WIDER Working Paper 2021/140

Informed participation: the effects of information treatment on panel non-response

Ricardo Santos and Sam Jones*

August 2021

United Nations University World Institute for Development Economics Research

wider.unu.edu

Abstract: This paper builds on a longitudinal school-to-work transition phone survey experiment to quantify the effects on attrition of communicating with participants. Specifically, we study the impact of sending topically relevant information on job market conditions via SMS at the start of each survey round. Testing various information treatments, which differ in their granularity, including survival analysis, we find they all significantly reduce the instantaneous risk of non-response, with an estimate of instantaneous hazard reduction of around 30 per cent. These results affirm the best-practice recommendation to establish communication with participants between survey rounds to continue survey participation.

Key words: survey participation, attrition, information, survival analysis

JEL classification: C41, C83, J20, J63

* UNU-WIDER, corresponding author: santos@wider.unu.edu

This study has been prepared within the UNU-WIDER project Inclusive growth in Mozambique – scaling-up research and capacity implemented in collaboration between UNU-WIDER, University of Copenhagen, University Eduardo Mondlane, and the Mozambican Ministry of Economics and Finance. The project is financed through specific programme contributions by the governments of Denmark, Finland, and Norway.

ISSN 1798-7237 ISBN 978-92-9267-080-1

https://doi.org/10.35188/UNU-WIDER/2021/080-1

Typescript prepared by Gary Smith.

United Nations University World Institute for Development Economics Research provides economic analysis and policy advice with the aim of promoting sustainable and equitable development. The Institute began operations in 1985 in Helsinki, Finland, as the first research and training centre of the United Nations University. Today it is a unique blend of think tank, research institute, and UN agency—providing a range of services from policy advice to governments as well as freely available original research.

The Institute is funded through income from an endowment fund with additional contributions to its work programme from Finland, Sweden, and the United Kingdom as well as earmarked contributions for specific projects from a variety of donors.

Katajanokanlaituri 6 B, 00160 Helsinki, Finland

1 Introduction

Research in economics and other social sciences relies increasingly on primary data collection. What can be done to enhance the quality of survey data is, therefore, critical. One of the most obvious concerns is non-response and its effects (Fowler et al. 2016; Marek et al. 2017; Pew Research 2017, 2019). Peytchev (2013) acknowledges that non-response reduces trust in survey estimates, as it may induce bias not only in mean and proportion estimates, but in their variance. The same can be said of attrition, as it essentially corresponds to sequential non-responses throughout the rounds of a panel survey. Williams and Brick (2018) join in, highlighting how attrition may be a source of significant bias. Conversely, Groves and Peytcheva (2008) signal, in their review, that high response rates can reduce these risks, even if not being a sufficient condition.[1] In the effort to prevent non-response bias, and as a guideline, Stoop et al. (2010) propose a benchmark response rate of 70 per cent for the European Social Survey, while acknowledging that in many countries this standard is not met.

In her introductory paper for a special edition focused on non-response bias, Singer (2006) cites Groves et al. (2002: xiii) when they state that: 'Declining cooperation rates increase the cost of conducting surveys ... [and] can also damage the ability of the survey statistics to reflect the corresponding characteristics of the target population.' Non-response, therefore, is not just a matter of bias. In order to prevent it, surveyors are required to make costly choices, such as over-sampling or implementing non-response follow-up surveys (Peytchev 2013).

The historic record of survey response rates does not reassure researchers. From Steeh (1981) to Smith (1995) and various later reviews in the literature on response rate trends (Battaglia et al. 2007; Burkell 2003; Curtin et al. 2005; Fowler et al. 2016; Pew Research 2019; Williams and Brick 2018), it becomes apparent that, while surveying methods change and evolve, a general trend of decreasing response rates is apparent in the Global North. This trend is, of course, influenced by the evolution in methods, from in-person to telephone (fixed and mobile; using random digit dialling or sourced phone listings), to SMS and interactive voice response (IVR), to web-based and smartphone-based, arguably with decreasing cost per contact but correlated decreasing efficacy. Such a trend is also evident within-method, as reported by Williams and Brick (2018) and Pew Research (2019). Stoop et al. (2010) found some cases of reversion in the downward trend, namely in the Netherlands, noting one case in which rates improved from 46 to 70 per cent. However, they also note that these improvements required costly procedural changes.

While phone surveys are a staple in the Global North, there is a paucity of information on how they are being implemented in the Global South. In the only review found of phone surveys in the southern hemisphere, Gibson et al. (2017) reviewed ten surveys that used computer-assisted telephone interviewing (CATI) applications or were human operated (i.e. of a similar nature to the survey we implemented for this paper). These surveys were conducted in Africa (Liberia, Mali, South Sudan, and Tanzania), Latin America (Brazil, Honduras, and Peru), the Middle East (Lebanon), and South Asia (Bangladesh), with durations ranging between 5 and 27 minutes. Seven of these surveys provided airtime incentives between US$1 and US$5, four of which used this to evaluate its effect on survey response[2]. They recorded a wide range of survey response rates,[3] between 99 and 35 per cent, with only half being above the 70 per cent standard proposed by Stoop et al. (2010).

---

[1] Groves and Peytcheva (2008) found cases where, due to high correlation between participation and survey variables, high response wasn't enough to prevent high relative non-response bias.

[2] Incentives were found to increase response/reduce attrition, although one study suggested non-monotonicity of effects by incentive value

[3] Reported response rate for panels referred to the first round.

Against this backdrop, we find slim evidence of the effects of any particular measure put in place to reduce non-response when conducting surveys in developing countries, to the limited exception of monetary incentives, and especially for phone interviews. While the current literature already recommends a set of measures, we find it relevant to complement and strengthen the evidence, testing their efficacy in the context of low-income and informal economies.

In this paper, we build on a school-to-work transition survey experiment to produce and present causal effects of communicating with participants between panel rounds on survey participation. Specifically, we study the impact of providing information on job-market entry conditions within the framework of a school-to-work transition survey.

Three alternative information treatments are tested, using peer-based information collected by the survey itself. The treatment branches are distinguished by the type of information provided: general information on the proportion of participants that already found a job and their average wage; specific job attainment and wage information on those participants from the same university; and similar specific information on those participants with a degree from the same study area.

The results of this experiment suggest that there are advantages to providing relevant information to survey participants as an incentive. The suggestion offered is that information treatments significantly reduce the instantaneous risk of non-response at any given round, with an estimate of hazard reduction of around 12 per cent. When decomposing by treatment type, we find their impact to be statistically similar, with an estimate hazard reduction of around 31–34 per cent. These results appear to be robust, particularly on the effect of an information treatment and on the information regarding peers from the same university.

We also find evidence that women and older participants tend to have significantly higher propensities to drop-out from the panel and that some other survey procedures, such as obtaining more than one phone contact number from participants, not rushing the interview, or running the interviews mid-week, appear to correlate with lower risk of non-response. On the other hand, we find a significant correlation between changes in the contact numbers provided by participants and their eventual drop-out from the panel. Finally, we found no robust evidence of a penalty for adopting a more intensive contact regime, with monthly instead of quarterly waves.

This study contributes to the literature on survey methodologies. Its results reinforce and re-affirm the recommendation that, when conducting a panel survey, it is good practice to establish communication with participants between survey rounds, as suggested by Schoeni et al. (2013). In particular, it adds strength to the argument that sharing relevant information on the topic of a survey, particularly in information-poor environments, is a cost-effective instrument to incentivize participation.

In the following sections we will briefly summarize the main insights from the literature on measures to address non-response, followed by a description of the survey design, experiment design, data used, and method applied in our test. These will be followed by a presentation and discussion of the results, then the conclusion.

2 Addressing non-response

Non-response is found to correlate with various factors, from the respondent's characteristics (Olson and Smyth 2015; Olson et al. 2019; Porter and Whitcomb 2005), to interviewer characteristics (Broome 2019; Kibuchi et al. 2020; Olson and Smyth 2015; West and Blom 2016), to the typology of questions (Hansen 2006; Olson and Smyth 2015; Olson et al. 2019), to preventive measures implemented by surveyors (Broome 2019; Burkell 2003; Cantor et al. 2007; Debell et al. 2019; Fowler et al. 2016; Kibuchi

et al. 2020; Lynn 2013; Porter and Whitcomb 2003; Sakshaug and Eckman 2017; Smets 2018; Smith et al. 1995; Spreen et al. 2019; Williams and Brick 2018). The latter deserve some further description and are thoroughly reviewed by Schoeni et al. (2013).

Critically, survey practice and literature provide guidelines on measures to prevent non-response. Burkell (2003), citing Dillman (2000), highlights a three-pronged strategy: (1) establish the respondent's trust; (2) increase the expected rewards of participation; and (3) reduce the social costs of participation. Schoeni et al. (2013) offer a comprehensive list of tools that can be used, from which we highlight the following:

- *Monetary*[4] *and non-monetary incentives for:* interview completion; providing contact information between waves; supplemental data collection efforts; additional end-game incentive for more resistant respondents; finder's fee or assistance making contact with non-response individuals.

- *Communication with respondents between waves:* respondents' contact information update requests; sending newsletters; maintenance of a respondent website for contact update and sharing of study materials; supplemental between-wave studies.

- *At survey design stage:* minimize interview length; use external administrative records as a substitute for information requested from respondents; reduce interview frequency; design mixed surveys to offer alternative modes of interview;[5] include engaging content; consider allow the preloading of prior-wave information (that gets updated) to increase interview efficiency; if relevant, prepare the questionnaire in multiple languages to accommodate respondents.

- *During fieldwork:* advance warning of interview;[6] call respondents' persons of contact (using previously provided information) in case of contact failure; seek support from related respondents on new contact information for non-respondents; use directory or internet assistance; contact reluctant respondents on problem-specific issues; allow interviews to be completed in multiple sessions; receive and advertise endorsement of the study by prominent individuals; promise confidentiality of responses; ongoing monitoring of response rates by sub-samples and survey components, relaying results to field staff, highlighting achievements and points of improvement; send personalized 'thank you' messages; provide respondents with additional information on the study's results, including examples of how they are used; re-contact non-respondents.

- *Regarding interviewers:* employ experienced individuals; invest in training to motivate on the study's importance, the need to develop rapport with respondents, persuasion and refusal-conversion; offer gifts and send study newsletters to stimulate interviewers' identification with the research; offer response rate bonuses.

Other measures, such as increasing the number of contact attempts (Williams and Brick 2018) or conducting non-response follow-up surveys (Sakshaug and Eckman 2017) are also put in place to reduce non-response.

In the survey we conducted, we sought to follow best practices and adopted various of the proposed measures, as we discuss below. However, for the purposes of this paper, the main research question

[4] This type of strategy was the object of varied studies, of which we highlight Burkell (2003), Porter and Whitcomb (2003), Cantor et al. (2007), Fowler et al. (2016), Williams and Brick (2018), Debell et al. (2019), Spreen et al. (2019), and Kibuchi et al. (2020).

[5] The relative efficacy of mixed survey designs was the subject of studies such as those by Fowler et al. (2016) and Lynn (2013).

[6] Studied also by Smets (2018) and Smith et al. (1995), in the latter case a letter sent with additional information explaining the study.

is to assess the effect of communication with respondents between waves, specifically the provision of information on peer labour market outcomes (employment and wage) to early graduate job-seekers participating in a panel survey. This led us to purposefully design the survey so as to randomly select respondents into receiving, or not, preliminary results from the survey, in advance of each round.

A necessary sub-question of the study, however, was: would the information provided induce a reduction in non-response? As reviewed in the literature, the hypothesis is that this form of between-waves communication with respondents induces higher participation. To test it is a relevant procedural survey quality, a non-response prevention question, and the objective of this paper.

In the next section we further present the survey design and explore this crucial component.

3 Survey design

In March 2017 we started a longitudinal school-to-work survey of Mozambican university students, comprising students in their final year of undergraduate studies. The baseline survey was conducted at six of the largest universities, which, jointly, teach approximately three-quarters of the entire population of Mozambican university students. A total of 27 faculties participated, allowing us to survey students from 106 different courses.[7] In total, 2,174 finalists were surveyed at the baseline (1,024 women and 1,150 men). The courses were grouped into seven distinct study fields: *education*, *languages and humanities*, *social sciences*, *management and law*, *natural sciences*, *engineering*, *industry and construction*, *agriculture*, and *health and welfare*.[8]

When designing the baseline, we took care to ensure that the best practices proposed by Dillman (2000) were followed and to use some of the tools suggested by Schoeni et al. (2013). In the absence of a full institutional review board at both UNU-WIDER and local institutions at the time of the surveys, approval for undertaking the research was received from each participating university prior to fieldwork and upon discussion of the research and survey design and procedures. Once permission was granted, we implemented the interviews in the classrooms, in almost all cases using slots that would otherwise be taken by regular classes. All participants were aged 18 and above. Although the classroom arrangement meant we had a somewhat *captive audience*, students gave written informed consent to participate in the baseline survey and follow-up telephone rounds, following a presentation of the study that allowed for questions to be asked and answered and where they were informed, orally and in writing, that the data collected would be anonymized. They were also informed they would receive an airtime incentive of MZN50 (around US$0.80) for a completed interview. Students were free to not participate and to drop out at any time. Participants were asked to provide up to four phone numbers: a primary and a secondary personal phone number, the phone number of a friend, and the phone number of a relative. The latter two numbers were requested to allow re-contact attempts in the case the participant had changed their primary and secondary phone numbers or there were other difficulties in reaching them.[9]

---

[7] The universities were: Universidade Eduardo Mondlane (UEM), Universidade Pedagógica (UP), Universidade Politécnica (AP), and Universidade São Tomás de Moçambique (USTM) in their Maputo campuses, and Universidade Católica de Moçambique (UCM) and Universidade Zambeze (UZ) in their Beira campuses.

[8] The survey baseline sample size, for a theoretical proportion p of 50 per cent of respondents finding a job, with a confidence interval $(1-\alpha)$ of 95 per cent and as per Cochran (1977), allows for error margins of 1.7 per cent in the total sample, of up to 2.4 per cent in each gender sub-sample, and up to 6.3 per cent in the least represented study field. The implementation of both baseline and follow-up phone surveys is further explained in Jones et al. (2018, 2020a).

[9] For further details on the design and results of the baseline survey, see Jones et al. (2018).

Following the baseline, from March 2018 survey participants were tracked on a quarterly basis by telephone over 18 months (i.e. for six rounds) to the end of 2019 at least, while a sub-sample of 100 participants was set to be contacted monthly. In preparation of this follow-up phase, all phone primary and secondary numbers were tested. Of those surveyed in the baseline, 2,100 agreed to remain in the follow-up sample and took part in the survey experiment.

The phone survey fieldwork protocol determined that, at the beginning of each phone round, all participants received an SMS informing them that they would soon be contacted. Prior to this, all participants in the treatment arms of the information experiment, further described below, received an informative SMS. No other SMS communication existed between rounds. SMS messages received from participants were not responded to, but an email address was shared so that participants could reach the phone survey implementer with any queries. Up to ten contact attempts were made to the primary and, if necessary, the secondary phone numbers. If the participant didn't respond, surveyors sought to establish whether they were out of reach, unable to answer,[10] refused to answer during that round but remaining in the roster, or, if permanently out of reach or unable or unwilling to participate, being then removed from the roster. No attempts were made to recover those that exited the roster on a permanent basis.[11].

3.1 Experiment design

While conducting the follow-up phone survey, an information experiment was conducted. Its main aim was to investigate the effects of labour market information on job-seekers' beliefs and on labour market outcomes, most notably on unemployment duration for university graduates, as discussed by Jones and Santos (2000). With that purpose, we provided peer information on wages and employment likelihood as a treatment. The effect of this information treatment on survey participants' response was a necessary quality assurance test. While ancillary to the policy question, we find it to be a relevant procedural question, deserving of being documented and analysed.

In our experiment, we elicited detailed information about participants' work status, including their specific position, type of contract, and current wage, in each round of the survey. We used the information from each prior round to design three distinct information treatments. Namely:

1. *General message:* summarized wage and employment information from the entire sample. For example: 'Survey results at Dec.1st: of all graduates in Mozambique (class of 2017), 59% are working and their average wage = 14,000 Mts / month.'

2. *University-specific message:* summarized wage and employment information from the sub-sample of participants that attended the same university as the recipient. For example: 'Survey results at Dec.1st: of all graduates from your university (class of 2017), 52% are working and their average wage = 24,000 Mts / month.'

3. *Field-specific message:* summarized wage and employment information from the sub-sample of participants in the same study field as the participant. For example: 'Survey results at Dec.1st: of all graduates from your area of studies (class of 2017), 50% are working and their average wage = 13,500 Mts / month.'

The information messages were sent by SMS at the beginning of each telephone survey round, excluding the first. As they were based on previous information up to the time the SMS was sent, the information contained in each SMS varied by survey round. Also, in the second and third types of message, the

[10] Inability to answer would be caused by being out of the country, being in hospital, or another reasonable justification.

[11] The phone survey fieldwork was conducted by IPSOS. For further details on the design and results of the survey, see Jones et al. (2020a).

information varied by individual according to the specific university they had attended or their field of study.

In terms of exposure to the treatments, directly after the baseline survey individuals were randomly allocated to one of four treatment arms, distinguished by the type of messages they would receive. Specifically:

- The *control* group received no information messages.
- Group 1 (*general*) received the general (all-student) message in all relevant rounds.
- Group 2 (*university*) received the university-specific message in all relevant rounds.
- Group 3 (*field*) received the field-specific message in all relevant rounds.

Randomization across the four experimental arms was stratified by study field and gender, with a target of around 500 participants in the control group and 400 in each treatment arm.

The first row of Table 1 presents the initial allocation (undertaken after the baseline was concluded). The remaining rows presents the number of responders over the follow-up rounds and by treatment arm, giving a sense of the attrition experienced throughout the survey.

A first finding was that throughout the 18 months of follow-up surveys, cumulative attrition reached a maximum of 12 per cent, keeping the study's response rate healthily above the 70 per cent European Social Survey benchmark. We can also immediately find small differences in attrition across the arms, with higher than average attrition (16.1 per cent) in the control group, lower than average (7.8 per cent) in the 'general information' treatment group, and average attrition levels in the remaining treatment groups. Overall, the realized experiment is well-powered.[12]

Table 1: Observations across survey rounds, by experimental arm

	Experimental arm						
Round	Cont.	Gen.	Uni.	Field	Mixed	Total	% base
Baseline	504	397	400	398	401	2,100	1.00
1	486	393	390	390	392	2,051	0.98
2	473	389	379	377	386	2,004	0.95
3	456	381	376	375	383	1,971	0.94
4	439	377	367	366	372	1,921	0.91
5	428	372	361	359	360	1,880	0.90
6	423	366	353	350	357	1,849	0.88

Note: the cells report the raw number of observations by experimental arm and round number; the final column gives the overall follow-up rate relative to the baseline sample.

Source: authors' compilation based survey responses.

4 Descriptive statistics

As discussed in Section 2, respondents' characteristics can be significantly correlated with their likelihood of not responding, in any particular round, and eventually dropping from a panel survey. In

[12] Using the sub-samples obtained in the last round, a *t*-test of differences in proportions between treatment and control groups (jointly) would be expected to detect a difference in response rates of around 7 percentage points with a power of 80 per cent at the 5per cent significance level.

this particular study, gender, age, the intrinsic usefulness of the information, respondents' skills, and academic performance are considered to define the respondents' profiles.

As can be seen in Table 2, experiment participants, at the baseline, reported a wage expectation of around MZN33,000 (approx. US$500), a notably optimistic value for the Mozambican labour market.[13] We can also see that the sample is relatively balanced in terms of gender, with almost 45 per cent female participants, and in terms of age. Very low proportions of participants had prior job information (around 20 per cent) or had a job waiting (around 12 per cent), while more than half (around 60 per cent) had prior work experience. Focusing on the academic experience and skills, we can see that the average number of years at university was around 4.3, without great variability,and that there is not much deviation around mean records in skills tests results (Raven's and academic, the latter focusing on numeracy and literacy) between experimental arms. We can also see that the level of proficiency in English is homogeneously low (around 10 per cent). Nevertheless, close to half of the participants (around 45 per cent in all experimental arms) considered themselves excellent students when compared with their colleagues.

Another set of correlates worthy of our analysis relates to the survey and interviewing processes themselves. Using survey administrative data, we constructed information on the average number of contact phone numbers each offered in the baseline interview and the average call duration. We can see that a very high proportion of participants provided four phone numbers, leading to an average of $\approx$3.94 numbers provided. Call duration was, on average, slightly longer than ten minutes. Notably, a higher proportion of participants in the control group changed their phone numbers.[14] The distribution of interviews by weekday is relatively balanced between days and, other than on Saturdays and, to a lesser degree, Thursdays, balanced among experimental arms. A higher proportion of the interviews, $\approx$65 per cent, were in the mornings.

A note should be made regarding the notable sample balance across the treatment arms. We found statistically significant joint differences between treatment and control arms in the call duration and proportion of interviews that occurred on Thursdays and Saturdays. These, however, are numerically minute and found not to characterize any specific treatment or control arm. Only one significant difference happened by design: of all participants, 38 per cent of those in the control arm and none in any other experimental arms were contacted monthly. We therefore control for that. Finally, it should be noted that other than the random selection into the monthly contact sub-sample, all survey process correlates are time varying and are, therefore, incorporated as such into the empirical model.

Table A1 in Appendix A confirms the experimental arm's balance among the universities from where participants obtained their education. A great majority of participants studied either at UEM or at UP,with around 35 per cent originating from each. Close to 20 per cent studied in the Beira campuses of UCM (around 9 per cent) and UZ (around 11 per cent). The sample is completed by participants originating from two private universities of Maputo, USTM and AP, with around 5 per cent from each. The distribution is also balanced among study areas. The highest share of participants studied social sciences (around 45 per cent), followed by courses in education (around 30 per cent), engineering (8 per cent), health (6 per cent), agriculture (around 5 per cent), natural sciences (4 per cent), and humanities (around 2 per cent).

[13] Jones et al. (2020b) find evidence of a strong optimistic streak.

[14] We cannot exclude the possibility that this behaviour is endogenous.

Table 2: Descriptive statistics

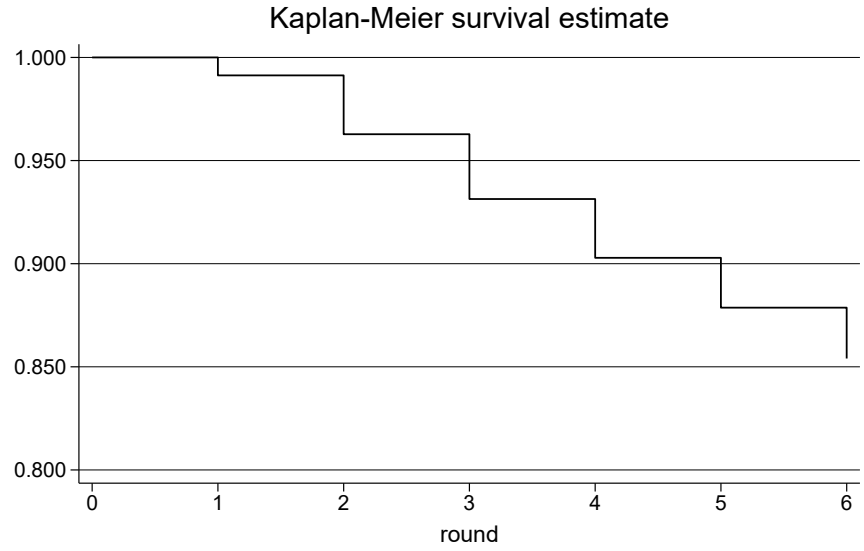
	Experimental arm				
	Cont.	Gen.	Uni.	Field	*F*-test
Expected wage (baseline)	34.22	33.14	33.03	31.02	0.094
	(0.36)	(0.38)	(0.34)	(0.32)	–
Female	0.44	0.44	0.45	0.45	0.717
	(0.01)	(0.01)	(0.01)	(0.01)	–
Age	0.18	–0.30	–0.09	–0.10	0.890
	(0.12)	(0.13)	(0.13)	(0.12)	–
Has job info.	0.19	0.18	0.21	0.21	0.757
	(0.01)	(0.01)	(0.01)	(0.01)	–
Has job waiting	0.15	0.12	0.12	0.12	0.101
	(0.01)	(0.01)	(0.01)	(0.01)	–
Prior work experience	0.62	0.61	0.61	0.58	0.430
	(0.01)	(0.01)	(0.01)	(0.01)	–
Years in university	4.37	4.33	4.43	4.36	0.968
	(0.02)	(0.02)	(0.02)	(0.02)	–
Raven's test	–0.07	0.01	–0.03	0.09	0.229
	(0.02)	(0.02)	(0.02)	(0.02)	–
Academic test	0.02	–0.10	0.06	0.01	0.459
	(0.02)	(0.02)	(0.02)	(0.02)	–
Excellent academics	0.41	0.47	0.48	0.41	0.926
	(0.01)	(0.01)	(0.01)	(0.01)	–
Proficient in English	0.09	0.09	0.09	0.12	0.106
	(0.01)	(0.01)	(0.01)	(0.01)	–
Phone numbers given	3.95	3.94	3.93	3.93	0.690
	(0.00)	(0.01)	(0.01)	(0.01)	–
Average interview duration	12.68	11.38	11.02	10.94	0.010
	(0.26)	(0.22)	(0.21)	(0.22)	–
Interviews on Monday	0.20	0.20	0.21	0.21	0.436
	(0.01)	(0.01)	(0.01)	(0.01)	–
Interviews on Tuesday	0.20	0.18	0.19	0.19	0.100
	(0.01)	(0.01)	(0.01)	(0.01)	–
Interviews on Wednesday	0.16	0.17	0.16	0.16	0.036
	(0.01)	(0.01)	(0.01)	(0.01)	–
Interviews on Thursday	0.21	0.22	0.21	0.22	0.004
	(0.01)	(0.01)	(0.01)	(0.01)	–
Interviews on Friday	0.13	0.16	0.17	0.15	0.365
	(0.01)	(0.01)	(0.01)	(0.01)	–
Interviews on Saturday	0.10	0.07	0.07	0.07	0.000
	(0.00)	(0.01)	(0.01)	(0.01)	–
Interviews in the morning	0.65	0.65	0.66	0.64	0.047
	(0.01)	(0.01)	(0.01)	(0.01)	–
Monthly sub-sample	0.38	0.00	0.00	0.00	0.000
	(0.01)	(0.00)	(0.00)	(0.00)	–

Note: the cells show means and mean standard errors (in parentheses) across different experimental arms of baseline variables, based on one observation for each individual observed at least once in the follow-up rounds, N = 2,069; 'F-test' reports the probability that the means in the treatment arms jointly differ from the control arm.
Source: authors' compilation based on survey responses.

5 Method

In our experiment we seek to study the treatments' effects on participants' permanence in the survey panel. This equates to conducting a time-to-event analysis, in which the event is non-response or survey drop-out.[15] As we can see in Table 1 and profiled in Figure 1, there is an increasing non-response throughout the survey rounds. In order to capture average treatment effects of information provision on the response rate of the treated (ATT), we run a survival analysis on the event of dropping from the panel.

Figure 1: Non-parametric survival



Source: authors' compilation based on survey responses.

In our main empirical approach we adopt a semi-parametric Cox model for the hazard function, $h(t)$,[16] accounting for time-varying correlates. The empirical function is, as such:

$$h(t) = h_o(t)\exp(T\beta_1 + X'\beta + g(t)Z'\gamma) \tag{1}$$

where $h_o(t)$ denotes the baseline hazard—that is, the common risk of non-response in time t, given that participants responded before. The β parameters of the exponential function, including the treatment, T, indicate the expectation that the X time-fixed correlates will induce proportional shifts in the risk of non-response at any given period. The γ parameters assume that, in each period t, the specific values of time-varying correlates Z also induce proportional shifts in the non-response hazard. In this empirical model, we adopt a linear interaction of time-varying variables with t—that is, $g(t) = t$.

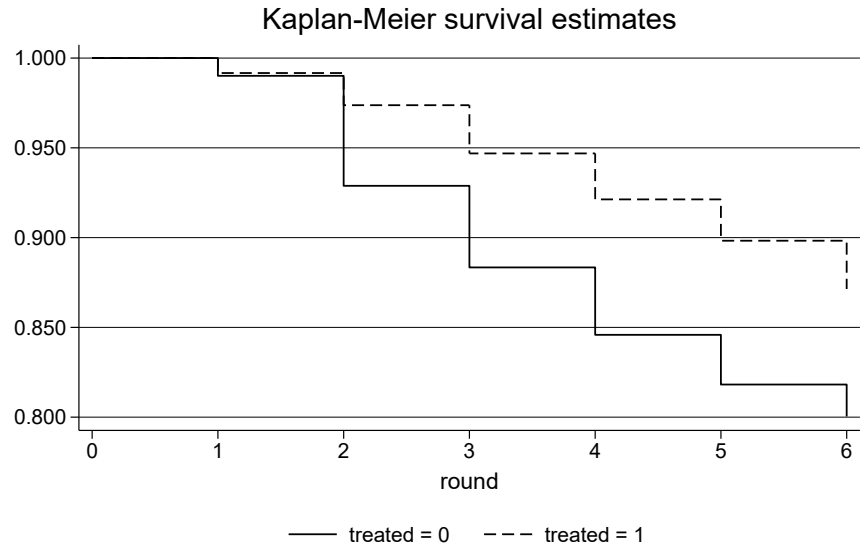
We consider two empirical models to capture the information treatment. In the first, simple model we do not distinguish treatment arms: T represents having received labour market information and β_1 the average effect of the various information types. In the full model, we distinguish the treatment arms and, therefore, T represents the vector of alternative treatments: general information; university-specific information; or field-specific information.

[15] In this empirical approach, a non-response is treated as a failure, even if the participant returned later.

[16] Two alternative parametric distributions, exponential and Gompertz, are used for robustness testing.

Figure 2 presents the Kaplan–Meier survival curves for the treated and not-treated sub-samples. These non-parametric survival curves are distinctly different, suggesting that the information treatment may have, indeed, induced a proportionately lower rate of non-response and slowed attrition.[17]

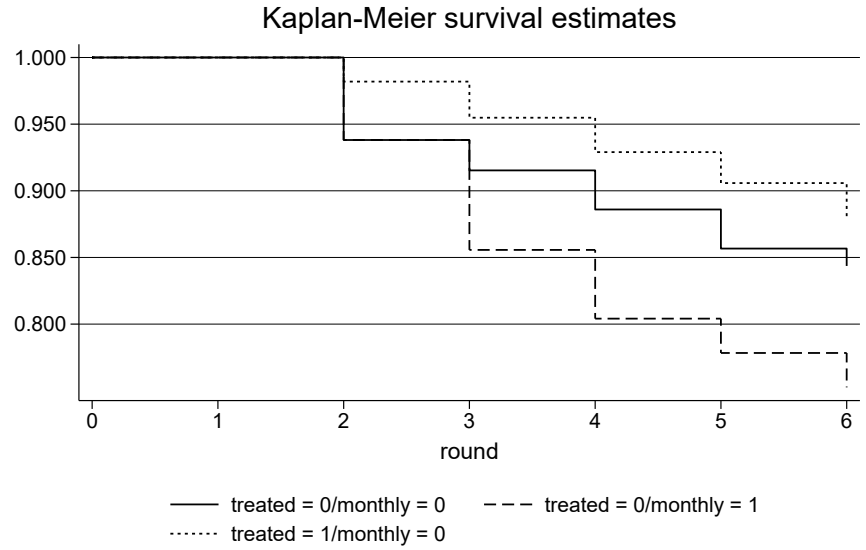
Figure 2: Treatment vs control



Source: authors' compilation based on survey responses.

This could, however, be the average effect of having run monthly survey rounds solely on the control arm. If that is the case, the running of a monthly survey should be considered as a treatment in itself. A non-parametric analysis, presented in Figure 3, suggests that, while having a role in increasing attrition, the running of monthly rounds may not be sufficient to explain the different profiles and that the information treatment itself still plays a role.

Figure 3: Treatment vs control: identifying participants in monthly rounds



Source: authors' compilation based on survey responses.

[17] A Cox regression-based Wald test on equality of survival rates significantly rejects the null hypothesis, with a χ^2 test statistic of 26.84, further reinforcing the suggestion of an effect from the information treatment.

The vector of time-fixed control correlates, X, encompasses the following variables: expected wage at the baseline survey; gender (1 if female); age; whether participants are considered to have information about potential jobs (1 if yes); whether they had a job waiting at the baseline (1 if yes); whether they had prior work experience; average years to graduate; grade in the Raven's test; grade in the academic (literacy and numeracy) test; self-assessed performance (against peers); self-assessed quality of English; university of graduation; study area; and, on the survey process, the number of phone contacts given by participants and whether participants were interviewed monthly (1 if yes). The vector of time-varying control correlates, Z, encompasses the following variables: phone call duration in each interview; whether the participants changed their phone numbers; enumerator fixed effects; day-of-interview fixed effects; whether the previous interview was conducted in the morning (1 if yes).

In the next section we discuss the results of the experiment on attrition.

6 Results

Table 3 presents the results of the Cox proportional hazards model, as per Equation 1, using a range of specifications from the most simple to a complete model including a full range of controls. Columns (1) and (2) correspond to the simplest models, without survey process controls nor individual characteristics; while the first presents an estimate of the average information treatment effect, the second presents estimates of average effects by information type. Column (3) presents the non-discriminated information treatment model with added personal controls, and column (4) presents an alternative, with the survey process controls. A first comparison of pseudo log-likelihood and Akaike information criterion (AIC) indicators strongly suggests that integrating survey process controls in the analysis improves the fit. Columns (5) and (6) present the main, most complete models, incorporating both personal and process controls, to estimate the information treatment effects. The complete regression is presented in Tables A2 and A3 in Appendix A.

We start discussing the experiment's results by focusing on the first two columns. The suggestion offered is that information treatments do indeed support the retaining of respondents, reducing the hazard estimate by 39 per cent. This would correspond to a strong reduction in the instantaneous risk of non-response among those who had, up until each given round, been full survey participants. Decomposing by treatment arms, we find a first suggestion of strong effects from either type of information, with general and field-specific information appearing more effective in preventing non-response.

The introduction of controls, as per the following columns, leads to a lower estimated ATT of information on non-response, particularly when added survey procedures and interviewer fixed effects are incorporated into the empirical model. The estimated hazard rate reduction amounts to a still relevant 12 per cent in the most complete model specification, with full individual controls and interview-related controls.

Similarly, the evidence of an effect from the different information treatments weakens in the full model. The estimated hazard rate reduction attributable to each treatment type appears to be statistically similar, between 31 and 34 per cent in the most complete model specification.[18]

[18] All the treatment effect estimates are higher than the minimum average detectable effect determined by the test design.

Table 3: Correlates of attrition: hazard ratios, Cox regression

	Simple	Info types	Personal	Process	Main model	Main types
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.61***		0.59***	0.89*	0.89*	
	(0.07)		(0.07)	(0.05)	(0.06)	
General		0.36***				0.67***
		(0.07)				(0.09)
University		0.46***				0.71***
		(0.07)				(0.07)
Field		0.37***				0.68***
		(0.06)				(0.08)
Expected wage (baseline)			1.00		1.00	1.00
			(0.00)		(0.00)	(0.00)
Female			2.39***		1.10*	1.10*
			(0.32)		(0.06)	(0.06)
Age			1.02*		1.01**	1.01**
			(0.01)		(0.01)	(0.01)
Has job info.			1.48***		1.03	1.03
			(0.21)		(0.07)	(0.07)
Years in university			0.97		0.97	0.96*
			(0.06)		(0.02)	(0.02)
Speaks English			2.40***		1.07	1.07
			(0.38)		(0.06)	(0.06)
UCM			0.66		0.89	0.85
			(0.17)		(0.09)	(0.09)
UZ			0.75		0.83*	0.82**
			(0.19)		(0.08)	(0.08)
Monthly sub-sample				1.20	1.24	1.06
				(0.16)	(0.17)	(0.13)
Phone contacts given				0.72*	0.70**	0.71**
				(0.12)	(0.12)	(0.11)
Call duration				0.76***	0.76***	0.77***
				(0.03)	(0.04)	(0.03)
Interviews on Wednesday				0.95**	0.95**	0.95**
				(0.02)	(0.03)	(0.03)
Pseudo Log. Lik.	–2,279	–2,121	–2,088	–1,270	–1,268	–1,266
AIC	4,560	4,248	4,222	2,589	2,630	2,630
Obs.	11,725	11,707	11,707	11,707	11,707	11,707
Interviewer f.e.	No	No	Yes	Yes	Yes	Yes

Note: significance: * 10, ** 5, *** 1 per cent. Columns (1) and (2) correspond to the simplest models, without survey process controls nor individual characteristics; while the first presents an estimate of the average information treatment effect, the second presents estimates of average effects by information type. Column (3) presents the non-discriminated information treatment model with added personal controls, and column (4) presents an alternative, with the survey process controls. Columns (5) and (6) present the main, most complete models, incorporating both personal and process controls, to estimate the information treatment effects; coefficients are hazard odd-ratios; robust standard errors in parentheses.

Source: authors' compilation based on survey responses.

We also find evidence that women and older participants tend to have significantly higher propensities to drop-out from the panel and that some other survey procedures, such as obtaining more than one phone contact number from participants, not rushing the interview, or running the interviews mid-week appear to correlate with lower risk of non-response. On the other hand, we find a significant correlation between changes in the contact numbers provided by participants and their eventual drop-out from the panel. Finally, we found no robust evidence of a penalty of adopting a more intensive contact regime, with monthly instead of quarterly waves. We also find that neither the university nor the study area of graduation were significant correlates to non-response hazard.

All other characteristics presented in the descriptive statistics were also included in the empirical models. As they were not found to be significant in any of the models, we do not present the results here.

6.1 Robustness

For robustness, we start by comparing the results of the non-discriminated information treatment, with five different statistical distributions. The first two columns of Table 4 apply to a time-to-event empirical model. The first column corresponds to an exponential distribution, and the third to a Gompertz distribution. The third and fourth columns present alternative empirical strategies that approximate our study's research question. The third column corresponds to an ordered probit regression in which the dependent variable is the last round in which the participant is observed, while the fourth column refers to a Poisson regression in which the dependent variable is the number of rounds each respondent participated in. As can be seen, the main result holds for the information treatment. Results on being part of the monthly sub-sample remain non-significant.

The second panel presents the alternative empirical strategies, but now for the discriminated treatment. The results again suggest that each of the treatment effects are robust to different time-to-event distributions. The alternative specifications suggest that only the university-specific information has a significant effect. This, however, presents a contradictory result, suggesting a lack of robustness.

Table 4: Robustness

	Info treatment			
	Exp.	Gompertz	Ord. probit	Poisson
Treated	0.93	0.72***	1.08***	1.05***
	(0.05)	(0.07)	(0.02)	(0.01)
Monthly sub-sample	1.19	1.09	0.97	0.98
	(0.14)	(0.23)	(0.02)	(0.02)
Obs.	11,707	11,707	2,065	2,065
	Info types			
	Exp.	Gompertz	Ord. probit	Poisson
General	0.86**	0.51***	0.94	0.89
	(0.06)	(0.06)	(0.07)	(0.13)
University	0.88**	0.55***	0.85**	0.78*
	(0.05)	(0.06)	(0.05)	(0.11)
Field	0.85**	0.46***	0.92	0.97
	(0.06)	(0.05)	(0.06)	(0.05)
Monthly sub-sample	1.15	0.89	0.91***	0.94***
	(0.14)	(0.19)	(0.02)	(0.02)
Obs.	11,707	11,707	2,065	2,065

Note: significance: * 10, ** 5, *** 1 per cent. The empirical models are similar to the 'main model' and 'main types' models, including all personal characteristics and survey process correlates; in the ordered probit model the dependent variable is the last round in which the participant is observed; in the Poisson model the dependent variable is the number of rounds each respondent participated in; coefficients are odd-ratios; robust standard errors in parentheses.

Source: authors' compilation based on survey responses.

7 Discussion and conclusion

The challenge of reducing non-response must be met to ensure surveys can be run efficiently and with high quality. In the Global North, surveys face decreasing response rates. However, not much is known about response rates to surveys conducted in the Global South.

In Mozambique a survey on the school-to-work transition of university graduates was conducted, responding to the severe lack of labour market information among this important group. As part of this survey, an experiment was conducted that sought to test whether the access to information on labour market outcomes, namely on rates of employment and average entry wages of their peers, would impact job-seekers' expectations and their own results.

The information provision experiment gave rise to the opportunity to also test its effect on the response rate. This is, on one hand, a necessary test to ensure there is no bias in the assessment of effects on wage expectations.[19] On the other hand, it stands in itself as a relevant opportunity to study the potential procedural advantages of giving back to survey participants by offering them relevant information on their participation in the survey itself.

The results of this experiment suggest that such advantage exists. The suggestion offered is that information treatments significantly reduce the instantaneous risk of non-response at any given round, with an estimate of hazard reduction of around 10 per cent. However, the results on typology of information given are not robust. The regression analysis also did not establish a statistically significant negative impact of running monthly rounds of interviews instead of conducting them on a quarterly basis.

As reviewed by Gibson et al. (2017), even in Global South settings, arguably with lower incidence of survey fatigue, only around half of telephone surveys had response rates above 70 per cent at the first round. In this survey, the response rates were kept above 88 per cent after the sixth round, 18 months after the panel started.

The reflection on these results cannot overlook the special characteristics of the target population—highly educated young people with access to mobile phones. However, it can be argued that these are also the result of adopting a set of practices that sought to closely emulate those proposed in the literature. Some of these in particular deserve highlighting:

- Participants were duly informed about the survey aims and procedures, including:
  - information that all data were protected and anonymized;
  - information about the participation incentives; and
  - allowing for and answering questions they had, prior to reading and signing the consent forms.
- As mentioned above, airtime incentives were given for completion of each interview, in each of the rounds.
- Participants were asked for up to four contact numbers including from one relative and from one friend, allowing for lost contacts to be recovered.
- At the beginning of each round, all participants received an SMS notifying them that they should expect a contact.
- Up to ten contact attempts were made to the primary and, if necessary, the secondary phone numbers.

Added to these practices and from this study's results, we can affirm that giving back to participants, by offering them relevant information, especially information related to the survey itself, increases their participation. We, therefore, recommend it as a best practice.

We also find the suggestion that participants in school-to-work panel surveys may not prize a certain type of information above another. That should not mean, however, that the type of information provided is irrelevant. Jones and Santos (2000) offer evidence that more granular labour market information, espe-

[19] It was, therefore, briefly addressed by Jones and Santos (2000).

cially that related to job-seekers' study areas, is more relevant to them and more likely to be integrated into their own labour market expectations.

The weaker robustness of empirical evidence on different survey participation behaviour depending on the type of information offered may, arguably, be the result of a lower sophistication of the target group, who are still learning how to process and value labour market information. It nevertheless places an intriguing choice on whether to provide possibly easier-to-procure information that still stimulates higher response rates, or information that may be more useful to participants. Eventually, one hopes, job-seekers will start valuing as most significant the type of information that most increases their knowledge of the labour market, solving this puzzle.

References

Battaglia, M.P., M. Khare, M.R. Frankel, M.C. Murray, P. Buckley, and S. Peritz (2007). 'Response Rates: How Have They Changed and Where Are They Headed?' In J.M. Lepkowski, C. Tucker, J.M. Brick, E.D. d. Leeuw, L. Japec, P.J. Lavrakas, M.W. Link, and R.L. Sangster (eds), *Advances in Telephone Survey Methodology*. Chichester: Wiley-Blackwell.

Broome, J. (2019). 'Phone Surveys: Introductions and Response Rates'. In P. Liamputtong (ed.), *Handbook of Research Methods in Health Social Sciences*, Singapore: Springer Singapore. https://doi.org/10.1007/978-981-10-5251-4_26

Burkell, J. (2003). 'The Dilemma of Survey Nonresponse'. *Library & Information Science Research*, 25(3): 239–63. https://doi.org/10.1016/S0740-8188(03)00029-X

Cantor, D., B.C. O'Hare, and K.S. O'Connor (2007). 'The Use of Monetary Incentives to Reduce Nonresponse in Random Digit Dial Telephone Surveys'. In J.M. Lepkowski, C. Tucker, J.M. Brick, E.D. d. Leeuw, L. Japec, P.J. Lavrakas, M.W. Link, and R.L. Sangster (eds), *Advances in Telephone Survey Methodology*. Chichester: Wiley-Blackwell.

Cochran, W.G. (1977). *Sampling Techniques*, 3rd edition. Chichester: Wiley.

Curtin, R., S. Presser, and E. Singer (2005). 'Changes in Telephone Survey Nonresponse Over the Past Quarter Century'. *Public Opinion Quarterly*, 69(1): 87–98. https://doi.org/10.1093/poq/nfi002

Debell, M., N. Maisel, B. Edwards, M. Amsbary, and V. Meldener (2020). 'Improving Survey Response Rates With Visible Money'. *Journal of Survey Statistics and Methodology*, 8: 821–31. https://doi.org/10.1093/jssam/smz038

Dillman, D.A. (2000). *Mail and Internet Surveys: The Tailored Design Method*, 4th edition. New York: Wiley.

Fowler, F.J., A.M. Roman, R. Mahmood, and C.A. Cosenza (2016). 'Reducing Nonresponse and Nonresponse Error in a Telephone Survey: An Informative Case Study'. *Journal of Survey Statistics and Methodology*, 4(2): 246–62. https://doi.org/10.1093/jssam/smw004

Gibson, D.G., A. Pereira, B.A. Farrenkopf, A.B. Labrique, G.W. Pariyo, and A.A. Hyder (2017). 'Mobile Phone Surveys for Collecting Population-Level Estimates in Low- and Middle-Income Countries: A Literature Review'. *Journal of Medical Internet Research*, 19(5) :e139. https://doi.org/10.2196/jmir.7428

Groves, R.M., D.A. Dillman, J.L. Eltinge, and R.J.A. Little (2002). *Survey Nonresponse*. New York: Wiley.

Groves, R.M., and E. Peytcheva (2008). 'The Impact of Nonresponse Rates on Nonresponse Bias: A Meta-Analysis'. *Public Opinion Quarterly*, 72(2): 167–89. https://doi.org/10.1093/poq/nfn011

Hansen, K.M. (2006). 'The Effects of Incentives, Interview Length, and Interviewer Characteristics on Response Rates in a CATI-Study'. *International Journal of Public Opinion Research*, 19(1): 112–21. https://doi.org/10.1093/ijpor/edl022

Jones, S., and R. Santos (2000). 'Updating Great Expectations: The Effect of Peer Salary Information on Own-Earnings Forecasts'. WIDER Working Paper 138/2020. Helsinki: UNU-WIDER. https://doi.org/10.35188/UNU-WIDER/2020/895-5

Jones, S., F. Mambo, E. Mazive, Y. Paris, R. Santos, and G. Xirinda (2018). 'Baseline Survey on the School-to-Work Transitions of University Graduates in Mozambique'. Report. Helsinki: UNU-WIDER, University of Copenhagen, and University of Eduardo Mondlane.

Jones, S., R. Santos, and G. Xirinda (2020a). 'Final Report on the Survey on the Education-Employment Transitions of University Finalists'. Report. Helsinki: UNU-WIDER.

Jones, S., R. Santos, and G. Xirinda (2020b). 'Misinformed, Mismatched, or Misled? Explaining the Gap Between Expected and Realised Graduate Earnings in Mozambique'. WIDER Working Paper 2020/47. Helsinki: UNU-WIDER. https://doi.org/10.35188/UNU-WIDER/2020/804-7

Kibuchi, E., P. Sturgis, G.B. Durrant, and O. Maslovskaya (2020). 'Do Interviewers Moderate the Effect of Monetary Incentives on Response Rates in Household Interview Surveys?'. *Journal of Survey Statistics and Methodology*, 8(2): 264–84. https://doi.org/10.1093/jssam/smy026

Lynn, P. (2013). 'Alternative Sequential Mixed-Mode Designs: Effects on Attrition Rates, Attrition Bias, and Costs'. *Journal of Survey Statistics and Methodology*, 1(2): 183–205. https://doi.org/10.1093/jssam/smt015

Marek, P.M., A.V. Peterson, Jr., and M. Henning (2017). 'Design and Results for a Survey of Nonrespondents in a Longitudinal Cohort of Young Adults'. *Journal of Survey Statistics and Methodology*, 5(4): 509–34. https://doi.org/10.1093/jssam/smx013

Olson, K., and J.D. Smyth (2015). 'The Effect of CATI Questions, Respondents, and Interviewers on Response Time'. *Journal of Survey Statistics and Methodology*, 3(3): 361–96. https://doi.org/10.1093/jssam/smv021

Olson, K., J.D. Smyth, and A. Ganshert (2019). 'The Effects of Respondent and Question Characteristics on Respondent Answering Behaviors in Telephone Interviews'. *Journal of Survey Statistics and Methodology*, 7(2): 275–308. https://doi.org/10.1093/jssam/smy006

Pew Research (2017). 'What Low Response Rates Mean for Telephone Surveys'. Available at: https://assets.pewresearch.org/wp-content/uploads/sites/12/2017/05/12154630/RDD-Non-response-Full-Report.pdf (accessed 17 October 2020).

Pew Research (2019). 'Response Rates in Telephone Surveys Have Resumed Their Decline'. Available at: https://www.pewresearch.org/fact-tank/2019/02/27/response-rates-in-telephone-surveys-have-resumed-their-decline/ (accessed 6 October 2020).

Peytchev, A. (2013). 'Consequences of Survey Nonresponse'. *Annals of the American Academy of Political and Social Science*, 645(1): 88–111. https://doi.org/10.1177/0002716212461748

Porter, S.R., and M.E. Whitcomb (2003). 'The Impact of Lottery Incentives on Student Survey Response Rates'. *Research in Higher Education*, 44(4): 389–407. https://doi.org/10.1023/A:1024263031800

Porter, S.R., and M.E. Whitcomb (2005). 'Non-Response in Student Surveys: The Role of Demographics, Engagement and Personality'. *Research in Higher Education*, 46(2): 127–52. https://doi.org/10.1007/s11162-004-1597-2

Sakshaug, J.W., and S. Eckman (2017). 'Following Up with Nonrespondents Via Mode Switch and Shortened Questionnaire in an Economic Survey: Evaluating Nonresponse Bias, Measurement Error Bias, and Total Bias'. *Journal of Survey Statistics and Methodology*, 5(4): 454–79. https://doi.org/10.1093/jssam/smw039

Schoeni, R.F., F. Stafford, K.A. McGonagle, and P. Andreski (2013). 'Response Rates in National Panel Surveys'. *Annals of the American Academy of Political and Social Science*, 645(1): 60–87. https://doi.org/10.1177/0002716212456363

Singer, E. (2006). 'Introduction: Nonresponse Bias in Household Surveys'. *Public Opinion Quarterly*, 70(5): 637–45. https://doi.org/10.1093/poq/nfl034

Smets, L. (2018). 'Impact of Phone Reminders on Survey Response Rates: Evidence from a Web-Based Survey in an International Organization'. Working Paper 8305. Washington, DC: World Bank. https://doi.org/10.1596/1813-9450-8305

Smith, T.W. (1995). 'Trends in Non-Response Rates'. *International Journal of Public Opinion Research*, 7(2): 157–71. https://doi.org/10.1093/ijpor/7.2.157

Smith, W., T. Chey, B. Jalaludin, G. Salkeld, and T. Capon (1995). 'Increasing Response Rates in Telephone Surveys: A Randomized Trial'. *Journal of Public Health (Oxford)*, 17(1): 33–38.

Spreen, T.L., L.A. House, and Z. Gao (2019). 'The Impact of Varying Financial Incentives on Data Quality in Web Panel Surveys'. *Journal of Survey Statistics and Methodology*, 8: 832–50. https://doi.org/10.1093/jssam/smz030

Steeh, C.G. (1981). 'Trends in Nonresponse Rates, 1952-1979'. *Public Opinion Quarterly*, 45(1): 40. https://doi.org/10.1086/268633

Stoop, I., J. Billiet, A. Koch, and R. Fitzgerald (2010). *Improving Survey Response: Lessons Learned from the European Social Survey*. Hoboken, NJ: Wiley-Blackwell. https://doi.org/10.1002/9780470688335

West, B.T., and A.G. Blom (2016). 'Explaining Interviewer Effects: A Research Synthesis'. *Journal of Survey Statistics and Methodology*, 5: 175–211. https://doi.org/10.1093/jssam/smw024

Williams, D., and J.M. Brick (2018). 'Trends in U.S. Face-To-Face Household Survey Nonresponse and Level of Effort'. *Journal of Survey Statistics and Methodology*, 6(2): 186–211. https://doi.org/10.1093/jssam/smx019

Appendix A: extra tables

Table A1: Baseline average characteristics across experimental arms

	Experimental arm				
	Cont.	Gen.	Uni.	Field	*F*-test
UEM	0.36	0.36	0.35	0.34	0.690
	(0.01)	(0.01)	(0.01)	(0.01)	–
UCM	0.08	0.10	0.09	0.09	0.293
	(0.01)	(0.01)	(0.01)	(0.01)	–
UZ	0.11	0.11	0.10	0.09	0.962
	(0.01)	(0.01)	(0.01)	(0.01)	–
USTM	0.05	0.05	0.07	0.06	0.908
	(0.00)	(0.00)	(0.01)	(0.01)	–
UP	0.34	0.31	0.36	0.38	0.091
	(0.01)	(0.01)	(0.01)	(0.01)	–
AP	0.06	0.07	0.03	0.04	0.122
	(0.00)	(0.01)	(0.00)	(0.00)	–
Education	0.31	0.30	0.32	0.32	0.465
	(0.01)	(0.01)	(0.01)	(0.01)	–
Humanities	0.02	0.02	0.02	0.01	0.287
	(0.00)	(0.00)	(0.00)	(0.00)	–
Social sciences	0.44	0.45	0.45	0.44	0.656
	(0.01)	(0.01)	(0.01)	(0.01)	–
Natural sciences	0.04	0.04	0.04	0.04	0.906
	(0.00)	(0.00)	(0.00)	(0.00)	–
Engineering	0.08	0.08	0.08	0.08	0.786
	(0.01)	(0.01)	(0.01)	(0.01)	–
Agriculture	0.05	0.05	0.05	0.05	0.493
	(0.00)	(0.00)	(0.00)	(0.00)	–
Health	0.06	0.06	0.06	0.06	0.752
	(0.00)	(0.00)	(0.00)	(0.00)	–

Note: the cells show means and mean standard errors (in parentheses) across different experimental arms of baseline variables, based on one observation for each individual observed at least once in the follow-up rounds, N = 2,069; 'F-test' reports the probability that the means in the treatment arms jointly differ from the control arm. UEM–AP refer to universities (dummy variables); study fields are given in the final panel (dummy variables).

Source: authors' compilation based on survey responses.

Table A2: Correlates of attrition: personal characteristics

	Personal only		Main Model	
	Simple	Info types	Simple	Info types
Treated	0.59***		0.89*	
	(0.07)		(0.06)	
General		0.35***		0.67***
		(0.06)		(0.09)
University		0.47***		0.71***
		(0.07)		(0.07)
Field		0.36***		0.68***
		(0.06)		(0.08)
Expected wage (baseline)	1.00	1.00	1.00	1.00
	(0.00)	(0.00)	(0.00)	(0.00)
Female	2.39***	2.39***	1.10*	1.10*
	(0.32)	(0.33)	(0.06)	(0.06)
Age	1.02*	1.02*	1.01**	1.01**
	(0.01)	(0.01)	(0.01)	(0.01)
Has job info	1.48***	1.48***	1.03	1.03
	(0.21)	(0.22)	(0.07)	(0.07)
Has job waiting	0.71	0.68	1.01	1.02
	(0.17)	(0.17)	(0.07)	(0.07)
Prior work experience	0.94	0.95	0.95	0.96
	(0.12)	(0.13)	(0.04)	(0.04)
Years in university	0.97	0.97	0.97	0.96*
	(0.06)	(0.06)	(0.02)	(0.02)
Raven's test	0.93	0.94	1.00	1.00
	(0.07)	(0.07)	(0.03)	(0.03)
Academic test	0.92	0.91	0.97	0.97
	(0.06)	(0.06)	(0.02)	(0.03)
Excellent academics	1.15	1.14	1.02	1.03
	(0.14)	(0.14)	(0.05)	(0.05)
Speaks English	2.40***	2.44***	1.07	1.07
	(0.38)	(0.39)	(0.06)	(0.06)
UCM	0.66	0.66	0.89	0.85
	(0.17)	(0.17)	(0.09)	(0.09)
UZ	0.75	0.74	0.83*	0.82**
	(0.19)	(0.18)	(0.08)	(0.08)
USTM	1.05	1.04	0.94	0.93
	(0.24)	(0.24)	(0.07)	(0.07)
UP	0.70**	0.70*	0.94	0.93
	(0.13)	(0.13)	(0.09)	(0.09)
AP	1.55*	1.52*	1.00	1.03
	(0.35)	(0.35)	(0.07)	(0.08)
Humanities	1.08	1.06	0.90	0.92
	(0.32)	(0.32)	(0.12)	(0.13)
Social sciences	1.31	1.33	1.13	1.12
	(0.24)	(0.24)	(0.09)	(0.09)
Natural sciences	0.93	0.93	1.03	1.01
	(0.24)	(0.24)	(0.10)	(0.10)
Engineering	1.06	1.08	1.15	1.13
	(0.32)	(0.33)	(0.16)	(0.16)
Agriculture	1.06	1.06	1.01	1.00
	(0.32)	(0.32)	(0.12)	(0.12)
Health	1.26	1.31	1.09	1.18
	(0.40)	(0.41)	(0.14)	(0.16)
Pseudo log. lik.	–2,088	–2,067	–1,268	–1,266
AIC	4,222	4,184	2,630	2,630
Obs.	11,707	11,707	11,707	11,707
Interviewer f.e.	No	Yes	No	Yes

Note: Significance: * 10, ** 5, *** 1 per cent. Columns (1) and (2) correspond to models, with controls for participants' personal characteristics; columns (3) and (4) correspond to the main model, with survey and process controls; columns (1) and (3) present the non-discriminated information treatment model and columns (2) and (4) present estimates of the different information treatment effects; coefficients are hazard odd-ratios; robust standard errors in parentheses.

Source: authors' compilation based on survey responses.

Table A3: Correlates of attrition: survey-related factors

	Process only		Main model	
	Simple	Info types	Simple	Info types
Treated	0.89*		0.89*	
	(0.05)		(0.06)	
General		0.70***		0.67***
		(0.09)		(0.09)
University		0.72***		0.71***
		(0.07)		(0.07)
Field		0.70***		0.68***
		(0.08)		(0.08)
Monthly sub-sample	1.20	1.06	1.24	1.06
	(0.16)	(0.12)	(0.17)	(0.13)
Phone contacts given	0.72*	0.73**	0.70**	0.71**
	(0.12)	(0.12)	(0.12)	(0.11)
Call duration	0.76***	0.76***	0.76***	0.77***
	(0.03)	(0.03)	(0.04)	(0.03)
Enumerator 1	0.85	0.89	0.88	0.92
	(0.17)	(0.17)	(0.17)	(0.16)
Enumerator 2	1.04	1.04	1.01	1.01
	(0.05)	(0.05)	(0.05)	(0.05)
Enumerator 6	0.97	0.95	0.95	0.93
	(0.03)	(0.04)	(0.04)	(0.04)
Enumerator 7	0.72	0.73	0.72	0.72
	(0.17)	(0.16)	(0.17)	(0.16)
Enumerator 9	0.99	0.97	0.97	0.95
	(0.05)	(0.05)	(0.05)	(0.06)
Enumerator 10	0.76	0.82	0.83	0.89
	(0.21)	(0.23)	(0.25)	(0.28)
Enumerator 12	0.97	0.97	0.97	0.97
	(0.05)	(0.06)	(0.06)	(0.06)
Enumerator 14	0.98	0.97	0.97	0.95
	(0.03)	(0.03)	(0.03)	(0.04)
Enumerator 15	0.97	0.95	0.96	0.94
	(0.03)	(0.03)	(0.04)	(0.04)
Enumerator 16	1.01	0.98	1.02	0.99
	(0.03)	(0.04)	(0.04)	(0.04)
Enumerator 17	0.98	0.97	0.97	0.96
	(0.03)	(0.03)	(0.03)	(0.04)
Enumerator 18	0.96	0.95	0.94	0.92
	(0.04)	(0.04)	(0.04)	(0.05)
Enumerator 19	0.96	0.95	0.95	0.93*
	(0.03)	(0.04)	(0.04)	(0.04)
Enumerator 20	0.63***	0.62***	0.63***	0.61***
	(0.07)	(0.07)	(0.07)	(0.07)
Enumerator 22	0.83	0.82*	0.82*	0.80*
	(0.09)	(0.09)	(0.10)	(0.10)
Interviews on Tuesday	1.01	1.01	1.01	1.01
	(0.01)	(0.01)	(0.01)	(0.01)
Interviews on Wednesday	0.95**	0.95**	0.95**	0.95**
	(0.02)	(0.03)	(0.03)	(0.03)
Interviews on Thursday	0.97	0.96	0.97	0.96
	(0.02)	(0.02)	(0.02)	(0.02)
Interviews on Friday	0.99	0.99	0.99	0.99
	(0.03)	(0.03)	(0.03)	(0.03)
Interviews on Saturday	0.99	0.98	0.98	0.98
	(0.01)	(0.02)	(0.02)	(0.02)
Morning interview	0.99	0.99	0.99	0.99
	(0.01)	(0.01)	(0.01)	(0.01)
Pseudo log. lik.	–1,270	–1,268	–1,268	–1,266
AIC	2,589	2,590	2,630	2,630
Obs.	11,707	11,707	11,707	11,707
Interviewer f.e.	Yes	Yes	Yes	Yes

Note: significance: * 10, ** 5, *** 1 per cent. Columns (1) and (2) correspond to models, with survey process controls; columns (3) and (4) correspond to the main model, with survey and process controls; columns (1) and (3) present the non-discriminated information treatment model and columns (2) and (4) present estimates of the different information treatment effects; coefficients are hazard odd-ratios; robust standard errors in parentheses.

Source: authors' compilation based on survey responses.