

Panel Conditioning in the General Social Survey*

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July 8, 2013

DRAFT: PLEASE DO NOT CITE OR QUOTE WITHOUT THE AUTHORS' PERMISSION

*The National Science Foundation (SES-0647710) and the University of Minnesota's Life Course Center, Department of Sociology, College of Liberal Arts, and Minnesota Population Center have all provided support for this project. We would like to thank Eric Grodsky, Michael Davern, Phyllis Moen, Chris Uggen, and Scott Long for their helpful comments and suggestions. Any errors, however, are solely our responsibility. Please direct correspondence to Andrew Halpern-Manners, Department of Sociology, Indiana University, 744 Ballantine Hall, 1020 E Kirkwood Avenue, Bloomington, IN 47405. Email: ahm@indiana.edu.

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Abstract

Does participation in one wave of a survey have an effect on respondents' answers to questions in subsequent waves? In this article, we investigate the presence and magnitude of "panel conditioning" effects in one of the most frequently used data sets in the social sciences: the General Social Survey (GSS). Using longitudinal records from the 2006, 2008, and 2010 surveys, we find evidence that at least some GSS items suffer from this form of bias. To rule out the possibility of contamination due to selective attrition and/or unobserved heterogeneity, we strategically exploit a series of between-person comparisons across time-in-survey groups. This methodology, which can be implemented whenever researchers have access to at least three waves of rotating panel data, is described in some detail so as to facilitate future applications in data sets with similar design elements.

Panel Conditioning in the General Social Survey

Sociologists have long recognized that longitudinal surveys are uniquely valuable for making causal assertions and for studying change over time. Scholars have also long been aware of the many special challenges that accompany the use of such surveys: they are more expensive to administer, they raise greater data disclosure concerns, and they suffer from additional forms of non-response bias. Nevertheless, researchers have generally been content to assume that longitudinal surveys do *not* suffer from the sorts of “testing” or “reactivity” biases that sometimes arise in the context of experimental or intervention-based research. The implicit assumption is that answering questions in one round of a survey in no way alters respondents’ reports in later waves. If this assumption is false, scholars risk mischaracterizing the existence, magnitude, and correlates of changes across survey waves in respondents’ attitudes and behaviors.

In this article, we investigate the presence and magnitude of “panel conditioning” effects in the General Social Survey (GSS).¹ The GSS is a foundational data resource in the social sciences, surpassed by only the U.S. Census and the Current Population Survey in terms of overall use (Smith 2008). In 2006, the survey made the transition from a replicating cross-sectional design to a design that uses rotating panels. Respondents are now asked to participate in up to three waves of survey interviews, with an identical set of core items appearing in each wave.

¹ We use the term “panel conditioning” synonymously with what has been called, among other things, “time-in-survey effects” (Corder and Horvitz 1989), “mere measurement effects” (Godin, Sheeran, Conner, and Germain 2008), “question-behavior effects” (Spangenberg, Greenwald, and Sprott 2008), and “self-erasing errors of prediction” (Sherman 1980).

The core GSS questionnaire touches on a variety of social and political issues, including abortion, intergroup tolerance, crime and punishment, government spending, social mobility, civil liberties, religion, and women's rights (to name just a few). Basic socio-demographic information is also collected from each respondent at the time of their interview and then re-collected in subsequent rounds.

Our primary objective is to determine whether panel conditioning influences the overall quality of these data.² Along the way, we provide a useful methodological framework that can be used to identify panel conditioning effects in other commonly-used data sets. Simply comparing response patterns across individuals who have and have not participated in previous waves of a survey is a good first step, but more sophisticated techniques are needed to convincingly differentiate between panel conditioning and biases introduced by panel attrition (Das, Toepoel, and van Soest 2011; Warren and Halpern-Manners 2012). As we describe in more detail below, our approach (which can be implemented in any longitudinal data set that contains at least three waves of overlapping panel data) resolves this issue by strategically exploiting between-person comparisons across rotation groups.

We believe that this is an important contribution to the emerging literature on panel conditioning effects in social science surveys. Most prior research on this subject, including our own, has focused on the incidence and magnitude of panel conditioning using a narrow subset of

² Researchers whose analysis only includes first-time GSS respondents (or who are only analyzing data that were collected prior to 2008) do not need to worry about panel conditioning effects.

attitudinal or behavioral measures (e.g., employment status *or* life satisfaction). These analyses have tended to use weaker methods to measure panel conditioning effects and have rarely considered the prevalence of the problem across topical domains. In this article, we offer a *general assessment* of panel conditioning in an omnibus survey that is heavily used by social scientists for a wide variety of research purposes. Our results should be valuable to users of the GSS and to researchers who are interested in identifying panel conditioning effects in other data sets that also include an overlapping panel component.

The remainder of this paper is organized into four main sections. In the section that follows, we summarize the literature on panel conditioning and provide a theoretical rationale for examining the issue within the context of the GSS. Next, we describe the methodology we use to identify panel conditioning effects. This discussion is meant to be non-technical so as to facilitate future applications in data sets with similar design elements. In the third section, we present our main findings and then subject these findings to a falsification test. Finally, we conclude by discussing the implications of our research for scholars who work with the GSS, as well as other sources of longitudinal social science data.

Panel Conditioning and the GSS

When does survey participation change respondents' actual attitudes and behaviors? When does survey participation change merely the quality of their reports about those attitudes and behaviors? Elsewhere, we have developed seven theoretically-motivated hypotheses about the circumstances in which panel conditioning effects are most likely to occur (Warren and Halpern-

Manners 2012).³ These hypotheses are grounded in theoretical perspectives on the cognitive processes that underlie attitude formation and change, decision-making, and the relationship between attitudes and behaviors (see, e.g., Feldman and Lynch 1988). In short, responding to a survey question is a cognitively and socially complex process that may or may not leave the respondent unchanged and/or equally able to provide accurate information when re-interviewed in subsequent waves. Five of these hypotheses suggest that panel conditioning effects could potentially arise within the context of the GSS.

First, respondents' attributes may at least appear to change across waves when items (like many of those featured on the GSS) require them to provide socially non-normative or undesirable responses (Torche, Warren, Halpern-Manners, and Valenzuela 2012). The experience of answering survey questions can force respondents to confront the fact that their attitudes, behaviors, or statuses conflict with what mainstream society regards as normative or appropriate (Schaeffer 2000; Toh, Lee, and Hu 2006; Tourangeau, Rips, and Rasinski 2000).⁴ Some respondents may react by bringing their *actual* attitudes or behaviors into closer conformity with social norms. Others may simply avoid cognitive dissonance and the embarrassment associated with offering non-normative responses by bringing their *answers* into

³ Similar hypotheses can be found in reviews by Cantor (2008), Sturgis et al. (2009), and Waterton and Lievesley (1989).

⁴ Examples from the GSS include questions that deal with respondents' racial attitudes, their history of substance use, their sexuality, their past criminal behavior, and their fidelity to their spouse or partner.

closer conformity with what they perceive as socially desirable.⁵ In both cases, the end result would be the same: researchers would observe changes over time in respondents' attributes that would not have occurred had the initial interview not taken place.

Second, respondents' attributes may appear to change across waves as they attempt to manipulate the survey instrument in order to minimize their burden (see, e.g., Bailar 1989). Respondents sometimes find surveys to be tedious, cognitively demanding, and/or undesirably lengthy (Krosnick 1991; Krosnick, Holbrook, Berent, Carson, Hanemann, Kopp, Mitchell, Presser, Ruud, Smith, Moody, Green, and Conaway 2002; Tourangeau, Rips, and Rasinski 2000). To get around these hassles, respondents in longitudinal studies may learn how to direct or manipulate the survey experience in such a way that minimizes the overall amount of time or energy that they have to devote to it (Duan, Alegria, Canino, McGuire, and Takeuchi 2007; Wang, Cantor, and Safir 2000).⁶ In the GSS, for example, a respondent may learn during their first interview that they are asked to provide many additional details about their job characteristics and work life. In order to reduce the duration of follow-up surveys, some

⁵ It is important to distinguish these sorts of changes from social desirability bias. In some cases, the *mere thought* of providing a non-normative answer may cause respondents to alter the way that they characterize themselves on a baseline survey and in all subsequent interviews (Tourangeau and Yan 2007). In other cases, the *experience* of admitting to something that is socially undesirable may change the way respondents describe themselves in later waves—because of the feelings of embarrassment or shame that the initial interview provoked. Although both of these things could be happening at the same time within the same survey, our focus in this article is only on the latter problem. For more information about the former problem, the interested reader should see Schaeffer (2000) and Tourangeau and Yan (2007).

⁶ This question answering strategy can be thought of as a very strong form of *satisficing*. Not only are respondents seeking to provide “merely satisfactory answers” (Krosnick 1991), they are also deliberately seeking to avoid additional follow-up questions.

respondents may subsequently report that they are out of the labor force or unemployed.⁷ The result would be the appearance of change across waves when no change has actually occurred.

Third, as hypothesized by Waterton and Lievesley (1989:324), it is possible that some respondents change their answers to survey questions as they gain an “improved understanding of the rules that govern the interview process.” When first interviewed, participants in the GSS may not have had full access to the information requested from them, may not have known how to make use of various response options, or may not have known how or when to ask clarifying questions. Upon re-interview, these individuals may be better prepared and more cognizant of “how surveys work.” While this may translate into undesirable manipulation of the survey instrument, as posited above, it may also lead to more accurate and complete responses over time. This would again result in the appearance of change over time when respondents’ underlying attributes remain the same (see, e.g., Mathiowetz and Lair 1994; Sturgis, Allum, and Brunton-Smith 2009).

Fourth, respondents may become more comfortable with and trusting of the survey experience after being exposed to the survey process and interviewers (van der Zouwen and van Tilburg 2001). Survey methodologists have found that respondents’ judgments about the relative benefits and risks associated with answering survey questions are significantly related to the

⁷ This sort of “burden avoidance” behavior can also occur within the context of a cross-sectional survey if respondents learn, through repetition, that certain types of answers lead to additional items (see, e.g., Kessler, Wittchen, Abelson, McGonagle, Schwarz, Kendler, and Knäuper 1998; Kreuter, McCulloch, Presser, and Tourangeau 2011). We thank an anonymous reviewer for pointing this out.

chances that they provide complete and accurate answers (Dillman 2000; Krumpal 2013; Rasinski, Willis, Baldwin, Yeh, and Lee 1999; Willis, Sirken, and Nathan 1994). As respondents become more familiar with and trusting of the survey process and with interviewers and interviewing organizations, they may become less suspicious and their confidence in the confidentiality of their responses may grow. Participating in the GSS may provide evidence about the survey's harmless nature, reduce suspicion, or increase respondents' comfort level. Any of these effects could lead to changes in respondents' reported attitudes or behaviors.

Finally, respondents' answers to factual questions may change over time as they acquire more and better information about the topic at hand (Toepoel, Das, and van Soest 2009). After an initial interview, respondents may "follow-up" on unfamiliar items by consulting external sources and/or people who are knowledgeable in the area. In this scenario, prior questions serve as stimuli for obtaining the type of information that is needed to give correct responses in later waves. In many cases, it may not even be necessary that respondents remember that they encountered the item during a previous interview. As Cantor (2008:136) points out, all that matters is that "the process of answering the question the first time changes what is eventually accessible in memory the next time the question is asked." The GSS includes a number of "knowledge tests" that may be especially prone to this form of panel conditioning.

Unfortunately, these hypotheses have not been well-validated using the sorts of data sets social scientists typically rely on. One consequence of this is that we know very little about the

nature and magnitude of panel conditioning in important data resources like the GSS.⁸ Whereas most large-scale surveys provide users with methodological documentation about issues like sampling, attrition, and missing data, we know of none that routinely provides information about panel conditioning based on strong methods for understanding such biases. In the short run, we hope that our empirical estimates of panel conditioning in the GSS will improve the scholarship that is based on analyses of these data. In the longer run, we intend for our research design to serve as a methodological model for assessing panel conditioning in surveys like the GSS that employ rotating panel designs.⁹

Data and research design

The GSS is a large, full-probability survey of non-institutionalized adults in the United States. It has been administered annually (1972-1993) or biennially (1994 onward) since 1972 by NORC at the University of Chicago. In 2006, the GSS switched from a cross-sectional design to a rotating panel format. Under the new setup, subsets of about 2,000 respondents are randomly selected in each wave for re-interview two and four years later. The longitudinal panel that began the GSS in 2006 was re-interviewed in 2008 and 2010; the panel that began in 2008 was re-interviewed in

⁸ We know of two previous analyses that have examined panel conditioning effects in the GSS (Smith and Son 2010; Warren and Halpern-Manners 2012). Both focused on a fairly narrow subset of survey items ($n < 25$), and neither ruled out alternative explanations for the observed results (including selective attrition, random measurement error, and social desirability bias). The present article represents an improvement on both fronts.

⁹ Other widely-used, nationally representative surveys that employ a rotating panel design include the Current Population Survey and the Survey of Income and Program Participation. Panel conditioning effects have been assessed in both of these surveys (Bailar 1975; Halpern-Manners and Warren 2012; McCormick, Butler, and Singh 1992; Solon 1986), but only for a very select subset of items.

2010 and will be re-interviewed again in 2012. As described below, our focus is on responses to the 2008 survey by two groups of individuals: those who were interviewed for the first time in 2006 (or Cohort A) and those who were interviewed for the first time in 2008 (or Cohort B).

At first glance, it might seem that the easiest way to identify panel conditioning effects in these data would be to compare the responses given by individuals who were new to the survey in 2008 (Cohort B) to those given by individuals who first participated in 2006 (Cohort A). The problem with this approach is its inability to distinguish the effects of panel *conditioning* from the effects of panel *attrition*. Whereas the new rotation group may be representative of the original target population (i.e., non-institutionalized adults living in the United States at the time of the 2008 survey), the 2006 cohort may have suffered from non-random attrition between the 2006 and 2008 waves. Unless credible steps are taken to adjust for the resulting panel selectivity, differences in responses between cohorts cannot be clearly attributed to panel conditioning (Halpern-Manners and Warren 2012).

Various methodologies have been proposed to deal with this issue (see, e.g., Das, Toepoel, and van Soest 2011; Warren and Halpern-Manners 2012). One of the most common involves the use post-stratification weights (Clinton 2001; Nukulki, Hadfield, Subias, and Lewis 2007). Under this approach, attrition is assumed to be random conditional on a pre-determined set of observable characteristics, which are then used to generate weights that correct for discrepancies between different cohorts of respondents. As others have pointed out, the overall effectiveness of this technique depends entirely on whether or not assumptions concerning “ignorability” are met

(Das, Toepoel, and van Soest 2011; Sturgis, Allum, and Brunton-Smith 2009; Warren and Halpern-Manners 2012). If the two cohorts under consideration (i.e., the 2006 and 2008 cohorts) differ in ways that are not easily captured by the variables used to construct the weights, contamination due to panel attrition cannot be ruled out.

One way around this problem is to “pre-select” individuals that have the same underlying propensity to persist in the sample. Consider, for example, Cohorts A and B as defined above. These groups of respondents began the GSS in 2006 and 2008, respectively. If we systematically select individuals from both cohorts who participated in *at least the first two waves of survey interviews*, and then examine their responses in 2008, we can accurately identify the effects of panel conditioning in that year. Both sets of respondents experienced the same social and economic conditions at the time of their interview in 2008, and both exhibited the same propensity to persist in (or attrite from) the GSS panel (because both participated in the same number of waves).¹⁰ The key difference between the groups is that members of the 2006 cohort were experienced GSS respondents in 2008 and members of 2008 cohort were not.¹¹

This is the approach that we use in our analysis. Using panel data from the 2006, 2008, and 2010 waves of the GSS, we were able to identify 3,117 respondents who completed at least the

¹⁰ Both sets of respondents were probably also subject to similar levels of non-response bias, although this is not something that we can verify using available data.

¹¹ The age distribution of respondents will vary slightly between cohorts because the treatment group has aged two years since their initial interview (and thus cannot be 18 or 19 years old), whereas the control group has not. In supplementary analyses, we truncated the age distribution so that *all* respondents were above the age of 20 in 2008 and then recalculated our estimates. The results were substantively identical and are available from the first author upon request.

first two rounds of survey interviews.¹² Of these respondents, 1,536 entered the sample in 2006 (the 2006 cohort) and 1,581 entered the sample in 2008 (the 2008 cohort). If the responses given by individuals in the first group are significantly different than the responses given (in the same year) by individuals in the second, we can infer that these differences came about from panel conditioning. No adjustments for panel attrition are necessary and person weights are not needed to correct for sub-sampling and/or non-response.¹³ By design, the 2006 and 2008 cohorts have already been equated on both observed *and* unobserved characteristics.¹⁴

As noted above, items on the GSS span a wide variety of substantive topics (Smith, Kim, Koch, and Park 2007). Although theory suggests that some of these topics may be more or less prone to panel conditioning effects, we feel it is important (for the sake of completeness) to examine every instance in which such biases could possibly occur. For this reason, we considered

¹² We only use the 2010 data for the purposes of sample selection; we do not actually analyze respondents' answers from that wave of the survey.

¹³ Random sampling in two different years (e.g., 2006 and 2008) does not guarantee the same population characteristics when the composition of the population changes gradually over time. To confirm that the differences we attribute to panel conditioning are not due to slight compositional changes that occurred between 2006 and 2008, we fit a series of auxiliary models that included controls for various socio-demographic characteristics (i.e., age, gender, householder status, and race/ethnicity). Our conclusions with respect to panel conditioning were robust to the inclusion of these variables.

¹⁴ This approach would provide invalid results if there is an important attrition-by-cohort interaction. Even if members of the 2006 and 2008 cohorts were equally likely to leave the sample, it may still be the case that attriters from these cohorts differ with respect to socioeconomic, demographic, or other attributes that might predict responses to the survey items we consider. To explore this possibility, we pooled our data files and ran a regression model predicting attrition. For independent variables, we included indicators of the respondent's age, gender, socioeconomic status, race/ethnicity, region of residence, marital status, party affiliation, household size, happiness, and health. We then created interactions between these measures and the respondent's cohort. None of these interactions were significant at the $p < 0.05$ level. This provides reassurance that the process generating attrition was similar across groups.

all 2008 GSS variables that met two very basic requirements: (1) the item had to be answered by the respondent and not the survey interviewer; and (2) the variable in question had to be empirically distinct from other measures in our analysis. The first rule meant that items like “date of interview” and “sex of interviewer” were excluded from the study. The second rule meant that we considered variables like “age” and “year of birth,” but not both.¹⁵

After eliminating items that did not satisfy these criteria, we were left with a total of 310 variables. To analyze panel conditioning effects in each of these measures, we carried out hypothesis tests comparing the response patterns in 2008 across cohorts. For continuous measures we used *t*-tests to compare group means; for categorical measures we used chi-square tests (if all cell sizes were in excess of 5) and Fisher’s exact tests (if they were not).¹⁶ Because the GSS employs a split-ballot design, where certain items are only asked of certain individuals in a given year, members of the 2006 cohort did not necessarily receive the “treatment” for all variables in our sample.¹⁷ Such cases were removed from the analysis using pairwise deletion. See Appendix Table A1 for complete information on all measures, including sample sizes disaggregated by treatment status.

Results

¹⁵ A third stipulation is that the variables under consideration had to appear on the 2006 *and* 2008 waves of the survey. For the most part, this limits our analysis to items that belong to the GSS’s replicating core.

¹⁶ In very rare instances ($n = 7$), results for a Fisher’s exact test could not be obtained for computational reasons. In these cases, we consolidated response categories to reduce data sparseness and then carried out chi-square tests instead.

¹⁷ Core GSS items appeared in the same order for both cohorts of respondents in 2008.

Our analysis includes significance tests for 310 different items; this makes it extremely susceptible to multiple comparison problems. Even if the null hypothesis (of no panel conditioning) is true for every item in our data set, the probability of finding at least one statistically significant effect just by chance is $1 - (1 - 0.05)^{310} \approx 1$, assuming a standard α -level of 0.05. To address this issue, we examined the *distribution* of test statistics across all items in our sample. Under the null, the p -values obtained from our tests should be uniformly distributed between 0 and 1 (Casella and Berger 2001). Approximately 5% of the test statistics should be below 0.05, another 5% should fall between 0.05 and 0.09, and so on throughout the entire $[0, 1]$ interval. Depending on where they occur in the distribution, departures from this pattern could indicate an over-abundance of significant results.

Figure 1 gives a visual summary of the main findings. In the panel on the left, we provide a simple histogram of the p -values we obtained from our comparisons of the 2006 and 2008 cohorts. In the panel on the right, we provide a quantile-quantile (Q-Q) plot comparing the empirical distribution of these values (as indicated by the black circles) to a theoretical null distribution (as indicated by the red line).¹⁸ In both instances, there is clear clustering of

¹⁸ Q-Q plots are widely used in genetics research to visualize results from large numbers of hypothesis tests (see, e.g., Pearson and Manolio 2008). To draw the plot, we rank-ordered the p -values ($n = 1, \dots, 310$) from smallest to largest and then graphed them against the values that would have been expected had they been sampled from a uniform distribution. As noted above, the red line indicates the expectation under the null and the black circles represent the actual results. Following convention, we show the relevant test statistics as the $-\log_{10}$ of the p -value, so that an observed $p = .01$ is plotted as “2” on the y -axis and $p = 10^{-5}$ as “5.”

estimates in the extreme low end of the distribution.¹⁹ Overall, 63 of the 310 tests that we conducted were significant at a 0.10 level (whereas 31 would be expected by chance); 37 were significant at a 0.05 level (whereas 16 would be expected by chance); and 22 were significant at a 0.01 level (whereas 3 would be expected by chance). We take this as evidence that panel conditioning exists in the GSS among certain subsets of items.

In order to confirm this interpretation, we calculated p -values that have been adjusted for the False Discovery Rate (FDR) using the algorithm of Benjamini and Hochberg (1995). Many techniques exist for dealing with multiple comparison problems and there is some debate over which is the most appropriate (Gelman, Hill, and Yajmia 2012). The FDR is generally thought to be more powerful than Bonferroni-style procedures, and is frequently used when the volume of tests is high. Instead of controlling for the chances of making even a single Type 1 error, the FDR controls for the expected *proportion* of Type 1 errors among all significant results. In total, the FDR-adjusted estimates include 8 significant results at the $p < 0.05$ level and 19 significant results at the $p < 0.10$ level (see Appendix Table A1). If we set the FDR threshold to 5%, we can say with confidence that only 1 of these “discoveries” occurred by chance.

The direction and magnitude of panel conditioning effects

These results suggest that some people may respond differently to GSS questions depending on whether or not they have previously participated in the survey. Although this is an important

¹⁹ The null hypothesis that the observed values are uniformly distributed was easily rejected using a Kolmogorov-Smirnov test ($D = 0.18, p < .0001$).

finding in its own right, users of these data should also be interested in knowing which variables are subject to panel conditioning, in what direction the observed effects operate, and how big they are from a substantive standpoint. In this section, we describe the direction and magnitude of panel conditioning biases in the 2008 survey and provide some preliminary thoughts about possible mechanisms. To be appropriately conservative when interpreting the results for individual variables, we focus on items that (with a few exceptions) produced FDR-adjusted p -values < 0.10 . The exceptions to this rule are noted in the text below.

First, members of the 2006 and 2008 cohorts sometimes differed in their responses to attitudinal questions about “hot-button” issues. Examples include items dealing with pre-marital sex (*premarsx*), first amendment rights and racism (*spkrac*), and governmental aid to minorities (*natracey*). As indicated in Table 1, members of the 2006 cohort were 14% more likely to say that sex before marriage is always or almost always wrong; 10% more likely to say that people have a right to make hateful speeches in public; and 23% more likely to say that current levels of assistance for African Americans are neither too high nor too low. These effect sizes are generally in line with estimates that have been produced in past panel conditioning research (see, e.g., Torche et al. 2012).

Second, panel conditioning effects emerged in several questions related to household composition. These include items dealing with the respondent’s relationship to the household head (members of the 2006 cohort were more likely to be the head or spouse), the number of adults present (members of the 2006 cohort reported more adults), the number of visitors

present (members of the 2006 cohort reported more visitors), and the number of family generations that live with the respondent (members of the 2006 cohort reported more generations). The fact that experienced GSS respondents reported higher numbers in all of these cases may be related to our hypothesis concerning survey skill and/or trust. After completing the survey for the first time, respondents may become more willing to open up, to report on more people, or to ask follow-up questions about who qualifies as living in their household.²⁰

Third, members of the 2006 and 2008 cohorts frequently differed in their responses to questions about demographic and economic attributes. Respondents with prior survey experience were 20% more likely to be divorced or widowed, 11% more likely to be upwardly mobile relative to their parents, and 31% less likely to refuse to answer questions about their personal income. Although we cannot provide definitive tests, these patterns could also be attributable to differences in respondents' trust. As we discussed earlier, being interviewed repeatedly may make the interview process seem less threatening to the respondent, which could decrease their need to give guarded and/or socially desirable responses in the follow-up wave (van der Zouwen and van Tilburg 2001). That this would occur for potentially sensitive items like those listed above makes good theoretical sense.²¹

Finally, we found large and consistent differences between groups with respect to their

²⁰ These variables are not good candidates for “burden” effects because respondents receive very few additional questions for each household member that they report.

²¹ We also found that members of the 2006 cohort were much more likely to give out information about their home phone. This is, again, consistent with a “trust” effect.

knowledge about science. Although these differences were typically not below the FDR-adjusted $p < 0.10$ threshold, the frequency with which they occurred is at the very least suggestive of a “true” effect. As shown in Table 1, respondents in the treatment group were markedly more likely to answer correctly questions about the source of radioactivity (*radioact*), the efficacy of antibiotics in killing viruses (*viruses*), the ongoing process of plate tectonics (*condrift*), and the relative sizes of electrons and atoms (*electron*). One possible explanation for these results is the “learning hypothesis” that we proposed earlier: if respondents who previously participated in the GSS seek out information about questions that have one objectively correct answer, we would expect to see differences between cohorts on precisely these sorts of items.

A note on exceptions

Although the empirical patterns that we present in Table 1 are generally consistent with theoretical expectations, there are also plenty of counter-examples where the treatment and control groups did not differ in predictable or meaningful ways. We did not *always* find differences between cohorts when examining questions about socially-charged issues, nor did we observe significant effects for *all* items that required factual knowledge or increased levels of respondent trust (for the complete set of results, see Appendix Table A1). These inter-item inconsistencies do not invalidate our findings, but they do suggest the need for more finely-grained analyses that are capable of isolating and carefully testing the various hypotheses that we outlined earlier. We will return to this idea later on in the discussion section.

Falsification test

In the final part of our analysis, we carry out a falsification test to confirm the adequacy of our empirical approach. As a part of its mission to provide up-to-date information about a wide variety of topics, the GSS frequently introduces new survey content through the use of special topical modules. This allows us to perform an important methodological check. Using the same analytic setup as before, we can test for differences between cohorts on items that have *not* previously been answered by anyone in the sample, *regardless of which cohort they belong to*. In the absence of any contaminating influences, we would expect to see a similar distribution of responses across groups for these measures. Any other result (e.g., non-zero differences between the treatment and control groups on items that should not, in theory, differ) would call into question the internal validity of our empirical estimates.

We present results from these comparisons in Table 2. In total, there are 19 variables that (1) were *not* asked in 2006; (2) *were* asked of both cohorts in 2008; and (3) meet the selection criteria that we defined earlier. Among these items, only one (*autonojb*) shows any evidence of variation between cohorts, and that evidence disappears when corrections are made for multiple comparisons.²² None of the estimated tests are significant at a 0.01 level and only two reach significance at the 0.10 level (with 19 comparisons we would expect to see ~1 significant result by

²² We excluded three employment-related variables (*ownbiz*, *findnwjb*, and *losejb12*) from these analyses because they closely resemble items that appeared on the 2006 survey. One of these variables produced a significant difference between cohorts; the other two did not.

chance, assuming a Type 1 error rate of 0.05).²³ This is a reassuring finding for our purposes, as it minimizes the possibility that the two cohorts differ in ways that could spuriously produce some or all of what we previously deemed to be panel conditioning effects.

Discussion

Sociologists who work with longitudinal data typically assume that the changes they observe across waves are real and would have occurred even in the absence of the survey. Whether or not this assumption is justified is an important empirical question, one that should be of concern to methodologists and non-methodologists alike. In this article, we provided an analytic framework for detecting panel conditioning effects in longitudinal surveys that include a rotating panel component. To demonstrate the utility of our approach, we analyzed data from recent waves of the GSS. Results from these analyses suggest that panel conditioning influences the quality of a small but non-trivial subset of core survey items. This inference was robust to a falsification test, and cannot be explained by statistical artifacts stemming from panel attrition and/or differential non-response.

What should applied researchers make of these findings? Our analysis suggests that panel conditioning exists in the GSS on a broad scale, but it is much less clear about the specific content domains that are most affected by this form of bias. As we mentioned at the outset, panel

²³ None of the comparisons were significant after adjusting the p -values for the FDR, and a Kolmogorov-Smirnov test could not reject the null hypothesis that the distribution of results was uniform ($D = .21$, $p = 0.81$).

conditioning is a complex interactive phenomenon that involves a range of cognitive processes and subjective individual assessments. Predicting when and where it will occur is a difficult theoretical exercise. We have attempted to provide some guidance to users of the GSS by listing the variables that show the most evidence of possible effects. We would advise researchers to weigh this information carefully when conducting studies with these data. Although panel conditioning does not always present itself in an intuitive or internally consistent manner, it would be wrong to dismiss it as an unimportant methodological issue.

There is obviously much more work still to be done in this area. The analytic techniques described herein can be usefully applied in any longitudinal data set that contains overlapping panels. An interesting future application would be to examine heterogeneity in panel conditioning among different sub-groups of respondents. In our analysis, we sought to identify the *average treatment effect* taken over all members of the sample. In reality, these effects may vary considerably across individuals, across social contexts, and across topical domains (see, e.g., Zwane, Zinman, Van Dusen, Pariente, Null, Miguel, Kremer, Karlan, Hornbeck, Giné, Duflo, Devoto, Crepon, and Banerjee 2011). A treatment effect of zero in the population may nevertheless be non-zero for certain sub-groups with particular experiences and/or predispositions. Identifying who these individuals are, and how they differ from others, would go a long way toward refining our theoretical understanding of why panel conditioning occurs.

Another worthwhile extension would be to conduct stand-alone experiments that allow for a closer examination of possible mechanisms. These experiments would not need to be

complicated; it would probably be enough to assign individuals at random to receive alternate forms of a baseline questionnaire and then to ask all questions of all individuals in a follow-up. To speak to the issue in a way that is broadly useful to sociologists, the questions would need to be similar or identical to those that routinely appear in other widely-used surveys, like the GSS, *and* would need to be carefully selected in order to isolate the various social and psychological processes that we described earlier. This would obviously require considerable effort and careful planning, but we believe it is the best way to produce a general and theoretically-informed understanding of panel conditioning in longitudinal social science research.

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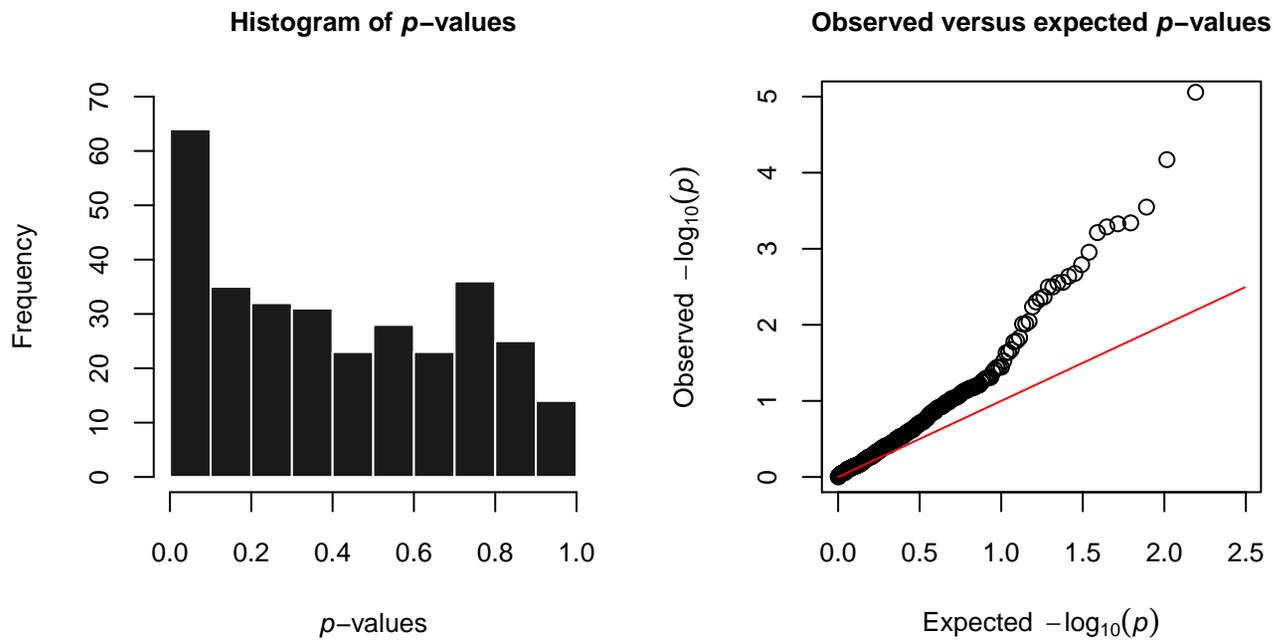


Figure 1: Histogram and Q-Q plot of observed p -values. The panel on the left shows the observed distribution of p -values for all items in our sample ($n = 310$). Under the null, the values should be uniformly distributed between 0 and 1. The panel on the right compares the observed distribution to a theoretical (null) distribution. If p -values are *more* significant than expected, points will move up and away from the red line. If p -values are uniformly distributed, the circles will track closely with the red line throughout the entire range. See text for further details.

Table 1. Size and direction of estimated effects in 2008, illustrative results

Variable	Description of response options/measure	Estimate (% or mean)	
		2006 cohort	2008 cohort
phone	Respondent refuses to give information about their phone	1.17	7.93
visitors	Average number of visitors in the household	0.05	0.01
parsol	Respondent's standard of living is higher than their parents' standard of living	66.21	59.50
rplace	The respondent is the householder or their spouse	91.70	88.21
adults	Average number of adults in the household	1.97	1.87
natracey	Respondent thinks current levels of public assistance for blacks are about right	53.51	43.60
marital	Respondent is divorced or widowed	25.88	21.56
spkrac	Respondent agrees that people have a right to make hateful speeches in public	67.08	60.81
rincom06	Respondent refuses to report income	4.27	6.05
famgen	Reports that there is only one generation in household	53.26	57.12
premarx	Respondent reports that sex before marriage is always or almost always wrong	34.94	30.75
radioact	Correctly answers question about the source of radioactivity	84.79	79.40
viruses	Correctly answers question about efficacy of antibiotics	65.64	59.35
condrift	Correctly answers question about plate tectonics	91.34	87.21
electron	Correctly answers question about sizes of electrons/atoms	75.77	70.45

Note: The 2006 cohort is restricted to respondents who were interviewed in 2006 and 2008; the 2008 cohort is restricted to respondents who entered the panel in 2008 and were also interviewed in 2010. Comparisons *between* cohorts are made in 2008, the year that they overlap in the sample. All of the variables presented in this table produced FDR adjusted p -values below .10, except for the science knowledge items. We included these items because of the consistency across measures (all four were significant by conventional standards and all four effects were in the same, theoretically sensible, direction). See text for more details, and Appendix Table A1 for the full set of results.

Table 2. Results from falsification tests

Variable description	Name	Tests for differences between cohorts	
		p	FDR-adjusted p
Trying to start a business	startbiz	0.50	0.75
Number of full-time jobs since 2005	work3yrs	0.67	0.78
Number of years worked for current employer	curempyr	0.54	0.75
Amount of pay change since started job	paychnge	0.40	0.75
Was pay higher/lower/the same in previous job?	pastpay	0.28	0.68
Why did the respondent leave their previous job?	whyleave	0.35	0.75
Does more trade lead to fewer jobs in the U.S.?	moretrde	0.27	0.68
Computer use at work	wkcomptr	0.12	0.66
Can job be done without a computer?	wocomptr	0.82	0.82
Have any co-workers been replaced by computers?	autonojb	0.02	0.22
Frequency of meetings with customers, clients, or patients	meetf2f1	0.15	0.66
Frequency of meetings with co-workers	meetf2f2	0.28	0.68
Frequency of communication with co-workers outside the U.S.	intlcowk	0.22	0.68
Does the respondent receive health insurance from their employer?	emphlth	0.82	0.82
Is there another name for the respondent's insurance or HMO policy?	othplan	0.58	0.75
Gender of sex partners	sexsex18	0.09	0.66
Ever been the target of sexual advances by a co-worker/supervisor?	harsexjb	0.55	0.75
Has respondent been the target of a sexual advance by a religious leader?	harsexcl	0.66	0.78
Do they know others who have been the target of sexual advances?	knwclsex	0.47	0.75

Note: These items were not asked of the 2006 cohort in 2006, but were asked of both cohorts in 2008. Variable names are given in the “name” column. The FDR-adjusted p is the p -value adjusted for the False Discovery Rate. Adjustments were made using the procedures of Benjamini and Hochberg (1995). See text for more details.

Table A1. Sample sizes and test statistics for all items ($n = 310$)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	<i>p</i>	Adjusted <i>p</i>
Abortion is acceptable for any reason	abany	1,024	1,034	1.75	1	0.19	0.61
Is abortion okay if chance of serious defect?	abdefect	1,009	1,020	0.09	1	0.77	0.90
Is abortion okay if woman's health is in jeopardy?	abhlth	1,021	1,025	0.21	1	0.65	0.90
Is abortion okay if woman does not want more kids?	abnomore	1,022	1,034	0.68	1	0.41	0.77
Abortion is acceptable for financial reasons	abpoor	1,024	1,036	0.14	1	0.71	0.90
Abortion is acceptable in event of rape	abrape	1,002	1,024	0.37	1	0.54	0.84
Abortion is okay if the woman is not married	absingle	1,023	1,041	0.30	1	0.58	0.86
Number of household members ages 18+	adults	1,515	1,578	3.50		0.00	0.02
Sci. research should be supported by public dollars	advfront	475	1,119	1.07	3	0.77	0.90
Opinion of affirmative action	affrmact	959	974	1.64	3	0.65	0.90
Age	age	1,514	1,571	3.08		0.00	0.06
Age when first child was born	agekdbrn	1,106	1,146	-0.31		0.76	0.90
Ever read horoscope or astrology report	astrolgy	492	1,163	0.33	1	0.57	0.86
Believes astrology is scientific	astrosci	474	1,123	2.88	2	0.24	0.68
Frequency of attendance at religious services	attend	1,533	1,574	9.59	8	0.29	0.71
Number of household members ages 0-5	babies	1,515	1,560	0.92		0.36	0.76
Feelings about the bible	bible	1,520	1,553	4.73	3	0.19	0.61
Believes the universe began with a huge explosion	bigbang	358	846	3.17	1	0.07	0.44
Agrees right and wrong is not black and white	blkwhite	1,511	1,535	3.59	3	0.31	0.72
Nativity	born	1,535	1,581	0.02	1	0.90	0.94
Believes father's gene determines sex of child	boyorgrl	219	850	3.45	1	0.06	0.44
Favor or oppose the death penalty for murder	cappun	1,452	1,496	0.51	1	0.48	0.81
Number of children	childs	1,535	1,580	0.96		0.34	0.75

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Ideal number of children	chldidel	979	998	-1.32		0.19	0.61
Subjective class identification	class	1,531	1,567	2.01	3	0.57	0.86
How close the respondent feels to African Americans	closeblk	1,038	1,060	9.87	8	0.27	0.71
How close the respondent feels to whites	closewht	1,040	1,060	11.56	8	0.17	0.59
Believes anti-religionists should be allowed to teach	colath	1,010	1,050	0.01	1	0.91	0.94
Believes communist teachers should be fired	colcom	993	1,024	2.03	1	0.15	0.56
Highest college degree	coldeg1	178	423	14.77	7	0.04	0.34
Believes homosexuals should be allowed to teach	colhomo	1,021	1,055	0.61	1	0.44	0.79
Believes militarists should be allowed to teach	colmil	1,007	1,038	4.39	1	0.04	0.34
Believes racists should be allowed to teach	colrac	1,010	1,045	3.33	1	0.07	0.44
Ever taken any college-level science course	colsci	488	1,160	8.71	1	0.00	0.06
Number of college-level science courses	colscinm	185	429	1.11		0.27	0.70
Respondents understanding of questions	comprend	1,533	1,581	2.94	2	0.23	0.67
Confidence in military	conarmy	1,015	1,044	5.21	2	0.07	0.44
Confidence in major companies	conbus	1,002	1,039	1.54	2	0.46	0.81
Confidence in organized religion	conclerg	1,003	1,039	3.41	2	0.18	0.61
Believes the continents have been moving	condrift	439	1,024	5.15	1	0.02	0.25
Confidence in education	coneduc	1,023	1,050	1.89	2	0.39	0.76
Confidence in executive branch	confed	1,006	1,034	0.03	2	0.98	0.99
Confidence in financial institutions	confinan	1,014	1,047	2.51	2	0.28	0.71
Confidence in supreme court	conjudge	1,006	1,031	2.47	2	0.29	0.71
Confidence in organized labor	conlabor	974	1,012	2.29	2	0.32	0.72
Confidence in congress	conlegis	1,005	1,039	1.91	2	0.39	0.76

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Confidence in medicine	conmedic	1,017	1,054	0.80	2	0.67	0.90
Confidence in press	conpress	1,015	1,046	0.59	2	0.75	0.90
Confidence in scientific community	consci	974	1,014	1.99	2	0.37	0.76
Participation/recording consent	consent	1,536	1,579	0.14	1	0.71	0.90
Confidence in television	contv	1,016	1,047	1.23	2	0.54	0.84
Feelings about courts' treatment of criminals	courts	1,430	1,466	0.71	2	0.70	0.90
Highest degree	degree	1,535	1,581	3.32	4	0.51	0.83
Specific denomination	denom	761	873	6.08	6	0.41	0.77
Denomination in which the respondent was raised	denom16	799	883	50.48	26	0.00	0.06
Believes whites are hurt by affirmative action	discaff	1,004	1,038	0.01	2	1.00	1.00
Equally/less qualified women get jobs instead of men?	discaffm	494	481	4.20	3	0.24	0.68
Equally/less qualified men get jobs instead of women?	discaffw	483	529	2.09	3	0.55	0.84
Believes divorce should be easier or more difficult	divlaw	955	986	0.65	2	0.72	0.90
Ever been divorced or separated	divorce	831	883	0.54	1	0.46	0.81
Own or rent dwelling	dwelown	1,003	1,024	0.23	2	0.89	0.94
How many in family earned income	earnrs	1,532	1,580	-0.15		0.88	0.94
Believes the earth goes around the sun	earthsun	460	1,081	0.02	1	0.89	0.94
Years of education	educ	1,533	1,579	1.56		0.12	0.50
Believes electrons are smaller than atoms	electron	388	890	3.80	1	0.05	0.41
Believes govt. should reduce income inequality	eqwlth	1,011	1,048	7.26	6	0.30	0.71
Believes human beings developed from animals	evolved	438	1,028	1.29	1	0.26	0.69
Ever worked as long as one year	evwork	452	518	1.12	1	0.29	0.71
Familiar with experimental design	expdesgn	466	1,095	0.14	1	0.71	0.90

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Knows why experimental design is preferred	exptext	459	1,068	0.13	1	0.72	0.90
Believes people are fair or take advantage of others	fair	1,019	1,056	3.16	2	0.21	0.63
Reason not living with parents when 16	famdif16	403	502	4.93	4	0.25	0.68
Number of family generations in household	famgen	1,536	1,581	17.70	6	0.01	0.08
Living with parents when 16	family16	1,534	1,581	5.36	8	0.72	0.90
Afraid to walk at night in neighborhood	fear	1,038	1,075	0.44	1	0.51	0.83
Believes mother working does/does not hurt children	fechld	994	1,019	14.49	3	0.00	0.06
Better for men to work and women to tend home?	fefam	995	1,011	0.62	3	0.89	0.94
Make special effort to hire/promote women?	fehire	494	532	1.79	4	0.78	0.90
For or against preferential hiring of women	fejobaff	491	464	2.33	3	0.51	0.83
Are women suited for politics?	fepol	946	974	0.06	1	0.81	0.92
Do preschool kids suffer if mother works?	fepresch	989	1,010	1.04	3	0.79	0.91
Change in financial situation in last few years	finalter	1,532	1,576	1.50	2	0.47	0.81
Opinion of family income	finrela	1,517	1,566	6.96	4	0.14	0.53
How fundamentalist is the respondent?	fund	1,402	1,515	3.95	2	0.14	0.53
How fundamentalist was the respondent at age 16?	fund16	1,476	1,525	1.31	2	0.52	0.84
Opinion of how people get ahead	getahead	1,042	1,069	0.95	2	0.62	0.89
Confidence in the existence of god	god	1,529	1,567	3.79	5	0.58	0.86
Standard of living will improve	goodlife	1,021	1,053	8.60	4	0.07	0.44
Number of grandparents born outside the U.S.	granborn	1,418	1,498	3.16	4	0.53	0.84
Opinion of marijuana legalization	grass	944	969	0.72	1	0.40	0.76
Opinion of gun permits	gunlaw	1,034	1,063	0.08	1	0.78	0.91
Happiness of marriage	hapmar	692	760	3.16	2	0.21	0.63

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
General happiness	happy	1,525	1,576	4.42	2	0.11	0.50
Condition of health	health	1,043	1,076	0.67	3	0.88	0.94
Opinion of government aid for African Americans	helpblk	989	1,016	1.07	4	0.90	0.94
Are people helpful or selfish?	helpful	1,019	1,059	3.69	2	0.16	0.57
Should government do more or less?	helpnot	983	1,018	6.81	4	0.15	0.54
How important is it for kids to learn to help others?	helpoth	517	1,051	1.84	4	0.78	0.91
Should government improve standard of living?	helppoor	1,001	1,035	2.74	4	0.60	0.87
Should government help pay for medical care?	helpsick	1,001	1,031	4.76	4	0.31	0.72
Is the respondent Hispanic?	hispanic	1,533	1,578	21.83	19	0.26	0.69
Attitude toward homosexual relations	homosex	998	1,011	4.72	3	0.19	0.61
Believes the center of the earth is very hot	hotcore	434	1,028	0.17	1	0.68	0.90
Number of hours worked last week	hrs1	749	952	2.59		0.01	0.13
Ever took a high school biology course	hsbio	467	1,111	0.09	1	0.77	0.90
Ever took a high school chemistry course	hschem	469	1,111	2.64	1	0.10	0.48
Highest level of math completed in high school	hsmath	454	1,092	13.38	9	0.15	0.54
Ever took a high school physics course	hsphys	465	1,109	0.04	1	0.83	0.93
Does the respondent or their spouse hunt?	hunt	1,043	1,075	3.05	3	0.38	0.76
Who would respondent have voted for in 2004?	if04who	328	482	0.91	2	0.63	0.90
Family income when age 16	incom16	1,513	1,561	3.12	4	0.54	0.84
Total family income	income06	1,488	1,529	35.57	25	0.08	0.45
Rating of African Americans' intelligence	intlblks	980	993	5.44	6	0.49	0.82
Rating of whites' intelligence	intlwhts	980	993	3.31	6	0.77	0.90
Internet access at home	intrhome	488	1,158	1.34	1	0.25	0.68

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Could respondent find equally good job?	jobfind	507	651	5.40	2	0.07	0.44
Likelihood of losing job	joblose	510	658	0.39	3	0.94	0.96
Standard of living compared to children's	kidssol	994	1,030	5.30	5	0.38	0.76
Believes lasers work by focusing sound waves	lasers	362	857	0.12	1	0.72	0.90
Allow incurable patients to die	letdie1	971	467	0.02	1	0.89	0.94
Beliefs about immigration	letin1	975	1,007	1.53	4	0.82	0.93
Allow anti-religionist's book in the library?	libath	1,025	1,051	2.83	1	0.09	0.46
Allow communist's book in library?	libcom	1,015	1,045	2.18	1	0.14	0.53
Allow homosexual's book in library?	libhomo	1,022	1,058	0.04	1	0.84	0.94
Allow militarist's book in library?	libmil	1,018	1,048	3.07	1	0.08	0.45
Allow racist's book in library?	librac	1,023	1,051	2.09	1	0.15	0.54
Is life exciting or dull?	life	1,035	1,065	0.87	2	0.65	0.90
Would live in area where half of neighbors are black?	liveblks	995	1,018	4.77	4	0.31	0.72
Would live in area where half of neighbors are white?	livewhts	994	1,015	2.09	4	0.72	0.90
Number of employees at work site	localnum	780	979	3.90	6	0.69	0.90
Mother's years of schooling	maeduc	1,340	1,398	-0.83		0.40	0.77
College major	majorcol	193	422	9.41	5	0.09	0.46
Feelings about relative marrying an Asian	marasian	995	1,021	5.83	4	0.21	0.64
Feelings about relative marrying an African American	marblk	998	1,021	3.59	4	0.46	0.81
Feelings about relative marrying a Hispanic	marhisp	996	1,021	4.43	4	0.35	0.75
Should homosexuals have the right to marry?	marhomo	1,033	1,065	4.33	4	0.36	0.76
Marital status	marital	1,534	1,577	18.24	4	0.00	0.04
Feelings about relative marrying a white person	marwht	998	1,023	2.28		0.68	0.90

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Mother's socioeconomic index	masei	817	1,017	1.23		0.22	0.65
Mother's employment when respondent was 16	mawrkgrw	1,428	1,501	0.07	1	0.79	0.91
Mother self-employed or worked for somebody else	mawrkslf	872	1,033	0.15	1	0.70	0.90
Men hurt family when they focus too much on work	meovrwrk	999	1,019	7.15	4	0.13	0.51
Geographic mobility since age 16	mobile16	1,536	1,581	3.22	2	0.20	0.63
Believes nanotechnology manipulates small objects	nanoknw1	156	411	1.49	1	0.22	0.65
Believes nanoscale materials are different	nanoknw2	120	326	0.11	1	0.74	0.90
Familiarity with nanotechnology	nanotech	474	1,132	4.72	3	0.19	0.61
Costs and benefits of nanotechnology	nanowill	305	701	4.78	2	0.09	0.46
Amount spent on foreign aid	nataid	746	720	1.37	2	0.51	0.83
Amount spent on foreign aid (version y)	nataidy	750	782	0.84	2	0.66	0.90
Amount spent on national defense	natarms	758	742	2.17	2	0.34	0.75
Amount spent on national defense (version y)	natarmsy	741	779	2.44	2	0.29	0.71
Amount spent on assistance for childcare	natchld	1,445	1,464	3.97	2	0.14	0.53
Amount spent on assistance to big cities	natcity	699	687	5.84	2	0.05	0.42
Amount spent on assistance to big cities (version y)	natcityy	681	697	2.24	2	0.33	0.74
Amount spent on drug rehabilitation	natdrug	754	735	9.42	2	0.01	0.13
Amount spent on drug rehabilitation (version y)	natdrugy	711	759	2.15	2	0.34	0.75
Amount spent on education	nateduc	767	761	2.73	2	0.26	0.69
Amount spent on education (version y)	nateducy	754	800	0.90	2	0.64	0.90
Amount spent on environmental protection	natenvir	755	754	0.69	2	0.71	0.90
Amount spent on environmental protection (version y)	natenviy	746	786	0.25	2	0.88	0.94
Amount spent on welfare	natfare	747	734	2.07	2	0.36	0.76

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Amount spent on welfare (version y)	natfarey	750	795	1.03	2	0.60	0.87
Amount spent on health	natheal	769	749	0.52	2	0.77	0.90
Amount spent on health (version y)	nathealy	754	795	0.16	2	0.92	0.95
Amount spent on transportation	natmass	1,465	1,477	4.22	2	0.12	0.50
Amount spent on parks and recreation	natpark	1,504	1,554	1.74	2	0.42	0.78
Amount spent on assistance to blacks	natrace	695	676	1.69	2	0.43	0.79
Amount spent on assistance to blacks (version y)	natracey	669	711	15.14	2	0.00	0.02
Amount spent on highways and bridges	natroad	1,506	1,537	1.79	2	0.41	0.77
Amount spent on scientific research	natsci	1,444	1,475	0.66	2	0.72	0.90
Amount spent on social security	natsoc	1,483	1,511	1.77	2	0.41	0.77
Amount spent on space exploration	natspac	730	724	1.21	2	0.55	0.84
How often does the respondent read the newspaper?	news	1,003	1,026	1.93	4	0.75	0.90
Main source of information about events in the news	newsfrom	492	1,162	3.38	9	0.97	0.98
Does science give opportunities to future generations?	nextgen	480	1,134	1.49	3	0.71	0.90
How important is it for children to obey parents?	obey	517	1,051	3.10	4	0.54	0.84
Test of knowledge about probability	odds1	457	1,085	1.24	1	0.26	0.69
Test of knowledge about probability	odds2	457	1,089	1.12	1	0.29	0.71
Gun in home	owngun	1,038	1,074	0.79	2	0.67	0.90
Father's years of education	paeduc	1,116	1,165	-1.00		0.32	0.72
Were parents born in U.S.?	parborn	1,530	1,577	3.30	6	0.86	0.94
Standard of living compared to parents	parsol	1,012	1,042	21.25	4	0.00	0.02
Part- or full-time work	partfull	969	1,159	9.94	1	0.00	0.05
Father's socioeconomic index	pasei	1,112	1,223	-0.22		0.83	0.93

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Father self-employed?	pawrkslf	1,205	1,283	1.88	1	0.17	0.59
Agrees that morality is a personal matter	permoral	1,494	1,538	7.10	3	0.07	0.44
Telephone in household	phone	1,536	1,577	157.30	4	0.00	0.00
Is birth control okay for teenagers between 14-16?	pillock	983	482	6.29	3	0.10	0.48
Pistol or revolver in home	pistol	1,036	932	1.23	2	0.54	0.84
Okay for police to hit citizen who said vulgar things?	polabuse	1,003	1,022	0.04	1	0.83	0.93
Okay for police to hit citizen who is attacking them?	polattak	1,017	1,047	0.91	1	0.34	0.75
Okay for police to hit citizen if trying to escape?	polescap	979	1,021	0.02	1	0.88	0.94
Ever approve of police striking citizen	polhitok	982	484	0.10	1	0.75	0.90
Okay for police to hit murder suspect?	polmurdr	990	1,033	0.03	1	0.86	0.94
Think of self as liberal or conservative?	polviews	1,493	1,520	4.96	6	0.55	0.84
Is pope infallible on matters of faith or morals?	popespks	335	167	4.46	4	0.35	0.75
How important is it for a child to be popular?	popular	517	1,051	8.54	4	0.06	0.44
Feelings about pornography laws	pornlaw	1,022	1,055	4.74	2	0.09	0.46
Belief in life after death	postlife	1,377	1,395	0.17	1	0.68	0.90
How often does the respondent pray?	pray	1,527	1,564	9.58	5	0.09	0.46
Should bible prayer be allowed in public schools?	prayer	959	989	0.71	1	0.40	0.77
Feelings about sex before marriage	premarsex	973	1,005	12.52	3	0.01	0.09
Which candidate did the respondent vote for in 2004?	pres04	954	976	11.42	3	0.01	0.13
Number of household members ages 6-12	preteen	1,515	1,560	2.44		0.01	0.19
Agrees that sinners must be punished?	punsin	1,441	1,474	0.32	3	0.96	0.97
Thinks blacks' disadvant. are due to discrimination?	racdif1	959	990	2.37	1	0.12	0.51
Thinks blacks' disadvant. are due to disabilities?	racdif2	980	1,008	0.73	1	0.39	0.76

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Thinks blacks' disadvant. are due to lack of education?	racdif3	979	998	0.61	1	0.44	0.79
Thinks blacks' disadvant. are due to lack of will?	racdif4	955	987	0.12	1	0.73	0.90
Race	race	1,536	1,581	0.53	2	0.77	0.90
Any African Americans living in neighborhood?	raclive	1,470	1,528	0.02	1	0.88	0.94
Feelings about open housing laws	racopen	1,038	1,066	1.89	2	0.39	0.76
Racial makeup of workplace	racwork	534	637	7.29	4	0.12	0.50
Believes all radioactivity is man-made	radioact	434	1,034	5.77	1	0.02	0.20
Ever had a "born again" experience?	reborn	1,517	1,557	0.01	1	0.93	0.95
Region of residence at age 16	reg16	1,536	1,581	4.33	9	0.89	0.94
Participates frequently in religious activities?	relactiv	1,530	1,568	16.99	9	0.05	0.41
Has a religious experience changed life?	relexp	1,526	1,567	0.08	1	0.77	0.90
Turning point in life for religion	relexper	1,524	1,566	2.40	1	0.12	0.50
Religious preference	relig	1,534	1,574	3.60	4	0.46	0.81
Religion in which respondent was raised	relig16	1,531	1,570	9.43	11	0.58	0.86
Strength of religious affiliation	reliten	1,421	1,465	1.12	3	0.77	0.90
Try to carry religious beliefs into other dealings	rellife	1,511	1,555	1.28	3	0.73	0.90
Any turning point when less committed to religion?	relneg	754	1,570	0.17	1	0.68	0.90
Does respondent consider self a religious person?	relpersn	1,518	1,563	2.63	3	0.45	0.81
Type of community when 16	res16	1,534	1,580	1.80	5	0.88	0.94
Continue to work if became rich?	richwork	582	704	5.73	1	0.02	0.20
Rifle in home	rifle	1,036	925	2.57	2	0.28	0.71
Respondent's income	rincom06	831	1,009	47.49	25	0.00	0.08
Agrees that immoral people corrupt society	rotapple	1,509	1,537	3.99	3	0.26	0.69

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Does the gun in the house belong to the respondent?	rowngun	333	382	0.78	2	0.68	0.90
Relationship to head of household	rplace	1,535	1,578	26.24	7	0.00	0.02
Is the respondent a visitor in the household?	rvisitor	1,536	1,581	0.23	1	0.63	0.90
Satisfaction with financial situation	satfin	1,532	1,576	4.84	2	0.09	0.46
Job satisfaction	satjob	1,054	1,207	3.72	3	0.29	0.71
Ever tried to convince others to accept Jesus?	savesoul	1,530	1,573	0.27	1	0.60	0.87
Do the benefits of scientific research outweigh the costs?	scibnfts	452	1,084	4.51	2	0.10	0.48
Main source of info about science and technology	scifrom	488	1,152	7.30	9	0.61	0.87
Has a clear understanding of scientific study?	scistudy	485	1,148	4.95	2	0.08	0.46
What it means to study something scientifically	scitext	334	844	17.83	5	0.00	0.06
Likely source of information about scientific issues	seeksci	489	1,142	4.72	7	0.69	0.90
Socioeconomic index	sei	1,386	1,497	2.28		0.02	0.25
Feelings about sex education in public schools	sexeduc	990	1,010	0.28	1	0.60	0.87
Shotgun in home	shotgun	1,036	926	3.48	2	0.18	0.60
Number of siblings	sibs	1,534	1,579	-0.24		0.81	0.92
Frequently spend an evening at a bar?	soctbar	1,002	1,026	4.94	6	0.55	0.84
Frequently spend evenings with friends?	soctfrend	1,001	1,024	11.55	6	0.07	0.44
Frequently spend evenings with neighbors?	socommun	999	1,025	7.37	6	0.29	0.71
Frequently spend evenings with relatives?	socrel	1,003	1,026	2.54	6	0.86	0.94
How long does it take earth to travel around the sun?	solarrev	308	803	1.24	2	0.54	0.84
Favor spanking to discipline children?	spanking	996	1,012	0.23	3	0.97	0.98
Spouse's religious denomination	spden	342	431	26.34	25	0.39	0.76
Spouse's years of education	speduc	692	757	0.66		0.51	0.83

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Has spouse ever worked as long as a year?	spevwork	166	226	0.03	1	0.87	0.94
How fundamentalist is spouse currently?	spfund	660	727	4.51	2	0.10	0.48
Hours spouse worked last week	sphrs1	413	502	1.53		0.13	0.51
Should anti-religionists be allowed to speak publicly?	spkath	1,039	1,068	0.53	1	0.47	0.81
Should communists be allowed to speak publicly?	spkcom	1,020	1,051	4.14	1	0.04	0.37
Should homosexuals be allowed to speak publicly?	spkhomo	1,028	1,059	0.75	1	0.39	0.76
Should militarists be allowed to speak publicly?	spkmil	1,022	1,057	2.93	1	0.09	0.46
Should racists be allowed to speak publicly?	spkrac	1,036	1,059	8.93	1	0.00	0.06
Spouses religious preference	sprel	695	758	11.94	11	0.37	0.76
Does respondent consider self a spiritual person?	sprtprsn	1,512	1,558	2.16	3	0.54	0.84
Spouse's socioeconomic index	spsei	609	710	0.86		0.39	0.76
Is spouse self-employed?	spwrkslf	641	732	0.61	1	0.43	0.79
Spouse's labor force status?	spwrksta	693	759	2.40	7	0.93	0.96
Is suicide okay if disease is incurable?	suicide1	967	994	0.07	1	0.79	0.91
Is suicide okay if person is bankrupt?	suicide2	994	1,010	3.86	1	0.05	0.41
Is suicide okay if person dishonored their family?	suicide3	989	1,012	1.39	1	0.24	0.68
Is suicide okay if person is tired of living?	suicide4	980	997	0.28	1	0.60	0.87
Federal income tax	tax	1,016	1,044	1.61	2	0.45	0.80
Number of household members ages 13-17	teens	1,515	1,559	0.87		0.38	0.76
Is sex before marriage okay for people ages of 14-16?	teensex	996	1,019	1.28	3	0.73	0.90
How important is it for kids to think for themselves?	thnkself	517	1,051	8.76	4	0.07	0.44
Does science make our way of life change too fast?	toofast	478	1,134	7.82	3	0.05	0.41
Can people be trusted?	trust	1,021	1,060	4.30	2	0.12	0.50

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	p	Adjusted p
Hours per day watching television	tvhours	1,001	1,023	-1.39		0.17	0.58
Ever unemployed in last ten years?	unemp	1,024	1,060	0.02	1	0.88	0.94
Union membership	union	1,022	1,055	0.23	3	0.97	0.98
Number in household who are unrelated	unrelat	946	1,083	-0.76		0.44	0.80
Expect another world war in the next 10 years?	uswary	944	970	0.02	1	0.90	0.94
Believes antibiotics kill viruses as well as bacteria	viruses	454	1,075	5.32	1	0.02	0.24
Number of visitors in household	visitors	1,536	1,581	4.45		0.00	0.00
Did the respondent vote in the 2004 election?	vote04	1,501	1,541	19.21	2	0.00	0.01
Weeks worked last year	weekswrk	1,525	1,568	-2.10		0.04	0.34
Presence of children under six	whoelse1	1,533	1,581	0.50	1	0.48	0.81
Presence of older children	whoelse2	1,533	1,581	0.52	1	0.47	0.81
Presence of other relatives	whoelse3	1,533	1,581	3.65	1	0.06	0.43
Presence of other relatives	whoelse4	1,533	1,581	0.35	1	0.56	0.84
Presence of other adults	whoelse5	1,533	1,581	0.13	1	0.72	0.90
No one else present	whoelse6	1,533	1,581	4.72	1	0.03	0.31
Ever been widowed	widowed	1,012	1,033	0.01	1	0.94	0.96
Does respondent or spouse have supervisor	wksub	964	1,132	1.36	1	0.24	0.68
Does supervisor have supervisor	wksubs	691	914	8.05	1	0.00	0.08
Does respondent or spouse supervise anyone	wksup	961	1,133	1.53	1	0.22	0.65
Does subordinate supervise anyone	wksups	267	421	4.28	1	0.04	0.35
Rating of blacks on wealth scale	wlthblks	985	1,002	9.93	6	0.11	0.50
Rating of whites on wealth scale	wlthwhts	985	1,004	5.15	6	0.53	0.84
Wordsum score	wordsum	744	900	1.63		0.10	0.48

(Continued)

Table A1. (Continued)

Variable	Mnemonic	Sample sizes		Difference between cohorts			
		2006	2008	chi ² /t	df	<i>p</i>	Adjusted <i>p</i>
Rating of blacks on laziness scale	workblks	985	989	6.70	6	0.35	0.75
How important is it for a child to work hard?	workhard	517	1,051	6.11	4	0.19	0.61
Rating of whites on laziness scale	workwhts	988	993	7.94	6	0.24	0.68
Government or private employee	wrkgovt	1,430	1,503	1.61	1	0.20	0.63
Self-employed or works for somebody else	wrkslf	1,446	1,534	0.17	1	0.68	0.90
Labor force status	wrkstat	1,535	1,580	13.41	7	0.06	0.44
Believes blacks can overcome prejudice without help	wrkwayup	989	1,014	0.99	4	0.91	0.94
Had sex with person other than spouse	xmarsex	1,037	1,056	1.81	3	0.61	0.88
Seen x-rated movie in last year	xmovie	1,024	1,054	1.06	1	0.30	0.72
Astrological sign	zodiac	1,510	1,536	7.60	11	0.75	0.90

Note: The adjusted *p* is the *p*-value adjusted for the False Discovery Rate. Bolded *p*-values are significant at the .05 level. Degrees of freedom for chi-squared tests are given in the column labeled df. Sample sizes for each cohort are given in the column labeled sample sizes. See text for additional details.