Survey Research Methods (2015)
Vol. 9, No. 2, pp. 71-82
doi:10.18148/srm/2015.v9i2.6139

ISSN 1864-3361
http://www.surveymethods.org

Undercoverage and Nonresponse in a List-sampled Telephone Election Survey

Oliver Lipps
Swiss Centre of Expertise in the Social Sciences (FORS)
Lausanne, Switzerland

Nicolas Pekari
Swiss Centre of Expertise in the Social Sciences (FORS)
Lausanne, Switzerland

Caroline Roberts
University of Lausanne
Lausanne, Switzerland

For landline telephone surveys in particular, undercoverage has been a growing problem. However, research regarding the relative contributions of socio-demographic bias and other composition effects is scarce. We propose to address this issue by analyzing an election survey which used a sample from a register-based sampling frame containing basic socio-demographic information and to which telephone numbers were subsequently matched.
With respect to socio-demographic representation of the final sample, we find that difficult to match groups are also difficult to contact, while those who cooperate tend to have different characteristics.We find bias due to undercoverage to be of greater magnitude than noncontact bias, while noncooperation falls between the two. A decreasing number of landline phones, an increasing unwillingness to have landlines publicly listed, and low matching probabilities will make the use of additional effort more and more indispensable to keep undercoverage bias at a reasonable level.
As for substantive variables, both additional efforts to match missing telephone numbers and the construction of better weights are successful in closing the gap between survey estimates of voting behavior and true values from the election results.

Keywords: undercoverage; noncontact; noncooperation bias in substantive items; individual register sampling frame; telephone number matching; telephone election survey

1 Undercoverage and nonresponse in telephone surveys

Telephone surveys based on telephone list samples are increasingly challenged by a growing problem of undercoverage resulting from two developments: one, a dramatic increase in the proportion of "mobile-only" households (Mohorko, de Leeuw, & Hox, 2013); and two, the fact that an increasing proportion of individuals no longer wish to be listed in a public directory (Blumberg & Luke, 2014; Ernst Stähli, 2012; Joye, Pollien, Sapin, & Ernst Stähli, 2012; Link & Fahimi, 2013; Von der Lippe, Schmich, & Lange, 2011). This coverage problem is compounded by the fact that people who do not have their numbers listed, and especially those without a landline, differ from those with a listed landline telephone number based on a variety of characteristics. This is true for example with respect to age (Busse & Fuchs, 2012; Lipps & Kissau, 2012; Mohorko et al., 2013), municipality size, civil status (Lipps & Kissau, 2012), ethnic group or region (Cobben & Bethlehem, 2005), or substantive variables such as political interest, party choice (Joye et al., 2012), political left-right self-placement, or life satisfaction (Mohorko et al., 2013). As a consequence, estimators of both point estimates and associations, such as regression coefficients, are at risk of bias (Peytchev, Carley-Baxter, & Black, 2011, 2010).

Since 2010, the Swiss Federal Statistical Office (SFSO) has maintained a frame of individuals residing in Switzerland based on population registers. This frame offers great potential for research into bias from nonobservation (Roberts, Lipps, & Kissau, 2013): In addition to an almost zero coverage error, individuals can be sampled directly, which eliminates the need for household screening to identify a target individual. There is evidence that screening for survey eligibility leads to underreporting of household members (Tourangeau, Kreuter, & Eckman, 2012) and an increase in nonresponse (Lipps & Pollien, 2011). Furthermore, the frame provides basic demographic information about all sample members.

For surveys that draw samples from this register but still

Contact information: Oliver Lipps, Swiss Centre of Expertise in the Social Sciences (FORS), 1015 Lausanne, Switzerland (oliver.lipps@fors.unil.ch)

use the telephone to collect the data, the remaining issue is that the sampling frame itself does not contain telephone numbers. These need to be separately searched and matched to the sample. Matching rates may differ across different methods. For example Brick, Williams, and Montaquila (2011), using commercial sources that match telephone numbers to an address-based sample in the US, achieved a 57% telephone matching rate. The SFSO matches register-based samples against their own register of telephone numbers, which includes both publicly listed and unlisted numbers, but no mobile phone numbers. With this, the SFSO achieves an average matching rate of randomly sampled individuals of 76% (Joye et al., 2012).

This raises the question of whether additional efforts to obtain numbers for unmatched sample members would be able to reduce coverage errors. For example, the SFSO has had good experiences using postcards to ask unmatched individuals for either a landline or a mobile telephone number (von Erlach & Zweers, 2012). With samples supplied to external institutions, where only publicly-listed numbers are made available, the question of coverage error is even more important. In a regional register-based random sample survey (Lipps & Kissau, 2012), an additional automated matching procedure was carried out using publicly-available commercial marketing databases and directories of national telephone companies. This was followed by manual searches using a variety of directories and information from other members of the household, and finally, a postcard asking for telephone contact details. Altogether, these procedures resulted in a total matching rate of 86%. Each additional step, and especially the postcard request, made it possible to obtain numbers for sample members with socio-demographic characteristics that were underrepresented in the automatic matching, thereby reducing bias on frame variables.

In addition to undercoverage, nonresponse continues to be an important source of bias from nonobservation (Brick & Williams, 2013). Brick (2011), however, considers undercoverage as "more insidious than nonresponse [because] the . . . undercovered are not subject to the same stimulus to respond to the survey" (p.885). The author cites an example where an RDD survey with a 40% response rate had no evidence of large nonresponse bias while the same survey provides evidence of undercoverage bias, although the coverage rate was above 80%. Among the few studies that examine the relation between undercoverage and nonresponse error in telephone surveys, Peytchev et al. (2011) compared respondents and nonrespondents from an RDD survey with respondents from the nonresponse follow-up and a mobile phone follow-up survey and found that they differed on socio-demographic variables, especially in the mobile phone survey. In addition, the authors reported both nonresponse and undercoverage bias for estimates of survey variables. Differences between RDD respondents and RDD nonrespondents went in the opposite direction compared to differences between RDD respondents and mobile phone respondents. Furthermore, they found that coverage bias changed direction due to post-stratification and compounded with bias from nonresponse. One problem in the study from Peytchev et al. (2011) is that their benchmark survey – even if well-designed – is not free from nonresponse and undercoverage. The sample for the telephone survey analyzed in this paper is not drawn by RDD. Instead, it comes from an individual population register, which ensures coverage of practically 100%, to which telephone numbers need to be subsequently searched from different sources and matched. Therefore, the different nonobservation components can be attributed to each step.

A further motivation for our study was to distinguish the bias from the two main components of nonresponse: noncontact and noncooperation, something that was not done by Peytchev et al. (2011) for instance. Even though the importance of this distinction has been recognized for a long time (Deming, 1947), undertaking it has been neglected until now in empirical research with the exception of Mishra, Dooley, Catalano, and Serxner (1993) and Olson (2007) . Unfortunately, Mishra and colleagues used the "pseudo"-nonrespondents as proxies for the true nonrespondents: because nonrespondents are not measured, they study those respondents who most resemble the true nonrespondents. These include respondents interviewed after much difficulty. However, the underlying "continuum" hypothesis does often not hold true (Stoop, 2005). In Olson's dissertation, she draws noncontact and noncooperation indicators from a review of the household survey nonresponse literature, notably Groves and Couper (2012), and assesses correlates of a number of variables with each of these indicators. However, the data used were representative of specific populations only: one survey made use of information from divorce certificates and the other was a student aid survey. Measurement errors in the records were also an issue in both surveys. Empirically, she found that using an approach that separates contact and cooperation in an adjustment procedure worked better than one that did not separate these errors. The advantage of our study compared to that of Olson (2007) is the use of a sample which is representative of all national adult citizens based on records that are virtually error free von Erlach and Zweers (2012). In addition, we also analyze nonobservation bias due to the undercoverage of substantive variables. Finally, we can study whether the additional effort to match telephone numbers, including a small web follow-up survey, progressively reduces bias.

The paper is structured as follows: The first part deals with the importance of error due to undercoverage and nonresponse, based on a univariate analysis of (socio-demographic) frame variables. Undercoverage is studied by differentiating between three samples obtained by increasing telephone number matching efforts through additional methods, plus a small web follow-up survey. In turn, nonresponse is examined by differentiating between the samples of contacted and responding individuals. Then, we compare the bias of voting turnout and party choice obtained in the three telephone number matched samples and the web follow-up, and effects from different post-survey adjustment weights. We then provide a summary of our findings and offer concluding remarks.

2 Data and Matching Rates

The data used in the present study were collected for the Swiss Electoral Study 2011 – "Selects" (More information available at www.selects.ch), a Computer Assisted Telephone Interviewing (CATI) survey conducted every four years after the Swiss federal elections. The sample was drawn by the SFSO and is representative of Swiss citizens aged 18 years or older, and living in Switzerland at the time of the 2011 Swiss federal election that took place on October 23. A random sample stratified by the 26 Swiss cantons (NUTS 3) was used, with small cantons oversampled to a minimum of 100 respondents each.

The results of the different matching procedures are shown in table 1. Of the 8,162 sample members drawn, 5,530 (67.8%) were matched a listed telephone number by the SFSO. In a second step, a further 1,338 (16.4%) could be matched by the survey agency using various methods. Most additional numbers were found using software provided by Getstone (http://www.getstone.ch/), which matches information from the official telephone directory, as well as manual searches using the latter. The numbers found using these methods were virtually of equal quality compared with those that were delivered by the SFSO, the difference being mostly due to some wrongly matched phone numbers. In addition, some phone numbers were found using a marketing database. This method resulted in many more numbers that were incorrect or did not work, showing the information to be of inferior quality. The relatively large number of phone numbers found (see e.g., Lipps & Kissau, 2012) in these steps is likely in part due to the relatively conservative matching done by the SFSO. For instance, a slight difference in the spelling of a name will cause the matching to fail. A more flexible matching procedure would deliver a higher number of phone numbers but with a higher risk of including wrong numbers, as happened with the additional matching efforts. Getstone, for example, advertises on its website the use of phonetic and fuzzy matching technologies (Getstone, 2015), which supports this hypothesis. Another issue is that a phone number will usually not be found for individuals who are not listed as holders of the telephone connection,[1] as the sampling frame lacks information on household composition. Next, a postcard asking for a phone number was sent to the still unmatched individuals before the beginning of the survey fieldwork. The postcard was sent together with a post-cheque worth 20 Sfr. as an unconditional incentive. Of the 1,294 individuals to whom a postcard was addressed, 124 (1.5%) returned it with a telephone number (response rate 9.9%). These included both fixed line and mobile phone numbers and both were used to conduct the survey, assuming that results are comparable (Häder, Häder, & Kühne, 2012). In the case a person did not respond to the first letter, a reminder was sent, which also offered the possibility of completing the questionnaire online. This option was taken by 85 individuals (1.0%). Altogether, 86.7% of the individuals could either be matched with a telephone number or filled out the survey online. It has to be noted again that Selects is restricted to Swiss citizens. Because undercoverage is generally higher for foreigners in Swiss telephone surveys (Lipps & Kissau, 2012; von Erlach & Zweers, 2012), this matching rate would probably be lower in a comparable survey including foreigners.

Table 1
Sources used to match telephone numbers to the register samples. Data: Selects 2011.

Source of telephone number	n	%
Total sample size	8,162	100
SFSO register (listed and delivered)	5,530	67.8
Number matched by other methods	1,338	16.4
Number delivered by postcard	124	1.5
Web follow-up	85	1.0
No Number available	1,085	13.3

3 Representativity bias due to undercoverage and nonobservation

In this section, we compare frame variable distributions across different subsamples, at different stages of the coverage / response process. To analyze coverage error, we compare discrepancies in frame variable categories between the total sample and the different subsamples resulting from different methods of obtaining telephone numbers. To analyze noncontact error, we examine discrepancies in frame variable categories between those where a contact attempt was made and those that were successfully contacted. Finally, to analyze noncooperation bias, we examine discrepancies in frame variables between those who were successfully contacted and those who ultimately cooperated. We use the following frame variables and their respective categories:

- Age groups: 18-30 years, 31-44 years, 45-58 years, 59-72 years, 73+ years
- Size of municipality of residence, representing the degree of urbanization: more than 100,000 inhabitants, 20-

[1] This is particularly relevant for young adults living with their parents.

100,000 inhabitants, 10-20,000 inhabitants, 5-10,000 inhabitants, 2-5,000 inhabitants, less than 2,000 inhabitants

- Civil status: married, single and never married (hereafter referred to as single), divorced, widowed
- Gender
- Language region: Swiss-German, French or Italian

In table 2 we show frequency distributions for each of the above variables for the different subsamples described, including the target distribution (total sample).

First, if only automatically matched numbers had been available for the survey, underrepresentation of adults up to their mid-40s and overrepresentation of individuals from 59 years onwards would amount to 3-4% points. The reason for this bias is that older people are both more likely to possess a landline (Joye et al., 2012) and their telephone numbers are more likely to be listed[2]. In terms of marital status, the findings run in parallel to those of age, in part due to the correlation between the two variables. For example, single people are even more underrepresented than young people. One of the reasons for this is that unlike mobile phones which are individualized devices, landline phones are collective devices in households and – due to relatively high fixed costs – benefit from economies of scales in larger households (Lipps & Pekari, Forthcoming). For the same reason, married people are more likely to possess a landline. In addition, people living in larger municipalities are underrepresented, as are French or Italian speakers. With the exception of small municipalities these biases can be reduced by adding numbers from other sources. The postcard procedure and the web follow-up have a smaller absolute impact on representation, but this is mainly due to the small sample sizes. If this is taken into account, the actual effect is considerably larger. It is likely that improvements in fieldwork could increase these sample sizes. However, it is also possible that as the samples become larger, the socio-demographic characteristics would become less complementary.

We find that with respect to *nonresponse* bias, younger people under the age of 59 are harder to contact but cooperate to a higher extent. People between the age of 59 and 72 are both easier to contact and cooperate to a greater degree. People over 72 are easier to contact but then very often refuse to cooperate. The distributions of marital status generally follow their age-specific correlates. For example, like younger individuals, single people are harder to contact but also cooperate to a higher extent. The sample is not considerably biased due to nonresponse error regarding gender: Women are better covered and are easier to contact but in turn refuse more often. The latter is probably due in part to the specific survey topic at hand, which has been found to be more interesting to men Inglehart and Norris (2003). The representation of municipalities of different sizes appears to be biased to only a small extent by nonresponse as well as the language region bias, although Swiss-German speakers tend to cooperate more often.

Using only people with an automatically matched number would create a large bias of more than 4% points (single or married individuals). We find that additional matching efforts pay off: the underrepresentation of singles decreases by 0.3% points by adding other telephone number sources, a further 0.3% points using the postcard procedure, and another 0.4% points by offering a web survey to those unwilling to provide their phone number. The overrepresentation of married individuals decreases by 0.3% points, 0.4% points, and 0.3% points, with the three additional efforts respectively.

Noncontact generally increases the bias resulting from undercoverage: for example, the representation of singles drops by a further 2.7% points and that of married individuals increases by a further 3.0% points. Similar results, with a smaller bias, hold for young people who are both more difficult to match and to contact, or older people who are both easier to match and to contact.

Cooperation in turn seems to have a mitigating effect on the bias resulting from undercoverage and noncontact: while young or single individuals, who are underrepresented among those contacted, cooperate to a higher extent, the opposite holds for older individuals. For the latter, this effect is considerable, dropping the representation of people aged 73+ by 5.9% points to an extent that older respondents in the end are underrepresented in the sample of respondents. For most other successfully contacted person groups, the pre-existing bias also reduces due to noncooperation. Exceptions are the 45-72 year olds, the married, those living in municipalities with 5,000-10,000 inhabitants, and both language regions.

The effect of undercoverage and noncooperation on representation bias is larger than the effect of noncontact, if we compare the strongest bias from each group. In terms of representation, the married present the strongest coverage bias (+3.4% points), which is also the case for noncontact (+3.0% points). For noncooperation errors, the strongest bias is among the 73+ years old (-5.9% points). While we wish to compare the different reasons for nonobservation in terms of their overall frame variable representation bias the differences in distribution of frame variables, measured by the χ^2 values, are not comparable across the different nonobservation samples due to their different sizes. Therefore, we use the sum of the absolute percentage point differences across all categories for all frame variables compared with the respective base sample, similar to the index of dissimilarity (O. Duncan & B. Duncan, 1955). In other words, we compare the four differently covered distributions (in Table 2: AM, OS, PC and web) with the total sample, the contacted sample with the finally matched sample, and the cooperating sample with the contacted sample across all categories for all frame

[2]For people from 59 years onwards: 89.4%; for people up to 58: 81.9%; available from the Selects Rolling Cross Section sample (Lutz, De Rocchi, & Pekari, 2013).

Table 2
Distribution and bias of frame variables for different samples. Data: Selects 2011.

	total sample %	Automatch only (AM) %	AM + oth. sources (OS) %	AM/OS + postcard (PC) %	AM/OS/PC + web (web) %	Con- tacted (cont) %	Coop- erating (coop) %
18–30 years old	20.1	16.6	17.2	17.5	17.7	16.5	18.0
31–44 years old	21.5	18.8	20.1	20.3	20.5	19.0	21.9
45–58 years old	26.3	26.8	27.3	27.2	27.1	26.9	28.1
59–72 years old	19.8	22.1	21.5	21.3	21.2	22.6	23.0
73+ years old	12.3	15.6	14.0	13.7	13.6	15.0	9.1
% difference to reference distribution[a]		12.3	8.7	7.6	6.9	5.7	11.9
Single	30.6	26.5	26.8	27.1	27.5	24.8	26.6
Married	53.1	57.5	57.2	56.8	56.5	59.5	60.1
Widowed	7.0	8.2	7.9	7.7	7.7	8.0	5.5
Divorced	9.2	7.8	8.2	8.4	8.4	7.7	7.8
% difference to reference distribution[a]		11.1	9.8	8.7	8.0	6.7	5.0
Women	51.7	52.4	52.3	52.3	52.2	52.5	50.9
Men	48.3	47.6	47.7	47.7	47.8	47.5	49.1
% difference to reference distribution[a]		1.4	1.2	1.2	1.0	0.6	3.2
>100K inhabitants	9.3	8.1	8.2	8.3	8.5	7.8	8.4
20–100K inhabitants	10.7	9.9	10.2	10.2	10.3	10.1	11.1
10–20K inhabitants	16.6	16.0	16.3	16.3	16.3	16.0	16.8
5–10K inhabitants	17.0	17.5	17.2	17.3	17.3	17.5	17.9
2–5K inhabitants	25.0	26.6	26.1	25.9	25.8	26.0	24.5
<2K inhabitants	21.4	21.9	22.1	22.0	21.9	22.5	21.3
% difference to reference distribution[a]		5.2	3.9	3.6	3.1	2.2	5.5
Swiss-German	75.1	77.3	76.1	76.0	76.0	76.7	78.2
French or Italian	24.9	22.7	23.9	24.0	24.0	23.3	21.8
% difference to reference distribution[a]		4.4	2.0	1.8	1.8	1.4	3.0
N	8,162	5,530	6,868	6,992	7,077	5,703	2,371
Sum of differences to reference distribution		34.4	25.6	22.9	20.8	16.6	28.6

[a] reference of AM, OS, PC, and web is the total sample, reference of cont is the web, and reference of coop is cont.

variables. As a reading example, the 12.3% point difference of the age distribution of the automatically matched (AM) sample to that of the total sample is the sum of the absolute representation differences across all age groups:

$$|20.1 - 16.6| + |21.5 - 18.8| + |26.3 - 26.8| + |19.8 - 22.1| + |12.3 - 15.6| \quad .$$

Next, we sum up these measures across the categories (last line in Table 2). These figures amount to 20.8% points for the finally matched sample (34.4% points if only automatically matched numbers were available, 25.6% points if, in addition, other telephone number sources are included, and 22.9% points if numbers from the postcard procedures are added), 16.6% points for the contacted sample, and 28.6% points for the respondents (see the last line in table 2). Measured in this way, the initial coverage bias can almost be halved by additional effort. We also find the bias from noncontact (16.6% points) to be almost as strong as the bias from undercoverage (20.8% points). Noncooperation bias (28.6% points) lies between the initial (34.4% points) and the final (20.8% points) coverage bias. When interpreting these biases, we must keep in mind that the underlying measure depends on the categories of the frame variables and that these are to some extent correlated.

4 Bias in voting turnout and party choice

A particular strength of the survey at hand is its use of questions on political behavior asked right after the elections, as reported behavior can then be compared with official results. In this section, we analyze bias in substantive variables, focusing on the bias in voting turnout and party choice based on respondents from different matching steps. Specifically, we analyze if the bias in socio-demographic representation translates into a bias in substantive variables with increasing efforts of matching telephone numbers and the addition of a web survey. In addition, we check if socio-demographic[3] weights using variables included in the sampling frame help to reduce eventual biases. We also include a variable only available in the survey data but with a relatively high additional explanation power, namely education (whether the person has a university education or not).[4] We use the person-weighted distributions from the pooled 2010/2011 Swiss census surveys[5] to construct the socio-demographic weights.

As we consider only respondents, sample sizes are much smaller at this stage. Voting turnout is known to be overly high in election studies, both due to overreporting and selection bias. Overreporting occurs when people feel pressured to say they voted even if they didn't, due in particular to social desirability (Holbrook & Krosnick, 2010). Selection bias is due to the fact that people who are more involved in politics are also more likely to answer a survey on elections (Sciarini, Goldberg, & Tawfik, 2013; Selb & Munzert, 2013). Accordingly, the design-weighted proportion of voters in the survey of 75.3% is too high compared to the 48.5% official figure. Although the figure drops both with more effort invested and using adjustment weights, the overall reduction is small. While the starting value is 26.8% points too high, the final value (73.3%) is still 24.8% points too high. Nevertheless, when interpreting the decrease, the small marginal sample sizes of the last two steps, as well as the fact that education is a binary variable with a low probability of having a high school level (12.5%), must be taken into account. The biggest effect comes from the inclusion of the 83 web respondents with a marginal voting turnout value that is close to the overall target value of 48.5%. The second biggest effect is due to adding education to the set of socio-demographic adjustment variables, with the marginal voting turnout value decreasing by .5% points in the automatically matched sample and about 4% points in the web sample. We also see that a combination of additional matching efforts and weighting pays off: the strongest marginal effect occurs by weighting the web sample.

As a second substantive variable, a variable was constructed to describe the actual vote choice, which distinguishes between left, center, and right parties[6] (Table 4).

Note that the sample sizes further decrease since only those who claimed to have voted are asked about the party they voted for. Here, effects from additional matching efforts or weighting are not as obvious as for voting turnout. In the Swiss electoral studies, left and center parties have had a tendency to be overrepresented and right underrepresented. As for the effects from weighting and additional matching effort, respectively, we note that only adding education as a further adjustment variable and – to a minor extent – matching efforts have positive effects on representing the true party choice. Concerning right parties, and taking the small sample size of the additional samples into account, adding the socio-demography only or the socio-demography + education-weighted web sample improves the estimated vote share.

5 Summary and Conclusion

The aim of this paper is to analyze both representation bias of socio-economic groups due to undercoverage and nonresponse using the auxiliary variables available in a register-based sampling frame, and bias in substantive variables due to undercoverage in a telephone election survey. Undercoverage is studied using subsamples that result from successive procedures for matching individuals with different sources of telephone numbers, as well as a follow-up web survey. Nonresponse is studied by distinguishing noncontact and noncooperation as sources of subsequent losses of respondents. We first focus on the size and direction of representation bias from the different sources of nonobservation. Then, for substantive variable bias, we study substantive research variable distributions with successive matching procedures combined with two different weighting schemes.

Our starting point is a sample of adult Swiss citizens, which was randomly drawn from the register-based frame of individuals by the Swiss Federal Statistical Office (SFSO) and ensures practically 100% coverage of the population. While this register has the added advantage of including basic socio-demographic variables, phone numbers are not included. Instead, these must be identified using other information sources, the first of which is an automatic matching with a register of publicly listed telephone numbers also maintained by the SFSO. Subsequent matching is done with other databases, mostly commercial telephone directories and manual searches on the Internet. This step is followed by a postcard sent to respondents asking for a telephone number,

[3] Weights use the variables marital status (married vs. not married), gender, language (Swiss-German speaking area vs. French/Italian speaking area), and age-groups.

[4] Adding university level or not to the set of socio-demographic variables increases the McFadden pseudo R^2 for voting turnout from .047 to .055 and for party choice from .027 to .036.

[5] http://www.bfs.admin.ch/bfs/portal/en/index/infothek/erhebungen__quellen/blank/blank/rs/01.html. Due to the different way the education level was asked for in Selects 2011 and the Swiss census surveys 2010/2011, the only comparable categories that could be constructed were university education and non-university education.

[6] This operationalization was chosen due to the high number of parties in Switzerland and the low number cases for some parties after taking into account only voters.

Table 3
Voting turnout in different samples. Data: Selects 2011.

	Official sources %	Auto-match only (AM) %	AM and oth. sources (OS) %	AM/OS and postcard (PC) %	AM/OS/PC and web (web) %
Design weighted	48.5	75.3	75.1 (74.4)	74.9 (70.0)	74.1 (54.2)
Socio-demography weighted		74.7	74.7 (74.8)	74.7 (69.0)	74.0 (48.9)
Socio-demography + Education weighted (univ.)		74.2	73.9 (72.8)	73.9 (65.7)	73.3 (44.8)
N		1,799	2,182	2,284	2,367

Marginal values in brackets (only additional sample members)
[a] Source: http://www.politik-stat.ch

Table 4
Party blocks voted for in different samples. Data: Selects 2011.

	Official figures %	Auto-match only (AM) %	AM and oth. sources (OS) %	AM/OS and postcard (PC) %	AM/OS/PC and web (web) %
Design weighted					
Left	28.9	33.7	33.9 (34.7)	33.9 (33.1)	34.1 (41.0)
Center	25.7	28.6	28.1 (25.9)	28.2 (29.4)	28.0 (21.7)
Right	45.4	37.7	38.0 (39.3)	37.9 (37.5)	37.9 (37.3)
Socio-demography weighted					
Left		33.2	32.3 (35.2)	32.7 (43.6)	32.7 (41.3)
Center		29.2	28.9 (25.4)	28.6 (25.2)	28.4 (16.6)
Right		37.6	38.8 (39.4)	38.7 (31.2)	39.0 (42.1)
Socio-demography and education weighted					
Left		30.4	29.1 (32.4)	29.4 (37.2)	29.3 (34.1)
Center		29.3	29.3 (25.6)	29.1 (29.1)	28.9 (14.5)
Right		40.3	41.6 (42.0)	41.5 (33.7)	41.8 (51.4)
N		1,233	1,501	1,567	1,603

Marginal values in brackets (only additional sample members)
[a] Source: http://www.politik-stat.ch

and finally offering a web questionnaire for those interested in participating but unwilling to complete the survey by telephone.

We first compare the distributions of the socio-demographic variables available in the sampling frame (age, size of municipality, civil status, gender, and language region) between the different samples. We find that, in the automatically matched sample, younger people, singles or divorced, French or Italian speakers, and those living in larger municipalities are underrepresented. This is due to them having a lower proportion of landlines and their lower probability of being listed in the public directory. This bias can be somewhat reduced by using telephone numbers from additional sources. One of the reasons for this is that younger people who live in their parents' home and who are not holders of the household telephone connection, or individuals whose name or address is misspelled, are more likely to be matched with these sources than with the stricter matching procedure used with the SFSO telephone register. Though the postcard (N=124) and the web (N=85) follow-ups have a very small sample size compared to the follow-up using telephone numbers from additional sources (N=1,338), their effects in reducing bias of socio-economic groups is considerable relative to their size. Additional effort, for instance increasing the number of reminders, could also increase the relative size of these two samples. In addition, as the proportion of households with fixed lines decreases and, for those with a landline, the likelihood of not having the phone number listed increases, these additional means of contact will become increasingly important to maintain an acceptable level of representativity.

An alternative approach would have been to directly invite individuals for whom no phone number was found to take part in the web survey, as well as to give them the possibility of calling in for a phone interview, in a mixed-mode approach. However, this poses the problem of measurement differences in aural and visual modes as well as the differences related to the presence or not of an interviewer. This is in particular a problem for scalar questions, where it has been consistently showed that telephone interviews lead to more positive answers regarding opinion and attitude questions (Dillman, Smyth, & Christian, 2014). Given that these types of questions are central in an election study, the aim here was to maximize the number of telephone interviews to avoid problems that could arise by mixing the two modes. This is especially important as the combination of selection and measurement effects make it difficult to isolate the bias due to measurement (see e.g. De Leeuw, 2005). In addition, for a repeated cross-sectional survey like Selects, the inclusion of additional survey modes could compromise the time series[7].

Noncontact (based on the matched and fielded sample) tends to work in the same direction as undercoverage: those who are more easily matched tend to be easier to contact. This is true especially for older or married individuals, person groups that typically tend to be at home more often (Groves & Couper, 2012; Stoop, 2005). In turn, people who are less likely to own a landline, and thus are harder to match, such as the young, singles, or people living in larger municipalities, are also harder to contact. Noncooperation tends to mitigate bias from undercoverage and noncontact at least for the socio-demographic variables considered. On the other hand, for older or widowed people, the correcting effect of noncooperation overshoots the mark: while these groups are overrepresented among the contacted, they are underrepresented among the respondents. We measure bias from the different errors of nonobservation as the sum of absolute percentage point differences across the categories of all variables compared to the respective base sample (e.g., coverage bias is calculated by comparing the total sample and the covered sample(s), contact bias is based on the final matched sample, and cooperation bias on the contacted sample). Measured in this way, the representation bias due to undercoverage is greater in magnitude than noncontact bias, even if telephone numbers from additional sources and web interviews are included. Importantly, the magnitude of noncooperation bias is larger than noncontact bias, and lies between the smallest (final matched sample) and the largest (automatically matched sample) coverage sample. This seems to confirm the validity of concerns about undercoverage bias in relation to recent trends such as the increase in mobile-only households and unlisted telephone numbers. However, these results depend very much on how the fieldwork is executed, and as such, the generalizability of our results is likely to be weaker in this respect than for the direction of the different sources of bias.

Our analyzes of substantive variable bias shows that, on the one hand more effort made to find additional telephone numbers, using different means of communication to access people, and offering alternative survey modes, and, on the other hand, using well-designed weights pays off. Regarding voting turnout, the more difficult cases (those who didn't vote) can be found increasingly in the additional samples, especially those who answered by web. In addition to sample composition effects, the latter finding may also be due to less overreporting in the absence of an interviewer and less socially desired answers in web surveys (De Leeuw, 2005). Weights are increasingly effective for the marginal additional samples as well: while weights composed of socio-demographic variables and education decrease voting turnout by about 1% point in the automatically matched sample, this effect amounts to 1.6% points in the additional sample of telephone numbers from other sources, 4% points in the addi-

[7] The published dataset of Selects 2011 only contains the data from the CATI interviews. The additional web interviews were done as an experiment only for the reasons cited here.

tional sample of those sending a phone number by postcards, and 9% points in the additional sample of web respondents. Excluding education from this weight has much smaller effects.

Concerning party choice, similar conclusions hold for the effects of more effort and weighting. We analyzed this using three blocks: voters of left, center, and right parties. Socio-demographic and education weights are very successful in reducing the overly high proportion of left voters. All samples resulting from the different efforts reproduce the true order of party strength, where the right receives most votes and the center the least. However, right voters are consistently underrepresented. Using socio-demographic weights without education goes in the right direction but is not enough, while additional coverage effort leaves the proportion of left party voters unchanged. As for increasing the proportion of right party voters to be closer to the actual results, including the education weight leads to the best result and this is similar for all samples. Interestingly, socio-demographic and education weights increase the proportion of right party voters in the web sample from 37% to 51% (compared to 38% to 40% in the automatically matched sample), resulting in an overrepresentation of right voters. However due to the small N the inclusion of the web sample must not be over-interpreted.

Our findings imply that sampling frames with a minimal undercoverage should be used and noncontact kept as small as possible, especially because both tend to create representation bias in the same direction. Undercoverage will continue to increase in landline surveys. For example, matched phone numbers for the upcoming Selects 2015 survey are heavily down from 2011, although the same matching algorithms were used. Interestingly, the problem is rather related to the increasing number of people who have their number unlisted rather than abandoning the landline altogether. This is also likely to be an indication that, although people can in theory be reached, they are less and less accepting to be called by strangers. Lipps (Forthcoming) confirms in a recent mixed mode landline/face-to-face survey, where the landline was the primary mode and face-to-face was used for households without a (matched) landline, that undercoverage causes the largest bias in the telephone sample. Thus, to correctly represent the population regarding its socio-demographic characteristics, it is crucial to include households without a matched landline telephone by recruiting them with a different mode. If the trends that work against telephone surveys continue in their current direction, as they are very likely to do, it is also very possible that the mode of the interview itself will at some point need to switch from telephone-only to mixed mode surveys, despite issues in combining the two modes described earlier.

As subsequent noncooperation tends to mitigate the bias from undercoverage and noncontact, a certain noncooperation rate might even help to improve representativity in the respondent sample. This would mean a certain paradigm shift away from minimizing all sources of nonobservation towards balancing undercoverage and noncontact on one hand, and noncooperation on the other. Evidently, this is only true for the representativity of the sample based on the socio-demographic variables considered here. In addition to reducing undercoverage and noncontact, our substantive variable analysis shows the importance of using weights closely related to the variables of interest. A reassuring result of our research is that investing more effort in increasing coverage and using better weights each work in the right direction in the sense that more effort improves survey estimates (see Stoop, 2005) and should be used simultaneously. When comparing better weights against more field efforts, our experience with the inclusion of education shows that more sophisticated weights may well reduce bias. Generally, however, we believe that to correctly represent the population, fieldwork-related improvements are more important than efforts to achieve better weights. In multivariate analyses, socio-demographic variables on which weights are generally based on are usually controlled for and thus do not offer much benefit. Furthermore, even for univariate statistics such as mean values, weights are often not able to correct bias arising from under-coverage and nonresponse (Vandenplas & Lipps, 2014). The main focus of election studies is on the determinants of participation and vote choice. It is widely known that voters and people who are interested in politics will almost always be overrepresented, both because of the importance of interest in survey topic and because of the overlap between determinants of voting and taking part in surveys. Voters of certain parties also tend to be either over- or underrepresented in many countries. This overrepresentation itself is not necessarily an issue, especially as the true figures are known from election results and weighting is thus made relatively easy. However, weighting the results does not solve the issue of a too small variance within the underrepresented group and a representation of only certain types of non-voters or party supporters. Simply weighting the underrepresented groups might make univariate statistics less biased, but will not necessarily help improving the understanding of the manifold and intricate mechanisms behind these phenomena of interest.

Studies that achieve very high response rates through extensive additional survey efforts, including additional survey modes (e.g., Voogt, 2004), may come very close to true values of the population, but this is not always feasible in terms of the necessary resources. However, in surveys with smaller observation rates, such as the survey at hand, there also seems to be room to improve substantive variable estimates with reasonable additional costs by investing more effort in increasing coverage, a higher response rate, or using better weights. Using more than one mode for contacting respondents, which was the main focus in the current study, has the advantage of reducing coverage error while not creating issues due to difference in measurement in different modes.

References

Blumberg, S. & Luke, J. (2014). Wireless substitution: early release of estimates from the National Health Interview Survey. July–December 2013. Retrieved from http://www.cdc.gov/nchs/data/nhis/earlyrelease/wireless201407.pdf

Brick, J. M. (2011). The future of survey sampling. *Public Opinion Quarterly*, *75*(5), 872–888.

Brick, J. M. & Williams, D. (2013). Explaining rising nonresponse rates in cross-sectional surveys. *Annals of the American Academy of Political and Social Science*, *645*(1), 36–59.

Brick, J. M., Williams, D., & Montaquila, J. (2011). Address-based sampling for subpopulation surveys. *Public Opinion Quarterly*, *75*(3), 409–428.

Busse, B. & Fuchs, M. (2012). The components of landline telephone survey coverage bias. The relative importance of no-phone and mobile-only populations. *Quality and Quantity*, *46*(4), 1209–1225.

Cobben, F. & Bethlehem, J. (2005). *Adjusting undercoverage and nonresponse bias in telephone surveys.* CBS Discussion paper 05006, Voorburg/Heerlen.

De Leeuw, E. (2005). To mix or not to mix data collection modes in surveys. *Journal of Official Statistics*, *21*(5), 233–255.

Deming, W. E. (1947). Some criteria for judging the quality of surveys. *Journal of Marketing*, *12*(2), 145–157.

Dillman, D. A., Smyth, J. D., & Christian, L. M. (2014). *Internet, phone, mail, and mixed-mode surveys: the Tailored Design Method*. New York: John Wiley & Sons.

Duncan, O. & Duncan, B. (1955). A methodological analysis of segregation indexes. *American Sociological Review*, *20*, 210–217.

Ernst Stähli, M. (2012). Telephone surveys in Switzerland: spotlight. In M. Häder, S. Häder, & M. Kühne (Eds.), *Telephone surveys in Europe: research and practice* (pp. 25–36). Heidelberg: Springer.

Getstone. (2015). Merge/purge. Retrieved from http://www.getstone.ch/services/adress-dublettenabgleich

Groves, R. M. & Couper, M. P. (2012). *Nonresponse in household interview surveys*. New York: John Wiley & Sons.

Häder, M., Häder, S., & Kühne, M. (2012). Data quality in telephone surveys via mobile and landline phone. In *Telephone surveys in Europe: research and practice* (pp. 247–262). New York: Springer.

Holbrook, A. L. & Krosnick, J. A. (2010). Social desirability bias in voter turnout reports: tests using the item count technique. *Public Opinion Quarterly*, *74*(1), 37–67.

Inglehart, R. & Norris, P. (2003). *Rising tide: gender equality and cultural change around the world*. Cambridge University Press.

Joye, D., Pollien, A., Sapin, M., & Ernst Stähli, M. (2012). Who can be contacted by phone? Lessons from Switzerland. In M. Häder, S. Häder, & M. Kühne (Eds.), *Telephone surveys in Europe: research and practice* (pp. 85–102). New York: Springer.

Link, M. & Fahimi, M. (2013). Telephone survey sampling. In P. S. Levy & S. Lemeshow (Eds.), *Sampling of populations: methods and applications* (Chap. 15). New York: Wiley.

Lipps, O. (Forthcoming). Non-observation bias in an address-register-based CATI/CAPI mixed mode survey. Forthcoming in Methods, Data, Analyses.

Lipps, O. & Kissau, K. (2012). Nonresponse in an individual register sample telephone survey in Lucerne (Switzerland). In M. Häder, S. Häder, & M. Kühne (Eds.), *Telephone surveys in Europe: research and practice* (pp. 455–487). New York: Springer.

Lipps, O. & Pekari, N. (Forthcoming). Sample representation and substantive outcomes in web with and without incentive compared to telephone in an election survey. Forthcoming in Journal of Official Statistics.

Lipps, O. & Pollien, A. (2011). Effects of interviewer experience on components of nonresponse in the european social survey. *Field Methods*, *23*(2), 156–172.

Lutz, G., De Rocchi, T., & Pekari, N. (2013). *The Swiss 2011 rolling cross-section study: design, field work and data quality*. FORS Working Paper Series, paper 2013–4.

Mishra, S., Dooley, D., Catalano, R., & Serxner, S. (1993). Telephone health surveys: potential bias from noncompletion. *American Journal of Public Health*, *83*(1), 94–99.

Mohorko, A., de Leeuw, E., & Hox, J. (2013). *Coverage bias in European telephone surveys: developments of landline and mobile phone coverage across countries and over time.* Survey Methods: Insights from the Field. doi:10.13094/SMIF-2013-00002

Olson, K. (2007). *An investigation of the nonresponse error – measurement error nexus*. Ann Arbor: University of Michigan.

Peytchev, A., Carley-Baxter, L. R., & Black, M. C. (2011). Multiple sources of nonobservation error in telephone surveys: coverage and nonresponse. *Sociological Methods and Research*, *40*(1), 138–168.

Peytchev, A., Ridenhour, J., & Krotki, K. (2010). Differences between RDD telephone and abs mail survey design: coverage, unit nonresponse, and measurement error. *Journal of Health Communication*, *15*(S3), 117–134.

Roberts, C., Lipps, O., & Kissau, K. (2013). *Using the Swiss population register for research into survey methodology.* FORS Working Paper Series, paper 2013–1.

Sciarini, P., Goldberg, A., & Tawfik, A. (2013). *Nonresponse bias, measurement error and total turnout bias in post-election surveys.* Paper prepared for the meeting of the Swiss, German and Austrian election studies, Geneva, March 27–28.

Selb, P. & Munzert, S. (2013). Voter overrepresentation, vote misreporting, and turnout bias in postelection surveys. *Electoral Studies*, *32*(1), 186–196.

Stoop, I. (2005). *The hunt for the last respondent: survey nonresponse in sample surveys*. The Hague: Social and Cultural Planning Office.

Tourangeau, R., Kreuter, F., & Eckman, S. (2012). Motivated underreporting in screening interviews. *Public Opinion Quarterly*, *76*(3), 453–469.

Vandenplas, C. & Lipps, O. (2014). *Robustness of items within and across surveys*. FORS Working Paper Series, paper 2014-3, Lausanne.

Von der Lippe, E., Schmich, P., & Lange, C. (2011). Advance letters as a way of reducing non-response in a national health telephone survey: differences between listed and unlisted numbers. *Survey Research Methods*, *5*(3), 103–116.

von Erlach, E. & Zweers, U. (2012). *Mikrozensus Aus- Und Weiterbildung 2011*. FORS – SFSO Workshop, June 21. Neuchâtel.

Voogt, R. (2004). *I'm not interested. nonresponse bias, response bias and stimulus effects in election research. (phd thesis)*. Amsterdam: University of Amsterdam.

Appendix A
Translated text of the invitation

Dear Mr / Ms ...

Thanks to the election study Selects, it has been possible to analyze voting behavior in Switzerland since 1995. Located at FORS at the University of Lausanne and funded by the Swiss National Science Foundation, this project is supported by several Swiss universities. After the Swiss federal elections of October 23, we wish to investigate what motivates Swiss citizens and study how they evaluate political parties. You can find more information about the project at www.selects.ch.

Your collaboration is essential for us to conduct this study. We are interested in your opinion, independently of your political views and interest and even if, like many other Swiss citizens, you did not take part in the election.

Your name has been drawn at random from the register of the Federal Statistical Office and we would like to conduct an interview of about 30 minutes with you after the elections. Unfortunately, we do not have your telephone number. To enable the survey institute DemoSCOPE, which conducts the survey for us, to contact you after the elections, we kindly ask you to provide a phone number and your PID (personal identification number). You can use the enclosed business reply card or call us directly on the toll-free number [telephone number] or via e-mail at [e-mail].

The project manager of Selects, Professor Georg Lutz, will be happy to provide more information about the project ([telephone number] or [email]),

This study is for scientific purposes only. Your answers will be strictly confidential and completely anonymized. To provide reliable results, we need as many of the selected people as possible to participate. To express our thank for your valuable collaboration, we have enclosed a post-cheque of Fr. 20.- which you can cash at every post-office during the next three months.

We thank you in advance for your valuable help.

Signature [head of the project Selects] [responsible for the survey institute]

Appendix B
Translated text of the final reminder

Dear Mr./Ms...

A few days ago, we sent you a letter kindly asking you to provide us with a telephone number in order to contact you for a telephone interview for the scientific election study Selects after the October 23 federal elections. Unfortunately, we have not heard from you yet.

We depend on your help to conduct the project. As a thank you, we had enclosed a post-cheque worth 20 Fr. with the previous letter. You can cash this at any post office during the next three months.

We would be grateful if you could provide your telephone phone, or another number where you can be reached, as well as the PID (personal identification number) attributed to you (toll-free number [telephone number] or via [e-mail]).

As an alternative, you also have the possibility to participate in our survey online:

Link www.selects2011.ch

Personal Login: [login]

The project manager of Selects, Professor Georg Lutz, will be happy to provide you with more information regarding the project ([phone number] or [e-mail]). We remind you that this study is for scientific purposes only and that your answers will be treated with strict confidentiality and will be completely anonymized.

Sincerely,

Signature [head of the project Selects] [responsible for the survey institute]