

WP 4 deliverable

Push-to-web communication with persons and households:

Advance letters, reminders, flyers, envelopes, QR codes, and incentives.

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Introduction

Starting in 2005, Statistics Netherlands (CBS) has gradually incorporated web data collection into its data collection strategy for surveys involving persons and households. First, web data collection was only implemented in experimental settings. From 2012 it was incorporated into the data collection strategy of the Labour Force Survey (LFS) after an extensive period of testing. From that moment onwards web data collection has become common practice. Typically, a sequential mixed-mode strategy is used, in which web interviewing (CAWI – computer assisted web interviewing) is followed by telephone (CATI – computer assisted telephone interviewing) or face-to-face (CAPI – computer assisted personal interviewing) interviewing. Other designs than this complete mixed mode design are also used however (web – CATI, web – CAPI, web – mail). Whether a sample unit goes to CATI or CAPI in a complete mixed mode design depends on the availability of a telephone number and the size of the household: larger households are interviewed face-to-face. Non-respondents in CATI are generally not transferred to CAPI. The web approach takes place in month t , the telephone approach in month $t+1$, and the face-to-face approach in month $t+2$.

Not all non-responding households of the web phase are followed up in the subsequent mode, however. In order to make optimal use of the cheaper web mode, and also to be able to keep the number of sample units that go to CAPI and CATI stable each month, a relatively large sample is drawn for the first web phase, of which a subsample is drawn for the follow-up phases. The subsample is stratified by interviewer region and known telephone, but is otherwise random.

The data collection strategy for the first wave comprises the following steps:

1. All sample units receive a letter containing the internet address of the web questionnaire and a personal login. For household samples the letter is addressed to 'the inhabitants of [address]'. Person samples are addressed by name, which is derived from the communal registry. The person or household selected is requested to complete the questionnaire via the internet. All household members need to use the same login to gain access to the questionnaire: i.e. household members do not receive an individual login.
2. Two reminders are sent to non-respondents two weeks and three to four weeks after the advance letter. Two reminders was determined in prior research to be the optimal number.
3. One week after the second reminder the access to the web questionnaire is closed.
4. Follow-up in the other modes is mentioned in the advance letter, but no separate letters are sent before the CATI or CAPI follow-up. Mentioning of the follow up mode was also experimentally shown to be preferable to not mentioning it: for some respondents, the

¹ The views in this paper are those of the author and do not necessarily represent those of Statistics Netherlands.

‘threat’ with an interviewer is an extra leverage to make them access the web questionnaire. For other respondents, the ‘promise’ of an interviewer makes them wait patiently for the call or visit.

The consecutive designs, starting with web, constitute a substantial saving of data collection costs. Increasing web response has therefore been the focal point of many experiments. In chapter 1, various CBS experiments on advance letters, flyers, and envelopes that were performed in recent years will be described. Also in chapter 1 an experiment on the use of QR codes to stimulate web response is described. Chapter 1 ends with a description of the use of social media in inducing people to respond. In chapter 2 literature on incentives is summarized, followed by the results of a number of CBS experiments with various kinds of incentives, both in cross sectional as in longitudinal surveys will be summarized.

In all of these experiments, not only overall response has been the target of investigation, but also the distribution of response over various sub-groups in the population. In the incentive experiments we also looked into data quality and the effect on substantive variables.

1. Advance letters, envelopes, flyers and QR codes in web surveys

Statistics Netherlands does not have a registry of email addresses of the general population, so the only way to alert sample persons or households to our request of participation is with a letter. The letter contains the ‘standard’ introduction to the survey and the survey request, but also the web address, a login code, a password and a short instruction how to log on to the web is included. This additional content made the letter long (one and a half page long) and complicated. An initial attempt to simplify and shorten the letter led to a disastrously low response rate, however (Luiten and de Groot, 2014). As we did not understand the reason for this, we decided to systematically vary aspects of the letter in a series of experiments. The research team consisted of people from the data collection department, data collection methodologists and communication experts.

For the experiments we make use of 10% of the sample for the Labour Force and we vary on the LFS advance letter and reminder letters. The LFS uses an address sample. Letters to sample units are addressed as ‘to the inhabitants of *address*’. Using the LFS for these experiments is sub-optimal, because of the household aspect. This makes it unclear who has read the letter and who subsequently made the decision to participate. The LFS is however the only survey where monthly experiments in the production could be performed without hazard for the substantive findings, while at the same time rendering sufficient data to be able to look at subgroup response rates. Promising results were replicated in the LFS for additional possibilities for detail, and also in person sample surveys.

Of all sample addresses, the age, gender, household composition, and ethnic background of the persons living there is known. For the analyses we made use of the same strata that were also used in the LFS sampling procedure:

1. Households with at least one person of 65 years of age or older. The persons can be of western or non-western background.
2. Households where at least one person is unemployed and registered at the unemployment office.
3. Households with children from 14 to 26 year old.
4. Households where at least one person is of non-western ethnic background.
5. All other households.

In this chapter I summarize the experiments we did with advance letters, envelopes and flyers. In each paragraph an overview of the experiments is followed by a description of the background of the experiment, and the results. In all analyses three dependent variables are examined: the first is the number of households starting the questionnaire, the second the number who drop out during filling in of the first person questionnaire. The first measure is a more direct measure of the effect of the letter. The second measure may show if the letter rises expectations that are disappointed in the questionnaire. The third measure is the resulting response rate. Households are considered as response if all household members have responded. A second measure of break-off is the number of partial responses, i.e., if there are missing questionnaires for some household members. Partial response also impacts on ultimate response rates, but are not shown here.

Subgroups analyses are made on the strata described above, and in addition on a number of background variables: age and gender. These variables consist of the mean value of these variables for the household core of one or two persons. Hence, the variable Gender consists of the values men (either single men or households consisting of two men), women, and mixed gender households. In addition, for each household urbanicity, income and ethnic composition in terms of the percentage of non-western migrants of the neighborhood are determined on postcode 6 grids. These very fine grained grids of (parts) of streets are standardly used in nonresponse analyses and have proven to be highly predictive of response behavior. Linking sample households with background variables was not always unequivocally possible. In a number of cases it could not be determined of which persons a household consisted, or it looked like more than one household lived on a certain address. In those cases, the household information was recoded as missing, and analysed as a separate value.

Analyses are performed with logistic regressions and analyses of variance. For overview and simplicity, only the level of significance is mentioned in the tables. Because in some cases the number of households in subgroups is relatively low, significance levels of $<.10$ are reported as well. Additional information on the analyses is available from the author.

1.1. Experimental letters

In the letter experiments, we tried to vary only one aspect of the original LFS letter. The following experimental letters were used:

1. Visually shortened letter, to make all information visible on the front page
2. More and strengthened persuasion arguments
3. Simplified linguistic level
4. Strengthened 'what's in it for me' message
5. More detailed login information
6. A small 'reminder' note block in the letter
7. Full blown approach
8. Moment of delivery of the letter
9. Informed consent text

In all cases, two reminder letters were sent in the same format. See appendix 1 for the original LFS advance letter. As can be seen in this letter in the appendix, at the bottom of page 2, additional text is added after the signature of the head of the data collection department. The first paragraph is additional information on data security. The second paragraph is the so called 'informed consent' clause, where respondents are informed that their responses will be linked to registry information. The clause and the additional security information is referred to in the letter. In all experiments but

the last one, the clause is not changed for the experiments, as this would have legal implications. Appendix 1 also gives a translation of the letter.

1.1.1. (Visually) shorter letter

Literature (e.g., Dillman, Smyth & Christian, 2014) stresses that a short advance letter with a maximum length of one A4 is crucial. We managed to confine the information of the original LFS letter to one A4 by putting all the information pertaining to logging on to the website (address, passwords and instruction) in a cadre in the letter, in a space that was otherwise blank. Appendix 2 shows the experimental letter. In violation of our endeavor to change only one aspect, the phrase on the security of the internet connection was no longer included in the letter.

Results

Table 1. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		<i>p</i>	contr	exp	<i>p</i>	contr	exp	<i>p</i>
	%	n	%	n		%	%		%	%	
65 and over	26,4	557	31,1	61	ns	7,5	10,5	ns	24,2	27,9	ns
unemployed	26,3	2.240	22,5	249	ns	17,1	16,1	ns	20,2	17,7	ns
hh with children 14-26	28	3.791	29,5	421	ns	22,4	28,2	ns	18,8	18,5	ns
non western background	18,8	1.848	16,6	205	ns	28,2	38,2	ns	12,1	8,8	ns
all others	34,2	4.068	29,6	452	<i>p</i> =.05	13,8	9	ns	27,5	24,8	ns
overall	29,9	11.665	28,4	1.185	ns	17,1	18,2	ns	22,7	21,3	ns

In addition, a significant interaction was found between (neighborhood) Income and experimental condition ($p < .05$), indicating that in the second and third income quartile, the percentage of households starting the questionnaire was significantly lower in the experimental condition than in the control condition, see table 2. A second interaction was found with Gender ($p < .10$), indicating that mixed gender household cores had a lower starting rate, see table 3. Higher starting rates on the other hand for the highest income households and for male household were not significant.

Table 2. Percentage of starting households by Income and experimental condition.

Income	started				
	control		experiment		<i>p</i>
	%	n	%	n	
Lowest quartile	21,6	2240	18,2	214	ns
2nd quartile	29,0	2314	21,3	249	$p < .05$
3rd quartile	34,3	2440	27,9	270	$p < .05$
highest quartile	37,1	2638	41,1	331	ns
missing	21,2	1033	26,9	119	ns

Table 3. Percentage of starting households by Gender and experimental condition.

Gender	started				
	control		experiment		<i>p</i>
	%	n	%	n	
men	24.4	1721	28.3	187	ns
mixed	35.6	5706	31.9	623	$p < .10$
women	23.5	2210	20.3	256	ns
missing	21.0	1028	26.9	119	ns

No other interactions were found.

It is clear that just visually shortening the letter did not lead to increased response rates, but had in contrast borderline negative overall results, and negative results for some subgroups. Putting the

login information in the cadre meant that less information on the correct procedure to log on to the web was given. The usual instruction, short as it may be, was replaced by a visual example. As is reported in Luiten (2015), a survey among respondents in the CATI and CAPI follow-up rounds of web of reasons why they did not participate in web, indicated that the computer and internet literacy of a fairly large part of households should not be overestimated. As a result, a new experiment was designed, in which extra care was taken to describe the login process, see paragraph 1.1.5.

1.1.2. More and strengthened persuasion arguments

In the standard advance letter hardly any arguments are mentioned why people should participate in the LFS. The arguments that are mentioned may not be equally attractive to all people ('for the quality of the figures it is very important that you participate', 'You represent many other people'). In this experimental letter we tried to reinforce the arguments why people should participate, both altruistic arguments ('you help CBS / the researcher / society') and more egoistic arguments ('what's in it for you'). The altruistic arguments are easy to find, and a number of those were used: '*we need your help ...*', '*you help us ...*'. Egoistic arguments are harder to find for the LFS. This survey has a strong political use, but the link between what is good for society or for politics and what is good for the individual is not explicit. Nevertheless, these were the only kind of arguments we kept coming up with. Apart from the phrase that is also used in the regular letter 'This research is an indispensable source of figures about ...', the following phrase was included: '*Because of the results, we know how things stand in the labor market, in education and in social security*'. In addition, we added that this is one of the most important CBS surveys, and that the respondent is one of a small number of households chosen to participate.

Results

Table 4 shows that strengthening the persuasion arguments did not have an effect on the percentage of households starting with the web questionnaire. It did, however, have an undesired effect in terms of a larger number of households breaking off the questionnaire. This phenomenon was observed in all subgroups, hence almost reaching significance in the overall results. Perhaps the appeal for help did not agree with the subsequent contents of the questionnaire. The increase of more than 8 percentage points of elderly households who started with the questionnaire was not significant with these sample numbers, but these elderly households also had less partial response, and agreed more often to participate in round 2 of the LFS, resulting in a marginally significant higher percentage of elderly households entering the second wave (results not shown here).

Table 4. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment			contr		exp	contr		exp
	%	n	%	n	p	%	%	p	%	%	p
65 and over	28,5	565	37,1	62	ns	8,1	8,7	ns	25,7	33,9	ns
unemployed	25,1	2.211	22,8	246	ns	16,8	23,2	ns	18,5	14,6	ns
hh with children 14-26	28,2	3.755	28,2	418	ns	26,9	28,8	ns	18	16	ns
non western background	16,1	1.909	18,7	214	ns	26,3	35	ns	10,5	8,9	ns
all others	33	4.062	30,6	451	ns	13,3	18,8	p<.10	26,8	23,9	ns
overall	28.9	11.665	28.1	1.185	ns	17.7	21.9	p=.06	21.8	19.9	ns

No further interactions between background variables and dependent variables were found.

1.1.3. Decreased linguistic complexity of the letter

Not only in the Netherlands, but in the European context, it is recommended that the language level of communication with the general public should not exceed level B1. This is the level that is

understood by 95% of the population. In order to attain this kind of clear language only high frequent words should be used, and sentences should be no longer than 15 words. The standard advance letter for the LFS has a higher complexity (B2), with a large number of words that are too difficult. The letter was redesigned according to these criteria, while maintaining the original contents. Appendix 4 gives a translation of this simpler letter.

Results

This experiment had very positive results, to the extent that in all strata the number of households starting with the questionnaire was higher, significant in more than one group, and significant overall. We also found that the experimental group showed a higher break off, so that the final response result was no longer significantly higher. Because of the positive results, this experiment was replicated. Table 5 below shows the results of the combined experiments.

Table 5. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr		exp	contr		exp
	%	n	%	n		%	%		%	%	p
65 and over	28,5	1.167	29,7	128	ns	6,0	7,9	ns	25,9	27,3	ns
unemployed	26,7	4.468	27,9	499	ns	16,5	21,6	p=.10	20,4	19,2	ns
hh with children 14-26	27,5	7.632	32,5	850	p<.01	24,6	22,5	ns	18,1	21,1	p<.05
non western background	17,0	3.874	20,7	430	p<.10	24,1	28,1	ns	11,7	12,8	ns
all others	34,6	8.238	36,4	916	ns	14,4	15,3	ns	28	28,9	ns
overall	29,7	22.589	32,7	2.402	p<.01	17,3	18,1	ns	22,7	24,4	p<.10

Although there is a significantly higher number of households that start filling in the questionnaire with the simpler letter, there is regrettably also a higher number of households that break off, almost reaching significance in the group of unemployed people. As a result, the overall increase in response was no longer significant overall, although still significant in the subgroup of households with children in the age group 14 to 26. We reasoned that the higher number of starters, but also the higher number of break-offs could be both the effect of the LFS questionnaire, that also contains complicated language, but also the effect of having to fill in the questionnaire on the web. People with low literacy also have low computer skills (e.g., Buisman and Houtkoop, 2014). Nevertheless, these results induced us to make it a policy to write all advance letters, flyers and other communication with respondents with this lower linguistic complexity. In addition, we have started a program to lessen the linguistic complexity of survey questions.

No further interactions with other background variables were found.

1.1.4. Strengthened 'what's in it for me' message

In paragraph 1.1.2 we have shown that the increase in altruistic persuasion arguments lead to positive results for elderly households, but not for other households. In this experiments we tried the opposite tract: strengthening the what's in it for me message. The experimental letter started with a number of stimulating questions about topics that the design team thought would appeal to the public. The first paragraph read as follows:

'Does everyone get paid the same amount for the same work? How many job vacancies exist and where? How do men and women distribute care and work? With which education do I get the best opportunity to find work? At what age are we going to retire? All questions about the labor market. Questions that CBS can answer, but only with your help!'

In the advance letter as well as the reminders, a footnote indicated three different websites where interesting CBS animations could be found.

Results

Table 6 showed that this approach again did not lead to higher response rates, but on the contrary to significantly lower response rates. The percentage of households starting the questionnaire was significantly lower, and the percentage break-off was marginally higher. Additional analyses, see table 7 showed that break-off was significantly higher in households who lived in somewhat mixed ethnic neighborhoods. The interpretation of this finding is not clear however.

Table 6. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment			contr	exp		contr	exp	
	%	n	%	n	p	%	%	p	%	%	p
65 and over	25,7	567	20,6	63	ns	8,2	7,7	ns	23,3	19	ns
unemployed	26,4	2.138	23,1	238	ns	15,2	16,4	ns	19,8	18,1	ns
hh with children 14-26	27,5	3.783	20	419	p<.01	25,1	32,1	ns	18	11,7	p<.01
non western background	16,3	1.906	12,2	213	p=.10	24,4	19,2	ns	11,4	8,9	ns
all others	32,9	4.109	28	457	p<.05	14,7	18	ns	26,6	21,7	p<.05
overall	28,7	11.665	23,3	1.185	p<.0	18	22,1	p<.10	21,8	17	p<.0

Table 7. Percentage of households breaking off by neighborhood composition and experimental condition.

	break-off				
	control		experiment		<i>p</i>
	%	n	%	n	
% non-western background					
0%	16,5	1130	15,4	104	ns
1-5%	14,2	557	30,4	56	<i>p</i> <.01
6-15%	16,5	617	29,1	55	<i>p</i> <.05
16-35%	21,8	339	21,2	33	ns
>35%	20,5	185	0	10	ns
missing	30,9	230	27,8	18	ns

1.1.5. Detailed login information

From prior research into the reasons why respondents in CATI or CAPI did not answer the questionnaire in web (Luiten, 2015), it appeared that technical problems and unfamiliarity with computer or internet were an important reason not to participate in web. Especially the practice to type in the web address in a search engine instead of the address bar led to many a telephone call to the technical helpdesk. In this experiment, the procedure for logging in to the website and entering the login code is described in detail in the letter of invitation, in a clear step-by-step plan with visual support. See appendix 5 for an example and translation.

Results

Table 8 shows that the more detailed instruction led to a significantly higher number of households with children starting the questionnaire. On the other hand, it had an almost significant effect on elderly households. There were no differential effects of the instruction on break-off, so as a result the response results mirror those of the start results. This result was somewhat unexpected, as we

had actually targeted the elderly with this instruction. They were the ones who indicated the largest number of problems with computer and internet (Luiten 2015). Perhaps the instruction had the unwanted effect of overwhelming these households. The analyses of the other background variables showed that especially the households living in neighborhoods with a high percentage of non-western migrants were susceptible to this measure, see table 9. This is a highly welcome result, as this is a very difficult group to get response from. No other interactions were found.

Table 8. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment			contr		exp	contr		exp
	%	n	%	n	p	%	%	p	%	%	p
65 and over	28,2	570	19	63	p=.10	6,2	0	ns	25,3	17,5	ns
unemployed	25,8	2.207	27,8	345	ns	18,2	19,1	ns	19,5	19,6	ns
hh with children 14-26	29	3.779	35,1	419	p<.01	26,9	28,6	ns	18,5	22,4	p=.05
non western background	15,7	1.894	15,7	210	ns	31,5	39,4	ns	9,8	8,1	ns
all others	34,3	4.070	35,5	453	ns	13	16,8	ns	28,5	27,4	ns
overall	30	11.665	31.9	1.185	ns	18.1	20.9	ns	22.8	23.1	ns

Table 9. Percentage of households starting by ethnic composition of neighborhood by experimental condition.

started					
% non-western background	control		experiment		
	%	n	%	n	p
0%	35,6	3382	38,3	368	ns
1-5%	35,7	1747	31,1	215	ns
6-15%	32,5	2111	33,8	219	ns
16-35%	25,6	1556	29,6	179	ns
>35%	14,9	1204	25,0	140	<.01
missing	16,2	665	12,5	64	ns

For some groups in society, a clear instruction on how to log on to the web is necessary. Another option is the approach Statistics Netherlands, following the practice of Statistics Denmark, has taken since this experiment, i.e., to make the login procedure more user friendly. New and redesigned questionnaires hence forward all have the same (easy) internet address. Passwords and login codes determine the correct questionnaire. The result is that the (frequently used) internet address is also found in search engines, and can be used from there as well.

1.1.6. A small 'reminder' notepad included with the letter.

One of the most common reasons non-respondents on web gave for not completing the questionnaire in web was that they had forgotten it (Luiten, 2015). In an attempt to help them remember to fill in the questionnaire, a small notepad with adhesive sticky tapes was added to the envelope. The notepad's cover was embossed with a 'don't forget' logo. See appendix 6 for an example. Adding the notepad had two additional advantages: it could make the envelope more noticeable in the mail, and the notepad is an incentive.

Results

Table 10 shows that there was one group where the incentive had a marginally significant effect on the number of people who started filling in the questionnaire, notably the unemployed. However, the same group also had a marginally significantly higher percentage break-off, so that the net effect for

this group was nil. The group with children 14 to 26 also showed a somewhat higher break-off. No interactions with other background variables were found.

Table 10. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr	exp	p	contr	exp	p
	%	n	%	n							
65 and over	27,8	576	23,4	64	ns	4,4	0	ns	25,9	18,8	ns
unemployed	25,9	2.175	31,0	242	p<.10	17,9	26,7	p<.10	19,5	19,4	ns
hh with children 14-26	29,1	3.686	31,5	410	ns	23,1	29,5	p=.10	19,8	18,3	ns
non western background	17,2	1.904	15,0	410	ns	26,8	25,0	ns	11,5	9,4	ns
all others	34,5	4.152	36,9	461	ns	14,3	11,8	ns	27,7	30,8	ns
overall	30,2	11.665	32,1	1.185	ns	17,1	19,5	ns	23,1	23,3	ns

The notebook obviously did not succeed in its intended role of getting people to remember filling in the questionnaire. It did not particularly do well as an incentive either, but that was not our intention. We know from literature that incentives in kind (a product) are far less effective than monetary incentives (see chapter 2 or Luiten and Groffen, 2018, for an overview). In addition, the household setting where it is unclear who would receive the incentive, does not help.

There are other things that were noteworthy about these results. Figure 1, see below, shows that the experimental condition had a clearly higher response in the beginning of the fieldwork. After the first reminder the difference becomes less and after the second reminder the difference disappears. That means that the notebook was instrumental in inducing respondents to respond right away. That could be an interesting result, if it precludes the survey organization from sending reminder letters. We therefore calculated the costs that had to be made for the experimental condition including the notebook and compared to the control group. It appeared that although fewer letters needed to be sent in the experimental condition, the costs savings were not sufficient to finance the incentive.

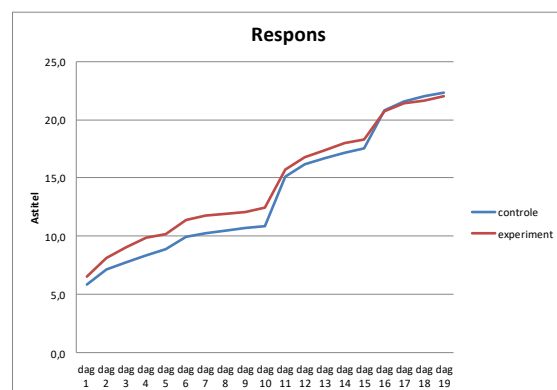


Figure 1. Response per fieldwork day by experimental condition

1.1.6.1 Replication in a person sample: the Safety monitor

A replication experiment was performed in a person survey: the Safety Monitor. 2000 sample persons of 16 years and older were sent the reminder notepad with the advance letter. Table 11 shows that response rates were significantly higher with the notepad (p<.00.).

Table 11. Response by experimental condition.

	n	%
no notepad	19.444	39,5
notepad	2.000	44,0

Subsequent analyses showed that the incentive did not have an effect in the age group of 65 years and older, had a marginally significant ($p < .10$) effect in the age groups from 30 to 63, but had especially effect in the youngest age group (16 – 29), $p < .001$. The increase in response in the latter group was 6 percentage points. In addition, the incentive had no effect with persons with a non-western migration background, a marginal effect with persons with a native background ($p = .05$), and a significant effect with persons with a western migration background ($p < .05$). The incentive was not successful in all income groups, but was particularly successful in the middle income group ($p < .001$).

1.1.7. Full blown ‘all the way’ approach

In this experiment, the principle that only one element is varied at a time was released. For one time we wanted to maximize the creative potential of the research group, in which various specialists in the field of communication were represented. This has led to a considerably adjusted approach. In the first place, households were sent an attractive card one week before the letter of invitation (a so-called pre-notification), which stated that they had been selected for CBS research ('You have been selected!'), and that they will within shortly be invited to participate. The card provides information about the CBS and contains information to navigate to CBS's LFS website. The twitter address and the face book address are also given. See appendix 7 for the pre-notification card.

The subsequent advance letter was also considerably modified: due to the information in the pre-notification card, part of the information that is normally stated in the letter could be omitted. The letter was therefore considerably shorter and easily fitted on one A4. We also worked with headings in a contrasting color, see appendix 8 for an example. The style of the headings and their content is derived from successful research on an advance letter in the United Kingdom (Nicolaas and Smith, 2015). The first headline in the letter of formal notice was: 'You have been chosen to participate in CBS research'. The first headline in the first reminder was: 'This is a reminder to take part in CBS research' and the first headline in the second reminder was: 'This is your last chance to participate in CBS research via the internet'. See appendix 8 for the advance letter and translation.

In between sending the advance letter and the first reminder, another card was sent, this time in the form of a new year card, with the text *'in 2015 we have been able to publish reliable information on a wide range of topics, with the help of people like you. We really appreciate that'*. The other side of the card read *'happy 2016. We like to continue publishing what actually happens. Can we count on you again? On behalf of all employees of CBS, I wish you happy holidays and a prosperous 2016'*. On the inside of the card, CBS facts were presented (for example, *300,000 people found work in the second quarter of 2015*). The rationale for sending the card is that we wanted an extra contact moment without 'nagging'.

Results

To the disappointment of the research group, this experiment was spectacularly unsuccessful, and *lowered* starting and response rates significantly overall and in all subgroups, significantly in some of them. See table 12 for results. There were no differences between experimental and control group in the amount of break-off. That means that the difference in response is entirely due to differences in starting the questionnaire.

Table 12. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr		p	contr		p
	%	n	%	n		%	%		%	%	
65 and over	32,5	573	19,7	63	p<.05	7,3	8,3	ns	28,7	16,4	p<.05
unemployed	26,0	2.173	22,5	239	ns	16,4	17,9	ns	19,9	16,5	ns
hh with children 14-26	28,4	3.691	21,8	412	p<.01	26,2	21,1	ns	18,1	15,3	ns
non western background	16,9	1.901	15,3	407	ns	22,6	21,2	ns	12	11,6	ns
all others	33,8	4.158	28,2	473	p<.05	14,4	14	ns	26,8	22,3	p<.05
overall	30	11.665	24,2	1.185	p<.0	18,1	16,4	ns	22,4	18,4	p<.01

Because of these results, no further analyses on other background variables were performed.

As this experiment was based on literature that stressed the importance of the number of contacts (e.g., Dillman et.al., 2014) and on success stories from other institutes (Nicolaas and Smith, 2015) we wondered what triggered these results. We decided to call back about 100 non-respondents with questions on the approach. It appeared that especially the advance notification had a detrimental effect. On it was mentioned the address of the general CBS website. People had tried to access that website in the expectation to be able to fill in the questionnaire. That was of course not possible, and led to frustration. The fact that this was a pre-notification of things to come had escaped many respondents.

1.1.8. Moment of delivery

From the very first when CBS started doing web surveys, advance letters were always targeted to arrive on Friday. The idea being that people would have time during the weekend to fill in the questionnaire. An experiment was performed where another delivery date was chosen, i.e., Wednesday. This experiment was proposed for three reasons: The delivery date of Friday was based on intuition, but was not tested. In literature the Friday also figures prominently as delivery date, but again, not much research into the issue. And finally, sending all advance letters on the same day puts a large burden on the postal services. If it would be feasible to spread sending the letters, that would lessen the burden.

Results

Table 13 shows that over all groups there was no difference in the percentage of people starting the questionnaire. There was one group that even showed a marginally higher login rate (the elusive ‘all others’). Surprisingly, the group of elderly showed a marginally lower login rate, and even a significantly lower response rate. Why this would be the case, especially in this group that would intuitively be least of all dependent on weekends to fill in questionnaires, remains a mystery.

Table 13. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr		p	contr		p
	%	n	%	n		%	%		%	%	
65 and over	29,3	583	20,3	64	p=.10	11,7	23,1	ns	25,2	14,1	p<.05
unemployed	26,0	2.367	28,5	263	ns	19,7	16,0	ns	19,6	20,9	ns
hh with children 14-26	27,4	3.916	28,5	435	ns	23,1	22,6	ns	18,5	19,3	ns
non western background	16,9	1.990	14,0	221	ns	26,5	29,0	ns	11,5	9	ns
all others	34,6	4.069	38,7	452	p<.10	15,3	16,6	ns	27,5	30,1	ns
overall	29,7	10.890	31,4	1.210	ns	18,3	18,4	ns	22,5	23,2	ns

There were no interactions with other background variables.

1.1.9. Informed consent

As described in paragraph 1.1, each advance letter (and also the reminders) contains a clause in which is explained that the respondent's data are linked to registry information, and which registries those are. What is explained in this clause is determined in collaboration with the CBS legal department.

The clause reads:

Statistics Netherlands not only collects data itself but also receives many files from other institutions. For example, the data from the population administrations, the centers for work and income (UWV WERKbedrijven), the social services, the payroll administrations of many companies. We automatically combine the information you give in this study with information we receive from other institutions. With this combined information Statistics Netherlands compiles statistics on Dutch society and we work as economically as possible².

From qualitative research we know that this clause is not fully understood by many people. In addition, they wonder whether CBS will *inform* the institutions mentioned above of the answers respondents give, instead of *receiving* information. Some of the institutions are responsible for giving out unemployment benefits, for example. People who fear that those institutions may learn of unofficial side activities through their response may be reluctant to participate in the survey. In close collaboration with the legal department, we have made a new version, where the specific examples of the collaborating institutions are removed. The text now reads:

Statistics Netherlands not only collects data itself but also receives many files from other institutions. With this combined information Statistics Netherlands compiles statistics on Dutch society and we work as efficiently as possible.

Results

Table 14 shows that there were (marginally) significant differences between experimental and control conditions for some groups, both in percentage of starting, break-off and response. In all cases these effects indicated that the adapted informed consent phrase led to more starting, less break-off, and higher response.

Table 14. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr	exp	p	contr	exp	p
	%	n	%	n							
65 and over	32,1	574	31,7	63	ns	8,7	0	ns	27,9	30,2	ns
unemployed	25,3	2.322	22,5	258	ns	16,0	20,7	ns	19,4	16,7	ns
hh with children 14-26	28,0	3.862	32,9	428	p<.05	24,7	234,0	ns	18,5	22,9	p<.05
non western background	17,4	1.994	16,7	221	ns	24,6	24,3	ns	11,5	11,3	ns
all others	34,4	2.138	36	240	ns	16	10,3	p<.10	27,1	31,2	p<.10
overall	30	10.890	31,2	1.210	ns	18,2	15,4	ns	22,5	24,8	p<.10

Additional analyses showed a marginally significant interaction with Age for both starting and response (p<.10), indicating that the groups of 45 to 65 year old people and the group with missing background information were susceptible to the adaptation. Table 15 shows results:

Table 15. Percentage of households starting by Age by experimental condition.

² Meant is: by not having to ask you this, we work as efficiently as possible, but this nuance is lost in translation.

Age	started				
	control		experiment		p
	%	n	%	n	
<30	24	981	17,6	119	ns
30-44	26,0	2808	23,8	328	ns
45-65	34,9	4902	38,9	530	<.10
65 and over	33,4	875	32,3	99	ns
missing	22,6	1324	29,9	134	<.10

This adaptation is implemented in all CBS surveys. However, for surveys with third party partners we cannot use this short version. The partner needs to be mentioned in the informed consent clause, in order to be able to receive micro data. If this is the case, we mention the kind of registries we link with in general terms.

1.1. Experiments with envelopes

Results of research into the reasons that respondents did not participate in web, but did participate in other modes (Luiten, 2015) showed that a large number of them had simply not seen the (three!) letters. In three experiments we tried to address this issue. In the first experiment we simply used a larger envelope. In the second experiment we also used envelopes that stood out more. The third experiment was a replication of the second one in another survey with a person sample. Sections 1.2.1 and 1.2.2 below describe the experiments and the results.

1.2.1. Sending the second reminder in an A4 envelope (instead of the standard A5)

In this experiment, the second (and last) reminder was sent in an A4 envelope, instead of the standard A5. The idea was to stress for respondents who would presumably recognize the CBS envelope, the importance of the occasion, and to induce them to actually open the envelope. For the results, the extra response after the second reminder was compared with the control group. This experiment was embedded in the experiment with additional persuasion arguments, hence the difference between the control and experimental groups after the advance letter.

Results

Although the larger envelope did lead to somewhat higher increase in response, compared to the control group, this difference was not significant. The initial difference of about 2 percentage points between the control and experimental groups after the advance letter still exists at the end of the fieldwork period.

Table 16. Response by letter (advance letter, 1st and 2nd reminder).

	control	experiment	increase control	increase experiment	p
after advance letter	10,2	8,1			
after 1st reminder, immediate	14,0	12,4	3,8	4,3	
after 1st reminder, total	16,1	13,6	5,9	5,5	
after 2nd reminder, immediate	19,4	17,2	3,3	3,6	ns
after 2nd reminder, total	21,8	19,9	5,7	6,3	ns

1.2.2. Using envelopes that stand out in the mail

As the size of the envelope did not seem to matter much, we tried another way to make the envelope stand out in the mail. An A5 envelope was developed in the CBS house style that is also used in publications and also in the flyer, for example. It shows a colored banner and a statistic. The advance letter and the two reminder letters each had a different color and a different statistic. The envelope of the advance letter was 'CBS-blue', and the statistic was: 'today our country has 16.9 mjn

inhabitants’. The first reminder envelope was green, and the statistic was ‘4325 people move on an average day’. The second reminder was red, and the statistic was ‘the population grows with 136 people on an average day’.



Results

In one group, the new envelopes led to a marginally higher percentage of starters, and a significantly higher response. Additional analyses also showed that for (single) men and households in the highest income groups the new envelopes led to a lower number of starters. Additionally, the new envelopes led to higher percentages of break off in highly urban regions. Tables 17 to 21 show these results.

Table 17. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		<i>p</i>	contr		<i>p</i>	contr		<i>p</i>
	%	<i>n</i>	%	<i>n</i>		%	%		%	%	
65 and over	26,6	575	19	63	ns	3,9	8,3	ns	25,4	17,5	ns
unemployed	26,2	2.104	29,5	234	ns	19,6	23,2	ns	19,3	20,5	ns
hh with children 14-26	28,2	3.705	32	412	<i>p</i> =.10	28,1	27,3	ns	18,1	22,1	<i>p</i> <.05
non western background	16,2	1.856	17,4	207	ns	25,3	36,1	ns	11	10,1	ns
all others	32,8	4.167	32	463	ns	13,7	17,6	ns	26,1	24,6	ns
overall	29	11.665	29	1.185	ns	18,6	21,7	ns	21,8	21,4	ns

Table 18. Percentage of households starting by Gender and experimental condition.

Gender	Started				
	control		experiment		p
	%	n	%	n	
male(s)	24,1	1694	17,8	191	p=.05
mixed	34,8	5715	36,3	584	ns
female(s)	22,8	2205	21,9	270	ns
missing	20,6	1051	29,3	140	p<.05

Table 19. Percentage of households starting by Income and experimental condition.

Income	Started				
	control		experiment		p
	%	n	%	n	
Lowest quartile	20,7	2218	24,4	234	ns
2nd quartile	26,6	2320	26,4	265	ns
3rd quartile	33,4	2478	32,9	289	ns
highest quartile	38,3	2592	32,3	257	p<.10
missing	20,5	1052	29,3	140	p<.05

Table 20. Percentage of households starting by Household composition and experimental condition.

household composition	Started				
	control		experiment		p
	%	n	%	n	
single	25,3	2511	20,2	317	p<.05
partners	33,8	6131	35	622	ns
single parnet	19,7	930	22,6	102	ns
missing	20,6	1051	29,3	140	p<.05

Table 21. Percentage of households breaking off Urbanicity and experimental condition.

Urbanicity	Break-off				
	control		experiment		p
	%	n	%	n	
Highly urban	21,2	608	33,8	74	p<.05
urban	18,2	731	14,4	90	ns
middle	17,2	564	14	57	ns
rural	19,1	632	22,3	53	ns
Highly rural	19,2	577	24,2	66	ns

1.2.3. Replication in a person survey: the ICT survey

The envelopes in the experiment in the ICT were slightly changed compared to the envelopes in paragraph 1.2.2. No statistical facts are depicted, as all statistical facts made the envelopes look like commercial mailings. In stead, the text in the banner read: *'your answer counts!'*. The reminder envelopes had again different colours, and different texts. The text in the banner of the first reminder was *'Won't you forget us? Your answer counts'*. The third reminder had the tekst *'Your answer counts. You can still join!'*. In addition, the urgency was depicted by the colour red of the banner and a running figurine. See below for examples of the first and third envelop. All envelopes had a A5 format.



Results

These envelopes resulted in a slightly increased response overall and in most subgroups. The difference failed to reach significance, however. In spite of these results, CBS decided to continue using these envelopes standardly, for persons, household samples and business surveys. The new envelopes fit in with the corporate identity in other communication products, like website and reports. In slight alteration compared to the experiment, the color of the envelope is 'CBS blue'. The text on the banner is again '*your answer counts*'. See below for an example of the new envelopes, that are used for advance letters and reminder letters alike.



1.3. Experiments with flyers

Three experiments were performed: sending no flyer at all, using another front, and using another contents. Paragraphs 1 to 3 describe the experimental conditions and the results. Some of the experiments were replicated in a person sample. These are described in paragraph 1.4.1.

1.3.1. Flyer versus no flyer

Standardly, an advance letter is sent with a flyer included in the envelop. In the experimental condition, the flyer was left out. We had three reasons for proposing this experiment: If leaving out the flyer is feasible, that would save a lot of trees and money. The survey methodology literature is not clear on the effect of adding a flyer. In addition, in an experiment with another survey we found

³ NB the picture on this reminder envelope had the same size as the other envelopes.

The standard flyer is a folded A4 sheet of paper. The text on the front page reads ‘why does CBS ask you?’ A photo is shown of people cycling and walking across a bridge, see below.

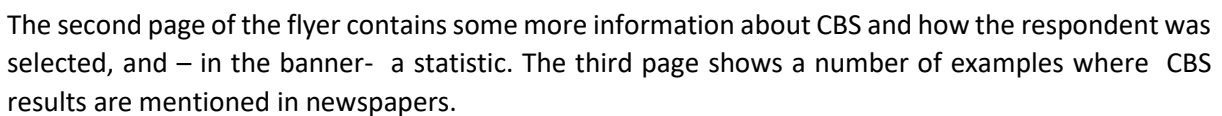


Table 22 shows that not sending a flyer in the LFS led to a marginally significant lower login percentage, although this did not translate to a (marginally) significantly lower response percentage. Additional analyses showed that there were additional groups who reacted negatively to the absence of the flyer, related to the ethnic composition of the neighborhood.

Table 22. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		<i>p</i>	contr	exp	<i>p</i>	contr	exp	<i>p</i>
	%	n	%	n		%	%		%	%	
65 and over	28,3	587	30,8	65	ns	7,8	10	ns	25,4	27,7	ns
unemployed	26,1	2.590	25,7	288	ns	17,4	10,8	ns	19,7	20,5	ns
hh with children 14-26	27,5	3.818	23,8	425	<i>p</i> =.10	25,3	22,8	ns	18,3	16,2	ns
non western background	14,5	1.985	16,4	220	ns	23,0	27,8	ns	10,0	10,5	ns
all others	34,1	3.971	31,1	441	ns	15,4	15,3	ns	26,8	24,3	ns
overall	29,2	10.890	26,5	1.210	<i>p</i> <.10	18,2	16,5	ns	21,9	20,2	ns

Table 23. Percentage of households starting, by ethnic composition of the neighborhood by experimental condition.

	started				
	control		experiment		<i>p</i>
	%	n	%	n	
% non-western background					
0%	34,4	3382	34,8	368	ns
1-5%	36,6	1747	25,4	215	<i>p</i> <.01
6-15%	31	2111	28,4	219	ns
16-35%	25,4	1556	18,6	179	<i>p</i> <.10
>35%	15,7	1204	19,8	140	ns
missing	19,5	665	18,7	64	ns

1.3.2. Different picture on the flyer

The research group felt that the picture on the flyer was not really attractive, with the two grumpy men. And in addition, we felt that the picture did not do justice to the image that Statistics Netherlands wants to convey. We wondered if a more attractive picture, and one that had more to do with our work, would have a positive effect on response rates. Hence this experiment, where the picture on the flyer was replaced with the picture below. A secondary goal was to create a more ethnically neutral flyer.

Regretfully, also the title of the flyer was replaced with the name of the survey (instead of ‘why does CBS ask you). The effect of the two measure cannot be separated.



Results

The new picture lead to a significantly higher response in the age group of 65 and older, but did not have any effect on the other groups. The hoped for positive effect on the group with a non-western background was not observed. See table 24 for results.

Table 24. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started			break off			response		
	control		experiment				contr	exp	
	%	n	%	n	p	%	%	%	p
65 and over	27,6	590	35,4	65	ns	11,7	0	24,1	35,4
unemployed	24,8	2.536	25,9	282	ns	19,3	13,7	18,5	19,9
hh with children 14-26	27,5	3.838	25,3	427	ns	24,5	24,1	18,5	16,9
non western background	15,9	1.955	15,7	218	ns	26,1	33,3	10,5	8,3
all others	32,1	4.013	35	446	ns	17	17,3	25,3	27,6
overall	28,3	10.890	29	1.210	ns	19,5	17,7	21,2	22,1

No further interactions with other background variables were observed.

1.3.3. Different contents

In this experiment, the flyer with the new photo of paragraph 1.3.2 and the standard contents was compared with an experimental flyer with a different content. This flyer was survey dedicated, with content relevant for the LFS. The rationale for this experiment was that more and more survey dedicated flyers were being developed. This costs a lot of time and effort, and we wondered if that was worth our while. Again however, not only the contents were changed, but also the form. In this experimental flyer, the form was more figurative and less linguistic, see below.

Waar u het voor doet!

Eén van de belangrijkste onderzoeken van CBS op het gebied van de arbeidsmarkt is de enquête beroepsbevolking. We onderzoeken bijvoorbeeld wat voor werk mensen doen, hoeveel mensen nu niet werken, of ze dat misschien wel zouden willen en of ze een opleiding volgen.

De informatie is onmisbaar voor de Nederlandse overheid en de Europese Unie om beleid te maken. Hiermee kan ingespeeld worden op diverse ontwikkelingen op de arbeidsmarkt.

Met de informatie uit het onderzoek wordt onder andere:

- de arbeidsparticipatie van de Nederlandse bevolking bepaald;
- het nationale werkloosheidscijfer berekend;
- de aansluiting van het onderzoek op de arbeidsmarkt vastgesteld;
- gekeken naar de afstemming tussen werk en zorgtaken.

Aan de rechterkant vindt u enkele resultaten uit eerder onderzoek. Zo krijgt u alvast een idee waar uw informatie aan bijdraagt.

Hartelijk dank voor uw inzet!



49% van alle werkenden werkt in deeltijd



Results

The new contents and form of the flyer did not have any effect at all, see table 25.

Table 25. Percentage of households starting, breaking off and responding in the LFS web questionnaire.

	started					break off			response		
	control		experiment		p	contr	exp	p	contr	exp	p
	%		%			%	%		%	%	
65 and over	28,5	589	27,7	65	ns	6,0	11	ns	26,5	23,1	ns
unemployed	22,1	2.649	21,1	294	ns	18,0	16,0	ns	17,0	17,0	ns
hh with children 14-26	25,3	3.809	24,3	423	ns	25,0	25,0	ns	16,9	16,5	ns
non western background	14,9	1.972	14,2	218	ns	26,0	23,0	ns	9,8	9,6	ns
all others	31,9	4.003	34,4	445	ns	15	16	ns	24,9	27	ns
overall	27,1	10.890	27,3	1.210	ns	18	19	ns	20,4	20,7	ns

1.3.4. Replication in a person survey: the Safety Monitor

The three experiments described above were replicated in an experiment in the Safety Monitor, a survey among persons. For the experiment, only persons of 16 years and older participated. The standard approach in the Safety monitor is to send an advance letter without flyer⁴. For the experiment, three conditions were compared to this control group: sending the standard CBS flyer ('Why does CBS ask you?'), sending a graphic folder (analogous to the one in paragraph 1.3.3, but with a contents dedicated to the Safety Monitor), and again a flyer with a again different picture from the standard and the experimental picture from paragraph 1.3.2. This time, the picture was chosen by a panel of field interviewers. They were asked to choose the picture that they thought would most appeal to respondents. The choices are depicted below. The first and second picture appealed equally to several interviewers. The third and fourth far less. The research group subsequently chose the first picture for the experiment.

The control condition consisted of over 15.000 sample persons. Each of the experimental conditions of 2.000 persons.



⁴ The reason for this is that part of the fieldwork is performed by a third party.

Results

Both the standard flyer and the flyer with a different picture had a significantly higher response than the condition with no flyer, see table 26. The difference between no flyer and a graphic flyer was not significant.

Table 25. Response by experimental condition

	n	%
No flyer	15.446	38,8
Standard flyer	2.000	44,0
Flyer with picture	2.000	41,1
Graphic flyer	2.000	42,7

Subsequent subgroup analyses showed that there were subtle differences between age, income and migration groups into which flyer differed significantly with the control condition, but the overall results are more informative here.

1.3.5. Discussion flyer experiments

The results of these experiments show that, just as with interventions to the letter, interventions in the brochure do not lead to spectacular results, but can significantly stimulate or deter specific groups. Some conclusions:

1. Whether or not enclosing a flyer leads to increased response rates depends on the survey, the contents, and on the picture on front of the flyer. In addition, not all subgroups in the population are equally susceptible to the flyer. Careful testing should determine when and to whom to send a flyer. For example, as a result of testing, a flyer is only sent to people with non-western migration backgrounds in the in the CBS Transportation Survey. Not sending flyers is obviously the more sustainable solution, and would in addition save money.
2. These experiments made us aware of the potential effect of the image used for the flyer. Field tests are not always feasible for new flyers, but at the very least we will do a short pretest of both image and contents with our panel of field interviewers.
3. Whether 'dedicated' folders in lead to a higher response than a general one could not be concluded unequivocally with these experiment. Replication in other studies is recommended.

1.4. Experiment with QR code

We expect that facilitating smartphone response would be beneficial to especially young persons' web response rates. To that end, CBS routinely includes QR codes in advance letters for surveys shorter than 10 minutes. Whether including a QR code actually draws in young people to do the survey on smartphone and what the general effect is on response rates, was not tested formally however. Previous research suggested that including a QR code could actually be detrimental for response (Smith 2017). In this experiment we tested what the effect of including a QR code in the letter is on response rates and the influence on the distribution of Age.

1.4.1. Method

The experiment was performed in the survey of consumer sentiments (SCS). This survey is eight minutes long. It has a web – CATI sequential mixed mode design. An advance letter is sent with a login code and password for the web questionnaire. The SCS is incentivized with a lottery incentive of iPads.

Two reminder letters are sent. Non-respondents of the web phase are subsequently approached for a CATI interview, provided that a telephone number can be found. The SCS web questionnaire is suitable for smartphone. The sample consists of persons of 16 year and older.

For the experiment, a parallel sample was drawn. For the experiment, only the web part of the SCS was used. The web results of the regular SCS served as control group. The experimental group consisted of 1284 sample persons, the control group of 3000 persons. See appendix 10 for an example of the letter with QR code.

Three measures are analyzed: the percentage of sample units starting the questionnaire, the percentage break off, and the response rate. In addition, the device that people used to login was studied. Although analytic power is too low to draw firm conclusions concerning the effect of age, I did look into that, and report preliminary findings.

1.4.2. Results

Table 26 shows the percentages of sample persons starting the questionnaire, the percentage of people who started filling in the questionnaire, but broke off prematurely, and the percentage response⁵ by QR condition.

Table 26. % sample persons starting the web questionnaire by Time and QR condition

	started	break-off	response
no QR code	43,1	5,6	41,1
QR code	43,7	3,8	41,4
<i>p</i>	ns	p<.10	ns

Including the QR code in the letter has no significant effect on either starting the questionnaire and subsequent response, but there is a marginally significant effect ($p=.10$) on the percentage of break-off. Break-off is higher when a QR code is offered.

I wondered whether the higher break-off in the QR condition had something to do with device use. It is known from previous research that break-off is higher when the questionnaire is filled in on smartphone than on other devices. In this experiment we see the same pattern, with 9.2% break-off when people use the smartphone, 4.5% for tablet users, and 3.8% for pc/laptop users ($\chi^2_{(4)} = 36.9$, $p < .001$). The presence of a QR code had influence on the choice of device, see table 27.

Table 27. % of people choosing smartphone, tablet of pc/laptop by QR code

	QR code	no QR code
smartphone	23,7	13,6
tablet	16,8	20,3
pc/laptop	59,1	65,6

When a QR code is offered, people do indeed choose to respond via smartphone more often than in the no-QR condition ($\chi^2_{(4)} = 25.8$, $p < .001$). These data suggest that the higher break-off in the QR condition may partially be an effect of pushing people to smartphone. The QR by device interaction on the percentage of break-off was however not significant, see table 28.

⁵ Response rate is percentage of responses from sample.

Table 28. % of people breaking off by device and QR code

	QR code	no QR code
smartphone	9,5	8,0
tablet	5,3	2,7
pc/laptop	4,1	3,3

We studied whether the inclusion of a QR code did increase response rates in the group of younger persons. The inclusion of a QR code did not lead to a higher number of young people logging in or responding. The only effect of the QR code was that elderly people (65 and over) broke off significantly more if they used a QR code to log on (5.5% with QR code, 0.7% without, $p < .01$).

Summarizing, providing a QR code in the letter did not influence login or response, but did lead to a higher level of break-off. This may be the result of the fact that including the QR code led people to use their smartphone more. And smartphone respondents tend to break-off to a much larger extent than respondents on other devices, a finding that was replicated in these data as well.

CBS invests in questionnaire development for smartphones. Ideally, however, these measures should stimulate respondents who would otherwise not have responded. Pushing respondents to smartphones instead of their tablets or pc/laptops could actually backfire.

1.5. Discussion communication experiments

The results of these experiments show that there are but few interventions that had a generally positive effect on all groups in society. The two exceptions were lower linguistic complexity, and adapting the informed consent paragraph. In all other interventions, positive effects for one group were counterbalanced by negative effects for other groups. These findings suggest possibilities for differential approaches where each group gets the letter that most appeals to them. For example, elderly persons would get a letter that appeals to their altruistic tendencies, in a standard envelope. Middle aged households would get a letter with a clear login instruction in an envelope that stands out in the mail. Some groups get a flyer, and other groups do not. This however implies a large increase in the amount of work it takes to design and keep letters updated. An automated system to get the right letter to the right respondent is almost a prerequisite.

Another finding of these experiments was that small differences may have large effects. One example is the phrasing of the informed consent paragraph, in small letters outside the main letter text. Especially in view of the finding, seen in many a qualitative interview, that many respondents do not even see or read the back of the letter, this finding is especially strong. Another striking finding is the effect of something perhaps as trivial as the photo that is chosen for a flyer. The take away message from all these experiments is that small differences in wording and look and feel can have large effects. If at all possible, new or alternative versions of letters and other materials should be carefully tested.

The extensive experiences of the research group led to the installation of an editor group, consisting of people of various expertise (data collection, methodology, communication, substantive experts), who are responsible for new letters and other communication materials. No longer is just one person responsible for writing these important letters.

All these experiments have had repercussions for the LFS letter. It is now considerably different from the LFS letter with which we started and has contained elements of many experiments: a new introduction, more appeals to altruism, headings for relevant subsections (on how to participate, the safety of data and where to go with questions), another name for the survey (not 'labour force survey', but 'work'), the offer of an incentive, and it is substantially shorter. See appendix 11 for the present LFS letter and its translation.

2. Incentives in Official Statistics: Effects on response, target variables and representativeness.

With the standard design of most CBS person and household surveys, where web is the first mode in a mix with CATI and/or CAPI follow up, it is obvious that a high web response is of considerable financial importance. The more web response, the fewer follow up interviews in expensive other modes. Hundreds of experimental studies have shown that incentives are an effective tool for increasing survey response rates. Other purposes are strived after as well: diminishing bias or improving response distribution, thanking respondents, enhancing interviewer confidence, and increasing data quality. Several meta-analyses have shown that incentives work in all survey modes: mail, web and interviewer modes.

Incentives are classified on two dimensions: the timing of giving them and whether they are monetary or non-monetary. Prepaid or unconditional incentives are offered to all sample persons or households, before the start of the survey. Those can be a small gift, a gift certificate or a small amount of money, enclosed in the advance letter. Postpaid, conditional or promised incentives are offered to those who respond to the survey. A distinction is further made between conditional incentives to all respondents, or lottery incentives where a limited amount of incentives is raffled among respondents. Monetary incentives include vouchers, cash, or loyalty points that can be exchanged for money. There is a wide range of non-monetary incentives used in the literature: bikes, tablets, trips, mouse pads. But also donations to charities, or study results.

Several other considerations are relevant as well: the value of the incentive (from \$1 to several hundreds of dollars or euro's in cash or in gifts), and the mode of the survey (mail, web, telephone or face-to-face).

In the following paragraphs, I shortly summarize findings on the effect of incentives in cross-sectional and panel studies: the effect of incentives on response rates, on sample composition and bias, and on data quality. Subsequently, some experiments with conditional and unconditional incentive performed by Statistics Netherlands are discussed.

2.1. Effects of incentives on response rates in cross-sectional surveys⁶

Table 29 shows a summary of findings of several meta-analyses on mail surveys. The table distinguished prepaid versus postpaid incentives, and cash versus non-monetary incentives.

Table 29. Meta-analyses of the effect of incentives on response rates in mail surveys

	cash		non-monetary	
	prepaid	postpaid	prepaid	postpaid
Fox et al 1988	+14.8%			
Hopkins et al 1992	+19.2%	+7.3%		
Church 1993	+19.1%	+4.5%	+7.9%	+1.2%
Edwards et al 2002	OR ¹ 2.02		OR 1.19	
Jobber et al 2004	+15.2%			

1 Odds ratio. An OR is the odds of the event occurring in one group (e.g., incentive group) divided by the odds of the event occurring in the other group (e.g., control group). If an experimental intervention (e.g., offering an incentive) has no effect, the OR is 1. If it reduces the chance of having the event, the OR is less than 1; if it increases the chance of having the event, the OR

⁶ This chapter is partly derived from Luiten, A., and Groffen, D. (2018). The effect of a lottery incentive on response, representativeness and data quality in the LFS. CBS discussion paper.

is bigger than 1. The smallest value an OR can take is zero. Thus, in Edwards et al., (2002) with an OR of 2.02, prepaid cash incentives increased the odds of a response by 102%.

These analyses show that monetary incentives are two to five times as effective as non-monetary incentives, monetary prepaid incentives are two to three times as effective as monetary postpaid incentives. The difference between prepaid and postpaid incentives is larger with cash incentives. Reviews of the literature by Singer, Van Hoewyk, Gebler, Raghunathan and McGonagle (1999) and Singer, Van Hoewyk, and Maher (2000) showed that the effects of incentives are larger in mail surveys than in interviewer surveys. A recent analysis by Pforr et al (2015) for ten German face-to-face study showed that these findings were replicated many years later, and in different cultural circumstances. Interestingly, the 15 to 19 percentage points increase in response rates for unconditional monetary incentives are the same rates that were found in research with unconditional incentives in web surveys by Gajic, Cameron and Hurley (2010) and several experiments by Statistics Netherlands, see below. Most meta-analyses found a linear relationship between the value of the incentive and the increase in response rates, although the values studied are generally quite low; the highest incentive studied was in Edwards et al (2002), with an incentive of \$15.

While the results for unconditional incentives are unequivocal in showing that even incentives as small as \$0,25 have a significant positive influence on response rates, results for conditional incentives are more diverse. Especially the literature on lottery incentives shows mixed findings. Nevertheless, this is the incentive that Statistics Netherlands presently uses in most surveys. All Statistics Netherlands' social surveys have a mixed mode design, starting with web. Non-respondents are approached for either a telephone or face-to-face interview. With this design, even a modest increase in web response rates will lead to substantial cost reduction, especially if face-to-face interviewing is part of the design. After a review of the literature, we concluded that this was the incentive that would be most efficient, in terms of the balance between costs and gains. However, the raffle design should meet three preconditions: the raffled prize should be large (Stevenson, Dykema, Cyffka, Klein and Goldrick-Rab, 2012; Laguilles, Williams, and Saunders, 2011; Sauermann and Roach, 2012; Gajic, Cameron and Hurley, 2010), the prize should be salient in the advance letter (Zang, Lonn, and Teasley, 2016), and respondents should learn of their winnings right after filling in the questionnaire. The latter precondition was based on the findings of Tuten, Galešić and Bošnjak (2004), who found that immediate versus delayed notification (one month later) led to significantly higher response rates for the immediate condition. Statistics Netherlands has subsequently experimented with various designs, subjects and populations. We found that a raffle of iPad mini's among a general population sample of 10 to 20 year olds led to an increase in response rates of 12 percentage points. Subsequent experiments with other populations and the entire age range, where we raffled iPads, consistently led to increases of about 7 percentage points. An experiment with raffled vouchers worth €250 increased response to a lesser amount, i.e., 5 percentage points. Although theoretically the monetary incentive should have led to a higher increase than the iPad lottery, in this experiment the second precondition of salience was not adhered to. Instead of the picture of iPads that is prominent in the advance letter, the raffle of vouchers was only mentioned in the text. See appendix 8 for an example of an advance letter with a salient role for the incentive. Perhaps a fourth precondition is that we are not explicit about the chance of winning the incentive, we merely mention that a number of iPads is raffled. We raffle one iPad per 2000 sample units, with a minimum of two, so that we can always mention plurals. For larger surveys, we mention the number of iPads that will probably be won (e.g., you stand a chance of winning one of the 25 iPads that we make available for those who participate). Qualitative research made clear that being explicit about the chance of winning would be detrimental for response. On the other hand, mentioning a relatively large number of incentives makes people believe that they have

a higher chance. Respondents who are curious about the chances of receiving the incentive, can find the information on Statistics Netherlands' website however.

What does the literature say about lottery incentives? Singer and Kulka (2002) give an overview of the results of nine lottery experiments in mail surveys. Four of them had positive effect, but five more did not have an effect. However, the incentives in these experiments had very little value. Singer and Ye (2013) updated the review by including the new experiments that tested lotteries in web surveys. Among the seven studies identified, five included a control condition of no incentive and only two of five studies showed positive effects of lotteries. Thus, the early literature suggested that lottery incentives can be effective, but not consistently.

Bošnjak and Tuten (2003) compared \$2 promised with \$2 prepaid at first contact with a lottery of 2 x \$50 and 4 x 25 and found that the lotteries were more successful than the other conditions. Heerwegh (2006) a lottery of €25 vouchers increased response in a student sample, but only for women. Göritz (2006a) performed a meta-analysis, based to a large extend on web surveys in access panels that offer small prizes. She concluded that lotteries are usually mildly effective. In an updated meta-analysis in 2015 she came to the same conclusion.

Gajic, Cameron and Hurley (2010) compared a no incentive condition with \$2 included in the advance letter, a low lottery cash draw of 10 x \$25 and a high lottery cash draw of 2 x \$250 in a web survey among the general public. They found that the prepaid incentive led to the highest response rate and the lowest dropout rate (+14.4 percentage points compared to no incentive, + 13.4 percentage points compared to low lottery + 8.3 percentage points compared to the high lottery). The highest dropout rate was found in the low lottery condition. Gajic et al, calculated the costs per complete record in the four conditions, and compared those to the cost effectiveness of each, that is how much extra an incentive costs per additional completed. By this criterion it could be determined which incentive should be used to obtain the most completed surveys for a given design. In this case that was not the incentive with the highest response rate, but the high lottery incentive. Gajic et al, conclude that prepaid incentives should be the incentive of choice when a high response rate is desired and costs are not a tight constraint. On the other hand, the high lottery is better suited to situations in which as many responses as possible should be obtained given a fixed budget.

Sauermann and Roach (2013) likewise experimented with the probability of winning and size of prize in a web survey among graduate students and postdoctoral researchers. A no incentive condition was compared to five pay conditions that each had a total payoff of \$500, but differed in the chance of winning and in the size of the prize (100x\$5, 50x\$10, 20x\$25, 10x\$50 and 5x\$100). Subjects were not told about the size of the subject pool nor the expected number of respondents and so had no objective idea about the chance of winning. The response rate was highest for the condition with the largest prize and the lowest chance of winning: the response went from 25% for the no incentive condition to 31% in the condition with the highest incentive (odds ratio of 1.32). The no incentive and the 100x\$5 conditions had the lowest response rates, but the 10 x \$50 lottery did not significantly differ from those conditions. Sauermann and Roach conclude that a fixed budget for lottery prizes is more effective if used for a small number of large prizes than for a large number of small prizes. Göritz and Luthe (2013a) experimented in three lotteries with cash prizes that were either paid in one lump sum or split into multiple smaller prizes. Response was higher with a lottery than with the control group (OR = 1.18), when raffling the pay out in a lump sum (OR = 1.30) and with higher single prize sizes (OR = 1.02 per €10).

LaRose and Chai (2014) found that a lottery incentive of a \$50 gift voucher increased response over a non-incentive condition, although the lottery was less successful than an unconditional incentive of

\$0,25. Lemcke, Schmich and Abrecht (2018) found that a lottery of €50 vouchers led to an increase of almost 3 percentage points in a survey among the general population.

From the literature review we concluded that lottery prizes should be large, if they were to be successful. But how large is 'large'? Stevenson et al, (2012) used an iPad as well as the largest incentive. Laguilles, Williams and Saunders (2011) compared the effect of two different iPods versus \$50 gift cards. Sauermann et al., (2012) used \$100 as largest prize. Gajic et al.(2012) raffled \$250. However, examples are to be found in the literature of comparable prizes that do not have the desired effect: Porter and Withcomb (2003) found no effect of \$200 in an experiment among students. Halpern et al (2011) showed that some populations are not susceptible to these kind of lotteries at all: neither a lottery of \$250 nor of \$5000 worked in a sample of medical practitioners. Likewise, a lottery of €2000 did not work in a sample of nurses, but an unconditional incentive of \$5 did have an effect.

A special kind of unconditional non-monetary incentive is offering study results. Scherpenzeel and Toepoel (2014) offered study results to members of the Dutch probabilistic LISS panel. Various feedback scenario's all had no effect. Göritz and Luthe (2013b) likewise saw no effect of this kind of feedback in commercial and non-profit access panels. Göritz and Luthe (2014) found a negative effect on response and item response in a commercial panel. Tuten, Galešić and Bošnjak (2004) found that this kind of incentive works well if the topic salience is high for the respondent or if study results are tailored to each participant. After reviewing the scant research on this topic, Göritz (2015) concluded in her review that it is best to avoid offering study results, except for tailored or highly salient topics.

More research has been done on the practice of donations to charities: survey organisations offer respondents the chance to donate an amount of money to a charity of their choice. Boyle, Heyworth, Landrigan, Mina, and Fritschi (2012) performed a meta-analysis on this kind of incentive. The analysis encompassed 12 studies, from 1978 to 2010. Some of those showed large positive effects, others negative effects while most showed small effects. The meta-analysis showed that offers of donations, compared with a no-incentive control group, may increase the likelihood of response by 4%. This difference was not significant however. Additional analyses of the topic (social or health versus commercial) and study location (Australasia, North America, or Europe) did not reveal differences in these findings.

2.2. Effect on sample composition and bias

Several experiments have shown that incentives in mail and interviewer modes do not have much influence on sample composition (Brick et al 2005; Cantor et al 2008; Furse and Stewart, 1982; Goetz et al, 1984; James and Bolstein, 1990, Shettle and Mooney 1999, Warriner et al 1996, Willimack et al 1995, Singer et al 1999 in five studies). Other experiments showed that incentives may improve representation of traditionally underrepresented groups: young people (Dillman, 1996, Miller, 1996, Storms and Loosveldt, 2004, Gajic, Cameron and Hurley, 2010); minorities (Berlin et al 1992; Mack, Huggins, Keathly and Sunsukchi, 1998) and those with lower incomes (Mack et al 1998) or less education (Berling et al, 1992, Nederhof, 1983, Singer, van Hoewyk, and Maher 2000, Petrolia and Bhattacharjee, 2009). Couper et al (2006) mention that people with low education, singles, and unemployed are more susceptible to cash incentive than to gifts in kind.

2.3. Effect on data quality

A number of studies have looked into the issue of data quality as a result of giving or promising an incentive. Theoretically, the incentive could reduce measurement errors if they create a sense of obligation to the researcher, that causes respondents to put in more effort. On the other hand, there is a risk of increased measurement error if the incentive convinces respondents who are only

motivated by the incentive to participate. Indicators of data quality are skipping items, 'don't know' answers or refusals, clumping of numerical estimates around common multiples such as 5 or 10, straight-lining sets of items (giving the same answer; e.g., the middle answer in a grid of questions), speeding through the survey, early break off, and divergent scale scores and factor structures.

Singer, Van Hoewyk, Gebler, Raghunathan, & McGonagle (1999) concluded in a meta-analysis of incentives in telephone and face-to-face surveys that incentives do not appear to affect the quality of responses, measured by item nonresponse or the number of words in response to open ended questions. Another study by Singer, Van Hoewyk, and Maher (2000) however, showed that both promised and prepaid incentives reduced item nonresponse.

Tzamourani and Lynn (1999) summarize previous research and state that incentives seem to increase the quality of answers to open-ended questions and often decrease non-response of close-ended questions. In an experiment that compared no incentive with a £3 and £5 incentive, they found that the respondents in the £5 condition spent more time filling in the questionnaire, but other measures of data quality showed either very small or inconsistent differences.

Ryu, Couper and Marans (2006) found no statistically significant differences in response distributions between respondents who received an unconditional \$5 cash versus an in-kind incentive (a park pass), but did not compare with a control group without incentive.

Medway (2012) undertook three experiments to study the issue. She studied 12 indicators of respondent effort. In the first experiment, an unconditional incentive of \$5 in a telephone survey led to a reduction of item nonresponse and interview length. There was no effect on other indicators of effort, like response order effects and responses to open answers. In a second experiment in a mail survey on sensitive issues, again with a prepaid incentive of \$5, the incentive led to an increase in the number of undesirable attitudes and behaviors. No effect on less sensitive questions was found. There was a pattern of reduced bias for three items that could be checked with administrative data, but these were not significant. The third experiment also used prepaid incentives and looked into measurement invariance of scale questions. No effect of the incentive was found. Medway concluded that prepaid incentives had minimal impact on measurement error.

Cole and Wang (2015) studied data from surveys among a higher education population, in the National Survey of Student Engagement, who received a survey through their schools. Half of these schools offered incentives, mostly in the form of lotteries. The results show that both first year and senior students had significantly less missing data, showed less straight-lining, took more time filling in the questionnaire, and showed better scale quality when an incentive was offered. Cole and Wang warn that, although the results were significant, differences were actually very small.

Lemcke, Schmich, and Albrecht (2018) experimented with conditional incentives and a lottery in a mixed mode (web and mail) survey among the general population. The incentives were a lottery of 100 x €50, stamps, a voucher of €10. The €10 voucher diminished the number of missing values. The other incentives did not have any effect on either missing values, straight-lining, rounding, social desirable answers and measurement invariance in scale questions.

The overall impression from this short summary is that incentives, be they unconditional, conditional or lottery incentives, either have no effect on data quality, or a small positive effect: respondents sometimes, but not always, take more time, have less missing data, report more fully in open questions, and show less straight-lining. The goal of attaining better data quality will not be the primary reason to include incentives in a study, but the fact that they either have no influence on data quality or may even increase data quality is reassuring for survey managers who want to use incentives to increase response rates.

2.4. CBS incentive experiments: cross sectional surveys.

In this chapter, a short summary is given of a number of CBS experiments in cross sectional surveys with various kinds of incentives: unconditional incentives of €5, included in the advance letter, conditional incentives of €10, €20 and €30, and lottery incentives of iPads, iPad minis and €250 vouchers. Paragraphs 2.4.1 to 2.4.3 describe response results of unconditional, raffled and conditional incentives. Paragraph 2.4.2. describes the influence of the various incentives on sample composition. Paragraph 2.4.3 describes the effect on data quality, 2.4.4 on target variables and 2.4.5 on fieldwork costs. Chapter 2.5 describes an experiment in the Labour Force Survey.

2.4.1. The effect on response rates.

The next three paragraphs describe the effects of the three different kind of incentives (unconditional incentives, raffled conditional incentives and conditional incentives for all participants) on the response rates of various CBS surveys.

2.1.4.1. Unconditional incentives: €5 gift certificates

The potential effect of incentives in mail surveys has long been established. The literature sites some experience with incentives for web surveys in selected populations, but the effect in the general population is less well researched. CBS has performed several experiments with both conditional and unconditional incentives. Literature suggests that monetary incentives work best. Because of Dutch postal legislation, we are not allowed to send real money however, so we opted for the next best solution: widely usable gift certificates. The worth of the gift certificate, which we included in the advance letter, was €5. In three experiments in the survey of Living Conditions this lead to an increase in web response of 12 percentage points for the survey of Living Conditions, of 17 percentage points for the survey of Travel Behaviour, and of 19 percentage points for the survey of Social Cohesion. These increases in response are in line with the increase found in meta analyses of the impact of unconditional monetary incentives in mail surveys (Fox et al., 1988, Hopkins et al., 1992; Church, 1993; Jobber et al., 2004).

The unconditional incentives not only increase the web response, but also the response in the subsequent CATI and CAPI round. Table 30 gives an example of findings in the survey of Travel Behaviour.

Table 30. Effect of an unconditional incentive on the web, CATI and CAPI response of the survey of Travel Behaviour.

	no incentive	incentive
CAWI %	20.7	37.4
CATI %	45.7	49.0
CAPI %	49.2	57.0
N	314	374
Response %	57.0	67.9

Because of the higher web response, and the higher general response that enables a smaller sample size, including an unconditional incentive in the advance letter can result in a decrease of fieldwork costs in a survey that includes a CAPI component.

2.1.4.2. Conditional incentives: raffles

The first experiment with raffled incentives was in a web only survey among children and young adults. Three iPad-minis mini's were raffled. The number was not announced, only that there were several

iPads. We announced in the advance letter that the winners would be notified at once after filling in the questionnaire. It is also mentioned in a footnote that people who do not want to win an iPad (e.g., for religious reasons) can indicate so in the questionnaire. We accomplished this by determining in the sample who will win if they respond. The 'win or not' characteristic triggers a part of the questionnaire that mentions the fact of winning (or not) to the respondent. The number of iPads that can potentially be won is a function of the expected response rate. That means that in some instances we give out more iPads than expected, but the opposite is also possible.

A picture of a number of iPads figured prominently in the advance letter, see appendix 9 for an example. In this first experiment, the raffle increased response with 12 percentage points from 21% in the control group to 33% in the experimental group. The effect was uniform across age groups, see tables 31 and across groups with different ethnic background, see table 32.

Table 31. Response rates by incentive by age group

Age	no incentive		incentive		p
	%	N	%	N	
10-11	21,4	1011	31,6	1013	***
12-13	26,8	669	40,0	662	***
14-15	25,6	540	39,0	543	***
16-17	17,9	553	34,6	549	***
18-22	19,3	1334	29,8	1342	***
Total	21,7	4107	33,8	4109	***

*** p < .001

Table 32. Response rates by incentive by ethnic background group

background	no incentive		incentive		p
	%	N	%	N	
Native Dutch	26,4	1423	38,8	1426	***
Marocco	12,3	585	23,1	584	***
Dutch Antilles	19,4	470	33,9	472	***
Surinam	19,2	496	33,3	496	***
Turkey	17,8	552	27,2	551	***
Other	27,4	581	38,4	580	***
Total	21,7	4107	33,8	4109	***

*** p < .001

Especially this latter finding is encouraging. In other experiments we have found that members of some ethnic groups in the Netherlands either do not react, or even react negatively to incentives. Further experiments need to establish if this finding is linked to the specific population of young persons, or to the form of the incentive.

The incentive proved, at least in this population, almost as effective as an unconditional incentive, but at a fraction of the costs. Instead of the more than €20.000,= that would have been spent on an unconditional incentive, we spent €1000,= on three iPad-minis.

This success made us curious if this could work in other samples as well. In an experiment in the Travel Survey, with a sample from the entire population we raffled 2 iPads. Because we want to be able to say that we raffle multiple iPads, the minimum number is always 2. In an experiment in the Survey of Employee Conditions, we raffled (several) iPads and (several) gift certificates worth €250⁷. The iPad

⁷ We use a chance mechanism of one winner (iPad or €250) per 2000 respondents, but the respondents are not aware of their chance of winning. The mechanism is published on the CBS website, but is not announced in the letter.

increased response by 5 percentage points in the Travel Survey and by 7 percentage points in the survey of Employee Conditions. The raffle of €250 gift certificates increased response by 5 percentage points.

2.1.4.3. Conditional incentives: gift certificates for respondents

In certain circumstances, where we ask relatively much from respondents, we prefer to give something to all respondents. Therefore we experimented in the mixed mode (web-cati) EU-SILC with promised €10 gift certificates for respondents. The web response increased with almost 11 percentage points, but contrary to the findings with the unconditional incentives, the response in the follow up CATI mode did not increase, and even had a tendency to be lower than the response in the control group. In an experiment in the (web only) Household Budget Survey, we compared promised gift certificates of €20 and €30, in half of the cases complemented with an unconditional incentive of €5. In line with findings in literature, the unconditional incentives had a larger impact than the conditional incentives. The larger conditional incentive proved to be significantly more effective than the €20 incentive. Without the unconditional incentive, people were not much inclined to participate in the HBS, neither with the €20 promised incentive, nor with the €30. The unconditional incentive on top of the promised incentive further increased the response. See table 33.

Table 33. Response rates by incentive

HBS 2013	postpaid €20,=	postpaid €30,=
unconditional €5,=	18.3	20.1
no incentive	11.3	14.0

2.4.2. CBS experiments: the influence of incentives on sample composition

In all experiments we find that unconditional incentives increase differences between subgroups, as some groups react more strongly to the incentive than other groups, and some groups may actually show decreasing response. Differential reaction to incentives may be an expedient result, if the incentive brings in respondents of difficult groups. Mostly we found that that is not the case, however. In earlier experiments in CAPI (Wetzels, Schmeets, van den Brakel, and Feskens, 2008), we found that an unconditional incentive of postal stamps could increase overall response with almost 8 percentage points, but the incentive did not have any effect on persons of non-western ethnic background. We saw the same phenomenon in the incentive experiments in both mixed mode and unimode web surveys. In the housing survey, persons of first generation non-western background had a (substantially) lower response rate with the unconditional incentive than those in the control group. In general it can be said that there is a high positive correlation between the response rates of subgroups in the control group, and the response gain in the incentive conditions, for both the unconditional and the conditional promised incentives: the higher the response without incentive, the stronger the reaction to the incentive. In a mixed mode setting with interviewer follow-up, we manage to counterbalance the effect, but in web only, or web-mail surveys, this may be an unwanted result.

The raffled incentives seem to suffer less from differential influence of the incentive. For example, in the Travel Survey, the increase in web response with the unconditional incentive was 21% for native Dutch persons, while the increase for people from non-western background was 6%. The increase as a result of the iPad raffle in the same survey was more homogeneous: 6% increase for native Dutch persons against 3% for persons with non-western background. Likewise, in the survey amongst children and young adults, there was no interaction between ethnic background and incentive condition.

Another way to look at the effect of the incentive, is to look at the variation in the adjustment weights for the variables in the weighting model. Change in weight variation is an indication that subgroups react differently to the incentives. For the web-mail mixed mode survey amongst Dutch employees we studied the effect of either an iPad or €250 gift certificate raffle on the adjustment weights variation. In this survey, both incentives succeeded in significantly decreasing the variation: from .32 in the no incentive group to .19 in the iPad group and .18 in the gift certificate group.

2.4.3. CBS experiments: Incentives and data quality

In all experiments we compared data quality of web responses with and without incentive. We looked into the rate of missing items, the time respondents took to fill in the questionnaire, and in some cases to the amount of straightlining. In none of the experiments did we find an effect of the incentive on these measures of data quality, not even in the experiments amongst children and young adults who would learn right after filling in the questionnaire if they had won an iPad. If anything, there were more indications that data quality was higher in the incentive conditions.

2.4.4. Incentives and target variables

In two studies, we studied whether using incentives had any influence on a number of point estimates. In the web-cati-capi Travel Survey, neither an unconditional incentive, nor a raffle of iPads had any influence on the target variables, compared to the control condition. This means that the choice of incentive in this survey can be made based on efficiency and costs only. In the web-mail survey of Employee Conditions, the incentive did have a significant influence on the weighted point estimates. Together with the increased representativeness of the incentive conditions, this suggests that the incentives succeeded in bringing in 'other' respondents in this study.

2.4.5. Incentives and fieldwork costs

Giving all sample persons a gift certificate of €5 is obviously very costly. However, in a mixed mode data collection where CAPI is one of the modes, the increase in web response is such that more than that amount is saved in fieldwork costs. In addition, the increase in response allows a smaller sample, resulting in substantially *lower* fieldwork costs.

The promised incentive, conditional upon response, is cheaper than the unconditional incentive. However, the gain in response were not high enough to overcome the costs of the incentive in the web-cati design of the experiment. Of course, considerations of quality may still induce one to use this kind of incentive. These analyses have not yet been performed.

Finally, the raffled incentive of either iPads or large gift certificates is very cost effective. Although the increase in response is not so high as with the unconditional incentive, the costs are a mere fraction. Statistics Netherlands has chosen this incentive as the 'default' incentive in new surveys.

2.5. CBS incentive experiments: longitudinal survey (LFS)⁸

One of the measures that was taken to increase web response, and thus cost efficiency, was the introduction of an incentive in the LFS. The previous experiments proved that incentives are successful in cross-sectional CBS person surveys, but the LFS is different: it is a household survey and a panel survey.

⁸ This work was supported by Eurostat Grant 07131.2017.003-2017.596 'Quality improvements for the Labour Force Survey'. See Luiten, A. and Groffen, D. (2018) for a complete report.

In January 2017 a lottery incentive of iPads was introduced in the first wave of the LFS. From January to June, only 10% of the sample was included in the offer of the incentive. When we were confident that the incentive had the desired effect of increasing the web response, without notably effecting the LFS estimates, the ratio of incentive offers was gradually increased, until the entire sample was offered the incentive in December 2017. Although we had a general idea about the effect of the incentive, we wanted to perform more in depth analyses of the effect of the incentive. Notably, we wanted to know what the response effects were for various subgroups, what the effect was on recruitment for subsequent waves, whether the incentive also had an effect on the subsequent wave response, and therefore on attrition, and the effect on data quality: the speed of filling in the questionnaire, and the number of missing items. And finally, we wanted more in depth analyses on the effect on substantive variables for various subgroups.

Before we venture into the discussion of these issues, first a description of the design of the Dutch LFS. The LFS uses a five wave rotating panel design in which households are interviewed in five consecutive quarters before rotating out of the sample. The survey is voluntary, that is, sample units are not obliged to participate. The yearly sample for the first wave is divided into twelve equally sized household samples. Sample addresses are drawn from the Municipal Personal Records Database which contains personal details of everyone who lives in the Netherlands. The target population consists of people residing in the Netherlands, aged 15+ and living in private households. Only when all household members (15+) complete the questionnaire, the household is considered to have responded. Proxy answering is allowed.

Like most Statistics Netherlands' surveys, the LFS uses a sequential mixed-mode strategy in which web interviewing (CAWI – computer assisted web interviewing) is followed by telephone (CATI – computer assisted telephone interviewing) or face-to-face (CAPI – computer assisted personal interviewing) interviewing. Whether a sample unit goes to CATI or CAPI depends on the availability of a telephone number and the size of the household: larger households are interviewed face-to-face. Non-respondents in CATI are not transferred to CAPI. CBS make use of a rolling reference week for the LFS. In that way we can take our time for each mode: the web approach takes place in month t , the telephone approach in month $t+1$, and the face-to-face approach in month $t+2$.

Not all non-responding households of the web phase are followed up in the subsequent mode, however. In order to make optimal use of the cheaper web mode, and also to be able to keep the number of sample units that go to CAPI and CATI stable each month, a relatively large sample is drawn for the first web phase, of which a subsample is drawn for the follow-up phases. The subsample is stratified by interviewer region and known telephone, but is otherwise random. About half of the non-respondents in web is not followed up in other modes.

The data collection strategy for the first wave comprises the following steps:

1. All sample units receive a letter containing the internet address of the web questionnaire and a personal login. In the letter a household residing at the selected address is requested to complete the questionnaire via the internet. All household members need to use the same login to gain access to the questionnaire: i.e. household members do not receive an individual login. The letter offering the incentive shows a prominent picture of a number of iPads. See the appendix for an example of the advance letter offering an incentive.
2. Two reminders are sent to non-respondents two weeks and three to four weeks after the advance letter.
3. One week after the second reminder the access to the web questionnaire is closed.
4. Follow-up in the other modes is mentioned in the advance letter, but no separate letters are sent before the CATI or CAPI follow-up.

5. At the end of the first wave questionnaire, respondents are asked if they are willing to participate in the second wave of the LFS. If so, their telephone number is asked.
6. The second wave is CATI for all sample units who gave their telephone numbers, regardless of the mode of the first wave. If no telephone number is available, respondents are excluded from the survey. The second wave takes place three months after the first. A new advance letter is sent for each new wave, but no more incentives are offered.

This process is repeated for the third, fourth and fifth wave.

2.5.1. Response

Table 34 shows the wave 1 response rates for the three modes and the total response after three waves. The data shown are from the three months where the proportion incentive - no incentive was more or less equal. The total response in this table is low, as it is the response from the initial sample, not compensating for the subsampling of CATI and CAPI addresses, nor adjusting for ineligibility in CATI and CAPI. Would we compensate for subsampling and ineligibility, total response rate amounts to some 50 to 55%. Weighting for inclusion probability, therefore compensating for the oversampling of groups in the population with low response propensities and the under sampling of people of 65 years and older would further increase the response rates. The calculation shown here was deemed to be the simplest one to compare the two conditions.

The incentive led to a significant increase in response of almost three percentage points in web, but leads to an equally large decrease of response in CATI. In CAPI there is also a decrease in response, but this does not reach significance. Because the number of CAWI sample is far larger than in the other two modes, the incentive still leads to a significantly higher overall response.

The increase in web response is smaller than in person surveys, where this incentive in this kind of population leads to an increase of five to seven percentage points on average, see chapter 2.4. It is probable that the household component is responsible for this finding.

Table 34. Wave 1 response in CAWI, CATI, CAPI and overall, by incentive.

	no incentive		incentive		<i>p</i>
	sample n	response %	sample n	response %	
CAWI	16.335	21.4	19.965	24.3	***
CATI	3.159	35.7	1.212	32.7	*
CAPI	2.932	39.0	3.527	37.7	ns
Total	16.335	35.3	19.965	37.0	***

* $p < .05$, ** $p < .01$, *** $p < .001$, ns not significant

The decrease in response in the other modes is remarkable, as it was not hitherto seen in other CBS experiments. The interpretation of this finding is that some people who would otherwise have answered in CATI or CAPI were drawn to CAWI by the incentive. Even if the incentive would not have had an effect on the overall response rate, this outcome would still have been desirable, to the extent that it pushes people to the cheapest mode.

Another important aspect of the success of a panel design is the number of households that are recruited for subsequent waves. Table 35 shows the percentage of households in the three modes that express a willingness to be contacted again, and provide their telephone number. The incentive leads to a substantial increase in the number of households who are recruited for wave 2.

Table 35. Panel recruitment by mode and incentive condition.

	no incentive		incentive		<i>p</i>
	n responses	recruited	n responses	recruited	
CAWI	3485	67,5	4841	74,3	***
CATI	1125	91,5	1212	90,6	ns
CAPI	1130	90,5	1304	91,1	ns
total	5740	76,7	7357	80,0	***

*** $p < .0001$ ** $p < .001$ * $p < .05$ ns not significant

Table 36 shows results for the wave 2 and wave 3 response. The mode in these waves is CATI. The table shows that there is no effect at all of the incentive in the later waves. The data for waves 4 and 5 were not yet available at the time of writing, but in view of the findings for the earlier waves, differences are not to be expected.

Table 36. Response rates by incentive in wave 2 and wave 3.

	no incentive		incentive		<i>p</i>
	sample n	response %	sample n	response %	
wave 2	3.944	76.6	3.877	76.4	ns
wave 3	3.198	83.1	3.166	83.4	ns

* $p < .05$, ** $p < .01$, *** $p < .001$, ns not significant

It is to be expected that an incentive that you did not win in the previous wave does not influence the decision to take part in wave 2 or 3. The lack of effect of the incentive means however, that the higher number of households that were recruited as a result of the incentive do not drop out in the second wave.

2.5.2. Representativeness

The calculation of subgroups response rates is less straightforward in the LFS compared to in person surveys. Response rates are calculated on household level, but the auxiliary variables are derived from the communal registries on a person level. To derive the household composition, the person information is aggregated. In table 37 the response rates by age, income, gender, ethnic background, household type and urbanicity are described. Because of the aggregated household information, 'age' is the mean age of the household core (either one or two persons). This introduces some extra variation in response propensities, because not all household cores are homogeneous as to age. For 'ethnicity' three categories were defined: native and mixed native – other background, non-western migration background and western background or mixed western – nonwestern background. The category 'gender' consists of single sex households and mixed sex households. Income and urbanicity are derived at the postcode 6 level, a fine grained mapping on street level or even finer.

Table 37 shows that both web response and overall response rates are higher, the older the household core: the highest web response rates are found in the 65+ households. On the other hand, the effect of the incentive is larger, the younger the household. Concerning income, we see that the higher the income, the higher the response, both in web as in the total response. The incentive increases web response especially in the middle income in this experiment. The increase in total response in the incentive condition is fairly uniform across the four income groups. The incentive has also differential effect on gender. The increase in web response is highest in the mixed sex households, while for women, the incentive hardly has any appeal. These results should be interpreted with some caution

however: most of the single sex households will be singles, and singles have lower response rates than people living with a partner.

In previous research we have found that incentives have far less impact on people from non-western backgrounds (Luiten, 2016, van Geffen, Luiten, and van der Pol 2016), and we find similar results for households. The incentive increases web response for native households but hardly or not at all for immigrants. When we look at household composition, it is perhaps not surprising that the incentive appeals most to households consisting of partners with children, although the increase is less for single parents. Finally, web response is higher in less urban areas, although somewhat less extreme and linear than in interviewer modes. The incentive increases web response in the areas where response is already highest. This pattern is repeated in the total response.

Table 37. Subgroup response rates.

		no incentive			incentive		
		n	RR web	RR total*	n	RR web	RRtotal*
mean age of household core	<30	1818	14	19	2197	19	27
	30-44	4159	18	24	5147	22	32
	45-64	7275	26	32	8854	28	38
	>=65	1254	30	38	1556	31	46
neighbourhood income in quartiles (€)	<1900	3885	15	19	4571	17	24
	1900-2300	3882	19	24	4895	23	32
	2300 - 2800	4320	24	29	5244	27	35
	>=2800	4247	27	32	5254	29	37
gender of household core	male(s)	2759	20	25	3442	23	31
	mixed	8748	24	31	10548	28	39
	female(s)	3269	21	27	4100	22	32
ethnic background	autochtonous and mixed	11680	26	32	14461	29	40
	non-western background	2025	8	12	2395	9	17
	western background and	1071	17	23	1234	17	26
type of household	single	4078	22	27	5061	24	33
	partners, no children	3781	30	36	4493	32	44
	partners, with children	5324	20	27	6524	24	35
	single parent	1502	16	23	1923	17	27
urbanicity	> 2500 addresses / km ²	4084	18	20	5132	20	26
	1500-2500 addresses / km ²	3910	22	26	4775	24	31
	1000-1500 addresses / km ²	2868	21	27	3550	25	34
	500-1000 addresses / km ²	2781	25	30	3411	28	37
	< 500 addresses / km ²	2691	23	29	3096	26	37

* respons total: response from entire sample, not corrected for subsampling in subsequent cati and capi approach.

Although response rates and relative differences in response rates between groups give a fair impression of the distribution of response over the groups, and hence about representativeness of the response for various groups, it is informative to look at the difference between the percentage response in a given group, and the size of that groups in the sample. This is what is called relative bias: the difference of the proportion of respondents in a particular group, compared to the proportion of the sample units in that group. Calculation of relative bias shows in one glance whether a group is over or underrepresented, and to what extent. Table 38 shows this calculation for the response rates and sample distribution of the auxiliary variables discussed above.

The columns show for each auxiliary variable what percentage of the sample and what percentage of the response each subcategory represents. For example, 13% of the no-incentive sample exists of

households younger than 30 years old, while 8% of the web response is from this group. If the response would be completely representative of the sample, 13% of responses should also have come from this group. As it is, the youngest group is under represented in the response: 4.8 percentage points in web, and 4.3 percentage points in the total response. The absolute relative bias sums this discrepancies for each subcategory. It can be seen that the introduction of the incentive substantially improves representativeness for age, especially in the web round. The follow-up modes further improve representativeness, especially in the no-incentive condition. But also in the total response is the representativeness better in the incentive condition than in the control group. Which subgroup is over or under represented is not changed by the incentive, only the extent to which. Likewise, representativeness for income is improved in the incentive condition, although the lowest income groups remain severely under represented. Another category that is better represented as a result of the incentive is type of household, mostly because 'partners without children' is less severely over represented, and 'partner with children' less underrepresented. Singles, however, are not better represented as a result of the incentive. They are also one of the few groups who do not profit from the CATI and CAPI follow up: they are less underrepresented in web than in the total response. The incentive does not influence representativeness for all auxiliary variables: for ethnicity and urbanicity hardly any difference is seen between the two conditions, and for gender the distribution becomes even somewhat worse. When we sum the relative bias values for all these categories, it gives an indication that for these categories, the design with incentive has increased representativeness, both for the web response and the total response (96.1 vs 86.3 for web and 85.9 vs 76.4 for the total response, for control and incentive conditions respectively).

Table 38. Subgroup relative bias

		no incentive					incentive				
		% of sample	% of response		over/under representation		% of sample	% of response		over/under representation	
			web	total	web	total		web	total	web	total
mean age of household core	<30	13	8	8	-4,8	-4,3	12	9	9	-3,3	-3,1
	30-44	29	23	24	-6,0	-4,5	29	25	26	-4,1	-3,1
	45-64	50	58	56	8,0	6,0	50	55	54	5,3	3,7
	>=65	9	11	11	2,8	2,8	9	11	11	2,1	2,5
		abs rel bias			21,6	17,5	abs rel bias			14,7	12,4
neighbourhood income in quartiles (€)	<1900	24	16	17	-7,3	-6,6	23	16	17	-7,0	-5,7
	1900-2300	24	22	22	-2,2	-1,9	25	23	24	-1,6	-0,7
	2300 - 2800	26	29	29	2,9	3,0	26	29	29	3,2	2,4
	>=2800	26	33	32	6,6	5,5	26	32	30	5,3	4,0
		abs rel bias			19,0	17,0	abs rel bias			17,1	12,8
gender of household core	male(s)	19	16	16	-2,4	-2,6	19	17	17	-2,0	-2,5
	mixed	59	63	63	4,1	4,1	58	64	64	5,3	5,2
	female(s)	22	20	21	-1,7	-1,5	23	19	20	-3,3	-2,7
		abs rel bias			8,2	8,1	abs rel bias			10,6	10,4
ethnic background	autochtonous and mixed	79	90	89	10,7	9,7	80	91	89	10,6	9,0
	non-western background	14	5	6	-9,0	-8,1	13	5	6	-8,4	-7,0
	western background and	7	5	6	-1,8	-1,5	7	5	5	-2,2	-2,0
		abs rel bias			21,5	19,3	abs rel bias			21,2	18,0
type of household	single	28	27	26	-0,9	-1,5	28	27	26	-1,4	-2,1
	partners, no children	26	34	32	8,5	6,5	25	32	30	6,9	5,5
	partners, with children	36	31	33	-4,8	-2,9	36	34	36	-2,2	-0,6
	single parent	10	7	8	-2,8	-2,2	11	7	8	-3,3	-2,8
		abs rel bias			17,0	13,1	abs rel bias			13,8	10,9
urbanicity	> 2500 addresses / km ²	25	21	20	-4,4	-5,5	26	22	21	-4,2	-5,2
	1500-2500 addresses / km ²	24	24	24	0,2	0,4	24	24	23	-0,3	-0,8
	1000-1500 addresses / km ²	18	18	18	0,0	0,5	18	19	19	0,8	1,1
	500-1000 addresses / km ²	17	20	20	2,7	2,5	17	19	20	2,4	2,6
	< 500 addresses / km ²	16	18	19	1,4	2,1	16	17	18	1,3	2,3
		abs rel bias			8,7	10,9	abs rel bias			9,0	11,9

2.5.3. Data quality

Literature suggests that incentives have either no effects on data quality, or a positive effect, although a minority of studies also found negative effects, see chapter 2.3. In this paragraph three measures of data quality are discussed: the percentage of ‘don’t know’ answers in all questions answered, the percentage of refusals, and the pace of filling in the questionnaire, operationalized as the questionnaire duration divided by the number of questions. The literature usually also studies what is called ‘straightlining’ as a measure of quality: the tendency to give the same answer in a battery of grid questions. The Dutch LFS does not have these kind of questions, so this latter analysis is not possible.

Missing values in the Dutch LFS consist mostly of ‘don’t know’ answers, which makes sense in a survey where proxy answering is allowed. The missing values are expressed as a percentage of the number of questions answered. The percentage of refusals is very low in the three modes, and in neither mode is there an effect of the incentive, see table 39.

Table 39 Percentage of refusals by mode and incentive

	no incentive	Incentive	<i>p</i>
CAWI	0.47	0.48	ns
CATI	0.49	0.47	ns
CAPI	0.53	0.52	ns

*** $p < .0001$ ** $p < .001$ * $p < .05$ ns not significant

The incentive does have an effect on the number of DK answers, see table 40. Both in CAWI and in CAPI, the number of DK answers is lower in the incentive condition, tested with analyses of variance. There is no effect in CATI. In the next paragraphs we study whether these general findings hold for all subgroups that we have studied earlier.

Table 40. Percentage of don’t know answers by mode and incentive

	no incentive	incentive	<i>p</i>
CAWI	3.24	2.88	***
CATI	0.85	0.85	ns
CAPI	0.78	0.70	**

*** $p < .0001$ ** $p < .001$ * $p < .05$ ns not significant

Respondents know that they will find out if they won an iPad immediately after filling in the questionnaire. This perspective may induce very eager respondents to speed through the questionnaire, with a risk of reduced data quality. The pace of filling in the questionnaire was determined by dividing questionnaire length by the number of eligible persons in the household. Time measurement was determined at the same moment in the questionnaire for both conditions (households in the incentive condition received some more questions, to do with the incentive).

In CAWI a significant effect of incentive was found, indicating that questionnaire length was *longer* in the incentive condition: 15.2 minutes in the incentive condition versus 14.3 minutes in the control condition. The same pattern was found in the other modes. This is not surprising, as the previous paragraphs showed that the number of substantive answers increased in the incentive condition.

2.5.4. Substantive variables

We have seen that the incentive leads to a different distribution of subgroups in the response and a different distribution across modes. These differences should be corrected by weighting adjustment, but only if there are no measurement differences between modes. Measurement differences occur if the same question to the same person leads to other answers in different modes. Various studies for the Dutch LFS have shown that mode effects are foremost the result of differences in coverage and

response rates between modes, and not of differences in measurement (Schouten, van den Brakel, Buelens, van der Laan and Klausch, 2013; Klausch, 2014; Klausch, Hox and Schouten, 2017). That means that mode difference can be tackled by adjustment through weighting. Similar findings have been reported by Körner (2014) for the German LFS, in the context of the ESSnet DCSS on mixed mode data collection (Luiten et al., 2014). Differences in the distribution over modes as a result of the incentive, can therefore be expected to be corrected by the weighting model.

In the previous chapters on response patterns, sample households were followed through fieldwork, from CAWI to CATI or CAPI, where CAWI is studied for month t, CATI for month t+1 and CAPI for month t+2. For the study of substantive variables, another approach is needed. Monthly statistics are made from the combined data of CAWI in month t, the CATI data that were sampled in month t-1, the CAPI data that were sampled in month t-2 and all wave 2 to 5 data in month t. The effect of the incentive on substantive variables in any given month will be difficult to gauge, especially since no separate weighting models were developed for the data with and without incentive. To try and see whether some information can nevertheless be gleaned from the data, first wave data from three months were studied, based on when the fieldwork was performed. These three months were studied, because the distribution between incentive and no incentive was fairly equal in total, and incentives were also offered in the months where the CATI and CAPI samples came from. Weighted data were studied, even though the weighting model was not performed separately for incentive and no incentive conditions.

The first variable studied is the classification into labour status: employed, unemployed and inactive. The results do not lead to unambiguous findings, however, see table 41. The table shows column percentages of the three classes, per month and per incentive condition.

Table 41. Labour force classification by incentive

	September		October		November	
	no incentive	incentive	no incentive	incentive	no incentive	incentive
employed	60.9%	60.5%	64.8%	59.5%	60.9%	61.6%
unemployed	2.8%	3.0%	3.6%	2.6%	3.5%	2.6%
non-labor force	36.3%	36.6%	31.6%	37.9%	35.6%	35.8%

The only finding that is stable in all three months, is that the incentive draws in some more people who are not in the labour force. The classification in the other two categories is volatile, however. In September and October the incentive is related to a lowering of the estimation of employed people, but in November to an increase. Likewise, the number of unemployed is higher in the incentive condition in September, but lower in the other two months. To fully understand the impact of the incentive on the employment estimates, separate weighting models should be developed for the two conditions.

Table 42 shows similar analyses of the effect of the incentive on the SOI3 classification of education. Again, these are wave 1 findings from September, October, and November.

Table 42. SOI3 classification by incentive

	September		October		November	
	no incentive	incentive	no incentive	incentive	no incentive	incentive
Lower	31.5%	28.5%	33.1%	30.5%	34.9%	29.6%
Middle	39.8%	35.9%	39.0%	36.8%	38.3%	35.8%
Higher	26.3%	32.5%	26.1%	30.5%	24.7%	32.4%
Unknown	2.5%	3.1%	1.9%	2.2%	2.0%	2.2%

In contrast to the employment findings, these analyses lend themselves to unambiguous interpretation: the introduction of the incentive leads to relatively more higher educated respondents in the sample, as well as more respondents with an unknown education. Mirroring this finding is a relatively lower percentage of middle and lower educated respondents. This finding may have implications for the employment estimates as well if there is a difference in employment status related to education. Which there is: people with a higher education are employed to a much higher extend than people with a lower education (77% vs 41% in this data file). The suggestion made above to study the impact of the incentive on employment estimates by developing differential weighting schemes should be considered.

2.5.5. Summary and discussion of LFS findings

The lottery incentive has been successful in increasing response, representativeness and data quality of the LFS and has the potential to save costs in at least four ways. A short summary:

- The lottery incentive leads to higher web response in wave 1, and higher overall response. The incentive does not affect response in the second and third wave. The effect could not yet be determined for the later waves, but we can safely assume that there will be no effect there as well.
- It is our interpretation that the incentive leads to a shift to the cheaper mode of people who would otherwise have been respondents in the other modes.
- The incentive leads to faster response, necessitating less reminders
- The incentive increases representativeness for several important variables, i.e., age and income. For some other variables no difference in representativeness was found. Over all variables, the design with incentives showed less variation in subgroup response propensities than the design without incentives.
- Subgroup variance in response propensities increases adjustment weights and therefore increases variance. If variance in the adjustment weights is decreased, sample size may be decreased as well, a third way in which costs can be reduced.
- The incentive leads a substantially higher number of households to agree to be contacted for wave 2. These households stay in the panel to the same extend as households without incentive. This indicates that the response gain in the first wave as a result of higher response and higher panel recruitment continues in the following waves. This is the fourth way in which the incentive saves costs.
- The incentive leads to higher data quality: especially the number of 'don't know' answers diminishes significantly. The effect is strongest in CAWI and is found specifically in smaller households, for people with a lower education, and native and non-western respondents. In this latter group, the effect is offset by a higher number of refusals, however. A higher number of refusals is found in the youngest two age groups as well.
- The fact that people in the incentive condition put more effort in the answering process is reflected in the pace of filling in the questionnaire: People in the incentive condition take a minute more time.
- There are strong indications that the incentive increases the relative number of higher educated persons in the response. This may have implications for the estimation of labour force status. If this is indeed the case could not be determined. The findings in the present analysis did not allow stable interpretation.

Although the literature on lottery incentives shows mixed findings in the literature, Statistics Netherlands has managed to find a *modus vivendi* to make these incentives work. The lottery incentive did not have an influence on the response propensities in the second and later waves, however. Incentive schemes can be designed with this purpose in mind: several studies in literature showed that relatively small unconditional incentives keep their effect for many waves. CBS is presently redesigning the LFS to become person sample based. New incentive experiments will wait for this redesign, as incentive effects are larger in person samples. In the present design, with the large initial sample, no other incentive than a lottery can be economically viable. One adaptation that is possible in the present practice, is to link the chance of winning relative to the size of the household (and to communicate that), a practice that is remarked upon in the literature as well. It seems unfair that the larger households have to put in far more effort, but have the same chance of winning.

An obvious alternative for the present lottery incentive is an unconditional incentive. Statistics Netherlands has extensive experience with unconditional €5 vouchers in other surveys. They have proven to substantially increase (web) response rates and can be economically viable in a design including CAPI, as is the LFS. The literature suggests that these kinds of incentives will also increase registration into the panel, and will keep their effect for several waves. The amount offered should depend on one other element: the timing of the introduction of the panel. Presently, respondents are not informed of the fact that they are in a panel: they are recruited for each next wave. It is to consider to recruit persons for the panel at the very beginning. This would probably lead to lower initial response, but presumably to higher wave response. In this scenario, an unconditional incentive with a higher value than the €5 that are presently used should be considered.

Unconditional incentives would be our preferred first choice for experimentation. Conditional incentives can be considered, as they are used in many panels, although the rewards offered in most are higher than what we would be prepared to pay. A conditional incentive that could be tried is offering one to people who stay in the panel for the entire five waves. But then again, that would entail mentioning that they are actually in a panel.

As the literature does not offer one clear cut panacea, a series of carefully designed experiments should determine which of these alternatives offers the optimal balance between costs, response, quality and attrition.

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Appendix 1. Original LFS letter – page 1

(see further for a translation)



Centraal Bureau
voor de Statistiek

contactpersoon Jeldrik Bakker
Methodoloog
/
uw brief van
ons kenmerk
bijlage(n)
onderwerp

CBS Heerlen
CBS-weg 11
6412 EX Heerlen
Postbus 4481
6401 CZ Heerlen
+31 45 570 60 00
www.cbs.nl

Geachte mevrouw/mijnheer,

Graag nodig ik u uit om mee te werken aan een belangrijk onderzoek van het Centraal Bureau voor de Statistiek (CBS). Het gaat om de Enquête Beroepsbevolking. Dit onderzoek van het CBS is in Nederland een onmisbare bron voor cijfers over arbeid, opleiding, werkloosheid en arbeidsongeschiktheid.

Elke maand trekt het CBS ongeveer 11000 adressen uit alle adressen die er in Nederland zijn. Deze keer zit uw adres in onze selectie. Voor de kwaliteit van de statistieken van het CBS is het van groot belang dat zo veel mogelijk benaderde mensen meedoen. Het is voor ons dus belangrijk dat juist u meedoet. U vertegenwoordigt als het ware veel andere inwoners van Nederland.

Ik zou het zeer op prijs stellen als u bereid bent om de vragenlijst op het internet in te vullen. U vindt de vragenlijst op het volgende internetadres: <https://vragenlijst.cbs.nl/EBB16a>

Om uw gegevens tegen misbruik te beschermen, gebruiken we een beveiligde verbinding. U dient daarvoor niet het gebruikelijke 'http' maar 'https' in te typen. Het is belangrijk dat u dit in de adresbalk boven in uw scherm doet. Intypen in Google of een andere zoekmachine werkt niet.

Als u op deze website bent, wordt u gevraagd een gebruikersnummer en een toegangscode in te vullen.

Uw gebruikersnummer is: 5723-321-894

Uw toegangscode is: 7239273

Na het invullen van uw gebruikersnummer en uw toegangscode komt u in de vragenlijst.

Wij zijn ons ervan bewust dat niet iedereen internet heeft. Daarom zal een medewerker van het CBS u over een aantal weken bezoeken of bellen. Mocht u de vragenlijst dan nog niet ingevuld hebben.

Bij al onze onderzoeken is uw privacy volledig gewaarborgd. Op de achterzijde van deze brief leest u daar meer over.

Als u vragen of problemen heeft die samenhangen met het gebruik van het internet, dan kunt u onze helpdesk e-mailen: contactcenter@cbs.nl onder vermelding van 'EBB'. Mocht u vragen hebben naar aanleiding van deze brief of over het onderzoek dan kunt u telefonisch contact opnemen met het CBS

Als u vragen of problemen heeft die samenhangen met het gebruik van het internet, dan kunt u onze helpdesk e-mailen: contactcenter@cbs.nl onder vermelding van 'EBB'. Mocht u vragen hebben naar aanleiding van deze brief of over het onderzoek dan kunt u telefonisch contact opnemen met het CBS Contact Center te Heerlen: (045) 570 64 00. Het Contact Center is bereikbaar van maandag tot en met vrijdag tussen 9.00 en 17.00 uur. Daarnaast kunt u terecht op onze website: www.cbs.nl

U doet ons een groot plezier als u een van de komende dagen de internetvragenlijst invult.

Wij danken u alvast hartelijk voor uw tijd en medewerking.

Vriendelijke groet,



Harry A. Wijnhoven
Hoofd directeur Dataversmelting (a.i.)

Bij al onze onderzoeken is uw privacy gewaarborgd. Dit is een verplichting van het CBS die in een speciale wet is vastgelegd. Om uw gegevens te beveiligen heeft het CBS tal van maatregelen getroffen. Zo is er een strenge geheimhoudingsplicht voor alle medewerkers, op straffe van rechtsvervolging. Gegevens over mensen worden zo snel mogelijk gescheiden van de namen en de adressen. De gegevens worden verwerkt met goed beveiligde computersystemen waartoe onbevoegden geen toegang hebben. De wet garandeert dat uw gegevens alleen voor statistische doeleinden worden gebruikt. Geen enkele instelling kan toegang opnemen tot de gegevens die het CBS verzamelt. In de statistische informatie die het CBS naar buiten brengt, zijn persoonlijke gegevens nooit te herkennen.

Het CBS verzamelt niet alleen zelf gegevens maar krijgt ook veel bestanden van andere instellingen. Bijvoorbeeld de gegevens van de bevolkingsadministraties, de centra voor werk en inkomen (UWV WERKbedrijven), de sociale diensten, de salarisadministraties van veel bedrijven. Wij combineren automatisch de informatie die u zelf in dit onderzoek geeft met informatie die we van andere instellingen krijgen. Met deze gecombineerde informatie stelt het CBS statistieken samen over de Nederlandse samenleving en werken we zo zinnig mogelijk.

Translation

Dear Madam, Sir,

I would like to invite you to participate in an important study by the Central Bureau for Statistics (CBS). It concerns the Labor Force Survey. This study by Statistics Netherlands is an indispensable source of data on labor, education, unemployment and incapacity for work in the Netherlands.

Every month, Statistics Netherlands draws approximately 11,000 addresses from all addresses in the Netherlands. This time your address is in our selection. For the quality of Statistics Netherlands Statistics it is of great importance that as many approached people as possible participate. It is therefore important for us that you participate. You represent, as it were, many other residents of the Netherlands.

I would very much appreciate it if you are willing to fill in the questionnaire on the internet. You can find the questionnaire at the following internet address: <https://vragenlijst.cbs.nl/EBB16a>

To protect your data against misuse, we use a secure connection. You do not have to type in the usual 'http' but 'https'. It is important that you do this in the address bar at the top of your screen. Typing in Google or another search engine does not work.

If you are on this website, you will be asked to enter a user number and an access code.

Your user number is: 5723-321-894

Your access code is: 7239273

After completing your user number and your access code, you will enter the questionnaire. We are aware that not everyone has internet. That is why an employee of the CBS will visit or call you in a few weeks, if you have not yet completed the questionnaire.

In all our investigations your privacy is fully guaranteed. You can read more about this on the back of this letter. If you have questions or problems related to the use of the internet, you can e-mail our helpdesk: contactcenter@cbs.nl stating 'EBB'. Should you have any questions about this letter or about the research, you can contact the CBS Contact Center in Heerlen by phone: (045) 5706400. The Contact Center is available from Monday to Friday between 9.00 and 17.00. You can also visit our website: www.cbs.nl

You do us a great favor if you fill in the internet questionnaire one of the next few days. We thank you in advance for your time and cooperation.

Sincerely, Harry, J.A. Wijnhoven

Director of Data Collection

Appendix 2. Letter on one A4



Centraal Bureau
voor de Statistiek

ons kenmerk
onderwerp
datum

CBS Heerlen
Postbus 4481
6401 CZ Heerlen

Geachte mevrouw/meneer,

Graag nodig ik u uit om mee te werken aan een belangrijk onderzoek van het Centraal Bureau voor de Statistiek (CBS). Het gaat om de Enquête Beroepsbevolking. Dit onderzoek van het CBS is in Nederland een onmisbare bron voor cijfers over arbeid, opleiding, werkloosheid en arbeidsongeschiktheid.

Elke maand selecteert het CBS ongeveer 11.000 adressen uit alle adressen die er in Nederland zijn. Deze mensen vertegenwoordigen als het ware veel andere inwoners van Nederland. Deze keer zit uw adres in onze selectie. Voor de kwaliteit van de statistieken van het CBS is het van groot belang dat zo veel mogelijk van de geselecteerde mensen meedoen. Daarom is het belangrijk dat juist u meedoet!

Ik zou het zeer op prijs stellen als u bereid bent om de vragenlijst op het internet in te vullen: <https://vragenlijst.cbs.nl/ebb15a>. De inloggegevens vindt u in het kader. Wij zijn ons ervan bewust dat niet iedereen internet heeft. Daarom zal een medewerker van het CBS u over een aantal weken bezoeken of bellen, mocht u de vragenlijst dan nog niet ingevuld hebben.

Bij al onze onderzoeken is uw privacy volledig gewaarborgd. Op de achterzijde van deze brief leest u daar meer over.

Heeft u vragen, bel ons gerust op (045) 570 64 00 of mail naar contactcenter@cbs.nl. Wij zijn bereikbaar van maandag tot en met vrijdag tussen 9.00 en 17.00 uur.

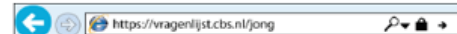
U doet ons een groot plezier als u een van de komende dagen de internetvragenlijst invult.

Wij danken u alvast hartelijk voor uw tijd en medewerking.

Vriendelijke groet,

Harry, J.A. Wijnhoven
Hoofddirecteur Dataverzameling (a.i.)

Gelijk aan de slag



Gebruikersnummer: 1234 567 891
Toegangscode: 12345

Appendix 3. Stronger and more persuasion arguments

Dear Sir / Madam,

One of the most important studies of the Central Bureau for Statistics (CBS) is the Labor Force Survey. This research is an indispensable source for figures on labor, employment history, education, unemployment and incapacity for work.

Statistics Netherlands tries to use existing data as much as possible, but we need help for a number of additional data. For this research we therefore select a small number of randomly chosen households. Your household is one of them. You help us by participating in this study. You can complete the questionnaire on our website. Because of the results, we know how things are on the labor market, with education and social security.

I really appreciate it when you fill out the questionnaire on the internet. You can find the questionnaire at the following internet address:

<https://vragenlijst.cbs.nl/ebb15a>

To protect your data against misuse, we use a secure connection. It is important that you type the internet address in the address bar at the top of your screen. Typing in Google or another search engine does not work. If you are on the CBS website, you will be asked to enter a user number and an access code.

Your user number is:

Your access code is:

After completing your user number and your access code, you will enter the questionnaire.

We are aware that not everyone has internet. That is why an employee of the CBS will visit or call you in a few weeks, if you have not filled in the questionnaire.

In all our surveys your privacy is fully guaranteed. You can read more about this at the bottom of this letter.

If you have any questions, do not hesitate to call us on (045) 570 64 00. We are available from Monday to Friday between 9 am and 5 pm. You can also mail to contactcenter@cbs.nl.

You do us a great favor if you fill in the internet questionnaire one of the next few days.

We thank you in advance for your time and cooperation.

Appendix 4. Lower linguistic complexity

Dear Sir / Madam,

I would like to invite you to participate in an important study by CBS. We do the research so that we know how many people work, attend training, be unemployed, disabled or retired.

Every month, CBS chooses 11,000 addresses from all addresses in the Netherlands. This time your address is in our selection. For the quality of the results, it is important that as many people as possible participate. That's why we want you to participate too. You represent many other people in the Netherlands.

I would very much appreciate it if you would complete a questionnaire for this study. The questionnaire is on the internet. The address of the website is:

<https://vragenlijst.cbs.nl/ebb15a>

To protect your data, we use a secure website. It is important that you type the internet address in the address bar at the top of your screen. Do not do this in Google or any other search engine.

The website asks for a user number and an access code.

Your user number is:

Your access code is:

We know that not everyone has internet. Have you not completed the questions in a few weeks? Then an employee of CBS will visit or call you shortly thereafter.

Your information is safe in all our surveys. You can read more about this on the back of this letter.

If you have any questions, do not hesitate to call us on (045) 570 64 00. We are available from Monday to Friday between 9 am and 5 pm. You can also mail to contactcenter@cbs.nl.

You do us a great favor if you answer the questions one of the coming days.

We thank you for your help and time.

Kind regards,

Appendix 5. Detailed login instruction

Geachte mevrouw/meneer ,

Graag nodig ik u uit om mee te werken aan een belangrijk onderzoek van CBS. Het gaat om de Enquête Beroepsbevolking. Dit onderzoek van CBS is in Nederland een onmisbare bron voor cijfers over arbeid, opleiding, werkloosheid en arbeidsongeschiktheid.

Elke maand trekt CBS ongeveer 11 000 adressen uit alle adressen die er in Nederland zijn. Deze keer zit uw adres in onze selectie. Voor de kwaliteit van de statistieken van CBS is het van groot belang dat zo veel mogelijk benaderde mensen meedoen. Het is voor ons dus belangrijk dat juist u meedoet. U vertegenwoordigt als het ware veel andere inwoners van Nederland.

Ik zou het zeer op prijs stellen als u bereid bent om de vragenlijst op het internet in te vullen.

Gelijk aan de slag

Stap 1: Start internet op.

Stap 2: Typ <https://vragenlijst.cbs.nl/ebb15b> in de adresbalk van uw browser, bovenaan het scherm. Dat ziet er zo uit:



Opzoeken in Google of een andere zoekmachine werkt niet.

Stap 3: Het kan zijn dat uw computer de vragenlijst niet vanzelf opstart. Klik in dat geval op een van de woordjes 'hier'.

Stap 4: Vul het gebruikersnummer en de toegangscode in.

Gebruikersnummer: 0142 - 170 - 560

Toegangscode: 377419

Stap 5: Klik op Akkoord om te beginnen.

Z.O.Z.

(the rest of the letter was the same as the control letter).

Translation (of the login instruction):

Getting started

Step 1: Start the internet

Step 2: Type <https://vragenlijst.cbs.nl/ebb15b> in the address bar of your browser, at the top of your screen. That looks like this: (see picture above)

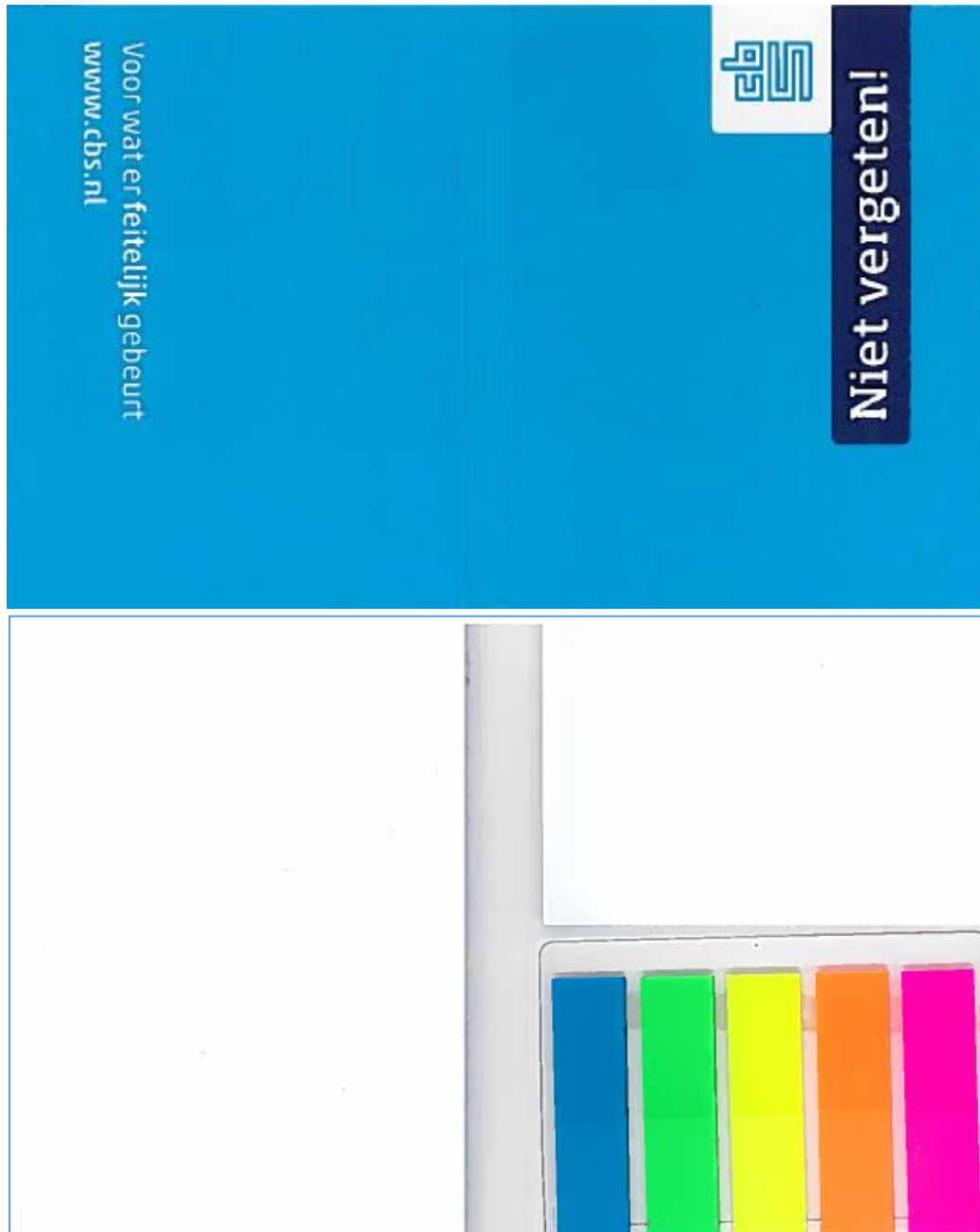
Looking up the address in Google or another search engine will not work

Step 3: It may be that your computer does not start the questionnaire automatically. In that case, press one of the words 'hier'.

Step 4: Fill in the user number and access code (see picture above)

Step 5: Click 'akkoord' to start

Appendix 6. Notepad included with letter



Appendix 7. Pre-notification card



U bent gekozen!



Met uw hulp kunnen we Nederland voorzien van onafhankelijke betrouwbare en actuele informatie. CBS is de onderzoeksorganisatie van Nederland. Wij zijn geen commerciële organisatie.

Wat we doen?
Met uw informatie publiceren we dagelijks wat er feitelijk gebeurt. We geven een beeld van hoe we in Nederland leven: het werk dat we doen, de zorg die we delen of bijvoorbeeld de inhoud van onze boodschappenkar.

Waarom u?
Met uw informatie kunt u een groot deel van de inwoners van ons land vertegenwoordigen. U bent dus echt belangrijk voor ons. Binnenkort ontvangt u daarom een uitnodiging om mee te doen aan ons onderzoek.

Nieuws, uitleg en achtergronden
Kijk op [CBS.nl](https://www.cbs.nl) als u geïnteresseerd bent in informatie of nieuws over ons.

U kunt ons ook volgen op:

 @statistiekcbs met dagelijks nieuwe cijfers over Nederland

 statistiekcbs met leuke feiten en cijfers over Nederland

PostNL
Port Betaald

**Centraal Bureau
voor de Statistiek**

Postbus 24500
2490 HA Den Haag

Wij rekenen op u

Appendix 8. Advance letter 'all the way' experiment.

U bent gekozen om mee te doen aan CBS onderzoek

CBS is de onderzoeksorganisatie van Nederland. Wij publiceren dagelijks over allerlei onderwerpen die belangrijk zijn voor Nederland. Dit keer doen wij onderzoek naar wat mensen doen in het dagelijks leven. Werkt u, bent u op zoek naar werk, studeert u, bent u gepensioneerd of zorgt u voor iemand?

Doet u mee?

Ik zou het zeer op prijs stellen als u onze vragenlijst wilt invullen. Dat kan heel eenvoudig via internet. Gebruikt u geen internet? Dan zal een medewerker van CBS u over een aantal weken bellen of bezoeken.

Zo doet u mee

De vragenlijst staat op internet. Het adres van de website is:

<https://vragenlijst.cbs.nl/ebb15a>

Het is belangrijk dat u het internetadres in de adresbalk bovenaan uw scherm typt. *Doe dit niet in Google of een andere zoekmachine.*

Op de website wordt gevraagd naar een gebruikersnummer en een toegangscode.

Uw gebruikersnummer is: 1234 – 567 – 890 Uw toegangscode is: 12345

Uw gegevens zijn veilig

Uw gegevens worden alleen voor ons onderzoek gebruikt. Dat staat in de wet. Zodra we uw gegevens ontvangen, koppelen we ze direct los van uw naam en adres. U blijft dus anoniem. Op de achterzijde van deze brief leest u daar meer over.

Heeft u vragen?

Bel ons gerust op (045) 570 64 00. Wij zijn bereikbaar van maandag tot en met vrijdag tussen 9.00 en 17.00 uur. U kunt ook naar contactcenter@cbs.nl mailen.

U doet ons een groot plezier als u een van de komende dagen de vragen beantwoordt.

Ik dank u hartelijk voor uw hulp en tijd.

Met vriendelijke groet,

Astrid Boeijen

Translation

You have been chosen to participate in Statistics Netherlands research

CBS is the research organization of the Netherlands. We publish daily about all kinds of subjects that are important for the Netherlands. This time we do research into what people do in daily life. Do you work, are you looking for a job, are you studying, are you retired or are you looking after someone?

Are you in?

I would very much appreciate it if you would fill in our questionnaire. This can be done very easily via the internet. Do you not use the internet? Then a CBS employee will call or visit you in a few weeks.

This is how you participate

The questionnaire is on the internet. The address of the website is <https://vragenlijst.cbs.nl/ebb15a>. It is important that you type the internet address in the address bar at the top of your screen. Do not do this in Google or any other search engine. The website asks for a user number and an access code. Your user number is: 1234 - 567 - 890 Your access code is: 12345

Your data are safe

Your data will only be used for our research. That is in the law. As soon as we receive your data, we

immediately disconnect them from your name and address. So you remain anonymous. You can read more about this on the back of this letter.

[Do you have questions?](#)

Call us at (045) 570 64 00. We are available from Monday to Friday between 9.00 and 17.00. You can also mail to contactcenter@cbs.nl.

You do us a great favour if you answer the questions one of the coming days.

Thank you very much for your help and time.

Sincerely,

Astrid Boeijen,

Head of data collection

Appendix 9. LFS advance letter with lottery of iPad.



<aanhef>

Werk hebben is voor de meeste mensen heel belangrijk. Maar lukt dat ook voor iedereen? Hoeveel mensen zoeken een baan? En hoe zit het met het aantal flexcontracten? Hoe verdelen we onze tijd tussen werken en zorgen? En hoeveel mensen volgen op dit moment een opleiding? Om dit soort vragen te beantwoorden, voert het CBS de 'Enquête Beroepsbevolking' uit.

Elke maand trekt het CBS een klein aantal adressen uit alle adressen die er in Nederland zijn. Deze keer zit uw adres in onze selectie. Uw huishouden vertegenwoordigt veel andere inwoners in Nederland. Het is daarom belangrijk dat u aan dit onderzoek meedoet. U helpt ons daar enorm mee. Of u nu in vol- of deeltijd werkt, een ~~flexcontract~~ heeft, met pensioen bent, een opleiding volgt, freelancer bent of geen baan heeft, u bent even belangrijk voor ons.

Als dank voor uw hulp maakt u kans op één van de 25 iPads ¹.

Hoe kunt u meedoen?

U kunt de vragenlijst invullen via internet. Om de gegevens te beschermen gebruiken we een beveiligde verbinding. U vindt de vragenlijst op het volgende internetadres:

<url>

Typ dit adres in de adresbalk bovenaan uw scherm, niet in Google of een andere zoekmachine.

U kunt inloggen met:

Gebruikersnummer: <re_id>

Toegangscade: <inlogcode>

¹ Aan het eind van de vragenlijst ziet u direct of u een iPad krijgt toegestuurd. Wilt u geen iPad ontvangen? Dan kunt u dat in de vragenlijst aangeven.

Appendix 10. Letter with QR code

<aanhef> <Naam>,

Voor de toekomst van ons land zijn onderzoeken van het CBS onmisbaar. Het CBS laat zien hoe de Nederlandse samenleving zich ontwikkelt. Eén van de meest gevraagde CBS-cijfers is het consumentenvertrouwen. Om dit cijfer maandelijks te kunnen geven, doen wij elke maand onderzoek. Hierin komen vragen aan de orde als: Vindt u dat de prijzen het afgelopen jaar zijn gestegen of gedaald? Is het wat u betreft een gunstige tijd om grote aankopen te doen? Verwacht u dat de werkloosheid zal stijgen of dalen?



Voor dit onderzoek vraagt het CBS een aantal personen om een vragenlijst in te vullen. U bent daar één van. U vertegenwoordigt veel andere inwoners in Nederland. Het is daarom belangrijk dat u aan dit onderzoek meedoet. U helpt ons daar enorm mee.

Het invullen van de vragenlijst duurt ongeveer 5 minuten. Als dank voor uw hulp maakt u kans op één van de iPads¹ die we beschikbaar stellen.

Hoe kunt u meedoen?

U kunt de vragenlijst invullen via internet. Om de gegevens te beschermen gebruiken we een beveiligde verbinding. U vindt de vragenlijst op het volgende internetadres:
<https://antwoord.cbs.nl>

Uw gebruikersnaam: <Gebruikersnaam>

Uw wachtwoord: <Wachtwoord>



U kunt de vragenlijst ook via uw smartphone of tablet invullen. Scan de QR-code en u komt direct in de vragenlijst. U hoeft dan geen gebruikersnaam en wachtwoord in te vullen.

Geen internet? Een medewerker van het CBS kan u over een paar weken bellen, als de vragenlijst dan nog niet is ingevuld.

Uw gegevens zijn veilig

Bij al onze onderzoeken zijn uw gegevens veilig. Onderaan deze brief leest u daar meer over.

¹Aan het eind van de vragenlijst ziet u direct of u een iPad krijgt toegestuurd. Wilt u geen iPad ontvangen? Dan kunt u dat in de vragenlijst aangeven.

Appendix 11. 2019 LFS letter



Centraal Bureau voor de Statistiek
Postbus 24500 | 2490 HA Den Haag
Postbus 4481 | 6401 CZ Heerlen
www.cbs.nl

16 jaar
EBB-X
Geslacht is m

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correspondentienr. <correspondentienummer>
onderwerp CBS-Onderzoek
datum 15 augustus 2017

<aanhef> <Naam>,

Werk hebben is voor de meeste mensen heel belangrijk. Maar lukt dat ook voor iedereen? Hoeveel mensen hebben een baan? En om welk soort werk gaat het dan? Hoeveel mensen zoeken een baan? Om dit soort vragen te beantwoorden voert het CBS het onderzoek 'Werk' uit.

Met de resultaten uit dit onderzoek kan het CBS beschrijven hoe goed of slecht het gaat op de arbeidsmarkt in Nederland. Deze informatie helpt de overheid en bedrijven om de juiste beslissingen te nemen.

Voor dit onderzoek vraagt het CBS een aantal personen om een vragenlijst in te vullen. U bent daar één van. U vertegenwoordigt veel andere inwoners in Nederland. Het is daarom belangrijk dat u aan dit onderzoek meedoet. U helpt ons daar enorm mee.

Als dank voor uw hulp maakt u kans op één van de iPads¹ die we beschikbaar stellen.

Hoe kunt u meedoen?

U kunt de vragenlijst invullen via internet. Om de gegevens te beschermen gebruiken we een beveiligde verbinding. U vindt de vragenlijst op het volgende internetadres:

<https://antwoord.cbs.nl>

Uw gebruikersnaam: <Gebruikersnaam>

Uw wachtwoord: <Wachtwoord>

Uw gegevens zijn veilig

Bij al onze onderzoeken zijn uw gegevens veilig. Onderaan deze brief leest u daar meer over.

Heeft u vragen?

Bel ons gerust op (045) 570 64 00. Wij zijn bereikbaar van maandag tot en met vrijdag tussen 9.00 en 17.00 uur. U kunt ook naar contactcenter@cbs.nl mailen. Of kijk op www.cbs.nl/werk voor meer informatie.

¹Aan het eind van de vragenlijst ziet u direct of u een iPad krijgt toegestuurd. Wilt u geen iPad ontvangen? Dan kunt u dat in de vragenlijst aangeven.

vervolg op achterzijde



Translation

Having a job is very important to most people. But does that everyone succeed in finding one? How many people have a job? and what kind of work is it? How many people are looking for a job? To answer these kinds of questions, Statistics Netherlands carries out the 'Work' survey.

Statistics Netherlands uses the results from this survey to describe how good or bad the labour market in the Netherlands is. This information helps the government and companies to make the right decisions.

For this study, Statistics Netherlands asks a number of persons to complete a questionnaire. You are one of them. You represent many other residents in the Netherlands. It is therefore important that you participate in this study. You help us tremendously with that.

As a thank you for your help, you can win one of the iPads that we make available.

How do you participate?

You can complete the questionnaire via the internet. To protect the data we use a secure internet connection. You can find the questionnaire at the following internet address:

<https://antwoord.cbs.nl>

Your user name: <username>

Your password: <Password>

Your data are safe.

Your data are safe in all our investigations. You can read more about this at the bottom of this letter.

Do you have questions?

Call us on (045) 570 64 00. We are available from Monday to Friday between 9 am and 5 pm. You can also mail to contactcentre@cbs.nl. Or visit www.cbs.nl/werk for more information.

You do us a great favor if you fill in the questionnaire one of the coming days.

Thank you in advance for your time and cooperation.

Sincerely,

<signature and name>