later. In the refreshment sample drawn at the same time, it is 11.27 percent. The difference of 5.20 percentage points is statistically significant. Without further assumptions, the implied lower bound on the panel conditioning effect is 3.44 percentage points and significantly positive ($SE = 1.64$). Thus, even without making further assumptions, we find significant evidence of panel conditioning. The main reason why we find this here and not in the example on campylobacter is the lower attrition rate—10.7 percent versus 18.8 percent. Under additional assumptions, the point estimates of the panel conditioning effect are always 5.20 percentage points (and, as expected, significantly larger than zero). The reason why the point estimates are all virtually identical is that the sample analogues of $\Pr(W = 1|Z_1 = 1)$ and $\Pr(W = 1|Z_1 = 0)$ are almost the same, implying that the attrition bias is zero under any of the additional assumptions.

In the examples from the CentERpanel, no attrition bias was found, perhaps due to the modest size of the attrition rate. In the other panel we considered, the Survey of Health, Ageing and Retirement in Europe, attrition is much larger. SHARE is a cross-national panel database of micro data on health, socioeconomic status, and social and family networks of more than 40,000 individuals aged 50 or older. Interviews were conducted face to face. See www.share-project.org for more details. Waves 1 and 2 were administered in 2004 and in 2006-2007. In our examples we focus on two cognitive functioning tests for the birth cohorts 1939-1949. A panel conditioning bias might arise here since the same exercises were given in both waves. In the first (numeracy) question, respondents were asked how many people out of 1,000 would be expected to get a disease if the chance of getting the disease is 10 percent. In the second, respondents were asked to (immediately) recall words from a list of 10 words that was read aloud by the interviewer.

Table 2. Panel Conditioning and Attrition Bias in Cognitive Skill Questions

	Numeracy question		Ten words recall list	
Size Sample 1	10,608		10,608	
Attrition rate (percent)	31.4		31.4	
Size Sample 2	3,000		3,000	
Total bias (percentage points)	2.77*		2.45*	
	PC	*AB*	*PC*	*AB*
Interval estimate	(–23.4, 7.93)	(–5.16, 26.2)	(–20.1, 11.3)	(–8.85, 22.5)
Assumption 2	2.15*	0.62*	1.49	0.96*
Assumption 3	1.32	1.45*	–0.08	2.53*
Assumption 4	1.31	1.46*	–0.12	2.57*

*$p < .05$.

The results are presented in Table 2, with additional details in the online Appendix 2 (Example F). For both questions the total bias is positive and significant, with better cognitive scores in the panel than in the refreshment sample. This can be due to panel conditioning as well as attrition. The attrition rate is 31.4 percent, too high for the bounds to be informative. Under the completely missing at random assumption, the attrition bias is zero and the panel conditioning effect is equal to the total bias and therefore significantly positive. Under MAR (Assumption 2), the panel conditioning effect is smaller and significant only for the numeracy question, while the attrition bias is significantly positive for both questions. Under (any of) the stationarity assumptions, we find strong and significant positive attrition biases and small and insignificant panel conditioning effects for both questions. The estimates of the panel conditioning effects are therefore sensitive to the assumption made on attrition. In this case, the stationarity assumptions seem more plausible than MAR or CMAR, since deteriorating lack of cognitive skills may be a plausible reason to discontinue participation in the survey. It is reassuring that it hardly makes a difference *which* stationarity assumption is made (Assumption 3 or 4). The example also illustrates that the stationarity assumptions on attrition do not imply that attrition bias plays a minor role.

Transition Rates

Until now we have focused on estimating (changes over time in) simple population fractions. In this section, we look at the harder problem of estimating a transition probability, for which neither the original sample nor the refreshment sample provide consistent estimates. We show that we can use a similar methodology and similar additional assumptions, but they lead to interval identification rather than point identification, and the intervals are informative only if the initial state probability is large.

This extension has useful practical applications. For example, some public policies have education components or are designed to increase program take-up rates. Consider evaluation of a policy where there is a treated group (exposed to the education program) and a control group and both groups are followed over time. The standard difference-in-differences design where the impact of a policy is estimated as the before and after difference in an outcome (e.g., knowledge of the program) between the treated and untreated groups will estimate the gain in knowledge after eliminating panel conditioning (because the control group is also subject to panel conditioning). But this underestimates the increase in knowledge for the program because the panel conditioning (learning about the program by hearing it mentioned) should count as a benefit of the program. Estimates of the size of panel conditioning effects—though only interval identified—are estimates of this underestimate of benefits.

Without loss of generality, we consider the probability $\Pr(Z_2(1) = 1|Z_1 = 1)$. The standard way of estimating this, ignoring attrition bias and panel conditioning, is to take the fraction of observations with $Z_2(2) = 1$ among the panel observations ($W = 1$) with $Z_1 = 1$. This is a consistent estimator for $\Pr(Z_2(2) = 1|W = 1, Z_1 = 1)$. The total (asymptotic) bias of the standard estimator is therefore given by

$$TB^{tr} = \Pr(Z_2(2) = 1|W = 1, Z_1 = 1) - \Pr(Z_2(1) = 1|Z_1 = 1).$$

TB^{tr} is not point identified because we cannot estimate the second term. In a similar way as in the third section, however, the following sharp bounds can be derived:

$$\ell \leq \Pr(Z_2(1) = 1|Z_1 = 1) \leq r, \text{with}$$
$$\ell = \max\left(0, \frac{\Pr(Z_2(1) = 1) - \Pr(Z_1 = 0)}{\Pr(Z_1 = 1)}\right), \quad r = \min\left(1, \frac{\Pr(Z_2(1) = 1)}{\Pr(Z_1 = 1)}\right).$$

All probabilities in ℓ and r can be estimated directly using sample analogues. The width of this interval is at most $\Pr(Z_1 = 0)/\Pr(Z_1 = 1)$, which decreases with $\Pr(Z_1 = 1)$, showing that the bounds are more informative the larger $\Pr(Z_1 = 1)$.

As before, we can decompose the total bias into a panel conditioning effect (PC^{tr}) and an attrition bias (AB^{tr}). The decomposition is given by[4]

$$\begin{aligned} TB^{tr} &= PC^{tr} + AB^{tr} \\ &= [\Pr(Z_2(2) = 1|Z_1 = 1) - \Pr(Z_2(1) = 1|Z_1 = 1)] \\ &\quad + [\Pr(Z_2(2) = 1|W = 1, Z_1 = 1) - \Pr(Z_2(2) = 1|Z_1 = 1)]. \end{aligned}$$

Sharp bounds for the panel conditioning effect PC^{tr} can be derived in a similar way as in the third section. They are given by $\ell \leq PC^{tr} \leq r$, with

$$\begin{aligned} \ell &= \Pr(Z_2(2) = 1, W = 1|Z_1 = 1) - \min\left(\frac{\Pr(Z_2(1) = 1)}{\Pr(Z_1 = 1)}, 1\right) \\ r &= \Pr(Z_2(2) = 1, W = 1|Z_1 = 1) + \Pr(W = 0|Z_1 = 1) \\ &\quad - \max\left(0, \frac{\Pr(Z_2(1) = 1) - \Pr(Z_1 = 0)}{\Pr(Z_1 = 1)}\right). \end{aligned}$$

The distance between the bounds is at most $(1 - \Pr(Z_1 = 1))/\Pr(Z_1 = 1) + \Pr(W = 0|Z_1 = 1)$. This is larger than in the third section (unless $\Pr(Z_1 = 1) = 1$) and does not tend to zero if the attrition probability tends to zero. This is because even without attrition, the fact that Z_1 is not observed for the refreshment sample prevents point identification. For the same reason, additional assumptions on attrition are not enough to obtain point identification, though they reduce the width of the interval.

Table 3 presents the interval estimates of the panel conditioning effect for a few examples with large probabilities for the initial state ($\Pr(Z_1 = 1)$ or $\Pr(Z_1 = 0)$). Even for initial state probabilities of about 0.8, we find rather large and not very informative intervals for the total bias. In all examples except one, the interval estimate for the total bias contains zero, implying that we cannot reject the null that the total bias is zero. As a consequence, we cannot conclude that there is a panel conditioning effect (without making additional assumptions).

Let us now focus on the third example in Table 3. The initial state we consider is that the respondent answers no to the question, ''Have you ever heard of Stichting Pensioenkijker'' (see the fourth section), which is the answer of 92 percent of Sample 1 (our estimate of $\Pr(Z_1 = 0)$). The bounding interval on the total bias in the estimate for $\Pr(Z_2(1) = 0|Z_1 = 0)$ (in percentage points) is (–10.31, –2.14). Equivalently, the interval estimate for the total bias in $\Pr(Z_2(1) = 1|Z_1 = 0)$ is (2.14, 10.31), suggesting that the usual panel based estimate of the probability of learning about Stichting Pensioenkijker

Table 3. Bounds on Total Bias and Panel Conditioning Effects in Transition Rates

Question	Initial state	Total bias (in percentage points)	Panel conditioning effect
Example A, Variable 1			
Campylobacter	$\hat{\Pr}(Z_1 = 0) = 0.81$	(–11.97, 12.01)	(–28.20, 14.90)
Example A, Variable 2			
Salmonella	$\hat{\Pr}(Z_1 = 1) = 0.97$	(–3.33, 0.02)	(–21.27, 0.77)
Example C, Variable 3			
St. Pensioenkijker	$\hat{\Pr}(Z_1 = 0) = 0.92$	(–10.31, –2.14)	(–19.50, –0.60)
Example F, Variable 1			
Numeracy question	$\Pr(Z_1 = 1) = 0.82$	(–8.16, 14.22)	(–35.48, 17.03)
Example F, Variable 2			
Words recall list	$\Pr(Z_1 = 1) = 0.68$	(–17.12, 27.89)	(–40.99, 32.82)

Note: The total bias and panel conditioning effect are those for estimating $\Pr(Z_2(1) = 0|Z_1 = 0)$ for the first and third variable and $\Pr(Z_2(1) = 1|Z_1 = 1)$ for the other variables. Details of the examples can be found in the online Appendix 2 (available at http://smr.sagepub.com/supplemental).

between Time 1 and Time 2 may be too high due to panel conditioning or attrition. The interval estimate for the panel conditioning effect on $\Pr(Z_2(1) = 1|Z_1 = 0)$ is (0.60, 19.50), and the positive lower bound suggests that panel conditioning at least partially explains the overestimation. Unfortunately, the lower bounds for the total bias and panel conditioning effect are not statistically different from zero (with standard errors 1.59 and 1.54, respectively), and larger samples would be needed to draw final conclusions.

Conclusion

We have analyzed panel conditioning and attrition effects on estimates of binary outcome probabilities in two-wave panel surveys, combining a panel survey with a refreshment sample. We have shown that without additional assumptions, point identification of the panel conditioning effect or the attrition bias are not possible, but the panel conditioning effect is identified up to an interval. How informative this bounding interval is depends on its width, which is driven by the attrition rate. In many practical cases, the attrition rate is so large that meaningful inferences are not possible without making further assumptions. We considered several additional assumptions on the attrition process and showed how they lead to point identification of the panel conditioning effect. The plausibility of these assumptions has to be studied case by case. In most of our examples we found similar results for each of the assumptions, but in some examples ignorable attrition and stationarity of the attrition bias led to quite different conclusions.

We found that panel conditioning is an issue in knowledge questions but not in questions on attitudes, actual behavior, or expectations concerning the future. In one example, the bounding interval analysis showed that panel conditioning is significant even without making assumptions on the attrition process. In all knowledge questions, each of the additional assumptions led to a significantly positive panel conditioning effect, suggesting that answering the question once induces some people to increase their knowledge about the phenomenon in the question before taking part in the next survey. In principle, there might be a potential impact of the variation in time frames of our examples: In all examples with a significant panel conditioning effect, there were one or two years between waves, compared to a few months in many other examples. However, Kruse et al. (2009) found evidence of panel conditioning for a political knowledge question with a time frame of only eight weeks.

Extending the approach to nonbinary outcomes or to conditional distributions given time invariant covariates X like race, birth year, or gender is straightforward. Such extensions may also be useful because they require additional assumptions that are more plausible—assuming independence of attrition and health knowledge, for example, is less plausible than assuming conditional independence given age and education. We showed that extensions to transition rates (or other time varying regressors) are less straightforward, since transition probabilities are not point identified in the presence of either panel conditioning or attrition. The framework remains useful, but the bounds are less informative and additional assumptions on attrition are insufficient for point identification.

The conclusion that for most question types no panel conditioning is found seems reassuring. One reason may be that interviewer effects are excluded, since most of our examples are based upon data collected with an Internet panel. This is in line with the finding of Van der Zouwen and Van Tilburg (2001), who find that panel conditioning is mainly caused by interviewer behavior. Of course more evidence of this would be needed before a general conclusion can be drawn. For questions concerning knowledge, panel conditioning seems an issue that researchers should be aware of. Even without concerns about panel conditioning, refreshment samples were already shown to be useful tools to analyze selective attrition (Hirano et al. 2001), and this article shows their usefulness for analyzing panel conditioning. Thus, this article supports the conclusion that for survey designers, a solid and sizable refreshment sample may be as important as reducing attrition by another fraction of a percentage point.

Acknowledgements

The authors are very grateful to Ramon van den Akker and three anonymous reviewers for helpful comments. This paper uses data from SHARE wave 1 (release 2.0.1) and wave 2 (release 1.0.1). SHARE data collection in 2004-2007 was primarily funded by the European Commission through its 5th and 6th framework programmes (project numbers QLK6-CT-2001- 00360; RII-CT- 2006-062193; CIT5-CT-2005-028857). Additional funding by the US National Institute on Aging (grant numbers U01 AG09740-13S2; P01 AG005842; P01 AG08291; P30 AG12815; Y1-AG-4553-01; OGHA 04-064; R21 G025169) as well as by various national sources is gratefully acknowledged (see http://www.share-project.org for a full list of funding institutions).

Declaration of Conflicting Interests

The author(s) declared no conflicts of interest with respect to the authorship and/or publication of this article.

Funding

The author(s) received no financial support for the research and/or authorship of this article.

Notes

1. Fitzgerald, Gottschalk, and Moffitt (1998) and others refer to this as no selection on unobservables.
2. Standard errors for the estimates (point estimates or lower and upper bounds of the interval estimates) were calculated using the central limit theorem and the delta method (see, e.g., Greene 2003).
3. Imbens and Manski (2004) explain why this is not a formal test.
4. The alternative decomposition is presented in the online Appendix 1 (found at http://smr.sagepub.com/supplemental).

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