Panel Conditioning: Change in True Value versus Change in Self-Report

Ting Yan[1], Stephanie Eckman[2]
[1]Institute for Social Research, University of Michigan, 426 Thompson Street, Ann Arbor, MI, USA 48106
[2]Institute for Employment Research, Weddigenstr 20, Nuremberg, Germany 90478

Abstract
Panel conditioning is an important source of measurement error unique to panel surveys. It refers to the phenomenon where participation in repeated interviews changes respondents' true behaviour, attitudes, or knowledge, or their reporting of their true behaviour, attitudes, and knowledge. A major weakness of (and a challenge to) the existing research on panel conditioning is its inability to distinguish between true change and change in reporting behavior. Existing studies are heavily reliant on assumptions and models when studying panel conditioning because they have no external gold-standard data source. This paper examines panel conditioning effects using data from four waves of a large German panel survey on labour market outcomes (PASS). Because administrative data on employment and unemployment benefit receipt status are available for nearly all respondents, we are able to separate panel conditioning due to change in true status and panel conditioning due to change in self-report of the true status without depending on assumptions. Our results show that PASS respondents are more likely to change their true behavioural status the longer they stay in the panel. In addition, they are less likely to misreport their behavioural status the longer they stay in the panel.

Key Words: panel conditioning, measurement error, longitudinal surveys, administrative data, misreport

1. Introduction

Longitudinal surveys are an important source of information for social scientists to study social phenomena, to understand social processes, and to make causal inferences. Longitudinal surveys collect information from the same sample elements over several time periods, making it possible to track change at the sample element level over time. Compared to cross-sectional surveys, longitudinal surveys allow for better estimates of gross change, better estimates of stability (or instability), and identification of causality (Binder, 1998; Kasprzyk, Duncan, Kalton, and Singh, 1989; Kalton and Citro, 2000; Lynn, 2009). However, longitudinal surveys are susceptible to unique non-sampling errors. Panel conditioning – also known as time-in-sample bias, rotation group bias, or panel effect – is one such non-sampling error unique to longitudinal surveys (Kasprzyk et al., 1989; Sturgis, Allum, and Brunton-Smith, 2009; Warren and Halpern-Manners, 2012).

Panel conditioning refers to the phenomenon where participation in earlier waves of a longitudinal survey affects respondents' true behaviour, attitudes, or knowledge, or their reports of their behaviour, attitudes or knowledge (Neter and Waksberg, 1964). In other words, respondents' true status, or their report of the true status, or both, are "conditioned" by their participation in prior interviews. We refer to the first type as "conditioned change in true status" and to the second type as "conditioned reporting." An example of conditioned change in true status would be respondents who pay more attention to a political race because they are participating in a panel about voting and know they will be asked their opinions about the candidates. An example of conditioned reporting would be respondents who learn to say "no" in response to filter questions asking about recent household purchases to avoid burdensome follow up questions about those purchases.

Both aspects of conditioned changes are problematic when estimating change over time using longitudinal survey data. Conditioned reporting introduces measurement error at the individual level. By contrast, conditioned change in true status introduces measurement error at the estimate level. Both types can bias estimates made from the survey data, especially estimates of over-time changes.

Investigations of panel conditioning have been conducted as early as 1950s. However, the literature in this area lacks definitive conclusions on the presence, the direction, and the magnitude of panel conditioning effects in longitudinal surveys (Bailar, 1989; Sturgis et al., 2009). One contributing factor is the lack of a unified theory explaining the nature and the mechanism of panel conditioning effects in surveys (Sturgis et al., 2009; Warren et al., 2012). Three mechanisms have been proposed to account for panel conditioning. The *cognitive stimulation* account (and its variations) suggests that participation in one or more prior interviews causes respondents to think more about the topic of the survey. As a result of this reflection and deliberation, respondents with prior involvement in the survey gain more knowledge on the topic (Toepoel, Das, and van Soest, 2009), develop more crystallized attitudes (Sturgis et al., 2009), become more "politicized" (Waterton and Lievesly, 1989), and change their behaviour in line with the survey topic (Battaglia, Zell, and Ching, 1996; Clausen, 1969; Kraut and McConahay, 1973; Traugott and Katosh 1979; Yalch, 1976). This account hypothesizes that participation in the repeated interviews conditions respondents' true status.

Two other accounts focus on the effect of prior participation on response behaviour and reporting of the true status. According to the *better respondents* account, exposure to and experience with prior interviews allows sample respondents to have a better idea of the interviewing process and to become more comfortable about and more trusting of the interviewing process. As a result, they become better respondents in the later waves by providing more truthful reports and fewer "don't know" responses (Waterton and Lievesley 1989).

By contrast, the *worse respondents* account hypothesizes that prior participation could tip off sample respondents to how the interview is structured and how they can reduce the burden of answering the survey items. Consequently, respondents learn to say "no" to filter questions to reduce burden in the later waves (Bailar 1989; Mathiowetz and Lair, 1994; Shields and To, 2005; van der Zouwen and van Tilburg, 2001; Wang et al., 2000; Warren et al., 2012), to speed through the survey, and to satisfice by taking mental short-cuts in the response process (Toepoel, Das, and van Soest, 2008).

In principle, the three mechanisms can be tested empirically and conditioned change in true status and conditioned reporting are separable. But in practice, without validation data, it is impossible to attribute any change in self-reports over time to conditioned change in true status or to conditioned reporting. Existing studies on panel conditioning effects are heavily reliant on assumptions and models when evaluating panel conditioning effects. Furthermore, these studies tend to confound panel conditioning with panel attrition and reporting error (Warren et al., 2012).

This paper examines panel conditioning effects using data from four waves of a large German panel survey on labour market outcomes (PASS). For many respondents, administrative data on employment and unemployment benefit receipt status are available, and thus we are able to separate panel conditioning due to change in true status and panel conditioning due to change in reporting of the true status without depending on assumptions. As a result, we are able to test which of the three mechanisms are at work. We are not able to fully control for attrition bias, but plan to expand this work in the future to account for this.

We aim to address the following research questions:
1. How much of wave-to-wave change observed in PASS reflects change in true status and how much is due to misreporting?
2. As they participate in more waves of the survey, are respondents more likely to change their true status?
3. As they participate in more waves, are respondents more likely to misreport their true status?
4. Which of the three proposed mechanisms (cognitive stimulation, better respondents, worse respondents) can account for the observed panel conditioning effects?

2. Data and Analysis Methods

2.1 Data

The data used for this analysis are from four waves of the German Panel Study Labor Market and Social Security (PASS), a household panel survey for labour market, welfare receipt, and poverty research in Germany. It is commissioned by the Institute for Employment Research (IAB) under the mandate of the Department of Labour and Social Affairs. The study's primary purpose is to create a longitudinal dataset portraying the dynamics of households receiving a new welfare benefit scheme called Unemployment Benefit II (UBII).

PASS is a dual-frame mixed mode survey. The general population sample is drawn from a commercial database of residential addresses whereas the recipient sample is selected from the Federal Employment Agency's register list of UBII recipients. Both samples share the same PSUs, which are postal codes. The first wave of data collection was conducted between December 2006 and July 2007. Respondents are followed and interviewed annually. The survey consists of a household interview with one designated household member and then additional personal surveys with all members 15 years or older (Bethmann et al 2011).

The variable of interest concerns respondents' receipt of UBII. The question about is asked in each wave of the household respondent, because this benefit is provided to a "benefit unit" (essentially a household) and not to individuals.[1] The text of the question is:

> Thinking of the time since [LAST INTERVIEW], Have you or any other members of your household at any time obtained unemployment benefit 2 ("Arbeitslosengeld 2")?

In addition to survey data, supplementary administrative data on UBII receipt status and employment situations are also available. These data are generated whenever a household receives UBII payments from the government and for this reason are expected to have very little measurement error (Bender and Haas, 2002; Jacobebbinghaus and Seth, 2007) For those respondents who consented to data linkage, their data may be merged to administrative records on UBII receipt. In wave 3, 86% of respondents were successfully linked (Bethmann et al 2011).

Before each new wave of data collection, a refresher sample was drawn from the list of benefit recipients. As a result, at each wave of data collection, there are sample groups that have been interviewed for a different number of times. Shown in Table 1 are the numbers of completed interviews by wave and sample group. Recipient Sample 2, for example, was first interviewed in 2007/2008 and was interviewed twice later (in 2008/2009 or 2009/2010). Before the start of wave 4, Recipients Sample 1 and the general population sample have both been interviewed three times already and sample group 2 twice. Sample group 3 had their second interview at wave 4.

To control for attrition bias, we restricted our analyses to respondents who completed all interviews they were supposed to. For Recipient Sample 1, only those who completed all four waves were retained. For Recipient Sample 2, we kept respondents who completed all three interviews and for Recipient Sample 3, we restricted to those who participated in both waves.

Table 1: Number of Completed Interviews, and Percent Respondents Reporting UBII, by Wave and Sample Group

	Wave 1 (2006/2007)	Wave 2 (2007/2008)	Wave 3 (2008/2009)	Wave 4 (2009/2010)
Recipient Sample 1	1,547	1,547	1,547	1,547
	79%	72%	65%	62%
Recipient Sample 2		395	395	395
		66%	54%	48%
Recipient Sample 3			523	523
			63%	58%
General Population Sample	1,236	1,236	1,236	1,236
	11%	11%	10%	8.6%

2.2 Analysis Methods

To answer our research questions about panel conditioning in the PASS survey, we make use of the linked survey and administration data. At each wave, we have the household

[1] In rare cases, a household can contain more than one benefit unit. We drop cases that fall into this category because it is not clear how they would report UBII receipt.

respondents' reports of receipt of UBII as well as the receipt indicator for the date of the interview derived from the administrative data. We use two methods to break observed change in reported UBII status into true change and change in measurement error.

In the first method, the response to the UBII question given by respondent *i* in sample group *j* at the t^{th} interview (y_{ijt}) can be decomposed into three parts:

$$\begin{aligned} y_{ijt} &= Y_{ij1} + \alpha_{ijt} + \beta_{ijt} \qquad (1) \\ &= Y_{ij1} + (Y_{ijt} - Y_{ij1}) + (y_{ijt} - Y_{ijt}) \end{aligned}$$

In equation (1), Y_{ij1} is the true value for respondent *i* in sample group *j* at the first interview and α_{ijt} is change to the true value from the first to the t^{th} interview. β_{ijt} is the measurement error associated with being in sample for the t^{th} time: the non-random error in the reported values for respondent *i* in sample group *j* at the t^{th} interview.

Aggregating across all cases, the estimated proportion of respondents in sample group *j* who reported receiving UBII at the t^{th} interview has three terms, as shown in equation (2). The first term is the proportion of respondents in sample group *j* who received UBII at the 1^{st} wave. The second term is the change in the proportion receiving UBII between the 1^{st} interview and the t^{th} interview and the third term is the aggregate measurement bias at the t^{th} interview.

$$\overline{y_{jt}} = \overline{Y_{j1}} + (\overline{Y_{jt}} - \overline{Y_{j1}}) + (\overline{y_{jt}} - \overline{Y_{jt}}) \qquad (1)$$

The estimate of over-time change in the proportion reporting receipt of UBII between the t^{th} interview and the 1^{st} interview for sample group j reduces to the sum of three terms:

$$\begin{aligned} \overline{y_{Jt}} - \overline{y_{J1}} &= \left(\overline{Y_{J1}} + \left(\overline{Y_{Jt}} - \overline{Y_{J1}}\right) + \left(\overline{y_{Jt}} - \overline{Y_{Jt}}\right)\right) - \left(\overline{Y_{J1}} + \left(\overline{Y_{J1}} - \overline{Y_{J1}}\right) + \left(\overline{y_{J1}} - \overline{Y_{J1}}\right)\right) \\ &= \left(\overline{Y_{Jt}} - \overline{Y_{J1}}\right) + \left(\overline{y_{Jt}} - \overline{Y_{Jt}}\right) - \left(\overline{y_{J1}} - \overline{Y_{J1}}\right) \qquad (2) \end{aligned}$$

The first term in this sum is the change in the proportion truly receiving UBII between the 1^{st} interview and the t^{th} interview whereas the last two terms are measurement bias at the t^{th} interview and the 1^{st} interview separately. We can use this equation to separately analyse the two types of panel conditioning discussed above – conditioned change in true status (the first term) and conditioned changes in reporting (the last two terms).

In addition, we fit logistic regression models to examine the association between time-in-sample and the probability to change true UBII receipt status and to report the true status accurately. The dependent variable is whether a respondent's true receipt status changed from the first wave to the t^{th} wave and whether a respondent's report at the t^{th} wave was accurate. The independent variables are the time-in-sample for the respondent at each wave. With this model, we are able to test the significance of the relations found with the first method.

3. Results

3.1 Decomposing Observed Over-time Change

We first calculated over-time changes in the proportion of respondents receiving UBII using survey data (Column 1 in Table 2). We see that fewer people reported receiving

UBII in later waves. For instance, for sample group 1, 7.1 percentage points fewer UBII benefits receivers were reported in wave 2 than in wave 1. In other words, between wave 2 and wave 1, the proportion of respondents reporting having received UBII was reduced by 7.1 percentage points. The difference from wave 3 to wave 1 for this sample group is even bigger – a drop of 14.1 percentage points – and the change in the fourth wave is larger still. This downward trend in benefit receipt is in fact consistent with findings from other economic and labour surveys. For instance, the Current Population Survey (CPS) conducted by the Bureau of Labor Statistics in the United States also reported fewer respondents being unemployed in the second and subsequent waves of the survey (Halpern-Manners and Warren, 2012; also see Bailar, 1975). However, without validation data, we wouldn't necessarily know whether this reduction was caused by more people moving out of unemployment benefits (due to improvement in economic conditions, or due to conditioned change in true status) or by a change in reporting behavior (due to increasing or decreasing measurement error). Most analyses on panel conditioning use only survey data, and thus they cannot differentiate between the two types of conditioning and true change in the underlying social phenomenon – which is not panel conditioning at all but the phenomenon that panel surveys are designed to capture.

Using the supplementary administrative data we were able to decompose this over-time change estimate into true changes and changes in reporting. Table 2 displays the decomposition terms – true changes in column 2 and changes in reporting (or measurement error) in columns 3 and 4 based on equation 3.

Table 2: Decomposing Estimates of Over-time Change

Sample Group	Over-time Change	(1) Estimate of over-time change using survey data $(\overline{y_{Jt}} - \overline{y_{J1}})$	(2) Over-time change using administrative data $(\overline{Y_{jt}} - \overline{Y_{j1}})$	(3) Measurement bias at wave *t* $(\overline{y_{jt}} - \overline{Y_{jt}})$	(4) Measurement bias at wave 1 $(\overline{y_{j1}} - \overline{Y_{j1}})$
Recipient Sample 1	Wave 2 to Wave 1	-7.1%	-9.4%	-3.1%	
	Wave 3 to Wave 1	-14.1%	-17.5%	-2. 0%	-5.4%
	Wave 4 to wave 1	-17.8%	-20.6%	-2.5%	
Recipient Sample 2	Wave 3 to Wave 2	-11.9%	-14.7%	-3.8%	-6.6%
	Wave 4 to Wave 2	-17.8%	-20.0%	-4.3%	
Recipient Sample 3	Wave 4 to Wave 3	-5.5%	-9.8%	-3.1%	-7.3%
General Population Sample	Wave 2 to Wave 1	-0.2%	-0.6%	-0.6%	
	Wave 3 to Wave 1	-0.9%	-1.6%	-0.3%	-1.1%
	Wave 4 to wave 1	-2.4%	-2.3%	-1.1%	

Shown in column 2 of Table 2 are changes in true status for the responding households. Here we see the same downward trends – a reduction in the proportion of cases receiving UBII from wave 1 to the later waves – which confirms that the observed reports are not artifacts. In fact, we can now see that the downward observed changes in survey data underestimate the true changes over-time: the true change is larger (in absolute value) than the change in the reports. This pattern holds for all sample groups.

The effects of measurement bias in the reports on over-time estimates are displayed in the last two columns of Table 2. In all situations, measurement bias is negative, indicating that respondents under-reported UBII receipt and the survey data underestimated the true proportion of UBII recipients. The negative bias in wave estimates suggests the presence of social desirability bias in this item. In addition, the absolute magnitude of measurement bias falls in later waves compared to the first wave. For instance, respondents in sample group 1 underreported UBII receipt by 5.4 percentage points at wave 1 but the extent of underreporting was reduced to 3.1 percentage points at wave 2. The only exception is the general population sample who underreported UBII receipt by -1.1 percentage points in wave 1. The absolute size of measurement bias is reduced for estimates at waves 2 and 3, but came back to -1.1 percentage points at wave 4. Counter intuitively, the reduction of measurement bias in the subsequent waves hurt, instead of improving, estimates of over-time change. This is because, according to equation 3, the bias in the estimate of over-time change between two waves is the difference between the measurement bias associated with a later wave and the measurement bias at the first wave. As a result, the reduction in a later wave's bias (compared to an earlier wave's bias) results in a larger bias for the estimate of over-time change between the two waves. Nonetheless, the absolute size of measurement bias due to misreporting is small and contributed less to the observed over-time change estimates than did the change in the true value.

3.2 Modeling Effect of Time-in-sample on True Status Changes

Table 2 demonstrates that the observed reduction in the proportion of respondents reporting UBII receipt can largely be attributed to true changes in the UBII receipt status for PASS respondents, suggesting that respondents are more likely to move out of UBII receipt status the longer they stay in the panel. To formally test this association, we fit a logistic regression model (Model 1) with the dependent variable being whether or not a respondent's true status at wave *t* changed compared to his/her initial status at wave 1 and the independent variable being the number of times he/she was interviewed at wave *t*. As shown in the first row of Table 3, the likelihood that PASS respondents changed their true receipt status at a later wave increased the longer they stayed in the sample.

To further study the direction of the change in true status, we fit a multinomial regression model (Model 2) with the dependent variable being true status not changed (reference category), true status changed to "receiving UBII," and true status changed to "not receiving UBII." It is clear from table 3, that the impact of time-in-sample is significantly associated with the likelihood of respondents changing their true status to "not receiving UBII," but not with changing their status to "receiving UBII." Table 3 confirms the positive relationship with the number of times PASS respondents are interviewed and their likelihood to change their true status in later waves to "not receiving UBII."

Table 3: Effect of Time-in-sample on True Status Change

	Odds Ratio Estimates of "Time-in-sample" (p value)		
	Model 1	Model 2	
	True UBII Status Changed	True Status Changed to "Receiving UBII"	True Status Changed to "NOT Receiving UBII"
Time-in-Sample	1.23 (<0.001)	1.06 (0.31)	1.28 (<0.001)
n	9662	9662	

Note: Estimates of constant terms not displayed

We take Table 3 as a piece of evidence supporting the 'cognitive stimulation' account. The PASS questionnaire is about labor and employment, a salient topic to most people. The questionnaire also asks about various types of assistance offered to unemployment benefit recipients (such as jobs, free consultation, and test to determine qualifications for certain jobs and so on). It is possible that the salience of the topic of the survey caused respondents to reflect on their situation and motivated them to change their situation. It is also possible that questions about the assistance programs available to them made the respondents aware of the existence of these forms of assistance and motivated them to make use of them.

Of course, another possibility is that a changing economic climate that moved people off UBII and into employment regardless of their participation in the PASS survey. In future work we plan to address this argument by benchmarking our results against movements out of UBII by similar households that were not selected for the survey.

3.3 Effect of Time-in-Sample on Misreporting

Table 2 also shows that measurement bias at later waves was smaller. To formally test the association between time-in-sample and respondents' probability to report accurately on UBII receipt, we fitted the same sets of logistic regression models. For Model 1, the dependent variable is whether the respondents misreported their true status or not. For Model 2 (the multinomial regression model), the dependent variable is a 3-level variable indicating that respondents accurately reported their status (the reference category), that respondents provided a false positive report, and that respondents provided a false negative report.

Shown in Table 4, the longer respondents stayed in the panel, the less likely they were to misreport their true UBII receipt status. Furthermore, there was a significant negative relationship between the number of times PASS respondents were interviewed at wave t and their likelihood to provide a false negative report of their true status at wave t. By contrast, there was no relationship between the number of times PASS respondents were interviewed and their likelihood to provide a false positive report.

Table 4: Effect of Time-in-sample on Accuracy of Self-reports

	Odds Ratio Estimates of "Time-in-sample" (p value)		
	Model 1	Model 2	
	Misreported True Status	Provided a False Positive Report	Provided a False Negative Report
Time-in-Sample	1.21 (<0.001)	0.91 (0.104)	0.79 (<0.001)
n	13,363	13,363	13,363

Note: Estimates of constant terms not displayed

We take Table 4 as a piece of empirical evidence supporting the 'better respondents' account. Prior participation in the survey allowed respondents to have a better understanding of the interviewing process, to become more comfortable about and more trustful of the survey sponsor and interviewers. They've learnt to answer more truthfully about their unemployment benefit receipt status.

4. Discussion

Without validation data, estimates of over-time change using longitudinal data rely only on respondents' self-reports. However, over-time changes estimated this way do not provide information on how much of the estimated changes reflect true changes and how much of the over-time change in the estimates is caused by measurement error. Taking advantage of the availability of supplementary administrative data matched to a German panel survey, we decomposed the estimates of over-time changes and separated true changes from artificial changes due to measurement error. We've found that, for PASS, the observed over-time underestimated true over-time changes by 2 to 4 percentage points for the recipient samples and less than 1 percentage point for the general population sample. Furthermore, measurement bias in wave estimates caused by misreporting the true status is small in magnitude in general and is reduced in later waves. Therefore, the observed over-time changes reflected mostly true changes. Measurement error is of lesser concern.

We also found that, the longer PASS respondents stayed in the panel and the more interviews they did, the more likely they were to change their true UBII receipt status by moving off unemployment benefit, lending support to the "cognitive stimulation" account. In addition, the longer PASS respondents stayed in the panel and the more interviews they did, the more likely they were to accurately report their UBII receipt status. Specifically, the more interviews they did, the less likely they were to provide a negative false report of their UBII receipt status, supporting the "better respondents" mechanism.

This paper demonstrated that participation in repeated interviews changed both respondents' true status and the reporting of their true status. Conditioned changes in reporting, in this case, lead to more accurate reporting and, thus, data of better quality and better estimates of means and/or totals at each wave. However, the reduction in measurement bias in later waves doesn't necessarily improve estimates of over-time changes which draw upon data from two waves. As a matter of fact, it hurts the change estimates (see equation 3 and empirical evidence in Table 2). Therefore, we'd like to caution survey organizations to pay attention to the first round of a longitudinal interview and to try hard to reduce measurement error at the first interview.

Conditioned changes in true status are more problematic because it makes the panel less representative of the target population: those who are selected and who participate in the panel study are changed by their participation and thus no longer represent the target population of unselected persons. A rotation panel design could help to address this problem, by bringing in new respondents each wave, but implementation of such a design is expensive and may not be feasible in all situations. However, we strongly encourage survey organizations conducting longitudinal surveys to draw a refresher sample from time to time. The addition of a refresher sample makes it possible to examine panel conditioning effects.

Acknowledgements

We'd like to thank Mark Trappmann and Johannes Eggs of the IAB for providing the data and helping us to make sense of it. We'd also like to thank Frauke Kreuter for encouraging us to pursue this research and for providing great comments and suggestions along the way.

References

Bailar, B. A. (1989). "Information Needs, Surveys, and Measurement Errors." In *Panel Surveys*, edited by D. Kasprzyk, G. J. Duncan, G. Kalton, and M. P. Singh, pp. 1-24. New York: Wiley.

Battaglia, M. P., Zell, Z., and Ching, P. (1996). "Can Participating in a Panel sample Introduce Bias into Trend Estimates?" In *JSM Proceedings*, Survey Research Methods Section. Alexandria, VA: American Statistical Association. 1010-1013.

Bender, S., and Haas, A. (2002). "Die IAB-Beschäftigtenstichprobe." IAB-Kompendium Arbeitsmarkt- und Berufsforschung. (Beiträge zur Arbeitsmarks- und Berufsforschung, BeitrAB 250), 3-12.

Bethmann, A., and Gebhardt, D. (2011). "User Guide: Panel Study Labour Market and Social Security (PASS) Wave 3." Institute for Employment Research, Nuremberg.

Binder, D. A. (1998). "Longitudinal Surveys: Why Are These Surveys Different From All Other Surveys?" *Survey Methodology*, 24, 101-108.

Clausen, A. R. (1969). "Response Validity: Vote Report." *Public Opinion Quarterly*, 37, 398-406.

Jacobebbinghaus, P., and Seth, S. (2007). "The German Integrated Employment Biographies Sample IEBS." Schmollers Jahrbuch. *Zeitschrift fuer Wirtschafts-und Sozialwissenschaften*, 127, 335-342.

Kalton, G., and Citro, C. F. (2000). "Panel Surveys: Adding the Fourth Dimension." In *Researching Social and Economic Change*, edited by D. Rose. Pp. 36-53. London:Routledge.

Kasprzyk, D., Duncan, G., Kalton, G., and Singh, M. P. (Eds.) (1989). *Panel Surveys*. Wiley: New York.

Kraut, R. E. and McConahay, J. B. (1973). "How Being Interviewed Affects Voting: An Experiment." *The Public Opinion Quarterly* 37, 398-406.

Lynn, P. (Ed.) (2009*). Methodology of Longitudinal Surveys*. Wiley.

Mathiowetz, N. A. and Lair, T. J. (1994). "Getting Better? Change or Error in the Measurement of Functional Limitations." *Journal of Economic and Social Measurement* 20, 237-262.

Neter, J., and Waksberg, J. (1964). "Conditioning Effects From Repeated Household Interviews." *Journal of Marketing*, 28, 51-56.

Shields, J, and To, N. (2005). "Learning to Say No: Conditioned Underreporting in an Expenditure Survey. In *JSM Proceedings,* Survey Research Methods Section. Alexandria, VA: American Statistical Association. 3963-3968.

Sturgis, P., Allum, N., and Brunton-Smith, I. (2009). "Attitudes Over Time: The Psychology of Panel Conditioning." In *Methodology of Longitudinal Surveys*, edited by P. Lynn. Pp. 113-126. New York: Wiley.

Toepoel, V., Das, M., and van Soest, A. (2009). "Relating Question Type to Panel Conditioning: Comparing Trained and Fresh Respondents." *Survey Research Methods*, 2, 73-80.

Traugott, M. W. and Katosh, J. P. (1979). "Response Validity in Surveys of Voting-Behavior." *Public Opinion Quarterly* 43, 359-377.

van der Zouwen, J. and van Tilburg, T. (2001). "Reactivity in Panel Studies and its Consequences for Testing Causal Hypotheses." *Sociological Methods & Research,* 30, 35-56.

Wang, K., Cantor, D., and Safir, A. (2000). "Panel Conditioning in a Random Digit Dial Survey." In *JSM Proceedings,* Survey Research Methods Section. Alexandria, VA: American Statistical Association. 822-827.

Warren, J. R., and Halpern-Manners, A. (2012). "Panel Conditioning in Longitudinal Social Surveys." *Sociological Methods & Research. Forthcoming.*

Waterton, J. and Lievesley, D. (1989). "Evidence of Conditioning Effects in the British Social Attitudes Panel." In *Panel Surveys* edited by D. Kasprzyk, G. J. Duncan, G. Kalton & M. P. Singh. Pp. 319-339. New York: John Wiley & Sons.

Yalch, R. F. (1976). 'Pre-election Interview Effects on Voter Turnout." *Public Opinion Quarterly*, 40, 331-336.