

# Voting to Tell Others\*

Stefano DellaVigna  
UC Berkeley and NBER

John A. List  
U Chicago and NBER

Ulrike Malmendier  
UC Berkeley and NBER

Gautam Rao  
UC Berkeley

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## Abstract

Why do people vote? We argue that social image plays a significant role in explaining turnout: people vote because others will ask. The expectation of being asked motivates turnout if individuals derive pride from telling others that they voted, or feel shame from admitting that they did not vote, provided lying is costly. We design a field experiment to estimate the effect of social image concerns on voting. In a door-to-door survey about election turnout, we experimentally vary (i) the informational content and use of a flyer pre-announcing the survey, (ii) the duration and payment for the survey, and (iii) the incentives to lie about past voting. Our estimates suggest significant social image concerns. For a plausible range of lying costs, we estimate the monetary value of voting ‘because others will ask’ to be in the range of \$5-\$15 for the 2010 Congressional election. In a complementary get-out-the-vote experiment, we inform potential voters before the election that we will ask them later whether they voted. We find suggestive evidence that the treatment increases turnout.

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# 1 Introduction

Why do people vote? Answers to this classical question broadly fall into two classes. The first class is *pivotal voting*: Individuals vote because they may affect the outcome of the election (Downs (1957), Ledyard (1984), and Palfrey and Rosenthal (1983, 1985)). This motivation can explain voting in small elections, but less so in large-scale elections (Feddersen (2004)). The second class of explanations is *norm-based voting*: Individuals vote because they believe it is the right thing to do—even if their individual vote may not affect the outcome of the election (Riker and Ordeshook (1968), Harsanyi (1977), Blais (2000), and Feddersen and Sandroni (2000)). This explanation has proven difficult to test empirically.

We propose a model that builds on norm-based voting, but can be estimated empirically. We posit that one reason why individuals vote is *because others will ask*. Individuals care about what others think of them. They may derive pride from telling others that they voted and they may feel shame from admitting that they did not vote, similar in spirit to Harbaugh (1996).<sup>1</sup> We also assume that individuals incur disutility from lying, consistent with the laboratory evidence: in cheap talk experiments, individuals tell the truth more often than would be predicted in a model with no disutility from lying (Gneezy (2005), Sánchez-Pagés and Vorsatz (2007), and Erat and Gneezy (2012)).

In this model, an individual is motivated to vote (also) because she anticipates that others will ask if she voted. If she votes, she can advertise her ‘good behavior’ whenever asked. If she does not vote, she faces the choice of being truthful but incurring shame, or stating that she voted but incurring a lying cost. This trade-off is reflected in the known fact that 25 to 50 percent of non-voters lie when asked about their past turnout (Silver, Anderson, and Abramson, 1986).

Our model of voting is reduced-form and does not capture the myriad of other motivations to vote. Yet, its simplicity allows us to estimate the model parameters using field data. We design a natural field experiment (Harrison and List, 2004) that is tightly linked to the model, and we estimate the value of voting that is due to social image. This type of field experiment with parameter estimation (Card, DellaVigna, and Malmendier, 2011) is uncommon in the literature. We show the insights gained from placing greater emphasis on parameter estimation in an environment where experiment and theory are tightly linked.

The main experiment took place in the summer and fall of 2011 in towns around Chicago. We visited households and asked whether they were willing to answer a short survey, including a question on whether they voted in the 2010 congressional election. In some cases, we posted a flyer on the doorknob a day in advance to announce the upcoming survey. Unbeknownst to the households, we used voting records to restrict the sample to households where either all

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<sup>1</sup>Gerber et al. (2012) provides evidence of social-image concerns in that survey respondents report a higher social evaluation of voters, compared to non-voters.

registered members voted in the 2010 elections (henceforth, voting households) or none of the registered members voted in 2010 (non-voting households). We do not visit households with a mixed 2010 voting record.

The field experiment has three main sets of treatments. In the first set, we randomize the information on the flyer. In one group, the flyer informs households that the next day we will ask for their participation in a door-to-door survey. In a second group, the flyer specifies that the survey will be about “*your voter participation in the 2010 congressional election.*” Changes in the share of households opening the door and completing the survey between the first and the second group indicate the value of being asked about voting. An increase in the participation of voting households indicates the pride of saying that they voted. A decrease among non-voting households indicates shame from admitting that they did not vote and the cost of lying.<sup>2</sup>

We find that, on average, voters do not sort in. In fact, voting households are slightly less likely to answer the door and do the survey when they are informed about the turnout question. Non-voters sort out significantly, decreasing the survey participation by 20 percent.<sup>3</sup>

These results may depend on the election considered. The 2010 elections were disappointing for Democrats and positive for Republicans, including in Illinois the loss of President Obama’s seat in the Senate. The lack of pride among voters may reflect disappointment, given that the neighborhoods visited were largely Democratic. Indeed, if we restrict the analysis to voters registered for the Republican primaries, we find evidence of sorting in.

The findings on sorting provide prima facie evidence of social-image utility. In order to quantify the utility value, we need to measure the value of time and the cost of sorting in and out of answering the survey. To do so, we introduce a second set of (crossed) randomizations, in which we vary the promised payment for the survey (\$10 versus \$0) and the pre-announced duration (5 minutes versus 10 minutes). We find that the effect of reducing payment by \$10 is comparable to the sorting response of non-voters to the election flyer, implying significant social-image utility.

In order to estimate the value of voting ‘*because others will ask,*’ we need additional counterfactual social-image values, such as the reduction in utility for voters had they said they did not vote. These counterfactuals are not provided by the sorting moments.

We thus introduce a third set of crossed treatments. We randomize incentives to provide a different response to the turnout question. Specifically, we inform half of the respondents of the ten-minute survey that the survey will be eight minutes shorter if they state that they did not

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<sup>2</sup>This randomization also includes a group with no flyer, as well as a group with an opt-out box.

<sup>3</sup>We also randomize the information provided by the surveyor at the door. For half of the households, they indicated a survey “on your voter participation in the 2010 congressional election.” This manipulation, which was crossed with all other manipulations, did not have a significant effect on survey take-up for either voters or non-voters.

vote in the 2010 congressional election. For voters, this treatment amounts to an incentive to lie and permits us to quantify the disutility of voters were they to say (untruthfully) that they did not vote. For the 50 percent of non-voters who lie, this treatment provides an incentive to tell the truth. We provide a parallel \$5 incentive in the 5-minute survey to state that one did not vote.

The results reveal that non-voters are significantly more sensitive to these incentives than voters. When incentivized, the share of non-voters who lie decreases significantly, by 12 percentage points, while the share of voters who lie increases only insignificantly, by 2 percentage points. The results are similar for time and monetary incentives, and reveal a strong preference of voters for saying that they voted.

We combine the moments from the three sets of treatments to estimate the parameters of our model using a minimum-distance estimator. The benchmark estimates provide no evidence of pride in voting. On average, voters get negative utility from saying that they voted. However, voters obtain an even lower utility, by \$15, from saying that they did not vote. Non-voters are estimated to be on average indifferent between saying (truthfully) that they did not vote or lying and saying that they voted, with negative average utility from either option. We estimate substantial heterogeneity in social image, especially among voters.

These estimates identify the key parameters up to a scaling for the lying cost, which is unidentified. Since the lying cost is an integral part of the social-image value of voting, we adopt two approaches to address this limitation. First, we compute the value of voting for a range of values of the lying cost, including one we estimate from the laboratory evidence in Erat and Gneezy (2012). In this range, the implied value of voting ‘because of being asked *once*’ is in the range of \$1-\$3 for voters. Second, we identify the subsample of households with similar turnout histories prior to 2010, but different turnout in 2010. Voters and non-voters (in 2010) in this subsample are likely to be similar, and we assume that they have the same social-image and lying parameters. Under this assumption, we estimate lying costs of \$5, leading to a value of being asked once of \$1.50. Hence, the estimates are quite similar under both approaches.

To compute the overall value of voting due to being asked, we scale up the estimated value of being asked *once* by the average number of times asked. Our survey respondents report being asked, on average, five times whether they voted in the 2010 congressional election, implying an estimated value of voting ‘because others will ask’ in the range of \$5-\$15, a sizeable magnitude. Note that this estimate likely understates the value of voting due to being asked, since it is based on being asked by a (previously unknown) surveyor. The social-image utility and the lying cost from interactions with family, friends, and co-workers are likely to be larger.

Two implications are worth emphasizing. First, in a reasonable range of lying costs, the value of voting ‘because others ask’ is larger for voters than for non-voters, consistent with cross-sectional differences in turnout. Second, while the estimates are based on a congressional election and we do not have estimates for presidential elections, our survey respondents report

being asked nearly twice as often about voting in presidential elections, compared to congressional elections. Hence, for comparable social-image values, the social-image value of voting in presidential elections is about twice as high, in the range of \$10-\$30, consistent with the observed higher turnout in presidential elections.

The main field experiment was designed to measure the value of voting without affecting voting itself. Instead, we rely on sorting, survey completion, and survey responses. Yet, the model suggests an intervention to increase turnout: individuals with social-image motives are more likely to vote the more they expect to be asked. Experimentally increasing such expectation should thus lead to an increase in turnout.

In November of 2010 and of 2012, we did just that. A few days before the election, a flyer on the doorknob of treatment households informed them that ‘*researchers will contact you within three weeks of the election [...] to conduct a survey on your voter participation.*’<sup>4</sup> A control group received a flyer with a mere reminder of the upcoming election. The results are consistent with the model, though statistically imprecise. In 2010, the turnout of the treatment group is 1.3 percentage points higher than the control group (with a one-sided  $p$ -value of 0.06). In 2012, the turnout difference is just 0.1 percentage points (not significant). The results are consistent with the contemporaneous results of Rogers and Ternovski (2013), who also inform a treatment group that they may be called after the election about their voting behavior, and also find a positive (marginally significant) impact on turnout.

Finally, we would like to mention some caveats and alternative interpretations. First, the results are specific to their time and location—the 2010 congressional elections in Illinois. As we discussed, the lack of estimated pride in voting is likely related to the disappointing results for Democrats in 2010. It will be interesting to apply this methodology to other elections.

Second, the benchmark estimates rely on a number of assumptions, some of which are restrictive. In a series of robustness checks, we relax these assumptions, including allowing for measurement error in the voting record. We also address the concern that the observed ‘sorting out’ among non-voters may reflect a dislike of talking about politics, independent of their non-participation in the election. When we allow for such distaste, we lose the ability to estimate one of the social-image parameters. Still, the value of voting ‘due to being asked’ is identified and in fact remains unchanged, since it is identified by the lying treatments, which are immune to these concerns.

This paper complements a substantial literature on get-out-the-vote field experiments (e.g., Green and Gerber (2000)), summarized in Green and Gerber (2008). Most closely related is Gerber, Green, and Larimer (2008): a mailer announcing that voter participation will be made public to neighbors leads to a large increase in turnout, presumably because individuals care about being seen as voters as opposed to non-voters. This social-pressure intervention is extended in follow-up papers, including Rogers and Ternovski (2013). We build on Gerber,

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<sup>4</sup>We follow up with a door-to-door visit, as advertised.

Green, and Larimer (2008), but introduce post-election treatments and focus on the link between model and experiment, allowing us to quantify the underlying social-image parameters.

The paper also contributes to the vast literature on why people vote. Our main contribution, in addition to proposing the model of voting ‘because others will ask’, is to provide an estimate of the value of voting, which is rare in the literature. Among the few papers, Coate and Conlin (2004) and Coate, Conlin and Moro (2008) estimate, respectively, a group-rule utilitarian model and a pivotal-voting model on alcohol-regulation referenda data. Their estimates are up to a scaling for the voting cost, which is not identified. Levine and Palfrey (2007) estimate a pivotal-voting model, but use laboratory elections where parameters can be controlled. In contrast, we obtain estimates of the value of voting by virtue of the design of the field experiment.

The paper also relates to the literature on social image. The theoretical papers micro-found social-image concerns as signaling models (Benabou and Tirole, 2006; Andreoni and Bernheim, 2010; Ali and Lin, 2013). The empirical papers show that workers become more productive when they earn public rewards for their output (Ashraf, Bandiera, and Jack (2013)), that contributions to public goods increase when rewards are public (Ariely, Bracha, and Meier, 2009; Lacetera and Macis, 2010), and that energy consumption declines when social comparisons of energy usage are provided (Schultz et al. (2007); Allcott (2011)). Our study attempts to bring these literatures closer by providing estimates of the social-image parameters. We hope that future research strengthens the ties, providing estimates of the underlying signaling game.

Finally, this paper also complements a small but growing literature which emphasizes the role of models in field experiments (Banerjee et al. (2013), DellaVigna, List, and Malmendier, 2012) as well as to the literature on structural behavioral economics (Laibson, Repetto, and Tobacman (2007), Conlin, O’Donoghue, and Vogelsang (2007)). We envision the combination of these two literatures—where this paper resides—to be a growth area.

The remainder of the paper proceeds as follows. The next section introduces the model. Section 3 summarizes the experimental design. Sections 4 and 5 present, respectively, the reduced-form results and structural estimates for the main experiment. Section 6 introduces the get-out-the-vote experiment. Section 7 concludes.

## 2 Model

**Voting.** Voting depends on four factors: pivotality, warm glow, cost of voting, and expected social image. Individuals vote if the net expected utility of doing so is positive:

$$pV + g - c + N [\max(s_V, s_N - L) - \max(s_N, s_V - L)] \geq 0. \quad (1)$$

The first three terms in expression (1) capture the standard model of voting. The first term is the expected utility of being pivotal (Downs, 1957), with a pivotality probability  $p$  and value  $V$  assigned to deciding the election. The second term,  $g$ , is the warm glow from voting (as

in Riker and Ordeshook (1968)). The third term,  $-c$ , is the transaction cost of going to the polls. Since our experimental design does not focus on these components, only their sum will matter, which we denote by  $\varepsilon = pV + g - c$ . We assume  $-\varepsilon$  has c.d.f.  $H$ .

The crux of the model is the fourth term, the social-image motivation to vote (in the spirit of Harbaugh (1996)). An individual expects to be asked whether she voted  $N$  times, and has to decide whether to be truthful or to lie. Assume first that she has voted. In this case, she can (truthfully) state that she voted, which earns her utility  $s_V$ ; or she can lie and look like a non-voter, which earns her utility  $s_N$ , minus a psychological lying cost  $L$ . Therefore, the utility a voter receives from being asked is  $z^v \equiv \max(s_V, s_N - L)$ . Now assume that she did not vote. In this case, she can either state the truth and obtain the utility from appearing to be a non-voter,  $s_N$ , or lie and obtain  $s_V$  minus the lying cost  $L$ . Hence, the utility of being asked for a non-voter is  $z^{nv} \equiv \max(s_N, s_V - L)$ . The term in square brackets in (1) is therefore the net utility gain from voting due to being asked once.

The terms  $s_V$  and  $s_N$  capture how much the individual cares about being seen as a public good contributor (voter), or not, by others. These terms can be understood as reduced form representations for a signalling model, such as Benabou and Tirole (2006) and Ali and Lin (2013). Experimental evidence suggests that information about whether a person votes affects how favorably they are viewed by others (Gerber et al. 2012).

The term  $L$  captures the utility cost of lying. We assume that the cost of lying is non-negative,  $L \geq 0$ , and additive with respect to the social-image term. The assumption of positive lying costs is motivated both by introspection and by experimental evidence documenting that in cheap talk communication games, which are similar to survey questions, a sizeable portion of subjects prefer to tell the truth even when lying is profitable.

In the general case, we do not impose any restrictions on  $s_V$  and  $s_N$ , but we consider two special cases: (i) *Pride in Voting* ( $s_V > 0$ ): individuals care (positively) about stating that they are voters; (ii) *Stigma from Not Voting* ( $s_N < 0$  and  $s_V - L < 0$ ): individuals dislike both (truthfully) admitting to being non-voters and (untruthfully) saying that they are voters. Notice that both conditions could hold, for  $s_V > 0 > s_N$ , provided  $L$  is large enough.

Using the abbreviated notation  $\varepsilon$  for the other reasons to vote, we can rewrite the voting condition (1) as  $N\Phi(s_V - s_N, L) + \varepsilon \geq 0$ , where

$$\Phi(s_V - s_N, L) = \begin{cases} L & \text{if } s_V - s_N \geq L \\ s_V - s_N & \text{if } -L \leq s_V - s_N < L \\ -L & \text{if } s_V - s_N < -L. \end{cases} \quad (2)$$

As expression (2) shows, voting depends on the net social-image value  $s_V - s_N$  and on the cost of lying  $L$ . Figure 1 displays  $\Phi(s_V - s_N, L)$  as a function of  $s_V - s_N$  for  $L = 10$  and makes it clear that, in order for social image to contribute to voting, the net utility  $s_V - s_N$  must be non-zero and the lying cost  $L$  must be positive. If either of these conditions is not met, then

the individual either does not care about image, or can always signal the best-case scenario, irrespective of her true actions. Also notice that as long as individuals prefer to signal that they are voters ( $s_V - s_N > 0$ ), the net value of being asked for voting is weakly positive.

**Door-to-Door Survey.** To estimate this model, we design a door-to-door survey in which individuals are asked, among other questions, whether they voted. We model the behavior of an individual whose home is visited by a surveyor. If the visit is pre-announced by a flyer and the person notices the flyer (which occurs with probability  $r \in (0, 1]$ ), she can alter her probability of being at home and opening the door. A “survey flyer” (denoted by  $F$ ) informs the reader when the surveyor will visit, but leaves the content of the survey unspecified. An “election flyer” (denoted by  $FE$ ) additionally informs the reader that the survey will be about her voter participation in the previous election.

Once the surveyor visits the home, the respondent opens the door with probability  $h$ . If she did not notice the flyer (or did not receive one),  $h$  is equal to a baseline probability  $h_0 \in (0, 1)$ . If she noticed the flyer, she can optimally adjust the probability to  $h \in [0, 1]$  at a cost  $c(h)$ , with  $c(h_0) = 0$ ,  $c'(h_0) = 0$ , and  $c''(\cdot) > 0$ . That is, the marginal cost of small adjustments is small, but larger adjustments have an increasingly large cost. We allow for corner solutions at  $h = 0$  or  $h = 1$ . In the estimation, we assume  $c(h) = (h - h_0)^2 / 2\eta$ .

If the individual is at home at the time of the surveyor’s visit, she must decide whether to complete the survey. Consumers have a baseline utility  $s$  of completing a generic 10-minute survey for no monetary payment. The parameter  $s$  can be positive or negative to reflect that individuals may find surveys interesting, or they may dislike surveys. In addition, individuals receive utility from a payment  $m$  and disutility from the time cost  $c$ , for a total utility from survey completion of  $s + m - c$ . The time cost  $c$  equals  $\tau v_s$ , where  $\tau$  is the duration of the survey in fraction of hours, and  $v_s$  is the value of one hour of time. In addition, as in DellaVigna, List and Malmendier (2012), the respondent pays a social pressure disutility cost  $S \geq 0$  for refusing to do the survey when asked in person by the surveyor. There is no social pressure if the individual is not at home when the surveyor visits. We further assume that the respondent is aware of her own preferences and rationally anticipates her response to social pressure. In addition to the baseline utility  $s + m - c$  of doing a survey, there is the additional utility from being asked about voting,  $z^v$  for voters and  $z^{nv}$  for non-voters, as defined above.

We also vary whether the survey content is announced to the respondent when she opens the door with two ‘announcement’ treatments,  $a \in \{I, NI\}$ . When informed that the survey will ask about her voter participation ( $a = I$ ), an individual will consider the utility of being asked about voting,  $z^i$ , while deciding whether to complete the survey. If she is instead not informed at the door ( $a = NI$ ), she will neglect  $z^i$  - provided she has not already seen an election flyer. This announcement treatment is in the spirit of the election flyer treatment, but by design can only affect survey completion, not the probability of answering the door.

Finally, in some treatment cells we provide an incentive for the respondents to say that



they did not vote; the incentive is either in terms of time—an 8-minute shortening of the survey duration—or money—an extra \$5 for 1 more minute of questions. We denote by  $I$  the monetary value of the incentive. By incentivizing the respondent to say she did not vote, a voter is provided an incentive to lie, and will lie if  $s_N^v - L^v + I \geq s_V^v$ . In contrast, a non-voter is provided an incentive to tell the truth, and will do so if  $s_N^{nv} + I \geq s_V^{nv} - L^{nv}$ . By comparing the treatments with and without incentive  $I$ , we estimate the distribution of  $s_V - s_N + L$  for voters and of  $s_V - s_N - L$  for non-voters. Note that this treatment is unanticipated, and hence does not appear in the respondent’s decision to answer the door or participate in the survey.

**Solution.** Conditional on answering the door, the respondent of type  $i \in \{v, n\}$  agrees to the survey if  $s^i + m - c^i + z^i \geq -S^i$  assuming the respondent knows that the survey is about election and if  $s^i + m - c^i \geq -S^i$  otherwise. Working backwards, consider a respondent of type  $i$  who sees a survey flyer (which does not mention the election questions). The decision problem of staying at home (conditional on seeing a flyer) is  $\max_{h \in [0,1]} h \max(s^i + m - c^i, -S^i) - (h - h_0)^2 / 2\eta^i$ , leading to the solution  $h^{i*} = \max[\min[h_0 + \eta^i \max(s^i + m - c^i, -S^i), 1], 0]$ . An increase in pay  $m$  or a decrease in the time cost  $c$  will increase the probability of being at home and completing a survey. The parameter  $\eta^i$  determines the elasticity with respect to incentives of home presence. Alternatively, for a respondent who sees the election flyer the solution is given by  $h^{i*} = \max[\min[h_0 + \eta^i \max(s^i + m - c^i + z^i, -S^i), 1], 0]$ . If  $z^i > 0$ , the respondent will stay at home with a weakly higher probability with the election flyer, compared to the survey flyer, and vice versa if  $z^i < 0$ .

Finally, for both the survey flyer and the election flyer, there is a variant with an opt-out box which makes avoidance of the surveyor easier. In this condition, agents can costlessly reduce the probability of being at home to zero. Formally,  $c(0) = 0$  and  $c(h)$  is as above for  $h > 0$ .<sup>5</sup> The optimal probability of being at home  $h^*$  remains the same as without the opt-out option if there is no social pressure and, hence, no reason to opt out (since the respondent can costlessly refuse to do the survey) or if the agent expects to derive positive utility from completing the survey. In the presence of social pressure, however, the respondent opts out if the interaction with the surveyor lowers utility.

The following Propositions summarize the testable predictions about the impact of the election flyer (Propositions 1 and 2), about the incidence of lies about past turnout (Proposition 3) and about the expected number of times asked (Proposition 4).<sup>6</sup>

**Proposition 1. (Pride in Voting)** *With Pride in Voting, the probability of home presence  $P(H)$  and of survey completion  $P(SV)$  for voters is higher under the election flyer than under the survey flyer:  $P(H)_{FE}^v \geq P(H)_F^v$  and  $P(SV)_{FE}^v \geq P(SV)_F^v$ . Parallel results hold for the opt-out flyers:  $P(H)_{OOE}^v \geq P(H)_{OO}^v$  and  $P(SV)_{OOE}^v \geq P(SV)_{OO}^v$ . The probability of survey*

<sup>5</sup>This formalization allows a costless reduction of  $h$  to 0 but not to other levels. This is not a restriction because agents who prefer to lower  $h$  below  $h_0$  (at a positive cost) will strictly prefer to lower  $h$  to 0 at no cost.

<sup>6</sup>The proofs are in the Appendix.

completion for voters is higher when informed at the door that the survey is about voting:  $P(SV)_I^v \geq P(SV)_{NI}^v$ .

**Proposition 2. (Stigma from Not Voting)** *With Stigma from Not Voting, the probability of home presence  $P(H)$  and of survey completion  $P(SV)$  for non-voters is lower under the election flyer than under the survey flyer:  $P(H)_{FE}^{nv} \leq P(H)_F^{nv}$  and  $P(SV)_{FE}^{nv} \leq P(SV)_F^{nv}$ . Parallel results hold for the opt-out flyers:  $P(H)_{OOE}^v \leq P(H)_{OO}^v$  and  $P(SV)_{OOE}^v \leq P(SV)_{OO}^v$ . The probability of survey completion for non-voters is lower when informed at the door that the survey is about voting:  $P(SV)_I^{nv} \leq P(SV)_{NI}^{nv}$ .*

**Proposition 3. (Lying about Voting).** *If the net social-image utility is positive, the probability of lying about past voting,  $P(L)$ , should be zero for voters and larger for non-voters assuming no incentives to lie ( $I = 0$ ):  $P(L)^v = 0 \leq P(L)^{nv}$  for  $s_V - s_N > 0$ . For any social-image utility, the probability of lying is (weakly) increasing in the incentive  $I$  for voters and (weakly) decreasing in  $I$  for non-voters:  $\partial P(L)^v / \partial I \geq 0$  and  $\partial P(L)^{nv} / \partial I \leq 0$ .*

**Proposition 4. (Times Asked)** *The probability of voting is increasing in the number of times asked  $N$  if the social-image utility is positive and lying costs are positive:  $\partial P(V) / \partial N \geq 0$  for  $s_V - s_N > 0$  and  $L > 0$ .*

### 3 Experimental Design

**Logistics and Sample.** We employed 50 surveyors and many flyer distributors, mostly undergraduate students at the University of Chicago, who were paid \$10.00 per hour. All surveyors conducted surveys within at least two treatments, and most over multiple weekends.<sup>7</sup> The distribution of flyers took place on Fridays and Saturdays, and the field experiment took place on Saturdays and Sundays between July 2011 and November 2011. The locations are towns around Chicago shown in Figure 2.<sup>8</sup> Each surveyor is assigned a list of typically 13 households per half-hour on a street (constituting a surveyor-route), for a daily workload of 8 routes (10am-12pm and 1-3pm). Every half-hour, the surveyor moves to a different street in the neighborhood and begins a new route of 13 homes, typically entering a different treatment in the next route. Surveyors do not know whether a treatment involves a flyer, though they can presumably learn that information from observing flyers on doors.

To determine the sample in each of the towns visited, we obtain voting records from the Election unit of the Cook County Clerk’s office in January 2011. We begin with the full sample of addresses with at least one adult registered to vote. We then reduce the sample to households with homogeneous voting records in the congressional elections of November 2010:

<sup>7</sup>Additional details about the experiment, including the recruitment process, are in the Online Appendix.

<sup>8</sup>Arlington Heights, Elk Grove Village, Evanston, Glenview, Hoffman Estates, Lincolnwood, Mount Prospect, Northbrook, Oak Park, Park Ridge, Schaumburg, Skokie, Streamwood, Wilmette, and Winnetka. On almost all days, we visited one or two towns on a given day.

either every registered voter at the address voted in 2010, or no one did. Next, we randomize these households to a treatment at the surveyor-route level. Houses are grouped into surveyor-routes, which are then randomized to treatments. The treatment is a combination of four crossed interventions: (i) flyer treatments, (ii) payment and duration of the survey, (iii) survey content announcement at the door, and (iv) incentives to claim non-voter status.

**Treatments.** Each household was randomized into five flyer treatments with equal weights: *No Flyer*, *Survey Flyer*, *Election Flyer*, *Opt-Out Flyer*, and *Election Opt-Out Flyer*. Households in the *No Flyer* treatment receive no flyer. Households in the *Survey Flyer* treatment receive a flyer on the doorknob announcing that a surveyor would approach the home the next day within a specified hour (e.g., 3pm - 4pm, see top left example in Figure 3). Households in the *Election Flyer* treatment receive a similar flyer, with the added information that the survey will be about ‘*your voter participation in the 2010 congressional election*’ (second flyer from left in Figure 3). Households in the *Opt-Out Flyer* treatment receive a flyer as in the *Survey Flyer* treatment, except for an added check-box which the household can mark if it does not wish to be disturbed (third flyer from left in Figure 3). Similarly, the flyer in the *Election Opt-Out Flyer* treatment has an added opt-out check box. The flyers were professionally produced.

A second crossed randomization involves the duration of the survey as well as the compensation offered (if any) for completing the survey. The bottom row of Figure 3 displays flyers for the three treatments: (5-Minutes, No Payment), (10-Minutes, \$10 Payment), and (5-Minutes, \$10 Payment), each sampled with equal probability. In each of these treatments we reiterated the compensation and duration at the door.

The third set of crossed treatments involves how the surveyors described the survey once, after a knock on the door, a household member answered. The respondents were told “*We are conducting confidential \_ \_ \_ minute surveys in \_ \_ \_ today. [You would be paid \$ \_ \_ \_ for your participation.]*”, with the empty fields filled depending on the payment and duration treatments and the assigned town. The *No Information* group was then simply asked “*Do you think you might be interested?*”. The *Information* group was instead told “*The survey is about your voter participation in the 2010 congressional election. Do you think you might be interested?*”. Hence, the Information treatment provides information about the content of the survey in a similar way to the Election Flyer treatment. Respondents in the *Election Flyer* or *Election Opt-out Flyer* already knew about the content, provided they read the flyer. The top part of Figure 4 summarizes this first set of crossed treatments.

The fourth set of crossed treatments, summarized at the bottom of Figure 4, involves incentives to affect the response to a turnout question. In control surveys, individuals are simply asked whether they voted in the 2010 congressional election. For a subject in a 10-minute, \$10 survey in the treatment group, we offer an 8-minute incentive to the respondent to state that he or she did not vote. After the first question in the survey, the surveyor reads aloud: ‘*We have 10 minutes of questions about your voter participation in the 2010*

*congressional election, but if you say that you did not vote then we only have 2 minutes of questions. Either way you answer you will be paid \$10. That is, we have 10 minutes of questions, but if you tell us no to the question “did you vote in the 2010 congressional election” then we only have 2 minutes of questions to ask. Regardless of your answer you will earn \$10.’* The surveyor then points to where the survey ends if the respondent answers ‘no’, in which case the survey is indeed much shorter.

For respondents assigned to a 5-minute survey, we did not assign a time discount which could only have been a modest 3-minute reduction. Instead, we provide a monetary incentive to the treatment group as follows (with the material in brackets applying only to the (5-Minutes, \$10 Payment) conditions): ‘*We have 5 minutes of questions about your voter participation in the 2010 congressional election, but if you say that you did not vote then we have 1 extra minute of questions and we will pay you an extra \$5 for answering these additional questions [IF PAID: for a total of \$15]. If you say that you voted then we will just ask you the original 5 minutes of questions. [IF PAID: and pay you \$10 as promised.] That is, we have 5 minutes of questions, but if tell us no to the question “did you vote in the 2010 congressional election” then we have 1 extra minute of questions and you will earn an additional \$5 for answering these questions.’* Conditional on a 5-minute or a 10-minute survey, we determined the incentive or no-incentive treatment with equal weights.

Finally, we followed the promises made: we pay the individuals as promised, and we conducted a longer survey when the survey was advertised as lasting 10 minutes rather than 5 minutes. Further, in the treatments with a lying incentive, if the subject responded ‘no’ to the turnout question, the survey duration and payment were altered as promised.

**Sample.** We reached a total of 14,475 households. From this initial sample, we exclude 1,278 observations in which the households displayed a no-solicitor sign (in which case the surveyor did not contact the household) or the surveyor was not able to contact the household for other reasons (including, for example, a lack of access to the front door or a dog blocking the entrance). The final sample includes 13,197 households.

## 4 Reduced-Form Estimates

**Answering the door and survey completion.** We present graphical evidence in Figure 5 on the share of households answering the door and completing the survey as a function of the survey details, pooling across the five flyer treatments. Voters are very responsive to incentives, going from 33 percent answering the door for a \$0, 5-minute survey to 39 percent for the \$10, 5-minute survey. Hence, a \$10 incentive induces a 6 percentage point (20 percent) increase in the share answering the door. The effect is similarly large for the share completing the survey, a 6 percentage points (45 percent) increase. The elasticity of non-voters with respect to incentives is smaller with regards to answering the door, but is large with respect to survey

completion: 5 percentage points (62 percent).

Having established that households are responsive to the survey incentives, we turn to the key flyer treatment—whether the flyer informs the household about the election question. Figure 6a plots the results for voters, pooling across the different survey durations and payment incentives. We do not observe much difference for voters in the share answering the door, or the share completing the survey, between the Survey Flyer and the Flyer Election treatments. In the Opt-out treatments, we observe a *decrease* in the share answering the door and in the share completing a survey when the survey informs about the election. Thus, there is no evidence of pride from voting, and it appears that voters prefer not to be asked whether they voted.

For non-voters (Figure 6b), the difference between the Flyer and the Flyer Election treatments is large: there is a 6 percentage point drop (20 percent) in the probability of answering the door. The size of this effect is comparable to the effect of a \$10 incentive to complete the survey. There is a similar 3 percentage point (25 percent) decrease in the share completing a survey when the flyer announces the election question. The impacts are consistent but smaller in the opt-out treatments, with a 1.5 percentage point (15 percent) decrease in the share answering the door when the flyer mentions elections. These results indicate strong avoidance of non-voters, pointing to shame from admitting to not voting and disutility from lying.

These findings may depend on the context. The results of the 2010 congressional elections were very disappointing for Democrats, including in Illinois the loss of President Obama’s seat in the Senate, and correspondingly positive for Republicans. The lack of evidence for pride among voters may well be due to disappointment, given that the neighborhoods visited were largely Democratic. While our results are from a single election, we can differentiate the response based on the primary registration. In Figure 7 we present separate results for households with voters who participated in Republican primaries (left panel) versus households with voters registered in Democratic primaries (right panel).<sup>9</sup> Indeed, we detect sizeable sorting in by Republican voters in response to the election flyer, indicative of pride in voting in an election with positive results for the party. Among Democratic voters, instead, we observe sorting out as in the overall results, consistent with disappointment about the election. Among voters who did not participate in a primary (not shown), we also detect sorting out.

We now examine the effects of announcing the survey content at the door. Figure 8 plots survey completion rates by the door announcement type (Informed or Not Informed), pooling across all the flyer treatments. For voters, the effects of the door announcements are similar to those of the flyer announcements: there is no increase in survey completion from being informed about the voting question, and thus no evidence of pride. But non-voters also show

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<sup>9</sup>We record the most recent participation in primary elections by any registered member of the household. We define as ‘households with registered Republican voters’ households where at least one voter has voted in a Republican primary, and no voter has voted in a Democratic primary. Vice versa for the definition of households with registered Democrats.

essentially no effect on survey completion from being informed at the door. This is in contrast to the flyer treatments, where the election flyer leads to a sharp drop in answering the door and in survey completion by non-voters. We speculate that the difference (not captured in the model) could be that the flyer gives individuals time to think through the decision problem, while they must respond immediately when warned only at the door.

In Table 1, we present the regression analysis underlying Figures 5, 6a and 6b, 7, and 8 both with no controls and with fixed effects for surveyor  $i$ , day-town  $t$ , and hour-of-day  $h$ . We estimate, separately for voters and non-voters, the OLS regression:

$$y_{i,j,t,h} = \alpha + \Gamma T_{i,t,h} + \eta_i + \lambda_t + \zeta_h + \varepsilon_{i,j,t,h} \quad (3)$$

where the dependent variable  $y_{i,j,t,h}$  is, alternatively, an indicator for whether individual  $j$  opened the door ( $y^H$ ) or agreed to complete the survey ( $y^S$ ). The vector  $T_{i,t,h}$  contains indicators for the various survey treatments, with the baseline No-Flyer treatment for a \$0, 5 minute survey as the omitted group. We cluster the standard errors at the surveyor $\times$ date level.<sup>10</sup> Table 1 shows that the results shown in the previous figures are robust to the inclusion of the surveyor, date-location, and hour fixed effects. In the Online Appendix we present two sets of robustness results. In Online Appendix Table 1 we allow for different effects of a surveyor on different dates and location by including surveyor\*date\*location fixed effects; the results are unaffected. In Online Appendix Table 2 we present separate estimates for the first two month of the experiment (July and August 2011) and the next two months (October and November 2011); the results are comparable.<sup>11</sup> Finally, in Online Appendix Table 3 we present the results split by political registration, as in Figure 7.

**Lying about voting.** Next, we estimate the rates at which voters and non-voters misrepresent their voting behavior, and how these lies respond to the randomized incentives to lie (for voters) or to tell the truth (for non-voters). For the sample of individuals who completed the survey, we estimate the OLS regression

$$y_{i,j} = \alpha + \Gamma T_{i,j} + \eta_i + \varepsilon_{i,j} \quad (4)$$

where  $y_{i,j} = 1$  if individual  $j$  lied about her voting behavior to surveyor  $i$ , and 0 otherwise, and  $T_{i,j}$  is an indicator for whether respondent  $j$  is provided an incentive to say she did not vote. Due to the smaller sample, only surveyor fixed effects  $\eta_i$  are included in regressions.

In Table 2 and Figure 9, we present the results from these estimations. Recall that the incentive was always to say that one did not vote. Thus, we expect voters in the treatment condition to lie more than in the control, and non-voters to lie less. In Panel A of Table 2, to

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<sup>10</sup>For space reasons, the specification in Table 1 assumes an additive effect between the flyer treatments, the payment and duration treatments and the door information treatments. The empirical moments used for the estimation, listed in Appendix Table 1, are more disaggregated.

<sup>11</sup>We did not run the experiment in September 2011.

maximize power we pool across all survey treatments and across the 8-minute and \$5 incentive. Note first that non-voters, in the absence of any lying incentive, lie about 46 percent of the time about past turnout. This rate is within the range of previous results using the American National Election Studies and validated voter records (Silver, Anderson, and Abramson, 1986), and indicates that non-voters care about the social image that they convey. We also observe a 12 percent lying rate for voters, which could be explained by measurement error in the match to the voting records, or by a genuine preference among some voters to look like a non-voter.<sup>12</sup>

Turning to the effect of the incentives, the treatments have a small effect on voters: they lie 2.7 percentage points more when incentivized to do so, which is not statistically significant at conventional levels. For non-voters, in contrast, the effect is a highly significant 12 percentage point (25 percent) decrease in lying rates. Thus, voters appear to greatly dislike lying and claiming to be non-voters (relative to telling the truth), while non-voters are more easily moved between telling the truth and falsely claiming to be voters.

Do the results differ for the 8-minute time discount versus the \$5 incentive? Figure 9 shows that the results are very similar for the two types of incentives, especially for non-voters, suggesting an implied value of time of about \$35 per hour. Panels B-D in Table 2 further show that the results are similar whether the 5-minute survey was paid or unpaid.

**Summary.** To summarize the reduced form results, among voters we find little sorting on average into the home in the election flyer treatment, and therefore little evidence of pride in voting on average (though there is evidence among Republicans). But this does not imply that social image does not motivate their voting behavior. In fact, even with substantial incentives of \$5 earned or 8 minutes saved, over 85% of voters refuse to say they did not vote. This indicates that voters have a high lying cost  $L^v$ , a low social-image value of being a non-voter  $s_N^v$ , or both. Both these factors induce a high social-image value of voting. For non-voters, we find substantial sorting out in the election flyer treatment, indicating that that non-voters experience stigma on average from not-voting. Further, close to half of non-voters lie and claim to be voters when asked. This implies that on average they are indifferent between the options:  $s_V^{nv} - s_N^{nv} = L^{nv}$ . A \$5 incentive reduces lying by 25%, indicating that a substantial share of non-voters are close to the margin in their decision to tell the truth or lie. In the next section, we utilize all the experimental treatments to estimate the social-image value of voting.

## 5 Structural Estimates

**Set-up.** To estimate the model of Section 2, we impose additional assumptions, some of which are relaxed below. Since all parameters are allowed to differ between voters and non-

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<sup>12</sup>Notice that non-registered voters do not appear in our voting records. Hence, some of the households which we classify as ‘voting households’ may include some non-voters, accounting for some of the lying rate for these households. In the Structural Estimates, we present results which allow for measurement error.

voters, for simplicity we omit the superscript  $i = \{v, nv\}$ . We assume that the social-image variables  $s_V$  and  $s_N$  are independently normally distributed across individuals, with differing means  $\mu_V$  and  $\mu_N$  but the same standard deviation,  $\sigma_V = \sigma_N$ , which we denote by  $\sigma_{SI}$ . The normality assumption allows for individuals who prefer the social image associated with not voting ( $s_V < s_N$ ). We also assume a normal distribution with parameters  $\mu_s$  and  $\sigma_s$  for the utility  $s$  of completing an unpaid 10-minute survey, as well as a quadratic cost of changing plans to be at home,  $c(h) = (h - h_0)^2 / 2\eta$ .

The key parameters of interest are: (i)  $\mu_V$ , the mean social-image utility from saying that one voted; (ii)  $\mu_N$ , the mean social-image utility of saying one did not vote; (iii)  $\sigma_{SI}$ , the standard deviation of the social-image utilities; (iv)  $L$ , the lying cost. In the benchmark specification, the parameters are identified up to the cost of lying; we thus display the results as a function of an assumed lying cost.

We also identify the following auxiliary parameters: (i)  $h_0$ , the baseline probability of opening the door; (ii)  $r$ , the probability of observing (and remembering) the flyer; (iii)  $\eta$ , the responsiveness of the probability of opening the door to the desirability of being at home; (iv)  $\mu_s$  and  $\sigma_s$ , the mean and standard deviation of the baseline utility of doing a survey; (v)  $v^s$ , the value of one hour of time; (vi)  $S_s$ , the social pressure cost associated with saying no to the survey request. The total number of parameters is 11, including  $L$ , for voters and as many for non-voters for a total of 22 parameters.

To estimate the model, we use a classical minimum-distance estimator. Denote by  $m(\xi)$  the vector of theoretical moments as a function of the parameters  $\xi$ , and by  $\hat{m}$  the vector of observed moments. The minimum-distance estimator chooses the parameters  $\hat{\xi}$  that minimize the distance  $(m(\hat{\xi}) - \hat{m})' W (m(\hat{\xi}) - \hat{m})$ . As a weighting matrix  $W$ , we use the diagonal of the inverse of the variance-covariance matrix. Hence, the estimator minimizes the sum of squared distances, weighted by the inverse variance of each moment. As a robustness check, we also use the identity matrix as the weight. We discuss further details in Appendix A.

To list the moments  $m(\xi)$ , we introduce the following indices:  $i \in \{v, nv\}$  indicates voters and non-voters,  $k \in \{NF, F, FE, OO, OOE\}$  indicates the flyer treatments,  $m$  indexes the payment and duration treatments,  $m \in \{\$0, 5min; \$10, 10min; \$10, 5min\}$ ,  $a$  indicates the treatments on survey information at the door,  $a \in \{I, NI\}$ , and  $l$  indexes incentives to lie,  $l \in \{NoInc, 8min, \$5\}$ . The moments  $m(\xi)$  are: (i) the probability opening the door in survey treatments  $k, m$ ,  $P(H)_{k,m}^i$ ; (ii) the probability of completing the survey in survey treatments  $k, m$ ,  $P(SV)_{k,m}^i$ ; (iii) the probability of checking the opt-out box in the Opt-Out treatments,  $P(OO)_{k,m}^i$  for  $k \in \{OO, OOE\}$  (iv) the probability of completing the survey in the survey content treatments, given the flyer treatments:  $P(H)_{a,k}^i$  and (v) the probability of lying about past turnout conditional on completing the survey, given incentive  $l$ ,  $P(L)_l^i$ .<sup>13</sup> The

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<sup>13</sup>We present pooled moments across some of the treatments for two reasons. In some cases we do not expect any impact of the treatment on the relevant moment, such as of the lying incentives on the probability of



empirical moments  $\hat{m}$  are estimated in a first stage model using the same controls as in the main regressions, and are listed in Appendix Table 1.

**Identification.** While the parameters are estimated jointly, it is possible to address the main sources of identification of individual parameters. The difference in home presence and survey completion between the Flyer and Flyer-election, and between the Opt-Out and Opt-Out election treatments, identify key social-image parameters (for a given elasticity  $\eta$  and value of time  $v$ , see below). For voters, they identify the mean social-image utility  $\mu_V^v$ . For non-voters, given that on average half of non-voters lie in our sample (absent incentives to do otherwise), the average social-image utility from admitting to not voting  $\mu_N^n$  equals approximately the utility of lying,  $\mu_V^n - L^n$ . Hence, the sorting response for non-voters to the election surveys identifies both  $\mu_N^n$  and  $\mu_V^n - L^n$ .

The heterogeneity in social image  $\sigma_{SI}$  and the average utility difference between answering truthfully and lying are both identified from the lying incentives. For example, an 8 minute incentive reduces the share of non-voters lying by 12 percentage points (Table 2, Panel D), implying  $\Pr(s_N^{nv} < s_V^{nv} - L^{nv} < s_N^{nv} + (8/60)v_s^{nv}) = 0.12$  or  $\Pr(0 < s_V^{nv} - L - s_N^{nv} < (8/60)v_s) = 0.12$ , where  $s_V^{nv} - L - s_N^{nv}$  is normally distributed with variance  $2\sigma_{SI}^{nv2}$ .

As for the auxiliary parameters, the mean and standard deviation of the value of completing a survey,  $\mu_s$  and  $\sigma_s$ , are identified from the survey completion rates for different monetary incentives. The value of time  $v_s$  is identified from the comparison between payment increases (from \$0 to \$10) and duration decreases (from 10 to 5 minutes). The baseline probability of answering the door,  $h_0$ , is pinned down by the share opening the door in the no-flyer treatments. The probability of observing and remembering the flyer,  $r$ , is identified by the fraction of households checking the opt-out box in the Opt-out treatment (10 to 13 percent), which equals  $rh_0F_s(c - m)$ , and by the fraction opening the door in these treatments. The elasticity of opening the door  $\eta$  with respect to incentives and the social pressure  $S_s$  are related to the fraction opening the door in the different survey treatments.<sup>14</sup>

**Benchmark Estimates.** The benchmark estimates (Table 3) provide no evidence of pride for voters: voters on average dislike informing others that they voted:  $\mu_V^v = -5.86$  (se 1.94). However, voters dislike lying even more:  $\mu_N^v - L^v = -24.81$  (se 5.14) is the disutility from saying that they did not vote. Notice that we cannot parse the extent to which this disutility is due to a large net social-image utility  $\mu_V^v - \mu_N^v$  or a large lying cost  $L^v$ . There is substantial heterogeneity in these signalling values:  $\sigma_{SI}^v = 12.35$  (se 3.10), implying that 32 percent of

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opening the door or completing the survey. In other cases, we pool to keep the list of moments readable and to guarantee a sizeable sample in each cell, when the model does not imply important differences across the pooled treatments; for example, we do not consider the impact of the survey content treatment separately as a function of the survey duration and payment.

<sup>14</sup>Consider a respondent of type  $i$  who dislikes answering a survey and hence will say no and incur the social pressure cost  $S_s$ . In the flyer treatment  $F$ , she will choose to be at home with probability  $h_0^i - \eta^i r^i S_s^i$  (barring corner solutions for  $h$ ). Hence, knowing  $h_0^i$ ,  $\eta^i$ , and  $r^i$ , it is possible to identify  $S_s^i$ .

voters *do* take pride in saying they voted.

For non-voters, we estimate significant stigma on average from admitting that they did not vote:  $\mu_N^{nv} = -4.61$  (se 2.40). On average, non-voters are nearly indifferent between admitting they did not vote and lying and claiming they voted ( $\mu_V^{nv} - L = -4.23$ , se 2.20), consistent with the finding that about half of non-voters lie in the control treatments. Heterogeneity across individuals is sizeable but smaller than for voters:  $\sigma_{SI}^{nv} = 6.20$  (se 1.29).

Turning to the auxiliary parameters, we estimate that on average neither voters nor non-voters like unpaid surveys, but there is a substantial heterogeneity, with voters being more likely to complete unpaid surveys. (Voters are likely public good providers generally). The estimated time value is \$65 per hour for voters and \$19 for non-voters, a difference consistent with the strong positive correlation between income and turnout. Voters are also estimated to incur higher social pressure costs from declining to participate in the survey ( $S_s^v = \$1.76$  versus  $S_s^n = \$0.06$ ) and a lower elasticity of home presence ( $\eta^v = 0.13$  versus  $\eta^n = 2.86$ ), although the elasticity for non-voters is imprecisely estimated.<sup>15</sup>

**Value of Voting.** Using the estimates, we compute the average social-image value of voting due to being asked *once*,  $\int \Phi(s_V - s_N, L) dF(s_V, s_N)$ . Since the benchmark model is identified up to the lying cost, we cannot point identify this value. We can, however, plot the social-image value of voting as a *function of the lying cost  $L$*  for a range of plausible values, shown in Figure 10a for both voters and non-voters. If lying is entirely costless ( $L = 0$ ), the social-image value is zero, since non-voters and voters can costlessly claim their preferred social image. As the underlying cost of lying increases, the value of voting is inverse-U-shaped for voters, while it rises monotonically for non-voters. The intuition for voters is as follows. As  $L$  rises, as Figure 1 illustrates, the value of voting (weakly) increases in  $L$  for a *given* positive  $s_V - s_N$ , since lying becomes costlier. However, as  $L$  increases, the net estimated social image  $s_V - s_N$  mechanically declines. (The data pins down the value for voters of saying that they did not vote,  $s_N - L$ ; as  $L$  increases,  $s_N$  must increase to compensate, lowering  $s_V - s_N$ ) Initially, the first force dominates since the lying cost is likely to be binding. For high enough lying cost, however, the second force shifting the net social image dominates, ultimately leading to a negative value of voting for high enough  $L$ . The intuition for non-voters is parallel.

The social-image value of voting in Figure 10a has two important implications for voting. First, even if we do not know the lying cost, for a range of plausible values the correspondent value of voting is quite flat. For a lying cost in the range between \$2 and \$15, the value of voting due to being asked once for voters lies between \$1.5 and \$4. To provide a benchmark estimate from a different context, In Appendix D we estimate the lying cost using the data from Erat and Gneezy (2012), a representative cheap talk laboratory experiment, and obtain an estimate for the lying cost in this range, of \$7. Hence, even though we are not point identified, the range of uncertainty for the value of voting is not too large provided one agrees on the range

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<sup>15</sup>We discuss the imprecision of the estimates of  $\eta$  and  $S$  in Appendix C.

for  $L$ . (For non-voters instead the estimates of the value of voting increase sharply with  $L$ ). Second, we can contrast the value of voting for voters and non-voters, provided we assume the same lying cost. For values of the lying cost up to \$7, the estimated value of voting is larger for voters than non-voters, consistent with observed cross-sectional differences.

To obtain the ultimate value of voting because others ask, we scale up the value of being asked once by the expected number of times asked,  $N$ . We measure this parameter with survey questions on how often the survey respondents has been asked whether they voted in the 2010 congressional election by friends, relative, coworkers, and other people. The Online Appendix Figure 1 displays the c.d.f. of the total number of times asked: 60 percent of respondents report being asked at least once, and 20 percent report being asked more than 10 times. On average, hence, respondents report being asked around 5 times for the 2010 congressional election, with similar magnitudes for voters and non-voters ( $N^v = 4.89$  and  $N^{nv} = 5.33$ ). The Figure also reports the number of times people report being asked for the 2008 presidential election: the average is about twice as high, with 40 percent of people reporting to be asked at least 10 times. This number is consistent with the corresponding figures in the Cooperative Congressional Election Study as reported in Gerber et al. (2012).

To obtain the total value of voting *because others ask* in the 2010 election, we multiply the value of voting due to being asked once by the average times asked, leading to an overall value of voting for voters, for the range of lying costs, between \$6.5 and \$15, sizeable magnitudes (Figure 10b and Table 3).<sup>16</sup> We can also use these estimates to conjecture the social-image value of voting for presidential elections. Assuming that the social-image parameters for presidential elections are at least comparable to the ones in congressional elections, we can multiply the value of voting due to being asked once by the number of times people report being asked in the 2008 presidential election. The implied value of voting in presidential elections, also plotted in Figure 10b, is about twice as large, in the range of \$13-\$30. The model is therefore also consistent with the observed higher turnout in presidential elections.

Finally, we turn to the welfare effect of being asked once if one has voted (Table 3). For voters, this is the average value of  $z^v = \max(s_V^v, s_N^v - L^v)$ , and is estimated to be  $-4.63$  (se 1.98). Interestingly, non-voters are estimated to have a less negative utility from being asked,  $-0.92$  (se 2.15) because non-voters' social-image utility distribution is closer to zero.

**Decomposing the Benchmark Estimates.** To highlight the identification, in Table 4 we re-estimate the model using subsets of the moments. When we use only the lying moments (Column 2), the levels of the social-image parameters  $\mu_V$  and  $\mu_N$  are not identified, but we can estimate the average *difference*  $\mu_V - \mu_N$  (given a lying cost), as well as the heterogeneity  $\sigma_{SI}$ . Hence, holding  $\mu_V$  fixed, we can estimate the other social-image parameters. The implied

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<sup>16</sup>We estimate the social-image utility when asked by a surveyor. If social-image concerns or lying costs when interacting with friends, family and colleagues are higher than those in a one-shot interaction with a surveyor, then our estimates are likely to be lower bounds of the social-image value of voting.

value of voting is then essentially the same as when using the much richer set of moments. This highlights the key role that the lying treatments play in estimating the value of voting.

Alternatively, we utilize all the moments other than the experimental variation in incentives to lie (Columns 3 and 4). (We include the lying rates in the control group) These moments suffice to estimate all of the ancillary parameters as well as  $\mu_V$  for voters. However, the other parameters are not identified because the heterogeneity term  $\sigma_{SI}$  is unidentified. Thus, we present two special cases with fixed low heterogeneity (Column 3,  $\sigma_{SI}^{nv} = \sigma_{SI}^v = \$5$ ) and fixed high heterogeneity (Column 4,  $\sigma_{SI}^{nv} = \sigma_{SI}^v = \$20$ ). The estimates reveal substantial sensitivity to the assumed  $\sigma$ . The key contribution of these sorting moments is not to the value of voting, but to the welfare effect of being asked, which is not identified using only the lying moments.

Finally, in Column 5 we report estimates excluding the moments split by whether households are informed at the door about the election topic. When we exclude these moments, the estimated social-image parameters indicate a larger dislike of being asked about voting, especially for non-voters. This change, however, has essentially no effect on the estimated value of voting which, as we showed, largely depends on the lying incentives.

**Robustness.** In Table 5, we explore the robustness of the parameter estimates to alternative assumptions. First, we consider an alternative explanation of the results: the sorting out of non-voters may be due to a dislike of talking about politics, rather than any stigma from admitting to not voting. We thus allow for a utility of talking about politics which is independent of whether one voted or not (Column 2). With this extra parameter, we lose the ability to estimate a social-image parameter, but the estimated value of voting, which is identified by the lying treatments (provided a value of time), is unchanged.

Next, we consider two forms of measurement error. First, notice that the voting records do not include information about non-registered adults in a household. Since these individuals are necessarily non-voters, the person answering the door in an apparent voting household may actually be a non-voter. (This would explain why 10% of voting households appear to lie about voting even absent incentives to lie). In Column 3, we assume that 10% of respondents in voting households are actually non-voters. In Column 4, we allow for measurement error for both groups of households, and assume that 10% of respondents in a voting (or respectively, non-voting) household are non-voters (respectively, voters). Both specifications lead to a somewhat larger value of voting for a given lying cost.

We also consider a number of other robustness checks in Appendix Table 2. We show that the results are very similar if (i) we equally weight the moments instead of utilizing the inverse of the variances of each moment as weights; (ii) we assume positively correlated  $s_V$  and  $s_N$ , with correlation coefficient  $\rho = 0.5$ ; (iii) we assume negatively correlated  $s_V$  and  $s_N$ , with  $\rho = -0.5$ . We can also, instead of estimating the elasticity of sorting  $\eta$ , fix it at either a large value ( $\eta^v = \eta^{nv} = 1.0$ ) or a small value ( $\eta^v = \eta^{nv} = 0.02$ ); these specifications are associated with a substantially worse fit, with some shifts in the social-image parameters.

**Full Estimation.** Thus far, we have been unable to identify the psychological cost of lying  $L$  separately from the utility terms  $s_V$  and  $s_N$ . However, we would be able to estimate all parameters, including the lying cost, if we made the additional assumption that voters and non-voters share the same social-image parameters. To see this, consider that the estimates allow us to identify  $\mu_V^v$  and  $\mu_N^v - L^v$  for voters, and  $\mu_N^{nv}$  and  $\mu_V^{nv} - L^{nv}$ ; if one assumes  $\mu_V^v = \mu_V^{nv}$ ,  $\mu_N^v = \mu_N^{nv}$  and  $L^v = L^{nv}$ , the value of the lying cost is identified.

In general, the assumption of equality for voters and non-voters is unpalatable. Different social-image parameters might be the very difference between voters and non-voters. The assumption, however, is less problematic in a subsample of voters and non-voters with similar voting histories. We eliminate always-voters and never-voters and consider households with individuals who vote some of the time, some of whom happened to vote in 2010, while others happened not to. Within this sub-group, individuals who voted in 2010 and individuals who did not vote in 2010 are more likely to be similar.

Formally, we use the individual-level voting records for all elections from 2004 to 2010, primaries included, to predict the probability that an individual will vote in November of 2010. We then restrict the sample to households where *all* registered individuals have a predicted probability of voting between 25 and 75 percent, leaving us with a sample of 5,901 households. Column 1 of Table 6 reports the estimation results. The mean utility of truthfully saying one voted ( $\mu_V$ ) is estimated near zero, with a net signalling utility  $\mu_V - \mu_N$  of 4.50. The estimated lying cost is  $L = \$4.63$  (se \$1.08), in the ballpark of plausible values. The implied total signaling value of voting in the 2010 elections is estimated at \$8.28 for voters and \$8.80 for non-voters (since the average number of times asked is slightly higher for non-voters).

The estimates in Column 1 require that not only the social signaling and lying parameters be the same across voters and non-voters, but that the auxiliary parameters be the same as well. In Column 2, we remove the latter assumption. Allowing for differences in the auxiliary parameters has very little effect on the key parameter estimates, but improves the fit of the model quite a bit, from an SSE of 132.49 (Column 1) to an SSE of 100.79. Finally, in Column 3 we report estimates where we allow all parameters to differ between voters and non-voters. This is the parallel of the benchmark specification in Table 3, but restricted to this subsample, and hence does not allow for point identification of all parameters. Allowing for this extra difference leads to only a relatively small increase in the fit, to an SSE of 97.20.

## 6 Get-out-the-vote Experiment

The experiments described above are designed to measure the value of voting without affecting voting itself. Instead, we rely on the sorting of households, the willingness to complete a survey, and incentivized responses, conditional on a past voting behavior (which we observe). Yet, the model suggests a natural treatment to increase voter turnout. As Proposition 4 states,

individuals with social-image motives are more likely to vote the more frequently they expect to be asked about voting, an expectation which we can manipulate experimentally.

In November of 2010 and of 2012, we set out to do just that for towns surrounding Chicago. In the five days before the election date, we posted a flyer on the doorknob of households in the treatment group informing them that ‘*researchers will contact you within three weeks of the election [...] to conduct a survey on your voter participation*’. Figure 11 shows the flyer for the 2012 election. (After the election, we follow up with a door-to-door visit, as advertised). Since this flyer could also impact turnout through a reminder effect, we compare this group to a group which received a flyer with a mere reminder of the upcoming election, also displayed in Figure 11. Finally, a control group received no flyer. After the election, we obtain the voting record for all individuals with residence at the addresses targeted in this experiment and we examine the impact on voter turnout.

Table 7 reports the results for both the November 2010 and the November 2012 intervention using an OLS specification: the dependent variable is an indicator for whether the individual voted in the specific election. Notice that there may be multiple individuals at one address, each of which is a separate observation. The November 2010 experiment has a sample size of 31,306 individuals targeted. The turnout in the control group (which received no flyers) is 60.0 percentage points. Compared to this control group, the mere reminder had no effect, leading to an estimated decrease of .2 percentage point. Compared to the flyer with a mere reminder, the flyer with announcement of future question about voting raises turnout by 1.4 percentage points, a sizeable effect, if insignificant. In Column 2, we add controls for the full history of voting of the households in all elections between 2004 and the election in question. Adding controls in a randomized experiment should not affect the point estimates if the experiment is conducted properly, but can reduce the residual variance, and hence increase precision. Indeed, the controls have very little impact on the point estimates, but they nearly halve the standard errors since past voting is highly predictive of future voting (the  $R^2$  increases from 0.00 to 0.40). In this specification, the estimated effect of the flyer with announcement of future asking is an extra 1.3 percentage points in turnout, with a two-sided p-value of 0.12 (one-sided p-value of 0.06). While not quite statistically significant, the sizeable effect is certainly consistent with the predictions of the model.

Columns 3 and 4 display the estimates for the November 2012 election. In this later election, we were able to deploy a larger flyering team, guaranteeing a sample size of 93,805 individuals. Given the different nature of the election (presidential versus congressional), the baseline turnout in the control group is higher, at 73.1 percentage points, leaving a smaller share of non-voters to be convinced. We find suggestive evidence that the reminder flyer itself may have increased turnout, with little evidence of a differential effect of the flyer with announcement of the future visit. In the specification with controls (Column 4), the differential effect is estimated to be 0.1 percentage points, not significant. The smaller effect in this second

election is consistent with two interpretations. The more competitive election is likely to have reduced the number of individuals on the margin of voting. Alternatively, the point estimates for the 2010 election may overstate the magnitude of the result.

Overall, the evidence is qualitatively consistent with the predictions of the model, if statistically imprecise. The results are consistent with the contemporaneous and independent results of Rogers and Ternovsky (2013) who similarly inform a treatment group that they may be called after the election about their voting behavior, and find a similarly positive (marginally significant) impact on turnout.

## 7 Conclusion

We have presented evidence from field experiments designed to provide estimates for a model of turnout: individuals vote because they expect to be asked, and they anticipate the disutility associated with admitting to not voting, or with lying about voting. The combination of three crossed experimental arms allows us to estimate all but one of the key parameters. We show that for a range of assumptions about the unidentified lying cost we obtain estimates of the value of voting due to being asked in the range of \$5-\$15, a sizeable magnitude for a congressional election. For a subsample with medium propensity to vote, we identify a value of voting due to being asked of \$8. We conjecture larger magnitudes for presidential elections.

A methodological ingredient of this paper is the tight link between a simple model and the experimental design. This allows us not only to derive reduced-form results, but to use such results to estimate the underlying parameters. As such, this paper attempts to bridge a gap between two thriving, but largely separate literatures: the theoretical literature on voting and the reduced-form field experiments on get-out-the-vote and turnout. We hope that methodologies similar to the ones in this paper will be useful in providing further insights.

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## A Appendix A - Mathematical Appendix

**Proof of Propositions 1 and 2.** We consider first the probability of being at home. As discussed in the text, the probability of being at home will be: (i)  $h_0$  in the absence of flyer, or if the person does not see the flyer; (ii)  $h^{i*} = \max[\min[h_0 + \eta^i \max(s^i + m - c^i, -S^i), 1], 0]$  if the person saw a survey flyer, and (iii)  $h^{i*} = \max[\min[h_0 + \eta^i \max(s^i + m - c^i + z^i, -S^i), 1], 0]$  if the person saw an election flyer. Under *Pride in Voting*,  $z^v = \max(s_V^v, s_N^v - L^v) \geq s_V^v$  is positive. Hence,  $h^*$  will be at least as high under *FE* than under *F* for voters. Conversely, under *Stigma from Not Voting*,  $z^{nv} = \max(s_V^{nv} - L^{nv}, s_N^{nv})$  is negative, and hence  $h^*$  will be lower under *FE* than under *F* for non-voters. Under opt-out, a person who sees the flyer will opt out (and hence set  $h^* = 0$ ) if  $s^i + m - c^i < 0$  under *OO* and if  $s^i + m - c^i + z^i < 0$  under *OOE*. Under *Pride in Voting*,  $z^v$  is positive; hence, for any set of parameters, if the person opts out under *OOE*, she will also do so under *OO* (but not the converse). Hence, for any given set of parameters treatment, the probability of being at home is lower under *OO* than under *OOE* and thus  $P(H)_{OOE}^v \geq P(H)_{OO}^v$ . Conversely, under *Stigma from Not Voting*,  $z^{nv}$  is negative so the converse result applies and  $P(H)_{OOE}^{nv} \leq P(H)_{OO}^{nv}$  follows.

Turning to the probability of answering a survey, conditional on answering the door, an individual will agree to the survey if  $s^i + m - c^i + z^i \geq -S^i$  assuming she knows that the survey has an election topic and if  $s^i + m - c^i \geq -S^i$  in case she does not know. By the same token as above, holding constant the selection into being at home, the person will be more likely to complete the survey if informed about the election topic under *Pride* and if not informed under *Stigma*. Hence, the conclusion  $P(SV)_I^v \geq P(SV)_{NI}^v$  under *Pride* and  $P(SV)_I^{nv} \leq P(SV)_{NI}^{nv}$  under *Stigma* hold (remember that the treatments *I* and *NI* take place after the sorting decision).

To consider the effect of *F* and *FE* on  $P(SV)$  we need to take into account the selection into answering the door. We consider separately the following four exhaustive cases: (i)  $\max(s^i + m - c^i + z^i, s^i + m - c^i) < -S^i$ . In this case,  $P(SV) = 0$  under any condition; (ii)  $\min(s^i + m - c^i + z^i, s^i + m - c^i) \geq -S^i$ . In this case, the person will complete the survey conditional on being at home, so  $P(H) = P(SV)$ , and the comparison follows from the results above on  $P(H)$ ; (iii)  $s^i + m - c^i + z^i < -S^i \leq s^i + m - c^i$ . In this case, which occurs for non-voters under *Stigma*,  $P(SV)_{FE} = 0 \leq P(SV)_F = P(H)_F$ ; (iv)  $s^i + m - c^i < -S^i \leq s^i + m - c^i + z^i$ . In this case, which occurs for voters under *Pride*,  $P(SV)_F = 0 \leq P(SV)_{FE} = P(H)_{FE}$ . Under *Pride*, cases (i), (ii), and (iv) apply and pairwise comparisons for all these cases show  $P(SV)_{FE}^v \geq P(SV)_F^v$ . Under *Stigma*, cases (i), (ii), and (iii) apply and pairwise comparisons for all these cases show  $P(SV)_{FE}^{nv} \leq P(SV)_F^{nv}$ .

Turning to  $P(SV)_{OO}$  and  $P(SV)_{OOE}$ , consider that, conditional on seeing the flyer, any person who answers the door will complete the survey. (Otherwise, this person could have costlessly opted out.) Therefore, the results on  $P(SV)_{OO}$  and  $P(SV)_{OOE}$  follow directly from the results on  $P(H)_{OOE}$  and  $P(H)_{OO}$ .

**Proof of Proposition 3.** A voter will lie if  $s_N^v - L^v + I \geq s_V^v$  or  $-(s_V^v - s_N^v) - L^v \geq -I$ . Under the assumption  $s_V^v - s_N^v > 0$  and given  $L \geq 0$ , the left-hand side in the second expression is always negative; hence, a voter will never lie with no inducement ( $I = 0$ ). And increase in  $I$  makes it more likely that the expression will be satisfied and thus (weakly) increases lying.

We consider then a non-voter. The lying condition for non-voters is  $s_V^{nv} - L^{nv} \geq s_N^{nv} + I$  or  $(s_V^{nv} - s_N^{nv}) - L^{nv} \geq I$ . The left-hand side can be positive or negative depending on whether the net signalling utility or the lying cost is larger; hence, non-voters may lie even absent incentives  $I$ . Increased incentives  $I$  make it less likely that the inequality will be satisfied and hence (weakly) reduce lying.

**Proof of Proposition 4.** Individuals vote if the net expected utility in (1) is positive.

Remembering that  $H$  is the c.d.f of  $-(pV + g - c)$ , we can rewrite the probability of voting as  $H [N [\max(s_V, s_N - L) - \max(s_N, s_V - L)]]$ . Under the assumptions  $s_V - s_N > 0$  and  $L > 0$ , it follows that  $\max(s_V, s_N - L) = s_V$  and that  $s_V > \max(s_N, s_V - L)$ . Hence, the term in square brackets is positive and the conclusion follows.

## B Appendix B - Experiment Implementation

Each flyer distributor’s participation in the study followed two steps: (1) an invitation to work as a paid volunteer for the research center and (2) participation as a distributor of flyers in the door-to-door campaign. Each surveyor’s participation in the study typically followed four steps: (1) an invitation to work as a paid volunteer for the research center, (2) an in-person interview, (3) a training session, and (4) participation as a surveyor in the door-to-door campaign.

## C Appendix C - Estimation Appendix

The classical minimum-distance estimator chooses the parameters  $\hat{\xi}$  that minimize the distance given by  $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$ . As a weighting matrix  $W$ , we use the diagonal of the inverse of the variance-covariance matrix. Hence, the estimator minimizes the sum of squared distances, weighted by the inverse of the variance of each moment. (Given the large number of moments, weighting the estimates by the inverse of the full variance-covariance matrix is problematic computationally. As a robustness check, we also use an identity weighting matrix, as reported in Appendix Table 2.)

To compute the theoretical moments  $m(\xi)$ , we derive closed-form expressions for the moments when possible (for example, the probability of being at home and completing the survey in the no flyer, survey flyer, and survey opt out treatments). When this is not possible, we use two types of numerical integration algorithms to calculate the moments. When integrating over a single random variable such as  $z^v = \max\{s_V^v, s_N^v - L^v\}$  or  $z^{nv} = \max\{s_N^{nv}, s_V^{nv} - L^{nv}\}$ , we first derive expressions for the probability density function of  $z^i$  and then use highly accurate numerical integration algorithms based on adaptive Simpson quadrature, pre-implemented in Matlab as the *quadl* and *quadgk* routines. When double integrating over the distributions of two random variables with dependent limits of integration - the survey utility  $s$  and the social-image taste  $z^i$  - we use a 12-point non-adaptive Gaussian quadrature method for computational efficiency.

The empirical moments  $\hat{m}$  are estimated in a first-stage model using the same controls as in the main regressions, and are listed in Appendix Table 1. In particular, all the moments other than the lying moments are calculated conditional on fixed effects for surveyor, day-town, and hour-of-day. We run OLS regressions with the relevant dependent variable (such as answering the door or completing the survey), treatment indicators for each of the relevant treatments, interacted with voters and non-voters indicators, as well as the demeaned fixed effects indicated above. (That is, we assume that the fixed effects have the same impact on voters and non-voters). We estimate these models jointly on the entire sample of voters and non-voters. For the probability of lying moments, no fixed effects are used, given the smaller sample of survey respondents.

Under standard conditions, the minimum-distance estimator using weighting matrix  $W$  achieves asymptotic normality, with estimated variance  $(\hat{G}' W \hat{G})^{-1} (\hat{G}' W \hat{\Lambda} W \hat{G}) (\hat{G}' W \hat{G})^{-1} / N$ , where  $\hat{G} \equiv N^{-1} \sum_{i=1}^N \nabla_{\xi} m_i(\hat{\xi})$  and  $\hat{\Lambda} \equiv Var[m(\hat{\xi})]$  (Wooldridge, 2002). We calculate  $\nabla_{\xi} m(\hat{\xi})$  numerically in Matlab using an adaptive finite difference algorithm.

To calculate the minimum distance estimate, we employ a common sequential quadratic

programming algorithm (Powell, 1983) implemented in Matlab as the `fmincon` routine. We impose the following constraints:  $\mu_j^i \in [-999, 999]$  for  $i \in \{v, nv\}$  and  $j \in \{V, N\}$  (finite social-image utilities),  $\sigma_{SI}^i > 0$  (positive standard deviation of social-image utilities),  $L \geq 0$  (non-negative lying costs),  $S^s \geq 0$  (social pressure non-negative),  $\sigma^s > 0$  (positive standard deviation of altruism),  $h_0, r \in [0, 1]$  (probabilities between zero and one),  $\eta \in [0, 99]$  (finite elasticity of home presence) and  $v_s \in [0, 999]$  (finite and non-negative value of time). We begin each run of the optimization routine by randomly choosing a starting point, drawn from a uniform distribution over the permitted parameter space. The algorithm determines successive search directions by solving a quadratic programming sub-problem based on an approximation of the Lagrangian of the optimization problem. To avoid selecting local minima, we choose the run with the minimum squared distance of 120 runs. For the results presented here, the best estimate is achieved in about 15-40 percent of all runs, depending on the specification.

In the benchmark specification, the estimates for two closely related auxiliary parameters - the elasticity of home presence  $\eta$  and the social pressure cost  $S_s$  - are worth discussing in more detail. First, note that social pressure is largely identified by the fact that those who see the survey flyer and intend to say no to the surveyor optimally choose a home presence probability  $h^* = h_0 - \eta S_s$  (barring corner solutions). Taking into account that only a share  $r$  sees the flyer, the observed probability of home presence under a flyer, assuming that all respondents intend to turn down the survey and again ignoring corner solutions, is  $h_0 - r\eta S_s$ . Of this expression,  $h_0$  is precisely estimated from the share answering the door in the no flyer treatment, and  $r$  from the share checking the opt out box. Thus, the amount of sorting out between the no flyer and survey flyer treatments helps identify  $\eta S_s$ . To disentangle these terms, the elasticity  $\eta$  is identified from the sorting in and out of the home as a function of the survey payment and duration. Interestingly, the benchmark estimate for  $\eta S$  (Table 3) is similar for voters and non-voters, but the estimates of the individual parameters are very different. For voters,  $\eta^v$  is reasonably small (0.13, se = 0.09), and the social pressure cost sizeable, if smaller than in DellaVigna, List, and Malmendier (2012):  $S_s^v$  (\$1.76, se = 1.20). For non-voters, however,  $\eta^{nv}$  is extremely large and very imprecisely estimated, with then a tiny social pressure estimate. For such large value of  $\eta$ , non-voters are in practice always at corner solutions, and the exact value of  $\eta$  is not pinned down, given that any large  $\eta$  gives essentially the same solution. Notice that when we impose a fixed, small value of  $\eta$  (Column 6 in Appendix Table 2), the fit is substantially worse, but ultimately the estimated value of voting is about the same. The parameters  $\eta$  and  $S_s$  do not play a critical role for the estimate of the value of voting.

## D Appendix D - Estimation of Lying Cost in Laboratory Experiment

Erat and Gneezy (2012) study lying behavior by conducting a sender-receiver game in the lab with 517 subjects. The game provides incentives for the “sender” to lie to the “receiver”, for either altruistic or selfish motives. First, the sender is informed about the true outcome from rolling a six-sided die. She is then asked to send a cheap-talk signal of the outcome to the receiver. Next, the receiver chooses one of the six possible outcomes and, if this choice matches the state, payoff bundle A is implemented; otherwise, payoff bundle B is implemented. Importantly, the sender knows the payoffs A and B, while the receiver does not. The payoffs are varied to examine how lying by the sender depends on whether the lie is likely to help the receiver at a cost to the sender (an altruistic lie), help both the sender and receiver (a pareto lie), or help the sender at the cost of the receiver (a selfish or spiteful lie).

The payoffs for lying and truth-telling in each of five decisions are listed in Online Appendix Table 4, with the sender’s payoff listed first. Thus, in Decision 1, lying results in a payoff of (19,30) - \$19 to the sender and \$30 to the receiver (assuming that the receiver chooses the signaled number). We assume a model of simple altruism with lying costs and model the sender

as maximizing the utility function:

$$\max_{\{A,B\}} U = \{s_A + \alpha r_A, s_B + \alpha r_B - L + \varepsilon\}$$

where  $s_i$  is the sender’s monetary payoff in outcome  $i \in \{A, B\}$ ,  $r_i$  is the receiver’s payoff,  $\alpha$  is the sender’s altruism towards the receiver,  $L$  is the psychological cost of lying and  $\varepsilon$  is a mean-zero utility shock to payoff bundle B (or equivalently, to payoff bundle A). To estimate the model, we impose the following assumptions: Lying cost  $L$  and altruism  $\alpha$  are both assumed to be identical across individuals. The utility shock  $\varepsilon$  is distributed normally with mean zero and standard deviation  $\sigma_\varepsilon$ . We also assume that the receiver always follows the sender signal.

We estimate the model using a classical minimum distance estimator, with the shares lying in each decision as the five moments. The moments are weighted by the inverse of the variance of each moment. The intuition for the identification is straightforward. Conditional on altruism, the response of lying rates to the sender and receiver’s monetary payoffs from lying identifies the lying cost as well as the variance of the error term.

The results suggests a substantial cost of lying,  $L = \$7.0$  (se \$1.4). The estimated lying cost is consistent with the reduced form observation that a third to a half of subjects choose not to lie even when the private gain from doing is \$10 (Decisions 3 and 5). The estimated altruism is  $\alpha = 0.29$  (se 0.17) – senders value a dollar to the receiver as much as 29 cents to themselves. Finally, the standard deviation of the error term is  $\sigma_\varepsilon = \$18.6$  (se \$4.0). This heterogeneity is consistent with the fact that increasing the private incentive to lie from \$1 to \$10 increases lying by only 16 percentage points (Decision 2 vs. 3), suggesting a relatively low local density. At these estimated parameter values the fit of the moments is good, as Online Appendix Table 4 shows.

Extrapolated to the setting of our field experiment, this mean lying cost would imply a substantial social-image motivation for voting. In the benchmark specification, a lying cost of \$7 implies a social-image value of voting in congressional elections of \$16.9 for voters and \$18.8 for non-voters. Of course, we must be cautious in translating the lying cost estimated in this experiment to that in our survey experiments. One difference is that in our setting, the surveyor does not actually know if the respondent is lying (since our surveyors were blinded to the true voting status of the respondents and since the respondents likely are unaware that we know their voting status). In Erat and Gneezy (2012), in contrast, the sender knows that her lying or truth-telling is observed by the experimenter. In addition, the sample in Erat and Gneezy (2012) consists of undergraduate students, while our sample consists of adult voters and non-voters in Chicago suburbs.