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# Assessing Trends and Decomposing Change in Nonresponse Bias: The Case of Bias in Cohort Distributions

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## Abstract

Survey research is still confronted by a trend of increasing nonresponse rates. In this context, several methodological advances have been made to stimulate participation and avoid bias. Yet, despite the growing number of tools and methods to deal with nonresponse, little is known about whether nonresponse biases show similar trends as nonresponse rates and what mechanisms (if any) drive changes in bias. Our article focuses on biases in cohort distributions in the U.S. and German general social surveys from 1980 to 2012 as one of the key variables in the social sciences. To supplement our cross-national comparison of these trends, we decompose changes into within-cohort change (WCC) and between-cohort change. We find that biases in cohort distributions have remained relatively stable and at a relatively low level in both countries. Furthermore, WCC (i.e., survey climate) accounts for the major part of the change in nonresponse bias.

## Keywords

nonresponse bias, trend, decomposition, survey climate, cohort distributions

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## Introduction

Declining response rates are a well-known trend in survey research and have been reported for different countries in several studies (Atrostic et al. 2001; Curtin, Presser, and Singer 2005; de Leeuw and de Heer 2002; Groves and Couper 1998; Stoop et al. 2010). In light of this trend, various related issues have been discussed—for example, strategies to increase response rates (e.g., Dillman, Smyth, and Christian 2014), whether response rates are actually related to survey quality (Groves and Peytcheva 2008; Keeter et al. 2000; Kreuter 2013; Schouten, Cobben, and Bethlehem 2009), and definitions of nonresponse bias (e.g., Bethlehem 2002). From the discussion about the causes for the rising nonresponse rates (e.g., Brick and Williams 2013; Tourangeau 2004), we can infer that two general mechanisms may be driving this trend: changes in individuals' willingness to participate and changes in the target population's composition (i.e., cohort replacement). Analytically, we can refer to these changes as change that occurs within cohorts and change that occurs between cohorts.

For the undesirable consequence of nonresponse (i.e., nonresponse bias), to the best of our knowledge, we lack evidence first on whether biases are stable over time or whether a trend of increasing biases has developed or is developing.[1] Second, the effects of changes in cohorts' willingness to participate and changes in cohort replacement on nonresponse biases remain unknown. Third, while trends in nonresponse rates were compared cross-nationally in several studies (Curtin et al. 2005; de Leeuw and de Heer 2002; Groves and Couper 1998; Stoop et al. 2010), similar efforts were not undertaken to examine trends in nonresponse biases. This omission is particularly unfortunate, since societal developments are frequently being compared between countries, for example, in analyses of social mobility (e.g., Breen 2004; Breen and Luijkx 2004) or attitudes toward social groups (e.g., Davidov 2011; Meuleman, Davidov, and Billiet 2009).

In general, investigating nonresponse bias over time and highlighting the role of within-cohort change (WCC) and between-cohort change (BCC) seem worthwhile for several reasons. First and foremost, substantial substantive research is being conducted on how variables change over time. If nonresponse biases change over time, this could severely compromise our analytical findings, since the resulting variation could be mistaken for substantive change in the outcome variables if appropriate correction methods are not applied. This potential change becomes even more important when trends in biases differ across countries. Second, nonresponse bias is an important aspect of data quality, which has influenced major innovations in survey methodology (Peytchev 2013). On the one hand, nonresponse adjustment methods (e.g., propensity score weighting) have been proposed, several of which rely on modeling nonresponse (Kalton and Flores-Cervantes 2003; Kreuter et al. 2010; Kreuter and Olson 2011). On the other hand, adaptive/responsive designs have been developed to reduce nonresponse bias (Dillman et al. 2014; Groves and Heeringa 2006). Some of these designs specifically tailor the survey to a group of respondents to lower nonresponse. Frequently, predictive models are used to define these groups, and often these models resemble prediction models for nonresponse (e.g., Peytchev et al. 2010). Hence, again, we would benefit from a better understanding of the driving forces behind nonresponse to improve our models and stimulate theoretical discussion. Third, we do not know how nonresponse bias will evolve further. To anticipate, and hence to act accordingly, we need a better understanding of how biases have changed until now and the time-related mechanisms behind them.

Consequently, this article draws on the case of cohort distributions' nonresponse biases and explores them over time to address three research objectives. First, we outline an analytical perspective that provides a better understanding of how the nonresponse bias of a variable changes over time, and whether within or between cohort mechanisms drive that change. Second, we draw on the general social surveys of Germany and the United States to illustrate cross-national trends in cohort distributions' nonresponse bias between 1980 and 2012. Third, we apply our proposed method to show how to decompose change in nonresponse bias into WCC and BCC. As argued below, cohort distributions constitute a suitable case to illustrate a time-related perspective on nonresponse biases and to propose an analytical approach to assess changes and the forces driving them.

The next section introduces social exchange theory and leverage-saliency theory as a theoretical framework to explain the variation in respondents' participation propensities due to a different survey climate (i.e., WCC) and cohort replacement (i.e., BCC). Based on this discussion, we argue how the theoretical framework may be used as a respondent-based explanation for change in nonresponse bias. Furthermore, cohort distributions are introduced as an illustrative case to study the role of WCC and BCC on nonresponse bias. The subsequent section outlines data preparation and the decomposition method. After discussing the results of our analysis, we present our concluding remarks and an outlook for further research opportunities.

## Background

### *Change in Survey Participation and Theoretical Background*

To explain the trend of increasing nonresponse rates, different possible sources for respondents' lack of willingness to participate have been introduced. Among them are the rising exposure to marketing efforts and oversurveying (Presser and McCulloch 2011), increasing concerns about privacy protection (Singer, Mathiowetz, and Couper 1993; Singer and Presser 2008), economic and political conditions (Harris-Kojetin and Tucker 1999), and technological development (Brick et al. 2006; Link and Oldendick 1999; Steeh et al. 2001). Tourangeau (2004) and, more recently, Brick and Williams (2013) provide good overviews of these ideas. While Tourangeau (2004) puts stronger emphasis on societal change and its role in changing nonresponse rates, Brick and Williams (2013) focus on the role of the survey climate. Survey climate describes the social context of the period in which a survey is conducted. If the survey climate changes between two surveys, the respondents' willingness to participate will change as well. Put differently, a variation in the social context of the respondents' decision to participate in a survey will lead to change. One important contribution of this discussion was to clarify that changes in individuals' willingness to participate and the composition of the target population may lead to changing response rates. Put in a more generalized form: "( . . . ) the proximate sources of aggregate change are net change among individuals and population turnover" (Firebaugh 1997:20). Analytically, this change also is known as *WCC* and *BCC*.

Social exchange theory and leverage-saliency theory are the most prominent approaches to explain survey (non)response (Brick and Williams 2013:56). Social exchange theory asserts that survey response is a function of the rewards and costs of participating as well as the respondent's trust that the rewards outnumber the costs (Dillman et al. 2014). Social exchanges exceed economic exchanges, since the rewards are not limited to monetary goods, are subjective to the respondent, and cannot be bargained for. If respondents judge the expected rewards to be higher than the costs, participation in a survey is the likely decision. Leverage-saliency theory provides us with a further respondent-level decision-making model for survey participation (Groves, Singer, and Corning 2000). These authors argue that respondents' attributes will have leverage on the likelihood of participating in a survey (e.g., concerns about the credibility of the funding organization or interest in the survey's topic). The saliency of the respondents' attributes ultimately determines their survey participation. These attributes can be made more (or less) salient to respondents when designing the survey, for example, by tailoring the recruitment interview to mitigate concerns (Groves and Couper 1998). From the perspective of social exchange theory, respondents' attributes that hinder or facilitate participation can be conceived as constituting their perception of rewards, costs, and trust. For example, salient concerns about the credibility of a funding organization may negatively affect trust.

We argue that both theories help to better understand how change in the climate of a survey or in the composition of a target population may lead to changing response rates. With respect to the climate of a survey (i.e., WCC), this means that decision-making about survey participation differs between two (or more) points of time. This difference may be the result of other attitudes becoming salient, the saliency of the same attributes decreasing or increasing, or methodological interventions being implemented in a subsequent survey that affect the saliencies of attributes. Again, from the social exchange perspective, one may interpret this difference as the rewards and costs of participation changing over time. With respect to the composition of the target population (i.e., BCC), this means that newly emerging cohorts differ from other cohorts in terms of which specific attitudes are salient for survey participation, and in terms of trust and their expectations toward the rewards and costs of participation. Accordingly, if the average response propensity for younger cohorts is higher than for older cohorts, the response rate will increase—otherwise it will decrease.

## *Nonresponse Bias in Cohort Distributions*

If the determinants of nonresponse are related to the variables of interest, a nonresponse bias exists (cf. Groves 2006). Consequently, bias has to be understood as an estimate-specific indicator (Bethlehem 2002). This fact may have hindered further investigation and helps to explain the lack of research on changing biases, since a multitude of analyses would be needed to assess the nonresponse bias for different variables. Analyzing the change in nonresponse bias for a broad set of variables would result in the use of aggregated indicators and, hence, a potential loss of precision and insight. We argue that limiting the perspective on cohort representation (i.e., bias in a survey's cohort distributions) can be a first step toward shedding light on the important issue of changing nonresponse bias and toward developing an analytical method that may be applied to other variables. When referring to a *cohort*, we mean the "birth cohort, those persons born in the same time interval and aging together" (Ryder 1965:844). Bias in a survey's cohort distributions occurs when the response propensities of different cohorts are not equal, and some cohorts are overrepresented while others are underrepresented in a survey.

Cohort distributions are important to the social sciences for several reasons. First, as the identification problem of age–period–cohort analyses shows (Glenn 2005), age, period, and cohort variables are linear combinations of each other. In other words, we can deduce one from the others.[2] For example, in a cross-section survey carried out in 2000, we can conclude that a 60-year-old respondent was born in 1940. Accordingly, by addressing nonresponse bias in cohort distributions, we are able to derive similar biases in the age and period variables of a survey. Second, age, period, and cohort are routinely used as control variables, which means that often they are assumed to correlate with other variables of interest. According to Peytcheva and Groves (2009), a general correlation does not exist between bias in sociodemographic and substantive variables. However, in those cases in which a correlation is present and strong enough, the empirical link may result in biased distributions of the related variables. Third, with respect to age–period–cohort analyses, a whole stream of research has been devoted to disentangling the effects of the three variables (cf. Yang and Land 2013). These methods are well known and frequently used in sociology, as aging societies and time-related analyses have gained increasing attention. Accordingly, a bias or an increasing bias can be considered a significant issue. Fourth, from the perspective of measurement, cohort (or age) is one of the variables for which external reference distributions are available for a longer period of time. When assessing nonresponse bias, we frequently have to draw on reference distributions, since information on the gross sample are unavailable or limited. Thus, the availability of external references for cohort distributions allows to assess a variable's nonresponse bias and, most importantly, to do so for a longer period of time. This makes cohort distributions a well-suited and convenient variable for the purpose of the present study.

It is important to note that bias in cohort distributions can be corrected by methods like poststratification adjustment because the true population distributions are (mostly) known. However, we should not let the fact that correction methods are available disguise the importance of a bias in this variable. As argued above (and later in this article), by focusing on cohort distributions, we can learn about the (in)stability of a bias, mechanisms that drive change, and—in general—explore how to assess the change in biases.

## Change in Nonresponse Bias

Nonresponse bias is not a static indicator, but—similar to response rates—is embedded in the societal context and subject to change over time. To understand this change in biases over time, we can again identify two different mechanisms: change in the survey climate and cohort replacement.

First, individuals change over time (i.e., change occurs within cohorts). With respect to surveys, this means that individuals' willingness to participate changes due to aging or periodical effects. This shift may be the result of individual change without methodological intervention but also can be stimulated by a researcher through a modification of survey design (e.g., implementing incentives). Social exchange theory and leverage-saliency theory provide us with a theoretical framework for both effects—changes in individual participation and survey design. On the one hand, an individual's attitudes which have leverage on their willingness to participate and/or their saliencies may change (e.g., in one year, the most salient attribute may be concerns regarding data privacy, while the next year, the focus changes to the credibility of the funding organization). For a change in cohort distributions' nonresponse bias to occur, the variation in cohorts' participation propensities has to differ between two surveys. This difference may occur over the course of a life (i.e., age effect) or be influenced by the current social context (i.e., period effect). For instance, a debate about data privacy may become salient to the decision to participate (and, thus, increase the perceived costs and lower trust) for some cohorts, but not for all. On the other hand, researchers may have implemented tools that make certain attitudes that facilitate survey participation more salient to respondents (e.g., by raising respondents' awareness that their data will be stored in an anonymized way) and by doing so increase their rewards, decrease costs, or enhance trust. Examples of such tools are to prepay incentives, render participation an important task, or proof sponsorship by a legitimate and trusted authority (Dillman et al. 2014). If a subsequent survey is designed to better convert members of a cohort with a low participation propensity to respondents, this could lower the nonresponse bias.

Second, the changing composition of a population drives a trend (i.e., change occurs between cohorts). In the case of increasing nonresponse bias, this means that older cohorts—who showed a higher affinity toward survey participation—vanish, and newer cohorts, less willing to participate in a survey, join the population. Again, our theoretical framework provides a better understanding of this process. If emerging cohorts differ from older cohorts in terms of which attitudes are salient for survey participation and how they judge the rewards and costs of participating, their response propensities also will vary. Over time, the composition of the population changes and, thus, the proportion of cohorts who are more prone to be underrepresented differs. Consequently, bias changes due to cohort replacement.

Both mechanisms may operate simultaneously. They can contribute to change in the same or in different directions and, hence, may add up or equal out. Therefore, analytically separating these two mechanisms seems desirable.

## Data and Method

To analyze nonresponse bias over time, we used the cumulated general social survey data sets from Germany (Allgemeine Bevölkerungsumfrage der Sozialwissenschaften, ALLBUS) and the United States (General Social Survey, GSS) that cover the period 1980 to 2012. General social surveys are a major data source for social science research and seem to be an ideal representation of a state-of-the-art survey. Using the general social surveys from the United States and Germany enabled us to compare the changes in nonresponse bias between contexts of low (Germany) and high (United States) participation rates. For instance, in 2012, the ALLBUS had a response rate of 37.6 percent, while the GSS reported 71.4 percent.

The ALLBUS is a biennially fielded face-to-face cross-sectional survey based on a representative sample of the German population older than 18. The cumulative data set for Germany included 17 surveys (1980–2012). In the United States, the GSS was mostly annually fielded until 1994—without surveys in 1979, 1981, 1992, and 1993—and biennially after 1994. Similar to the ALLBUS, the GSS is a representative sample of the population of the United States older than 18. To further ensure comparability between both countries, we created a cumulative data set of 16 surveys covering the even years between 1980 and 2012 for the GSS. The questionnaires of both survey programs cover different topics of social science research.

To operationalize the respondents' cohorts, we used information on the respondent's age to assign each respondent to 1 of the 11 birth cohorts (before 1901, 1901–1910, 1911–1920, 1921–1930, 1931–1940, 1941–1950, 1951–1960, 1961–1970, 1971–1980, 1981–1990, 1991 or later). In a second step, we calculated the relative frequency of cohorts per survey (i.e., one marginal distribution per survey).

In both surveys, design weights that account for unequal probabilities of selection were applied for calculating the surveys' cohort distributions. That is, for the ALLBUS, east–west transformation weights (that were provided with the data set) had to be used because respondents in Eastern Germany were more likely to be selected. For the GSS, a transformation weight was created (cf. Stephenson 1978) that corrects for unequal selection probabilities due to different household sizes. In addition, the oversample of blacks in 1982 was excluded.

To determine the underrepresentation/overrepresentation of a cohort in a survey and, hence, the nonresponse bias for respective cohorts, we used data from the U.S. Census Bureau and the German Federal Statistical Office.[3] Based on the number of U.S./German residents older than 18, we calculated the true relative frequency of cohorts for each year in our observation period for each country (i.e., one marginal distribution per country per year).

Dissimilarity indices are well-known measures of the differences between two distributions and can be interpreted as the number of percentage points one distribution needs to be changed to resemble the other distribution (Duncan and Duncan 1955). When comparing the relative frequencies $r$ of $c$ cohorts between the $i$th ALLBUS or GSS ($r^s_{ci}$) to the respective target population ($p_{ci}$), *dissimilarity* can be defined as

$$D_i^* = \sum\nolimits_1^c \frac{|r^s_{ci} - p_{ci}|}{2},$$

where $\frac{|r^s_{ci} - p_{ci}|}{2}$ denotes the dissimilarity of a specific cohort, which is referred to as $d_{ci}$.[4] This cohort-specific dissimilarity indicates a misrepresentation of the respective cohort and, therefore, a nonresponse that is problematic for this group of respondents (i.e., bias).[5]

The dissimilarity index $D_i^*$ does not directly acknowledge the number of observations of each cohort. Instead, it relies on relative frequencies. Thus, $D_i^*$ treats each category $c$ (i.e., cohorts) equally. Note that the use of this index may result in small cohorts that indicate a high dissimilarity. Accordingly, we propose a corrected dissimilarity index $D_i$ that is the sum of cohort-specific dissimilarities adjusted for the true population share $p_{ci}$ of the respective cohort

$$D_i = \sum\nolimits_1^c p_{ci} \times d_{ci}.$$

To discriminate WCC or BCC, Firebaugh (1997) has proposed a reformulation of a decomposition method first introduced by Kitagawa (1955) and advanced by Das Gupta (1978). Typically, this method is used to identify group-related inequality, for example, unemployment rates between different ethnical groups. Yet, as Firebaugh shows, it can be reformulated to decompose change. Therefore, we describe the change in dissimilarity between two surveys $i = 0$ and $i = 1$ (i.e., changing nonresponse bias) as

$$\Delta D = D_1 - D_0.$$

Describing the corrected dissimilarity indices in full detail gives

$$\Delta D = \sum\nolimits_{1}^{c} p_{c1} \times d_{c1} - \sum\nolimits_{1}^{c} p_{c0} \times d_{c0}.$$

In addition, the manipulation of the formula according to Kitagawa (1955) and Das Gupta (1978) as proposed by Firebaugh (1997) leaves us with

$$\Delta D = \underbrace{\sum\nolimits_{1}^{c} \frac{p_{c1} + p_{c0}}{2} \times \Delta d_c}_{\text{WCC}} + \underbrace{\sum\nolimits_{1}^{c} \frac{d_{c1} + d_{c0}}{2} \times \Delta p_c}_{\text{BCC}},$$

where $\Delta d_c$ denotes how the dissimilarity of cohort $c$ changes between two surveys and $\Delta p_c$ is the degree to which the cohort's population share changes between two surveys.

Applying the decomposition to change identifies two components of change: WCC and BCC. On the one hand, WCC describes the share of change that can be attributed to a change in the willingness of the same cohorts to participate. This change refers to what we typically know as age and period effects. For instance, assume that at a given point of time, a survey's topic would gain huge interest and, thus, participation would be judged as important. In this example, cohorts' response propensities would rise compared to previous studies what might result in a decreasing bias. On the other hand, BCC is the sum of change that originates from the substitution of older by younger cohorts. This component captures differences between the cohorts' response propensities and how variation in a population's composition affects change.

Both components' signs can be straightforwardly interpreted. A positive sign indicates that the respective component contributed to increasing bias and vice versa. For example, a positive WCC would indicate that specific cohorts of the population became increasingly reluctant to participate in surveys, which led to an increasing dissimilarity over time (i.e., an increasing nonresponse bias) or that one survey lacked the tools to convert individuals of a specific cohort into respondents. If all the cohorts of a population became more reluctant to the same degree, the overall bias will not change, since the degree of the overrepresentation and underrepresentation of each cohort remains the same.

A positive BCC would indicate that older cohorts with low dissimilarity are leaving the population, while younger cohorts with higher dissimilarity are entering the population. Put differently, in this case, the respondents from the newly entering cohort would show lower response propensities, while the older cohorts are more likely to participate in a survey. Accordingly, the older cohorts vanish, whereas the younger cohorts emerge and are even more underrepresented than their predecessors. Thus, due to population reproduction, the nonresponse bias for surveys would increase over time (i.e., the BCC is positive).

The overall change is the result of summing up WCC and BCC. Different signs may balance out and, hence, a trend in WCC could be hidden by an opposite trend in BCC and vice versa.

In the context of the proposed method, the Online Appendix of the present study features two supplemental sections. In the first section (Online Appendix A.1), we present a generalized form of the formulas that can be applied to other variables in order to assess nonresponse bias conditional for cohorts and, thus, to decompose changes in a variable's nonresponse bias into WCC and BCC. The second section (Online Appendix A.2) provides an illustrative example that walks the reader through the necessary calculations. This example is based on real data that we used in our analysis (ALLBUS 1998 and 2000).

It needs to be noted that the decomposition method does not overcome the identification problem of age–period–cohort analysis, and mixed effects are possible (for a critical discussion of the method, see Firebaugh 1990; Glenn 2005; Rodgers 1990). However, we agree with Firebaugh (1997:22)—with respect to our research question, it is of interest "(h)ow much social change comes from alteration of opinions, and how much comes from the replacement of older adults with younger ones" and, hence, the decomposition method can yield valuable results.

To identify whether a change in willingness or cohort replacement contributed to a change in nonresponse bias, we compared the relative frequencies of each cohort by using the respective general survey data and the true population values provided by the statistical offices. Hence, we created 363 data points for $d_{ci}$ (11 cohorts, 33 surveys). Additionally, we used the true population values to calculate the population share of each cohort ($p_{ci}$). To assess change—and its underlying mechanisms—we calculated $D_i^*$, $D_i$, and decompositions of $\Delta D$ for the transition of each survey to the next (e.g., 1980–1982, 1982–1984). In a series of additional analyses, we tried to widen the scope of the decomposition. Thus, we divided the observation time into four equally sized periods of 8 years (1980–1988, 1988–1996, 1996–2004, 2004–2012), two periods of 12 years (1988–2000, 2000–2012), and one period of 18 years (1994–2012). For each of these periods, we—again—decomposed change in the nonresponse biases. The longer-term comparisons account for a larger set of cohorts compared to focusing on subsequent surveys. That is because they are more likely to incorporate cohorts that

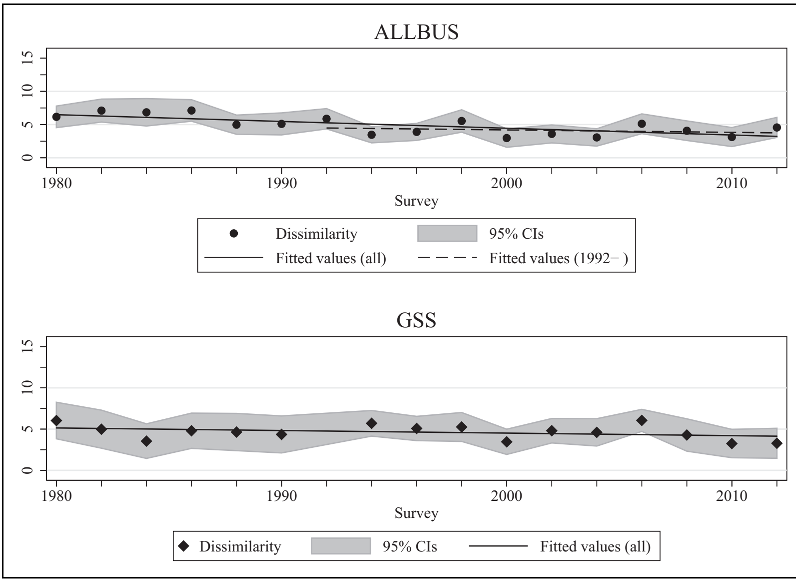


**Figure 1.** Dissimilarity indices over time with fitted values for ALLBUS and GSS (1980–2012).

were part of the prior survey but were fully replaced until the more recent survey and cohorts that were not part of the population when the prior survey was conducted. For example, when comparing the surveys of 1994 and 2012, the cohort born between 1981 and 1990 was not eligible to participate in the survey of 1994.

## Results

Before addressing the results of the decompositions, we will show how nonresponse bias (i.e., dissimilarity) in cohort distributions changes over time. Figure 1 presents dissimilarity indices ($D_i^*$) for each survey from 1980 to 2012 with bootstrapped 95 percent confidence intervals. To gain a better understanding of a potential trend, we fitted regression lines to the observed values. For Germany, the first regression line considers all surveys, while the second discards all data prior to the German reunification. Since 1992, the ALLBUS has aimed at representing the German population, including Eastern Germany. However, even before 1992, we only saw a minor variation in $D_i^*$ over time. An analysis of this trend over time suggests that biases are declining but to a very low degree ($\beta = -0.102$, $p < .05$).

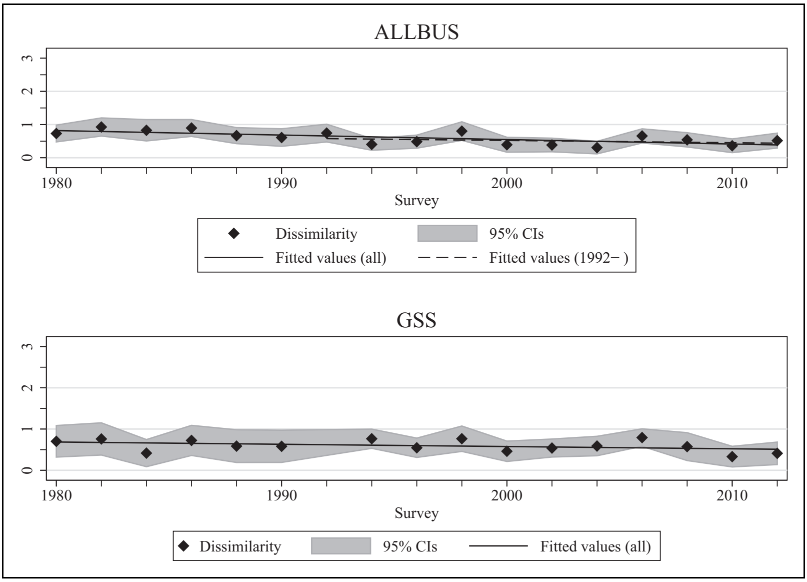


**Figure 2.** Corrected dissimilarity indices over time with fitted values for ALLBUS and GSS (1980–2012).

Focusing on the German population after 1992, we do not find the slightly decreasing level of dissimilarity to be significant ($\beta = -0.037$, $p > .05$), and thus assume that the bias has remained stable after 1992.

For the United States, we see a similar trend in the cohort distribution's nonresponse bias, which seems to be stable over time ($\beta = -0.031$, $p > .05$). Finding similar trends in both countries is remarkable, given the fact that they are known to differ strongly in their survey response rates.

One may argue that the dissimilarity indices are sensitive to bias in small cohorts, since cohort sizes (i.e., the size of the respective category $c$) are not directly controlled for. Therefore, we suggest the use of a corrected dissimilarity index $D_i$ that accounts for the relative size of each cohort in the population. Figure 2 shows the trend of nonresponse bias in cohort distributions as indicated by the corrected index. First, we see that the overall degree of bias becomes smaller in both plots compared to Figure 1. This result suggests that mostly small cohorts, which do not account for larger shares of the population, are misrepresented. The overall degree of bias in cohort distributions already seemed small without the adjustment. Consequently, we believe this result to be an encouraging finding for both general social

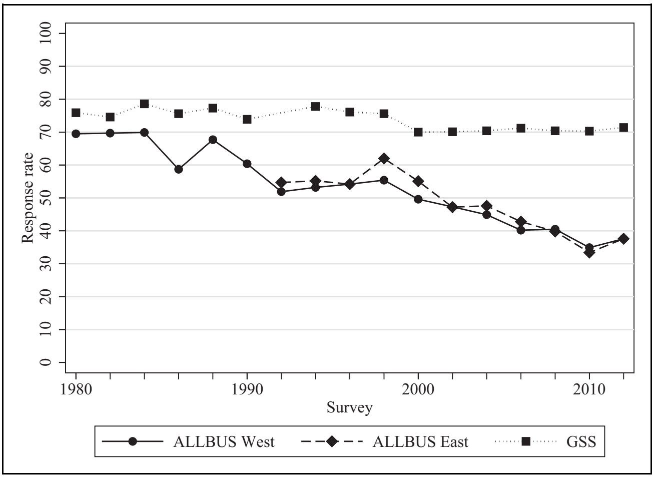


**Figure 3.** Response rates of the ALLBUS and GSS (1980–2012).

surveys: The cohort distribution as an important variable can be ascribed with good quality as far as nonresponse bias is concerned. Second, the slightly negative trend in biases is—again—reflected in the data. Nonresponse biases in cohorts have only slightly decreased over time in Germany ($\beta = -0.013$, $p < .05$, after 1992: $\beta = -0.007$, $p > .05$), and in the United States, they have moved strongly toward stability ($\beta = -0.005$, $p > .05$).

The trends in the biases of cohort distributions confirm the findings from previous research that high nonresponse rates do not necessarily result in higher nonresponse biases (e.g., Groves 2006). Figure 3 shows the response rates for the 17 ALLBUS surveys separated by East and West Germany and for the 16 GSS surveys.[6] Also, in line with previous research, nonresponse rates are increasing (Curtin et al. 2005; de Leeuw and de Heer 2002; Groves and Couper 1998; Stoop et al. 2010). At the same time, our data do not reflect this trend in terms of an increasing misrepresentation of cohorts.[7] This finding is supported by regressing the response rates of the surveys on the dissimilarity indices. The response rates after 1992 for Eastern ($\beta = 0.029$, $p > .05$) and Western Germany ($\beta = 0.030$, $p > .05$) do not significantly correlate with dissimilarity nor do the response rates in the United States since 1980 ($\beta = 0.099$, $p > .05$). Replicating the analyses based on the corrected indices supports this finding (Eastern Germany: $\beta = 0.006$, $p > .05$; Western Germany: $\beta = 0.007$, $p > .05$; United States: $\beta = 0.016$, $p > .05$).

The description of the proposed decomposition methods highlights the important fact that the total change between two points of time consists of at

**Table 1.** Decompositions of Changes in Corrected Dissimilarity Indices.

| | ALLBUS | | | GSS | | |
|---|---|---|---|---|---|---|
| Period | WCC | BCC | Change (*SE*) | WCC | BCC | Change (*SE*) |
| 2-Year interval (survey to survey) | | | | | | |
| 1980–1982 | .227 | −.031 | .196 (.190) | .028 | .030 | .058 (.278) |
| 1982–1984 | −.063 | −.036 | −.099 (.218) | −.349 | .005 | −.344 (.257) |
| 1984–1986 | .074 | −.005 | .069 (.208) | .278 | .030 | .308 (.247) |
| 1986–1988 | −.234 | .004 | −.230 (.177) | −.161 | .022 | −.139 (.276) |
| 1988–1990 | −.029 | −.028 | −.057 (.183) | −.003 | −.001 | −.004 (.281) |
| 1990–1992 | .160 | −.025 | .135 (.193) | | | |
| 1992–1994 | −.325 | −.016 | −.341 (.162) | | | |
| 1994–1996 | .081 | .001 | .082 (.134) | −.232 | .013 | −.219 (.167) |
| 1996–1998 | .339 | −.022 | .317 (.174) | .230 | −.011 | .220 (.199) |
| 1998–2000 | −.391 | −.019 | −.410 (.184) | −.295 | −.009 | −.305 (.200) |
| 2000–2002 | .003 | −.011 | −.008 (.154) | .051 | .027 | .078 (.167) |
| 2002–2004 | −.071 | −.008 | −.078 (.141) | .021 | .028 | .049 (.162) |
| 2004–2006 | .349 | .003 | .351 (.146) | .175 | .029 | .205 (.161) |
| 2006–2008 | −.106 | −.011 | −.117 (.156) | −.234 | .015 | −.220 (.204) |
| 2008–2010 | −.163 | −.016 | −.179 (.151) | −.238 | −.005 | −.243 (.214) |
| 2010–2012 | .165 | −.009 | .156 (.155) | .065 | .015 | .080 (.189) |
| 8-Year interval | | | | | | |
| 1980–1988 | −.097 | .032 | −.064 (.178) | −.152 | .035 | −.117 (.281) |
| 1988–1996 | −.111 | −.070 | −.181 (.159) | .057 | −.097 | −.040 (.235) |
| 1996–2004 | −.111 | −.068 | −.179 (.139) | .091 | −.048 | .042 (.172) |
| 2004–2012 | .224 | −.013 | .211 (.151) | −.190 | .012 | −.177 (.185) |
| 12-Year interval | | | | | | |
| 1988–2000 | −.183 | −.091 | −.274 (.169) | −.034 | −.091 | −.125 (.238) |
| 2000–2012 | .149 | −.025 | .124 (.162) | −.082 | .032 | −.050 (.189) |
| 18-Year interval | | | | | | |
| 1994–2012 | .152 | −.038 | .114 (.145) | −.376 | .021 | −.354 (.185) |

*Note:* Bootstrapped standard errors (*SE*) were estimated based on 5,000 replications. WCC = within-cohort change; BCC = between-cohort change.

least two components of change (WCC and BCC). Both components may contribute to change in opposite directions and absorb each other. Thus, high degrees of change may occur in one of the components but remain hidden if we solely focus our attention on the total change. Therefore, additional analyses of the changes in cohort distributions' nonresponse biases are required.

Table 1 summarizes the results of decomposing the changes in cohort distributions' nonresponse biases into WCC and BCC. In the first step, we focus on the findings for shorter periods of time (i.e., comparing subsequent surveys). With few exceptions, we have found that WCC and BCC both contribute to changing nonresponse bias in cohort distributions on levels that do not dramatically differ from the total change. Along with this first finding, we saw that WCC, in the vast majority of cases, greatly exceeds BCC, which indicates that the changes in nonresponse biases are mainly driven by the change of the willingness to participate over time. The target population of a specific survey was more reluctant or less reluctant toward participating in a survey, and presumably this result was independent from any impact of cohort replacement. This finding holds true for both countries. Despite the fact that German surveys generally suffer from very low response rates compared to the U.S. responses, our findings for both countries did not hint at cohort replacement being the driving factor for the changes. If a bias changes between surveys, it is due to periodic shocks (i.e., events) or age-related factors that hinder/promote participation as our results suggest. Yet, we did not find a consistent pattern in the direction of these effects. That is, in one year, WCC is positive, and in the following year, it is negative (ALLBUS: 8 negative out of 16; GSS: 7 negative out of 14). In the case of age effects, we would expect the effect to be homogeneous over time, even if its magnitude is as small as in our analysis. Thus, we did not find that specific cohorts were increasingly prone to nonresponse over their course of life, rather only between two subsequent surveys. These findings strongly hint at the important role of periodic effects, which are the manifestation of two different sources of changing nonresponse: the survey climate and—related to that—data collection protocols (i.e., survey design). We are not able to strictly disentangle these two effects in the present analysis, and so further investigation of the topic is needed.

Keeping this constraint in mind, changes in the ALLBUS methodology between 1992 and 1994, 1996 and 1998, and 1998 and 2000 provide us with an illustrative example for change in nonresponse bias that is most likely the result of adjusting the survey design (i.e., periodic WCC). Between 1980 and 2012, the ALLBUS used two different sampling techniques: Arbeitskreis Deutscher Markt- und Sozialforschungsinstitute (ADM) sampling and register sampling. ADM sampling is a three-stage sampling process that includes random route as a second stage and a Kish selection grid as a third stage. For register sampling, the target persons (i.e., the sampling frame) are directly drawn from the population registers of selected municipalities (two-stage sampling). Register sampling is generally considered advantageous to ADM sampling in Germany (Häder 2015:157). As Häder (2015) argues, the latter is more prone to problems (among others) with respect to sampling the relevant cases and field monitoring. Consequently, we assume that switching to the "better" sampling design (i.e., register) will result in a reduction of nonresponse bias. The ALLBUS relied on ADM sampling from 1980 to 1992 as well as in 1998. In all other years, register samples were used. Accordingly, the periods 1992 to 1994, 1996 to 1998, and 1998 to 2000 are transitions between the two sampling techniques. For each transition from ADM to register sampling (1992–1994, 1998–2000), we found the WCC to be highly negative compared to other periods (both $\Delta D > 0,\ p < .05$). After switching from a register back to an ADM sample in 1996 to 1998, the nonresponse bias increased (i.e., positive WCC) to a relatively high level ($\Delta D > 0,\ p < .1$). BCC remained of minor importance for each of these periods. Put bluntly, switching to the better sampling technique seems to have decreased the nonresponse bias, whereas switching in the opposite direction resulted in an increasing bias. This example illustrates how changes in survey design may drive changes in nonresponse bias.

For the United States, our results suggest a stability of nonresponse bias in cohort distributions for both the WCC and BCC components. This finding contrasts with the significant changes we reported for the ALLBUS. While the GSS relied on different sampling frames that were created in 1980, 1990, and 2004, these different sampling frames are all based on one or more stages of area sampling due to the "absence of any satisfactory population register in the USA" (Smith, Marsden, and Hout 2015:2103). In addition, the GSS has had other methodological innovations, for instance, providing a Spanish-language version since 2006 and switching the survey mode from paper and pencil to computer-assisted personal interviewing in 2002. However, for none of these years did we find an effect on nonresponse bias similar to the changes observed in Germany.

Drawing on longer periods (Table 1, bottom) further supports our general findings. WCC exceeds the magnitude of BCC in all but two cases (GSS, 1988–1996 and 1988–2000). The robustness of these findings is somewhat surprising because using longer periods to decompose change results in BCC becoming increasingly important as cohort replacement ($\Delta p_c$) obviously becomes larger. However, the results remain stable and, again, we found changes in bias to be erratic—what hints at the influence of periodic effects. As argued before, this finding may be the result of the factors in the respondent's decision-making functions to be affected by the specific context of each survey.

## Conclusion

Analyzing and decomposing the trend of cohort distributions' nonresponse biases provide us with deeper insights about how to generally assess changes in nonresponse bias over time. Increasing nonresponse rates and unknown trends in nonresponse biases are what currently troubles survey methodology and potentially compromises substantive analyses.

First, our results, based on the case of cohort distributions, are in line with the finding that the general trend in declining survey participation does not necessarily manifest in an increasing nonresponse bias. The trend may endanger statistical power due to lower case numbers, but our estimators are not necessarily subject to nonresponse biases. In the analyses, we focused on biases in the surveys' cohort distributions, since cohort distributions are a frequently used variable in the field, age and period are functionally related, and age–period–cohort analyses are based on this variable. Accordingly, a bias or an increasing bias can be considered an important issue. With respect to the ALLBUS and GSS, we did not find a trend of increasing bias in cohort distributions between 1980 and 2012. On the contrary, signs were even present for a slight decrease in nonresponse biases. While this may be an encouraging result, comparing survey estimates (based on cohorts) over time may be affected by this changing bias. In other words, in more current surveys, the estimators are less biased than in older surveys. This may result in variation that is solely the effect of changing bias, although it is misinterpreted as substantive change between two points of time. Consequently, our findings highlight the importance for survey programs to provide their users with the tools to correct for this (admittedly) slightly changing bias. Adjusting the cohort distribution of each survey to the population's true distribution for each year seems like a viable way to address this issue.

Second, our findings indicate that the major part of change in cohort distributions' biases can be attributed to a change in the survey climate between surveys. This finding is in line with an assumption made by Brick and Williams (2013) who followed the trend of different response indicators over time and discussed the negative impact of survey climate in society as an explanation for declining participation. Our findings suggest that cohorts differ in their willingness to participate in surveys and, hence, a bias exists, although the emergence of new cohorts does not worsen this issue. The major mechanisms behind changes in nonresponse bias (in a positive or negative sense) are periodic effects, since we have found that the change within cohorts is heterogeneous over time. For example, possible sources of periodic effects might be public debates that hinder survey participation or the design features of a survey that influence the participation of specific cohorts.

Third, the finding that mostly period effects determine whether a given bias changes is important for the development of new methods. In light of the continuing discussion on nonresponse, more flexible data collection protocols are being more frequently used (cf. Dillman et al. 2014). These protocols enable us to better adjust surveys to contextual conditions or to apply special treatments to groups of respondents, for example, by providing respondents alternative modes in a sequential mixed-mode design (e.g., Dillman et al. 2009) or offering incentives (e.g., Pforr et al. 2015; Singer and Ye 2013). However, it should be clear that using such a treatment does not necessarily solve the problem and may introduce other sources of error (e.g., for mixed methods, see de Leeuw 2005; Revilla 2010). Our findings indicate that methodological development should center its efforts on creating methods that focus on function rather than cohorts. In other words, practitioners need to assess the current survey climate and identify the requirements for participation. For example, if data privacy considerations are a huge debate in society, interviewers can be trained to counter these fears. Methods that meet these requirements may be applied to different cohorts as soon as the respective *conversion function* is needed. In contrast, strapping a method to a selected cohort would severely hinder its applicability to other cohorts, a limitation that is not needed, as our findings suggest.

The study we have presented is not without some limitations, which highlight further research opportunities regarding the consequences of nonresponse and their evolution over time. First, we focused on bias in cohort distributions due to its important role for social science research and the availability of external reference distributions. Our approach to assessing trends in bias and evaluating the role of changing opinions and cohort replacement seems to be applicable to other variables of interest or different indicators of bias (Online Appendix A.1). Thus, it may be worthwhile to identify a different set of variables of interest and then replicate our analyses if reference distributions (conditional for cohorts) are available. Second, we proposed an analytical approach that draws on external reference distributions to measure nonresponse bias rather than drawing on sampling frames to calculate indicators such as response propensities. The rationale for this decision was that gross samples are seldom easily accessible—even more so for very long periods of time (as investigated in the present study) for which we have to rely on old surveys. Future studies with access to gross samples (with auxiliary information) could move this line of research further forward by applying more sophisticated methods to analyze trends in nonresponse biases, for instance, by applying cross-classified random effects age–period–cohort models (Yang and Land 2006, 2013). However, the approach of the present study was developed to be applicable to most surveys for which these data are not—or only partially—available. Third, we drew on two long sequences of repeated cross-sectional face-to-face surveys fielded in Germany and the United States. This approach enabled us to compare two extremes in terms of response rates—Germany with its low rates and the United States with its high rates. Further, both survey programs gave us an opportunity to follow trends over a long period of time due to the same target populations, similar methodology, and survey topic. Still, it may be interesting to extend the approach of this article to different countries and surveys. When doing so, future studies could extend the analysis to cover other modes that have become increasingly popular over the past decades (e.g., telephone or web surveys). Investigating mode-specific differences in trends in biases could help to identify modes that are worthwhile to be the focus of additional research efforts. The method proposed in this article constitutes a viable way to assess changing nonresponse bias for all variables for which researchers are able to obtain the required reference distributions over a period of time.

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## Notes

1. While evidence exists (e.g., Groves 2006) that low response rates do not necessarily lead to nonresponse bias, we cannot infer from this finding that nonresponse biases are stable over time.
2. The three factors are interrelated as follows: age = period − birth year, birth year = period – age, and period = birth year + age.
3. Age distributions of both populations are available online: United States: http://www.census.gov/population/international/data/idb/ (accessed March 30, 2015); Germany: https://www-genesis.destatis.de/genesis/online (accessed February 14, 2015).
4. To allow for the more intuitive interpretation of the indices (i.e., percentage points), each $d_{ci}$ was multiplied by 100.
5. Dissimilarity indices often are used as a merely descriptive indicator of inequality. If they are to be used for inference, it is possible (but not straightforward) to calculate measures of uncertainty, for instance, by relying on resampling methods. In the present article, we applied bootstrapping with 5,000 replications to calculate standard errors for $D$.

6. Response rates are reported as provided in each survey's documentation. Calculations for GSS (Smith et al. 2015) and ALLBUS (Blohm and Koch 2013) correspond to response rate 5 (RR5) of the American Association for Public Opinion Research (2015) standard definitions.
7. This result confirms our assumption that using the dissimilarity in cohort distributions provides an indicator that draws on the logic of nonresponse bias. Consequently, although this indicator is less sensitive for the overall magnitude of response rates, it identifies the problems caused by declining participation.

## Supplemental Material

Supplemental material for this article is available online.

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