

Cash vs. Vouchers vs. Gifts in Web Surveys of a Mature Panel Study—Main
Effects in a Long-Term Incentives Experiment across Three Panel Waves

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Abstract

In this study, we evaluate short- and long-term effects of three different prepaid incentives on young panellists' cooperation and response rate in three waves of a mature panel study with sequential multi-mode design. The survey experiment involved an alternative study design in order to analyse the effect of different types of prepaid incentives, taking selective attrition into account as well as considering problems related to causal inference (and the stable unit treatment value assumption behind such problems). The subjects were students from randomly selected school classes who have finished their compulsory school. The findings are clear: cash provides the strongest direct, positive effect on the overall response rate, as well as on the latency until response after first contact. The other incentives did not work as efficiently as cash. Additionally, cash is the most likely to minimise social selectivity in response. Finally, cash provides the potential to convert refusals in previous waves into cooperation.

Keywords

Incentives; subjective value; panel attrition; response rate; survey experiment; web survey; causal inference

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1 Introduction

In social sciences, panel studies have become increasingly prominent in gathering longitudinal data on trajectories in the individuals' life course (Blossfeld et al., 2011), their developments (Blossfeld et al., 2009), and social processes, in order to explain structural changes (Wagner et al., 2007). However, sociologists deplore declining response rates in social-scientific surveys across historical periods (e.g. Ernst Stähli and Joye, 2016; Pforr et al., 2015; Dillmann et al., 2014; Andreß et al., 2013; Groves, 2004; Schnell, 1997); low response rates decrease the effective sample size, which increases the standard errors used in statistical multivariate estimations (Goldenberg et al., 2009). At the same time, this problem is particularly important in panel surveys. According to Jäckle and Lynn (2008: 105), nonresponse in panel surveys is particularly problematic, since even low wave-on-wave nonresponse rates can lead to substantial losses in sample size. Since dropout from a panel study is mostly socially selective (Voorpostel, 2010; Lipps, 2007; Behr et al., 2005), the increasing panel attrition across waves might result in biased findings (Lugtig, 2014). It is often revealed that respondents with low educational attainment are typically more likely to drop out of panel surveys (Becker and Glauser, 2018; Jäckle and Lynn, 2008: 107). Additionally, nonresponses exacerbate multivariate analysis of processes in terms of biased estimations with minor statistical precision (e.g. Groves and Peytcheva, 2008) due to right censoring of interesting trajectories (Becker and Glauser, 2018; Blossfeld et al., 2007). Thus, due to cumulative dropout from a panel study and its consequences for statistical analysis of longitudinal data, observing individuals in a panel design is challenging and costly in terms of the number of individuals lost in subsequent panel waves (Andreß et al., 2013).

In order to prevent unit nonresponse and panel attrition, several strategies are used to optimise the response and retention rate (Laurie et al., 1999). One of the most efficient and effective strategies is using prepaid monetary incentives in order to increase the interviewees' willingness to take part in the survey and to enhance the response quality of less motivated respondents (Ernst Stähli and Joye, 2016). The success of using "incentives as a motivator for survey participation" (Singer and Bossarte, 2006: 412) has been confirmed in a huge number of controlled experiments, while less is known about their effects in online surveys embedded in a mature panel study (Becker and Glauser, 2018; Pforr et al., 2015; Singer and Ye, 2013; Scherpenzeel and Toepoel, 2012; Sánchez-Fernández et al., 2010; Goldenberg et al., 2009; Singer and Couper, 2008; Göritz, 2006; Singer et al., 2000, 1999b; Warriner et al., 1996; Church, 1993). It is frequently found that monetary incentives generate higher response rates than no or non-monetary incentives, and that unconditionally prepaid cash incentives are more effective in terms of increased response rates than promised incentives (Ernst Stähli and Joye, 2016: 430; Becker and Mehlkop, 2011: 8; Singer et al., 2000: 186; Warriner et al., 1996). This is also valid for web-based surveys. For example, Gajic et al. (2012) found higher contact and response rates in a web-based survey.

Furthermore, there is no strictly linear correlation of response rate and value of a cash incentive. In fact, one can even notice a declining effect of the values given (Dillman et al., 2014). Controlling for respondents' effort (e.g. length and complexity of the questionnaire; time exposure; mode of interview), rather low amounts are sufficient to stimulate interviewees' responses (Szelényi et al., 2005). For Germany or Switzerland, as two examples, it has been found that 5 Euros (Mehlkop and Becker, 2007) or 10 Swiss francs (Becker and Mehlkop, 2011) seem to be enough to increase the respondents' survey participation (Scherpenzeel and Toepoel, 2012). Fewer experimental studies exist for panel surveys (Lipps, 2010; Laurie and Lynn, 2009; Jäckle and Lynn, 2008) but these provide rather similar findings (Becker and Glauser, 2018). In several studies, for example, it is found that incentives could improve sample compositions in panel surveys by stimulating responses from interviewees with disproportionately low participation rates, such as individuals with

lower levels of education or those originating from lower social classes or living in households with less economic welfare (Becker and Glauser, 2018; Laurie and Lynn, 2009; Jäckle and Lynn, 2008; Ryu et al. 2005; Singer et al. 2000, 1999a). Pforr et al. (2015: 24) stress that a later rise in incentive values increases response rates in the wave of implementation (see Ryu et al., 2005). They found “weak support for a constant increase in retention rates when incentives are consistently offered across multiple waves. Decreasing incentives in a later wave does not decrease retention rates” (Pforr et al., 2015: 24).

Although previous research has extensively analysed whether prepaid monetary incentives have a positive effect on the response rate (Singer and Couper, 2008), there is still uncertainty about the reliability of these findings. Since the bulk of these findings is based on controlled experiments that distinguish between randomised treatment and non-treatment groups, we have to deal with the fundamental problem of causal inference (Holland, 1986). As a result, we do not know if respondents which did not receive a prepaid cash incentive would have taken part in a panel survey if they had received this incentive unconditionally. What would they have done if they had received a non-monetary gift instead of cash? And what would they have done if they had received an equivalent voucher instead of money? In order to overcome the methodological limitations of such experiments we suggest another design in the context of a mature panel study with several waves. Our alternative design provides real information on counterfactuals.

The remainder of the article is organised as follows. In section 2, the theoretical framework is presented alongside a summary of the state of research relate to the background of the state of research related to the efficiency of prepaid incentives to increase response rates in panel surveys. Section 3 provides a description of the data set, variables, and statistical procedures. The findings are presented in section 4, while conclusions are given in the final section.

2 Theoretical background

2.1 *How to explain (non-)response and the efficiency of prepaid incentives*

The use of respondent incentives is an important element of the strategy to minimise unit nonresponse and panel attrition (Dillman et al., 2014). Why do some respondents answer and why do some respondents not answer in surveys even after they have received an incentive? Answers to this question are often sought by applying two theories (e.g. Singer and Bossarte, 2006). According to the economic exchange theory the respondents’ rational cost-benefit calculation in deciding whether to participate in a survey is the key mechanism behind response or nonresponse (Becker and Mehlkop, 2011). Therefore, it is rational to enclose an unconditional gift like a prepaid monetary incentive in an advance letter. This “cash in the hand” is a compensation for the respondents’ efforts and time devoted to answering the questionnaire. The higher the amount of cash incentive, the more likely the respondents are to take part in the survey. The social exchange theory (Dillman et al., 2014), alternatively, claims that prepaid monetary incentives are the researchers’ investment in mutual trust which is necessary for social interaction between interviewer and interviewee. On the one hand, unconditional prepaid cash is a “symbol of trust” (Dillman et al., 2014). “By enclosing a ‘token of appreciation’, the researcher is demonstrating trust that the potential respondent will answer the survey. Individuals are motivated to act by the return they expect and receive from others, so trust on the part of the giver is a necessary component of social exchange” (Ryu et al., 2005: 91). According to Millar and Dillman (2011), sending cash incentives in advance deemphasises the purely economic “payment” context of incentives and instead creates a type

of social encouragement that stresses the importance of the survey.¹ Experiments by LaRose and Tsai (2014: 113) provide evidence that respondents therefore need to receive the “gift” materially (Becker and Mehlkop, 2011). On the other hand, due to the norms of reciprocity, interviewees feel obliged to respond (Becker and Glauser, 2018; Dillman et al., 2014; Scherpenzeel and Toepoel, 2012; Becker and Mehlkop, 2011; Diekmann, 2004; Gouldner, 1960; Mauss, 1984 [1924]). Cooperation in a survey is their counter-gift to the researcher’s gift (Becker and Glauser, 2018). However, since there is no research on whether respondents actually feel an obligation to respond and to cooperate with researchers in order to discharge the debt in relation to the advance payment (Singer and Ye, 2013), it could be assumed that an incentive might also substitute for a lack of motivation to participate for other reasons, such as civic duty or interest in the topic (Porst, 1999; Singer and Couper, 2008: 50).² According to these theoretical approaches, it is logical that the response rate is higher for individuals who have received prepaid incentives unconditionally, compared to individuals who have received no incentive or those who have received an incentive provided that they filled out the questionnaire. In particular, according to the social exchange theory, any sort of prepaid incentive should have a similar effect (Ryu et al., 2005: 92).³

Utilising the sociological theory of rational action, the theory of subjectively expected utility, it could be assumed that an unconditional prepaid monetary incentive increases the “chance” (Weber, 1922) of response in terms of the respondents’ inclination to cooperate in the sense of instrumentally or value-driven rational social action. The effect of cash might be larger than the effects of other incentives due to the universalistic character of money. From the view of the theory of subjectively expected utility, the differences in respondents’ likelihood to respond depend not only on material differences in respect of incentives but also—from the *respondents’ subjective view and evaluation*—on differences in the subjective values of the incentives. However, this could explain why prepaid cash is the most efficient incentive (Pforr et al., 2015: 24), in contrast to quasi- resp. semi-monetary incentives, such as a cash card of a hypermarket chain (Becker and Glauser, 2018), or non-monetary gifts, such as a ballpoint pen (Ernst Stähli and Joye, 2016: 430; Warriner et al., 1996).

Indeed, this is in line with alternative sociological interpretations. One famous interpretation is provided by the German sociologist Georg Simmel (1900) and his seminal work on the philosophy of money. What counts is the subjective value of money—i.e. the respondents’ view on the good they have received. According to Simmel (1900), money is the most general form of social interaction. Money has a double function. On the one hand, it demonstrates the relative price of a good and, on the other hand, it has a value itself since it is applicable for each purpose everywhere and at all times. This means that “cash has a universally understood value” (Ryu et al., 2005: 91). In contrast to other incentives, such as a voucher or ballpoint pen, money is a universal medium, and therefore it is of interest to each interviewee, whatever their social sphere. Furthermore, money is an absolute medium for exchange since it is

¹ Therefore, in accordance with the social exchange theory (Dillman et al., 2014), it has to be considered “if the incentive is seen as preceding a request that is disproportionately large in comparison, the incentive will be ineffective” (Ryu et al., 2005: 92).

² Additionally, to mention the mechanism, the possible normative sanction for nonresponse as well as the shame due to failure to take part results in the subjects’ cognitive dissonance (Festinger, 1957). This dissonance will be solved by accepting the norm of reciprocity and the actual cooperation with the researcher (Becker and Glauser, 2018; Becker and Mehlkop, 2011).

³ However, this argument is problematic for two reasons (Becker and Glauser, 2018: 5). On the one hand, it has to be stressed that such a gift is not highly institutionalised, like presents on a person’s birthday, at Christmas or during a state visit. Furthermore, the gift is given by researchers who are not familiar with the interviewees. Of course, this problem will be solved partially by a kind advance letter in which the researchers introduce themselves and their project. On the other hand, it should be noted that the researchers have no power to sanction the subjects’ defection in terms of non-cooperation and unit nonresponse.

absolutely indeterminate in regard to its use. Modern money can be used for any exchange, i.e. its mean is absolutely open. The use of money is not related to dates and deadlines. By this way, social certainty will be created independent from presence and future. Money contributes to the objectification of social interactions between one human being and another human being. Trust in money is universal since it is the mean of economic interactions.

This is not true for other unconditional monetary incentives, such as a voucher which could be used to buy goods and services. Becker and Glauser (2018) found that a cash card of a value of 10 Swiss francs for a duopolistic hypermarket in Switzerland had a positive effect on the young panellists' response rate (the increase in response rate was 7 percentage points), while— to give another example—another experiment with prepaid cash (10 Swiss francs) resulted in an increase of 24 percent points (Becker and Mehlkop, 2011). According to Ryu et al. (2005), this difference can be explained by the fact that cash has an immediate value, and, in contrast to other incentives, such as a voucher, cash has many different uses. The value of such a cash card for a hypermarket may depend on the degree to which one is interested in going to the (online or local) hypermarket shop, the degree to which one might prefer to use this voucher due to the individual identification with this hypermarket instead of other supermarkets, and to what extent one has access to the (online) shop. Compared to money, such vouchers are not a universalistic medium. This is true for non-monetary gifts, such as ballpoint pens, that provide a very important but restricted application. If this argument on the subject value of money is true, it could be assumed that individuals from hard-to-reach households with lower socio-economic status and related financial welfare would be more likely to be sensitive to the cash they receive from researchers at once and without any terms (Martinez-Ebers, 1997). This might minimise responses being biased by social origin or socio-economic status.

2.2 *How to reveal the real effect of prepaid incentives in panel studies with online surveys*

As already mentioned above, the results of incentive manipulations are regularly observed in a classical experimental design using randomised experimental and control groups as well as controlled assignment of treatments. However, the question arises how we can overcome the fundamental problem of causal inference. In our case, we are not able to isolate the individual causal effect δ_i of unconditional prepaid incentives— $\delta_i = Y_i^1 - Y_i^0$, whereby $Y_i^1 =$ potential outcome for individuals i with treatment and whereby $Y_i^0 =$ potential outcome for individuals i without treatment—on individuals' response rate since it is possible to observe only a single event D for an individual: $Y = DY^1 - (1 - D)Y^0$, while the other event for the same individual is a counterfactual (Angrist and Pischke, 2009). Given that we can observe an individual i only in one situation at the same time, this fundamental problem of causal inference means that it is not possible to isolate the individual causal effect δ_i since we have just half of the necessary information we need to identify the total individual causal effect. In practice, we have to substitute missing information with assumptions. Of course, from the sociological perspective we are not interested in the individual but in the average treatment effect (ATE). So ATE will be calculated instead of the individual causal effect: $ATE = E[\delta] = E[Y^1 - Y^0] = E[Y^1] - E[Y^0]$ —i.e. the difference of average treatments of the control and experimental groups will be taken into account by comparing like with like in cross-section: $ATE = E\langle Y^1 | D = 1 \rangle - E\langle Y^0 | D = 0 \rangle$. The rationale behind this is that randomisation solves the problem of missing information because in an ideal situation the expected value of the unobserved individual effect is equivalent to ATE.

As seen above for the state of research, different prepaid incentives—such as money, vouchers, pens, or other non-monetary gifts—will be considered in a cross-sectional survey or in a single panel wave in contrast to non-treatment. However, to reveal the effect of such incentives it is not necessary to distinguish between treatment and non-treatment if the design

of a multiple-wave panel is considered. Therefore, for our purpose, we suggest another experimental design by using the longitudinal character of a mature panel study with seven waves (Becker and Glauser, 2018). We do not observe the *ATE* separately for control and experimental groups at the same time in a single panel wave; we do not split our sample in each of the panel waves randomly into these two groups. Taking into account that unconditional prepaid material incentives work immediately, there is no need for a control group indicating non-treatment. However, taken three different incentives into account, we distinguish three waves in regard to distributing the treatments. The first incentive, a voucher (cash card for a Swiss hypermarket, worth 10 Swiss francs), was awarded to each of the contactable interviewees as part of the fifth wave of our panel study. In the sixth wave, they received a fancy gift (a ballpoint pen with a gravure: www.dab.edu.unibe.ch), and, finally, in the seventh panel wave, they received prepaid money (10 Swiss francs cash enclosed in the advance letter). Please note that each of the incentives is—as recommended by Dillman et al. (2014)—unconditional.⁴ As suggested by Laurie and Lynn (2009) for longitudinal surveys, we choose to change the value of the incentives offered between waves and to vary the form the incentive takes, switching from voucher (worth 10 Swiss francs) to non-monetary gift (worth about 2 Swiss francs) and to cash (10 Swiss francs banknote). In this way we try to reveal the relative effectiveness of possible combinations over panel waves. It is assumed that prepaid incentives with a relatively low value result in significant improvements in response rates of a mature panel or in the maintenance of relatively high response rates at least across panel waves. In this respect, we agree with Laurie and Lynn (2009: 230) that small gifts given regularly across waves are more effective than an occasional large gift given in the first wave (see also: Becker and Glauser, 2018). As mentioned by Laurie and Lynn (2009: 230), “in particular, we still know relatively little about the effect of changing incentive amounts (...) during a longitudinal survey, targeting particular groups based on demographic characteristics or previous response history, the use of differential incentive amounts for different cases or circumstances and the longer term effect of incentives on attrition and bias.”

Overall, the three experimental groups comprise all of the contactable interviewees. In this way, we seek to overcome the fundamental problem of causal inference in the setting of a controlled survey experiment. This means that we do not have to make any assumptions regarding counterfactuals (Morgan and Winship, 2007) due to an exhausted set of options. Additionally, as a result of this design, causal inference will be revealed from a series of treatments for the same individuals at different points in time (Pötter and Blossfeld, 2001). Thus, it is possible to take the main theoretical assumptions of rational action theories, such as economic exchange theory, social exchange theory, and the theory of subjectively expected utility, into account (Becker and Mehlkop, 2011). When the same respondents are observed at different points in time it will be possible to detect the causal impact of different prepaid incentives on their inclination to respond exactly (Blossfeld and Rohwer, 1997).

3 Panel study, variables, and statistical procedures

3.1 *DAB panel study*

The experiment was conducted in the context of a panel study on determinants of educational choice and vocational training opportunities (Becker and Glauser, 2018; Glauser and Becker, 2016; Glauser, 2015).⁵ In order to reconstruct educational trajectories prospectively, the data

⁴ The interviewees received their incentive enclosed in the advance letter since the response rates are significantly higher when using unconditional incentives compared to promised incentives or incentives sent via the internet (Görizt, 2004, 2006; Scherpenzeel and Toepoel, 2012: 472–474).

⁵ The data sets of the first three waves are already available at FORS (<https://forsbase.unil.ch/project/study-public-overview/14834/0/>). The data of the experiment may be requested from the authors.

collection was limited to juveniles born around 1997 and who were enrolled in regular classes in public schools in German-speaking cantons of Switzerland. The panel data is based on a random stratified gross sample of 296 school classes (8th grade), out of a total universe of 3,045 classes. After contacting the headmasters and teachers, 215 out of 296 school classes were ready to participate in the online survey in the first wave (Glauser, 2015).

In the first three waves, the students were interviewed in the context of their school class by means of an online questionnaire when they were in the middle of the eighth grade (Wave 1: January–February 2012), as well as at the beginning (Wave 2: August–October 2012) and at the end (Wave 3: May–June 2013) of the ninth grade (*Table 1*). The response rates—RR2 suggested by AAPOR (2015: 52)—amounted to between 90 and 96 per cent realised by web-based surveys.

Table 1: Samples and response in the DAB panel – online mode only

| | Wave 1 Jan-Feb 2012 | Wave 2 Aug-Oct 2012 | Wave 3 May-Jun 2013 | Wave 4* Oct-Nov 2014 | Wave 5 Jun-Aug 2016 | Wave 6 May-Jun 2017 | Wave 7 May-Jun 2018 |
|----------------------------|------------------------|------------------------|------------------------|-------------------------|------------------------|------------------------|------------------------|
| <i>Sample size</i> | | | | | | | |
| Gross sample | 4,059 | 4,059 | 4,059 | 4,059 | 4,059 | 4,059 | 4,059 |
| Contactable individuals | 3,894 | 3,707 | 3,436 | 2,652 (1,153) | 2,861 | 2,712 | 2,494 |
| <i>Type of survey</i> | | | | | | | |
| Online survey | yes | yes | yes | yes | yes | yes | yes |
| CATI survey | no | no | no | yes | yes | yes | yes |
| PAPI survey | no | no | no | no | no | yes | yes |
| Incentive | no | no | no | voucher | voucher | ballpoint pen | money |
| <i>Realised interviews</i> | | | | | | | |
| Individuals | 3,680 | 3,343 | 3,302 | 2,237 | 2,229 | 2,058 | 2,005 |
| of whom: Online | 3,680 | 3,343 | 3,302 | 1,227 (653) | 1,334 | 1,374 | 1,625 |
| CATI & PAPI | – | – | – | 1,010 | 895 | 684 | 380 |
| <i>Response rate in %</i> | | | | | | | |
| School classes | 73% | 94% | 98% | – | – | – | – |
| Contactable individuals | 95% | 90% | 96% | 84% | 78% | 76% | 80% |
| Online | 95% | 90% | 96% | 52% (57%) | 46% | 51% | 65% |
| CATI and PAPI | – | – | – | 32% | 32% | 25% | 15% |

* Experimental split – in brackets: only individuals receiving incentive in Wave 4 (for details: Becker and Glauser, 2018)

15 months after leaving compulsory school, the contactable interviewees—who therefore have to be pursued individually—were interviewed again in the fourth wave (October–November 2014). The total response rate was about 84 per cent if the interviews by the web survey and the CATI survey are taken into account.⁶ In the online mode only, 52 per cent of the contactable individuals completed the web-based questionnaire.⁷ Furthermore, it should

⁶ The fieldwork was carried out according to the tailored design method TDM suggested by Dillman (2000; Dillman et al., 2014). The aim of TDM is to customise scientific survey procedures for specific survey situations in order to minimise sources of survey error (coverage, sampling, unit nonresponse, measurement etc.) given the (time) resources available. For this purpose, the cultivation of contacts and the communication with the survey target persons are considered to be of central importance. In our panel study, due to efficiency and methodological advantages, the online mode was chosen in each of the waves (Couper and Bosnjak, 2010). It is often argued that web surveys have several advantages, including shorter transmitting time, lower delivery cost, more design options, and less data entry time. Furthermore, in online surveys, there are less frequent answers that are due to social desirability (Heerwegh, 2009). There is evidence that it is useful to start with the less expensive mode of the web-based online survey at first preferred by the youth (Smyth et al., 2014: 142) and then—after the reminders—to offer the expensive mode of CATI (Dillman et al., 2014). The social selection inherent in having access to the internet (Couper et al., 2007; Schonlau et al., 2009), or the preference for another mode, should be compensated for by the CATI—in other words, to be more precise, survey nonresponse should be reduced by a sequential mix of modes (De Leeuw, 2005: 240). Meanwhile, positive experiences exist of mixing online surveys with CATI (Becker and Glauser, 2018; Cernat, 2014; De Leeuw, 2005; De Leeuw et al., 2011). Higher response rates for a combination of both these modes are reported (Greene et al. 2008).

⁷ Each of the online surveys and mail surveys (PAPI) was realised by the Department of Sociology of Education at the University of Bern using “UniPark EFS Survey” in Waves 4, 6 and 7, and “LimeSurvey” in Wave 5. The CATI in the fourth wave was realised by a commercial polling agency (M.I.S Trend in Lausanne), in Wave 5 by

be noted that in this wave a controlled method experiment was conducted (Becker and Glauser, 2018). One half of these individuals received a voucher—i.e. a hypermarket cash card worth 10 Swiss francs—enclosed in the advance letter as an unconditional prepaid incentive. The other half of the individuals made up the control group who did not receive an incentive. The randomisation was realised at the level of the individuals' previous school class in order to avoid those class mates who benefitted not feeling envious and those class mates who benefitted not feeling ashamed (Becker and Glauser, 2018: 8). Provided that the individuals received this prepaid incentive, the response rate was 57 per cent and was significantly higher than the response rate for interviewees who did not receive a gift.

In the fifth wave, which took place three years after the completion of compulsory education (June 2016), about 78 per cent of contactable individuals responded, 46 per cent of them in the online mode and 32 per cent in the CATI mode. As mentioned above, the contactable interviewees got a voucher (a hypermarket cash card worth 10 Swiss francs). In Wave 6 (May–June, 2017), about 76 per cent of contactable individuals responded, 51 per cent of them in online mode and 25 per cent in the CATI and PAPI modes. All of the individuals got an engraved ballpoint pen as a prepaid incentive. Finally, one year later (May–June, 2018), in Wave 7, about 65 per cent of the contactable individuals responded in the online mode. All of them received 10 Swiss francs in cash (banknote), enclosed in the advance letter. Considering the low response rate of 15 per cent in the CATI mode and the PAPI mode, the total response rate was 80 per cent. At this point it was becoming increasingly difficult to stimulate the respondents to take part in telephone interviews and mail surveys.

It should be noted that we limit our analysis to the web mode. In our panel, there is no significant effect of prepaid incentives on the CATI or PAPI mode offered for respondents who were not ready to answer the online questionnaire (Becker and Glauser, 2018).

3.2 Dependent and independent variables

The dependent variable is the likelihood of participation in each of the most recent three waves of the DAB panel study. Hereby, we are mainly interested in the duration (measured on a daily basis) between contact and start of completing the questionnaire.

The three types of treatment—the voucher (hypermarket cash card worth 10 Swiss francs), the ballpoint pen (non-monetary gift), and the cash (a 10 Swiss francs banknote)—are the most important explaining variables. To analyse sensitivity, the reference categories will be changed across different estimations. For reasons of statistical control, we consider the individuals' gender, previous schooling (basic secondary school; advanced secondary school; Gymnasium), grade point average (GPA) in the ninth grade (6=highest grade – 1=lowest grade), and the social class of their parents (EGP class schema by Erikson and Goldthorpe, 1992). It should be noted that missing values for previous schooling and social origin have been considered as a category in multivariate estimations.

3.3 Statistical procedures

For multivariate analyses, we apply different procedures, such as comparative-static binary logistic regression (Long, 1997), as well as dynamic procedures of event history analysis (Blossfeld et al., 2007). The effects of incentives will be estimated by semi-parametrical (Cox regression) and parametric models implying different distributions of the hazard rate—i.e. the marginal value of conditional probability that responses occur as a causal effect of a prepaid incentive. In the case of exponential distribution, the hazard rate is constant across duration

time. For the Weibull distribution, it is assumed that—depending on parameter a —the hazard rate is increasing or decreasing monotonically. This is also true for the Gompertz model.

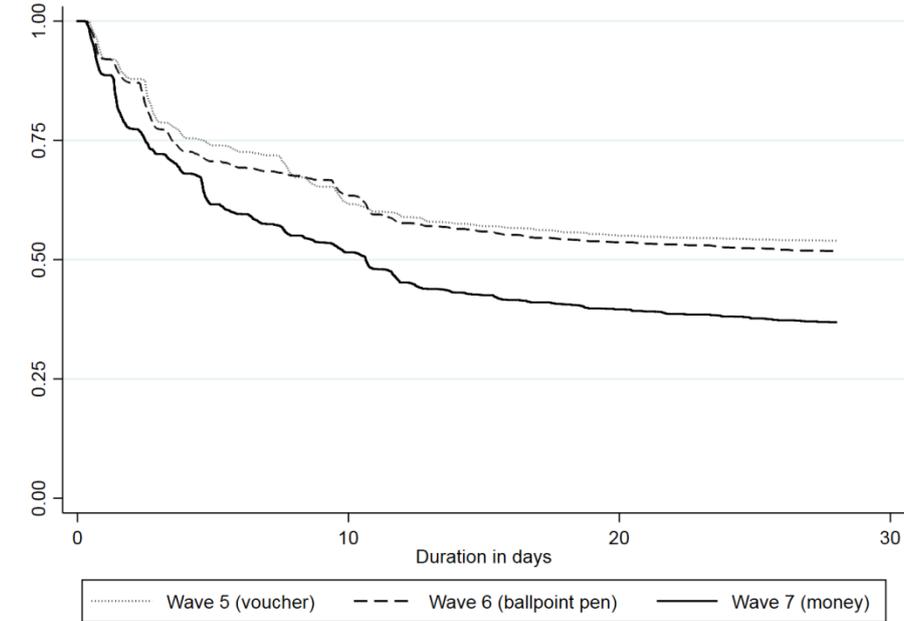
Finally, in order to minimise the problem of positive selection of beneficiary respondents across waves, as well as comparing something that is incomparable due to assigning different incentives at different points in time, panel regression was used. Panel regression is useful to detect the fixed effects of incentives on individual responses across waves (Brüderl and Ludwig, 2015). For individuals involved in each wave this model is employed if dependent and independent variables vary across waves. The mixed effect model is estimated if constant covariates, such as gender or social origin, are included in the estimation.

4 Empirical results

4.1 Impact of incentives on duration until response

As theoretically assumed, and if each of the three waves are taken into account, it is found, on the one hand, that prepaid incentives lead to comparatively high response rates in the web-based surveys. About 53 per cent of the contactable interviewees who received an unconditional prepaid incentive responded, while just 47 per cent did not respond in spite of receiving a prepaid gift.

Fig. 1: Impact of incentives on latency until response to online survey (Kaplan-Meier estimates)



By survival analysis—and in line with our theoretical assumption—different speeds of responses have been revealed for the types of incentives (*Figure 1*). The lowest latency is observed for Wave 7. After 10.6 days, half of the interviewees who received the cash incentive had started the questionnaire. However, since the response rate was too low, it is not possible to calculate the median for the latencies of interviewees who received a voucher in Wave 5 or ballpoint pen in Wave 6.

The Log-rank and Wilcoxon tests provide significant differences between the survival curves for each of the incentives.⁸ There is a polarised difference between the cash incentive and the

⁸ The field time for the web survey was limited to 41 days, as a maximum. The survival analysis revealed that the events became very rare after four weeks.

other incentives. The latencies for vouchers and ballpoint pens are not significantly different. Overall, cash is the most efficient incentive in terms of relative response rate and latency.

4.2 *Impact of incentives on the individuals' inclination to respond*

This preliminary conclusion is confirmed by multivariate analysis (*Table 2*). Estimating comparative-static logit regression or dynamic models of event history analysis, it is found that cash only has a significant positive effect on individuals' response, while the effects of the other incentives are not systematic. The relative chance—if estimated coefficients are calculated in terms of odds ratios—that individuals will respond in the web-based online survey is about two times higher when they are given cash (with no conditions), compared to the other incentives (model 1 or models 3–6).

According to the estimated fixed effects (model 2), this is also true for those contactable interviewees that were available for each of the waves. These interviewees have been considered apart since we have to be aware that the assignment to the experimental conditions was not completely at random but was little affected by the self-selection of participants into each of the panel waves. The fixed effects panel regression, which takes into account this methodological limitation of our design, confirms the “naïve” findings of the comparative-static and dynamic estimations. The response rate increases by at least about 10 percentage points on average provided that the individuals receive cash instead of other incentives. If the previous incentive in the setting of a classical experiment in Wave 4 (Becker and Glauser, 2018) is taken into account it is found that there is no positive self-selection due to this first benefit (Singer and Ye, 2013).

In line with theoretical assumptions and previous research, there are differences between the types of incentive. Cash results in significantly increased response rate, while the effects for the other incentives used are much lower and statistically insignificant. These findings are still valid if some of the contactable individuals' characteristics are taken into account. Women are more likely to respond to online questionnaires than their male counterparts (models 7 and 8). Individuals who have been enrolled in higher school tracks participate more often than individuals in lower school tracks. This is also valid for high achieving persons: the higher their average mark, the greater their inclination to take part in the panel. As found in a previous experiment, these characteristics indicate the interviewees' efficacy to cooperate and to respond. Finally, we did not reveal any effects of the interviewees' origin due to their social class and related socio-economic status. Compared to a previous experiment (Becker and Glauser, 2018), the effect of social class on response will be eliminated by the incentives.

Overall, there are eight main findings. First, even in a mature panel, there are positive effects of incentives on response rates, varying across the type of gift given. Second, the unconditional prepaid money (“cash in the hand”) is the most efficient incentive. Third, the effect of cash minimises the latency for participating in the web surveys across panel waves. Fourth, the positive effect of a prepaid monetary incentive is still significant if one takes the individuals' social characteristics into account. Fifth, it seems to be that the subjectively evaluated value of an incentive accounts for the anticipated effect on survey participation since the effect of a 10-Swiss-francs banknote was significantly stronger than the effect for a cash card (voucher) with the same objective value. Sixth, in spite of monetary incentives there is a social bias in response caused by the individuals' education and achievement (i.e. their efficacy to respond). Seventh, the models of event history are more likely to fit the data than the comparative-static estimations. Eighth, and finally, due to the positive selection of beneficiary respondents in Wave 4 and the following waves, we are aware that the estimated effects of incentives could be somewhat overestimated.

Table 2: Effect of incentives on response in online survey—event history models, logit regression, and fixed-effect regression (FE)

| | Models | Logit (1) | Logit (FE: 2) | Cox (3) | Gompertz (4) | Weibull (5) | Exponential (6) | Exponential (7) | Logit (ME: 8) |
|---|--------|----------------------|---------------------|---------------------|----------------------|----------------------|----------------------|----------------------|---------------------|
| <i>Incentives (Wave)</i> | | | | | | | | | |
| Voucher (5) | | <i>Reference</i> | <i>Reference</i> | <i>Reference</i> | -0.067 (0.039) | -0.492 (0.039)*** | <i>Reference</i> | <i>Reference</i> | <i>Reference</i> |
| Ballpoint pen (6) | | 0.085 (0.054) | 0.007 (0.012) | 0.067 (0.039) | <i>Reference</i> | -0.425 (0.040)*** | 0.071 (0.045) | 0.079 (0.047) | 0.008 (0.012) |
| Money (7) | | 0.696 (0.056)*** | 0.113 (0.012)*** | 0.468 (0.037)*** | 0.399 (0.037)*** | <i>Reference</i> | 0.550 (0.045)*** | 0.548 (0.046)*** | 0.113 (0.012)*** |
| Incentive in Wave 4 (experimental split) | | | | | | | | | 0.016 (0.017) |
| <i>Gender (Reference: male)</i> | | | | | | | | | |
| Female | | | | | | | | 0.360 (0.038)*** | 0.108 (0.017)*** |
| <i>Schooling (Ref.: basic secondary school)</i> | | | | | | | | | |
| Advanced secondary school | | | | | | | | 0.681 (0.050)*** | 0.180 (0.021)*** |
| Gymnasium | | | | | | | | 1.214 (0.062)*** | 0.321 (0.024)*** |
| Missing value | | | | | | | | 0.378 (0.072)*** | 0.199 (0.033)*** |
| Grade point average | | | | | | | | 0.259 (0.029)*** | |
| <i>Social origin (Ref.: Unskilled worker (VII))</i> | | | | | | | | | |
| Upper service class (I) | | | | | | | | 0.187 (0.097) | 0.040 (0.039) |
| Lower service class (II) | | | | | | | | 0.175 (0.093) | 0.063 (0.037) |
| Non-manual routine services (III a/b) | | | | | | | | 0.159 (0.091) | 0.050 (0.036) |
| Petty bourgeoisie and farmers (IV a/b/c) | | | | | | | | 0.196 (0.112) | 0.057 (0.045) |
| Engineer, foreman and skilled worker (V/VI) | | | | | | | | -0.012 (0.095) | -0.004 (0.038) |
| Missing value | | | | | | | | -0.164 (0.099) | 0.003 (0.045) |
| Constant | | -0.159 (0.037)*** | 0.513 (0.007)*** | | -2.478 (0.032)*** | -1.953 (0.028)*** | -3.657 (0.032)*** | -5.769 (0.165)*** | 0.255 (0.036)*** |
| Ln p resp. Gamma resp. | | | | | -0.123 (0.003)*** | -0.499 (0.009)*** | | | |
| N of cases [cluster] | | 8,076 | 5,952 [1,984] | 8,076 | 8,076 | 8,076 | 8,076 | 8,076 | 5,952 [1,984] |
| Response | | 4,201 | 3,960 [1,320] | 4,201 | 4,201 | 4,201 | 4,201 | 4,201 | 3,960 [1,320] |
| Wald resp. F resp. LR Chi ² (d.f.) | | 178.01 (2) | 59.51 (2; 1,984) | 190.11 (2) | 188.77 (2) | 186.96 (2) | 180.99 (2) | 871.46 (13) | 441.13 (13) |

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; β -coefficient (in brackets: standard error of the estimated coefficients); FE = fixed effect; ME = mixed effect

4.3 Impact of incentives on the social selectivity of respondents

In order to understand the social structure of cooperative respondents, we estimate the impact of material incentives on response separately for the different types of incentive. It is found that females are more likely to take part in web-based online surveys than men—independent of the receipt of any incentive and its subjective value (Table 3). This selectivity is also true for the school type in which the interviewees were previously enrolled.

Table 3: Effect of incentives on selectivity of the response in online survey

| | Models | | | Logit | | | |
|---|------------|----------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| | Incentives | Exponential | | Voucher | Pen | Cash | |
| <i>Gender</i> | | | | | | | |
| Female | | 0.304 (0.067)*** | 0.411 (0.069)*** | 0.369 (0.065)*** | 0.319 (0.079)*** | 0.478 (0.082)*** | 0.439 (0.088)*** |
| <i>Schooling</i> | | | | | | | |
| Basic secondary school | | Reference | Reference | Reference | Reference | Reference | Reference |
| Advanced secondary school | | 0.724 (0.088)*** | 0.710 (0.090)*** | 0.621 (0.083)*** | 0.800 (0.100)*** | 0.808 (0.103)*** | 0.746 (0.105)*** |
| Gymnasium | | 1.182 (0.109)*** | 1.324 (0.110)*** | 1.153 (0.104)*** | 1.325 (0.132)*** | 1.521 (0.138)*** | 1.564 (0.159)*** |
| Missing value | | 0.239 (0.128) | 0.629 (0.125)*** | 0.289 (0.121)* | 0.304 (0.143)* | 0.672 (0.144)*** | 0.353 (0.152)* |
| Grade point average | | 0.262 (0.051)*** | 0.261 (0.051)*** | 0.258 (0.049)*** | 0.266 (0.060)*** | 0.306 (0.062)*** | 0.304 (0.067)*** |
| <i>Social origin</i> | | | | | | | |
| Upper service class (I) | | 0.253 (0.177) | 0.142 (0.170) | 0.180 (0.161) | 0.356 (0.200) | 0.161 (0.202) | 0.092 (0.218) |
| Lower service class (II) | | 0.321 (0.170) | 0.034 (0.164) | 0.174 (0.154) | 0.426 (0.191)* | 0.083 (0.194) | 0.203 (0.211) |
| Non-manual routine services (III a/b) | | 0.330 (0.166)* | 0.032 (0.159) | 0.126 (0.149) | 0.412 (0.187)* | 0.033 (0.188) | 0.097 (0.204) |
| Petty bourgeoisie and farmers (IV a/b/c) | | 0.417 (0.199)* | 0.039 (0.194) | 0.153 (0.189) | 0.493 (0.230)* | 0.070 (0.233) | 0.155 (0.253) |
| Engineer, foreman & skilled worker (V/VI) | | 0.097 (0.174) | -0.155 (0.168) | 0.018 (0.155) | 0.187 (0.196) | -0.152 (0.196) | -0.027 (0.210) |
| Unskilled worker (VII) | | Reference | Reference | Reference | Reference | Reference | Reference |
| Missing value | | -0.004 (0.179) | -0.413 (0.174)* | -0.072 (0.163) | 0.036 (0.199) | -0.377 (0.203) | -0.151 (0.217) |
| Constant | | -5.877 (0.285)*** | -5.674 (0.282)*** | -5.172 (0.274)*** | -2.535 (0.331)*** | -2.499 (0.340)*** | -1.850 (0.366)*** |
| N of cases | | 2,870 | 2,712 | 2,493 | 2,870 | 2,712 | 2,493 |
| N of response (in %) | | 1,321 (46%) | 1,306 (48%) | 1,574 (63%) | 1,321 (46%) | 1,306 (48%) | 1,574 (63%) |
| Wald resp. LR Chi ² (d.f.) | | 227.93 (11) | 276.51 (11) | 232.15 (11) | 227.93 (11) | 276.51 (11) | 232.15 (11) |

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; β -coefficients (in brackets: standard error of the estimated coefficients)

However, for the individuals who were given a ballpoint pen or money, it is found that—in contrast to the panellists who received a voucher—the effect of their social origin (and the related economic welfare) on response became statistically insignificant. This result is somewhat surprising since it has to be assumed that interviewees originating from economically weak families would be sensitive to incentives with higher values, such as the voucher and money with the same material value. On the one hand, this means that money is particularly effective for working class children. On the other hand, it is obvious that the social selectivity of responses in web surveys is caused by the interviewees' educational level and literacy. Inclination to participate in panel waves is consistently biased by the individuals' educational level and achievement.

4.4 Impact of incentives on respondents who refuse to participate

Finally, we investigate if there is a lagged impact of cash on the 'conversion' of those who had refused to participate in the survey but who subsequently chose to participate in the most recent wave. Since in the previous estimations, persons are also considered who did not take

part in each of the waves, the estimation in this section is an analysis of the sensitivity of the effects of positive selections across consecutive waves.

It seems to be that “cash in the hand” is efficient in regard to mobilising individuals who have refused in one of the previous waves, at least for web survey participation. About 44 per cent of individuals who refused to participate in the fifth wave (n=1,188), and a third of those who refused to do so in Wave 6 (n=1,107) took part in the most recent wave. These percentages are higher than in the other shift: about 29 per cent of refusers in Wave 5 participated in the next wave.

Table 4: Effect of incentives on participation in Wave 7—refusals in previous waves only

| | Models | | Logit | |
|---|--------|----------------------|----------------------|----------------------|
| | No. | Exponential | 3 | 4 |
| <i>Type of incentive</i> | | | | |
| Voucher (Wave 5) | | <i>Reference</i> | <i>Reference</i> | <i>Reference</i> |
| Ballpoint pen (Wave 6) | | -0.186 (0.051)*** | -0.172 (0.050)*** | -0.309 (0.086)*** |
| <i>Gender</i> | | | | |
| Female | | | 0.150 (0.050)** | 0.264 (0.087)** |
| <i>Schooling</i> | | | | |
| Basic secondary school | | | <i>Reference</i> | <i>Reference</i> |
| Advanced secondary school | | | 0.357 (0.066)*** | 0.565 (0.103)*** |
| Gymnasium | | | 0.657 (0.077)*** | 1.214 (0.155)*** |
| Missing value | | | 0.202 (0.096)* | 0.299 (0.157) |
| Grade point average | | | 0.143 (0.037)*** | 0.249 (0.066)*** |
| <i>Social origin</i> | | | | |
| Upper service class (I) | | | -0.089 (0.088) | 0.155 (0.169) |
| Lower service class (II) | | | 0.046 (0.080) | 0.400 (0.158)* |
| Non-manual routine services (III a/b) | | | -0.018 (0.077) | 0.285 (0.150) |
| Petty bourgeoisie and farmers (IV a/b/c) | | | 0.009 (0.115) | 0.319 (0.211) |
| Engineer, foreman and skilled worker (V/VI) | | | 0.035 (0.117) | 0.312 (0.157)* |
| Unskilled worker (VII) | | | <i>Reference</i> | <i>Reference</i> |
| Missing value | | | -0.181 (0.095) | 0.370 (0.212) |
| Constant | | -4.155 (0.033)*** | -5.180 (0.192)*** | -2.269 (0.344)*** |
| N of episodes | | 2,375 | 2,375 | 2,375 |
| N of response | | 954 (40.2%) | 954 (40.2%) | 1,483 (42.6%) |
| Wald resp. LR Chi ² (d.f.) | | 13.43 (1) | 142.80 (12) | 14.14 (1) |

† $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; β -coefficients (in brackets: standard error of the estimated coefficients)

Multivariate estimations reveal that individuals who got a voucher in Wave 5 were more likely to participate in the last panel wave than the interviewees who were given a ballpoint pen in the previous wave (see models 1–4 in Table 4). It is interesting that individuals who refused to participate in Wave 6 in spite of being given an engraved ballpoint pen were less likely to be ready to take part in the next wave—in spite of the receipt of cash enclosed in the advance letter inviting them to participate in Wave 7. It is possible that the non-monetary gift was counterproductive in regard to engaging them in relation to the next online survey. However, for previous multivariate analysis it should be noted that we have not taken additional requests for survey participation (reminders) such as SMS, web SMS, and e-mails into account. In a previous survey experiment in Wave 4, we confirmed empirically that incentives are pivotal while reminders are sufficient (Becker and Glauser, 2018: 11). In the

context of the tailored design method (TDM), unconditionally prepaid cash is just one of the strategic elements necessary for improving response and retention rate (Dillman et al., 2014).

5 Summary and conclusion

Starting from general research into survey methodology as regards response rates in social-scientific surveys and improving them by means of unconditional prepaid material incentives, it is the aim of our contribution to evaluate short-term and long-term effects of such incentives on the response rates in three consecutive waves in our mature DAB panel study (Becker and Glauser, 2018). The main problem in previous experimental studies on the effects of prepaid monetary incentives has been the fundamental problem of causal inference occurring in traditional experiments with random assignment of different treatment conditions (Holland, 1986). This problem implies that we cannot at a given point in time observe the individual causal effect of D on the outcome Y for a single unit of observation but only for either D or *not* D for any subject. In particular, this is true for experiments that split individuals randomly into experimental and control groups at a single point in time (Blossfeld and Rohwer, 1997). Therefore, we did not make this random split. Each of the interviewees was assigned to different treatments in each of the different panel waves. In practice, in the first point in time (Wave 5 of our panel), we assigned a cash card of a Swiss hypermarket as a semi-monetary incentive to each individual. In the next wave, Wave 6, individuals received a non-monetary gift (an engraved ballpoint pen) and in the most recent wave we sent them a 10 Swiss francs banknote enclosed in the advance letter. Taking into account that it is empirically confirmed that prepaid incentives always have a positive effect, by comparison to non-treatment, this means that we use an alternative but efficient way of observing the counterfactual of what would have happened to units in a treatment group if they had not been exposed to that treatment (Angrist and Pischke, 2009; Morgan and Winship, 2007). There was no need to consider a control group receiving no treatment since it is taken for granted that each prepaid material incentive will work worse or better (Ernst Stähli and Joye, 2016). In this way, we might overcome the methodological challenges posed by problems of causal inference. Since previous prepaid incentives (in Wave 4, for example) have no significant effect on responses across consecutive waves (for details see: Becker and Glauser, 2015) it could be assumed that the stable unit treatment value assumptions (SUTVA) behind the problem of causal inference have been satisfied (Angrist and Pischke, 2009; Morgan and Winship, 2007). The first main requirement for valid causal inference is that there is no interference between treated and untreated subjects such that the assigned treatment to beneficiary interviewees has no influence on the potential outcomes of non-beneficiary units. The second main requirement is that there are no different “forms or versions of each treatment level, which lead to different potential outcomes” (Imbens and Rubin, 2015: 10).

The subjects were young students in German-speaking regions of Switzerland who were asked at each of the panel waves to take part in our web-based online survey regarding their educational aspirations and trajectories (Glauser and Becker, 2016; Glauser, 2015). We executed this experiment after the students had finished their compulsory schooling and after a survey experiment was carried out as a pre-test (Becker and Glauser, 2018). According to different approaches, such as the economic and social exchange theories as well as the theory of subjectively expected utilities, it was assumed that panellists who received unconditional prepaid donations of subjectively high value (cash) would be more likely to cooperate with the researchers than the same panellists who received a gift of lower value (ballpoint pen) at another point in time. Furthermore, we expected that panellists who received cash would start earlier in filling out the online questionnaire than panellists who received non-monetary incentives with the same, or a different, objective value. Based on social mechanisms such as cost-benefit calculations and the norm of (altruistic) reciprocity, as well as the idiosyncratic

characters of monetary and non-monetary incentives, it was additionally assumed that—due to the character of money as a universal medium—interviewees would be more likely to cooperate when they received cash instead of a voucher (of the same objective value) or an engraved ballpoint pen (of a rather low value). Additionally, we expected that panellists who had refused to take part in the interview in previous waves would be more likely to cooperate in the most recent panel wave after receiving “cash in the hand”. Finally, our experimental design was limited to the first mode we offered to the juvenile interviewees—namely the web-based online surveys—since we learned that the effect of prepaid incentives faded away rather rapidly after assigning them to the interviewees and this effect could not be transferred to another survey mode at a later point in time of the field work, such as CATI or PAPI. Overall, we observe no direct long-lasting effects of prepaid incentives across modes and waves (Becker and Glauser, 2018).

The empirical findings for the online mode are significant. Prepaid incentives worked as theoretically assumed, while cash (monetary incentive) was the most efficient incentive in regard to the likelihood and speed of response. Therefore, for panel studies, it is recommended not to use incentives such as cash cards (a quasi-monetary incentive) and non-monetary incentives (ballpoint pens). The fact that it is only money that really counts, however, is not in line with the social exchange theory, though it corresponds with the arguments about money made by Simmel (1900). Furthermore, we have revealed that after assigning the prepaid money there is no significant social selectivity in regard to respondents’ social origin but the differences in the responses across individual characteristics such as gender, schooling, and achievement remain in each of the panel waves. We will have to analyse in future research if these socially selective responses depend on the juveniles’ experiences in their educational trajectories and other areas in their life course, such as critical events, failures, and records. Finally, we have indirect indications that monetary incentives are more likely to be efficient as regards ‘converting’ interviewees who initially refused to cooperate to cooperation, compared to other incentives. However, this last finding is rather preliminary. On the one hand, the effect is rather weak. On the other hand, confounding factors, such as changes in the executive project management, the survey research institute responsible for the CATI mode, and the different reminders, could not be controlled for.

Of course, there are serious limitations of our study. First, in spite of the fact that the findings of our methodological experiment are in line with previous findings, and with the social mechanisms (such as reciprocity, cognitive dissonance, subjective evaluation of additional benefits, and trust in social interaction) claimed by the theoretical approaches, it should be noted that we were not able to verify these mechanisms for the correlation of D (cause) and Y (outcome) empirically. Second, the assignment to experimental conditions across successive panel waves was undermined by a rather low degree of non-random self-selection of participants in the course of panel attrition—i.e. by positive selection into waves affecting the retention rate. For future research into survey methodologies, therefore, we identify two challenges. First, there is an urgent need to test the exchange and rational choice theories explaining individuals’ cooperation and the effects of unconditionally prepaid incentives in social-scientific surveys – and, in particular, the claimed social mechanisms – directly and exhaustively. Second, in terms of cumulative research, there is an additional need to improve and replicate the type of experiment we report on here.

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