Empirical Study on the Size of Nonresponse Bias

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Acknowledgement: Mehdi Zare, Stockholm University, helped us with the Solna survey.

Abstract
There are expressions for nonresponse bias, all of which require population quantities. In one expression for nonresponse bias, due to Bethlehem (1988, 2009), the bias is approximately equal to a function of the population covariance between the study variable and the response propensity (probability) and the population mean of the propensities. The covariance is hard to estimate (due to nonresponse). To empirically examine the covariance and the nonresponse bias, we have done two studies where the sample values of survey variables are known and the response propensities are estimated.
The first study is a mail survey of a population of residents in the city of Solna in Sweden, 20-74 years of age. The questionnaire consists of items on marital status and income; we have obtained the true values of those from the Swedish Tax Agency. We also know birth country, the type of area of residents, specific age and gender of each sampled individual.
The second study is a web survey at Stockholm University, the population is faculty employees at the department of psychology. This survey is a census and the variables that we regard as our study variables are income from university and total income. The true values of income from university are given by the HR-department and total income from the Tax Agency.
Key Words: nonresponse bias, true values, mail survey

1. Introduction

In many countries in Europe, Australia and Northern America nonresponse levels in social surveys are rising (de Leeuw and de Heer, 2002). However, it is not clear whether nonresponse bias is aggravating in the same pace as the nonresponse rates. There are some mixed messages as to the link between nonresponse rates and bias. On the one hand, theoretical work indicates that the nonresponse rate plays a direct and vital role for the nonresponse bias, see for example (1) below and the interpretation of this in Bethlehem (2009, Ch. 9). On the other hand, a number of recent empirical studies suggest rather the opposite, for example Kreuter (2013), Davern (2013) and Moore et al. (2016). See also other references in Brick (2013). Groves (2006) and Groves and Peytcheva (2008) compile nonresponse bias estimates from a large number of studies and conclude that the nonresponse rate is a poor predictor of nonresponse bias. Brick and Tourangeau (2017, p. 738) re-analysed the data of Groves and Peytcheva (2008) and concluded that 'response rates may not be very good predictors of nonresponse bias, but they are far from irrelevant' and that nonresponse rates do provide useful indicators of nonresponse bias.
We report on two surveys where we have evaluated the nonresponse bias in the presence of substantial nonresponse. Our idea was to put questions to randomly selected individuals which we know the answer to, in order to compare respondents with nonrespondents. We

focus on a theoretical expression for nonresponse bias from Bethlehem (1988), although Särndal and Lundström (2005) have presented a more general expression. Brick (2013) provides a review of the literature on nonresponse models and bias expressions.

2. Framework

We observe all units in a response set r, which is a subset of the sample s of size n. The inference framework is design-based. A population unit is assumed to have a propensity (probability) θ_k to respond to a particular survey item at a particular point in time, using the survey protocol, $k = 1,2, \ldots, N$. Then the bias is

$$Bias_{pq}(\bar{y}_r) = E_{pq}(\bar{y}_r) - \bar{Y} \approx Cov(y, \theta)/\bar{\theta}_U, \qquad (1)$$

where $Cov(y,\theta)$ is the finite population covariance of study variable $\mathbf{y} = (y_1, y_2, \ldots, y_N)'$ on a population U and $\boldsymbol{\theta} = (\theta_1, \theta_2, \ldots, \theta_N)'$, $\bar{\theta}_U$ is the population mean of the propensities and the expectation is taken over the sampling design $p(s)$, which is the probability that sample s is drawn, and the conditional response probability $q(r|s)$, which is the probability that the response set is r (Bethlehem 1988). The estimator of the population mean is $\bar{y}_r = \sum_r w_k y_k / \sum_r w_k$. The notation Σ_r stands for summation over the response set r and $w_k = \pi_k^{-1}$, π_k being the inclusion probability. The main approximation in (1) arises from a first-order Taylor series approximation.
The relative bias is

$$Bias_{pq}(\bar{y}_r)/\bar{y}_U \approx \rho(y,\theta) cv_y cv_\theta = \rho(y,\theta) cv_y \sigma_\theta / \bar{\theta}_U, \qquad (2)$$

where $\rho(y,\theta)$ is the finite population Pearson correlation coefficient of $\mathbf{y}$ and $\boldsymbol{\theta}$, σ_θ is the population standard deviation of $\boldsymbol{\theta}$ and cv_y is the population coefficient of variation of $\mathbf{y}$ (see Bethlehem, 1988). If $\bar{y}_r$ is replaced with the poststratification estimator, (1) becomes (as noted by Bethlehem, 1988)

$$Bias_{pq}(\bar{y}_r) = \sum_{h=1}^{H} \frac{N_h}{N} \frac{\sum_{U_h} \theta_k y_k}{t_{\theta:U_h}} - \bar{y}_U \qquad (3)$$

where N_h is the number of units in poststratum h and $t_{\theta:U_h} = \sum_{U_h} \theta_k$, the sum taken over units in poststratum h. Note that poststratum refers here to a subset of the population, not a subset of the sample.
For a binary study variable,

$$y_k = \begin{cases} 0 \\ 1 \end{cases}$$

and an estimate of the population proportion, you can show with a little algebra that (3) is

$$Bias_{pq}(\hat{p}_{post:r}) = -\frac{1}{N} \sum_{h=1}^{H} M_h (1 - \bar{\theta}_{1h}/\bar{\theta}_{U_h}) \qquad (4)$$

where $M_h = t_{y:U_h}$ is the number of 'ones' in poststratum h, $\bar{\theta}_{1h} = \sum_{U_h} \theta_k y_k \,/\, M_h$, so $\bar{\theta}_{1h}$ is the average propensity of those who have $y_k = 1$ in poststratum h.
The MAR condition, defined in Little and Rubin (2002, p. 12), is largely the same as the condition $Cov(y,\theta) = 0$ in (1). A design-based version of MAR is

$$q(r|s, \boldsymbol{x}_s, \mathbf{y}_s) = q(r|s, \boldsymbol{x}_s, \mathbf{y}_{obs}) \qquad (5)$$

where $\boldsymbol{x}_s$ are the auxiliary variables in the sample and $\mathbf{y}_{obs}$ are the observed study variable values. The condition (5) is essentially the same as the condition for the response mechanism to be ‘unconfounded’, defined in Lee et al. (1994). In this paper, we shall say that the response mechanism is ignorable if (5) is satisfied. If the response mechanism is ignorable then there will be no nonresponse bias, provided that the correct $\boldsymbol{x}_s$ enters the estimation, for example, to define poststrata.

Some other conditions for the nonresponse bias to vanish that centre on the response propensities are as follows. If the response propensities are constant in (2), then there is no nonresponse bias. Also, if the response propensities are constant *within poststrata* in (4), the bias is zero. Also, if $\bar{\theta}_{1h} = \bar{\theta}_{U_h}$ for all h in (4), the response mechanism is ignorable.

3. Other studies

Data about the level and spread of response propensities and correlations with study variables are scarce in the literature (Brick 2013). Kreuter et al. (2010) report on correlation between response and auxiliary variables in five large social surveys and find that all but one correlations are smaller than 0.10 in absolute terms. The only exception is for the correlation (≈ 0.50) between response and the paradata variable ‘negative comment during recruitment’ (for example, respondent saying ‘I don’t trust surveys’) in the 2004 American National Election Survey before the Kerry-Bush election. The interviewers recorded paradata from their initial doorstep chats with a household member.

Meng (2018) estimates the correlation between response and preference for Clinton or Trump, respectively, before the US election 2016. The data come from YouGov’s 2016 Cooperative Congressional Election Study. The state-level estimates for Clinton correlate with response between about -0.006 and 0.005, centred at zero, and for Trump between about -0.01 and 0.001, centred at -0.005. That is, the response propensity was smaller for Trump supporters than for those of Clinton.

Groves et al. (2004) find that the nonresponse bias in the proportion ‘being interested in the topic of the survey’ is less than 5 percentage points for a number of surveys and topics. They base it on an expression of the difference in the biases of the estimated proportion of people interested in the topic, where in one survey the topic is made salient and in the other survey it is not. Both surveys estimate the same proportion in the same population.

Groves et al. (2006) conducted two surveys where one item was about participating in bird watching. The way the surveys were presented was randomised as ‘Survey on birds, bird-watching, and birding’ and ‘Survey on the design of indoor shopping malls’. The frames were donors to the WWF and a general list of adults from a commercial company. In both surveys the ‘shopping mall’ treatment lead to a substantially higher response rate, about 14 percentage points higher in both surveys. However, the proportion of respondents in the USD2 incentive group who reported bird watching was about 8 percentage points higher among those who obtained the ‘birding’ treatment (larger differences in the group who were not given a monetary incentive).

4. Details about the two surveys

Our first study involves one self-administered mail survey in Solna in Sweden. The target population was all residents, 20-74 years of age, size about 60 000. Solna is a municipality with a population of about 78 000, with varying socio-economic status and voting rates. However, the education level is higher than the average in Sweden. Solna is located in the Stockholm metropolitan area, with a relatively low local income tax rate. The survey design was a SRS with sample size 500, acquired from the Tax Agency who drew it from the high quality population register they maintain. The questionnaire consisted of items on marital status and income; we had obtained the true values of those from the Tax Agency. We also know the address, age and gender of each sampled individual. Towards the end of the work with this paper, we obtained from the Tax Agency also data on birth country for every person in the sample.

The survey contained eight items, several of them were adapted from questions used by the SOM institute, which is an independent survey institute at University of Gothenburg in Sweden. The SOM institute studies attitudes and habits in a range of areas, in particular political attitudes. The first three items in our survey were about confidence in politicians in general, in the work of the Parliament and of Solna council. The fourth question read: How pleased are you in general with the life you are leading? These items originated from the SOM Institute. Then two items followed which we know the answer to: income and marital status. Item 7 was: did you vote in the election to the parliament 2014 and the final item was about the education of the respondent. At the bottom of page 3 in the four-page questionnaire the respondent was encouraged to give comments on the backside. We left ample space for comments because we got the impression in a pilot that some respondents really wanted to comment on some of the items.

On Thursday May 31 an A5-sized postcard was sent out as a prenotification. One week later the questionnaire was sent in an A5 letter. It contained a cover letter, the questionnaire, an addressed return envelope with pre-payed postage, all folded to fit into the covering envelope. In 250 cases, a lottery ticket worth SEK 10 (about one euro), with SEK 100 000 as the largest prize, was included. All postcards, envelopes and letters carried the logo of Stockholm University and the language was Swedish. The cover letter was in 400 cases signed in blue ink by the second author (DH). The rest of the cases had only the name of DH in print. On Wednesday June 13 another A5-sized postcard was sent as a thank you/gentle reminder. There were lovely pictures of Stockholm University in summer at the front page of both postcards. The name and address of the recipient was printed on the postcards and glued on as labels on the envelopes.

Apart from the usual information in the cover letter we stated the telephone number and email address of DH with encouragement to get in touch if there was anything the recipient wanted to talk about. Three people called and one sent an email with some comments. One comment by a caller was: ‘you are going to get a hefty nonresponse rate, have you thought about that!’ Since we did not want to be untruthful about the purpose of the survey, we just said in the cover letter that survey was about ‘some important issues’. The heading was ‘Hjälp forskningen’ (help research).

There was a link to a google form in the cover letter. As the cover letter was on paper, we included an offer to request a link to the survey by sending DH an email. One person did so.

The identification number, a three digit number, was printed at the bottom of cover letter and at the bottom of the last page of the questionnaire. One respondent had torn the number off.

Although we to a large extent adhered to advice by Dillman (2000, Ch. 4), there were several exceptions, notably:

- The prenotice letter was a postcard, without a real signature.
- No actual stamps on the envelopes were used in the first round, neither on the envelope containing the questionnaire nor on the addressed envelope for response. We used window envelopes, displaying the name and address of the recipient that were printed on the cover letter.
- In the fourth and subsequent rounds we did not mail replacement questionnaires to all nonresponding persons. Instead, we opted to make use of a response propensity indicator. Details are given below.

A reminder with a new questionnaire was sent to the 46 nonrespondents with the lowest value of the indicator (5.1) in Särndal and Lundquist (2014). The variance of the indicators for all units in the sample resembles the Mahalanobis distance between the sample and response set (see p. 366-368 in Särndal and Lundquist, 2014). The idea behind focusing on those with lowest values is that should you be successful in obtaining responses from these, then the variance of the indicators is reduced, and hence the distance between the response set and the sample. Thus one can say that the response set becomes more representative of the sample and hence also the population. A second reminder, with a link to the web questionnaire only, was sent to the 30 nonrespondents with the lowest value of the indicator. In all, the reminders resulted in eight responses, one refusal (by email, stating that he/she felt 'bullied' by too many letters from us). Eight individuals were identified as overcoverage through the reminder (the letters came back with 'address unknown').

The second study was a mail survey at Stockholm University, the population is faculty employees at the Department of Psychology. This survey was a census with population size of 82 and the variables that we regard as our study variable was salary from Stockholm University. The true values of salary from university were given by the HR-department. We invited the faculty members by email to take part in a survey without telling them that our purpose was to estimate nonresponse bias. One reminder was emailed after two weeks to the nonrespondents, with a note in the subject line that the survey was mobile phone friendly.

The first three of 13 items were about victimisation of any crime, and if so, whether the respondent had reported that to the police and subsequent experiences of the way the police addressed the report. The fourth item was about general concerns of crime in society. In two items we used randomised response with forced yes (Chaudhuri and Mukerjee, 1988). The respondents were asked to flip a coin and, depending on the outcome, either tick 'yes' or answer the question. These two items were about harassment and threats. Many of these items were taken with some adjustment from the Victimisation survey conducted annually by the Swedish Institute for Crime Prevention. Other items were about, for example, how long the respondent been working at Stockholm University and how many hours the respondent spent at the campus during the reference week.

In the census of members of the psychology department, 23 out of 82 responded, 28%. One had a missing value for salary (data on salary obtained from the HR-department). The departmental average salary was 45 289; it turned out the there was virtually no difference between respondents and nonrespondents: 45 100 for nonrespondents and 45 012 for respondents. The paper focuses on the first study.

5. Results of first study: descriptive statistics

Out of 500 sampled individuals, six were immediately identified as overcoverage (they did not live in Solna according to the sample file from the Tax Agency). Another sampled individual, we were told, had moved overseas. The mail to a further 15 individuals were returned with 'address unknown' stamped or written on the envelope by Swedish mail. We obtained responses from 195 individuals, 24 of whom responded by web. The response rate was 41%, 195 out of 478, or by AAPOR definition 3, 40%. The gender difference was noticeable although not very large: 38% and 43% of the women and men responded. Figure **1** displays number of responses per day. Note that June 18, June 25 and July 2 are Mondays; the fact that we could not receive paper questionnaires in the week-ends may explain why the Mondays are higher than other week-days. Note also that older respondents tended to respond earlier than the younger ones.

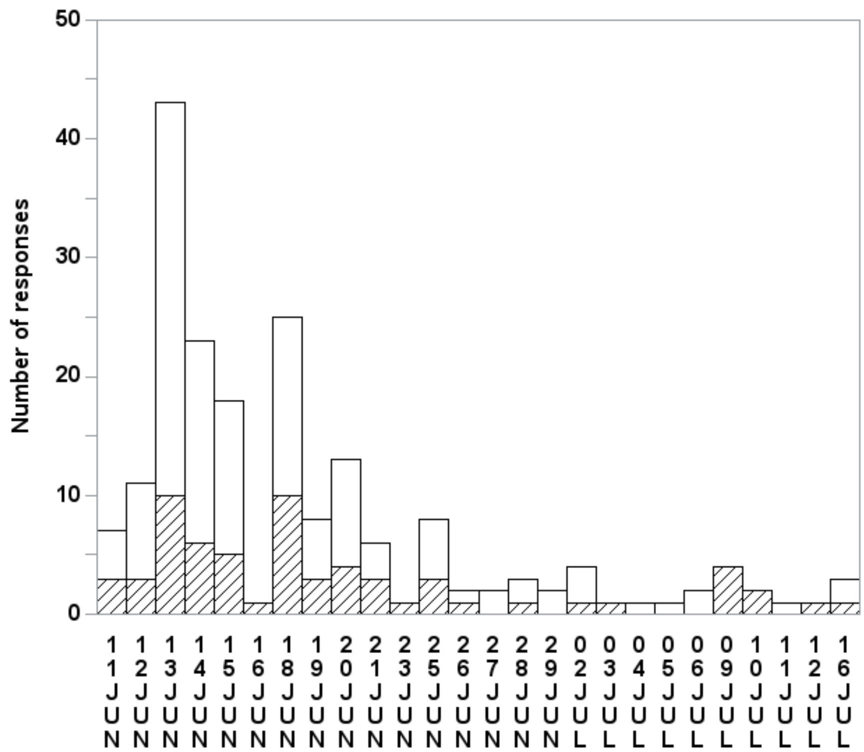

Figure 1: Number of responses per day up to and including July 16. The dashed areas correspond to respondents born in 1980 or later, the white areas to older respondents. The missing dates are all week-ends or a national holiday.

All names were classified into two categories: Swedish-sounding or non-Swedish sounding. Tables **1** and **2** report on the number of responses in age groups and in the two name categories. The response rates are lower among those with non-Swedish name, as expected, and the shape of response rates over age groups is also what we anticipated. There is no evidence of interaction between name and age that could explain response rate.

Table 1: Non-Swedish name. Number of responses, with row percent

Age group	Nonresponses	Responses	All
1990-98	16 73%	6 27%	22 100%
1980-89	37 72	14 28	51
1970-79	36 82	8 18	44
1960-69	17 59	12 41	29
1950-59	8 47	9 53	17
1943-49	4 67	2 33	6
All	119 70	51 30	170

Table 2: Swedish name. Number of responses, with row percent

Age group (born)	Nonresponses	Responses	All
1990-98	28 60%	19 40%	47
1980-89	52 66	27 34	79
1970-79	29 58	21 42	50
1960-69	32 58	23 42	55
1950-59	19 34	36 66	54
1943-49	4 18	18 82	22
All	164 53	144 47	308

There was a general election to the Riksdag (Parliament) in 2014. The national turnout was 85.8%, in Solna 85.9%. In our survey, 97% reported that they voted for the Riksdag, 170 out of the 175 people that reported that they were eligible to vote (you need Swedish citizenship to vote for the Riksdag and you have to be at least 18 years old). Two respondents chose the category ‘prefer not to say’ and one ‘don’t know’.

Table **3** contrasts self-reported education among the respondents with official statistics of Solna. Statistics Sweden maintains a register of the highest level of education of every citizen. The register contains only education obtained in Sweden. For this paper, we have only access to population numbers in Solna for the education levels in Table **3**. It is rather unusual to see a lower response rate among university educated than other groups.

Table 3: Response rates by education

Education level	Number of responses	Population in Solna	Proportion of responses
Less than nine years in school	1	1 081	0.09%
Primary school	14	3 460	0.40%
Secondary school	77	18 065	0.43%
University or similar	100	32 635	0.31%

Comparing self-reported income with that of the Tax Agency, we note a fairly high degree of measurement error, which will be analysed elsewhere.

6. Assessment of biases in the first study

As we know income (as reported by the Tax Agency) for the vast majority of the sampled individuals, we can ascertain the bias by simply comparing the sample and the response set means. For 43 people, the Tax Agency did not provide a value of income. For twelve of those we 'imputed' with their self-reported income. The remaining 31 cases were nonrespondents and are ignored in our analyses of bias of income. The sample and response set mean income was about 352 000 and 383 000, with cvs 0.58 and 0.62, respectively.

We estimated the response propensities $\boldsymbol{\theta} = (\theta_1, \theta_2, \dots, \theta_N)'$ with logistic regression, Cox hazard modelling (Olson and Groves 2012) and a parametric model. In order not to over-smooth the propensities and risk giving an overly optimistic view of the nonresponse bias, we have been very liberal with the choice of explanatory variables in the modelling, adding gender to age and Swedish/non-Swedish name, see Tables **4**-**6**. The mean propensity were 0.41, 0.33 and 0.32 for logistic regression, the Cox hazard model and the generalised gamma model, respectively. The minimum, lower quartile, median, upper quartile and maximum of the fitted propensity using logistic regression were 0.15, 0.30, 0.40, 0.51 and 0.73.

Table 4: Estimates of propensities by Logistic regression model

Explanatory variable	coeff	robust std error	p-value
gender	-0.30	0.195	0.124
Swedish/non-Swedish name	0.68	0.208	0.001
age	0.03	0.007	0.000
constant	-2.13	0.353	0.000

Table 5: Estimates of propensities by Cox hazard model

Explanatory variable	coeff	robust std error	p-value
gender	-0.192	0.139	0.166
Swedish/non-Swedish name	0.513	0.162	0.002
age	0.025	0.005	0.000

Table 6: Estimates of propensities by generalised gamma model

Explanatory variable	coeff	robust std error	p-value
gender	0.244	0.074	0.001
Swedish/non-Swedish name	-0.212	0.067	0.002
age	-0.005	0.003	0.061
constant	1.975	0.245	0.000

In order to evaluate our three models we evaluated percentage correctly classified for different values of a threshold value, for which an object is classified as responding if the propensity is above the threshold. Threshold values between 0.3 and 0.6 were evaluated, see Table **7**.

Table 7: Percentage correctly classified, as responding or not responding, compared with actual response/nonresponse

Model threshold value	Logistic regression	Cox hazard	Generalised gamma
0.3	51 %	41	25
0.4	60	52	25
0.5	60	55	65
0.6	65	58	65

Table 8 reports on the bias of income computed with the sample statistics substituted for the population parameters in (1). We denote the sample statistics that estimates the population parameters by $\widehat{\mathrm{Cov}(y,\theta)}$, $\hat{\bar{\theta}}_U$, and similarly, for parameters in (2) and (4). Table 8 contains also the corresponding relative bias. The standard error of the bias was estimated with jackknife using the macro in SAS Institute (2005). The estimated bias is significantly different from zero for propensities estimated with logistic regression and the proportional hazards model. To gauge the size of the bias, it is useful to compare it to square root of the estimated variance of the mean, which is 10 436. That is, the bias for the logistic and Cox hazards models is large. Poststratifying by age does not help, on the contrary, the relative bias grew to 0.21. At first glance this seems surprising, given that age is associated with both income and response propensity. However, the estimated correlation $\rho(y,\theta)$ was about the same in most poststrata as in the whole sample and the other factors in (1) did not reduce much through poststratification.
Post-stratifying with a cross-classification of Swedish/non-Swedish name and age groups or birth country (e.g. born in Sweden/not born in Sweden) and age groups did not change the bias appreciably. Using Swedish/non-Swedish name is not proper poststratification because we do not have the population totals of that variable.
While the parametric generalised gamma model fitted worst, it produced the best estimate of mean income.

Table 8: Estimated bias of mean of income (standard deviation in parenthesis). No poststratification.

	Logistic regression	Cox proportional hazards	Parametric model
$\rho\widehat{(y,\theta)}$	0.246	0.162	-0.019
$\widehat{cv}_y$	0.60	0.60	0.60
$\hat{\sigma}_\theta$	0.14	0.15	0.15
$\hat{\bar{\theta}}_U$	0.41	0.33	0.32
Bias	18 326.1 (4234.4)	16 208.0 (5177.8)	-1890.2 (4133.7)
Relative bias	0.050	0.044	-0.005

We turn now to the bias of estimated proportion of divorced. The sample and response set proportions of divorced were 0.099 and 0.158, that is, a difference of about 0.06 with a larger proportion among the respondents than the nonrespondents. We chose the category divorced rather than any other marital status category because the difference between sample and response set was largest for divorced. Like above, we use sample statistics instead of population parameters to compute the bias according to (4). Without poststratification, we found with logistic regression that $\hat{\hat{\theta}}_1 = 0.469$, and, as has been mentioned, $\hat{\hat{\theta}}_U = 0.408$ and $\hat{P} = 0.123$, and the bias by (4) is 0.019. A jackknifed standard error is 0.007 (SAS Institute, 2005), hence the bias by (4) is significantly different from zero. With five groups, two age groups for people born in Sweden, and three for people not born in Sweden, the bias by (4) went down to 0.002 with a standard error of 0.0002. The biases for both poststratification and no poststratification were largely the same with Cox hazards and the parametric model.

Note that we for income and proportion of divorced do not use the self-reported data (apart for 'imputed' income for twelve respondents). There is another study variable that offers some insight into nonresponse bias: self-reported voting turnout in the 2014 Parliament election. We are able to estimate the proportion of voters based on self-reported data from the respondents and compare with the official turnout in Solna, which was 89.4 and 88.2% for age groups 30-49 and 50-64, respectively. We estimated the turnout with poststratification using 3 age groups within each of the broad age groups 30-49 and 50-64, and the proportion among the respondents within poststratum as domain estimator. Further poststratification leads to too small numbers of respondents in poststrata. The estimates in the age group 30-49 were 96.8% and 87.4% with the poststratified estimate last. In group 50-64, the estimates were 94.7% and 82.8%. At least the estimated turnout in the older group is biased. We note that the poststratified estimates are lower than the real turnout. Poststratification may have overcompensated the social desirability bias that we would expect.

Although any comparison of one municipality with the country is, of course, debatable, it may still be of some interest to compare the responses to two of the items with the national estimates of the SOM Institute. One question read 'How pleased are you in general with the life you are leading?'. The estimated proportion who are 'very pleased' was 40.5% and 35.1% and (the poststratified estimate last). The national estimate of the SOM Institute was 38% in 2017.

The estimated proportions with high or very high confidence in politicians in general were 35.2 and 28.8% (again the poststratified estimate last), compared to the national estimate 39% of the SOM Institute.

7. Discussion

With known true values for two variables we can produce valid estimates of nonresponse bias. The unit nonresponse rate was about 60%. There was a small proportion of item nonresponse to the income question but we focus on unit nonresponse. For income we found that the relative bias was about 5%, with or without poststratification. For proportion divorced the bias was about two percentage units without poststratification, and about 0.2 percentage points without poststratification.

The strength of this study is the validity of the estimates of nonresponse bias. The main weakness lies in its reach: it is not clear whether the results can be generalised beyond this study in Solna in Sweden.

In our study we compared three different models for estimation of response propensities. There was a significant difference in size of bias when we estimated the mean income, where the parametric model performed best. When we estimated percentage divorced the three models gave about the same size of bias. To evaluate the three models, percentage correctly classified objects were calculated. The parametric model did not do as well as the two other models for lower values of the threshold. Further work will be done to evaluate the impact on bias for different models of the response propensities.

The mean income of the respondents is slightly higher than that of the sample, and the voting turnout is clearly higher among the respondents. Brick and Tourangeau (2017) provide a useful typology of response propensity models. In their first three models, the random, the design-driven and the demographic-driven propensities model, most of the variation in response propensities are due to ‘transient influences’ (something that distracts the respondent temporarily), design features or demographic characteristics that are only weakly associated with the characteristics of the sampled persons. These are in general not deleterious. The fourth response propensities model is potentially more difficult; it is referred to as correlated propensities by Brick and Tourangeau (2017). This model is similar to the not missing at random response mechanism, NMAR (Little and Rubin, 2002), or a non-ignorable response mechanism. Brick and Tourangeau (2017) mentions ‘a sense of civic obligation’ as a cluster of variables (e.g. whether you vote) often related to response propensity. If such a variable is also a study variable, the correlated propensity model may be the model in the typology that fits best. In our study we have estimated the nonresponse bias for mean income and proportion divorced. The fact that we have higher mean income and a higher voting turnout in our response set than in our sample, suggests that in our study, the response mechanism may be of the ‘correlated propensities’. The correlation between propensities and income was not weakened by poststratification by age groups and birth country. For the variable being divorced or not, the weak correlation was further attenuated by poststratification.

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