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Essays in Behavioral and Development Economics

by

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Professor Edward Miguel
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Abstract

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This dissertation is comprised of three essays in empirical economics. These essays are united by three clear intellectual and methodological themes. First, each essay attempts to bring theories and insights from psychology to bear on open questions in economics, with a focus on topics of importance to developing countries. Second, they use experiments - both randomized field experiments and natural experiments - to test economic theory. Finally, each paper attempts to measure economically important but difficult-to-observe behaviors and preferences - self-control problems in Chapter 1, social norms in Chapter 2, and discrimination and fairness preferences in Chapter 3.

In chapter 1, coauthors Liang Bai, Edward Miguel, Ben Handel and I construct a simple model of preventive health behavior under present biased time preferences, and show how beliefs about future time preferences (sophistication, partial naivete, and perfect naivete) affect how agents are predicted to use, under-use or misuse different types of commitment contracts. We propose a type of commitment contract that has the potential to benefit not just sophisticated present biased agents, but also naifs. We conduct a field experiment focused on increasing the share of patients who actively manage their hypertension by visiting a doctor periodically. The experiment is closely tied to the theory, allowing us to estimate the key parameters of the model.

In chapter 2, coauthors Stefano DellaVigna, John List, Ulrike Malmendier and I ask the question: 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 that 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.

In Chapter 3, I exploit a natural experiment in India to identify how mixing rich and poor students in schools affects social preferences and behaviors. A policy change in 2007 forced many private schools in Delhi to meet a quota of poor children in admissions. This led to a sharp increase in the presence of poor children in new cohorts in those schools, but not in older cohorts or in other schools. Exploiting this variation, I study impacts on three classes of outcomes: (i) prosocial behavior, (ii) discrimination, and (iii) academic outcomes. First, I find that having poor classmates makes wealthy students more prosocial. In particular, they become more likely to volunteer for a charity at school. Second, having poor classmates makes wealthy students discriminate less against poor children, measured by their teammate choice in an sports contest. Third, I find marginally significant negative effects on test scores in English, but no effect on Hindi or Math. Overall, I conclude that mixing in schools had substantial positive effects on the social behaviors of wealthy students, at the cost of negative but arguably modest impacts on academic achievement. To shed light on mechanisms, I exploit administrative records on the idiosyncratic assignment of students to study groups and find that the effects on social behaviors are largely driven by personal interactions between wealthy and poor students, rather than by changes in teacher behavior or curriculum.

For *Big-Thatha*, who lit a flame in the mind of a ten-year old boy. For my parents, who gave me more freedom than I had earned at every stage of my childhood. And, most of all, for the love of my life, Urvi, for reasons too many to list here.

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Chapter 1

Self-Control and Chronic Illness: Theory and Evidence From a Field Experiment

With Liang Bai, Benjamin Handel and Edward Miguel

1.1 Introduction

Preventive care is considered by many public health experts to be an essential means of improving population health outcomes, while potentially reducing overall health expenditure by preventing or attenuating serious (and expensive) health conditions. Yet inefficient utilization of preventive care is a stylized fact of health markets across a range of countries and contexts. In the US, scholars have identified the roles of consumer information, health externalities, insurance market failures and time preferences in causing an under-utilization of preventive care (Kenkel 2000). In developing countries, preventive care technologies such as vaccinations, anti-malarial bed nets, water purification, and the management of chronic conditions are often vastly underutilized (Dupas 2011).

Recent literature has identified the following three key reasons that consumers might underinvest in preventive care, even in the absence of market failures due to externalities and moral hazard: *(i) time inconsistency, also known as self-control problems or present bias; (ii) a high price elasticity of demand for preventive care, such that even moderate increases in price cause dramatic reductions in demand; and (iii) a lack of information or awareness on the part of consumers.*

This project seeks to carefully identify the importance of these three mechanisms in the context of the rapid growth of hypertension in India. While our analysis focuses on the take up of commitment contracts for preventive care for hypertension, the insights have implications for preventive care utilization for other services as well , e.g., diabetes. To this end, we run a randomized control trial where treatment groups are offered different types of commitment contracts, relative to a control group that only pays for medical care in a typical, linear, fee-for-service manner. The randomized control trial, conducted on a population of approximately 2,000 households in rural Punjab, is also paired with detailed household field surveys and detailed administrative data on health care utilization for each household. The trial has six treatment arms, corresponding to (i) control, (ii) control group with discount coupons, (iii) fixed commitment contract offer, (iv) fixed commitment contract offer with discount, (v) flexible commitment contract (where the consumer chooses the commitment amount), and (vi) flexible commitment contract with discount. We study the implications of such contracts in the context of the rich demographic and health data that the field surveys and administrative data provide. The lessons we learn in this setting have policy relevance throughout the developing world, not least of all for rapidly growing countries such as India and China. But our findings also have implications in countries like the US, where diabetes and cardiovascular disease are major health challenges.

Our intervention was implemented in conjunction with our partner organization E-HealthPoint, which builds and operates community-scale clinics ("E-HealthPoints") and water treatment facilities ("Waterpoints") in rural Punjab. E-HealthPoint brings modern health care to patients through telemedical consultations with qualified doctors, diagnostic testing, and high-quality medicines via a licensed pharmacy. Our analysis focused on participation in preventive health care "camps" run weekly by E-HealthPoint. These camps were designed to focus on hypertension and give patients both preventive treatment, consultation about how to maintain low blood pressure, and, if necessary, hypertensive medications. The commitment contract interventions mentioned above had households pay money up front, and then receive money back for each regularly scheduled visit they successfully made to a health camp. The partnership between E-HealthPoint, a private organization, and our research team has been a very effective partnership for implementing cutting-edge research and economic methods with on-the-ground infrastructure.

This project is one of the first experimental evaluations of the optimal two-part commitment contract (with an upfront lump sum payment and low or negative per visit prices) derived by DellaVigna and Malmendier (2004) and previously only supported by correlational evidence. While other experimental research (such as Ashraf et al. 2006) has studied commitment devices using third-party verifiers of behavior in collaboration with financial institutions, none have studied the kinds of simple and self-enforcing contracts that provide incentives for sophisticated consumers to overcome their time inconsistency problem. Even amongst the few existing experimental studies of time inconsistency in the context of health behavior, little attention has been paid to interventions which might benefit unsophisticated (i.e., overconfident) time-inconsistent consumers. Since unsophisticated consumers are far more likely to procrastinate indefinitely on health investments, such an intervention has the potential for large effects on behavior and welfare. Informed by relevant theory (O'Donoghue and Rabin 1999), we design and evaluate such an intervention: time-limited discounts for the purchase of preventive care. Furthermore, our study is one of the first to rigorously estimate the demand for management and prevention of chronic disorders like hypertension in a developing country context. We go beyond merely estimating the demand for this type of prevention by studying the roles played by information and time preferences in the apparent under-investment in preventive care in developing countries.

Even at this preliminary stage, our results suggest that commitment contracts increase preventive health care utilization. Preliminary analysis shows take-up rates of approximately 14% for different forms of commitment contracts without a paired discount. With a paired discount, 26% of respondents offered a fixed commitment contract (in which the terms are given to them) take up that contract, while 39% of respondents offered a flexible contract (in which they can set the commitment level) take up that contract. This suggests that discounts and flexibility of the commitment contract are important determinants of participation. Furthermore, and perhaps most importantly, entering into a commitment contract has a meaningful impact on visits to E-HealthPoint health camps. Overall, 28.9% of those who sign up for a commitment contract visit at least one E-HealthPoint health camp (to leverage that commitment) while only 8.9% of those from a control group not offered such a contract do. We provide detailed analysis on many dimensions with respect to commitment contract take up and subsequent health service utilization. Finally, we include a detailed theoretical economic model of commitment contract participation and health care utilization: this will be the basis for future research by the team for this randomized control trial. This future work will directly link theoretical models of commitment to the data we observe in our setting.

Section 2 discusses the empirical setting of the study, and Section 3 describes the experimental design. Section 4 present a theoretical model of commitment contracts and consumer health care utilization. Section 5 describes the data, while Section 6 presents empirical results. Section 7 concludes.

1.2 Study Setting

Hypertension, otherwise known as high blood pressure, is one of the most prevalent chronic diseases across the globe. In 2008, approximately 40% of adults ages 25 and over have been diagnosed with hypertension worldwide, and the disease accounted for at least 45% of deaths due to heart disease (World Health Organization 2013). Growing concerns about the public health consequences of hypertension have taken center stage in global health policy as well. Indeed, the World Health Organization (WHO) declared "control your blood pressure" as the theme for World Health Day 2013, with the goal of focusing attention on prevention and control of hypertension.

In low-income countries such as India, where the public health system is plagued by high absenteeism rates and low service quality (Banerjee and Duflo 2009), the disease burden from hypertension is particularly high. According to the Association of Physicians of India, the prevalence of hypertension in the last 6 decades has grown almost 13-fold nationally in urban areas and almost 8-fold in rural areas.[1] Nevertheless, most hypertensive patients in India go undiagnosed, and few are actually managing their disease. For instance, one study in urban India found that only one-third of the study population were aware of their high blood pressure, and of those who knew they were hypertensive, less than half kept their blood pressure under control (Mohan et al. 2007). Amidst this backdrop of low awareness and treatment of hypertension in India, we leverage recent insights from behavioral economics to examine the determinants of preventive health among hypertensive individuals in a rural setting.

Our study was carried out in 4 rural villages – Doda, Harikekalan, Mallan, and Rajiana – located in the state of Punjab in Northern India. The study was implemented in partnership with E-HealthPoint, an organization which delivers clean drinking water and primary medical care services to rural markets using community health clinics. In particular, E-HealthPoint conducts "Hypertension Days" wherein an experienced doctor from a nearby city visits each village every week to treat hypertension patients.[2] The consultation fee to see the doctor during "Hypertension Days" is Rs. 20 (excluding the cost of medicines, lab tests). During the patient's visit, the doctor takes health measurements (blood pressure, BMI, and waist circumference), provides the patient with information about hypertension, and prescribes an appropriate treatment plan. The doctor also encourages the patient to make dietary and lifestyle changes such as decreasing salt intake and maintaining a healthy weight.

1.2.1 Sample Selection

Since "Hypertension Days" are targeted towards patients with high blood pressure, our study sample consists of individuals above the age of 30 who have hypertension or are at high risk for hypertension. We follow widely accepted medical guidelines and define hypertensive patients as those with systolic blood pressure above 140 or diastolic blood pressure above 90.[3] To identify hypertensive individuals, a team of enumerators first screened all members of a particular household by measuring their blood pressure using an automatic blood pressure measurement device.[4] If the systolic or diastolic blood pressure reading is above the thresholds previously described, the enumerator immediately invites the individual to participate in the study and to complete the baseline survey. In the event that more than one household member has hypertension, the member with the highest stage of hypertension (stage 1 or stage 2), is selected to take part in the study.

[1] More information on the epidemiology of hypertension is India is available from the Journal of the Association of Physicians of India, February 2013, Volume 61.

[2] Note that all individuals, even those without hypertension, are able to see the doctor during "Hypertension Days." However, hypertension patients receive priority, given that the program was launched specifically to address high blood pressure.

[3] Both the Association of Physicians of India and the NIH define hypertension in this manner.

[4] Enumerators were trained rigorously in operating the device. The survey team used the Citizen CH-452 model for measuring blood pressure. This model has been validated by the ESH protocol and was selected for the project in consultation with a medical doctor.

Furthermore, in the event that more than one household member has the same hypertension stage, the member with the highest systolic blood pressure reading is selected to take part in the study. Finally, high hypertension risk individuals were identified using a hypertension risk score algorithm based on age, gender, family history of hypertension, family history of diabetes, tobacco use, physical activity, and waist circumference.[5] Across all 4 villages our sample, a total of 20,824 individuals from 4028 households were screened. From the screening, 2004 households with at least one hypertensive member and an additional 276 households with at least one high hypertension risk member were selected for the study, for 2280 total households. Of these 2280 households, 1720 agreed to participate. The main sample for this paper thus consists of these 1720 respondents who completed the baseline survey.

1.3 Experimental Design

During the same household visit in which the baseline survey is administered, the respondent was offered a commitment contract or price discount coupons to visit the weekly "Hypertension Day" in their village for 3 times in 6 months. The commitment contracts and coupons were designed to be valid for 3 visits in 6 months since the Indian Hypertension Guidelines recommend that hypertension patients visit the doctor at least every two months to manage their disease.[6] We randomized the type of contract offered to each household, stratified by hamlet and household head's education, by using a computer prior to the enumerator's visit.[7] Specifically, households were randomized into one of following 6 groups with equal probability:

Group 1: Fixed (standard) commitment contracts *without* discount. This group was offered a commitment contract for 3 visits to the "Hypertension Days" during a 6-month period. As part of the contract, the respondent was required to pay in advance for all 3 doctor visits (Rs. 60 or Rs. 20 per visit). The respondent was also asked to pay an additional commitment amount of Rs. 30, which she receives back in equal installments of Rs. 10 each time she visits the doctor. In other words, the respondent pays a total of Rs. 90 up front, and receives Rs. 10 on each of the 3 visits.

Group 2: Fixed (standard) commitment contracts *with* discount. This group was offered a commitment contract identical to that of Group 1, except that the respondent in this group received a 50% discount on the consultation fees for 3 visits, and in

[5]This 100-point hypertension risk score algorithm is based on current literature and was developed in consultation with Dr. Sumeet Ahluwahlia and Dr. Hemant Madan.

[6]See http://www.apiindia.org/pdf/hsi_guidelines_ii/managehypert.pdf.

[7]Before commencing the study, we conducted a census in all 4 villages in our sample and collected data on household characteristics. We used this census data to randomize households with a Stata program.

some instances, also paid a higher commitment amount of Rs. 45. Thus, the total upfront payment is Rs. 60, i.e., Rs. 30 for consultation fees plus Rs. 30 as the commitment amount, or Rs. 75, i.e., Rs. 30 for consultation fees plus Rs. 45 as the commitment amount.

Group 3: Flexible (self-designed) commitment contracts *without* discount. While the commitment amount is fixed at Rs. 30 or Rs. 45 in Groups 1 and 2, in Group 3, the respondent chooses the commitment amount beginning with Rs. 0.[8] As above, the respondent receives this committed amount back in 3 equal installments every time she visits the doctor. The respondent is also required to pay in advance for 3 visits to the "Hypertension Days," so the total upfront payment is Rs. 60 for consultation fees plus the respondent's selected commitment amount.

Group 4: Flexible (self-designed) commitment contracts *with* discount. This group was offered a commitment contract identical to that of Group 3, except that the respondents in this group received a 50% discount on the consultation fees for 3 visits. Hence, the total upfront payment is Rs. 30 for consultation fees plus the respondent's selected commitment amount.

Group 5: Discount coupons. Each respondent in this group received 3 discount coupons. Each coupon provided a 50% discount on the consultation fee. These discount coupons are valid for the same 6-month period as the commitment contracts in Groups 1 to 4.

Group 6: No discount coupons (control). This group was not offered any commitment contracts or discounts on consultation fees. Each respondent only received information about managing hypertension and a flyer with the times and location of "Hypertension Days," but these were provided to all participants in the study.

Respondents in Groups 1 to 4 can avail of their respective commitment contracts in several ways. First, they can accept the commitment contract on the spot with the enumerator, who subsequently collects the payment. Second, respondents can sign up for the commitment contract with E-HealthPoint's village health workers (VHW) and health coordinators (HC), both of whom are well-known in the village since they often go door-to-door to assess the community's health needs. Specifically, about 3 to 4 days after the enumerator offered the commitment contract to a particular household, the VHW and HC visited households in Groups 1 to 4 who had not yet signed up for the contract. The VHW and HC then asked these respondents whether they would like to take up the commitment contract, as well as reminded them about the "Hypertension Days" schedule.

[8] In practice, the respondent's chosen commitment amount is rounded up or down so that it is divisible by 3.

Note that the VHW and HC visited *all* households in the study to remind them about "Hypertension Days," including those in the control group, to hold constant any effects the VHW and HC's visit may have. Lastly, respondents in Groups 1 to 4 were also able to sign up for the commitment contracts directly at the "Hypertension Day" clinic at any time during the course of the study.[9]

While both the commitment contracts (Groups 1 to 4) and price discount coupons (Group 5) covered 3 visits to "Hypertension Days," respondents were given the opportunity to renew these commitment contracts and coupons at the clinic for the remainder of the 6-month program. These renewals were described to respondents when the commitment contracts and discount coupons were initially introduced by enumerators. In the case of commitment contracts, for example, respondents who completed 3 visits in the first 2 months of the program can take up another commitment contract for 3 visits in the remaining 4 months. Similarly, for discount coupons, respondents who have used up all 3 coupons in the first 2 months of the program can ask for another set of 3 coupons, which will be valid for the remaining 4 months.

A final set of treatments were implemented 2 weeks before the conclusion of the 6-month program. In each village, half of the respondents were randomly selected to receive a short reminder about "Hypertension Days." These respondents were personally visited by our team of enumerators, and were informed that there were 2 weeks left until the commitment contracts or price discount coupons expire, if applicable. The other half of the respondents served as control, and did not receive any reminders.

1.4 Model

The simple preliminary empirical analysis above suggests that commitment contracts can have an important impact on preventive health care utilization. It is important to "look under the hood" to determine exactly which aspects of behavior and decision-making guide the effectiveness, or lack thereof, of commitment contracts for increasing preventive health care utilization. This section presents a simple, in progress economic model of preventive health care utilization. The research team will work on this model and connect it to the data over the next several months.

1.4.1 A Simple Model of Health Investments

The diagnosis, monitoring and management of hypertension (like other preventive health behaviors) can be thought of as an investment in personal health. Each involves tradeoffs between initial costs (financial as well as effort and time costs) and potential future benefits from improved health. A simple economic model of health investment

[9] Although "Hypertension Days" are only held once a week at the clinic, the clinic is open Mondays through Fridays to sell medicine and conduct lab tests. Each respondent could sign up only for the commitment contract she was originally offered.

helps formalize the three mechanisms mentioned above, and illuminates how they may lead to inefficient levels of preventive health care utilization. Consider an individual who must make decisions in every period about how to allocate scarce resources between consumption of goods and possible preventive care investments. Her decision problem can be represented as the inter-temporal "utility" function:

$$U_t = u\left(h_t, c_t, prevent_t\right) + E_t \sum_{i=1}^{T} D\left(i\right) u\left(h_{t+i}, c_{t+i}, prevent_{t+i}\right),$$

where the utility incurred in each period $u\left(h_t, c_t, prevent_t\right)$ depends directly on present health status h_t, consumption of goods c_t, but also potentially the direct pleasure or discomfort of taking a particular preventive health action $prevent_t$ (such as the discomfort of walking a mile to the clinic for a hypertension test).

In each period, she must decide whether or not to make a preventive health investment ($prevent_t = 1$ or 0). If she does not make the investment, with a probability $\overline{\pi}$ she suffers a negative health shock in the *next* period. However, if she chooses to make the health investment, she reduces her probability of a negative health shock in the next period.

$D\left(i\right)$ is a *discount function* which captures the extent to which the consumer underweights utility experienced in periods in the future from today's perspective. For simplicity, we assume that income in each period is exogenous, equal to W_t. A_t represents consumer financial assets at time t. We close the model with the budget constraint $p_c c_t + p_p prevent_t = W_t + rA_t$, where p_p is the price of preventive health care, p_c is the price of consumption goods, and r is the interest rate on savings. Guided by this broad framework, we will use a series of experiments to explore three main ways in which economic incentives can affect the demand for preventive care and move utilization towards a socially efficient level.

1.4.2 Time Inconsistency or Self Control Problems

Making decisions about preventive health investments involves trading off costs in the present against expected benefits in the future. Thus, any factor that drives a wedge between how present and future costs and benefits are valued might have large effects on preventive behavior. An obvious candidate for such a wedge is "time preferences," which capture the extent to which people discount the future relative to the present while making decisions. In the simple model described above, this corresponds to the function $D\left(i\right)$, the factor by which utility i periods in the future is discounted relative to the present. Clearly, people who discount the future at a higher rate will value preventive care less, all else equal.

However, recent research has explored a more powerful mechanism through which time preferences might affect behavior – the existence of a present bias in time preferences (O'Donoghue and Rabin 1999). Present bias captures the psychology of a discretely greater concern for utility experienced in the present, relative to all future periods of time. This feature is elegantly captured in the quasi-hyperbolic model of time discounting (Phelps and Pollak 1968; Laibson 1997). In this formulation, the present value of a flow of utilities $(u_s)_{s\geq t}$ as of time t is

$$U_t = u_t + \beta \sum_{s=1}^{\infty} \delta^s u_{t+s}$$

Here, the discount factor between today and the next period is $\beta\delta$ while the discount factor between any two consecutive periods in the future is simply δ. If , $\beta < 1$ we have non-constant time discounting – the discount rate between today and tomorrow differs from the discount rate between tomorrow and the day after. This results in *time-inconsistency* – a situation in which an agent systematically chooses to deviate from a plan he had thought perfectly optimal when formulated in the previous period. An illustration would be the (fictional) reviewer who plans to read the grant tomorrow, and does not follow through on this plan when tomorrow actually arrives. Theoretical work by O'Donoghue and Rabin (1999, 2001) has made clear that a crucial role is played by whether the consumer is sophisticated about their present bias. A sophisticated consumer is one who is fully aware of the time inconsistent nature of her preferences. Conversely, an unsophisticated (or "naïve") consumer believes that, beginning next period, she will no longer be present biased. Thus, an unsophisticated consumer is overconfident about her future time-inconsistency problem. These two types can be modeled by introducing a parameter $\hat{\beta}$ to represent the agent's beliefs about his future present bias β. Then a sophisticated consumer is characterized by $\hat{\beta} = \beta$ while an unsophisticated consumer believes $\hat{\beta} = 1$. Following O'Donoghue and Rabin (2001), we can also consider partially sophisticated consumers, who merely underestimate their future present bias, $\beta < \hat{\beta} < 1$. The literature has made the following predictions for investment tasks (tasks with immediate utility costs and delayed benefits) under time inconsistency:

1. Sophisticated consumers value "commitment": Sophisticated consumers are willing to pay for simple contracts which commit them to future actions by restricting their future choices, often by making certain actions more expensive (Thaler 1980, Laibson 1997). An example would be a smoker who makes a deposit in a savings account which does not allow withdrawals until he verifiably quits smoking (Ashraf et al. 2006). These commitment contracts appeal to the *long-run selves* of sophisticated consumers – they are most effective when the consumer perceives all the costs and benefits as being in the future. The smoker is most likely to make the commitment when the pain of quitting begins tomorrow rather than today.

Ashraf et al. (2006) find that 28% of those offered a costly commitment device to quit smoking accept it. In a quite different setting, Benartzi and Thaler (2004) show that 78% of people offered a soft commitment to increase savings in the future accept the commitment.

2. Overconfident consumers do not value commitment: Since unsophisticated consumers are overconfident about their ability to complete the investment task tomorrow,

they do not recognize their need for commitment contracts (O'Donoghue and Rabin 1999). And partially sophisticated consumers (who know they have a time inconsistency problem, but underestimate its magnitude) may even under-commit to a task, accepting a contract which does not provide sufficient incentives to follow through (DellaVigna and Malmendier 2004). They might thus be actively hurt by being offered the commitment device: consider the smoker who deposits money in the commitment account but then fails to quit smoking, thus losing his deposit.

3. Commitment contracts for repeated tasks: When an investment task must be done repeatedly (such as attending a gym, or periodically visiting a clinic to have an illness monitored), sophisticated consumers will benefit from contracts which help them commit to repeating the task. When the contracts are limited to freely chosen fee structures, they will take the form of a two-part tariff, with high lump sum "membership" fees and subsequent per-visit prices chosen well below marginal costs (DellaVigna and Malmendier 2004). These contracts are attractive to the long-run selves of sophisticated consumers, who realize that the low (and possibly negative) marginal price of attendance will incentivize them to attend more, helping overcome their self-control problem. As above, unsophisticated consumers will not value such a contract. In related empirical work, the authors show that such contracts are well represented in such industries as health fitness centers and life insurance. This type of contract is especially relevant to the prevention and treatment of chronic medical conditions.

4. Deadlines: O'Donoghue and Rabin (1999) show that present-biased consumers can be induced to complete tasks more efficiently by using a system of increasing punishments, and particularly by using a deadline beyond which penalties are severe. Such a scheme allows those without self-control problems to find an efficient time to complete the task (say, a day when the opportunity cost of completing the task is low), while preventing those with self-control problems from procrastinating too long. Some empirical evidence supports this theory: in Kenya, Duflo et al. (2011) find that time-limited discounts on the purchase of fertilizers increases the adoption of fertilizer by up to 70%.

1.4.3 Price of High-quality Preventive Care

The price of preventive care p_p faced by individuals may differ dramatically from the long-run marginal social costs of health care provision. This could occur, for example, due to inefficient but profit-maximizing prices set by a monopolistic provider of healthcare services or products. Alternatively, utilization may be lower than socially desirable simply because of a lack of access to quality healthcare. For example, people in far-flung rural areas may simply face insurmountable travel costs in accessing health facilities. And valuable health investments, such as safe drinking water, may simply not be available at all in particularly disadvantaged locations. In such a setting, interventions which increase access to preventive care, either through the introduction of new services or through variation in prices, might dramatically shift use towards the socially efficient level. Finally, a great deal of evidence suggests that the demand for preventive care is highly "price elastic," especially in developing countries. In other words, even small increases in price above zero can lead to dramatic reductions in demand. In Kenya, Kremer and Miguel (2007) find that take-up of deworming medications drops from 80% to 20% when the price is raised from zero to US$0.30. In Zambia, Ashraf et al. (2010) find that take-up of a water-treatment product drops from 80% to 50% when the price increases from US$0.10 to $0.25.

1.4.4 Information About the Effectiveness of Preventive Care

People's beliefs $\tilde{\pi}$ about the true effectiveness of a particular preventive behavior might be biased. Thus, they might overestimate ($\tilde{\pi} > \pi$) or underestimate ($\tilde{\pi} < \pi$) the extent to which a particular preventive behavior reduces the probability or severity of a future negative health shock, leading to inefficient levels of utilization. In such a setting, an intervention which provides accurate information about preventive health measures could have a large impact.

Empirical evidence suggests that information can sometimes be effective in increasing preventive health investments. Madajewicz et al. (2007) and Jalan and Somanathan (2008) both show that households respond significantly to information about the purity of their drinking water, while Dupas (2011) finds that adolescent girls reduce risky sexual behavior when informed about risks. However, even comprehensive information may have negligible effects in comparison to modest price changes (Kremer and Miguel 2007). The relative effectiveness of information and price changes thus remains an unresolved and interesting question, which we will attempt to answer.

We shall also seek to understand the importance of the source of information about health behavior. In particular, social learning is widely thought to be an important mechanism for the diffusion of technologies (Foster and Rosenzweig 2010), but few studies have rigorously documented its importance in affecting health behavior, especially in less affluent populations. Leonard et al. (2009) use non-experimental evidence to argue that households in rural Tanzania learn about the quality of care offered by different providers from their neighbors' past experiences. Dupas (2010) uses a field experiment to show that social learning increases adoption when people initially underestimate the returns to adoption.

Our final empirical analysis with this model will leverage our randomized control trial, together with measures of information and social networks from our baseline and endline surveys to carefully identify the role of social networks in diffusing information about health behavior.

1.4.5 Time Preferences Model with Commitment Contracts

We model a present biased consumer's decision to sign up for a given commitment contract and subsequently the decision to visit a doctor. In the first stage, the respondent chooses whether to participate in a particular commitment contract. In the second stage, and given her commitment contract choice, she decides whether to visit the doctor. We assume that the decision to visit a doctor in the absence of commitment contracts depends on three factors: a doctor visit fee f paid at the time of visit, additional effort or monetary costs (e.g., time, transportation) c incurred at the time of visit, and a delayed health benefit b. Of the three factors, this paper focuses on the first component, the fee paid at the time of visit, and varies this component through commitment contracts.

Each commitment contract requires the respondent to pay $f - d + m$ in advance of visiting the doctor, where f is the doctor's visit fee, $d \in [0, f]$ is a possible discount on the doctor visit fee, and m is a non-negative additional commitment amount, which the respondent receives back only upon visiting the doctor. In other words, $m > 0$ creates a negative marginal price for a doctor visit in the future. [10] The decision to visit the doctor with a commitment contract, therefore, depends on three factors: the commitment amount received back m at the time of the visit, additional effort or monetary costs (e.g., time, transportation) c incurred at the time of visit, and a delayed expected health benefit b. We define $\underline{m}$ as the minimum (or only) commitment amount permitted in a commitment contract. Commitment contracts may be either (a) *fixed commitment contracts*, where $m = \underline{m} = m_{fix} \in (0, \infty)$ is a fixed value, or (b) *flexible commitment contracts*, where the respondent may choose any $m \in [0, \infty)$, and thus, $\underline{m} = 0$.

The simplest way to represent the respondent's choices and decisions is with a three-period model. In $t = 0$, the respondent is offered a single commitment contract and decides whether to accept it. If she does, she pays $f - d + m$. In $t = 1$, the respondent decides whether to visit the doctor given her commitment contract decision in $t = 0$. If she decides to visit the doctor without a commitment contract, she incurs costs $c + f$. If she decides to visit the doctor with a commitment contract, she incurs costs $c - m$. In $t = 2$, the respondent receives a health benefit b if she visited the doctor in $t = 1$.

To this three-period model we add exponential and present-bias discounting. A respondent with only exponential discounting has time consistent preferences. A respondent with present bias substantially discounts any time that is not the present, leading to time inconsistent preferences. She may want to visit a doctor at a given time in the future, but does not actually choose to visit a doctor when that time arrives. We use δ to denote the exponential discount rate and β to denote actual present bias.[11] We use $\hat{\beta}$ to denote predicted present bias (i.e., what a respondent at $t = 0$ predicts her present bias will be at $t = 1$). We assume that $\hat{\beta} \in [\beta, 1]$ (i.e., respondents assume they are weakly less time-inconsistent than they actually are) and that $\hat{\beta}$ is constant across periods. Based on β and $\hat{\beta}$, respondents can be classified into several groups that give us some intuition about how they behave. Respondents with $\hat{\beta} = \beta = 1$ are fully time consistent. Respondents with $\hat{\beta} = \beta \in (0, 1)$ are sophisticated in that they know how time inconsistent they will

[10] Note that each commitment contract reduces the effective fee paid at the time of visit to zero or a negative value by requiring a patient to pay the full doctor visit fee in advance. If the patient pays a positive commitment amount, then the effective fee paid at time of visit would be negative.

[11] We assume $\delta, \beta \in (0, 1]$ and that δ, β are constant across periods.

be in the future. Respondents with $\hat{\beta} = 1$ and $\beta \in (0, 1)$ are fully naive in that they incorrectly predict that they will be fully time consistent in the future. Most respondents are probably partially naive with $0 < \beta < \hat{\beta} < 1$ in that they predict they will be somewhat time inconsistent but are overly optimistic about how time inconsistent they will be.

Doctor Visit. We assume that a respondent visits a doctor if the net expected utility of doing so is positive. At $t = 0$, the respondent wants her future self to visit the doctor if:

$$\begin{cases} \beta\delta^2 b - \beta\delta\,(c + f) \geq 0 & \text{if not participating in a commitment contract} \\ \beta\delta^2 b - \beta\delta\,(c - m) - (f - d + m) \geq 0 & \text{if participating in a commitment contract} \end{cases} \tag{1.1}$$

In the case without a commitment contract, the first term, $\beta\delta^2 b$, represents the health benefit b discounted from $t = 2$ to $t = 0$. The second term, $-\beta\delta\,(c + f)$, represents the effort/monetary cost c incurred and doctor visit fee f, both discounted from $t = 1$ to $t = 0$. In the case with a commitment contract, the first term, $\beta\delta^2 b$, again represents the expected health benefit b discounted from $t = 2$ to $t = 0$. The second term, $-\beta\delta\,(c - m)$, represents the effort/monetary cost c incurred and commitment amount m received back, both discounted from $t = 1$ to $t = 0$. Finally, the third term, $-(f - d + m)$, represents the doctor visit fee f with discount d and commitment amount m paid for the commitment contract at $t = 0$.

At $t = 1$, the respondent actually visits the doctor if:

$$\begin{cases} \beta\delta b - (c + f) \geq 0 & \text{if not participating in a commitment contract} \\ \beta\delta b - (c - m) \geq 0 & \text{if participating in a commitment contract} \end{cases} \tag{1.2}$$

In the case without a commitment contract, the first term, $\beta\delta b$, represents the expected health benefit b discounted from $t = 2$ to $t = 1$. The second term, $-(c + f)$, represents the effort/monetary cost c incurred and doctor visit fee f paid at $t = 1$. In the case with a commitment contract, the first term, $\beta\delta b$, again represents the expected health benefit b discounted from $t = 2$ to $t = 1$. The second term, $-\beta\delta\,(c - m)$, represents the effort/monetary cost c incurred and commitment amount m received back at $t = 1$.

In the remainder of the model, we restrict our attention to cases in which the respondent would want her future self to visit a doctor even without a discount – i.e., cases in which the first inequality in Equation 1.1 holds. These respondents are our target population because they want their future selves to visit doctors even without discounts but face present-bias barriers that they might be able to overcome with commitment contracts. Studies that attempt to examine other issues (e.g., respondents face information barriers and perceive the health benefits from doctor visits to be lower than the actual health benefits) might also consider cases in which the respondent wants her future self to visit a doctor even only with a discount.

Commitment Contract Participation. We assume a respondent chooses to participate in a commitment contract if she predicts that participating in the contract will yield a higher expected utility than not participating in the contract. Each respondent who wants her future self to visit a doctor even without a discount falls into one of three following cases, depending on individual and commitment contract characteristics:

- Case 1: $c - \underline{m} < f + c \leq \hat{\beta}\delta b \leq \delta b$: the respondent predicts she will visit a doctor with or without a commitment contract
- Case 2: $c - \underline{m} \leq \hat{\beta}\delta b < f + c \leq \delta b$: the respondent predicts she will visit a doctor with a commitment contract, but not without one
- Case 3: $\hat{\beta}\delta b < c - \underline{m} < f + c \leq \delta b$: the respondent predicts she will visit a doctor with a commitment contract where $m \geq c - \hat{\beta}\delta b$, but not either (a) without a commitment contract or (b) with a commitment contract where $m < c - \hat{\beta}\delta b$

At $t = 0$, the respondent participates in a commitment contract if:

$$\begin{cases} \beta\delta^2 b - \beta\delta\left(c - m\right) - \left(f - d + m\right) \geq \beta\delta^2 b - \beta\delta\left(c + f\right) & \text{if Case 1} \\ \beta\delta^2 b - \beta\delta\left(c - m\right) - \left(f - d + m\right) \geq 0 & \text{if Case 2} \\ \beta\delta^2 b - \beta\delta\left(c - m\right) - \left(f - d + m\right) \geq 0 & \text{if Case 3 and } m \geq c - \hat{\beta}\delta b \\ -\left(f - d + m\right) \geq 0 & \text{if Case 3 and } m < c - \hat{\beta}\delta b \end{cases} \tag{1.3}$$

The left side of the inequality represents the predicted utility if the respondent participates in a commitment contract and the right side of the inequality represents the predicted utility if the respondent does not participate in a commitment contract. A respondent never participates in a commitment contract if Case 3 holds and $m < c - \hat{\beta}\delta b$, since $-\left(f - d + m\right) < 0$ for any values of the parameters. In all cases, if the respondent decides to participate in a commitment contract, she would want to choose the lowest permitted value of m such that the case still holds. Under a fixed commitment contract, the respondent chooses $m = \underline{m} = m_{fix}$, whereas under a flexible commitment contract the respondent chooses $m = \max\left\{c - \hat{\beta}\delta b, \underline{m} = 0\right\}$.

Graphical Representation. In both our graphical representation and experimental design, we restrict the discount on the doctor visit fee to two possible values: $d \in \left\{0, \frac{1}{2}f\right\}$. In *commitment contracts without a discount*, $d = 0$, and in *commitment contracts with a discount*, $d = \frac{1}{2}f$. We use this further simplification to derive respondent behavior in terms of β, $\hat{\beta}$, δ, b, c, f, and m_{fix}. The parameters β, $\hat{\beta}$, δ, b, and c are based on characteristics of individual respondents, whereas the parameters f and m_{fix} are determined by our experimental design.

In Figure 1.1, we graphically represent a respondent's predictions about her future doctor visit behavior and her actual doctor visit behavior for values of β and $\hat{\beta}$, holding other parameters fixed. The graph represents all respondents who would want their future selves to visit a doctor even without a discount. The horizontal sections represent whether the respondent predicts she will visit a doctor both with and without a commitment contract, only with a commitment contract, or not even with a commitment contract. The vertical sections represent whether the respondent actually visits a doctor both with and without a commitment contract, only with a commitment contract, or not even with a commitment contract.

In Figure 1.2, we add the respondent's decision about whether to participate in the commitment contract for our four types of commitment contracts: (a) fixed commitment contracts without a discount, (b) fixed commitment contracts with a discount, (c) flexible commitment contracts without a discount, and (d) flexible commitment contracts with a discount. With this information, we can identify which types of respondents would gain from, lose from, or not be affected by a commitment contract offering. In particular, respondents who participate in commitment contracts and subsequently visit the doctor gain from the commitment contract offering, whereas respondents who participate in commitment contracts and subsequently do not visit the doctor lose from the commitment contract offering.

Figure 1.4 assumes uniformly distributed β and $\hat{\beta}$ and depicts the fraction of respondents who do not want their future selves to visit the doctor without a discount, as well as the share of respondents who *do* - either without a discount, with a discount, or with a commitment contract, as well as whether they follow through with their plans.

1.5 Data

In this study, we use three main data sets in our analysis. First, a baseline survey was conducted prior to presenting the treatments to respondents. The baseline survey collected information on respondent and household characteristics, as well as the respondent's diet profile, smoking and drinking habits, health status, health-seeking behavior, health knowledge, and time and risk preferences.

Second, we collected data on attendance at "Hypertension Days" for all study participants. For the 6-month period in which commitment contracts and price discount coupons were valid, a member of our field staff was present during the weekly "Hypertension Days" in each village to record the household ID number and names of all study participants who came to see the doctor. Furthermore, we collected such attendance data for 1 month after contracts and coupons expired, which allows us to examine treatment effects in a setting where commitment contracts were no longer available.

Finally, an endline survey was conducted in each village 1 week after the commitment contracts and price discount coupons expired. This endline survey asked questions similar to those in the baseline, but in addition, it asked information on doctor visits at the "Hypertension Days" or other health care providers. This survey also included the respondent's weight and waist circumference measurements, as well as dietary and exercise changes.

1.5.1 Summary Statistics and Randomization Checks

Baseline characteristics for our sample are shown in Table 1.1. Respondents in our sample come from households that have 5.43 members on average and have a median annual household income of Rs. 50000. 60% of our sample is female, and 80% are currently married. Among household heads, the most common occupation is self-employed in agriculture and 45% can both read and write.

A large portion of our respondents also have characteristics that place them at risk for hypertension. For instance, the average age in our sample is 53.6, and the risk of high blood pressure increases with age. The average BMI in our sample is 25.4, where a BMI over 25.0 is generally considered to be overweight.

Despite the poor health status of individuals in our sample, few respondents actually see a doctor other than for illness. Based on self-reports at baseline, while 76% say that they "always" or "frequently" seek health care when they are feeling sick, only 6% of respondents visit the doctor for preventive care. While 71% of our sample are aware that they are hypertensive, only 60% of these individuals (who know that they are hypertensive) are currently taking medication to manage their hypertension. Behavioral barriers may play a critical role in explaining the low demand for health services in our setting. Indeed, 20% of individuals in our baseline data are present-biased, and almost all respondents (69%) strongly agree that they are often impatient.

A randomization check of our commitment contracts and discount treatments, available upon request, do not show any systematic, statistically significant differences in pairwise comparisons of each treatment group with the remainder of the sample across key variables. The few significant differences that we do observe are consistent with what would occur by chance.

1.6 Empirical Results

This section describes preliminary results for (i) the baseline survey, (ii) take up of commitment contracts, and (iii) utilization of health care services a function of treatment group and commitment contract take up.

1.6.1 Take Up

Table 1.2 studies commitment contract take-up by treatment group. A number of clear and interesting patterns emerge. First, take-up for the contracts without a discount is 13.7% (39 out of 284) for the "fixed" contract that specifies the exact contract ahead of time, and 14.1% (40 out of 283) for the "flexible" contract that allows users to specify the amount they commit. So at the undiscounted price-point, restricting the flexibility of the contracts does *not* reduce demand for the contract.

Second, discounts have a substantial impact on take-up: for the fixed contract with discount, 25.9% (72 out of 278) take up the contract while for the flexible contract 38.6%

(112 out of 290) take up the contract. These results suggest that both (i) discounts have a marked impact on take-up and (ii) that the flexible contract and the discount are complementary. People are more likely to take up the contract with discount when they have the option to specify the amount that they commit. In general, those who take up the flexible contract include a lower commitment amount than that specified by the fixed contract, which suggests that many consider the fixed contract to be “too strong”. The pattern of results is consistent with the model: the bundled discount nudges consumers with low demand for commitment to sign up, particularly when they can choose small commitment amounts.

Table 1.3 breaks down commitment contract participation as a function of hypertension knowledge and practices during the baseline survey. Interestingly, there does not seem to be a meaningful statistical difference in participation between respondents who are unaware of high blood pressure (indicating they may not have hypertension) and respondents who are aware of high blood pressure (hypertensive). This result is interesting since one would expect being aware of high blood pressure to be a driver of take-up. Among respondents who are aware of high blood pressure, those taking medication for hypertension appear to be less likely to participate in commitment contracts than those not taking medication for hypertension, except in the case of the flexible contract with discount (which receives high take-up among both groups). Respondents taking hypertension medication could be more likely to seek preventive health care, which might lead to higher commitment contract participation. However, respondents taking hypertension medication could also already have other health care providers, which might lead to lower participation in commitment contracts specifically for “Hypertension Days.”

1.6.2 Doctor Visits

The next step in the analysis is to assess the extent to which those who take up a commitment contract use E-HealthPoint services, in the form of health camp visits. Table 1.4 and Figure 1.6 compare doctor visits by treatment group.

A first result is the high price elasticity of health camp visits to price. Offering a 50% price discount coupon increases visits by about half, from 8.9% to 14.6%.

A second and sobering result is that offering consumers commitment contracts has little impact on attendance. Compared to the 8.9% attendance of the control group, 9.5% of those offered the full-price fixed commitment contract attend at least once. Attendance for those offered a full-price flexible contract takeaway is a similar 9.9%. Similarly, bundling discounts with commitment contracts does not increase attendance relative to simple price discounts. We conclude that offering consumers commitment contracts does not increase take up of preventive doctor visits to manage hypertension.

The third result, consistent with the model, is that *conditional on signing up for a commitment contract*, consumers in flexible self-designed contracts do worse than those in fixed take-it-or-leave-it contracts. Figure 1.7 shows that consumers in flexible contracts (who choose weaker commitment amounts, on average), at ten percentage points more likely to fail to visit the doctor. This result is consistent with the model. Partial naifs (and naifs enticed by the bundled discount) under-estimate their future self-control problem, and thus choose too-weak commitment amounts when offered the opportunity to pick their own commitment level. Contrary to the usual economic intuition, restricting choice can actually increase welfare with biased consumers.

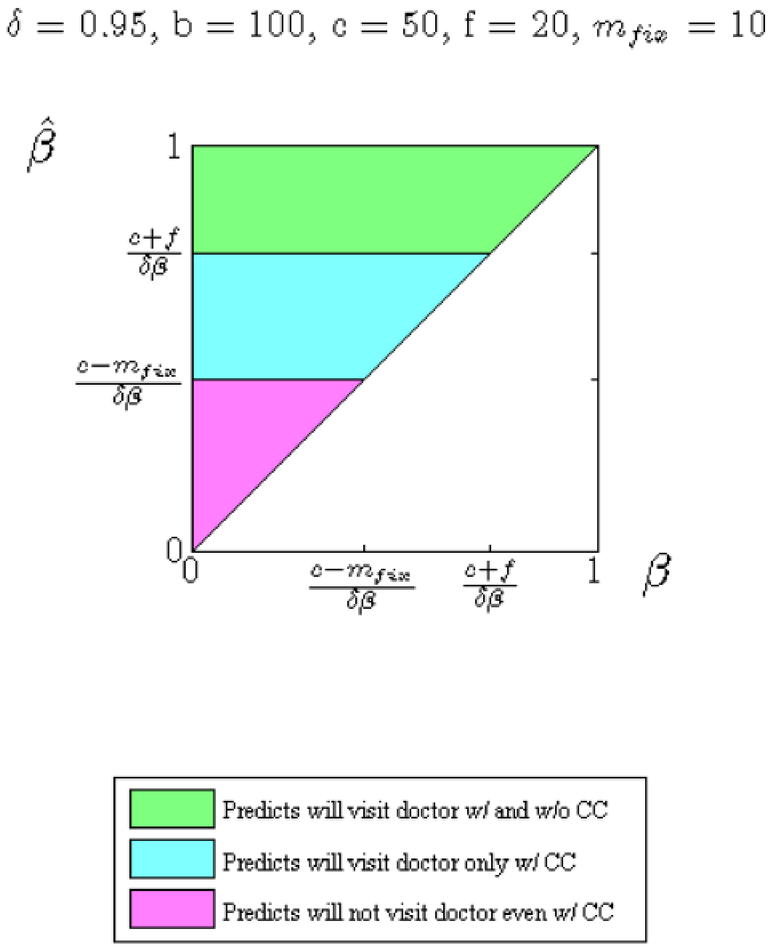
1.7 Conclusion and Future Work

This essay describes a theoretical model of preventive health care utilization under present biased time preferences, and the main results of a field experiment closely tied to the model. The model highlights the importance of present bias, and particularly sophistication and naivete about this present bias, in the take up and utilization of commitment contracts. It predicts that bundling discounts with commitment contracts can entice naifs and partial naifs to take up commitment contracts. It also suggests that restricting the flexibility of the contract will increase the follow-through rates of consumers conditional on signing up for a commitment contract. Thus, bundling strong fixed commitment with discounts could increase take up (by enticing unsophisticated consumers) and follow through (by maintaining strong marginal incentives).

The experiment provides partial support for the model. Bundling discounts with commitment does increase take up of commitment, especially for flexible contracts. And reducing flexibility does increase follow-through on the committed action. However, offering contracts does not increase overall preventive health behavior.

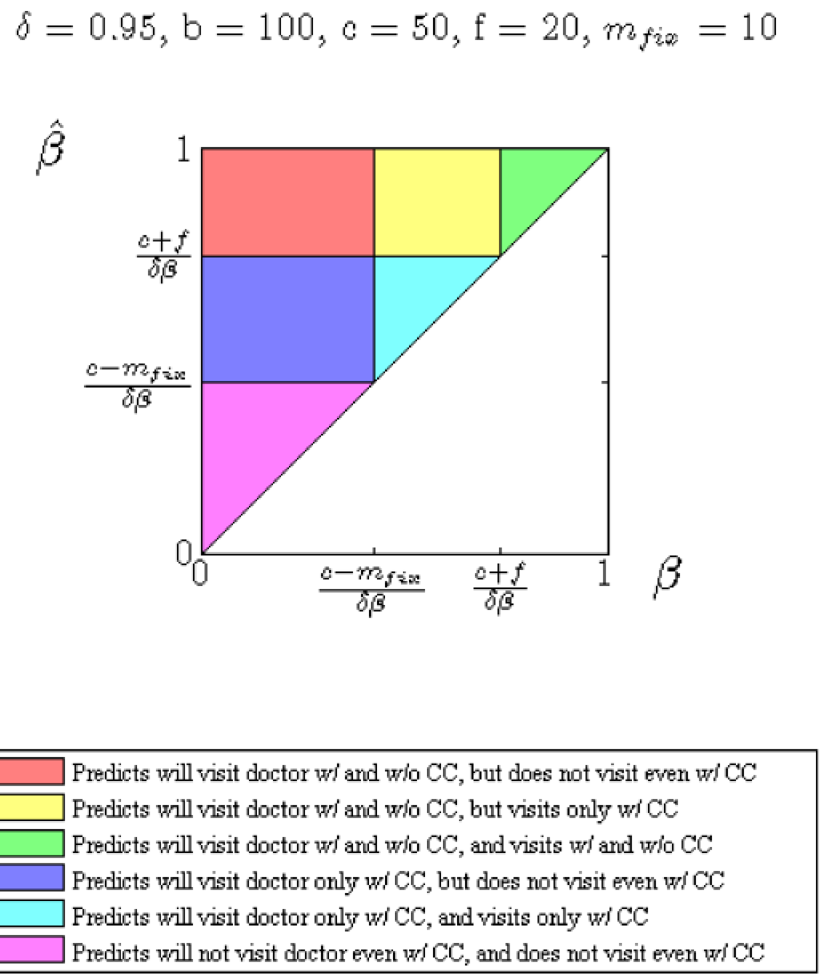
Future work could utilize the data generated by the field experiment to structurally estimate the theoretical model, so as to obtain the key time preference parameters. This would allow us to simulate alternative contract structures, and (with additional assumptions) evaluate welfare under the different contracts.

Figure 1.1: Respondent Behavior: Beliefs



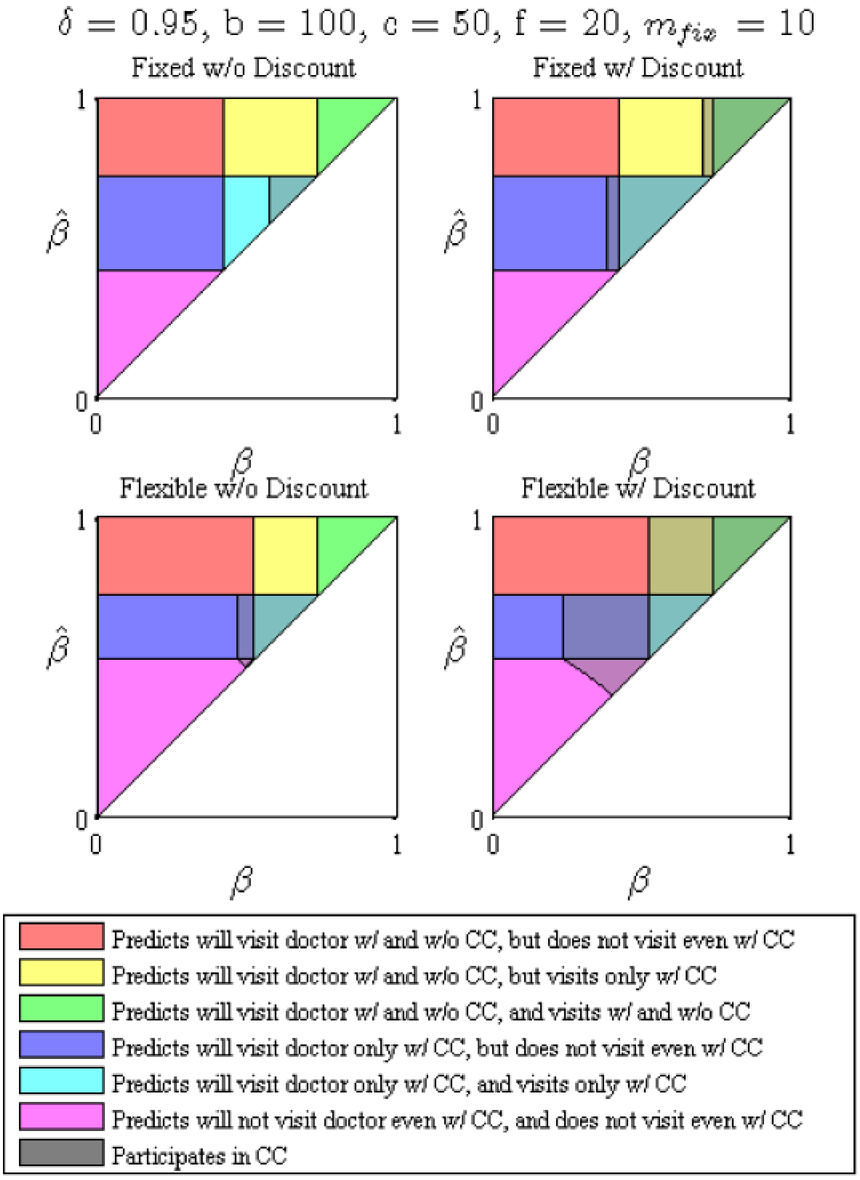
Notes: This figure shows whether a respondent predicts she will visit the doctor in cases with or without a commitment contract (CC) across all possible values of β (actual present bias) and $\hat{\beta}$ (predicted present bias), δ is the exponential discount rate, b is the delayed expected health benefit from a doctor visit, c is additional effort or monetary costs (e.g., time, transportation) incurred at the time of a doctor visit, f is the doctor visit fee (paid in advance for a CC), and m fix is the fixed commitment amount paid in advance and received back at the time of a doctor visit under a "fixed" CC. Under a "flexible" CC, the respondent may choose any non-negative commitment amount. The sample is limited to respondents who do want their future selves to visit the doctor even without a discount

Figure 1.2: Respondent Behavior: Beliefs versus Reality



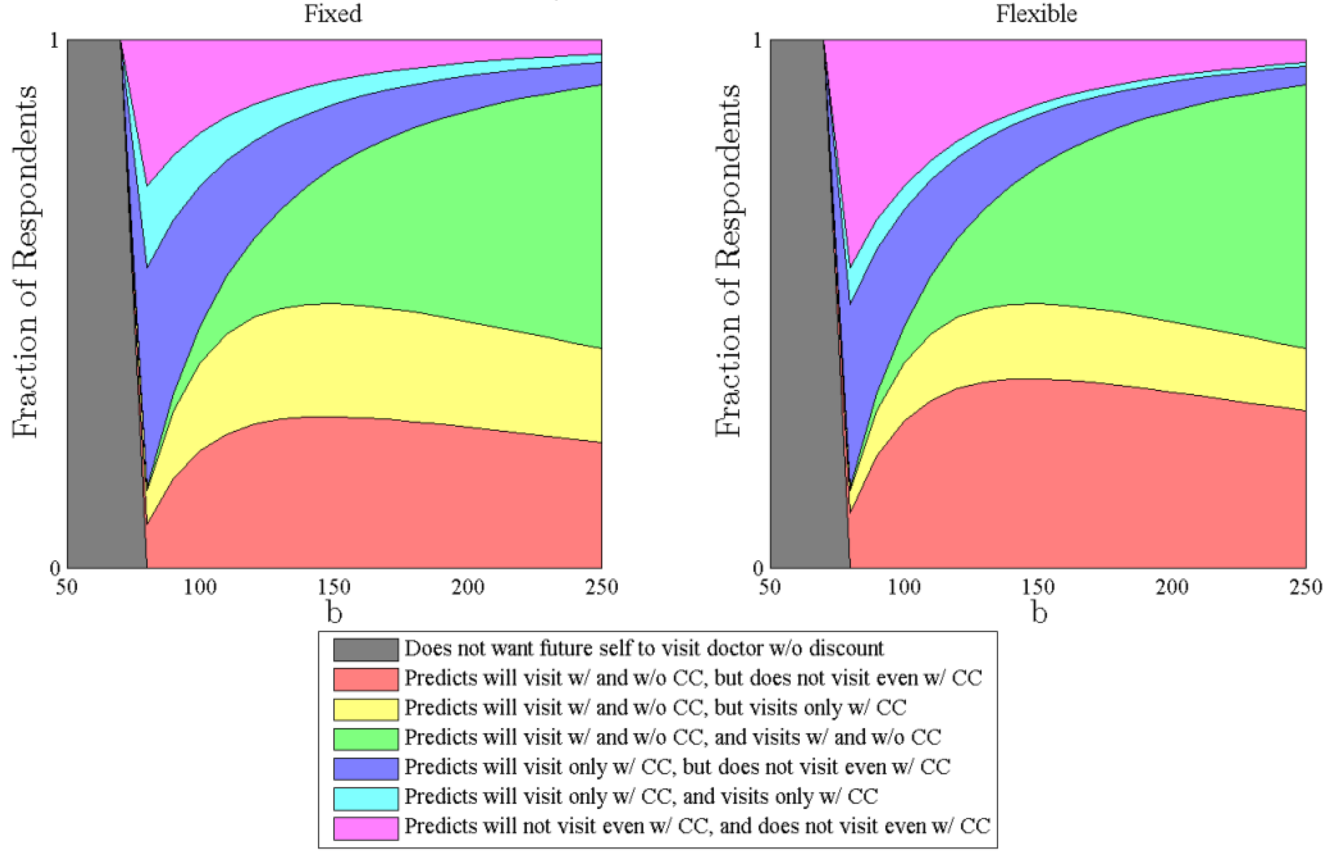
Notes: This figure shows whether a respondent predicts she will visit the doctor in cases with or without a commitment contract (CC) and whether she actually will visit the doctor in cases with or without a commitment contract (CC) across all possible values ofβ (actual present bias) and $\hat{\beta}$ (predicted present bias). The sample is limited to respondents who do want their future selves to visit the doctor even without a discount.

Figure 1.3: Commitment Contract Take-Up and Follow-Through



Notes: For each of four CC types, this figure shows whether a respondent predicts she will visit the doctor and whether she will actually visit the doctor in cases with or without the CC across all possible values ofβ (actual present bias) and $\hat{\beta}$ (predicted present bias). The figure also indicates whether a respondent participates in a CC with gray shading. The discount is 50% of the doctor visit fee f. The sample is limited to respondents who do want their future selves to visit the doctor even without a discount.

Figure 1.4: Commitment Contract Aggregate Demand



Notes: For fixed and flexible CC types and across a range of health benefit b values, this figure shows (a) the fraction of respondents who do not want their future selves to visit the doctor without a discount, and (b) of those respondents who do want their future selves to visit the doctor even without a discount, the fraction of respondents by predictions and actual behavior regarding whether they will visit the doctor in cases with and without the CC. Note that whether a CC offers a discount does not affect the fraction of respondents in each of these groups, but it does affect participation in CCs. β and $\hat{\beta}$ are assumed to have a joint uniform distribution over their possible values. Values for parameters δ, c, f, and m fix are fixed in these graphs.

Figure 1.5: Empirical Results: Take-up of Commitment Contracts

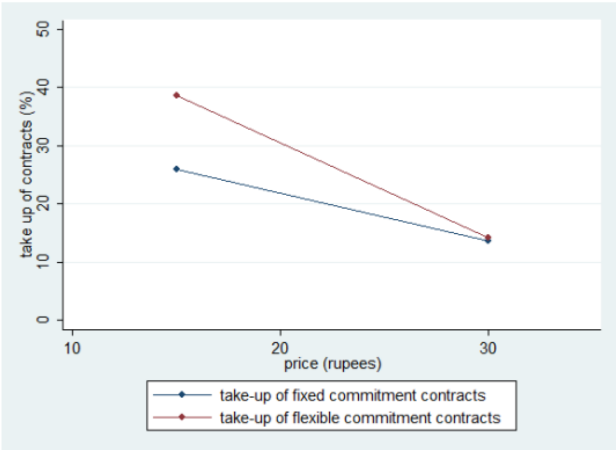


Figure 1.6: Empirical Results: Utilization Rates Across Treatments

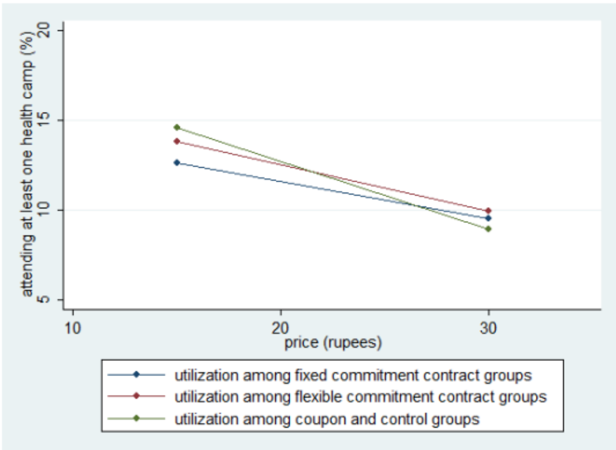


Figure 1.7: Empirical Results: Fraction of Zero-Visits Across Treatments

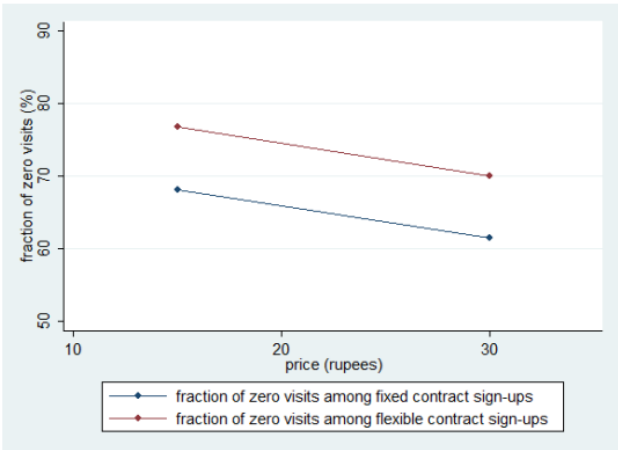


Table 1.1: Summary Statistics

	Mean	Std. Dev.	Min	Max	N
Respondent Demographic Characteristics					
Age	53.59	14.34	30	105	1720
Gender (0=Female; 1=Male)	0.40	0.49	0	1	1719
Currently married (0=No; 1=Yes)	0.80	0.40	0	1	1714
Caste (0=General; 1=SC, BC, BT)	0.45	0.50	0	1	934
Literacy (1=Can read and write; 0=Other)	0.35	0.48	0	1	1720
Occupation (1=Agriculture self-emp., 0=Other)	0.14	0.35	0	1	1604
Household (HH) Demographic Characteristics					
HH size	5.43	2.30	1	21	1720
HH head literacy (1=Can read and write; 0=Other)	0.45	0.50	0	1	931
HH head occupation (1=Agriculture self-emp., 0=Other)	0.37	0.48	0	1	906
Respondent Knowledge of and Trust in Health Care					
Heard of village private health clinic (1=Yes; 0=No)	0.98	0.14	0	1	1714
Trust in village private clinic (1=Trust; 5=Distrust)	1.94	1.34	1	5	1661
Heard of village EHP (1=Yes; 0=No)	0.88	0.33	0	1	1714
Trust in village EHP (1=Trust; 5=Distrust)	3.12	1.63	1	5	1122
Respondent Health Practices and Status					
Health visit at least frequently when sick (1=Yes; 0=No)	0.76	0.43	0	1	1714
Health visit when not sick in past year (1=Yes; 0=No)	0.06	0.24	0	1	1715
Know have high blood pressure (BP) (1=Yes; 0=No)	0.71	0.45	0	1	1714
Take medication for high BP (1=Yes; 0=No)	0.43	0.50	0	1	1425
Respondent Time and Risk Preferences					
Present-biased (1=Yes; 0=No)	0.20	0.40	0	1	1470
Time-consistent (1=Yes; 0=No)	0.46	0.50	0	1	1470
Often impatient (1=Agree; 5=Disagree)	1.84	1.41	1	5	1700
Procrastinates (1=Agree; 5=Disagree)	2.83	1.77	1	5	1683
Respondent Health Measures					
Body mass index (BMI)	25.44	10.79	13.3	395.4	1654
Body weight (kg)	64.94	14.66	30.8	164.9	1669
Height (cm)	160.55	10.23	62	189	1654
Waist circumference (cm)	95.95	11.49	43	174	1661
Systolic BP	142.42	24.46	87	264	1710
Diastolic BP	85.85	14.05	37	182	1710

Notes: This table presents the mean, standard deviation, minimum value, maximum value, and sample size (N) for each of these variables. For Caste, 1 refers to scheduled caste (SC), backward caste (BC), or backward tribe (BT). For "1=Trust; 5=Distrust," 1 refers to "Completely Trust" and 5 refers to "Completely Distrust." "Present-biased" refers to being more impatient in the short run (0-1 month). "Time-consistent" refers to being equally patient in the short run (0-1 month) and in the long run (6-7 months). For "1=Agree; 5=Disagree," 1 refers to "Strongly Agree" and 5 refers to "Strongly Disagree."

Table 1.2: Commitment Contract Participation by Treatment

	Participants	Total	Percent
Fixed CC Without Discount	39	284	13.7%
Fixed CC With Discount	72	278	25.9%
Flexible CC Without Discount	40	283	14.1%
Flexible CC With Discount	112	290	38.6%
Total	263	1135	23.2%

Table 1.3: Commitment Contract Participation by Baseline Hypertension Status

	Did Not Know Had High BP			Knew Had High BP			Not Taking Medication for High BP			Taking Medication for High BP		
	Participants	Total	Percent	Participants	Total	Percent	Participants	Total	Percent	Participants	Total	Percent
Fixed CC Without Discount	9	79	11.4%	30	205	14.6%	21	147	14.3%	10	93	10.8%
Fixed CC With Discount	25	91	27.5%	47	187	25.1%	36	144	25.0%	18	88	20.5%
Flexible CC Without Discount	12	82	14.6%	28	201	13.9%	22	129	17.1%	11	96	11.5%
Flexible CC With Discount	29	77	37.7%	83	210	39.5%	52	132	39.4%	44	102	43.1%
Total	75	329	22.8%	188	803	23.4%	131	552	23.7%	83	379	21.9%

Notes: This table divides respondents into whether they knew they had hypertension, i.e., high blood pressure (BP), and if so, whether they took medication for it at baseline.

Table 1.4: Doctor Visits by Treatment

	All Respondents			Participated in CC			Did Not Participate in CC		
	Visited	Total	Percent	Visited	Total	Percent	Visited	Total	Percent
Fixed CC Without Discount	27	284	9.5%	15	39	38.5%	12	245	4.9%
Fixed CC With Discount	35	278	12.6%	23	72	31.9%	12	206	5.8%
Flexible CC Without Discount	28	283	9.9%	12	40	30.0%	16	243	6.6%
Flexible CC With Discount	40	290	13.8%	26	112	23.2%	14	178	7.9%
CC Subtotal	130	1135	11.5%	76	263	28.9%	54	872	6.2%
Discount Only	42	288	14.6%						
Control	26	293	8.9%						
Total	198	1716	11.5%						

Chapter 2

Voting to Tell Others

With Stefano DellaVigna, John List and Ulrike Malmendier

2.1 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).[1] We also assume that individuals incur disutility from lying, consistent with the laboratory evidence: in cheap talk experiments, individuals often tell the truth even when lying would increase their payoffs (Gneezy (2005), Sánchez-Pagés and Vorsatz (2007), and Erat and Gneezy (2012)).

In this model, an individual is motivated to vote because she anticipates that others will ask if she did. If she votes, she can advertise her 'good behavior' when asked. If she does not vote, she faces the choice of being truthful but incurring shame, or saying that she voted but incurring the lying cost. This trade-off is reflected in the fact that 25 to

[1]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.

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 estimate the value of voting that is due to the social image motivation described above. This type of field experiment with parameter estimation is uncommon in the literature (Card, DellaVigna, and Malmendier, 2011). 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 registered members voted in the 2010 elections (henceforth, voting households) or none of the registered members voted in 2010 (non-voting households). We did 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 reflect 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.[2]

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. But non-voters sort out substantially more, decreasing their survey participation by 20 percent.[3]

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 prior 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.

[2]This randomization also includes a group with no flyer, as well a group with an opt-out box.

[3]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 survery take-up for either voters or non-voters.

The findings on sorting provide prima facie evidence of social-image utility. In order to quantify the utility value, we need to measure 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 (dis)utility.

In order to estimate the value of voting 'because others will ask,' we need additional counterfactual social-image values, such as the shame that voters would feel were they to say they did not vote. These counterfactuals are not provided by the sorting moments (since voters sort in anticipation of saying they voted).

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 the respondents of the ten-minute survey that the survey will be eight minutes shorter if they state that they did not 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 untruthfully 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 these social image utilities, especially among voters.

These estimates identify the key parameters up to an additive lying cost, which remains unidentified because it is always incurred jointly with the relevant social-image utility of claiming voting or non-voting. Since the lying cost is an integral part of the social-image value of voting (if lying is costless, there is no social-image motivation to vote), 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, for 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, our survey respondents report being asked nearly twice as often about voting in presidential elections. Hence, assuming similar 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.*'[4] 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

[4]We follow up with a door-to-door visit, as advertised.

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.

Third, the estimation of the parameters relies on the full set of 100 moments from the field experiment, making it difficult to highlight which moments play the most important role for the key parameter estimates. To address this issue, we plot the Gentzkow and Shapiro (2013) sensitivity measures, highlighting the critical role of the lying interventions.

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, 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 microfound 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), that campaign contributions depend on social comparisons (Perez-Truglia and Cruces, 2013), 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.

2.2 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:

$$pV + g - c + N\left[\max\left(s_V, s_N - L\right) - \max\left(s_N, s_V - L\right)\right] \geq 0. \tag{2.1}$$

The first three terms in expression (2.1) 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 when being asked about her turnout is $z^v \equiv \max\left(s_V, s_N - L\right)$. 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 $z^{nv} \equiv \max\left(s_N, s_V - L\right)$. The term in square brackets in (2.1) 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 of 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 ε for the other reasons to vote, we can rewrite the voting condition (2.1) as $N\Phi\left(s_V - s_N, L\right) + \varepsilon \geq 0$, where

$$\Phi\left(s_V - s_N, L\right) = \begin{cases} L & \text{if} s_V - s_N \geq L \\ s_V - s_N & \text{if} - L \leq s_V - s_N < L \\ -L & \text{if} s_V - s_N < -L. \end{cases} \tag{2.2}$$

As expression (2.2) shows, voting depends on the net social-image value $s_V - s_N$ and on the cost of lying L. Figure 2.1 displays $\Phi\left(s_V - s_N, L\right)$ 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 $c\left(h\right)$, 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 $c\left(h\right) = \left(h - h_0\right)^2 / 2\eta$.

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 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. 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 $\max_{h\in[0,1]} h \max\left(s^i + m - c^i, -S^i\right) - \left(h - h_0\right)^2 / 2\eta^i$, 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 pay m or a decrease in the time cost c will increase the probability of being at home and completing a survey. The parameter η^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 $h^{i*} = \max\left[\min\left[h_0 + \eta^i \max\left(s^i + m - c^i + z^i, -S^i\right), 1\right], 0\right].$ If $z^i > 0$, the respondent will 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 $h > 0$.[5] 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).[6]

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: $P(H)^v_{FE} \geq P(H)^v_F$ and $P(SV)^v_{FE} \geq P(SV)^v_F$. Parallel results hold for the opt-out flyers: $P(H)^v_{OOE} \geq P(H)^v_{OO}$ and $P(SV)^v_{OOE} \geq P(SV)^v_{OO}$ The probability of survey completion for voters is higher when informed at the door that the survey is about voting: $P(SV)^v_I \geq P(SV)^v_{NI}$.*

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: $P(H)^{nv}_{FE} \leq P(H)^{nv}_F$* and *$P(SV)^{nv}_{FE} \leq P(SV)^{nv}_F$. Parallel results hold for the opt-out flyers: $P(H)^v_{OOE} \leq P(H)^v_{OO}$ and $P(SV)^v_{OOE} \leq P(SV)^v_{OO}$. The probability of survey completion for non-voters is lower when informed at the door that the survey is about voting: $P(SV)^{nv}_I \leq P(SV)^{nv}_{NI}$.*

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 $L > 0$.

2.3 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. 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

[5] 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.

[6] The proofs are in Appendix A.

2011. The locations are towns around Chicago.[7] 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: 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 2.2). 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 2.2). 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 2.2). 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 2.2 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

[7] 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.

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 2.3 summarizes this first set of crossed treatments.

The fourth set of crossed treatments, summarized at the bottom of Figure 2.3, 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.

2.4 Reduced-Form Estimates

Answering the door and survey completion. We present graphical evidence in Figure 2.4 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 2.5a 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 2.5b), 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 2.6 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).[8] Indeed, we detect sizeable sorting in by Republican voters in response to

[8]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

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 2.7 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 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:

$$y_{i,j,t,h} = \alpha + \Gamma T_{i,t,h} + \eta_i + \lambda_t + \zeta_h + \varepsilon_{i,j,t,h} \tag{2.3}$$

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.[9] 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.

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

$$y_{i,j} = \alpha + \Gamma T_{i,j} + \eta_i + \varepsilon_{i,j} \tag{2.4}$$

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 η_i are included in regressions.

In Table 2 and Figure 2.8, 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

definition of households with registered Democrats.

[9]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.

A of Table 2, to 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.[10]

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 2.8 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.

[10]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.

2.5 Structural Estimates

Set-up. To estimate the model of Section 2.2, we impose additional assumptions, some of which are relaxed below. Since all parameters are allowed to differ between voters and non-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 μ_V and μ_N but the same standard deviation, $\sigma_V = \sigma_N$, which we denote by σ_{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 μ_s and σ_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) μ_V, the mean social-image utility from saying that one voted; (ii) μ_N, the mean social-image utility of saying one did not vote; (iii) σ_{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) η, the responsiveness of the probability of opening the door to the desirability of being at home; (iv) μ_s and σ_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 ξ, and by $\hat{m}$ the vector of observed moments. The minimum-distance estimator chooses the parameters $\hat{\xi}$ that minimize the distance $(m(\xi) - \hat{m})' W (m(\xi) - \hat{m})$. 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.

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, $l \in \{NoInc, 8\,\text{min}, \$5\}$. The moments $m(\xi)$ are: (i) the probability opening the door in survey treatments k, m, $P(H)^i_{k,m}$; (ii) the probability of completing the survey in survey treatments k, m, $P(SV)^i_{k,m}$; (iii) the probability of checking the opt-out box in the Opt-Out treatments, $P(OO)^i_{k,m}$ for $k \in \{OO, OOE\}$ (iv) the probability of completing the survey in the survey content treatments, given the flyer treatments: $P(H)^i_{a,k}$ and (v) the probability of lying about past turnout conditional on completing the survey, given incentive l, $P(L)_l^i$.[11] The empirical moments $\hat{m}$, 100 in total, are estimated in a first stage model using the same controls as in the main regressions.

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 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.

Value of Voting. Using the estimates, we compute the average social-image value of voting due to being asked once, $\int \Phi\left(s_V - s_N, L\right) dF\left(s_V, s_N\right)$. 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 2.9a 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

[11] 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 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.

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 2.9a 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, 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 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. 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 $N^{nv} = 5.33$). We also know 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 2.9 and Table 3).[12] 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 2.9b, is about twice as large, in the range of \$13-\$30. The model is therefore also consistent with the observed higher turnout

[12] 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.

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.

Identification and Sensitivity Analysis. We next discuss the intuition for the main sources of identification, and provide supporting evidence by calculating the sensitivity of the estimates to the individual moments following Gentzkow and Shapiro (2013). In particular, we compute a sensitivity matrix S, each element S_{ij} of which is the derivative of the estimated parameter $\widehat{\xi}_i$ with respect to the moment m_j.[13] We further normalize the measure so each element can be interpreted as the effect of a one standard deviation increase in the moment m_j on the expected value of the estimated parameter $\widehat{\xi}_i$, *holding other moments fixed* and measured in units of the asymptotic standard deviation of $\widehat{\xi}_i$. This calculation reveals the local sensitivity of the parameter estimates to each of the individual moments, and thus allows us to provide evidence on which features of the data and the experiment (locally) identify the model. Alternatively, the measure can be interpreted as revealing the sensitivity of the estimates to model misspecification.

First, consider the key social-image parameters, μ_V, μ_N and σ_{SI}. The difference in home presence and survey completion between the Flyer and Flyer-election, and between the Opt-Out and Opt-Out election treatments, play an important role. For voters, they help identify the mean social-image utility μ_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 μ_N^n must approximately equal the utility from lying, $\mu_V^n - L^n$. Hence, the sorting response for non-voters to the election surveys identifies both μ_N^n and $\mu_V^n - L^n$. A similar role is played by the difference in survey completion between the Information and No Information treatments. Finally, the response to the lying incentives is crucial for identifying the heterogeneity in social image σ_{SI} and the average utility difference between answering truthfully and lying. 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 $2\sigma_{SI}^{nv2}$.

The parameter sensitivity calculations at the estimated values largely confirm these intuitions. The home presence (PH) and survey completion (PSV) moments for the Flyer Election (WE) and Opt-Out Election (OOE) treatments largely show the expected effect: an increase in the moment (holding fixed the corresponding moments in the Flyer and Opt-

[13] We first compute the absolute sensitivity matrix $S = (\hat{G}'W\hat{G})^{-1}\hat{G}'W$, where W is the weighting matrix used in the minimum distance estimation, and $\hat{G} \equiv N^{-1}\sum_{i=1}^{N} \nabla_\xi m_i(\widehat{\xi})$ is the Jacobian of the moments with respect to the parameters. We then normalize the sensitivity measure as $\widetilde{S}_{ij} = S_{ij}\sqrt{\mathrm{Var}(m_j)/\mathrm{Var}\left(\widehat{\xi}_i\right)}$.

Out treatments) leads to an increase in the estimated μ_V^v and μ_N^v values.[14] The opposite pattern obtains for the Flyer and Opt-Out moments (holding constant the Fyer Election and Opt-Out Election moments), since it is the difference between the two that is most relevant. As predicted, an increase in survey completion in the Information treatment relative to the No Information treatment also increases μ_V^v and μ_N^v, since it implies a larger relative attractiveness of answering a survey about voter turnout. Finally, the estimated heterogeneity in signaling values σ_{SI}^v responds strongly to the lying incentive moments. More lying by voters in the incentive-to-lie treatments implies a greater mass of individuals close to the margin, and thus a lower estimate of σ_{SI}^v (i.e. a negative sensitivity value). More lying by voters when incentivized also implies a higher social-image value of not voting μ_N^v relative to voting (μ_V^v).

We also conduct the parallel analysis for non-voters. Similarly to the case of voters, the lying incentive moments play an important role, in the expected direction. Recall that non-voters are incentivized to tell the truth. Thus, a ceteris paribus increase in lying in the incentive treatment implies a smaller response to the incentive, and thus fewer marginal respondents due to a higher σ_{SI}^n. Conversely, an increase in lying in the control group implies a larger response to incentive, and thus a lower σ_{SI}^n. Higher lying in the control additionally implies a higher value of μ_V^n relative to μ_N^n. In contrast to the case of voters, the Information treatments play an important role for identification of μ_V and μ_N for non-voters. As we discussed above, the Information treatments do not provide evidence of sorting out, in contrast to the strong sorting out patterns in both the Flyer Election and the Opt-Out Election treatments. At the estimated parameters, since we explain the response to the Information treatments poorly for non-voters, the estimates become quite sensitive to these moments. This motivates a robustness check below in which we do not utilize these moments.

As for the auxiliary parameters, the mean and standard deviation of the value of completing a survey, μ_s and σ_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), and partly also by the response to the 8 minute time saving offered in the lying incentive. The baseline probability of answering the door, h_0, is pinned down by the share opening the door in the no-flyer treatments, and less directly by the share opting out in the opt-out treatments, since respondents are predicted to opt out only if they expect to be home in the first place. The probability of observing and remembering the flyer, r, is mainly 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 η with respect to incentives, and

[14] An exception occurs in each case for the unpaid 5 minute survey, where a ceteris paribus increase in the WE moments generally *reduces* the mean social image parameters. The fact that this decrease occurs not just for WE but also for W and the PSV_NW moments suggests that is through a mechanism unrelated to the one described above, and involves instead other important parameters which feed back to the estimation of μ_V and μ_N.

the social pressure S_s, are related to the share opening the door in the different survey treatments.[15] Identifying them separately is not obvious, since they often appear in the model in the product ηS. Indeed, the two parameters respond in opposite directions to each moment, and also are identified by a wider range of moments. The difficulty in separately estimating η and S motivates the robustness checks below where we fix η.

Decomposing the Benchmark Estimates. To further 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 μ_V and μ_N are not identified, but we can estimate the average *difference* $\mu_V - \mu_N$ (given a lying cost), as well as the heterogeneity σ_{SI}. Hence, holding μ_V fixed, we can estimate the other social-image parameters. The implied 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 μ_V for voters. However, the other parameters are not identified because the heterogeneity term σ_{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 σ. The key contribution of these sorting moments is not to the value of voting, but to the levels of the social-image terms and thus 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

[15] 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).

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.

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 μ_V^v and $\mu_N^v - L^v$ for voters, and μ_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 (μ_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, and hence does not allow for point identification of all parameters, but is restricted to this special subsample. Allowing for this extra difference leads to only a relatively small increase in the fit, to an SSE of 97.20.

For the preferred specification in Column 2, we again calculate the Gentzkow-Shapiro sensitivity measures, but this time including the estimated lying cost parameter L. We find that L is locally identified mainly by the lying incentives. A higher lying rate for voters when incentivized to lie results in a smaller estimated lying cost. Conversely, higher lying by non-voters when unincentivized implies a smaller lying cost. Greater survey completion in the unpaid election surveys (WE and OOE) also play a role, though with smaller effects. The sensitivity of the social-image parameters μ_V, μ_V and σ^v_{SI} and of the auxiliary parameters is similar to that reported for the benchmark estimates.

2.6 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.

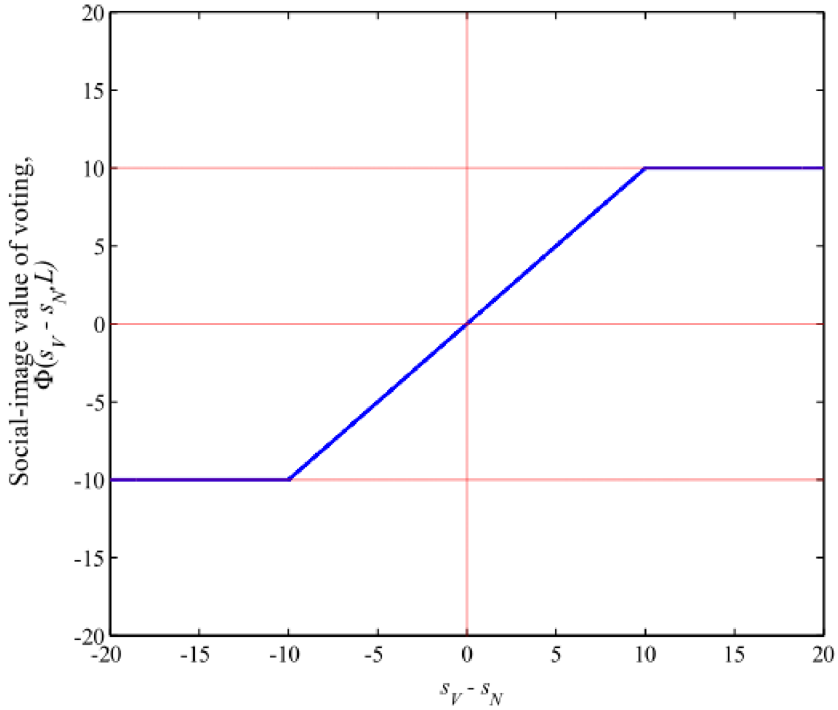
Overall, the evidence is qualitatively consistent with the predictions of the model, if statistically imprecise. The results are consistent with the contemporaneous and independent results of Rogers and Ternovsky (2013) who similarly inform a treatment group that they may be called after the election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

2.7 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.

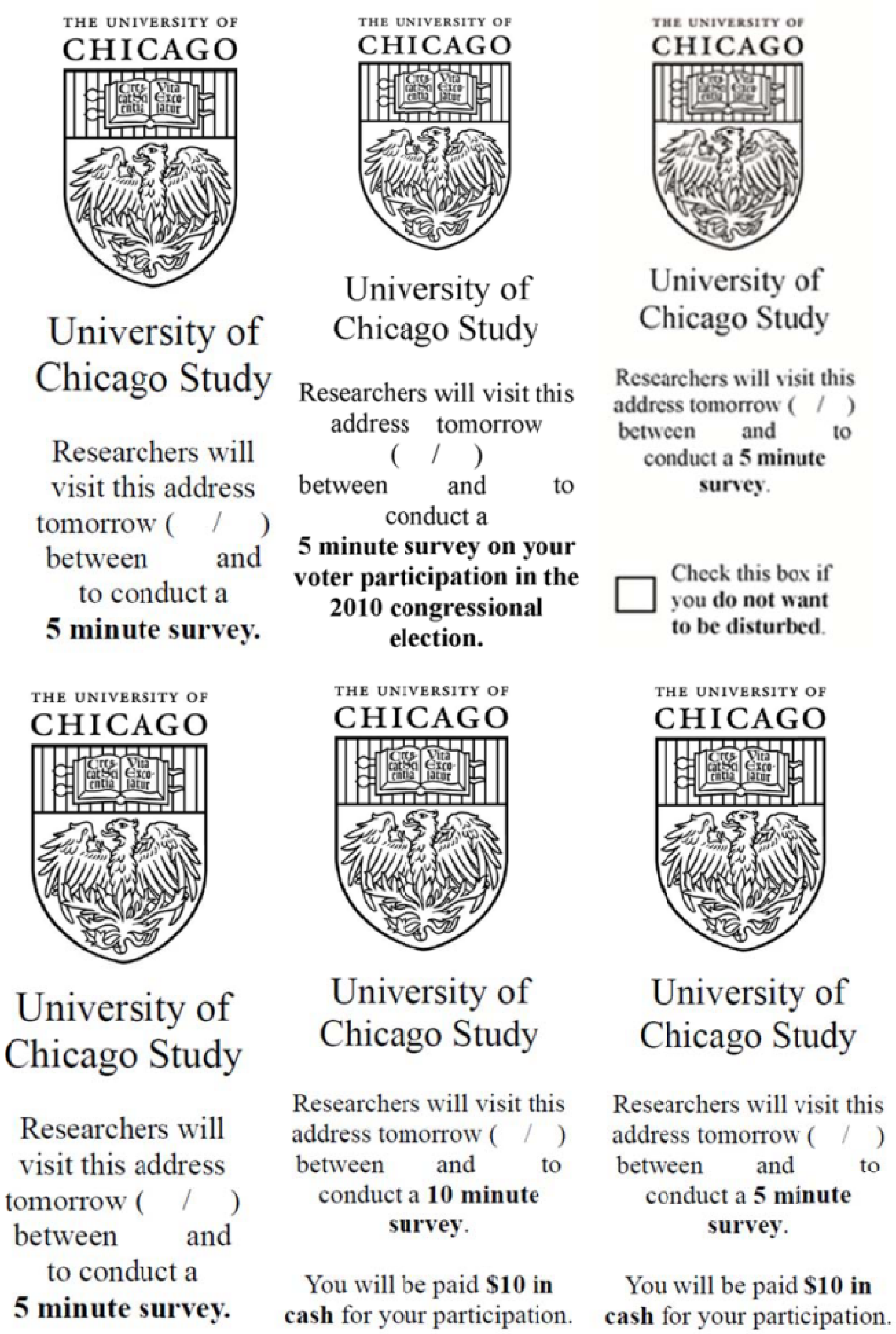
A methodological ingredient of this paper is the tight link between a simple model and the experimental design. This allows us not only to derive reduced-form results, but to use such results to estimate the underlying parameters. As such, this paper attempts to bridge a gap between two thriving, but 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.

Figure 2.1: Social-Image Value of Voting per Interaction (L = $10)



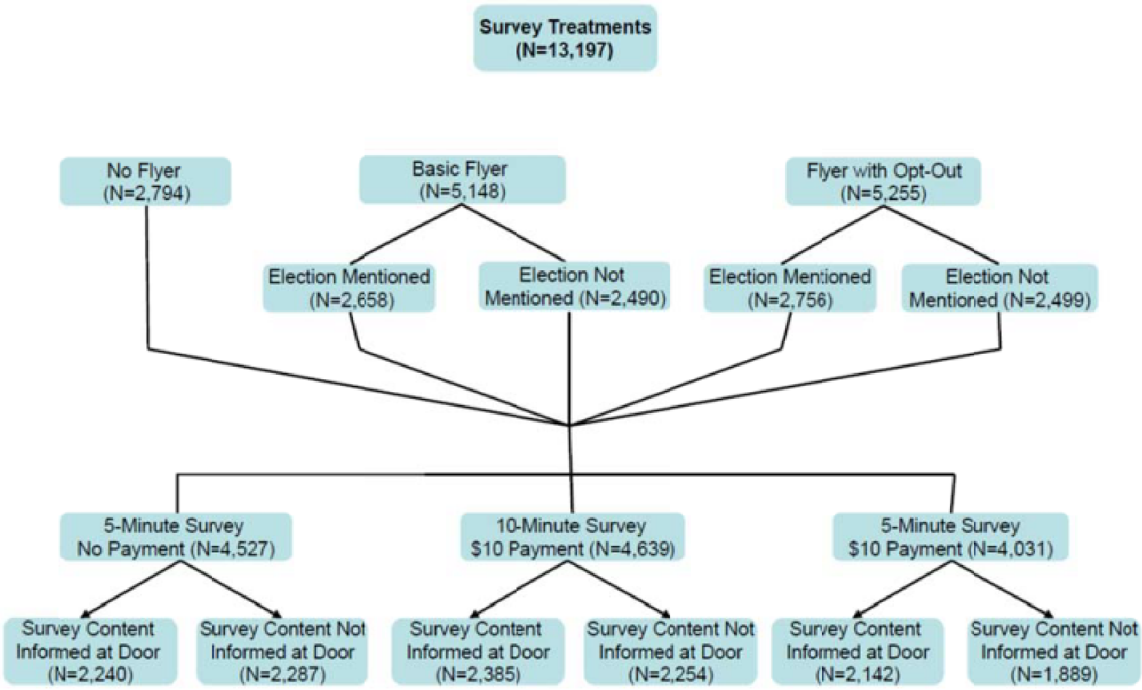
Note: Figure 1 plots the social-image value of voting due to the anticipation of being asked once, as a function of the net social image utility s_V-s_N and assuming a cost of lying L of $10.

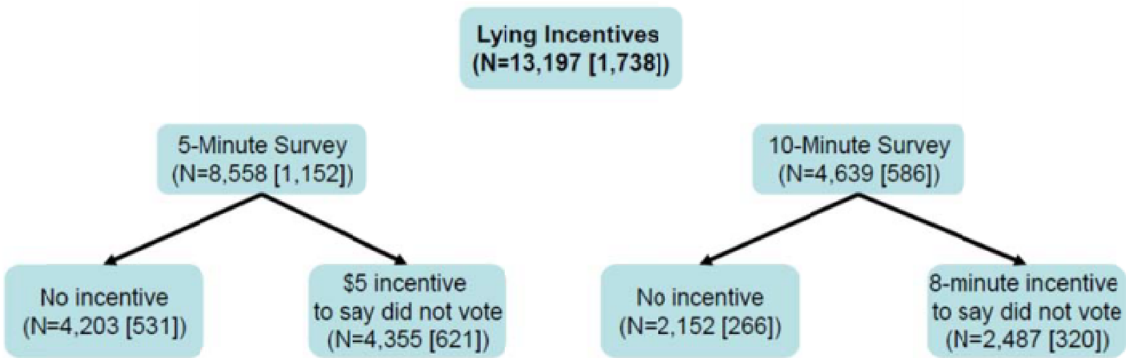
Figure 2.2: Flyer Samples



Note: The top three flyers are for (5-min., $0) surveys in treatments Flyer (left), Flyer election (center), and Opt Out (right). The bottom three flyers are for Flyer treatments (5-min., $0), (10-min., $10), and (5-min., $10).

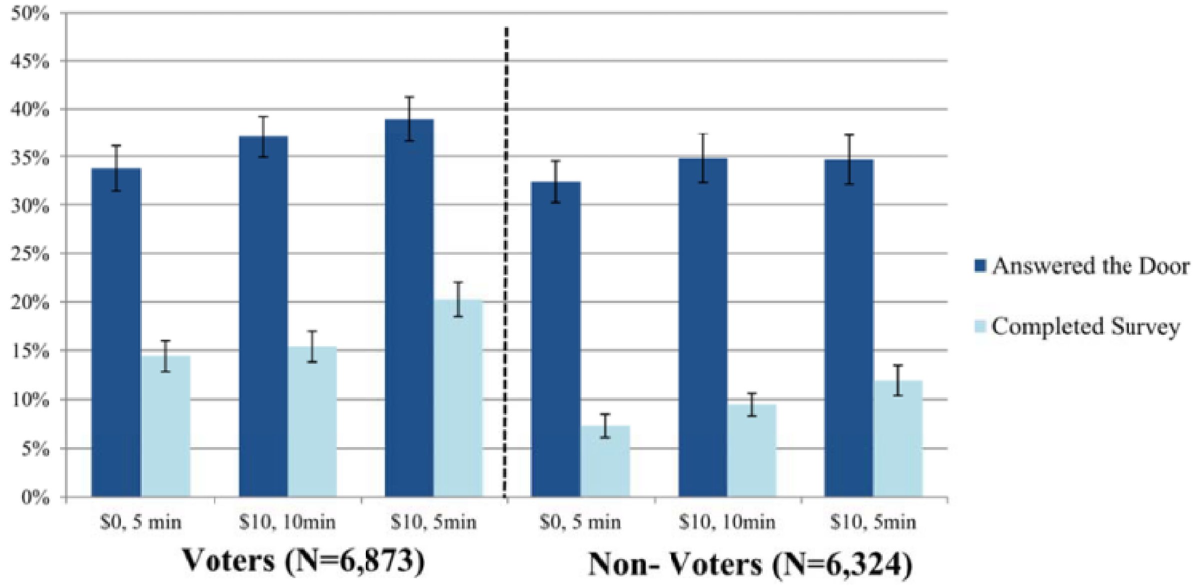
Figure 2.3: Experimental Treatments





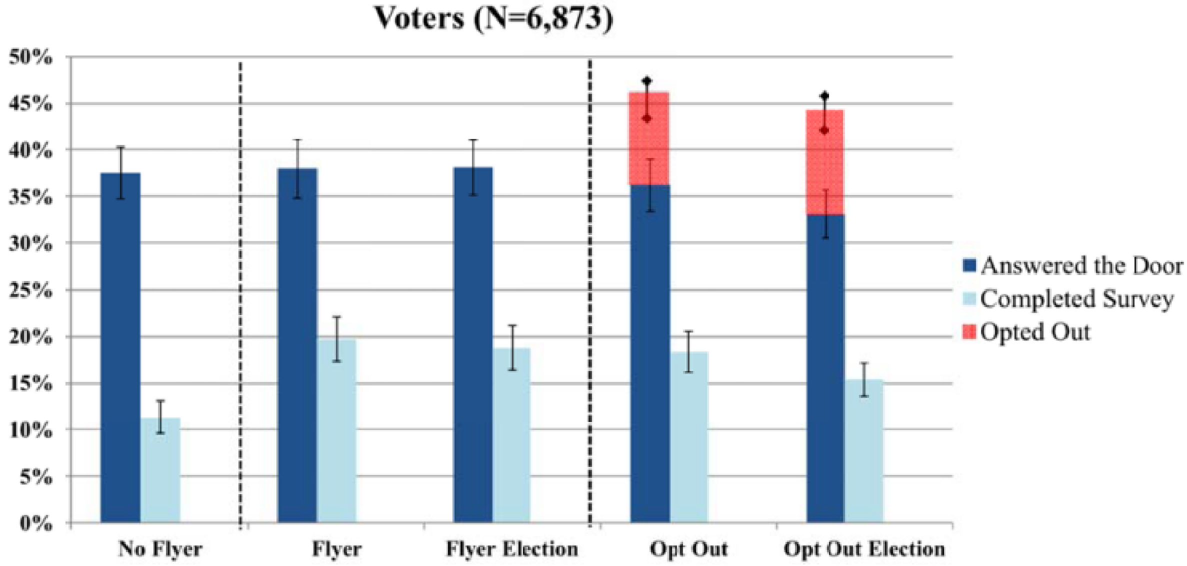
Note: Figure 4 presents the crossed experimental randomizations, with sample sizes in parentheses. On top are the five arms of the flyer treatment, crossed with whether respondents at the door are informed that the survey is about participation in the 2010 congressional election, crossed with survey duration and payment. At the bottom are the arms of the lying incentives, indicating both the initial sample size and [in square brackets] the sample size among individuals who responded to the survey. All arms are equally weighted and crossed.

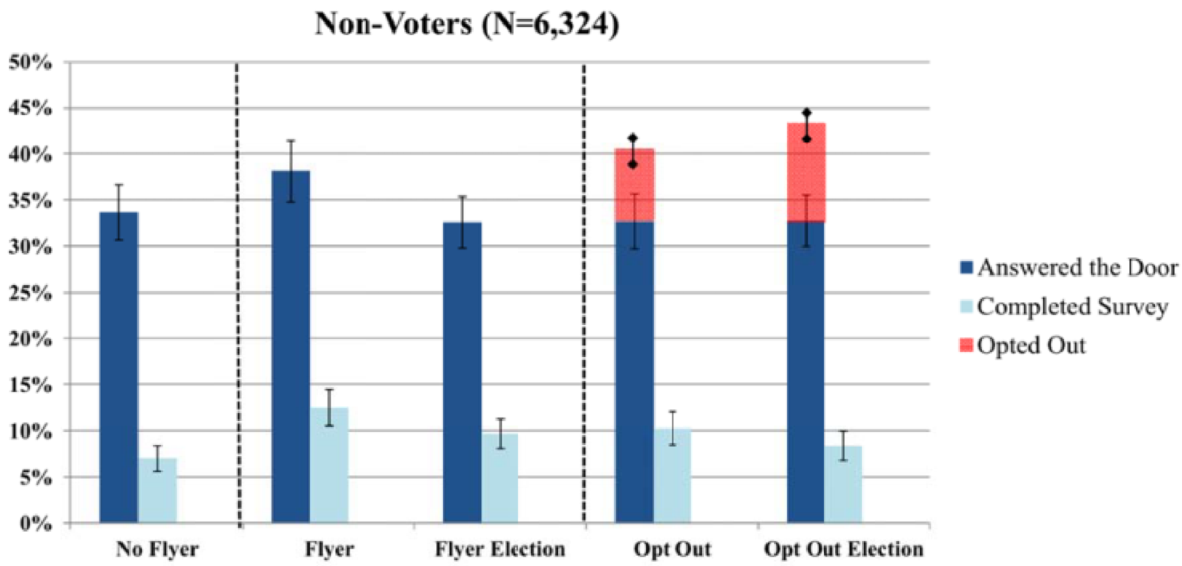
Figure 2.4: Response to Survey Duration and Payment



Note: Figure 5 presents the share of households answering the door and the (unconditional) share completing the survey across the three different combinations of payment and duration, separately for voting households and non-voting households. The averages are pooled across the different flyer treatments featured in Figure 4.

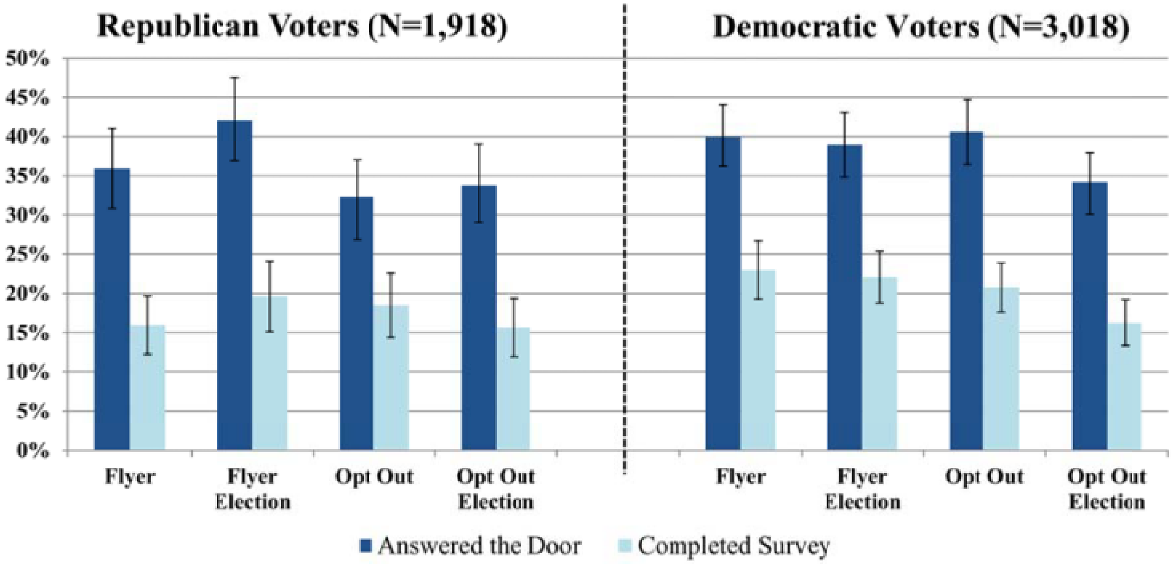
Figure 2.5: Response to Information about Election in Flyer





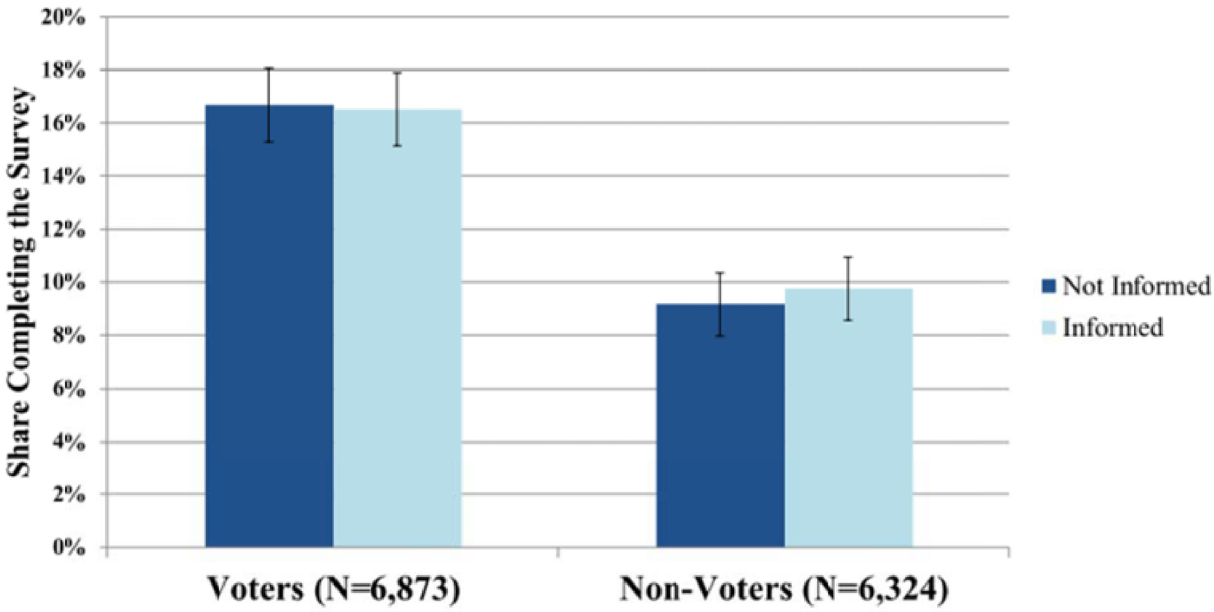
Note: Figure 6 presents the share of households answering the door, the (unconditional) share completing the survey, and (when applicable) the share opting out, separately for each of the five flyer treatments and separately for voting households and non-voting households. The averages are pooled across the three different payment and duration treatments featured in Figure 4.

Figure 2.6: Response to Flyer by Party Registration (for voters)



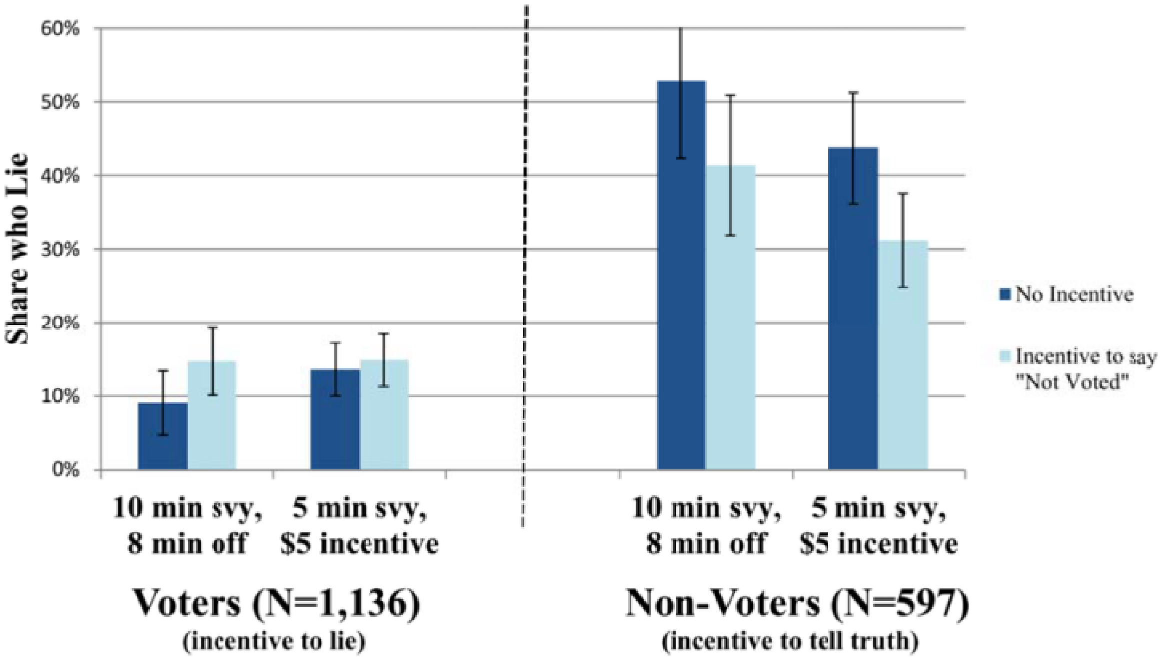
Note: Figure 7 presents the data from Figure 6a for *voting* households (omitting for space reasons the no-flyer treatment) split into two groups. In the left group, at least one household member voted at a Republican primary between 2004 and 2010. In the right group, at least one member voted at a Democratic primary between 2004 and 2010. Households with neither, or with voters participating in different party primaries, are not included.

Figure 2.7: Response to Announcement of Survey Content at Door



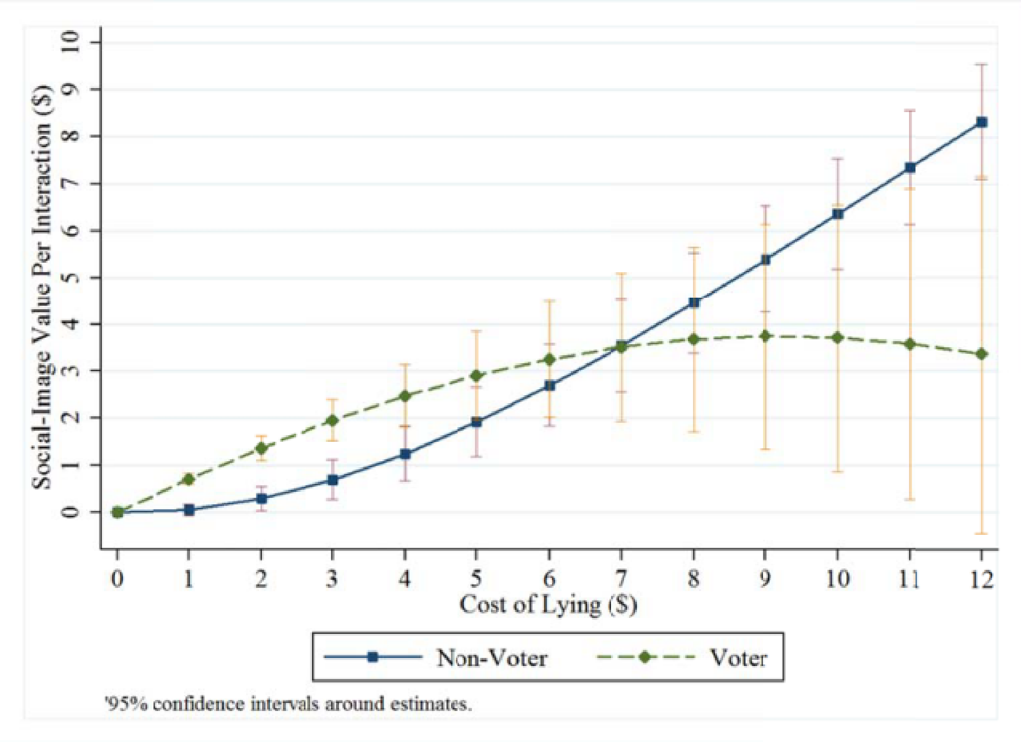
Note: Figure 8 presents the (unconditional) share of households completing the survey, separately for voting and non-voting households. The households in the *Not-Informed* treatment are not informed ex ante about the survey content at the door. The households in the *Informed* treatment are told at the door that the survey will be about their voter participation in the 2010 congressional election. The averages are pooled across the different flyer treatments featured in Figure 4.

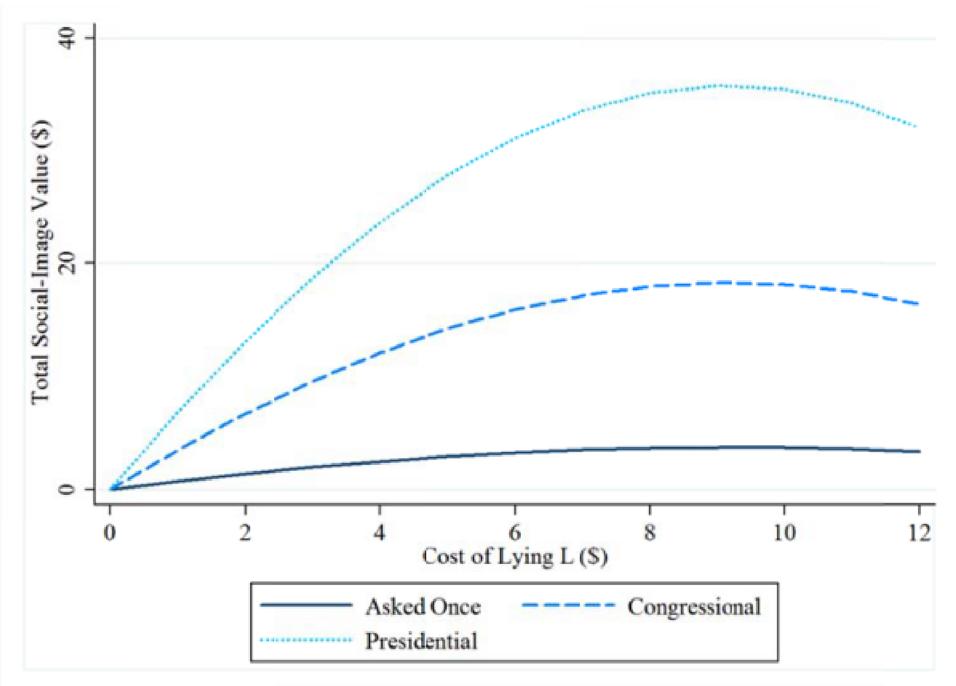
Figure 2.8: Response to Lying Incentives



Note: Figure 9 presents the share of households completing the survey whose response to the question "Did you vote in the 2010 congressional election?" differs from the official voting record, denoted as "Share who Lie." The shares lying are compared across treatments with an incentive to say that one did not vote to the treatments with no such incentive. The incentives are designed to induce voters to lie and non-voters to tell the truth. The averages are pooled across the different flyer and payment treatments featured in Figure 4. The sample sizes refer to the subsamples who answered the survey including the voting question.

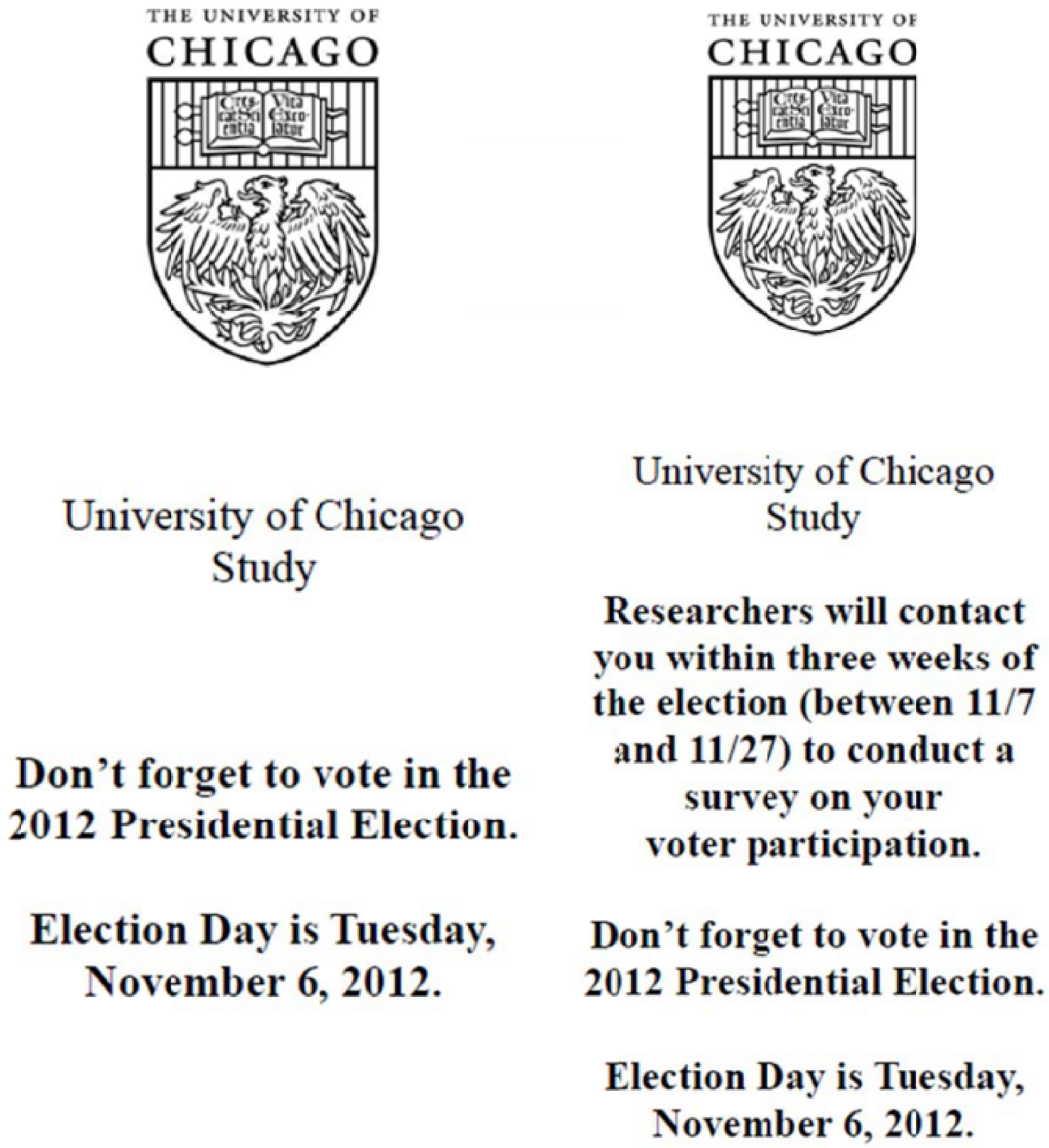
Figure 2.9: Social Image Value of Voting as a Function of Lying Cost





Note: Figure 10a plots the estimated value of voting due to being asked *once* for voters and non-voters at the benchmark estimate of the parameters, as a function of the lying cost L (which is not identified). The bars represent 95% confidence intervals. Figure 10b plots the same social-image value of voting for voters due to being asked once (same as Figure 10a), but also the total values for Presidential and Congressional elections. The latter estimates are the value due to being asked once, multiplied by the average number of times asked, taken from survey responses.

Figure 2.10: Flyer Samples for GOTV Treatment



Note: Figure 11 shows the door-knob flyers used in the Get-Out-The-Vote treatments in the days before the 2012 presidential election. The left flyer is for the treatment with Voting Reminder, the right flyer is for the treatment with Announcement Will Ask About Voting. Flyers for the 2010 election are similarly styled.

Table 2.1: Survey Treatments

Specification:	OLS Regressions							
Dependent Variable:	Indicator for Answering the Door				Indicator for Completing Survey			
Group:	Voters		Non-Voters		Voters		Non-Voters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.3458***		0.3206***		0.0909***		0.0457***	
	(0.017)		(0.017)		(0.012)		(0.010)	
$10/10min Treatment	0.0337**	0.0364**	0.0251	0.0243	0.0109	0.0132	0.0226***	0.0231***
	(0.016)	(0.015)	(0.016)	(0.015)	(0.010)	(0.010)	(0.008)	(0.009)
$10/5min Treatment	0.0515***	0.0596***	0.0227	0.0204	0.0602***	0.0683***	0.0465***	0.0467***
	(0.017)	(0.017)	(0.015)	(0.015)	(0.013)	(0.013)	(0.009)	(0.009)
Simple Flyer Treatments	0.0145	0.0128	0.0306	0.0286	0.0907***	0.0960***	0.0522***	0.0496***
	(0.018)	(0.018)	(0.019)	(0.018)	(0.013)	(0.013)	(0.010)	(0.010)
Flyer Treatments with Opt-out	-0.0195	-0.0232	0.0055	0.0052	0.0673***	0.0695***	0.0354***	0.0325***
	(0.019)	(0.019)	(0.018)	(0.018)	(0.013)	(0.013)	(0.010)	(0.010)
Mention of Election in Flyer	-0.0158	-0.0143	-0.0276**	-0.0278**	-0.0200*	-0.0194*	-0.0236***	-0.0238***
	(0.013)	(0.013)	(0.014)	(0.014)	(0.011)	(0.011)	(0.008)	(0.008)
Voters Informed at Door of Election Topic					-0.0024	0.0001	0.0042	0.0047
					(0.009)	(0.009)	(0.008)	(0.008)
Omitted Treatment	No Flyer, $0/5min Treatment				No Flyer, $0/5min, Not Informed Treatment			
Fixed Effects for Solicitor, Date-Location, and Hour		X		X		X		X
R2	0.0032	0.0279	0.0018	0.0338	0.0116	0.0350	0.0080	0.0269
N	6,873	6,873	6,324	6,324	6,873	6,873	6,324	6,324

Table 2.2: Lying Incentives

Specification:	OLS Regressions			
Dependent Variable:	Indicator for Lie (Stated Voting Does not Match Official Voting Record)			
Group:	Voters		Non-Voters	
	(1)	(2)	(3)	(4)
Panel A. All Survey Respondents				
Constant	0.1210***		0.4677***	
	(0.014)		(0.031)	
Time or Monetary Incentive To say Did Not Vote	0.0273	0.0225	-0.1204***	-0.1190***
	(0.020)	(0.019)	(0.040)	(0.040)
N	1,136	1,136	597	597
Panel B. $0, 5min. Treatments				
Constant	0.1479***		0.3971***	
	(0.028)		(0.061)	
5-Dollar Incentive to Say Did Not Vote	-0.0302	-0.0394	-0.0918	-0.1105
	(0.034)	(0.037)	(0.075)	(0.076)
N	329	329	163	163
Panel C. $10, 5min. Treatments				
Constant	0.1280***		0.4623***	0.3389**
	(0.024)		(0.046)	(0.152)
5-Dollar Incentive to Say Did Not Vote	0.0480	0.0550	-0.1452**	-0.1313**
	(0.036)	(0.034)	(0.059)	(0.065)
N	427	427	229	229
Panel D. $10, 10min. Treatments				
Constant	0.0909***		0.5281***	
	(0.022)		(0.053)	
8-Minute Incentive to Say Did Not Vote	0.0561*	0.0487	-0.1143	-0.0864
	(0.031)	(0.034)	(0.069)	(0.071)
N	380	380	205	205
Omitted Treatment	No incentive to say did not vote			
Fixed Effects for Location-Day		X		X

Table 2.3: Benchmark Minimum Distance Estimates

	Benchmark Estimates	
Voting Parameters	Voter	Non-Voter
Mean Value of saying voted	-5.86	-4.61
(μ_V for voters, μ_V-L for non-voters)	(1.94)	(2.4)
Mean Value of saying didn't vote	-24.81	-4.23
(μ_N-L for voters, μ_N for non-voters)	(5.14)	(2.2)
Std. Dev. Of s_V and s_N	12.35	6.20
	(3.1)	(1.29)
Total Signaling Value of Voting, as a Function of Lying Cost (times asked: voters=5.13, non-voters=6.03)		
L=0	0.00	0.00
L=2	6.84	1.75
L=5	14.60	11.53
L=10	19.13	38.25
Average Utility From Being Asked About Voting	-4.63	-0.92
	(1.98)	(2.15)
Auxiliary Parameters	Voter	Non-Voter
Mean Utility (in $) of Doing 10-Minute Survey	-25.01	-31.62
	(3.92)	(5.51)
Std. Dev. of Utility of Doing Survey	29.90	28.09
	(7.57)	(6.73)
Value of Time of One-Hour Survey	65.04	19.09
	(14.86)	(8.58)
Social Pressure Cost (in $) of declining survey	1.76	0.06
	(1.2)	(0.45)
Elasticity of Home Presence (η)	0.13	2.86
	(0.09)	(20.2)
Probability of seeing the flyer	0.38	0.30
	(0.02)	(0.02)
Baseline Probability of being home	0.38	0.36
	(0.01)	(0.01)
SSE	135.15	

Table 2.4: Survey Treatments

	Benchmark Model		Lying Rates Only		Sorting Only (Low Heterog.)		Sorting Only (High Heterog.)		No Survey Content Announcement at Door	
	(1)		(2)		(3)		(4)		(5)	
Voting Parameters	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter
Mean Value of saying voted	-5.86	-4.61	0.00	0.00	-5.25	-3.75	-8.7071	-19.791	-9.92	-13.60
(μ_V for voters, μ_V-L for non-voters)	(1.94)	(2.4)	(Assumed Value)		(3.02)	(1.74)	2.0275	2.4118	(3.46)	(4.48)
Mean Value of saying didn't vote	-24.81	-4.23	-22.68	0.72	-13.33	-3.17	-36.477	-17.887	-31.89	-12.41
(μ_N-L for voters, μ_N for non-voters)	(5.14)	(2.2)	(9.41)	(0.79)	(3.11)	(1.74)	2.9201	2.3937	(7.73)	(3.91)
Std. Dev. Of s_V and s_N	12.35	6.20	16.01	6.97	5.00	5.00	20.00	20.00	15.93	8.40
	(3.1)	(1.29)	(8.23)	(1.7)	(Assumed Value)		(Assumed Value)		(4.7)	(2.29)
Total Signaling Value of Voting, as a Function of Lying Cost										
L=0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
L=2	6.84	1.75	6.55	1.24	6.19	1.90	6.54	0.03	6.40	0.65
L=5	14.60	11.53	14.40	9.73	7.99	13.17	14.79	2.61	13.98	7.37
L=10	19.13	38.25	21.16	34.91	-8.29	41.55	23.69	13.32	20.15	29.71
Average Utility From Being Asked About Voting	-4.63	-0.92	Unidentified		-4.81	-0.63	-6.27	-7.53	-7.96	3.46
	(1.98)	(2.15)			(3.03)	(1.72)	(2.07)	(2.25)	(8.24)	(3.71)
Auxiliary Parameters	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter
Mean Utility (in $) of Doing 10-Minute Survey	-25.01	-31.62	.	.	-37.53	-30.72	-29.23	-34.75	-21.29	-22.36
	(3.92)	(5.51)			(9.5)	(4.28)	(5.64)	(6.44)	(2.87)	(3.12)
Std. Dev. of Utility of Doing Survey	29.90	28.09	.	.	51.77	26.31	35.55	31.63	25.07	20.50
	(7.57)	(6.73)			(18.42)	(4.92)	(10.26)	(8.01)	(5.4)	(3.64)
Value of Time of One-Hour Survey	65.04	19.09	14.63		80.44	30.15	94.68	35.17	78.36	27.01
	(14.86)	(8.58)	(10.41)		(18.15)	(15.33)	(23.37)	(25.29)	(16.09)	(11.28)
SSE	135.15		5.32		121.64		154.78		103.25	

Table 2.5: Survey Treatments

	Benchmark Model: Sorting and Lying		Allowing Utility from Talking About Politics		Assuming 10% Non-Registered in Voting Households		Assuming 10% Misclassified	
	(1)		(2)		(3)		(4)	
Voting Parameters	Voter	Voter	Voter	Voter	Voter	Non-Voter	Voter	Non-Voter
Mean Value of saying voted	-5.86	-4.61	0.00		-5.18	-4.52	-5.13	-4.08
(μ_V for voters, μ_V-L for non-voters)	(1.94)	(2.4)	(Assumed)		(1.95)	(2.43)	(1.92)	(2.77)
Mean Value of saying didn't vote	-24.81	-4.23	-18.99	0.39	-27.46	-4.06	-26.79	-2.59
(μ_N-L for voters, μ_N for non-voters)	(5.14)	(2.2)	(3.79)	(0.64)	(6.19)	(2.18)	(5.73)	(2.47)
Std. Dev. Of s_V and s_N	12.35	6.20	12.38	6.21	11.40	6.27	10.52	5.63
	(3.1)	(1.29)	(3.06)	(1.29)	(3.29)	(1.27)	(2.89)	(1.25)
Utility of Talking about Politics			-5.87	-5.39				
			(1.94)	(2.4)				
Total Signaling Value of Voting, as a Function of Lying Cost (times asked: voters=5.13, non-voters=6.03)								
L=0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
L=2	6.84	1.75	6.84	1.75	8.11	1.65	8.34	0.61
L=5	14.60	11.53	14.61	11.52	18.17	11.24	18.69	9.67
L=10	19.13	38.25	19.17	38.24	26.97	37.80	27.40	36.48
Average Utility From Being Asked	-4.63	-0.92	-4.61	-0.91	-4.57	-0.75	-7.48	-0.16
About Voting	(1.98)	(2.15)	(1.98)	(2.14)	(2.)	(2.12)	(4.13)	(2.3)
Survey Parameters	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter	Voter	Non-Voter
Value of Time of One-Hour Survey	65.04	19.09	65.14	19.13	76.13	17.11	75.91	13.39
	(14.86)	(8.58)	(14.68)	(8.58)	(16.75)	(8.63)	(16.14)	(9.46)
SSE	135.15		135.15		132.31		132.63	

Table 2.6: Survey Treatments

	Assuming Voters = Non-Voters		Assuming Voters = Non-Voters Partially		Voters and Non-Voters Different	
	(1)		(2)		(3)	
Voting Parameters	Voters = Non-Voters		Voters = Non-Voters		Voters	Non-Voter
Mean Utility from *truthfully* saying voted (μ_V)	-1.06		-2.34		-4.85	1.06
	(3.13)		(2.75)		(4.14)	(4.76)
Mean Utility from *truthfully* saying didn't vote (μ_N)	-5.56		-6.79		-12.45	-4.12
	(3.21)		(2.9)		(6.93)	(4.64)
Cost of Lying (L)	4.63		5.17		5.00	5.00
	(1.08)		(1.29)		(Assumed)	(Assumed)
Std. Dev. Of Sv and Sn	7.34		8.68		12.68	5.93
	(1.62)		(2.05)		(4.86)	(1.47)
	Voters	Non-Voters	Voters	Non-Voters	Voters	Non-Voters
Total Signaling Value of Voting (times asked: voters=5.50, non-voters=5.85)	8.28	8.80	7.82	8.32	8.92	12.88
Average Utility From Being Asked About Voting	0.02	-1.48	-0.82	-2.25	-2.31	-0.69
Survey Parameters	Voters = Non-Voters		Voters	Non-Voter	Voters	Non-Voter
Mean Utility (in $) of Doing 10-Minute Survey	-46.62		-34.81	-53.98	-37.78	-47.69
	(12.99)		(10.14)	(21.13)	(12.08)	(21.85)
Std. Dev. of Utility of Doing Survey	51.63		34.73	61.14	38.30	52.08
	(17.49)		(14.42)	(29.85)	(16.88)	(29.13)
Value of Time of One-Hour Survey	28.85		55.09	12.58	70.92	15.36
	(9.99)		(18.72)	(17.12)	(28.42)	(11.53)
SSE	132.49		100.79		97.20	

Table 2.7: Survey Treatments

Specification:	OLS Regressions			
Dependent Variable:	Indicator for Voting			
Election:	Congressional Elections in Nov. 2010		Presidential Elections in Nov. 2012	
	(1)	(2)	(3)	(4)
Constant	0.6000***		0.7312***	
	(0.0109)		(0.0033)	
Flyer with Voting Reminder	-0.0020	-0.0031	0.0060	0.0046
	(0.0152)	(0.0083)	(0.0056)	(0.0034)
Flyer with Announcement Will Ask About Voting	0.0120	0.0102	0.0023	0.0056
	(0.0157)	(0.0084)	(0.0056)	(0.0034)
Omitted Treatment	**No Flyer**		**No Flyer**	
Control for past Voting since 2004		X		X
Difference (Flyer Will Ask - Flyer Reminder)	0.0140	0.0133	-0.0037	0.0010
p-value for test of equality, 2-sided	p=0.365	p=0.120	p=0.561	p=0.811
p-value for test of equality, 1-sided	p=0.182	p=0.060*		p=0.405
R2	0.0001	0.4024	0.0000	0.3251
N	N = 31,306	N = 31,304	N = 93,805	N = 93,805

Chapter 3

Familiarity Does *Not* Breed Contempt

3.1 Introduction

Schools are de facto segregated across social and economic lines in many countries. Much research has examined the effects of such segregation on learning outcomes.[1] But desegregation and affirmative action efforts have historically been motivated not only by concerns about disparities in educational outcomes, but also by the argument that diversity in schools benefits society by positively influencing inter-group attitudes and social behavior (Schofield 1996). Yet empirical evidence on such effects is exceedingly scarce. More generally, little is known about how social preferences and behaviors are shaped, and whether they can be influenced by policy. This question is of particular interest in diverse and polarized societies, where the costs of social divisions are well documented.[2]

I focus on a particular dimension of diversity - economic status - and seek to answer the following question: What effect do peers from poor households have on students from relatively wealthy families? I use data on about two thousand students in fourteen schools in Delhi, India to measure the following outcomes: (i) generosity and prosocial behavior; (ii) tastes for socially interacting with or discriminating against the poor; and (iii) learning and classroom behavior.

My first econometric strategy exploits the plausibly exogenous staggered timing of a policy change that required elite private schools to offer places to poor students. This

[1] Buchmann and Hannum (2001) and Karsten (2010) report measures of educational segregation or stratification in a number of countries. Hattie (2002) and Van Ewijk and Sleegers (2009) provide meta-analyses of the effects of segregation on inequalities in learning.

[2] A large empirical and theoretical literature links greater social diversity, inequality and polarization with conflict (Esteban and Ray 2011), collective action problems (Bardhan et al. 2007), low levels of public good provision (Miguel and Gugerty 2005), political instability (Alesina and Perotti 1996) and diminished economic growth (Easterly and Levine 1997).

causes a sharp discontinuity across cohorts in the presence of poor students. In most schools, cohorts beginning schooling in 2007 or later have many poor students, while older cohorts are comprised exclusively of rich students. However, a small control group (about 4%) of elite private schools are entirely exempt from the policy for historical reasons, while another handful (6%) of schools complied with the policy a year late - in 2008 instead of 2007. I can therefore identify the effect of the presence of poor students (the "treatment") by comparing both within schools (comparing treated and untreated cohorts) and within cohorts (comparing treated and untreated schools) using a difference-in-differences regression model. This approach identifies the average effect on wealthy students of adding poor children to their classroom - an important estimate for policy.

The second econometric strategy isolates the role of personal interactions between rich and poor students by exploiting idiosyncratic variation in peer groups *within* the classroom. Some schools in the sample use alphabetic order of first name to assign students to group-work and study partners. In these schools, the presence of a poor child with a name alphabetically close to a given rich student provides plausibly exogenous variation in personal interactions with a poor student. This allows me to distinguish between changes occurring due to personal interactions between students, and the effects of other possible changes at the classroom level, say in teacher behavior or curriculum.

My first finding is that having poor classmates makes students more prosocial, as measured by their history of volunteering for charitable causes at school. The schools in my sample occasionally offer students opportunities to volunteer or fundraise for affiliated charities. One such activity involves attending school on two weekend afternoons to help fundraise for a charity for disadvantaged children. I collect attendance records from such events and find that having poor classmates increases the share of volunteers by 10 percentage points (se 2.5%) on a base of 24%, while having a poor study partner increases volunteering by an imprecisely estimated 13 percentage points (se 9%).

The second finding is that economically diverse classrooms cause wealthy students to discriminate less against other poor children outside school. I measure discrimination using data on team choice in a sports contest. I find that when the stakes are high - Rs. 500 ($10), about a month's pocket money for the older students - only 6% of wealthy students discriminate by choosing a slower rich student over a faster poor student. As the stakes decrease, however, I observe much more discrimination. In the lowest stakes condition (Rs. 50), almost a third of students discriminated against the poor. But past exposure to poor students reduces discrimination by 12 percentage points. I structurally estimate a simple model of taste-based discrimination and find that wealthy students dislike socially interacting with a poor teammate relative to rich one by an average of Rs. 34, about two days worth of pocket money. Having poor classmates reduces this distaste by 30%.[3]

[3]The observed behavior is inconsistent with a simple model of statistical discrimination. When students are asked which of the prospective teammates is most likely to win the relay race, they invariably (98%) select the fastest student in the ability revelation round, similarly across treated and control classrooms. This implies that a substantial number of students chose a rich teammate despite believing that

Having established the effects of having poor classmates on social preferences and behaviors, I turn attention to impacts on learning and classroom discipline. A traditional concern with integrating disadvantaged students into elite schools is the potential for negative peer effects on academic achievement. To evaluate this concern, I conduct tests of learning in English, Hindi and Math, and collect teacher reports on classroom behavior. I detect a marginally significant but meaningful decrease of 0.09 standard deviations in wealthy students' English language scores, but find no effects on their Hindi or Math scores, or on a combined index over all subjects. This pattern of findings is consistent with the measured achievement gap between poor and wealthy students, which is largest in English – perhaps because wealthy students are more likely to speak English at home. And while teachers do report higher rates of disciplinary infractions by wealthy students in treated classrooms, the increase comprises entirely of the use of inappropriate language (that is, swearing), as opposed to disruptive or violent behavior. My third finding is thus of negative but arguably modest effects on academic achievement and discipline.

For each of the outcomes above, I compare the effects of the two types of variation: across-classroom variation in the presence of poor students, and within-classroom variation driven by assignment to study groups. This sheds light on mechanisms underlying the results by teasing apart the effect of direct personal interactions from the impact of other changes such as those in teacher behavior or curriculum. I find that personal interactions are an important driver of the overall effects. For example, having a poor study partner alone explains 70% of the increase in "willingness to play" with a poor child. This likely underestimates the importance of personal interactions, since students surely also interact with other poor classmates outside their study groups.

This paper relates to four bodies of work in economics. First, a recent literature studies whether interaction reduces inter-group prejudice. Most closely related are Boisjoly et al. (2007) and Burns et al. (2013), who find that being randomly assigned a roommate of a different race at college increases inter-racial social interactions in later years.[4] Second, this paper relates to research on the effects of desegregation and (more generally) peer effects in education. Evidence on peer effects in learning is mixed, with impacts on non-academic outcomes such as church attendance and drug and alcohol use a more robust finding (see Sacerdote 2011 for a review). Consistent with this, I find substantial effects on prosocial behavior and discrimination, but mixed and overall modest effects on test scores. A third connection is to the growing literature on how distributional social preferences are shaped, for example by exposure to violent conflict (Voors et al. 2012) and the ideology of one's college professors (Fisman et al 2009). I add to this literature by showing that peers at school can also shape social preferences. Finally, this paper relates to research

he is *less* likely to help them win the race. This suggests that taste-based discrimination dominates in this setting.

[4]Outside of economics, a long tradition of related work in social psychology following Allport (1954) generally documents a negative correlation between inter-group contact and prejudice, but suffers from both selection problems and a reliance on stated attitudes rather than observed behavior as outcome measures (Pettigrew and Tropp 2006).

on the economics of discrimination. I contribute to this literature by showing evidence of taste-based discrimination in an experiment (albeit in a non-market setting amongst students), and more importantly by showing that past exposure to out-group members causally reduces such discrimination.[5]

My findings are also of relevance to policy makers: the policy I study will shortly be extended throughout India under the Right To Education Act, with consequences for many of India's 400 million children. This policy is controversial, with legal battles over its legitimacy reaching India's Supreme Court. Opponents have prominently argued that any gain for poor children will come at a substantial cost to the existing clientele of private schools. Proponents have responded that diversity will benefit even rich students by providing them with "a clearer idea of the world".[6] While we must be cautious in extrapolating from elite private schools in Delhi to the rest of India, my findings provide some support for each side of the debate. A radical increase in diversity in the classroom did have modest negative impacts on the academic achievement and behavior of advantaged students. But it also made them substantially more generous and prosocial, more willing to socially interact with poor children, and less likely to discriminate against them. A full accounting of the effects of economic diversity in schools on privileged students should consider all these effects.

The rest of this paper is organized as follows. In Section 2, I describe the policy change underlying the natural experiment. Section 3 discusses the two econometric strategies and addresses possible challenges to identification. Section 4 reports impacts on the first class of outcomes, prosocial behavior and generosity. Section 5 describes the experiments and results relating to discrimination and social interaction. Section 6 reports effects on learning and discipline. Section 7 briefly interprets and contrasts the key results, drawing out the main themes and clarifying this paper's contributions. Section 8 summarizes and discusses shortcomings and avenues for future research.

3.2 Background and Policy Experiment

In this section, I describe a policy change which forced most elite private schools in Delhi to offer places to poor children, thus integrating poor and wealthy students in the same classrooms. I briefly describe how the timing of the policy change varied across schools, as well as key features of the selection process for both poor and wealthy students. In particular, poor students are selected using randomized lotteries, while wealthy applicants are selected using a transparent scoring system, which does not allow the use of baseline test scores or ability measures.

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[5] Scholars have investigated the extent to which discrimination exists and matters in the labor market using audit studies (Bertrand and Mullainathan 2004), quasi-experiments (Goldin and Rouse 2000) and experiments (List 2004, Mobius and Rosenblat 2006).

[6] The Indian Express, April 13 2013. http://www.indianexpress.com/news/learning-curve/936084/

Delhi - like most cities in India - has a highly stratified school system. Public schools and a growing number of low-fee private schools serve the large population of urban poor. Relatively expensive 'elite' private schools cater to students from wealthy households.[7] These types of schools differ widely in affordability, school inputs and acceptance rates. Public schools are free, and students are typically guaranteed admission to at least one public school in their neighborhood. In contrast, elite private schools as I define them charge tuition fees in excess of Rs. 2000 per month (approximately $40, 25% of median monthly household consumption in 2010), and are vastly over-subscribed. Private schools in my sample report average acceptance rates of 11%, and monthly fees of up to Rs. 10,000.[8]

Policy Change. Many private schools in Delhi – including over 90% of the approximately 200 elite private schools – exist on land leased from the state in perpetuity at highly subsidized rates. A previously unenforced part of the lease agreement required such schools to make efforts to serve "weaker sections" of society. In 2007, prompted by the Delhi High Court, the Government of Delhi began to enforce this requirement. It issued an order requiring 395 private schools to reserve 20% of their seats for students from households earning under Rs. 100,000 a year (approximately $2000). Schools were not permitted to charge the poor students any fees; instead, the government partially compensated the schools. Decades after most of these private schools were founded, the policy change forced open their doors to many relatively poor children.

Two features of the policy change are particularly important for my analysis: (i) schools were not permitted to track the students by ability or socioeconomic status. Instead, they were required to integrate the poor students into the same classrooms as the rich, and (ii) the policy only applied to new admissions, which occur almost exclusively in the schools' starting grades (usually preschool). Thus, the policy did not change the composition of cohorts that began schooling before 2007.

Variation in timing. I divide elite private schools into three categories based on their response to the policy change. (i) *Treatment schools* were subject to the policy, and complied with it in the very first year. In these schools, cohorts admitted in 2007 or later have many poor students, while older cohorts comprise exclusively of wealthy students. This group includes about 90% of all elite private schools. (ii) *Delayed treatment schools* were also subject to the policy, but failed to comply in the first year - either because they expected the policy to be overturned in court, or because they felt the order was issued too late for them to modify their admission procedures. These schools complied with the policy a year later, in 2008, following a court ruling upholding the policy. This group comprises about 6% of all elite private schools. *Control schools* are the 4% of elite private schools which were not subject to the policy at all, typically because they were

[7] A loosely defined middle class typically sends it's children to private schools intermediate in their price and exclusivity to public and elite private schools.

[8] Parents of the wealthy students in the elite private schools I study apply to 8.8 schools on average and are offered admission to 1.8 of them. An article in the Indian Express memorably lamented that gaining admission to preschool in an elite private school in Delhi is harder than getting into Harvard.

built on land belonging to private charitable trusts or the federal government instead of the state government. In control schools, therefore, all cohorts comprise exclusively of rich students. The important point, discussed in detail in the next section, is that while schools are not randomly assigned to treatment, delayed treatment and control status, variation in the presence of poor children exists both within schools (across cohorts) and within cohorts (across schools).

Selection of Poor Students. If the seats for poor children are over-subscribed, schools are required to conduct a lottery to select beneficiaries. Conditional on applying to a given school, poor students are thus randomly selected for admission. While applications are free, they do involve the time costs of filling out and submitting the application form, and of obtaining documentation of income. Within the universe of eligible households, applicants are thus likely to be positively selected on their parents' preferences for their education, knowledge of the program, and their ability to complete the necessary paperwork. Since the children themselves are between 3 and 4 years of age when applying, it is less likely that their own preferences are reflected in the decision to apply.

The key point for this paper is that while the poor students may not be a representative sample of poor children in Delhi, they are without doubt from a very different economic class than the typical wealthy student in an elite private school. Figure 3.1 shows that the income cutoff of Rs. 100,000 per year is around the 45^{th} percentile of the household income distribution, and the average poor student in my sample is from the 25th percentile. In contrast, the typical rich student in the sample is from well above the 95th percentile of the consumption distribution. In the US, a corresponding policy change would see students from households making $23,000 a year attend the same schools as those making $200,000 a year.

Selection of wealthy students. The admissions criteria used by elite private schools to select wealthy (fee-paying) students are strictly regulated by the government, and publicly declared by the schools themselves. Schools rank applicants using a point system, with the greatest weight placed on distance to the applicant's home and whether an older sibling is already enrolled in the school. Other factors include a parent interview, whether parents are alumni, and gender (a slight preference is given to girls). Importantly, schools are prohibited from interviewing or testing students before making admissions decisions. Thus, it is difficult for schools to screen applicants on ability.[9] The overwhelming majority of admissions to elite private schools occur in preschool (students aged 3-4), which is the usual starting grade. New students are typically only admitted to higher grades when vacancies are created by transfers, which are rare: 1.7% per year in my sample.

[9]Schools may, of course, use parent interviews to judge the ability of applicants. But parents cannot easily provide schools with credible information about student ability in the interviews, since the child is typically under 4 years of age and has no prior schooling.

3.3 Econometric Strategies

The data includes 2032 students in 14 elite private schools in Delhi. The sample consists of 9 treatment schools, 2 delayed treatment schools and 3 control schools, recruited as part of a larger project studying a variety of learning and behavioral outcomes. I restrict attention to students in the four cohorts who began preschool between 2005 and 2008, stratified by classrooms within schools. Given the timing of the policy change, these students were in grades 2 (cohort of 2008) through 5 (cohort of 2005) when the data was collected.

Using this data, I exploit two types of variation to identify the effects of poor students on their rich classmates: whether or not poor students are present in a particular cohort and school, and idiosyncratic variation in interactions with poor students within the classroom.

3.3.1 Variation within schools and cohorts

The first approach identifies the average effect of having poor students in one's classroom. Recall that in treatment schools, wealthy students in grades 2 and 3 are "treated" with poor classmates, while grades 4 and 5 have no poor students. In delayed treatment schools, only grade 2 is treated, while grades 3-5 are untreated. And in control schools, grades 2-5 are all untreated. Restricting the sample to rich students, I estimate the following difference-in-differences specification by OLS:

$$Y_{igs} = \alpha + \delta_s + \phi_g + \beta \text{TreatedClassroom}_{gs} + \gamma X_{igs} + \varepsilon_{igs} \tag{3.1}$$

where Y_{igs} denotes outcome Y for student i in grade g in school s; X is a vector of controls, δ_s are school fixed effects, ϕ_g are grade or cohort fixed effects and ε_{igs} is a student specific error term. $\text{TreatedClassroom}_{gs}$ is the treatment indicator; it equals one if grade g in school s contains poor students, and is zero otherwise. β is thus the average effect of having a poor classmate, and is the key parameter to be estimated. I cluster standard errors at the grade-by-school level, since this is the unit of treatment. With 14 schools and 4 grades (2 through 5) per school in the sample, this results in 56 clusters. As a robustness check, I also cluster standard errors at the school level. Given the small number of schools ($k = 14$), I use the wild-t cluster bootstrap method of Cameron, Gelbach and Miller (2008).

Note that average differences in outcomes across schools are permitted; they are controlled for by the school fixed effects. Thus, I do not assume that treatment, delayed treatment and control schools would have the same average outcomes without treatment. Similarly, average differences across cohorts (or grades) are controlled for using cohort fixed effects. This is important, given the possibility of age effects in social behaviors and preferences, as shown by Fehr et al. (2008) and Almas et al. (2012). I only utilize variation within schools (comparing students in different cohorts) and within cohorts (comparing students in different schools).

The identifying assumption is that in the absence of treatment, the *gaps* in outcomes across the different types of schools would be the same across treated and untreated grades. This would be violated if, for example, even in the absence of the policy, treatment schools had (say) better teachers than control schools in grades 2 and 3, but not in grades 4 and 5.

Challenges to Identification. This identification strategy faces the following potential challenges, each of which I briefly address below. (i) Wealthy students may select into control schools based on their affinity for poor children. (ii) Treatment and delayed treatment schools have fewer seats for wealthy students after the policy change, which might increase the average ability of admitted students. (iii) There may be spillovers between treated and untreated grades within treated schools, and (iv) The policy may cause an increase an class size, which could directly affect outcomes.

The concern most relevant to estimating effects on social outcomes is that students might sort across the different types of schools based on their affinity for poor children.[10] In practice, this mechanism is of limited concern for the following reasons. First, it is difficult for parents to be picky, since acceptance rates at elite schools are low (about 10%) and less than 5% of such schools are control schools. Transfers between elite schools are also rare; control schools report very few open spaces in grades other than preschool each year.[11] Second, as a robustness check, I can restrict attention to students who had older siblings already enrolled in the same school. These students are likely to be less selected, both because parents might prefer to have both children in the same school, and because younger siblings of a current student are much more likely to be offered admission to the school. I show that none of the main results substantially change when restricting the sample in this way. Finally, the second identification strategy I describe below is entirely exempt from this concern, since it does not rely on variation across schools.

The main concern with estimating effects on academic outcomes is that the policy change may force treatment schools to become more selective when admitting wealthy students. And indeed, while the share of poor students in the incoming cohorts is around 18%, total cohort size only increases by 5%.[12] This implies that fewer wealthy students are accepted into treated private schools after the policy change. If schools select students based on academic ability, this would mechanically raise the average quality of admitted wealthy students, and bias my estimate of the effect on learning outcomes. I can deal with this concern as above - by restricting attention to the less-selected younger siblings of previously enrolled students, and by relying on the second identification strategy. However, it is also worth emphasizing that the schools are prohibited from testing or interviewing prospective students in starting grades. Since preschool applicants are between 3 and 4 years old, schools also have no prior test scores available while making their decisions. Thus, it is difficult for schools to screen applicants based on ability.

Spillovers between grades are likely minimal, since students spend over 85% of the school day exclusively with their assigned classmates, and little time interacting with

[10]For example, a parent who particularly dislikes the thought of his son sitting next to a poor child might try extra hard to have him enroll in one of the few control schools. Or students who find that they particularly dislike their poor classmates might transfer to a control school in later years.

[11]Additionally, I find that the number of applications to control schools relative to treatment schools does not increase after the policy change, suggesting that the policy change did not increase overall demand for the control schools amongst wealthy parents.

[12]The target of 20% reservation was not always met in the early years of the program.

students in other classrooms of the same grade, let alone students in other grades. To the extent that any spillovers do exist, they would bias against finding effects. And finally, class sizes increase by only 5% after the policy change. It is therefore unlikely that changes in class size could be important drivers of any effects.

The econometric strategy described above identifies the overall effect on wealthy students of having poor students integrated into their classrooms. This effect would be one important input to any evaluation of the costs and benefits of such programs. However, it tells us little about the mechanisms underlying any effects. In particular, it does not separate the effect of increased personal interactions between rich and poor students from other plausible classroom-level changes such as teachers spending more time teaching students about inequality and poverty.

3.3.2 Idiosyncratic variation within classrooms

The second approach uses membership in the same "study groups" as a proxy for personal interactions between students. Students in my sample spend an average of an hour a day engaged in learning activities in small groups of 2-4 students. Examples of such activities include collaborative craft projects, role playing exercises, and recitation. I collect data on study group membership in each school, and determine whether each student i has *any* poor children in his study group. I denote this binary measure by $HasPoorStudyPartners_i$.

In 8 of the 14 schools, students are assigned to study groups by alphabetic order of first name ($SchoolUsesAlphaRule = 1$). In the remaining schools, groups are either frequently reshuffled by teachers, or no systematic assignment procedure is used ($SchoolUsesAlphaRule = 0$). I obtain class rosters, and sort them alphabetically to compute whether each student i is immediately followed or preceded by a poor student. I denote this measure by $HasPoorAlphabeticNeighbor_i$. I then estimate the following regression by two-stage least squares:

$$Y_{icgs} = \alpha + \nu_{cgs} + \beta_1 \text{HasPoorStudyPartners}_i + \gamma X_i + \varepsilon_{igs} \tag{3.2}$$

where Y_{icgs} denotes outcome Y for student i in classroom c in grade g in school s, ν_{cgs} is a classroom fixed effect, and $\text{HasPoorStudyPartners}_i$ is instrumented for using $SchoolUsesAlphaRule_s * HasPoorAlphabeticNeighbor_i$.

This identification strategy isolates the effect of personal interactions between rich and poor students. Identification comes entirely from within treated classrooms in treatment and delayed treatment classrooms, and average differences across classrooms (or schools and cohorts) are controlled for using classroom fixed effects. Thus, this strategy is not subject to concerns about the sorting of wealthy students across different types of schools, or changes in class size or teacher behavior.

Note that this approach does not require that poor and rich students have a similar alphabetic distribution of names. Nor do I assume that a rich student's first name has no direct effect on his outcomes. Instead, I only utilize variation in study groups predicted by the differential effect of alphabetic ordering in schools which use versus do not use such ordering to assign study groups. In the most aggressive specification, the individual level controls include first letter of first name, and thus directly control for any average differences across alphabetic order of names.

Figure 3.2 graphically reports the first stage of this regression. It shows that in the schools which report using alphabetic order to assign study groups, having a name alphabetically adjacent to at least one poor student substantially increases the probability of having at least one poor study partner, from about 40% to 90%. In contrast, alphabetic adjacency has no effect in other schools. Table 3.1 provides the first stage regression, and reports that the instrument is strong, with an F-statistic of over 40.

3.4 Prosocial Behavior

Common sense and empirical evidence suggest that human beings care about others, and about fairness. Economists have argued for the importance of such "social preferences" in domains including charitable donations (Andreoni 1998), support for redistribution (Alesina and Glaeser 2005), voter turnout (Edlin et al. 2007), and labor markets (Akerlof 1984, Bandiera et al. 2005). Researchers have measured social preferences in the field using behaviors like charitable giving and public goods provision (DellaVigna 2009), and in the lab using dictator games (Kahneman et al. 1986), where the participant (the "dictator") typically decides how to split a pot of money between himself and an anonymous recipient.[13]

Recently, scholars have begun to investigate how social preferences are shaped by life experiences, including education (Jakiela et al. 2010), the ideology of one's college professors (Fisman et al. 2012), political violence (De Voors et al. 2012) and macroeconomic conditions (Fisman et al. 2013). Researchers have also begun to trace the emergence of social preferences in children, where egalitarian preferences are seen to emerge around age 4-8 (Fehr et al. 2008), while more sophisticated notions of fairness emerge in adolescence (Almas et al. 2010).

In this section, I estimate how having poor classmates affects the prosocial behavior of wealthy students.I begin by studying prosocial behavior in a setting familiar to students in elite private schools in Delhi. All the schools in my sample provide students with occasional opportunities to volunteer for charities. One such activity common across the schools involves spending two weekend afternoons in school to help fundraise for a charity serving disadvantaged children. The task itself is to help make and package greeting

[13]More sophisticated versions of dictator games might vary the identity of the recipient (Hoffman et al. 1996) or vary the exchange rate at which money can be transferred between the dictator's endowment and the recipient (Andreoni and Miller 2002). Choices made in such lab games have been shown to predict real-world behavior such as charitable donations (Benz and Meier 2008), loan repayment (Karlan 2005) and voting behavior (Finan and Schechter 2012).

cards, which are then sold to raise money for the charity. Participation in these events is strictly voluntary; only 28% of students choose to attend. Volunteering activities thus serve as a natural real-world measure of prosocial behavior.

I collect administrative data on attendance at these volunteering events, and apply both econometric strategies described in the previous section to identify the effects of poor students on their wealthy classmates. Figure 3.3a graphically depicts the difference-in-differences strategy, plotting the share of students volunteering by grade and school type. The graph shows that wealthy students in grades 4 and 5 - which have no poor students - have similar volunteering rates across the three types of schools (control, treatment and delayed treatment). This suggests that the control schools are not especially bad at teaching prosocial behavior; in cohorts unaffected by the policy change, all the schools have similarly prosocial students. However, wealthy students in treatment schools volunteer substantially more in grades 2 and 3 - precisely the grades which contain poor students. The same pattern is evident for delayed treatment schools, which are only treated in grade 2. This pattern of volunteering behavior suggests that it is having poor classmates that causes the increase in wealthy students' prosocial behavior.

Figure 3.3b shows the effect of having a poor study partner, and conveys the essence of the instrumental variable strategy. It shows the share of volunteers, separately by whether or not the wealthy student's name is alphabetically adjacent to at least one poor student in his class roster. The graph shows that wealthy students with names adjacent to a poor student are more likely to volunteer for the charity - but only in those schools which use alphabetic order to assign study groups. This result suggests that it is having a poor student in one's study group - and therefore personally interacting with a poor student - that causes an increase in prosocial behavior.

The regression results in Table 3.2 confirm these findings, and attach a precise magnitude to the effects. Column 1 reports the main difference-in-differences estimate and shows that having poor classmates increases volunteering by 10 percentage points (se 2.4), an increase of 43% or 0.25 standard deviations over the volunteering rate in control classrooms. The effect remains highly significant ($p < 0.01$) when standard errors are clustered at the school level using the wild cluster bootstrap method of Cameron et al. (2008). Column 2 reports the same specification estimated on the restricted sample of students who have older siblings in the same school. The results are similar and not statistically different: an increase in volunteering of 7 percentage points (se 3.0). Column 3 reports the instrumental variable estimate of the effect of having a poor study partner. It shows that having at least one poor study partner causally increases volunteering by 13 percentage points (se 8.6), an imprecisely estimated increase of 53% over students without any poor study partners.

3.5 Social Interactions and Discrimination

Discrimination is a pervasive and important phenomenon in labor markets (Goldin and Rouse 2000, Bertrand and Mullainathan 2004), law enforcement (Persico 2009), residential location choice (Becker and Murphy 2009) and other contexts. Theories of discrimination are of two main types: taste-based discrimination, reflecting an innate animosity towards individuals from a particular group (Becker 1957), and statistical discrimination, which results from imperfect information about productivity or ability (Phelps 1972, Arrow 1973, Aigner and Cain 1977).

Tastes for social interactions provide a natural foundation for taste-based discrimination. But social interaction models also explain features of residential patterns (Schelling 1971), collective action (Granovetter, 1978), job search (Beaman and Magruder 2012) and the marriage market. Changes in willingness to interact with members of other social groups are therefore a potentially important outcome of diversity in schools. Indeed, theory suggests that even small changes in these tastes can lead to large differences in aggregate outcomes, since social interaction models often feature multiple equilibria (Card et al. 2008).

In this section, I estimate how having poor classmates in school affects rich students' willingness to socially interact and work with other poor children in teams, or conversely to discriminate against them. I use novel data on team composition for a sports event with varying rewards for winning to measure these outcomes.

3.5.1 Discrimination in Team Selection

Design. The main idea is to study the tradeoff for wealthy students between choosing a high ability teammate (and thus increasing their own expected payoff) versus choosing a lower-ability teammate with whom they would prefer to socialize. The team task I consider is a relay race, in which ability is easily revealed through times in individual sprints. In addition to running the relay race together, participants in the sports contest were required to spend time socializing with their teammates.

The data consists of team composition of students from two elite private schools at a sports meet - one a treatment school, and the other a control school. In addition to students from the two elite private schools, selected athletic students from a public school catering to relatively poor students also participated in the sports contest.

The sports contest proceeded in four stages.

Group Assignment. First, students were randomized to different sessions (separately by gender) with varying stakes for winning the subsequent relay race - either Rs. 500, Rs. 200 or Rs. 50 per teammate for winning the race. 500 rupees are approximately a month's pocket money for the oldest students in the sample, so the stakes are substantial. Within each session, students mixed together to introduce themselves to each other for about fifteen minutes. School uniforms made group membership salient.

Ability Revelation and Team Selection. Students watched a series of one-on-one sprints, designed to reveal each runner's ability. In most cases, one runner was from the public school, while the other was from one of the private schools. However, some pairs included two students from private schools, or two from the public school. After each sprint, the rank (first or second) and times of the two runners were announced. Participants then privately indicated on a worksheet which of the two students they would like to have in their two-person team for a six-team relay race.

Choice Implementation and Relay Race. After the sprints were complete, six students were picked at random in each session to participate in the relay race, and one of their choices was randomly selected for implementation. The relay races were conducted and prizes were distributed as promised.

Socializing with Teammate. After the prizes were distributed, students spent approximately two hours socializing with their teammate.

Reduced Form Results. The first reduced form finding is significant discrimination against the poor on average. I classify a wealthy student as having discriminated against the poor if he or she chooses a lower ability (i.e. slower) rich student from another school over a higher ability poor student from the public school.[14] Averaging over the different reward conditions, participants discriminate 19% of the time. These are not just mistakes, since the symmetric mistake of "discriminating" against a rich student occurs only 3% of the time. And when participants are choosing between two runners from the same (other) school, they pick the slower runner only 2% of the time. Thus, only poor students competing against rich students are systematically discriminated against.

The second finding is that discrimination decreases as the stakes increase. In the control school, 35% of choices exhibit discrimination against the poor in the Rs. 50 condition, but this falls to 27% when the reward is Rs. 200, and only 5% in the highest stakes condition of Rs. 500. This result is shown by the solid line in Figure 3.4, which I interpret as a demand curve for discrimination.

The third and most important finding is a reduction in discrimination from having poor classmates and study partners. Figure 3.4 shows that for each level of stakes, wealthy students with poor classmates are less likely to discriminate against the poor. In addition, the slope of the demand curve for discrimination is higher for students with poor classmates. Figure 3.5(a) depicts the difference-in-differences estimates graphically by plotting rates of discrimination by school and grade. In the treatment school, discrimination is substantially lower than in the control school in the treated grades 2 and 3, but not in grades 4 and 5. Figure 3.5(b) instead depicts the reduced form of the IV strategy, plotting rates of discrimination by whether the student has a name alphabetically adjacent to a poor students. Consistent with the difference-in-differences result, the figure shows that students with names close to a poor student (and therefore a higher likelihood of having a poor study partner) discriminate less.

Regression estimates are reported in Table 3.3. Column 1 shows that having a poor

[14] I do not consider a choice to be discriminatory if it involves choosing ones own schoolmate over a higher-ability poor student, since participants may prefer to partner with children they already know.

classmate reduces discrimination by 12 percentage points (se 5).[15] This effect is comparable to the 11 percentage point reduction in discrimination caused by increasing the stakes from Rs. 50 to Rs. 200 (an increase of about \$3). Column 2 shows that having poor classmates has the biggest effect on discrimination in the lowest stakes condition (a 25 percentage point reduction). Column 3 reports the IV result that having a poor study partner reduces discrimination by 14.7 percentage points (se 8.8).[16]

Model and Structural Estimation. The reduced form results provide evidence of a reduction in discrimination. But they do not inform us of the magnitude of the distaste that wealthy students have for partnering and socializing with a poor child, nor how much this is changed by having poor classmates. In order to estimate these quantities, I structurally estimate a simple model of discrimination.

*Model***.** Suppose the decision-maker has expected utility:

$$U_t = p_t M + S_t \tag{3.3}$$

where p_t is the probability of winning the race with teammate t, M is the monetary reward for winning the race and S_t is the utility from socially interacting with teammate t. I assume that teammates are of two types, $t \in \{R, P\}$, where R denotes a rich student and P denotes a poor student.

Then, she chooses the rich teammate if

$$p_R M + S_R > p_P M + S_P$$

$$\Leftrightarrow S_R - S_P > (p_P - p_R)\, M$$

In the absence of a particular distaste for having a poor teammate, $S_P = S_R$. And in the absence of statistical discrimination, rich and poor students with the same performance in the sprint would be perceived to be equally able, $p_P = p_R$. Define $D_{poor} \equiv S_R - S_P$ as the distaste for interacting with a poor student (relative to a rich student), and $\delta_{poor} \equiv p_P - p_R$ as the perceived *increase* in probability of winning from having a poor teammate, provided the poor student won the ability-revelation sprint. Then, the decision-maker discriminates against a poor student if:

$$D_{poor} > \delta_{poor} M \tag{3.4}$$

[15]Since the discrimination experiment has wealthy students from only two schools, I do not attempt to cluster standard errors at the school level. Instead, I report unclustered standard errors and, as a robustness check, cluster at the school-by-grade level (8 clusters) using the wild cluster bootstrap method.

[16]The treatment school uses alphabetic order to assign study partners. Since the sample for this experiment does not include any other treatment schools which do not use such a rule, I directly use alphabetic adjacency to a poor student as the instrument for having a poor student in one's study group.

Similarly, in the case where the rich student wins the sprint, we can define the increase in probability of winning from choosing the rich teammate, δ_{rich}.

In order to estimate the model, I impose the following distributional assumption: D_{poor} is distributed normally with mean μ_D^T and standard deviation σ_D^T, separately for students from treated classrooms ($T = 1$) and untreated classrooms ($T = 0$). Consistent with the fact that 98% of students state that the winner of the ability sprint is more likely to win the relay race (regardless of whether the winner was rich or poor), I additionally impose the assumption of no statistical discrimination, $\delta_{poor} = \delta_{rich} \equiv \delta$.

Then, the parameters to be estimated are: (i) μ_D^1 and μ_D^0, the average distaste for having a poor teammate amongst students with and without poor classmates, respectively; (ii) σ_D^1 and σ_D^0 , the standard deviations of the distribution of distaste; (iii) δ, the increase in probability of winning from choosing the teammate who won the ability-revelation sprint.

I estimate these parameters using a classical minimum distance estimator. Specifically, the estimator solves $\text{Min}_\theta \, (m(\theta) - \hat{m})' \, W \, (m(\theta) - \hat{m})$, where $\hat{m}$ is a vector of the empirical moments and $m(\theta)$ is the vector of theoretically predicted moment for parameters θ. The weighting matrix W is the diagonalized inverse of the variance of each moment; more precisely estimated moments receive greater weight in the estimation.

The moments for the estimation are the following: (i) The probability of discriminating against a higher-ability poor student, separately by stakes $M \in \{50, 200, 500\}$ and treatment status $T \in \{0, 1\}$, and (ii) The probability of discriminating against a higher-ability rich student, by stakes M and treatment status T. The empirical moments are simply shares of students observed to discriminate in each condition, estimated by an uncontrolled OLS regression.

Identification. All 5 parameters are jointly identified using the 12 moments. The intuition for the identification is straightforward. Conditional on δ, the exogenous variation in the stakes M pins down the mean μ_D and standard deviation σ_D of the distribution of distaste D. Conditional on the distribution of D, the perceived increase in probability of winning from choosing a high-ability teammate (δ), is identified from comparing the probabilities of choosing (say) the poor student when he wins versus loses the ability-revelation sprint.

Estimates. The lower panel in Table 3.4 reports the empirical and fitted values of the moments. The model overall does a good job of fitting the moments, with the exception of slightly over-predicting discrimination against the poor in the lowest stakes condition. Table 3.4 also reports the structural estimates of the parameters. The perceived increase in probability of winning from choosing a high-ability teammate is imprecisely estimated, $\delta = 0.08$ (se 0.1). Students without poor classmates are estimated to feel an average distaste for having a poor teammate of μ_D^0 =Rs. 37 (se Rs. 4.4), with a standard deviation σ_D^0 = Rs. 6 (se Rs. 1.9). In contrast, treated students are estimated to have a substantially lower distaste of μ_D^1 =Rs. 26 (se Rs. 4.8) and a similar standard deviaion, σ_D^1 = Rs. 5 (se Rs. 2.1). The difference in average distaste of Rs. 11 is significant at the 10% level, and constitutes a 30% reduction relative to students without poor classmates.

3.6 Academic Outcomes

One concern with integrating disadvantaged students into elite schools is that wealthy students' academic outcomes may suffer as a result. This concern is motivated by the large literature studying peer effects in education, which has sometimes found substantial peer effects (Hoxby 2000, Hanushek et al. 2003) and at other times no evidence that peers affect test scores (Angrist and Lang 2004, Imberman et al. 2009). Classroom disruptions by poorly disciplined students have been proposed to an key mechanism underlying any negative effects (Lazear 2001, Lavy and Schlosser 2011, Figlio 2007). Indeed, principals in the schools I studied reported being particularly concerned about classroom disruptions and learning. In this section, I therefore turn attention to estimating the impact of poor students on the learning and classroom discipline of their wealthy peers.

3.6.1 Learning

To measure effects on learning, I report the results of simple tests of learning in English, Hindi and Math. I normalize the test score in order to provide standardized effect sizes.

I find that poor students do worse than rich students on average, but with substantial heterogeneity. Poor students score 0.32 standard deviations (s.d.) worse than wealthy students in English, 0.12 s.d. worse in Hindi and 0.24 s.d. worse in Math. The lower average learning levels of poor students make the possibility of negative peer effects very real. But the variance in poor students' test scores is similar to that of wealthy students; there is thus plenty of overlap in the distributions of academic achievement. For example, poor students have weakly higher scores than 40% of their wealthy study partners even in English.

Table 3.5 reports regression estimates of the effects of poor students on their wealthy classmates' test scores. The first two columns show a small and insignificant effect on an equally weighted average of standardized scores in the individual subjects. I also consider effects on each subject in turn. Most importantly, I estimate a 0.09 standard deviation reduction in average test scores in English in treated classrooms, significant at the 10% level. The coefficient on the IV regression of English scores on having a poor study partner is also negative, although quite imprecisely estimated. In contrast, I find no effects of poor classmates on wealthy students' test scores in Hindi or Math.

Considering the results for the different subjects together, the overall pattern is one of mixed but arguably modest effects on academic achievement. The only negative effect is on English scores. This is consistent with English being the subject with the largest achievement gap between rich and poor students, perhaps due to the fact that poor students almost exclusively report speaking only Hindi at home. But substantial learning gaps also exist in Math and (to a lesser extent) Hindi, and yet I detect no negative peer effects in those subjects. These latter non-effects are consistent with those of Muralidharan and Sundararaman (2013), who find no effects on the achievement of existing students in private schools in rural India when initially lower-achieving voucher-recipients enter their schools.[17] An additional mechanism, supported by anecdotal reports from teachers, is that the presence of poor students causes conversations between students to shift more from English to Hindi, which might well reduce wealthy students' fluency in English. However, I find no evidence of a significant increase in Hindi test scores.

3.6.2 Discipline

To measure classroom discipline, I ask teachers to report whether each student has been cited for any disciplinary infractions in the past six months[18]. I find that 22% of wealthy students have been cited for the use of inappropriate language (that is, swearing) in school, but only about 6% are cited for disruptive or violent behavior. Poor students are no more likely than rich students to be disruptive in class, but they are 12 percentage points more likely to be reported for using offensive language.

Table 3.6 reports regression estimates of the effects of poor students on disciplinary infractions by their wealthy classmates. The results suggest that having poor classmates increases the share of wealthy students reported for using inappropriate language by 7.5 percentage points (se 3.7). Having a poor study partner causes an even larger increase of 10 percentage points (se 6), an increase of about 45%. In contrast, I find precisely estimated zero effects on the likelihood of being cited for disruptive or violent behavior.

The finding that poor students do not make their wealthy classmates more disruptive – and indeed are no more disruptive than wealthy students themselves – is consistent with the absence of negative peer effects on Hindi and Math scores. In the context I study, concerns about diversity affecting test scores through indiscipline appear to be unwarranted. In contrast, the effects on inappropriate language use are substantial, although possibly without implication for learning and future academic achievement.

[17] But note that Muralidharan and Sundararaman (2013) study a quite different setting. I study the effects on quite wealthy students attending elite private schools in urban Delhi, while they study the effects on students attending relatively modest private schools in rural Andhra Pradesh, where the social and economic disparity between the existing and incoming students is likely much smaller.

[18] This question was asked at the end of the academic year

3.7 Discussion

Section 4 established that having poor classmates caused increases in prosocial behavior by wealthy students. This result contributes to a recent literature studying the factors that influence social preferences[19], and provides the first evidence that peers at school can shape a student's social preferences.

Section 5 showed that having poor classmates causes a reduction in discrimination by wealthy students against the poor, and increases their "willingess to play" with poor children. Unlike the generalized increase in generosity in the dictator games, then, the effect on tastes for new social interactions is directed: familiarity breeds fondness. This set of results contributes to the economics literature on inter-group contact, and is most closely related to recent research using randomized roommate assignments by Burns et al. (2013), who measure effects on racial prejudice using psychological tests, and effects on trust using lab experiments. I additionally measure discrimination, and study effects on prosocial behavior and academic outcomes.[20]

For both classes of social behaviors, I find that personal interactions between wealthy and poor students are an important driver of the results. Scaling the estimated effect of having a poor study partner by the relevant share of wealthy students with poor partners (68%), I find that having a poor study partner alone explains 70% of the overall classroom-level increase in willingness to play with a poor child, and 38% of the increase in volunteering behavior. These are likely underestimates of the importance of personal interactions, since membership in study groups underestimates total personal interactions.

It is also worth discussing the magnitudes of the estimated effects. The effects on social behaviors appear to be large. For example, the 0.25 standard deviation increase in prosocial behavior is similar to the causal effect of a one standard deviation increase in test scores in Jakiela et al. (2010). The estimated 30% reduction in distaste for having a poor teammate (relative to a rich one) is similarly substantial. In contrast, the overall effect on test scores is small (-0.02 standard deviations), although the reduction in English language scores of 0.09 s.d. is economically meaningful and comparable to the strength of peer effects on reading scores in Hoxby (2000).

3.8 Conclusion

In this paper, I exploit a natural experiment in education policy in India to estimate how greater economic diversity in classrooms affects wealthy students. I assemble a variety of evidence to reach three main findings. The first finding is that having poor classmates makes wealthy students more prosocial and charitable. The second finding is that wealthy students become less likely to exhibit taste-based discrimination against the poor. The

[19] Jakiela et al. (2010), Voors et al. (2012) and Fisman et al. (2012)

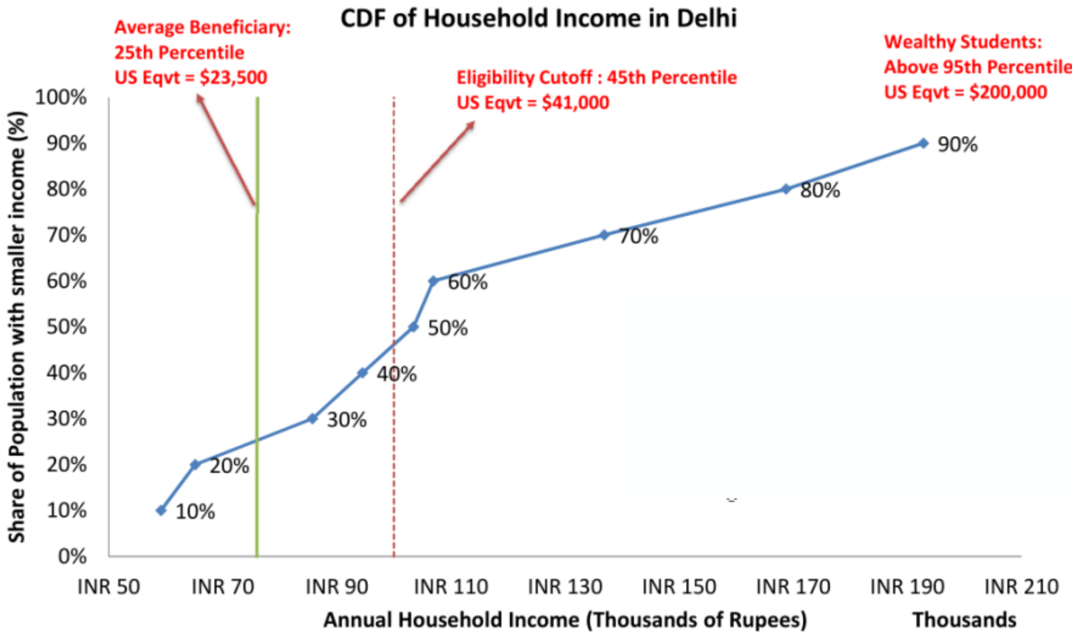
[20] Unlike much of the related literature in social psychology (Pettigrew and Tropp, 2006), I provide causal rather than correlational evidence, and use incentivized behavior to measure outcomes.

third finding is of mixed but overall modest impacts on academic outcomes, with negative effects on English language learning but no effect on Hindi or Math. Thus, my overall conclusion is that increased diversity in the classroom led to large and arguably positive impacts on social behaviors, at the cost of negative but modest impacts on academic outcomes.

One implication of this paper is that school policies involving affirmative action, desegregation and tracking should be evaluated not only on learning outcomes - which are of unarguable importance - but also on other economically important outcomes related to social behaviors. More generally, my findings support the view that increased interactions across social groups, perhaps especially in childhood, can improve inter-group behaviors. This has implications for diverse and polarized societies, where the costs of social divisions are thought to be high. Finally, my findings are of relevance to the planned expansion of this policy across India over the next few years, where it will touch many of India's 300 million school children.

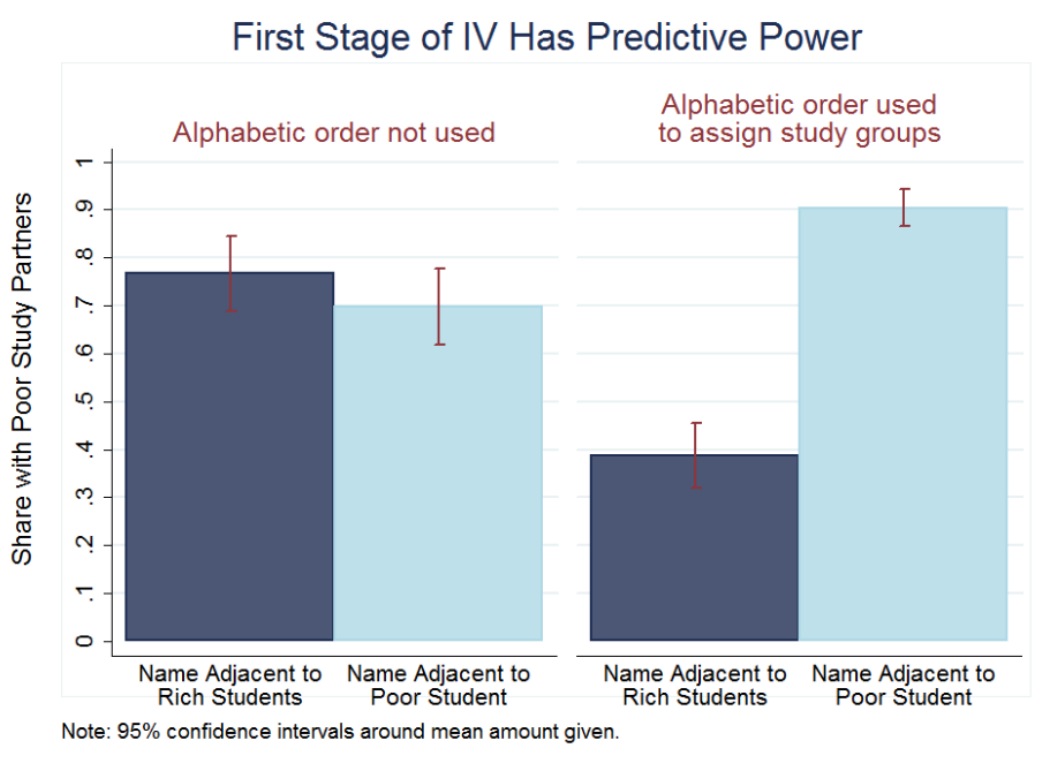
One limitation of this paper is that due to the recency of the policy experiment, it does not study important long-term outcomes and behaviors such as political beliefs, social interactions as adults, and marriage market choices. Another limitation is the very particular nature of the sample - wealthy students in elite private schools in Delhi. Finally, this paper does not examine the effects on poor students of attending elite private schools, which might have profound consequences for their academic achievement and life outcomes. In ongoing and future work, I hope to address these shortcomings, as well as examine other aspects of the impacts of diversity in schools.

Figure 3.1: Program eligibility and the household income distribution in Delhi



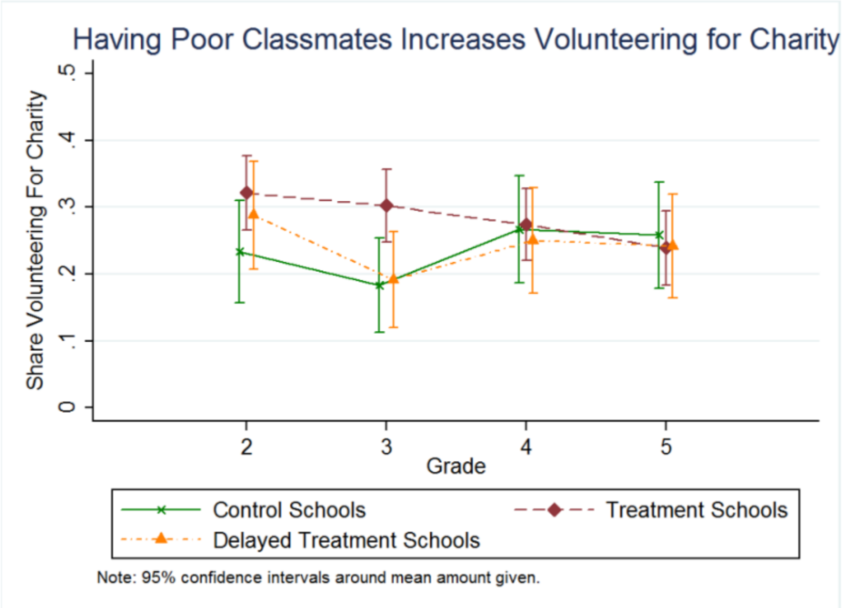
Note: This graph is based on the household consumption distribution reported in NSS-2010, with consumption amount converted to income levels using the ratio of household income to household consumption for urban Indian households reported in IHDS-2005.

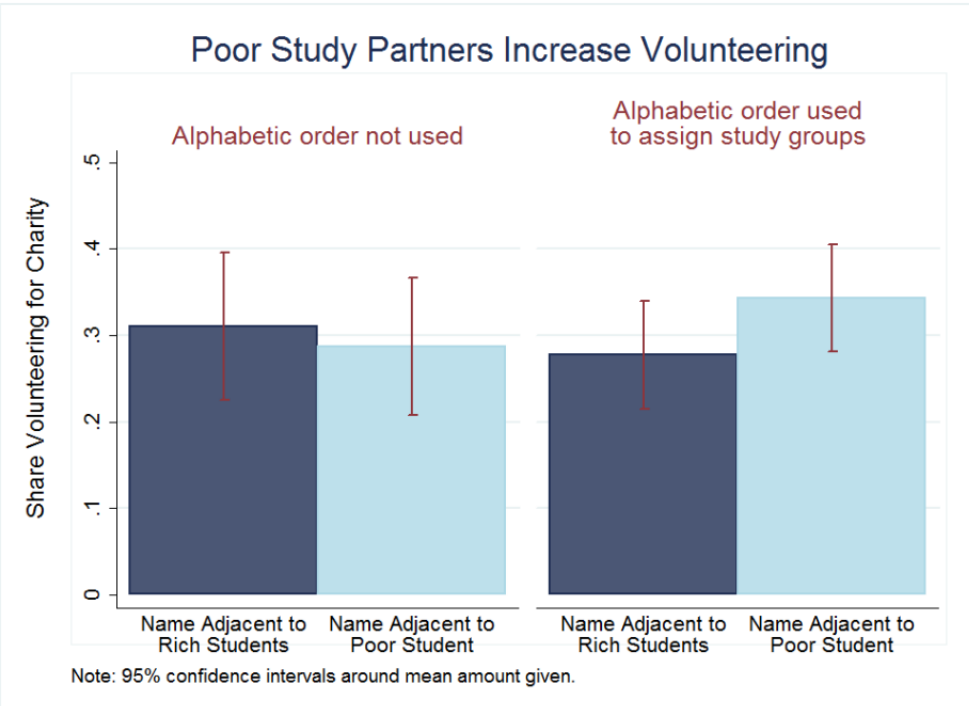
Figure 3.2: First Stage of Instrumental Variable Regression



Note: Having a name alphabetically adjacent to a poor student predicts having a poor student in one's study group, but only in the schools which explicitly use alphabetic ordering

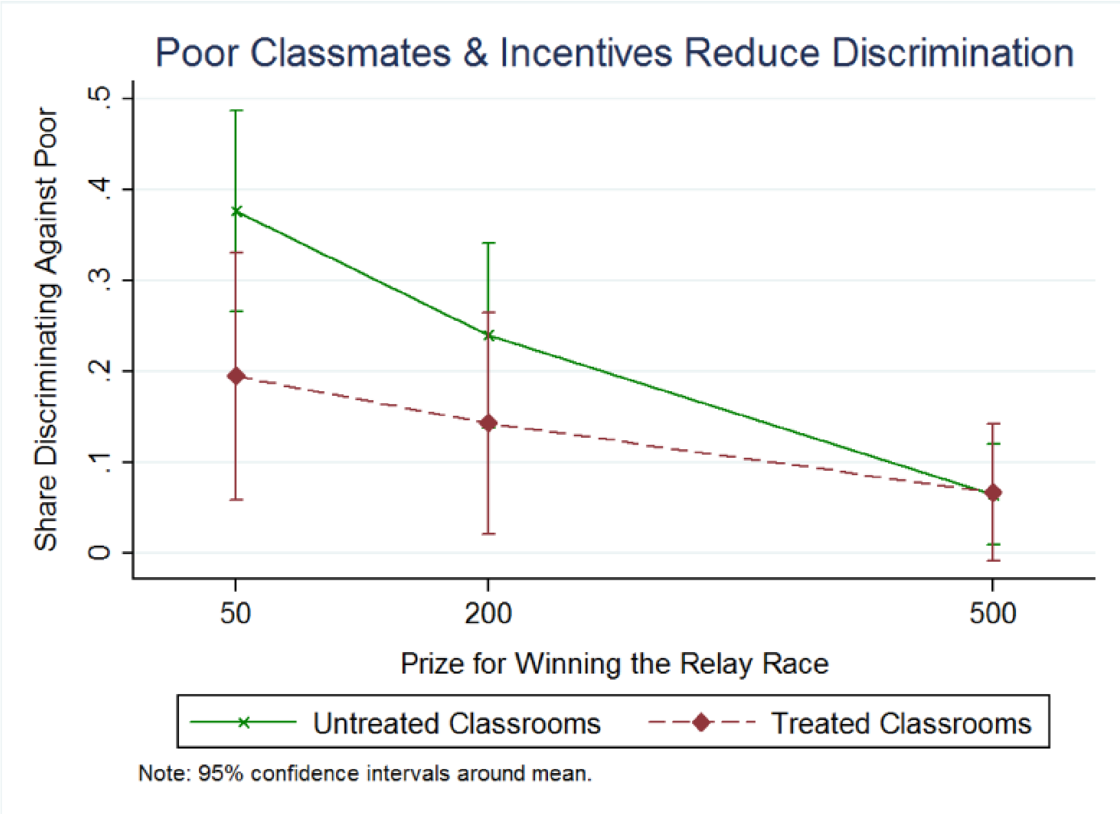
Figure 3.3: Volunteering for Charity





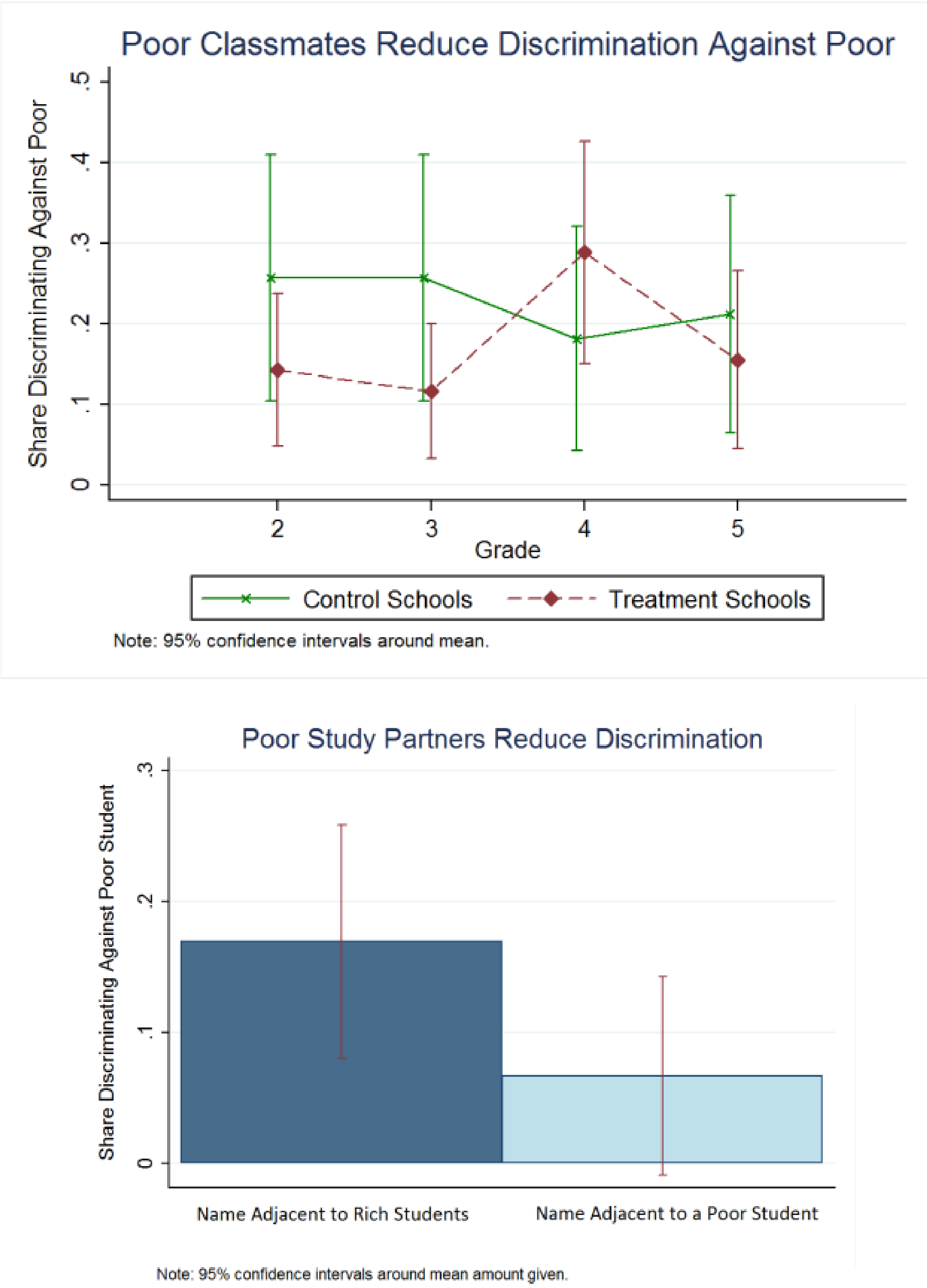
Note: The top panel plots the share of wealthy students who participate in voluntary charitable fundraising activities in school, separately by type of school. Error bars plot 95% confidence intervals (unclustered). The bottom panel plots share volunteering by whether the subject has a name alphabetically adjacent to any poor students, separately by whether schools use alphabetic order to assign study groups.

Figure 3.4: Demand Curve for Discrimination



Notes: This graph plots the share of wealthy students who discriminate against the poor (on the y axis) by the stakes of the decision, separately by whether the student has poor classmates (dotted red line) or not (solid green line). A student is classified as having discriminated against the poor if he chooses a lower-ability rich student over a higher-ability poor student.

Figure 3.5: Discrimination Against the Poor



Notes: The top panel plots the share of wealthy students who discriminate against the poor (on the y axis) by grade (on the axis), separately by school type. The control school is represented by the solid green line, while the treatment school is represented by the dotted red line. Error bars plot 95% confidence intervals (unclustered). The bottom panel plots discrimination rates by whether the participant has a name alphabetically adjacent to any poor students, only for the treatment school.

Table 3.1: First Stage of IV Regression

Dependent Variable:
Indicator for having at least one poor student in one's study groups

Instrument	(1) Has Poor Study Partner
(Name Adjacent to Poor Student) x (School Uses Alphabetic Rule)	0.519^{***} (0.0808)
Constant	0.456^{***} (0.0793)
N	677
F-Statistic	41.25

Note: Standard errors in parentheses. This table reports the results from regressing an indicator for whether the student has a poor study group partner on the excluded instrument, the direct effect of having a name immediately adjacent to at least one poor student, school and grade dummies, and a vector of second stage control variables. The F statistic reports results of a Wald test of a zero coefficient on the excluded instrument.

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Table 3.2: Volunteering for Charity

	Dependent Variable: Indicator for Volunteering for Charity			
	(1)	(2)	(3)	(4)
Specification:	DiD	DiD	IV	DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	0.105^{***}	0.0715^{**}		-0.0409
	(0.0244)	(0.0305)		(0.0766)
Has Poor Study Partner			0.130	0.208^{**}
			(0.0866)	(0.0881)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	0.06	.	.
Control Mean	0.242	0.220	0.245	0.242
Control SD	0.428	0.414	0.431	0.428
N	2017	1143	677	2017

Note. Standard errors in parentheses. This table reports results from linear probability models for the likelihood of volunteering for a charity. **Col 1** reports a difference-in-differences estimates of the effect of having poor students in one's classroom, with standard errors clustered at the school-by-grade level. The p-value reported in the table instead is calculated using clustering at the school level (k=14) using Cameron, Gelbach and Miller's wild-cluster bootstrap. **Col 2** reports the same specification as Col 1, but restricts the sample to students who have older siblings enrolled in the same school. **Col 3** reports IV estimates of the effect of having a poor study partner, and presents robust standard errors. **Col 4** reports a specification estimating both the classroom level effect using the difference-in-differences term and an additional effect of having a poor study partner, with standard errors clustered at the school-by-grade level. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: Egalitarian Preferences

Dependent Variable: Indicator for Choosing the more egalitarian option in a binary choice dictator game

	Equality Game (5,5) v (6,1)		**Disinterested Game 1** (0,4,4) v (0,8,3)		**Disinterested Game 2** (0,4,4) v (0,12,0)	
	DiD	IV	DiD	IV	DiD	IV
	Full Sample	Treated Class	Full Sample	Treated Class	Full Sample	Treated Class
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Classroom	0.0933*		0.123***		0.143***	
	(0.0525)		(0.0476)		(0.0321)	
Has Poor Study Partner		0.0516		0.0648		0.1052**
		(0.0908)		(0.0718)		(0.0517)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	Classroom	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.09	.	0.02	.	< 0.01	.
Control Mean	0.551	0.623	0.467	0.500	0.769	0.896
Control SD	0.498	0.486	0.499	0.501	0.422	0.306
N	2017	677	2017	677	2017	677

Note: Standard errors in parentheses. This table reports results of linear probability models of the likelihood of choosing the more equal or egalitarian of two options in three binary choice dictator games. Cols 1 and 2 report shares choosing (5,5) over (6,1). Cols 3 and 4 report shares choosing (0,4,4) over (0,8,3). Cols 5 and 6 report shares choosing (0,4,4) over (0,12,0). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Discrimination Against Poor Children

Dependent Variable: Indicator for choosing lower-ability wealthy student over higher-ability poor student

	(1) DiD-1 Full Sample	(2) DiD-2 Full Sample	(3) IV-1 Treated Class	(4) IV-2 Full Sample
Treated Classroom	-0.123**	-0.256***		
	(0.0466)	(0.0654)		
Prize = Rs. 200	-0.110**	-0.137**	-0.0582	-0.0415
	(0.0423)	(0.0540)	(0.0757)	(0.126)
Prize = Rs. 500	-0.250***	-0.314***	-0.126*	-0.101
	(0.0583)	(0.0498)	(0.0713)	(0.135)
(Treated Classroom) x (Prize = Rs. 200)		0.1153*		
		(0.0667)		
(Treated Classroom) x (Prize = Rs. 500)		0.186*		
		(0.0939)		
Has Poor Study Partner			-0.147*	-0.118
			(0.0885)	(0.156)
(Poor Study Partner) x (Prize = Rs. 200)				-0.0337
				(0.210)
(Poor Study Partner) x (Prize = Rs. 500)				-0.0510
				(0.227)
Fixed Effects	School, Grade	School, Grade	Classroom	Classroom
Control Mean	0.226	0.226	0.220	0.220
Control SD	0.419	0.419	0.418	0.418
N	342	342	116	116

Note: Standard errors in parentheses. This table reports results of linear probability models of the likelihood of discriminating: i.e. choosing a wealthy teammate despite the poor child winning the first round race. **Cols 1 and 2** report difference-in-difference estimates of the effect of having poor students in one's classroom, with robust standard errors. **Cols 3 and 4** report parallel IV estimates of the effect of having a poor study partner, with robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.5: Structural Estimates

Estimates	Control		Treated	Difference
Mean distaste for poor teammate relative to rich (μ_D), in Rupees	37 (4.4)		26 (4.8)	-11* (6.5)
Std. Dev. of distaste for poor teammate relative to rich (σ_D), in Rupees	6 (1.9)		5 (2.1)	-1 (2.8)
Boost in probability of winning from choosing high ability teammate (δ)		0.08 (.1)		

Moments (Probability of Discriminating	Against Poor		Against Rich	
	Empirical	Predicted	Empirical	Predicted
Control Students:				
Stakes = Rs. 50	35%	46%	4%	11%
Stakes = Rs. 200	27%	24%	3%	0%
Stakes = Rs. 500	5%	1%	2%	0%
Treated Students:				
Stakes = Rs. 50	20%	32%	6%	16%
Stakes = Rs. 200	15%	18%	4%	1%
Stakes = Rs. 500	6%	0%	2%	0%

Notes: Estimates from minimum-distance estimator using the moments shown, and weights given by the inverse of each moment's variance. Standard errors are in parentheses.

Table 3.6: Test Scores in English, Hindi and Math

Dependent Variable: Normalized Test Score

	Combined		English		Hindi		Math	
Specification:	DiD	IV	DiD	IV	DiD	IV	DiD	IV
Sample:	Full	Treated	Full	Treated	Full	Treated	Full	Treated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated Classroom	-0.024		-0.087*		0.0260		0.037	
	(0.068)		(0.050)		(0.0632)		(0.0466)	
Has Poor Study Partner		-0.025		-0.066		-0.181		0.0146
		(0.132)		(0.135)		(0.172)		(0.083)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade		School, Grade	Classroom	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.76	.	0.09	.	0.72	.	0.45	.
Control Mean	0	0.0466	0	0.0266	0	0.0372	0	0.0761
Control SD	0.594	0.628	1.000	1.127	1.000	1.021	1.000	0.974
N	2017	677	2017	677	2017	677	2017	677

Note: Standard errors in parentheses. This table reports effects on an index of test scores comprised of equally weighted normalized scores in English normalized test scores of wealthy students in English (Cols 3 and 4), Hindi (Cols 5 and 6), (Cols 7 and 8). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: Indiscipline

Dependent Variable: Indicator for being cited by teacher for *indiscipline* – either inappropriate language or disruptive behavior

	Cursing		**Disruptive Behavior**	
Specification:	DiD	IV	DiD	IV
Sample:	Full Sample	Treated	Full Sample	Treated
	(1)	(2)	(3)	(4)
Treated Classroom	0.0749**		-0.0181	
	(0.0368)		(0.0193)	
Has Poor Study Partner		0.1032*		-0.0251
		(0.0612)		(0.0488)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	Classroom	School, Grade	Classroom
p-value (CGM)	0.06	.	0.624	.
Control Mean	0.219	0.231	0.0619	0.0613
Control SD	0.414	0.423	0.241	0.240
N	2017	677	2017	677

Note: Standard errors in parentheses. This table reports linear probability models for the likelihood of being cited by the class teacher for two types of indiscipline - inappropriate language (Cols 1 and 2) and disruptive behavior (Cols 3 and 4). Odd numbered columns report difference-in-difference estimates of the effect of having poor students in one's classroom. In these columns, standard errors are clustered at the school-by-grade level, while the p-value reported in the table comes from clustering at the school level using Cameron, Gelbach and Miller's wild-cluster bootstrap. Even numbered columns report IV estimates of the effect of having a poor study partner, and present robust standard errors. Individual **controls** include gender, age, mother's education, distance of student's home from school, and whether the student uses a private (chauffeured) car to commute to school.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Appendix A

Proofs of the Propositions in Chapter 2

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) $h^{i*} = h_0 + \eta^i \max\left(s^i + m - c^i, -S^i\right)$ if the person saw a survey flyer, and (iii) $h^{i*} = h_0 + \eta^i \max\left(s^i + m - c^i + z^i, -S^i\right)$ if the person saw an election flyer. Under Pride in Voting, $z^v = \max\left(s_V^v, s_N^v - L^v\right) \geq s_V^v$ is positive. Hence, h^* will be at least as high under FE than under F for voters. Conversely, under Stigma from Not Voting, $z^{nv} = \max\left(s_V^{nv} - L^{nv}, s_N^{nv}\right)$ 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 $P\left(H\right)_{OOE}^v \geq P\left(H\right)_{OO}^v$. Conversely, under Stigma from Not Voting, z^{nv} is negative so the converse result applies and $P\left(H\right)_{OOE}^{nv} \leq P\left(H\right)_{OO}^{nv}$ 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 $P\left(SV\right)_I^v \geq P\left(SV\right)_{NI}^v$ under Pride and $P\left(SV\right)_I^{nv} \leq P\left(SV\right)_{NI}^{nv}$ 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 $P\left(SV\right)$ 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, $P\left(SV\right) = 0$ 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 $P(SV)^v_{FE} \geq P(SV)^v_F$. Under Stigma, cases (i), (ii), and (iii) apply and pairwise comparisons for all these cases show $P(SV)^{nv}_{FE} \leq P(SV)^{nv}_F$.

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 $s^v_N - L^v + I \geq s^v_V$ or $-(s^v_V - s^v_N) - L^v \geq -I$. Under the assumption $s^v_V - s^v_N > 0$ 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 $s^{nv}_V - L^{nv} \geq s^{nv}_N + I$ or $(s^{nv}_V - s^{nv}_N) - L^{nv} \geq I$. 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 (Eq 2.1) 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.