

How and Why Does the Targeted “Early Bird” Catch the Extra Worm? A Randomized and Controlled Experiment on the Effects of Prepaid and Time-Limited Postpaid Monetary Incentives on Participation in a Panel Study’s Web Survey

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Abstract

The aim of this randomized trial in Wave 10 of a long-running panel study was to reveal how and why a combination of an unconditional prepaid and a time-delimited conditional postpaid incentive is useful for enhancing survey participation. This trial was conducted in the context of a panel study that has been running since 2012. The target sample consisted of juveniles born around 1997 and attending public school in the German-speaking cantons of Switzerland. In addition to an unconditional 10 Swiss Francs (CHF) ($\approx \text{€}10 \approx \text{US\$}11$) incentive, selected panelists who had either responded late or not at all to the previous year’s panel study were promised an additional cash incentive of CHF 10 or CHF 20 if they completed the survey within a period of seven days. The trial examined how this targeted “Early Bird” monetary incentive (EBMI) affected response rates and waiting times when the amount was equal to or twice the prepayment. Additionally, the trial explored why there is an effect of these incentives. It is assumed that the positive effect is based on the target person’s preference for strong reciprocity. First, the combination of postpaid and prepaid incentives both prompted a faster response and increased the response rate of the treated panelists. Second, despite the provision of an EBMI, relatively few non-participants from the last wave could be persuaded to take part in the current study. Finally, the effect of the incentives was mediated by the panelists’ preference for reciprocity.

Keywords: prepayment, time-limited postpaid incentives, reciprocity, survey participation, panel



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Providing prepaid monetary incentives for target samples is standard in panel surveys (Pforr et al., 2015; Wong, 2020). Such incentives help to counteract the declining response rate in panel studies and the resulting selection bias of samples, and to shorten the fieldwork period in order to save time and costs, and are used in addition to other measures of survey management, such as cover letters, sequential mixed modes (combination of self- and interviewer-administered survey modes), push-to-web strategies, responsive survey designs, and reminders (Becker, 2022a; Becker, 2023a; Friedel et al., 2023; McGonagle et al., 2023; McGonagle, 2020; Calderwood et al., 2023). This is also the case for web surveys in longitudinal studies (Becker et al., 2019; Bianchi & Biffignandi, 2019; Sánchez-Fernández et al., 2010; Witte et al., 2023). The bulk of these studies report that “money in hand” is the most effective and efficient strategy, among other material incentives and conditional incentives, for boosting the rate and speed of response, regardless of the survey mode (e.g., Becker et al., 2019; Pforr, 2016; Singer & Yen, 2013; Wong, 2020). In spite of monetary prepayment, it is often observed in longitudinal studies that panelists tend to procrastinate regarding the completion of the questionnaire (Becker, 2024; Wong, 2020). Furthermore, it is costly for survey managers to motivate hard-to-reach and high-effort target persons for survey participation (McGonagle et al., 2023; Calderwood et al., 2023). Additionally, temporary or complete panel attrition also occurs regardless of the prepaid monetary incentive (Lugtig, 2014; Zagorsky & Rhoton, 2008).

Since the effect of incentives on the response rate persists in each of the subsequent waves (Wong, 2020), it is possible—by taking account of the experiences of previous surveys and the target persons’ data already collected—to adapt the incentivization to these panelists (Witte et al., 2023). Therefore, *targeted adaptive survey designs* tailored to different high-effort and hard-to-reach respondent groups are increasingly used in survey management for panel studies (Cabrera-Álvarez & Lynn, 2025; Lynn, 2017; McGonagle et al., 2023; Tourangeau et al., 2017). In particular, some studies have recently applied the combination of unconditional money in hand with conditional monetary incentives—which are offered for a few days or weeks early in the fieldwork period (Calderwood et al., 2023; Friedel et al., 2023; Witte et al., 2023). If the promised incentive—combined with a prepaid monetary incentive—is conditional on a time-limited offer, this appears to have a triggering effect on the target person’s propensity to participate in the survey (Cabrera-Álvarez & Lynn, 2025). In the context of long-running panel studies, a *time-limited postpaid monetary incentive*—a conditional incentive that is provided if the target persons complete a questionnaire prior to a tar-

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geted date—could be useful as a targeted approach to persuade those high-effort sample members who have a low propensity to provide a timely response to the researchers’ invitation to participate in a survey (Brown & Calderwood, 2014; Fomby et al., 2017; Friedel et al., 2023). Offering time-limited incentives—generous in their amount, compared to regular prepaid incentives—to low-propensity cases early in the fieldwork might result in decreased risk of unit nonresponse, in reduced nonresponse bias, and in lower survey costs (Cabrera-Álvarez & Lynn, 2025; Coopersmith et al., 2016; McGonagle et al., 2023). Indeed, several studies report that such “early bird” monetary incentives (EBMIs) successfully induce high-effort respondents to take part in surveys (Calderwood et al., 2020; Felderer et al., 2018; McGonagle et al., 2023; Wong, 2020).

Generally speaking, little is currently known about the effects of prepaid incentives combined with time-limited conditional postpaid incentives on the propensity of respondents, who differ in their past willingness to participate and their latency, to participate in a mature web-administered panel survey (Cabrera-Álvarez & Lynn, 2025; Witte et al., 2023; Wong, 2020). Temporary drop-outs and increasing panel attrition across waves are inconvenient (Lugtig, 2014), as they result in substantial losses in time- and event-related information on trajectories. Therefore, it is important to discover if EBMIs result in an increased likelihood of survey participation for low-propensity cases within a panelized sample (Brown & Calderwood, 2014; Fomby et al., 2017; McGonagle, 2020). In addition to the response rate, the latency until the start of participation is also of interest regarding panelists who have shown survey fatigue in previous panel waves. Does the time-limited incentive encourage the swift response of previously reluctant respondents and nonrespondents? Does such an EBMI significantly shorten the latencies of panelists who tend to postpone their response, measured by the time interval between the invitation and the respondents’ completion of the online questionnaire? Furthermore, decreasing sample sizes, unit nonresponse, and panel attrition often result in selection bias in the remaining sample, which leads to inefficient estimation and biased findings (Cabrera-Álvarez & Lynn, 2025; Lugtig, 2014). Missing data (where this phenomenon is not random) undermines the potential for the theory-driven reconstruction of complex trajectories (Blossfeld et al., 2019). Therefore, there is a need to reveal empirically whether EBMIs result in distorted sample composition in the case of a long-running panel, as well as whether EBMIs bring back panelists who were already thought to be lost in a previous wave.

Finally, it is a common result that panelists targeted by a prepaid incentive responded earlier in the field period than those targeted with a postpaid incentive (Becker et al., 2019; Wiant et al., 2018; Wong, 2020). According to LeClere et al. (2012), time-limited incentives differ from standard monetary incentives in that they require respondents’ swift participation and are intended to increase panelists’ survey response rate over a short time horizon. Some studies have

found that early bird incentives increased response during the time-limited period but did not increase response rates overall while other studies provide results showing an increase in the overall response rate (e.g., Calderwood et al., 2023; Friedel et al., 2023; McGonagle et al., 2023; Wong, 2020). Why does this type of targeted adaptive survey design result in the targeted persons' enhanced propensity to respond, in terms of a decreased time interval until completion of the questionnaire?

This panel study seeks to contribute to recent survey research by answering these questions regarding the effectiveness of EBMI in terms of increasing response rates and latencies until response. The initial probability-based sample of the panel study, that has been running since 2012, consisted of juveniles born around 1997 who grew up in the German-speaking part of Switzerland (Becker et al., 2020). To this end, a randomized trial was conducted on the effect of EBMI in Wave 10 (spring 2022). In this most recent wave, two randomly assigned sub-samples of high-effort and hard-to-reach panelists were promised CHF 10 or CHF 20 (EBMI) if they responded within seven days. Conditional incentives are then given to respondents after participation. It has to be noticed that in regard to incentivization, in this panel study, prepaid incentives are given in advance to all those who are contacted, irrespective of whether they participate in the survey. The respondents have received a prepaid monetary incentive (10 Swiss Francs (CHF) $\approx$ €10 $\approx$ US\$11) from Wave 7 onwards.

The present paper seeks to investigate the following: whether the offer of extra time-limited incentives, like an early bird bonus, works for a panelized sample of young respondents in terms of an overall increase in the response rate; whether a decreased latency of reply is achieved; whether an EBMI causes previously reluctant panelists and nonrespondents to participate in the most recent survey; and whether individuals' preference for reciprocity is a social mechanism that explains the EBMI effect.

Theoretical Considerations

A survey's methodology is key to optimizing the response rate (Olson, 2005). For this reason, a few studies have analyzed whether *targeted adaptive survey designs* that have a special emphasis on providing extra time-limited incentives (additional to the usual prepayment), in exchange for completing the questionnaire, contribute significantly to an enhanced response rate for a sample of targeted persons (e.g., Calderwood et al., 2023; Fomby et al., 2017; McGonagle et al., 2023; Wong, 2020). In a recent study, to give an example, Witte et al. (2023) report that the highest response probabilities among migrants were observed for combined unconditional and conditional monetary incentives. In general, these studies report how the incentives work—namely, they result in a statistically significant

increase in response rates induced by generous EBMI (e.g., Cabrera-Álvarez & Lynn, 2025; Friedel et al., 2023; McGonagle et al., 2023; Calderwood et al., 2023). Other studies also provide evidence of decreased latency resulting from EBMI (McGonagle, 2020), with no negative side effects on the final response rate after the deadline expires, on nonresponse errors, or on the survey cost (Coopersmith et al., 2016).

Despite these encouraging examples of the use of EBMI, the question of why such an incentive has a positive effect on the level of response rate and the speed of response is still unanswered (Singer & Ye, 2013). Identifying the social mechanisms that explain the positive effect of such incentives “can strengthen causal inferences by showing why, acting as they do, individuals bring about the social outcomes they do” (Hedström, 2009, p. 319).¹ In principle, a well-designed experiment makes it plausible whether a treatment has an impact on an outcome such as taking part in a survey, even if there is no theoretical idea about the mechanism involved. However, “the information obtained from the experiment will have the status of a nonexplanatory causal claim” (Hedström, 2009, p. 324). How can this non-trivial problem be overcome? Overall, mechanism-based explanations are more parsimonious, precise, and intelligible than ad hoc assumptions or correlation-based explanations.

Ad hoc assumptions are used in indirect tests of the social mechanisms (Hedström, 2009) that are assumed to bring about the effects of incentives (e.g., McGonagle et al., 2023). Usually, this starts with a lexical listing of approaches that seek to explain individuals’ propensity for taking part in surveys. For example: “(...) as the *leverage-salience theory* (Groves et al., 2000) would expect”. Some analyses apply one or other type of rational choice theory, such as the *theory of subjective expected utility* (Diekmann, 2022) to explain invitees’ decisions on whether or not to participate in a survey (Singer, 2011). These analyses argue that cost-benefit optimization is the basis for the decision and that an invitee’s evaluation of the cost and benefit (weighted by the subjective probability of successful participation) of alternative options is the social mechanism behind the decision (Singer & Ye, 2013). Since an individual will choose to take part if the benefit (e.g., social approval or benefit in terms of well-being and pleasure) outweighs the cost (relating to wasting their time, the invasion of privacy, and anticipated effort, such as giving the survey their attention, the process of deliberation, and

¹ According to Sørensen (1998, p. 240), a social mechanism “is an account of how change in some variable is brought about—a conceptualization of what ‘goes into’ a process”. According to Hedström (2009, p. 322), “a social mechanism is a precise, abstract, and action-based explanation which shows how the occurrence of a triggering event regularly generates the type of outcome to be explained”. A social mechanism connects an explanans (e.g. treatment) with the explanandum (e.g. survey participation) and tell us how the effect of the treatment on the decision to take part at the survey is generated. By shining a light on the black box—an “explanation” without any consideration of the social mechanism—it is possible to understand why an incentive usually leads to survey participation.

writing down their survey responses), it is argued that a prepaid incentive should influence individuals' subjective calculations in favor of survey participation by increasing the expected benefit and decreasing the expected cost. In the case of a conditional incentive, as outlined by Witte et al. (2023, p. 770), on such a view, an interviewee will participate if the expected costs of participation are lower than the promised incentive and additional benefits, and "higher promised incentives increase the number of individuals who deem benefits adequate to outweigh the costs of participation because opportunity costs and 'reservation incentives' are unevenly distributed".

However, it can be assumed that the costs and benefits of survey participation balance each other out. According to the *theory of bounded rationality* (Simon, 1990), which is another type of rational choice approach, many individuals who are invited to participate in an interview are rather indecisive concerning responding to a survey because they are unable to assess the costs and benefits accurately because they are not familiar with the social situation of an interview. Prepaid and conditional monetary incentives can be useful because they fuel respondents' extrinsic motivation, in addition to their intrinsic motivation, to a threshold level where that motivation surpasses their indecisiveness (Becker et al., 2019). It is plausible that the greater the cash incentive, the more likely respondents—who are still uncertain after receiving the unconditional cash—are to take part in a survey. If such respondents receive unconditional cash, it would also be rational for them to take the money but refuse to complete the questionnaire. If they are promised an amount of money if they complete the questionnaire, they cannot rule out the possibility that they will not receive the money at all (Becker et al., 2019). A prepaid incentive they have previously received could reduce their uncertainty about whether they will receive the conditional time-limited incentive (Dillman, 2000). After the deadline expires, however, they may no longer be willing to fill out the questionnaire. Overall, as already mentioned above, previous research has not tested the theoretical argument of rational choice theory directly or revealed the assumed social mechanisms empirically.

In sum, based on rational choice theory, the following two hypotheses are deduced. First, when panelists receive the EBMI, they are more likely to take part in the survey on time, compared to the case where panelists are provided with just a prepaid monetary incentive (Hypothesis 1). Compared to a less generous EBMI treatment (10+10 CHF), the response rate during the period up until the EBMI expires is higher when the EBMI treatment is more generous (10+20 CHF) (Hypothesis 2).

The *economics of strong reciprocity* (Fehr & Fischbacher, 2005) provides an alternative social mechanism—interviewees' reciprocal preferences—to explain the impact of incentives on survey participation. This mechanism-based explanation provides a complete explanation (Becker, 2023b). According to this approach, individuals' participation in a survey depends on their social prefer-

ences in regard to reciprocity (Fehr & Fischbacher, 2002, 2005). If the researchers’ unconditional prepayment is interpreted by respondents as a kind act of cooperation, they will expect to optimize their own and the researchers’ payoffs by engaging in a positive response to the donors’ request that they participate in the survey (Fehr et al., 2002). As a result, provided that the respondents have a strong preference for direct positive reciprocity, survey participation on the part of recipients is likely since the interviewee is interested in receiving a fair share of the common payoff.

As mentioned above, an adaptive survey design is used to motivate nonrespondents and reluctant respondents to take part in a survey by offering them a temporary increased postpayment. This EBMI strategy might be useful if the reciprocal mechanism of “tit-for-tat” (Axelrod, 1984)—that is, the preference for direct positive reciprocity—does not seem to work. Combined with the prepaid incentive, the EBMI might signal to interviewees that their cooperation is both essential and urgently required (McGonagle, 2020). It could then be assumed that high-effort target persons will interpret the EBMI, and a generous amount in particular, as a kind act on the part of the survey manager—that is, as constituting their appreciation for the individual completing the questionnaire on time—resulting in their increased reciprocal preference. Even when they miss the EBMI deadline, altruistic reciprocity, in terms of the target person’s interest in supporting the researchers’ scientific work—initiated by the special request that they participate in the survey—will result in an enhanced response propensity. The efforts of the survey manager are recognized by the respondents’ fair share of the common payoff, even when the target persons do not receive the promised cash since they have already received a gift. The stronger their preference for altruistic reciprocity, therefore, the less they will be discouraged, disappointed, or frustrated when the time-limited incentive expires—and the more likely they will be to cooperate.

Based on this background regarding the theoretical approach to reciprocity as a significant mechanism, it is assumed that individuals’ propensity for reciprocity results in survey participation provided that the incentives induce their reciprocal preference. In general, the effect of prepayment and an EBMI is mediated by the target person’s preference for strong reciprocity, that is, the stronger the panelists’ taste for reciprocity, the greater their response propensity (Hypothesis 3). Because no compensation is expected by nonrespondents after the EBMI deadline is missed, it is assumed that the invitees’ response propensity is more likely to depend on their strong reciprocity after the EBMI expires. The effect of strong reciprocity should be stronger for the period after the deadline than in the initial stage of the survey (Hypothesis 4).

Design of the Experiment

This randomized trial in Wave 10 has special features. First, from Wave 7 onwards, all interviewees received a prepaid incentive of CHF 10 in cash (Becker, 2023b; Becker et al., 2019). In this respect, unconditional prepayment is a baseline incentive for each of the interviewees. Second, based on their participation pattern in the prior wave (in particular, the timing of the target person’s response during the previous Wave 9), three categories of panelists (experimental groups) are distinguished (Table 1). The sample includes a group of panelists who do not require special treatment due to their loyalty to the panel study, as well as their constant propensity to respond swiftly within a week of the invitation (Category I). This overall reference group consists of panelists who completed the questionnaire within seven days during Wave 9. In regard to the EBMI, these 1,297 panelists are excluded from the experiment. Two other groups have to be treated with EBMI. The first group that responded in Wave 9 after one week and after receiving multiple reminders has been identified as a subgroup of high-effort respondents at risk of nonresponse based on their prior wave fieldwork effort (Category II). The second subgroup comprises nonrespondents in Wave 9 at risk of permanent panel attrition (Category III). This hard-to-reach subgroup of high-effort panelists who are assumed to be reluctant panelists showing survey fatigue should be converted to participants in order to overcome the risk that singular panel attrition becomes final dropout from the panel study (Lugtig, 2014).

Table 1
*Experimental Design in Wave 10**

	Category I	Category II	Category III
	Wave 9 ≤ 7 days	Wave 9 > 7 days	Nonresponse in Wave 9
Reference group (n = 1.297)	CHF 10 prepaid		
Control groups (n = 329)		CHF 10 prepaid	CHF 10 prepaid
Treatment groups (n = 643)			
Treatment I (n = 322)		CHF 10 prepaid + 10 CHF promised	CHF 10 prepaid + CHF 10 promised
Treatment II (n = 321)		CHF 10 prepaid + 20 CHF promised	CHF 10 prepaid + CHF 20 promised

Note. The residual category of panelists (n = 48) with an unknown postal address will not be considered in the analysis.

Third, both groups consist of the randomly assigned control groups (329 panelists) and treatment groups (643 panelists). In Wave 10, the treatments were applied in the following way. In all three categories, the reference and control groups received CHF 10 as an unconditional prepaid incentive enclosed in the invitation letter. Additionally, the first treatment groups (Treatment I) in Category II and Category III were promised an additional CHF 10 provided they completed the questionnaire within seven days of initial contact (EBMI). Since it is assumed that the value of the EBMI makes a difference (Cabrera-Álvarez and Lynn, 2025), interviewees in the second treatment group of Category II and Category III were promised CHF 20 as a time-limited postpaid incentive additional to the prepayment of CHF 10 if they completed the questionnaire within a week (Treatment II).

Each interviewee received the prepaid unconditional monetary incentive (CHF 10) at the start of the fieldwork with the invitation letter sent by the regular postal mail. The panelists in the treatment groups received an equal or larger postpaid promised incentive (CHF 10 or CHF 20) by postal mail if they responded by the fixed deadline. Since the prepayment has been standard for several waves, the individuals in Category II and III that were not promised the EBMI are the control groups. Within Category II and III, the panelists were randomly assigned to one of three different groups, which determined whether they were conditionally offered CHF 10, CHF 20, or nothing for responding in time.

The prepayment was enclosed with a personalized invitation letter sent via regular postal mail on Friday, April 29, 2022. In this letter, the incentives were described, and it was explained to the interviewees in the treatment groups that they would earn the EBMI when they completed the online questionnaire, up until May 7, 2022. Using the first-class postage option offered by Swiss Post, it was guaranteed that eligible target persons would receive this letter the day after it was sent out, that is, on April 30, 2022. This letter gave the panelists the URL for the survey website and the password for accessing the web-based questionnaire. In regard to the reputation of the researchers and the sponsor, the panelists were informed that the panel study had received a grant from a governmental agency and that it was being conducted by researchers at a cantonal university. One day later, the panelists received—via a personalized email—the clickable URL and a password to log on to the website (the researchers had the panelists’ email addresses). If they did not start completing the questionnaire after four days, they received personalized reminders via text message, with a link to the online survey.

Data, Variables, and Statistical Procedures

Data

The data analysis is based on paradata collected during the fieldwork period of Wave 10 of the established multi-wave DAB panel study, which has been running since 2012 (Becker et al., 2020). The online survey was conducted between April 29, 2022, and May 24, 2022. The paradata on the exact time references of the completion of the self-administered questionnaire are linked to information on the eligible panelists. The panel study is concerned with the determinants of educational and occupational choice, with training and job opportunities, as well as with the structure and dynamics of educational and occupational trajectories of young adults in the German-speaking cantons of Switzerland.

The initial target population consisted of eighth graders enrolled in regular classes in public schools during the 2011/12 school year. The panel data were based on a random and 10% stratified gross sample of 296 school classes, out of a total universe of 3,045 classes. After contacting the headmasters and the teachers, 215 out of 296 school classes were ready to participate in the online survey in the first wave (Glauser, 2015).

Since 2012, 10 panel waves have been realized. The average response rate is about 80% for each of the waves (Becker et al., 2019; Becker et al., 2020). In Wave 1, the gross sample consisted of 3,815 individuals, which decreased to 2,317 eligible panelists contacted in Wave 10 (Becker et al., 2022). About 72% of panelists who responded to the online survey in the fieldwork period of 26 days after survey launch were considered for empirical analysis.

Dependent and Independent Variables

The dependent variable is a panelist's survey response (i.e., completion of the online questionnaire). The response rate (RR1) is defined as the ratio of eligible units and their response in terms of starting and completing the online questionnaire (AAPOR, 2016, p. 61). Due to the exact time references for the survey launch and the completion of the questionnaire, it is possible to analyze the latency until a panelist's participation as an indicator of the speed of response initiated by the incentives (Becker, 2024).

Besides the treatments already discussed above, the panelists' preference for strong reciprocity—measured in Wave 8—is an important independent variable (Becker, 2023b). Two types of reciprocity are distinguished. Direct positive reciprocity is measured using the following item: "If someone does me a favor, I am ready to return it". Altruistic reciprocity is indicated by this item: "I am willing to go to great lengths and expense to help people who once helped me". The possible answer ranges from 1 for "Not true at all" to 5 for "Completely true". To distinguish between strong and weak or no reciprocity, in line with the eco-

nomics of strong reciprocity, these variables are converted into binary dummy variables, with the discrete values of “4” and “5” recoded as 1. This value indicates interviewees’ strong reciprocity. The other values become zero (weak or no reciprocity). It cannot be ruled out that those target persons who already have a low or no preference for reciprocity did not participate in Wave 8. Logically, these 277 panelists could not be asked about their reciprocity in this survey. Therefore, it is assumed that these nonrespondents from Wave 8 have a low or no preference for reciprocity. They are included in the multivariate analysis by imputing the missing information on their reciprocal preference by the value zero (Becker, 2023b). Thereafter, both indicators of reciprocity are combined with the following outcomes for reciprocal preference: (0) no or low reciprocity, (1) direct positive and/or altruistic reciprocity.

Different sociodemographic characteristics of the panelists are considered, such as their gender, social class origin, education, and language proficiency. Since there are gender-specific differences in response rates, gender is considered as a dummy variable for female target persons; men are the reference group (Becker, 2022b). Social class origin, correlating with access to the Internet, computer skills, and openness to scientific surveys, is indicated by the class scheme suggested by Erikson and Goldthorpe (1992), with the most privileged upper service class as the reference category. The education and language proficiency of a target person correlates positively with survey response rates (Groves & Couper, 1998; Wenz et al., 2021). Education is also positively correlated with appreciation of the utility of social-scientific research and information-gathering activities, as well as with digital affinity, computer literacy, and language skills (Herzing & Blom, 2019). The education of panelists is measured by the type of school in which they were enrolled at the end of their compulsory schooling—such as lower secondary schools with basic requirements, lower secondary schools with intermediate requirements and pre-gymnasiums, including schools with advanced requirements, and other miscellaneous school types, such as integrative schools without selection (Glauser, 2015). A panelist’s language proficiency is indicated by their standardized grade point average (GPA) in the German language class.

Statistical Procedures

Since the invited target person’s decision regarding survey participation is based on their ‘free will’ (Blossfeld, 1996), they can therefore choose their own time to respond to the request (Groves & Couper, 1998). As has been observed by social researchers, survey participation is the result of individuals’ decisions and can occur at any point in time (Becker, 2024). Survey participation therefore has to be modeled as a time-continuous, discrete-state stochastic process. Since the survey response is a stochastic event that develops over time (Aalen et al., 2008)

and can occur at any point in time after the survey launch and across different survey modes, and since preference for strong reciprocity declines over the length of the fieldwork period (Becker, 2023b), the techniques and statistical procedures of event history analysis are utilized (Aalen et al., 2008; Blossfeld et al., 2026). Event history analysis involves using statistical methods for analyzing stochastic processes with discrete states, such as survey response, and continuous time, such as the time that elapses in the fieldwork after survey launch (Blossfeld et al., 2026; Kalbfleisch & Prentice, 2002). In contrast to comparative-static estimations of survey response, these procedures are able to consider—besides the response rate at any point in time across the fieldwork period—the timing of survey response, which depends on the effect of the incentives provided, as well as the response speed (indicated by the latency of return across the time period that has elapsed since survey launch) (Becker, 2024). Besides time-constant factors (e.g., gender and social origin), time-dependent factors (such as time-limited offers of additional incentives) are suspected to influence the outcome and timing of survey participation at any time (Blossfeld & Rohwer, 1997). In sum, these event history data enable a causal analysis of survey participation (Blossfeld et al., 2026).

The parametric procedures of event history analysis model the likelihood of survey participation as a stochastic and time-variant function of individual resources and the different incentives.

This hazard rate

$$r(t) = \lim_{t' \rightarrow t} \frac{\Pr(t \leq T_y < t' | T_y \geq t)}{t' - t}$$

is defined as the marginal value of the conditional probability of such an event occurring—namely, completing the questionnaire in a web-based online survey—in the time interval $(t, t + \Delta t)$, given that this event has not occurred before time t (Blossfeld et al., 2019, p. 29). This hazard rate can be interpreted as the actors' propensity to change state, such as from nonresponse to response, dependent on incentives, such as unconditional prepayment and conditional time-limited postpayment. This propensity is defined in relation to a risk set at the moment t —that is, the set of individuals who do not respond until t , and therefore can respond since they did not respond before t (Becker, 2024).

Using this statistical procedure, it is possible to reveal the impacts of x on the probable occurrence of survey participation as the event y to be investigated: $DX_t \rightarrow D\Pr(DY_{t'})$, whereby $t < t'$, taking the timing of events into account. For the parametric modeling of response propensity, the piecewise constant rate analysis is used. The basic idea of this procedure is to split the time axis into periods (e.g., on a daily basis) and to assume that transition rates are constant in each of these intervals. Using this procedure, it is possible to describe the occur-

rence of survey participation in different phases of the field period. In our case, the latency is divided into two sub-episodes by reference to the deadline for the EBMI. According to this procedure, the impact of variables on survey participation is analyzed for the first period until the deadline (i.e., a week), on the one hand, and for the period after this date, on the other, by estimating a piecewise constant exponential model (Blossfeld et al., 2025, p. 81). Given both time periods, the transition rate from a given origin state (nonresponse) to a response as a destination state k is

$$r_k(t) = \exp\{\bar{a}_t^{(k)} + A^{(k)}\alpha^{(k)}\},$$

whereby $\bar{a}_t^{(k)}$ is a constant coefficient associated with one of both time periods, $A^{(k)}$ is a vector of covariates, and $\alpha^{(k)}$ is an associated vector of time-invariant coefficients.

Furthermore, by means of non-parametrical procedures, such as the Kaplan–Meier method of estimating failure rates, the patterns of response across the time that has elapsed since the invitation to the current wave are described on the basis of relative prevalence across the fieldwork period (McGonagle et al., 2023). By calculating indices, such as the median, it is possible to estimate how long it takes for 50% of panelists in the different treatment groups to start filling out the questionnaire. To detect significant differences in time-dependent failure rates between the control group and the treatment groups, the non-parametric log-rank test is used (Blossfeld et al., 2026). This test is particularly useful when processes are censored, that is, when the time of survey participation—like in the case of nonresponse during the fieldwork period or until the deadline for the EBMI—is not known for all target persons invited to take part in the survey. The log-rank test compares the observed response rates in the different groups across the entire fieldwork period. If there is no significant difference between the groups, then survey participation in the groups is random, regardless of group affiliation.

Empirical Results

Survey Participation and the Timing of Response

As a first step, the results of the randomized trial are described (Table 2). The response rate of 90% was highest for the overall reference group, consisting of highly motivated and loyal panelists (Category I). As theoretically expected, the response rates were higher for the treatment groups compared to the respective control groups. For those who needed more than a week to take part in the previ-

ous survey, it was observed that about 69% of the control group took part, while the response rate was about 74% for the group that was promised CHF 10 as an EBMI (Treatment I in Category II). In the case of the second treatment, the EBMI of CHF 20 (Treatment II in Category II), the response rate was about 83%.² T-tests provide significant differences (at a 5% level at least) between these groups.

The same findings were revealed for Category III. While about 17% of members of the control group took part in the survey, the response rate was about 19% for the first EBMI treatment and 24% for the second EBMI treatment. In line with Hypotheses 1 and 2, the EBMI obviously worked in the expected way, confirming that the greater the conditionally promised monetary incentive and the greater the postpaid monetary incentive, the higher the response rate for reluctant and nonresponding persons. This finding is in line with findings in previous studies (e.g., Cabrera-Álvarez & Lynn, 2025).

The conclusion changes if the timing of the response is considered. The median latency of the overall reference group was four days, that is, this was the amount of time that elapsed since the survey launch until 50% of the invited persons had completed the questionnaire. Those panelists who responded early during the previous Wave 9 showed the same behavior in the most recent survey. Within the group of panelists who participated in the previous survey after a week of fieldwork (Category II), the control group's median latency was 13 days. The latency of the treatment groups—proved by the log-rank test of survival analysis—was significantly shorter. The first EBMI treatment group in Category II, which was promised an additional CHF 10, met this benchmark after six days of fieldwork, while for the second EBMI treatment group, which promised an additional CHF 20, it took eight days.

By the EBMI deadline, seven days after the launch of the survey (“RR deadline” in the last column in Table 2), 75% of invited participants in the overall reference group had completed the questionnaire. In Category II, the response rate by this deadline was 36% for the control group, but 61% for the first EBMI treatment and 59% for the second EBMI treatment. The log-rank test tells us that the differences between the groups are significant. For the third category of previous nonrespondents, there is no median latency of survey participation, as less than

² Who belongs to the randomized control and treatment groups? If the likelihood of inclusion in these groups is estimated by a multinomial logistic regression, it is found that the higher panelists' educational level and achievement, the more likely they are to be in the overall reference group (Table A1 in the Appendix). The lower the panelists' educational level, the more likely is their nonresponse or procrastination in respect of survey participation. In other words, low educational achievement correlates with the categorization of high-effort panelists. The social selectivity in terms of gender and social origin is negligible. The residual group of 48 panelists with an unknown postal address has been excluded from the empirical analysis. Overall, this result indicates that the randomization was rather successful.

Table 2

Summary of the Results of the Randomized Trial

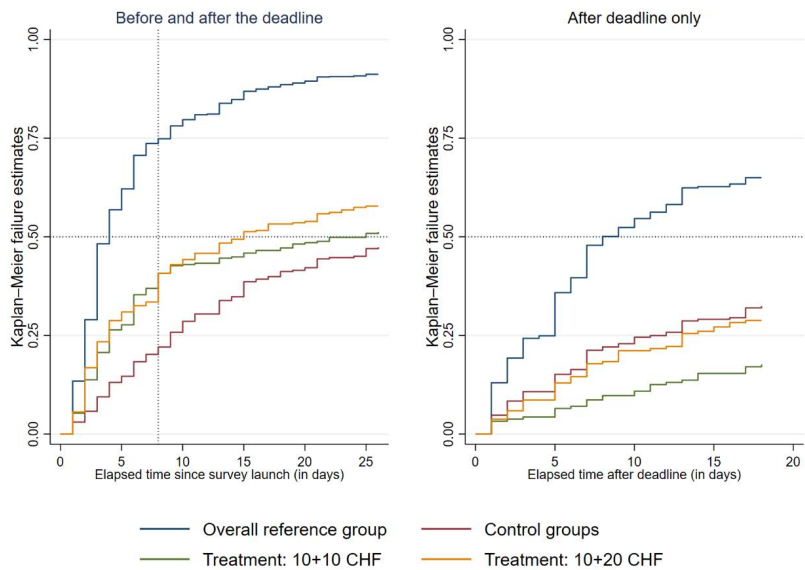
	N of cases	#Responses	RR ^a	t-test of RR	Latency ^b	Log-rank test of latency	RR dead-line ^c	Log-rank test of RR deadline
Category I								
Reference group: Wave 9 ≤ 7 days (CHF 10)	1,297	1,173	90.4%		4 days		75.1%	
Category II								
Control group: Wave 9 > 7 days (CHF 10)	186	128	68.8%		13 days		36.0%	
Treatment I (Wave 9 > 7 days): CHF 10+10	182	135	74.2%	$t = -1.22$	6 days	$x^2 = 7.69^{**}$	61.3%	$x^2 = 22.91^{***}$
Treatment II (Wave 9 > 7 days): CHF 10+20	181	150	82.9%	$t = -3.17^{***}$	8 days	$x^2 = 15.93^{***}$	58.6%	$x^2 = 18.65^{***}$
Total	549	413	75.2%					
Category III								
Control group: Nonresponse Wave 9 (CHF 10)	143	24	16.7%		no median		8.7%	
Treatment I (Nonresponse): CHF 10+10	140	26	18.6%	$t = -0.42$	no median	$x^2 = 0.32$	17.3%	$x^2 = 4.90^*$
Treatment II (Nonresponse): CHF 10+20	140	33	23.6%	$t = -1.42$	no median	$x^2 = 2.64$	20.7%	$x^2 = 9.63^{**}$
Total	423	83	19.6%					
Summary for categories II and III								
Control group	329	152	46.2%		25 days		23.7%	
Treatment I: CHF 10+10	322	161	50.3%	$t = -1.04$	18 days	$x^2 = 4.03^*$	42.2%	$x^2 = 22.32^{***}$
Treatment II: CHF 10+20	321	183	57.0%	$t = -2.76^{**}$	13 days	$x^2 = 12.39^{***}$	42.1%	$x^2 = 23.37^{***}$
No postal address	48	1	2.1%		not estimable		2.1%	
Total	2,317	1,670	72.2%					

^a Response rate. ^b Median. ^c Completed during time-limited incentive period of seven days. ^{*} $p < .05$; ^{**} $p < .01$; ^{***} $p < .001$.

50% participated in the survey. Nonetheless, in this category, the response rate was higher for the treatment groups (17% or 21%) compared to the control group (9%). According to the significant results of the log-rank test, this finding is in line with Hypothesis 1, which suggests that an EBMI results in earlier participation and a higher propensity to complete the questionnaire compared to members of the control group who only receive a prepaid monetary incentive.

Figure 1

Kaplan–Meier Failure Estimates



Note. Comparison of control and treatment groups in regard to EBMI deadline (dotted vertical line: deadline; dotted horizontal line: median latency).

Overall, the offer of a conditional postpaid cash incentive additional to the unconditional prepayment seems to be a useful strategy to motivate panelists with a low response propensity to take part in the initial stage of the survey. Around 40% of the panelists who only took part in the previous survey after a week were encouraged to respond within a week. Almost 13% of the nonrespondents who received the EBMI could be interviewed, while around 20% of the panelists nonresponding in the previous Wave 9 took part in the current survey independent of the type of incentive received. Overall, in line with previous studies, if the response rates for both of these groups—and their speed of reply—are considered, this finding might speak in favor of a prepaid incentive and EBMI.

This conclusion is supported empirically if the trajectory of survey participation is considered for the treatment independent of participants’ response pattern in the previous Wave 9 (Figure 1). In the period until the EBMI deadline, the development of the response rate is rather similar for both different treatments, while it differs significantly compared to the development of the control groups’ response rates. This development has also been observed in other studies (e.g., Cabrera-Álvarez & Lynn, 2025).

For the fieldwork period after the deadline, it is found—in line with Hypothesis 2—that the response rates for the generous EBMI treatment are higher than for the less generous treatment at each of the points in time. While the time-related differences between the control groups and the more generous prepaid treatment group is insignificant according to the log-rank test, the response rate of the less generous treatment group is significantly lower than for the control groups. There appears to be no effect whereby regret results in particularly low response rates for those who missed the EBMI (e.g., Calderwood et al., 2023).

The Role of the Deadline and the Preference for Reciprocity in Survey Participation

To analyze how an EBMI influences the willingness to participate before and after the deadline for postpayment, the fieldwork period was split into two time periods: before and after the EBMI deadline. The effects of incentives and the role of a preference for reciprocity are estimated separately for the different treatment groups only (Table 3). The estimated results are discussed step by step for the different categories of variables—such as time dimension, treatments, and reciprocal preference—for the time period until the offer of an EBMI expires (deadline) as well as for the stage after the deadline. It has to be kept in mind that the estimates of interest are controlled for panelists’ characteristics, such as gender, social origin, education, and language proficiency. The complete models are documented in Table A3 in the Appendix.³

Since individuals’ sense of reciprocity is universal, we start the discussion of results for both treatment groups of panelists being reluctant and nonresponding in the previous wave (Category II and III) together. First, in the initial stage of the survey, there are interesting indications that the propensity to respond depends on the time that remains for respondents to receive the EBMI. Obviously, there is an effect of the approaching deadline on panelists’ propensity to

³ Due to the episode splitting on a daily base, there is a discrepancy between the number of events until the deadline and the remaining risk sample after the deadline because the deadline was set at noon. This methodological issue arises when a coarser time dimension is chosen for episode splitting than the actual time dimension for events on an hourly base (for details: Blossfeld, 2025). Obviously, this was the case in three instances, although this small number of cases should not affect the model estimates.

take part in the survey. The closer the deadline, the more likely they are to take part in the survey. However, the transition rate—that is, the individuals' propensity to participate in the survey—increases by about $[(\exp(0.053) - 1) * 100\% =]$ 5% in this time interval of seven days. After the EBMI deadline, a decreasing propensity to participate is observed. This timing effect is statistically significant only if both treatment groups are considered at the same time. Their likelihood of taking part decreases by about 4% across the rest of the fieldwork period.

Second, the effect of different EBMI on the response propensity is analyzed. The previous finding is replicated for the initial stage of fieldwork until the EBMI deadline. For both treatment groups—the reluctant and nonresponding panelists—it is found that the EBMI enhances the response rate and reduces the latency of the response. In contrast to other studies (e.g., Cabrera-Álvarez & Lynn, 2025), the effect of the amount of the EBMI is not different for these treatment groups. However, the results are different for the treatment groups. While the less generous EBMI has a significantly greater effect on the response propensity of the reluctant respondents, the impact of the more generous EBMI on their willingness to take part in the survey in time is more significant for the nonrespondents. For the latter treatment groups, the smaller EBMI increases the individuals' propensity to participate in the survey by $[(\exp(0.759) - 1) * 100\% =]$ 136%, but the larger EBMI does so by $[(\exp(1.0) - 1) * 100\% =]$ 172% in this time interval. In the period after the deadline there is a negative effect of the less generous EBMI on the likelihood of survey participation while for the more generous EBMI there are no significant differences among the treated subgroups compared to their control group. This result is valid for the reluctant interviewees, in particular, while for the nonrespondents the effect is just significant at the 10% level. In other words, the less generous EBMI is counterproductive for those who missed EBMI. The reason for this surprising result should be investigated in future research.

Third, if the role of respondents' reciprocal preference is considered for the period until the EBMI deadline, it is found that the effects of the incentives are mediated by individuals' strong taste for reciprocity. This reciprocal effect is much larger for the nonrespondents than for the reluctant respondents. Overall, Hypothesis 3 is supported empirically: the stronger the panelists' reciprocal preference, the greater their response propensity in the initial stage of the survey.⁴ After the deadline for the EBMI, the correlation of the panelists' reciprocal preference with survey participation is rather strong and significant. When the EBMI expires, the incentive effects seem to be mediated by strong reciprocity.

⁴ The estimation is reanalyzed for the overall reference group (Table A2 in the Appendix). These panelists' reciprocal preference in regard to the usual prepayment dominates their response propensity in the stage after survey launch. In the initial stage, the effect of reciprocal preference is smaller than in later stages of the fieldwork period. For the entire period, reciprocity triggered by the prepayment significantly influences the likelihood of taking part in the survey.

Table 3

Effects of Different Incentives on Survey Participation – The Role of the Deadline

Categories	II and III		II		III	
	Until deadline	After deadline	Until deadline	After deadline	Until deadline	After deadline
Survey stages	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)
Estimate						
Time dimension						
Time: left to deadline	0.053*** (0.019)		0.023 (0.020)		0.103** (0.047)	
Time: passed since deadline		-0.036* (0.018)		-0.023 (0.020)		-0.007 (0.041)
Category II: Wave 9 > 7 days						
Cat. II: Control group			0	0		
Cat. II: Treatment I: CHF 10+10			0.586*** (0.147)	-0.645* (0.264)		
Cat. II: Treatment II: CHF 10+20			0.569*** (0.148)	0.102 (0.219)		
Category III: Nonresponse						
Cat. III: Control group:					0	0
Cat. III: Treatment I: CHF 10+10					0.759* (0.376)	-0.974 (0.534)
Cat. III: Treatment II: CHF 10+20					1.000** (0.361)	-0.711 (0.495)
Categories II and III						
Control groups	0	0				
Treatment I: CHF 10+10	0.537*** (0.136)	-0.780*** (0.235)				
Treatment II: CHF 10+20	0.573*** (0.135)	-0.125 (0.193)				
Reciprocity						
Low or no reciprocal preference	0	0	0	0	0	0
Strong reciprocity	1.246*** (0.164)	1.827*** (0.276)	0.420* (0.212)	0.880* (0.355)	0.994*** (0.291)	1.585*** (0.477)
Intercept	-4.654*** (0.250)	-4.999*** (0.378)	-3.458*** (0.296)	-4.068*** (0.488)	-5.569*** (0.573)	-5.368*** (0.758)
Number of cases	970	597	548	245	422	352
Number of events	370	140	310	115	60	25
Number of sub-episodes	7,560	8,562	3,744	2,933	3,816	5,629
LR χ^2 (d.f.)	147.2 (15)***	108.7 (15)***	38.0 (15)***	33.5 (15)**	39.5 (15)***	29.0 (14)*

Note: Piecewise constant rate model with exponential distribution of hazards. β -coefficients, and standard errors (SE), controlled for panelists' characteristics such as gender, social origin, education, and language proficiency. * $p < .05$; ** $p < .01$; *** $p < .001$.

The effect of strong reciprocity is larger for the period after the deadline than for the period before the deadline. Again, the impact is larger for nonrespondents in the previous wave. Overall, Hypothesis 4 is supported empirically.⁵

Finally, it should be noted that there are no substantial social structural differences in regard to the likelihood of taking part in the survey for the different groups (Table A3 in the Appendix). However, for Wave 10 of our panel study, as for the previous waves (Table A1), it can be seen that panelists who were previously enrolled at the lower school level have the greatest risk of nonresponse and panel attrition. It has to be stressed that combining unconditional prepayment and conditional postpayment does not produce a notable sample selection bias, either in the initial phase or in the phase after the deadline for additional offers expired.

Summary and Conclusions

The aim of this randomized trial was to find an evidence-based answer (among other possible answers) in regard to if, how, and why a time-limited postpayment is an efficient instrument to initiate survey participation among high-effort target persons, such as reluctant and previously nonresponding panelists, after they have already received a prepaid monetary incentive enclosed in an invitation letter. This study analyzed whether the EBMI resulted in substantially higher response rates and a shorter latency for panelists' survey participation when the EBMI was twice as large as the prepayment. Furthermore, the study investigated how these treatments worked for different subgroups of high-effort and hard-to-reach panelists. Finally, by applying a direct test, it tested if individuals' taste for strong reciprocity—initiated by the (un)conditional incentives of different amounts—is a significant social mechanism that results in an enhanced rate and speed of response in a web survey that was embedded in a long-running panel study. Therefore, it was tested whether respondents' preference for strong reciprocity mediated the causal relationship between unconditional prepayment, the EBMI and their response propensity.

The results are straightforward, even for our special population, and are in line with recent research on this topic (e.g., Friedel et al., 2023; McGonagle et al., 2023): the early bird does indeed catch the extra worm. For the combination of unconditional cash and conditional monetary incentives in different amounts used in the panel, it was evident that the panelists of the EBMI treatment groups—namely, those who participated rather late or not at all in the previous

⁵ The reanalysis— that is, the measure of reciprocal preference without imputation—provides implausible and statistical insignificant findings which do not correspond with previous analyses on the correlation between the panelists' reciprocal preference and their survey participation (Becker, 2023b).

wave—were more likely to take part in the survey in time than the respective control groups who received only a prepaid incentive. The combination of a prepaid incentive and a conditional, more generous incentive worked in this panel study as intended by the survey manager, in that they provided short- and long-term effects on the survey participation of high-effort panelists. These gains did not come at the cost of sampling bias. In this respect, our finding is in line with other recent experimental studies on EBMI (e.g., Calderwood et al., 2023; Witte et al., 2023). The targeted combination of unconditional prepayment and conditional postpayment is an efficient instrument for increasing the rate and speed of response among high-effort panelists (McGonagle, 2020). This was true for reluctant panelists who participated late in the previous wave, and, in particular, there were significant effects regarding bringing back nonrespondents to the survey by applying EBMI. In our case, it is found that the adaptive survey design tailored to different high-effort and hard-to-reach respondent groups, in general, and the EBMI, in particular, had the ability to win back nonrespondents with a risk of permanent panel attrition.

Finally, it is revealed that the effect of incentives seems to be mediated by respondents’ preference for strong direct and altruistic reciprocity, as a social mechanism (Becker et al., 2019). The role of reciprocity has previously been revealed empirically for the effect of unconditional prepayment but not for an EBMI (Becker, 2023b). To the best of our knowledge, this is the first and only study to date that has empirically demonstrated the effect of reciprocity in the context of an adaptive survey design in which a conditional EBMI is employed additionally to unconditional prepayment. Of course, reciprocity may be one of several mechanisms resulting in enhanced response rate due to an EBMI. The search must therefore continue for other social mechanisms that produce the effect of an EBMI.

In the theoretical part, we used two different versions of rational choice theory to argue that incentives positively influence the cost–benefit calculations of target individuals. However, due to a lack of data, we were unable to show that incentives reduced the costs and increased the benefits expected by the target individuals. In this respect, we have an incomplete explanation, a black box. Furthermore, it has to be observed directly that target persons regret having missed the deadline and thus the money promised to them (see also: Calderwood et al., 2023). This could explain why the missed, but less generous, EBMI resulted—in a counterproductive way—in a decreased propensity to participate in the survey for high-effort and hard-to-reach target persons. Furthermore, it can also be assumed that experienced panelists participate in surveys out of loyalty or habit. Finally, other reasons for deciding for or against participation can also be considered. While rational action theory mostly focuses on instrumental rationality, a broader version of rational choice theory can also consider other

rationalities, such as value rationality (e.g., support of social research) or social norms (e.g., politeness).

The trial has limitations. In a theoretical respect, it has been emphasized that—beside the preference for reciprocity as a social mechanism favoring cooperative action—incentives should enhance the response propensity by modifying interviewees' cost-benefit calculation in regard to survey participation (Singer, 2011). It is still an open question whether the incentives result in the target person's increased subjective expected benefits and decreased subjective expected costs. Therefore, it is not completely clear why these EBMI designs work. In order to overcome this shortcoming, the process of panelists' cost-benefit calculation should be observed directly. A promising approach seems to be measuring target persons' views on the costs and benefits of taking part in a survey (e.g., Haunberger, 2011). When they are asked about the expected costs and benefits, or the total utility, of survey participation in a previous survey, this could reveal whether their cost-benefit calculation contributes to predicting their participation in the next survey.

In a methodological respect, one could argue that there was no true control group in our trial. Since the effect of EBMI on motivating high-effort panelists was interesting, the trial focused on them. In the previous history of the panel study, each of the panelists received an unconditional prepaid incentive (Becker et al., 2019). In order to maintain this tradition, therefore, the reference group of loyal and motivated low-effort panelists has been excluded from the analyses. Of course, the study would have to be expanded to include the different types of panel attrition outlined by Lugtig (2014). In this respect, our findings are only valid for the comparison of two panel waves. Additionally, it should be noted that there are missing values on an important explaining variable, individuals' preference for reciprocity. To avoid excluding these units from the analysis, it was assumed that they exhibited weak or no reciprocity. This assumption can, of course, be disputed. However, previous analyses yielded plausible results, so this approach was retained. Nevertheless, a reanalysis with appropriate data is necessary.

In an empirical respect, it should be noted that a special target population was considered in the trial. Therefore, there is a need to gather evidence on the effect of EBMI for other samples of panelists. Furthermore, the target sample consisted of panelized individuals who had already experienced prepaid incentives as part of previous surveys. It would be useful to replicate this adaptive survey design with people with less experience with social science surveys. Such a replication should consider larger samples for the experimental groups. In our case, small samples and a low number of events might have resulted in a lack of statistical power to detect significant effects, due to the multiple treatments.

Data and Code Availability

Scientific use files for eleven waves of the DAB panel study are stored on SWIS-SUbase (<https://www.swissubase.ch/de/catalogue/studies/10773/20941/overview>) and curated by FORS (<https://forscenter.ch/>) under reference number: 31523. These data sets are available to the research community and can be downloaded from this repository (Data Access: <https://doi.org/10.48573/2php-jh42>; reference number: 946). Each of the software applications used is part of the Stata package. The custom code can be requested from the author.

Conflict of Interest

The authors declare there are no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Ethics

Ethics approval was not required for this study as it involved non-invasive procedures and fully anonymized data collection, posing no risk to participants, in accordance with institutional guidelines of the University of Bern and national guidelines of the Swiss National Foundation (SNF). All participants provided informed consent before participating.

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Appendix

Supplementary Information on the Sample, the Effects of the Experiment, and the Characteristics of the Experimental Groups

Table A1

Estimation of Sample Composition of Experiment Groups based on Socio-Demographic Characteristics

Experimental groups	Reference group		Control groups		Treatment groups				Residual group	
	t9 ≤ 7 days	t9 > 7 days	t9 > 7 days	Nonresponse	t9 > 7 days: CHF 10+10	t9 > 7 days: CHF 10+20	Nonresponse: CHF 10+10	Nonresponse: CHF 10+20	No postal address	
Estimate	AME (SE)	AME (SE)	AME (SE)	AME (SE)	AME (SE)	AME (SE)	AME (SE)	AME (SE)	AME (SE)	
Gender (Ref.: Male)										
Female	0.107* (0.021)	-0.013 (0.012)	-0.022* (0.010)	-0.020 (0.011)	-0.028* (0.010)	-0.017 (0.011)	-0.016 (0.010)	0.008 (0.006)		
Social origin (Ref.: Upper service class)										
Lower service class	-0.005 (0.035)	0.027 (0.020)	0.004 (0.017)	0.018 (0.019)	-0.009 (0.016)	-0.024 (0.020)	0.019 (0.017)	-0.030* (0.013)		
Routine non-manual employees	0.011 (0.033)	-0.012 (0.017)	0.008 (0.016)	0.014 (0.017)	0.014 (0.017)	-0.012 (0.019)	0.001 (0.015)	-0.025 (0.013)		
Farmers, small proprietors	-0.067 (0.049)	0.062* (0.030)	0.007 (0.023)	0.012 (0.026)	0.006 (0.023)	-0.000 (0.029)	-0.007 (0.020)	-0.011 (0.019)		
Foremen, skilled manual workers	-0.039 (0.037)	0.011 (0.019)	0.028 (0.019)	0.006 (0.019)	0.001 (0.018)	-0.016 (0.021)	0.023 (0.018)	-0.014 (0.015)		
Semi- / unskilled manual workers	-0.079 (0.051)	0.010 (0.028)	-0.008 (0.021)	0.028 (0.029)	0.039 (0.028)	0.013 (0.032)	0.027 (0.024)	-0.030* (0.015)		
Missing values	-0.053 (0.040)	0.031 (0.022)	0.027 (0.020)	0.018 (0.020)	0.006 (0.019)	-0.030 (0.021)	0.036 (0.019)	-0.034* (0.013)		
School type (Ref.: intermediate)										
Basic requirements	-0.139* (0.026)	0.025 (0.016)	0.039* (0.015)	-0.023 (0.013)	0.026 (0.014)	-0.001 (0.014)	0.054* (0.014)	0.019* (0.010)		
Pre-Gymnasium	0.104* (0.028)	-0.016 (0.015)	-0.032* (0.011)	0.004 (0.016)	-0.032* (0.011)	0.015 (0.017)	-0.026* (0.010)	-0.016* (0.005)		
Other types	-0.082* (0.035)	0.003 (0.019)	-0.006 (0.016)	0.015 (0.020)	0.018 (0.017)	0.021 (0.022)	0.021 (0.016)	0.009 (0.013)		
Language characteristics										
Language proficiency (GPA)	0.024* (0.011)	0.011 (0.007)	-0.002 (0.006)	-0.004 (0.006)	-0.007 (0.006)	-0.010 (0.006)	-0.010 (0.005)	-0.001 (0.004)		
Pseudo-R ²					0.0353					
Number of cases					2,317					

Table A1 continued

Note. Multinomial logistic regression; reference group: $t9 \leq 7$ days. Average marginal effects (AME), and standard errors (SE). * $p < .05$.

Table A2

Effects of Different Incentives on Survey Participation – The Reference Group Only

Survey stages	Until deadline		After deadline		Complete period	
Estimate	β (SE)		β (SE)		β (SE)	
Time dimension						
Time left to deadline	0.038**	(0.014)				
Past time since deadline			-0.058**	(0.018)		
Time since survey launch					-0.072***	(0.006)
Reciprocity						
Low or no reciprocal preference	0		0		0	
Strong reciprocal preference	0.538***	(0.152)	1.031**	(0.318)	0.630***	(0.137)
Intercept	-2.466***	(0.193)	-3.460***	(0.413)	-2.158***	(0.163)
Number of cases	1,297		278		1,297	
Number of sub-episodes	6,076		2,833		8,909	
Number of events	1,008		164		1,172	
LR (d.f.)	51.5 (13)		43.0 (14)		270.5 (14)	

Note. Piecewise constant rate model with exponential distribution of hazards. β -coefficients, and standard errors (SE); controlled for panelists' characteristics such as gender, social origin, and education. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A3

The Role of Panelists' Characteristics in Survey Participation

Survey stages Categories	Until deadline		After deadline		Complete period	
	II	III	II and III	II	III	II and III
Estimate	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)
Time dimension						
Time: left to deadline	0.023	(0.020)	0.103*	(0.047)	0.053**	(0.019)
Time: passed since deadline					-0.023	(0.020) -0.007 (0.041) -0.036*** (0.018)
Category II: Wave 9 > 7 days						
Cat. II: Treatment I: CHF 10+10	0.586***	(0.147)			-0.645*	(0.264)
Cat. II: Control group	0				0	
Cat. II: Treatment II: CHF 10+20	0.569***	(0.148)			0.102	(0.219)
Category III: Nonresponse						
Cat. III: Control group:		0			0	
Cat. III: Treatment I: CHF 10+10		0.759*	(0.376)		-0.974	(0.534)
Cat. III: Treatment II: CHF 10+20		1.000**	(0.361)		-0.711	(0.495)
Categories II and III						
Control groups			0			0
Treatment I: CHF 10+10			0.537***	(0.136)		-0.780*** (0.235)
Treatment II: CHF 10+20			0.573***	(0.135)		-0.125 (0.193)
Reciprocity						
Low or no reciprocal preference	0	0	0	0	0	0
Direct positive and/or altruistic reciprocity	0.420*	(0.212)	0.994*** (0.291)	1.246*** (0.164)	0.880* (0.355)	1.585*** (0.477) 1.827*** (0.276)

Table A3 continued

Survey stages Categories	Until deadline		After deadline		Complete period	
	II	III	II and III	II	III	II and III
	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)	β (SE)
Gender						
Reference category: male	0	0	0	0	0	0
Female	0.106 (0.117)	-0.235 (0.277)	0.088 (0.277)	0.280 (0.192)	0.078 (0.421)	0.237 (0.174)
Social origin						
Reference category: upper service class	0	0	0	0	0	0
Lower service class	-0.025 (0.205)	0.077 (0.458)	0.014 (0.458)	0.259 (0.346)	-0.862 (0.850)	-0.100 (0.312)
Routine non-manual employee	0.049 (0.197)	-0.755 (0.493)	-0.112 (0.493)	0.606 (0.333)	-0.855 (0.736)	-0.004 (0.295)
Farmer; small proprietor	0.188 (0.253)	-0.456 (0.688)	0.112 (0.688)	0.253 (0.449)	-0.669 (1.106)	-0.036 (0.406)
Foreman; skilled manual worker	0.120 (0.219)	-0.612 (0.523)	-0.039 (0.523)	0.660 (0.373)	0.605 (0.596)	0.275 (0.308)
Semi-skilled/unskilled manual worker	0.041 (0.289)	0.274 (0.543)	0.084 (0.543)	0.739 (0.437)	0.177 (0.414)	0.177 (0.414)
Missing values	0.063 (0.226)	0.074 (0.483)	0.013 (0.483)	0.355 (0.386)	0.433 (0.703)	0.082 (0.336)
School type						
Reference category: intermediate	0	0	0	0	0	0
Basic requirements	-0.184 (0.160)	-0.037 (0.320)	-0.211 (0.320)	-0.593* (0.248)	-0.772 (0.499)	-0.548* (0.219)
Pre-Gymnasium	0.045 (0.160)	0.567 (0.446)	0.241 (0.446)	0.069 (0.276)	-1.174 (1.063)	0.123 (0.260)
Other types	0.020 (0.184)	-0.351 (0.510)	-0.027 (0.510)	-0.256 (0.341)	-0.910 (0.769)	-0.320 (0.316)
Language characteristics						
Language proficiency (GPA)	0.093 (0.065)	0.057 (0.150)	0.080 (0.150)	-0.081 (0.104)	0.201 (0.232)	-0.034 (0.096)
Intercept	-3.458*** (0.296)	-5.569*** (0.573)	-4.654*** (0.573)	-4.068*** (0.488)	-5.368*** (0.758)	-4.927*** (0.390)
Number of cases	548	422	970	245	352	597

