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Self-Promoted Altruism: Looking Bad by Doing Good?

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Abstract

We experimentally test whether the need to actively self-promote one's own prosocialness for others to become aware of it has an adverse effect on prosocial behavior. In an experiment design resembling that of Ariely, Bracha, and Meier (2009), participants could demonstrate prosocialness by engaging in a real-effort task to collect money for charity. We find that self-promotion in general was considered negatively among participants, but cannot support our hypothesis that it adversely affects prosocial behavior in our experiment setting. Our results weakly suggest that females are less likely to engage in self-promotion and that a need to self-promote affects females' prosocial efforts more negatively than males'. The results point to the direction that social-image concerns may be less important than previously thought in the "Click for Charity"-setting. In light of this, we critically discuss the findings of Ariely, Bracha, and Meier (2009) and also suggest improved methods for future research.

Keywords: altruism, signaling, self-promotion, social cognition, behavioral economics

JEL: C90, D03, D64, Z13

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1. Introduction

People commonly contribute to charity, volunteer to work for good causes, donate blood, or engage in other activities that primarily are to the benefit of others or to society as a whole. Two interesting questions discussed by previous research are "how come some act more prosocially than others?" and subsequently "how can we encourage more of this type of behavior?" While the idea that prosocial behavior can arise from a genuine concern for others is certainly not foreign to economists (for example, see Smith, [1759] 2000), other motivations have been found to stimulate good deeds as well. Incentive sources can be said to be either internal or external. Internal rewards are derived if the actor not only considers the well-being of the recipient but also acts prosocially because he wishes to fulfill a social norm, or receives a private-good benefit from knowing he is the source of this well-being. Andreoni (1989, 1990) extended the altruism model to include this sort of *impure altruism* by introducing the notion of a *warm glow* feeling prosocial actors can experience that is to the benefit of themselves. While external rewards such as pay, gift cards, and thank-you letters can encourage prosocial behavior as well, research has also devoted attention to the detrimental effect extrinsic motivation may have as it can *crowd out* intrinsic motivation (see Frey and Jegen, 2001 for a review).

External rewards from acting prosocially can also be immaterial and come in the form of praise and prestige, for example. In essence, actors can yield reputational gains by signaling information about themselves to others (see e.g. Ellingsen and Johannesson, 2008; Bénabou and Tirole, 2006). In return, observers offer them praise, status, access to favorable relationships, among other rewards (Barclay, 2004; Price, 2006; Kahneman, Knetsch, and Thaler, 1986; Stiff and Van Vugt, 2008). As image motivation has been shown to be important source of prosocialness (Harbaugh, 1998), perhaps that is why charities for long have offered, for example, pins and bumper stickers for donations of the more casual size, and have named libraries and raised statues in recognition of larger donations. Some charities are also attempting to leverage the public's vast interest for social media channels. For example, the Swedish Blood Bank[1] has in a promotional campaign encouraged by-passers to register for blood donation, take a photo in their adjacent photo booth, and subsequently upload the photo on social media for others to know about their good deed (Eventomatic, 2016).

The effects of this kind of self-promoting behavior can however be double-edged. While someone may experience reputational gains from telling others about his prosocialness, the true intentions

[1] Swedish: *Blodcentralen*.

behind the agent's decisions may come under suspicion by his surroundings. Given that not only the agent but also those around him are aware of the positive effects of being liked by others, spectators of this self-promoted sort of altruism may discredit his actions merely as image-seeking and not truly altruistic. As opposed to having an observer incidentally notice a good deed, actively self-promoting one's prosocialness can in other words create a noisy signal where the observer may have difficulties inferring the person's underlying motivation. Berman et al. (2015) termed this as "the braggart's dilemma" and indeed found observers to be disapproving of self-promoted altruism. This trade-off between positive and negative reputational effects could concern prospective prosocial actors and may refrain them from telling others of what they have done. With eliminated—or at least diminished—reputational gains, some may even decide to not act prosocially (Bénabou and Tirole, 2006).

We test the hypothesis that a need to self-promote one's own prosocialness for others to become aware of it impair the motivation to act prosocially. We describe the underlying mechanism behind this as self-promotion causing the signal sent to observers about one's prosocial type to become diluted in comparison to when observers cannot infer that the agent himself chose to have his behavior known to others ("modest" altruism, as labeled by Bénabou and Tirole, 2006). Alongside this, we also test the frequency of self-promotion and investigate whether these results differ by gender. To the best of our knowledge, none of these hypotheses have been tested before. Having recruited a large number of participants (n=216), we use Ariely, Bracha, and Meier's (2009) "Click for Charity" experiment design to allow participants to signal prosocialness by pressing buttons to benefit a charitable cause. Despite finding that choosing to self-promote implies negative image effects, we cannot support our main hypothesis in this setting. As Ariely, Bracha, and Meier (2009) underpin their results with the assumption that they are chiefly explained by participants' image concerns, we devote a segment to critically discuss their findings as ours suggest that this assumption may be less robust than previously thought. We also find tentative evidence to suggest that women react more negatively to the self-promotion treatment than men do, and also find men to self-promote more often than women. However, we see these findings principally as a direction for future research rather than conclusive evidence of gender differences in this realm.

The remainder of this article is organized as follows: in Section II, previous research is presented. Section III introduces the experimental design and procedure, our empirical strategy and a critical discussion of the experiment design. Results are presented in Section IV. Section V discusses the findings and suggests direction for future research, and Section VI offers a conclusion of the paper.

2. Previous Research

2.1 Theoretical Evidence on Image Motivation and Prosocial Behavior

First, we examine the theoretical evidence on how prosocial behavior can be driven by image concerns. Long-standing traditions in social psychology have emphasized humans' desire to be viewed in a favorable light by others (see e.g. Baumeister, 1982; Jones and Wortman, 1973; Leary and Kowalski, 1990). Several economic models outline how such image concerns can encourage prosocial behavior. For instance, Akerlof (1980) models how a social norm, despite being costly to follow, can survive if breaking it means suffering a sufficiently large reputational loss. Bernheim's (1994) model applies a similar approach but instead incorporates social factors related to complying with norms directly into individuals' preference functions.

Other theoretical work has placed emphasis on how actors in turn react to others' norm compliance and what this says about their prosocial type. Levine (1998) builds on the idea that preference functions differ between individuals insofar as how much weight they place on others' utility versus their own, and show that these preferences also depend on the preference function of the person they are interacting with. For instance, his model can explain why people would incur a personal cost to punish others for breaking a social norm and why we care if an action was intentional or occurred by incident. In a similar vein, Ellingsen and Johannesson's (2008) model describes how an agent does not only care about the incentive he is given but also what this incentive says about the principal's character. In contrast to the standard principal-agent model where agents are unaffected by the level of fixed pay and unconditionally incentivized by remuneration that varies by performance, their model captures how an agent that receives a high fixed pay can be motivated to work harder as the incentive provided evidences that the principal trusts the agent. Meanwhile, a principal that decides on the reward only after the agent's performance has been evaluated instead signals a more transactional type of relationship. This, in turn, affects the agent's motivation negatively. Concluding, the model explains why we are more likely to act prosocial to individuals that have demonstrated prosocialness themselves.

Both Levine's (1998) and Ellingsen and Johannesson's (2008) work leverage the notion that prosocial actions allow an individual to signal what type of person he is and that this signal can affect how others perceive him, which in turn can stimulate the person's behavior. Andreoni and Bernheim (2009) equally build their theoretical approach on the concept that our actions are shaped by how we think others will interpret them but place emphasis on the idea that people like to be seen as fair by others. A number of related models have also explored the behavioral consequences of social image concerns more generally (e.g. Ireland, 1994; Bagwell and Bernheim,

1996). Other scholars have instead focused on how self-image, i.e. how a person views herself, can impact prosocial behavior. For example, Bodner and Prelec (2003) describe a model where self-image is uncertain and individuals place a high value on complying with social norms and personal beliefs, where past actions help infer as to which moral type one belongs to.

2.2 Image Motivation through an Avoidance of Stigma

There is an array of empirical evidence to support the notion that image concerns affect the supply of prosocial behavior. Cain, Dana, and Newman (2014) categorized how image concerns can incentivize prosocial behavior by describing two variants of image motivation, denoting how image concerns can either pressure people to "give in" or to motivate them to "give" by the promise of praise and recognition. We begin by examining the first aspect. As outlined by e.g. Akerlof (1980) and Bernheim (1994), social cues have been found to make norms more prevalent and costly to avoid. For example, in a modified version of the dictator game[2] set up by Dana, Cain, and Dawes (2006), dictators could exit the game after making their allocation choices if they forewent 10% of their total endowment. Comparing two treatments, they found that a significant share of dictators paid to exit when recipients would be informed of the game procedure if the dictator would not have exited, while almost none chose to do so when the recipient would be left unknowing in any case. In a follow-up study, Broberg, Ellingsen, and Johannesson (2007) found that dictators were willing to give up an average of 18% of their endowment to exit the game. These two studies, and a related one by Lazear, Malmendier, and Weber (2012), add to the narrative that some good deeds only occur as a result of social pressure.

Researchers have been able to translate similar findings into field settings as well. DellaVigna, List, and Malmendier (2012) designed a door-to-door fundraiser where one group of households were let known in advance that solicitors would visit them, and one group was not. They found that the share of households opening their doors after being informed was 9% lower than the share of households that were unaware of the coming fundraiser (34%). The authors estimated there was a "social pressure cost" of up to an average of $3.55 of saying no to a solicitor. In a related field study, Andreoni, Rao, and Trachtman (2017) found a verbal "please give"-request from solicitors (as opposed to them remaining silent, or ringing a bell) to both increase the amount of giving and the number of givers, but also to cause a significant share to avoid passing by the solicitor. The

[2] The dictator game is a common experiment design in the economics science, where one participant is assigned the role as dictator and one the role as recipient. In the standard version, the dictator is allocated a certain amount of money which he or she then decides how (if) it should be split with the other player. The recipient is left with no choice but to accept the amount that the dictator has decided on. See Engel (2011) for a review.

authors posit that being asked verbally stimulates empathy, which makes giving hard to resist and in turn causes some to choose to avoid the situation altogether.

2.3 Image Motivation through a Pursuit of Distinction

Another part of the literature has focused on the share of image motivation that encourages people to make good deeds as a way to set them apart from others ("giving," or prosocial behavior that people voluntarily engage in, in the words of Cain, Dana, and Newman, 2014). For instance, in public fundraisers where donors can opt to be recognized and are categorized by the size of the donation (e.g. as a "supporter" for donating 50–99 USD or as a "champion" for donating 100–199 USD), studies have found donors to round up their donation and by a margin slim-to-none place themselves within a category (Harbaugh, 1998; Andreoni and Petrie, 2004). In a similar fashion, Lacetera and Macis (2010) found blood donors to increase the frequency of their donations significantly as they approached a threshold that meant they would receive a prize—but only if the prize ceremony was announced in the local newspaper and not otherwise.

Karlan and McConnell (2014) proposed that some individuals argue that they promote their good deeds publicly in order to encourage others to also engage in prosocial activities, rather than for their own personal gain. In light of this, they conducted a laboratory experiment that teased apart the two theories and found that public recognition itself increased giving, but not the ability to influence others' giving. Hardy and Van Vugt (2006) put forward a notion of competitive altruism, placing an emphasis on how prosocial actions and actors can be put in comparison to others and how this spurs public prosocial behavior. Anthropologists have found this to be case in groups as diverse as the hunter-gatherer society of Ache in Paraguay, where hunters in exchange for social benefits often abstained from eating their own prey (Hill and Hurtado, 1996) and the Native American Kwakiutl tribe, where tribal chiefs competed for status by giving away their possessions (Cole and Chaikin, 1990).

2.4 Gender and Image Motivation

Social cues and reputational concerns have also been suggested to affect men more than women. Compared to women, men have been found to be more likely to aid strangers by engaging in both trivial and more heroic acts of helping (Latane, 1970; Johnson, 1996). Goldberg (1995) found this "evolutionary adaptive" behavior to also be present when observing panhandlers, finding men to donate more money to female panhandlers than they did to male panhandlers, while women donated less frequently but more equally to female and male recipients. More generally, men seem to help others more often than women, but this difference only holds when their good deeds can be observed and not otherwise (see Eagly and Crowley, 1986 for a review). Both genders have

nevertheless been found to engage in conspicuous charity to a certain extent (Griskevicius et al., 2007; Iredale, Van Vugt, and Dunbar, 2008). The gender differences are potentially explained by heterogeneous demand sides as some evidence suggests that women, in comparison with men, to a higher extent seek for altruistic traits in their romantic partners (Barclay, 2010).

2.5 The Demand Side of Prosocial Behavior

As image motivation fundamentally is a consequence of what others think, we further explore how prosocial behavior is perceived by observers by looking more generally at the demand side. Overall, the quest for a favorable social-image seems to be rational. For example, Kahneman, Knetsch, and Thaler (1986) found people to be more generous to those that they had observed to be generous in the past. Other studies show that prosocial individuals enjoy a higher status (Price, 2006), are seen as more trustworthy (Barclay, 2004), and are more desirable as friends and romantic partners (Stiff and Van Vugt, 2008; Miller, 2007).

Meanwhile, as described by e.g. Ellingsen and Johannesson (2008), intentions matter. Evidence from laboratory experiments shows that people are more inclined to interact with and reciprocate to people who has done them a favor when they can infer that the favor was motivated by a genuine concern rather than a cost-benefit analysis (Ames, Flynn, and Weber, 2004; Simpson and Willer, 2008) and when they can be certain of the favor-giver's intent (Rand, Fudenberg, and Dreber, 2015). Similarly, observers give less acclaim to donors who have been personally affected by the cause to which they are donating than donors who have not (Lin-Healy and Small, 2012). In general, people appear to be convinced that a lot of prosocial behavior takes place due to self-interest rather than genuine concern (Critcher and Dunning, 2011). Newman and Cain (2014) even found people to perceive actions that involved both charitable and personal benefits as less socially desirable than equivalent actions that did not generate any charitable benefits, and concluded that doing some good sometimes can be considered worse than doing no good at all. The distaste for conjoining prosocialness with other, selfish, motives also seems to be analogous for organizations (Peter McGraw, Schwartz, and Tetlock, 2012). In short, we seem to prefer prosocial actors that are perceived to act out of good spirit over those who are looking to reap benefits in return for good behavior.

Fiske's (1992) relational theory provides a framework to describe the underlying mechanisms as to why prosocial actions with potentially selfish end goals are stigmatized. Fiske and several other social psychologists have emphasized the difference between communal relationships, where helping others in need of a favor is appropriate, and exchange relationships, where receiving a favor in exchange for helping others is appropriate. Not only do people feel obliged to abide by

the distinct principles that each type of relationship entails, but they also impose them on other people (Fiske, 1992). There are in other words moral boundaries to when involving a cost-benefit analysis is considered appropriate (Fiske and Tetlock, 1997). For example, asking for money in return may be considered ordinary if your employer asks you to come to the office during your vacation, but not if your grandmother asks you to come help her change a lightbulb (Heyman and Ariely, 2004). Since we infer dispositional traits based on perceived motives of others (Reeder, 2009; Reeder et al., 2002), it seems reasonable to assume people will attempt to hide signs of potential breaches of the norms that govern communal and exchange relationships. As altruism by definition means unselfishly regarding for others' welfare (Batson, 1998), a conclusion near at hand is that most would consider it to fall under the communal relationship-frame.

2.6 How Noisy Signals Can Crowd Out Prosocial Behavior

Given that observers' perceptions are the fundamental driver behind the image motivation but that these perceptions seem to vary considerably depending on the situation at hand, one may wonder how the supply side of prosocial behavior is affected when incentives that could raise suspicion of the actor's true intentions are introduced. The idea of incentive sources interacting with each other is not novel. Psychologists have denoted the phenomena of extrinsic motivation crowding out intrinsic motivation as the *overjustification effect* and have found it to have significant impact on people's behavior with regards to, for example, education, work, and volunteering (see Deci, Koestner, and Ryan, 1999 for a review).

In a similar fashion, Bénabou and Tirole's (2006) model outlines how various incentive sources can have a detrimental effect on the reputational gains of being prosocial. The model is related to those of e.g. Ellingsen and Johannesson (2008) and Levine (1998) as it employs a signaling framework where actors' intentions play a fundamental role, but it more thoroughly investigates how prosocial behavior can be crowded out by extrinsic incentives. The model assumes heterogeneity in image concerns and individuals' degree of altruism, meaning inferences about people's underlying motives for acting prosocially in public are difficult to make. When prosocial behavior takes place in public and the observers can infer that the actor yields some sort of benefit (be it monetary or reputational, for example) from the action, their perception of the actor can change. Bénabou and Tirole (2006, p.1654) describe the presence of extrinsic incentives to diminish the reputational value of good deeds as they create "doubt about the extent to which [the good deeds] were performed for the incentives rather than for themselves." Thus, additional incentives can induce a partial or even net crowding out of prosocial behavior.

The notion of extrinsic incentives crowding out the signaling value of prosocial actions, and subsequently affecting those actors' behavior, was depicted in the now classic example of blood donors. Titmuss (1970) famously argued that providing monetary incentives to blood donors could in fact decrease supply as the signaling value of giving blood would diminish. In a study intimately related to the present one, Ariely, Bracha, and Meier (2009) experimentally tested a similar effect as they looked at the interaction effects of image motivation and extrinsic motivation on prosocial behavior in a laboratory setting. Participants were instructed to press the 'x' and 'z' keys on a computer keyboard, seated in a room with a group of other participants. Each pair of clicks produced a donation to a charitable cause. In a 2x2x2 design, the authors varied the visibility of the effort, extrinsic incentives (no personal compensation vs. a monetary payoff), and the nature of the cause (a "good" and a "bad" cause). As a direct effect of image motivation, the authors argue, the average number of pairs clicked increased from 548 pairs in the private condition to 822 pairs in the public condition ($p<0.05$) when there was no monetary payoff involved and contributions went to a "good" charity. Their key finding was however that while monetary incentives had a significant positive effect on effort in the private condition, it had no effect in the public condition. As outlined by Bénabou and Tirole (2006), a less-informative signal was said to have adversely affected participants' image motivation to act prosocially as the others in the room might have suspected they clicked for their own personal gain rather than as a way to generate money to charity. Mediated by the positive monetary incentive, this resulted in an insignificant difference of the means of clicks in the two public treatment groups. The authors concluded that while image can serve as a motivation to act prosocially, adding extrinsic motivation can make the signal about one's prosocial type noisier and effectively crowd out prosocial behavior.

In Bénabou and Tirole's (2006) model, the reputational incentive itself can also cause noise to signal sent. As the theoretical and empirical evidence on image motivation shows, appearing to look prosocial often entails personal advantages, which also serves as motivation for people to act more prosocially. Thus, if image concerns are perceived to be instrumental, this can be another source of noise which could affect the supply of prosocial behavior. For this very reason, bragging about one's good deeds in the pursuit of an improved social-image is often self-defeating. Berman et al. (2015) surveyed participants for their opinion of this sort of self-promoting behavior, and found substantial disapproval among the observers. To the best of our knowledge, however, Bénabou and Tirole's (2006) hypothesis that a need to actively self-promote one's prosocial behavior for others to know about it (and thus risk looking as if one is motivated by appearances and not out of a genuine concern for the cause) can crowd out prosocialness has however never been tested empirically—and this serves as the purpose for the present paper.

2.7 Summation of Previous Research

Resounding theoretical and empirical evidence show that image concerns can be a significant incentive source of prosocial behavior. Meanwhile, if observers suspect that a person acted prosocially only after considering a cost-benefit analysis, this can affect the image value negatively. Just like monetary rewards can crowd out the reputational value of prosocial actions, an inference that a person acted good only to receive praise or other reputational benefits can thus instead backfire and spoil the very same image effect yearned for (Bénabou and Tirole, 2006). Conjoining these two effects implies that proclaiming one's prosocialness to others can be a dilemma in itself. In other words, self-promotion of one's own altruism introduces noise to the signal sent to others, and could even revert the signal to a negative sign. Accordingly, we may hesitate to tell others about our good deeds since we could raise suspicion about our intent. Meanwhile, the people we would like to impress and befriend are not always around to see everything that we do. If telling others about our prosocialness means that our actions may be discounted, and not telling others means no one will know what we did, how is our prosocial behavior affected when we must actively self-promote it for others to know about it? Also, as research on gender differences suggests men's prosocial behavior to be more sensitive to social cues than women's, do their behavior differ with regard to self-promoting their prosocial behavior?

3. Experimental Design and Empirical Strategy

We conducted a laboratory experiment where participants could collect money to the Swedish Red Cross, using a design resembling that of Ariely, Bracha, and Meier (2009). The experiment was carried out at the Stockholm School of Economics (henceforth SSE) during four sessions over a total of three days in April 2017. For each session, we randomized participants into two separate rooms and treatment groups**.** Participants had prior to the experiment been instructed to bring a smartphone, which they were told to use in order to access an online survey. The survey was programmed using the software oTree (Chen, Schonger, and Wickens, 2016). Two doctoral students, who did not know the participants, acted as experimenters. We also conducted a post-study with participants from the first study starting four days after all sessions had been run to investigate whether our experiment design captured the effects we were seeking with regard to our research questions.[3] We completed and registered a pre-analysis plan at the AEA RCT registry[4] before the research project was implemented. The pre-analysis plan outlined the experiment design, the hypotheses and tests as depicted below, but did not describe the control variables we use as robustness checks in our regressions. We introduced the control variables to further examine if anything else than our treatment effect could have affected the participants' behavior.

3.1 Experimental Design

3.1.1 Participants

A total of two hundred sixteen students (one hundred thirteen women and one hundred three men) participated in the experiment. All participants were given a lunch voucher[5] as an incentive to participate. The recruitment was primarily targeted toward undergraduate students at SSE that had not attended courses in behavioral economics, but due to a relatively small primary recruitment pool (ca. 700 students) we extended our recruitment to include students in the graduate programs as well. The experiment was announced through emails sent out to all students in our primary recruitment group, messages in university-related groups on social media that was used by most students, and after lectures for undergraduate students. As a result of not having enough participants from the undergraduate program for our final session, we also sent out an email to graduate students that were attending courses in subjects other than Economics. In total, there

[3] As the questions in the post-study effectively debriefed participants about the study, we decided to ask them only after all sessions had been run primarily due to a worry that participants in the first sessions might inform future participants about the purpose of our experiment, which in turn would decrease the study's validity. Furthermore, we also wanted to decrease the risk of participants reflecting on both their own and specific participants' choices in the first study when they answered the questions included in the post-study as this could have biased their answers.

[4] The American Economic Association's registry for randomized controlled trials.

[5] The retail price of the voucher was 99 SEK. The USD/SEK exchange rate was around 8.8 at the time of the experiment.

were one hundred fifty-four undergraduate students, forty-eight graduate students and fourteen exchange students participating in the experiment.

Five participants failed to complete the survey and were thus excluded from the results. Eight participants clicked less than 300 button pairs and also wrote in a comment section we included that they had experienced problems that we have classified as "severe."[6] We used this information for a dummy variable coded 1 if the participant had indicated such problems (otherwise 0), which we used as a control variable in our regressions as a robustness check.

3.1.2 Procedure

The participants received a random seat number in one of two rooms just before the experiment started and were instructed to take their assigned seat, effectively randomizing both the room and seat for each participant. Each room represented one treatment group. We conducted the experiment for both treatment groups simultaneously during all four sessions. All groups consisted of twenty-six to thirty participants, except for one which consisted of twenty-three participants due to late dropouts.[7] In the rooms, the experimenters told the participants to remain silent, read the instructions on the board in the room and to enter a link to a website that directed them to our survey (see Appendix III for the complete instructions and the survey). Participants were also asked to raise their hand in silence and wait for the experimenter to approach them in case they had any questions.

In the survey, participants could choose to do a repetitive task (alternatingly pressing buttons) for a maximum of 5 minutes to donate money to the Red Cross. The task was implemented to allow participants to signal prosocialness through exerting a high effort. Importantly, participants did not have to donate any money themselves: they were instructed that we would donate money to the charity on their behalf. The donation amount increased as the participants clicked more pairs of buttons but with decreasing marginal returns, according to the following payment scheme: 0.25 SEK was donated for each of the first 100 pairs of button pressed, 0.188 SEK for each of the next 100 pairs, 0.094 SEK for each of the next 100 pairs, 0.047 SEK for each of the next 100 pairs, …, and 0.003 SEK for each pair above 700. The payment scheme had marginally decreasing returns as this was the approach of Ariely, Bracha, and Meier (2009), and we wanted to be as comparable to their study as possible. The amount of button pairs pressed is a primary outcome variable of this experiment.

[6] A table presenting the amount of buttons pairs pressed, the comment written, the treatment group and gender of each of these participants is shown in Appendix I.
[7] A table illustrating the number of participants by gender and group for each session is shown in Appendix II.

To test the hypotheses, the two treatment groups differed in two respects. Firstly, the groups differed in whether participants could choose to have their name associated with their donation or not, and secondly in a follow-up question that was asked in the end of the survey. In treatment group 1, participants were instructed that the ten individuals in the room with the highest donations would have their name announced and be asked to stand up to be recognized for their effort after everyone had finished the survey (hereby referred to as the "auto-promotion" condition).[8] Participants in treatment group 2 were instead instructed that the ten highest donations in the room would be announced after the surveys had been finished, and each of the corresponding participants would be able to *choose* whether they would have their name announced and be asked to stand up in recognition for their effort (hereby the "choice" condition). The frequency of self-promotion is another primary outcome variable of this experiment.

To ensure that the (non)-decision was common knowledge[9] among all participants and to remind the participants of the process post the clicking phase of the experiment, treatment-specific instructions were together with the payment scheme also presented on a board in the room during the entire experiment. Separating the groups in this distinct manner allowed us to test the effect of self-promotion on prosocial behavior in our specific setting. In the follow-up section, participations in the auto-promotion condition were asked: "would you yourself have donated a different amount if the participants with the ten highest donations would have had the CHOICE whether they want to be recognized by name or stay anonymous instead?" Participants in the choice condition were instead asked if they would have donated more if "ALL" ten participants with the highest donations would have been recognized by name. Answers to both questions were collected using a 5-point Likert scale (1="less," 2="somewhat less," 3="same," 4="somewhat more," 5="more"). The aggregate responses to these questions serve as a validation of our analysis regarding the main hypothesis and are presented in Appendix IV.

3.1.3 Supplemental Parts of the Survey

Before beginning the donation process, participants were asked which gender they identified with and were also asked to supply contact information to enable us to contact them after the

[8] This is in contrast to Ariely, Bracha, and Meier (2009), where all participants were asked to stand up in the public condition. Only the ten highest donors were asked to stand up in our study since participants in treatment group 1 who would have preferred to click few pairs of buttons in case their decision was private could have felt pressured to click more if all results had been presented. Meanwhile, the same type of participants in treatment group 2 simply could have opted to remain anonymous, which would mitigate our hypothesized treatment effect. The potential effects this could have are discussed in Section 5.2.2.

[9] Common knowledge in this case means that all participants knew that everyone else in the room had been faced with the same decision as themselves (mutual knowledge), and that all participants knew that all other participants also knew everyone had been presented the same decision (et cetera).

experiment. In order to establish there was an image value associated with contributing money to the Red Cross, we asked participants what they personally thought of "the Swedish Red Cross, its mission, and work," and also what they believed those around them thought (rated on a 1–10 Likert scale, from 1 "strongly dislike" to 10 "strongly like"). We present the result from these questions for the complete sample in Appendix IV. The answers to these questions were also tested as control variables in our regressions.[10] Participants could practice the task by clicking buttons for up to ninety seconds before commencing the actual task. We also asked four control questions to ensure that participants had read and understood the instructions of the experiment. If the correct answer to a control question was not provided, the screen displayed an error message and the participant had to try again until the correct answer was given to be passed on to the following page. The ten highest donations and names were announced per the treatment group and the choices of participants after all surveys in the room had been completed, and the Red Cross was in receipt of the donations within a week after all sessions had been conducted.

3.1.4 Post-study

Four days after the last session of the experiment had been held, we sent out a survey by email to everyone that had participated in our study (see Appendix V for the survey). The survey was designed with the program Qualtrics. Participation was un-incentivized in a strict monetary sense, but we advertised that respondents could access a debrief of the study and a summary of our results after all questions had been answered on the first page. One hundred and forty-two participants (66% of the total in the first study) completed the survey.

In the survey, respondents were first briefed about the two treatment groups and their difference with regard to the (non)-choice of promotion. Following this, respondents were asked to rank the social desirability (-5 to +5 on an 11-point Likert scale) of a participant whose donation was one of the five highest in the room and who beforehand knew that if the participant's donation was among the ten highest in the room he or she i) would be left with *no choice* as to whether or not she would be recognized, and was subsequently recognized when the donation process was over (a participant in the auto-promotion condition, denoted "type 1" below) ii) would be able to *choose* if she wanted to be recognized, and chose *not* to be recognized (a participant that chose to remain anonymous in the choice condition, type 2) iii) would be able to *choose* if she wanted to be recognized, and also *did* choose to be recognized (a self-promoter in the choice condition, type 3). Respondents were also asked to rate their own competitiveness on an 11-point Likert scale ranging

[10] In addition to the follow-up question specific for each treatment group, participants also answered an open-ended question about their motivation to donate as much as they did. Finally, we also asked if the participant had experienced any problem using the software.

from 0=”not competitive at all” to 10=”very competitive,” a measure which we implemented for explorative reasons rather than to aid in testing our main hypothesis since this measure was not included in our pre-analysis plan. By asking for respondents’ university registration number in both surveys, we could link the answers for 137 participants that completed the two surveys. A table of regressions testing the competitiveness variable is shown in Appendix VI.

3.2 Empirical Strategy

3.2.1 Primary Hypotheses

The focus of the study is to test if the need for an active choice to promote one’s own prosocial activities for others decreases the social image value, and thereby has an adverse effect on individuals’ prosocialness. Adjacent to this, we also investigate self-promotion frequency.

Hypothesis 1: *Participants act less prosocially (donate less) if an active choice is needed for the information about the behavior to become public as compared to an automatic announcement. In our experimental setting, we thus expect participants to click less in the choice condition compared to the auto-promotion condition.*

To investigate this hypothesis we run the following OLS regression:

$$b_i = \beta_0 + \beta_1\, Choice_i + \beta_2\, OwnPerc_i + \beta_3\, OthersPerc_i + \varepsilon_i$$

where b_i is the number of buttons clicked by individual i, $Choice_i$ is a dummy coded 1 if participant i was faced in the choice condition group and 0 if not, $OwnPerc_i$ is participant i’s own perception of the Red Cross, $OthersPerc_i$ is participant i’s belief about others’ perception of the Red Cross and ε_i are the robust (white correction) standard errors. The latter two variables serve as control variables in our robustness checks. We use a t-test to test:

$$H_0\colon \beta_1 = 0 \text{ against } H_1\colon \beta_1 \neq 0$$

We test the self-promotion frequency to investigate if some participants are intrigued by the opportunity to have their prosocialness advertised to others, while others are not. We see this as a robustness check to our experiment design: if all, or a vast majority of, participants would choose to self-promote (not to self-promote), our setting may not have captured the stigmatizing aspect of boasting about one’s prosocialness (image-boosting aspect of appearing prosocial). Thus, we hypothesize that the choice will not be unanimous, given multidimensional utility functions among participants.

Hypothesis 2: *A share of participants chooses not to disclose their identity in the choice condition.*

To investigate this hypothesis we conduct a t- test to test:

$$H_0: s_{anon} = 0 \text{ against } H_1: s_{anon} > 0$$

where s_{anon} is the share of participants who choose to stay anonymous in the choice condition.

Considering the literature on prosocial behavior in public settings that focuses on gender differences, we also test whether gender can explain a share of the variation in our regressions. As the experiment was not primarily designed to test gender differences, hypotheses 3 and 4 should be seen as supplementary to the two former hypotheses.

Hypothesis 3: *The effect of the self-promotion choice treatment is heterogeneous between men and women.*

To investigate this, we run the following OLS regression:

$$b_i = \beta_0 + \beta_1\, Choice_i + \beta_2\, Male_i + \beta_3\, Choice_i \times Male_i + \beta_4\, OwnPerc_i + \beta_5\, OthersPerc_i + \varepsilon_i$$

where b_i is the number of buttons clicked by individual i, $Choice_i$ is a dummy coded 1 if participant i was in the choice condition group and 0 if not, $Male_i$ is a gender dummy being 1 if participant i is male and 0 if female, and $Choice_i \times Male_i$ is an interaction variable of the two dummies. $OwnPerc_i$ is participant i's own perception of the Red Cross, $OthersPerc_i$ is participant i's belief about others' perception of the Red Cross, and ε_i are the robust (white correction) standard errors. The latter two variables serve as control variables in our robustness checks. We use a t-test to test:

$$H_0: \beta_3 = 0 \text{ against } H_1: \beta_3 \neq 0$$

Hypothesis 4: *The self-promotion frequency differs between men and women.*

Related to our second hypothesis, we will also test for heterogeneity with regard to gender for the self-promotion choice. We conduct a Pearson's Chi-squared test to test:

$$H_0: s_{anon}^{male} = s_{anon}^{female} \text{ against } H_1: s_{anon}^{male} \neq s_{anon}^{female}$$

3.2.2 Secondary Hypotheses

For the last hypotheses, we use the survey measures from the post-study. In the survey, participants compared three different types of behavior and rated the relative social desirability on an 11-point Likert scale from -5 to +5.

Hypothesis 5&6: *Participants rate the behavior of participants who actively choose to disclose her name as less socially desirable than …*

A …the behavior of a participant who had no choice but to be recognized.

B …the behavior of a participant who chose to stay anonymous.

We investigate these two hypotheses by using t-tests to test:

$$H_0: \overline{SD_{dif}^{3,1}} = 0 \quad \text{against} \quad H_1: \overline{SD_{dif}^{3,1}} \neq 0$$

and

$$H_0: \overline{SD_{dif}^{3,2}} = 0 \quad \text{against} \quad H_1: \overline{SD_{dif}^{3,2}} \neq 0$$

Where $\overline{SD_{dif}^{3,1}}$ and $\overline{SD_{dif}^{3,2}}$ are the differences of the average ratings of (i) behavior type 3 and 1, and (ii) behavior type 3 and 2 (as described in Section 3.1.4), respectively. Our hypotheses are that $\overline{SD_{dif}^{3,1}} < 0$ and $\overline{SD_{dif}^{3,2}} < 0$, which would mean that on average the behavior of a type 3 participant is seen as less socially desirable than the other two behaviors.

3.3 Critical Discussion and Limitations of the Research Design

3.3.1 Experiment Design

The main point of the experiment was to create a setting that would allow participants to signal their prosocial identity toward others and see whether a need to self-promote it could negatively affect their motivation to engage in prosocial behavior. This requires that the signal is informative in the sense that a participant's behavior can in fact be considered costly and thus seen as credibly prosocial by others. Our main concern prior to conducting the experiment was that clicking buttons would not be considered costly enough. If this would have been the case, the decision to choose to be recognized in the choice condition would not be associated with any positive reputational element at all (and would perhaps only be considered shameful), which in turn would not allow us test our hypotheses. Also, the fact that the participants could compare their results with others in the room might have induced some to see it more as a competition in button pressing rather than a way to contribute to charity.

We decided to use this design to test our hypotheses as this method had been established in literature on prosocial behavior, where these claims were disarmed.[11] Since there was an established method, we wanted to be comparable with previous studies. If the results from Ariely, Bracha, and Meier's (2009) study were due to the mechanisms they describe, we reasoned, their design should also suit our hypotheses. When considering the trade-off between making adjustments to potentially enhance the experiment design and making our study as comparable as possible to the original "Click for Charity"-study, we consistently chose the latter option. The reasoning behind this was that any adjustments would be based on speculations rather than data. We also argue that the effect we test is noticeably similar to that of Ariely, Bracha and Meier (2009), meaning that aiming for comparability makes this study's contribution more significant as it then not only adds to the literature on this specific topic, but also can provide insights to previous literature on related research questions as well.

We conducted the post-study to gain insights as to whether participants had perceived the experiment setting as we had hypothesized, and also asked participants to rate their own competitiveness in the post-study to exploratively investigate if a thirst for competition may have had an impact on our results. While participants undeniably compete in some sense in the "Click for Charity"-setting, we argue that trying to look good in front of others will in some way always be a competition as good deeds will, directly or indirectly, be compared with good deeds of others. This in turn would imply that the study's findings can be translated to settings outside of the laboratory as well. However, if participants perceived the competition to revolve pressing buttons rather than collecting money to charity, that would be an issue. We use participants' own rating of their competitiveness to understand to what extent this general sort of competitiveness could have affected the results.

Like Ariely, Bracha, and Meier (2009), we also asked participants what they thought of the Red Cross to confirm that collecting money to the organization was seen as something prosocial and thus image-boosting.

3.3.2 Experiment Setting

Due to a lack of access to a proper laboratory environment the experiment was carried out in classrooms at SSE, which might have introduced some noise to our results. To mitigate these effects and create an environment as similar to that of a laboratory as possible, we screened off

[11] See Ariely, Bracha, and Meier (2009) for detailed descriptions of the various tests they use to support their arguments that their results are not driven by competition, and that the signals participants can send in fact are informative. Moreover, we briefly discuss their arguments and our view on them after having conducted our experiment in Section V.

participants from each other with moving boxes cut to fit our purposes that were put on the tables where participants sat (see Appendix VII for photos of the experiment setting). This ensured that their effort decision remained private during the donation process. We also used the same classrooms for all four sessions and randomized both the rooms and experimenters with regards to the two treatment groups for each day of the experiment to decrease any possible bias related to the experiment setting.

We also deviated from Ariely, Bracha, and Meier's (2009) setting by having participants use their own smartphones rather than provide them with computers to use. The benefit of this was practical: we reasoned it would be easier to attract participants if the experiment was held as near as the main lecture hall and other classrooms as possible (the closest computer lab was located in a different building). We also reasoned that students would be more likely to be carrying smartphones than laptops. The drawback of not using a computer lab was that participants were using smartphones that potentially differed in design and quality, which could have affected their ability to press buttons. While arguably adding some amount of noise, any effect on our results would in expectation be equally distributed across sessions and groups since we employed a complete randomization strategy. The program we used to design the experiment also proved to come with some caveats. To acknowledge any effect technical issues could have on our results, we included a comment section in the survey where we probed participants if they had experienced any problems. This commentary and information about the number of button pairs pressed provided a way for us to identify observations with such errors.

3.3.3 Participants

The participants were recruited from SSE due to practical reasons, and other participants may have been more suitable for an ideal setting. Being a business school, SSE enrolls students with a demonstrated interest in various areas of business and economics, and some may have been familiar with the hypotheses we wanted to test. For this reason, we did not advertise the experiment to any students specializing in Economics. Since SSE is a relatively small university (ca. 2000 enrolled students) we were also concerned participants in the earlier sessions would be acquainted with participants in later sessions and describe the nature of the experiment to them. As a consequence of this, we conducted the sessions as near to each other in time as possible (three consecutive weekdays) and explicitly asked participants not to disclose what they had done.

3.3.4 External Validity of Experiments

In light of the recent large replication study conducted by Camerer et al. (2016), who found the average replicated effect of their sample studies to be just 66%, we also acknowledge that our

research method share the same caveat with similar studies. Given that the replicability rate of experiments in general is relatively low we are cautious of attributing external validity to our study by itself and instead advice the reader to examine our results in complement with other studies in the same field. Some other caveats that apply to experiments in general and also to ours are the issues of having a student sample pool and possible self-selection bias. While some studies have found students' behavior to be comparable with other groups (Fehr and List, 2004; Cooper et al., 1999), others have seen results opposing this (Alatas et al., 2007). Furthermore, the fact that participants themselves chose to participate in the experiment implies that our sample may not represent the student body, let alone society (Charness and Kuhn, 2011). This means that their behavior may not be generalizable to the complete population. We mitigated this self-selection bias by announcing our experiment to all students in our primary sample pool. Furthermore, we did not advertise what the experiment was about beforehand, meaning we did not target the recruitment to prosocial individuals in general.

On the other hand, experiments allow for a high degree of internal validity as the setting is more controlled than field experiments, for example. Furthermore, laboratory experiments are common in the economics science, and conducting our experiment with students makes it comparable with previous literature. We have also sought to enhance the replicability of our study by having a large sample size (n=216). Concluding, while experiments can be a useful tool to isolate a desired treatment effect, one should be cautious of attributing external validity to a single study by itself and take related literature into consideration as well.

4. Results

All participants that completed the survey are considered in the descriptive statistics, graphs and other results presented. In Tables 2 and 4 we also include one regression where the problem observations are controlled for as a robustness check in order to increase the study's transparency. As there is no objective way to decide which observations to control for, we stress that the reader should focus on the results where the eight observations are not controlled for. Changes in qualitative interpretations that occur due to controlling for participants whom experienced technical issues (as described in Section 3.1.1) should thus be seen only as indicative. Descriptive statistics are shown in Table 1, and results are presented in Table 2 and onwards.

Table 1 Descriptive Statistics of Clicks for Charity Between Groups

Dependent variable: Button pairs pressed

Treatment/Gender	Mean	1st quartile	Median	3rd quartile	Std. Dev.	n
Auto-Promotion	591.611	336.5	616.5	816.5	294.437	108
Self-Promotion Choice	600.660	341	640	875	292.883	103
Female	584.846	389	613.5	749	251.556	110
Male	608.208	300	679	913	333.215	101
Total	596.028	340	617	836	596.028	211

Hypothesis 1

Table 2 The Effects of Self-Promotion on Clicking for Charity

Dependent variable: Button pairs pressed

	(1) Treatment effect	(2) Own perception of RC	(3) Others' perception of RC	(4) Problem control
Self-Promotion Choice	9.049	1.085	1.461	-7.886
	(0.823)	(0.979)	(0.971)	(0.841)
Own Perception of Red Cross		22.68*	24.43	28.52*
		(0.052)	(0.154)	(0.080)
Others' Perception of Red Cross			-2.740	-9.668
			(0.878)	(0.572)
Constant	591.6***	436.6***	444.0***	486.8***
	(0)	(0)	(0)	(0)
Observations	211	211	211	211
R-squared	0.000	0.018	0.019	0.092
Control for Problems	No	No	No	Yes

Robust pval in parentheses

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

In the first regression in table 2 (1), we see that the effect of having to self-promote in order to be recognized is insignificant at all meaningful significance levels (p=0.823). Furthermore, the R-squared is very low (0.000), meaning relevant explanatory variables are missing. Controlling for participants' own perceptions of the Red Cross in (2) does not change the interpretation of the self-promotion variable's effect, but the control variable is itself significant at the 10% level (p=0.052). The control variable capturing how participants thought others' perceived the Red Cross is not significant in regression (3), but it is correlated with the other control variable as that one instead becomes insignificant. Controlling for the eight observations with problems in (4) reverts the treatment effect from positive to negative but does not change the qualitative interpretation as the self-promotion choice variable remains insignificant. The conclusion of these results is that the self-promotion treatment did not have any effect on how many buttons participants pressed in the complete sample, and that this is consistent after introducing various robustness checks. Thus, we cannot reject the null hypothesis.

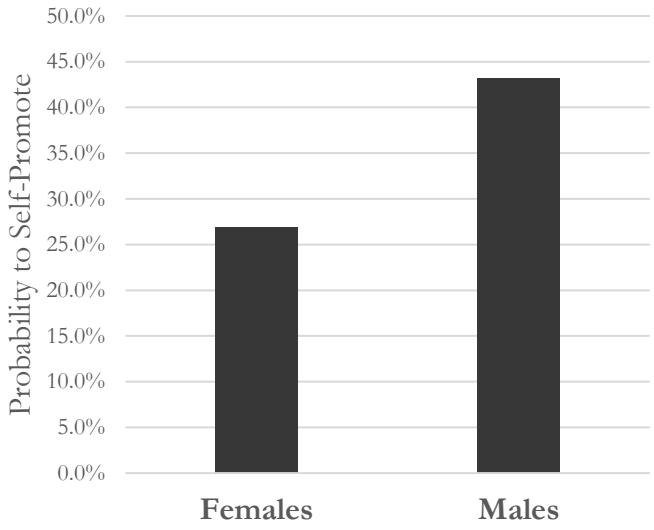
Hypotheses 2 and 4

Table 3 Self-Promotion Frequency

	Female (n)	Male (n)	Chi2 test	Total
Remained Anonymous	38	29	2.977* (0.084)	67
Self-Promoted	14	22		36
Total	52	51		103

Robust pval in parenthesis
*** p<0.01, ** p<0.05, * p<0.1

Figure 1 Self-Promotion Frequency Between Genders



67 of the 103 participants in the self-promotion condition chose to remain anonymous (s_{anon}=0.65). Comparing this with the null hypothesis s_{anon}=0 results in a t-value of -13.778, which is significant at the 1% level (p=0.000). Comparing decisions between males and females, 14 out of 52 females (26.9%, s_{anon}^{female}=0.731) in treatment group two opted to be recognized, while 22 out of 51 males (43.1%, s_{anon}^{male}=0.569) chose to do the same. Testing the difference using a Pearson Chi-Squared test yields 2.977, which is significant at the 10%-level (p=0.084).[12] Concluding, we find that a significant share of participants chose to stay anonymous and that men to a lesser extent than women chose to do so, a difference that is significant at the 10% level. We thus reject the null hypotheses for hypothesis 2 and 4 at the 1% and 10% significance level, respectively.

Hypothesis 3

Table 4 Self-Promotion and Gender Effects on Clicks for Charity

Dependent variable: Button pairs pressed

	(1) Gender differences	(2) Treatment effect	(3) Own perception of RC	(4) Others' perception of RC	(5) Problem control
Male	23.36	-31.30	-35.21	-35.01	-30.81
	(0.569)	(0.590)	(0.542)	(0.546)	(0.582)
Self-Promotion Choice		-44.97	-59.67	-59.26	-78.99*
		(0.349)	(0.208)	(0.217)	(0.099)
Self-Promotion Choice*Male		111.1	123.9	123.6	143.1*
		(0.176)	(0.129)	(0.133)	(0.071)
Own Perception of Red Cross			24.23**	25.41	29.94*
			(0.0399)	(0.147)	(0.072)
Others' Perception of Red Cross				-1.852	-9.003
				(0.918)	(0.601)
Constant	584.8***	606.1***	442.4***	447.3***	487.6***
	(0)	(0)	(0)	(0)	(0)
Observations	211	211	211	211	211
R-squared	0.002	0.011	0.031	0.032	0.111
Control for Problems	No	No	No	No	Yes

Robust pval in parentheses

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

[12] Excluding problem observations yields p=0.049.

Without any control variable, the difference between how many buttons males and females pressed is insignificant as shown in regression (1), where the dummy variable Male is coded 1 for males and 0 for females.[13] Introducing the self-promotion choice variable and combining this with the gender variable to form an interaction variable (2) does not result in significance for any of the variables in the regression. Controlling for participants' perceptions of the Red Cross in regression (3) yields no difference in the qualitative interpretation, but causes the gender differences to become marginally larger. The control variable is itself significant at the 5% level. Regression (4) shows that controlling for what participants' thought how others perceived the Red Cross did not affect how many buttons they pressed as this variable is insignificant. Introducing this control variable does not change the qualitative interpretation of the self-promotion variable nor the interaction variable.

Both the self-promotion dummy variable and interaction variable become significant at the 10% level ($p=0.099$ and $p=0.071$, respectively) when controlling for the eight observations with problems in regression (5). The self-promotion dummy variable in regression (5) illustrates that women on average clicked 79 pairs less in the choice condition compared to when in the auto-promotion condition, and the interaction variable shows that men on average clicked 143 pairs more than women in the choice condition. Adding up the self-promotion dummy variable with the interaction variable sums to 64, meaning men clicked 64 pairs more in the choice condition than they did in the auto-promotion condition (a t-test comparing men's button pairs pressed in the two treatment groups yields a p-value of 0.263). The results of regressions (3)-(5) point to the direction that the effect of having the choice to be recognized, rather than automatically being recognized, differs between men and women. These effects are however only significant once we exclude the observations with problems, which means one cannot draw any decisive conclusions from these results and we stress that they should only be seen as indicative. We cannot reject the null hypothesis at any meaningful significance level when we consider our complete sample.

[13] While we provided three possible responses to the question covering gender ("male," "female," and "other,") none of the participants chose the latter option.

Hypotheses 5 and 6

With regard to our secondary hypotheses, we asked respondents in our post-study to rate the social desirability of the three possible participant types on a scale from -5 to +5. Participants that actively chose to stand up were perceived to have acted less socially desirable than both those participants who had been left with no choice but to stand up ($SD_{dif}^{3,1}$= -1.23, p=0.000) and those that chose to remain anonymous ($SD_{dif}^{3,2}$= -0.98, p=0.003). Thus, we reject both null hypotheses at the 1% significance level.

Table 5 Social Desirability Rating Across Participant Types

Participant Type Rated	Mean	Std. Dev.	n	T-test Self-Promote	Std. Err.
No Choice (Type 1)	1.922535	2.114033	142	4.5156*** (0.000)	0.2729
Anonymous (Type 2)	1.676056	2.258316	142	3.0115*** (0.003)	0.3273
Self-Promoter (Type 3)	.6901408	2.647199	142	n/a	n/a

Robust pval in parenthesis
*** p<0.01, ** p<0.05, * p<0.1

5. Discussion

5.1 Analysis of the Results

In our experiment, we were not able to support our main hypothesis (1) that a need to actively self-promote one's prosocialness in turn has adverse effects on it with regard to our complete sample. Meanwhile, the decision whether to be recognized in front of the group was a divisive choice for participants as 65% chose to remain anonymous while the rest chose to be asked to stand up. We can thus reject the null hypothesis that no one would choose to remain anonymous (hypothesis 2) at a 1% significance level. An interpretation of this is that choosing to click for charity was easy, while the trade-off decision between looking bad by self-promoting and looking good by clicking a lot was more difficult. We can also reject the null hypothesis that self-promotion frequency does not differ between genders at a 10% significance level (hypothesis 3), as we find men to self-promote more frequently than women. Regressions (3)-(5) point to the direction that women were negatively affected by the self-promotion treatment and that the treatment effect differed between men and women, but we cannot reject hypothesis 4 at conventional significance levels when we do not control for our eight observations with technical issues.

Our results from the secondary study show that self-promoting (as opposed to remaining anonymous or having the promotion decision made someone else) did come with some negative image effects, as was hypothesized. We can reject the null hypotheses that self-promoters were seen as equally socially desirable as the other two types of participants (hypotheses 5 and 6) at the 1% significance level. Importantly, however, we argue that the overall image effect of self-promoting was ambiguous to some since more than a third chose to self-promote their result. Had self-promotion only been seen as negative among all participants, we claim that no one would have chosen to self-promote (and vice versa). This proves as a robustness check to our results and as a validation that our design captured the setting we sought to create in this regard.

5.2 Reconciliation of Our Results With Previous Research

5.2.1 Gender Differences in Public Prosocial Behavior and Self-Promotion

The findings that males were both more prone to self-promote and that their efforts were less affected by a need to do so (albeit the latter one not being significant at conventional levels) are likely to be complementary and are also in line with previous research (e.g. Eagly and Crowley, 1986). These results also point in the same direction as studies in similar laboratory settings. Böhm and Regner (2013) conducted a replication study of Ariely, Bracha, and Meier (2009) where all participants were exposed to both a public and a private setting and found that males were significantly more incentivized by public recognition compared to females. Similarly, males gave just half as much as females in Eckel and Grossman's (1998) double-anonymous dictator game,

but as Rigdon et al. (2009) introduced a weak social cue to an equivalent setting donations increased significantly—and the effect was entirely explained by the behavior of males. Connecting these results with ours presents a plausible causal interpretation: it seems reasonable to assume that males' willingness to promote their prosocial behavior is linked to them perceiving the image motivation of looking prosocial to be more important than females do. This is, as far as we know, a novel finding. For example, this could imply that charities should be particularly wary of providing ways for men to demonstrate their prosocialness to others. It is however important to note that this effect is only significant at the 10% level. Furthermore, as the task at hand to some extent had participants competing to generate the highest donation (see below for an extended discussion), this could also reconcile this result as there is a compelling assortment of research to suggest that men respond better to competition than women (see Niederle and Vesterlund, 2011 for a review). Just like in the original "Click for Charity"-study, the bridge between our results and implications for public goods situations hinges on the interpretation that people chiefly clicked for charity rather than for competition. With this in mind, we emphasize that these findings primarily provide an interesting direction for future research to further explore, and an important factor to keep in mind when conducting similar experiments, rather than conclusive evidence about gender differences in this realm.

5.2.2 Reconciliation of Our Study and the Original "Click for Charity"-study

While one explanation for our null result with regard to our complete sample could be that self-promoted prosocialness is not seen as something negative in general, the results from our post-study and the evidence from e.g. Berman et al. (2015) and Bénabou and Tirole (2006) speak against this. Instead, a plausible explanation seems to be that our experiment setting did not permit participants to signal enough about their prosocial type for this effect to take place. Put another way, perhaps clicking buttons for five minutes (after already committing to participate in the experiment) may not have been seen as costly—and thus prosocial—enough. For the same reason, it could be that being recognized for one's effort did not matter very much to participants.

Comparing our effect with the one of the original "Click for Charity"-study, the joint results could mean that self-promotion does not crowd out the signaling value of prosocial actions to the same extent as monetary incentives do. However, when reflecting on the findings from our post-study and the control variables' correlation with the pairs of buttons pressed, we argue that the results point to the direction that people's concerns for social-image may have a lower impact on their effort level in the "Click for Charity"-setting than what has previously been thought. In this light, our findings suggest that the results of Ariely, Bracha, and Meier (2009) might be less robust than the authors propose. This since their main finding of extrinsic motivation having adverse effects

on prosocial behavior by crowding out image motivation is fundamentally built on the assumption that image concerns did in fact motivate participants.

Ariely, Bracha, and Meier (2009) see a significant increase in pairs of clicks when participants receive a monetary incentive in the private condition, but not in the public condition. We propose three possible explanations as to why both Ariely, Bracha, and Meier's (2009) two public condition groups with a varying noise-to-signal ratio did not differ in their clicking and why our two treatment groups—also two public condition groups with a varying noise-to-signal ratio—equally did not differ in their button pressing.

First, one explanation could be that being able to compare one's effort (be it as a measure of prosocial type or more due to a thirst for competition in a general sense) could explain a non-negligible share of the variation in both studies. As described in Section 3.3.1, the external validity of the experiment design is questionable if participants saw it as a competition in button pressing rather than a competition in collecting money to charity. Similar to how a high score table can motivate a flipper player in a game hall to do better, we suspect that simply being able to compare one's donation (and potentially one's prosocial type) with others – anonymously or not – might have mattered more than being recognized for clicking for charity. In fact, we can regress the competitiveness variable collected from the post-study on the number of buttons pressed and find the two measures to correlate significantly at the 1% level. This effect is robust when introducing the other available variables as well (see Table A5 in Appendix VI). As the responses to this question were collected only after participants had been able to compare their result with others', this may however have had an effect on how they answered the question. Also, since participants rated their own competitiveness this is a subjective measure as interpretations of what e.g. "very competitive" means can differ. Thus, we are cautious of drawing conclusions too strong from this variable, but note that it indicates that the competitive aspect is important to consider.

Ariely, Bracha, and Meier (2009) meanwhile argue that competition cannot explain why people clicked more when their contributions went to the Red Cross, which most perceived as a "good" cause, as opposed to the National Rifle Association (NRA), which was perceived as "bad" by most, in the public condition. We however do not see this as conclusive evidence that participants in their experiment clicked more for image reasons. Quite on the contrary, our control variables show that each participant's own rating of the Red Cross seem to have mattered a lot for how much they clicked, while others' perceptions did not appear to have any effect. Furthermore, even though clicking a lot for the Red Cross may not have sent a very informative signal about a person's

prosocial type, clicking for the NRA could raise suspicion ("why would you ever contribute to such a controversial organization?") and deter participants from clicking.

A second explanation, which relates to the idea that the act of clicking was not seen as costly, could be that generating one of the ten highest donations would not have been very motivating. Even if the case was that no prestige or image reward is associated with clicking a lot (meaning being recognized for doing so would not matter), clicking very little could for the same reason be very informative about a person ("if it costs you so little, why would you not click for a good cause?") In Ariely, Bracha, and Meier (2009), *all* participants had to stand up and tell others how much money they donate. This means that subjects in their study that in a private setting would have chosen to exert a low effort may have been pressured into clicking in the public treatment in order to avoid the stigma of clicking very little. Compared to our study, the same type of participant would not have to be shamed into clicking as only the highest donations were announced, meaning there would be less variation left that our treatment could affect. If it was the pressure share of image motivation that influenced the studies' results, one could argue that this might affect how Ariely, Bracha, and Meier's (2009) findings translate into settings outside of the lab. For example, the authors' discussions on how a subsidy for buying hybrid cars could lead to less people buying these vehicles (as observers might think the cars were purchased due to the monetary incentive rather than the will to improve the environment) is founded on the assumption that people were voluntarily buying cars, as opposed to being pressured into doing so.

Thirdly, we also cannot rule out that very motivated participants could have been limited by the program we used to execute the experiment. In Ariely, Bracha, and Meier's (2009) study, the average number of clicks in the most motivated group was 968 (max. 1449, std. dev. 281), while the equivalent numbers of buttons pressed for the most motivated group in this experiment was 600 (max. 1149, std. dev. 292). This would imply negative effects on our results since the difference between motivated and less motivated individuals would have been smaller, meaning our treatment effect may have been smaller than it would have been if another program had been used. On the other hand, our sample sizes were significantly larger than those of Ariely, Bracha, and Meier (2009) as our treatment groups consisted of at least 103 observations each while theirs consisted of 27 observations on average. To some extent this should thus mediate the effect. As always when it comes to empirical studies, heterogeneous samples (and the sizes of them) may also explain the difference.

5.3 Suggestions for Future Research

Reconciling our findings with those of Ariely, Bracha, and Meier (2009), we are cautious of drawing decisive conclusions about the reputational incentive's effect on participants' efforts in the "Click for Charity"-setting as Ariely, Bracha, and Meier (2009) argue that extrinsic motivation crowd out image motivation, while our results suggest otherwise. We thus advice future research to further investigate this in order to bridge our two seemingly conflicting results. To further develop the understanding of image concerns, extrinsic motivation, and their joint effect on prosocial behavior, we suggest that scholars dedicate more attention to illustrate that the prosocial behavior at hand i) is considered costly (meaning that signals sent can be informative) and ii) is not elicited by competition in a general sense. For example, a field setting where people are not directly comparing themselves with others may be more appropriate. In laboratory settings, researchers could make actions costlier and more alike real life by, similar to dictator games, allocate money to participants which they then could decide how much they would like to keep and how much they would like to donate. Varying the incentive sources between groups could then help establish how giving behavior is affected when, say, measures of self-promotion or extrinsic incentives are introduced. Finally, we suggest future research to take note of potential gender differences in such settings and, depending on the hypotheses, either attempt to isolate or eliminate these effects in the experiment design.

6. Conclusion

Using the "Click for Charity" design put forward by Ariely, Bracha, and Meier (2009), we allow participants to signal prosocialness by engaging in a repetitive task (alternatingly pressing buttons) in order to generate money to the Swedish Red Cross. Our two treatment groups differ in the way the results were presented to the other participants in the room. In the auto-promotion condition, all the ten highest donations are called out and the associated participants are asked to stand up in order to be recognized for their efforts. The ten highest donations are also called out in the choice condition, but the participants in this group have to choose whether or not they wish to be recognized in case their donation would be one of the ten highest in the room. We establish a setting where this choice was common knowledge among participants, meaning the other participants in the room would be able to infer whether a person had chosen to self-promote his or her prosocial behavior or not. The variation in pairs of buttons pressed thus depends on whether or not a need to actively self-promote one's prosocial behavior for others to know about it would make participants less motivated to engage in the real-effort task. In their seminal paper on prosocial behavior and incentives, Bénabou and Tirole (2006) outline that such self-promotion of one's prosocialness can dilute the signal sent to observers to the extent that the supply of prosocial behavior decreases. To the best of our knowledge, this hypothesis has never been tested before. Adjacent to our main hypothesis, we also test the self-promotion frequency and whether self-promotion and its effect on prosocial behavior differ between men and women.

We cannot support our main hypothesis in our experiment setting. We find the decision to self-promote to be decisive in the complete sample, as 35% in the choice condition chose to be asked to stand up while 65% chose to remain anonymous. The self-promotion frequency for men was 43%, while the same number for women was 27%. This difference is significant at the 10% level. We also find women to be more affected than men by the choice to self-promote, but cannot reject the null hypothesis at conventional significance levels. Finally, we also find that participants perceive other participants that self-promoted their efforts to be less socially desirable than i) participants that were recognized, but did not actively make the choice to be recognized and ii) participants that chose not to be recognized (both differences being significant at the 1% level).

Our results provide tentative evidence to suggest that males are not only more incentivized to act prosocially in public settings, but are also more willing to actively promote their good behavior to others in comparison with women. This could have implications for e.g. charities as this, translated to a fundraising-setting, indicate that men might be more interested in demonstrating to others

which charities they support. However, we see this as a direction for future research rather than conclusive evidence on gender differences regarding prosocial behavior and incentive sources.

Rather than rejecting the theory behind Bénabou and Tirole's (2006) model, we argue that the "Click for Charity"-setting may not be the most distinguished way to test aspects of the reputational incentive for being prosocial as our findings indicate that being recognized in front of others may not be a universal motivator for people to exert a higher effort in this specific setting. As this is an assumption that underpins Ariely, Bracha, and Meier's (2009) main finding, we devote a segment to critically discuss the underlying effects behind their results. This study thus not only contributes to the literature by testing the researching question at hand for the first time, but also through providing an improved understanding of the "Click for Charity"-design and its potential drawbacks. We conclude our paper by providing direction for future research and suggesting alternative methods to explore how social-image can incentivize prosocial behavior.

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Appendix I: Problem Observations

Table A1 Observations With Problems Classified as Severe

Buttons Pressed	Comment (”Do you have any additional comment? Did everything work ok? (optional)”)	Male	Treatment
229	Was slightly laggy on the phone	1	1
64	No	1	2
254	Old iPhone, browser was slow, couldn't press more often	1	2
1	No	1	1
268	my web page was super slow though	1	1
206	The touching screen doesn't work that well.	0	1
254	NO	0	1
251	As explained before, the UI of the button is badly designed preventing us from pressing buttons fast	1	2

Observations displayed above were coded as 1 for the dummy variable “Problem observations”.

Appendix II: Descriptive Statistics

Table A2 Descriptive Statistics of Participants and Sessions

Session and Treatment	S1 Auto	S1 Choice	S2 Auto	S2 Choice	S3 Auto	S3 Choice	S4 Auto	S4 Choice	Total
Female (n)	11	9	18	22	19	8	12	14	113
Male (n)	17	17	10	7	8	15	15	14	103
Total	28	26	28	29	27	23	27	28	216*

*Five of these observations are excluded from the results as the participants did not finish the complete survey

Appendix III: Primary Study

Information Presented on the Board

Donating

To donate, press the two buttons on your screen in alternate order. Each pair of buttons counts as one donation according to the scheme on the left.

It is your choice if and how much to donate. You have a maximum of 5 minutes.

To continue click the “Next” button in the bottom of the page to continue.

Donation Scheme

Pair 1–100 will be worth 0.25 SEK (25 öre) each
Pair 101–200 will be worth 0.188 SEK (18.8 öre) each
Pair 201–300 will be worth 0.094 SEK (9.4 öre) each
Pair 301–400 will be worth 0.047 SEK (4.7 öre) each

Pair 401–500 will be worth 0.023 SEK (2.3 öre) each
Pair 501–600 will be worth 0.012 SEK (2.3 öre) each
Pair 601–700 will be worth 0.006 SEK (1.2 öre) each
Pair 701–… will be worth 0.003 SEK (0.6 öre) each

Extra Details *(Treatment Group 1)*

The participants with the 30% highest donations will be announced by name and asked to stand up while the amount they donated to the Red Cross is announced.

Extra Details – (*Treatment Group 2*)

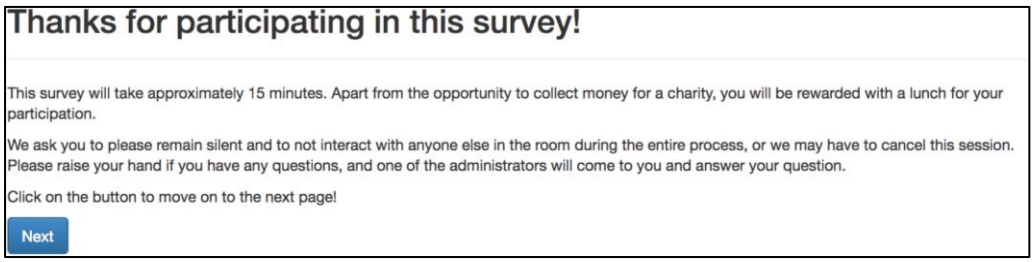
The 30% highest donations in the room will be announced in the end of the experiment.

You will have the **choice to be recognized** for your donation if it is among the highest 10. In such case your name will be announced and you will be asked to stand up while the amount you donated is announced.

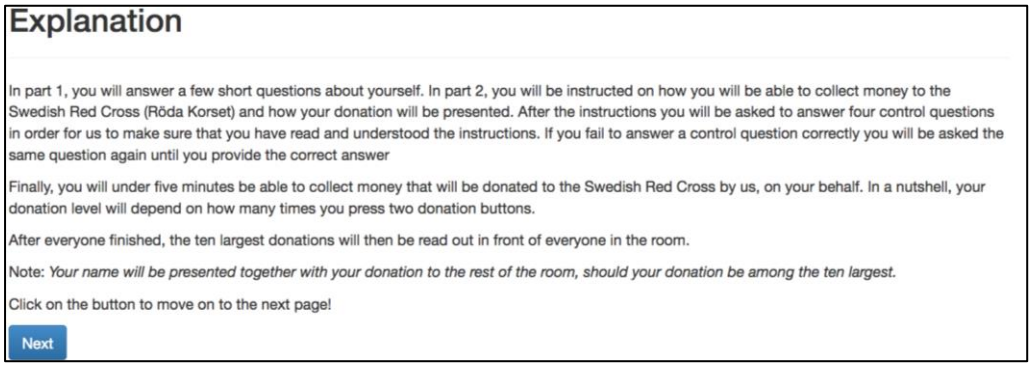
You have the choice to stay anonymous. In such case, only the amount you donated is announced as an anonymous donation.

Survey

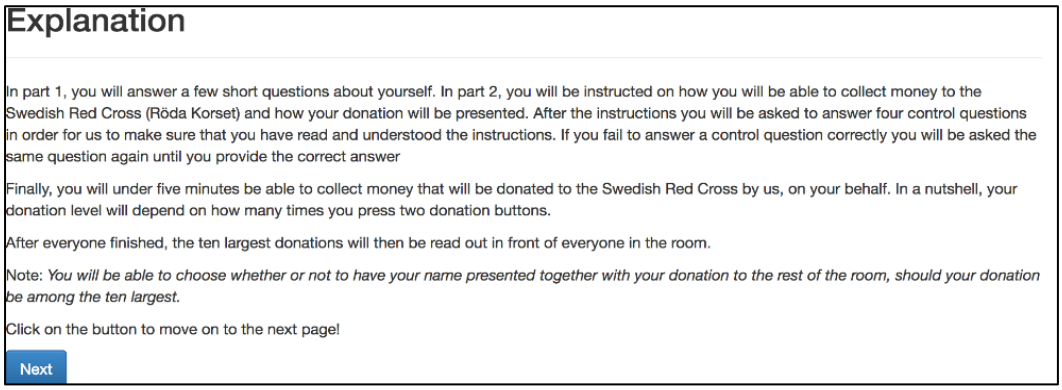
Page 1



Page 2 – Specific for treatment group 1



Page 2 – Specific for treatment group 2



Explanation

In part 1, you will answer a few short questions about yourself. In part 2, you will be instructed on how you will be able to collect money to the Swedish Red Cross (Röda Korset) and how your donation will be presented. After the instructions you will be asked to answer four control questions in order for us to make sure that you have read and understood the instructions. If you fail to answer a control question correctly you will be asked the same question again until you provide the correct answer

Finally, you will under five minutes be able to collect money that will be donated to the Swedish Red Cross by us, on your behalf. In a nutshell, your donation level will depend on how many times you press two donation buttons.

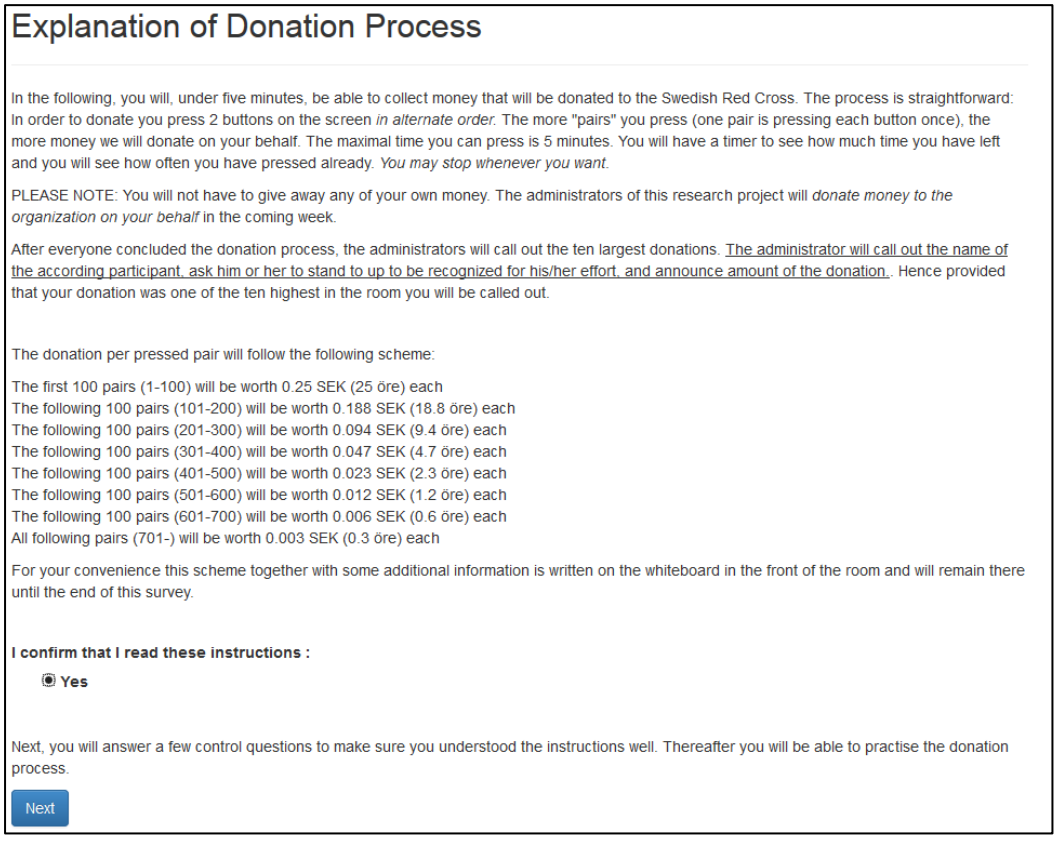
After everyone finished, the ten largest donations will then be read out in front of everyone in the room.

Note: *You will be able to choose whether or not to have your name presented together with your donation to the rest of the room, should your donation be among the ten largest.*

Click on the button to move on to the next page!

Next

Page 3 – Specific for treatment group 1



Explanation of Donation Process

In the following, you will, under five minutes, be able to collect money that will be donated to the Swedish Red Cross. The process is straightforward: In order to donate you press 2 buttons on the screen *in alternate order*. The more "pairs" you press (one pair is pressing each button once), the more money we will donate on your behalf. The maximal time you can press is 5 minutes. You will have a timer to see how much time you have left and you will see how often you have pressed already. *You may stop whenever you want.*

PLEASE NOTE: You will not have to give away any of your own money. The administrators of this research project will *donate money to the organization on your behalf* in the coming week.

After everyone concluded the donation process, the administrators will call out the ten largest donations. The administrator will call out the name of the according participant, ask him or her to stand to up to be recognized for his/her effort, and announce amount of the donation.. Hence provided that your donation was one of the ten highest in the room you will be called out.

The donation per pressed pair will follow the following scheme:

The first 100 pairs (1-100) will be worth 0.25 SEK (25 öre) each
The following 100 pairs (101-200) will be worth 0.188 SEK (18.8 öre) each
The following 100 pairs (201-300) will be worth 0.094 SEK (9.4 öre) each
The following 100 pairs (301-400) will be worth 0.047 SEK (4.7 öre) each
The following 100 pairs (401-500) will be worth 0.023 SEK (2.3 öre) each
The following 100 pairs (501-600) will be worth 0.012 SEK (1.2 öre) each
The following 100 pairs (601-700) will be worth 0.006 SEK (0.6 öre) each
All following pairs (701-) will be worth 0.003 SEK (0.3 öre) each

For your convenience this scheme together with some additional information is written on the whiteboard in the front of the room and will remain there until the end of this survey.

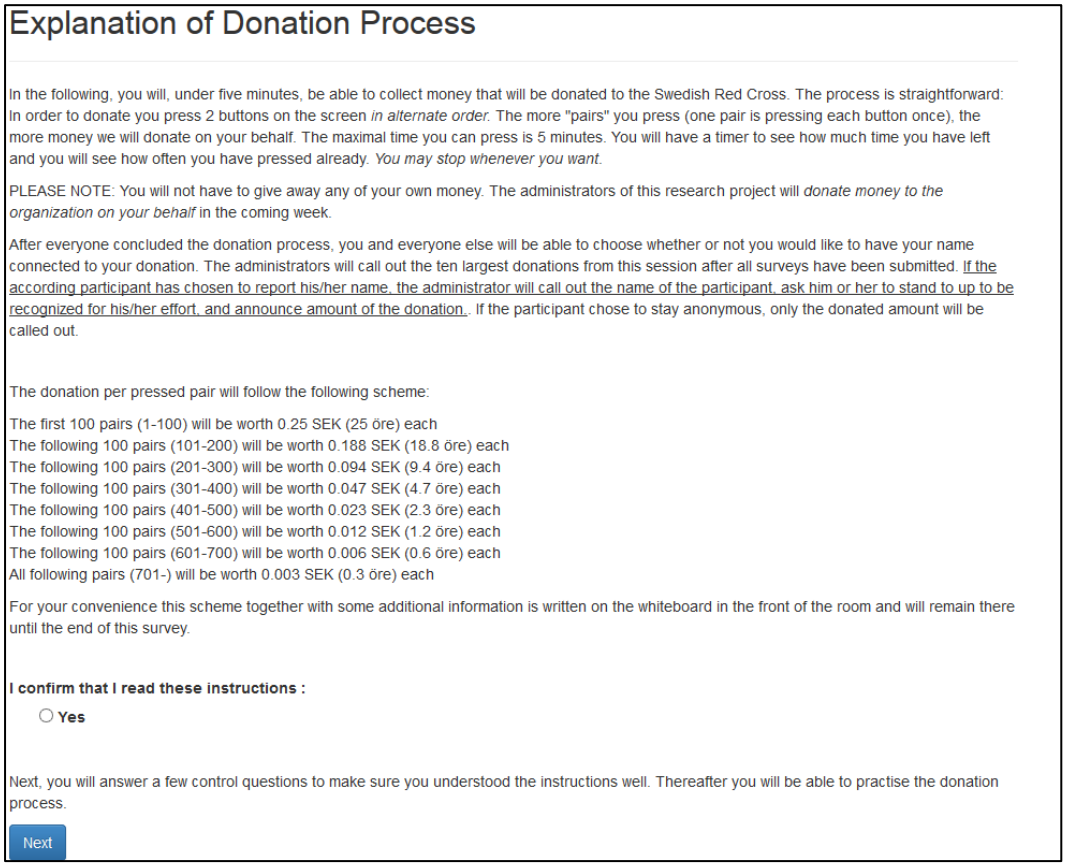
I confirm that I read these instructions :

◉ **Yes**

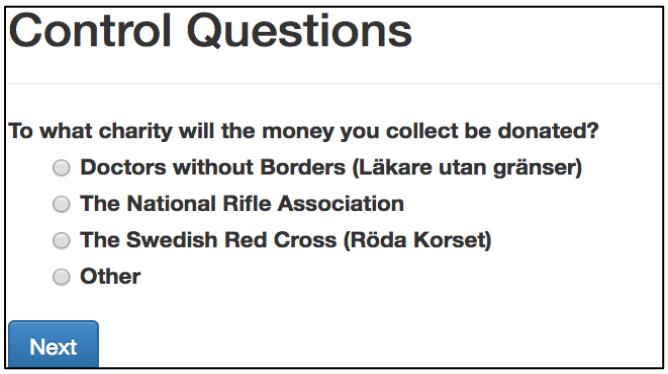
Next, you will answer a few control questions to make sure you understood the instructions well. Thereafter you will be able to practise the donation process.

Next

Page 3 – Specific for treatment group 2



Page 4



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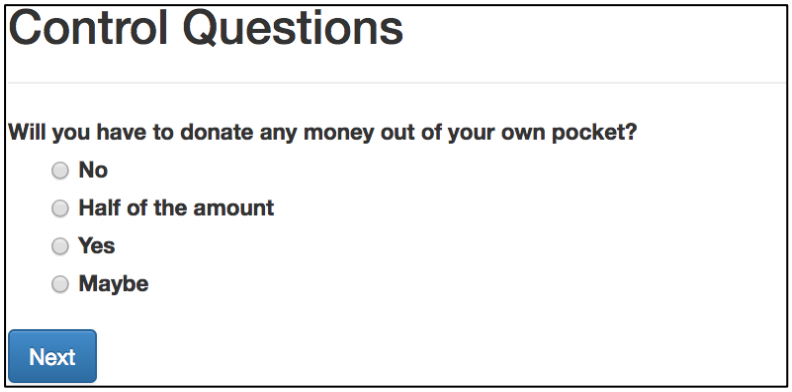

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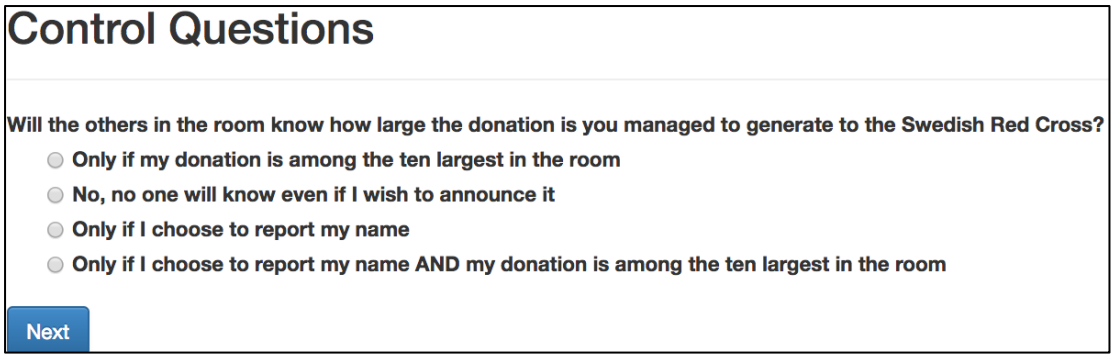

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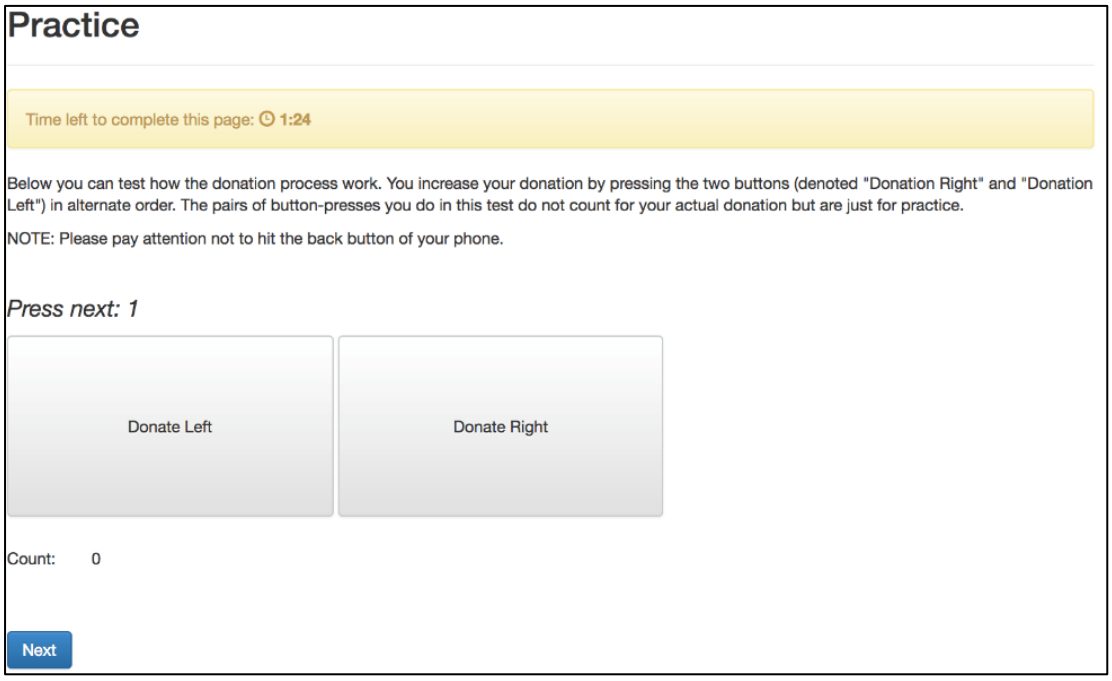

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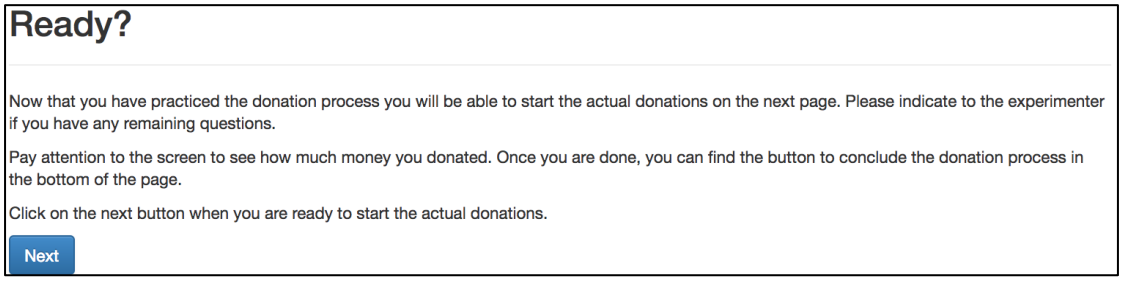

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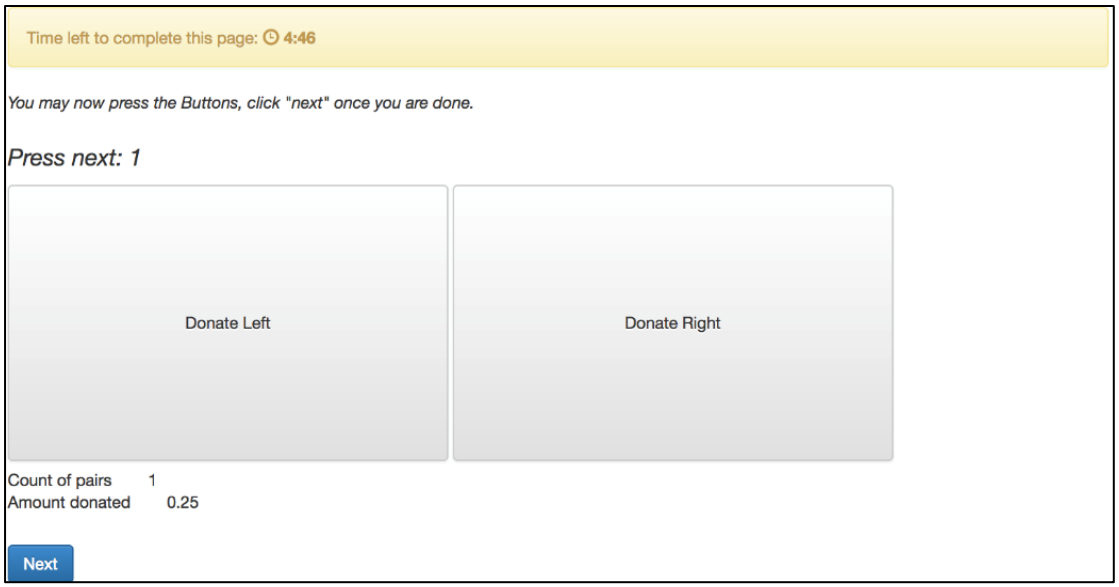
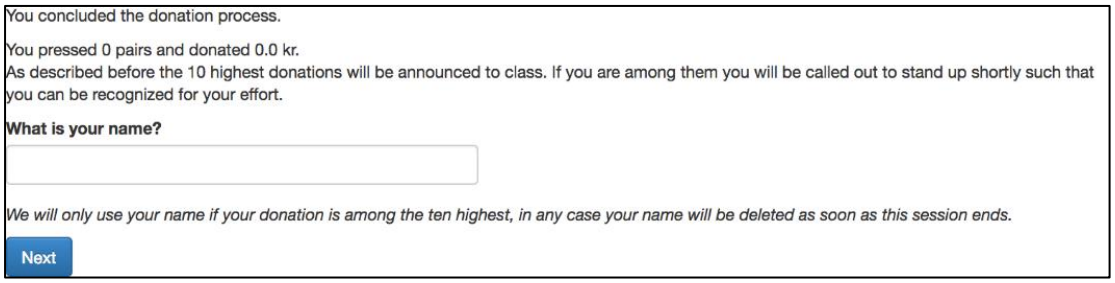


Page 11 – Specific for treatment group 1



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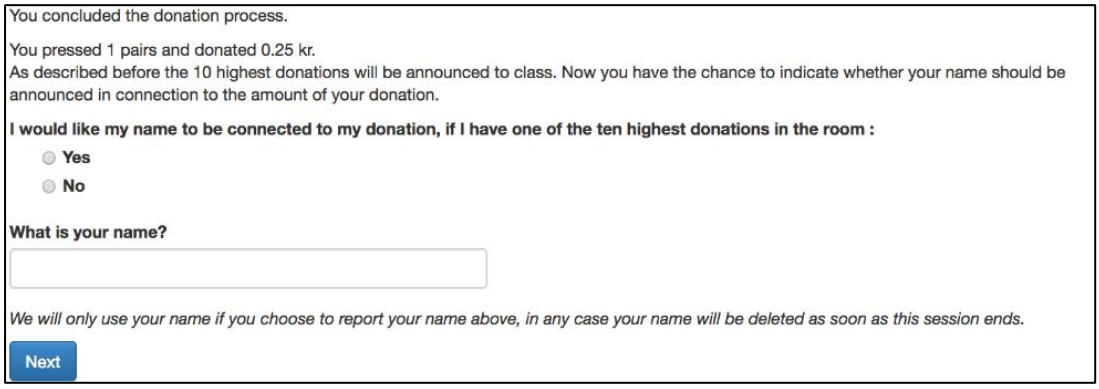

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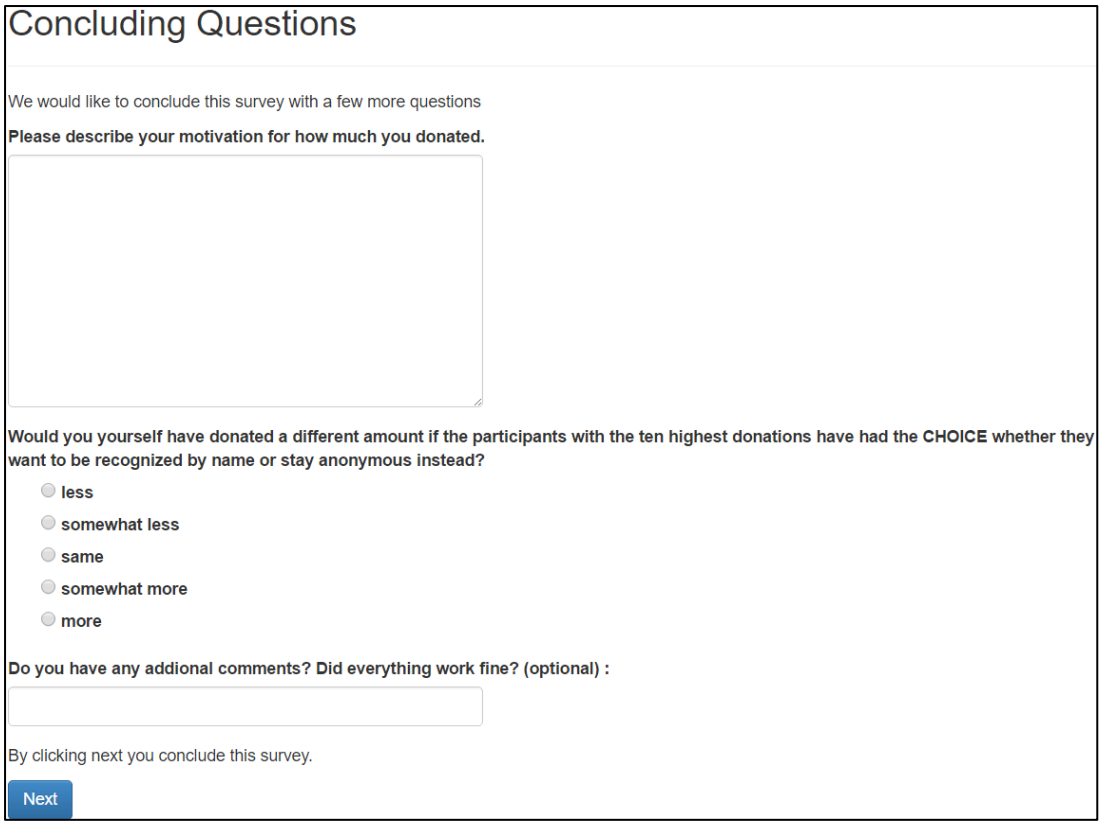

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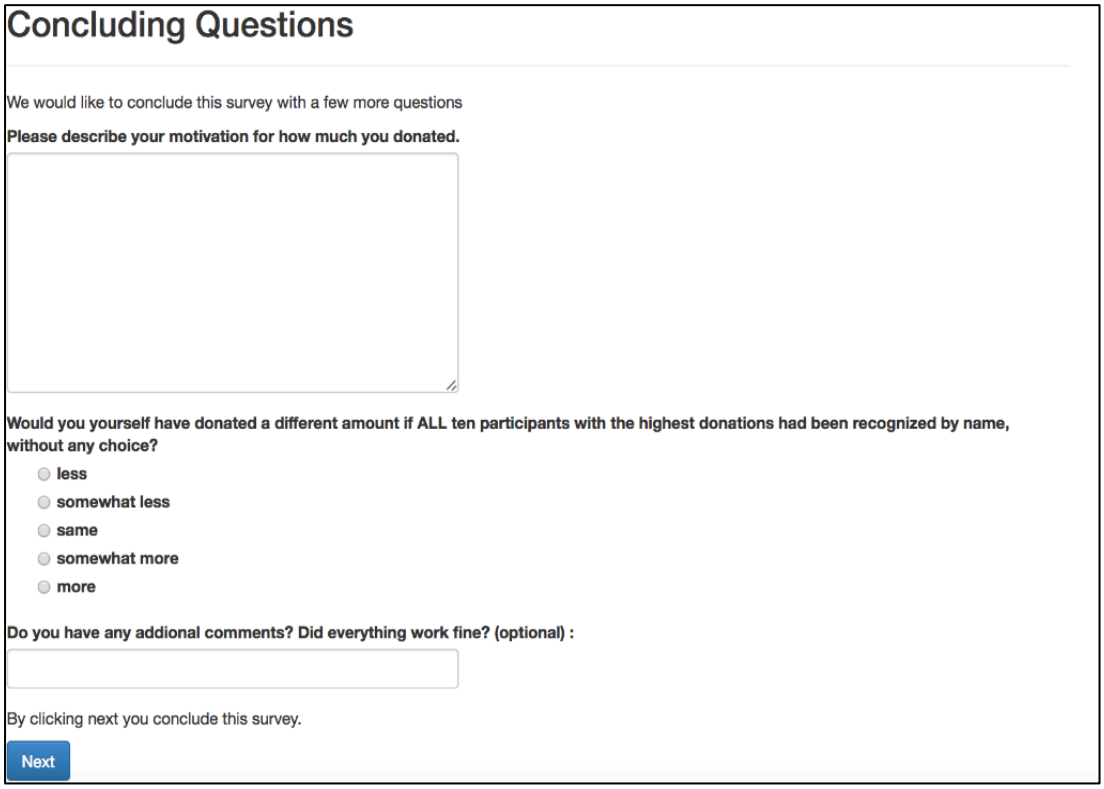

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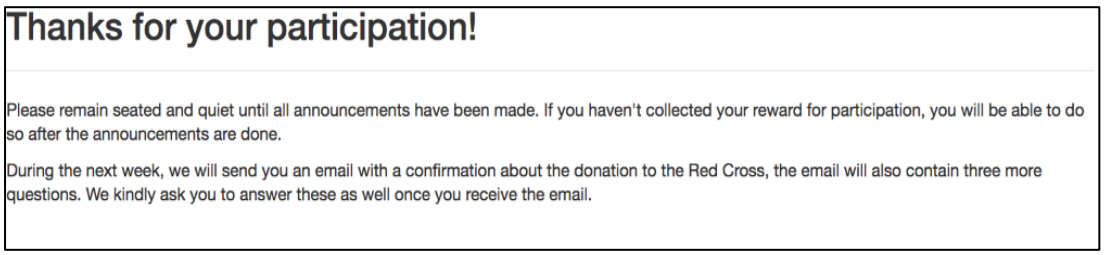


Appendix IV: Survey Measures

Table A3 Perceptions of the Red Cross

Variable	Mean	1st quartile	Median	3rd quartile	Std. Dev.	n
Own perception of RC	7.005	6	7	8	1.753	211
Others' perception of RC	7.223	6	7	8	1.634	211

The table above shows what participants themselves thought of the Red Cross ("Own perception of RC") and what they believed others thought of the Red Cross ("Others' perception of RC"). Responses to both questions were recorded on a Likert-scale from 1–10, where 1="strongly dislike" and 10= "strongly like". The results show that a large number of responses were centered around 6–8 and that the two measures captured a similar sentiment for the complete sample.

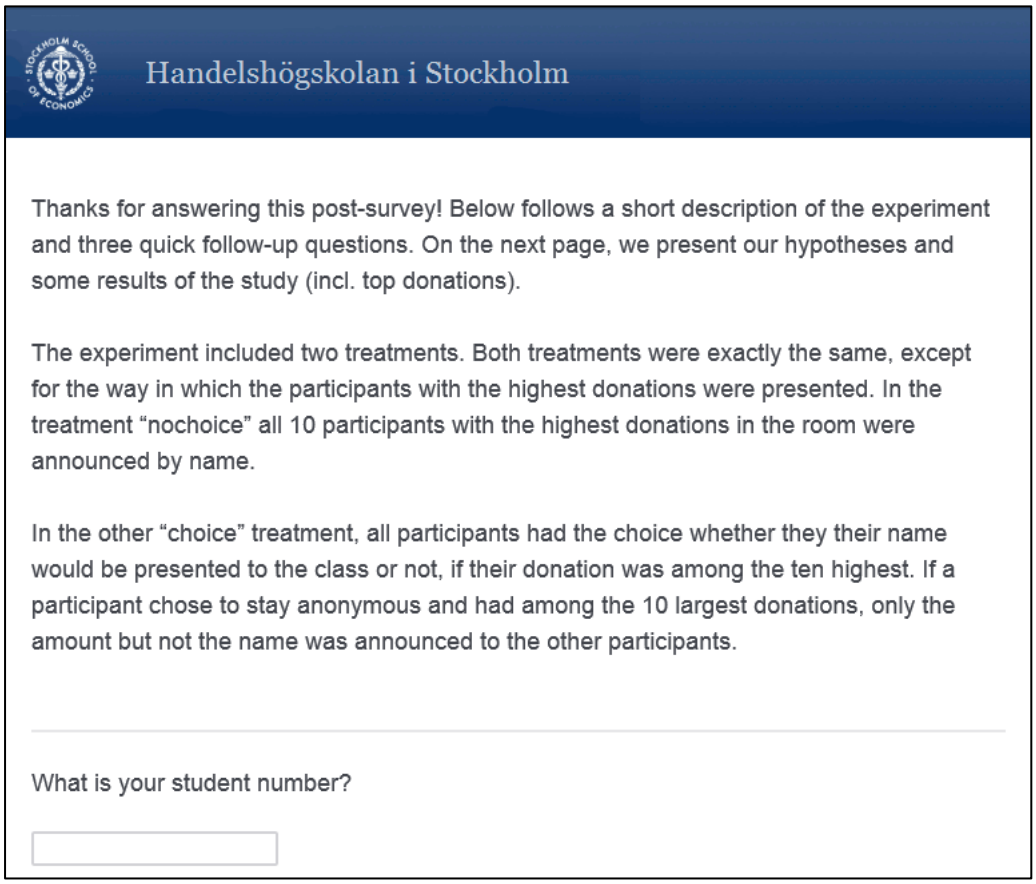
Table A4 Participants' Reported Effort if in the Other Group

Treatment group	Decreased	Same	Increased	n
Auto-Promotion	25%	56%	19%	108
Self-Promotion Choice	14%	74%	12%	103

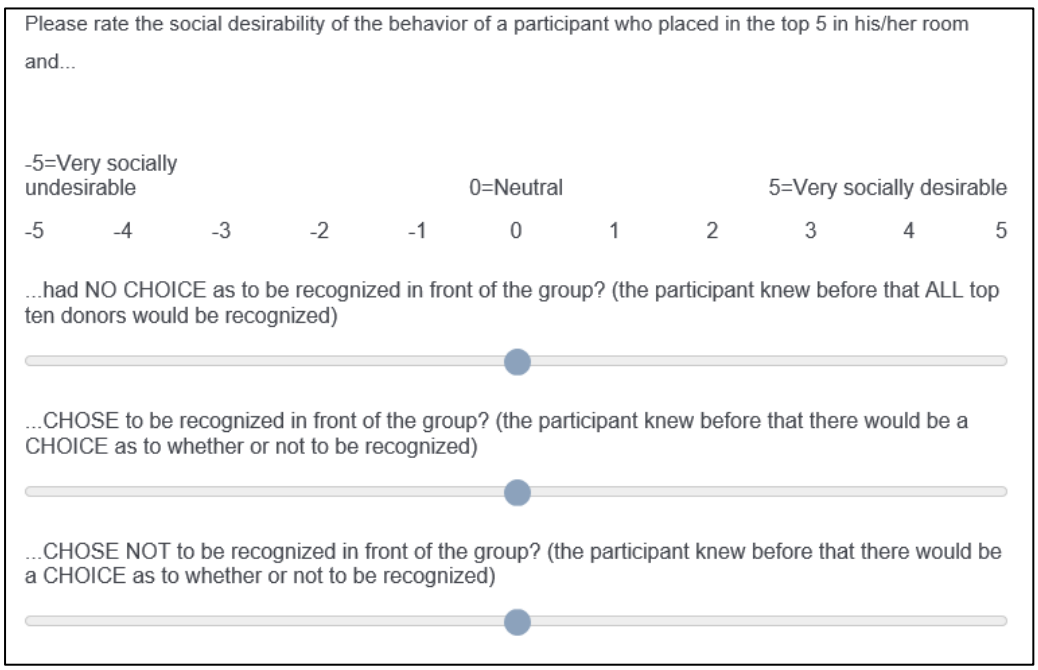
The table above displays what participants answered how their effort would change if they would have been in the other group. Answers were recorded on a scale 1–5 from 1="less" to 5="more", which were then coded to "Decreased" if participants answered 1 or 2, "Same" if participants answered 3, and "Increased" if participants answered 4 or 5. The results point to the hypothesized direction, but not decisively.

Appendix V: Post-study Survey

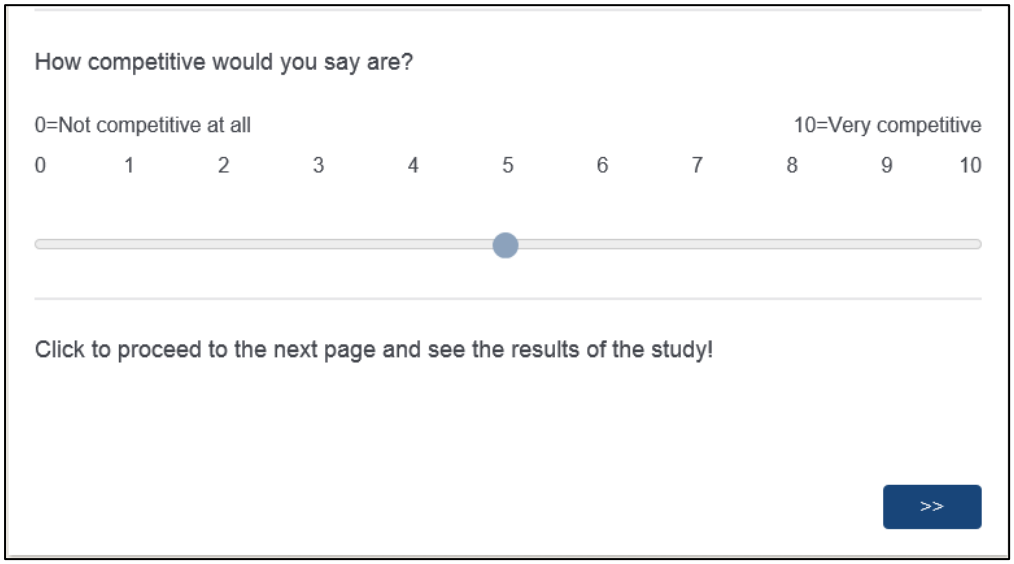
Page 1, part 1



Page 1, part 2



Page 1, part 3



The second page of the survey displayed a summary of the results from the first study, and involved no new data collection.

Appendix VI: Robustness Regression

Table A5 Regression Including All Available Variables

Dependent variable: Button pairs pressed

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Competitiveness	Treatment effect	Gender effect	Own perception of RC	Others' perception of RC	Control for problems
Competitiveness	38.38***	38.33***	35.05***	35.77***	35.20***	34.81***
	(0.0004)	(0.0005)	(0.0038)	(0.0067)	(0.0068)	(0.0021)
Self-Promotion Choice		22.88	-40.10	-39.14	-36.25	-78.39
		(0.629)	(0.487)	(0.500)	(0.540)	(0.175)
Male			-23.75	-24.44	-21.48	-33.36
			(0.734)	(0.726)	(0.758)	(0.612)
Self-Promotion Choice*Male			143.1	141.5	137.6	180.7*
			(0.143)	(0.158)	(0.174)	(0.0575)
Own Perception of Red Cross				-2.129	3.991	5.146
				(0.893)	(0.859)	(0.808)
Others' Perception of Red Cross					-9.555	-11.20
					(0.664)	(0.602)
Constant	333.4***	322.9***	356.7***	366.8***	395.8***	444.6***
	(0.0001)	(0.0004)	(0.0003)	(0.0039)	(0.0020)	(0.0003)
Observations	137	137	137	137	137	137
R-squared	0.098	0.100	0.120	0.120	0.122	0.251
Control for Problems	No	No	No	No	No	Yes

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5 shows regressions with all available variables as a robustness check. The competitiveness variable is significant at the 1%-level in regressions 1-6. These results imply that competitiveness explains a significant share of the variation in our experiment, and potentially also that of Ariely, Bracha, and Meier (2009). As mentioned above, one should however be cautious of drawing conclusions too strong from this measure as participants' rated their own competitiveness only after participating in the experiment.

Appendix VII: Photos of the Experiment Setting

Figure A1 Experiment Room 1

Figure A2 Experiment Room 2