The Limits of Social Recognition: Experimental Evidence from Blood Donors

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Abstract

Does social recognition motivate prosocial individuals? We run large-scale experiments at Italy's main blood donors association, testing social recognition in social media and peer groups. We experimentally disentangle visibility concerns and peer comparisons, and study how exposure to different social norms affects giving. In three studies, we find that a simple ask to donate is at least as effective as offering social recognition. A survey experiment with blood donors indicates that social recognition backfires when offered to people that are already perceived as good citizens. Our results suggest that increasing visibility of good actions can backfire when perceived as image-seeking.

Keywords: Prosocial behavior, blood donations, social recognition, natural field experiment, social media, WhatsApp.

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Well functioning societies rely on the contributions of good citizens – such as mission oriented workers, community leaders and volunteers – who routinely support their communities even if they are not monetarily compensated for the full social value of their actions. An active research agenda investigates complementary forms of compensation that foster good citizenship, including future benefits in social interactions, psychological utility from helping others and social rewards (Bursztyn and Jensen, 2017; Ashraf and Bandiera, 2018).

This paper addresses how social rewards shape good citizenship. In particular, we study how one commonly used social reward – recognition – motivates repeat blood donors. Theory emphasizes conformity with a socially relevant group, social pressure, and costly signaling of a socially desirable identity as reasons why people respond to social recognition (Bernheim, 1994; Akerlof and Kranton, 2000; Bénabou and Tirole, 2006, 2011). However, it is unclear whether social recognition increases the motivation of all kinds of potential contributors. On the one hand, stronger identification with a group can raise the stakes of norm adherence; on the other hand, once a strong altruistic reputation is established, adding social recognition as a reward may dilute the positive signal or even present good actions as selfish image-seeking behavior (Bénabou and Tirole, 2006).

Through partnership with the largest Italian association of blood donors—Avis—we embed an experiment into the infrastructure of the Tuscany regional branch.[1] For our main study, we select eligible donors that can be contacted through WhatsApp, asking them to donate blood (or blood plasma) in the following month. We offer them social recognition using either of two main approaches. First, we follow a classical intervention from Gerber et al. (2008) that informs donors, at the beginning of the study period, of their peers' recent engagement in a civic activity, followed up at endline with information on who took part in the activity during the study period. We put this in practice by creating random groups of twenty donors to whom participants in the experiment can relate. Second, we introduce a social media campaign that rewards participants who donate in the study period by prominently listing their names on highly subscribed public Facebook pages of Avis Toscana.

[1]Several features make the setting exceptional: (i) we can eliminate concerns that results could be driven by awareness of being observed by researchers and concerns of participants acting out of desire to please the experimenter, (ii) we can ensure high levels of participants' engagement with our intervention thanks to the availability of official trusted communication channels, and (iii) we have direct access to administrative records of blood donations from the regional health authorities that we can precisely link to experimental data.

We test social recognition against two natural benchmarks: not being solicited and being solicited with a simple ask. We find that participants give more in the social recognition treatments than when they are not solicited. However, the simple ask is at least as effective at encouraging giving as any of the social recognition interventions, and significantly more effective than the condition offering social recognition on Facebook.

The Facebook social media recognition intervention replicates an initial study – conducted two years prior to the main study. In the initial study, the prospect of social recognition led participants to give more than they would have without solicitation, but a simple ask was at least as effective. The initial study presented some limitations. In particular, poor engagement of donors with the organization's treatment emails. Thus, the intention-to-treat estimate is based on the 23 percent of compliers who read email communications of the organization and are presumably the most motivated donors. In our main study we replicate the initial experiment and we vary whether eligible participants are contacted via email or WhatsApp. We obtain similar results in the replication that we conducted via email: engagement is low (17 percent email opening rate) and the prospect of recognition on social media does not inspire giving any more than a simple ask. The replication through WhatsApp instead delivers much greater engagement (91 percent of participants read our messages) and finds that donations *decrease* with social recognition relative to a simple ask ($\hat{\beta} = -0.015$, $p = 0.026$).

We interpret these as negative results in that a simple ask is found to be more effective than recognition incentives that are costly for organizations to implement. These results were unexpected to us. Models of social image concerns can explain these as an *overjustification effect*: If agents are heterogeneous in their desire to be seen as altruistic by others, visible acts of good citizenship signal both the agent's prosocial type and their desire to impress others. Increasing visibility of good actions can backfire when the prevalent sentiment is that good actions constitute image-seeking behavior (Bénabou and Tirole, 2006, p. 1665).[2]

We use a pre-registered survey experiment, with 3016 participants from the main study, to provide more direct evidence for this backfiring interpretation. We measure inference about the types of donors who do give blood in our study. We then collect three sets of beliefs

[2]We thank Roland Bénabou for this helpful suggestion. Explicit illustration of how heterogenous image concerns deliver this result, in a model of social signaling, is provided in Appendix B.

about both a selected sample of repeat donors or the general population: beliefs about key primitives of social signaling models, predictions about the sign of the gap in donations from an intervention that compares the *Simple ask* and *Facebook* messaging, and qualitative evidence of what donors themselves believe to be the main mechanisms at play in such intervention.

We first establish that inference about those who donate in the study period is consistent with the idea that donations signal both altruism and image concern. Moving to primitives, we show that participants expect the distribution of altruistic preferences to be much more concentrated on high altruism among repeat donors than in the general population, and the distribution of image concern to be flatter among repeat donors. Predictions of study participants are that similar social recognition interventions are more likely to succeed with the general population and backfire with repeat donors. Taken together these findings help explain our results in relation to a large literature — of which we provide a meta-analysis that overwhelmingly reports positive effects of social recognition on acts of good citizenship; recognition backfires when good actions do little to improve altruistic image and instead signal image concern. Qualitative responses are also consistent with this view and indicate that other mechanisms, such as privacy concerns (Goldfarb and Tucker, 2011) or aversion to control systems (Ellingsen and Johannesson, 2008), are considered to play a lesser role in our setting.

The part of our intervention that studies social recognition in groups of twenty donors also sheds light on mechanisms. First, we design the social recognition treatments as a modular intervention, so to expose different donors to different module combinations and disentangle the effects of peer comparisons from visibility. Second, we exploit rich experimental variation in the social norms of giving that subjects are exposed to to map the giving schedule along the full support of possible descriptive norms. We find that donors who get offered visibility, on top of peer comparisons, do not give blood at significantly higher rates. Exposure to higher (or lower) descriptive norms of giving does not encourage more giving.

We causally investigate social proximity as a moderating factor of social recognition. Half the participants are randomly assigned to twenty-donors groups that include people from all over Tuscany (*distant*), while the other half is assigned to groups that only include people who donate at the same blood collection center and potentially know each other (*close*).

We show that greater social proximity to a group increases the chance that fellow group members know each other, but in turn greater social proximity neither leads to an increase in donations that are visible to other group members nor it leads to more giving.

This paper contributes to a broad literature on social recognition (see Bursztyn and Jensen, 2017, for a review).[3] Few of these papers focus on settings where people already have a strong identity and reputation as good citizens. Exceptions include Ager et al. (2017) who study status seeking on the intensive margin of performance among World War II pilots and Soetevent (2005) who study church offerings in a repeated experiment. Among repeat blood donors, we provide surprising evidence that prospective social recognition does not motivate people to give. This contrasts with other studies that report suggestive positive effects of social recognition on blood giving: Lacetera and Macis (2010) use non-linear social incentives to provide quasi-experimental evidence on how the prospect of reaching a milestone of donations that qualifies for a publicly awarded medal affects the time lag between donations. Meyer and Tripodi (2021) manipulate in a field experiment the visibility of *pledges* to give blood (but not giving itself) and find increased pledging. We zoom out and provide a meta-analysis of field experiments on social recognition for acts of good citizenship, which finds a positive meta-analitical effect that remains significant even after accounting for publication bias (Andrews and Kasy, 2019; DellaVigna and Linos, 2022) and highlights our study as the only evidence of significant crowding-out.[4] Further, we design additional interventions that capture the mechanisms through which social recognition backfires.

Our work complements a large literature on peer and social influence (e.g. Frey and Meier, 2004; Shang and Croson, 2009; Krupka and Weber, 2009; Allcott, 2011; Ferraro and Price, 2013; Kessler, 2017; Cantoni et al., 2019; Drago et al., 2020; Oh, 2021; Becker, 2021), which much has developed our understanding of how these forces can be molded in natural settings. We exploit rich variation in the behavior of groups of peers to offer granular

[3]Relevant to our work is the extensive literature on how prosocial behavior responds to image concerns (Ariely et al., 2009), social norms (Frey and Meier, 2004; Fellner et al., 2013), and social pressures (DellaVigna et al., 2012). Support for these social influence mechanisms comes also from studies on other forms of good citizenship, such as child immunization (Karing, 2018), energy conservation (Allcott and Rogers, 2014) and voting (Gerber et al., 2008).

[4]Related social recognition interventions have been found to have negative effects in other settings, such as school attendance (Robinson et al., 2021), when social recognition is awarded *retrospectively* as a surprise.

evidence on how donors respond to different levels of descriptive social norms.[5] We provide the first causal test from the field for social proximity as a moderator of social influence, which we find to be weak in comparison to existing correlational results (e.g. Bond et al., 2012). We offer new insights to a strand of this literature on how context shapes the direction of social recognition effects: Existing evidence demonstrates that the composition of *observers* in social recognition interventions matters (Bursztyn et al., 2019; Braghieri, 2021), while our paper sheds light on the importance of composition of the *observed*. We further showcase how measuring inferences that people make about the underlying motives of others (Bursztyn et al., 2022), can be used to anticipate how a policy will play out at scale.

Blood products are essential in medicine and cannot yet be generated artificially (Shaffer, 2020). In most countries, the balancing of demand and supply of blood is made hard by the absence of a price mechanism (blood donors cannot be monetarily compensated), and giving blood is purely a civic-minded act (World Health Organization, 2009). This landscape has led to growing interest from social scientists in investigating blood donations as a measure of social capital of a community (Guiso et al., 2004), and in addressing a key policy objective by developing non-monetary instruments that can be applied to address blood shortages (Heger et al., 2020). This study offers the first experimental evidence on the effects of social recognition on blood donations, and offers guidance on how natural features of local collection systems can be leveraged to harness social recognition. Furthermore, this paper showcases how modern social media tools, such as the WhatsApp Business API can serve to run massive personalized campaigns, as an innovation to be applied to a broader literature on the social and economic impacts of media (DellaVigna and La Ferrara, 2015).

The rest of the paper is organized as follows. Section 1 describes the setting and research design of our interventions. Section 2 provides experimental results from our interventions. Section 3 presents the survey experiment. Section 4 addresses further policy questions of inter-temporal substitution and congestion effects. Section 5 concludes.

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[5] In comparison, existing research identifies adherence to social norms primarily from variation in information about one level of average group behavior (e.g. Frey and Meier, 2004), or information about the behavior of one peer in dyadic comparisons (e.g. Schultz et al., 2007). Papers that leverage rich group comparisons include Allcott (2011); Ferraro and Price (2013); Beshears et al. (2015), and to the best of our knowledge the only working paper that maps the causal effects of different levels of group norms is Akesson et al. (2021).

1. Research design

1.1. Institutional setting

The study was conducted in partnership with the Tuscany branch of *Avis* (`Avis.it`) – the largest Italian association of blood donors. Blood collection in Italy relies on blood donor associations. In 2018, 92 percent of donors in Italy are affiliated with a blood donor association (Catalano et al., 2019). Blood donor associations are in charge of donor recruitment and they support local hospitals in the scheduling of appointments for donations. In some regions these associations also handle the collection of blood directly and commercialization of intermediate blood products.

Working with *Avis Toscana* allows us to reach the vast majority of blood donors in the region, and it gives us access to several official communication channels to contact donors. Avis Toscana is also one of the few regional branches in the country to have access to a rich data infrastructure that links administrative individual-level donation data from the universe of hospitals and other blood collection centers available to donors. In turn, this setting is particularly suitable for investigations that combine experimental interventions to accurate administrative data on actual donation behavior.

We conduct the main part of the experiment using Avis Toscana's official WhatsApp account. With the support of the customer engagement service Twilio (`Twilio.com`), we deploy a new approach for conducting experiments. Twilio allows us to access the WhatsApp Business API through which we can simultaneously contact very large numbers of registered donors with personalized messages. The WhatsApp Business API tool was introduced by WhatsApp in August 2018, and is primarily used by large firms and organizations for personalized service communications with their customers and beneficiaries.[6] Conducting experiments through the WhatsApp Business API presents at least four substantive advantages over sms, mail and email experiments: availability of reliable information on subject engagement with the experiment, ease of conducting longitudinal studies, ease in establishing trust with recipient through official verification of the organization's account (green

[6]Facebook reviews all templates of messages that organizations want to send to their contact lists and does not allow advertisement or mass campaigns.

check mark), at a relatively low cost (4.70 USD every 100 messages).[7]

1.2. Intervention 1: Delivery and mechanisms of social recognition

We design an experiment with the objectives of testing theory and developing an understanding of how to harness social recognition in a relevant setting. First, we take a classical social recognition intervention that features peer comparisons and visibility in small social groups (Gerber et al., 2008), and we decompose it to separately identify the two motivational mechanisms. Second, we introduce experimental variation in the social proximity of the group of peers to whom donations are made visible and with whom subjects compare their donation behavior. Third, we benchmark the effects of social incentives to a simple ask. Fourth, we embed in the experiment two replications of an initial study, originally conducted in 2019, where donors are offered broader recognition on social media.

The intervention is summarized in Figure 1. A few days before the intervention, Avis Toscana used their official WhatsApp channel to conduct a short survey, unrelated to this particular study. We use delivery receipts from this campaign to identify donors who use WhatsApp and exclude the rest. Donors selected to participate in the study are randomly partitioned in groups of twenty and treatment is assigned at this twenty-donors group level. All experimental messages sent to donors encourage them to donate in the month of March 2021. The *No ask* treatment serves as a passive control. The *Peer + Visibility* treatment mirrors the social recognition intervention in Gerber et al. (2008), providing a social comparison with fellow group members over donations made in the past 11 months and promises an

[7]The WhatsApp Business API allows organizations to reach out to their beneficiaries or customers only when consent for being contacted is provided. An ideal feature of our setting is that all Avis donors provide consent to be contacted by Avis for calls and initiatives that encourage them to donate blood.

We systematically review prior studies that have used WhatsApp as a channel to conduct an experiment. From a Google Scholar search that we ran on April 20, 2021 with keywords "whatsapp" and "experiment", we scraped the 996 most relevant search results. From our reading of the abstracts we discard studies that are not experiments. Moura and Michelson (2017) run a series of get-out-the-vote experiments and manually send videos via WhatsApp to about 1,000 eligible voters. Hoffmann and Thommes (2020) combine WhatsApp, and standard text messages to contact repeatedly 41 truck drivers over a two months period. The closest approach to ours is introduced in Bowles et al. (2020), who use the 'broadcast list' feature of private WhatsApp accounts to contact 27,000 newsletter subscribers with non-personalized messages. This method has the clear advantage that it does not require paying text message fees to WhatsApp. However, it has the key disadvantages that messages cannot be tailored at the individual level, longitudinal studies are impractical, and the reputational benefits of a verified business accounts are not available.

image reward at the end of the month – by making March 2021 donations publicly observable within the group. The *Peer* treatment features a message with the same contents except for the paragraph on the visibility incentive. In the *Facebook* treatment, donors are informed that their donations of March 2021 will be acknowledged broadly through the highly visited Facebook page of the organization. The *Simple ask* has a paragraph that is common to all treatments with instructions for how to schedule an appointment. For comparability with other treatments, in *Simple ask* we tie the ask to March 2021 by using a sentence that is generic for a blood collection organization that "as in every month, we are in need of blood" (see Appendix Table D.2 for English translation of treatment messages).

Groups of 20 donors that constitute the unit of randomization are constructed using one of two possible protocols: *close* groups randomly match people who typically donate at the same collection center, and *distant* groups randomly match people from all over Tuscany – a region of 3.7 million inhabitants spread over an area of 23,000 square kilometers (9,000 square miles). As an illustration, Appendix Figure C.1 plots the mailing address of members of the two median average distance *close* and *distant* groups.

In carrying out our stratified randomization, we first randomly assign each participant of the study sample to the *close* or *distant* matching protocol and execute this protocol to randomly form groups of 20. For stratification purposes, for close and distant groups separately, we create 8 partitions of groups for the three variables we aim to stratify on. These partitions vary in a $2 \times 2 \times 2$ fashion in whether they include above/below median female share, above/below median average age, above/below median past donations in the group. We randomize treatment between groups of 20 from within each partition.

Procedures. A first screening for including Avis donors in the study was done before the pre-intervention survey. We include only donors registered at one of the 65 largest local Avis centers in the region, if they are considered "active" – that is, if their last donation was done in the last 5 years – and if the latest donation took place at a blood collection center with at least 500 donors that satisfy these criteria. We exclude from the study donors that have not provided a phone number to Avis. This leaves us with 43,247 donors, a sample covering 52.08 percent of active Avis Toscana donors.

Between February 26 and March 1 of 2021, our partner invites through the official What-

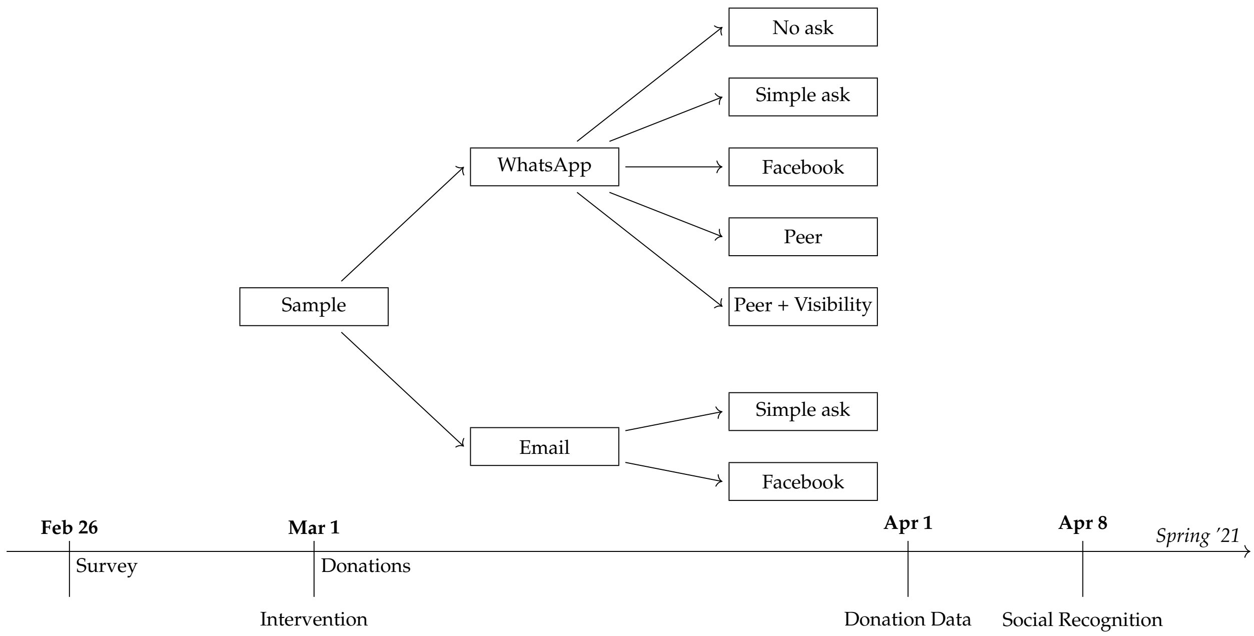

Figure 1: Design overview

sApp account of Avis Toscana this pool of donors to answer a short survey. With this invite, donors are offered a simple procedure to opt-out from research studies that Avis Toscana may conduct through this channel, by simply replying with keyword "NORICERCA" into the WhatsApp chat. The short survey, unrelated to this paper, was explicitly presented as a research study, whereas the experimental communications sent after a few days were not. We see this as a strength of our study. Natural field experiments (Harrison and List, 2004) are often considered to have greater external validity but they are also criticized on ethical grounds for not eliciting consent from study participants. Our study strikes a middle ground between eliciting consent and avoiding that behavior would be overly influenced by the subjects' perception of being part of a research study.[8] After excluding donors that are not eligible to donate and did not receive this initial message (4,041), those who opted out prior to treatment (376) and those who opted out after treatment (69), our final study sample includes 38,761 donors: 25,323 of which were assigned to being contacted via WhatsApp, 9,002 via email and 4,436 received no further message (*No ask*). In Appendix Table C.1

[8]Study participants will receive follow-up surveys and communications after the end of the one-month donation period of the study, which can of course give away that this is a research project. However, the timing of these communications preserves this as a natural field experiment for the collection of our main pre-registered outcome—March 2021 donations.

we compare observable characteristics of the population of active Avis Toscana donors (column 1) to the final study sample (column 2) to show representativeness of the latter for the former.

Over three days, from March 2 to 4, our partner sent all treatment messages out evenly spread over business hours, randomizing the order at which messages were sent to different donors.[9] We programmed a bot that handled responses from donors and automatically provided receipt of confirmation when subjects indicated preference to opt-out from the research.

On March 31 our partner solicited feedback on the degree of satisfaction for the initiative in a random sample of 10,000 subjects from treatments *Simple ask*, *Facebook*, *Peer* and *Peer + Visibility*.[10] On April 1, our partner shared with us individual-level donation data and sent a message to all donors in the *Facebook* and *Peer + Visibility* treatments to remind them that they could opt-out of their name being shared publicly next to their donation behavior in March. We take opt-out requests until April 7. On April 8, participants in the *Peer + Visibility* treatments receive tables reporting which members of their group have and have not donated in March 2021 (see e.g. Appendix Figure D.1, panel a), and our partner posted on the Facebook page of Avis Toscana the donations of donors in the *Facebook* treatments, grouping donations in 65 posts by local Avis centers (see e.g. Appendix Figure D.1, panel b).[11] On April 8, our partner also sent a short follow-up question to 1,384 subjects in the *Peer + Visibility* treatment to measure their beliefs about how likely they think it is that they know at least one member of their group, to confirm the differential social relevance of the group composition in *close* vs. *distant* groups (see panel c of Appendix Figure C.5).

Sample. Appendix Table C.1 shows that strong predictors of donation behavior, such as past donation behavior and gender (women are allowed to donate less frequently), are well

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[9]Per WhatsApp usage limits, we could reach up to 10,000 distinct contacts per day.

[10]Participants were asked for a feedback on a quantitative scale (from 0 to 10) for half of the sample and on a qualitative scale for the other half. Exact wording of the question and results are presented in Figure C.2.

[11]For each of the 65 local Avis centers, Avis Toscana made a post on its page (`facebook.com/AvisToscana`), tagging the Facebook page of the local Avis center. Avis Toscana's page itself has over 7000 followers, while followers of the pages of local Avis centers range between a few hundreds and a few thousands depending on their catchment area and social media activity. Our understanding is that most of these followers are Avis donors.

balanced across treatments. In the treatments administered via WhatsApp the share of participants that have received the message is very close to 100 percent, and about 90 percent of subjects included in the study read the message within 30 days. In comparison, the emailing opening rate is much lower, around 17 percent. Interest in graphical contents that provide visual illustration of the social rewards is relatively low and varying across treatments. Finally, we see virtually no opt-out from the research study after treatment (69 out of 38,761 participants), though dropout is somewhat larger (between 0.2 percent and 0.4 percent) in social recognition treatments administered through WhatsApp. All the results that we present in Section 2 are unchanged if we include dropouts.

1.3. Intervention 0: Social media recognition

As already mentioned, part of our main intervention presented in Section 1.2 replicates an initial intervention. This intervention was conducted in 2019 to investigates whether the prospect of social recognition on social media encourages repeat blood donors to give blood when asked. The two main treatments are comparable to the *Simple ask* and *Facebook* treatments in the experiment presented in Section 1.2, which are compared to a control group of donors who do not receive an explicit invitation to donate.[12]

Procedures. Avis Toscana donors were included in the study if they satisfied the following criteria: they had provided an email address and they were eligible to donate in November both blood and plasma. This leaves us with 15,326 donors, a sample covering 15.62 percent of active Avis Toscana donors (who donated in the 5 years prior to the experiment).

Between October 29 and October 31 of 2019, our partner sent out treatment messages via email to this pool of donors. Donors were encouraged to donate in the month of November. On December 1 we obtain donation data for the month of November, and email participants to collect their opt-in consent for sharing their name on social media posts. On December 16 Avis makes the Facebook posts to recognize the donations of donors that have given consent. The timeline of this experiment is described in Figure E.1. Online Appendix E describes this experiment in greater detail.

[12]The objectives of this experiment were slightly broader, but beyond the scope of this paper. We provide the full experimental design and set of findings for this study in Appendix E.

There are two main differences in the implementation of this intervention relative to the subsequent replications: First, experimental communications were delivered exclusively via email. Second, the randomization was clustered at a higher level–the Avis center level. We also made changes to the privacy policy for the implementation of social recognition, moving away from an opt-in policy in favor of a more inclusive opt-out and expanded the pool of study participants to donors who (i) would eligible to give *either* blood or plasma (rather than *both*) in the study period and (ii) had provided a phone number to the organization (regardless of whether they provided also an email address). Table 1 summarizes these design differences between this initial experiment and the more recent replications.

Table 1: Design differences between the initial experiment and subsequent replications

	’19	’21	’21
Randomization level	Avis center level. 67 clusters.	Twenty-donors group level. 681 clusters.	Twenty-donors group level. 677 clusters.
Privacy policy	Opt-in at the end of study period. Donors choose between revealing full name, first name only, neither.	Generic research opt-out offered at the beginning of the study period and specific privacy opt-out offered at the end of the study period. Donors choose between revealing full name with last name initial and not revealing.	
Receive emails	86.3%	77.0%	76.0%

Notes: “Receive emails” is the share of study participants who have successfully received emails from the organization in the past.

Sample. Table E.5 shows that the final sample of 15,326 donors is well balanced across treatments on age and gender. However, there are small imbalances in past donation behavior. We account for these by controlling for individual characteristics in econometric specifications. Post treatment balance tests show that the opening rate of treatment emails is very similar across treatments (Table E.6). This is important in a design with treatment randomization at the level of Avis centers, where contact lists are maintained, because it rules out differential quality of contact lists across centers as a confound.

1.4. Identifying the mechanisms of social recognition

To fix ideas, we present two simple conceptual frameworks that highlight our approach for identifying social recognition and distinguishing its mechanisms. Our premise is that social recognition entails both elements of peer comparisons and social image concerns. Peer comparison models typically resemble the following (see e.g. Bernheim, 1994; Immorlica et al., 2017; Goette and Tripodi, 2021):

$$U(D_i) = a_i D_i - c(D_i) - s\mu_i p(D_i, D_{-i})$$

where agents experience consumption utility due to potentially heterogeneous joy of giving and a private cost, and social utility that depends on the salience of social comparisons s. In this literature, the extent to which people care about social comparisons can also be heterogenous and the social comparison function $p(.)$ has been modeled in different ways. Perhaps most prominent is the distinction between models of norm adherence where people experience benefits to behave like others to fit-in $p(D_i, D_{-i}) = |D_i - D_{-i}|$, and models of status seeking where people care about being better than others $p(D_i, D_{-i}) = max(D_{-i} - D_i, 0)$.

Models of social signaling (e.g. Bénabou and Tirole, 2006) feature a similar consumption utility term. A key parameter of the social utility term is how visible the agent's good deeds are, and agents choose how prosocial to act in order to signal that they care about the positive externalities of their actions (high a).

$$U(D_i) = a_i D_i - c(D_i) - v\mu_i r(a|D_i)$$

It has been hard in this literature to distinguish between social image concerns and peer comparisons, primarily because equilibrium outcomes are sensitive to unobservable parameters. Our approach for distinguishing these two mechanisms is to test a social recognition intervention that features both visibility and peer comparisons, and experimentally manipulate visibility v (as in e.g. Andreoni and Petrie, 2004; Ariely et al., 2009) and the salience of peer comparisons s (as proposed by Cialdini et al., 1990) in a cross-randomized fashion. Instead, when we test for the effectiveness of broader social media recognition on donations, our focus is on its aggregate effect. These analyses are complemented with a survey experiment on mechanisms introduced in Section 3.

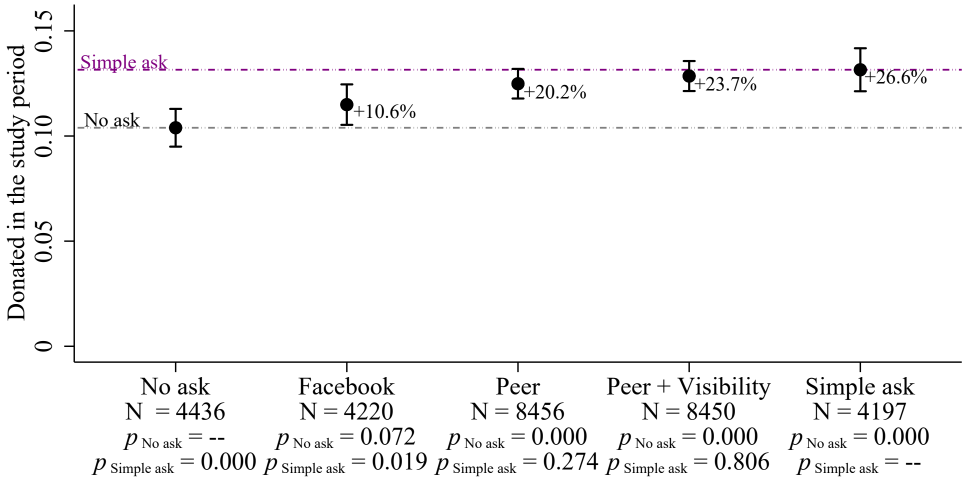

Figure 2: Response to different asks

Notes: Comparison of the share of participants who donated either blood or plasma in the one-month study period, across all the different asks implemented via WhatsApp. Capped ranges are 95 percent confidence intervals. Treatment effects estimated with regression analyses that control for individual characteristics are reported in Appendix Table C.2. For each treatment T, p_{C} denotes the p-value of the difference $\hat{\beta}_{\mathrm{T}} - \hat{\beta}_{\mathrm{C}}$, for C being either No ask or Simple ask.

2. Results

In this section we overview how different forms of social recognition affect the share of donors that makes a donation of either blood or plasma in the study period, we compare the results from a social media recognition intervention that was replicated twice after an initial implementation, we study how blood donors respond to social norm information, and we study treatment effect heterogeneity with respect to social proximity and donation history.

2.1. Social recognition and when a simple ask is enough

We begin by comparing the donation response to different asks included in the study, holding constant the communication channel—WhatsApp. In a control treatment where donors receive no experimental message from the organization the share of participants who give blood in the study period is 0.103.[13] Offering donors the prospect of being recognized on social media (*Facebook*) increased donations by 10.6 percent. Donations also increase significantly when we offer donors peer comparisons (in *Peer* and *Peer + Visibility*), by informing

[13]This is similar to the number of donations in the one-month study period, because less than 0.1 percent of participants make more than one donations in these 30 days (see Appendix Table C.1).

them of how much they donated recently relative to a random set of peers, and we find that making prospective donations visible among group members makes little difference. However, none of these social rewards are more effective than a *Simple ask*, which increases donations by 26.6 percent.[14] Mean comparisons across treatments are summarized in Figure 2. Taken together, these four treatments are estimated to generate in a month 21.3 extra donations every thousand donors, relative to the counter-factual of business-as-usual (as in *No ask*). Given the costs of sending personalized WhatsApp messages through the Business API, this campaign was cost effective at a price point of 2.21 USD per extra donation.[15]

2.2. Social media recognition

Table 2 reports the results of three experiments: an initial experiment and two conceptual replications testing how social media recognition affects blood donations. From the first experiment we estimate that simply asking and social recognition both affect donations significantly, however the results are not robust to controlling for local branch fixed effects (the level at which social media recognition was randomized)—as it can be noted from comparing columns 1 and 2.[16] Another limitation of this experiment was the poor delivery rate of experimental communications (22.62 percent), which were sent via email.

In two subsequent replications we deal with two key limitations of the initial intervention. For both replications, we improve the quality of the randomization by varying treatment assignment *within* branches. Between replications, we vary the communication channel

[14]One could be concerned that the *Simple ask* is more effective because more concise, and that for that reason more likely to be read. If message length drives the effectiveness of different asks, then we should also expect the longest treatment message – *Peer + Visibility* – to lead to less donations than the second shortest message – *Facebook*. A t-test based on parameter estimates from column 3 of Appendix Table C.2 rejects this one-sided null hypothesis ($p = 0.004$).

[15]This compares e.g. to the estimated 50 USD per extra donation from cold calling repeat donors in Bruhin et al. (2015).

[16]The initial experiment randomized treatment assignment at the cluster level to avoid treatment contamination, whereby fellow donors from the same center may discuss the research study and the different treatments they were exposed to. As we expand on in Online Appendix E, randomizing treatment across only 67 clusters of varying size led to imbalances in the distribution of donor characteristic. From this first experiment, we observe that intra-class correlation of donations in the study period is sufficiently low (ICC=0.016) to rule out any meaningful scope for contamination within branch, which gives us confidence in an alternative design choice that varies social recognition across donors of the same branch.

Table 2: Facebook experiments

	(1) ’19	(2) ’19	(3) ’21	(4) ’21	(5) ’21	(6) ’21
	Baseline category: No ask					
Simple ask	0.014*	0.025***	0.016**	0.014**	0.028***	0.027***
	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
Facebook	0.017**	0.008	0.009	0.010	0.012*	0.012*
	(0.007)	(0.008)	(0.007)	(0.006)	(0.007)	(0.007)
Donors’ observables	Yes	Yes	Yes	Yes	Yes	Yes
Local branch FE	No	Yes	No	Yes	No	Yes
Observations	14993	14993	13438	13438	12853	12853
Clusters	67	67	681	681	677	677
R2	0.060	0.069	0.050	0.056	0.055	0.064
Opening rate	22.62%	22.62%	17.21%	17.21%	90.63%	90.63%
Facebook - Simple ask	0.003	-0.017	-0.007	-0.005	-0.016	-0.015
$\hookrightarrow$ p-value	0.768	0.122	0.321	0.481	0.023	0.026

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Treatment effects estimated using a linear probability model, where the dependent variable indicates whether the subject donated either blood or plasma in the study period—March 2021. *Simple ask* and *Facebook* are binary treatment indicators. Donors’ observables include: age groups (18-38, 39-51, 52+), gender and past donations. Standard errors in parentheses are clustered at the level of the unit of randomization: for the 2019 Email experiment (columns 1 and 2) we cluster at the local branch level; for the 2021 experiments (columns 3-6) we cluster at the 20-donors group level. All columns estimate the model for all blood donors in treatments *No ask*, *Simple ask* and *Facebook*.

while holding everything else constant.

We find that changing the communication channel, from email to WhatsApp, had a very strong impact on engagement: the delivery rate of treatment messages rose from 17 to 91 percent. Both in the second and in the third experiment we estimate a positive and significant intention-to-treat effect of a *Simple ask* on donations. The effect is larger, though not significantly so, in the WhatsApp experiment (t-test, $p = 0.135$), and in both experiments we find that making donations more visible tends to backfire. In the third experiment, where much greater engagement improves the scope for our intervention to influence donation behavior, we identify a significant crowding out effect of social recognition ($\hat{\beta}_{Facebook} - \hat{\beta}_{Simple\ ask} = -0.015$, $p = 0.026$). This is a surprising result in light of the existing literature, which we summarize and discuss in a meta-analysis presented in Appendix A. An obvious explanation for the finding could be that donors specifically dislike the way social recognition was implemented via Facebook channels and stop donating to express such distaste. We rule out this explanation with a sentiment analysis. On March 31 of 2021 we surveyed a random sample of donors who took part in the 2021 study, immediately at the end of donation window: Appendix Figure C.2 shows that sentiment towards treatment communications is very and similarly favorable in the *Simple ask* and *Facebook* treatments. An explanation, based on social signaling, that accounts for this finding is that people shy away from activities that can make them appear image concerned. We come back to this point in Section 3, where we provide direct evidence for this signaling mechanism and rule out further alternative explanations.

2.3. Peer comparisons

Using data from the *Peer* and *Peer + Visibility* treatments we can study systematically how random exposure to different social norms, captured in the experiment by average past donations by fellow group members, can shape behavior (see Appendix Figure C.3 for the distribution of social norms that participants are exposed to). In Appendix Table C.3 we estimate the effect of social norms on giving using two complementary approaches. First, we use a model where social norms are assumed to linearly affect behavior. Because the distribution of social norms is not uniform, this specification is relatively sensitive to extreme values of the social norms. Second, we use a model where social norms are discretized into quintiles.

We find no evidence that donors respond linearly to social norms (Appendix Table C.3, columns 1 and 4). Qualitatively, the analysis with discretized social norms is suggestive that donations respond non-monotonically to social norms with a peak at the second quintile. However, an F-test does not rule out the null of equal treatment effects from receiving information of social norms from the different quintiles ($p = 0.116$).[17] A concern with these analyses could be that the lack of peer comparison effects are due to limited variation on the social norm that is observed. Appendix Figure C.4 shows that peer effects are no different even when we compare effects of being exposed to saliently different social norms, say 0.75 and 1.75 average past donations, making this interpretation unlikely.

[17]Similarly, with a coarser discretization of the social norm in above and below median, we still don't find any effect of peer comparisons on donations (Appendix Table C.3, columns 3 and 6).

2.4. Heterogeneity

Social proximity. Using experimental variation in group composition we study how social proximity causally moderates social norm adherence and visibility concerns. This can be done using data from treatments *Peer* and *Peer + Visibility*, where there is common knowledge about group composition: a random half of donors assigned to these treatments are matched to fellow donors from the same donation center (*Close*) while the other half are matched to donors scattered across Tuscany (*Distant*).

Before proceeding with the main analysis, we collect evidence to demonstrate that the experimental variation in geographical proximity that we introduce meaningfully affects social proximity perceived by subjects. The week after the intervention we texted on WhatsApp a sample of 1384 donors from the *Peer + Visibility* treatment to ask how likely they thought it was that they knew at least one member of their group.[18] Panel c of Appendix Figure C.5 shows that donors are almost twice as likely to believe that they know someone from their group if they are assigned to a *Close* group ($p < 0.001$). In Panels a and b we also provide further supporting evidence. We show that donors are more likely to provide unsolicited responses to the treatment message if they are in *Close* treatments, and conditional on responding they write more. From reading what subjects write, these effects seem driven by an increased likelihood and length of excuses that are made for not being able to donate—which is in line with the in the interpretation that perceived social proximity with fellow group members increases the relevance of peer comparisons.

Moving to the main analysis, in Appendix Table C.4 we estimate linear probability models that interact social proximity (assignment to a Close or Distant 20-donors group) with individual elements of social recognition that our intervention is designed to identify. In Panel A we ask whether social proximity increases visibility concerns towards fellow group members, and we find no evidence for such causal moderation ($\beta = 0.005$, $p = 0.593$). In Panel B of we ask whether social proximity strengthens norm-adherence, as it is often found in studies where social proximity is not randomly assigned (e.g. Topa, 2001; Leider et al., 2009; Bond et al., 2012; Goette and Tripodi, 2021; Bicchieri et al., 2022). We estimate

[18] We sent out this survey after subjects had received the list of names of their group members. Subjects were asked to provide a probability in percentage points from 0 to 100 that they knew at least one person from their group. We obtained 895 valid responses.

an interaction effect between the binary indicator for social proximity *Close* and the social norm to not be statistically different from 0 ($\beta = 0.004$, $p = 0.836$). In column 3, we also estimate the effect of being exposed to social norms from different quintiles of the distribution and find no evidence of substantial heterogeneity. Taken together, these results paint social proximity as a weak moderator of social influence.

Past donation frequency. Do more frequent donors respond differently to social recognition interventions? Numerous studies find that past donor activity predicts not only future activity, but also the strength in the response to various forms of appeals (see e.g. Landry et al., 2010; Lacetera et al., 2014; Goette and Stutzer, 2020). Our experimental design allows us to study for which appeals more active donors display a stronger response and whether they display stronger or weaker norm-adherence.

In Appendix Table C.5 we estimate linear probability models with an interaction term for frequency of past donations (whether the donor made an above median number of donations in the 11 months prior to the intervention) in our analyses of the response to individual treatments as well as norm adherence. Panel A shows that response to all types of asks (other than the *No ask* control) is slightly stronger for frequent donors, but point estimates for these interaction terms are not statistically significant at the 5 percent level. Panel B shows that norm adherence is consistently weak both for frequent and infrequent donors.

3. Survey experiment for blood donors' assessments

We conduct a follow-up survey experiment with donors who participated in our main study. We see this sample selection as a natural one both because Avis donors are the target population of the policy and because social recognition via Facebook is implemented on pages that are primarily subscribed by fellow blood donors. This survey leverages the blood donors' intimate knowledge of the study setting to shed light on the backfiring effects of social media recognition and tie a seemingly surprising result with the body of existing evidence discussed in Appendix A. We do so in four steps: i) we measure beliefs of the primitives of the environment that affect social signaling equilibria, ii) we elicit predictions of the treatment effects estimated for the main intervention, iii) we elicit the inferences blood donors make about unobservable characteristic of peers who give blood with and without social recognition, and iv) we gather qualitative evidence of what repeat donors believe are the channels through which social media recognition encourages giving.

3.1. Design

Survey items:

1. Perceived distribution of altruism; probabilistic beliefs over a 4-type distribution
2. Perceived distribution of image concern; probabilistic beliefs over a 4-type distribution
3. Predicted treatment effect of social media recognition; most likely sign of the treatment effect of *Facebook* relative to *Simple ask*
4. Behavioral motives for social media recognition to encourage or discourage donations
5. Perceived altruism and image concern type of repeat donor who gives blood following the treatment communication

There are four versions of the survey, which we assign randomly, each with a distinct set of questions to minimize confusion and survey length. Importantly, for survey items 1 to 4 we vary between subjects the population about which beliefs are elicited (either repeat blood donors or the general population). For survey item 5, we vary between subjects the experimental treatment about which beliefs are elicited and provide full description of the exact treatment messaging. The four versions of the survey are summarized in Figure 3.

Implementation. A random sample of 20000 blood donors from the initial experiment were invited to take part in the survey on August 19. The survey ran for a week, during which we collected 3016 complete responses. This sample is representative of the initial experimental sample with respect to age and gender and it over-represents more engaged donors of the organization (Appendix Table C.6). The sample also has similar coverage of all treatments, with the exception of the *Peer + Visibility* treatment (Appendix Table C.1).[19]

[19] Participants in this treatment could be less willing to answer this survey for they already received the highest number of follow-up messages after the one-month donation period.

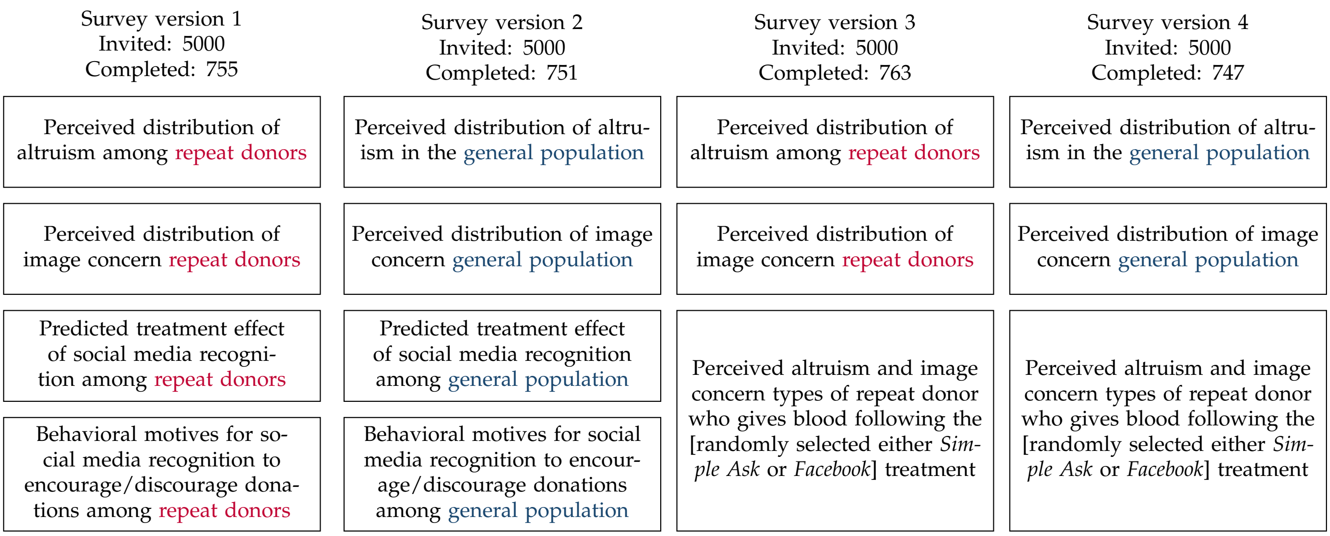

Figure 3: Design overview

The survey experiment and main hypotheses were pre-registered and full description of the materials and procedures can be found in Online Appendix F.

3.2. Experimental results

In a population where altruistic preferences are heterogeneous, charitable activities can provide positive recognition utility as they signal altruism. However, agents may differ in the degree to which they care about being seen altruistic by others and may shy away from public displays of altruism to avoid being perceived as image concerned. The net effect of social recognition interventions on the total supply of charity is generally ambiguous, and more likely to be positive when the signaling tends to concentrate on the - desirable - altruistic trait. In Appendix B we provide simulations to illustrate result from Bénabou and Tirole (2006) for how increasing visibility of donations can backfire in the presence of heterogeneity in image concern. In the rest of this section we provide evidence in support of this model and assess the importance of alternative explanations.

Perceived distribution of model primitives. Participants expect a flatter distribution of altruistic types in the general population than among repeat donors and, conversely, a flatter distribution of image concern types among repeat donors than in a general population sample (Figure 4, panels A and B). This shows relatively little scope for signaling altruism for repeat donors – 77.3 percent of them are considered to be either somewhat or very altruistic – and is consistent with a signaling interpretation whereby signaling of a desirable trait (altruism) is overshadowed by signaling of an undesirable trait (image concern).

Predictions of experimental results. Panel C of Figure 4 shows that the majority of participants (40.5 percent) predict that the most likely outcome of the social recognition intervention administered among repeat blood donors is for it to have no effect on donations. A negative effect is deemed more likely (33.0 percent) than a positive effect (26.5 percent). In comparison, a positive effect is predicted to be the most likely scenario when a similar intervention is administered to the general population. By matching survey responses to treatment assignment in the initial experiment, we can investigate whether these predictions merely reflect self-serving beliefs of donors who took part in the *Facebook* treatment, to justify their lack of donation. The evidence presented in Appendix Figure C.5 shows that this is not the case: a positive effect of the intervention is deemed more likely in general population samples regardless of initial treatment assignment.

Inference. Modal beliefs on the type of repeat donor who give blood following the treatment message are consistent with the decision to donate signaling both lower altruism and higher image concern when social recognition is available. As shown in panel d of Figure 4, participants expect the modal altruism type of their peers who give in the study period to be 3.470 in *Simple ask* and 3.051 in *Facebook* ($p < 0.001$), both being at least *somewhat* altruistic. The modal image concern type is expected to drop from 3.017 in *Facebook* to 2.669 in *Simple ask* ($p < 0.001$), corresponding to a level between *not very* and *somewhat* image concerned.

Perceived motives and alternative explanations. We asked participants to separately consider the potential reasons why we may observe a positive or negative effect of the social media recognition intervention, and different participants make these evaluations depending on whether the intervention is rolled out among repeat donors or the general sample. Unless otherwise noted, we aggregate these responses in this discussion (but report disaggregated numbers in Appendix Figure F.1). Among the reasons why we may find a *positive* effect, we allow for an open answer and include three explicit options often discussed in the literature: that donations on social media can motivate (a) people who seek to inspire others to donate, (b) people who seek to be seen as prosocial, and (c) people who may otherwise forget. 39.2 percent of respondents select (b) as the main reason, followed relatively closely by (a) that is selected by 30.2 percent of respondents. Participants expect that impressing others, by demonstrating to be an altruist, is relatively more important in the general population than it is for repeat donors – who themselves care more about inspiring others to donate. Among the reasons why we may find a *negative* effect, we also allow for an open answer and include three explicit options often discussed in the literature: that recognizing donations on social media can discourage (a) people that are privacy concerned, (b) people who worry that their donation may signal image concern, and (c) people who may feel controlled or manipulated. The majority of respondents (50.5 percent) select (b) as the main reason.

Taken together the findings of the survey experiment confirm the conjecture, based on social signaling theory, that the distribution of unobservable traits in the population to which social recognition is offered can determine whether such policy is beneficial or detrimental to the supply of good citizenship. We provide evidence that repeat donors are able to anticipate the effects that we estimate from our experimental intervention, that they are able to reconcile our findings with the findings from populations that are not selected along the positive traits on which a signaling opportunity is offered, and that they expect the primitives of a social signaling model to be consistent with greater scope among blood donors for signaling image concern. Qualitative perceptions of the main crowding out channels confirm the social signaling interpretation that we propose and rule out prominent alternative explanations that would otherwise be empirically indistinguishable with our data, such as privacy concerns (e.g. Goldfarb and Tucker, 2011) and aversion to control systems (Ellingsen and Johannesson, 2008).

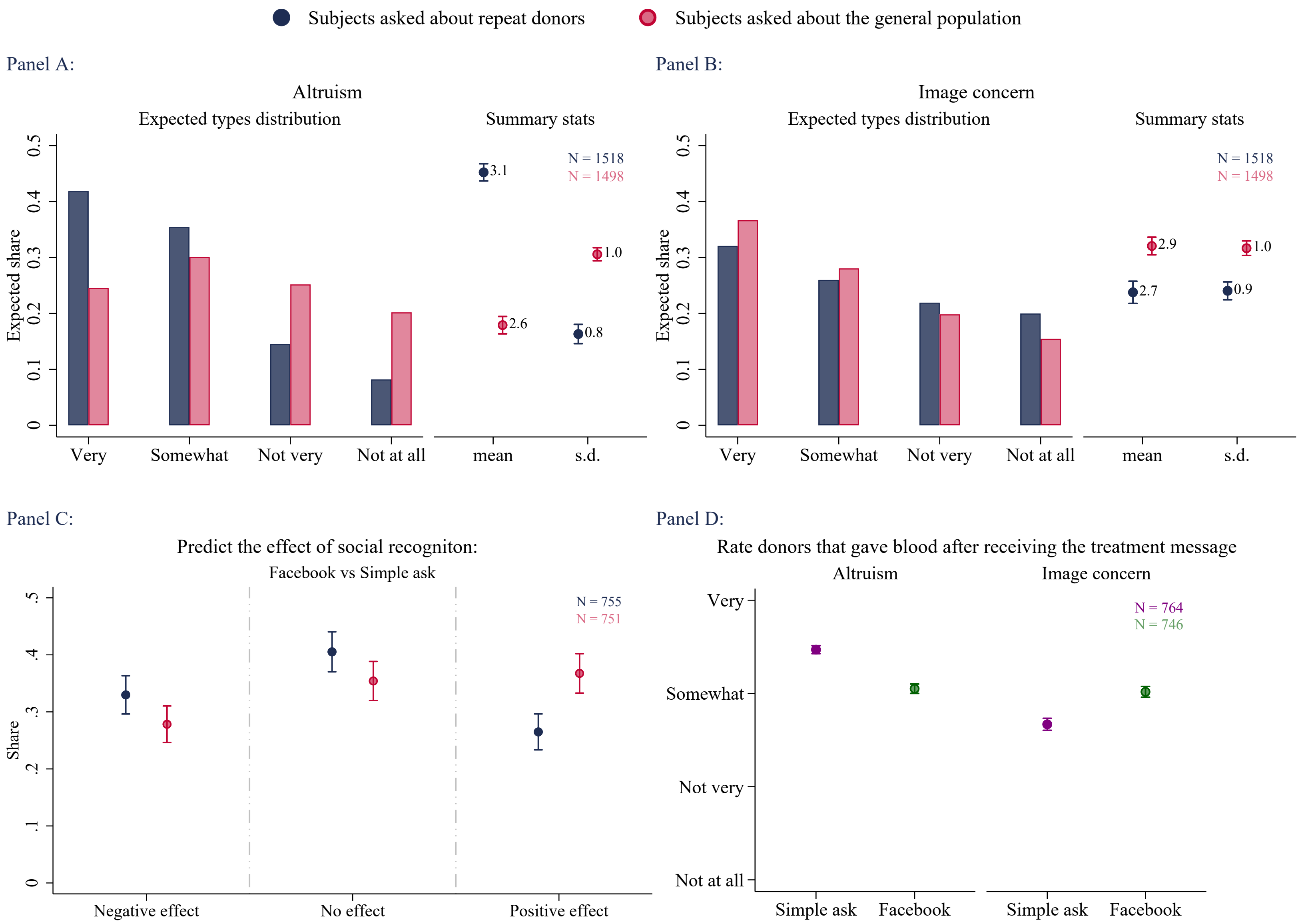

Figure 4: Overview of results from the survey experiment

Notes: Panel A reports the average perceived distribution of altruism, among repeat blood donors and in the general population, along with the mean and standard deviation of such distribution. Panel B reports the average perceived distribution of image concern, among repeat blood donors and in the general population, along with the mean and standard deviation of such distribution. Panel C reports the share of respondents predicting that the effect of the social media recognition intervention, among repeat blood donors and in the general population, will be either negative, null, or positive. Panel D reports the modal types that respondents attribute to repeat donors who give blood during the study period; some respondents (N=764, in purple) are asked to judge donors in treatment *Simple ask* while other respondents (N=746, in green) are asked to to judge donors in treatment *Facebook*. For all panels, error bars are 95 percent confidence intervals.

4. Local and inter-temporal spillovers

Having shown that a simple ask goes a long way and that social recognition does not achieve the policy objectives set by the intervention, this section addresses questions that readers might have about spillover effects. While our intervention was optimized for testing the short-term effects of social recognition and provide clear tests of mechanisms, the empirical setting raises at least two important questions for policy. Are the effects of similar interventions over-estimated by ignoring the congestion effects they can generate? Do such interventions lead to inter-temporal substitution of donations? As we explain in turn, we can offer a valid answer to the first but not to the second question.

Congestion. One concern is that the treatment effects identified against a passive control group of donors (*No ask*), who do not get asked to donate, may at least in part capture negative spillovers of the intervention on these donors.[20] This is a setting with potential capacity constraints, which may lead to congestion, crowding out the donations of registered donors who were not in a treatment group that explicitly asked them to donate and suddenly find it harder to schedule an appointment.[21]

To address this concern, we leverage a Difference-in-Differences approach that compares the behavior of donors from different branches of the organization. Of the 160 local branches of AVIS within Tuscany, only 65 were included in our study. Excluded branches are of course not identical to included branches, but serve as a useful comparison for they rely on different operators to schedule appointments for affiliated donors and on different blood collection centers. We compare individual monthly donations between included and ex-

[20]Institutional features of our setting exclude the kind of displacement effects, found in Lacetera et al. (2012, 2013), from attracting donors who would have given blood elsewhere absent the intervention. In Tuscany, blood collection is organized by blood donors associations and executed by blood collection centers at hospitals. However, blood donors cannot simultaneously be affiliated to more than one association and all their donations are attributed to the organization to which they belong irrespective of whether they schedule their donation appointment with the organization. Therefore, all donations from donors in our study sample are accounted for in our estimations.

[21]Donors that are asked to donate will call the local branch to schedule an appointment. This can make it harder for other donors of the same local branch of Avis (the organization), who would have spontaneously called, to schedule an appointment. At the blood collection center level (the hospital), donors taking up the most sought-after slots can increase the hassle costs of giving for others.

cluded branches over time, and we identify changes relative to parallel trends that can be attributed to our intervention. More formally, we estimate the following model:

$$Blood_{ict} = \gamma_c + \lambda_t + \sum_{\tau=-24}^{-1} \delta_\tau D_{c\tau} + \sum_{\tau=0}^{2} \delta_\tau D_{c\tau} + X_{ic} + \varepsilon_{ict} \tag{4.1}$$

which is a two-way fixed effect model with 24 lags and 2 leads, and include as control variables age and gender. The outcome is monthly donations from individual i, affiliated with local chapter c, in month t. We allow for correlation of the error term within c. Figure C.7 reports raw data for average monthly donations over time in the local chapters excluded from the intervention, in the treatment groups that receive no ask to donate and in treatment groups that receive an explicit ask to donate in this intervention. In Appendix Figure C.8 we report the leads and lags from Equation (4.1) estimated in two separate samples: Panel a, pools the *No ask* treatment from the local chapters included in the experiment and the excluded local chapters and shows an estimate for $\delta_0 = -0.001$ ($p = 0.908$); Panel B, pools all other treatment messages administered via WhatsApp at the local chapters included in the experiment and the excluded local chapters and shows an estimate for $\delta_0 = 0.019$ ($p < 0.001$). Taken together these findings rule out that congestion affects donors in *No ask* and confirms that solicitations administered through WhatsApp lead to a net increase in donations.

Inter-temporal substitution. Did the intervention affect overall donations or did it simply cause donors to shift earlier an activity that was planned for a later date at the expense of future donations? Unfortunately, this experiment is ill suited to study longer term effects; as we explain in experimental procedures, different treatments receive different messages after the end of the one-month donation window, which were necessary for ethical reasons or to delve into mechanisms. These messages could serve as reminders to donate and confound the analysis of longer term effects. To substantiate these concerns, in Appendix Table C.7, we first assume away this confound and estimate what would be the effect of the different ask treatments on the number of donations an individual makes in the two months following the intervention. We then estimate a similar model that includes an indicator for who receives a message, that was sent to a random sample of study participants on March

31, asking to share the degree of appreciation for the treatment message they were exposed to. This message alone increases donations significantly and illustrates the nature of the confound. Columns 7 and 8 replace this indicator with a count variable for how many messages a participant received after the donation window of the study. While this variable is correlated with treatment, in that some treatments receive more messages (*Peer+Visibility* receives the most), these regressions further indicate that we should abstain from interpreting longer term effects of this intervention.

5. Conclusions

Does social recognition motivate repeat contributors? We study this question in a natural setting using a series of experiments embedded in the regular activities of a blood donor association. We find no evidence that social recognition motivates repeat blood donors on the targeted margins any more than a simple ask. A more widely visible form of recognition on social media leads to a crowding-out of donations relative to a simple ask. How does this happen?

We provide evidence for an under-documented implication of models of social recognition, that publicity can backfire if people are concerned about appearing image concerned (Bénabou and Tirole, 2006). This mechanism can trump the classical signaling of good traits especially in settings where a one-off public action is less informative of their altruism — as is the case of repeat donors with an established reputation of good citizens. A follow-up survey experiment offers several pieces of evidence supporting the interpretation that repeat donors are less concerned about signaling altruism than they are about not being perceived as image seeking. Our survey also shows that the way social recognition plays out across empirical settings can explain why – in contrast to the existing literature – we observe crowding-out. This is good news for theory, when we think about external validity, because it showcases how economic models can guide researchers and practitioners in predicting when similar interventions are likely to fail. It is bad news for policy, because incentivizing *good citizenship* with recognition turns out to be harder than we may have thought.

Organizations and practioners are often left to translate proof-of-concept policy tools into actual policy. Studies like ours fill an important gap between these two steps, a space where we think academics can do more to identify the limits of such policy tools — following the guidance of our models. Understanding who responds to different incentives is of primary importance for the targeting of specific policies. This paper tackles this question with a static theoretical framework (Bénabou and Tirole, 2006) in a classical empirical application (Guiso et al., 2004). More research can shed light on the dynamic consequences of these findings.

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A. Social recognition and good citizenship: Meta-analysis

To put our evidence into context, we conduct a selective meta-analysis of the field experimental literature on social recognition and good citizenship. Studies included in this meta-analysis are summarized in Table A.1, where we also remark design features that may contribute to explaining differences in experimental findings: First, we point out differences in the composition of the audience observing the act of good citizenship, which ranges from individual observers, to peers, to broader populations. Second, we mark studies that include an active control group that allows to rule out simple reminder effects that may bring an activity top of mind and act as a confound. Third, we mark studies where participants are asked to do an activity at a specific point in time versus studies where there is more flexibility.[22] Our study does not systematically differ in these characteristics from studies that report significant positive effects.

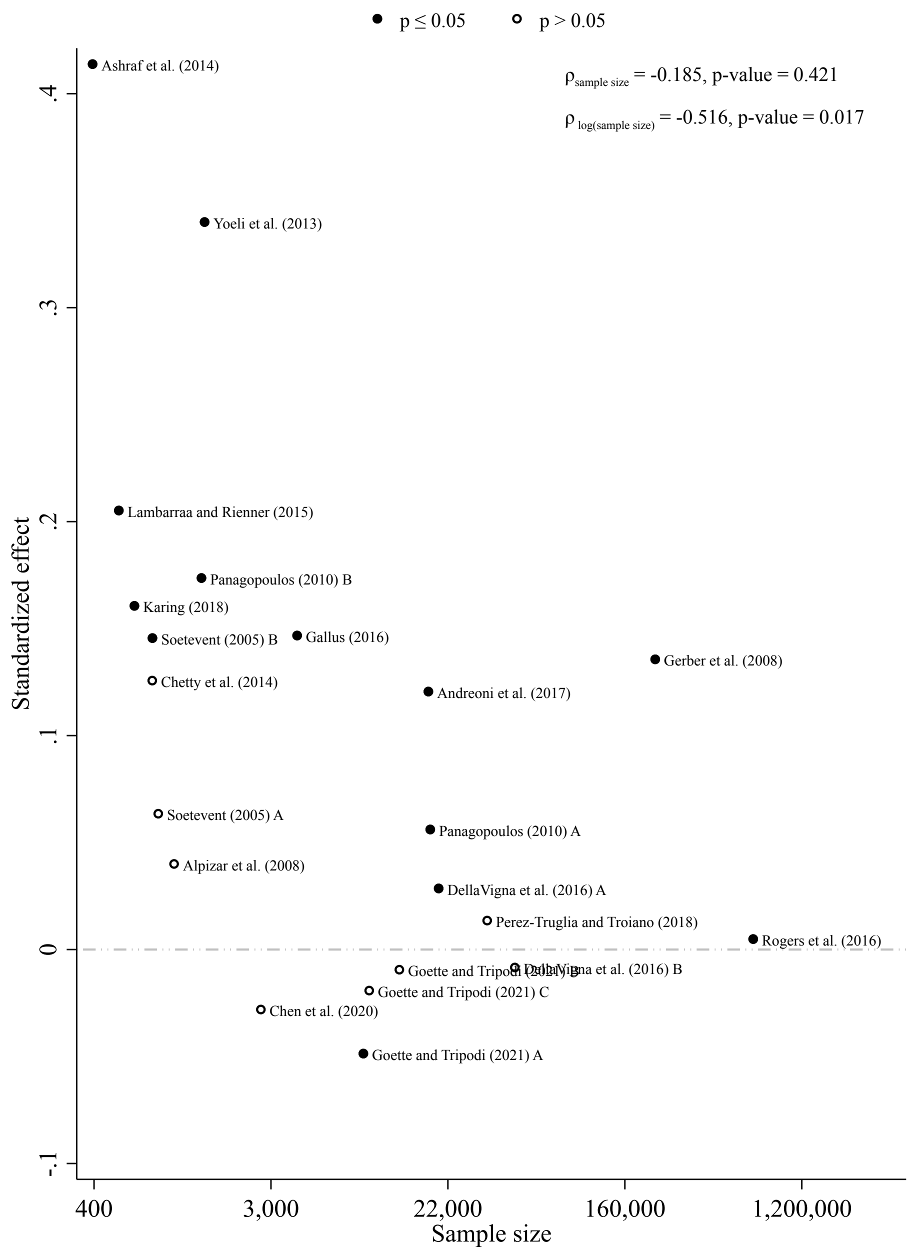
In Figure A.1 we plot standardized effect size and sample size across studies, from which we make two observations. First, we observe a negative correlation between standardized effect size and sample size, which is in line with other meta-analyses (DellaVigna and Linos, 2022). Concerns about publication bias would suggest that the effects of social recognition may be smaller than meta-analytic effects obtained with standard methods, but not zero – because large studies still find the effects of social recognition on good citizenship to be positive. Indeed, we estimate an unweighted meta-analytic effect of 0.116. A meta-analytic effect that takes into account potential publication bias (Andrews and Kasy, 2019) is estimated to be 0.076 (95% CI: [0.029, 0.123]). Second, ours is the only study to provide evidence that social recognition can have negative effects on targeted good citizenship outcomes.[23] We view this as an important and surprising result that should spur interest in understanding how to harness social recognition.

[22]See Andreoni and Serra-Garcia (2021) for a discussion of the negative interactions between time-inconsistency and willingness to help others.

[23]In an internal replication of the social recognition effects on voting in presidential elections, DellaVigna et al. (2016) report small and imprecise negative effects. Lambarraa and Riener (2015) finds that charitable donations decrease with visibility in a cultural setting with a strong religious prescription of anonymous giving. We flip the sign of this effect for a 'good citizen' in Islam is one who gives money to charity privately.

Table A.1: Experimental field evidence of social recognition for good citizenship

Article	Description	Social Recognition Audience	Salience control	Now-or-Never Design
Alpizar et al. (2008)	Charitable giving. Voluntary contributions increase by 2 p.p. ($p = 0.264$) when anonymity is not granted, relative to the anonymous condition.	Solicitor	Y	Y
Andreoni et al. (2017)	Charitable giving. Givers increase by 66 percent ($p < 0.01$) and amount donated increases by 90 percent, ($p < 0.01$) when they cannot avoid the ask of the offer, relative to control where people can avoid the ask.	Solicitor and those around	Y	Y
Ashraf et al. (2014)	HIV prevention. Among agents recruited to sell condoms, the share selling at least one pack increases by 11 p.p. ($p < 0.1$) and the average number of packs sold is twice as large ($p < 0.01$) when sellers are granted social recognition, relative to a control group that receives nothing.	Clients, public ceremony	Y	N
Chen et al. (2020)	Wikipedia contributions. Share of economics professors accepting to make Wikipedia contributions to topics that are likely citing their work falls by 1.5 p.p. when they are prospected social recognition, compared to a control group without recognition.	Visitors of the project's Wikipedia page	Y	N
Chetty et al. (2014)	Review times. Referees work faster ($p = 0.029$) reducing the median review time by 2.3 days when they are prospected social recognition, compared to a control receiving the standard request.	Peers	Y	N
DellaVigna et al. (2016)	Voting. In 2010 congressional elections turnout increases by 1.4 p.p. (one-sided $p = 0.06$) when voters are prospected to be asked about turnout, relative to a control group without this information. In 2012 elections the increase is 0.1 p.p. (one-sided $p = 0.405$).	Solicitor	Y	Y
Gallus (2017)	Wikipedia contributions. New contributors are 7 p.p. ($p < 0.01$) more likely to remain active in the following month when they receive social recognition, relative to a control group receiving nothing.	Visitors of the contributor's profile	N	N
Gerber et al. (2008)	Voting. Turnout increases by 5.6 p.p. ($p < 0.01$) when households are promised social recognition, relative to control group that is not.	Household and neighbors	Y	Y
Karing (2018)	Childhood immunization. The number of children completing the required vaccines in the first year increases by 9.8 p.p. ($p < 0.05$) when social recognition is granted, relative to control group where this information remains private.	Anyone who sees the child's bracelet	Y	N
Lambarraa and Riener (2015)	Charitable giving. Givers decrease by 9 p.p. ($p = 0.054$) and amount donated falls by 6.8 percentage ($p = 0.133$) when people are not granted anonymity, relative to a control group with anonymity ensured.	Attendees of a board event	Y	Y
Panagopoulos (2010)	Voting. Turnout increases by 2 p.p. (one-tailed $p < 0.05$) when people are promised social recognition in case they vote, relative to a control group receiving nothing. Turnout increases by 6.9 p.p. (one-tailed $p < 0.01$) when people are instead promised social recognition in case they do not vote, relative to the same control group.	Readers of a local newspaper	Y	Y
Perez-Truglia and Troiano (2018)	Tax compliance. Likelihood of compliance among tax delinquents in the first quartiles of initial debt increases by 2.1 .p. ($p < 0.01$) when they are exposed to social pressure, compared to a control group not exposed to social pressure.	Drawn households in the area	Y	N
Rogers et al. (2016)	Voting. Turnout increases by 0.22 p.p. ($p < 0.05$) when people are prospected the possibility of being asked about turnout, relative to control group that has no information.	Surveyors	Y	Y
Soetevent (2005)	Charitable giving. Proceeds increase by 10.1 percent ($p < 0.01$) when offers are collected by means of an open basket rather than a closed bag.	Those sitting close by	Y	Y
Yoeli et al. (2013)	Energy conservation. Participation to an energy conservation program increases by 5.8 p.p. ($p < 0.01$) when social recognition is granted, relative to a control group without it.	Neighbors	Y	N



Alpizar et al. (2008). T: Non-anonymous voluntary contribution ($N = 502$). C: Anonymous voluntary contribution ($N = 495$).

Andreoni et al. (2017). T: Impossibility to avoid ask of charitable giving ($N = 8896$). C: Possibility to avoid the ask of charitable giving ($N = 8766$).

Ashraf et al. (2014). T: Social recognition for sale of condoms ($N = 185$). C: No social recognition for sale of condoms ($N = 212$).

Chen et al. (2020). T: Social recognition for Wikipedia contribution ($N = 1,329$). C: No social recognition for Wikipedia contribution ($N = 1,315$).

Chetty et al. (2014). T: Review times publicly posted ($N = 347$). Standard review request ($N = 432$).

DellaVigna et al. (2016) A. T: Informed about subsequent survey regarding turnout for 2010 elections ($N = 9,039$). C: Simple reminder of upcoming 2010 elections ($N = 10,805$).

DellaVigna et al. (2016) B. T: Informed about subsequent survey regarding turnout for 2012 elections ($N = 23,436$). C: Simple reminder of upcoming 2012 elections ($N = 23,501$).

Gallus (2017). T: Public award for being a new Wikipedia contributor ($N = 1,617$). C: No award for being a new Wikipedia contributor ($N = 2,390$).

Gerber et al. (2008). T: Peer information and image pressure to vote at the 2006 elections ($N = 38,201$). C: Simple reminder about the 2006 elections ($N = 191,243$).

Goette and Tripodi (2021) A. T: Blood donors contacted via WhatsApp are prospected social recognition on Facebook ($N = 4,234$). C: Blood donors contacted via WhatsApp receive a simple ask to donate ($N = 4,219$).

Goette and Tripodi (2021) B. T: Blood donors contacted via WhatsApp are prospected social recognition vis-a-vis other donors ($N = 8,476$). C: Blood donors contacted via WhatsApp receive a simple ask to donate ($N = 4,219$).

Goette and Tripodi (2021) C. T: Blood donors contacted via email are prospected social recognition on Facebook ($N = 4,544$). C: Blood donors contacted via email receive a simple ask to donate ($N = 4,473$).

Karing (2018). T: Bracelet informative of child's vaccination status ($N = 319$). C: Bracelet uninformative of child's vaccination status ($N = 318$).

Lambarraa and Riener (2015). T: Non-anonymous charitable donations ($N = 269$). C: Anonymous charitable donations ($N = 265$).

Panagopoulos (2010) A. T: Social recognition on newspaper for voters in 2007 general elections ($N = 2,951$). C: Simple reminder about 2007 general elections ($N = 15,090$).

Panagopoulos (2010) B. T: Social recognition on newspaper for non-voters in 2007 general elections ($N = 674$). C: Simple reminder about 2007 general elections ($N = 685$).

Perez-Truglia and Troiano (2018). T: Tax delinquents exposed to social pressure ($N = 171,79$). Tax delinquents not exposed to social pressure ($N = 17,155$).

Rogers et al. (2016). T: Informed about a possible subsequent survey regarding turnout for 2010 elections ($N = 347,054$). C: Simple reminder of upcoming 2010 elections ($N = 346,929$).

Soetevent (2005) A. T: Open basket to gather donations for the parish during service ($N = 406$). C: Closed bag to gather donations for the parish during service ($N = 428$).

Soetevent (2005) B. T: Open basket to gather donations for an external cause during service ($N = 380$). C: Closed bag to gather donations for an external cause during service ($N = 401$).

Yoeli et al. (2013). T: Participation in an energy preservation program is observable. C: Participation in an energy preservation program is unobservable.

Figure A.1: Selected meta-analysis of field experimental evidence for social recognition on good citizenship

Notes: Standardized effects are calculated as the ratio between the difference in means and the standard deviation of the control group. P-values are obtained from t-tests for the equality of means. For Ashraf et al. (2014), where the standard deviation of the outcome in treatment and control is not reported, we use the standard deviation of the overall sample. In Yoeli et al. (2013), a breakdown for the number of observations by treatment is not reported; we assume treatment groups to be equally sized. Meta analytic effects are estimated, excluding the present study. Unweighted: 0.116; Weighted by sample size: 0.021; Weighted by $\frac{1}{\sigma}$: 0.055; With publication bias correction (Andrews and Kasy, 2019): 0.076.

B. Simulating the backfiring effect of social recognition

In this section we use simulations to demonstrate a feature of the Bénabou and Tirole (2006) model (general argument presented on p. 1665 of the original manuscript), that heterogeneity in image concerns can make donations non-monotonic in the extent to which actions are visible.

In a model with homogenous image concern (B.1), i's decision to donate depends on her private costs of donating $c(d)$, how much she values the benefit that her donation generates for society (a_i, which is private information) and how generous those who can observe her actions perceive her to be $E(a|d)$. This social recognition utility only plays a role if actions are visible ($v > 0$) and is proportional to visibility v.[24]

$$U_i(d) = \begin{cases} a_i - c + vE(a|d=1) & \text{if } d = 1 \\ vE(a|d=0) & \text{if } d = 0 \end{cases} \tag{B.1}$$

In a model with heterogenous image concern (B.2), image concern ν_i (that was normalized to 1 for homogenous image concern) is private information. Agents want to appear altruistic (signal high a) while not appearing like they donate *to be seen* altruistic (signal low ν).

$$U_i(d) = \begin{cases} a_i - c + v[\nu_i E(a|d=1) - E(\nu|d=1)] \\ v[\nu_i E(a|d=0) - E(\nu|d=0)] \end{cases} \tag{B.2}$$

In Figure B.1 we fix the primitives of the model and characterize all symmetric equilibria for the homogenous and heterogeneous image concern cases, for levels of visibility v on a fine grid between 0 and 1.[25] We assume that when no one donates, there are no signaling benefits of deviating. In panel b, we can see that when donations are private ($v = 0$) only very altruistic agents donate (equilibrium I). As visibility increases, eventually also the

[24] Friedrichsen and Engelmann (2018) demonstrate empirically that image concern and altruism are negatively correlated. The crowding out result illustrated with this simulation does not hinge on this correlation, and Bénabou and Tirole (2006) show it can be found for any joint distribution of image concern and altruism.

[25] Symmetric are equilibria in which all agents of the same type play the same strategy in equilibrium (but not necessarily off equilibrium). Characterizing the full set of asymmetric equilibria, in which agents of the same type play different strategies, is computationally infeasible. We know these equilibria exist, but in any case characterizing them would not change the main point we are trying to illustrate.

more image concerned donors with low altruism are persuaded to donate (equilibria II and III), as they get a relatively good image as highly altruistic and not very image concerned. Eventually, however, visibility is sufficiently high to attract the least altruistic and most image concerned types, which will crowd out altruistic types who actually care relatively little about being seen as altruistic (but still do not like being seen as image concerned). This composition effect makes the image of those who donate worse on both dimensions, altruism and image concern, and is key in generating crowding out of donations.

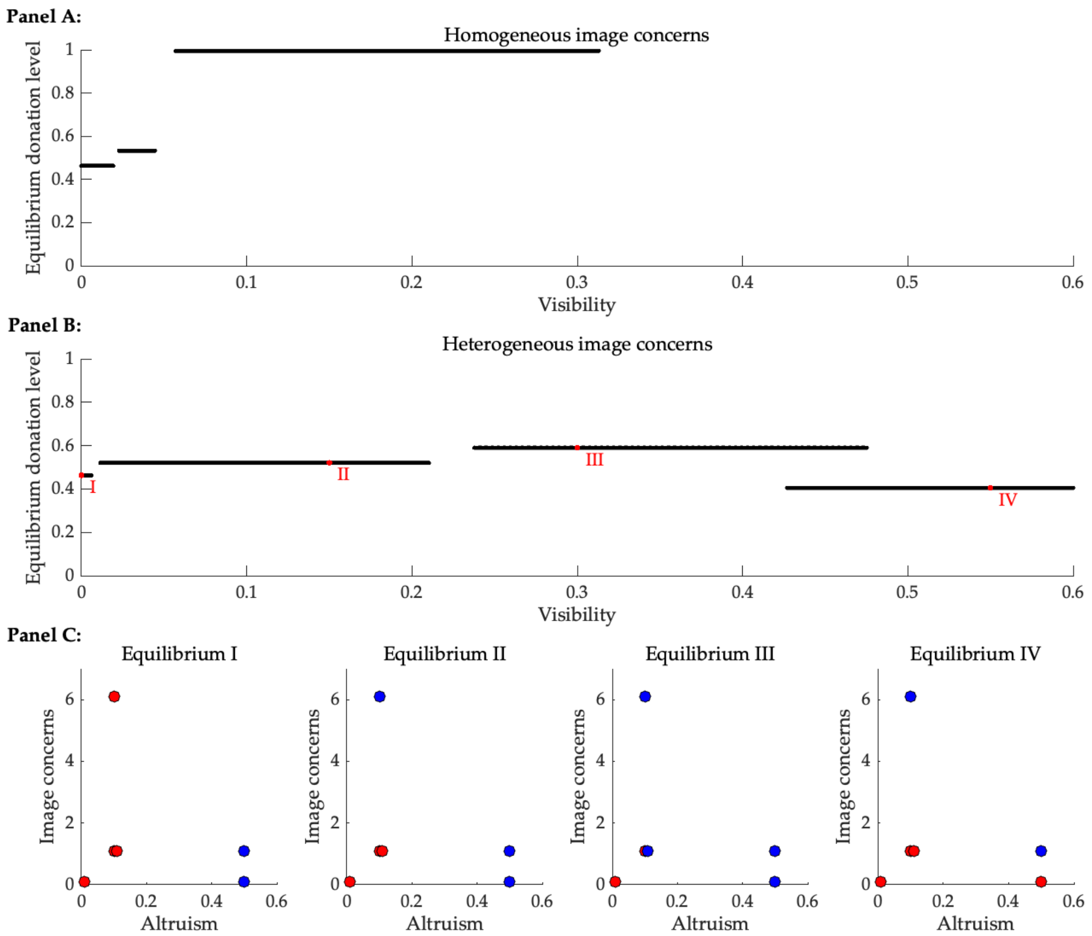


Figure B.1: Symmetric equilibria in a model of social recognition, with and without heterogeneity in image concern

Notes: Panel B shows the levels of donation for symmetric equilibria when types (pairs of altruism and image concerns) are $\{(0.01, 0.1), (0.1, 1.1), (0.1, 6.1), (0.11, 1.1), (0.5, 0.1), (0.5, 1.1)\}$ distributed according to probabilities $(0.46\%, 40.51\%, 5.79\%, 6.94\%, 11.57\%, 34.72\%)$. Panel A collapses image concerns to the population mean. Panel C shows in blue types who donate, and in red types who do not donate, for equilibria I, II, III and IV indicated in Panel B.

Panel a instead shows that donations are monotonic in v when image concerns are homogeneous in the population, hence donations do not signal this trait. This is a pattern that can be proved more generally for any $F(a)$.

Appendix for Online Publication

C. Additional Figures and Tables

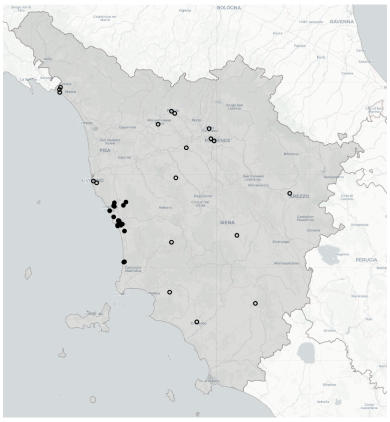


Figure C.1: Donors' location in *Close* and *Distant* treatments

Notes: For the 1,022 *Close* groups and the 1,022 *Distant* groups we use mailing address of each donor to compute geographical distance between every pair of group's members, and then average this measure of distance at the group level. This figure reports the median distance group in *Close* (filled dots) and *Distant* (empty dots).

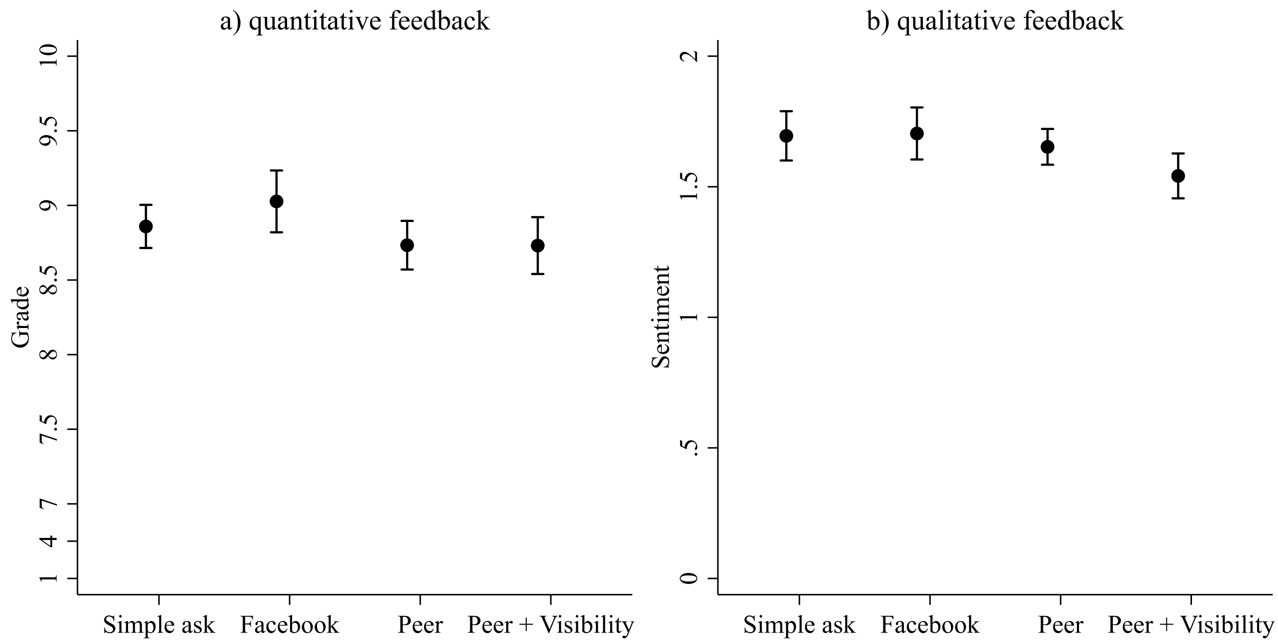


Figure C.2: Feedback

Notes: 5,029 (4,071) subjects were contacted to provide a quantitative (qualitative) feedback. Panel a 1,463 answers, panel b 980 answers. Panel a reports average grade respondents gave on a scale from 1 to 10, where "1" means "not appreciated at all" and "10" means "extremely appreciated". Panel b reports analysis of responses to the question "**In one sentence**, how did you appreciate the message we sent through this chat to encourage your donation?". Average sentiment from text analysis is based an algorithm developed by Neuraly (`neuraly.ai`) and built on the BERT language representation model (Devlin et al., 2018) to categorize text responses as either Positive (2), Neutral (1), or Negative (0).

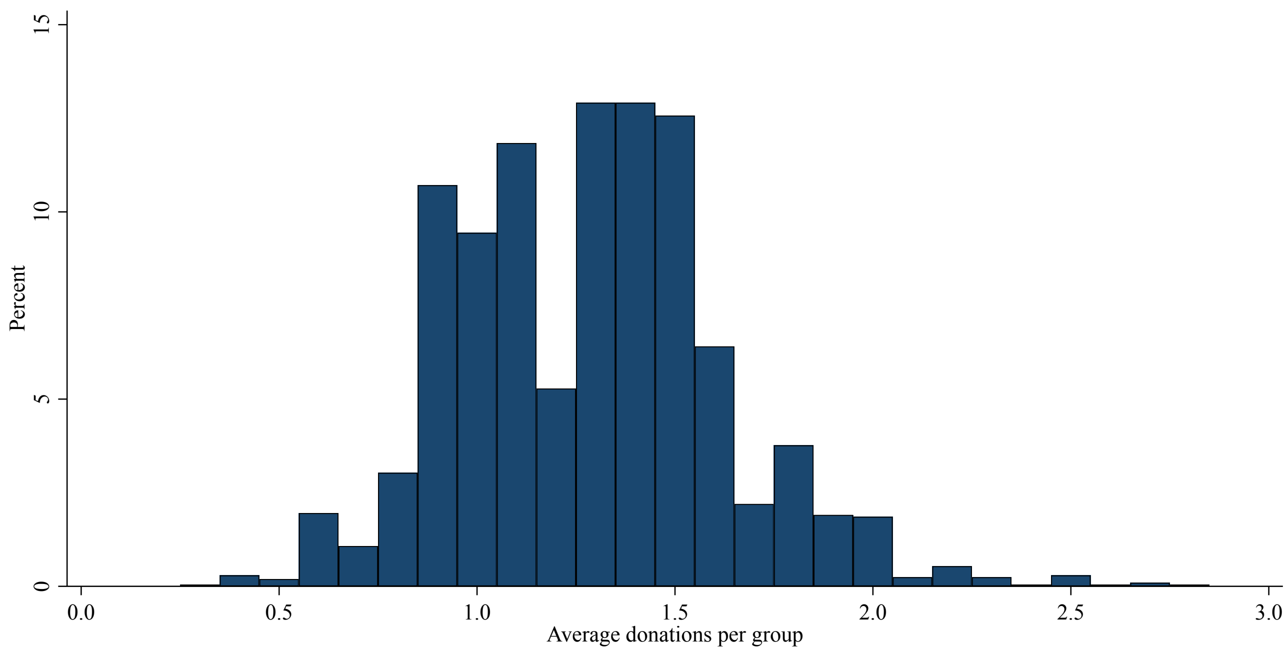


Figure C.3: Distribution of the social norm per group

Notes: Distribution of the average number of blood products donations made by the group's members in the 11 months among the 2,044 groups of donors. The value of the social norm has been revealed only to the people belonging to groups assigned to the *Peer* (456 groups) or *Peer + Visibility* (455 groups) treatments.

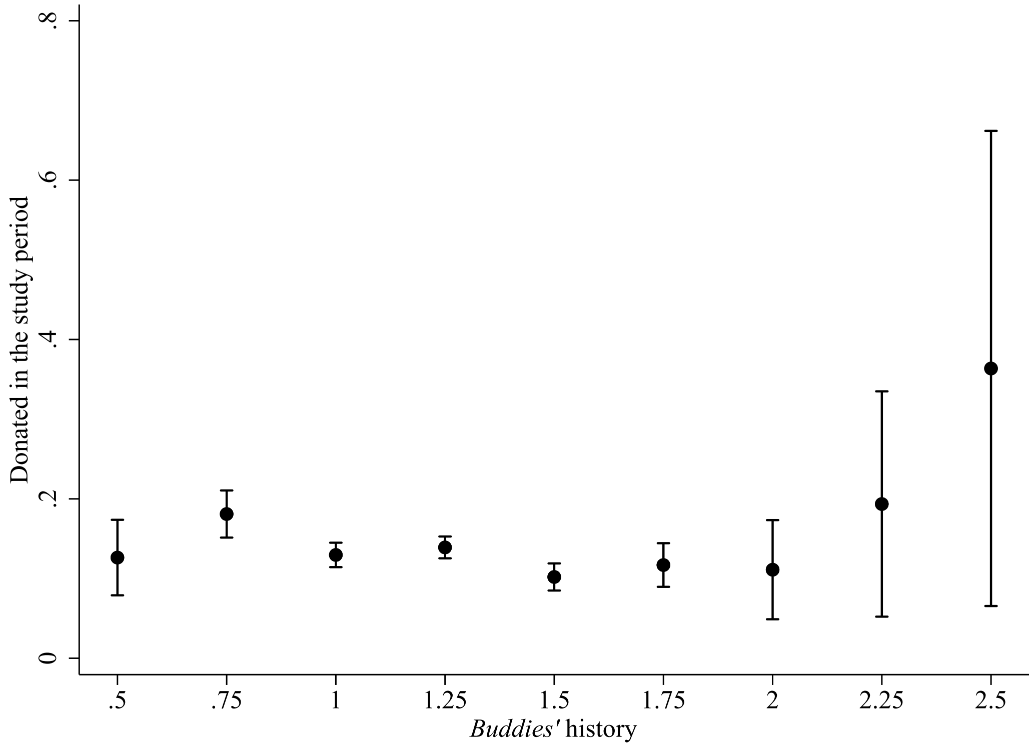


Figure C.4: Salient norm

Notes: Share of subjects who donated in the study period according to the social norm they were exposed to, and 95 percent confidence intervals. For each level S of the social norm we consider subjects exposed to a social norm in the interval $(S \pm 0.05)$. *Buddies'* history is the average number of donations made by the fellow group members in the past 11 months.

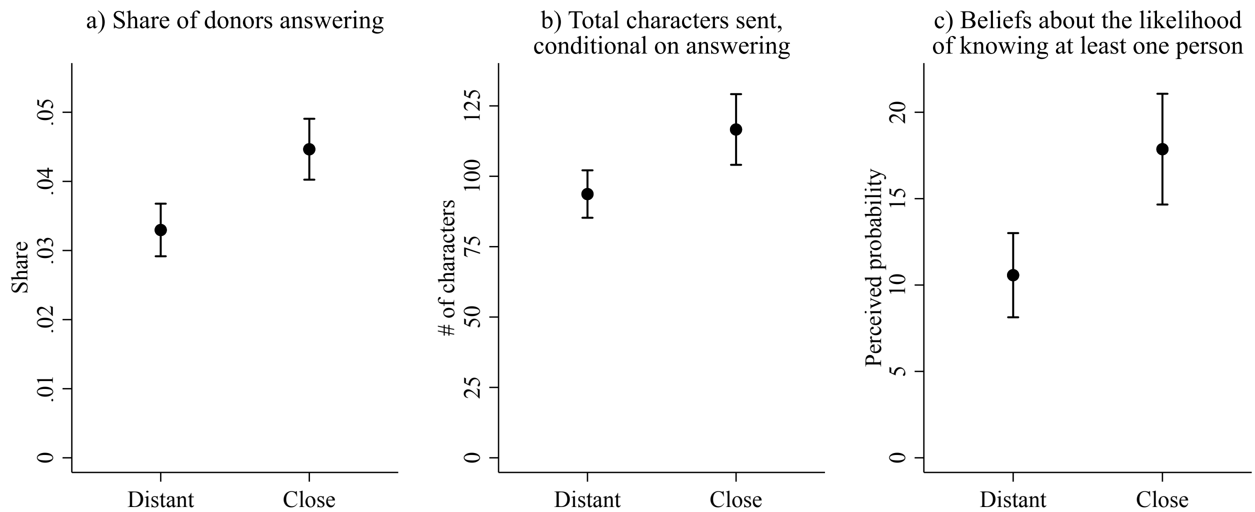


Figure C.5: Engagement and beliefs: *Distant* vs *Close*

Notes: Averages across *Distant* and *Close* treatments, and 95 percent confidence intervals. In panel a the outcome is the share of donors giving unsolicited responses to the treatment message ($N = 17,008$), in panel b the outcome is the number of characters of these responses conditional on responding ($N = 677$), and in panel c the outcome is the perceived probability of knowing at least one other group member ($N = 895$), which we elicit only among a subset of participants in the *Peer + Visibility* treatment.

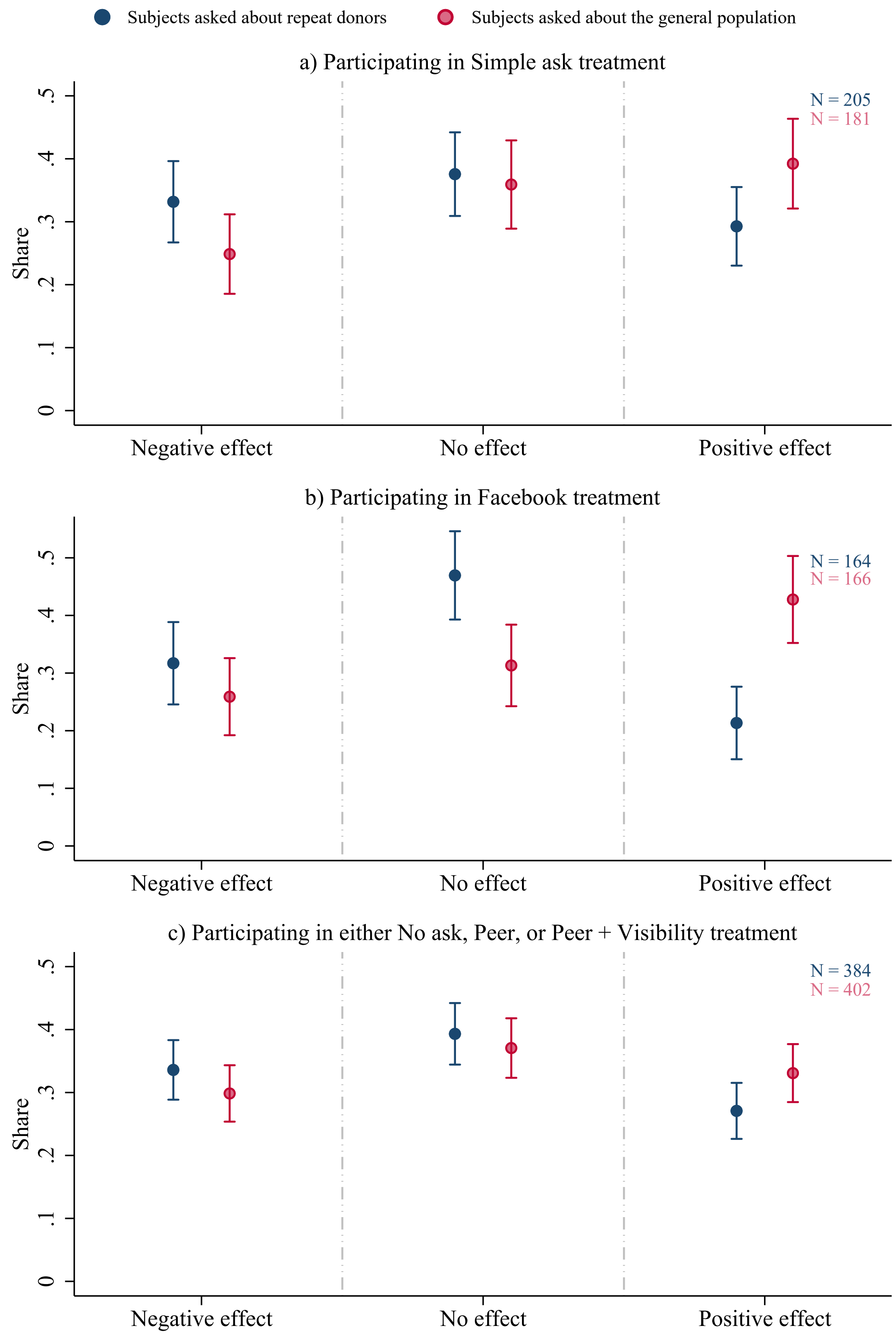


Figure C.6: Predict the effect of social recognition by treatment assignment

Notes: Share of respondents by treatment assignments predicting that the effect of the social media recognition intervention, among repeat blood donors and in the general population, will be either negative, null, or positive. Capped ranges are 95 percent confidence intervals.

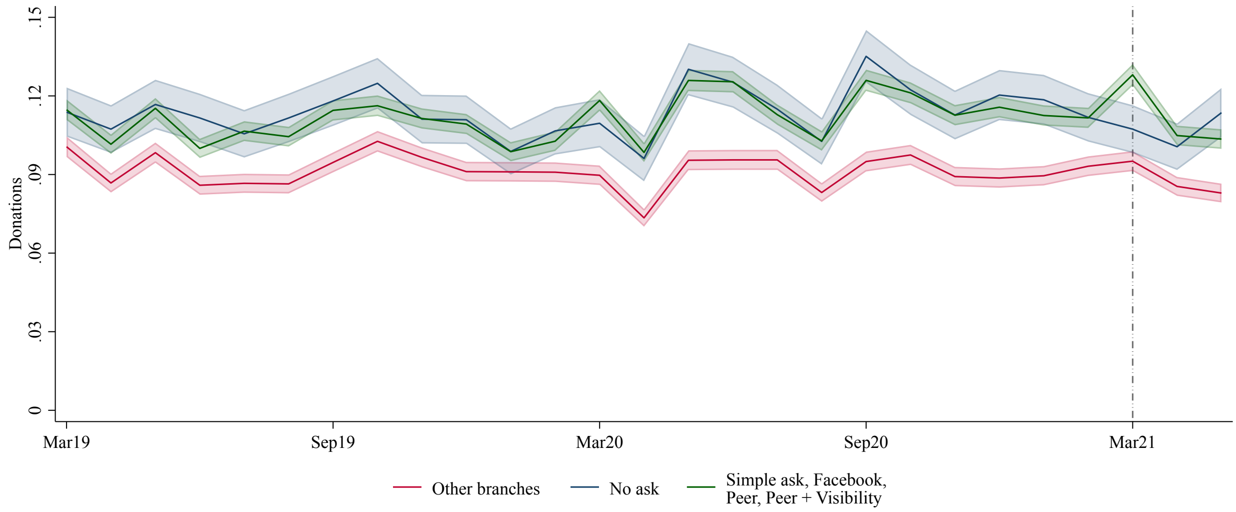


Figure C.7: Average monthly donations

Notes: Average monthly donations with 95 percent confidence intervals are presented for any of the three conditions. In the "Other branches" condition are included 22,841 donors distributed in the 94 local branches excluded from the study. Donors are instead 4,463 (25,479) and distributed over 63 (64) local branches for the *No ask* (*Simple ask, Facebook, Peer, Peer + Visibility*) condition. A vertical dotted line highlights the study period.

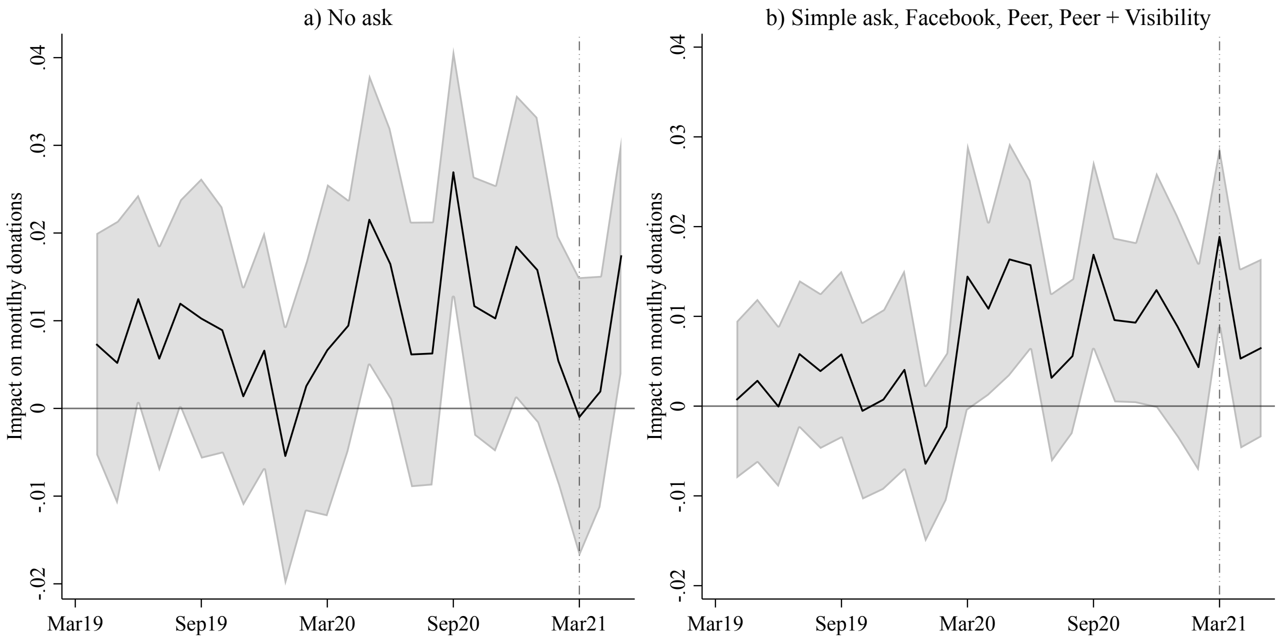


Figure C.8: Variation in the monthly donation

Notes: Estimates of δ_τ from Equation (4.1) for each month $\tau \in \{-24, ..., +2\}$ are reported along with 95 percent confidence intervals, presented separately for donors in the *No ask* (panel a) and *Simple ask, Facebook, Peer, Peer + Visibility* conditions (panel b). In the Other branches condition are included 22,841 donors distributed in the 94 local branches excluded from the study. Donors are instead 4,463 (25,479) and distributed over 63 (64) local branches for the *No ask* (*Simple ask, Facebook, Peer, Peer + Visibility*) condition. A vertical dotted line highlights the study period.

Table C.1: Demographics, donation behavior, and engagement (means and standard errors in parentheses)

			WhatsApp					Email		
	Active donors	Study sample	No ask	Simple ask	Facebook	Peer	Peer + Visibility	Simple ask	Facebook	p-value
(a) Measured before treatment										
Female	0.396	0.391	0.388	0.395	0.389	0.393	0.396	0.387	0.388	0.904
	(0.000)	(0.002)	(0.007)	(0.008)	(0.008)	(0.005)	(0.005)	(0.007)	(0.007)	
Age	43.569	44.147	44.101	44.103	44.137	44.189	44.042	44.223	44.287	0.962
	(0.000)	(0.064)	(0.192)	(0.194)	(0.194)	(0.137)	(0.136)	(0.191)	(0.187)	
Past donations	1.126	1.317	1.330	1.322	1.326	1.319	1.308	1.322	1.303	0.816
	(0.000)	(0.007)	(0.020)	(0.021)	(0.021)	(0.014)	(0.015)	(0.021)	(0.020)	
Can donate blood in March	0.788	0.762	0.760	0.769	0.755	0.761	0.761	0.762	0.765	0.851
	(0.000)	(0.002)	(0.006)	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)	(0.006)	
Can donate plasma in March	0.885	0.877	0.874	0.883	0.868	0.879	0.875	0.879	0.884	0.293
	(0.000)	(0.002)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	
Past blood donations	0.795	0.911	0.943	0.903	0.907	0.914	0.904	0.907	0.904	0.550
	(0.000)	(0.005)	(0.014)	(0.015)	(0.014)	(0.010)	(0.010)	(0.014)	(0.014)	
Past plasma donations	0.293	0.366	0.357	0.377	0.367	0.367	0.360	0.376	0.367	0.845
	(0.000)	(0.005)	(0.014)	(0.015)	(0.015)	(0.010)	(0.010)	(0.015)	(0.014)	
Email succesful before		0.759	0.779	0.751	0.752	0.755	0.754	0.766	0.763	0.017
		(0.002)	(0.006)	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)	(0.006)	
(b) Measured after treatment										
Blood donations March 2021	0.070	0.084	0.071	0.087	0.081	0.088	0.091	0.080	0.080	0.003
	(0.000)	(0.001)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	
Plasma donations March 2021	0.027	0.038	0.034	0.045	0.035	0.037	0.038	0.041	0.034	0.058
	(0.000)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	
WhatsApp text received		0.990		0.992	0.988	0.992	0.990			0.195
		(0.001)		(0.001)	(0.002)	(0.001)	(0.001)			
WhatsApp text read		0.909		0.906	0.907	0.910	0.913			0.558
		(0.002)		(0.005)	(0.004)	(0.003)	(0.003)			
Time to read text (hours) | read		2.021		1.954	1.953	1.981	2.127			0.743
		(0.065)		(0.158)	(0.152)	(0.106)	(0.120)			
Picture asked		0.083			0.056	0.097	0.082			0.000
		(0.002)			(0.004)	(0.003)	(0.003)			
Email read		0.172						0.176	0.168	0.354
		(0.004)						(0.006)	(0.006)	
Time to read email (hours) | read		99.347						93.796	105.035	0.076
		(3.278)						(4.402)	(4.863)	
Participate in follow-up survey		0.078	0.082	0.084	0.075	0.082	0.062	0.090	0.079	0.000
		(0.001)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	
Opt-out after treatment		0.002	0.000	0.001	0.002	0.002	0.005	0.000	0.000	0.000
		(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	
Observations	83047	38761	4436	4197	4220	8456	8450	4460	4542	

Notes: Past donations are computed over the 11 months before the experiment. Email successful before is the share of donors successfully reached via email by the organization in the year before the experiment. WhatsApp text read for WhatsApp treatments represents a lower bound, as some users may have deactivated read receipts in their privacy options. Opt-out after treatment people are not considered in any statistic and reported only in the dedicated row. The p-value in the last column is from a Kruskal-Wallis test comparing the 7 groups.

Table C.2: Treatment effects on donations

	(1)	(2)	(3)
	Baseline category: No ask		
Simple ask	0.028***	0.028***	0.028***
	(0.007)	(0.007)	(0.007)
Facebook	0.011	0.012*	0.012*
	(0.007)	(0.007)	(0.006)
Peer	0.021***	0.021***	0.021***
	(0.006)	(0.006)	(0.006)
Peer + Visibility	0.025***	0.025***	0.026***
	(0.006)	(0.006)	(0.006)
Donors' observables	No	Yes	Yes
Local branch FE	No	No	Yes
Observations	29759	29759	29759
Clusters	1588	1588	1588
F-test	0.000	0.000	0.000
R2	0.001	0.052	0.058

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Treatment effects estimated using a linear probability model, where the dependent variable indicates whether the subject donated either blood or plasma in the study period—March 2021. *Simple ask*, *Facebook*, *Peer* and *Peer + Visibility* are binary treatment indicators. Donors' observables include: age groups (18-38, 39-51, 52+), gender and past donations. Standard errors in parentheses are clustered at the level of the unit of randomization the 20-donors group level. All columns estimate the model for all blood donors in the treatments that were administered through WhatsApp: *No ask*, *Simple ask Facebook*, *Peer* and *Peer + Visibility*. For each column, we report a test of joint significance of the reported treatment effects.

Table C.3: The response to social norm information

	All donors			Treated donors		
	(1)	(2)	(3)	(4)	(5)	(6)
Buddies' history	-0.001			-0.003		
	(0.008)			(0.008)		
Buddies' history in the 1st quintile		-0.013*			-0.013	
		(0.008)			(0.008)	
Buddies' history in the 2nd quintile		-0.000			0.001	
		(0.008)			(0.008)	
Buddies' history in the 4th quintile		-0.011			-0.013	
		(0.008)			(0.009)	
Buddies' history in the 5th quintile		-0.015*			-0.015*	
		(0.008)			(0.009)	
Above median norm			-0.004			-0.006
			(0.005)			(0.005)
Donors' observables	Yes	Yes	Yes	Yes	Yes	Yes
Local branch FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16906	16906	16906	15402	15402	15402
Clusters	911	911	911	911	911	911
F-test	0.921	0.147	0.466	0.724	0.116	0.268
R2	0.057	0.058	0.056	0.056	0.056	0.055

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Effects of social norm information on donation estimated using a linear probability model, where the dependent variable indicates whether the subject donated either blood or plasma in the study period—March 2021. In columns 1 and 4 we estimate the linear effect of social norm information. For columns 2 and 5 we split in quintiles the support of social norms that individual study participants observe and estimate the effect of exposure to each quintile. In columns 3 and 6 we estimate the linear effect of being exposed to a social norm above the median value. *Buddies'* history is the average number of donations made by the fellow group members in the past 11 months. The omitted category in columns 2 and 4 is *Buddies'* history in the 3rd quintile. Above median norm is a dummy variable taking value of one if the subject is exposed to a social norm above the median value. Columns 1 to 3 estimate the model for all blood donors in treatments *Peer* and *Peer + Visibility*. Columns 4 to 6 exclude participants that did not engage with the experimental materials (either did not open the email or did not read our WhatsApp text, depending on the channel through which the experiment was conducted). Standard errors in parentheses are clustered at the 20-donors group level. For each column, we report a test of joint significance of the reported treatment effects.

Table C.4: Heterogeneous treatment effects, by social proximity

Panel A	(1)	*Panel B*	(2)	(3)
Close	-0.004	Close	-0.006	0.004
	(0.007)		(0.021)	(0.013)
Peer + Visibility	0.003	*Buddies'* history	-0.006	
	(0.007)		(0.012)	
Peer + Visibility $\times$ Close	0.005	*Buddies'* history $\times$ Close	0.004	
	(0.010)		(0.017)	
		Buddies' history in Q1		-0.007
				(0.011)
		Buddies' history in Q2		0.004
				(0.011)
		Buddies' history in Q4		-0.013
				(0.011)
		Buddies' history in Q5		-0.012
				(0.012)
		Buddies' history in Q1 $\times$ Close		-0.012
				(0.017)
		Buddies' history in Q2 $\times$ Close		-0.006
				(0.017)
		Buddies' history in Q4 $\times$ Close		-0.000
				(0.017)
		Buddies' history in Q5 $\times$ Close		-0.007
				(0.017)
Donors' observables	Yes	Donors' observables	Yes	Yes
Local branch FE	Yes	Local branch FE	Yes	Yes
Observations	15402	Observations	1540 2	15402
Clusters	911	Clusters	911	911
R2	0.057	R2	0.056	0.057

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Effects of visibility and social norm information on donation estimated using a linear probability model, where the dependent variable indicates whether the subject donated in the study period—March 2021. These effects are estimated in models that interact the treatment variable with an indicator for whether the donor is randomly assigned to a *Close* or *Distant* 20-donors group. social proximity. In column 1 we estimate the effect of visibility (*Peer + Visibility* v. *Peer*). In column 2 we estimate the linear effect of social norm information. For column 3 we split in quintiles the support of social norms that individual study participants observe and estimate the effect of exposure to each quintile. *Buddies'* history is the average number of donations made by the fellow group members in the past 11 months. The omitted category in columns 2 and 3 is *Buddies'* history in the 3rd quintile. The estimation sample includes participants of the *Peer* and *Peer + Visibility* treatments excluding those who did not engage with experimental materials (because they did not read our WhatsApp text). Standard errors in parentheses are clustered at the 20-donors group level.

Table C.5: Heterogeneous treatment effects, by frequency of past donations

Panel A	(1)	*Panel B*	(2)	(3)
Frequent donor	-0.052***	Frequent donor	-0.002	-0.030*
	(0.014)		(0.027)	(0.018)
Simple ask	0.016**	*Buddies'* history	0.005	
	(0.007)		(0.009)	
Facebook	0.006	*Buddies'* history × Frequent donor	-0.022	
	(0.007)		(0.020)	
Peer	0.014**	*Buddies'* history in Q1		-0.021**
	(0.006)			(0.010)
Peer + Visibility	0.021***	*Buddies'* history in Q2		0.002
	(0.006)			(0.010)
Simple ask × Frequent donor	0.029*	*Buddies'* history in Q4		-0.014
	(0.016)			(0.010)
Facebook × Frequent donor	0.013	*Buddies'* history in Q5		-0.009
	(0.015)			(0.010)
Peer × Frequent donor	0.017	*Buddies'* history in Q1 × Frequent donor		0.021
	(0.013)			(0.018)
Peer + Visibility × Frequent donor	0.015	*Buddies'* history in Q2 × Frequent donor		-0.005
	(0.013)			(0.019)
		Buddies' history in Q4 × Frequent donor		-0.003
				(0.019)
		Buddies' history in Q5 × Frequent donor		-0.018
				(0.018)
Donors' observables	Yes	Donors' observables	Yes	Yes
Local branch FE	Yes	Local branch FE	Yes	Yes
Observations	29759	Observations	15402	15442
Clusters	1588	Clusters	911	911
R2	0.059	R2	0.057	0.058

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Effects of the various asks implemented and social norm information on donation estimated using a linear probability model, where the dependent variable indicates whether the subject donated in the study period—March 2021. These effects are estimated in models that interact the treatment variable with an indicator for whether the donor is one who did an above median number of donations in the 11 months prior to the intervention—is a Frequent donor. In column 1 we estimate the effect of various asks implemented (*Simple ask*, *Facebook Peer* and *Peer + Visibility* v. *No ask*). In column 2 we estimate the linear effect of social norm information. For column 3 we split in quintiles the support of social norms that individual study participants observe and estimate the effect of exposure to each quintile. *Buddies'* history is the average number of donations made by the fellow group members in the past 11 months. The omitted category in columns 2 and 4 is *Buddies'* history in the 3rd quintile. The estimation sample for column 1 includes participants of all treatments administered through WhatsApp (*No ask*, *Simple ask*, *Facebook Peer* and *Peer + Visibility*), whereas for columns 2 and 3 it includes only participants of the *Peer* and *Peer + Visibility* treatments; participants who did not engage with experimental materials (because they did not read our WhatsApp text) are excluded. Standard errors in parentheses are clustered at the 20-donors group level.

Table C.6: Demographics, donation behavior, and engagement (means and standard errors in parentheses)

	Study sample	WhatsApp	Email	Survey Follow-up
(a) Measured before treatment				
Female	0.391	0.393	0.387	0.426
	(0.002)	(0.003)	(0.005)	(0.009)
Age	44.147	44.114	44.255	44.353
	(0.064)	(0.073)	(0.134)	(0.231)
Past donations	1.317	1.319	1.312	1.878
	(0.007)	(0.008)	(0.014)	(0.028)
Can donate blood in study period	0.762	0.761	0.764	0.670
	(0.002)	(0.002)	(0.004)	(0.009)
Can donate plasma in study period	0.877	0.876	0.882	0.822
	(0.002)	(0.002)	(0.003)	(0.007)
Past blood donations	0.911	0.913	0.906	1.182
	(0.005)	(0.005)	(0.010)	(0.018)
Past plasma donations	0.366	0.365	0.372	0.595
	(0.005)	(0.006)	(0.010)	(0.024)
Email succesful before	0.759	0.757	0.765	0.797
	(0.002)	(0.002)	(0.004)	(0.007)
(b) Measured after treatment				
Donated blood in study period	0.084	0.085	0.080	0.112
	(0.001)	(0.002)	(0.003)	(0.006)
Donated plasma in study period	0.037	0.038	0.037	0.063
	(0.001)	(0.001)	(0.002)	(0.004)
Multiple donations in study period	0.000	0.000	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.001)
WhatsApp text received	0.990	0.990		1.000
	(0.001)	(0.001)		(0.000)
WhatsApp text read	0.909	0.909		0.939
	(0.002)	(0.002)		(0.006)
Time to read text (hours) | read	2.021	2.021		1.184
	(0.065)	(0.065)		(0.147)
Picture asked	0.083	0.083		0.202
	(0.002)	(0.002)		(0.010)
Email read	0.172		0.172	0.205
	(0.004)		(0.004)	(0.015)
Time to read email (hours) | read	99.347		99.347	98.000
	(3.278)		(3.278)	(10.561)
Opt-out after treatment	0.002	0.002	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	38761	29759	9002	3011

Notes: Past donations are computed over the 11 months before the experiment. "Email successful before" is the share of donors successfully reached via email by the organization in the year before the experiment. "WhatsApp text read" for WhatsApp treatments represents a lower bound, as some users may have deactivated read receipts in their privacy options. "Opt-out after treatment" people are not considered in any statistic and reported only in the dedicated row. We were not able to match 5 subjects in the Survey Follow-up to administrative data.

Table C.7: Treatments effect on donations

	(1) March 2021	(2) April 2021	(3) May 2021	(4) March April May 2021	(5) April 2021	(6) March April May 2021	(7) April 2021	(8) March April May 2021
Simple ask	0.027***	-0.002	-0.016**	0.010	-0.005	0.006	-0.005	0.006
	(0.007)	(0.007)	(0.006)	(0.010)	(0.007)	(0.011)	(0.007)	(0.010)
Facebook	0.011*	0.008	-0.007	0.013	0.006	0.009	-0.003	-0.001
	(0.006)	(0.006)	(0.006)	(0.010)	(0.006)	(0.011)	(0.008)	(0.013)
Peer	0.021***	0.004	-0.009	0.016*	0.001	0.012	0.001	0.012
	(0.006)	(0.005)	(0.006)	(0.009)	(0.006)	(0.009)	(0.006)	(0.009)
Peer + Visibility	0.025***	0.011**	-0.004	0.032***	0.008	0.029***	-0.009	0.010
	(0.006)	(0.005)	(0.006)	(0.009)	(0.006)	(0.009)	(0.010)	(0.016)
Feedback Message					0.007*	0.008		
					(0.004)	(0.006)		
Additional Messages							0.009**	0.010*
							(0.004)	(0.006)
Donors' observables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local branches FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	29759	29759	29759	29759	29759	29759	29759	29759
Clusters	1588	1588	1588	1588	1588	1588	1588	1588
R2	0.058	0.055	0.063	0.175	0.055	0.176	0.055	0.176
Simple ask - Facebook	0.016	-0.010	-0.009	-0.003	-0.010	-0.003	-0.002	0.007
p-value(Simple ask - Facebook)	0.021	0.129	0.162	0.757	0.130	0.758	0.823	0.564
(Peer + Visibility) - Peer	0.005	0.007	0.005	0.017	0.007	0.017	-0.009	-0.002
p-value((Peer + Visibility) - Peer)	0.307	0.149	0.287	0.025	0.148	0.025	0.241	0.878

Notes: Simple ask, Facebook, Peer, and Peer + Visibility are binary indicators taking the value of 1 if the donor is in the particular treatment, 0 otherwise. Above median is a binary indicator taking the value of 1 if the donor made more donations than the median in the past 11 months. Feedback Message is a binary variable taking the value of 1 if the donor was randomly selected to be sent a message asking for a feedback regarding the treatment message. Additional Messages is a discrete variable taking values from 0 to 3 and describing how many additional messages were sent to the donor during April 2021 (including the feedback message). Standard errors in parentheses are clustered at the group level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D. Experimental materials

Table D.1: Email treatment messages

Simple ask	Facebook
Hi $donor_name, As in every month, we are in need of blood. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you. Best regards, Adelmo Agnolucci President of Avis Regionale Toscana	Hi $donor_name, To encourage people to donate, on April 8 we are posting on the Facebook page of $Avis_center the donations made in March 2021 by the our members using a post similar to the example here below. With your participation you'll be able to share your experience with your friends, inspire them, and tell them how important the donation is. We hope this message encourages you to donate this month. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you. Best regards, Adelmo Agnolucci President of Avis Regionale Toscana

Table D.2: WhatsApp treatment messages

Simple ask	Facebook	Peer	Peer + Visibility
Hi $donor_name, As in every month, we are in need of blood. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you.	Hi $donor_name, To encourage people to donate, on April 8 we are posting on the Facebook page of $Avis_center the donations made in March 2021 by the our members using a post similar to the example you receive if you reply SEE to this message. With your participation you'll be able to share your experience with your friends, inspire them, and tell them how important the donation is. We hope this message encourages you to donate this month. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you.	Hi $donor_name, AVIS Toscana kicks-off *buddy donors*, our initiative to keep your informed about what and when our members donate, to recognize and inspire the donation. With *buddy donors* you are randomly assigned to a group of 20 AVIS members of your donation center $donation_center $city. Over the past 11 months, your group mates made **#.#** donations on average, while you donated **#** times (reply 'SEE' to see this graphically). We hope this message encourages you to donate this month. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you.	Hi $donor_name, AVIS Toscana kicks-off *buddy donors*, our initiative to keep your informed about what and when our members donate, to recognize and inspire the donation. With *buddy donors* you are randomly assigned to a group of 20 AVIS members of your donation center $donation_center $city. Over the past 11 months, your group mates made **#.#** donations on average, while you donated **#** times (reply 'SEE' to see this graphically). **On April 8, we are revealing to all members of this group, who the group members are (first name and initial of the last name) and what they donated in March 2021**, in a table (reply 'SEE' to see example). We hope this message encourages you to donate this month. In case you gave blood recently, you may still be eligible to donate plasma or other blood products! Visit https://www.avis.it/it/i-tipi-di-donazione to check the recommended intervals between different types of donations, and contact $Avis_center to schedule your next appointment. We look forward to hearing from you.

Notes: Text in orange applies to *Close* treatments. In *Distant* treatments, this is replaced with from all over Tuscany.

(a)

Nome	Donazioni sangue	Donazioni plasma
[redacted]	1	0
[redacted]	1	0
[redacted]	0	0
[redacted]	1	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	1	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	1
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0
[redacted]	0	0

(b)

Figure D.1: Donations disclosure

Notes: Panel (a) presents the table sent at the end of the study period to donors in the *Peer + Visibility* treatment listing the donations of the group members during the study period. The first column contains first name and first letter of the last name of each member (erased here to comply with consent). The second and the third columns report the number of blood and plasma donations, respectively. Panel (b) presents the post made by the research partners on the public Facebook page of the organization for donations from donors in *Facebook* treatment.

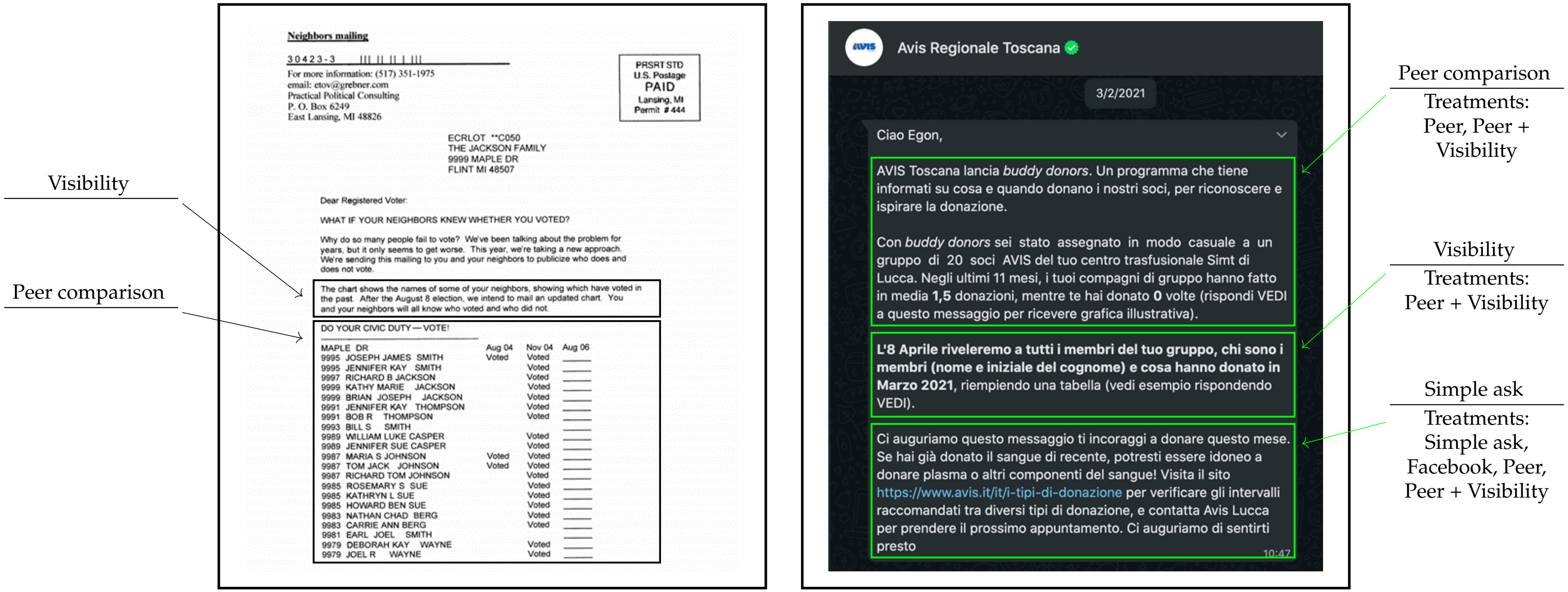


Figure D.2: Intervention of this study compared to a classical study (Gerber et al., 2008)

E. Initial social media recognition experiment

In this appendix we provide an extensive description of the initial social media recognition experiment developed in 2019. The study was conducted jointly with the Tuscan chapter of Associazione Volontari Italiani Sangue (AVIS) to investigate the effectiveness of the common practice of recognizing publicly the virtuous behavior of donors. After describing the main setting of the experiment, we will present the immediate effects of our intervention, as well as the long-run impact measured a year later—as per pre-registration (#AEARCTR-0004890).

E.1. Experimental design

The study features a control group plus four treatments, two concerning social media recognition and two related to a simple encouragement to make a donation, both defined analogously for blood and plasma. The whole design is summarized in Figure E.1. The *Facebook Blood (Plasma)* treatment consists of a baseline communication inviting donors to make a blood (plasma) donation in the coming month, plus the prospect of being publicly recognized through the official Facebook pages of Avis Toscana.

In the other two treatments, that we call *Simple ask Blood* and *Simple ask Plasma*, donors receive only the baseline communication inviting them to make either a blood or a plasma donation in the coming month. These two treatments serve to identify the impact of recognition on social media net of reminder and simple ask effects.

Finally, people in the *No ask* control group do not receive any communication at all. This pure control group allows us to measure the overall impact of the intervention, assessing not only the net effect of promising social recognition but also of the whole communication campaign.

For both the *Simple ask* and the *Facebook* treatments we also cross-randomize whether the email contained information about the current level of local shortages of blood supply.

E.1.1. Procedures

The study involved Avis Toscana donors from 67 selected local branches, provided that they gave Avis an email address and were eligible to donate both blood and plasma in November

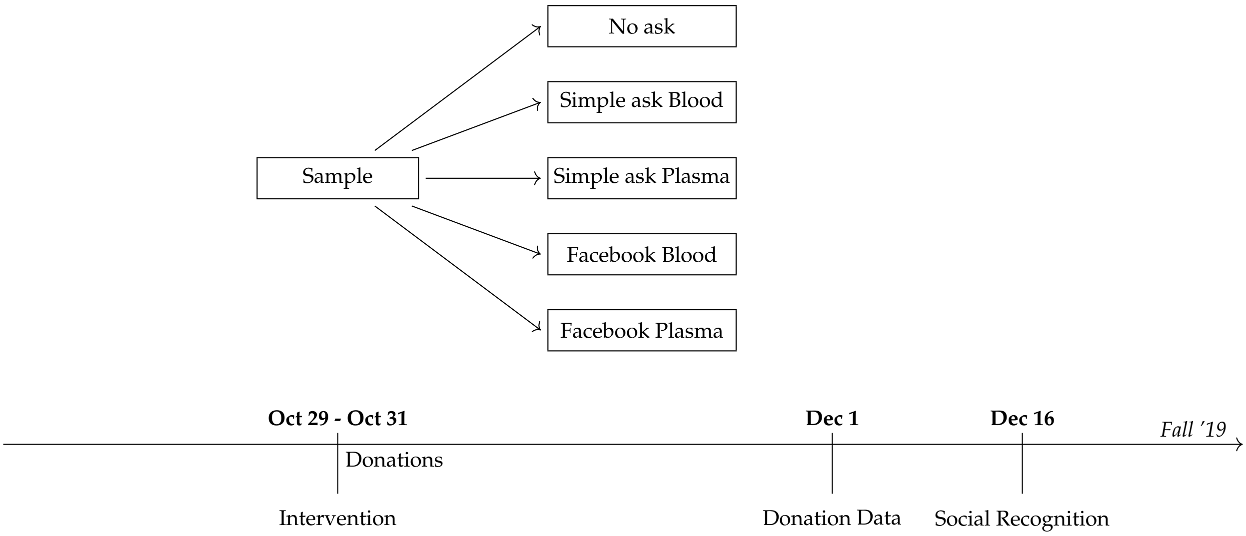


Figure E.1: Initial social media recognition experiment, design overview

2019.

All the communications were tailored to the individual donor. The message was personalized with the donor's first name and with other specific information, such as the name of the local branch that the volunteer is registered at (see Table E.4).

Treatment emails were sent from an institutional email address of Avis Toscana by means of a Python program that took three days to run, from October 29 to October 31 of 2019. On December 1 we obtain donation data for the month of November, and send via email to participants an online form to collect their opt-in consent for sharing their name on social media posts. In this form, participants are asked to indicate if they want their full name or their name and last name initial to be acknowledged in the post. Finally, on December 16 the posts on the Facebook page of Avis Toscana started to roll out.

Randomization was done partially at the local branch level and partially at the individual level. The 67 local branches of the Tuscan chapter of Avis participating in the study were randomized into: i) *Facebook Blood*, ii) *Facebook Plasma*, iii) a super-group including both the two *Simple ask* treatments and the *No ask* control group. We chose to randomize social recognition across branches to minimize potential treatment contamination among volunteers of the same branch. Among donors in group iii), the randomization between the *No ask*, *Simple ask Blood* and *Simple ask Plasma* treatments was done at the individual level. Finally, the randomization of information regarding local shortages of blood supply was entirely done

Table E.1: Original social media recognition experiment. Demographics, donation behavior, and engagement (means and standard errors in parentheses)

		Treatment groups					
			Simple ask		Facebook		
	Whole sample	No ask	Blood	Plasma	Blood	Plasma	p-value
(a) Measured before treatment							
Female	0.375	0.377	0.375	0.369	0.371	0.380	0.898
	(0.004)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	
Age	45.828	45.623	45.911	45.957	45.790	45.918	0.812
	(0.097)	(0.194)	(0.223)	(0.222)	(0.226)	(0.218)	
Past donations	1.193	1.172	1.258	1.192	1.167	1.184	0.006
	(0.011)	(0.023)	(0.027)	(0.026)	(0.027)	(0.026)	
Past blood donations	0.616	0.595	0.625	0.612	0.635	0.620	0.153
	(0.007)	(0.013)	(0.015)	(0.015)	(0.015)	(0.015)	
Past plasma donations	0.541	0.542	0.577	0.536	0.519	0.533	0.006
	(0.009)	(0.018)	(0.022)	(0.021)	(0.022)	(0.021)	
Email successful before	0.863	0.864	0.858	0.855	0.952	0.789	0.000
	(0.003)	(0.006)	(0.007)	(0.007)	(0.004)	(0.007)	
(b) Measured after treatment							
Donated blood in study period	0.090	0.080	0.097	0.086	0.095	0.094	0.095
	(0.002)	(0.004)	(0.006)	(0.005)	(0.005)	(0.005)	
Donated plasma in study period	0.049	0.046	0.053	0.053	0.047	0.049	0.610
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	
Multiple donations in study period	0.001	0.000	0.001	0.001	0.000	0.001	0.300
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	
Email read	0.226		0.229	0.223	0.238	0.216	0.212
	(0.004)		(0.008)	(0.008)	(0.008)	(0.007)	
Observations	15326	3738	2814	2776	2905	3093	

Notes: "Past donations" are computed over the 11 months before the experiment. "Email successful before" describe the share of donors successfully reached via email by the organization in the year before the experiment. The p-value in the last column is from a Kruskal-Wallis test comparing the 5 groups.

at the individual level.

E.1.2. Sample

The final sample is presented in Table E.1. Overall, 15326 volunteers were included in the study, with an average age of 46 years and a female share close to 38 percent. Study participants made on average 1.19 donations (of any blood product) in the 11 months prior to the experiment, of which the large part were blood (0.62) and plasma (0.54) donations.

The sample is balanced across treatments on age and gender, but not past donations. The email opening rate is similar across treatments, ruling out that differences in donations could be due to differences in opening rates across treatments.

Table E.2: Treatment effects on donations in the study month

	All donors			Treated donors		
	(1) Blood	(2) Plasma	(3) Either	(4) Blood	(5) Plasma	(6) Either
	Baseline category: No ask			*Baseline category: Simple ask*		
Simple ask	0.013**	0.007	0.014*			
	(0.005)	(0.005)	(0.008)			
Facebook	0.012	0.006	0.017**	-0.030	0.002	-0.004
	(0.009)	(0.004)	(0.007)	(0.019)	(0.012)	(0.016)
Donors' observables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9229	9420	14993	1309	1268	2577
Clusters	66	67	67	46	41	65
R2	0.055	0.069	0.060	0.075	0.101	0.059
Facebook - Simple ask	-0.001	-0.001	0.003	-0.030	0.002	-0.004
$\hookrightarrow$ p-value	0.885	0.801	0.768	0.123	0.896	0.803

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Treatment effects estimated using a linear probability model, where the dependent variable indicates whether the subject donated blood (columns 1 and 4), plasma (columns 2 and 5) or either (columns 3 and 6) in the study period—November 2019. Donors' observables include: age groups (18-38, 39-51, 52+), gender and past donations. Standard errors are clustered at the local branch level. Columns 1 and 4 estimate the model for donors in treatments *No ask*, *Simple ask Blood* and *Facebook Blood*, columns 2 and 5 estimate the model for donors in treatments *No ask*, *Simple ask Plasma* and *Facebook Plasma*, and columns 3 and 6 pool all treatments. Columns 1-3 estimate the model for all donors in these treatments. Columns 4-6 estimate it only for treated donors, those who opened the treatment email.

E.2. Results

In this section we present the main results of the study, considering both immediate donations (in the coming month) and donations in the correspondent month one year later. Overall, we find that the prospect of recognition on social media does not provide any extra stimulus for donors; a simple ask to donate appears sufficient to motivate volunteers.

E.2.1. Immediate Effects

In Table E.2 we estimate the immediate effects of treatment messages that solicit blood donations on donations of blood (column 1), the immediate effects of treatment messages that solicit plasma donations on donations of plasma (column 2), and the immediate effects of the pooled treatment messages on total (blood and plasma) donations. Neither of the models we estimate find that social recognition (*Facebook*) leads to more donations than a *Simple ask*. Even if the *Facebook* appears to perform slightly better when considering blood and plasma jointly, the difference is not significant *Simple ask* (t-test for the difference equal to zero reports $p = 0.838$). Re-estimating the model for donors who opened the email (columns 4-6), leads to similar findings.

Table E.3: Treatment effects on donations one year later

	All donors			Treated donors		
	(1) Blood	(2) Plasma	(3) Both	(4) Blood	(5) Plasma	(6) Both
	Baseline category: No ask			*Baseline category: Simple ask*		
Simple ask	0.007	0.001	0.004			
	(0.006)	(0.005)	(0.007)			
Facebook	0.002	0.000	0.004	-0.013	0.006	0.010
	(0.006)	(0.005)	(0.008)	(0.013)	(0.011)	(0.013)
Donors' observables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9229	9420	14993	1309	1268	2577
Clusters	66	67	67	46	41	65
R2	0.031	0.071	0.055	0.025	0.095	0.057
Facebook - Simple ask	-0.005	-0.001	0.000	-0.013	0.006	0.010
$\hookrightarrow$ p-value	0.574	0.897	0.962	0.318	0.582	0.442

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Treatment effects estimated using a linear probability model, where the dependent variable indicates whether the subject donated blood (columns 1 and 4), plasma (columns 2 and 5) or either (columns 3 and 6) in a one-month period one year later—December 2020. Donors' observables include: age groups (18-38, 39-51, 52+), gender and past donations. Standard errors are clustered at the local branch level. Columns 1 and 4 estimate the model for donors in treatments *No ask*, *Simple ask Blood* and *Facebook Blood*, columns 2 and 5 estimate the model for donors in treatments *No ask*, *Simple ask Plasma* and *Facebook Plasma*, and columns 3 and 6 pool all treatments. Columns 1-3 estimate the model for all donors in these treatments. Columns 4-6 estimate it only for treated donors, those who opened the treatment email.

E.2.2. Effects 1 year later

The impact on donations in November 2020, that is one year after the intervention, is presented in Table E.3. We find that none of the treatments has a long lasting effect. Both the *Simple ask* and the *Facebook* treatments have no significant impact on donation made one year later, no matter whether blood and plasma donations are considered separately or jointly.

Table E.4: Initial social media recognition experiment, email treatment messages

Simple ask Blood	Simple ask Plasma	Facebook Blood	Facebook Plasma
Hi $donor_name, Do you know that since **$date** you are eligible to make a new **blood** donation? **Donating regularly is important**! It helps both your community that needs blood and Avis in its mission of creating social capital. You can always check the need for whole blood in Tuscany at this link, but don't wait for an emergency to donate. We hope you will donate **this month**! Best regards, $president_Avis_center_name President $Avis_center Adelmo Agnolucci President of Avis Regionale Toscana	Hi $donor_name, Do you know that since **$date** you are eligible to make a new **plasma** donation? **Donating regularly is important**! It helps both your community that needs plasma and Avis in its mission of creating social capital. You can always check the need for whole blood in Tuscany at this link, but don't wait for an emergency to donate. We hope you will donate **this month**! Best regards, $president_Avis_center_name President $Avis_center Adelmo Agnolucci President of Avis Regionale Toscana	Hi $donor_name, Do you know that since **$date** you are eligible to make a new **blood** donation? **Donating regularly is important**! It helps both your community that needs blood and Avis in its mission of creating social capital. You can always check the need for whole blood in Tuscany at this link, but don't wait for an emergency to donate. We hope you will donate **this month**! To encourage people to donate, in December **we are posting on the Facebook page of $Avis_center the donations made in November by the our members** using a post similar to the example here below. With your participation you'll be able to share your experience with your friends, inspire them, and tell them how important the donation is. Best regards, $president_Avis_center_name President $Avis_center Adelmo Agnolucci President of Avis Regionale Toscana	Hi $donor_name, Do you know that since **$date** you are eligible to make a new **plasma** donation? **Donating regularly is important**! It helps both your community that needs plasma and Avis in its mission of creating social capital. You can always check the need for whole blood in Tuscany at this link, but don't wait for an emergency to donate. We hope you will donate **this month**! To encourage people to donate, in December **we are posting on the Facebook page of $Avis_center the donations made in November by the our members** using a post similar to the example here below. With your participation you'll be able to share your experience with your friends, inspire them, and tell them how important the donation is. Best regards, $president_Avis_center_name President $Avis_center Adelmo Agnolucci President of Avis Regionale Toscana

Notes: Text in blue is present according to the cross-randomization of the *information* condition.

Table E.5: Original social media recognition experiment: Demographics, donation behavior and engagement

	Whole sample	Treatment groups: No ask	Simple ask	Facebook	p-value
(a) Measured before treatment					
Female	45.828	45.623	45.934	45.856	0.484
	(0.097)	(0.194)	(0.158)	(0.157)	
Age	0.375	0.377	0.372	0.376	0.864
	(0.004)	(0.008)	(0.006)	(0.006)	
Past donations	1.193	1.172	1.225	1.176	0.005
	(0.011)	(0.023)	(0.019)	(0.019)	
Past blood donations	0.616	0.595	0.618	0.627	0.121
	(0.007)	(0.013)	(0.011)	(0.011)	
Past plasma donations	0.541	0.542	0.557	0.526	0.006
	(0.009)	(0.018)	(0.015)	(0.015)	
Email successful before	0.863	0.864	0.856	0.868	0.198
	(0.003)	(0.006)	(0.005)	(0.004)	
(b) Measured after treatment					
Donated blood in study period	0.090	0.080	0.091	0.094	0.054
	(0.002)	(0.004)	(0.004)	(0.004)	
Donated plasma in study period	0.049	0.046	0.053	0.048	0.286
	(0.002)	(0.003)	(0.003)	(0.003)	
Multiple donations in study period	0.001	0.000	0.001	0.000	0.087
	(0.000)	(0.000)	(0.000)	(0.000)	
Email read	0.226		0.226	0.226	0.966
	(0.004)		(0.006)	(0.005)	
Observations	15326	3738	5590	5998	

Notes: This table reports means and standard errors in parentheses. "Past donations" are computed over the 11 months before the experiment. "Email successful before" describe the share of donors successfully reached via email by the organization in the year before the experiment. The p-value in the last column is from a Kruskal-Wallis test comparing the 3 groups.

Table E.6: Demographics, donation behavior, and engagement of donors in the Facebook treatments

	’21			’21		
	Unread	Read	p-value	Unread	Read	p-value
(a) Measured before treatment						
Female	0.381	0.416	0.011	0.376	0.393	0.359
	(0.006)	(0.013)		(0.017)	(0.006)	
Age	44.211	44.465	0.330	38.724	44.678	0.000
	(0.146)	(0.327)		(0.458)	(0.143)	
Past donations	1.267	1.530	0.000	1.125	1.345	0.000
	(0.016)	(0.037)		(0.048)	(0.016)	
Can donate blood in March	0.773	0.719	0.000	0.825	0.755	0.000
	(0.005)	(0.011)		(0.014)	(0.005)	
Can donate plasma in March	0.886	0.859	0.002	0.905	0.872	0.008
	(0.004)	(0.009)		(0.010)	(0.004)	
Past blood donations	0.879	1.035	0.000	0.748	0.921	0.000
	(0.011)	(0.024)		(0.031)	(0.011)	
Past plasma donations	0.353	0.460	0.001	0.337	0.375	0.262
	(0.011)	(0.029)		(0.036)	(0.011)	
Email succesful before	0.728	0.943	0.000	0.774	0.749	0.123
	(0.005)	(0.006)		(0.015)	(0.005)	
(b) Measured after treatment						
Donated blood in study period	0.076	0.097	0.005	0.070	0.085	0.138
	(0.003)	(0.008)		(0.009)	(0.003)	
Donated plasma in study period	0.035	0.044	0.096	0.032	0.041	0.229
	(0.002)	(0.005)		(0.006)	(0.002)	
Multiple donations in study period	0.001	0.001	0.674	0.001	0.000	0.263
	(0.000)	(0.001)		(0.001)	(0.000)	
Optout	0.000	0.000	0.025	0.005	0.001	0.022
	(0.000)	(0.000)		(0.003)	(0.000)	
Observations	7453	1549		789	7628	

Notes: This table reports means and standard errors in parentheses. “Past donations” are computed over the 11 months before the experiment. “Email successful before” is the share of donors successfully reached via email by the organization in the year before the experiment. “Opt-out after treatment” people are not considered in any statistic and reported only in the dedicated row. The p-values in columns 3 and 6 are from a Kruskal-Wallis test comparing the “Not read” and “Read” groups for both the communication channels.

F. Survey experiment materials

As per pre-registration, we recruited for this experiment a random sample of 20000 repeat donors included in the main experiment that was conducted in the spring of 2021. 3016 subjects completed the survey. Subjects were randomized into 1 of 4 versions of the survey, each summarized in Table F.1. The survey was programmed in Qualtrics and launched on August 19, through a personalized Whatsapp invitation that subjects received from the official account of Avis Toscana. We kept the survey open for 7 days.

Table F.1: Versions of the survey

Version 1	Version 2	Version 3	Version 4
Q1, Q2	Q3, Q4	Q1, Q2	Q3, Q4
Q5, Q6, Q7	Q8, Q9, Q10	Q11, Q12 or Q13, Q14	Q11, Q12 or Q13, Q14

Survey questions labeled in Table F.1 are presented in Online Appendix F.1.

Table F.2: Follow-up survey: demographics, donation behavior, and engagement (means and standard errors in parentheses)

		Survey versions				
	Whole sample	Version 1	Version 2	Version 3	Version 4	p-value
Female	0.426	0.430	0.449	0.426	0.398	0.249
	(0.009)	(0.018)	(0.018)	(0.018)	(0.018)	
Age	44.365	44.633	44.719	44.270	43.835	0.457
	(0.231)	(0.459)	(0.471)	(0.450)	(0.465)	
Past donations	1.894	1.975	1.798	1.906	1.896	0.246
	(0.028)	(0.061)	(0.054)	(0.054)	(0.055)	
Past blood donations	1.231	1.216	1.210	1.235	1.262	0.729
	(0.018)	(0.037)	(0.036)	(0.037)	(0.037)	
Past plasma donations	0.567	0.652	0.499	0.571	0.545	0.092
	(0.023)	(0.052)	(0.043)	(0.041)	(0.045)	
Observations	3016	755	751	763	747	

Notes: Past donations are computed over the 11 months before the experiment. The p-value in the last column is from a Kruskal-Wallis test comparing the 4 groups.

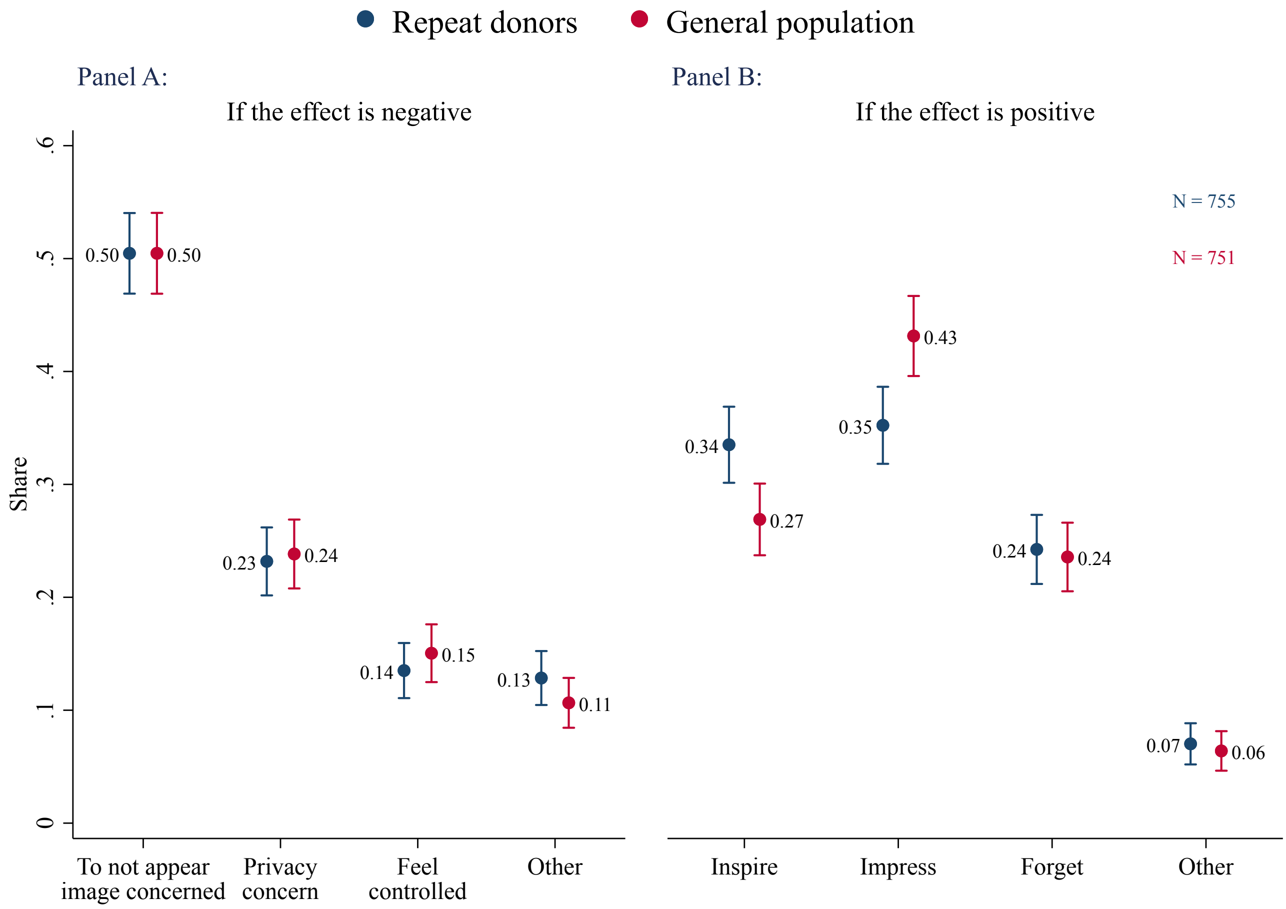


Figure F.1: Main mechanisms of social recognition expected by repeat donors

Notes: Panel A is based on question Q7 and Q10 (Figures F.3 panel b and F.5), while Panel B is based on question Q6 and Q9 (Figures F.3 panel c and F.4).

F.1. Questions

(a) Q1

Let's call **altruistic** a person that is willing to sacrifice for the sake of helping others even if they get nothing in return.

Think of a **random sample of 100 repeat blood donors from Avis Toscana**, how many of these would you believe to be

Please enter the number of people out of 100 that you would attribute to each of the following 4 types. The sum of the 4 numbers you provide should be 100.

Very altruistic	0
Somewhat altruistic	0
Not very altruistic	0
Not at all altruistic	0
Total	0

(b) Q3

Let's call **altruistic** a person that is willing to sacrifice for the sake of helping others even if they get nothing in return.

Think of a **random sample of 100 adults (18+) from Tuscany**, how many of these would you believe to be

Please enter the number of people out of 100 that you would attribute to each of the following 4 types. The sum of the 4 numbers you provide should be 100.

Very altruistic	0
Somewhat altruistic	0
Not very altruistic	0
Not at all altruistic	0
Total	0

(c) Q2

We would call **image concerned** a person that is willing to sacrifice for the sake of being seen by others as charitable and impress.

Think of a **random sample of 100 repeat blood donors from Avis Toscana**, how many of these would you believe to be

Please enter the number of people out of 100 that you would attribute to each of the following 4 types. The sum of the 4 numbers you provide should be 100.

Very image concerned	0
Somewhat image concerned	0
Not very image concerned	0
Not at all image concerned	0
Total	0

(d) Q4

Let's call **image concerned** a person that is willing to sacrifice for the sake of being seen by others as charitable and impress.

Think of a **random sample of 100 adults (18+) from Tuscany**, how many of these would you believe to be

Please enter the number of people out of 100 that you would attribute to each of the following 4 types. The sum of the 4 numbers you provide should be 100.

Very image concerned	0
Somewhat image concerned	0
Not very image concerned	0
Not at all image concerned	0
Total	0

Figure F.2: Questions 1-4

(a) Q5

Think of a **random sample of 100 repeat blood donors from Avis Toscana**, who received a Whatsapp message from Avis Toscana to encourage their donation.

Half of them were randomly assigned to receive a simple ask to donate blood in March 2021 due to shortages, while the other half was additionally offered the opportunity to be recognized for their donation on the public Facebook page of Avis Toscana.

Which of these two groups do you think was more likely to donate in March 2021?

- The group that received just the simple ask via Whatsapp
- The group that on top of the ask via Whatsapp was offered public recognition
- Both groups donated similarly

(b) Q7

From these two groups, imagine that the group making more donations in March 2021 was <u>the group that received just the simple ask via Whatsapp</u>.

Which of the following do you think would be the most important reason for this finding?

- Social recognition decreases giving among people who don't want to be seen as image concerned
- Social recognition decreases giving among people who are strongly concerned about their privacy
- Social recognition decreases giving among people who would feel controlled
- Other (please specify)

(c) Q6

From these two groups, imagine that the group making more donations in March 2021 was <u>the group that on top of the ask via Whatsapp was offered public recognition</u>.

Which of the following do you think would be the most important reason for this finding?

- Social recognition increases giving among people who want to inspire others on Facebook with their donation
- Social recognition increases giving among people who would easily forget if they only receive a simple ask
- Social recognition increases giving among people who want to impress others for their altruism on Facebook with their donation
- Other (please specify)

(d) Q8

Think of a **random sample of 100 adults (18+) from Tuscany**, who received a Whatsapp message from Avis Toscana to encourage their donation.

Half of them were randomly assigned to receive a simple ask to donate blood in March 2021 due to shortages, while the other half was additionally offered the opportunity to be recognized for their donation on the public Facebook page of Avis Toscana.

Which of these two groups do you think was more likely to donate in March 2021?

- The group that received just the simple ask via Whatsapp
- The group that on top of the ask via Whatsapp was offered public recognition
- Both groups donated similarly

Figure F.3: Questions 5-8

From these two groups, imagine that the group making more donations in March 2021 was the group that on top of the ask via Whatsapp was offered public recognition.

Which of the following do you think would be the most important reason for this finding?

- Social recognition increases giving among people who want to inspire others on Facebook with their donation
- Social recognition increases giving among people who want to impress others for their altruism on Facebook with their donation
- Social recognition increases giving among people who would easily forget if they only receive a simple ask
- Other (please specify)

Figure F.4: Question Q9

Notes: Options were displayed in random order.

From these two groups, imagine that the group making more donations in March 2021 was the group that received just the simple ask via Whatsapp.

Which of the following do you think would be the most important reason for this finding?

- Social recognition decreases giving among people who would feel controlled
- Social recognition decreases giving among people who don't want to be seen as image concerned
- Social recognition decreases giving among people who are strongly concerned about their privacy
- Other (please specify)

Figure F.5: Question Q10

Notes: Options were displayed in random order.

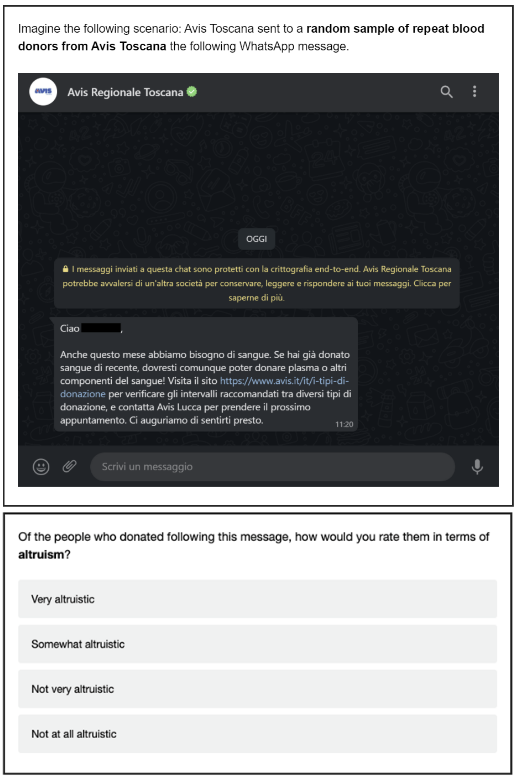


Figure F.6: Question Q11

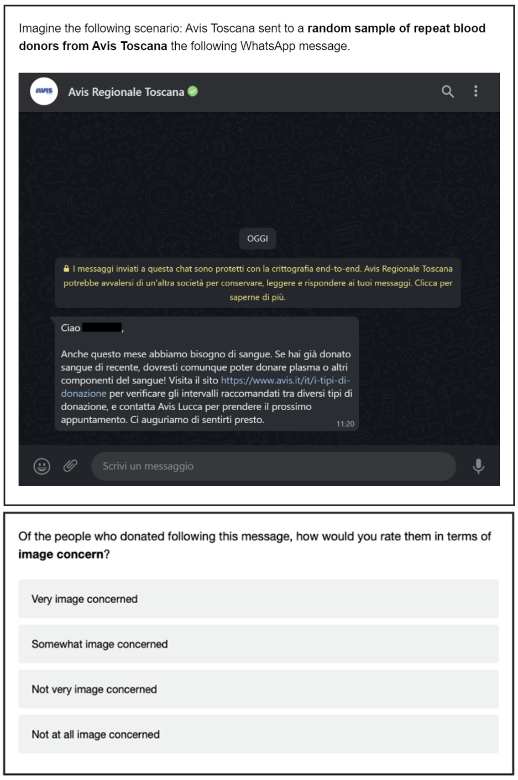


Figure F.7: Question Q12

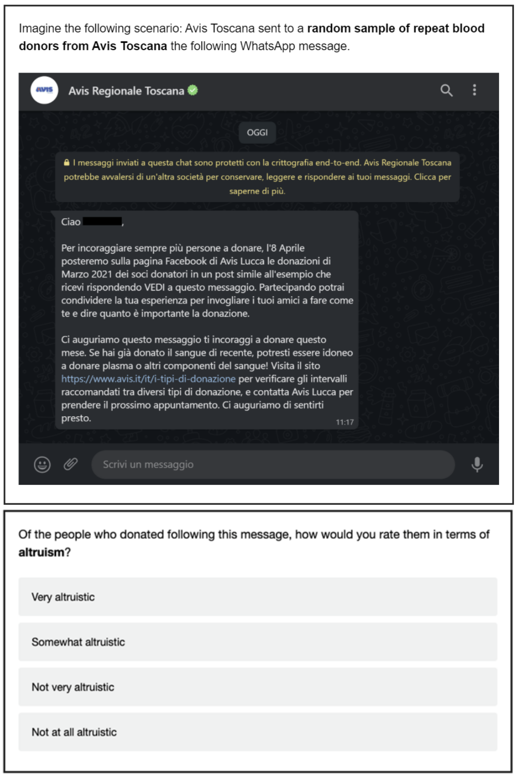


Figure F.8: Question Q13

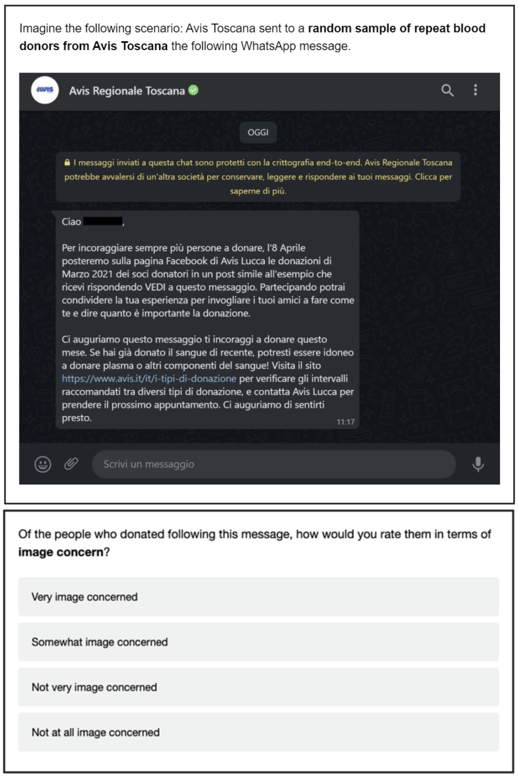


Figure F.9: Question Q14