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Nudging debtors to pay their debt: Two randomized controlled trials☆

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ABSTRACT

We conducted two large-scale, highly powered randomized controlled trials intended to encourage consumer debt repayments. In Study 1, we implemented five treatments varying the design of envelopes sent to debtors. We did not find any treatment effects on response and repayment rates compared to the control condition. In Study 2, we varied the letters' contents in nine treatments, implementing factorial combinations of social norm and (non-)deterrence nudges, which were either framed emotively or non-emotively. We find that all nudges are ineffective compared to the control condition and even tend to induce backfiring effects compared to the agency's original letter. The results of this study contrast with the findings of other studies, which indicate that comparable nudges are highly effective. Thus, our results are more consistent with the literature suggesting that the success of nudging interventions is limited to certain conditions.

1. Introduction

Traditionally, the archetype of the perfectly rational citizen was used as a fundamental assumption for the design of policy and administrative instruments (Kuehnhanss, 2018). Yet, accompanied by an increasingly critical view of this assumption, insights based on the fast-growing discipline of behavioral science have become more relevant for policymakers and various administrative institutions who act as choice architects and who frequently apply "nudges" (Lunn, 2014; OECD, 2017).

Following the classical definition by Thaler and Sunstein (2008, p. 6), a *nudge* is "*any aspect of the choice architecture that alters people's behaviour in a predictable way without forbidding any options or significantly changing their economic incentives*." Nudges have been shown to offer non-pecuniary ways to subtly induce decisions that can foster personal welfare and the common good while simultaneously preserving individual freedom of choice (see, e.g., Johnson and Goldstein, 2003; Larrick and Soll, 2008; Gallagher and Updegraff, 2012; John and Blume, 2017; Allcott and Kessler, 2019).[1]

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[1] Over the last decade, several public institutions have formed nudge units (see, e.g., Halpern, 2015), geared toward integrating insights from the behavioral sciences into policy design, which have been shown to be effective on average (DellaVigna and Linos, 2020). In particular, the study by

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In this paper, we address the research question of whether simple nudges can help (over)indebted customers repay their private debt. In particular, we present the results of two highly powered randomized controlled trials (RCTs; referred to as *studies* henceforth), in which we used visual framing nudges in Study 1 and factorial combinations of several non-visual nudges in Study 2.[2] For this, we collaborated with a private European debt collection agency (referred to as agency henceforth) that purchases outstanding debts and then collects them from the debtors. In total, 76,000 debtors who owe money to the agency were randomly selected and contacted by hard-copy letter in the course of the two studies. The vast majority of the amounts owed by debtors are consumer debts. The goal of our two studies was to nudge debtors to pay their debts or at least contact the debt collection agency, which also offers debtors individual solutions in the form of personalized payment plans.[3] Arranging such a plan with a debt collection agency is the last resort before the case is brought to an enforcement agency.

Failing to meet one's debt obligations can adversely affect individuals. Helping clients to repay their debts is beneficial in multiple ways. Debt problems can lead to financial penalties or reduced access to future financial services, where often only very expensive credit alternatives such as payday loans remain (Bhutta et al., 2015). In addition to the negative financial effects there is evidence that debt problems decrease psychological well-being (Brown et al., 2005; Webley and Nyhus, 2001), impair decision-making and psychological functioning (Ong et al., 2019), likely decrease job performance (Garman et al., 1996; Carrell and Zinman, 2014), and induce poor health outcomes (Gathergood, 2012; Turunen and Hiilamo, 2014). Thus, evaluating the effectiveness of different policy instruments to spur debt repayments is of societal relevance. We contribute to the literature by investigating the impact of various forms of low-cost nudges on debt repayments and rates of outreach to the debt collector to arrange customized payment plans.

In Study 1, we varied the envelope design of approximately 35,000 hard-copy letters in five conditions to attract debtors' attention and to induce payment plan arrangements and/or repayments. The letters, which all contained the same simplified version of the agency's original standard text, were sent by the debt collection agency to debtors to remind them to repay their debts. In the control treatment, a blank green envelope without visual nudges was implemented. In each of four treatment groups, the green envelope was augmented by one of the following visual nudges: a stamp (or world cloud) indicating the importance of the letter; human eyes watching; a picture of playfully illustrated colorful birds. To compare the success of the interventions with those cases in which no contact with debtors was initiated, we obtained data on responses and repayments from about 7,000 debtors who were not contacted at all. We did not find evidence on systematic effects of the visual nudges on the response and repayment rates of the debtors compared to the control condition. Moreover, only three of the four visual nudges resulted in higher response and repayment rates compared to those of the sample of debtors who were not contacted.

In Study 2, we held the collection agency's preferred envelope design from Study 1 constant, but varied the contents of approximately 41,000 hard-copy letters in nine treatments. The debt collection agency sent the letters only to debtors who had not already been contacted in Study 1. In a baseline treatment, the debt collection agency sent their original (i.e., non-simplified) letter without any nudges to debtors. In all other treatments, the simplified version of the debt collection agency's standard letter text was used. Thus, the control treatment employed the simplified text without any additional nudges, ensuring an internally consistent experimental design. For all other treatments, we added factorial combinations of different nudges to the simplified text, such as descriptive social norm nudges and (non-)deterrence nudges, which were either framed in an emotive or non-emotive way. Compared to the control treatment, we did not find evidence for a systematic impact on the response rate for any of the nudges, except for the non-normative, non-emotive deterrence nudge, which slightly increased response rates. Strikingly, however, with the exception of the non-emotional deterrence nudge, which was not accompanied by a descriptive social norm nudge, all other nudges, as well as the mere simplification of the letter, produced backfiring effects resulting in lower response rates compared to the agency's original standard letter. We found qualitatively similar results with respect to repayment rates and repayments.

This paper contributes to several strands of research. First, we add to the literature on large-scale nudging interventions and nudges on debt repayment. There are numerous scientific studies demonstrating the effectiveness of nudging at scale, for example, with respect to tax compliance or individual retirement planning (see, e.g., Choi et al., 2004; Thaler and Benartzi, 2004; DellaVigna and Linos, 2020). Yet, there also exists empirical evidence suggesting that some large-scale nudging interventions have little to no effect (see, e.g., Löschel et al., 2020; Bird et al., 2021) or may even backfire by causing exactly the opposite of the intended behavior (see, e.g., Liu et al., 2016). Moreover, there is little evidence on the impact of nudging on debt repayment, with what exists to date seeming to indicate rather mixed results. McHugh and Ranyard (2012) show experimentally that providing information about total costs and loan duration leads to higher average debt repayments by bank clients. Similarly, Jones et al. (2015) demonstrate that adding information to standard credit card statements, such as

Benartzi et al. (2017) suggests that the cost-adjusted impact of nudges can be greater than traditional policy instruments, advocating for the even broader implementation of nudges by policymakers.

[2] The relevance of implementing multiple types of nudges for a given outcome has been illustrated by a comprehensive meta-analysis based on 100 publications, which shows that the effect sizes reported in the literature vary across different categories of nudging interventions (Hummel and Maedche, 2019). Overall, Hummel and Maedche (2019) have demonstrated that nudge treatments lead to statistically significant effects in only 62% of the cases.

[3] The idea behind our nudging study that aims at increasing debt repayment rates is that, in principle, people want to pay their debts (e.g., Hallsworth et al., 2017). In this case, nudges are expected to work, as it helps people to act accordingly. In contrast, nudges can backfire when, for instance, people are opposing the nudge or do not want to act (see, for instance, John et al., 2019; Dewies et al., 2021).

reminders of potential penalties and due dates, increases the repayment of credit card debt. However, the authors do not find evidence for the impact on so-called credit revolvers, that is, debtors who typically do not repay their debt in full each month. In a related manner, Hershfield and Roese (2014) report that debtors given a single periodic "minimum" payoff scenario propose higher payments and are more likely to pay off the balance in full than those given a dual payoff scenario. Salisbury (2014) provides mixed evidence on the effect of nudging on debt repayment and shows that the effect may also depend on the specific types of debtors, particularly with respect to knowledge on compound interest. Adams et al. (2018) do not find an effect of removing an automatic minimum payment option from credit card activation, intended to make a payment option that would pay off debt faster more salient. All of the these studies have focused on only one or two specific types of nudges. We contribute to the literature by measuring the effectiveness of visual interventions and factorial combinations of several non-visual types of nudges on debtors' behavior in two highly powered randomized controlled trials.

Second, we add to the literature on specific types of nudges, such as visual framing nudges in Study 1. Visual framing manipulations have been used to support text messages to capture attention and induce the desired behavior (Schneider et al., 2001; Hankammer et al., 2020; Nelson et al., 2021). We expect that design elements of the envelope such as a stamp, a word cloud, or a picture with playfully illustrated colorful birds will serve as visual cues to draw debtors' attention to the letter and encourage them to open it. The latter is especially emphasized in the conditions with the stamp and the word cloud with the corresponding note "Important".

Furthermore, Manesi et al. (2016) find that images of human watching eyes can increase prosocial behavior. The authors argue that lifting the veil of anonymity through these images promotes desirable prosocial behavioral patterns. However, there is also conflicting evidence. In two meta-analyses, Northover et al. (2017) find no evidence for an influence of an exposure to images of watching eyes on prosocial behavior. Nevertheless, there appears to be robust evidence that several types of antisocial behavior are reduced by images of watching eyes (Dear et al., 2019). Since not paying back debt might be construed as anti-social behavior, we considered this kind of visual nudge to potentially be effective in preventing people from simply ignoring the letter from the debt collection agency. However, our results call for caution, as the visual nudges did not produce the hypothesized results.

Moreover, with Study 2, we contribute to the literature on descriptive social norm nudges.[4] With this approach, individuals are provided with descriptions of the prevailing social norm, intending to convey that their own behavior is at odds with that of the norm-compliant majority. Social norm nudges of this kind are supposed to create an incentive to behave in accordance with the norm by changing one's behavior and, on average, have been shown to be effective in inducing the desired behavior in a variety of domains (John et al., 2019). For example, Goldstein et al. (2008) provide evidence from two field experiments where hotel guests have been nudged more effectively toward a more sustainable usage of towels – compared to standard environmental messages – by providing them with descriptions of social norms (majority behavior). Moreover, Hallsworth et al. (2016) show that the prescription rate of antibiotics decreased among general practitioners with high prescription rates when they received institutional feedback on their rate compared to the local majority. Similar results have been reported with respect to energy usage when individuals were informed about average energy use in their neighborhood (Nolan et al., 2008; Allcott, 2011; Allcott and Rogers, 2014) and with respect to sustainable grocery consumption (Demarque et al., 2015). Furthermore, Gerber and Rogers (2009) provide evidence that the suggestion that a great many people will attend upcoming elections significantly increases participants' communicated willingness to go to the polls themselves. In a financial and mandatory context, there exists evidence for a positive effect of descriptive social norm nudges on tax compliance (Hallsworth et al., 2017) what prompted us to test this type of nudging in our likewise mandatory and financial context of debt.

Nonetheless, results on the effect of descriptive social norm nudges seem to be mixed. In particular, it appears that descriptive social norm nudges are particularly susceptible to null effects (Schultz et al., 2008; Fellner et al., 2013; Castro and Scartascini, 2015; Cranor et al., 2020; Dimant et al., 2020) or even backfire (Schultz et al., 2007; Beshears et al., 2015; John and Blume, 2018; Richter et al., 2018; Bicchieri and Dimant, 2019). In general, the effects of social norm nudges can vary widely with respect to the domain and groups to which they are applied. Based on a review on the effects of social norm nudges, John et al. (2019) argue that such nudges may be more effective when they intervene in areas that are more voluntaristic and characterized by more active civic engagement, compared to activities that are more mandatory. Similarly, Dewies et al. (2021) have reported the results of an unsuccessful natural field experiment in a Dutch local government department in which reminder and social norm-nudges were applied to try to increase compliance with a mandatory policy. Based on data from a post-experimental survey, the authors suggest that the specific nudges in such a mandatory context were perceived by some sub-groups as too paternalistic or undermining autonomy, which may have led to conscious rejection of the nudges and the underlying policy. Furthermore, Costa and Kahn (2013) have shown that the impact of descriptive social norm nudges on energy conservation can vary considerably across groups with different political and environmental convictions.

[4] The literature distinguishes between descriptive social norms, which refer to how most other people behave, and injunctive social norms, which describe how most other people think one should behave. For our nudging intervention in Study 2, we focus on the first type – descriptive social norms. For an examination of the effects of injunctive social norm nudges and comparisons to descriptive social norm nudges, see, e.g., Reno et al. (1993), Schultz et al. (2007), Demarque et al. (2015), Bhanot (2021).

With Study 2, we also add to the literature on the effectiveness of deterrence nudges. A deterrence nudge usually includes or highlights the threat of legal or economic sanctions that will occur in case of non-compliance (see, e.g., Slemrod et al., 2001; Meiselman, 2018; Fishbane et al., 2020). A meta-analysis by Antinyan and Asatryan (2020), incorporating over 40 randomized controlled trials, indicates that, on average, non-deterrence nudges do not have an effect on tax compliance whereas deterrence nudges tend to have an effect, although a very modest one. Therefore, due to the similar financial and mandatory context of taxes and debt, we expect the deterrence nudges to be effective in nudging debtors to pay their debt. Finally, we contribute to the small literature on emotive nudges. Esposito et al. (2017) demonstrate that emotive warning messages can reduce the rate of purchases of incompatible goods compared to traditional, non-emotive messages.

2. Study 1: Envelope designs

Study 1 was conducted during November 2019. In particular, the cooperating debt collection agency distributed 34,925 hard-copy letters to debtors during three days (November 18, to 20, 2019), that is, about 11,640 each day.[5] All clients received the identical letter, but in different envelopes. While the design of the envelopes was varied in five treatments, the content of the letter was kept constant across all conditions. The different envelope designs were intended to attract the recipients' attention and induce a response to arrange a customized repayment plan or an immediate repayment.

2.1. Treatment design

We implemented (i) a control treatment (*Control*), with a blank green envelope containing only the agency's logo in the upper left corner (which corresponded to the envelope design previously used by the agency), (ii) a green envelope with a stamp that reads "Important! Open me" (treatment *Stamp*; top left panel in Fig. 1), (iii) a green envelope with a word cloud emphasizing that the letter contained "important information" translated into the ten languages most spoken by the agency's clients (treatment *Cloud*; top right panel in Fig. 1), (iv) a green envelope with playfully illustrated colorful birds (treatment *Birds*; bottom left panel in Fig. 1), and (v) a green envelope with friendly-looking eyes illustrated as if they were watching the recipient from inside the letter (treatment *Eyes*; bottom right panel in Fig. 1). In all treatments, the envelopes included the logo of the debt collection agency in the upper left corner, which has been disguised in all panels in Fig. 1 to preserve the anonymity of the cooperating agency. Similarly, in Fig. 1, text in the local language that would allow inference about the location of the agency has been blurred. The five treatments were randomly assigned to debtors; likewise, the distribution of treatments was randomized across the three days on which letters were mailed, such that each day had a similar distribution of treatments. In addition, the agency provided us with data on a randomly selected sample of 7,073 additional debtors who were not contacted as part of this study (*No Letter*).

In collaboration with the agency, we decided to shorten the original letter text, emphasize the most crucial information, and arrange the contents in a more clear-cut manner. The simplification of the letter was inspired by the common premise that the choice architecture should be designed in a way that minimizes frictions in the decision-making process (see, e.g., Halpern, 2015). As John and Blume (2018, p. 1) put it: "[...] simplification [...] reduces the cognitive burden of reading official communication, increases the directness of the response, and highlights the key communication, making it more salient that the individual has to act." John and Blume (2018) find a 3.8 percentage point increase in people paying local taxes on time in a central London local authority by simplification of a council tax bill. The final version of the letter, which is a simplified version of the agency's previous standard letter, reads: *"We want to help you to be free of your debts. You do currently have one or more cases with us with unpaid debts. On "My Page" you find a breakdown of your debts that today amounts to [amount]. You find "My Page" on [url]. Below you see information about how you can pay and contact us."* In addition, the letter contains an overview about the different payment and contact options and the note that *"[if] you do not have the possibility of paying your debt, contact us so that we can review your case and find a solution that works for you."*

2.2. Power analysis

Based on a comprehensive a-priori power analysis (see https://osf.io/7dnkw/ for details), our sample size of approximately 7,000 letters per treatment (the numbers vary between 6,897 and 7,087) ensures that we can reliably (with a probability greater than 90%) detect an effect size (in terms of Cohen's h) of $h \geq 0.069$ between conditions, assuming a two-sided criterion for detection that allows for a maximum type-I error rate of $\alpha = 0.5\%$ (for details, see the preregistration for trial 1).[6] Note that we assume a 0.5% significance threshold and interpret p-values in the range of $0.005 < p < 0.050$ as suggestive evidence throughout the paper (Benjamin et al., 2018). Accurate predictions of detectable differences in response

[5] The reason for spreading out the distribution of letters over three days is that the agency needed to ensure that they would have sufficient resources to handle incoming phone calls and e-mails. To counter any systematic effects that could potentially arise due to different mailing dates, the five treatment conditions have been randomized by the three days, such that each day had a similar distribution of treatments.

[6] Cohen's h is a proper standardized effect size measure of the distance between two proportions, rather than relative differences. The reason for relying on a standardized measure is that interpreting relative differences in terms of (comparable) economic magnitudes would require to factor in the respective base rates, which Cohen's h takes into account. Cohen's h is typically interpreted based on the same rule of thumb as Cohen's d, i.e., $h = 0.2$ corresponds to a "small" effect, $h = 0.5$ indicates a "medium" effect, and $h = 0.8$ portrays a "large" effect (Cohen, 1988). Given our sample sizes and our choices for the significance threshold and the power level, we are able to detect an effect as small as $h = 0.069$ which qualifies as a "very small" effect.

Fig. 1. Study 1: Envelope designs. The figure shows the envelopes used in treatments *Stamp* (top left), *Cloud* (top right), *Birds* (bottom left), and *Eyes* (bottom right). In all treatments, the envelopes showed the logo of the debt collection agency in the top left corner, which has been canvased in all four panels to preserve the anonymity of the cooperating agency. Likewise, text in the national language that would allow readers to infer the location of the collaborating debt collection agency has been blurred in the two top-row panels.

rates, however, are only possible compared to the *No Letter* sample, where we could assume a response rate of 4.0% based on previous mailings sent by the agency. Thus, our sample size allows the reliable detection of a 1.5 percentage point change in response rates compared to the *No Letter* condition (e.g., from 4.0% to 5.5%).

2.3. Data

The debt collection agency provided us with data that can be categorized into four levels: (i) debtor information, (ii) debt information, (iii) debtor activity records, and (iv) debtor payment history. Thus, the data not only allows identifying responses (to arrange customized repayment plans) and repayments, but also accounts for a debtor's repayment history, total debt size, and demographic information. The data were provided for all debtors contacted by the debt collection agency during Study 1 and for the reference sample of debtors not contacted during the RCT (*No Letter*). All data were pseudonymized; only the collection agency holds the unique key to trace back the data provided to us to the agency's non-anonymous data records.

At the debtor level, we received information on the debtors' year of birth, their gender, and their annual income (in terms of income classes).[7] At the debt level, we obtained information on the current amount of debt[8] and the date the debt collection agency took over the debt.

[7] We obtained data on debtors' income as an ordinal variable with five levels. To avoid the context in which the country in which the RCT has been conducted can be inferred from the currency units (as required by the collaborating debt collection agency), we use generic labels ("income class 1" through "income class 5"). We provide the interval boundaries of the income level in terms of US dollar equivalents as rough estimates: class 1 refers to yearly incomes below $10,000; class 2 indicates incomes of $10,000–$20,000; class 3 indicates $20,000–$30,000; class 4 indicates $30,000–$40,000; and class 5 refers to yearly incomes exceeding $40,000. Note that information on debtors' income is only available for part of the sample. In particular, we lose 5959 observations (17.1%), leaving us with a sample of 28,966, whenever we account for the impact of income.

[8] Since the distribution of clients' debt is substantially right-skewed, we used the log-transformed value of the amount due instead of its level value in all our analyses. Using the log transformation has been preregistered as conditional on the distribution of the eventual data.

Key information to measure the effectiveness of our nudging interventions is available from debtors' payment history and activity log. Specifically, the payment history information includes all previous payments (i.e., the amount and the date of payments) recorded at the debtor level. The activity log file contains timestamps for each type of communication that occurred between the debt collection agency and a particular debtor. Following our pre-analysis plan, we identify debtors' responses to the mailing based on any of the communication means offered in the letter, that is, phone calls, e-mails, and hard-copy letters, within four weeks starting from the day of the outgoing mail (i.e., until December 14, 15, or 16, depending on whether the letter was sent on November 18, 19, or 20).[9]

A secondary line of analyses focuses on debtors' repayments of their debts. Following our pre-analysis plan, we identify debtors' payments within four weeks starting from the day after the outgoing mail (i.e., December 15, 16, or 17, depending on whether the letter was sent on November 18, 19, or 20). Based on the payment records, we define two dependent variables: (i) a binary variable indicating whether at least one payment has been recorded within four weeks and (ii) the relative amount repaid within four weeks, that is, the sum of payments relative to the total amount of debt.

2.4. Results

Unless otherwise indicated, all analyses reported in the paper (and the appendix) were preregistered in the pre-analysis plan corresponding to this RCT (see https://osf.io/7dnkw/ for details). Overall, our sample consists of $n = 34,925$ observations (excluding the *No Letter* reference sample; $n = 7,073$). Debtors are, on average, 43.3 years old ($sd = 13.2$); 43.8% are female. The income distribution among debtors in our sample is positively skewed: 39.3% belong to income class 1 (less than \$10,000), 27.5% to class 2 (\$10,000–\$20,000), 20.5% to class 3 (\$20,000–\$30,000), 9.2% to class 4 (\$30,000–\$40,000), and 3.4% to class 5 (higher than \$40,000). The average debt (at the time the letters were distributed) amounts to \$3,002.92 ($sd = 5,680.88$) and is considerably right-skewed; the median amount of debt is \$853.19 ($min = \$14.04$, $max = \$207,862.60$). The debts have been resting on the agency's books between 0.1 and 7.0 years, with a mean of 3.2 years ($sd = 2.2$). Only 9.1% of the debtors in our sample have effected repayments in the past.

Response rates As highlighted by Fig. 2, we do not find evidence for statistically significant differences in response rates between the *Control* condition (characterized by a blank green envelope), and the treatments that included visual framing nudges as an amendment. In addition, we report no statistically significant differences in response rates between the four visual framing nudges *Eyes, Cloud, Stamp*, and *Birds*.[10] Compared to the non-contacted sample (*No Letter*), the *Control* condition and all visual framing nudges ultimately result in higher response rates, with the exception of the *Eyes* treatment. Yet, it is remarkable to note that the effects of the treatment interventions compared to the *No Letter* condition – even though statistically significant – are rather small in magnitude: the standardized effect size estimate of the largest difference in response rates (in terms of Cohen's h) between conditions is as small as $h = 0.069$. The differences in response rates and the results of pairwise z-tests of proportions are provided in Table A4 in the Appendix.

While our study primarily focuses on the identification of causal effects attributable to the various nudging interventions,[11] the data provided by the debt collection agency allows for additional empirical investigations of the determinants of consumer debt repayments. Given the detrimental consequences of unsettled debt, we consider this approach particularly worthwhile as the results may serve as a basis for more targeted interventions in the future.

To identify debt- and debtor-level characteristics that are systematically associated with whether the agency is contacted in response to the letter, we pool the data from all treatments and model the dichotomous response indicator as the dependent variable in multivariate logit regressions. In particular, in a first model, we regress the response indicator on (i) the size of the debt in logs, (ii) an indicator variable on whether previous payments have been made, and (iii) the length of time the debt has been on the agencies' books in years. In a second model, we add (iv) the interaction term of (i) and (ii); in a third model we include (v) the interaction term of (i) and (iii) instead. In a fourth model, without the interaction terms, we add the debtors' (vi) gender, (vii) age, and (viii) dichotomous indicators of (ordinal) income categories to the equation. In models 5 and 6, we include the interaction terms together with debtors' socio-economic characteristics; models 7–9 are identical to models 4–6, but control for (ix) the possibility of imperfect randomization between treatments by adding treatment controls.[12] The estimation results of the nine regression specifications (in terms of odds ratios) are reported in Table 1.

[9] Types of communication include "letter sent," "letter received," "outgoing call," "incoming call," "outgoing email," "incoming email," "short message (SMS) sent," and "payment plan activity." The debtor's response indicator takes value one if at least one of the debtor's communication means was used to contact the debt collection agency (i.e., "letter received," "incoming call," "incoming email," and "payment plan activity") and zero otherwise, and constitutes one of the dependent variables in our main analysis.

[10] Although marginal in terms of effect sizes, we find suggestive evidence on lower response rates in the *Eyes* treatment compared to the *Stamp* ($h = 0.039$, $p = 0.021$) and *Birds* ($h = 0.047$, $p = 0.006$) condition. For further details, please refer to Table A4 in the Appendix.

[11] Although the coefficient estimates of the treatment indicators will not be affected by controlling for variability in the covariates across treatments if the randomization to treatment conditions was successful, the standard errors of the estimates could be affected by systematic relationships between the dependent variable and control variables. It turns out that the conclusions about the treatment effects remain (qualitatively) robust for all dependent measures (i.e., response rates, repayment rates, and repayments) when controlling for heterogeneity in the covariates considered in the analyses below. In particular, the coefficient estimates of the treatment effects are virtually identical with and without controls (pointing at successful randomization) and none of the results changes in terms of statistical significance.

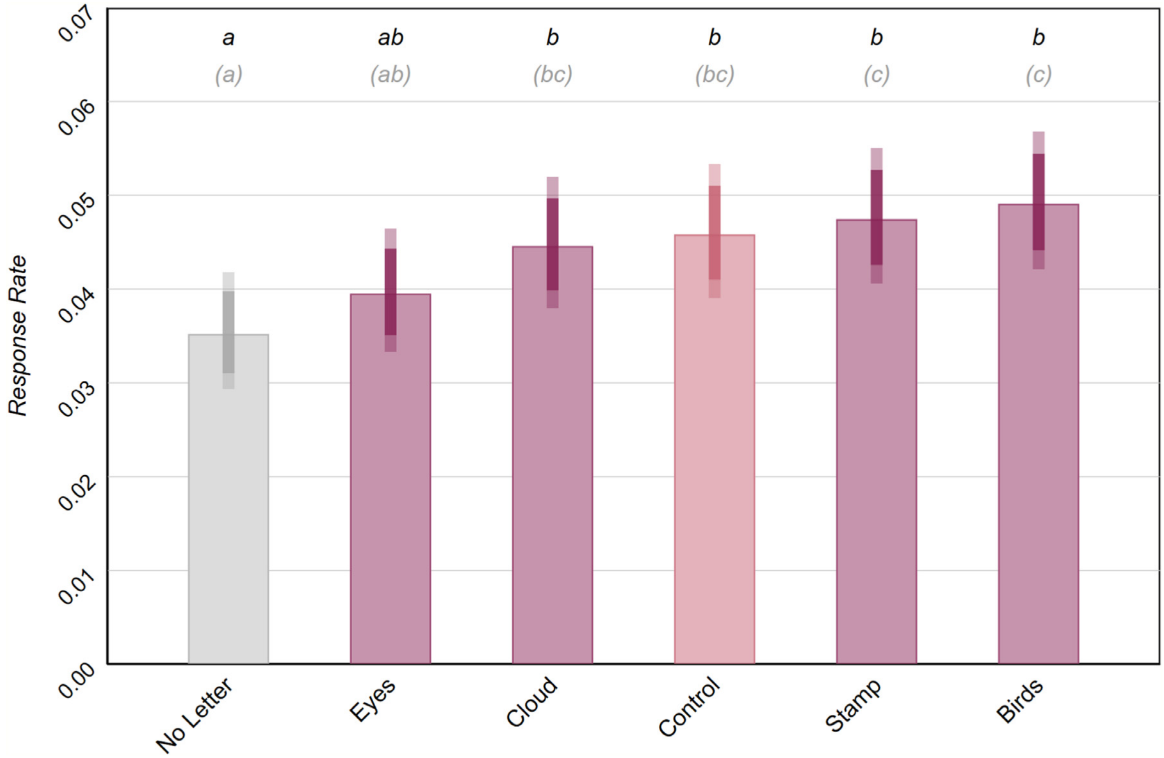

Fig. 2. Study 1: Response rates separated by treatment conditions. Error bars indicate 95% and 99.5% confidence intervals (Clopper-Pearson) for proportions. Black letters indicate significance groupings, that is, treatment conditions with a common letter in the group label do not significantly differ in means ($p > 0.005$). Gray letters in parentheses indicate suggestive evidence groupings ($\alpha = 0.05$). Summary statistics are provided in Table A1; differences in response rates and the results of pairwise z-tests of proportions are provided in Table A4.

Table 1
Study 1: Regression analyses of response rates. The table shows the results of logistic regressions of the response indicator on (i) the size of the debt (in logs; *Debt*), (ii) an indicator variable capturing whether previous payments have been recorded (*Prev. Repayment*), (iii) the length of time the debt has been on the agency's book (*Debt Since*), (iv) gender, (v) age, and (iv) indicator variables for income classes. The sample in all models consists of the five treatment conditions (*Control, Stamp, Cloud, Birds*, and *Eyes*); the reference sample (*No Letter*) is not included in the analyses. In models adjusting for income, income class 1 serves as the reference category. $\times$ indicates interactions. Robust standard errors are provided in parentheses. * $p < 0.05$, ** $p < 0.005$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Debt (log)	1.025	1.016	0.879**	1.023	1.023	0.879**	1.023	1.023	0.878**
	(0.016)	(0.017)	(0.020)	(0.018)	(0.020)	(0.023)	(0.018)	(0.020)	(0.023)
Prev. Repayment	3.190**	1.665	3.155**	3.043**	3.098*	3.004**	3.044**	3.054*	3.005**
	(0.220)	(0.709)	(0.219)	(0.231)	(1.455)	(0.229)	(0.231)	(1.432)	(0.230)
Debt Since (in Years)	0.771**	0.771**	0.392**	0.777**	0.777**	0.406**	0.777**	0.777**	0.405**
	(0.011)	(0.011)	(0.032)	(0.013)	(0.013)	(0.037)	(0.013)	(0.013)	(0.037)
Debt $\times$ Prev. Repayment		1.068			0.998			1.000	
		(0.046)			(0.047)			(0.047)	
Debt $\times$ Debt Since			1.075**			1.071**			1.072**
			(0.009)			(0.010)			(0.010)
Gender (Female = 1)				1.128*	1.128*	1.137*	1.126*	1.126*	1.135*
				(0.066)	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)
Age (in Years)				0.999	0.999	0.999	0.999	0.999	0.999
				(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Income Class 2				1.167*	1.167*	1.185*	1.167*	1.167*	1.184*
				(0.090)	(0.090)	(0.092)	(0.090)	(0.090)	(0.092)
Income Class 3				1.283**	1.283**	1.282**	1.283**	1.283**	1.282**
				(0.103)	(0.103)	(0.103)	(0.103)	(0.103)	(0.103)
Income Class 4				1.291*	1.291*	1.287*	1.287*	1.287*	1.283*
				(0.133)	(0.133)	(0.132)	(0.133)	(0.133)	(0.132)
Income Class 5				1.658**	1.658**	1.641**	1.655**	1.655**	1.639**
				(0.227)	(0.226)	(0.224)	(0.226)	(0.226)	(0.223)
Treatment Controls	No	No	No	No	No	No	Yes	Yes	Yes
Observations	34,925	34,925	34,925	28,966	28,966	28,966	28,966	28,966	28,966
Pseudo R^2	0.044	0.044	0.050	0.047	0.047	0.052	0.048	0.048	0.053

As indicated by the estimates reported in models 1, 4, and 7 in Table 1, we do not find evidence for a systematic effect of the size of the debt (in logs) on the likelihood of responding to the letter. However, we find that debtors who had already made repayments to the agency in the past are significantly more likely to respond to the letter than debtors who had not yet rendered repayments. The effect of previous repayments on the likelihood of contacting the agency to arrange a payment plan is sizeable: the odds for debtors who effected repayments previously – holding the other covariates at fixed levels – are about three times higher than the odds of debtors with a blank payment history. Furthermore, we find that it is statistically significantly less likely that the letter will be responded to later the longer the claim exists on the agency's books: on average, the odds that a debtor contacts the agency decrease by more than 20% for each additional year of indebtedness. Models 2, 5, and 8 in Table 1 indicate that we do not find evidence for a systematic interaction effect of the size of debt and the indicator for previous repayment. Yet, we report that the interaction effect of the amount of debt and the duration of indebtedness is significantly positive (models 3, 6, and 9): the negative associations between the likelihood of a response and the amount of outstanding debt, as well as the duration of the debt in the agency's book, mitigate with increasing debt duration and larger outstanding debt amounts, respectively. Finally, we report suggestive evidence that female debtors, on average, are more likely to respond to the letter compared to male debtors, whereas we do not find a statistically significant association between debtors' age and the likelihood of a response. Finally, we find that – compared to debtors in the lowest income class – the odds of contacting the agency monotonically increase for debtors in higher income classes.

As an exploratory, non-preregistered extension, we also examine whether the impact of any of the covariates on the likelihood of responding to the letter differs between treatments. Specifically, we estimate model 4 reported in Table 1 for each of the treatments separately. The corresponding results are reported in Table A9 in the Appendix. We use Wald tests after performing seemingly unrelated regressions to test whether the coefficient estimates differ significantly between treatments ($\alpha = 0.005$). While we observe some heterogeneity in coefficient estimates across the different treatment conditions, we do not find evidence for systematic differences in the coefficients for any covariate in any pairwise comparison between treatments.

Repayment rates In a second line of analyses, we focus on the results of the visual framing nudges on repayment rates within four weeks after the letters were sent. Overall, the repayment rates turn out to be very small, varying between 0.931% in the *Eyes* treatment and 1.261% in the *Stamp* treatment. Figure A1 in the Appendix shows the repayment rates across the five treatment conditions and the non-contacted sample (*No Letter*); detailed summary statistics are provided in Table A2 in the Appendix. As indicated by Fig. A1 in the Appendix, similar patterns as the results on response rates emerge. Specifically, we do not find evidence for statistically significant differences in repayment rates between any of the visual nudge treatments and the *Control* condition; neither do we find systematic differences in repayment rates between the visual nudges themselves. However, as for the analysis of response rates, we find that all visual framing nudges and the control condition result in a higher repayment rate compared to the repayment rate in the non-contacted sample (*No Letter*). Yet, standardized effect size estimates turn out to be very small across all comparisons; the "largest" effect (*Stamp* vs. *No Letter*) amounts to $h = 0.090$. We leave it again to the reader to assess the economic relevance of these results. A detailed summary of the differences in repayment rates and the results of pairwise z-tests for proportions between all conditions are provided in Table A5 in the Appendix.

In addition to the pairwise comparisons of repayment rates between treatments, we examine the impact of debtor-level characteristics on the likelihood of repayments applying the same regression models as above. The corresponding results are provided in Table A7 in the Appendix. On the one hand, we find that the probability of effecting a repayment, on average, significantly decreases with the amount of debt. On the other hand, we report that the odds of repayments are more than five times higher for debtors who have effected repayments in the past compared to debtors without a payment history. Yet, the positive effect of previous repayments turns out to be moderated by a significant interaction effect with the amount of debt: debtors who have already repaid parts of their debt are relatively less likely to make further repayments the higher their debt position (as of the date on which the letters were sent) is. Furthermore, the odds of repaying (part of) the debt significantly decrease by roughly 26% for each additional year the debt has been dwelling on the agency's books. As opposed to the results on response rates (see Table 1), we do not find evidence for gender effects but a significantly negative impact of debtors' age on the likelihood of actually making a repayment. Finally, in an exploratory analysis, we examine whether the impact of debtor-level characteristics on the likelihood of repayments systematically differs between treatments. Table A10 in the Appendix tabulates the regression estimates for all covariates, separated by treatments. Although the effects of some covariates vary between conditions, Wald tests run after seemingly unrelated regressions suggest that none of the differences in coefficient estimates between any of the pairwise comparisons between models is statistically different from zero ($p > 0.005$).

[12] While all covariates included in the regression analyses have been preregistered, we slightly deviate from the pre-analysis plan regarding how the variables enter the regression equations. In particular, we preregistered regression models including both interaction terms simultaneously. For the sake of interpretability, we decided to report estimates for both interaction terms separately instead; moreover, we chose to expand our analysis by models not taking into account potential moderating effects.

Repayments Finally, we turn to the third dependent variable of interest: debtors' actual repayments in response to the letter (as a fraction of the outstanding debt). While the average repayment rate (across all treatments) is as low as 1.1% (see Fig. A1), the amounts repaid by those who actually settle (part of) their debt turn out to be substantial: on average, debtors settle 51.9% of their obligations.[13] Yet, the very low repayment rate – which is considerably below what we have anticipated when drafting the pre-analysis plan – implies that the number of observations in all analyses on debtors' repayments is smallish ($n = 373$ across the five treatments). Accordingly, the statistical power of the findings is limited, and the results should be interpreted with caution.

The average repayments in each of the treatment conditions as well as in the reference sample of non-contacted debtors are illustrated in Fig. A2 in the Appendix; the corresponding statistics are tabulated in Table A3. As for response and repayment rates, we do not find any evidence for systematic differences in debtors' average repayments attributable to the visual nudges, neither compared to the *Control* condition nor in pairwise comparisons between treatments. Indeed, average repayments do not even differ significantly from the *No Letter* condition; we report only suggestive evidence that three out of four treatment interventions tend to result in higher average repayments compared to the reference sample of non-contacted debtors. A detailed summary of the differences in average repayments and the results of pairwise independent sample t-tests between all conditions are provided in Table A6 in the Appendix.[14]

Apart from examining treatment differences, we again address how the various debtor-level characteristics affect the extent to which debtors repay their debt using the same explanatory covariates as in the analysis of the determinants of response and repayment rates. Since the dependent variable – that is, recorded repayment as percentage of the outstanding debt – is continuously scaled within the unit interval, we fit fractional response models using a logit link for the conditional mean (see the preregistration for further details). The corresponding results are provided in Table A8 in the Appendix. As with respect to repayment rates, we find that the amount repaid is significantly negatively related to the overall outstanding debt. That is, debtors with relatively lower debts are more likely to repay relatively larger parts of their obligations. Notably, however, we find that – on average – debtors who have effected repayments in the past are significantly less likely to repay relatively larger parts of the outstanding debt compared to debtors with a blank payment history. Both the interaction effect of the amount of debt and previous repayment records and the interaction effect of the amount of debt and the time for which the debt has been resting on the agency's books turn out to be statistically insignificant. With respect to the demographic covariates, we do not find systematic associations with debtor's gender or age, but suggestive evidence for a positive relationship with higher income levels compared to the lowest income class.[15] Yet, we remain cautious in interpreting the results on the determinants of repayments due to the relatively small number of observations.

3. Study 2: Nudging interventions

Study 2 was conducted in February 2020. In particular, the debt collection agency distributed about 41,000 hard-copy letters to debtors who owed money to the agency and who have not been contacted in the course of the first RCT. As in Study 1, the letters were distributed to debtors during three days (February 12, to 14, 2020), that is, about 13,670 each day.[16]

3.1. Treatment design

In Study 1, all debtors received the same letter but in different envelopes. In Study 2, we kept the envelope constant, but debtors received letters with systematically varied content. Specifically, we introduced a condition that represented the agencies' original, non-simplified letter text (*Baseline*), a control treatment that included the simplified version of the text without additional nudges (N_0; equivalent to the letter used in Study 1), and seven additional treatments that added factorial combinations of different types of nudges to the standard mailing that were hypothesized to increase response rates and debt repayments. The factorial design allows us to accurately measure and compare the effects of individual and combined nudges on our outcome variables. The preregistration of Study 2 indicates that the envelope design resulting in the highest response rate in Study 1 will be used for all treatments in Study 2. As response rates, repayment rates, and average repayments did not statistically significantly differ in Study 1, the agency decided on which envelope to use in Study 2: the agency opted for the design *Stamp* (see the top left panel in Fig. 1). The nine treatments were randomly assigned to debtors, and the distribution of treatments was randomized across the three days on which letters were mailed.

[13] Note that 28 of 373 (7.5%) of debtors who have actually repaid (part of) their debt within four weeks have repaid more than 100% of the outstanding debt. As we preregistered fractional regressions to investigate whether debtor-level characteristics systematically affect the relative size of repayments, all values larger than 100% have been set to 100%.

[14] Note that in the pre-analysis plan, we did not explicitly mention the use of two-sample t-tests. While we thoroughly outlined the empirical data analysis, the preregistration is silent about what test to use to examine treatment differences in average repayments. Independent sample t-tests appear to be a natural and conservative choice. Applying univariate fractional response regressions with a logit link instead – similar to the preregistered models for the empirical analysis – yields qualitatively and quantitatively robust results, which are provided upon request.

[15] We abstain from examining potential differences in the effects of covariates between treatments (as we do for the analysis of the determinants of response and repayment rates). Although this type of analysis has not been preregistered, it appears to be a reasonable extension of our analysis. However, the low number of observations per treatment – ranging between 32 and 88 (see Table A3 for details) – precludes any meaningful analysis on the treatment level and/or between treatment conditions.

[16] As in Study 1, the reason why the distribution of letters was spread out over three days is that the debt collection agency needed to make sure that they would have sufficient resources to handle incoming phone calls and e-mails.

Table 2
Study 2: Treatment overview. The indicators N_k, E_k, and D_k denote the absence ($k = 0$) or presence ($k = 1$) of descriptive social norm, emotive phrasing, and deterrence nudges, respectively. In addition to the eight treatment conditions summarized in the table, we implemented an additional *Baseline* treatment, in which the agency's previous (non-simplified) standard mailing was used.

		Personal Consequences Nudge				
		no	*non-deterrence*		*deterrence*	
			non-emotive	*emotive*	*non-emotive*	*emotive*
Norm Nudge	*no*	N_0	$N_0E_0D_0$	$N_0E_1D_0$	$N_0E_0D_1$	–
	yes	N_1	$N_1E_0D_0$	$N_1E_1D_0$	$N_1E_0D_1$	–

One dimension of the factorial treatment design was to add the following *descriptive social norm nudge* before the standard text of the letter (which holds true for the country where the RCT was conducted): *"Approximately 1 out of 9 persons in [country] has just like you a debt to a debt collection agency. Thus, you belong to a small part of the population that has a debt of this kind."* The second dimension of the factorial design consisted of three different nudges emphasizing *personal consequences* of (not) repaying their debts. For the sake of denotation, we use the indicators N_k, E_k, and D_k (with $k \in \{0, 1\}$) to label the various combinations of descriptive social norm, emotive phrasing, and deterrence nudges, respectively. Particularly, we used the following phrasing, added before the standard text of the letter (but below the social nudge, if applicable):[17]

- **#1 ($N_0D_0E_0$ & $N_1D_0E_0$):** *"Pay your debt today and save a lot of money! It has the following benefits for you: (i) You avoid additional fees and interest; (ii) You avoid worsening your future economic situation; (iii) You avoid legal actions."*
- **#2 ($N_0D_0E_1$ & $N_1D_0E_1$):** *"Pay your debt and feel free! It has the following benefits for you: (i) You avoid additional fees and interest; (ii) You avoid worsening your future economic situation; (iii) You avoid legal actions."*
- **#3 ($N_0D_1E_0$ & $N_1D_1E_0$):** *"To not pay your debt is very expensive for you! It has the following consequences for you: (i) Additional fees and interest will be added; (ii) You risk worsening your future economic situation; (iii) You risk legal actions."*

Note that the different versions of the personal consequences nudges varied along two dimensions: (i) While #1 and #2 were framed as non-deterrence nudges (D_0), that is, positive (in the sense of benefits resulting from paying off the debt), #3 provided the same information as a deterrence nudge (D_1), that is, negatively framed (in the sense of negative consequences resulting from not repaying the debt). (ii) While #1 and #3 provided objective, non-emotive information (E_0), #2 appealed to the recipient's emotions (*"Feel free!"*; E_1). In this way, the treatment design can be summarized as a 2 (norm; N_k) × 2 (emotive; E_k) × 2 (deterrence; D_k) factorial design (see Table 2).[18] We did not implement the combination of an emotive deterrence nudge, because the corresponding wording of the information contradicted a policy of the cooperating debt collector regarding the communication with debtors.

To thoroughly examine the impact of the nudges summarized in Table 2, we implemented an additional *Baseline* treatment that incorporated the previous standard letter used by the agency for correspondence purposes prior to the first of our two randomized control trials. Thus, in total, Study 2 included nine different treatment conditions.

3.2. Power analysis

Based on comprehensive a-priori power analyses, our sample size of about 4,550 letters ($\approx 41,000 \div 9$) per treatment ensures that we can reliably detect a very small standardized relative effect of Cohen's $h = 0.086$ between conditions with a power of 90% ($\alpha = 0.005$, two-sided tests; see https://osf.io/7dnkw/ for details). Accurate predictions of detectable differences in response rates corresponding to an h of 0.086 are possible compared to the control condition (N_0). Here, we assume a response rate of 4.7%, which conforms to the response rate in the condition with the chosen envelope from Study 1 (*Stamp*), which was used in Study 2. Based on these assumptions, our sample size guarantees that we can reliably detect a 2.4 percentage point change in response rates (e.g., from 4.7% to 7.1%) compared to the control condition (N_0).

3.3. Data

The debt collection agency provided us with the same data as in Study 1. In particular, we obtained data records on (i) debtor's demographics (gender, age, and income),[19] (ii) debt information (amount of debt, time for which the debt has been

[17] Note that in the letters sent to debtors the enumeration of personal consequences was not displayed in the text as in the list below, but rather as a bulleted (vertically arranged) list. Our contracted agreement with the debt collection agency prevents us from publishing copies of the letters for data privacy reasons, as the company and/or the company's country of residence could be inferred from the letters.

[18] We thank an anonymous reviewer for astutely pointing out that the experimental design could also be described more simply as a 2 × 3 design, since we introduce the descriptive social norm nudge dimension and the personal consequences dimension with three different manifestations. However, since we have already pre-registered the 2 × 2 × 2 factorial design described here with our pre-analysis plan and follow this plan very strictly in both studies, we decided to adhere to this design description also in the paper.

[19] Similar to Study 1, information on debtors' income is only available for parts of the sample. In particular, we loose 10,992 observations (26.5%), leaving us with a sample of 30,482 whenever we account for the impact of income.

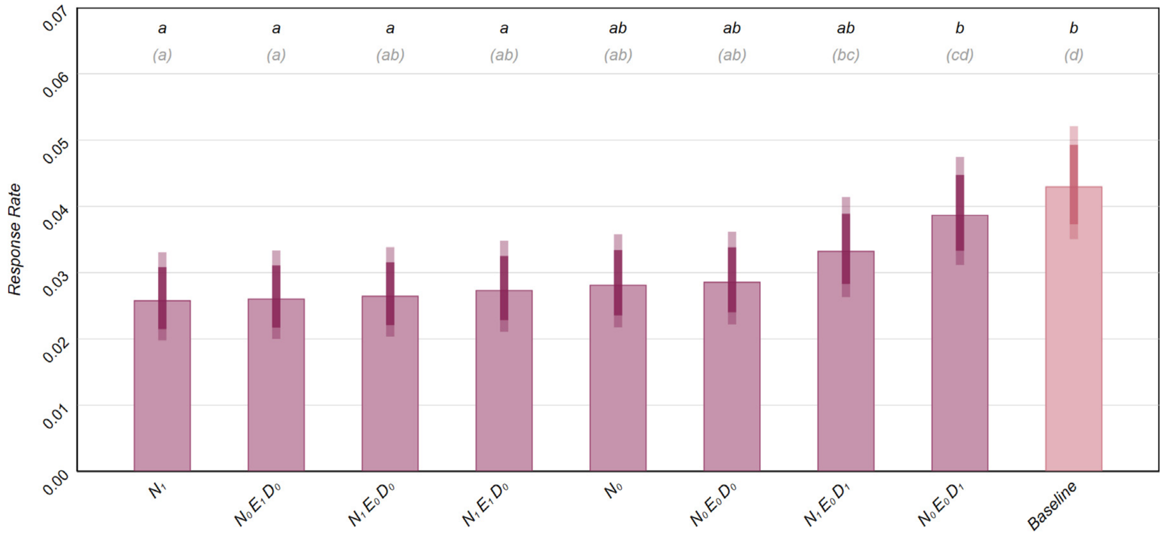

Fig. 3. Study 2: Response rates separated by treatment conditions. Error bars indicate 95% and 99.5% confidence intervals (Clopper-Pearson) for proportions. Black letters indicate significance groupings, that is, treatment conditions with a common letter in the group label do not significantly differ in means ($p > 0.005$). Gray letters in parentheses indicate suggestive evidence groupings ($\alpha = 0.05$). Summary statistics are provided in Table B1; differences in response rates and the results of pairwise z-tests of proportions are provided in Table 4.

on the agency's books), (iii) debtor activity records (logs of communication initiated by the debtors), and (iv) the debtors' payment history. For further details on the available data, please refer to the corresponding subsection in the description of Study 1. As opposed to Study 1, we did not obtain a reference sample of non-contacted debtors.

As in Study 1, the preregistered analyses focus on three dependent variables: (i) a dichotomous variable indicating whether a debtor responded to the mailing based on any communication means offered in the letter (i.e., via phone, e-mail, or hard-copy letter), (ii) an indicator variable identifying whether repayments have been recorded, and (iii) – for those debtors who did effect a repayment – the repayment as a percentage of the outstanding debt. As in Study 1, we identify responses and repayments within four weeks starting from the day of the outgoing mail. For further details, please refer to the preregistration (https://osf.io/7dnkw/).

3.4. Results

Unless otherwise indicated, all analyses reported in the paper (and in the appendix) have been preregistered (see https://osf.io/7dnkw/ for details). Overall, our sample comprises $n = 41,474$ debtors that have been randomly assigned to one of the nine conditions (sample sizes vary between 4,543 and 4,675 across treatments). Debtors are, on average, 42.8 years old ($sd = 13.4$); 44.4% are female. The income distribution among debtors in our sample is positively skewed: 39.6% belong to income class 1 (less than \$10,000), 27.6% to class 2 (\$10,000–\$20,000), 20.1% to class 3 (\$20,000–\$30,000), 9.2% to class 4 (\$30,000–\$40,000), and 3.4% to class 5 (higher than \$40,000). The average debt (at the time the letters were distributed) amounts to \$2,861.75 ($sd = 5,090.39$) and is considerably right-skewed; the median debt level is \$929.32 ($min = \$10.06$, $max = \$205,587.70$). The debts have been resting on the agency's books between 0.1 and 7.3 years, with a mean of 3.4 years ($sd = 2.2$). Only 9.7% of the debtors in the sample have made repayments in the past. Overall, the sample descriptives in Study 2 are very similar to those of the sample in Study 1, suggesting that the randomization of debtors into the two RCTs was effective.

Response rates Fig. 3 illustrates the response rates to the letters for the nine treatment conditions. Detailed summary statistics are provided in Table B1 in the Appendix. As indicated by Fig. 3, none of the treatment interventions differs statistically significantly from the control condition N_0 (characterized by the simplified letter without any nudges). While treatment $N_0E_0D_1$ results in a significantly higher response rate compared to conditions N_1, $N_0E_1D_0$, $N_1E_0D_0$, and $N_1E_1D_0$, all remaining pairwise comparisons turn out not to be statistically significant. The differences in the effectiveness between the "personal consequences" nudges, which indicate a statistically significant higher response rate for the informative deterrence nudge ($N_0E_0D_1$) compared to three of the four non-deterrence nudges, are largely consistent with the results of Antinyan and Asatryan (2020). Notably, however, all interventions tend to result in backfiring effects: the *Baseline* condition, using the agency's previous (non-simplified) letter, actually induces the highest response rate, thus the highest degree of the desired action. Four of the nudging interventions (N_0, $N_0E_0D_0$, $N_1E_0D_1$, and $N_0E_0D_1$) do not statistically differ from the *Baseline* condition, whereas four interventions (N_1, $N_0E_1D_0$, $N_1E_0D_0$, and $N_1E_1D_0$) result in significantly lower response rates compared to the *Baseline*. Similar to Study 1, however, even the statistically significant treatment effects turn out to be smallish in terms of magnitude: none of the differences exceeds a standardized effect size of $h = 0.095$. Again, we leave it

Table 3
Study 2: Regression analyses of response rates. The table shows the results of logistic regressions of the response indicator on (i) the size of the debt (in logs; *Debt*), (ii) an indicator variable capturing whether previous payments have been recorded (*Prev. Repayment*), (iii) the length of time the debt has been on the agency's book (*Debt Since*), (iv) gender, (v) age, and (iv) indicator variables for income classes. In models adjusting for income, income class 1 serves as the reference category. × indicates interactions. Robust standard errors are provided in parentheses. * $p < 0.05$, ** $p < 0.005$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Debt (log)	1.134**	1.152**	1.107**	1.118**	1.133**	1.077*	1.119**	1.134**	1.077*
	(0.019)	(0.021)	(0.031)	(0.023)	(0.025)	(0.038)	(0.023)	(0.025)	(0.038)
Prev. Repayment	3.433**	8.423**	3.429**	3.132**	6.768**	3.126**	3.112**	6.873**	3.106**
	(0.243)	(3.707)	(0.243)	(0.265)	(3.598)	(0.264)	(0.264)	(3.661)	(0.264)
Debt Since (in Years)	0.822**	0.821**	0.756**	0.829**	0.828**	0.728**	0.831**	0.830**	0.728**
	(0.013)	(0.013)	(0.065)	(0.016)	(0.016)	(0.078)	(0.016)	(0.016)	(0.078)
Debt × Prev. Repayment		0.913*			0.925			0.923	
		(0.041)			(0.049)			(0.049)	
Debt × Debt Since			1.009			1.014			1.014
			(0.009)			(0.011)			(0.011)
Gender (Female = 1)				1.155*	1.157*	1.158*	1.164*	1.165*	1.166*
				(0.079)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)
Age (in Years)				0.990**	0.991**	0.990**	0.990**	0.990**	0.990**
				(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Income Class 2				1.068	1.066	1.071	1.075	1.073	1.077
				(0.096)	(0.096)	(0.096)	(0.097)	(0.097)	(0.097)
Income Class 3				1.068	1.068	1.069	1.078	1.077	1.079
				(0.103)	(0.103)	(0.103)	(0.104)	(0.104)	(0.104)
Income Class 4				1.713**	1.717**	1.709**	1.745**	1.748**	1.741**
				(0.189)	(0.189)	(0.189)	(0.193)	(0.193)	(0.193)
Income Class 5				1.811**	1.819**	1.809**	1.861**	1.870**	1.858**
				(0.284)	(0.285)	(0.284)	(0.293)	(0.293)	(0.292)
Treatment Controls	No	No	No	No	No	No	Yes	Yes	Yes
Observations	41,474	41,474	41,474	30,482	30,482	30,482	30,482	30,482	30,482
Pseudo R^2	0.041	0.042	0.041	0.048	0.048	0.048	0.052	0.052	0.052

to the reader to gauge the economic relevance of these results. Details of differences in response rates and in-depth results of pairwise *z*-tests of proportions are provided in Table 4 in the Appendix.

Apart from identifying causal effects attributable to the nudging interventions,[20] we follow up on the empirical analyses – as initiated in Study 1 – with the goal of identifying debt- and debtor-level characteristics that explain whether the agency is contacted in response to the letter. Particularly, we estimate the same regression models as in Study 1; the results are given in Table 3.

As indicated by models 1, 4, and 7 in Table 3, we find that the likelihood of debtors reaching out to the agency in order to arrange a repayment plan significantly increases with higher levels of debt: holding all other predictors constant, a one percent increase in (non-log-transformed) outstanding debt increases the odds of debtors responding to the letter by approximately 0.1%. Although the effect is moderate in size, it is worthwhile to note that outstanding debt turns out to have a systematic effect on response rates in Study 2. Recall that we do not find evidence for a systematic effect of the size of debt on the likelihood of responses in Study 1. The differential effect of outstanding debt on response rates in the two RCTs may suggest that the nudging interventions in Study 2 affect debtors with different characteristics and different types of debt compared with the intervention in Study 1, albeit the observation that the various treatment interventions did not systematically affect response rates.[21] As in Study 1, we report that debtors who had effected repayments to the agency in the past are significantly more likely to respond to the letter than debtors who had not yet made repayments. Particularly, the odds for debtors who repaid part of their debt previously are about three times higher than the odds of debtors with a blank payment record. Also similar to Study 1, we find that the probability of responding to the agency's letter significantly decreases with the time for which the client's debt has been resting on the agency's books: the odds that a debtor contacts the agency in response to the letter, on average, decrease by around 20% for each additional year of indebtedness. In line with the findings in Study 1, we do not find evidence for a significant interaction effect of the size of debt and the indicator variable for previous repayment records. In contrast to Study 1, however, we do not find evidence for

[20] As for Study 1, it turns out that the conclusions about the treatment effects remain unchanged for all dependent measures when controlling for variability in the covariates considered in the regression analyses across treatments. Particularly, the coefficient estimates of the treatment effects are virtually identical with and without control variables and none of the results changes in terms of statistical significance.

[21] In a non-preregistered exploratory analysis, we test whether the effect of outstanding debt on the likelihood of debtors responding to the agency's letter indeed statistically differs between the two RCTs. To do so, we test whether the difference between the coefficients of outstanding debt in model 1 reported in Table 1 and Table 3 differs significantly from zero, using a Wald test after seemingly unrelated regressions. Indeed, the impact of debt on the probability of responding to the letter turns out to be statistically significantly larger in Study 2 compared to the effect in Study 1 ($\chi^2(1) = 21.869$, $p < 0.001$). The difference in the impact of debt on the response rate also turns out to be statistically significant for model 4 (controlling for socio-economic characteristics; $\chi^2(1) = 11.980$, $p < 0.001$), and model 7 (controlling for treatment indicators; $\chi^2(1) = 12.044$, $p < 0.001$).

a statistically significant interaction effect between the size of debt and the time for which the debt has been dwelling on the agency's books.[22] Furthermore, similar to Study 1, we report suggestive evidence on female debtors being more likely to respond to the letter compared to male debtors. In contrast to the results in Study 1, where we do not find evidence for a systematic relationship between debtors' age and the likelihood of responding to the letter, we report a significantly negative association between debtors' age and response rates. Finally, we find that only the higher income classes 4 and 5 are associated with higher response rates in this trial compared to the lowest income class. Overall, the differential findings with respect to the effect of debt size, the interaction effect of debt size and debt duration, and the effects of debtors' age and income between the two RCTs might be interpreted in terms of suggestive evidence that the nudging interventions – despite not systematically affecting the response rate – in fact, lead to subtle disparities in the way different recipients of the letter react to the agency's reminder to pay their debts.

As an exploratory extension, we again examine whether the impact of any of the independent variables discussed differs systematically between treatments. In particular, we estimate the multivariate logit regression as reported in model 4 in Table 3 separately for each of the treatment conditions. The corresponding results are tabulated in Table 9 in the Appendix. We conduct Wald tests after seemingly unrelated regressions to test whether the coefficient estimates differ statistically significantly between treatments ($\alpha = 0.005$). We find no evidence for systematic differences in coefficient estimates for any nine of the covariates in all of the 36 pairwise comparisons between treatment conditions each, except for the effect of *Gender* in the comparison of $N_0E_0D_0$ vs. $N_1E_0D_1$ and $N_0E_0D_0$ vs. $N_0E_1D_0$, and *Age* in the comparison of $N_0E_0D_0$ vs. $N_0E_1D_0$ (see the notes of Table 9 for details). Although the impact of some covariates varies noticeably between treatment conditions, the insignificant differences between the vast majority of coefficient estimates suggest that, overall, the effects of debtor-level characteristics on the likelihood of debtors responding to the agency's letter are largely homogeneous across conditions.

Repayment rates In a second line of analyses, we focus on the effect of the various nudging interventions on the likelihood that debtors effect a repayment within four weeks after the letters were sent out. Figure B1 in the Appendix shows the average repayment rates to the letter for the eight treatment interventions and the *Baseline* condition; the corresponding summary statistics are provided in Table B2 in the Appendix. As illustrated by Fig. B1, similar patterns compared to the results on response rates emerge: all but two out of the 36 pairwise comparisons of repayment rates between conditions turn out to be statistically insignificant ($p > 0.005$), and even the largest effect size is as small as $h = 0.065$. Details on the differences in repayment rates and comprehensive results of pairwise z-tests of proportions are provided in Table 5 in the Appendix.

The regression analyses presented in Table 7 in the Appendix reveal similar drivers of repayment rates as reported for the sample in Study 1. In particular, we find that the likelihood of debtors' repaying part of their debt significantly decreases with the size of their outstanding debt: on average, a one percent increase in (non-log-transformed) outstanding debt decreases the odds of debtors repaying any amount in response to the letter by roughly 0.3%. Possible reasons for this effect in both studies could be, that possibly financial considerations play a role and those who owe higher amounts to the debt collection agency also have debts elsewhere, which prevents them from repaying any amount. There might also be a psychological explanation, and debtors are more likely to be discouraged from repaying something as a result of higher debts, because the problem overwhelms them and they suppress it, while those with lower debts feel able to pay off the debt and act accordingly. Moreover, we find that debtors who have effected repayments in the past are about four times more likely to repay part of their debt in response to the letter. As in Study 1, we also find that the likelihood of repayments significantly decreases with the time for which the debt has been resting on the agency's books: each additional year of indebtedness – ceteris paribus – reduces the odds of repayments by more than 20%. With respect to socio-economic characteristics, we find that the likelihood of repayments is negatively related to debtors' age and positively related to high income classes; gender and medium income levels turn out not to be significantly associated with the probability of making repayments.

In an explorative extension, we again examine whether the effects of debtor-level characteristics differ systematically between treatment conditions. Table 10 presents the results of logit regression analyses of the repayment indicator on the covariates discussed above for each of the nine treatment conditions. While we observe quite some heterogeneity in the impact of some covariates across treatments, pairwise Wald tests after seemingly unrelated regressions suggest that only the impact of gender and age varies systematically between conditions. For details, please refer to the notes of Table 10.

Repayments As a third dependent variable, we examine debtors' repayments (as a fraction of their outstanding debt). As in Study 1, repayment rates turn out to be very low: pooled across all treatment conditions, only about 1.0% of debtors did effect a repayment. Although the average amount repaid (as a fraction of the outstanding debt) of around 50.2% is sizeable,[23] the unexpectedly low share of debtors who repay debt in response to the agency's letter implies that the sample in the empirical investigation of repayments is very limited ($n = 426$ across the nine treatments). The results discussed below should therefore be interpreted with caution.

[22] Wald tests after seemingly unrelated regressions (non-preregistered) to test whether the interaction effects are systematically differing between Study 1 (Table 1) and Study 2 (Table 3) result in statistically significant results for model 3 ($\chi^2(1) = 29.747$, $p < 0.001$), model 6 ($\chi^2(1) = 17.221$, $p < 0.001$), and model 9 ($\chi^2(1) = 17.206$, $p < 0.001$) alike.

[23] Similar to Study 1, some debtors (48 out of 426 who repaid part of their debt within four weeks) have repaid more than 100% of the outstanding debt. As we preregistered fractional regressions to investigate whether debtor-level characteristics systematically affect the relative size of repayments, all values larger than 100% have been set to 100%.

Figure B2 in the Appendix illustrates the average relative repayments by treatments; the corresponding summary statistics are provided in Table B3 in the Appendix. As for debtors' response and repayment rates, we do not find evidence for systematic effects of the various treatment interventions on repayments compared to the *Baseline* condition. We further report only one statistically significant difference between treatments ($N_1E_0D_1$ vs. $N_0E_0D_1$). Comprehensive results on the differences in repayments between treatments and pairwise two-sample t-tests are provided in Table 6 in the Appendix.[24]

As in Study 1, the impact of debtor-level characteristics on repayment amounts is examined using fractional response regressions; the corresponding results are summarized in Table 8. Overall, the results obtained from the empirical analyses closely mirror the findings in Study 1. In particular, we find that the amount repaid (as a percentage of the outstanding debt) is significantly negatively related to the size of the debt and significantly lower for debtors who have effected repayments in the past. We do not find evidence for a systematic effect of the time of indebtedness, and interaction effects between both debt level and previous payments and debt level and duration turn out to be statistically insignificant. Furthermore, we do not report systematic effects of gender and age on the repayment amount; however, we find tentative evidence that repayment amounts increase with higher income levels.[25]

4. Discussion and Conclusion

Failing to pay one's debt can have severe consequences on the individual level. Indebtedness often translates into restricted access to financial services (Bhutta et al., 2015) and has been shown to decrease psychological well-being (Brown et al., 2005; Webley and Nyhus, 2001), impair decision-making and psychological functioning (Ong et al., 2019), likely decrease job performance (Garman et al., 1996; Carrell and Zinman, 2014), and induce poor health outcomes (Gathergood, 2012; Turunen and Hiilamo, 2014). Helping clients to get out of the debt trap and to pay their obligations can thus be beneficial in various regards.

In this paper, we presented the results of two highly powered preregistered randomized controlled trials on the large-scale applicability of nudging interventions in the economic context of consumer debt repayments. In collaboration with a debt collection agency, we implemented various nudging interventions when contacting debtors with payment reminders to encourage them to pay their debts and/or to contact the agency to arrange personalized payment plans.

In Study 1, we applied visual nudges aiming at increasing debtors' attention to the letter and to induce a repayment and/or a response. We sent a total of roughly 35,000 hard-copy letters with different envelope designs to debtors in five treatments. We did not find evidence for systematic effects attributable to the treatment variations – neither on response rates, nor on repayment rates, nor on repayment amounts – for any envelope design compared to the control treatment using a blank envelope without visual nudges. Put differently, the visual nudges incorporated into the various envelope designs turned out to be ineffective in increasing repayments and individualized repayment plan arrangements. The question arises as to what could be possible reasons why the visual framing nudges may have failed and may even have counteracted an eye-catching effect. Since some of the images on the envelopes consist of colorful or rather unofficial-looking illustrations, it could be that the letters were perceived as less serious and less consequential, despite the logo of the debt collection agency. However, this is speculative and more research is needed. Although response and repayment rates were higher in all conditions of the RCT – compared to a reference sample of debtors who were not contacted during the data collection –, the positive effects associated with sending hard-copy payment reminders to debtors turned out to be very modest in terms of effect sizes. A detailed analysis of the actual repayments shows that for most of the nudges the average extra repayment barely covers the costs for mailing the letter: comparing *No Letter* with *Control* we find that sending a letter increased the average repayment (per contacted debtor) from $3.19 to $4.70. Even for the most successful intervention (*Birds*), the net benefit is only $4.35 ($7.54 repayment in *Birds* vs. $3.19 in *No Letter*). Hence, the results, while sometimes statistically significant, do not point toward a strong economic impact.

In Study 2, we varied the contents of the letter sent to roughly 41,000 debtors in nine treatments, implementing factorial combinations of descriptive social norm nudges and (non-)deterrent information nudges, which were either framed in an emotive or a non-emotive way. Strikingly, compared to the control condition (*Control*), none of the nudging interventions induced a significant effect, neither in terms of response rates nor with respect to repayments. Compared to the *Baseline* condition, in which debtors received the agency's previous (non-simplified) payment reminder, most social norm and (non-)deterrence nudges rather led to backfiring effects. Coffman et al. (2015) have introduced and successfully tested a model framework that predicts that direct and indirect information nudges are more likely to backfire when the baseline take-up rate for the desired behavior is low. In our Study 2, the *Baseline* condition in which the original, non-simplified agency letter was sent is an appropriate reference point for our baseline take-up rate in terms of the desired behavior of debtors. This behavior was intended to occur more frequently as a result of the nudges. With respect to the response rate of 4.30% and the repayment rate of 1.38% in the *Baseline* condition, the baseline take-up rate in our study is very low. Thus, our results indicating backfiring effects are qualitatively consistent with the theory and empirical results of Coffman et al. (2015).

[24] As for Study 1, we missed explicitly mentioning the use of independent sample t-tests in the pre-analysis plan. Using fractional response regressions with a logit link – as preregistered for the empirical analysis of repayments – yields qualitatively and quantitatively similar results, which are gladly provided upon request.

[25] As in Study 1, we abstain from the exploratory exercise investigating potential differences in the effects of debtor-level characteristics on repayment amounts between treatment conditions due to the unexpectedly small sample size.

The backfiring effects may also suggest that the common premise that choice environments should be designed in a way that decisions are as easy as possible (see, e.g., Halpern, 2015; John and Blume, 2018) turned out to be detrimental in our setting. Compared to our simplified version of the payment reminder (used as the boilerplate template in all treatment conditions), the letter previously employed by the agency was riddled with legal terminology that might induce the perception that the letter should be taken seriously. This interpretation is consistent with the conjecture expressed in Study 1 that the colorful illustrations may have rendered the letter appear less serious and thus created an impression of less need for action. Combined with the results discussed in the literature, where such simplifications and visual nudges led to behavioral changes in the desired direction, this might suggest that this trade-off between simplicity of presentation and attracting attention on the one hand and seriousness, on the other hand, is particularly crucial in our financial context of debt repayments.

Regardless of the consistently low standardized effects of nudging interventions that we also found in Study 2, it is worth bearing in mind, with regard to possible large-scale implementations of these low-cost nudges, that this is likely to result in a non-negligible number of debtors being influenced in their behavior by such interventions. This can sometimes be to their own detriment, as the reported backfiring effects suggest. Thus, the implementation of low-cost nudges is not necessarily innocuous, neither from a personal nor from a welfare perspective.

Consequently, with our results we support a growing strand in the literature suggesting that the relationship between nudging interventions and desired behavior is much more complex than has long been assumed, adding evidence of null effects and backfiring effects from different types and combinations of nudges. Since a publication bias (Franco et al., 2014) may have led, in part, to the consistent attribution of exclusively positive effects to nudging interventions over a long period of time (see, e.g., Hummel and Maedche, 2019; DellaVigna and Linos, 2020), it is even more important to disclose highly powered studies that report null or backfiring effects of nudges. Future research should investigate the reasons for the backfiring results, as the particular setting might entail special features that could be detrimental to the application of certain types of nudges. We can only speculate on the reasons, but studies by, for instance, Costa and Kahn (2013), Bicchieri and Dimant (2019), John et al. (2019), Dimant et al. (2020), and Dewies et al. (2021) could point in promising directions: (i) According to Costa and Kahn (2013), the effects of nudges can vary considerably across groups with different views and convictions (in their case, environmental convictions). Applied to our setting, one could conjecture that the recipients in our studies constitute a specific group of people, not only relative to the society as a whole, but likely even within the group of debtors with unsettled consumer debt. Systematic characteristics of debtors in our sample could thus be a potential explanation of why nudging interventions that have been shown to be effective in other domains of application are utterly ineffective or even detrimental in the realm of debt repayments. (ii) John et al. (2019) argue that nudges may be more effective when they intervene in areas that are more voluntaristic and characterized by more active civic engagement, compared to activities that are more mandatory. It goes without saying that debt repayments fall within the category of mandatory activities. Accordingly, one could hypothesize that a lack of voluntarism renders the use of nudging interventions ineffective in our domain of application. Similar to Dewies et al. (2021), it also cannot be ruled out that the nudges in this mandatory context were perceived by some debtors as too paternalistic or as interfering with personal autonomy. (iii) Regarding the application of the descriptive social norm nudge it is possible that the very general information about the behavior of the majority of people across the country created too few incentives for conformity. Bicchieri and Dimant (2019) argue that such nudges often fail for reasons related to lack of identification with the reference group to which the norm information refers. It could be that people simply do not care about what distant others are doing, but, depending on the specific behavior at hand, only care about people in their immediate geographic or social environment, such as neighbors, friends, and family. Moreover, Bicchieri and Dimant (2019) and Dimant et al. (2020) argue that a certain framing of descriptive social norm nudges may be ineffective if it does not actually shift the perception of the social norm based on the majority behavior described. Furthermore the authors argue that vague classifications of the reference group could create moral wiggle room for the formation of self-serving beliefs. Finally, regarding descriptive social norm nudges John and Blume (2018) note: “There is a considerable evidence from across a range of domains, such as energy, littering, voting, and recycling waste, that it works [Ǫ].” The authors argue that a common feature of these domains is that compliance with the norm is non-controversial and has a clear public benefit. Since, to our knowledge, our study is the first to apply a descriptive social norm nudge in the context of private debt, we cannot rule out the possibility that compliance with the norm of having no outstanding consumer debt may not be considered uncontroversial. Moreover, the negative externalities of unpaid consumer debt, for example for creditors, might be overlooked.

While all three lines of argumentation appear intuitively appealing, our setting does not allow us to test for their empirical validity. Yet, we deem the aforementioned points being potentially fruitful avenues for future research in this area.

Finally, our empirical investigation of the debt- and debtor-specific determinants of the likelihood of responding to the letter or effecting repayments, as well as the determinants of the magnitude of the amounts repaid, revealed systematic patterns in debtor behavior. Notably, however, exploratory analyses suggested that the various nudging interventions did not systematically moderate the driving forces of debtors’ contacting the agency or repaying their debt. Put differently, our analyses did not identify any specific subgroups among debtors in our sample for which the nudging interventions turn out to be effective. Yet, the identification of systematic patterns in debtor-level characteristics seem to call for more targeted interventions in the future. That is, to help heterogeneous groups of debtors to repay their obligations and pass on the associated consequences likely requires policy measures and economic levers that are tailored to the specific requirements of the particular groups. Our empirical exercise may serve as a first step toward identifying the relevant characteristics for more targeted interventions.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2022.04.006.

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