Lost on the Way: Nonresponse and its Influence on Turnout Bias in Postelection Surveys

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Abstract

Earlier studies on turnout bias in postelection surveys have focused on vote over-reporting (measurement bias) and have disregarded nonresponse bias associated with voter overrepresentation. Based on a unique data set of validated votes collected for two different ballots, we offer an in-depth analysis of the size and sources of nonresponse bias. On the aggregate level, we find that voter overrepresentation accounts for a larger share of total turnout bias than misreporting. On the individual level, past voting record, ballot vote, and age appear as strong predictors of survey participation. Breaking down further the analysis, we show that these factors contribute to nonresponse bias through both contact and cooperation. By contrast, the effects of some other factors work at cross-purpose in the contact and cooperation stages.

Overestimation of turnout is a classic flaw in postelection surveys. It has a double cause: Voter overrepresentation among survey respondents (nonresponse bias), and misreporting of respondents claiming that they voted whereas they did not (measurement bias).[1] In the late 1980s, the American National Election Studies (ANES) directors decided to abandon the vote validation procedure, that is, the matching of self-reported turnout with official records, arguing that government record checks contained so many errors that they were not more valid than the self-reports of survey respondents (Berent, Krosnick, & Lupia, 2011). They reaffirm this view in a recent report based on the 2008 ANES turnout validation exercise, and claim that the upward trend in turnout bias in ANES does not stem from lying or memory failure, but from the increased overrepresentation of

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[1]The concepts of "nonresponse bias" and "measurement error" are central to survey participation literature, whereas vote validation studies use the more specific concepts of "overrepresentation of voters" and "misreporting" (or "overreporting"). In this article, we use both interchangeably.

actual voters among survey respondents (Berent et al., 2011, see also Burden 2000, p. 389).

This view is, however, not dominant. According to a competing and wide-spread conception, turnout bias is primarily a function of vote overreporting and only secondarily a function of voter overrepresentation (e.g., Ansolabehere & Hersh, 2012; Deufel & Kedar, 2010). The extensive—mainly U.S.—literature has advanced our understanding of overreporting (Belli, Traugott, & Beckman, 2001; Bernstein, Chadha, & Montjoy, 2001; Cassel, 2003; Deufel & Kedar, 2010; Katz & Katz, 2010; Presser & Traugott, 1992; Sigelman, 1982; Silver, Anderson, & Abramson, 1986). However, we still know little about the size and determinants of voter overrepresentation.

Admittedly, the analysis of voter overrepresentation requires reliable information about the validated votes of both citizens who were interviewed and who were not. This condition is rarely fulfilled. Existing vote validation studies focus on survey respondents, but do not validate the votes of nonrespondents. Given that both voter overrepresentation and vote overre-porting contribute to the overestimation of turnout in surveys, focusing on misreporting and ignoring nonresponse obviously misses an important component of the analysis of turnout bias.[2]

Against this background, the purpose of this article is to offer an in-depth analysis of the size and sources of nonresponse bias. To that end, we use a unique data set collected on two distinct elections, which provides highly reli-able information on validated turnout of both survey respondents and nonre-spondents. This enables us to separate nonresponse bias from vote measurement error and to evaluate how they each account to total turnout bias. The data set also allows for a fine-grained analysis of the two constitutive elements of survey participation, namely contact and cooperation. Moreover, we take advantage of the strong differences in response rate existing between the two surveys to explore in how far these differences matter for turnout bias. Finally, to our knowledge our Swiss study is one of the few that analyzes turnout bias outside the United States, which will help us to evaluate whether and to what extent our results conform to—or contrast with—existing findings.

Theory

Survey Participation, Nonresponse Bias, and Total Turnout Bias

Overestimation of turnout in surveys results from both nonresponse bias (voter overrepresentation) and measurement bias (vote overreporting).

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[2]In a companion paper, we take a closer look at the relationship between voter overrepresentation and vote overreporting. To that end, we apply a Heckman selection model in which the outcome equation estimates the probability of overreporting conditional on the probability of participation in the survey (Sciarini & Goldberg, forthcoming).

According to a widespread view, measurement error accounts for the bulk of total turnout bias (e.g., Ansolabehere & Hersh, 2012; Deufel & Kedar, 2010). In Ansolabehere and Hersh's (2012) data from the Cooperative Congressional Election Study (CCES), overreporting accounts for roughly two thirds of total turnout bias, and voter overrepresentation for only one third. The respective share of voter overrepresentation and vote overreporting is more balanced in the ANES data, where in some years nonresponse contributed to half of total turnout bias—or even more (Burden, 2000; Deufel & Kedar, 2010, p. 294). Differences in sampling design may account for the varying share of nonresponse and measurement biases between CCES and ANES data.[3] However, the nonresponse bias is sizeable in both cases and, therefore, deserves a closer look.

Differences in citizens' propensity to participate in a survey, which result in nonresponse bias, may have various reasons. In general, participation in a survey is a function of accessibility to be contacted, and of the ability and willingness of contacted persons to cooperate (Goyder, 1987; Groves, Cialdini, & Couper, 1992; Groves & Mathiowetz, 1984; Quinn, Gutek, & Walsh, 1980; Smith, 1983; Stoop, 2005). On the one hand, the determinants of accessibility and ability to participate in a survey are fairly well-known. Different survey modes such as face-to-face and telephone surveys share some commonalities in that respect, in particular with regard to the effects of age, civil status, or professional status. Young persons, working people, and singles who are rarely at home are harder to *contact* than married people and the elderly. While age generally has a positive influence on the likelihood of contact, empirical records show that beyond a certain age threshold, health problems and social isolation decrease the *ability* to respond to surveys. Online surveys, for their part, face specific problems of coverage, such as the underrepresentation of older people and of people without Internet access (e.g., Atkeson, Adams, & Alvarez, 2014).

On the other hand, contacted persons' *willingness* to participate in a survey and answer questions of interest is likely to vary across surveys, for example, depending on the survey topic. Groves (2006; also Groves & Peytcheva, 2008) has put forward several models relating the likelihood that members of a sample will participate in a survey to the probability that they will answer survey questions. According to the "common cause model," the same causal factors may influence both survey participation and the response to some survey variables, thus generating a covariance between the two attributes. Groves, Singer, & Corning's (2000) leverage-salience theory further helps to specify the conditions under which variations in response propensity tend to result in nonresponse bias (see also Groves & Peytcheva, 2008; Groves,

[3]Most noticeably, the ANES is a probability sample, whereas the CCES is an opt-in, nonprobability sample. Nonprobability samples notoriously overrepresent politically engaged persons (e.g., Chang & Krosnick, 2009; Yeager et al., 2011).

Presser, & Dipko, 2004). This theory states that a given survey attribute (e.g., its topic) has different "leverages" on the survey participation decision, depending on the importance that the sampled people assign to the attribute. Whether or not the attribute is made salient to the sampled person (e.g., at the start of the interview) will further influence the activation of the potential leverage.

When a factor with great leverage on the survey participation decision is also an item of survey measurement, survey statistics based on that factor are likely to have a large nonresponse bias. Applying the leverage-salience argument to the topic of our study, it is safe to argue that participation in a postelection survey has a (far) greater leverage among voters than nonvoters. As Brehm (1993, p. 70) puts it, participation in a survey about politics and political participation are two sides of the same coin, meaning that people who vote more are also more likely to participate in surveys about politics.

Socio-demographic characteristics that relate to political participation are also likely to influence willingness to respond to a postelection survey. Age is again one of these factors. Political interest and information, as well as social integration, increase with age, which leads to higher political involvement (e.g., Riley, Foner & Waring, 1988) and, arguably, higher survey participation. A similar reasoning applies to another factor relating to social integration, namely, residence duration (e.g., Cassel & Hill, 1981; Squire, Wolfinger, & Glass, 1987). Citizens who have lived in the same place for a long time care more about (local) politics and are, therefore, more likely to participate both in politics and in surveys about politics. In summary, we assume that participation in postelection surveys is higher among voters, middle-aged people, married persons, and long-term residents.

Response Rate, Nonresponse Bias, and Measurement Error

According to the "continuum of resistance," the likelihood of having nonvoters in a survey increases with a higher response rate (Burden, 2000). Given that nonvoters are usually reluctant to participate in political surveys, increasing the response rate should help to increase the share of nonvoters in the realized sample and, therefore, to reduce voter overrepresentation. Some scholars have, however, questioned the "continuum of resistance." They argue that increasing the response rate is likely to increase the share of nonvoters only if the response rate approaches 100%. Below that percentage, increasing response rate may in fact have the reverse effect, namely, to bring an even more disproportionate share of active voters in the sample (Lin & Schaeffer, 1995; Selb & Munzert, 2013). Empirically, findings from survey research in various fields suggest that a low response rate does not necessarily lead to higher nonresponse bias (e.g., Curtin, Presser, & Singer, 2000; Groves et al., 2004; Groves, 2006; Groves & Peytcheva, 2008).

Olson (2006) has extended the "common cause model" to relationships between response rate and measurement error. She argues that the same factors that influence the propensity to participate in a survey may also influence the propensity to respond inaccurately to survey questions. As a result, efforts to increase response rate may come at the price of increasing measurement errors, as more citizens with something embarrassing to report will be included in the survey. In the present context, increasing the response rate may reduce voter overrepresentation, and yet, it may also increase the number of embarrassed nonvoters prone to overreport among survey respondents (Selb & Munzert, 2013; Tourangeau & Yan, 2007; Tourangeau, Groves, & Redline, 2010). The comparison of two surveys with different response rates will help to check whether these expectations hold.

Data

Having recorded values available for estimating nonresponse bias is rare (Olson, 2006, p. 752). This holds all the more true for turnout data. In that sense, our two data sets are exceptionally rich. They include validated turnout data for the whole sample, that is, for both survey respondents and nonrespondents. The data sets were both collected in the canton of Geneva, Switzerland, the first in the context of the October 2011 national elections and the second in the following year in the aftermath of a compulsory referendum on the new cantonal Constitution. In both cases, the cantonal administration drew a random sample of Geneva's citizens from the official vote registry. This served as the basis for a telephone survey carried out by a survey company. The initial sample comprised 1,500 individuals in the first survey and 2,500 in the second. In Switzerland, citizens do not need to register to vote. Therefore, the official vote registry includes all eligible voters.

After the survey fieldwork was completed, the resulting file was sent to the cantonal administration that added the information regarding actual turnout.[4] While most validation studies use official records to ex post verify self-reported votes, which is error-prone (Berent et al., 2011),[5] our validated data are highly reliable. To the extent that the official register was used as the sampling frame from the start, no matching was required. In addition, vote validation was easy and virtually error-free, as it occurred through an electronic reading of the voting card that each voter had to sign and send back by postal mail or put in the ballot box on Election Day. The files comprising information about both

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[4]We thank the Service of Votes and Elections (SVE) and the Statistical Cantonal Office (OCSTAT) for providing us with the validated turnout data.

[5]According to Berent et al.'s (2011) analysis of several large American states, official government records of registration and turnout contain numerous errors with strong variations across states. Common means of validation underestimate registration and turnout rates because they fail to locate records for respondents who were classified as not registered when they were registered.

reported and actual turnout at the 2011 national election and the 2012 referendum, respectively, were then de-identified (anonymized) and sent to us.[6]

For each person included in the initial sample, we do not only have information regarding whether she participated in the current vote. We also have information about her actual vote (or abstention) in all previous ballots held in the canton of Geneva between 1996 and 2011–2012, that is, roughly 80 referendums or elections at the national or local level (Sciarini, Cappelletti, Goldberg, & Lanz, forthcoming; Tawfik, Sciarini, & Horber, 2012). More specifically, a citizen's past voting record is measured as the share of ballots (elections and referendums) in which she participated, out of the maximum number of ballots in which she could participate.[7]

The data set also provides information about individuals' key socio-demographic characteristics, namely, age, sex, marital status, and residence duration. Given that age does presumably not have a linear effect, we include age as a simple, square, and cubic term. Marital status distinguishes between singles (ref. category), married, and divorced/widowed persons. Finally, residence duration distinguishes citizens living in Geneva for less or more than 10 years.

Furthermore, the data set also informs about the reasons why a person did not respond to the survey. These reasons were recoded into the two main components of nonresponse, that is, contact and cooperation. We further divided noncontacts into two sub-categories, by distinguishing people for whom we could not find any phone number, from people whom we could not reach. Similarly, we specified the reasons for noncooperation by distinguishing "inability" from "unwillingness" (see Table A1).

Contact, Cooperation, and Nonresponse Rate

While both surveys are based on the same sampling procedure, they differ from one another in terms of sponsorship, level of effort to reduce nonresponse, and interview duration. First, while both surveys were announced in advance with a letter sent to all sampled citizens, the sponsor was different. The 2011 survey took place in parallel to the Swiss national election study. The announcement letter was sent by the University of Lausanne, which hosts the Swiss election studies. By contrast, the 2012 survey was sponsored by the Geneva state government. According to prior studies, government sponsored surveys achieve higher response rates by increasing the commitment felt by sampled persons

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[6]The wording of the turnout question asked in both surveys was as follows: "In general, when there is an election or a direct democratic vote in Switzerland, more than half of citizens do not vote. Last Sunday there was an election/a direct democratic vote. Did you participate in that vote?"

[7]Taking the example of the 2011 survey, for a person residing in Geneva since 1996 the maximal number of ballots equates to the total number of ballots held between 1996 and 2011, namely 79. For a younger person who got his/her voting rights after 1996, or for a person who arrived in Geneva after 1996, the maximal number of ballots is of course lower than 79.

(De Leeuw & de Heer, 2002; Groves & Peytcheva, 2008). Second, the survey company was not the same in both surveys. The company that conducted the 2012 survey made a stronger effort to reduce nonresponse, with measures that aimed to increase contact rate. Sampled citizens for whom we did not find any telephone number benefited from special treatment. Instead of the standard letter announcing the forthcoming survey, these persons received a letter asking for their phone number. Two weeks later, the persons who did not respond received a second letter offering two possibilities. They could either call a hotline to set up an appointment (or do the interview straight away) or fill the questionnaire online through the Internet. The same possibilities were offered to sampled citizens with a phone number but with whom no contact could be established during the first 10 days of fieldwork.

Third, the 2011 survey was highly demanding, as it rested on a long questionnaire. During the introduction, interviewers announced the estimated interview length, 25 min in the 2011 survey and between 10 and 15 min in the 2012 survey. According to past research, increasing the announced interview time reduces response rate in both telephone and web surveys (Galesic & Bosnjack, 2009; Marcus, Bosnjak, Lindner, Pilischenko, & Schuetz, 2007; Roberts, Eva, Allum, & Lynn, 2010). Therefore, the far longer duration of the 2011 survey was likely to have detrimental consequences for the propensity to cooperate.

All mentioned differences arguably account for the sharp difference in response rate between the two surveys (Table 1). From the initial sample of 1,500 persons drawn for the 2011 survey, 810 persons could be contacted and 392 were able and willing to participate in the survey, which results in an AAPOR RR1 of 26.3%. The response rate is thus low, but not unusual for postelection surveys, which commonly display lower response rates than other surveys (Groves & Peytcheva, 2008). The response rate was almost twice as high in our second survey. Of the 2,500 persons included in the initial sample, 1,780 could be contacted and 1,231 could be interviewed (1,067 by phone and 164 by self-administration through the Internet). The AAPOR RR1 thus amounts to 49.2%. We can see from Table 1 that the higher response rate reached in the second survey is a result of both higher contact rate and—even more so—higher cooperation rate. Cooperation is measured and analyzed conditional on successful contact.

While our data set offers full coverage of the voting-eligible population, it does not escape the main source of slippage in a register-based sampling process, namely, incomplete matching between addresses and phone numbers (Green & Gerber, 2006). In both surveys, we were unable to obtain phone numbers for about 20% of the initial sample. However, our data allow us to identify citizens with unknown phone numbers, and to check whether they have a distinct socio-demographic profile. We come back to that issue toward the end of the empirical section.

Table 1

Response Rate, Contact Rate, and Cooperation Rate

Year	Initial sample	Contacts	Contact rate (%)	Interviews	Cooperation rate (%)	Response rate (AAPOR R1) (%)
2011	1,500	804	53.6	392	48.8	26.3
2012	2,500	1,754	70.2	1,231	70.2	49.2

Note that the two surveys differ in an additional way. The 2011 survey related to national elections and the 2012 to a cantonal referendum. However, the differences in institutional ballot type are unlikely to matter in the present case. While national elections are of utmost importance in most countries, this does not hold to the same extent in Switzerland (Bochsler & Wasserfallen, 2013; Franklin, 2004). On the one hand, in the Swiss consensus democracy where the executive power is shared by the main political parties, national parliamentary elections do hardly have any influence on the composition of the national government. On the other hand, as a result of the strong development of direct legislation, elections compete with direct democratic votes. This, in turn, contributes to citizens' "participatory fatigue" and accounts for Switzerland's low turnout in comparative perspective (Franklin, 2004; Lutz & Selb, 2007). At the 2011 national elections, turnout amounted to 44.8% in the Geneva district. The turnout rate regarding the cantonal referendum of October 2012 was even lower (32.5%).[8]

Results

Nonresponse Bias, Measurement Bias, and Total Turnout Bias

Figure 1 presents the size of total turnout bias in the two surveys and shows how much nonresponse bias and measurement bias contribute to that total. The total turnout bias is measured by the difference between reported turnout among survey respondents and validated turnout in the initial sample. It amounts to 22.8 percentage points in the first survey and to 19.4 percentage points in the second. This is a rather high bias from a comparative perspective. In Selb and Munzert's (2013) collection of 128 postelection studies the average difference between sample and official turnouts is 14 percentage points. However, their collection includes several elections with high turnout

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[8]The especially low turnout rate of the cantonal referendum was not because of the weak importance of the ballot at stake. Quite to the contrary, the replacement of the—more than a century—old cantonal constitution was a major political event. The direct democratic vote was supposed to be the coronation of a long decision-making process that lasted more than 3 years. However, only some small parties and organizations of the far right and far left opposed the new constitution, which arguably accounts for the low mobilization of voters.

Figure 1
Total turnout bias decomposed.
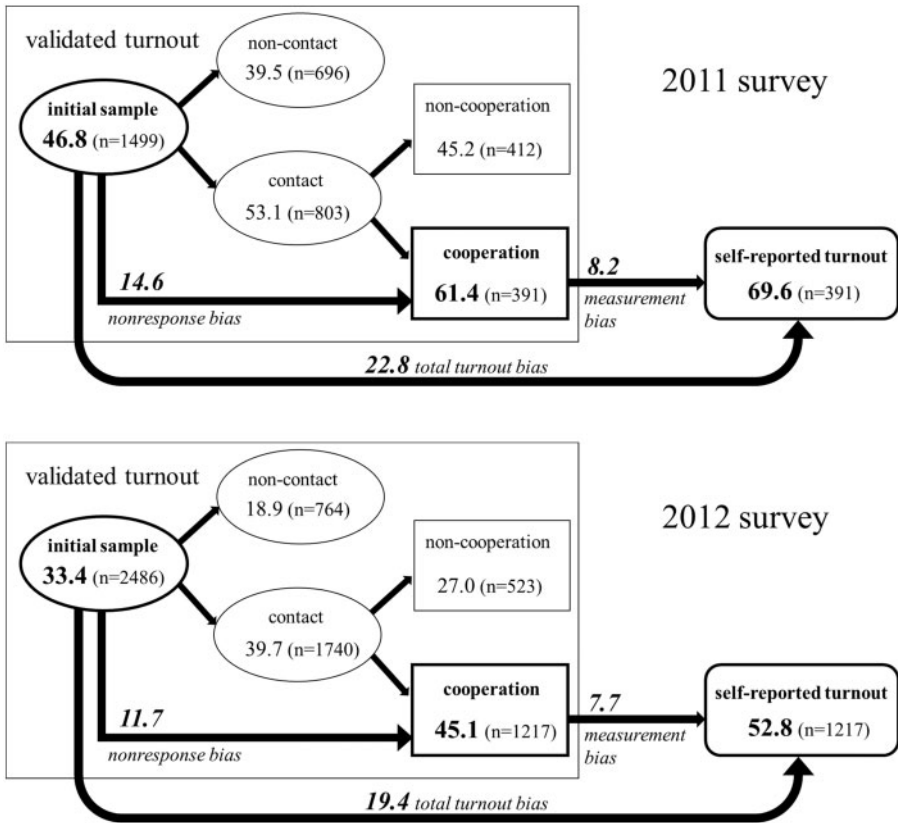
Note. Numbers in the text boxes represent the validated turnout per category and self-reported turnout, respectively. Numbers related to the arrows show the total and nonresponse/measurement bias. This figure excludes respondents who did not answer the turnout question (1 person in 2011 and 14 persons in 2012)

rates. In such cases, a sort of "ceiling effect" occurs and necessarily limits the size of possible turnout bias. Such a "ceiling effect" does not hold in a low turnout country such as Switzerland—or the United States. The size of the total turnout bias in our two surveys is close to that observed in presidential elections in the United States (Burden, 2000; Deufel & Kedar, 2010; McDonald, 2003).

The difference between validated turnout in the initial sample and validated turnout among cooperative persons (survey respondents) informs about nonresponse bias, that is, the bias induced by overrepresentation of actual voters in the realized sample (Figure 1). In both studies, the nonresponse bias is sizeable. It accounts for 64% of total turnout bias in

2011 (14.6/22.8) and for 60% in 2012 (11.7/19.4). The size of measurement bias (vote overreporting) is calculated as the difference between the validated and reported turnout among survey respondents.[9] Vote misreporting accounts for 36% (8.2/22.8) of total bias in the first survey and 40% (7.7/19.4) in the second. Therefore, in our Swiss data, total turnout bias is primarily a function of the overrepresentation of politically active citizens among survey respondents, and only secondarily a function of vote overreporting.[10]

While nonresponse bias is large in both surveys, it is especially large in the survey with lower response rate. In addition, in our data a higher response rate does not go along with a higher measurement error. Despite the fact that the response rate is almost two times higher in the 2012 survey, the absolute level of overreporting is slightly lower than in the 2011 survey (7.7 vs. 8.2). The difference in overreporting bias between both surveys is even larger if calculated in relative terms, that is, as the share of respondents who overreported out of the group of nonvoters among survey respondents. The relative share of overreporting is 21% in the 2011 survey (8.2/38.6) and 14% in the 2012 survey (7.7/54.9). These results run against the argument of the extended common cause model (Olson, 2006) that increasing the response rate attracts more embarrassed nonvoters into the sample and increases measurement error.

Table A2 shows that the possibility to fill the questionnaire online contributed to the smaller nonresponse bias and, even more so, to the smaller measurement error of the 2012 survey. This confirms Voogt and Saris' (2005) results that a mixed survey mode helps to decrease bias in the data. Further, Table A3 indicates that in both surveys the nonresponse bias is especially large during the first week of interviews and much lower afterward. This means that the survey company is most likely to interview an overproportional share of voters during the first days of fieldwork.

Figure 1 offers even more detailed results regarding validated turnout by showing the respective values separately for the contact and cooperation stages. In both surveys, the validated turnout is much smaller among noncontacts than among contacted people. The same holds for noncooperative people, who show a lower validated turnout than cooperative persons, that is, survey respondents. The respective shares of contact and cooperation for the nonresponse bias are almost equally big, with a slightly higher share owing to contact in 2012 and a slightly higher share owing to cooperation in 2011.

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[9]As is often the case, the share of voters who underreport, saying they abstained whereas they participated, is very low (roughly 1% of the realized sample).
[10]The distribution of nonresponse and overreporting in our Swiss data is closer to that in the ANES than in the CCES. Unlike the CCES, both the ANES and our Swiss surveys are based on probability samples. This may account for the fairly similar distribution of nonresponse and overreporting.

Survey Participation and Voter Overrepresentation

The large size of nonresponse bias in our data calls for a more detailed examination of the factors leading to nonresponse. To that end, we turn to an individual-level analysis. As already mentioned, on the individual level, survey participation depends first on the possibility to establish a contact with a sampled person, and second on the willingness and ability of that person to cooperate and respond to survey questions. Accordingly, we start with the analysis of survey participation (Table 2, Model 1), and then study its two components separately (contact in Model 2 and, for contacted persons, cooperation in Model 3).

Given that in each model the dependent variable is binary, we rely on probit regressions. The three models allow us to assess which variables play a role for survey participation in general and more specifically at which stage of the survey process. Such information is important, as it may help survey researchers to identify specific socio-demographic groups with a contact or cooperation problem and, therefore, to implement corrective measures that will decrease nonresponse bias. Moreover, comparing the coefficients between both surveys will also give us a hint whether differences in response rate result in different determinants of survey participation.

As expected, being married and being an active voter increases the probability of *survey participation* (model 1). In the 2012 survey, both variables measuring past voting record and participation in the current vote have a significant effect. In the 2011 survey, the coefficient for participation in the current vote does not reach statistical significance. However, an additional analysis, not reported here, shows that this coefficient gets significant if we exclude the past voting record from the model. Thus, our results are in line with the claim of leverage-salience theory (Groves et al., 2000) that participation in a survey has a greater leverage among people interested in the survey topic, in the present case (frequent) voters. More specifically, they confirm that there is a strong relationship between political participation and participation in a survey about politics (Brehm, 1993). While this relationship is often assumed, it has rarely been tested for reasons of data unavailability.

Age also has a significant effect on survey participation, but this effect is difficult to grasp based on coefficients. Therefore, we plot the predicted probabilities (and related 95% confidence intervals) of survey participation by age in Figure 2. Both graphs show an overall declining trend but with marked differences between the two surveys. Young persons have a probability of around 60% to take part in the survey. In the 2012 survey, this probability remains fairly stable for people up to 70 years, before falling abruptly afterward. In 2011, in contrast, the youngest people are a kind of positive outlier. The probability of interview decreases sharply from people aged 20 to people aged 35 years, and then stabilizes until the age of 70, however at a much lower

Table 2
The Determinants of Survey Participation, Contact, and Cooperation (2011 and 2012)

	Model 1—survey participation		Model 2—contact		Model 3—cooperation	
	2011	2012	2011	2012	2011	2012
Woman	0.003	0.158*	0.183*	0.176*	−0.166	0.090
	(0.075)	(0.053)	(0.070)	(0.056)	(0.098)	(0.067)
Age	−0.226*	−0.092*	−0.157*	−0.036	−0.171*	−0.107*
	(0.045)	(0.028)	(0.038)	(0.028)	(0.058)	(0.035)
Age^2	0.004*	0.002*	0.003*	0.001	0.003*	0.002*
	(0.001)	(0.001)	(0.0007)	(0.0005)	(0.001)	(0.0007)
Age^3	−0.00003*	−0.00001*	−0.00002*	−0.000007*	−0.00002*	−0.00001*
	(0.000006)	(0.000006)	(0.000005)	(0.000003)	(0.000007)	(0.000004)
Marital status (ref. single)						
Married	0.368*	0.194*	0.449*	0.122	0.161	0.205*
	(0.116)	(0.078)	(0.101)	(0.082)	(0.162)	(0.097)
Divorced/widow	0.394*	0.053	0.209	−0.107	0.432*	0.173
	(0.138)	(0.093)	(0.121)	(0.097)	(0.190)	(0.117)
Residence duration (>10 years)	0.166	0.137	0.381*	0.389*	−0.250	−0.266
	(0.136)	(0.098)	(0.123)	(0.098)	(0.214)	(0.146)
Participation in current vote	0.101	0.260*	−0.024	0.156*	0.233	0.255*
	(0.097)	(0.069)	(0.090)	(0.076)	(0.126)	(0.085)
Past voting record	0.964*	0.951*	0.609*	0.926*	0.859*	0.679*
	(0.156)	(0.103)	(0.142)	(0.109)	(0.202)	(0.130)
Constant	2.126*	0.466	1.429*	−0.092	2.874*	1.995*
	(0.678)	(0.441)	(0.589)	(0.439)	(0.904)	(0.572)
N	1,500	2,500	1,500	2,500	804	1,754
Pseudo R^2	0.080	0.089	0.080	0.085	0.119	0.063

Note. Standard errors in parentheses.
Model 3 is based on the subsample of contacted persons.
*p < .05.

Figure 2
Predicted probability of survey participation by age


2011   2012
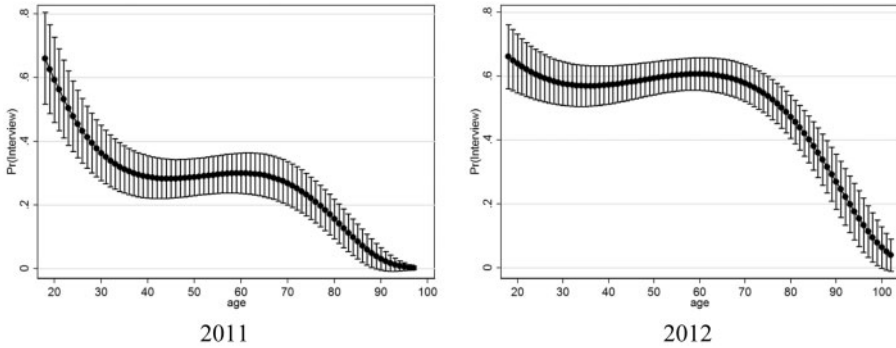
level than in the 2012 survey. A second downward trend again occurs among people over 70 years. In sum, our results do not confirm our expectation that response propensity would peak among middle-aged people. In our data, the likelihood of survey participation peaks among the youngest citizens, especially in the 2011 survey.

The results regarding residence duration also run against our expectations, as in both surveys, this variable has no significant influence on survey participation. Finally, the effects of gender and being divorced/widowed differ between the two surveys. In the 2012 survey, women have a higher likelihood of survey participation than men. In the 2011 survey, participation is more likely among divorced/widowed people than among singles (reference category).

The separate models for contact and cooperation in Models 2 and 3 provide a finer-grained view of the sources of nonresponse and help to test whether and to what extent the effects of contact and cooperation add up and jointly contribute to survey participation or, to the contrary, whether these effects cancel each other out. The latter would hold if, for example, a given variable has a positive influence on the likelihood of contact, but a negative influence on the likelihood of cooperation.

Three variables, namely age, past voting record, and participation in the current vote have consistent effects on contact and cooperation. Figure 3 shows the predicted probabilities of *contact* by age. In 2011, the curve has a u-shaped form among young and middle-aged people, and then turns into an inverted u-shape among older people. It is especially the group of people aged 30–50 that displays a low odds of contact. The curvilinear form is far less marked in the 2012 survey with a higher response rate. Here, the likelihood of contact is fairly stable from people aged 20–75, and declines only among the

Figure 3
Predicted probability of contact by age

2011   2012
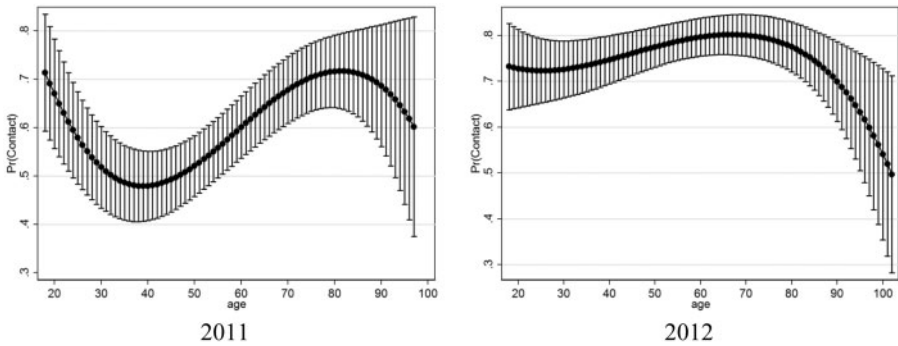
oldest citizens. The higher level of efforts made in that second survey presumably accounts for the more stable contact pattern across age.[11]

At a first glance, the result that contact decreases with age does not conform to that of U.S. studies. There, it has been found that young people participate less politically—and are harder to survey—as a result of their higher residential mobility and of the residential-based registration system (Ansolabehere, Hersh, & Shepsle, 2012, pp. 336–337). However, in Switzerland, citizens do not need to register to vote and mobility is not as high as in the United States. Young people aged 18–25 are likely to still live with their parents and, therefore, to be more easily reachable for an interview than the subsequent age-groups.

In both surveys, age has also a strong effect on *cooperation* (Model 3). As the predicted probabilities to cooperate plotted in Figure 4 show, cooperation decreases almost linearly with age in the 2011 survey, whereas in the 2012 survey, cooperation remains stable (and high) until the age of 70, and then decreases abruptly. Previous research in the United States has found few robust effects of age on cooperation (Groves & Couper, 1998, pp. 135–136). The lack of robustness is even more marked in cross-national studies (Stoop, Billiet, Koch & Fitzgerald, 2010, pp. 19–20). According to our data, Switzerland belongs to those countries in which cooperation is lower among older age-groups.

The two variables measuring political participation (past voting record and current vote) show similar effects on both contact and cooperation. The effect of past voting record has a significantly positive effect in all models and in both surveys. Current vote has a significantly positive impact on both contact

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[11]To some extent, the possibility to fill the 2012 survey online has also contributed to that stability. Online participants are on average about 8 years younger than CATI participants, which increases contact rates especially for people around 30–40 years. However, the patterns concerning age still differ between both surveys when excluding online participants in 2012 (results not shown).

Figure 4
Predicted probability of cooperation by age

2011	2012
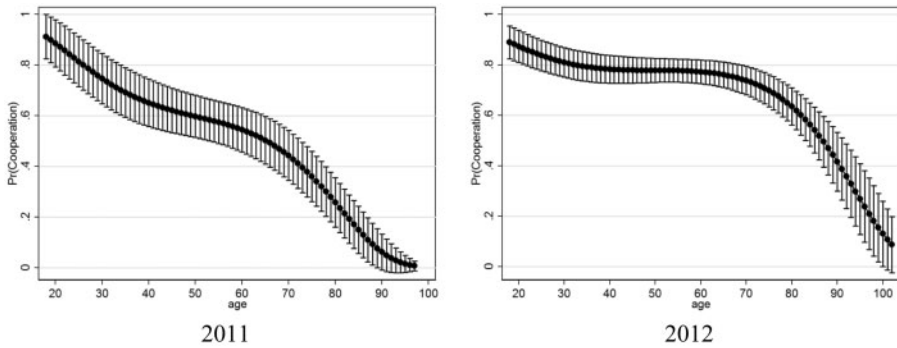
and cooperation in the 2012 survey, but not in the 2011 survey. However, the effects again get significant once we exclude the past voting record variable from the model in 2011. In sum, both citizens who vote frequently and citizens who participated in the current vote have a higher likelihood to be contacted and to cooperate.

Next, we turn to variables whose effects differ between the contact and cooperation stages. Starting with gender, we can see from Table 2, Model 2 that in both surveys women have a higher probability of being contacted than men. However, in 2011, this effect is counter-balanced by the lower likelihood of women to cooperate (Model 3). Although the latter effect is not significant, it nevertheless accounts for the absence of a gender effect on survey participation in 2011 (Model 1).

Similarly, when discussing the survey participation model, we did not pay too much attention to the effect of residence duration, which is not significant in both surveys. Looking at the contact and cooperation model shows that this variable in fact matters, but its effects work at cross-purpose in the two survey stages. More concretely, long-term residents have a higher likelihood of being contacted, but they have a lower—albeit not significant—propensity to cooperate, which ultimately leads to the nonsignificant impact of residence duration on survey participation.

Finally, in the survey participation model, the coefficients of married persons are significant and positive in both surveys, which at first glance suggests that similar mechanisms are at work. On closer inspection, however, the overrepresentation of married people among survey respondents has different causes in the two surveys. It stems mainly from a higher contact probability in the 2011 survey, and from a higher cooperation probability in the 2012 survey.

Summing up, based on our data we can be fairly confident that especially age, citizens' usual voting propensity, and—to a lesser extent—participation in

the vote of interest are important and consistent determinants of survey participation and its constitutive components contact and cooperation. Other factors such as sex or marriage, by contrast, are more sensitive to survey characteristics and related response rates. However, having only information about the final outcome—survey participation—and not about the contact and cooperation stages leading to that outcome can hide some of the effects induced by different response rates. For example, the stronger effort made in the 2012 survey to contact sampled citizens has arguably helped to lower the overrepresentation of married people among survey respondents.

In Tables A4 and A5, we break down the analysis further to the two main sources of noncontact and noncooperation, respectively. On the one hand, we run a model that contrasts people who could be contacted, from people for whom we could not find any phone number and from people who could not be reached. On the other hand, we run a model contrasting persons who cooperated to persons who did not for reasons of inability or unwillingness. Age again stands out as a relevant predictor. First, the results regarding the sources of no contact (Table A4) show that age is a more significant determinant for the "no phone" problem than for reaching the sampled persons (especially in 2011). The "no phone" problem concerns mainly people aged 30–40 years (the so-called "mobile generation" with no fixed landline or unknown mobile number) and very old people, who are likely to be in homes for the aged. The nonavailability of a (fixed) phone may thus also be the main driver of the contact pattern found in Figure 3. Second, the analysis of the sources of no cooperation (Table A5) reveals that both ability and willingness to cooperate are much lower among oldest citizens.

Tables A4 and A5 also help to explain the found patterns for residence duration from Table 2. The higher contact rate among long-term residents stems mainly from the high distribution of fixed landlines (see negative effect for "no phone" in Table A4), and the lower cooperation rate from a higher unwillingness to cooperate (see the positive coefficient in Table A5, though only significant in 2012). These two results are perhaps linked together: long-term residents may get bothered by the repeated calls of survey companies.

Conclusion

The question whether turnout bias is primarily a function of measurement error or of nonresponse bias is debated in the literature. According to a widespread view, overreporting contributes to a greater extent to the overestimation of turnout than voter overrepresentation. Some scholars disagree and argue that turnout bias is primarily a function of voter overrepresentation in surveys about politics. While these contrasting views may be owing to differences in survey characteristics, our data contributes to this debate by

providing highly reliable information about the validated vote of both citizens who were interviewed and citizens who were not. In both surveys, the non-response bias associated with the overrepresentation of voters among survey respondents is sizeable. It accounts for almost two thirds of the total turnout bias.

The size of nonresponse bias is especially large in the survey with a lower response rate. Although according to survey research, lower response rate is not necessarily conducive to higher nonresponse bias, our results suggest that there may be such a link in postelection surveys. Further, our results do not confirm that efforts to increase the response rate increase the size of measurement error by bringing more embarrassed nonvoters into the sample. In relative terms, the share of vote overreporting is in fact lower in the survey with the higher response rate.

Taking advantage of the richness of our data, we could conduct an in-depth analysis of individual determinants of survey participation and related components (contact and cooperation). This has enabled us to identify at which stage of the survey process nonresponse operates, and to distinguish factors whose effects reinforce each other in both stages, from factors whose effects work at cross-purpose. Age belongs to the first group of factors, as it—nonmonotonically—influences survey participation through both contact and cooperation. Similarly, our analysis confirms that voting and responding to a survey about politics go hand in hand. Both contact and cooperation are higher among frequent voters and—to a lesser extent—persons who partici-pated in the vote of interest. Conversely, the higher likelihood of contact among long-term residents and their lower cooperation propensity cancel each other out.

Our findings have broader implications. First, the result that turnout bias is primarily a matter of nonresponse bias alerts us about the importance of studying the bias induced by voter overrepresentation. While the bulk of the existing vote validation literature has focused on overreporting, our results call for a more thorough consideration of how sample selection affects the analysis of turnout bias. Second, our study tends to highlight the virtuous effects of a higher response rate in postelection surveys. The higher contact and cooper-ation rates achieved in the 2012 survey have led to a lower nonresponse bias, without increasing the measurement bias. Third, survey practitioners may find some useful information in both our general results regarding survey partici-pation and our more specific results regarding contact and cooperation. For instance, the possibility to respond online may help to bring in more of the so-called "mobile generation" with unknown phone numbers.

Of course, our results—especially those regarding the differences in re-sponse rate—must be taken with a grain of salt because they are based on two surveys only. More generally, given the sensitiveness of the size and forms of

turnout bias on differences in survey characteristics and institutional contexts, a study like ours has limitations in terms of external validity and generalizability. We nevertheless hope that our results will motivate other researchers to take into account the nonresponse bias and constitutive elements in future analysis of turnout bias, and this ideally in both the United States and other contexts to widen the scholarly debate.

Acknowledgments

Table A1
Most Common Reasons and Recoding for Nonparticipation in the Survey

Noncontact	No phone	No or invalid phone number
	Not reached	Returned letter, answering machine, no answer, target person (tp) no longer lives in household, tp went abroad, tp inaccessible during survey period (e.g., in holidays), appointment scheduled
Noncooperation	Inability	Age or health problem, language problem, tp died, lives in home for the elderly, tp overwhelmed
	Unwillingness	Tp refused, against/not interested in surveys, no time, not interested in topic

Note. The table presents only the most common mentioned reasons that prevented a successful interview. The original data sets comprise even more specific reasons.

Table A2
Total Turnout Bias, Nonresponse Bias, and Measurement Error According to Survey Mode in 2012

Survey method	Validated turnout in the initial sample (%)	Validated turnout among respondents (%)	Reported turnout among respondents (%)	Total turnout bias (%)	Bias estimates	
					Nonresponse (%)	Measurement (%)
CATI (n)	33.4 (2,486)	45.2 (1,059)	53.5 (1,059)	20.1	11.8	8.3
online (n)		44.3 (158)	48.1 (158)	14.7	10.9	3.8

Note. This table excludes the 14 respondents who did not answer the turnout question.

Table A3
Nonresponse Bias and Measurement Error According to the Timing of the Interview

Year	Validated turnout in the initial sample (%)	Timing of the interview	Validated turnout among respondents (%)	Reported turnout among respondents (%)	Bias estimates	
					Nonresponse (%)	Measurement (%)
2011	46.8	First week	71.4	77.4	24.6	6.0
		Second week	52.7	62.5	5.9	9.8
		Third week	48.8	60.0	2.0	11.2
2012	33.4	First week	49.8	57.1	16.4	7.3
		Second week	38.0	46.5	4.6	8.5
		Third week	38.2	46.6	4.8	8.4

Note. This table excludes respondents who did not answer the turnout question (one person in 2011 and 14 persons in 2012).

Table A4
The Determinants of No Contact: Multinomial Logit Model Contrasting "No Phone" and "Not Reached" to "Contact"

Variables	2011 (vs. contact)		2012 (vs. contact)	
	No phone	Not reached	No phone	Not reached
Woman	−0.142	−0.412*	−0.387*	−0.065
	(0.147)	(0.132)	(0.107)	(0.151)
Age	0.413*	0.146*	0.158*	−0.155*
	(0.079)	(0.073)	(0.052)	(0.074)
Age^2	−0.008*	−0.002	−0.004*	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Age^3	0.00005*	0.00001	0.00002*	−0.00001
	(0.00001)	(0.000009)	(0.000006)	(0.000009)
Marital status (ref. single)				
Married	−0.843*	−0.668*	−0.383*	0.231
	(0.204)	(0.192)	(0.152)	(0.236)
Divorced/widow	−0.255	−0.388	0.208	0.253
	(0.245)	(0.230)	(0.177)	(0.278)
Residence duration (>10 years)	−0.865*	−0.367	−0.805*	−0.040
	(0.231)	(0.244)	(0.169)	(0.301)
Participation in current vote	−0.051	0.100	−0.296	−0.261
	(0.193)	(0.171)	(0.154)	(0.209)
Past voting record	−1.287*	−0.789*	−1.698*	−1.224*
	(0.306)	(0.270)	(0.215)	(0.294)
Constant	−4.877*	−1.826	−1.343	1.194
	(1.221)	(1.133)	(0.819)	(1.154)
N (in category)	1,500 (317)	1,500 (379)	2,500 (534)	2,500 (212)
Pseudo R^2	0.071		0.077	

Note. Standard errors in parentheses.
*$p < .05$.

Table A5
The Determinants of No Cooperation: Multinomial Logit Model Contrasting "Inability" and "Unwillingness" to "Cooperation"

VARIABLES	2011 (vs. survey participation)		2012 (vs. survey participation)	
	Inability	Unwillingness	Inability	Unwillingness
Woman	0.396	0.264	0.333	−0.206
	(0.292)	(0.167)	(0.274)	(0.117)
Age	0.490*	0.208*	0.175	0.154*
	(0.212)	(0.104)	(0.170)	(0.062)
Age^2	−0.009*	−0.003	−0.003	−0.003*
	(0.004)	(0.002)	(0.003)	(0.001)
Age^3	0.00006*	0.00002	0.00002	0.00002*
	(0.00002)	(0.00001)	(0.00002)	(0.000008)
Marital status (ref. single)				
Married	−0.125	−0.315	0.239	−0.402*
	(0.499)	(0.275)	(0.435)	(0.166)
Divorced/widow	−1.281*	−0.607	−0.478	−0.218
	(0.577)	(0.325)	(0.481)	(0.200)
Residence duration (>10 years)	−0.445	0.585	−0.498	0.545*
	(0.602)	(0.387)	(0.563)	(0.269)
Participation in current vote	−0.661	−0.316	−0.915*	−0.359*
	(0.368)	(0.215)	(0.338)	(0.152)
Past voting record	−1.474*	−1.409*	−1.348*	−1.095*
	(0.588)	(0.347)	(0.475)	(0.231)
Constant	−9.999*	−4.062*	−6.393*	−3.114*
	(3.671)	(1.597)	(3.094)	(1.005)
N (in category)	804 (100)	804 (312)	1,754 (84)	1,754 (439)
Pseudo R^2	0.149		0.092	

Note: Standard errors in parentheses. Models are based on the subsample of contacted persons.
*$p < .05$.

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