

Laudenbach, Christine; Siegel, Stephan

Working Paper

Personal communication in an automated world: Evidence from loan repayments

SAFE Working Paper, No. 428

Provided in Cooperation with:

Leibniz Institute for Financial Research SAFE

Suggested Citation: Laudenbach, Christine; Siegel, Stephan (2024) : Personal communication in an automated world: Evidence from loan repayments, SAFE Working Paper, No. 428, Leibniz Institute for Financial Research SAFE, Frankfurt a. M., <https://doi.org/10.2139/ssrn.3153192>

This Version is available at:

<https://hdl.handle.net/10419/303038>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Christine Laudenbach | Stephan Siegel

Personal Communication in an Automated World: Evidence from Loan Repayments

SAFE Working Paper No. 428 | September 2024

Leibniz Institute for Financial Research SAFE
Sustainable Architecture for Finance in Europe

info@safe-frankfurt.de | www.safe-frankfurt.de
Electronic copy available at: <https://ssrn.com/abstract=3153192>

Personal Communication in an Automated World: Evidence from Loan Repayments

CHRISTINE LAUDENBACH and STEPHAN SIEGEL*

ABSTRACT

We examine the effect of personal, two-way communication on the payment behavior of delinquent borrowers. Borrowers who speak with a randomly assigned bank agent are significantly more likely to successfully resolve the delinquency relative to borrowers who do not speak with a bank agent. Call characteristics related to the human touch of the call, such as the likeability of the agent's voice, significantly affect payment behavior. Borrowers who speak with a bank agent are also significantly less likely to become delinquent again. Our findings highlight the value of a human element in interactions between financial institutions and their customers.

JEL Classification: D14, G11

Keywords: Personal Communication, Consumer Finance, Loan Repayment, Promise Keeping, Social Distance.

*Christine Laudenbach is with Goethe University Frankfurt and Leibniz Institute for Financial Research SAFE. Stephan Siegel is with Foster School of Business, University of Washington. Both authors declare that they have no relevant or material financial interest that relates to the research described in this article. We would like to thank Jenny Pirschel who contributed significantly to earlier versions of this article. We are grateful for valuable support from Martin Andresen as well as for helpful comments from Tobias Berg, Tabea Bucher-Koenen, Francesco D'Acunto, Matt Ghatak, Tobias Gesche, Andreas Hackethal, Chris Hrdlicka, Zwetelina Iliewa, Lena Janyts, Dean Karlan, Abhiroop Mukherjee, Thomas Richardson, Sunil Wahal, Mark Westerfield, Johannes Wohlfart, Gaiyan Zhang, and from seminar and conference participants at the FMA Asia 2019, RBFC 2018, Experimental Finance 2018, CICF 2017, Boulder 2017 Summer Conference on Consumer Financial Decision Making, ESMT Berlin, University of Bonn, Goethe University, Maastricht University, University of Illinois (Chicago), Ohio State University, University of Mannheim, University of Muenster, University of Washington, and ZEW. We also thank Samin Jalali and Youpeng Zhang for excellent research assistance. Siegel acknowledges generous research support from the Global Business Center at the University of Washington Foster School of Business. Any errors or omissions are our own.

Correspondence: Stephan Siegel, Foster School of Business, University of Washington, 4273 E. Stevens Way NE, Seattle, WA 98195; email: ss1110@uw.edu.

Consumer finance is changing rapidly. Consumer financial services are increasingly provided online, drastically changing the nature of the interaction between financial service providers and their customers. The traditional two-way, face-to-face communication in a bank branch is being replaced by one-way, often automated communication in the form of letters, emails, and text or online messages. In Germany, for example, the number of bank branches dropped by about 40% between 2000 and 2015, and about 60% of bank–customer interactions now take place outside of bank branches (see Accenture (2015), Deutsche Bundesbank (2016)). While this change in interaction between banks and their customers creates opportunities for faster and less costly provision of financial services, the less personal form of communication could have important consequences. For example, households can increasingly obtain credit instantaneously and often without much personal interaction with the lender. However, it is unclear whether the less personal relationship and communication affects borrowers’ behavior, especially when borrowers experience difficulties making timely payments.

To address the importance of the form of communication between banks and their customers, particularly their borrowers, we compare the effect of person-to-person communication to the effect of impersonal, one-way communication on the payment behavior of borrowers who have fallen behind on their monthly loan payments. Specifically, while all borrowers in our sample receive written communication about the missed payment in the form of a letter from the lender with a two-week deadline for payment, “treated” borrowers also speak with a randomly assigned bank agent. During the phone conversation the bank agent informs the borrower about the delinquency and asks the borrower to make the necessary payment within a few weeks after the call. Importantly, the agent does not convey any additional information relative to the bank’s letter nor can the agent change the existing loan terms or accept payment over the phone. Treated borrowers who fail to pay within the agreed upon time frame receive a similar follow-up call. In contrast, “untreated” borrowers—that is, borrowers who do not speak with a bank agent—receive a second letter from the bank if they do not make payment by the end of the two-week deadline. Importantly, the bank provides the same information to treated and untreated borrowers and monitors the timely payment by both types of borrowers. Differences in payment behavior therefore allow us to learn about the importance of the form of communication between banks and their borrowers.

Our data are from a large German bank with a substantial presence in the retail banking segment. The consumer loans in our main sample are in the form of point-of-sale (POS) installment loans that are originated by the bank but initiated through retail stores as part of a sales transaction of certain consumer durables, such as furniture, appliances, and electronics.

Given the nature of POS loans, borrowers typically do not have a pre-existing relationship with the bank. In 2009, the bank set up a bank-operated, early collection call center to speak with borrowers early on in the delinquency in order to resolve the delinquency occurrence and to minimize the probability of default and loan termination. The data in our sample are part of the bank's first assessment of the call center's activity in 2012. Specifically, the data set consists of delinquent POS borrowers with a valid phone number and without a pre-existing relationship with the bank whose records enter the call center for the first time between January and June 2012. Importantly, the data allow us to compare treated and untreated borrowers within the same institution and at the same point in time.

While the bank's objective is to reach all delinquent borrowers, not all borrowers speak with an agent. Some borrowers make payments before they are reached and others are not reached over the two-week window, during which bank agents make up to ten call attempts (see Figure 1 for an overview of the process). Whether a borrower speaks with a call center agent is, of course, not entirely randomly determined and might reflect borrowers' characteristics that could also affect borrowers' payment behavior. In order to address endogeneity concerns and identify a causal effect of personal, two-way communication between the bank and its delinquent borrowers, we exploit random variation in the likelihood that a borrower is reached. For administrative reasons, the bank treats Saturday, the day with the highest chance of reaching borrowers, and Monday, the day with the lowest chance, as one day and assigns borrowers randomly between both days.¹ Even borrowers with multiple call attempts might therefore never receive a call on a Saturday. Since the number of calls itself is likely endogenous, we only exploit variation with respect to the day of the first call. As we explain in detail below, borrowers with a first call on Saturday have the highest overall chance (96%) of speaking with a bank agent, while borrowers with a first call on Monday have the lowest overall chance (43%). All other borrowers have an intermediate overall chance (71%) of speaking with a bank agent.

[Figure 1 about here]

Our instrumental variable (IV) regressions reveal a significant and substantial difference with respect to payment, default, and loan termination between treated borrowers who speak with a bank agent and untreated borrowers who do not. Specifically, we find that personal communication between a borrower and a bank agent increases the probability of timely repayment by 34.4 percentage points, while decreasing the chance of default and termination

¹The bank has since optimized its procedures. The lack of sophistication during our sample period can be explained by the early nature of the early collection call center.

by 23.8 percentage points and 12.4 percentage points, respectively. While these estimates are large, it is important to recall that they reflect local average treatment effects (LATEs) and not average treatment effects (ATEs). We indeed find that marginal treatment effects (MTEs) differ across borrowers, with MTEs increasing in absolute value in the unobserved resistance to treatment. That is, borrowers who are hard to reach due to unobservables respond more to treatment than easy-to-reach borrowers. Finally, under additional assumptions, the estimated MTEs imply ATEs, which are approximately half the size of the IV estimates but still statistically significant for all three outcomes and hence consistent with an important effect of personal communication on borrowers payment behavior.

A number of different mechanisms are possibly consistent with our results. First, borrowers might ignore the letter from the bank, for example, by mistaking it for advertising mail or they might find it difficult to process written information relative to spoken content. The phone conversation with a bank agent would then mainly function as a reminder. Second, borrowers might be aware of the delinquency but might not expect a phone call from the lender. The bank's effort associated with the phone call would then signal the seriousness of the delinquency and make the bank's enforcement intention more salient than a machine-generated letter. Third, it is also possible that the treatment effect is due to the subset of treated borrowers who receive a follow-up call, suggesting that an additional contact with the borrower in the form of personalized monitoring is particularly important. Finally, the phone conversation with a bank agent might add a personal element to the existing payment obligation and the personal communication treatment could motivate treated borrowers to behave in a more trustworthy way. To shed light on the likely mechanism, we first document that the call effect is not limited to borrowers with small outstanding loan amounts who might have forgotten about their payment obligation or who might be surprised about the bank's collection efforts, but holds as well among borrowers with large outstanding loan amounts who are less likely surprised by the bank's collection efforts. Next, for a separate data set of delinquent borrowers, we survey call center agents and ask them to record borrowers' reactions to the phone conversation. We find that borrowers who seem surprised about the bank's phone call (based on agents' assessments) are not more likely to make timely payment compared to borrowers who do not appear to be surprised. These results do not suggest that the call mainly serves as a payment reminder or as a means to emphasize the serious nature of the delinquency. When focusing on payment outcomes unaffected by a follow-up call, we find that most of the treatment effect obtains without a possible follow-up call. This suggests that the increased frequency of communication in the form of personalized monitoring is not the main determinant of the observed treatment effects. Finally, we examine whether the personal aspect of a phone conversation with a bank agent increases borrowers' willingness

to keep their payment promises. If the human touch of the phone call operates through the borrower's prosocial or trustworthy behavior, the effect should be stronger the lower the social distance between the borrower and the call center agent. Therefore, in our main sample, we compare the call effect between German and non-German borrowers. Consistent with a possibly larger social distance and hence a reduced activation of prosocial behavior among non-German borrowers, who are likely less integrated into German society, we find a smaller effect for non-German borrowers. In the data set corresponding to our follow-up survey, we are able to link individual agents to the borrowers with whom they speak. Even though all agents receive the same communication training, use the same protocol, and convey the same information, we find significant differences in payment behavior across borrowers who speak with different, randomly assigned call center agents. Using voice recordings for some of the agents, we are able to assess the likeability of the agents' voices. The likeability of agents' voices should be irrelevant for the effect of the call on borrowers' payment behavior if calls function only as reminders. However, if the phone conversation operates through borrowers' prosocial behavior, a likeable agent voice could matter, as it might lower the perceived social distance and increase borrowers' promise-keeping behavior. We indeed find that the likeability of agents' voices significantly increases the probability of payment, consistent with the personal nature of the call playing an important role in its effect on borrowers' payment behavior.

In two extensions, we analyze possible long-term effects of the phone conversation and extend our analysis to a different type of loan at the same bank. First, examining the persistence of the effects of personal communication beyond the initial delinquency, we find that talking to a bank agent not only reduces the short-term probability of loan termination, but also reduces the probability of a future delinquency and loan termination. These results further confirm that the treatment effect extends beyond alerting borrowers to the current delinquency as well as monitoring timely repayment. Last, we provide evidence in support of the external validity of our results by showing that similar treatment effects obtain in a sample of bank customers who have their primary checking account with the bank and become delinquent with respect to their overdraft credit lines.

Our study contributes to research on the importance of the human element in bank lending, in particular with respect to the relationship and the communication between banks and their customers. Berg (2015) documents the value of human risk assessment in the case of mortgage lending decisions, while Agarwal et al. (2017) provide high-level evidence that institution-specific human capital-related factors are an important determinant of loan renegotiations. Other studies have highlighted the importance of bank relationships in screening and monitoring borrowers (see Petersen and Rajan (1994) and, for an overview, Kysucky

and Norden (2016)) as well as serving as an incentive for borrowers to make timely payments (see Puri, Rocholl, and Steffen (2017), Agarwal et al. (2018)). The relationship between a bank and its customers is naturally shaped by individual bank agents. Drexler and Schoar (2014), for example, show that severing the relationship between a borrower and her loan officer reduces the borrower's future loan access at the bank, while Schoar (2012) and Karlan, Morten, and Zinman (2015) highlight the importance of the personal nature of the relationship between loan officers and borrowers for loan repayment. Our study provides evidence that personal, two-way communication with a delinquent borrower significantly affects the probability that the delinquency is resolved and that default and loan termination are avoided. Similar to the role of borrower–bank relationships, the form of communication acts as a non-contractual factor on loan repayment behavior. However, in our setting the bank does not collect any new information about the borrower nor does the bank renegotiate the existing loan terms as part of the phone conversation with the borrower. Finally and importantly, in the case of our main sample of POS loans, the effect of reducing moral hazard obtains in the absence of any existing meaningful relationship between the bank and borrower.

Our findings are also related to several papers that examine the effectiveness of treatments and interventions designed to improve the payment behavior of retail borrowers. Cadena and Schoar (2011) and Medina (2021) show that simple reminders of upcoming payments can lower delinquencies and late fees, in some cases as effectively as economic incentives in the form of refunds or future rate reductions. However, Karlan, Morten, and Zinman (2015) find that simple reminders are only effective if they include the name of a loan officer that is known to the borrower. Moulton, Collins, and Loibl (2015) provide evidence that first-time home buyers in the United States who receive quarterly financial coaching experience significantly lower mortgage delinquencies than comparable borrowers who do not receive coaching. Finally, in a setting similar to ours as it focuses on the recovery and default of already delinquent borrowers, Bursztyn et al. (2019) find that among delinquent credit card borrowers in Indonesia, text messages emphasizing moral or credit-reputation-based incentives significantly increase repayment and, for high-risk borrowers, significantly reduce default. We show that the form of communication can make a significant difference with respect to loan outcomes of delinquent borrowers that is comparable in size to other economic or behavioral interventions.

A large body of theoretical as well as experimental research argues and shows that human behavior towards other humans is at least partly prosocial, i.e., altruistic, fair, and trustworthy (see, for example, Rabin (1993), Fehr and Schmidt (1999), Akerlof and Kranton (2000), Battigalli and Dufwenberg (2007), Andreoni and Bernheim (2009)). However, compliance

and promise-keeping behavior seem significantly less prevalent in the interaction between humans and institutions or machines, even if these machines are equipped with human features like pre-recorded voice messages (Haran (2013), Melo et al. (2016)). Our findings provide important field evidence consistent with prosocial and promise-keeping behavior, which is elicited partly through personal, two-way communication as opposed to machine-generated, one-way communication and increases as the perceived social distance decreases.

Finally, our paper is related to recent research on the impact of technology and new communication formats on consumer finance. For example, D’Acunto, Prabhala, and Rossi (2019) document that robo-advising can reduce behavioral biases, while Carlin, Jiang, and Spiller (2018) show that access to online advice, particularly in video form, improves financial decision making. Berg et al. (2020) show that digital footprints have the potential to improve access to credit for the unbanked by overcoming information asymmetries between lenders and borrowers. Our findings do not cast doubt on such improvements, but our results suggest that retaining or adding a human element in the communication, for example between lenders and borrowers, might increase customers’ willingness to honor an existing financial obligation. Our results therefore suggest that costly personal communication will likely continue to play a role even if much less costly information transmission is readily available.

I. Institutional Setup and Data

A. *Institutional Setup: POS Loans and Early Collection*

Our main data of POS loans are from a large German bank. The bank is among the top five largest retail banks in Germany and among the top 10 largest retail banks in Europe, serving more than 10 million retail customers in Germany through a Germany-wide network of more than 1,000 bank branches. It offers a wide range of retail banking products, such as checking and savings accounts, brokerage and investment services, and consumer loans and mortgages. The bank is also one of the top five POS loan providers in Germany, underwriting sales financing for several national and international retail chains in Germany. A POS loan is a form of installment credit that is originated at the time of the purchase of certain durable items, such as furniture, appliances, or electronics.² Because POS loans are offered through a retailer as part of a sales transaction (but underwritten by a financial institution, such as a bank), most POS borrowers have no other relationship with the POS lender.

Based on a survey by the German Association of Credit Banks, in 2012 about 40%

²Car financing through a car dealer can also be considered POS financing. However, car financing is not part of our data set.

of adults in Germany used at least one form of uncollateralized consumer financing, with 28% having at least one installment loan, excluding car loans, and 26% using some form of revolving credit. Among borrowers with installment loans, 68% had traditional personal consumer loans, typically from a bank, while 30% (or 8.4% of all adults) used POS loans to finance the purchase of non-car durables. According to the survey, average initial POS loan amounts were between €600 to €800 for purchases of electronics and €1,900 for purchases of appliances and furniture, with average maturities between 18 and 24 months. Based on data for several European countries (Eurofinas (2013), Deloitte (2019)), POS loans, which have similar characteristics across Europe, account for about 52% of all new installment and revolving credit contracts per year. In recent years, POS lending has also been growing in the United States (where consumer finance has traditionally been dominated by revolving credit in the form of credit cards) (McKinsey & Company (2020)), reflecting borrowers' growing preference to borrow at the point of sale at interest rates that are often heavily subsidized by merchants.

As one of the leading POS lenders in Germany, the bank uses lending and collection processes that are similar to those of other providers in Germany as well as Europe, especially since the 2008 Consumer Credit Directive of the European Union has harmonized consumer lending across Europe. According to the bank, about 8% of POS borrowers become delinquent with respect to their POS loan. Two weeks after a borrower misses a payment, the bank sends a payment request to the borrower in the form of a machine-generated letter that is sent via regular postal mail (see Internet Appendix Figure IA1 for a sample letter). The letter informs the borrower about the delinquency and asks the borrower to pay the outstanding amount within two weeks. Once the loan is 45 days past due, the bank's in-house advanced collection department attempts to reach the borrower to accelerate the collection process and, if necessary, to make loan modifications. The loan is in default once the delinquency exceeds 90 days. At that point, the bank typically terminates the loan and might sell the loan or engage a third party for collection.

A few years before our sample period, the bank introduced an additional step into the early collection efforts for all its retail lending products, including POS loans. The bank started operating an additional and separate in-house call center to reach out to borrowers with payments that are overdue for at least 20 days. Similar to the machine-generated letter that is still sent out to delinquent borrowers at day 15 of the delinquency, the phone call notifies the borrower of the delinquency and asks the borrower to pay within 45 days of the delinquency (see Figure 1 for an overview). The bank considers the phone call a "customer care call" and does not pressure or threaten the borrower nor does the bank offer any loan modifications during this early stage of the collection process.

The call center agents are bank employees who receive a largely fixed salary. Variable compensation, at most 10% of agents' fixed salary, is based on overall customer satisfaction and team performance. Agents usually speak accent-free German, and calls are very standardized as agents follow highly prescribed protocols. Employees are trained to interact professionally with customers even if a customer may be frustrated. Anecdotal evidence from surveys conducted by the bank as well as from our on-site visit suggests that customers' reactions to the bank's customer care calls are overwhelmingly positive.

Each borrower is called once per day at most. Borrowers who are not reached on a given day are generally, but with an important exception that we will discuss in detail below, called again the next day. The process continues for about two weeks and up to 10 call attempts. The process stops when the borrower is reached by a call center agent or when the borrower makes the outstanding loan payment.

Calls are made between 8 a.m. and 7 p.m. on weekdays and between 9 a.m. and 1 p.m. on Saturdays. Each day, the call center manager determines the sequence in which different loan product groups, such as POS loans, overdrafts, or mortgages, are processed. However, the order in which individual borrowers within a given product group are called is randomly determined through an auto-dialer. Importantly, the loan amount, the days in delinquency, or other loan- or customer-specific characteristics do not influence the order in which the auto-dialer selects borrowers on a given day. Finally, borrowers and call center agents are randomly matched via the auto-dialer.

When call center agents reach a borrower by phone, they introduce themselves and inform the borrower about the delinquency, even though borrowers, having already received the bank's letter, should be aware of the delinquency. They then ask borrowers to pay the overdue amount (see Internet Appendix Figure IA2 for a stylized sample call). While the default time period is two weeks, borrowers can propose a shorter and, if within the 45-day payment target, a longer period. Agents are instructed and trained not to intimidate or threaten borrowers but to maintain a friendly and professional attitude in these customer care calls. Importantly, agents are not authorized to accept payment over the phone, agree on any kind of credit deferral, or change the general payment schedule. After the call, borrowers receive a confirmation letter with payment instructions. Borrowers who are reached but fail to pay within the agreed period enter the call center process again. If reached again before day 45, the call center agent will ask for payment by day 45.

Untreated borrowers, i.e., those who are not reached within the initial two-week period, are expected to pay within the two-week period communicated in the initial letter by the bank. If they are still delinquent 35 days after the beginning of the delinquency, they receive

a second letter from the bank with the request to pay the outstanding amount within one week.

Finally, all borrowers who do not pay within 45 days of delinquency, regardless of whether they have been reached by an agent or not, are transferred to the advanced collection department that has substantially more discretion to negotiate a repayment schedule with the borrower or to terminate the loan.

While the overall debt collection process followed by the bank in our sample is similar to the process of other banks in Germany and other countries, including the United States (Consumer Financial Protection Bureau, 2017), details of the early collection process differ across lenders and countries. Calling delinquent borrowers during early stage collection is a common practice in the United States as well as in many emerging markets (International Finance Corporation (2012), Consumer Financial Protection Bureau (2017)). However, German banks, including the bank in our study, introduced early collection calls only in the last decade. Indeed, the main data set in our study was part of the first review conducted to assess the effectiveness of calling delinquent borrowers relatively early in the delinquency. Our findings offer some reasons why financial institutions around the world maintain or reintroduce personal elements, even if in an ad-hoc way, in the communication with their customers.

B. Data

B.1. Administrative Bank Data

Our main sample consists of POS borrowers who become delinquent for the first time and whose records enter the call center data pool at day 20 of their delinquency, between January and June 2012.³ We exclude all borrowers who did not provide a phone number at the time of loan application or whose phone numbers are out of service or no longer affiliated with the borrower. Furthermore, we include only borrowers with at least one call attempt. Finally, to abstract from any possible effect due to an existing bank relationship, we exclude borrowers who have their main checking account with the bank. Panel A of Table I summarizes the characteristics for our sample of 3,448 delinquent POS borrowers. Detailed definitions of all variables are provided in Internet Appendix Table IA1. The majority of borrowers are male and German citizens, making up 65% and 80% of borrowers, respectively. Only a small

³Borrowers who have filed for personal bankruptcy and borrowers with any ongoing judicial collection efforts (“gerichtliches Mahnverfahren”) are excluded from our sample. In the collection process, their files are sent directly to the advanced collection department.

faction are students (10%) or retired (4%) at the time of the loan application. On average, borrowers are 34 years old. The average initial loan amount is €1,110. The average (median) interest rate is 1% (0%). Indeed, 84% of the POS loans in our sample have an interest rate of 0%, which is common for POS loans. The average loan maturity is about 17 months at origination and delinquency occurs, on average, eight months after origination. The missed payments that caused the delinquency range between €25 and €558, with a mean (median) amount of €70 (56).

[Table I about here]

On average, call center agents make 3.24 initial call attempts per borrower. They ultimately reach and speak with 72% of the borrowers in our sample, in which case our treatment variable *Talk* takes on the value of one as opposed to zero. Borrowers with few call attempts are either reached or make payments early in the process and therefore remain unreached. Given the limited scope of agents' actions, most phone conversations are short, with an average (median) duration of about 2.68 (2.19) minutes. We do not have transcripts of the phone conversations, but based on discussions with call center agents as well as our own observations of about 100 calls during a visit to the call center, some calls last longer mainly due to the time it takes to get the relevant person on the phone and due to some borrowers explaining the circumstances of the delinquency or engaging in small talk.

To assess how delinquent borrowers respond to speaking with a bank agent, we consider three outcomes related to the current delinquency: *Payment*, *Default*, and *Termination: Now*. *Payment* is equal to one if the borrower pays the outstanding amount by the bank's target of 45 days since the initial occurrence and zero otherwise. *Default* is equal to one if the borrower's payment remains overdue for more than 90 days and zero otherwise. Default is an outcome that is particularly important from the bank's perspective, as default implies that the bank must recognize impairment losses. Finally, *Termination: Now* is equal to one if the loan is terminated by the bank as a result of the current delinquency and zero otherwise. Once the loan is terminated, the entire loan balance becomes due and the original loan agreement can no longer be restored even if the borrower paid the overdue installment payments. Termination also leads to a persistent negative entry ("hartes Negativmerkmal") in the borrower's credit report.⁴ The summary statistics show that 70% of borrowers make

⁴Given these significant negative consequences for borrowers, a lender can only terminate a loan by law if several conditions are met. In particular, the borrower must have missed at least two payments and the total overdue amount must exceed 10% of the original loan amount. The bank must also send a written notice to the borrower warning that the loan will be terminated if the borrower does not pay within two

the outstanding payment within the 45-day window, while 24% of borrowers default and 10% have their loans terminated by the bank.⁵

Of the 72% of treated borrowers, about 37% fail to pay within the initially agreed time period, typically one or two weeks after the call and before day 45, and therefore re-enter the call center process. About 70% of them are reached again, so that 25% of treated borrowers receive a follow-up call. To examine the effect of the follow-up call and to better understand the mechanism behind the treatment effect, we examine two alternative payment outcomes: *Payment: Alt. 1* and *Payment: Alt. 2*. Both outcomes abstract from the effect of a possible follow-up call by considering payment outcomes before follow-up calls (*Payment: Alt. 1*) or by assuming that payment probabilities of borrowers with follow-up calls correspond to those of borrowers without follow-up calls (*Payment: Alt. 2*). Both alternative outcomes, therefore, have lower average values (0.58 and 0.67, respectively) than *Payment*.

Finally, we also observe the future payment behavior of borrowers who successfully resolve the initial delinquency until the end of 2014. We can therefore also analyze whether these borrowers become delinquent again and, if so, when and whether the loan was later terminated. The summary statistics show that conditional on resolving the first delinquency, 37% of borrowers become delinquent again 98 days (on average) after the first delinquency. An additional 8% of loans are terminated in the future, resulting in an overall termination probability of 18%.

B.2. Survey Data

To further examine how a phone call to a delinquent borrower affects the borrower's payment behavior, we conducted a survey among several call center agents about some of their conversations with delinquent borrowers in February 2016. The resulting data set consists of 13 of the 30 call center agents who volunteered to participate in the survey as well as 245 borrowers with whom these agents spoke in February 2016. Panel B of Table I reports summary statistics for the outcome variable *Payment* as well as for call, loan, and borrower characteristics of this sample. While all borrowers have recently missed a payment, borrowers are not necessarily delinquent for the first time. Indeed, 36% of borrowers have spoken with a call center agent within the last six months regarding an earlier delinquency. Furthermore, the sample includes installment as well as revolving loans in the form of overdraft lines of

weeks and that termination will adversely affect the borrower's credit report.

⁵*Payment* and *Default* do not sum to one, as some delinquent borrowers make the necessary payment after 45 days but before 90 days past due. Similarly, not all loans in default are terminated, as some borrowers make payments after 90 days past due to avoid loan termination.

credit. In addition, we have information about the participating call center agents' gender and age and an assessment of the likeability of the voice of eight call center agents. Finally, in the survey we ask call center agents to describe whether a borrower was surprised to receive a phone call from the bank or to learn about the delinquency. Based on the call center agents' impression, 40% of the borrowers in this sample appeared surprised about the call and 27% seemed surprised about the delinquency itself.

II. The Effects of Personal Communication on Delinquent Borrowers

A. Identification

To detect a causal effect of personal communication with a bank agent on borrowers' payment behavior, we would ideally carry out an experiment in which we randomly assign the treatment to all delinquent borrowers whose records enter the call center. However, such experimental data are not available to us. Instead, our main variable of interest, *Talk*, reflects whether a given borrower could be reached by a call center agent. A simple regression of any of the outcome variables on *Talk* is therefore subject to endogeneity concerns for two reasons. First, omitted variables could be correlated with a borrower's payment behavior as well as with the likelihood of reaching the borrower. For example, some borrowers might be hard to reach by phone and might also be less likely to pay the delinquent amount on time, causing a simple regression estimate to overstate the effect of the phone conversation.⁶ Second, regression estimates could also suffer from reverse causality as delinquent borrowers who make the necessary payments *before* they are reached by a call center agent are no longer called by the call center. In those cases, *Talk* equals zero because these borrowers have already paid the outstanding amount, causing a simple regression estimate to understate the effect of the phone conversation.

To avoid endogeneity concerns due to omitted variables and reverse causality, we exploit (i) significant variation in the chance of reaching a borrower by phone across different days of the week as well as (ii) random assignment of calls to borrowers to certain days of the week. Figure 2 shows the fraction of daily call attempts that resulted in a conversation with a borrower for all days during our sample period for all 11,186 call attempts associated with

⁶Given that the bank's phone number is hidden and that unknown calls (i.e., calls without a phone number), were still common in Germany in 2012, it appears unlikely that borrowers who intended not to pay could easily ignore a phone call from the bank, especially as borrowers in our sample became delinquent for the first time and were likely unaware of the bank's early collection process. However, a correlation between *Talk* and unobserved payment determinants could arise in many other ways.

the 3,448 borrowers in our sample. While on average only 22% of call attempts reached a borrower, there is substantial variation. While some of the variation is related to special events, such as holiday weekends, there is substantial variation across weekdays. The average daily reachability was highest on Saturdays when 64% of call attempts led to a conversation with a borrower, while the average daily reachability was lowest on Mondays (9%) and intermediate on Tuesdays through Fridays (21%). Importantly for administrative reasons, the bank treats Saturday, which has reduced working hours, and Monday as one day and determines the order in which borrowers are called over the course of this “one day” randomly through an auto-dialer. Therefore, even with 10 call attempts, a borrower might never be called on a Saturday or a Monday. However, the overall chance of ever being called on a Saturday or a Monday varies with the day of first call, which is the second day after a borrower’s file enters a call center. As Internet Appendix Figure IA3 shows, borrowers with a call center entry on a Thursday have their first call either on Saturday or Monday. If they are not reached during the first call attempt, which is unlikely for those called on Saturday but likely for those called on Monday, the up to nine additional call attempts include *one* more randomly assigned call on a Saturday or on a Monday at most. Borrowers with an entry on Monday, Tuesday, Wednesday, or Friday have their first call between Tuesday (for those with an entry on Friday) and Friday (for those with an entry on Wednesday). If unreached on the first call attempt, the up to nine additional call attempts include up to *two* calls that are each randomly assigned to Saturday or Monday. Given the significant differences in daily reachability, borrowers with a first call on Saturday (Monday) should have a higher (lower) overall chance of being reached compared to borrowers first called between Tuesday and Friday. Figure 3 confirms that the overall reachability of borrowers, that is, the average of *Talk*, varies substantially depending on the day that a borrower is first called. For borrowers with a first call on either Saturday, Tuesday through Friday, or Monday, the average values of *Talk* are 0.96, 0.71, and 0.43, respectively.⁷

[Figure 2 about here]

While the day on which a borrower is first called depends partly on the random assignment between Saturday and Monday, a second source of randomization is the day that borrowers’ files enter the call center, as calls begin two days after call center entry.⁸ According to the bank’s policy, files are sent to the call center 20 days after a missed payment and payments

⁷While we observe all call attempts for each borrower, for identification we focus on the day of the first call, as the total number of call attempts is likely endogenous.

⁸We confirm that 75% of the first calls in our data are made exactly two days after the entry date, while 99.5% are made between two and four days. When manually checking cases for which the difference between

are typically due on the first or fifteenth of a month, which are the two default due date options on the POS loan application forms.⁹ The day of the week that a given file arrives in the call center is hence determined by the choice of the due date at the time of the loan application and the particular day of the week on which the due date occurs in the month that a borrower misses her payment. It therefore seems unlikely that the day of the week on which borrowers' files arrive in the call center is systematically related to any borrower or loan characteristics. In summary, our identification approach relies on a borrower's chance (or risk) of being called on a Saturday (or Monday). This chance varies with the day of the first call, which is determined by two random factors: the day that a borrower's file arrives in the call center, which we argue is *de facto* random, and the bank's administrative procedure to treat Saturday and Monday as one day with random assignment by an auto-dailer between both days (see Internet Appendix Figure IA3 for a summary).

In order to evaluate the random nature of the day of the first call, in Table II we provide a three-way comparison of borrower and loan characteristics between those who receive their first call on Saturday, Monday, or another day of the week (i.e., Tuesday through Friday). Column (4) reveals that among the 15 characteristics, only age is significantly different between those who receive their first call on Saturday and those with their first call on Monday. Borrowers who are first called on Saturday are on average about four years older than borrowers first called on Monday. A significant age difference also exists between borrowers with a first call on Saturday and a first call on Tuesday through Friday. In addition, these two sets of borrowers differ in the probability that a borrower is retired (7% vs. 3%). Finally, comparing borrowers with a first call on Monday to those with a first call on Tuesday through Friday, we find a significant difference with respect to the probability of living in East Germany (9% vs. 5%). Overall, out of 45 pair-wise comparisons, we find that only four yield a significant difference, which seems consistent with the assignment of the day of the first call indeed being random.

[Table II about here]

Given the pronounced differences in reachability between Saturday, Monday, and the other days of the week, it might be surprising that the bank does not call more borrowers

the day of the first call and the entry date is larger than two, we find that differences are explained by public holidays during our sample period (Easter Monday, Ascension of Christ, and Whit Monday).

⁹While applicants can choose another day as their monthly payment date, at least 85% of the loans in our sample can indeed be traced back to a due date on the first or fifteenth of the month, 20 days before the arrival date.

during the weekend and that the bank does not optimally select the day on which borrowers are (first) called. While increasing the workload for call center agents on the weekend is difficult given Germany's labor market regulation, the bank has since changed its procedures and now optimizes the assignment of borrowers to the day on which they are (first) called. However, because the call center's processes had not been optimized in the early years, we can better identify the effect of personal communication on delinquent borrowers.

B. Results from IV Regressions

To identify the effect of personal communication between a bank agent and a delinquent borrower, *Talk*, on borrower i 's payment behavior, we employ a linear instrumental variable (IV) regression:

$$Y_i = \alpha + \gamma \textit{Talk}_i + \beta X_i + L + M + \mathcal{E}_i. \quad (1)$$

We measure the outcome, Y_i , to the current delinquency with three indicator variables: *Payment*, *Default*, and *Termination: Now*. In addition to *Talk*, these outcomes depend on observable loan and borrower characteristics, X_i , as well as location and month fixed effects, L and M . Since the decision to speak with a bank agent is likely endogenous, we employ *First Call Saturday* and *First Call Monday* as instruments for *Talk*. As explained above, the day of the first call attempt is randomly determined and therefore unrelated to unobservable borrower characteristics. However, borrowers with a first call on Saturday, 6.4% of our sample, have a higher chance of speaking with a bank agent, while borrowers with a first call on Monday, 6.1% of our sample, have a lower chance of speaking with a bank agent compared to borrowers with a first call between Tuesday and Friday.

Table III reports results for the first stage of our IV estimation. In particular, we use a linear probability model and regress *Talk* on both instruments, borrower and loan characteristics, as well as location and month fixed effects. Consistent with the evidence in Figure 3, column (1) of Table III shows a significant positive effect of *First Call on Saturday* and a significantly negative effect of *First Call on Monday* on *Talk*. Both effects are sizeable as they increase and decrease the probability of a phone conversation by 23.8 percentage points and 26.0 percentage points, respectively. In column (2), we report corresponding results for only those borrowers whose files arrive in the call center on Thursday and who are first called on Monday or Saturday. That is, we exploit only the random assignment between Saturday and Monday but ignore all other observations. While the sample is much smaller, results again reveal that being first called on Saturday increases the likelihood of a phone conversation with a bank agent by 52.0 percentage points relative to being first called on Monday. Finally, for both regressions the F -test statistics is substantially above 10, reducing

concerns about weak instruments (Stock and Yogo (2005)).

[Figure 3 about here]

[Table III about here]

Columns (1) through (3) of Table IV, Panel A, report the two-stage least squares (2SLS) regression results for the three main outcomes for our full sample, the sample underlying the results in column (1) of Table III. The point estimates for *Talk* are significantly positive for *Payment* and significantly negative for *Default* and *Termination: Now*. In particular, we find that speaking with a bank agent increases timely repayment by 34.4 percentage points, while decreasing default and termination by 23.8 percentage points and 12.4 percentage points, respectively. Several loan and borrower characteristics display significant associations with the payment outcome. Borrowers who pay a positive interest rate, for example 10% p.a., are less likely to repay within 45 days (-6.7 percentage points) and more likely to experience default (+12 percentage points) and loan termination (7.1 percentage points) compared to the majority of borrowers with 0%-interest rate loans. Borrowers with loans that have been in existence longer seem to be more likely to repay and less likely to default. Male borrowers seem to be slightly less likely to repay and more likely to see their loans terminated, while payment outcomes seem to improve with age for all relevant age levels.¹⁰ Finally, we observe a substantial deterioration in repayment for the two borrowers who are unemployed.

[Table IV about here]

For comparison, we also report the OLS estimates in columns (4) to (6) of Panel A. In absolute terms, the OLS coefficient estimates are smaller than the corresponding 2SLS estimates for all three outcomes. This difference could suggest that OLS results are more impacted by a downward bias due to reverse causality compared to an upward bias due to omitted variables. However, we also note that 2SLS estimates represent local average treatment effects (LATE) but not necessarily average treatment effects (ATE). Before discussing heterogeneous treatment effects in detail below, we first address possible concerns about the assumption that the day of the arrival of a borrower's file in the call center is de facto random. Panel B of Table IV reports results for the smaller sample of only those borrowers whose records enter the call center on a Thursday and whose first call therefore occurs on a Saturday or Monday, as determined entirely randomly by an auto-dialer. For compactness, we report coefficient estimates only for *Talk*, the main variable of interest. However the same

¹⁰Note that *Age squared* is defined as $Age^2/100$.

control variables as in previous tables are included throughout. While the effects sizes of *Talk* are a bit smaller in absolute terms, *Talk* continues to have a substantial impact on borrowers' payment behavior.

C. Heterogeneous Treatment Effects

It is important to keep in mind that the 2SLS estimates reflect local average treatment effects (LATE), which are the treatment effects of the “compliers” (i.e., those borrowers for whom the instruments change whether or not they speak with a bank agent), but not of those borrowers who speak (or do not speak) with the bank agent independently of the instruments (Imbens and Angrist (1994)). If treatment effects differ across borrowers, the 2SLS estimates likely differ from the average treatment effect (ATE), as compliers are a subset of borrowers. Treatment effects can differ due to observable as well as unobservable differences across borrowers. For example, an important observable difference across borrowers could be the interest rate borrowers pay. Borrowers with 0%-interest rate loans might respond to the personal communication treatment differently relative to borrowers who pay a high interest rate, given that the latter group might be more financially constrained. Similarly, treatment effects might differ by borrowers' unobservable resistance to treatment. For example, borrowers who are easier to reach or generally have lower resistance to treatment might also benefit more from a conversation with a bank agent.

To shed light on the extent of heterogeneous treatment effects across borrowers, we allow the effect of observable as well as unobservable loan and borrower characteristics to vary with *Talk*, which we abbreviate with $T \in \{0, 1\}$, such that:

$$Y_i = \alpha + \gamma \text{Talk}_i + \beta_T X_i + L + M + \mathcal{E}_{T,i}. \quad (2)$$

While $\beta_{T=1} - \beta_{T=0}$ captures heterogeneous treatment effects due to observable differences, $E[\mathcal{E}_{T=1,i} - \mathcal{E}_{T=0,i} | U_i = u]$ reflects different treatment effects due to unobserved resistance to treatment U_i . Specifically, U_i indicates a quantile of the distribution of unobserved resistance, V_i , in the underlying selection model (i.e., $\text{Talk}_i = \mathbb{1}\{f(X_i, Z_i) > V_i\}$), and is by construction bounded by the 0/1 interval.¹¹

We apply the local IV method by Heckman and Vytlacil (1999, 2001, 2005) and estimate

¹¹That is, a borrower speaks with a bank agent if the treatment probability given by the propensity score $P(X_i, Z_i)$ exceeds the quantile, U_i , of the distribution of resistance levels corresponding to the borrower's level of resistance, V_i . That is, $\text{Talk}_i = 1$ if $P(X_i, Z_i) > U_i$. For a detailed discussion, see Cornelissen et al. (2016).

marginal treatment effects (MTEs) that characterize the treatment effect for a borrower with observable characteristics X_i and unobservable resistance to treatment, U_i . As is common in the applied MTE literature, we assume that MTEs are additively separately in U and X , such that

$$MTE(X_i = x, U_i = u) = \gamma + x(\beta_{T=1} - \beta_{T=0}) + E[\mathcal{E}_{T=1,i} - \mathcal{E}_{T=0,i} | U_i = u]. \quad (3)$$

To estimate MTEs, we use a parametric second-order polynomial approximation as well as a more flexible semi-parametric approximation.¹² All standard errors are bootstrapped with 100 repetitions, accounting for clustering by location.

C.1. Observable Differences

We first consider different treatment effects due to observable differences across borrowers. Table V, Panel A, reports the corresponding results.¹³ For each outcome and each estimation method (“polynomial” and “semi-parametric”), we report estimates for $\beta_{T=0}$ as well as $\beta_{T=1} - \beta_{T=0}$. While $\beta_{T=0}$ indicates the association between a given characteristic and the outcome, $\beta_{T=1} - \beta_{T=0}$ tests whether the effect of a phone conversation differs for specific borrower or loan characteristics, similar to an interaction with *Talk*. Differences across borrowers or loan characteristics can provide insights into the underlying mechanism. For example, as mentioned above, borrowers with 0%-interest rate loans might respond differently compared to borrowers who pay a high interest rate. However, all of the $\beta_{T=1} - \beta_{T=0}$ estimates associated with *Interest Rate* are statistically insignificant, given large standard errors, but also appear inconsistent in terms of a possible differential treatment effect. That is, for *Payment* and *Termination: Now*, borrowers with higher interest rate loans seem to experience a larger treatment effect in absolute terms, while they seem to experience a smaller treatment effect with respect to *Default*. Overall, these results do not seem to suggest that the treatment effect of *Talk* mainly reflects the behavior of borrowers with 0%-interest rate loans but not those with positive interest rate loans. When considering differences between

¹²We employ the *mtefe* estimator of Andresen (2018) with a linear probability model as the selection model. We modify the estimator to allow for non-estimated MTEs outside the common support as well as for bootstrapped clustered standard errors. See Andresen (2019) for an application.

¹³As before, we include month and location fixed effects. However, we do not allow for differential impact for treated and untreated borrowers with respect to these fixed effects. We also do not allow the effect of *Unemployed* to vary by treatment status, as only very few borrowers (0.1%) are unemployed, causing difficulty with our bootstrapping procedure.

borrowers with longer versus shorter maturity loans, the direct associations ($\beta_{T=0}$) between *Repayment Terms* and all three outcomes suggest that borrowers with longer maturity loans experience worse outcomes. However, the relevant $\beta_{T=1} - \beta_{T=0}$ estimates suggest that the treatment effect in absolute terms is larger for these borrowers, and statistically significantly so for *Default* and *Termination: Now*.

[Table V about here]

While most of the other $\beta_{T=1} - \beta_{T=0}$ estimates are statistically insignificant, it is interesting to briefly discuss the findings associated with *Non-German*, *Age*, and *Retired* in particular together with the relevant selection effects. For example, Table III suggests that older or retired borrowers are more likely to speak with a bank agent. It is possible that such borrowers answer the phone because of the expected benefits of a phone conversation. However, at least with respect to *Payment*, we find that older and retired borrowers benefit significantly less from a phone conversation with a bank agent compared to younger or non-retired borrowers, suggesting that their selection into treatment is not associated with higher treatment effects. Finally, when considering non-German borrowers Table III provides suggestive, even if statistically insignificant, evidence that non-German borrowers are less likely to speak with a bank agent. At the same time, the $\beta_{T=1} - \beta_{T=0}$ estimate for non-German borrowers reveals treatment effects that are muted relative to German borrowers, providing some, even though largely statistically insignificant, evidence of negative (positive) selection on observable losses (gains) by non-German (German) borrowers. In summary and keeping in mind that for some of the estimates of $\beta_{T=1} - \beta_{T=0}$ standard errors are relatively large, we find only limited statistically significant evidence of heterogeneous treatment effects based on observable characteristics.¹⁴

C.2. Unobservable Differences

We next consider heterogeneous treatment effects due to unobservable differences. Specifically, in Figure 4, we report MTEs, evaluated for a borrower with average observable charac-

¹⁴In Internet Appendix Table IA2, we compare the average observable characteristics for the full sample to the average observable characteristics estimated for the compliers. We then multiply the differences in means between both groups with the corresponding point estimates of $\beta_{T=1} - \beta_{T=0}$ from Table V, Panel A, to obtain the differences between the average treatment effects (ATEs) and the LATE estimates due to observable characteristics. We find that the differences due to observables are small.

teristics, over the range of unobservable resistance to treatment, U_i .¹⁵ We again report results for the “polynomial” (blue/gray line) and “semi-parametric” (orange/black line) approximation approach as well as bootstrapped standard errors. Given that U_i represents quantiles of the resistance to treatment, the unit interval represents 100% of the borrowers in our sample. Due to the discrete nature of our two instruments and the existing variation of the observable borrower and loan characteristics that enter the selection model, we can only estimate MTEs for 55% of our borrowers (i.e., for those with $0.40 \leq U_i \leq 0.95$ (solid lines)). That is, conceptually the set of compliers contains only borrowers with $0.40 \leq U_i \leq 0.95$, while borrowers with low resistance to treatment (i.e., borrowers with $U_i < 0.40$), are always-takers and borrowers with particularly high resistance to treatment (i.e., borrowers with $U_i > 0.95$) are never-takers.¹⁶ We first focus on the estimated MTEs for $0.40 \leq U_i \leq 0.95$. Panel A of Figure 4 shows these MTEs for *Payment* as the outcome. Consistent with the positive effect of a phone conversation on borrowers’ payment behavior, both estimation approaches yield positive MTEs. Moreover, both MTE lines are increasing as unobserved resistance to treatment increases. That is, borrowers who are more “reluctant” or less likely to speak with a bank agent due to unobserved characteristics show a higher increase in their likelihood to pay due to the phone conversation compared to borrowers who are less reluctant or more likely to speak with a bank agent. Such negative selection into treatment based on unobservables counters the possible concern that those who stand to “benefit” from the conversation are more likely to select to speak with a bank agent. It suggests instead that borrowers who are harder to reach due to unobserved characteristics are more likely to improve their payment behavior due to the phone call.

[Figure 4 about here]

Panels B and C of Figure 4 report the corresponding results for *Default* and *Termination: Now*. Consistent with our 2SLS results, the marginal treatment effects are generally negative. As in Panel A, we observe that those with the largest unobserved resistance to treatment benefit more from the phone conversation than those with lower levels of resistance.

¹⁵Averages are calculated across the entire population of borrowers. However, given that observable differences have little impact on differences in treatment effects, averages calculated across compliers yield similar MTEs.

¹⁶Technically speaking, the common support of the propensity score distribution of treated and untreated borrowers excludes borrowers with low and particularly high propensity scores. Note that in order to strengthen the conditions for what constitutes support, we trim 0.5% of the tails of treated and untreated borrowers. See Andresen (2018) for details.

C.3. Average Treatment Effects

The 2SLS estimates, which represent the treatment effects of compliers, are weighted averages of the MTEs estimated for borrowers with $0.40 \leq U_i \leq 0.95$, while average treatments effects (ATEs) of a randomly selected borrower are equally weighted averages of the MTEs for all borrowers over the entire 0/1 interval of unobserved resistance to treatment.¹⁷

While Figure 4 suggests important variation of the estimated effects of *Talk* across borrowers with different unobserved resistance to treatment, in order to calculate ATEs we need to make additional assumptions about the MTEs for borrowers with low resistance to treatment ($U_i < 0.40$) and for borrowers with particularly high resistance to treatment ($U_i > 0.95$) in order to calculate average treatment effects.

Inspection of Figure 4 suggests that estimated MTEs are reasonably close to zero as U_i approaches 0.40. Since it seems unlikely that a phone conversation with a borrower who has missed a payment could reduce the probability of repayment or increase the probability of default or termination, we assume that MTEs are zero for $U_i < 0.40$. While this assumption rules out marginal treatment effects that have the opposite effect relative to the intended effect for borrowers with low resistance to treatment, it is a conservative assumption in the sense that our assumption also rules out any intended effects for these borrowers. For the 5% of borrowers with $U_i > 0.95$, we assume that the MTEs are equal to the MTEs at $U_i = 0.95$. With these assumptions (shown as the non-solid lines in Figure 4), we can calculate ATEs for our sample by averaging all MTEs over the entire 0/1 interval of unobserved resistance to treatment. We report the resulting ATEs in Panel B of Table V. While all ATEs are statistically significant, their absolute size is substantially smaller than the 2SLS estimates reported above. That is, for the average (i.e., randomly selected) borrower, personal communication with a bank agent increases *Payment* by between 16.6 and 20.1 percentage points, while it decreases *Default* by 10.5 to 14.0 percentage points and *Termination: Now* by 7.1 to 6.3 percentage points. To understand the sensitivity of the ATE calculation to the assumption that MTEs are zero for $U_i < 0.40$, we can consider the impact of alternative assumptions. For example, if we assume that in the case of *Payment* MTEs for $U_i < 0.40$ are 0.10 instead of zero, the ATE would increase by 0.04 ($=0.40 \times 0.10$).

Finally, the estimated ATEs seem broadly in line with evidence from similar interventions in retail credit markets (see Internet Appendix Table IB1 for an overview and Internet Appendix B for a detailed discussion). In a setting reasonably closely related to ours, given

¹⁷Internet Appendix Figure IA4 compares the weights used to calculate the local average treatment effects related to the 2SLS estimates as well as the equal weights used to calculate the average treatment effects.

a focus on delinquent as opposed to all borrowers, Bursztyn et al. (2019) find effects sizes for moral as well as reputational incentive treatments, especially among higher risk borrowers, that are in absolute terms similar to ours. Other studies, such as Cadena and Schoar (2011), Moulton, Collins, and Loibl (2015), Karlan et al. (2016), and Medina (2021), that evaluate the effect of reminders, financial incentives, or financial coaching on non-delinquent borrowers find smaller ATEs in absolute terms but similar effects relative to the baseline probability of the control group. Overall, our estimation of MTEs as well as our assumptions about the MTEs of borrowers with low and extremely high resistance to treatment yield ATEs that seem plausible given the related literature.

In summary, we document substantial heterogeneity in marginal treatment effects across borrowers with more or less unobserved resistance to treatment. The slopes of the MTE curves indicate that borrowers with more unobserved resistance to treatment experience larger treatment effects compared to borrowers with lower resistance, suggesting that extending treatment to borrowers currently not reached would increase the effect of speaking with a bank agent. Heterogeneous marginal treatment effects also imply that the 2SLS estimates represent treatment effects specific to the set of compliers. Indeed, with additional assumptions, we find that the average treatment effects are smaller than our 2SLS estimates, given that compliers are concentrated among borrowers with above average resistance to treatment.

III. Mechanism

The bank's main objective when calling delinquent borrowers is to obtain payment within 45 days and to avert default and loan termination. While the personal communication with a bank agent appears to significantly contribute to these objectives, several different mechanisms potentially explain the payment behavior of borrowers who speak with a bank agent relative to the behavior of those who do not speak with a bank agent. First, the results may be driven by borrowers who have not paid attention to the bank's earlier letter, who are reminded about their existing payment obligation, and who learn about the delinquency through the phone call. Similarly, the call from the bank could make the bank's enforcement process more salient, as borrowers may be surprised by the banks' reaction to even a small delinquency. It is also possible that the personalized monitoring in the form of a follow-up call for some treated borrowers is an important part of the observed treatment effect. Finally, the personal communication with an individual bank agent might change the nature of the existing obligation from a payment commitment towards an impersonal financial institution to a commitment towards an individual, thereby triggering prosocial behavior due to, for

example, self-image or social image concerns. To better understand to which extent these different mechanisms are at work, we examine cross-sectional variation in our main sample, two alternative intermediate payment outcomes, as well as results from our survey of call center agents.

A. *Payment Reminder and Attention to the Bank's Enforcement Process*

At the time of the delinquency, the outstanding loan balance varies substantially across borrowers. Borrowers with a large outstanding loan balance are likely aware of their outstanding loan, their payment obligation, and the missed payment. They might therefore not be surprised about a phone call from the bank. Borrowers with small outstanding loan balances, however, are more likely unaware of the delinquency. Even if they are aware of their overdue payment, they might be particularly surprised about the bank's collection efforts and might perceive the phone call as a signal about the seriousness of the delinquency. We therefore compare the effect of *Talk* between borrowers with a below median outstanding amount (i.e., less than €466) and borrowers with an above median outstanding amount. Table VI reports 2SLS regression results for both subsamples and all three outcomes. We do not find any significant differences in the coefficient estimates for *Talk* between the two subsamples, suggesting that the effect of *Talk* is unlikely due to borrowers who are unaware of the delinquency or surprised by the bank's attention to even small delinquencies. If anything, the size of the coefficient estimate is larger for borrowers with higher outstanding loan amounts.

[Table VI about here]

Next, to further examine whether the call makes the bank's collection efforts more salient we make use of the responses of our survey of call center agents. As part of the survey, we ask agents to note after each phone call whether the borrower seemed surprised about receiving a call from the bank or learning about the delinquency itself. Specifically, agents reacted to a statement such as "The customer was surprised about the call (delinquency)" by selecting a number between 1 and 5, where 1 corresponds to "Do not agree at all" and 5 to "Strongly agree." We form two corresponding indicator variables, *Surprise: Call* and *Surprise: Delinquency*, which equal one if agents select 4 ("Agree") or 5 ("Strongly Agree") and zero otherwise. For 40% of borrowers in the survey sample, agents reported that the borrower seemed surprised about the call, while 27% of borrowers appeared surprised about the delinquency.

Even though our surprise measure comes from a small sample and relies on agents' assessments, we test whether the borrower's surprise about receiving a phone call affects her payment behavior with an OLS regression with *Payment* as the outcome variable. We do not have information on defaults or termination for the survey sample. We do not use an IV regression, given that all borrowers in our survey sample speak with a bank agent.¹⁸ Given that our survey sample differs slightly from our main sample, we include additional control variables, such as the type of loan, whether or not the borrower has spoken with a call center agent during the past six months, and whether the call is the first call or a possible follow-up call. Column (1) of Table VII reveals that borrowers who are reached by phone and who appear to be surprised about the bank's call have a payment probability that is only 3.9 percentage points higher compared to borrowers who do not seem surprised about the phone call, which is statistically insignificant. In column (2), we exclude follow-up calls and find an even smaller statistically insignificant effect. In columns (3) and (4), we repeat the analysis while controlling for whether a borrower seems surprised about learning about the delinquency itself. The surprise about the phone call again does not seem to affect the payment behavior.¹⁹

[Table VII about here]

Overall, the evidence from borrowers with small outstanding loan amounts and from borrowers who are surprised about receiving a phone call is not consistent with the hypothesis that the call functions as a reminder or highlights the bank's commitment to collecting overdue payments. Below we present additional evidence about cross-sectional differences of the call effect as well as about persistent effects of the call beyond the current delinquency that seem inconsistent with the hypothesis that the call effect is a reminder effect.

¹⁸We account for standard errors being correlated at the agent level. Since the number of clusters in our sample is small (13 agents), we use the wild bootstrap-*t* approach with 1,000 repetitions and report *p*-values (see Cameron, Gelbach, and Miller (2008)). Significance levels are generally similar for standard errors that are robust to double-clustering at the location and agent level.

¹⁹While we cannot rule out that at least some of the borrowers who seem surprised about the delinquency actually know about the delinquency but pretend not to know as they might be embarrassed, we would not necessarily expect their payment behavior to differ from the payment behavior of borrowers who are also reached but not surprised about the delinquency.

B. *Personalized Monitoring through Follow-up Calls*

While all treated borrowers speak with a bank agent, some receive a follow-up call. The follow-up call occurs when a borrower agrees to pay the delinquent amount, which in most cases is within one or two weeks of the phone call, but then fails to make the payment. In this case, the borrower's file re-enters the early collection call center's process and, if reached, the borrower is reminded of the outstanding payment and asked to make payment by day 45. For these borrowers, who represent about 25% of all treated borrowers, treatment therefore consists of two phone calls with the second phone call being triggered by the borrower's failure to pay as initially agreed.

To understand to which extent the treatment effect is due to the follow-up call, which provides borrower-specific monitoring of the repayment agreement and increases the intensity of the personal communication, we consider the outcome *Payment: Alt. 1*. For untreated borrowers, the outcome equals *Payment* and reflects the effect of the initial letter as well as of the follow-up letter to borrowers who are still delinquent at day 35. However, for treated borrowers, it reflects the outcome based on the bank's initial phone call. That is, for treated borrowers, *Payment: Alt. 1* is one if a borrower pays as agreed within a few weeks but zero if the borrower does not pay and the borrower's case re-enters the call center process for a potential follow-up call. Assuming that no payment is made at all by treated borrowers who fail to pay as initially agreed represents a conservative assumption and should constitute a lower bound for the treatment effect in the absence of a follow-up call. An alternative approach is to assume that all treated borrowers whose records re-enter the call center repay with the same probability as the subset of borrowers who are not reached for a follow-up call. In *Payment: Alt. 2*, we implement this approach. Specifically, for the subset of treated borrowers who fail to repay as initially agreed and who are not reached for a follow-up call, we regress *Payment* on all independent variables except for *Talk* from Panel A of Table IV. We then use the predicted payment outcome for all treated borrowers who fail to repay as initially agreed, whether or not they are reached for a follow-up call. In summary, both *Payment: Alt. 1* and *Payment: Alt. 2* eliminate the effect of a follow-up call. *Payment: Alt. 1* assumes that no payment will be made by those who fail to pay as initially agreed, while *Payment: Alt. 2* assumes that repayment patterns for these borrowers mimics the payment behavior of those who are not reached for follow-up call.

Panel A of Table VIII summarizes the average payment outcomes for different subsets of borrowers. For treated borrowers, the comparison highlights the focus on the initially agreed payment by *Payment: Alt. 1* as well as the assumption underlying *Payment: Alt. 2* that treated borrowers with a follow-up call pay with the same probability as those not reached

for a follow-up call.²⁰

[Table VIII about here]

In Panel B of Table VIII, we report the corresponding 2SLS estimates. In column (1), we report the baseline result for *Payment* from Table IV, Panel A, for comparison. Column (2) reports results for *Payment: Alt. 1*. Even under this conservative assumption, we find a significant treatment effect of *Talk* of 0.223, about 65% of the size of the treatment effect in column (1). In column (3), we report results for *Payment: Alt. 2*. We find a significant effect of *Talk* of 0.310, about 90% of the effect in column (1). These results are consistent with a positive but modest effect of follow-up calls on payment outcomes. Specifically, the follow-up call seems to account for between 10% and at most 35% of the full treatment effect. We therefore conclude that the effect of *Talk* is not mainly due to increased communication or the personalized monitoring of timely repayment.

C. Prosocial Behavior and Promise Keeping

The personal communication with an individual bank agent might change the nature of the existing financial obligation from impersonal (a payment commitment to a remote financial institution) to a more personal commitment (a payment commitment to an individual), increasing prosocial behavior like promise keeping. To test the possible role of prosocial behavior, we build on prior research showing that prosocial behavior towards other humans decreases with social distance (e.g. Hoffman, McCabe, and Smith (1996), Conrads and Lotz (2015)). Since social distance is likely larger between people of different cultural or ethnic backgrounds, we examine in a first step to which extent the effect of *Talk* differs between German borrowers and non-German borrowers who live in Germany. While we find no significant difference in the payment behavior between both groups in our main results, it is possible that borrowers who are foreign residents in Germany perceive a larger social distance between themselves and the call center agents compared to German borrowers. In Table IX, we report 2SLS estimates of the effect of *Talk* for both subsamples. The results reveal that in absolute terms, the effect of *Talk* is between 10.8 percentage points (*Termination: Now*) and 34.5 percentage points (*Default*) larger for German borrowers compared to non-German borrowers. However, the difference is statistically significant only for *Default*.²¹ While this

²⁰Differences in *Payment: Alt. 2* between both groups are due to differences in observables that predict payment.

²¹These subsample results are consistent with the corresponding estimates for $\beta_{T=1} - \beta_{T=0}$ in Table V, which also suggest that non-German borrowers might benefit less from speaking with a bank agent compared

result is consistent with increased prosocial behavior due to the phone conversation, we cannot rule out an alternative explanation (e.g., foreign borrowers struggle to understand the call center agent on the phone).

[Table IX about here]

To gain more insight into the personal dimension of the phone call, we again employ our survey data. While our survey data set has only 245 observations, one advantage of this data set is a unique identifier for 13 agents who speak to borrowers in our survey data set. Since all call center agents use the same protocol and convey the same information, it is not clear that agent effects on borrowers' payment behavior should vary across agents. However, when regressing *Payment* on 12 agent fixed effects while controlling for loan and borrower characteristics, we find jointly significant agent fixed effects (p -value = 0.00). Since agents are randomly assigned to borrowers by an auto-dialer, this finding suggests that some agent characteristics cause differences in borrowers' payment behavior.²²

While agent fixed effects reflect many possible agent characteristics, we observe agents' gender and age as well as the likeability of the voice of eight agents who provided us with a recording of the standard opening of their calls to delinquent borrowers.²³ Each recording is analyzed by six to seven undergraduate students at Goethe University in Frankfurt/Main, Germany. Students rate each voice recording between 1 ("Not At All Likeable") and 5 ("Very Likeable"). We construct the variable *Likeable Voice* as the fraction of raters that rate an agent's voice as 4 ("Likeable") or 5 ("Very Likeable"). *Likeable Voice (adjusted)* represents the version of *Likeable Voice* that has been adjusted for rater fixed effects.²⁴ While 53% of raters

to German borrowers.

²²In Internet Appendix Table IA3, we repeat the same regression for several loan and borrower characteristics (Panel A). We find jointly significant agent fixed effects only for borrowers' age. Since we have verified the randomization through the auto-dialer with multiple bank representatives, we believe that this result is accidental. Moreover, we find no evidence that the assignment to a specific agent (Panel B) or to agents based on agent gender or age (Panel C) is associated with borrower or loan characteristics.

²³We do not know why some agents decided to provide us with a voice recording while others did not. However, it is important to note that agents do not know the payment outcomes associated with the borrowers they have spoken with. A written agreement between the bank and the employees' council ("Betriebsvereinbarung") prevents the bank from analyzing call outcomes at the level of the individual agent. Indeed, comparing *Payment* between agents with and without recordings, we find a slightly lower average for the group with recordings (0.79) compared to the group without recordings (0.85).

²⁴We first convert each rating into an indicator, which is one for ratings of four and five, and zero otherwise.

consider an agent's voice likeable or very likeable on average, there is dispersion across agents. Since all call center agents have been selected by the bank based on their communication skills and receive ongoing communication training, there should be no material difference in agents' ability to communicate effectively. If calls function only as reminders, differences in the likeability of agents' voices should be irrelevant. However, a likeable voice could lower the perceived social distance between a borrower and the bank agent and increase borrowers' prosocial behavior, thereby affecting *Payment*.

Using the subset of 135 observations from our survey sample for which we have data about agents' voices, we find that 84.5% of borrowers who speak to one of the four agents with a likeable voice (i.e., *Likeable Voice* > 0.50) pay within 45 days, while only 71.6% of borrowers who speak with the remaining four agents pay within 45 days. Table X provides corresponding evidence from OLS regressions, controlling for agents' age and gender as well as loan, call, and borrower characteristics. While agents' age and gender do not have a significant effect, column (1) of Table X suggests that a one standard deviation increase in *Likeable Voice* is associated with a 4 percentage points increase in the probability of payment. In column (2), we employ the rater adjusted likeability measure and find very similar results.

[Table X about here]

While our results based on agent characteristics are from a rather small sample and should therefore be interpreted with caution, the results seem to suggest that the personal dimension rather than the informational, reminder-like, or signaling-related characteristics of the conversation with a bank agent seems to determine the success of the treatment. An increase in promise-keeping behavior due to personal interaction would be consistent with a large experimental literature that has highlighted prosocial behavior caused by deeply rooted preferences for fairness and altruism, guilt aversion, and social image concerns. Of course, we cannot rule out that borrowers mistakenly assume that individual call center agents will be personally involved with decisions regarding borrowers' current or future loans and that borrowers behave in a more trustworthy way, expecting call center agents, especially those who appear friendly and likeable, to reciprocate in the future (He, Offerman, and Ven (2017)). However, even in this case, the personal dimension of the interaction with the borrower would still be essential.

We then form the average of this indicator for each rater and subtract it from each indicator. *Likeable Voice (Adjusted)* is the average of the adjusted indicators across raters, which we form for each agent.

IV. Extensions

A. Persistence of the Treatment Effect

For the borrowers in our main sample, we observe all loan-related payments until August 2014, about two and a half years after the initial delinquency. We are therefore able to observe most of the loans until maturity. Our main three outcome variables, *Payment*, *Default*, and *Termination: Now*, reflect the outcome of the initial delinquency. However, we are also able to observe whether a borrower becomes delinquent again and whether the loan is ultimately terminated before maturity. While 10% of loans are terminated as a consequence of the initial delinquency, 18% of all loans are terminated by August 2014. To understand whether the effect of *Talk* is persistent and therefore also affects the outcome of future delinquencies, in Table XI, Panel A, we examine the effect of *Talk* on *Termination: Overall*, an indicator variable that equals one if a loan is terminated at any point before maturity and zero otherwise. For comparison, we report the effect of *Talk* on *Termination: Now* from Table IV in column (1). Column (2) reports the corresponding 2SLS estimate for *Termination: Overall*. The effect of *Talk* almost doubles when we consider the overall probability of the loan being terminated as opposed to the probability of termination directly following the initial delinquency. As revealed by the *F*-test for coefficient equality, the effect of *Talk* is also statistically significantly larger for *Termination: Overall* compared to *Termination: Now*. This result suggests that personal communication between a bank agent and a delinquent borrower not only decreases the probability that the loan is terminated directly following the initial delinquency but also reduces payment difficulties during the remaining term of the loan.

[Table XI about here]

In Panel B of XI, we explore the payment behavior following the initial delinquency in more detail. We observe that 37% of the 3,092 borrowers who successfully resolve the initial delinquency fall behind on their payments at least once again and, on average, do so about 98 days after the initial delinquency. Given that we observe future payment behavior only for borrowers who have resolved the initial delinquency, our IV approach is no longer valid since the sample is selected on recovery from the first delinquency, making our instruments endogenous. Instead, we report OLS estimates of the effect of *Talk* on future payment outcomes. To assess any potential endogeneity bias, we follow Oster (2019) and compare the degree of selection on unobservables relative to observables. Specifically, we report the magnitude of unobservables relative to observables that is needed for the unobservables to explain our

results. This magnitude is captured by the parameter δ . Oster (2019) suggests that values of δ less than one indicate concerns about possible endogeneity (for recent applications, see, for example, Gorodnichenko and Weber (2016) or Hagendorff, Lim, and Nguyen (2022)).²⁵ Column (1) suggests that for borrowers who resolve the initial delinquency *Talk* significantly reduces the probability of a future delinquency by 7.9 percentage points. A value of 1.97 for δ suggests that this effect is unlikely explained by unobservables. In column (2), we examine the effect of *Talk* on the amount of time until the next delinquency. The estimate suggests that treated borrowers who resolve the initial delinquency but become delinquent again do so about 29 days later compared to untreated borrowers. However, δ is only 0.81 and does not allow us to rule out endogeneity. Finally, in column (3), we directly examine the probability that the loan is terminated after a successful recovery from the first delinquency. Consistent with the results in Panel A, we find that *Talk* significantly reduces *Termination: Later* by 4.9 percentage points and with δ of 2.85, endogeneity is less of a concern.

Taken together, the results in Table XI suggest that the treatment effect of *Talk* extends beyond alerting borrowers to the current delinquency and has a persistent effect on borrowers' payment behavior such that the probability of a future delinquency and loan termination is significantly reduced.

B. External Validity

Our main results focus on installment loans distributed at the point of sale. While POS loans provide a well-defined, internationally comparable, and growing form of consumer credit, to what extent our findings apply to other loans and borrowers is an open question.

To study the importance and impact of personal, two-way communication on non-POS borrowers' payment behavior, we provide evidence from borrowers who have exceeded their overdraft facilities. Overdraft facilities allow retail borrowers to withdraw a particular amount over and above their checking account balance. The amount is typically determined by the account holder's monthly salary. The bank charges interest on the part of the facility that is used in a given month. Unlike in the United States (see, e.g., Stango and Zinman (2014)), there are no additional fees associated with the use of the overdraft facility. A borrower becomes delinquent when she exceeds the overdraft limit. At that point, the interest rate that the borrower must pay typically increases substantially. In Germany and other European countries such as Italy and Sweden, overdraft facilities are the dominant

²⁵In our application, we set R_{max} to one, resulting in the most conservative estimates of δ . Furthermore, observables include a large set of borrower and loan characteristics as well as location fixed effects, while we consider time fixed effects as part of the baseline specification.

form of revolving credit for consumers, with 16% of surveyed German adults in 2012 reporting that they use an overdraft facility to borrow money, while only 10% report using a credit card to borrow money.

Differently from a POS loan, an overdraft facility requires an existing bank relationship, as overdraft facilities are linked to a borrower's main checking account, typically the account to which salary payments are transferred by the employer. As a large German bank, the lender whose data we use has a substantial presence in retail banking in Germany and therefore is a major provider of overdraft credit. The early collection process for borrowers who have exceeded their overdraft limit is similar to the one for POS loans.

Our sample contains 2,499 overdraft borrowers whose records enter the call center for the first time between January and June 2012 by exceeding their credit line and remaining delinquent for at least 20 days such that their records are sent to the early collection call center. The summary statistics in Internet Appendix Table IA4 suggest that, on average, delinquent borrowers have a credit line of €1,008, which they have exceeded by €167.34, on average. Consistent with the existence of an established relationship between the bank and these borrowers, we observe that the repayment at day 45 is more likely and default and termination are less likely compared to the sample of POS loans.

In Table XII, we apply the same IV estimation as in Table IV to test the effect of personal communication on delinquent borrowers' payment behavior in the context of overdraft credit. We find that *Talk* increases timely payment by 20.4 percentage points, while *Talk* decreases default and termination by 15.3 percentage points and 12.2 percentage points, respectively. While the point estimates suggest effect sizes for *Payment* and *Default* that are smaller than the corresponding 2SLS estimates for the POS sample, standard errors are relatively large. Indeed, all three estimates are significant only at the 10% level.

[Table XII about here]

Overall, these additional results are consistent with personal touch playing an important role in the communication between the bank and its delinquent borrowers, which is not limited to POS borrowers without an existing bank relationship but extends to borrowers with an existing bank relationship.

However, a general caveat of our study is that all of our results reflect the treatment effect for borrowers whose files are sent to the early collection call center but exclude borrowers whose files do not enter the call center, as is the case for borrowers who have filed for personal bankruptcy as well as borrowers with any ongoing judicial collection efforts against them. It is likely that treatment effects would be lower for these borrowers if they were included in

the bank's early collection process.

V. Conclusion

At a time when interactions between financial institutions and their retail customers become increasingly automated and impersonal, we examine the importance of personal communication between a large bank and its customers. Our results, which are based on comparing the payment behavior of delinquent borrowers who speak with a randomly assigned bank agent to the payment behavior of delinquent borrowers who do not speak with a bank agent, suggest that personal communication significantly increases the likelihood of successfully resolving the delinquency by a substantial margin.

Detailed data for a small sample of phone conversations and bank agents point to the human element of the communication, such as the likeability of the agent's voice, being an important element of the call's effect rather than the call serving as a reminder or a signal about the bank's monitoring and enforcement efforts.

The setting of our study does not allow us to rule out that a robocall (i.e., an automated call delivering a recorded message) could affect borrowers' payment behavior in a similar way as do calls by "live" bank agents. However, recent experimental evidence by Maréchal et al. (2021) suggests that the presence of an actual human being might be critical.

As the interaction and communication between financial institutions and their customers evolve, our findings highlight the value of retaining or reintroducing a human element in this interaction and suggest that personal communication will likely continue to play a role, even if much less costly information transmission is readily available.

Initial submission: December 13, 2018; Accepted: December 29, 2021

Editors: Stefan Nagel, Philip Bond, Amit Seru, and Wei Xiong

References

- Accenture, 2015, Banking Customer 2020: Rising Expectations Point to the Everyday Bank Technical Report.
- Agarwal, Sumit, Gene Amromin, Itzhak Ben-David, Souphala Chomsisengphet, Tomasz Piskorski, and Amit Seru, 2017, Policy intervention in debt renegotiation: Evidence from the home affordable modification program, *Journal of Political Economy* 125, 654–712.
- Agarwal, Sumit, Souphala Chomsisengphet, Chunlin Liu, and Nicholas S. Souleles, 2018, Benefits of relationship banking: Evidence from consumer credit markets, *Journal of Monetary Economics* 96, 16–32.
- Akerlof, George A., and Rachel E. Kranton, 2000, Economics and identity, *The Quarterly Journal of Economics* 115, 715–753.
- Andreoni, James, and B. Douglas Bernheim, 2009, Social image and the 50-50 norm: A theoretical and experimental analysis of audience effects, *Econometrica* 77, 1607–1636.
- Andresen, Martin Eckhoff, 2018, Exploring marginal treatment effects: Flexible estimation using stata, *The Stata Journal* 18, 118–158.
- Andresen, Martin Eckhoff, 2019, Child care for all? Treatment effects on test scores under essential heterogeneity, Working Paper, University of Oslo.
- Battigalli, Pierpaolo, and Martin Dufwenberg, 2007, Guilt in games, *American Economic Review* 97, 170–176.
- Berg, Tobias, 2015, Playing the devil’s advocate: The causal effect of risk management on loan quality, *The Review of Financial Studies* 28, 3367–3406.
- Berg, Tobias, Valentin Burg, Ana Gombovic, and Manju Puri, 2020, On the rise of fintechs: Credit scoring using digital footprints, *The Review of Financial Studies* 33, 2845–2897.
- Bursztyn, Leonardo, Stefano Fiorin, Daniel Gottlieb, and Martin Kanz, 2019, Moral incentives in credit card debt repayment: Evidence from a field experiment, *Journal of Political Economy* 127, 1641–1683.
- Cadena, Ximena, and Antoinette Schoar, 2011, Remembering to pay? Reminders vs. financial incentives for loan payments, NBER Working Paper 17020.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, 2008, Bootstrap-based improvements for inference with clustered errors, *The Review of Economics and Statistics* 3, 414–427.
- Carlin, Bruce I., Li Jiang, and Stephen Spiller, 2018, Millennial-style learning: Search intensity, decision-making, and information-sharing, *Management Science* 64, 2973–3468.
- Conrads, Julian, and Sebastian Lotz, 2015, The effect of communication channels on dishonest behavior, *Journal of Behavioral and Experimental Economics* 58, 88–93.
- Consumer Financial Protection Bureau, 2017, The consumer credit card market, Annual Report.

- Cornelissen, Thomas, Christian Dustmann, Anna Raute, and Ute Schoenneberg, 2016, From LATE to MTE: Alternative methods for the evaluation of policy interventions, *Labour Economics* 41, 47–60.
- D’Acunto, Francesco, Nagpurnanand Prabhala, and Alberto G. Rossi, 2019, The promises and pitfalls of Robo-advising, *Review of Financial Studies* 32, 1983–2020.
- Deloitte, 2019, The future of credit – A European perspective, Monitor Deloitte.
- Deutsche Bundesbank, 2016, Bankstellenbericht 2015, Annual Report.
- Drexler, Alejandro, and Antoinette Schoar, 2014, Do relationships matter? Evidence from loan officer turnover, *Management Science* 60, 2722–2736.
- Eurofinas, 2013, Key facts and figures, Annual Report.
- Fehr, Ernst, and Klaus M. Schmidt, 1999, A theory of fairness, competition, and cooperation, *The Quarterly Journal of Economics* 114, 817–868.
- Gorodnichenko, Yuriy, and Michael Weber, 2016, Are sticky prices costly? Evidence from the stock market, *American Economic Review* 106, 165–199.
- Hagendorff, Jens, Sonya S. Lim, and Duc Duy Nguyen, 2022, Lender trust and bank loan contracts, *Management Science* 69, 1758–1779.
- Haran, Uriel, 2013, A person–organization discontinuity in contract perception: Why corporations can get away with breaking contracts but individuals cannot, *Management Science* 59, 2837–2853.
- He, Simin, Theo Offerman, and Jeroen Ven, 2017, The sources of the communication gap, *Management Science* 63, 2773–3145.
- Heckman, James J., and Edward Vytlacil, 1999, Local instrumental variables and latent variable models for identifying and bounding treatment effects, *Proceedings of the National Academy of Sciences* 96, 4730–4734.
- Heckman, James J., and Edward Vytlacil, 2001, Local instrumental variables, in C. Hsiao, K. Morimune, and J. L. Powell, eds., *Nonlinear Statistical Modeling: Proceedings of the Thirteenth International Symposium in Economic Theory and Econometrics: Essays in Honor of Takeshi Amemiya. International Symposia in Economic Theory and Econometrics*, 1–46 (Cambridge University Press, Cambridge; New York and Melbourne).
- Heckman, James J., and Edward Vytlacil, 2005, Structural equations, treatment effects, and econometric policy evaluation, *Econometrica* 73, 669–738.
- Hoffman, Elizabeth, Kevin McCabe, and Vernon L. Smith, 1996, Social distance and other-regarding behavior in dictator games, *American Economic Review* 86, 653–660.
- Imbens, Guido W., and Joshua D. Angrist, 1994, Identification and estimation of local average treatment effects, *Econometrica* 62, 467–475.

- International Finance Corporation, 2012, Responsible debt collection in Emerging Markets, IFC Advisory Services Report.
- Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman, 2016, Getting to the top of mind: How reminders increase saving, *Management Science* 62, 3393–3672.
- Karlan, Dean, Melanie Morten, and Jonathan Zinman, 2015, A personal touch in text messaging can improve microloan repayment, *Behavioral Science & Policy* 1, 25–31.
- Kysucky, Vlado, and Lars Norden, 2016, The benefits of relationship lending in a cross-country context: A meta-analysis, *Management Science* 62, 90–110.
- Maréchal, Michel André, Alain Cohn, and Tobias Gesche, 2021, Honesty in the digital age, *Management Science* 68, 827–845.
- McKinsey & Company, 2020, Global Banking Practice – McKinsey on Payments, Technical report.
- Medina, Paolina, 2021, Side effects of nudging: Evidence from a randomized intervention in the credit card market, *The Review of Financial Studies* 34, 2580–2607.
- Melo, De Celso, Stacy Marsella, and Jonathan Gratch, 2016, People do not feel guilty about exploiting machines, *ACM Transactions on Computer-Human Interaction* 23, 1–17.
- Menger, Andrew, 2015, CLUSTE: Stata module to estimate the statistical significance of parameters when the data is clustered with a small number of clusters, *Statistical Software Components S457989*, Boston College.
- Moulton, Stephanie, J. Michael Collins, and Samek Anya Loibl, Caezilia, 2015, Effects of monitoring on mortgage delinquency: Evidence from a randomized field study, *Journal of Policy Analysis and Analysis* 34, 184–207.
- Oster, Emily, 2019, Unobservable selection and coefficient stability: Theory and evidence, *Journal of Business and Economic Statistics* 37, 187–204.
- Petersen, Mitchell A., and Raghuram G. Rajan, 1994, The benefits of lending relationships: Evidence from small business data, *Journal of Finance* 49, 3–37.
- Puri, Manju, Joerg Rocholl, and Sascha Steffen, 2017, What do a million observations have to say about loan defaults? Opening the black box of relationships, *Journal of Financial Intermediation* 31, 1–15.
- Rabin, Matthew, 1993, Incorporating fairness into game theory and economics, *American Economic Review* 83, 1281–1302.
- Schoar, Antoinette, 2012, The personal side of relationship banking, Working Paper, MIT.
- Stock, James H., and Motohiro Yogo, 2005, Testing for weak instruments in linear IV regression, in D. W. K. Andrews, and J. H. Stock, eds.: *Essays in Honor of Thomas Rothenberg*, pp. 80–108 (Cambridge University Press).

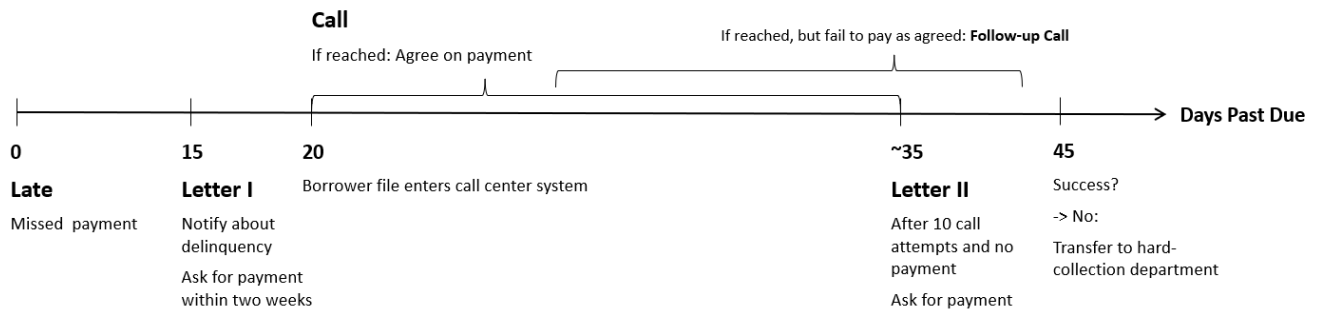


Figure 1. Timeline. This figure describes the bank's response to delinquent loans over time (in days). Day zero is the day on which the borrower misses a regular loan payment and becomes delinquent.

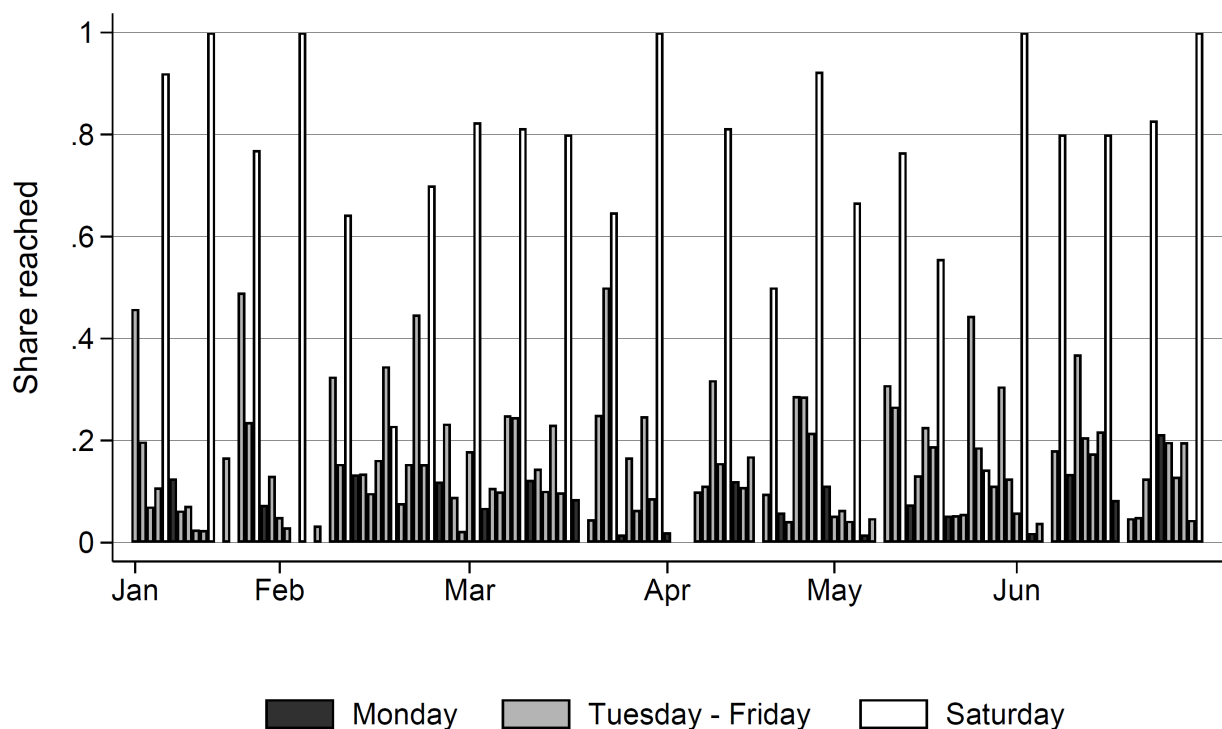


Figure 2. Daily Reachability. This figure shows the reachability for each day during our sample period of January to June 2012. The daily reachability is calculated as the ratio of the number of calls in which a borrower is reached to all calls that are placed during a given day. Days with no call attempts (e.g., Sundays and public holidays) are excluded from the figure. Gaps mean that no borrower from our main sample of 3,448 POS borrowers is reached that day.

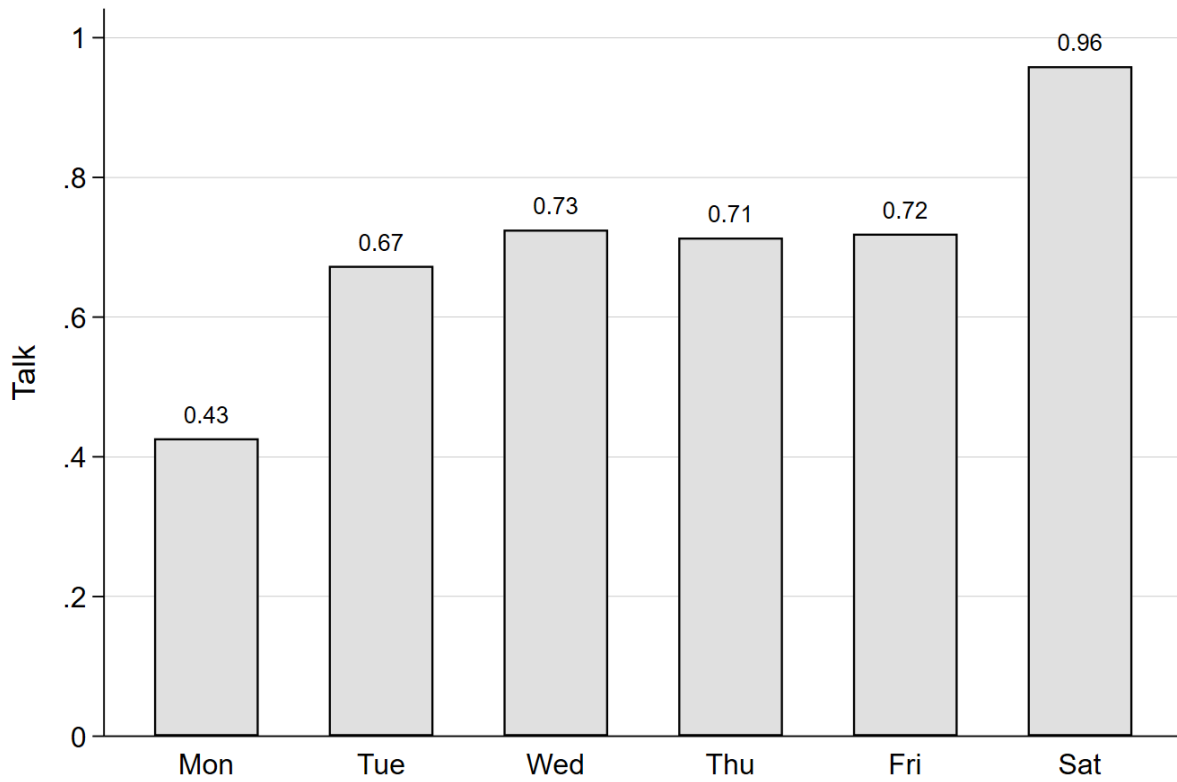


Figure 3. Overall Reachability by Day of the First Call. This figure shows the average of *Talk* by the day of the first call attempt, calculated across all 3,448 POS borrowers in our main sample.

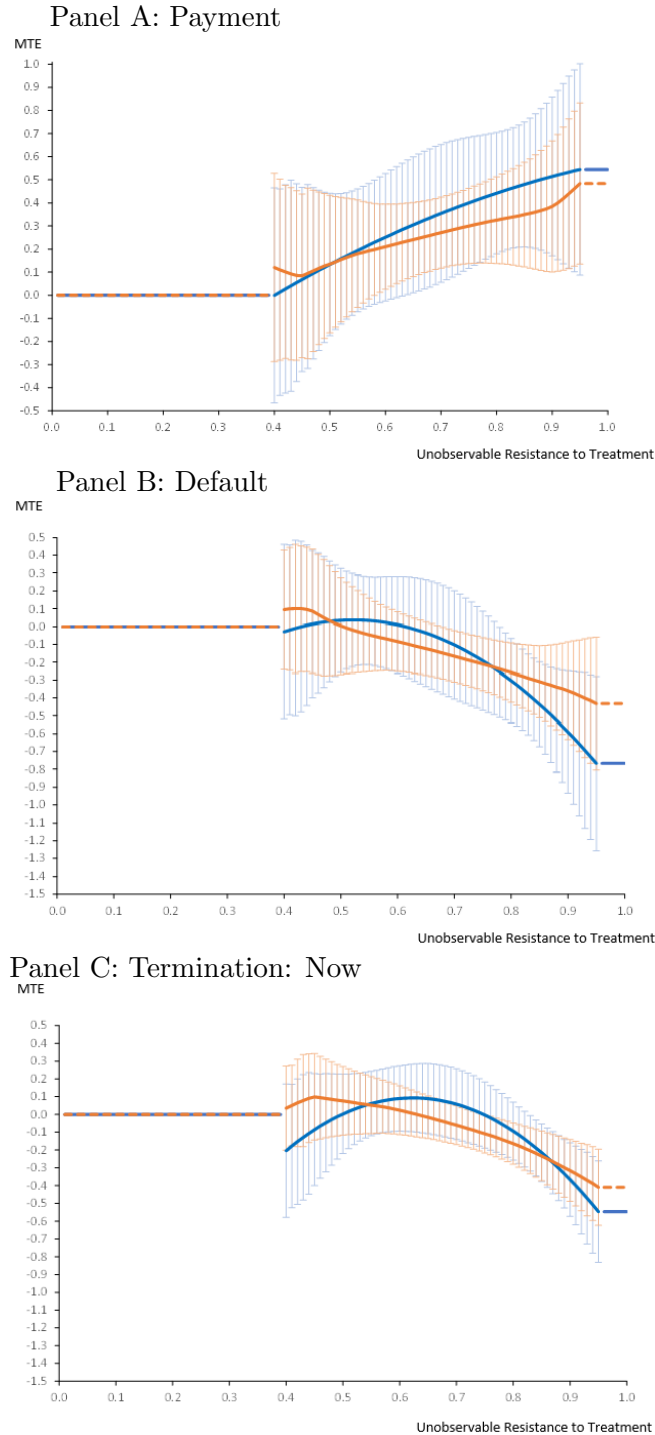


Figure 4. Marginal Treatment Effects. This figure shows estimated marginal treatment effects (MTEs) over the range of unobservable resistance to treatment across the 0/1 interval for *Payment* (Panel A), *Default* (Panel B), and *Termination: Now* (Panel C) for the main sample of POS borrowers. The blue (gray) line shows MTE estimates from a parametric second-order polynomial approximation. The orange (black) line shows MTEs estimates from a semi-parametric approximation. Bootstrapped standard errors are shown corresponding to the 95% confidence interval. MTEs below the resistance level of 0.40 and above 0.95 are imposed, not estimated.

Table I
Summary Statistics

This table reports summary statistics for our main sample of delinquent POS borrowers (Panel A) as well as for our survey sample (Panel B). All variables are defined in Internet Appendix Table IA1.

Panel A: Main Sample						
	N	Mean	Median	SD	Min	Max
<i>Borrower Characteristics</i>						
Male	3,448	0.65	1	0.48	0	1
Age	3,448	33.78	30	12.16	18	75
Non-German	3,448	0.20	0	0.40	0	1
Student	3,448	0.10	0	0.31	0	1
Retired	3,448	0.04	0	0.18	0	1
Unemployed	3,448	0.00	0	0.02	0	1
<i>Loan Characteristics</i>						
Installment (in €000's)	3,448	0.07	0.06	0.05	0.03	0.56
Initial amount (in €000's)	3,448	1.11	0.76	1.21	0.15	17.30
Interest rate	3,448	0.01	0	0.03	0	0.13
Repayment term (in months)	3,448	16.88	12	12.38	1	82
Time since origination (in months)	3,448	8.28	6	7.76	0	66
Outstanding loan amount (in €000's)	3,448	0.71	0.47	0.92	0.03	15.14
<i>Call Characteristics</i>						
Number of call attempts	3,448	3.24	3	3.05	1	10
Talk	3,448	0.72	1	0.45	0	1
Call duration (in minutes)	2,432	2.68	2.19	1.71	0.53	18.68
<i>Payment Behavior</i>						
Payment	3,448	0.70	1	0.46	0	1
Default	3,448	0.24	0	0.43	0	1
Termination: Now	3,448	0.10	0	0.30	0	1
Payment Alt. 1	3,448	0.58	1	0.49	0	1
Payment Alt. 2	3,448	0.67	1	0.43	-0.23	1.25
Termination: Later	3,092	0.08	0	0.28	0	1
Termination: Overall	3,448	0.18	0	0.38	0	1
Future delinquency	3,092	0.37	0	0.48	0	1
Time to next delinquency (in days)	1,140	97.57	57	112.01	1	815

Table I (continued): Summary Statistics

Panel B: Survey Sample						
	N	Mean	Median	SD	Min	Max
<i>Borrower Characteristics</i>						
Male	245	0.67	1	0.47	0	1
Age	245	40.98	38	15.95	18	87
Delinquency last 6M	245	0.36	0	0.48	0	1
<i>Loan Characteristics</i>						
Delinquent amount (in €)	245	820	360	1,195	25	6,046
Installment loan	245	0.12	0	0.33	0	1
<i>Call Characteristics</i>						
First call	245	0.80	1	0.40	0	1
Call duration (in minutes)	245	2.23	1.68	1.80	0.02	10.26
<i>Payment Behavior</i>						
Payment	245	0.82	1	0.39	0	1
<i>Agent Assessment</i>						
Surprise delinquency	245	0.27	0	0.44	0	1
Surprise call	245	0.40	0	0.49	0	1
<i>Agent Characteristics</i>						
Male agent	13	0.63	1	0.51	0	1
Age agent	13	34.23	36	10.22	21	46
Likeable voice	8	0.53	0.57	0.16	0.33	0.86
Likeable voice (adjusted)	8	-0.04	-0.04	0.14	-0.18	0.25

Table II
Random Assignment: Differences in Characteristics by Day of First Call

This table reports mean borrower and loan characteristics separately for those who receive their first call on a Saturday (column (1)), Monday (column (2)), or another day of the week (column (3)). Columns (4) - (6) report *p*-values corresponding to mean difference tests with Sidak correction for multiple comparisons. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) First Call Saturday (N=220)	(2) First Call Monday (N=212)	(3) First Call Other Day (N=3,016)	(4) <i>p</i> -val. (1)–(2)	(5) <i>p</i> -val. (1)–(3)	(6) <i>p</i> -val. (2)–(3)
Male	0.61	0.60	0.65	1.00	0.489	0.401
Age	37.84	33.86	33.48	0.002	0.000	0.961
Non-German	0.18	0.15	0.21	0.873	0.580	0.117
Student	0.09	0.13	0.10	0.319	0.808	0.463
Retired	0.07	0.05	0.03	0.562	0.004	0.318
Unemployed	0.00	0.00	0.00	1.00	0.971	0.973
North Germany	0.22	0.16	0.20	0.347	0.890	0.407
East Germany	0.05	0.09	0.05	0.204	1.00	0.042
South Germany	0.38	0.37	0.40	0.988	0.885	0.654
West Germany	0.35	0.38	0.34	0.863	0.998	0.612
Installment	0.07	0.07	0.07	0.971	0.989	0.893
Initial amount	1.11	1.06	1.12	0.971	1.00	0.903
Interest rate	0.01	0.01	0.01	0.853	0.808	0.997
Repayment term	16.33	15.72	17.00	0.940	0.818	0.372
Time since origination	7.61	7.35	8.40	0.980	0.381	0.165

Table III
First Stage

This table reports coefficient estimates from a linear probability model of *Talk* on two instrumental variables, *First Call Saturday* and *First Call Monday*, for the main sample of POS borrowers. In column (1), all borrowers are included. In column (2), only borrowers whose first call attempt occurs on a Saturday or Monday are included. All regressions include borrower and loan characteristics as well as location and month fixed effects. Standard errors are clustered at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Talk	(2) Talk
First call Saturday	0.238*** (0.018)	0.520*** (0.047)
First call Monday	-0.260*** (0.037)	
Installment	0.036 (0.260)	-0.317 (0.720)
Initial amount	0.013 (0.015)	0.059* (0.031)
Interest rate	-0.239 (0.316)	-1.040 (0.937)
Repayment term	-0.000 (0.001)	-0.002 (0.004)
Time since origination	-0.005*** (0.001)	-0.001 (0.004)
Male	0.021 (0.016)	-0.010 (0.050)
Non-German	-0.021 (0.018)	-0.009 (0.069)
Age	0.009** (0.005)	0.037** (0.015)
Age squared	-0.009 (0.006)	-0.045** (0.019)
Student	0.064** (0.027)	0.143 (0.098)
Retired	0.072 (0.053)	0.261* (0.156)
Unemployed	-0.210 (0.383)	
Month FE	Yes	Yes
Location FE	Yes	Yes
Observations	3,448	432
Adjusted R^2	0.038	0.334
F -statistic	155.92	121.27

Table IV
Talk Effect on Repayment Behavior

This table reports the effects of *Talk* on *Payment*, *Default*, and *Termination: Now* estimated by two-stage least squares (2SLS, columns (1) - (3)), as well as ordinary least squares (OLS, columns (4) - (6)) estimations. Panel A reports results for the full sample of POS borrowers. Panel B reports 2SLS results for the sub-sample of POS borrowers whose first call occurs on a Saturday or Monday. All regressions include borrower and loan characteristics as well as location and month fixed effects. Standard errors are clustered at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: All Borrowers						
	(1) Payment	(2) Default	(3) Termination: Now	(4) Payment	(5) Default	(6) Termination: Now
	2SLS	2SLS	2SLS	OLS	OLS	OLS
Talk	0.344*** (0.089)	-0.238*** (0.077)	-0.124*** (0.046)	0.201*** (0.021)	-0.121*** (0.016)	-0.072*** (0.015)
Installment	-0.133 (0.312)	0.246 (0.298)	-0.039 (0.168)	-0.125 (0.305)	0.239 (0.292)	-0.042 (0.168)
Interest rate	-0.666* (0.375)	1.201*** (0.350)	0.712*** (0.263)	-0.711* (0.382)	1.238*** (0.350)	0.728*** (0.262)
Initial amount	0.009 (0.016)	-0.006 (0.015)	0.002 (0.009)	0.011 (0.016)	-0.008 (0.015)	0.002 (0.009)
Repmnt. term	-0.002 (0.001)	0.005*** (0.001)	0.001 (0.001)	-0.002 (0.001)	0.005*** (0.001)	0.001 (0.001)
Time s. orig.	0.003** (0.001)	-0.008*** (0.001)	-0.006*** (0.001)	0.002** (0.001)	-0.007*** (0.001)	-0.005*** (0.001)
Male	-0.033** (0.015)	0.023 (0.014)	0.016* (0.010)	-0.030** (0.015)	0.021 (0.015)	0.015 (0.010)
Non-German	0.030 (0.021)	-0.013 (0.018)	-0.016 (0.010)	0.027 (0.021)	-0.010 (0.020)	-0.015 (0.011)
Age	0.018*** (0.005)	-0.017*** (0.005)	-0.009** (0.004)	0.020*** (0.004)	-0.018*** (0.005)	-0.010*** (0.004)
Age squared	-0.017*** (0.006)	0.015*** (0.006)	0.009* (0.005)	-0.019*** (0.005)	0.016*** (0.006)	0.009* (0.005)
Student	0.015 (0.031)	-0.004 (0.025)	-0.003 (0.017)	0.024 (0.030)	-0.011 (0.024)	-0.006 (0.017)
Retired	-0.050 (0.057)	0.064 (0.052)	-0.017 (0.037)	-0.040 (0.057)	0.056 (0.053)	-0.020 (0.037)
Unemployed	-0.573*** (0.100)	0.161 (0.384)	0.324 (0.351)	-0.601*** (0.054)	0.184 (0.350)	0.335 (0.340)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,448	3,448	3,448	3,448	3,448	3,448

Table IV (continued): Talk Effect on Repayment Behavior

Panel B: First Call Saturday or Monday

	(1) Payment	(2) Default	(3) Termination: Now
	2SLS	2SLS	2SLS
Talk	0.278*** (0.102)	-0.165* (0.091)	-0.088* (0.046)
Controls	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Location FE	Yes	Yes	Yes
Observations	432	432	432

Table V
Heterogeneous Treatment Effects

This table reports results from the estimation of marginal treatment effects (MTEs). Panel A reports the effect of observable borrower and loan characteristics for untreated borrowers, $\beta_{T=0}$, as well as the differential effect between treated and untreated borrowers, $\beta_{T=1} - \beta_{T=0}$, on *Payment*, *Default*, and *Termination: Now* estimated by local IV (Heckman and Vytlacil (1999, 2001, 2005)). Results in columns (1), (3), and (5) are based on a second-order polynomial MTE model. Results in columns (2), (4), and (6) are based on a semi-parametric MTE model. The effect of *Unemployed* as well as the location and month fixed effects are assumed constant between treated and untreated borrowers. Panel B reports average treatment effects (ATEs) for a borrower with average observable characteristics. Columns (1), (3), and (5) are based on a second-order polynomial MTE model. Columns (2), (4), and (6) are based on a semi-parametric MTE model. All standard errors are bootstrapped with 100 repetitions and account for clustering at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Heterogeneous Treatment Effects due to Observable Characteristics

	Payment		Default		Termination: Now	
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{T=0}$						
Interest rate	-0.974 (1.919)	-0.923 (1.920)	0.423 (1.803)	0.049 (1.831)	1.048 (1.123)	0.721 (1.123)
Initial amount	0.070 (0.076)	0.069 (0.075)	-0.051 (0.065)	-0.051 (0.065)	-0.039 (0.039)	-0.043 (0.040)
Repmnt. term	-0.012* (0.007)	-0.011 (0.007)	0.017*** (0.006)	0.017*** (0.007)	0.010** (0.005)	0.010** (0.005)
Time s. orig.	0.008 (0.008)	0.008 (0.007)	-0.014** (0.007)	-0.014** (0.007)	-0.014*** (0.005)	-0.013*** (0.005)
Installment	-1.206 (1.312)	-1.196 (1.306)	1.499 (1.413)	1.461 (1.442)	0.284 (0.824)	0.288 (0.827)
Male	0.067 (0.095)	0.083 (0.095)	-0.021 (0.085)	-0.052 (0.081)	0.030 (0.055)	0.004 (0.055)
Non-German	0.158 (0.121)	0.170 (0.121)	-0.199 (0.124)	-0.219* (0.125)	-0.117 (0.080)	-0.126 (0.079)
Age	0.076*** (0.029)	0.082*** (0.028)	-0.032 (0.027)	-0.038 (0.025)	-0.003 (0.016)	-0.019 (0.016)
Age squared	-0.089** (0.036)	-0.095*** (0.036)	0.036 (0.034)	0.043 (0.031)	0.003 (0.021)	0.023 (0.020)
Student	0.090 (0.233)	0.130 (0.231)	0.131 (0.176)	0.115 (0.177)	0.027 (0.133)	-0.014 (0.133)
Retired	0.792** (0.364)	0.822** (0.376)	-0.315 (0.310)	-0.352 (0.316)	0.013 (0.245)	-0.068 (0.250)

Table V (continued): Heterogeneous Treatment Effects

	Payment		Default		Termination: Now	
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_{T=1} - \beta_{T=0}$						
Interest rate	0.446 (2.838)	0.331 (2.841)	1.149 (2.599)	1.676 (2.640)	-0.497 (1.621)	-0.044 (1.616)
Initial amount	-0.089 (0.095)	-0.086 (0.094)	0.073 (0.082)	0.074 (0.084)	0.061 (0.053)	0.065 (0.054)
Repmnt. term	0.014 (0.009)	0.013 (0.010)	-0.017** (0.008)	-0.017** (0.008)	-0.013** (0.007)	-0.013** (0.007)
Time s. orig.	-0.007 (0.010)	-0.007 (0.010)	0.009 (0.009)	0.009 (0.009)	0.012* (0.006)	0.011* (0.006)
Installment	1.505 (1.637)	1.500 (1.627)	-1.815 (1.653)	-1.782 (1.698)	-0.510 (0.997)	-0.511 (1.004)
Male	-0.141 (0.128)	-0.160 (0.126)	0.060 (0.111)	0.102 (0.106)	-0.020 (0.070)	0.014 (0.070)
Non-German	-0.175 (0.163)	-0.193 (0.163)	0.256 (0.161)	0.284* (0.163)	0.142 (0.109)	0.154 (0.108)
Age	-0.077** (0.038)	-0.084** (0.038)	0.018 (0.038)	0.026 (0.034)	-0.012 (0.021)	0.009 (0.020)
Age squared	0.095** (0.047)	0.102** (0.046)	-0.024 (0.046)	-0.033 (0.042)	0.011 (0.027)	-0.014 (0.025)
Student	-0.107 (0.322)	-0.152 (0.319)	-0.191 (0.250)	-0.170 (0.249)	-0.052 (0.178)	0.001 (0.178)
Retired	-1.050** (0.437)	-1.083** (0.449)	0.458 (0.387)	0.502 (0.391)	-0.053 (0.289)	0.047 (0.294)
$\beta = \beta_{T=0} = \beta_{T=1}$						
Unemployed	-0.564 (0.365)	-0.541 (3.349)	0.220 (0.440)	0.193 (8.644)	0.378 (0.508)	0.353 (4.235)
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,448	3,448	3,448	3,448	3,448	3,448

Panel B: Average Treatment Effects (ATE)

	Payment		Default		Termination: Now	
	(1)	(2)	(3)	(4)	(5)	(6)
Average treatment effects	0.201*** (0.053)	0.166*** (0.046)	-0.140*** (0.049)	-0.105** (0.049)	-0.071** (0.035)	-0.063** (0.030)

Table VI
Talk Effect by Size of the Outstanding Loan Amount

This table reports the effects of *Talk* on *Payment*, *Default*, and *Termination: Now* estimated by 2SLS estimations, separately for POS borrowers with outstanding loan amounts below and above the median amount. All regressions use *First Call Saturday* and *First Call Monday* as instruments and include borrower and loan characteristics as well as location and month fixed effects. *p*-values of tests of the equality of coefficient estimates between both sub-samples are reported. Standard errors are clustered at the location level. All variables are defined in Internrt Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Payment	Payment	Default	Default	Term.: Now	Term.: Now
	<i>Low</i> Amount	<i>High</i> Amount	<i>Low</i> Amount	<i>High</i> Amount	<i>Low</i> Amount	<i>High</i> Amount
Talk	0.322*** (0.101)	0.357*** (0.138)	-0.132 (0.084)	-0.310** (0.133)	-0.088 (0.054)	-0.140* (0.077)
<i>p</i> -value for the test: Talk ^{Low} = Talk ^{High}		0.729		0.244		0.474
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,724	1,724	1,724	1,724	1,724	1,724

Table VII
Borrowers' Surprise about the Delinquency or the Call

This table reports the effects of *Talk* on *Payment* estimated by OLS regression for the survey sample. In columns (1) and (3) all borrowers are included. In columns (2) and (4) we exclude follow-up calls from the regression. Given the small size of the sample and the absence of time variation, neither month nor location fixed effects are included. All variables are defined in Internet Appendix Table IA1. *p*-values are presented in parentheses and account for clustering at the agent level using a wild bootstrap-*t* approach with 1,000 repetitions (see Cameron et al. (2008) and Menger (2015)). ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	Payment			
	(1)	(2)	(3)	(4)
	All	First Call Only	All	First Call Only
Surprise: Delinquency	0.024 (0.498)	-0.012 (0.822)	0.002 (0.950)	-0.024 (0.666)
Surprise: Call			0.039 (0.596)	0.022 (0.720)
Delinquent amount	-0.026 (0.416)	-0.038 (0.262)	-0.028 (0.334)	-0.039 (0.190)
Installment loan	-0.209** (0.032)	-0.091 (0.104)	-0.208** (0.036)	-0.090 (0.108)
Male	-0.041 (0.458)	0.023 (0.604)	-0.041 (0.446)	0.022 (0.630)
Age	-0.012 (0.122)	-0.013* (0.078)	-0.012* (0.088)	-0.013* (0.050)
Age squared	0.000 (0.102)	0.000* (0.060)	0.000* (0.058)	0.000** (0.038)
Delinquency last 6M	-0.094 (0.300)	-0.097 (0.282)	-0.095 (0.312)	-0.098 (0.280)
First call	0.131* (0.078)		0.130* (0.070)	
Call duration	-0.014 (0.354)	-0.027 (0.124)	-0.014 (0.322)	-0.028 (0.124)
Constant	1.051*** (0.000)	1.200*** (0.000)	1.060*** (0.000)	1.204*** (0.000)
Observations	245	196	245	196
Adjusted R^2	0.082	0.039	0.080	0.034

Table VIII
Follow-up Calls

Panel A of this table reports averages for *Payment* as well as two alternative outcome variables, *Payment: Alt. 1* and *Payment: Alt. 2*, for subsets of delinquent POS borrowers. For untreated borrowers (*Talk*=0), both alternative payment outcomes equal *Payment*. For treated borrowers (*Talk*=1), *Payment: Alt. 1* is one if a borrower pays as agreed during the initial call, but zero otherwise. *Payment: Alt. 2* instead assumes that treated borrowers who do not pay as initially agreed repay at the probability predicted by the subset that are not reached for a follow-up call. Panel B reports the effects of *Talk* on *Payment*, *Payment: Alt. 1*, and *Payment: Alt. 2* estimated by estimated by 2SLS estimations. All regressions include borrower and loan characteristics as well as location and month fixed effects. Standard errors are clustered at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Average Payment Outcomes				
Subset of Borrowers	N	Payment	Payment: Alt. 1	Payment: Alt. 2
Untreated borrowers (Talk=0)	950	0.55	0.55	0.55
Treated borrowers (Talk=1)	2,498	0.75	0.59	0.72
<i>thereof</i> : Fail to pay as agreed	914	0.44	0	0.33
- Reached for follow-up call	631	0.49	0	0.34
- Not reached for follow-up call	283	0.31	0	0.31
Panel B: Talk Effect without Follow-up Call				
	(1) Payment	(2) Payment Alt. 1	(3) Payment Alt. 2	
Talk	0.344*** (0.089)	0.223** (0.106)	0.310*** (0.095)	
Controls	Yes	Yes	Yes	
Month FE	Yes	Yes	Yes	
Location FE	Yes	Yes	Yes	
Observations	3,448	3,448	3,448	

Table IX
Talk Effect by Nationality

This table reports the effects of *Talk* on *Payment*, *Default*, and *Termination: Now* estimated by 2SLS estimations, separately for German and Non-German POS borrowers. All regressions use *First Call Saturday* and *First Call Monday* as instruments and include borrower and loan characteristics as well as location and month fixed effects. *p*-values of tests of the equality of coefficient estimates between both sub-samples are reported. Standard errors are clustered at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Payment <i>German</i>	(2) Payment <i>Non-German</i>	(3) Default <i>German</i>	(4) Default <i>Non-German</i>	(5) Term.: Now <i>German</i>	(6) Term.: Now <i>Non-German</i>
Talk	0.388*** (0.097)	0.189 (0.158)	-0.320*** (0.087)	0.025 (0.186)	-0.156*** (0.053)	-0.048 (0.106)
<i>p</i> -value of the test: Talk ^{German} = Talk ^{Non-German}		0.135		0.016		0.124
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,745	703	2,745	703	2,745	703

Table X
Agent Characteristics and Payment

This table reports the effect of *Likeable Voice* and *Likeable Voice (adjusted)* on *Payment* estimated by OLS regression for the survey sample excluding borrowers, for whom voice recordings are not available. Given the small size of the sample and the absence of time variation, neither month nor location fixed effects are included. All variables are defined in Internet Appendix Table IA1. *p*-values are presented in parentheses and account for clustering at the agent level using a wild bootstrap-*t* approach with 1,000 repetitions (see Cameron et al. (2008) and Menger (2015)). ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Payment	(2) Payment
Likeable voice	0.248* (0.092)	
Likeable voice (adjusted)		0.261** (0.040)
Male agent	0.188 (0.134)	0.175 (0.150)
Age agent	0.001 (0.708)	0.002 (0.406)
Delinquent amount	-0.077*** (0.002)	-0.079*** (0.002)
Installment loan	-0.331*** (0.002)	-0.335*** (0.002)
Male	-0.054 (0.230)	-0.057 (0.176)
Age	-0.018 (0.104)	-0.018 (0.122)
Age squared	0.000 (0.138)	0.000 (0.114)
Del. Last 6M	-0.135 (0.232)	-0.137 (0.232)
First call	0.132*** (0.008)	0.129** (0.036)
Call duration	-0.018 (0.538)	-0.019 (0.556)
Constant	0.916*** (0.000)	1.059*** (0.000)
Observations	135	135
Adjusted R^2	0.169	0.169

Table XI
Long-term Effects of Talk

This table reports effects of *Talk* on different outcome variables for the main sample of POS borrowers. Panel A reports the effects of *Talk* on *Termination: Now* and *Termination: Overall* estimated by 2SLS estimations with *First Call Saturday* and *First Call Monday* as instruments. The table also reports the *p*-value of a test of the equality of the coefficient estimates for *Talk* in columns (1) and (2). Panel B reports OLS estimates of the effect of *Talk* on *Future Delinquency*, *Time to Next Delinquency*, and *Termination: Later*. We follow Oster (2019) and compare the degree of selection on unobservables relative to observables. Specifically, we report the magnitude of unobservables relative to observables that is needed for the unobservables to explain our results (δ). All regressions include borrower and loan characteristics as well as location and month fixed effects. All variables are defined in Internet Appendix Table IA1. Standard errors are clustered at the location level. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Loan Termination			
	(1) Termination: Now	(2) Termination: Overall	
Talk	-0.124*** (0.046)	-0.235*** (0.069)	
<i>p</i> -value of the test: $Talk^{Now} = Talk^{Overall}$		0.021	
Controls	Yes	Yes	
Month FE	Yes	Yes	
Location FE	Yes	Yes	
Observations	3,448	3,448	
Panel B: Conditional Outcomes			
	(1) Future Delinquency	(2) Time to Next Delinquency	(3) Termination: Later
Talk	-0.079*** (0.019)	28.554*** (6.868)	-0.049*** (0.011)
Controls	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Location FE	Yes	Yes	Yes
Observations	3,092	1,140	3,092
R^2	0.097	0.213	0.079
δ (Oster, 2019)	1.97	0.81	2.85

Table XII
External Validity: Evidence from Overdraft Lines of Credit

This table reports the effects of *Talk* on *Payment*, *Default*, and *Termination: Now* estimated by 2SLS estimation for a sample of borrowers who are delinquent on their overdraft lines of credit associated with their checking account. All regressions use *First Call Saturday* and *First Call Monday* as instruments and include borrower and overdraft characteristics as well as location and month fixed effects. Standard errors are clustered at the location level. All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Payment	(2) Default	(3) Termination: Now
Talk	0.204* (0.110)	-0.153* (0.090)	-0.122* (0.072)
Delinquent amount	-0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Amount credit line	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Male	-0.026** (0.011)	0.025*** (0.008)	0.011* (0.006)
Non-German	-0.005 (0.015)	0.004 (0.014)	0.007 (0.009)
Age	0.001 (0.003)	-0.003 (0.003)	-0.000 (0.002)
Age squared	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Student	0.010 (0.018)	-0.010 (0.013)	-0.006 (0.009)
Retired	0.008 (0.033)	-0.005 (0.024)	0.004 (0.015)
Unemployed	-0.026 (0.045)	0.014 (0.033)	0.009 (0.017)
Month FE	Yes	Yes	Yes
Location FE	Yes	Yes	Yes
Observations	2,499	2,499	2,499

Internet Appendix for Personal Communication in an Automated World: Evidence from Loan Repayments

Christine Laudenbach and Stephan Siegel*

This internet appendix presents additional figures and tables (Section A.) and discusses the size of treatment effects in related studies (Section B.).

*Citation format: Laudenbach, Christine, and Stephan Siegel, Internet Appendix for “Personal Communication in an Automated World: Evidence from Loan Repayments,” *Journal of Finance*, DOI: MS-20180987.

A. Additional Figures and Tables

	XY Bank
Hans Mustermann Musterstraße 23 12345 Musterstadt	
	Monday, 5th March 2012
Your account number: 105 1234567	
Dear Mr. Mustermann -	
The balance on your consumer loan account does not conform to the terms of the loan agreement. Currently, the account is in arrears with EUR 175 .	
Please use the following bank account to pay the outstanding arrears until the 19th of March :	
XY Bank	
Account Number: 7654321	
Bank Sort Code: 54089312	
You may receive dunning letters, which inform you about the overdue payment, until the full repayment of arrears. In case these arrears are repaid by the above date, no further action is required.	
Please act now, because this is the only way to avoid the commencement of dunning proceedings.	
In the future, please contact our employees on time, before you get into payment difficulties. Experience has taught us that a short call is often sufficient to find a solution.	
Kind regards.	
XY Bank	
This is a machine generated letter without a signature.	

Figure IA1. Sample Letter. This figure presents a sample letter that is sent to all delinquent borrowers on day 15 of the delinquency. The lender is referred to as “XY Bank.”

Agent (A): Hello my name is Fritz Mueller and I am calling from XY Bank. Am I speaking with Mr Hans Mustermann?

Borrower (B): Yes, you are.

A: Could you please tell me your birth date and your zip code for verification?

B: 01.05.1963 and my zip code is 12345

A: Perfect, thank you. I am calling, since the actual balance on your consumer loan account does not conform to the terms of agreement. Currently, the account is in arrears of EUR 175. We are very interested in finding a joint solution. How do you intend to repay the amount outstanding?

B: Yes, sorry. This month it did not work out as planned. I have already planned to settle the balance. At the beginning of next month, once my paycheck comes in, I will be able to transfer the money.

A: All right, that's within two weeks then.

B: Yes.

A: You will receive a letter with the information on which account to transfer the outstanding arrears within two weeks.

B: Ok, thank you.

A: Perfect. Thanks for your time and have a nice day.

Figure IA2. Sample Call Transcript. This figure presents the transcript of a stylized sample phone conversation between a call center agent (A) and a delinquent borrower (B). The lender is referred to as “XY Bank.”

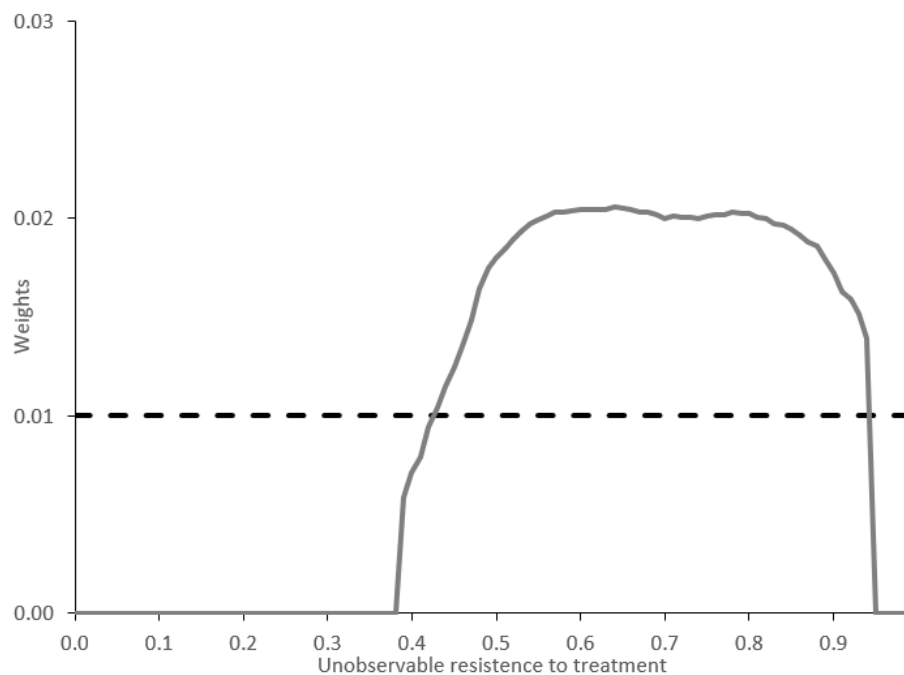


Figure IA4. Weights: LATE vs. ATE. This figure presents the weights associated with different levels of unobservable resistance to treatment for the calculation of local treatment effects (LATEs) related to the 2SLS estimates (solid gray line) as well as for the calculation of average treatment effects (ATEs) (non-solid black line).

Table IA1
Variable Descriptions

Variable	Description
Age	Age of the borrower.
Age agent	The age of the call center agent speaking with a borrower.
Call duration	The duration of the phone call, in minutes.
Default	An indicator variable that equals one if the borrower's payment is overdue for more than 90 days and zero otherwise.
Delinquent amount	The amount of the borrower's shortfall, in thousands. For POS borrowers it equals the installment amount. For overdraft borrowers, it equals the amount that the credit line is overdrawn.
Delinquency last 6M	An indicator variable, which is one if the borrower was delinquent (more than 20 days) during the previous six months and zero otherwise.
East Germany	An indicator variable that equals one if the borrower lives in the East of Germany (i.e., Brandenburg, Berlin, Saxony, Saxony-Anhalt, Thuringia), and zero otherwise.
First call	An indicator variable, which is one if this is the first phone call related to the current delinquency and zero in case of a follow-up call.
First call Monday	An indicator variable that equals one if the first call attempt is on a Monday and zero otherwise.
First call Saturday	An indicator variable that equals one if the first call attempt is on a Saturday and zero otherwise.
Future delinquency	An indicator variable that equals one if a borrower becomes delinquent for a second time, after resolving the first delinquency, and zero otherwise.
Initial loan amount	The borrower's full initial loan amount, agreed upon at origination, in thousands.
Installment	The monthly loan installment, which equals the borrower's shortfall amount, measured at the beginning of the collection period, in thousands.
Interest rate	The borrower's interest rate of the consumer loan, agreed upon at origination.
Likeable voice	The fraction of six to seven raters that rate an agent's voice as 4 ("Likeable") or 5 ("Very Likeable") and zero otherwise.

Table IA1 (continued): Variable Descriptions

Variable	Description
Likeable voice (adjusted)	We first convert each rating into an indicator, which is one for ratings of four and five, and zero otherwise. We then form the average of this indicator for each rater and subtract it from the indicator. Likeable Voice (Adjusted) is the average of the adjusted indicators across raters, which we form for each agent.
Male	An indicator variable that equals one if the borrower is male and zero if female.
Male agent	An indicator variable that equals one if the agent is male and zero if female.
Non-German	An indicator variable that equals one if the borrower is a German resident but not a citizen and zero otherwise.
North Germany	An indicator variable that equals one if the borrower lives in the north of Germany (i.e., Schleswig-Holstein, Mecklenburg-Western Pomerania, Hamburg, Bremen, Lower Saxony) and zero otherwise.
Outstanding loan amount	The borrower's outstanding loan amount, measured at the beginning of the delinquency, in thousands.
Payment	An indicator variable that equals one if the borrower resolves the delinquency within 45 days of its initial occurrence by repaying the outstanding amount and zero otherwise.
Payment: Alt. 1	An indicator variable that equals <i>Payment</i> for untreated borrowers. For treated borrowers it is one if a borrower pays as agreed within a few weeks, and zero otherwise.
Payment: Alt. 2	A variable that equals <i>Payment</i> for untreated borrowers. For treated borrowers who fail to repay as initially agreed and who are not reached for a follow-up call, we regress <i>Payment</i> on all independent variables except for <i>Talk</i> and then use the predicted payment outcome for all treated borrowers who fail to repay as initially agreed, whether or not they are reached for a follow-up call.
Repayment term	Length of the loan measured by the number of total monthly payments.
Retired	An indicator variable that equals one if the borrower is a retiree and zero otherwise.
South Germany	An indicator variable that equals one if the borrower lives in the south of Germany (i.e., Bavaria, Baden-Wuerttemberg) and zero otherwise.

Table IA1 (continued): Variable Descriptions

Variable	Description
Student	An indicator variable that equals one if the borrower is a student and zero otherwise.
Surprise: Delinquency	An indicator variable that equals one if an agent selects 4 (“Agree”) or 5 (“Strongly Agree”) on a 1-5 scale answering the following statement: “The customer was surprised about the delinquency” and zero otherwise.
Surprise: Call	An indicator variable that equals one if an agent selects 4 (“Agree”) or 5 (“Strongly Agree”) on a 1-5 scale answering the following statement: “The customer was surprised about the call” and zero otherwise.
Talk	An indicator variable that equals one if a borrower speaks with a call center agent on the phone and zero otherwise.
Termination: Now	An indicator variable that equals one if the loan has been terminated by the bank as a result of the current delinquency and zero otherwise.
Termination: Later	An indicator variable that equals one if the loan has been terminated by the bank as a result of a future delinquency and zero otherwise.
Termination: Overall	An indicator variable that equals one if the loan has been terminated by the bank at any point and zero otherwise.
Time since origination	The number of months since loan origination.
Time until next delinquency	The number of days between the first and second delinquency (as measured by their entry into the call center system) for borrowers who become delinquent at least twice between January 2012 and August 2014.
Unemployed	An indicator variable that equals one if the borrower is unemployed and zero otherwise.
West Germany	An indicator variable that equals one if the borrower lives in the west of Germany (i.e., Hesse, North Rhine-Westphalia, Rhineland-Palatinate, Saarland) and zero otherwise.

Table IA2
Effects of Observable Heterogeneity

Columns (1) and (2) of this table report the mean of observable characteristics estimated for all borrowers as well as the compliers. In columns (3) through (8), we multiply the difference in means for each characteristic with the corresponding point estimates of $\beta_{T=1} - \beta_{T=0}$ from Table V, Panel A, and sum the effects for each column to obtain the overall effect of differences in observable characteristics on the difference between average and local average treatment effects.

	Mean		(Mean (All) - Mean (Compliers)) \times ($\beta_{T=1} - \beta_{T=0}$)					
	All	Compliers	Payment		Default		Termination: Now	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interest rate	0.010	0.012	-0.001	-0.001	-0.003	-0.004	0.001	0.000
Initial amount	1.112	0.980	-0.012	-0.011	0.010	0.010	0.008	0.009
Repay. term	16.882	15.979	0.012	0.012	-0.015	-0.015	-0.012	-0.012
Time s. orig.	8.284	7.046	-0.008	-0.009	0.011	0.011	0.015	0.014
Installment	0.070	0.067	0.004	0.004	-0.005	-0.005	-0.001	-0.001
Male	0.646	0.606	-0.006	-0.007	0.002	0.004	-0.001	0.001
Non-German	0.204	0.183	-0.004	-0.004	0.005	0.006	0.003	0.003
Age	33.782	35.403	0.125	0.136	-0.029	-0.042	0.019	-0.015
Age squared	12.892	14.388	-0.142	-0.153	0.035	0.050	-0.017	0.021
Student	0.104	0.097	-0.001	-0.001	-0.001	-0.001	0.000	0.000
Retired	0.796	0.817	0.022	0.023	-0.010	-0.011	0.001	-0.001
Overall Effect of Observable Heterogeneity			-0.009	-0.011	0.001	0.003	0.016	0.019

Table IA3
Random Assignment of Call Center Agents

This table reports different results on the randomization of call center agents using the survey sample. Panel A reports p -values of the joint significance of agent fixed effects from OLS regressions of loan (Delinquent Amount), borrower (Male, Age), call (First Call) and payment characteristics (Delinquency last 6M) on the remaining loan and borrower characteristics and agent fixed effects using the survey sample. p -values are bootstrapped with 1,000 repetitions. Panel B reports p -values of the joint significance of loan and borrower characteristics from linear probability models predicting the agent that is matched to a given borrower. p -values account for clustering at the agent level using a wild bootstrap- t approach with 1,000 repetitions (see Cameron et al. (2008) and Menger (2015)). Panel C reports results from linear regression of the age and gender of the agent that is matched to a given borrower on loan and borrower characteristics. p -values are presented in parentheses and account for clustering at the agent level using a wild bootstrap- t approach with 1,000 repetitions (see Cameron et al. (2008) and Menger (2015)). All variables are defined in Internet Appendix Table IA1. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Agent Fixed Effects

	(1) Delinquent Amount	(2) Male Borrower	(3) Age Borrower	(4) First Call	(5) Delinquency Last 6M
Controls	Yes	Yes	Yes	Yes	Yes
Agent FE	Yes	Yes	Yes	Yes	Yes
p -value of joint test of Agent FE	0.57	0.50	0.00	0.64	0.75
Observations	245	245	245	245	245

Panel B: Predicting Matched Call Center Agents

	(1) ID 1	(2) ID 2	(3) ID 3	(4) ID 4	(5) ID 5	(6) ID 6	(7) ID 7	(8) ID 8	(9) ID 9	(10) ID 10	(11) ID 11	(12) ID 12	(13) ID 13
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p -value of joint test of Controls	0.54	0.91	0.25	0.31	0.58	0.91	0.90	0.28	0.67	0.86	0.41	0.14	0.73
Observations	245	245	245	245	245	245	245	245	245	245	245	245	245

Table IA3: The Role of Agents in the Process

Panel C: Predicting Agent Gender and Age		
	(1)	(2)
	Age Agent	Male Agent
Delinquent amount	0.00 (0.47)	-0.00 (0.30)
Installment loan	-1.15 (0.48)	0.03 (0.66)
Male	-0.79 (0.52)	-0.07 (0.25)
Age	0.09 (0.42)	-0.00 (0.85)
Age squared	-0.00 (0.57)	0.00 (0.50)
Del. last 6M	1.17 (0.21)	0.00 (1.00)
First call	0.67 (0.29)	-0.05 (0.44)
Constant	0.67 (0.29)	-0.05 (0.44)
Observations	245	245
R^2	0.01	0.02

Table IA4
Summary Statistics: Overdraft Sample

This table reports summary statistics for borrowers who are delinquent for the first time on their overdraft lines of credit. All variables are defined in Internet Appendix Table IA1.

	N	Mean	Median	SD	Min	Max
<i>Borrower Characteristics</i>						
Male	2,499	0.60	1	0.49	0	1
Age	2,499	35.01	32	12.84	14	75
Student	2,499	0.27	0	0.45	0	1
Retired	2,499	0.04	0	0.21	0	1
Unemployed	2,499	0.04	0	0.21	0	1
<i>Loan Characteristics</i>						
Delinquent amount (in €)	2,499	167	100	181	25	1,343
Amount credit line (in €)	2,499	1,008	600	1,201	0	7,300
<i>Call Characteristics</i>						
Talk	2,499	0.69	1	0.46	0	1
Call duration (in minutes)	1,785	2.52	2.04	1.69	0.25	24.43
<i>Payment Behavior</i>						
Payment	2,499	0.92	1	0.26	0	1
Default	2,499	0.05	0	0.22	0	1
Termination: Now	2,499	0.02	0	0.15	0	1

B. Discussion: Effect Sizes

We discuss the size of the treatment effects in this study in comparison to related studies. For a summary, see Internet Appendix Table IB1. While many papers examine credit-related interventions that aim at preventing delinquency and default, only a few papers study treatments targeted at already delinquent borrowers. Similar to our setting, Bursztyn et al. (2019) use a sample of credit card borrowers in Indonesia who have missed their most recent payment. In particular, the authors study the effect of different incentives on borrowers' probability of recovering from the delinquency within 10 days. Differently from our setting, the treatment does not include any follow-up communication. They find that text messages sent to delinquent borrowers emphasizing moral or credit-reputation-based incentives increase the repayment probability by 9 percentage points and 13 percentage points, respectively, or by 28% and 41% relative to the (10-day) repayment probability of untreated borrowers of 32%. For a sub-sample of high-risk borrowers, treatment effects are even larger, ranging between 14 percentage points and 19 percentage points or 65% and 73% in relative terms.

Among delinquent POS borrowers in Germany, we find that a short phone conversation with a bank agent with a possible follow-up call has an average treatment effect on the 45-day repayment probability of 17 percentage points to 20 percentage points. While the effect size is slightly larger than those in Bursztyn et al. (2019) in absolute terms, it is very similar in relative terms, a 29% to 36% increase relative to the (45-day) repayment probability of untreated borrowers of about 56% to 58%.¹

With respect to default, Bursztyn et al. (2019) do not find any treatment effects for their full sample. However, they find large effects for the sub-sample of high risk borrowers, captured by a reduction of the default probability by 9 percentage points (moral obligation) and 11 percentage points (credit reputation), relative to a default probability of 13 percentage points for the control group. While we find a significant average treatment effect of -10 percentage points to -14 percentage points for all borrowers, its relative size is smaller given a higher overall default rate in our sample.

Looking at future delinquencies, Schoar (2012) finds that among Indian small business borrowers, for borrowers with one previous late payment, a personal contact with a bank agent reduces the probability of a second late payment by between 7 percentage points and 22 percentage points, depending on the intensity of treatment relative to untreated borrowers who receive only a payment reminder in form of a text message and relative to an overall probability of a second delinquency of 31%. We find that treated borrowers are about 8 percentage points less likely to become delinquent again compared to a sample average of 37%. The effect size in our study is therefore similar to the effect size of the low-intensity treatment, which consists of a reminder phone call by randomly assigned bank agents.

Our effect sizes can also be compared to those of interventions that occur before, not after, the payment due date. For example, Karlan, Morten, and Zinman (2015) use a sample of Philippine microloan borrowers and find that personalized payment reminders in the form of text messages reduce the probability of an unpaid outstanding balance 30 days after the maturity of the loan by between 5 percentage points and 8

¹Given the observational nature of our data, we do not have a control sample. For comparison purposes, we calculate the expected outcome in the absence of a phone call (p_c) for our sample by solving the relationship between the observed outcome for the full sample (p_f), the 72% treatment probability, and the estimated average treatment effect (ATE): $p_f = 0.28p_c + 0.72(p_c + ATE)$.

percentage points or by 36% to 57% relative to the probability of an unpaid balance of 14% among untreated borrowers. Interestingly, they find that only personalized reminders have an effect on the repayment probability, while non-personalized reminders do not. Moulton, Collins, and Loibl (2015) provide evidence from a field experiment with first-time home buyers in the United States. They find that quarterly contact from a financial coach reduces 90-day mortgage delinquencies by 20% for all borrowers and by 41% for high-risk borrowers relative to borrowers who do not receive coaching. Finally, Cadena and Schoar (2011) show that payment reminders as well as economic incentives in the form of refunds or future rate reductions improve repayment behavior among Ugandan micro borrowers by about 30%, while Medina (2021) finds that simple phone notifications sent to Brazilian credit card borrowers about upcoming payments reduce the occurrence of late fees by about 9%.

Overall, our findings provide evidence that personal communication between a delinquent borrower and a bank agent can significantly improve the borrower's immediate and future repayment behavior. The effect sizes are substantial but comparable to what other studies have documented in broadly related settings.

Table IB1
Effect Sizes

Outcome	Loan Type	Baseline	Treatment Effect (abs.)	Treatment Effect (rel.)	Authors	Comment
Delinquent Borrowers						
Payment (by day 45)	POS	56 to 58%	+17 to +20pp	+29 to +36%	THIS PAPER	
Payment (by day 10)	Credit card	32%	+9pp	+28%	Bursztyn et al. (2019)	Moral incentives
Payment (by day 10)	Credit card	32%	+13pp	+41%	Bursztyn et al. (2019)	Financial incentives
Payment (by day 10)	Credit card	26%	+14 to +19pp	+65 to +73%	Bursztyn et al. (2019)	High-risk borrowers
Default	POS	31 to 34%	-10 to -14pp	-33 to -41%	THIS PAPER	
Default	Credit card	5%	0pp	0%	Bursztyn et al. (2019)	All borrowers
Default	Credit card	13%	-9 to -11pp	-69 to -85%	Bursztyn et al. (2019)	High-risk borrowers
Future delinquency	POS	37%*	-8pp	-22%	THIS PAPER	
Future delinquency	Small bus. loan	31%*	-7pp	-23%	Schoar (2012)	Personal contact: Low intensity
Future delinquency	Small bus. loan	31%*	-22pp	-71%	Schoar (2012)	Personal contact: High intensity
All Borrowers						
Pay any late fees	Credit card	29%	-3pp	-9%	Medina (2019)	Reminder: Push notification
Perfect repayment	Microloan	27%	+9pp	+32%	Cadena/Schoar (2011)	Cash-back incentive
Perfect repayment	Microloan	27%	+7pp	+26%	Cadena/Schoar (2011)	Future reduced-rate incentive
Perfect repayment	Microloan	27%	+9pp	+33%	Cadena/Schoar (2011)	Reminder: SMS
Default	Microloan	14%	-5 to -8pp	-36 to -57%	Karlan et al. (2015)	Reminder: Personalized SMS
Ever 90 days late	Mortgage	18%	-4pp	-20%	Moulton et al. (2015)	Coaching: All borrowers
Ever 90 days late	Mortgage	32%	-13pp	-41%	Moulton et al. (2015)	Coaching: Borr. with prior defaults

* The numbers reflect the average of the full sample, not of the control group.

Recent Issues

No. 427	Gül Demirtaş, Christian Strenger, Tobias Tröger	Sustainability Skills Disclosure for Boards: An Essential Prerequisite for Assessing Sustainability Competence
No. 426	Peter Andre, Ingar Haaland, Christopher Roth, Mirko Wiederholt, Johannes Wohlfart	Narratives about the Macroeconomy
No. 425	Leo Kaas, Georgi Kocharkov, Nicolas Syrichas	Understanding Spatial House Price Dynamics in a Housing Boom
No. 424	Matteo Bagnara	The Economic Value of Cross-Predictability: A Performance-Based Measure
No. 423	Tobias Berg, Florian Heider	Leverage and Risk-Taking in a Dynamic Model
No. 422	Julian Detemple	Thoughts about the Dictator and Trust Game
No. 421	Claes Bäckman, Patrick Moran, Peter van Santen	Mortgage Design, Repayment Schedules, and Household Borrowing
No. 420	Konstantin Bräuer, Andreas Hackethal, Tobin Hanspal, Samuel M. Hartzmark	Educating Investors about Dividends
No. 419	Andreas Hackethal, Philip Schnorpfel, Michael Weber	Inflation and Trading
No. 418	Ioannis Asimakopoulos, Tobias Tröger	Reform of the CMDI Framework – Driving Off with the Breaks On
No. 417	Thiago Fauvrelle, Max Riedel, Mathias Skrutkowski	Collateral Pledgeability and Asset Manager Portfolio Choices During Redemption Waves
No. 416	Olga Balakina, Claes Bäckman, Anastasiia Parakhoniak	Beyond Connectivity: Stock Market Participation in a Network
No. 415	Monica Billio, Aoife Fitzpatrick, Carmelo Latino, Loriana Pelizzon	Unpacking the ESG Ratings: Does One Size Fit All?
No. 414	Peter Andre, Teodora Boneva, Felix Chopra, Armin Falk	Misperceived Social Norms and Willingness to Act Against Climate Change
No. 413	Errikos Melissinos	Real Term Premia in Consumption-Based Models