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 signaling utility plays a significant role in explaining voting behavior: people vote *because others might ask*. We construct a model in which individuals may derive pride from signaling to others that they voted and, conversely, feel shame or guilt from admitting that they did not vote. We design a field experiment that is tightly linked to this model and allows us to estimate the key parameters of the theory. In three inter-related treatments, we study the cost voters and non-voters of the 2010 congressional election are willing to incur to sort into and out of situations in which they might be asked whether they voted. We find that signaling utility significantly affects the sorting of non-voters. For a broad range of plausible values of lying cost, we estimate a value of voting 'just because others will ask' of $10-$15. This value is sizeable enough to explain election 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 (Down (1957), Ledyard (1984), and Palfrey and Rosenthal (1983, 1985)). This motivation can explain voting behavior in small elections (although see Coate et al. (2008)), but not in large-scale elections. The second class of explanations is *norm-based voting*: Individuals vote because they believe it is the right thing to do – even if, as individuals, they may not affect the outcome of the election (Harsanyi (1977, 1992), Knack (1992), and Blais (2000)). This explanation has proven difficult to estimate empirically.

In this paper, we introduce a model of voting that builds on norm-based explanations, but can be estimated empirically. We posit that one reason why individuals vote is *because others will ask*. If individuals care about what others think of them, they may derive pride from telling others that they voted, and conversely shame or guilt from admitting that they did not vote. In other words, individuals like to signal to others that they are public good contributors, or are good citizens more generally.[1]

The second key component of the model is that individuals incur disutility from lying about voting. This assumption is consistent with the recent laboratory evidence on honesty, which finds that individuals tell the truth more often than would be predicted in a model with no disutility from lying (Gneezy (2005), Duffy et al.(2006), Sánchez-Pagés and Vorsatz (2007), and Erat el al.(2012)). If non-voters dislike lying strongly enough, they will admit that they did not vote.

An individual with signaling utility and lying costs is motivated to vote because she anticipates that others will ask if she voted. If she votes she can advertise her behavior of a 'good citizen' whenever asked. If she does not vote, instead, she faces the choice of being truthful and incurring shame, or stating untruthfully that she voted and incurring a lying cost.

Our model of voting is reduced-form and does not capture the myriad of other motivations to vote. Yet, it has the advantage that, unlike many models of voting, the model parameters can be estimated directly using field data. We design a natural field experiment (Harrison and List, 2004) that is tightly linked to the theory with the explicit purpose of estimating the key parameters of the model. In doing so, we are able to estimate the value of voting that is due to signaling utility. This type of field experiment with parameter estimation (Card, DellaVigna, and Malmendier, 2011) is uncommon in the literature, and this paper therefore also serves a methodological purpose: We show the insights gained from placing greater emphasis on parameter estimation in an environment where experiment and theory are tightly linked.

The field experiment has three key groups of treatments. The first set of treatments analyzes the sorting of voters and non-voters into and out of environments where they may be asked

[1] See the related work of Harbaugh (1996) and Gibson et.al (2013), discussed more fully below.

about their voting behavior. In the Spring, Summer, and Fall of 2011, we went door-to-door and asked households whether they were willing to answer survey questions, including whether they voted in the 2010 congressional election. Unbeknownst to the households, we knew whether the members of the households had voted. Importantly, we randomized the information which the household receives in advance of the door-to-door visit. In one group, we notified households with a flyer on the door-knob that the next day we would ask for their participation in a door-to-door survey. In a second group, we also informed the household of the upcoming survey, but we further specified in the flyer that the survey will be about "your voter participation in the 2010 congressional election."

We leverage the between-treatment differences in the share of households answering the door and completing the survey to derive the signaling value. Specifically, for voting households, we interpret an increase in survey response as the signaling value from the pride of saying that one voted. If this pride is large, we expect households to be more likely to answer the door and complete the survey when they know the survey is about their voter participation. For non-voting households, we interpret the difference in sorting between the two treatments as the effect of the shame from not voting and of lying costs.

Our first result is that we find no evidence of sorting in by voters, but conversely we find significant sorting out by non-voters. Namely, voting households are about equally likely to answer the door and do the survey when they are informed about the purpose of the survey, while non-voting households reduce participation by about 20 percent. This provides prima facie evidence that signaling utility matters for non-voters, but we cannot estimate these values without extra information on the cost of sorting in and out of answering the survey.

To better interpret this evidence, we introduce a second set of treatments. In a crossed randomization, we measure the cost of sorting with value-of-time and monetary manipulations. In particular, we randomize the promised payment for the survey ($10 versus $0) and the promised duration of the survey (5 minutes versus 10 minutes). By comparing the response to such incentives to the response to the flyer content we estimate a monetary value for some of the signaling parameters. Importantly, we find that the observed sorting out of non-voters in response to the election flyer is comparable to the effect of reducing payment by $10, implying significant utility from signaling.

These treatments provide some evidence on the signal value of voting, but they do not provide the necessary counterfactuals. For example, for voters we do not observe how they would have sorted *had they not voted*. Hence, we are not able to estimate their net signaling utility, which is necessary to determine the value of voting. Conversely, for non-voters we do not observe their behavior had they voted.

In order to estimate some of these unobserved parameters, we introduce a third set of (crossed) treatments. Within the door-to-door respondents who agreed to complete the survey, we introduce randomized incentives to induce them to state that they did not vote. Namely, we inform half of the respondents to the 10 minute survey that the upcoming survey will be 8 minutes shorter if they state that they did not vote in the 2010 congressional election. We conduct a parallel treatment for the respondents of a 5-minute survey and provide a treatment group a $5 incentive to similarly state that they did not vote.

This treatment permits us to obtain the counterfactual of how voters would feel were they to say (untruthfully) that they did not vote. Conversely, for the non-voters who lie about voting (about 50% in the control group), we obtain how they would feel if they told the truth.

The results of these lying manipulations indicate that non-voters are significantly more sensitive to the incentive than voters. While an 8-minute discount lowers the share of non-voters who lie by 10 percentage points, it only raises the share of voters who say that they did not vote by 2 percentage points.

By combining these three sets of treatments, we are able to estimate several key parameters of the model, namely the average signaling utility parameters of saying one voted and one did not vote, and the heterogeneity in such parameters. One parameter which remains unidentified is the lying cost. However, we can compute the value of the other parameters given an assigned cost of lying.

For a broad range of plausible values of the lying cost, The implied value of voting because of being asked *once* is in the range of $2 to $3. To compute the overall value of voting due to being asked, we incorporate the information from survey respondents on how many times they were asked about voting in the off-year congressional election, which is on average 5 times. Hence, the implied value of voting for the congressional elections of 2010 just because others will ask is in the range of $10-$15, a sizeable magnitude. Furthermore, while we do not observe the signalling value estimates for presidential elections, if we are assume that they are at least as large, we can compute a lower bound. Given that our respondents report being asked an average of 9 times whether they voted in the 2008 presidential elections, the implied value of voting because asked is in the range of $18-$27. Hence, the observed higher turnout in presidential election can in part be due to the fact that potential voters expect to be asked more frequently.

This paper complements a rapidly-growing literature on get-out-the-vote experiments aimed at increasing turnout (e.g., Green, Gerber, and Nickerson (2003) and Gerber, Green, and Larimer (2008)). The field experimental evidence in this paper is not directly aimed at affecting turnout, but rather aims to understand the turnout decision based on self-reports of voting, and on the sorting decision on whether to answer a survey about voting. But, of course, with an understanding of our model's parameters one could imagine get-out-the-vote campaigns that would be successful. This paper also complements a small but growing literature which emphasizes the role of models in field experiments (DellaVigna, List, and Malmendier, 2012) as well as to the literature on structural behavioral economics (Laibson, Repetto, and Tobacman (2007) and Conlin, O'Donoghue, and Vogelsang (2007). We envision that a combination of these two literatures—where this paper resides—represents a strong growth area for future field experiments across the social sciences.

The remainder of our paper is structured as follows. The next section presents our theoretical model. Section 3 summarizes our experimental design. Section 4 discusses the empirical results. Section 5 concludes.

2 Model

We present a simple model of voting in the spirit of Harbaugh (2007). We assume that voting depends on four factors: pivotality, warm glow, cost of voting, and expected signalling. Of the four factors, this paper focuses on the fourth component: individuals expect that they will be asked whether they voted and they internalize the future signalling utility and lying cost when deciding whether to vote at present. In order to identify this determinant of voting, we examine how individuals respond to a (possibly anticipated) visit by a surveyor who will ask the respondent whether he or she voted.

Voting. We assume that individuals vote if the net expected utility of doing so is positive:

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

The first three terms in expression (1) capture in a nutshell the standard models of voting. The first term represents the utility of being pivotal (Riker and Ordeshook 1968; Harsanyi 1977; Feddersen and Sandroni 2006): p is the subjective probability of the voter being pivotal, and V is the value that the voter assigns 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 and voting. Since the experimental design of the paper is not focused on these components, only their sum will matter, which we denote $\varepsilon = pV + g - c$.

The crux of the model is instead the fourth term, which represents the social-signalling reason to vote. A potential voter expects to be asked N times whether he or she has voted. In each of these N occurrences, the individual decides whether to be truthful or to lie. Assume first that the individual has indeed voted. In this case, the individual can truthfully signal that she voted and obtain signalling utility s_V from looking like a voter. Alternatively, the individual may lie and look like a non-voter, in which case the signalling utility s_N occurs, and in addition the individual pays a psychological lying cost L. The utility of being asked about voting for a voter, therefore, is $\max\left(s_V, s_N - L\right)$.

Conversely, assume that the individual did not vote. In this case, the individual can either state the truth and obtain the signalling utility from being a non-voter, s_N, or lie and signal voting, but with a lying cost, $s_V - L$. Hence, the utility of being asked about voting for a non-voter is $\max\left(s_N, s_V - L\right)$. The term in square brackets in (1) is therefore the net utility gain from having voted, due purely to the fact of being asked once.

The social signalling model of voting has two key components, the signalling utility and the lying cost. The signalling utility terms s_V and s_N capture how much individuals care about being seen as public good contributors, or not, by others. These terms can be seen as reduced form representations for a full signalling model, such as for example Benabou and Tirole (2006). In the most general case, we do not pose any restrictions on s_V and s_N, but we do consider some special cases. The first case is *Pride in Voting* ($s_V > 0$ and $s_N = 0$): individuals care (positively) about signalling that they are voters, and experience no utility from signalling that they are non-voters. A second polar case is *Stigma from Not Voting* ($s_V = 0$ and $s_N < 0$): individuals earn no utility from saying they voted, but dislike signalling that they are non-voters. A third special case is *Pride and Stigma* ($s_V > 0$ and $s_N < 0$) which combines the two cases above. More generally, one can imagine that the net signalling utility is positive, that is, $s_v \geq s_N$.

The second component is an additive lying cost L which individuals pay when not telling the truth. So, for example, when a non-voter claims to have voted, she still obtains the signalling utility s_V from looking like a voter to the asker, but at the same time pays a disutility cost L from knowing that she lied. We assume that the cost of lying is non-negative, $L \geq 0$, and additive with respect to the signalling cost. The assumption of positive lying costs is motivated by a large experimental evidence documenting that even in cheap talk games, which are the equivalent of the survey questioning, a sizeable portion of subjects tell the truth, even in cases in which the theory predicts a babbling equilibrium.

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

$$\Phi\left(s_v - s_L, L\right) = \begin{array}{cl} L & \text{if } s_V - s_N \geq -L \\ s_V - s_N & \text{if } -L \leq s_V - s_N < L \\ -L & \text{if } s_V - s_N < -L \end{array} \quad (2)$$

Figure 1 displays $\Phi\left(s_v - s_L, L\right)$ as a function of $s_V - s_L$ for a given L and makes is clear that in order for social signalling to contribute to voting two conditions much be met. First, the net signalling utility $s_V - s_N$ must be non-zero. Second, the lying cost must be positive. If either of these conditions is not met, the individual either does not care about signalling, or can always signal the best-case scenario, irrespective of the true action. 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 to vote contributes positively to voting. Equation (2) has a second implication. To estimate the social signalling theory of voting, we need to estimate two terms (i) the signalling value of saying that one voted versus not, $s_V - s_N$, and (ii) the cost of lying L.

Door-to-Door Survey. To estimate these terms, we designed a door-to-door survey in which individuals are asked whether they voted. We model the behavior of an individual whose home is visited by a surveyor. We distinguish between the case of an unannounced visit and a pre-announced visit. In the latter case, since the flyer announces the time of the future visit, the individual can alter her probability of being at home and opening the door. The flyer itself is of one of two types. A "survey flyer" (denoted by F) informs the reader when the surveyor will visit, as well as the length of the survey and the remuneration (if any), while leaving 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. The technical details, including Lemmas are in the Appendix, and the proofs are in the Online Appendix.

Formally, in the first stage the household may receive a flyer regarding the upcoming visit and, if so, notices the flyer with probability $r \in (0, 1]$. In the second stage, 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 do not require symmetry around h_0, and we allow for corner solutions at $h = 0$ or $h = 1$.

If the individual is at home at the time of the surveyor's visit, she must decide whether to complete the survey. We assume that 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 care to contribute to public goods, or they may dislike surveys. We assume that the willingness to complete a survey s is distributed normally, with $s \sim F_s$. In addition, individuals get utility from receiving a payment m for doing the survey and disutility from the time cost c of the survey, both of which are deterministic for simplicity. The final element in the utility function is social pressure. As in DellaVigna, List and Malmendier (2012), the respondent pays a utility cost $S_s \geq 0$ for refusing to do the survey while the surveyor is present. 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. We denote voters by superscript v and nonvoters by nv. Then, let z^i be the value that an individual of type i places on being asked if she voted, where $i \in \{v, nv\}$. Given the set-up above, for voters $z^v = \max(s_V^v, s_N^v - L^v)$, while for nonvoters $z^{nv} = \max(s_N^{nv}, s_V^{nv} - L^{nv})$. (Notice that so long as the net signalling utility of voting $s_V - s_N$ is positive, voters never lie and hence $z^v = s_V$). We assume that (s_V^i, s_N^i) follows a joint normal distribution G^i, which differs for voters and non-voters. We assume that lying costs are deterministic, but potentially different across voters (L^v) and nonvoters (L^{nv}).

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—a shortening of the survey duration—or money—an extra \$5 for 1 more minute of questions. We denote by \$ the value of the incentive, translated into money. By incentivizing the respondent to say she did not vote, a voter is provided an incentive to lie, and will state that she voted if $s^v_V \geq s^v_N - L^v + \$$. In contrast, a nonvoter is provided an incentive to tell the truth , and will now say that she voted if $s^{nv}_V - L^{nv} \geq s^{nv}_N + \$$. By varying the extent of the incentive \$, we can estimate the value (or distribution) of $s_V - s_N + L$ for voters and the distribution of $s_V - s_N - L$ for nonvoters. Note that this treatment is unanticipated, and hence cannot figure in the respondent's decision to answer the door or participate in the survey.

Equilibrium. We now characterize the decisions of the agent. Conditional on answering the door, the respondent of type $i \in \{v, n\}$ agrees to complete the survey if $s^i+m-c^i+z^i \geq -S^i$. Note that once the respondent opens the door, she is informed that the survey is about voting, so correctly takes z^i into account while deciding whether to participate. But she does not account for the possible additional incentives to lie, which are unanticipated.

Working backward, consider then a respondent of type i who sees a survey flyer warning of the forthcoming visit, but not about the voter participation topic. The decision problem of staying at home (conditional on seeing a flyer) is $\max_{h\in[0,1]} h \max\left(s^i + m - c^i, -S^i\right) - \left(h - h_0\right)^2/2\eta^i$, leading to the solution $h^{i*} = \max\left[\min\left[h_0 + \eta^i \max\left(s^i + m - c^i, -S^i\right), 1\right], 0\right]$. An increase in the pay m or a decrease in the cost of time c will increase the probability of being at home and the probability of filling a survey; also, the parameter η^i determines the elasticity with respect to incentives of home presence.

Alternatively, consider respondents who see an election flyer, which also informs them that the survey will be about voter participation. Now the solution to the maximization problem will be $h^{i*} = \max\left[\min\left[h_0 + \eta^i \max\left(s^i + m - c^i + z^i, -S^i\right), 1\right], 0\right]$. If $z^i > 0$, the respondent will choose a weakly higher probability of being at home in the election flyer treatment, compared to the survey flyer.

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, we assume that agents can costlessly reduce the probability of being at home to zero. Formally, $c(0) = 0$ and $c(h)$ is as above for $h > 0$.[2] 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 derives 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.

Testable Predictions. The following Propositions outline testable predictions regarding the key outcomes, home presence and survey completion, for voters and nonvoters. Our first prediction compares the probabilities of being at home in the treatments with the survey flyer,

[2]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.

$P(H)_F^i$ and with the election flyer, $P(H)_{FE}^i$.

Proposition 1. *With Pride in Voting, the probability $P(H)$ is higher under election flyer than under the survey flyer (strictly for voters and weakly for nonvoters): $P(H)_{FE}^v > P(H)_F^v$ and $P(H)_{FE}^{nv} \geq P(H)_F^{nv}$. With Stigma from Not Voting, the probability $P(H)$ is lower under election flyer than under the survey flyer (strictly for nonvoters and weakly for voters): $P(H)_{FE}^v \leq P(H)_F^v$ and $P(H)_{FE}^{nv} < P(H)_F^{nv}$*

The next Proposition compares the impact of the different treatments on the unconditional probability of completing the survey, $P(SV)$.

Proposition 2. (Parallel to Proposition 1) *With Pride in Voting, the probability $P(SV)$ is higher under election flyer than under the survey flyer (strictly for voters and weakly for nonvoters): $P(SV)_{FE}^v > P(SV)_F^v$ and $P(SV)_{FE}^{nv} \geq P(SV)_F^{nv}$. With Stigma from Not Voting, the probability $P(SV)$ is lower under election flyer than under the survey flyer (strictly for nonvoters and weakly for voters): $P(SV)_{FE}^v \leq P(SV)_F^v$ and $P(SV)_{FE}^{nv} < P(SV)_F^{nv}$.*

3 Experimental Design

Logistics. 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. 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. Details about the recruitment process are in the Online Appendix.

The distribution of flyers took place on Fridays and Saturdays, and the field experiment took place on Saturdays and Sundays, all between July 2011 and November 2011. The locations are wealthy towns around Chicago [3]. 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.

Sample and Randomization. We begin with a sample of registered voters living in homes in towns surrounding Chicago and reduce it to households with homogeneous voting records - either everyone voted or everyone did not. Next, we randomize the remaining

[3]Arlington Heights, Elk Grove Village, Evanston, Glenview, Hoffman Estates, Lincolnwood, Mount Prospect, Northbrook, Oak Park, Park Ridge, Schaumburg, Skokie, Streamwood, Wilmette, and Winnetka.

households at the surveyor-route level within a particular town. The dimensions of cross-randomization pin down the specific flyer, surveyor script (Online Appendix), and survey that will be used to treat a surveyor route. We reached a total of 14,857 households in the survey treatments. From this initial sample, we exclude 1,292 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). We also exclude 352 observations with substantial inconsistencies in the recorded data.[4] The final sample includes 13,213 households in the survey treatments.

Flyer Treatments. Each household was randomized into five different flyer treatments - Baseline, Flyer: Survey, Flyer: Elections, Flyer with Opt-Out: Survey, and Flyer with Opt-Out: Elections. The Baseline treatment is the flyer control group - they receive no flyer to warn of the visit of a surveyor the subsequent day. The Flyer: Survey treatment involves placing a flyer on the door knob of an assigned house that would warn that household that a surveyor would approach the home the next day within a pre-specified hour (e.g., 3PM - 4PM) to conduct a survey. Flyer: Elections is identical to Flyer: Survey, only with the added information that the survey will be about voter participation. Flyer with Opt-Out: Survey takes the Flyer: Survey and adds a check-box to the bottom where a household can mark if it does not wish to be disturbed by the surveyor the next day. Similarly, Flyer with Opt-Out: Elections adds an opt-out check box to the Flyer: Elections flyer. All of the flyers used were professionally produced, and Figure 2 depicts an example of each flyer type. Note that all of the flyers list the length of the survey as well as the compensation offered (if any) for completing the survey, both of which were determined as part of the survey treatment randomization and cross-randomized with the five flyer treatments. Figure 3a depicts the dimensions of cross-randomization across the flyer and survey treatments. Each flyer-type was sampled with equal probability.

Survey Treatments. As in DellaVigna, List, and Malmendier 2012, surveyors approach pre-assigned houses in a town. The surveyor first knocks on the door of an assigned house. If no one answers, they move on to the next house. Otherwise, if someone opens the door, the surveyor goes into the surveyor script. As part of the script, the surveyor states that they are conducting confidential X minute surveys for \$Y, where X$\in$[5, 10] and Y$\in$[0,10] and both were randomized in advance. At this point, the surveyor asks if the individual is interested in participating in the described survey, and if the answer is yes, the surveyor conducts the survey. pays the individual appropriately, and leaves. The content of the survey was largely questions about past voting, how many times different types of people (friends, coworkers, relatives, etc.) had asked the individual about recent voter participation, hypothetical donations to charity,

[4]These observations indicate a number of different issues such as the presence of flyers on the door or on the floor for households in the no-flyer treatment, accidental administering of the wrong survey, etc.

as well as questions about income and house price. All of the following survey types were sampled with equal probability: (5-Minutes, No Payment), (5-Minutes, $10 Payment), and (10-Minutes, $10 Payment). These survey treatments are designed to estimate the elasticity of presence at home, response to incentives for being at home, as well the mean number of times an individual is asked about their voter participation. We use these estimates to inform the signaling utility model on value-of-time and the cost of sorting in or out (through observed door-answering behavior) when warned with a flyer.

Lying Treatments. Within the first two questions of the survey, the last randomized component is administered. In control surveys, individuals self-report on their voter participation, answering whether or not they voted in the 2010 congressional election. In the treatment surveys, we exogenously offer incentives for stating that they did not vote in the relevant election. The incentives take the form of time-off of the survey (8 minutes off) or direct financial incentive ($5). We operationalize these treatments as follows: In the lying treatment surveys, the first question on the survey asks about the year in which the individual purchased their home (specifically, whether before or after the year 2000). After receiving the answer to this question, the surveyor delivers a survey-treatment-specific script that describes the incentive. For example, for a 10-minute lying treatment survey the surveyor would read 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.

Alternatively, in the financial incentive lying treatments, the surveyor would read the following before the second question (with the material in brackets applying only to the survey treatments with a $10 base payment):

> 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.

In the case of the control surveys, the question about voter participation in the 2010 congressional election was the first question on the survey, and hence answers were purely self-report without additional incentive to answer in a particular way. The following lying treatments were each sampled with equal likelihood:(5-Minutes, No Incentive), (5-Minutes, \$5 Extra), (10-Minutes, No Incentive), and(10-Minutes, 8 Minutes Off). To summarize, we present the lying treatments in Figure 3b. These estimates, when used in conjunction with the survey treatment estimates, allow for the estimation of the average value of signaling one is a voter or a non-voter.

4 Reduced-Form Estimates

Answering the door and survey completion. The benchmark empirical specifications (Table 1) control for surveyor i, day-town t, and hour-of-day h fixed effects.[5] The identification thus comes from within-surveyor, within-day-town, within-hour variation in treatment. We estimate, separately for voters and non-voters, the OLS regression

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

where the dependent variable $y_{i,j,t,h}$ is, alternatively, an indicator for whether individual j opened the door (y^H) and 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. As such, the point estimates for Γ are to be interpreted as the effect of a treatment compared to the Baseline.[6] We cluster the standard errors at the surveyor$\times$date level.

Figure 4 plots the share of households answering the door and completing the survey as a function of the attractiveness of the survey. The estimates in the figure are pooled across the five flyer treatments and control for the fixed effects. The estimates indicate that voters are very responsive to incentives, going from a 33% share answering the door for a \$0-5minute survey to a 39% share of households answering the door 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 (6 percentage points, 45 percent). 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). Voters are about twice as likely as non-voters to complete the unpaid \$0, 5-minute survey, despite answering the door at a similar rate, consistent with higher altruism, or higher social pressure, for voters.

Figure 5a plots the average share answering the door and completing the survey across the five different flyer treatments for voters. These estimates are pooled across the different survey

[5]On almost all days, we visited one or two towns on a given day.

[6]The specification assumes that the impact of the fixed effects on the relevant outcomes is additive. We obtain essentially identical results using solicitor-time-date fixed effects.

time and payment incentives and again control for fixed effects. The key result is that we do not observe much difference for voters between the share answering the door in the Flyer and the Flyer Election treatments, and similarly we see only a small difference in the share completing a survey. Prima facie, this suggests that the average utility from pride from voting is limited, even amongst voters. Indeed, if anything the results suggest that even voters may prefer not to be asked about whether they voted.

Figure 5b plots the same estimates for non-voters. The result here is strikingly different. For non-voters, there is a 6 percentage point drop in the probability of answering the door between the Flyer and the Flyer Election treatments. This is strong evidence of avoidance - non-voters shy away from the surveyor visit when informed that they will be asked whether they voted. Interestingly, the size of this effect is about the size of a $10 incentive to complete the survey, indicating a sizeable demand for avoidance. There is a similar effect on the share completing a survey. The impacts are smaller for the opt-out treatment. These results are largely consistent with non-voters feeling shame from admitting to not voting, but also earning negative net utility from lying and claiming to be voters.

Table 1 presents the regression analysis underlying Figures 4 and 5, and shows that the estimates are robust to the exclusion of all fixed effects.

Lying about voting. We next estimate the rates at which voters and non-voters lie about their voting behavior, and the effect of the randomized incentives to lie more (for voters) or to lie less (for non-voters). In particular, we estimate OLS regressions of the form

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

where $y_{i,j} = 1$ if individual j lied about her voting behavior to surveyor i, and 0 otherwise. We restrict the sample to individuals who completed the survey, and thus the outcome is interpreted as lying conditional on answering the survey. $T_{i,j}$ is a dummy variable with value 1 if the respondent is provided an incentive to say she did not vote, and 0 otherwise. The regressions were first run pooled across all survey treatments, and then separately for each survey length and payment combination, since the size of the incentive differed across survey treatments. Due to the smaller sample, only surveyor fixed effects η_i were included in regressions.

In Figure 6 and Table 2, we present the results from these estimations. Recall that the incentive was always to say that one did not vote - and thus we expect voters to lie more and non-voters to lie less than they would in the control condition. Pooling across all survey treatments, Table 2 reveals that the incentive has 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 11 percent points 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.

Breaking this result down by lying incentive, respondents in the treatment group had an 8-minute or 5-dollar incentive to state that they did not vote. As Figure 6a shows, the effect of such incentives is very small for voters: only 1 percent (5 percent) of voters appear to change their response in response to the $5 incentive (8 minute incentive). For non-voters, instead, the effect of the incentive is substantial: a full 10 percent of respondents switch to admitting that they did not vote when incentivized to do so, similarly across the 8 minute and $5 incentives.

5 Structural Estimates

The reduced-form estimates provide qualitative evidence of the existence of a signalling value of voting. The observed patterns in answering the door and completing the survey are consistent with shame from not voting amongst non-voters, but little or no pride from voting for voters. In addition, the observed lying rates suggest positive costs of lying for non-voters, and either high lying costs or high shame from not voting for voters. However, this reduced form analysis does not allow for a quantitative estimate of the underlying signalling utility values, and thus does not allow us to estimate counterfactuals or attach a magnitude to the total social signalling motivation to vote. To do so, we therefore estimate the model parameters structurally, jointly utilizing the complete experimental design described above.

Set-up. We estimate the model of Section 2, imposing several additional assumptions, some of which are relaxed below. First, consider the parameters that determine the signalling value of voting. For voters, we assume that the utilities of truthfully saying one voted (s_V^v) or of lying and claiming one did not vote ($s_N^v - L^v$) are independently normally distributed across individuals, with differing means μ_V^v and μ_N^v but the same standard deviation, $\sigma_V^v = \sigma_N^v$. For non-voters, we analogously assume that s_N^{nv} and $s_V^{nv} - L^{nv}$ are each independently normally distributed with differing means μ_N^{nv} and μ_V^{nv} but the same standard deviation, $\sigma_N^{nv} = \sigma_V^{nv}$. Next, consider the ancillary survey parameters, which are all allowed to differ by voter and non-voter. We assume that the cost of leaving home $c(h)$ is symmetric around h_0 and quadratic: $c(h) = (h - h_0)^2 / 2\eta$. Additionally, we assume a baseline utility s of completing a 10-minute survey for no monetary payment, with $s \sim F_s$, a normal distribution with parameters μ_s and σ_s. Hence, we allow s to be negative for households that dislike doing surveys without compensation, or positive for households that like providing public goods. In addition, individuals receive utility from a payment m for completing the survey, and receive disutility from the time cost t of the survey, both of which are deterministic. The time cost t equals τv_s, where τ is the duration of the survey in fraction of hours, and v_s is the value of one hour of time. We denote by S_s the social pressure cost of saying no to a survey request.

The vector of parameters ξ that we estimate are: (i) μ_V^i, μ_N^i - the mean utility earned by an individual of type $i \in \{v, nv\}$ from saying she voted or did not vote, net of the unobserved cost of lying L^i. Note that we are not able to separately identify the cost of lying L^i itself; (ii)

σ_V^i, σ_N^i, the corresponding standard deviations of the normally distributed signalling utilities; (iii) h_0^i–the probabilities of opening the door in the no-flyer treatment for type i; (iv) r^i–the probability of observing (and remembering) the flyer; (v) η^i–the responsiveness of the probability of opening the door to the desirability of being at home; (vi) μ_s^i and σ_s - the mean and standard deviation(iv) v_i^s–the value of one hour of time; (v) S_s^i—the social pressure associated with saying no to the survey request; .

To estimate the model, we use a classical minimum-distance estimator. Denote by $m(\xi)$ the vector of moments predicted by the theory as a function of the parameters ξ, and by $\hat{m}$ the vector of observed moments. The minimum-distance estimator chooses the parameters $\hat{\xi}$ that minimize the distance $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$, where W is a weighting matrix. As a weighting matrix, 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.[7] As a robustness check, we also use the identity matrix as weight. To calculate the theoretical moments, we use a numerical integration algorithm based on adaptive Simpson quadrature, implemented in Matlab as the *quad* and *quad2d* routines.

As moments $m(\xi)$ we use the following probabilities (where $i \in \{v, nv\}$, $j \in \{NF, F, FE, OO, OOE\}$, k indexes survey treatments - \$0,5min; \$10,10min, \$10,5min; and l indexes incentives to lie - no incentives, 8 minute incentive to say you did not vote, \$5 incentive to say you did not vote): (i) the probability of type i opening the door in survey treatment k , $P(H)_k^i$; (ii) the unconditional probability of completing the survey in the various survey treatments, $P(SV)_k^i$; (iii) the probability of checking the opt-out box in the Opt-Out treatments, $P(OO)_k^i$ and (iv) the probability of type i lying about her voting behavior, given incentive l, and conditional on completing the survey in the first place, $P(L)_l^i$.[8] The corresponding 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.

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: $S^s \geq 0$ (social pressure non-negative), $\sigma^s > 0$ (positive standard deviation of altruism), $h_0^{2010}, h_0^{2011}, r \in [0,1]$ (probabilities between zero and one), and $\eta \in [0, 9999]$ (finite elasticity of home presence). 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

[7]Given the large number of moments, weighting the estimates by the inverse of the full variance-covariance matrix is problematic computationally.

[8]Note that we pool across the various survey treatments k to generate the lying rate moments, and thus the moment is only indexed by i and l.

squared distance of 50 runs.[9]

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.

Identification. While the parameters are estimated jointly, it is possible to address the main sources of identification of individual parameters. The mean signalling utilities μ_V^i and μ_N^i earned by individuals of type $i \in \{v, nv\}$, net of the cost of lying, are identified primarily by the sorting of respondents in the FE treatment relative to the F treatment.[10] Note that we are not able to estimate lying costs, L^i separately from the mean signalling utilities. Put differently, for voters we can estimate the mean values of s_V^v and $s_N^v - L^v$ and for non-voters we can identify the mean values of s_N^{nv} and $s_V^{nv} - L^{nv}$. The heterogeneity in this signalling utility, σ_V^i (assumed equal to σ_N^i), is identified mainly from the lying incentive treatments. For example, Column 3 of Table 2 shows that 53 percent of non-voters lie in the absence of incentives. But providing them with an 8 minute incentive to tell the truth reduces lying rates by 12 percentage points. Thus, $\Pr(s_V^{nv} - L > s_N^{nv}) = 0.53$ and $\Pr(s_V^{nv} - L > s_N^{nv} + (8/60)\, v_s) = 0.41$, or alternatively, $\Pr(0 < s_V^{nv} - L - s_N^{nv} < (8/60)\, v_s) = 0.12$, where $s_V^{nv} - L - s_N^{nv}$ is normally distributed with variance $(\sigma_V^{nv})^2 + (\sigma_N^{nv})^2$.

The baseline probabilities of answering the door, h_0^i are identified by the observed probabilities of type i opening the door in treatments without flyer. The probability of observing and remembering the flyer, r^i, is identified by two moments in the Opt-out treatment: the fraction of households of type i checking the opt-out box in the OO treatment (10 to 11 percent), which equals $r^i h_0^i F_s^i(c - m)$, and the fraction opening the door. The elasticity of opening the door η^i with respect to incentives is identified by the fraction opening the door in the survey treatments for different payments and survey durations.

The survey completion rates for varying amounts of compensation in the NF and F treatments identify the heterogeneity in the willingness to complete the survey, and hence σ_s. For example, the completion rate of a 5-minute survey for voters increases by 6 percentage point with a \$10 increase in pay (Table 1, Column 5). This indicates that $6/h_0 = 17$ percent of the population assigns negative value to doing a 5 minute survey for no payment, but assigns positive value to completing the same survey when receiving \$10. The survey completion rate also identifies the mean willingness to complete a 10-minute survey, μ_s. The value of time v_s is identified from the comparison between pay increases for the survey (from \$0 to \$10) and duration decreases (from 10 to 5 minutes). Finally, the social pressure S_s is identified by the

[9] For the results presented here, the best estimate is achieved in about 35 percent of all runs.

[10] The relative sizes of μ_V^i and μ_N^i are also informed by the lying rates. For example, the fact that approximately half of non-voters lie in the absence of additional incentives suggests, given the normality assumptions, that $\mu_V^{nv} = \mu_N^{nv}$.

share of people answering the door in the survey treatments. To see this, 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, η^i, and r^i, it is possible to identify S_s^i.

Estimates. Table 3 reports the benchmark estimates of the parameters along with standard errors. We first briefly discuss the ancillary survey parameters, and then discuss in some detail the main voter signalling parameters.

Ancillary parameters. The probability of being at home h_0 is precisely estimated to be 35.4 percent for non-voters and 38.1 percent for voters (unreported in Table 1). The share r of households that have read (and remember) the flyer is precisely estimated at 30.0 percent for non-voters and 37.7 percent for voters. While these estimates may appear low, many households may have just disregarded the flyer, or a household member may have seen it, but not informed the person opening the door. The elasticity of home presence η is imprecisely estimated to be 0.537 (s.e. 1.901) for non-voters and 0.162 (s.e. 0.141) for voters, implying that the cost of increasing the probability of being at home and answering the door by 10 percentage points is $0.1^2/2\eta^i = \$0.01$ for non-voters and \$0.03 for voters respectively.

The average utility for survey completion is estimated to be $-\$28.96$ for non-voters and $-\$23.92$ for voters, implying that, on average, households dislike completing 10-minute surveys for no pay. There is, however, significant heterogeneity ($\sigma_s^{nv} = \$24.70$ and $\sigma_s^v = \$27.02$), implying that a sizeable share of both voters and non-voters like doing surveys even for no pay. The value of time for one hour of survey completion is estimated to be \$23.39 (se \$9.54) for non-voters and \$69.89 (se \$15.73) for voters. Finally, the social pressure cost of turning down a survey request, S_s, is imprecisely estimated to be \$3.67 for non-voters and \$1.42 for voters.

Voter signalling parameters. The key voting signalling parameters are reported in the top panel of Table 3. For voters, we estimate an average utility (from a single interaction) of truthfully saying they voted, $\mu_V^v = -\$5.72$ (se \$2.06). That is, voters do not on average take pride in saying they voted, but rather dislike informing others about their voting behavior. However, voters dislike much more strongly lying and saying that they did not vote - we estimate that their average utility from doing so is $\mu_N^v = -\$24.83$ (se \$5.90). Thus, while voters do not on average enjoy saying that they voted, they much prefer it to lying and saying they did not - either because they they greatly dislike being thought of as non-voters, or because they greatly dislike lying, or some combination of the two. There is, however, substantial heterogeneity in these signalling values, with an estimated standard deviation $\sigma_V^v = \sigma_N^v = \$13.24$ (se 3.78). Thus, the estimates suggest that 33 percent of voters *do* take pride in saying they voted.

For non-voters, we estimate substantial stigma on average from admitting they did not vote: $\mu_N^{nv} = -\$7.16$ (se \$1.97). Indeed, on average, non-voters are nearly indifferent between admitting they did not vote and lying and claiming they voted ($\mu_V^{nv} = -\$7.82$, se \$2.29), consistent with the finding that about half of non-voters lie and claim to be voters in the control treatments. Heterogeneity across individuals is again substantial, with the signalling utilities having an estimated standard deviation $\sigma_N^{nv} = \sigma_V^{nv} = \$6.76$ (se \$1.56).

In Equation 1, the term $N\left[\max\left(s_V, s_N - L\right) - \max\left(s_N, s_V - L\right)\right]$ captures the total signalling motivation for voting - how much more an individual would attain in total signalling utility had she chosen to vote (in which case she earns $\max\left(s_V, s_N - L\right)$ from each interaction) than if she had chosen not to vote (in which case she earns $\max\left(s_N, s_V - L\right)$ from each interaction). Calculating this object requires two additional pieces of information. Firstly, we need to know N, the number of times an individual is asked about her voting behavior. We measure this using the survey question, "How often have you been asked about whether you voted?". Both voters and non-voters report being asked an average of around 5 times for a congressional election ($N^v = 4.89$ and $N^{nv} = 5.33$). Second, we meed to know L^i - the cost of lying for an individual of type $i \in \{v, nv\}$. Recall that we estimate the means of s_V^v and $s_N^v - L^v$ for voters, but do not separately estimate s_N^v and L^v. Thus, we do not know how voters would feel about saying they did not vote, *had they truly not voted*. Similarly, we do not estimate how a non-voter would feel about saying she voted, had she truly voted. This prevents us from point identifying the total signalling motivation.

However, we can calculate the implied signalling motivation given any assumed value of lying cost L. Table 1 and Figure 8 report the implied total signalling values for a range of plausible values of L. First, note that if lying is entirely costless ($L = \$0$), there can be no total signalling value from voting, since a non-voter could costlessly pretend to be a voter when asked. Indeed, Equation 2 shows that the per interaction signalling motivation of voting is bound above by L. As the underlying cost of lying increases from zero, the implied signalling motivation for non-voters rises monotonically, reaching \$8.96 for $L^{nv} = \$5$, \$31.58 for $L^{nv} = \$10$ and \$57.84 for $L^{nv} = \$15$. This happens for two reasons: first, an increase in L increases the upper bound of the signalling motivation, which is likely to bind for low L provided $s_V - s_N > -L$. Additionally, if half of all non-voters lie even when the lying cost is very high, they must deeply care to be thought of as voters, and thus their signalling value gain from truly voting is large (since they would be able to avoid the lying cost if they truly voted). For voters, on the other hand, the implied signalling motivation first rises and then falls with the underlying lying cost, reaching \$13.29 for $L^v = \$10$ \$17.49 for $L^v = \$10$ and falling to \$11.53 for $L^v = \$15$. The reasoning is parallel to the case with non-voters: for small values of L, increasing L increases the binding constraint that the signalling motivation cannot exceed L. But for larger values of L , the second effect dominates: holding constant the estimated mean of $s_N^v - L^v$,a larger underlying lying cost L^v implies a larger underlying mean utility from being seen as a non-voter, s_N^v, which reduces the gains from truly being a voter.

6 Conclusion

We have presented evidence for a field experimented designed to provides estimates for a model of turnout: individuals may vote because they expect to be asked, and they anticipate the disutility associated with admitting to non 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 $10-$15, a sizeable magnitude for a congressional election. We conjecture larger magnitudes for a presidential election.

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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