Promoting Rule Compliance in Daily-Life: Evidence from a Randomized Field Experiment in the Public Libraries of Barcelona*

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September 2011

Abstract

We study how to promote compliance with rules in everyday situations. We have access to data on the users of all public libraries in Barcelona, where, in contrast to other studies, compliance with rules can be observed perfectly. In this setting, we test the effect of sending email messages with different contents. We find that users return their items earlier if asked to do so in a simple email, showing that a general reminder of the users' duty is effective in promoting rule compliance. Furthermore, adding other contents to the general reminder does not increase compliance significantly.

Keywords: Rule Compliance, Field Experiment, Public Libraries.

JEL classification numbers: C93, D01, D03, D63, K42.

*We are grateful to Anna Surroca (*Serveis de Biblioteques of the Diputacio de Barcelona*), Judit Terma (*Consorci de Biblioteques de Barcelona*), and their respective teams, for their collaboration and assistance in conducting this study. We thank Antonio Cabrales, Pablo Casas, Vincent P. Crawford, Rosemarie Nagel, Imran Rasul, Pedro Rey-Biel, Carmit Segal, Christian Traxler and seminar participants at Universidad de Alicante, University of East Anglia, University of Essex, University of California San Diego, Universidad Carlos III, University of Granada, University of Pittsburgh, Universitat Pompeu Fabra, University Rom Tor Vergata and Stockholm University for helpful comments. Special thanks go to Stephan Litschig for constant feedback and discussions. Sergio Correia provided excellent research assistance. Financial support from the Spanish Commission of Science and Technology (ECO2008-01768, ECO2009-12836, ECO2009-11213 and SEJ2007-64340), Fundación Rafael del Pino, the Barcelona GSE research network, and the Government of Catalonia is gratefully acknowledged.

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1 Introduction

Understanding compliance with rules is crucial for modern societies. No matter whether we talk about careful driving, littering on the streets, picking up children from the kindergarten on time, or appropriate behavior in public places like metros or libraries, learning about effective tools for promoting compliance with rules is of obvious importance. While economists would naturally think about monetary incentives, it has been found that they may backfire (see Benabou and Tirole, 2003 and 2006, for theoretical arguments; Gneezy and Rustichini, 2000a and 2000b, and Mellstroem and Johannesson, 2008, for empirical studies), or that they are not feasible due to political restrictions. Therefore, it is crucial to understand whether there are other possible ways to promote compliance with rules. The goal of this paper is to analyze the effect of conveying various types of messages, in our case by email. Our interest in the potential effects of sending messages is that it offers a virtually costless and non-invasive intervention mechanism that is simple to implement and very flexible for our, as well as for other applications. Surprisingly, despite the advantages of this message intervention, little is known about its effectiveness.

The setting that allows us to study compliance with rules on a large scale is the Network of Public Libraries in the city of Barcelona. Here, in contrast to other studies, individuals' compliance with rules can be observed perfectly. The type of compliant behavior we analyze is whether users of the libraries return the items they borrowed on time. A user not returning an item by the due date is violating the rule, and generating a negative externality on the population of users. We evaluate whether we can get users to return the items they borrow earlier, by means of different email contents that are randomly allocated.

Our study will be informative for the optimal design of message contents in any setting where compliance with rules is desired. The study of the behavior of library users contingent on different message contents will improve our understanding on the effectiveness of such a mechanism, and serve as a basis for the design of future message interventions in other settings of interest.

There are important characteristics that make our study unique. First, we observe the behavior of all users of all public libraries in Barcelona over eleven months. During this time span, there were about 50,000 different users, who borrowed over a million

items in the 32 different libraries spread throughout the city of Barcelona. Therefore, we have data on a large number of individuals, in a daily-life situation, taking part in their natural environment, and over an extended period of time. Second, we observe *every* borrowing-returning transaction of items made by users. This allows us to measure compliant behavior with exact precision. Third, the rules that govern the interaction between the users and the libraries are simple and well-defined. In particular, the penalty associated with returning an item late does not involve any monetary fines, but the exclusion from the possibility of borrowing more items for a time period equal to the number of days the item is overdue. Finally, the rich data on users offers a unique opportunity to test for differential treatment effects with regard to previous compliance and demographic variables such as gender and nationality.

We randomize all users into groups receiving one of five different email messages, and study their behavior after receiving the email. One of the five email messages is a CONTROL message that provides a link to the webpage of the Network of Libraries.[1] All the remaining messages add content to the text in CONTROL. The first treatment message, called REMINDER, represents a general reminder of the users' duty to return the borrowed items on time. The second message, SOCIAL, adds to REMINDER an appeal to the effect individual behavior can have on the overall functioning of the public library services. The last two email treatments, LATE and PENALTY, are targeted only at those users who have recently returned at least one item late. Both LATE and PENALTY add to REMINDER the identification of the user as having recently returned items late. Finally, PENALTY builds on LATE and adds a reminder of the penalty associated with non-compliant behavior.

In our analysis we evaluate the effect of emails on the proportion of late returned items by user, and on the number of days that elapse between the return date and the due date. The first variable measures the propensity to comply with the rule, while the second variable measures the positive/negative externality that is imposed on other users when a user returns the item earlier/later than the due date.

Our main result is that compliant behavior can be promoted by sending an email that includes a general reminder of the users' duty to comply with the rule. All four

[1]The idea is that by comparing the effect of the treatment messages relative to the control, we are able to differentiate between the effect of the content of a treatment message and that of just getting an email from the Network of Libraries.

treatment emails significantly reduce both, the proportion of late returned items, and the number of days between the return date and the due date. The treatments reduce the proportion of late returned items by up to 10 percent and the number of days between the return and the due date by up to almost one day. These effects are not only statistically significant but also economically relevant, especially in light of the negligible costs associated with the intervention. Furthermore, we cannot reject the hypothesis that all four different contents have the same effect, showing that the additional contents to the general reminder do not increase rule compliance significantly.

As for the effectiveness over time, we show that the effect of getting one of these emails is short-term; the effect is significant during the first month after the email intervention, but not afterwards. However, the effect is reproduced when the same email is received for a second time, in our case two and a half months later. As such, our results suggest that sending multiple emails can help to keep compliance high.

With regard to heterogeneous treatment effects, we find that users with a higher proportion of late returns in the pre-treatment period react more strongly than users with a lower proportion of late returns. Interestingly, even the "good citizens" react positively to receiving an email. Hence, the email treatment is more effective precisely with those users whose compliance prior to the treatment was lower, and, importantly, does not generate crowding-out effects in those users that were complying with the rule before the intervention.

A natural interpretation of our results is that individuals pay limited attention to the duty to return items on time, and an email reminding them about this duty increases rule compliance (see Karlan et al. (2011) for a model of limited attention, and empirical evidence supporting it). This interpretation is consistent with our main results. Adding extra messages to the reminder email does not significantly change behavior, and precisely those users who were complying the least are the ones that react the most. An alternative interpretation of our results is that users interpret the treatment emails as a signal that the libraries care about rule compliance (maybe to a greater extent than expected), and that this triggers a positive reaction on them. According to this interpretation, users react to their beliefs about what the authorities expect from them. Although the result that a simple reminder increases rule compliance has important implications for institutions and authorities, future research should be directed to disentangle among the two possible interpretations given above.

Last, we investigate differences in reactions according to user demographics. With respect to gender, we find no significant differences in reactions to the treatments between women and men - despite evidence on gender differences in other economically relevant situations (see Croson and Gneezy, 2009). As for users' nationalities, we study reactions of users from Spain, Northern-Central Europe, Western and Southern Europe, English speaking countries (UK, USA, CA), East Europe and Russia, Latin America and Asia. Consistent with Fisman and Miguel (2007), we find different reactions depending on the users' nationalities. Interestingly, only Spaniards, people from English speaking countries and Asians react to the emails. We then evaluate whether Asians or citizens of English speaking countries react differently to the treatments than Spaniards do. Here, we see that users from English speaking or Asian countries react much more strongly than Spaniards.

Our paper is one of the first to study the effect of messages on compliance with rules. It differs from a strand of literature that investigates how providing information affects various individual choices such as retirement decisions (Duflo and Saez, 2003), prescription drug insurance plans (Kling et al. 2011), or school choices (Hastings and Weinstein, 2008; Jensen, 2010). These settings study the effect of providing information on very complex goods and services, whose characteristics are most likely only partially known by the individuals. In contrast, we study a familiar and everyday scenario where there is a simple and well-defined rule, which is known by the users. Consequently, it is less clear whether information will have any effect in this situation.

Studying compliance behavior in public libraries, we add to a limited literature on messages and rule compliance in other settings. First, there is work that studies the effect of messages on norm compliance. Schultz et al. (2007) and Ayres, Raseman and Shih (2009) show that home electricity consumption can be reduced when households get periodic reports on the consumption of comparable neighbors. Karlan et al. (2011) show that when individuals are reminded of their previous saving commitments, the likelihood of reaching their saving goal increases. Huck and Rasul (2010) show that sending reminding letters in fundraising campaings is effective. Finally, Dal Bó and Dal Bó (2009) conducted a series of laboratory two-player public good experiments to study the influence on individual contribution levels when players receive a message appealing to moral rules. The main difference between these studies and ours is that in our case there is a clear rule that dictates what to do, namely to return the items on

time, as opposed to some unwritten and informal norm or self-imposed norm. Second, two related papers analyze the effect of letters on individual compliance with rules; Pomeranz (2010) analyzes firms' tax compliance in Chile and Fellner, Sausgruber, and Traxler (2011) analyze citizens' subscriptions to TV licenses in Austria. In contrast to these studies, our setting includes a formal rule with relatively small punishment. Additionally, we can perfectly measure compliance with rules and study the effect of messages on different users on the basis of their previous compliance. This is important because there is evidence on crowding out effects of different interventions on individual behavior (see Frey and Jegen, 2001). Our study may therefore be informative for the optimal targeting of subjects, when the policy maker has detailed information about the compliance history of individuals.

The remainder of this paper is organized as follows. Section 2 describes the setting, namely the Network of Public Libraries in the city of Barcelona, and explains the design of the field experiment, as well as the identification strategy. Section 3 is devoted to the presentation and discussion of the results. Finally, Section 4 concludes.

2 The Field Experiment

2.1 The Setting: Network of Public Libraries of Barcelona

The Network of Public Libraries in the city of Barcelona is managed by a central body dependent on the City Hall of Barcelona and the Government of the Province of Barcelona. It encompasses 32 libraries spread throughout the city of Barcelona. Each library offers the possibility of borrowing items such as books, DVDs, CDs and magazines; other services such as internet access and exhibitions are also provided.

The rules governing the borrowing of different item types are clearly defined and are the same for all the 32 libraries. At the time of our study, a book could be borrowed for 21 days, while all other item types (DVDs, CDs and magazines) could be borrowed for 7 days. Users could also ask to extend the due date if no other user required that item. As for the maximum number of items to be taken, each user could simultaneously take a total of 30 items, 15 books and magazines, and 15 CDs and DVDs. The penalty associated with returning an item late involved being barred from borrowing new items for a time period equivalent to the number of days elapsed between the due date and

the actual return day. In particular, there was no monetary fine associated with not complying with the return policy.

2.2 Data and Email Contents

We observed the complete borrowing/returning behavior for every single user from January 2009 until the beginning of November 2009. For every transaction we observed (i) the user code, gender, age, and nationality, (ii) the item code and its characteristics, that is, whether it was a book, DVD, CD, or magazine, (iii) the dates of the transaction, that is, the date when the item was borrowed and returned, and (iv) the library where the transaction took place. With this information we were able to follow the exact borrowing behavior of every single user of the Network of Public Libraries in Barcelona. Given that our design is based on emails, we concentrate on the sample of those users with a known email.[2] This gives us about 50,000 different users, who borrowed over a million items.

The Network of Public Libraries in the city of Barcelona maintains constant communication with its users via email. Most emails include information on the activities organized in the different libraries of the city, such as exhibitions, and on opening hours. In collaboration with the Network, we designed five different email messages (see Table 1) that were randomly assigned to the users.

[Table 1 here]

Control refers to the control treatment. It provides a link to the webpage of the Network of Public Libraries. The rest of the treatment messages build on Control, adding different pieces of information. Reminder represents a general reminder of users' duty to return items on time. Social builds on Reminder, adding an appeal to the influence of individual behavior on the proper overall functioning of the public system of libraries. Email Late adds to the content of Reminder a statement that identifies the user as having recently returned an item late. Finally, Penalty builds on Late adding a reminder of the actual penalty associated with returning an item late.

In our analysis, we will compare the effect of receiving a Reminder, Social, Late or Penalty email, with that of receiving Control. That is, we will study whether

[2]The Network of Public Libraries knows the email addresses of about 40% of the registered users.

any of the four treatments improves with respect to a CONTROL message. Furthermore, the contents of the emails potentially allow us to distinguish between different motives for behavioral changes. For example, the difference in the texts between REMINDER and SOCIAL allows us to evaluate whether appealing to the importance of one's contribution to the good functioning of a public service, such as the libraries, is more effective than a generic reminder. If so, weak appeals to social preferences would have an effect on behavior (see Sobel, 2005). The final two emails were specifically designed to target users with late returns in the recent past. Comparing LATE with REMINDER is useful to test whether being identified as non-compliant with the rule has a different effect than the generic reminder. The idea was to see, whether being identified as a late user affects behavior, for example through triggering feelings of guilt or shame (Battigalli and Dufwenberg, 2007). Finally, PENALTY allows us to test for any differential effect of recalling the penalties associated with the violation of the rule. Any effect would be in line with the classical deterrence hypothesis (Becker, 1968).

It was our aim to design emails with general contents that could be applied to many settings of interest beside libraries. For this reason, no email makes any reference to particular items that may have been borrowed at the time of receiving the email. Also, we kept in mind that not all settings permit the type of precise data on individual behavior that we had at the moment of treatment (e.g., identifying users as late and non-late). In this vein, three of our emails, CONTROL, REMINDER and SOCIAL, are general in the sense of not using any information on the behavior of users prior to the treatment, and can therefore easily be adapted to other settings (e.g. driving alerts, voting, donating blood, or referee reports). On top of that, for cases where information on individual behavior is available to the policy maker, it is important to analyze potential effects of using such specific information. In our case, LATE and PENALTY use information on user history in order to directly target non-compliant individuals.

2.3 Randomization

We sent emails in two different waves. Wave 1 was sent on July 1st, 2009, when we reached about 36,700 users. Wave 2 was sent on September 15th, 2009, when we

reached about 38,300 users. Overall, we reached about 50,000 *different* users.[3]

In Wave 1 (resp. Wave 2) we considered all the active users between January 1st and May 5th, 2009 (resp. between March 1st and July 31st, 2009), and classified them into two categories: late users and non-late users. An active user is a user who borrowed at least one item during the time interval mentioned. A late user is a user who returned an item after the due date at least once during the time interval. A non-late user is a user who did not return any item late during the time interval. Late users were randomly assigned to the five different treatments, while non-late users were randomly assigned to Control, Reminder and Social only.[4] The randomization was carried out at the user level and in order to ensure balance across different libraries, we stratified the randomization using the library at which users signed up.

Note that in Wave 2 we have users who were already active in Wave 1 and new active users, namely those users who were active only between May and July. With regard to the new active users, we repeated the randomization procedure as in Wave 1. The active users in Wave 2 who were also active in Wave 1 received exactly the same email as in Wave 1.[5] Exceptions were those users who were allocated to Late or Penalty in Wave 1 but who, during the interval between March 1st 2009 and July 31st 2009, were never late again. There were about 700 of such users, who were excluded from the randomization, and hence received no email in Wave 2.

Table 2 reports the descriptive statistics of all users, both non-late and late, who were randomly assigned to treatments Control, Reminder and Social in Waves 1 (top) and 2 (bottom). Table 3 reports the descriptive statistics of late users only, randomly assigned to the five treatments in Waves 1 (top) and 2 (bottom). Note that late users in Control, Reminder and Social appear in both Tables 2 and 3. The last column in Tables 2 and 3 report the p-values for the F-Test of equality of variable means across all groups.

[Tables 2 and 3 here]

[3]In each wave we sent about 50,000 emails but not all emails were actually delivered. About 30% of email addresses turned out to be invalid and the email messages were returned to the server as messages that were never delivered. We therefore restrict our analysis to those users, to whom the message was delivered.

[4]There were 21,571 late users and 15,106 non-late users in Wave 1.

[5]There were 10,492 new active users in Wave 2, of which 5,191 were late uses and 5,301 were non-late users. There were about 28,500 users who were also active in Wave 1.

Consistent with the random assignment of users to treatments, the average user has similar values in the observable characteristics across the different treatments.[6]

In Tables 2 and 3, we can see the magnitude of the problem of late returns. Considering those users who have been late at least once, around 60% of the loans per user are returned after the due date. Furthermore, the typical late user returns the borrowed items on average 6.5 days later than the due date.

In our analysis, as is standard practice in any randomized field experiment, we concentrate on the post-treatment period, that is, on the behavior of users after the email intervention. For those users who received the email message in Wave 1, the post-treatment starts on the 1st of July. For those users who got the email for the first time in Wave 2, the post-treatment starts on the 15th of September. However, not all users who received the email treatment appear in the post-treatment period, that is, some users do not borrow any item at any time in the post-treatment period, so that we cannot observe their compliant or non-compliant behavior. One important issue that needs to be addressed is whether the randomization is still valid when we look at those users whose behavior can actually be observed during the post-treatment period. In particular, we would like to know whether attrition rates between pre and post-treatment periods are significantly different across the control and treatment groups. To address this issue, we define, separately for Waves 1 and 2, the attrition rates, that is, the share of users who received the email treatment but who did not borrow any item in the post-treatment period.

[Table 4]

On average, between 51 and 54% (see column (1) in Table 4), and 46 and 47% (see column (4) in Table 4), of emailed users did not borrow any item in the post-treatment period. However, that is unlikely due to the email intervention, but rather reflects natural fluctuations in borrowing rates over time.[7] More importantly, the null of equal attrition rates among control and treatment groups is not rejected at standard levels of

[6] An exception is the proportion of foreigners, possibly due to the valid email address correction (see footnote 3). However, the mean values do not show sizable differences.

[7] Indeed, computing the comparable attrition rates for those users with unknown email, we get an average of 50%. This shows that the attrition rates we observe after treatment are a rule rather than an exception.

significance, regardless of whether we use individual or joint tests, as shown in columns (2) and (5) of Table 4. When we add library fixed effects (shown in columns (3) and (6)), the coefficients of the treatments are not significant at the 5% level, although they come out jointly significant because library fixed effects are individually significant.[8] To summarize, this analysis prevents possible concerns about attrition being a handicap for the interpretation of our results.

2.4 Identification Strategy

We will focus on two different dependent variables. First, we look at the proportion of late returned items per user (*Proportion Late*). This is a direct measure of how users comply with the rule. Second, we use the average number of days between the return date and the due date per user (*"Actual−Due" Date*). When this difference is positive (resp. negative) the item was returned late (resp. early) compared to the due date. In contrast to the first dependent variable, which measures late/non-late per item in a binary way, this second variable also takes into account the extent of late or early returns.

In a randomized experiment like ours, the causal effect of the treatments can be estimated as follows:

$$Y_i = \alpha + \beta_1 Reminder_i + \beta_2 Social_i + \beta_3\ Late_i + \beta_4 Penalty_i + \epsilon_i \qquad (1)$$

where the dependent variable Y_i is either (i) the proportion of late returns per user, or (ii) the average number of days between the return date and the due date per user.[9] *Reminder*, *Social*, *Late*, and *Penalty* are dummy variables taking a value of 1 when user i was assigned to Reminder, or Social, or Late, or Penalty, respectively. The omitted treatment to which these variables are compared is Control.

Consistent with our design, we will estimate equation (1) in two different ways. First, we compare Reminder and Social to Control for all users, independent

[8]We also redid Tables 2 and 3 for those users who were treated and did borrow items in the post-treatment period (available upon request). We obtained the same results, qualitatively speaking, showing that the control and treated groups are comparable in all the observable characteristics.

[9]Note that the dependent variables are obtained by collapsing all the transactions at the user level. For example, for a user with 5 transactions that was late with 4 of them has a proportion of late returns of 4/5. In the subsequent analysis, when we add control variables, we also collapse them at the user level.

of whether they were late or not in the pre-treatment period. Second, we compare Reminder, Social, Late and Penalty to the Control restricted to all users who were late at least once in the pre-treatment period.

3 Results

3.1 Average Treatment Effects

We estimate equation (1) by OLS. Table 5 reports the results for Control, Reminder, and Social, covering all users, both late and non-late users, who got one of these emails in Waves 1 and 2. Table 5 reports the results for all five treatments restricted to the late users only.[10]

[Tables 5 and 6 here]

The first three columns in Tables 5 and 6 refer to the proportion of late returns per user, while the last three columns refer to the average number of days between the return date and due date per user. In both cases the first column reports the results of estimating equation (1) without any controls. The second column controls for users' demographics, month fixed effects and the number of borrowed items. We also add controls for users' behavior prior to the treatment (propensity for late returns and the average number of days between the return date and the due date per user prior to the treatment). The third column adds additionally dummies for the item type (whether it refers to a book, DVD, CD or a magazine) and library fixed effects.[11]

In Table 5, both Reminder and Social are significant and negative, showing that both email treatments significantly reduce the proportion of late returns and the number of days between the return date and the due date. Taking the estimates of the third column, receiving a Reminder email decreases the proportion of late returns by 1.4 percentage points (compared to Control), and 1.8 percentage points in the case of Social. Evaluated at the mean propensity of being late for the control

[10]The results in both Tables 5 and 6 are highly robust with regard to other specifications. If instead of collapsing the data at the user level, we estimate random effects with transaction level data, the results we obtain are both qualitatively and quantitatively similar (available upon request).

[11]In all specifications, we discard transactions that were due on a holiday, when the library was closed.

group (approximately 36 percent), the reduction in late returns lies between 4 percent (Reminder) and 5 percent (Social). Moreover, receiving a treatment email also significantly decreases the number of days between the return date and the due date: the Reminder and Social emails decrease this difference on average by almost half a day with respect to Control. Note also that the coefficients of the Reminder and the Social emails are not statistically different from each other, meaning that the appeal to social preferences, through one's contribution to the functioning of public libraries, did not affect users' behavior differently from the general reminder.

Focusing on previously late users (Table 6), we see that all four email treatments are negative and significant; both for the proportion of late returns per user and for the average number of days between the return date and the due date per user. For instance, from column (3) we see that the treatment effects (compared to the control) range from -2.4 percentage points for the Reminder to -4.3 percentage points for the Penalty. As for the number of days between the return date and the due date, the reduction lies between 0.54 and 0.87 days. Evaluated at the means of the control group, the treatment effects correspond to a reduction in the proportion of late returns up to 10 percent, and a reduction of over 100 percent for the number of days between the return and the due date. Concerning differences in the messages' effectiveness, we cannot reject the hypothesis that all four email contents affect users' behavior in an equal manner. That is, a general reminder of the users' duty to comply with the rule is enough to promote rule compliance, and the additional contents of the other email messages, using appeals to one's contribution on the functioning of public libraries or identifying users as having been late, do not additionally affect behavior.

One may wonder whether the effect mainly comes from the proportion of items that are pending at the time of receiving one of the email treatments or whether it is also the case that rule compliance improves more generally. This is important to understand when we think of the applicability to other settings. In order to address this question, we first create a variable, called *Pending*, which calculates the proportion of pending items per user at the time of receiving an email. Then, we interact the treatment dummies with the proportion of pending items at the user level. Table 7 reports the results.

[Table 7 here]

As can be seen, the interaction terms are insignificant for the proportion of late returns (columns (1) and (3)). Therefore, the effects found in Tables 5 and 6 came not only from the proportion of loans that were pending at the time of the email intervention; instead, the treatments affected all users' behavior, whether items were pending or not. On the other hand, for the average number of days between the return date and the due date (columns (2) and (4)), the interaction terms are negative and significant, implying that users with a larger proportion of pending items return their items earlier than users with a lower proportion of pending items.

3.2 Duration of the Treatment Effect

Having shown that receiving an email has a significant effect on behavior, we now address the question related to the duration of the effect. This is important to fully evaluate the impact of such an intervention. To this end, we partition the post-treatment period into four different time windows: (i) July 1-July 31: the effect in the first month following the first wave of emails, (ii) August 1-September 14: the time interval between a month after the first wave of emails and the beginning of the second wave, (iii) September 15-October 15: the effect one month after the second wave of emails, and (iv) after October 15.

Table 8 reports the estimates for equation (1) separately for the four time windows. The first page of Table 8 refers to treatments Control, Reminder, and Social, covering all users, while the second page of Table 8 reports the results for all five treatments (restricted to previously late users only).

[Table 8 here]

The tables show that the effect of getting an email is short term, but it is replicated after getting a second email. No matter whether we use the proportion of late returns per user as a dependent variable, or the average number of days between the return date and the due date, the effect lasts for one month. The first emails that were sent on July 1 had an effect in the period July 1-July 31, but the effect becomes insignificant in the period August 1-September 15. The same pattern can be observed for the emails that were sent on September 15. For most email messages (see the second page of Table 8), we can reject the null that treatments are the same in the first and the second time

window. However, with one exception (PENALTY and the proportion of late as the dependent variable), the treatment effects are the same for the first and the third time window. Therefore, users who stopped reacting to the first email react again upon reception of the second message, in a comparable manner.

3.3 Heterogenous Treatment Effects by User Characteristics

After estimating the average treatment effect of sending different emails, we now proceed to the analysis of heterogeneous reactions depending on relevant user-specific characteristics. We start testing for differential treatment effects that depend on users' previous behavior in terms of late returns/days between return and due date. Afterwards, we study whether there are significant differences in behavior depending on gender and nationality.

3.3.1 Previous Compliance with the Rule

It is conceivable that the reaction to the different treatments is related to the users' compliance history, that is, their behavior prior to the treatment. To test for this, we interact the treatment variables with *Prior Late* and *Prior "Actual−Due"*. *Prior Late* and *Prior "Actual−Due"* refer to the average user-specific proportion of late returns/days between return and due date in the pre-treatment period.

[Table 9 here]

Table 9 reports the results. We observe that the interaction terms are mostly negative and significant, suggesting that the less rule-compliant users were in the pre-treatment, the stronger is their reaction to the treatments. When we compare the treatments to CONTROL according to the proportion of late returns (columns (1) and (3)), we see that the REMINDER treatment has a stronger effect on those users who had a higher proportion of late returns prior to the treatment. When the treatment variables are interacted with the user-specific *Prior "Actual−Due"* (columns (2) and (4)), we see that the previous non-compliers have a stronger reaction to the REMINDER message, but also to the LATE and PENALTY messages.

To summarize, the email treatments are especially effective in changing the behavior of a very relevant sample of users, namely those breaking the rule more often. Also, it

is important to see that there are no crowding out effects. For users who have a value of *Prior Late* and *Prior "Actual−Due"* equal to 0, the estimated treatment effects are still negative (some of them significant), suggesting a positive effect on the "good types" as well.

3.3.2 Gender

Whether and why gender matters has increasingly attracted economists' attention. In a recent comprehensive survey, Croson and Gneezy (2009) find that women and men differ along some dimensions (e.g. competitiveness), but not necessarily in others (social preferences revealed in lab experiments). Our data offer an opportunity to measure gender differences in rule compliance in daily life, and also the reaction to different email treatments. For the subsequent analysis, we construct interaction variables between our gender variable, $Male$, and the treatment dummy variables.

[Table 10 here]

Table 10 reports the results. As for the $Male$ dummy, it is statistically insignificant, meaning that in the control group, women and men show similar patterns in compliant behavior.[12] As for the estimated interaction terms, the coefficients are (with one exception) insignificant, indicating that there are no gender differences in the reaction to the emails. In other words, both women and men are highly comparable when it comes to rule compliance and the reaction to messages aimed at promoting rule compliance.

3.3.3 Nationality

There is sound evidence that nationality is an important determinant of behavior in a variety of settings. Fisman and Miguel (2007), for example, show interesting nationality differences in the determinants of corruption. Our database allows us to distinguish between the users' countries of origin. Hence, we can evaluate whether the behavior of users differs by nationality, and whether there are differential reactions to receiving an email.

We classify users into 8 geographical areas according to their nationality: (i) Spain, (ii) Northern and Central Europe (Germany, Belgium, Denmark, Finland, Netherlands,

[12] We find the same result if we do not include previous compliance as a control.

Norway, Sweden, Switzerland, Austria), (iii) Southern and Western Europe (France, Italy, Greece, Portugal), (iv) English speaking countries (UK, US, Canada, Ireland, and Australia), (v) Eastern Europe and Russia (Bulgaria, Croatia, Slovakia, Estonia, Hungary, Lithuania, Poland, Rumania, Russia, Czech Republic, Ukraine, Georgia, Armenia), (vi) Latin America (Argentina, Bolivia, Brazil, Colombia, Cuba, Dominican Republic, Ecuador, Guatemala, Honduras, Mexico, Nicaragua, Paraguay, Peru, El Salvador, Uruguay, Venezuela, Chile, Costa Rica, Panama), and (vii) Asia (Philippines, Japan, Nepal, China, India, South Korea). Spain accounts for the vast majority of users (around 70%), followed by Latin America with 19%, Southern and Western Europe with 6%, and at the bottom of the distribution is Asia with 1%.

We first analyze whether foreign users differ in their proportion of late returns with respect to Spaniards. Table A.1 in the appendix reports the average user specific propensity for being late in the pre and post-treatment period by nationality groups (columns (1) and (2), respectively), where the omitted variable is Spaniards. It is clear that there are significant differences. The proportion of late returns is significantly higher for users from Latin America, Southern and Western Europe, and from the English speaking countries. On the other hand, Asian users seem to show a lower propensity for being late than Spaniards.

An interesting question is whether there are differential treatment effects that are related to different nationalities. Table 11 reports the results for the seven nationality groups separately. The first page of table 11 refers to the treatments Control, Reminder, and Social, covering all users, while the second page of Table 11 reports the results for all five treatments (restricted to previously late users only).

[Table 11]

As can be seen therefrom, there are remarkable differences. First, users from English speaking countries react significantly to every single treatment. They reduce the proportion of late returns by up to 30 percentage points and reduce the average number of days between the return date and the due date by up to 8.5 days. Previously late users from Asia also react significantly, in particular to the treatments Reminder, Late and Penalty. With the exception of Spain, we do not find consistent and significant effects for the other nationality groups.

We now directly compare the effects found for Spaniards with the effects found for English speaking and Asian countries, controlling for different initial propensities of being late, as well as different reactions depending on prior propensity to be late. Given that different nationality groups show very different proportion of late returns per user, as well as a different average number of days between the return date and the due date per user, one concern might be that some nationalities react more strongly not because of the nationality but because they had a very different compliant behavior to begin with. To deal with such concern and to test for the robustness of the results, we have replicated the analysis in Tables A.2 and A.3, including interactions between prior behavior and the treatment. As can be seen from Table A.2, users coming from English speaking countries react significantly more than Spaniards, for both measures of the dependent variable. For Asian users, as shown in Table A.3, we also see that the effect is significantly higher than for Spaniards, but only for the late users and treatments Reminder and Penalty. As such, users from English speaking countries and Asian users react strongly, despite having very different initial levels of compliance. Finally, we did a similar exercise for the other nationality groups, but we did not find significant results (all results are available upon request).

4 Conclusions

In this paper we study the effect of a very simple, versatile, and virtually costless mechanism, such as sending email messages, on promoting compliance with rules. The study was conducted in the Public Libraries of Barcelona, where compliance with rules means returning items on time. What makes our setting unique is that we observe a large number of users in a daily-life situation, where rules are simple and well-defined, and where compliance is perfectly measurable.

Using the methodology of a randomized field experiment, we show that sending email messages helps to promote compliance with rules. A general reminder of the users' duty to comply with the rule is effective in promoting rule compliance. Furthermore, adding other contents to the general reminder, appealing to one's contribution on the functioning of public libraries or identifying users as having been late in the past, do not bring a significant additional increase in rule compliance. Also, the effect seems to be short term but easily replicable when a second wave of emails is sent.

Finally, we also find differential treatment effects depending on users' characteristics. First, the email messages affect all users, although they are specially effective on those users who have shown a worse compliance with the rule in the past. Second, we find no differential treatment effect by gender. And third, we do find differential treatment effects depending on nationalities. In particular, users from English speaking countries react more strongly to the email treatments than Spaniards.

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Table 1—Email Messages

E-mail	Text
Control	Dear User, In the next webpage you will find information on the services and activities offered by the Libraries of Barcelona: http://www.bcn.es/biblioteques/ Best wishes, Libraries of Barcelona
General ***Reminder***	Dear User, **If at some point you borrow an item from the library, please remember that you have to return it on time.** Best wishes, Libraries of Barcelona In the next webpage you will find information on the services and activities offered by the Libraries of Barcelona: http://www.bcn.es/biblioteques/
Social *Motivation*	Dear User, **For a good functioning of the Public Libraries it is important to return the items that are borrowed on time.** If at some point you borrow an item from the library, please remember that you have to return it on time. Best wishes, Libraries of Barcelona In the next webpage you will find information on the services and activities offered by the Libraries of Barcelona: http://www.bcn.es/biblioteques/
Identification ***Late***	Dear User, **In the last months you have returned an item late.** If at some point you borrow an item from the library, please remember that you have to return it on time. Best wishes, Libraries of Barcelona In the next webpage you will find information on the services and activities offered by the Libraries of Barcelona: http://www.bcn.es/biblioteques/
Identification Late and Reminder of the ***Penalty***	Dear User, In the last months you have returned an item late. If at some point you borrow an item from the library, please remember that you have to return it on time. **Remember that the time that a user will be excluded from the possibility of borrowing an item will be the same number of natural days elapsed since the day that the item should have been returned. The maximum period for exclusion is one year.** Best wishes, Libraries of Barcelona In the next webpage you will find information on the services and activities offered by the Libraries of Barcelona: http://www.bcn.es/biblioteques/

Notes: The text in bold refers to the new addition of the treatment email. The words in bold in the first column represent the labels we will use in the paper.

TABLE 2
User Randomization into Treatments CONTROL-REMINDER-SOCIAL

	CONTROL			REMINDER			SOCIAL			*P*-Value
	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Equ. Means
Wave 1 (Active between 1.January-5.May)										
Male	9438	**0.42**	0.49	9059	**0.42**	0.49	9423	**0.42**	0.49	0.67
Age	9448	**32.71**	13.83	9062	**32.76**	13.89	9434	**33.07**	13.78	0.16
Foreign	9467	**0.28**	0.45	9080	**0.30**	0.46	9452	**0.30**	0.46	0.07
Proportion Late	9467	**0.33**	0.39	9080	**0.33**	0.39	9452	**0.33**	0.39	0.94
"Actual - Due" Date	9376	**1.74**	16.75	8995	**1.53**	16.37	9349	**1.31**	15.86	0.19
Nr. Loans Total	9467	**31.51**	53.46	9080	**31.65**	52.58	9452	**32.73**	58.33	0.25
Nr. Loans 2009 - Half 1	9467	**11.92**	18.80	9080	**11.89**	19.01	9452	**12.48**	22.21	0.08
Book	9467	**0.60**	0.42	9080	**0.60**	0.42	9452	**0.61**	0.42	0.30
CD	9467	**0.09**	0.23	9080	**0.10**	0.23	9452	**0.09**	0.23	0.49
DVD	9467	**0.28**	0.37	9080	**0.28**	0.37	9452	**0.27**	0.36	0.32
Magazine	9467	**0.03**	0.13	9080	**0.02**	0.12	9452	**0.02**	0.11	0.12
Wave 2 (Active between 1.March-31.July)										
Male	10037	**0.42**	0.49	9758	**0.41**	0.49	10151	**0.41**	0.49	0.81
Age	10049	**32.74**	14.09	9763	**32.49**	13.98	10157	**32.79**	13.73	0.28
Foreign	10064	**0.28**	0.45	9782	**0.29**	0.45	10180	**0.30**	0.46	0.01
Proportion Late	10063	**0.35**	0.39	9782	**0.36**	0.39	10180	**0.35**	0.39	0.92
"Actual - Due" Date	9923	**1.58**	14.69	9639	**1.54**	14.82	10047	**1.48**	14.33	0.89
Nr. Loans Total	10064	**30.28**	52.25	9782	**30.12**	51.25	10180	**31.40**	56.98	0.18
Nr. Loans 2009 - Half 1	10064	**11.18**	18.56	9782	**11.01**	18.67	10180	**11.67**	21.83	0.05
Book	10064	**0.62**	0.41	9782	**0.62**	0.41	10180	**0.61**	0.41	0.49
CD	10064	**0.09**	0.22	9782	**0.09**	0.22	10180	**0.09**	0.22	0.47
DVD	10064	**0.27**	0.36	9782	**0.27**	0.36	10180	**0.27**	0.36	0.31
Magazine	10064	**0.03**	0.13	9782	**0.03**	0.13	10180	**0.03**	0.12	0.23

Notes: All variables refer to all users, late and non-late, who were active in windows 1 (1 January-15 May) and 2 (1 March-31 July). All variables are obtained at the user level. Male takes a value of 1 in case of male, Age shows the user's age in years, and Foreign is a dummy variable taking a value of 1 in the case of Non-Spanish. Proportion Late measures the proportion of late returns per user, and "Actual - Due" Date measures the average number of days between the return date and the deadline per user. Number of Loans represents the number of loans per user. Book, CD, DVD and Magazine reflects the user's average share of Books, CD's, DVD's and Magazines. The *P*-Value in the last column is for the F-Test of equality of variable means across all three groups.

TABLE 3

User Randomization into Treatments CRONTOL-REMINDER-SOCIAL-LATE-PENALTY

	CONTROL			REMINDER			SOCIAL			LATE			PENALTY			*P*-Value
	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Obs.	**Mean**	Std. Dev.	Equ. Means
Wave 1 (Active between 1.January-5.May)																
Male	4315	**0.43**	0.49	4182	**0.43**	0.50	4351	**0.43**	0.50	4333	**0.43**	0.50	4304	**0.42**	0.49	0.97
Age	4321	**32.20**	12.78	4187	**32.30**	12.83	4355	**32.48**	12.55	4343	**32.41**	12.76	4312	**32.23**	12.58	0.82
Foreign	4331	**0.33**	0.47	4195	**0.35**	0.48	4367	**0.35**	0.48	4355	**0.34**	0.48	4323	**0.35**	0.48	0.08
Proportion Late	4331	**0.59**	0.33	4195	**0.58**	0.33	4367	**0.59**	0.33	4355	**0.58**	0.33	4323	**0.59**	0.33	0.22
"Actual - Due" Date	4270	**6.61**	20.79	4143	**5.94**	20.42	4301	**5.68**	19.37	4288	**5.91**	19.08	4269	**5.75**	18.22	0.20
Nr. Loans Total	4331	**46.15**	67.88	4195	**47.52**	67.61	4367	**48.54**	76.08	4355	**47.34**	74.48	4323	**48.00**	74.16	0.62
Nr. Loans 2009 - Half 1	4331	**17.48**	24.19	4195	**17.91**	24.63	4367	**18.50**	29.57	4355	**17.88**	25.38	4323	**18.35**	27.10	0.39
Book	4331	**0.50**	0.39	4195	**0.49**	0.39	4367	**0.50**	0.39	4355	**0.49**	0.39	4323	**0.48**	0.39	0.20
CD	4331	**0.12**	0.24	4195	**0.12**	0.24	4367	**0.12**	0.24	4355	**0.13**	0.24	4323	**0.12**	0.24	0.77
DVD	4331	**0.35**	0.36	4195	**0.36**	0.37	4367	**0.35**	0.36	4355	**0.36**	0.36	4323	**0.36**	0.36	0.48
Magazine	4331	**0.03**	0.12	4195	**0.03**	0.13	4367	**0.03**	0.12	4355	**0.03**	0.12	4323	**0.03**	0.12	0.69
Wave 2 (Active between 1.March-31.July)																
Male	4069	**0.43**	0.49	4014	**0.43**	0.49	4178	**0.42**	0.49	4158	**0.42**	0.49	4060	**0.42**	0.49	0.94
Age	4078	**32.18**	12.85	4019	**31.82**	12.74	4180	**32.12**	12.43	4166	**32.31**	12.81	4066	**31.78**	12.46	0.25
Foreign	4086	**0.33**	0.47	4029	**0.34**	0.47	4186	**0.36**	0.48	4178	**0.35**	0.48	4077	**0.37**	0.48	0.01
Proportion Late	4086	**0.62**	0.32	4029	**0.62**	0.32	4186	**0.61**	0.33	4178	**0.61**	0.32	4077	**0.61**	0.33	0.17
"Actual - Due" Date	3989	**6.55**	17.96	3940	**6.50**	18.20	4108	**6.02**	16.93	4067	**6.27**	17.70	3996	**6.23**	16.97	0.65
Nr. Loans Total	4086	**46.52**	68.75	4029	**46.92**	68.29	4186	**48.43**	76.93	4178	**46.78**	74.60	4077	**47.71**	74.96	0.75
Nr. Loans 2009 - Half 1	4086	**17.51**	24.55	4029	**17.67**	25.10	4186	**18.54**	30.35	4178	**17.77**	26.05	4077	**18.38**	27.46	0.32
Books	4086	**0.50**	0.39	4029	**0.49**	0.39	4186	**0.49**	0.39	4178	**0.49**	0.39	4077	**0.48**	0.39	0.63
CDs	4086	**0.12**	0.24	4029	**0.11**	0.23	4186	**0.12**	0.24	4178	**0.12**	0.24	4077	**0.12**	0.24	0.36
DVDs	4086	**0.35**	0.36	4029	**0.36**	0.37	4186	**0.36**	0.36	4178	**0.36**	0.36	4077	**0.36**	0.36	0.74
Magazines	4086	**0.03**	0.13	4029	**0.04**	0.14	4186	**0.03**	0.12	4178	**0.03**	0.12	4077	**0.03**	0.13	0.06

Notes: All variables refer to the late users who were active in windows 1 (1 January-15 May) and 2 (1 March-31 July). All variables are obtained at the user level. Male takes a value of 1 in case of male, age shows the user's age in years, and Foreign is a dummy variable taking a value of 1 in the case of Non-Spanish. Proportion Late measures the proportion of late returns per user, and "Actual - Due" Date measures the average number of days between the return date and the deadline per user. Number of Loans represents the number of loans per user. Book, CD, DVD and Magazine reflects the user's average share of Books, CD's, DVD's and Magazines. The *P*-Value in the last column is for the F-Test of equality of variable means across all five groups.

TABLE 4
Attrition

Wave 1 (Active between 1.January-5.May)

	Control-Reminder-Social			Control-Rem.-Social-Late-Penalty		
	(1)	(2)	(3)	(4)	(5)	(6)
Reminder		-0.0115	-0.0133*		-0.0101	-0.0117
		(0.00732)	(0.00731)		(0.0108)	(0.0108)
Social		-0.00189	-0.00272		0.00794	0.00716
		(0.00725)	(0.00723)		(0.0107)	(0.0107)
Late					-0.0155	-0.0168
					(0.0107)	(0.0107)
Penalty					-0.0109	-0.0114
					(0.0107)	(0.0107)
Constant	0.539***	0.544***	0.687***	0.475***	0.481***	0.646***
	(0.00297)	(0.00512)	(0.0185)	(0.00340)	(0.00759)	(0.0214)
Library FE	No	No	Yes	No	No	Yes
Observations	27999	27999	27999	21571	21571	21571
R-squared	0.0000	0.0000	0.006	0.0000	0.0000	0.007
Ho: all coefficients=0 (*p*-values)		0.2446	0.0000		0.1776	0.0000

Wave 2 (Active between 1.March-31.July)

	Control-Reminder-Social			Control-Rem.-Social-Late-Penalty		
	(1)	(2)	(3)	(4)	(5)	(6)
Reminder		-0.00349	-0.00536		0.00275	0.00188
		(0.00709)	(0.00708)		(0.0111)	(0.0110)
Social		0.00879	0.00820		0.0208*	0.0213*
		(0.00702)	(0.00701)		(0.0110)	(0.0109)
Late					-0.00314	-0.00215
					(0.0110)	(0.0109)
Penalty					-0.00120	-0.00126
					(0.0110)	(0.0110)
Constant	0.515***	0.514***	0.744***	0.461***	0.458***	0.669***
	(0.00288)	(0.00498)	(0.0246)	(0.00347)	(0.00780)	(0.0288)
Library FE	No	No	Yes	No	No	Yes
Observations	30032	30032	30032	20556	20556	20556
R-squared	0.000	0.000	0.007	0.000	0.000	0.008
Ho: all coefficients=0 (*p*-values)		0.2001	0.0000		0.1736	0.0000

Notes: The dependent variable is a dummy that takes value 1 if the user did not borrow any item in the post-treatment period and 0 otherwise. The top panel refers to Wave 1 and the bottom pannel refers to Wave 2. Columns (1), (2) and (3) refer to all users, while columns (4), (5) and (6) refer to late users only. Standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 5
CONTROL-REMINDER-SOCIAL

	Proportion Late			"Actual - Due" Date		
	(1)	(2)	(3)	(4)	(5)	(6)
Reminder	**-0.0129***	**-0.0123***	**-0.0138***	**-0.387***	**-0.395****	**-0.468****
	(0.00777)	**(0.00719)**	**(0.00715)**	**(0.203)**	**(0.192)**	**(0.189)**
Social	**-0.0167****	**-0.0160****	**-0.0184*****	**-0.314**	**-0.296**	**-0.409****
	(0.00772)	**(0.00714)**	**(0.00710)**	**(0.202)**	**(0.190)**	**(0.188)**
CD			0.0850***			3.434***
			(0.0142)			(0.373)
DVD			0.0924***			3.455***
			(0.00862)			(0.229)
Magazine			0.0776***			3.734***
			(0.0225)			(0.600)
August		0.0461***	0.0681***		-0.397	0.525
		(0.0131)	(0.0134)		(0.344)	(0.348)
September		0.00603	0.00730		-2.582***	-2.514***
		(0.0106)	(0.0106)		(0.278)	(0.276)
October		0.0303***	0.0253***		-3.247***	-3.414***
		(0.00924)	(0.00920)		(0.248)	(0.245)
November		-0.391***	-0.375***		-12.74***	-11.91***
		(0.0205)	(0.0204)		(0.545)	(0.540)
Age 20-40		0.00709	-0.00423		0.372	0.00343
		(0.00894)	(0.00897)		(0.240)	(0.238)
Age 40-60		-0.0549***	-0.0610***		-0.976***	-1.172***
		(0.00996)	(0.00996)		(0.266)	(0.263)
Age over 60		-0.105***	-0.104***		-1.748***	-1.620***
		(0.0159)	(0.0159)		(0.420)	(0.416)
Male		0.00540	-0.00146		0.0573	-0.225
		(0.00596)	(0.00597)		(0.159)	(0.157)
Foreign		0.0454***	0.0328***		1.141***	0.683***
		(0.00676)	(0.00684)		(0.180)	(0.181)
Number of Loans		-0.00256***	-0.00321***		-0.0443***	-0.0698***
		(0.000199)	(0.000209)		(0.00524)	(0.00546)
Prior Late		0.331***	0.317***			
		(0.00883)	(0.00884)			
Prior "Actual - Due"					0.227***	0.201***
					(0.00956)	(0.00950)
Constant	0.358***	0.276***	0.379***	-0.583***	1.963***	4.803
	(0.00546)	(0.0114)	(0.146)	(0.143)	(0.294)	(4.105)
Library FE	NO	NO	YES	NO	NO	YES
R-squared	0.000	0.152	0.166	0.000	0.107	0.138
Number of users	14605	14442	14442	14157	13990	13990
H0: Reminder=Social (*p*-value)	0.6245	0.6078	0.5208	0.7204	0.6074	0.7566
H0: Reminder=Social=0 (*p*-value)	0.0769	0.0636	0.027	0.128	0.0995	0.0257

Notes: *Proportion Late* measures the proportion of late returns per user, columns (1)-(2)-(3), and *"Actual - Due" Date* measures the average number of days between the return date and the due date per user, columns (4)-(5)-(6). See different email messages in Table 1. *CD*, *DVD* and *Magazine* are dummy variables for the item type (omitted category: *Book*), *August*, *September*, *October*, *November* months dummies (omitted category: *July*), and *Age 20-40*, *Age 40-60* and *Age over 60* are age dummies (omitted category: *Age under 20*). *Male* takes a value of 1 in case of male, *Foreign* a value of 1 in case of non-Spanish, and *Number of Loans* is the average number of loans per user. *Prior Late* and *Prior "Actual - Due"* refer to proportion of late returns per user and the average number of days between the return date and the due date, both prior to the treatment. Robust standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 6
CONTROL-REMINDER-SOCIAL-LATE-PENALTY

	Proportion Late			"Actual - Due" Date		
	(1)	(2)	(3)	(4)	(5)	(6)
Reminder	**-0.0266****	**-0.0235****	**-0.0239****	**-0.535***	**-0.565****	**-0.614****
	(0.0109)	**(0.0102)**	**(0.0101)**	**(0.297)**	**(0.281)**	**(0.276)**
Social	**-0.0275****	**-0.0270*****	**-0.0297*****	**-0.402**	**-0.425**	**-0.549****
	(0.0109)	**(0.0101)**	**(0.0100)**	**(0.295)**	**(0.279)**	**(0.275)**
Late	**-0.0252****	**-0.0274*****	**-0.0271*****	**-0.423**	**-0.496***	**-0.549****
	(0.0108)	**(0.0100)**	**(0.00994)**	**(0.293)**	**(0.278)**	**(0.273)**
Penalty	**-0.0391*****	**-0.0409*****	**-0.0433*****	**-0.674****	**-0.781*****	**-0.879*****
	(0.0108)	**(0.0101)**	**(0.00997)**	**(0.294)**	**(0.279)**	**(0.274)**
CD			0.0863***			3.100***
			(0.0149)			(0.405)
DVD			0.109***			3.173***
			(0.00930)			(0.257)
Magazine			0.109***			3.175***
			(0.0236)			(0.654)
August		0.0261*	0.0508***		-0.528	0.542
		(0.0145)	(0.0148)		(0.393)	(0.399)
September		0.0251**	0.0212*		-2.576***	-2.629***
		(0.0120)	(0.0120)		(0.326)	(0.323)
October		0.0392***	0.0316***		-3.874***	-4.124***
		(0.0104)	(0.0104)		(0.291)	(0.287)
November		-0.518***	-0.502***		-15.40***	-14.69***
		(0.0244)	(0.0242)		(0.683)	(0.675)
Age 20-40		0.0138	-0.00112		0.944***	0.515*
		(0.0103)	(0.0103)		(0.288)	(0.286)
Age 40-60		-0.0461***	-0.0549***		-0.512	-0.786**
		(0.0117)	(0.0117)		(0.325)	(0.321)
Age over 60		-0.0832***	-0.0843***		-0.754	-0.727
		(0.0198)	(0.0197)		(0.543)	(0.536)
Male		0.00577	-0.00151		0.106	-0.139
		(0.00647)	(0.00646)		(0.179)	(0.177)
Foreign		0.0496***	0.0377***		1.078***	0.719***
		(0.00700)	(0.00706)		(0.194)	(0.194)
Number of Loans		-0.00240***	-0.00298***		-0.0539***	-0.0727***
		(0.000191)	(0.000200)		(0.00511)	(0.00533)
Prior Late		0.301***	0.293***			
		(0.0109)	(0.0108)			
Prior "Actual - Due"					0.170***	0.156***
					(0.00969)	(0.00957)
Constant	0.440***	0.301***	0.158	0.759***	2.689***	4.478
	(0.00769)	(0.0145)	(0.150)	(0.209)	(0.369)	(3.690)
Library FE	NO	NO	YES	NO	NO	YES
R-squared	0.001	0.141	0.161	0.000	0.101	0.135
Number of users	12286	12205	12205	11846	11750	11750
H0: Reminder=Social (*p*-value)	0.9309	0.7307	0.5648	0.6554	0.6167	0.8135
H0: Reminder=Late (*p*-value)	0.8995	0.7013	0.7455	0.7043	0.8034	0.8125
H0: Reminder=Penalty (*p*-value)	0.2491	0.0851	0.0532	0.6353	0.4404	0.3365
H0: Late=Penalty (*p*-value)	0.1948	0.1744	0.1027	0.388	0.3019	0.2253
H0: Reminder=Social=Late=Penalty (*p*-value)	0.5471	0.3204	0.2225	0.779	0.605	0.5753
H0: Reminder=Social=Late=Penalty=0 (*p*-value)	0.0072	0.0016	0.0006	0.2129	0.0772	0.0283

Notes: *Proportion Late* measures the proportion of late returns per user, columns (1)-(2)-(3), *and "Actual - Due" Date* measures the average number of days between the return date and the due date per user, columns (4)-(5)-(6). See different email messages in Table 1. CD, DVD and Magazine are dummy variables for the item type (omitted category: Book), August, September, October, November months dummies (omitted category: July), and Age 20-40, Age 40-60 and Age over 60 are age dummies (omitted category: Age under 20). Male takes a value of 1 in case of male, Foreign a value of 1 in case of non-Spanish, and Number of Loans is the average number of loans per user. Prior Late and Prior "Actual - Due" refer to proportion of late returns per user and the average number of days between the return date and the due date, both prior to the treatment. Robust standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 7
Differential Treatment Effect with respect to the Proportion of Pending Items

	Control-Reminder-Social		Control-Rem.-Social-Late-Penalty	
	Prop. Late	"Actual - Due" Date	Prop. Late	"Actual - Due" Date
	(1)	(2)	(3)	(4)
Reminder	**-0.00890**	**0.00583**	**-0.0182**	**-0.0522**
	(0.00924)	**(0.238)**	**(0.0131)**	**(0.353)**
Social	**-0.0111**	**0.229**	**-0.0307****	**0.139**
	(0.00915)	**(0.236)**	**(0.0131)**	**(0.350)**
Late			**-0.0231***	**0.111**
			(0.0129)	**(0.346)**
Penalty			**-0.0334****	**-0.349**
			(0.0130)	**(0.350)**
Pending	0.249***	8.985***	0.267***	10.16***
	(0.0142)	(0.369)	(0.0202)	(0.548)
Reminder*Pending	**-0.00897**	**-1.288*****	**-0.0151**	**-1.718****
	(0.0190)	**(0.494)**	**(0.0278)**	**(0.752)**
Social*Pending	**-0.0161**	**-1.826*****	**0.000548**	**-2.334*****
	(0.0190)	**(0.494)**	**(0.0276)**	**(0.748)**
Late*Pending			**-0.00523**	**-1.891****
			(0.0273)	**(0.741)**
Penalty*Pending			**-0.0350**	**-1.844****
			(0.0273)	**(0.741)**
Constant	0.200	-1.347	-0.0436	-3.085
	(0.143)	(3.952)	(0.147)	(3.558)
Controls	YES	YES	YES	YES
Library FE	YES	YES	YES	YES
R-squared	0.204	0.203	0.201	0.200
Number of users	14442	13990	12205	11750
H0: Reminder=Social (*p*-value)	0.8101	0.3443	0.3403	0.5863
H0: Reminder=Late (*p*-value)			0.7086	0.637
H0: Reminder=Penalty (*p*-value)			0.2468	0.3982
H0: Late=Penalty (*p*-value)			0.421	0.1812
H0: Reminder=Social=Late=Penalty (*p*-value)			0.6355	0.4762
H0: Reminder=Social=Late=Penalty=0 (*p*-value)			0.0835	0.6426
H0: Reminder=Social=0 (*p*-value)	0.4404	0.5407		
H0: Reminder*Pend=Social*Pend (*p*-value)	0.7114	0.2792	0.5744	0.4127
H0: Reminder*Pend=Late*Pend (*p*-value)			0.7206	0.8162
H0: Reminder*Pend=Penalty*Pend (*p*-value)			0.4699	0.8655
H0: Late*Pend=Penalty*Pend (*p*-value)			0.2712	0.9491
H0: Reminder*Pend=Social*Pend=Late*Pend=Penalty*Pend (*p*-value)			0.5774	0.8568
H0: Reminder*Pend=Social*Pend=Late*Pend=Penalty*Pend=0 (*p*-value)			0.6655	0.0202
H0: Reminder*Pend=Social*Pend=0 (*p*-value)	0.6977	0.0007		

Notes: This table reports differential treatment effects with respect to the proportion of pending items per user. *Pending* measures the proportion of pending items per user on the moment the email treatment is received, while the interaction terms measure the differential treatment effects of the proportion of pending items. The full set of controls is used, as well as the library fixed effects. See the notes from previous tables. Robust standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 8

Treatment Effects over Time (Treatments Control-Reminder-Social)

	Proportion Late				"Actual - Due" Date			
	July 1 - July 31	**August 1 - Sept. 14**	**Sept. 15-Oct. 15**	**Oct. 15 onwards**	**July 1 - July 31**	**August 1 - Sept. 14**	**Sept. 15-Oct. 15**	**Oct. 15 onwards**
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Reminder	**-0.0290*** **	**-0.00818**	**-0.0108**	**-0.0149**	**-0.805** **	**-0.0564**	**-0.359** **	**-0.206**
	(0.0112)	**(0.0129)**	**(0.0110)**	**(0.0120)**	**(0.345)**	**(0.341)**	**(0.182)**	**(0.183)**
Social	**-0.0236** **	**-0.00554**	**-0.0235** **	**-0.00739**	**-0.883** **	**0.211**	**-0.545*** **	**-0.251**
	(0.0111)	**(0.0128)**	**(0.0108)**	**(0.0119)**	**(0.350)**	**(0.325)**	**(0.179)**	**(0.192)**
Constant	0.264	0.776***	0.837***	-0.290***	17.78**	8.006***	-7.453	-10.35***
	(0.165)	(0.0665)	(0.175)	(0.0297)	(8.829)	(2.308)	(8.922)	(0.777)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Library FE	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.132	0.129	0.146	0.109	0.128	0.151	0.184	0.153
Number of users	7029	5569	7379	6340	6934	5485	7200	5797
H0: Reminder=Social (*p* -value)	0.6268	0.8363	0.2465	0.5289	0.8181	0.4101	0.3062	0.8207
H0: Reminder=Social=0 (*p* -value)	0.0225	0.8108	0.0923	0.4626	0.0204	0.6785	0.0087	0.3591
Cross-Equation Joint Tests (*p* -values)	Proportion Late	"Actual-Due" Date						
H0: Reminder: (1)=(2)=(3)=(4)	0.0487	0.0164						
H0: Reminder(1)=Reminder(2)	0.2212	0.0564						
H0: Reminder(3)=Reminder(4)	0.8039	0.6914						
H0: Reminder(1)=Reminder(3)	0.2500	0.2203						
H0: Social: (1)=(2)=(3)=(4)	0.0470	0.0015						
H0: Social(1)=Social(2)	0.2845	0.0049						
H0: Social(3)=Social(4)	0.3159	0.4382						
H0: Social(1)=Social(3)	0.9963	0.3494						

TABLE 8 (continued)

Treatment Effects over Time (Control-Reminder-Social-Late-Penalty)

	Proportion Late				"Actual - Due" Date			
	July 1 - July 31	**August 1 - Sept. 14**	**Sept. 15-Oct. 15**	**Oct. 15 onwards**	**July 1 - July 31**	**August 1 - Sept. 14**	**Sept. 15-Oct. 15**	**Oct. 15 onwards**
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Reminder	**-0.0617*** **	**-0.0117**	**-0.0296***	**-0.0135**	**-1.325*** **	**-0.0755**	**-0.575** **	**-0.411**
	(0.0154)	**(0.0171)**	**(0.0154)**	**(0.0170)**	**(0.481)**	**(0.483)**	**(0.259)**	**(0.258)**
Social	**-0.0454*** **	**0.000166**	**-0.0224**	**-0.0235**	**-1.318*** **	**0.170**	**-0.613** **	**-0.407**
	(0.0155)	**(0.0171)**	**(0.0152)**	**(0.0167)**	**(0.503)**	**(0.451)**	**(0.252)**	**(0.283)**
Late	**-0.0389** **	**-0.0208**	**-0.0378** **	**-0.0128**	**-0.613**	**-0.513**	**-0.813*** **	**-0.355**
	(0.0154)	**(0.0172)**	**(0.0155)**	**(0.0172)**	**(0.496)**	**(0.461)**	**(0.251)**	**(0.263)**
Penalty	**-0.0754*** **	**-0.0115**	**-0.0288***	**-0.0336***	**-1.415*** **	**-0.383**	**-0.797*** **	**0.0792**
	(0.0153)	**(0.0170)**	**(0.0157)**	**(0.0173)**	**(0.487)**	**(0.436)**	**(0.253)**	**(0.247)**
Constant	0.372**	0.428*	0.0785	-0.300***	21.77**	6.729	-8.830***	-10.82***
	(0.147)	(0.235)	(0.208)	(0.0328)	(10.33)	(6.044)	(2.417)	(0.370)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Library FE	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.123	0.126	0.129	0.109	0.133	0.157	0.167	0.154
Number of users	6466	5294	6385	5457	6372	5205	6202	4907
H0: Reminder=Social (*p*-value)	0.2832	0.4876	0.6393	0.5527	0.9884	0.5920	0.8814	0.9879
H0: Reminder=Late (*p*-value)	0.1311	0.5939	0.5976	0.9684	0.1248	0.3407	0.3482	0.8395
H0: Reminder=Penalty (*p*-value)	0.3637	0.9939	0.9613	0.2469	0.8444	0.4886	0.3870	0.0560*
H0: Late=Penalty (*p*-value)	0.0153	0.5852	0.5700	0.2368	0.0897*	0.7553	0.9469	0.143
H0: Reminder=Social=Late=Penalty (*p*-value)	0.0673	0.6770	0.7980	0.5953	0.2980	0.3739	0.6985	0.1618
H0: Reminder=Social=Late=Penalty=0 (*p*-value)	0.0000	0.7052	0.1398	0.3676	0.0141**	0.4881	0.0088***	0.1567

Cross-Equation Joint Tests (*p*-values)	Proportion Late	"Actual - Due" Date
H0: Reminder: (1)=(2)=(3)=(4)	0.0947	0.1151
H0: Reminder(1)=Reminder(2)	0.0301	0.0209
H0: Reminder(3)=Reminder(4)	0.4805	0.7655
H0: Reminder(1)=Reminder(3)	0.1403	0.1431
H0: Social: (1)=(2)=(3)=(4)	0.2702	0.0493
H0: Social(1)=Social(2)	0.0482	0.0059
H0: Social(3)=Social(4)	0.9615	0.7045
H0: Social(1)=Social(3)	0.2911	0.1678
H0: Late(1)=Late(2)=Late(3)=Late(4)	0.5951	0.8655
H0: Late(1)=Late(2)	0.4321	0.8527
H0: Late(3)=Late(4)	0.2743	0.4075
H0: Late(1)=Late(3)	0.9626	0.6979
H0: Penalty: (1)=(2)=(3)=(4)	0.033	0.0428
H0: Penalty(1)=Penalty(2)	0.0054	0.0552
H0: Penalty(3)=Penalty(4)	0.8340	0.1153
H0: Penalty(1)=Penalty(3)	0.0337	0.2305

Notes: The table reports treatment effects for different time periods: The first month after the first email was sent (July1-July31), the second 6 weeks after the first email was sent (August 1-September 14), the first month after the second email was sent (September 15-October 15), the effect of the second email after one month (October 15 onwards). The first page of the table encompasses the users in treatments Control-Reminder-Social, as in Table 5, and the second page of the table corresponds to the users in treatments Control-Reminder-Social-Late-Penalty, as in Table 6. The full set of controls is used, as well as the library fixed effects. See the notes from previous tables. Robust standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 9
Differential Treatment Effect with respect to Prior Compliance

	Control-Reminder-Social		Control-Reminder-Social-Late-Penalty	
	Prop. Late	"Actual - Due" Date	Prop. Late	"Actual - Due" Date
	(1)	(2)	(3)	(4)
Reminder	**-0.00243**	**-0.516*** **	**0.00488**	**-0.381**
	(0.00981)	**(0.189)**	**(0.0193)**	**(0.282)**
Social	**-0.0137**	**-0.401** **	**-0.0260**	**-0.450**
	(0.00973)	**(0.188)**	**(0.0193)**	**(0.281)**
Late			**-0.0321***	**-0.414**
			(0.0191)	**(0.279)**
Penalty			**-0.0347***	**-0.705** **
			(0.0191)	**(0.281)**
Prior Late	**0.334*** **		**0.307*** **	
	(0.0149)		**(0.0234)**	
Reminder*Prior Late	**-0.0358***		**-0.0581***	
	(0.0212)		**(0.0331)**	
Social*Prior Late	**-0.0146**		**-0.00728**	
	(0.0210)		**(0.0328)**	
Late*Prior Late			**0.0100**	
			(0.0325)	
Penalty*Prior Late			**-0.0170**	
			(0.0323)	
Prior "Actual - Due"		**0.231*** **		**0.230*** **
		(0.0167)		**(0.0231)**
Reminder*Prior "Actual - Due"		**-0.0826*** **		**-0.124*** **
		(0.0223)		**(0.0296)**
Social*Prior "Actual - Due"		**0.00652**		**-0.0519**
		(0.0237)		**(0.0320)**
Late*Prior "Actual - Due"				**-0.0715** **
				(0.0302)
Penalty*Prior "Actual - Due"				**-0.0910*** **
				(0.0319)
Constant	0.375**	4.878	0.157	4.410
	(0.146)	(4.102)	(0.151)	(3.688)
Controls	YES	YES	YES	YES
Library FE	YES	YES	YES	YES
R-squared	0.166	0.139	0.162	0.136
Number of users	14442	13990	12205	11750
H0: Reminder=Social (*p*-value)	0.2511	0.5425	0.11	0.8073
H0: Reminder=Late (*p*-value)			0.0533	0.9048
H0: Reminder=Penalty (*p*-value)			0.386	0.2485
H0: Late=Penalty (*p*-value)			0.8328	0.2953
H0: Reminder=Social=Late=Penalty (*p*-value)			0.144	0.6429
H0: Reminder=Social=Late=Penalty=0 (*p*-value)			0.1102	0.1673
H0: Reminder=Social=0 (*p*-value)	0.325	0.0163		
H0: Reminder*Comp=Social*Comp (*p*-value)	0.3199	0.0001	0.1253	0.013
H0: Reminder*Comp=Late*Comp(*p*-value)			0.0384	0.0513
H0: Reminder*Comp=Penalty*Comp(*p*-value)			0.2092	0.2506
H0: Late*Comp=Penalty*Comp (*p*-value)			0.3987	0.5104
H0: Reminder*Comp=Social*Comp=Late*Comp=Penalty*Comp (*p*-value)			0.2049	0.069
H0: Reminder*Comp=Social*Comp=Late*Comp=Penalty*Comp=0 (*p*-value)			0.2832	0.0007
H0: Reminder*Comp=Social*Comp=0 (*p*-value)	0.2383	0		

Notes: This table reports differential treatment effects with respect to prior compliance. Prior Late measures the user-specific proportion of items that were returned late in the pre treatment period. Prior "Actual - Due" measures the average number of days between the return date and the due date per user in the pre treatment period. The full set of controls is used, as well as the library fixed effects. See the notes from previous tables. Robust standard errors in paranthesis. *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

Table 10
Differential Treatment Effect by Gender

	Control-Reminder-Social		Control-Reminder-Social-Late-Penalty	
	Proportion Late	"Actual - Due" Date	Proportion Late	"Actual - Due" Date
	(1)	(2)	(3)	(4)
Reminder	**-0.00770**	**-0.255**	**-0.0200**	**-0.302**
	(0.00945)	**(0.250)**	**(0.0134)**	**(0.368)**
Social	**-0.0207****	**-0.255**	**-0.0345*****	**-0.169**
	(0.00936)	**(0.247)**	**(0.0133)**	**(0.366)**
Late			**-0.0213**	**-0.0801**
			(0.0132)	**(0.363)**
Penalty			**-0.0475*****	**-0.915****
			(0.0132)	**(0.364)**
Male	0.00132	0.0613	-0.00120	0.382
	(0.0102)	(0.270)	(0.0144)	(0.394)
Reminder* Male	**-0.0141**	**-0.497**	**-0.00885**	**-0.721**
	(0.0145)	**(0.382)**	**(0.0203)**	**(0.557)**
Social*Male	**0.00552**	**-0.363**	**0.0110**	**-0.874**
	(0.0144)	**(0.379)**	**(0.0202)**	**(0.554)**
Late*Male			**-0.0134**	**-1.078***
			(0.0201)	**(0.551)**
Penalty*Male			**0.00979**	**0.0793**
			(0.0201)	**(0.553)**
Constant	0.375**	4.618	0.156	4.014
	(0.146)	(4.107)	(0.150)	(3.695)
Controls	YES	YES	YES	YES
Library FE	YES	YES	YES	YES
R-squared	0.166	0.138	0.161	0.135
Number of users	14442	13990	12205	11750
H0: Reminder=Social (*p*-value)	0.1689	0.9998	0.2812	0.7191
H0: Reminder=Late (*p*-value)			0.9244	0.5444
H0: Reminder=Penalty (*p*-value)			0.0395	0.0944
H0: Late=Penalty (*p*-value)			0.0462	0.0211
H0: Reminder=Social=Late=Penalty (*p*-value)			0.1278	0.0909
H0: Reminder=Social=Late=Penalty=0 (*p*-value)			0.0061	0.0882
H0: Reminder=Social=0 (*p*-value)	0.0818	0.4946		
H0: Reminder*Male=Social*Male(*p*-value)	0.1746	0.7253	0.3294	0.7839
H0: Reminder*Male=Late*Male(*p*-value)			0.8217	0.5183
H0: Reminder*Male=Penalty*Male(*p*-value)			0.3566	0.1491
H0: Late*Male=Penalty*Male (*p*-value)			0.2451	0.0347
H0: Reminder*Male=Social*Male=Late*Male=Penalty*Male (*p*-value)			0.5036	0.1643
H0: Reminder*Male=Social*Male=Late*Male=Penalty*Male=0 (*p*-value)			0.6721	0.1213
H0: Reminder*Male=Social*Male=0 (*p*-value)	0.3763	0.4016		

Notes: The table reports the differential treatment effect with respect to gender. *Male* is a dummy variable taking a value of 1 in case of male, and 0 in case of female. The full set of controls is used, as well as the library fixed effects. See the notes from previous tables. Robust standard errors in parantheses *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE 11
Treatment Effects by Nationality (Treatments Control-Reminder-Social)

	Proportion Late						
	Spain (1)	**Northern-Central Europe** (2)	**West-South Europe** (3)	**English Speaking** (4)	**Eastern-Russia** (5)	**Latin America** (6)	**Asia** (7)
Reminder	**-0.0111**	**-0.00536**	**-0.00472**	**-0.123****	**0.0915**	**-0.0254**	**-0.0666**
	(0.00839)	**(0.0550)**	**(0.0315)**	**(0.0558)**	**(0.0677)**	**(0.0180)**	**(0.102)**
Social	**-0.0148***	**-0.0456**	**0.00147**	**-0.207*****	**-0.0164**	**-0.0227**	**0.101**
	(0.00834)	**(0.0564)**	**(0.0310)**	**(0.0544)**	**(0.0661)**	**(0.0178)**	**(0.0869)**
Constant	0.260***	0.411***	0.280***	0.294**	0.182*	0.308***	-0.0764
	(0.0131)	(0.111)	(0.0816)	(0.116)	(0.101)	(0.0311)	(0.147)
Controls	YES	YES	YES	YES	YES	YES	YES
R-squared	0.152	0.126	0.190	0.261	0.226	0.167	0.413
Number of users	10395	265	745	224	185	2369	84
H0: Reminder=Social (*p* -value)	0.6576	0.4673	0.8416	0.145	0.0958	0.8786	0.0674
H0: Reminder=Social=0 (*p* -value)	0.1803	0.6758	0.9787	0.0008	0.2093	0.3006	0.1596

	"Actual - Due" Date						
	Spain (1)	**Northern-Central Europe** (2)	**West-South Europe** (3)	**English Speaking** (4)	**Eastern-Russia** (5)	**Latin America** (6)	**Asia** (7)
Reminder	**-0.373***	**-0.320**	**-0.708**	**-1.536**	**-1.367**	**-0.418**	**-0.546**
	(0.216)	**(1.138)**	**(0.907)**	**(2.002)**	**(1.784)**	**(0.513)**	**(1.983)**
Social	**-0.348**	**0.331**	**-0.271**	**-5.488*****	**-2.503**	**0.113**	**1.923**
	(0.215)	**(1.175)**	**(0.896)**	**(1.959)**	**(1.700)**	**(0.507)**	**(1.705)**
Constant	1.128***	-1.086	2.929	5.396	3.961	1.627*	-7.904***
	(0.327)	(2.435)	(2.310)	(4.047)	(2.442)	(0.863)	(2.664)
Controls	YES	YES	YES	YES	YES	YES	YES
R-squared	0.125	0.169	0.192	0.124	0.136	0.112	0.396
Number of users	10091	256	721	216	178	2284	82
H0: Reminder=Social (*p* -value)	0.9083	0.5692	0.6206	0.0586	0.5061	0.2899	0.1679
H0: Reminder=Social=0 (*p* -value)	0.1537	0.85	0.7322	0.0184	0.3405	0.541	0.306
Cross-Equation Joint Tests (*p* -values)	Prop. Late	"Actual-Due" Date					
H0: Reminder: (1)=(2)=...=(7)	0.3439	0.9874					
H0: Social: (1)=(2)=...=(7)	0.0401	0.0216					

TABLE 11 (continued)
Treatment Effects by Nationality (Control-Reminder-Social-Late-Penalty)

	Proportion Late						
	Spain	**Northern-Central Europe**	**West-South Europe**	**English Speaking**	**Eastern-Russia**	**Latin America**	**Asia**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reminder	**-0.0184**	**0.0258**	**0.0133**	**-0.224*** **	**0.0315**	**-0.0401***	**-0.206***
	(0.0123)	**(0.0669)**	**(0.0407)**	**(0.0690)**	**(0.0868)**	**(0.0236)**	**(0.111)**
Social	**-0.0253****	**-0.0150**	**0.0139**	**-0.295*** **	**-0.0788**	**-0.0256**	**-0.0211**
	(0.0122)	**(0.0681)**	**(0.0393)**	**(0.0684)**	**(0.0825)**	**(0.0233)**	**(0.100)**
Late	**-0.0290****	**0.0840**	**-0.0156**	**-0.186*** **	**0.0615**	**-0.0108**	**-0.207****
	(0.0121)	**(0.0669)**	**(0.0396)**	**(0.0659)**	**(0.0820)**	**(0.0235)**	**(0.0994)**
Penalty	**-0.0427*** **	**0.0263**	**-0.0463**	**-0.245*** **	**-0.0659**	**-0.0111**	**-0.280*** **
	(0.0122)	**(0.0710)**	**(0.0387)**	**(0.0662)**	**(0.0825)**	**(0.0233)**	**(0.104)**
Constant	0.274***	0.299	0.217**	0.444***	0.112	0.361***	0.0430
	(0.0172)	(0.184)	(0.0927)	(0.124)	(0.115)	(0.0366)	(0.141)
Controls	YES	YES	YES	YES	YES	YES	YES
R-squared	0.144	0.261	0.195	0.286	0.273	0.156	0.519
Number of users	8198	241	778	230	195	2308	79
H0: Reminder=Social (*p* -value)	0.5771	0.542	0.9874	0.3284	0.1812	0.526	0.0762
H0: Reminder=Late (*p* -value)	0.3868	0.3648	0.4713	0.5823	0.7142	0.2024	0.9899
H0: Reminder=Penalty (*p* -value)	0.0494	0.9949	0.1281	0.7649	0.229	0.205	0.4926
H0: Late=Penalty (*p* -value)	0.2634	0.4005	0.4192	0.3804	0.0966	0.9886	0.4617
H0: Reminder=Social=Late=Penalty (*p* -value)	0.2495	0.513	0.3404	0.4623	0.1869	0.5271	0.0455
H0: Reminder=Social=Late=Penalty=0 (*p* -value)	0.0105	0.6246	0.4845	0.0002	0.3028	0.4686	0.0178

	"Actual - Due" Date						
	Spain	**Northern-Central Europe**	**West-South Europe**	**English Speaking**	**Eastern-Russia**	**Latin America**	**Asia**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reminder	**-0.413**	**0.750**	**0.174**	**-6.450*** **	**-2.588**	**-0.797**	**-1.796**
	(0.332)	**(1.764)**	**(1.082)**	**(2.341)**	**(2.457)**	**(0.689)**	**(2.344)**
Social	**-0.343**	**2.613**	**0.138**	**-8.512*** **	**-4.938****	**-0.373**	**2.943**
	(0.332)	**(1.792)**	**(1.051)**	**(2.333)**	**(2.298)**	**(0.679)**	**(2.104)**
Late	**-0.638***	**3.497****	**-0.0316**	**-7.432*** **	**-1.902**	**0.605**	**-1.128**
	(0.329)	**(1.748)**	**(1.060)**	**(2.239)**	**(2.294)**	**(0.683)**	**(2.079)**
Penalty	**-0.823****	**2.547**	**-1.331**	**-7.085*** **	**-5.103****	**-0.0294**	**-2.397**
	(0.331)	**(1.861)**	**(1.040)**	**(2.219)**	**(2.317)**	**(0.684)**	**(2.157)**
Constant	1.676***	0.0302	-1.796	12.88***	3.520	2.639***	-5.836**
	(0.428)	(5.358)	(2.456)	(4.090)	(2.960)	(1.008)	(2.813)
Controls	YES	YES	YES	YES	YES	YES	YES
R-squared	0.112	0.182	0.206	0.166	0.198	0.130	0.442
Number of users	7902	237	747	217	184	2222	78
H0: Reminder=Social (*p* -value)	0.8333	0.2919	0.9723	0.4045	0.3164	0.522	0.0337
H0: Reminder=Late (*p* -value)	0.5006	0.107	0.8453	0.6784	0.7716	0.0354	0.7716
H0: Reminder=Penalty (*p* -value)	0.2208	0.3335	0.1445	0.7911	0.2823	0.2501	0.793
H0: Late=Penalty (*p* -value)	0.5761	0.5954	0.1977	0.8795	0.1428	0.3384	0.5347
H0: Reminder=Social=Late=Penalty (*p* -value)	0.4617	0.4398	0.3817	0.8641	0.3634	0.1894	0.0361
H0: Reminder=Social=Late=Penalty=0 (*p* -value)	0.1299	0.2526	0.5274	0.0015	0.1475	0.3041	0.0694
Cross-Equation Joint Tests (*p* -values)	Prop. Late	"Actual-Due" Date					
H0: Reminder: (1)=(2)=…=(7)	0.0905	0.1302					
H0: Social: (1)=(2)=…=(7)	0.0256	0.001					
H0: Late: (1)=(2)=…=(7)	0.0938	0.003					
H0: Penalty: (1)=(2)=…=(7)	0.0355	0.0064					

Notes: The table reports treatment effects for different groups of nationalities. The first page encompasses the users in treatments Control-Reminder-Social, analogue to Table 5 and the second page corresponds to the users in treatments Control-Reminder-Social-Late-Penalty, analogue to Table 6. Robust standard errors in parantheses *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE A.1
Prop. of Late Returns for Different Nationality Groups

	Prop. Late Before (1)	Prop. Late After (2)
Europe-North-Central	**-0.0137**	**-0.0109**
	(0.0111)	**(0.0172)**
Europe-West-South	**0.0245*** **	**0.0347*** **
	(0.00708)	**(0.0105)**
English Speaking Countries	**0.0369*** **	**0.0438** **
	(0.0116)	**(0.0178)**
Russia-East	**0.0076**	**0.0453** **
	(0.0130)	**(0.0192)**
Latin America	**0.0292*** **	**0.0552*** **
	(0.00436)	**(0.0064)**
Asia	**-0.0608*** **	**-0.01678**
	(0.0175)	**(0.0283)**
Constant	0.4115***	0.2977**
	(0.0131)	(0.1294)
Controls	YES	YES
Library FE	YES	YES
R-squared	0.0449	0.092
Number of users	59367	25591
H0: Nationality groups equal	0	0.0014
H0: Nationality groups equal=0	0	0

Notes: The table reports proportion of late returns per user for different groups of nationalities. The omitted variable is Spaniards. Column (1) refers to the pre-treatment period and column (2) to the post-treatment period. Full set of controls is used. Robust standard errors in parantheses *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE A.2
English Speaking Countries compared to Spain

	Prop. Late	"Actual - Due" Date	Prop. Late	"Actual - Due" Date
	(1)	(2)	(3)	(4)
Reminder	-0.00797	-0.469**	-0.00221	-0.309
	(0.0112)	(0.218)	(0.0230)	(0.336)
Social	-0.0112	-0.372*	-0.0308	-0.390
	(0.0111)	(0.218)	(0.0230)	(0.336)
Late			-0.0526**	-0.642*
			(0.0229)	(0.334)
Penalty			-0.0451**	-0.833**
			(0.0227)	(0.336)
English	0.149***	3.579***	0.183***	6.264***
	(0.0386)	(0.999)	(0.0504)	(1.370)
Reminder*English	**-0.117****	**-1.543**	**-0.184****	**-5.626*****
	(0.0574)	**(1.494)**	**(0.0751)**	**(2.050)**
Social*English	**-0.195*****	**-4.892*****	**-0.255*****	**-7.623*****
	(0.0566)	**(1.470)**	**(0.0741)**	**(2.035)**
Late*English			**-0.148****	**-7.308*****
			(0.0706)	**(1.942)**
Penalty*English			**-0.223*****	**-6.586*****
			(0.0721)	**(1.963)**
Prior Late	0.319***		0.277***	
	(0.0173)		(0.0277)	
Prior "Actual - Due"		0.209***		0.193***
		(0.0191)		(0.0264)
Reminder*Prior Late	-0.0124		-0.0343	
	(0.0247)		(0.0393)	
Social*Prior Late	-0.0158		0.00553	
	(0.0246)		(0.0393)	
Reminder*Prior "Actual - Due"		-0.0571**		-0.0933***
		(0.0258)		(0.0340)
Social*Prior "Actual - Due"		0.0185		-0.0427
		(0.0276)		(0.0375)
Late*Prior Late			0.0445	
			(0.0389)	
Penalty*Prior Late			-0.00215	
			(0.0386)	
Late*Prior "Actual - Due"				-0.0503
				(0.0347)
Penalty*Prior "Actual - Due"				-0.0816**
				(0.0379)
Constant	0.332	3.382	0.164	-0.333
	(0.208)	(5.707)	(0.215)	(6.254)
Controls	YES	YES	YES	YES
Library FE	YES	YES	YES	YES
R-squared	0.161	0.137	0.156	0.131
Number of users	10619	10307	8428	8119

Notes: The table reports differential treatment effects for users from the English speaking countries. The reference group is Spaniards (omitted). Interaction terms for differential treatment effects for users in the English speaking countries are shown. Interaction terms for differential treatment effects based on the behavior prior to the treatment are included. Full set of controls, as well as library fixed effects are included. See the notes from previous tables. Robust standard errors in parantheses *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.

TABLE A.3
Asia compared to Spain

	Prop. Late	"Actual - Due" Date	Prop. Late	"Actual - Due" Date
	(1)	(2)	(3)	(4)
Reminder	-0.00642	-0.471**	0.00319	-0.310
	(0.0112)	(0.216)	(0.0232)	(0.334)
Social	-0.0113	-0.373*	-0.0297	-0.396
	(0.0111)	(0.216)	(0.0232)	(0.334)
Late			-0.0430*	-0.652**
			(0.0231)	(0.332)
Penalty			-0.0411*	-0.839**
			(0.0229)	(0.334)
UKA-USA	-0.0374	-1.393	0.0860	-0.246
	(0.0746)	(1.888)	(0.0974)	(2.580)
Reminder*Asia	**-0.110**	**-0.759**	**-0.267***	**-2.578**
	(0.105)	**(2.699)**	**(0.141)**	**(3.808)**
Social*Asia	**0.124**	**2.217**	**-0.00630**	**1.402**
	(0.0929)	**(2.361)**	**(0.129)**	**(3.429)**
Late*Asia			**-0.194**	**-1.744**
			(0.126)	**(3.351)**
Penalty*Asia			**-0.264****	**-2.815**
			(0.128)	**(3.386)**
Prior Late	0.320***		0.283***	
	(0.0174)		(0.0280)	
Prior "Actual - Due"		0.204***		0.191***
		(0.0191)		(0.0265)
Reminder*Prior Late	**-0.0180**		**-0.0452**	
	(0.0249)		**(0.0396)**	
Social*Prior Late	**-0.0162**		**0.00391**	
	(0.0248)		**(0.0397)**	
Reminder*Prior "Actual - Due"		**-0.0564****		**-0.0940*****
		(0.0255)		**(0.0338)**
Social*Prior "Actual - Due"		**0.0215**		**-0.0390**
		(0.0275)		**(0.0375)**
Late*Prior Late			**0.0258**	
			(0.0392)	
Penalty*Prior Late			**-0.00934**	
			(0.0390)	
Late*Prior "Actual - Due"				**-0.0422**
				(0.0352)
Penalty*Prior "Actual - Due"				**-0.0763****
				(0.0378)
Constant	0.329	3.295	0.158	-0.454
	(0.208)	(5.657)	(0.215)	(6.216)
Controls	YES	YES	YES	YES
Library FE	YES	YES	YES	YES
R-squared	0.159	0.137	0.155	0.131
Number of user	10479	10173	8277	7980

Notes: The table reports differential treatment effects for Asian users. The reference group is Spaniards (omitted). Interaction terms for differential treatment effects for Asia are shown. Interaction terms for differential treatment effects based on the behavior prior to the treatment are included. Full set of controls, as well as library fixed effects are included. See the notes from previous tables. Robust standard errors in parantheses *: significant at the 10% level; **: significant at the 5% level; ***: significant at the 1% level.