Can Political Participation Prevent Crime?
Results from a Field Experiment about Citizenship, Participation, and Criminality

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Abstract (150 Words)

Democratic theory and prior empirical work support the view that political participation, by promoting social integration and pro-social attitudes, can reduce an individual's propensity for anti-social behavior, such as committing a crime. Previous empirical investigations have been limited to observational research, which is vulnerable to bias if there are omitted factors that affect both the propensity to participate and the risk of criminality. We present results from a field experiment in which 552,525 subjects aged 18-20 were encouraged to register and vote. Consistent with previous observational findings, we first confirm that there is a negative association between participation and subsequent criminality. However, comparing the randomly formed treatment and control groups, we find that although the intervention increased participation, it did not produce any reduction in subsequent criminality. Our results thus suggest that while participation is correlated with criminality, encouraging registration and voting has no causal effect on subsequent criminal behavior.

Following rules and acting in a pro-social manner is not always the natural thing to do. Scholars have identified the "problem" of teaching the next generation to act responsibly, to think about others, and to use foresight, as one of the key challenges that every society faces (Wilson 1995; Wilson and Herrnstein 1985). What is the role of political participation in this process of education and social integration? One area in which this question has immediate practical implications is criminology. Recidivism prevention is often conceptualized as a process of education and re-integration by which released criminals must learn or re-learn the positive habits of community life and citizenship (e.g., Braithwaite 1989; Hirschi 1969; Sampson and Laub 1993). Among the various components of this process of re-education, some have argued that political participation can have important positive effects on individuals, serving as a pathway for the individual to move toward becoming a self-regulating citizen (Uggen and Manza 2004; Uggen et al. 2004; Uggen and Schaefer 2005). In this paper we describe an effort to measure the causal effect of political participation on the likelihood that an individual engages in criminal behavior for a particularly interesting population: Young adults of color.

Multiple strands of normative political theory provide detailed arguments about the ways in which participation transforms the individual and thereby reduces the likelihood that an individual breaks the law. To help understand these arguments, we highlight several of the hypothesized mechanisms. Political participation, by requiring individuals to consider the arguments put forth by others about what choices are best, encourages individuals to consider the perspectives and interests of others (de Tocqueville [1840] 1969; Mill [1861] 1978). Participation may thereby reduce the tendency to act without regard for others (Rousseau [1762] 1968). As citizens develop a broader perspective, this may, in turn, reduce the likelihood that they take an action that will cause harm to another. Thus, participation may encourage a "democratic character," an enlightened self-interest incompatible with violence towards others. The act of participation may also trigger greater community engagement and interaction with community members who are themselves active participants in the community's collective life.

In addition, participating may change the individual's attitude toward the law if participation serves to legitimate the state. When voting, all individuals are given the chance to express their views, and

so by participating a citizen learns that her views are worthy and that avenues exist for the expression of her interests. This development of a sense of political efficacy (Barber 1984; Pateman 1970) combined with the idea that "one has a stake" in the system encourages respect for the democratic process (Thompson 1970), which extends to agreeing that even in cases where one does not like the particular outcome of governance, one should still abide by it.[1] This respect for the law may also arise because one who participates comes to understand collective decision making as a means for problem solving that is superior to avenues such as violence or resistance.

Although these mechanisms provide a theoretical basis to suppose that participation might produce citizens who are more law-abiding, there is to date only modest empirical evidence to support this proposition (Uggen and Manza 2004; Uggen and Schaefer 2005). Those who participate are, in fact, much less likely to be convicted of crimes. However, this association does not demonstrate that participation *causes* a reduction in criminal tendencies. There are many reasons why those who choose to vote are very different from those who do not, and these differences, which might include greater engagement in community life, a stable residential and employment history, and greater educational attainment, might also be directly related to the propensity to engage in criminal behavior.

In this paper, we present results from a large-scale field experiment that has a research design that permits us to address some of the challenges in assessing the causal effects of participation. The subject population for the experiment is approximately 550,000 non-white young adults who were subjects in a randomized field experiment conducted during the November 2010 election cycle in the United States. This experimental design, in which some subjects were randomly chosen to receive a pre-election intervention designed to increase voting, overcomes the problems associated with observational analysis in which individuals who vote do so for reasons that might also be correlated with the probability of becoming involved with the criminal justice system. Additionally, unlike some prior work, we use

[1] See also Laub et al. (1998), who argue that attachments to social institutions (e.g., marriages) discourage the violation of community norms.

administrative records to measure both participation and criminality—operationalized as being under state supervision—thereby avoiding concerns about systematic measurement error in reported participation and criminality.[2]

In the next section we review the existing empirical evidence regarding the link between participation and criminal behavior, as well as additional relevant literature from social psychology. In the subsequent section we describe our data and methods. The sections that follow present two analyses of the data. In the first, we perform an observational analysis of the relationship between participation and criminality within the control and treatment groups. We show that, as suggested by previous research, there is a strong association between non-participation and criminal convictions for this population. Those young adults who voted in 2010 are 55% less likely to be under state supervision two years later than those who did not vote.

Next, we use the random assignment employed in the experimental design to estimate the causal effect of participation on criminality. We begin by laying out the conditions under which an experimental registration intervention can be used to produce consistent estimates of the causal effect of voting on subsequent criminal behavior. We next present our experimental estimates, showing that those treated in the experiment are about .5 percentage points more likely to vote in the November 2010 election, an increase of 19% relative to the control group. However, in sharp contrast to the observational result, the treatment group shows no signs of a reduction in state supervision. Thus, when randomly formed groups are compared, we see that the observed negative correlation between voting and criminality does not appear to be causal in nature.

The larger implications of our research are quite broad. Methodologically, our work underscores the extreme challenges facing empirical researchers in testing normative theories about how political participation may alter individuals. Although our particular focus is on the relationship between voting

[2] Of course, there may be bias in which sorts of individuals and crimes are targeted for enforcement and prosecution, a topic beyond the scope of our analysis.

and criminality, the fact that we find very different results in our observational and experimental analyses highlights once again the need for rigorous experimental testing of many robust correlational results.

Indeed, the more general point is that when our prior estimates are based on research designs that cannot address fundamental sources of bias (such as unobserved heterogeneity), the apparent statistical precision of those estimates is misleading. That is because traditional estimates of precision (for example, the standard errors of a regression coefficient) do not account for the uncertainty produced by model misspecification. If there is substantial concern that there are omitted variables that explain both treatment assignment and the outcome of interest, then the regression results may be biased. Further, the uncertainty regarding the magnitude of bias produces an additional source of imprecision, one that is not incorporated into the reported standard errors. Consequently, we should not be surprised when improved research designs yield results far different from prior studies.

We present evidence from our credibly identified study that allows us to confidently reject the large negative association between voting and participation found in prior observational research (and in our own data, when analyzed in that fashion). Furthermore, we also show that even if the estimates from prior research are used as an informative baseline, our results should nonetheless cause scholars to revise substantially downward (toward zero) their estimate of the association between participation and criminality.

Finally, we discuss the limitations and implications of our analysis as well as directions for further investigation. For example, our interpretation of the experimental estimates requires some technical assumptions. Additionally, despite the large size of the experiment, our causal estimates of the effect of participation on subsequent criminal behavior are somewhat imprecise. Beyond these technical issues, we also consider whether different subject populations, efforts to encourage more sustained forms of participation, or a longer term focus on participation and criminal behavior over the life course might yield more promising results. Nonetheless, despite these limitations, from a policy standpoint, these results suggest that advocating voting is unlikely to be especially successful as a means of reducing criminal behavior. If voting did reduce criminality (or any other undesirable behavior), the low cost of

encouraging participation relative to the cost of, for example, incarceration, would make it an extremely cost-effective policy intervention. Unfortunately, although there may be many important reasons to encourage participation by all citizens, our research suggests that this particular strategy is unlikely to produce substantial positive effects with respect to criminality.

Literature Review

Above we described several theoretical arguments for how participation might promote pro-social behavior in general, and reduce criminality in particular. In this section we review the empirical evidence. Several previous studies demonstrate strong associations between participation and pro-social behavior and reduced criminality. Work by Uggen and colleagues, for example, links past voting (Uggen and Manza 2004) and volunteerism (Uggen and Janikula 1999) to reductions in reported criminality. Uggen and Janikula conclude that "by entering and committing to pro-social volunteer service, young adults may alter lifelong trajectories of deviant behavior, political participation, and civic engagement" (355). Similarly, a number of studies have focused on those already convicted of a crime and investigate how participation may reduce the risk of recidivism. Underlying this research is evidence that in places where felons are disenfranchised they report that the stigma associated with being unable to vote makes it harder to resume one's place in society after release (Uggen et al. 2004, see also Behan 2012). Empirically, Maruna (2001) shows that feelings of civic integration correlate with reduced recidivism, while Nirel et al. (1997) find that Israeli prisoners assigned to community service work in lieu of prison are less likely to recidivate.[3] In the U.S. context, Uggen and Schaefer (2005) show that released prisoners who voted after being released from prison were also less likely to recidivate. Practitioners have also embraced voting as a means to reduce recidivism, with the Florida Parole Commission (Pate 2011) crediting the restoration of voting rights with reducing the likelihood of returning to prison.

The evidence that voting might plausibly lead to a decline in criminal activity dovetails with the growing empirical literature in experimental social psychology documenting how small interventions can

[3] Nirel et al. (1997) report results from an observational study.

lead to large changes in subject behavior. Examples of these studies include research showing how a minor educational intervention, writing a short essay about why a chosen value is very important to the subject (as opposed to someone else), produces a significant improvement in students' grades over the subsequent three years of schooling (Cohen et al. 2006). The explanation offered for the obvious disproportionate relationship between a small number of 15-minute interventions and the large and persistent improvement in subject grades is that the initial intervention is the trigger for a virtuous cycle, in which more constructive attitudes are rewarded by better results, reinforcing the more positive attitudes and inducing additional positive behavior. Similarly, voting might create a more positive social outlook that then triggers a self-reinforcing cycle of subsequent positive social interactions.

Despite the previous research, there are still many reasons for concern that the observed association in prior research between participation and criminality may not be causal. Normative theorists are cognizant of the important challenges to moving from intuition and association to demonstrated causality. Mansbridge, for example, perceptively and candidly writes:

> Participation does make better citizens. I believe it, but I can't prove it…. [I]n the case of the educative effects of participation… the postulated effects took subtle forms that could not easily be captured in empirical studies of relatively small numbers of people. First, although, cross-sectional studies showed that people who participated in democratic politics also had many other admirable qualities, it was hard to find situations for study in which a researcher could measure the qualities of people before and after the addition of participation to see if participation itself had any causal effect in producing those admirable qualities…. Only a massive (and therefore prohibitively expensive) study would be likely to pick up the effects on character of participation in politics… (1995, 1-6)

As Mansbridge notes, participation may simply be a marker of the same pro-social tendencies that would cause someone to avoid breaking the law (again or in the first place). Thus, it may not be that participation leads to the world view that discourages criminality, but instead that there is some unobserved factor that causes both participation and a tendency to abide by the law. The danger from spurious correlation due to an unmeasured factor is the central difficulty in establishing causality in this, and many other areas, of empirical research.

An additional problem that empirical research faces is measurement error. Researchers often rely on self-reports of participation, criminality, and other factors in trying to assess the relationship between participation and criminality. But if individuals misrepresent these activities in systematic ways (for example, if someone who has not voted and has not gone to prison is more likely to report they voted than someone who has not voted but did go to prison), then measured participation may be negatively correlated with measured criminality.[4] (Similarly, it could be that individuals who vote underreport their criminality relative to those who do not.) Fortunately, these difficulties can be overcome through a randomized experiment and use of administrative records rather than self-reported behavior.

Data

The dataset for our primary analysis is created by matching information from a large-scale field experiment aimed at increasing participation among young people to information about their criminal behavior. The data about political participation come from an outreach effort conducted by the Voter Participation Center (VPC) in 2010. The VPC is a nonpartisan organization "dedicated to increasing the participation… of unmarried women… and other historically underrepresented groups."[5] The VPC contracted with an outside vendor of student lists to obtain information about individuals born between June 14, 1990 (turning 18 in June 2008) and September 30, 1992 (turning 18 in September 2010) and residing in one of 13 targeted states (AZ, CO, FL, IL, KY, MD, MO, NM, NV, OH, PA, TX, and WA). Consistent with the VPC's mission of seeking to increase participation by underrepresented groups, individuals were retained in their sample if they were identified as non-white.[6]

[4] For example, Vavreck (2007) finds that more educated and more politically engaged individuals are more likely to overreport turnout.

[5] See http://www.voterparticipation.org/about-us/, retrieved April 2015.

[6] According to researchers who conducted the initial outreach program for VPC, race was coded by the vendor's proprietary system that used the linguistic characteristics of each name and Census demographics for the address to predict individual race. VPC researchers determined this coding was

After this initial sample was selected, records were excluded if the individual was already registered to vote, deceased, had a post office box or commercial mailing address, appeared invalid, or had an address that was deemed undeliverable, vacant, or seasonal. This initial sample is approximately 664,000 names.

The VPC's outreach effort was a randomized field experiment conducted in late August 2010. They randomly selected 90% of the households[7] in their experimental population and sent them a non-partisan registration mailing informing them that they were eligible to register to vote.[8] The mailing included the relevant voter registration form with a postage paid envelope for returning the signed document to the appropriate authority.[9] A randomized experiment with a young adult population provides the ideal setting for examining the impact of participation on criminal behavior. Specifically, a wealth of empirical research demonstrates that among those individuals who eventually engage in criminal behavior, criminality largely begins prior to turning 20 (see, e.g., Blumstein and Cohen 1987). Thus, targeting this group offers the best chance to prevent or disrupt nascent patterns of criminality.

In 2011, the VPC's data were merged with information from voter files by Catalist to obtain the (post-treatment) 2010 registration and voting behavior of these individuals. We obtained the data from the experiment at this point.

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reasonable by assessing the correlation between the vendor's coding and both self-reported race in surveys as well as with race as coded in voter files for states subject to Section 5 of the Voting Rights Act.

[7] 86% of cases are single-address records.

[8] While 90% of the sample is assigned to treatment, because treatment is randomly assigned, there are no expected differences between the treatment and control groups. This random assignment is the key step necessary for estimating unbiased causal effects.

[9] An example of the mailing appears in the Supporting Information.

Of the 664,000 records in the sample, about .3% had only a year of birth and an additional 10.8% of records had only a year and month of birth.[10] Because we require a valid date of birth for our later matching efforts, we exclude those records, yielding a sample of 590,472 records. In order to identify individuals under state supervision, we obtained administrative records of state supervision from a subset of the states in the original experiment sample.

Our data sources are outlined in supporting information (SI) Table S1. To include a state in our analysis, which necessitates matching individuals in the experiment sample to these records, we required information about the name and date of birth of each offender. These data are public records, but what administrative details are part of the public record varies across states. In most cases, we were only able to obtain information about individuals currently incarcerated in state prisons, but in some states the records include either historical incarceration records or a broader set of state supervision statuses (e.g., parole and/or probation).

To merge these records to the experiment sample, we used the state of residence, name, and date of birth information from the experiment sample. Records were matched if the state, date of birth, last name, and first initial of first name were identical. Table 1 lists the number of records from the experiment sample for each state, as well as the number of individuals matched to the state criminal supervision records. We note that in addition to being limited to the subset of criminal records available in each state, this matching will only succeed for individuals who continue to reside in the same state as in 2010.[11]

[10] For records with only a month and year of birth, day of birth was often recorded as the 1st of the month. For this reason, we exclude all cases with birthdays on the first of the month.

[11] Although the process of matching between the experiment sample and the state supervision records may be imperfect, the error rate should be the same in both the treatment and control groups. As we show below, we find a robust observational correlation between voting and not being incarcerated that is similar to that found in prior work, implying random measurement error is not a large threat to inference. Further

Table 1 about Here

Finally, in order to obtain additional information about the places in which these individuals lived, we geocoded this data file to census block groups using ArcGIS 10.1.[12] We were able to successfully geocode 97.3% of the 568,101 records shown in Table 1, which allows us to merge in 2007-2011 American Community Survey (ACS) 5-year estimates for variables that are used in our subsequent statistical analysis. Our final dataset consists of 552,525 records.

Observational Benchmark: Those Who Vote are Less Likely to Become Criminals

What is the effect of participation on an individual's subsequent criminality? We begin by examining the individual-level relationship between voting and subsequent criminal supervision. The purposes of this analysis are to (1) replicate prior research showing a correlation between participation and subsequent criminality and (2) validate our procedure for matching subject records to the incarceration data. If we find this pattern in these data when analyzed observationally, then our next step is to ascertain whether the relationship persists when we analyze the data using the experimental design.

Our initial graphical analysis is presented in Figure 1. We first estimate a logit model using records in the control group (those not sent a treatment letter in the field experiment) to predict the probability that each individual is under state supervision in our dataset. That logit, estimated separately for gender groups (gender is male, female, or unknown, as reported by the list vendor), includes indicators for whether an individual is Black or Hispanic (an exclusive coding, all other races are the excluded category), state fixed effects, and the various ACS survey measures shown below in Table 2. The results

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information is available upon request. We also replicate our analysis in the Supplemental Information using a more strict definition of a name match (requiring the first names to match completely or for the first name in one database to appear at the beginning of the first name in the other database). Results are similar.

[12] Records were considered successfully geocoded if the ArcGIS match score was greater than 85 (out of 100) and there was a unique street address for a given match score.

of this logit produce the predicted probability of criminal supervision measure that appears on the horizontal axis in the figure.[13] In other words, instead of showing how some attributes of the sample vary as a single covariate takes on different values along the x-axis, the x-axis in the figure is an index that is formed using the set of covariates listed above. To make the graph more readable, we restrict this analysis to the 97% of records for which the predicted probability of state supervision is less than or equal to .04.

Figure 1 about Here

On the left vertical axis, we plot three quantities as local polynomial curves: The proportion of the sample that votes (the dotted line), the proportion of the sample that is under state supervision among those who *did not* vote in 2010 (the dashed line, with 95% confidence interval), and the proportion of the sample that is under state supervision among those who *did* vote in 2010 (the solid line, also with 95% confidence interval). We also show average supervision rates in .001 width bins of the predicted supervision score for non-voters (the open circles) and voters (the plus signs).

Figure 1 shows data from our entire sample after removing cases with a predicted probability of supervision greater than 4%. Most of the data is to the left of the .01 hash mark on the x axis, which is the predicted probability of state supervision based only on an individual's gender, race, and place of residence (the diamonds are a rug display showing 100 percentiles of average predicted probability of criminal supervision). This risk is low for most individuals and fully 74% of the sample has a predicted risk of supervision of less than 1%.

Note that the rates of voting in 2010 are modest for the sample, starting at about 3.1%, but decline with predicted risk of criminality (to about 2.4% for people/places with predicted supervision scores of .04). Of greater theoretical interest is that *for every level of risk of state supervision, there is clear divergence in actual supervision between those who voted and those who did not*. For nearly every partition of the sample shown in the figure, *individuals who voted are less likely to later be incarcerated.*

[13] For women and those with unknown gender, some states have no individuals under state supervision. We assign these cases a predicted supervision score of 0.

For example, among those with a predicted risk score of less than .001, rates of incarceration are .13% for those who did not vote but only .05% for those who did vote, implying that when we compare voters and nonvoters with the same expected rate of supervision, voters are 58% less likely to be incarcerated. Given the large sample sizes, these differences are highly statistically significant. We see larger absolute differences across voters and non-voters for higher risks of criminal supervision. For example, when the predicted probability of supervision is between .009 and .011, 1% of non-voters but only .3% of voters are supervised. This difference is statistically significant and represents a proportional reduction in the chances of being in state custody of 71%. Overall, then, there is a clear pattern that those who vote are less likely to later be under state supervision than those who do not, even when we account for each individual's race, gender, and state of residence, as well as important demographic characteristics of the places where they live.

The data presented in Figure 1 are from our entire sample. However, one might be concerned that the results presented there may arise mechanically because incarcerated individuals cannot vote. Specifically, suppose some individuals in the sample were first incarcerated before the 2010 election and remain incarcerated now. They did not vote, but they could not have done so simply because they were detained. To rule out this alternative explanation for the effect of not voting on increased criminality, in the SI we repeat our graphical analysis for the three states where we know when individuals were first placed under state supervision and find similar patterns.

An alternative concern about the Figure 1 analysis is that incarceration is relatively rare and may not fully reflect the more granular effects of political participation on illegal behavior, for example by discouraging more minor transgressions that would be unlikely to result in a prison sentence. In the SI, we address this concern by focusing on a single state, Florida, for which our supervision records are far more expansive in scope (they include individuals currently and formerly incarcerated, as well as those assigned to non-prison programs and parole). We continue to find that those who vote less are more likely to end up under state supervision.

We confirm this graphical presentation in Table 2, which reports OLS coefficients in which we examine the association between individual-level participation and the probability of being under state supervision after accounting for individual-level race and gender, state of residence, and place-level ACS measures. In the column (1) specification, which does not include a measure of participation, we find that both African Americans and Hispanics are more likely to be under state supervision, women and those whose gender is unknown are less likely to be under supervision, and that standard measures of place-level disadvantage and crime (e.g., concentrated urban areas, etc.) are associated with increased risks of incarceration.

Table 2 about Here

In the remaining columns, we examine the association between voting in 2010 and being under state supervision for different subsets of the data. In column (2) we find that voting reduces the risk of being under state supervision by 55% ($p<.001$). In column (3) we restrict our analysis to those cases for which the predicted risk of state supervision (the horizontal axis in Figure 1) is less than .01. This group comprises about 70% of the sample. For this sample, which we will refer to as the low-risk sample, the average proportional effect estimate is comparable: voting reduces the risk of supervision by 57%. Column (4) measures the relationship between voting and the risk of being under state supervision for those with a predicted risk of being under supervision greater than .01; we refer to this group as the high-risk sample. Those in the high-risk sample who vote are 60% less likely to be under state supervision.[14] We use this partitioning by predicted risk of supervision in our analysis that follows to demonstrate that it is not just individuals who appear ex ante at a low risk of being incarcerated who respond to the outreach encouraging participation. Thus, the experimental intervention is effective in encouraging participation for individuals with both low and higher ex ante predicted rates of incarceration.

[14] In the SI, we show these results are robust to focusing on states where we know individuals were not under supervision during the 2010 election and to the inclusion of more low-level punishments. We also assess the relationship between registration and the likelihood of coming under state supervision.

Overall, these data suggest a strong association between political participation and the subsequent likelihood that someone is incarcerated. We find that voters are less likely to end up under state supervision than non-voters. What remains uncertain, however, is whether these associations are causal in nature, the question we take up in the next section.

Experimental Analysis: Randomly Induced Participation Does Not Reduce Criminality

This section is divided into two parts. The first describes the strategy we use to measure the causal effect of participation, and the second presents the experimental estimates of this causal effect.

Identification Strategy

In this sub-section we formalize the problem of measuring the causal effect of voting on subsequent incarceration.[15] We first introduce some notation and we then use this notation to show that under standard assumptions an experiment can yield a consistent (asymptotically unbiased) estimate of the causal effect of voting.[16] Readers who are familiar with the technical assumptions of causal

[15] We assume the effect of participation occurs via voting, rather than through registering, because the prior theoretical literature stresses how activities that are deliberative and participatory—like voting—are transformative. In the SI, we present parallel analyses assuming that registration is the relevant participatory act. Note that our experiment, or any experiment that perturbs both registration and voting, cannot be used to ascertain through which causal pathway these effects might manifest. This is one reason that an explicit statement of the exclusion restriction must build on theory. In observational research, by contrast, the analysis *presumes* as-if random assignment without a mechanism for generating random variation in any potential treatment (e.g., voting).

[16] For those familiar with experiments with non-compliance, the setup we face is equivalent to an experiment with two-way non-compliance. Treatment is voting and some assigned to the treatment group do not vote (are untreated) while some assigned to the control group are treated (do vote). For a

identification in the potential outcomes framework using randomized experiments may wish to skip ahead.

Let Yi(Vi,Zi) represent whether individual i is subsequently incarcerated (Yi = 1 if incarcerated, 0 otherwise) given Vi (which is equal to 1 if the individual votes, 0 otherwise) and Zi (which is equal to 1 if the individual is in the experimental treatment group, 0 otherwise). Y is referred to as a potential outcome for individual i and Yi may take on different values depending on Vi and Zi. We will assume that Y, criminality, may be a function of whether the individual votes, V, but that, given V, Y does not depend directly on Z (recall that Z describes whether the subject was assigned to the treatment or control group). This assumption, our assumption 1, is referred to as the *exclusion restriction,* as it implies that we can write Y as Y(V), excluding Z from the expression.

In words, the exclusion restriction requires that the effect of the treatment assignment, Z, does not affect Y except through its effect on V, whether the subject votes or not. Applying this to the logic of causal influence in our study, what differs for subjects when Z=1 versus 0 is that a subject in the treatment group is sent a mailing encouraging participation that the control group does not receive. Our exclusion restriction assumption is therefore that receiving the mailing does not alter the propensity to take criminal actions that lead to state supervision, except (possibly) if the mailing alters the subject's voting behavior.

Using the exclusion restriction, we may define the average causal effect of voting on criminality as:

(1) $E(Yi(Vi=1)) - E(Yi(Vi=0)) = E(Yi(1) - Yi(0))$

where the function E(X) represents the population average of X.[17]

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discussion of identification and estimation of causal effects when there is two sided non-compliance, see Gerber and Green (2012), chapter 6.

[17] Our notation assumes that what matters for the subject's treatment and outcome is the subject's own assignment to treatment and the subject's own voting (and not that of the other subjects). This research

Our goal is to use the data we are able to gather from an experiment to estimate a causal effect of voting. To do this we must link the average causal effect of voting (equation 1) to population quantities we are able to estimate from the experimental design. A first step is to categorize the subjects according to how their voting behavior responds to assignment to treatment (Z=1) or control (Z=0). We consider three patterns of individual response:

1. *Always Voters* (type = AV), who vote whether assigned to treatment or control,

2. *Never Voters* (type = NV) who do not vote in either treatment or control, and

3. *Compliers* (type=C), who vote when assigned to treatment but do not vote when assigned to control.

There is an additional pattern, in which subjects vote when they are not contacted about participation (Z=0), but are induced to *not* vote if they are provided materials to facilitate participation (Z=1). We assume that this perverse sub-group can be ignored. Formally, this is the assumption of *monotonicity*, which is our second identification assumption. Monotonicity (the assumption that treatment assignment affects V in one direction) is summarized as the idea that assignment to the treatment group either has no effect on the subjects' decision to vote or that it encourages voting.

Next, we may decompose the population average of Y for the subjects when assigned to the treatment group (dropping the "i" subscripts) as:

(1.1) $E(Y|Z=1) = E(Y(V=1)|\text{subject is an AV})*P(AV) + E(Y(V=0|\text{subject is a NV})*P(NV) + E(Y(1)|\text{subject is C})*P(C)$,

where P(x) is the proportion of the subjects who are type x.

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approach follows the assumptions in prior empirical investigations and in the relevant theoretical accounts, which articulate mechanisms for a direct effect of participation on that individual's mindset and habits. The estimates presented here are properly viewed as estimates under the assumption of direct, but not indirect, effects.

Similarly, we may decompose the population average of Y for subjects when assigned to the control group as:

(1.2) $E(Y|Z=0) = E(Y(V=1)|\text{subject is an AV})*P(AV) + E(Y(V=0|\text{subject is a NV})*P(NV) + E(Y(0)|\text{subject is C})*P(C)$.

Notice that the only source of difference in the population average Y for the subjects when assigned to the treatment group versus control group is due to the change in behavior of the compliers, the type of individuals who vote only if assigned to the treatment group (the last term in each equation).[18] An implication of this is that we can only estimate the causal effect for this subgroup of individuals, the compliers; since the other types are only observed either voting or not voting, we never produce the data required to estimate Y in both the voting and non-voting state for these types. Algebraically this is seen when we examine the difference between the population averages given in (1.1) and (1.2):

(2) $E(Y|Z=1) – E(Y|Z=0) =$

$E(Y(1)|\text{subject is C})*P(C) - E(Y(0)|\text{subject is C})*P(C) =$

$[E(Y(1)|\text{subject is C}) - E(Y(0)|\text{subject is C})]*P(C) =$

$[E(Y(1)-Y(0)) \mid \text{subject is C}]*P(C)$.

Rearranging (2), the average causal effect on Y of voting for compliers can be expressed as:

(3) $[E(Y(1)-Y(0)|\text{subject is C}] = [E(Y|Z=1) – E(Y|Z=0)] / P(C)$.

Under the stated assumptions, we can estimate the causal effect of voting on criminality if we can estimate $[E(Y|Z=1) – E(Y|Z=0)]$ divided by the proportion of compliers in the population, P(C). Turning first to the numerator of this expression, when the treatment and control groups are formed by random assignment, an unbiased estimator of the average difference in the outcome when assigned to treatment and control for the population of subjects, $[E(Y|Z=1) – E(Y|Z=0)]$, is the sample difference of means. This quantity can be estimated by the slope coefficient from a regression of Yi on Zi, the subject's

[18] Note that random assignment means that, in expectation, the proportion of each type of subject is the same in both the treatment and control groups.

incarceration status regressed on the subject's group assignment. This quantity is also known as the experiment's ITT, the *intent to treat* effect.

Similarly, an unbiased estimate for the proportion of compliers in the population, P(C), is the difference in the share of the subjects who are actually treated (that is, who voted) in the treatment group minus the share voting in the control group. We note that this language may seem awkward, in that "treated" here takes on a different meaning. It is not simply whether one was in the treatment group, but rather whether one votes, which is the behavior of all *Always Voters* as well as the *Compliers* in the treatment group. The quantity P(C) can be estimated by the slope coefficient from a regression of Vi, treatment status, on Zi, individual group assignment. We will call this estimate α. A consistent estimate of the complier average causal effect (CACE) is obtained by calculating the ratio of these two differences in sample means.

(4) $\text{CACE} = \text{ITT}/\ \alpha.$

Rather than separately estimating the quantities to form this ratio, the CACE and its standard errors can be estimated directly as a two-stage least squares model in which Y is a function of V and Z, and the experimental group assignment (Z) is used as an instrument for V. Additional precision can be gained by including covariates in the first and second stage regression models.[19]

Experimental Estimates

Mann (2011) provides complete details about the experiment described above and analysis focused on the effects of the outreach on registration and participation. Here, we present analyses for the subset of the original data used in the remainder of this paper (those living in states where we obtained records of criminal supervision, with complete birthdays, and with successful geocoding for inclusion of the ACS measures).

[19] Although there is a theoretical possibility of introducing bias by adding covariates as controls, recent work suggests that this is often not an important practical concern (See Freedman 2008, Lin 2013, and Schochet 2010). In the SI, we present parallel analysis without covariates.

Table 3 about Here

Table 3 presents statistical analysis of the effect of the VPC's mailing efforts on voting in 2010.[20] These are the "first-stage" estimates in a two-stage least squares estimator. (For readers who are interested, we present parallel estimates using rare events logit in the Supporting Information.) In columns (1) and (2), we present the results for the entire sample. Models are shown both with (column [1]) and without (column [2]) covariates. In this sample, we find that 2.5% of the control group voted, and turnout was .5 percentage points higher among those sent the registration mailing ($p<.001$). This corresponds to an increase in voting of about 19%. We note that these effects are estimated with great precision; the t-statistics for the treatment effects are almost 7. Notice that in this sample the F-statistic for the first stage is well above the rule of thumb value of 10 suggested to avoid bias due to weak instruments (Staiger and Stock 1997). (See the next section for an extended discussion of the precision of our two stage least squares estimates.)

Before presenting the second-stage analysis, however, we consider the effect of the intervention for the low-risk and high-risk samples analyzed in Table 2. Columns (3) and (4) present this analysis for the low-risk sample. Among this sample, the treatment assignment has an effect on voting that is comparable to that found for the sample as a whole, and group assignment is highly statistically significant. For the high risk sample, treatment assignment increased voting by .3 percentage points (column [5] and [6], 12.9%, $p<.05$). This is a somewhat smaller absolute and proportional effect than for the entire sample, suggesting that more at-risk individuals are less likely to participate absent outreach, and less likely to participate in reaction to these outreach efforts.

We can now estimate the quantities necessary to answer our primary question of interest: Does participation reduce the risk of becoming intertwined with the criminal justice system? There are several ways to undertake this analysis, and so we present multiple approaches here. The key intuition that joins

[20] For all experimental analysis, we report standard errors clustered at the household level, because randomization took place at that level.

all of these approaches is that, unlike our observational analysis above, here we are not relying on naturalistic variation in participation. That is, whereas before we could not ascertain whether participation reduced criminality or if instead some other factor explained both a greater proclivity to participate in politics and a reduced risk of committing a crime, here we take advantage of the fact that the experiment randomly assigned some individuals to receive the outreach letter. Being sent the outreach letter is therefore unrelated to any differences in expected participation and criminality. But because being sent the letter is associated with an increase in voting, this randomly induced variation in participation allows us to assess the effect of participation on the risk of being incarcerated.

First, we estimate a reduced form model, in which we assess the relationship between being sent the treatment letter and the risk of being under state supervision. This is the estimate of the experiment's ITT effect. This model, estimated for different subsets of the sample, appears in columns (1), (3), and (5) of Table 4. This allows us to assess the direct relationship between being randomly assigned to receive a mailing encouraging participation and the subsequent risk of being under state supervision. It is agnostic as to the mechanism by which such a relationship could take place. As the OLS coefficients shown in the Table make clear, however, we do not find that the experimental intervention reduces the risk of criminality: For the entire sample (column [1]), the low risk sample (column [3]), and the high risk sample (column [5]), we find that being treated is associated with either a slight increase in the risk of being under state supervision (although none of these effects aside from the low-risk sample are statistically significant at conventional levels) or a very small negative point estimate (with a t-statistic of close to zero). Parallel analysis for these reduced form estimates using rare events logit appears in the Supporting Information.

Table 4 about Here

Second, we can assess the direct relationship between voting and reduced criminality by undertaking an instrumental variables analysis. This approach relies on the fact that the experimental intervention, as we know from the Table 3 analysis, increased voting. If we assume that change in participation is the only mechanism by which the mailing can affect future criminality (that is, if the

letters did not themselves reduce the risk of criminal behavior, but could instead do so only by altering engagement with the political system through voting, which is the exclusion restriction assumption described above), we can use the experimental design to estimate the effect of participation on criminality without the risk of bias that is present in our observational analysis due to unobserved factors that affect both behaviors. This is the estimate of the experiment's CACE, formalized above.

In the instrumental variables analysis, we first predict the probability of voting using the same specification presented in Table 3. We then use predicted participation, with exogenous variation induced by the randomly assigned experimental treatment, along with the other control variables shown in Table 4, to predict differences in the risk of state supervision. These two-stage least squares estimates also do not provide any evidence that increasing participation reduces the risk of criminal behavior. In column (2) we estimate that voting *increases* the risk of supervision (by 9.2 percentage points), but the effect is imprecisely estimated and indistinguishable from 0.

With the exception of the low-risk sample, this pattern—positive or approximately zero, but imprecise, estimates for the effect of participation on criminality—is repeated for the other models in Table 4. The only statistically significant finding is that, for the low-risk sample, voting is *positively* associated with criminality (column [4]). We interpret this unexpected relationship as due to chance, and view it as an unlikely finding if, in fact, the true effect of participation was a material decrease in criminality. In sum, there is no evidence that experimentally increasing participation is associated with reductions in criminal behavior. In the SI, we report virtually identical results when we limit the analysis to states for which we have information on the start dates of incarceration or lower-level punishments.

Overall, these results are sobering. While experimental outreach can increase political participation for this sample of youth, there is no evidence that it reduces criminality. The observational correlation between participating and avoiding being incarcerated is therefore not a causal one, but instead most likely the result of the joint effect of some omitted factor on both rates of participating and a tendency to obey the law. In an experimental setting in which participation is randomly manipulated,

inducing individuals to participate makes them no less likely to violate the law than those who were not contacted.

Implications for Estimates of the Relationship Between Participation and Criminality

Our experimental estimates of the effect of voting on subsequent criminality are positive and statistically insignificant. (The reduced forms estimates of the direct effect of the mailing are very close to zero). This leads naturally to a question of how these new findings should be incorporated into our beliefs about the causal effect of political participation on criminality that were formed based on prior research. For example, Uggen and Manza (2004; Figure 2) present analysis showing that those who had never previously been arrested and who voted in 1996 are 6.6 points less likely to be arrested between 1997 and 2000 than those who did not vote in 1996. This comparison has a 95% confidence interval of approximately -3.6 to -12.4 points. Our instrumental variables estimate of the effect of voting on subsequent incarceration for the entire sample, by contrast, is a positive 9.2 points with a 95% confidence interval of approximately -7.3 to 16.4 points. Thus, the 95% confidence for our experimental estimate includes their point estimate. (If we focus on the low risk sample, which is perhaps analogous to those who have never previously been arrested, our estimate is instead 11.6 points with a 95% confidence interval of 1.2 to 22.0 points.) How should we think about what the new study adds to existing evidence?

One possible approach is to ignore any concerns about unobserved heterogeneity as a potential explanation for the result reported in Uggen and Manza and simply incorporate the information from each study as independent estimates of some unobserved population coefficient, which is the true effect of voting on criminality. While deciding how to weight different studies is difficult, if given equal weight, the ex post estimate would be that the true population coefficient for how voting affects subsequent criminality is that it increases it by a statistically insignificant 1.3 points (95% confidence interval about -7 to +10 points). This would lead us to conclude there is no causal relationship between voting and criminality. Alternatively, we might give the prior research greater weight because it is apparently more precise, for example by weighting it four times as much as the new experimental estimate, in which case

our joint point estimate would be -3.3 points with a 95% confidence interval from -8 to 1 point. In either case, the implications are qualitatively the same: On equal or even reduced footing, these results would cause us to revise downward (toward zero) our estimates of the causal effect of voting on criminality.

Although it is common to combine research in this fashion (see, for example, Lau et al.'s [1999] meta-analysis of the effect of negative advertising), these approaches are likely to be fundamentally misleading. That is because the estimates reported in the prior research are not unbiased because they cannot account for unobserved heterogeneity. That is, even if we had a very large population of individuals and estimated an extremely precise (i.e., standard errors near 0) negative correlation between voting and subsequent criminality, this coefficient would not be informative of the causal effect of interest because it is a combination of the true causal effect and a bias term of unknown and uncertain magnitude. For this reason, in forming an update about causal effects, one should not be misled by the apparent precision of prior observational research, and is instead justified in treating the estimated correlation as largely uninformative about the causal effect of interest (Gerber et al. 2004). How do we know this is a problem in our application? For one, our own observational data shows a robust (negative) correlation between voting and subsequent criminality, but that result is not sustained when we leverage our experimental design. For another, Uggen and Manza find that their estimates are fragile: As they make more efforts to control for unobserved factors that explain differences between those who vote and those who do not, their estimates decay toward 0 (and are no longer statistically significant in some specifications). For this reason, if asked to give a best estimate, we would suggest our data provide an unbiased estimate that is statistically indistinguishable from 0.

What does our result mean in practical terms? One simple way to answer this question is to ask, given our estimate, what is the chance that the true effect is negative (that is, that voting does reduce criminality)? We can calculate this quantity from our experimental design using an exact randomization test where we simulate random assignment to treatment and estimate the reduced form relationship between treatment and criminality. If we adjust this quantity by the observed difference in voting between the actual treatment and control groups (the first stage of the two-squares least squares estimator), we can

calculate the expected distribution of the two-stage least squares estimate of the effect of voting on criminality under the sharp-null of no treatment effect. In 100,000 simulations, this is less than our observed coefficient 86% of the time, implying a p-value of .14. If we apply a similar sampling distribution to our +9.1 point estimate, we calculate that the probability we would observe this estimate given a true coefficient of -6.6 or larger (more negative) is less than .03. Thus, our estimate alone, despite its apparent imprecision, is powerful enough to safely reject the large negative estimate found in prior work.

Discussion and Conclusion

A classical justification for democracy is that self-government is more than just a method of preference aggregation; democracy also shapes citizens. Engaged citizenship results in the development of a set of skills that facilitates productive group interactions. The give and take of public debate teaches respect for difference, while the need to persuade others encourages empathy by rewarding those who learn to see the world though their fellow citizens' eyes. Empirical scholars have built on these insights to suggest that participation may be a means to encourage development of connections to the community and a pro-social mindset, and to thereby prevent criminality and reduce recidivism. Prior work has provided highly suggestive evidence in support of the posited relationship between participation and criminality. It is common to find a robust negative correlation between voting (and other forms of participation) and the risk of criminal behavior. In our analysis we confirm this correlational analysis using a large sample of youth whose participation and state supervision status are measured using administrative records. As in prior research, those who vote are less likely to become criminals than non-voters. The reduction in risk is both substantial and statistically significant.

There are, however, important methodological reasons for caution in interpreting this relationship as a causal one. For example, it could well be that voting has no causal effect but is merely a marker for factors that cause an individual to be less likely to engage in criminal activity. In light of the real possibility of spurious results, we revisit the link between voting and criminality and report results from a

large-scale field experiment in which young adults were randomly encouraged to register and vote. We find that this intervention meaningfully increases the propensity to participate in politics, but causes no reduction in subsequent criminal behavior. It appears that participation, in the form of voting, is very likely not an effective mechanism for reducing criminality.

Our experimental results have several implications. First, it does not appear that the *direct* effect of voting reduces criminal activity. Contrary to the observational findings, "the 'mere' act of voting" (Uggen and Manza 2004: 200) is not enough to produce a measurable causal effect on criminal behavior. Second, it does not appear that voting triggers a virtuous cycle of subsequent actions and attitude formation (a mechanism posited for the large changes produced by apparently "minor" psychological interventions) which leads to a large behavioral change from a (relatively) small participatory act. Third, the null findings suggest the value of re-examining how theoretical arguments for why voting might produce pro-social behavior have been deployed in this area. Upon re-consideration, it appears that the finding that voting does not transform the citizen is, in fact, quite consonant with the writings of leading democratic theorists. In the work of Rousseau, Mill, and others, participation may include the act of voting, but is much more than simply going to the polls. Thus, it may be that voting is "a step in the right direction" toward participation, but it is too weak a form of participation to encourage the rich individual development necessary to become a better citizen.

In the work of Pitkin (Pitkin and Schumer 1982) and others, the clearest case for the benefits of participation often seem to arise from forms of participation closer to governing rather than voting. That is, what causes one to fully develop as a citizen is participation in the process of debating, governing, compromising, etc., all of which are features of a deeper form of participatory democracy than simply showing up to cast an anonymous ballot in a November election (Pateman 1970). However, the empirical work inspired by these theories may have focused too much on the robust, though possibly spurious, association between voting and other outcomes. This suggests researchers might expand the scope of participation to include this richer account of citizenship, and then look for the causal effects of this deeper participation. If pursued based on observational methods, this possible analysis raises the same

thorny methodological questions about whether those who participate in this way are already different from those who do not. Thus one avenue for exploring this question is to seek efforts to experimentally encourage these far "deeper" forms of participation and understand their long-term effects.[21]

We present the first large-scale experimental assessment of the effects on criminality of randomly induced participation. The experimental findings are sharply different from those of the observational benchmark. Our experimental analysis illustrates the value in undertaking experimental tests of programmatic interventions that observational analysis suggests produce desirable results. The observational work may be suggestive of causality and, when the relationship demonstrated is very strong and theoretically grounded, the evidence may be quite convincing. However, in the case currently under review, we had both a strong association and theoretical support, and thus our negative findings may be read as a cautionary tale. The observational data cannot easily account for the myriad ways in which voters are different from non-voters, apart from their simple propensity to participate in politics.

It is important to note that our experiment is limited in several important ways. First, our estimates, like all analysis, rely on certain identification assumptions. In particular, we invoke an exclusion restriction, which requires that receiving a mailing has no direct effect on subsequent criminal behavior apart from the possible changes catalyzed by enhanced political participation as voting. It should be noted that if getting a letter does directly reduce criminality, our results would over-estimate the benefits of participation, which were found to be zero. Second, it is possible that participation does reduce criminality, but voting is too weak a form of participation to have the salutary effects ascribed to democratic engagement. Perhaps it would be better to teach the skills of deliberation and engagement, if such skills are the means by which individuals come to appreciate the process of lawful governance. Third, the subjects are a single population (young adults of color) who were targeted only for a very

[21] For example, Wantchekon (2012) reports the effect on turnout of a large-scale deliberation experiment in Benin, which is arguably a more engaged form of political participation than voting.

minimal intervention (a single mailing before the 2010 election). We do not know if similar results would hold for different populations or different outreach efforts.

Fourth, we estimate the complier average causal effect of the intervention. Although this is arguably the population of greatest relevance for those contemplating an intervention to encourage participation, other experiments might induce a different set of individuals to participate, and the effect of participation for these individuals might be different from that we find resulting from the intervention we study. Finally, although our findings clearly suggest that there is no empirical relationship between encouraging voting and reduced criminality, it would be useful to replicate this study to increase the precision of the estimated effects. Although our study leads us to conclude that the best guess of the effect of voting on criminality is zero, definitively ruling out the possibility of modest deterrent effects requires an even larger study than the one we analyze here, or the combination of several studies of similar size.

These concerns aside, our work presents the first large-scale experimental test of the relationship between political participation and subsequent involvement with the criminal justice system. Additionally, departing from prior work, we use administrative records to measure both participation and state supervision. Despite compelling observational findings and the allure of low-cost efforts to reduce criminality, we find no evidence that experimentally increasing participation is associated with reductions in criminal behavior. Unfortunately, while voting may be good for people, it does not appear to stop them from becoming criminals.

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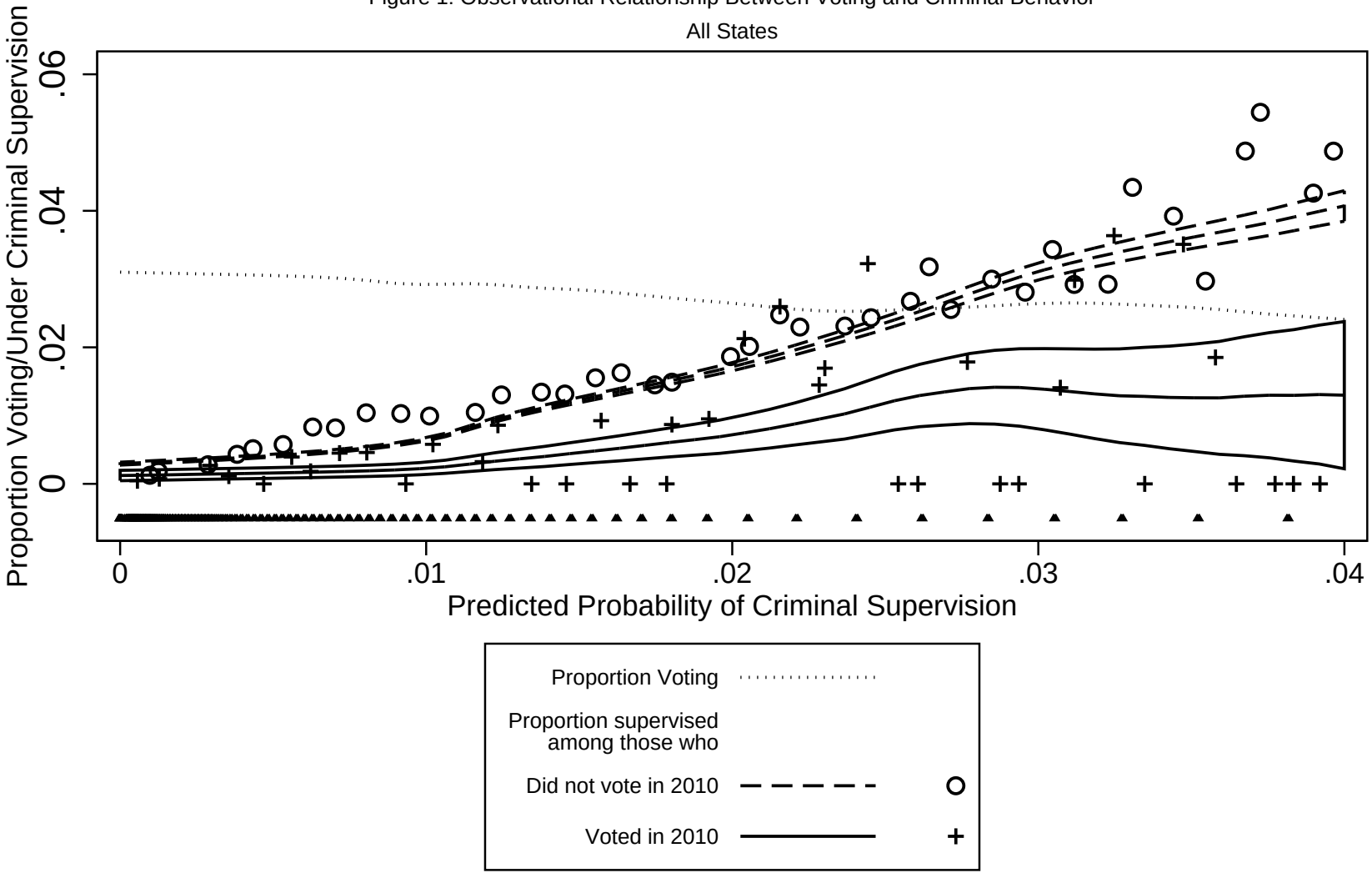
Figure 1: Observational Relationship Between Voting and Criminal Behavior

All States



Lines are local polynomials, with 95% confidence intervals for proportion under criminal supervision. Scatter plot is average in .001 unit bins. Analysis is for 97% of sample with Predicted Probability of Criminal Supervision<=.04. Diamonds are distribution of data on X axis by percentile.

Table 1: Field Experiment Sample and Number and Proportion of Records Matched to Supervision Records, by State

State	Number of individuals in field experiment	Individuals with Supervision Record Matches	Proportion of Individuals Matched
AZ	26,522	142	.0054
CO	14,870	85	.0057
FL	87,057	2,225	.0256
IL	72,513	754	.0104
MD	47,224	246	.0052
MO	19,501	350	.0179
NM	12,404	2	.0002
OH	41,184	410	.0100
PA	42,219	272	.0064
TX	186,684	581	.0031
WA	17,923	66	.0037
Overall	568,101	5133	.0090

Note: See text for details of matching procedure.

Table 2: Observational Benchmark: Relationship Between Voting in 2010 and Subsequent State Supervision

	(1)	(2)	(3)	(4)
	Under State Supervision (1=yes)		Under State Supervision (1=yes), Low-Risk Sample	Under State Supervision (1=yes), High-Risk Sample
Voted in 2010 (1=yes)		-0.0048	-0.0020	-0.0147
		[0.000443]***	[0.000343]***	[0.001561]***
African American (1=yes)	0.0069	0.0069	0.0032	0.0281
	[0.000304]***	[0.000305]***	[0.000270]***	[0.012015]**
Hispanic (1=yes)	0.0036	0.0036	0.0024	0.0120
	[0.000356]***	[0.000356]***	[0.000317]***	[0.012106]
Female (1=yes)	-0.0124	-0.0124	-0.0051	-0.0375
	[0.000283]***	[0.000283]***	[0.000251]***	[0.001489]***
Gender Unknown (1=yes)	-0.0071	-0.0071	-0.0018	-0.0173
	[0.000445]***	[0.000445]***	[0.000369]***	[0.001297]***
Proportion Black	0.0028	0.0028	0.0013	0.0012
	[0.000657]***	[0.000657]***	[0.000502]**	[0.001881]
Proportion Hispanic	-0.0064	-0.0064	-0.0008	-0.0183
	[0.000783]***	[0.000783]***	[0.000610]	[0.003163]***
Proportion of Kids < 18 in Female Headed Household	0.0037	0.0036	0.0011	0.0096
	[0.000741]***	[0.000741]***	[0.000550]**	[0.002165]***
Proportion of Families Below the Poverty Rate	0.0038	0.0037	-0.0006	0.0112
	[0.001157]***	[0.001157]***	[0.000812]	[0.003260]***
Proportion of Families Receiving Public Assistance	0.0044	0.0043	0.0002	0.0175
	[0.002957]	[0.002957]	[0.002166]	[0.007843]**
Proportion of Population Over 25 w/. < High School	0.0125	0.0124	0.0032	0.0322
	[0.001236]***	[0.001236]***	[0.000915]***	[0.004062]***
Log Pop. Density (1000 persons per sq mi.)	-0.0002	-0.0002	0.0000	-0.0008
	[0.000098]**	[0.000098]**	[0.000071]	[0.000388]**
Constant	0.0024	0.0025	0.0022	-0.0341
	[0.000340]***	[0.000341]***	[0.000306]***	[0.012239]***
Observations	552525	552525	411477	141048
R^2	0.013	0.013	0.002	0.010
Mean of Outcome in Sample	0.009	0.009	0.003	0.024
Includes State Fixed Effects?	Yes	Yes	Yes	Yes

Note: Cell entries are OLS regression coefficients with robust (Huber/White) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table 3: Experimental Estimates: Effect of Outreach on 2010 Participation

	(1)	(2)	(3)	(4)	(5)	(6)
	Voted in 2010 (1=yes)		Voted in 2010 (1=yes), Low-Risk Sample		Voted in 2010 (1=yes), High-Risk Sample	
Treated (Sent Registration Form 2010, 1=yes)	0.0049	0.0049	0.0055	0.0055	0.0030	0.0030
	[0.000713]***	[0.000714]***	[0.000836]***	[0.000839]***	[0.001348]**	[0.001350]**
African American (1=yes)	0.0101		0.0102		0.0194	
	[0.000835]***		[0.000910]***		[0.001075]***	
Hispanic (1=yes)	-0.0023		-0.0031		0.0135	
	[0.000911]**		[0.000950]***		[0.001795]***	
Female (1=yes)	0.0043		0.0041		0.0034	
	[0.000501]***		[0.000652]***		[0.001474]**	
Gender Unknown (1=yes)	-0.0037		-0.0052		0.0001	
	[0.000639]***		[0.000834]***		[0.001177]	
Proportion Black	-0.0019		-0.0017		-0.0021	
	[0.001111]*		[0.001389]		[0.001969]	
Proportion Hispanic	0.0021		0.0034		-0.0023	
	[0.001458]		[0.001677]**		[0.003271]	
Proportion of Kids < 18 in Female Headed Household	-0.0102		-0.0111		-0.0078	
	[0.001213]***		[0.001476]***		[0.002113]***	
Proportion of Families Below the Poverty Rate	-0.0126		-0.0137		-0.0096	
	[0.001772]***		[0.002192]***		[0.003006]***	
Proportion of Families Receiving Public Assistance	-0.0101		-0.0144		-0.0054	
	[0.004469]**		[0.005551]***		[0.007618]	
Proportion of Population Over 25 w/. < High School	-0.0179		-0.0174		-0.0196	
	[0.002015]***		[0.002434]***		[0.003740]***	
Log Pop. Density (1000 persons per sq mi.)	-0.0009		-0.0006		-0.0023	
	[0.000207]***		[0.000234]**		[0.000451]***	
Constant	0.0299	0.0251	0.0294	0.0257	0.0233	0.0233
	[0.001148]***	[0.000672]***	[0.001245]***	[0.000788]***	[0.003454]***	[0.001272]***
Observations	552525	552525	411477	411477	141048	141048
R^2	0.006	0.000	0.007	0.000	0.005	0.000
F-test Statistic	46.94	46.09	43.61	42.63	4.97	5.06
F-test p-value	0.000	0.000	0.000	0.000	0.026	0.021
Mean of Outcome in Sample	0.000	0.000	0.000	0.000	0.000	0.025
Includes State Fixed Effects?	Yes	No	Yes	No	Yes	No

Note: Cell entries are OLS regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table 4: Experimental Estimates: Effect of Outreach and Participation on Subsequent State Supervision

	(1)	(2)	(3)	(4)	(5)	(6)
	Under State Supervision (1=yes)	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes)	Under State Supervision (1=yes), low risk sample	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes), low risk sample	Under State Supervision (1=yes), high risk sample	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes), high risk sample
Treated (Sent Registration Form 2010, 1=yes)	0.0004		0.0006		-0.0001	
	[0.000406]		[0.000280]**		[0.001369]	
Voted in 2010 (1=yes)		0.0918		0.1160		-0.0192
		[0.084403]		[0.053516]**		[0.455307]
African American (1=yes)	0.0069	0.0059	0.0031	0.0020	0.0278	0.0282
	[0.000305]***	[0.000914]***	[0.000270]***	[0.000621]***	[0.012024]**	[0.014784]*
Hispanic (1=yes)	0.0036	0.0039	0.0024	0.0028	0.0118	0.0121
	[0.000356]***	[0.000416]***	[0.000317]***	[0.000374]***	[0.012115]	[0.013463]
Female (1=yes)	-0.0124	-0.0128	-0.0051	-0.0056	-0.0375	-0.0375
	[0.000283]***	[0.000462]***	[0.000251]***	[0.000346]***	[0.001489]***	[0.002178]***
Gender Unknown (1=yes)	-0.0071	-0.0068	-0.0018	-0.0012	-0.0173	-0.0173
	[0.000445]***	[0.000547]***	[0.000369]***	[0.000478]**	[0.001299]***	[0.001300]***
Proportion Black	0.0028	0.0030	0.0013	0.0015	0.0012	0.0012
	[0.000659]***	[0.000685]***	[0.000502]**	[0.000537]***	[0.001886]	[0.002109]
Proportion Hispanic	-0.0064	-0.0066	-0.0008	-0.0012	-0.0183	-0.0183
	[0.000785]***	[0.000815]***	[0.000611]	[0.000661]*	[0.003167]***	[0.003319]***
Proportion of Kids < 18 in Female Headed Household	0.0037	0.0046	0.0011	0.0024	0.0097	0.0096
	[0.000740]***	[0.001153]***	[0.000550]**	[0.000834]***	[0.002163]***	[0.004197]**
Proportion of Families Below the Poverty Rate	0.0038	0.0049	-0.0006	0.0010	0.0113	0.0111
	[0.001160]***	[0.001581]***	[0.000813]	[0.001118]	[0.003268]***	[0.005471]**
Proportion of Families Receiving Public Assistance	0.0044	0.0053	0.0002	0.0019	0.0176	0.0175
	[0.002958]	[0.003106]*	[0.002166]	[0.002384]	[0.007846]**	[0.008215]**
Proportion of Population Over 25 w/. < High School	0.0125	0.0142	0.0032	0.0052	0.0325	0.0321
	[0.001237]***	[0.001961]***	[0.000916]***	[0.001342]***	[0.004063]***	[0.009761]***
Log Pop. Density (1000 persons per sq mi.)	-0.0002	-0.0001	0.0000	0.0001	-0.0007	-0.0008
	[0.000098]**	[0.000127]	[0.000072]	[0.000082]	[0.000389]*	[0.001128]
Constant	0.0020	-0.0008	0.0015	-0.0019	-0.0344	-0.0340
	[0.000499]***	[0.002919]	[0.000398]***	[0.001868]	[0.012334]***	[0.017199]**
Observations	552525	552525	411477	411477	141048	141048
R^2	0.013		0.002		0.010	
Mean of Outcome in Sample	0.009	0.009	0.003	0.003	0.024	0.024
Includes State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes

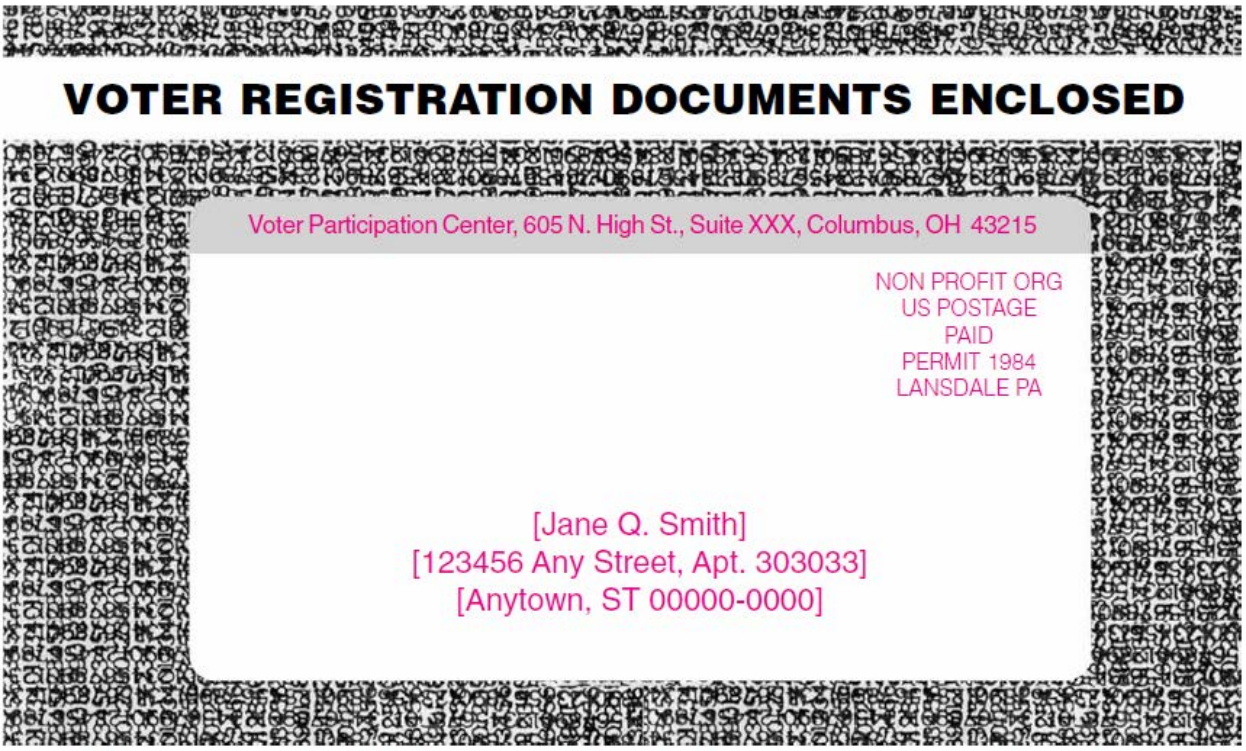
Note: Cell entries are regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01. In even numbered columns, these are second stage estimates from two-staged least squares estimates. See Table 3 for first stage results.

Supporting Information for:

Can Political Participation Prevent Crime?
Results from a Field Experiment about Citizenship, Participation, and Criminality

Sample Mailing (Ohio)

VOTER REGISTRATION DOCUMENTS ENCLOSED



Please return to the Ohio Secretary of State in the enclosed postage-paid envelope.

Important Voter Registration Information Inside

OPEN HERE

Register to vote today!

If you want to vote in this election you must be registered. That is why the Voter Participation Center is sending you this **official application form.**

Registering to vote is easy. Just mail in this form.

Review the attached form and instructions. Make sure your information is correct and complete. If you need to make corrections, you can write them directly on the application. Then sign and date the form and return it in the envelope provided.

Privacy is protected. If you use the enclosed envelope, your application will be delivered directly to your state elections office and no one else will see the contents.

Voting is important. Registering to vote is voluntary, but to vote, you must be registered. Act today. **Don't delay.**

Already Registered?

If you are not sure if you are registered to vote at your current address, you can check by visiting http://www.sos.state.oh.us/elections.aspx or by calling (614) 466-2585.

If you have already registered at your current address, there is no need to submit this application.

If you wish to be removed from our mailing list, please email this number: XXXXXXXX to info@voterparticipationcenter.org.

Complete and remove the attached form and mail in the postage-paid envelope today.

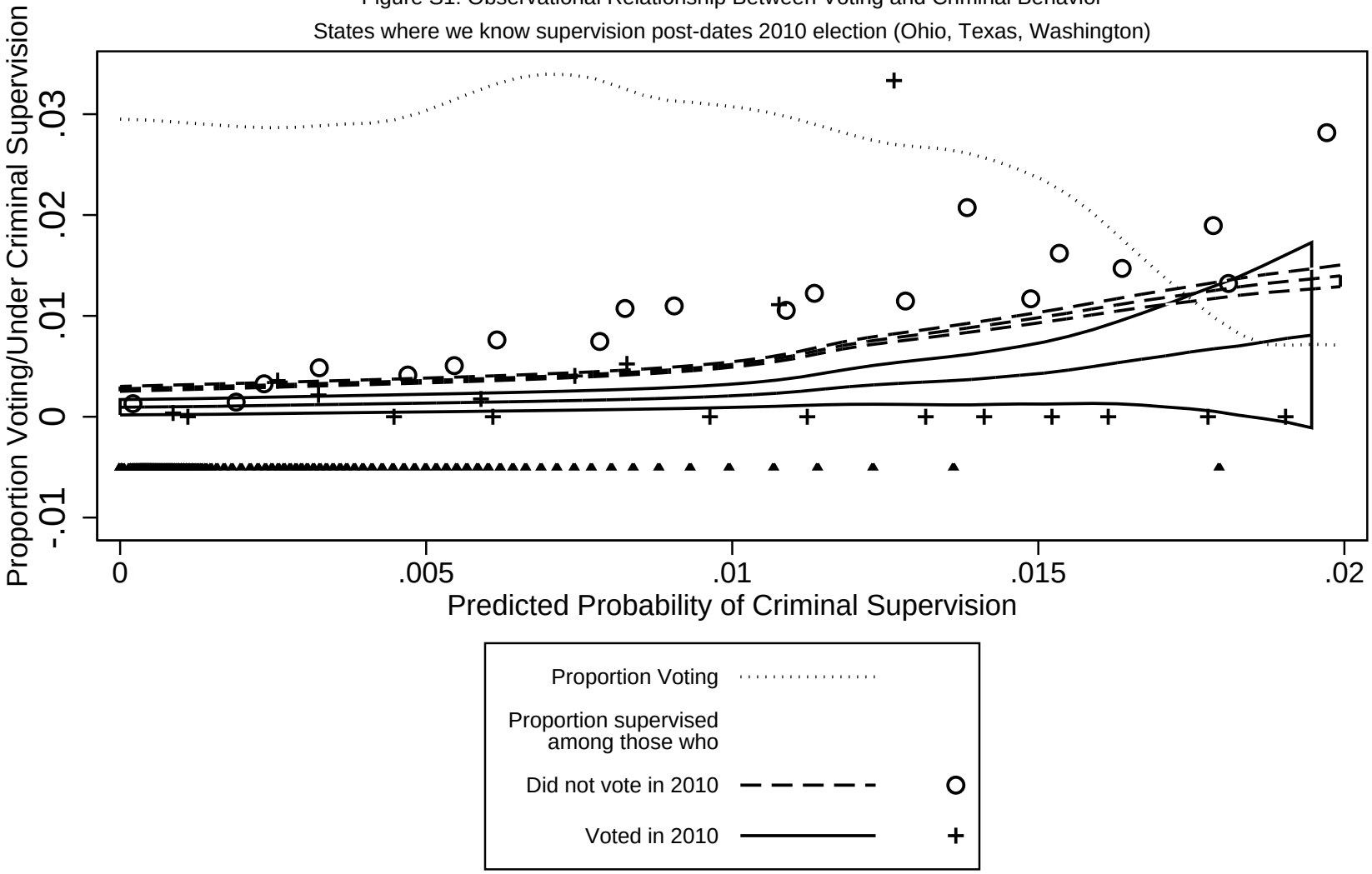
Complete and remove this form and mail in the attached envelope today!

The cost of the mailing and postage has been authorized by the Voter Participation Center, a project of Women's Voices. Women Vote. Women's Voices. Women Vote is a nonprofit, nonpartisan organization that does not support any candidate or party.

Voter Participation Center
605 N. High St.
Suite XXX
Columbus, OH 43215

Figure S1: Observational Relationship Between Voting and Criminal Behavior

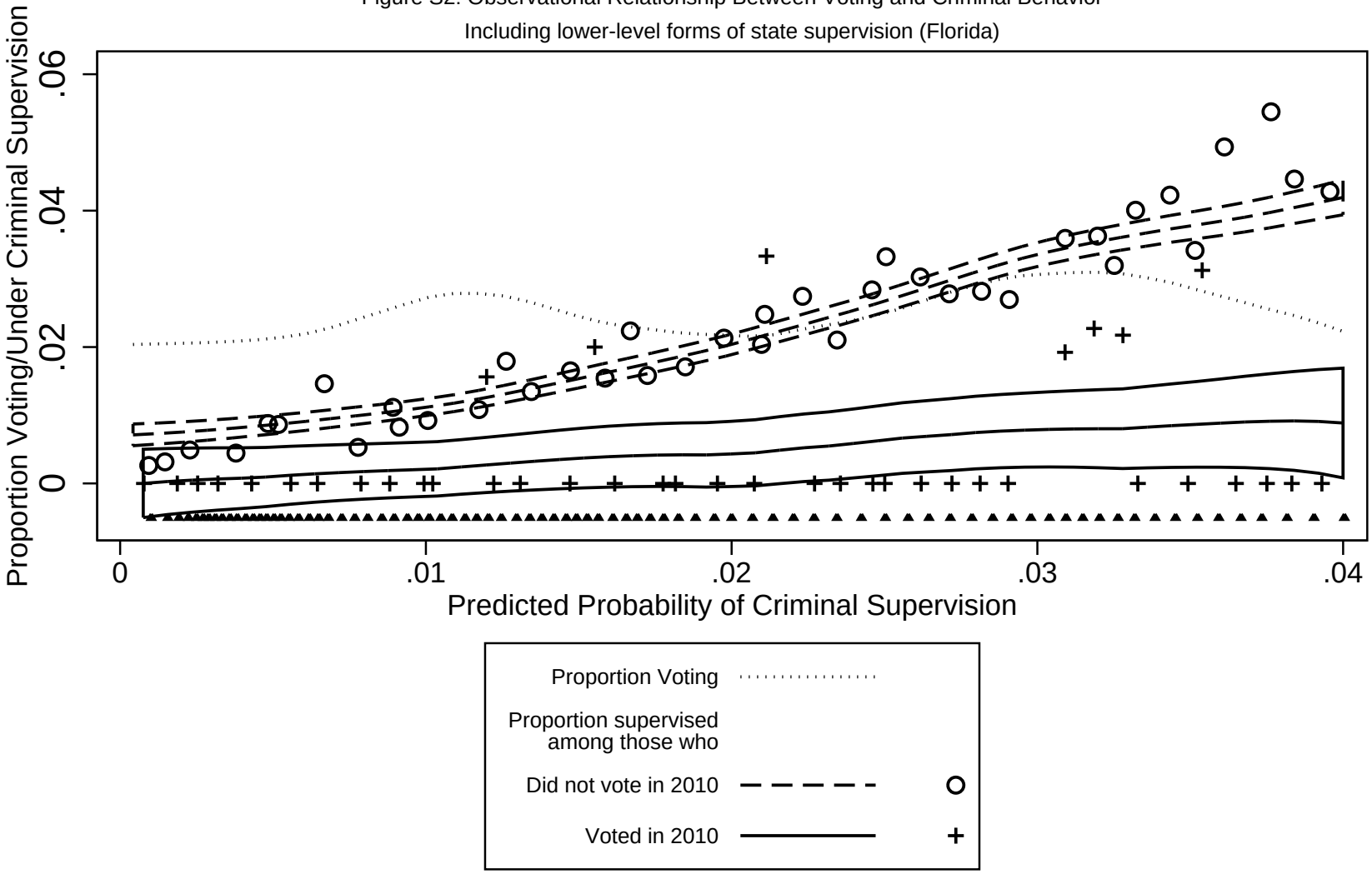
States where we know supervision post-dates 2010 election (Ohio, Texas, Washington)



Lines are local polynomials, with 95% confidence intervals for proportion under criminal supervision. Scatter plot is average in .001 unit bins. Diamonds are distribution of data on X axis by percentile.

Figure S2: Observational Relationship Between Voting and Criminal Behavior

Including lower-level forms of state supervision (Florida)



Lines are local polynomials, with 95% confidence intervals for proportion under criminal supervision. Scatter plot is average in .001 unit bins. Diamonds are distribution of data on X axis by percentile.

Table S1: Sources for State Supervision Information

State	Source	Number of individuals	Date data obtained
Arizona	Department of Corrections database of currently active inmates, http://www.azcorrections.gov/inmate_datasearch/Index_Minh.aspx	2689	July 23, 2013
Colorado	Department of Corrections database of currently supervised individuals (including parole), http://www.doc.state.co.us/oss/	1184	July 20, 2013
Florida	Department of Corrections databases of currently supervised and released individuals (including parole), http://www.dc.state.fl.us/ActiveOffenders, http://www.dc.state.fl.us/ActiveInmates/, http://www.dc.state.fl.us/InmateReleases/	22097	July 23, 2013
Illinois	Department of Corrections database of currently active inmates, http://www2.illinois.gov/idoc/Offender/Pages/InmateSearch.aspx	5947	July 21, 2013
Maryland	Department of Public Safety and Correctional Facilities inmate locator database for currently incarcerated individuals, http://www.dpscs.state.md.us/inmate/	2137	July 21, 2013
Missouri	Department of Corrections Offender Search database of currently supervised individuals (including probation and parole): https://web.mo.gov/doc/offSearchWeb/search.jsp	3977	July 22, 2013
New Mexico	Corrections Department offender information database of currently supervised individuals (including probation and parole): http://corrections.state.nm.us:8080/OffenderSearch/	80	July 22, 2013
Ohio	Department of Rehabilitation and Corrections census of currently incarcerated individuals, obtained directly from state. Records restricted to those who entered prison on or after June 1, 2011.	2546	July 1, 2013
Pennsylvania	Department of Corrections inmate locator for currently incarcerated individuals: http://inmatelocator.cor.state.pa.us/inmatelocatorweb/Criteria.aspx	2751	July 20, 2013
Texas	Department of Criminal Justice list “High Value Data Sets” of currently incarcerated individuals: http://www.tdcj.state.tx.us/info_services.html. Records are restricted to those whose offense date is after Nov. 15, 2010.	3787	July 2, 2013
Washington	Department of Corrections database of current and formerly incarcerated individuals (January 1, 2010 to August 1, 2013). Records restricted to those who entered prison on or after June 1, 2011.	1155	August 1, 2013

Note: States listed are those for which names and date of birth are available. Number of individuals is for those with birthdays between June 1, 1990 and September 30, 1992.

Table S2: Observational Benchmark: Relationship Between Participation (Registration and Voting) in 2010 and Subsequent State Supervision

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Under State Supervision (1=yes)	Under State Supervision (1=yes), Low-Risk Sample	Under State Supervision (1=yes), High-Risk Sample	OH, TX, WA: Under State Supervision (1=yes)		FL: Under State Supervision (1=yes)	
Registered in 2010 (1=yes)	-0.0028	-0.0008	-0.0102	-0.0011		-0.0099	
	[0.000279]***	[0.000223]***	[0.001010]***	[0.000292]***		[0.001446]***	
Voted in 2010 (1=yes)					-0.0023		-0.0206
					[0.000473]***		[0.001727]***
African American (1=yes)	0.0069	0.0032	0.0285	0.0029	0.0029	0.0203	0.0204
	[0.000305]***	[0.000270]***	[0.012014]**	[0.000301]***	[0.000301]***	[0.001315]***	[0.001315]***
Hispanic (1=yes)	0.0036	0.0024	0.0123	0.0020	0.0020	0.0081	0.0081
	[0.000355]***	[0.000317]***	[0.012105]	[0.000351]***	[0.000351]***	[0.001477]***	[0.001477]***
Female (1=yes)	-0.0124	-0.0051	-0.0374	-0.0051	-0.0051	-0.0310	-0.0311
	[0.000282]***	[0.000251]***	[0.001488]***	[0.000278]***	[0.000278]***	[0.001229]***	[0.001229]***
Gender Unknown (1=yes)	-0.0071	-0.0018	-0.0173	-0.0026	-0.0026	-0.0158	-0.0158
	[0.000445]***	[0.000370]***	[0.001297]***	[0.000459]***	[0.000459]***	[0.001750]***	[0.001750]***
Proportion Black	0.0028	0.0013	0.0013	0.0021	0.0021	0.0045	0.0043
	[0.000657]***	[0.000502]**	[0.001881]	[0.000792]***	[0.000792]***	[0.002845]	[0.002844]
Proportion Hispanic	-0.0064	-0.0008	-0.0181	-0.0013	-0.0013	-0.0120	-0.0122
	[0.000783]***	[0.000610]	[0.003162]***	[0.000789]*	[0.000789]*	[0.003165]***	[0.003165]***
Proportion of Kids < 18 in Female Headed Household	0.0036	0.0011	0.0095	0.0024	0.0024	0.0115	0.0115
	[0.000741]***	[0.000550]**	[0.002164]***	[0.000780]***	[0.000780]***	[0.003171]***	[0.003171]***
Proportion of Families Below the Poverty Rate	0.0037	-0.0006	0.0111	0.0001	0.0001	0.0101	0.0101
	[0.001157]***	[0.000812]	[0.003259]***	[0.001143]	[0.001143]	[0.005437]*	[0.005437]*
Proportion of Families Receiving Public Assistance	0.0044	0.0002	0.0173	0.0029	0.0028	-0.0052	-0.0052
	[0.002957]	[0.002166]	[0.007841]**	[0.003368]	[0.003368]	[0.017375]	[0.017381]
Proportion of Population Over 25 w/. < High School	0.0123	0.0031	0.0319	0.0032	0.0033	0.0180	0.0183
	[0.001236]***	[0.000915]***	[0.004063]***	[0.001158]***	[0.001158]***	[0.005506]***	[0.005502]***
Log Pop. Density (1000 persons per sq mi.)	-0.0002	0.0000	-0.0008	0.0001	0.0001	-0.0022	-0.0022
	[0.000098]**	[0.000071]	[0.000388]**	[0.000096]	[0.000096]	[0.000505]***	[0.000505]***
Constant	0.0029	0.0023	-0.0331	0.0022	0.0020	0.0206	0.0201
	[0.000348]***	[0.000312]***	[0.012238]***	[0.000363]***	[0.000354]***	[0.001529]***	[0.001522]***
Observations	552525	411477	141048	237858	237858	84250	84250
R^2	0.013	0.002	0.010	0.003	0.003	0.014	0.014
Mean of Outcome in Sample	0.009	0.003	0.024	0.004	0.004	0.026	0.026
Includes State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cell entries are OLS regression coefficients with robust (Huber/White) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table S3: State-by-State Replication of Table 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Under State Supervision (1=yes), State=AZ	Under State Supervision (1=yes), State=CO	Under State Supervision (1=yes), State=FL	Under State Supervision (1=yes), State=IL	Under State Supervision (1=yes), State=MD	Under State Supervision (1=yes), State=MO	Under State Supervision (1=yes), State=NM	Under State Supervision (1=yes), State=OH	Under State Supervision (1=yes), State=PA	Under State Supervision (1=yes), State=TX	Under State Supervision (1=yes), State=WA
Voted in 2010 (1=yes)	-0.0033	-0.0053	-0.0206	-0.0014	-0.0020	-0.0123	-0.0002	-0.0024	-0.0028	-0.0023	-0.0022
	[0.001172]***	[0.000652]***	[0.001727]***	[0.002277]	[0.001733]	[0.002835]***	[0.000131]	[0.001608]	[0.001836]	[0.000413]***	[0.000411]***
African American (1=yes)	0.0034	0.0047	0.0204	0.0087	0.0037	0.0140	0.0003	0.0042	0.0042	0.0028	0.0016
	[0.001120]***	[0.001291]***	[0.001315]***	[0.000957]***	[0.000727]***	[0.002299]***	[0.000327]	[0.000904]***	[0.000984]***	[0.000327]***	[0.000796]**
Hispanic (1=yes)	0.0049	0.0024	0.0081	0.0010	0.0015	0.0058	0.0002	-0.0021	0.0008	0.0021	0.0004
	[0.001395]***	[0.001771]	[0.001477]***	[0.001077]	[0.000947]	[0.004987]	[0.000181]	[0.001610]	[0.001414]	[0.000363]***	[0.000941]
Female (1=yes)	-0.0096	-0.0109	-0.0311	-0.0184	-0.0088	-0.0301	0.0000	-0.0102	-0.0103	-0.0041	-0.0040
	[0.001087]***	[0.001505]***	[0.001229]***	[0.000886]***	[0.000779]***	[0.002273]***	[0.000260]	[0.000954]***	[0.000947]***	[0.000291]***	[0.000843]***
Gender Unknown (1=yes)	-0.0076	-0.0085	-0.0158	-0.0120	-0.0053	-0.0202	-0.0002	-0.0070	-0.0051	-0.0015	-0.0018
	[0.001500]***	[0.002084]***	[0.001750]***	[0.001287]***	[0.001135]***	[0.003015]***	[0.000200]	[0.001275]***	[0.001307]***	[0.000507]***	[0.001360]
Proportion Black	0.0035	0.0063	0.0043	0.0026	0.0005	-0.0022	-0.0008	0.0010	0.0024	0.0017	0.0062
	[0.006650]	[0.005944]	[0.002844]	[0.001675]	[0.001362]	[0.004218]	[0.001361]	[0.001884]	[0.001625]	[0.000855]*	[0.005427]
Proportion Hispanic	-0.0007	0.0062	-0.0122	-0.0030	-0.0152	-0.0138	-0.0005	-0.0067	0.0005	-0.0011	-0.0022
	[0.002855]	[0.004922]	[0.003165]***	[0.002520]	[0.003347]***	[0.011453]	[0.000999]	[0.005624]	[0.002637]	[0.000788]	[0.002897]
Proportion of Kids < 18 in Female Headed Household	0.0029	0.0063	0.0115	0.0018	-0.0005	0.0078	0.0000	0.0020	0.0006	0.0024	0.0000
	[0.002828]	[0.004606]	[0.003171]***	[0.002176]	[0.002043]	[0.004875]	[0.000177]	[0.002227]	[0.001865]	[0.000826]***	[0.001999]
Proportion of Families Below the Poverty Rate	-0.0035	0.0050	0.0101	0.0070	0.0022	-0.0005	0.0012	0.0034	0.0053	-0.0010	-0.0019
	[0.003859]	[0.006280]	[0.005437]*	[0.003423]**	[0.004667]	[0.007510]	[0.001253]	[0.003028]	[0.003250]	[0.001175]	[0.005007]
Proportion of Families Receiving Public Assistance	0.0006	-0.0092	-0.0052	0.0086	0.0309	0.0152	-0.0033	-0.0001	0.0074	0.0027	-0.0021
	[0.011442]	[0.018563]	[0.017381]	[0.008633]	[0.012058]**	[0.020804]	[0.003081]	[0.007554]	[0.006413]	[0.003813]	[0.008501]
Proportion of Population Over 25 w/. < High School	0.0033	-0.0018	0.0183	0.0154	0.0159	0.0378	0.0008	0.0093	-0.0022	0.0025	0.0097
	[0.004573]	[0.008059]	[0.005502]***	[0.004429]***	[0.004410]***	[0.011228]***	[0.000657]	[0.004701]**	[0.004080]	[0.001142]**	[0.005341]*
Log Pop. Density (1000 persons per sq mi.)	0.0006	-0.0003	-0.0022	0.0003	0.0008	0.0006	0.0000	0.0005	0.0000	0.0000	-0.0001
	[0.000280]**	[0.000513]	[0.000505]***	[0.000327]	[0.000320]**	[0.000798]	[0.000025]	[0.000386]	[0.000312]	[0.000106]	[0.000217]
Constant	0.0058	0.0047	0.0201	0.0079	0.0037	0.0128	0.0000	0.0049	0.0062	0.0018	0.0024
	[0.001159]***	[0.001436]***	[0.001522]***	[0.000890]***	[0.000736]***	[0.002325]***	[0.000209]	[0.000934]***	[0.000984]***	[0.000362]***	[0.000889]***
Observations	25646	14601	84250	71904	46219	18957	11729	40620	41361	179925	17313
R^2	0.005	0.007	0.014	0.010	0.006	0.014	0.000	0.005	0.004	0.002	0.002
Mean of Outcome in Sample	0.005	0.006	0.026	0.010	0.005	0.018	0.000	0.007	0.006	0.003	0.002

Note: Cell entries are OLS regression coefficients with robust (Huber/White) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table S4: Logit Models, Observational Benchmark: Relationship Between Voting in 2010 and Subsequent State Supervision

	(1)	(2)	(3)	(4)
	Under State Supervision (1=yes)		Under State Supervision (1=yes), Low-Risk Sample	Under State Supervision (1=yes), High-Risk Sample
Voted in 2010 (1=yes)		-1.0014	-0.9038	-1.0543
		[0.147704]***	[0.245000]***	[0.184940]***
African American (1=yes)	1.4521	1.4583	1.3414	1.0395
	[0.097974]***	[0.097970]***	[0.117472]***	[0.586155]*
Hispanic (1=yes)	0.9601	0.9598	0.9660	0.4449
	[0.106105]***	[0.106066]***	[0.119918]***	[0.591339]
Female (1=yes)	-1.7105	-1.7081	-1.7805	-1.4101
	[0.041460]***	[0.041460]***	[0.076886]***	[0.066909]***
Gender Unknown (1=yes)	-0.6351	-0.6361	-0.6540	-0.6030
	[0.039480]***	[0.039467]***	[0.078961]***	[0.049665]***
Proportion Black	0.1506	0.1499	0.4167	0.0519
	[0.070101]**	[0.070040]**	[0.140415]***	[0.082105]
Proportion Hispanic	-0.5393	-0.5395	-0.2777	-0.6033
	[0.105587]***	[0.105492]***	[0.182221]	[0.135652]***
Proportion of Kids < 18 in Female Headed Household	0.4181	0.4122	0.3067	0.4287
	[0.077051]***	[0.076999]***	[0.149294]**	[0.090187]***
Proportion of Families Below the Poverty Rate	0.2907	0.2853	-0.0769	0.3864
	[0.110609]***	[0.110548]***	[0.220426]	[0.127778]***
Proportion of Families Receiving Public Assistance	0.6614	0.6521	0.4446	0.6460
	[0.283914]**	[0.283929]**	[0.584238]	[0.327457]**
Proportion of Population Over 25 w/. < High School	1.1244	1.1129	1.1106	1.0337
	[0.135745]***	[0.135643]***	[0.263112]***	[0.161586]***
Log Pop. Density (1000 persons per sq mi.)	-0.0180	-0.0195	0.0066	-0.0282
	[0.012847]	[0.012864]	[0.023610]	[0.015534]*
Constant	-6.6698	-6.6472	-6.6558	-6.2501
	[0.106279]***	[0.106346]***	[0.122249]***	[0.609577]***
Observations	552525	552525	411477	141048
Mean of Outcome in Sample	0.009	0.009	0.003	0.024
Includes State Fixed Effects?	Yes	Yes	Yes	Yes

Note: Cell entries are Logit coefficients with robust (Huber/White) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table S5: Experimental Estimates: Effect of Outreach on 2010 Registration

	(1)	(2)	(3)	(4)	(5)	(6)
	Registered in 2010 (1=yes)		Registered in 2010 (1=yes), Low-Risk Sample		Registered in 2010 (1=yes), High-Risk Sample	
Treated (Sent Registration Form 2010, 1=yes)	0.0182	0.0179	0.0212	0.0209	0.0095	0.0092
	[0.001509]***	[0.001523]***	[0.001787]***	[0.001804]***	[0.002774]***	[0.002791]***
African American (1=yes)	0.0218		0.0257		0.0616	
	[0.001722]***		[0.001878]***		[0.013801]***	
Hispanic (1=yes)	-0.0096		-0.0088		0.0473	
	[0.001954]***		[0.002051]***		[0.014107]***	
Female (1=yes)	0.0186		0.0183		0.0131	
	[0.001032]***		[0.001369]***		[0.002883]***	
Gender Unknown (1=yes)	-0.0097		-0.0147		0.0025	
	[0.001368]***		[0.001799]***		[0.002427]	
Proportion Black	-0.0166		-0.0278		0.0088	
	[0.002319]***		[0.002914]***		[0.004001]**	
Proportion Hispanic	0.0067		0.0054		0.0180	
	[0.003153]**		[0.003673]		[0.006600]***	
Proportion of Kids < 18 in Female Headed Household	-0.0260		-0.0261		-0.0227	
	[0.002540]***		[0.003110]***		[0.004331]***	
Proportion of Families Below the Poverty Rate	-0.0259		-0.0300		-0.0159	
	[0.003816]***		[0.004741]***		[0.006361]**	
Proportion of Families Receiving Public Assistance	0.0152		0.0324		-0.0293	
	[0.009673]		[0.012204]***		[0.016099]*	
Proportion of Population Over 25 w/. < High School	-0.0843		-0.0921		-0.0593	
	[0.004385]***		[0.005334]***		[0.007975]***	
Log Pop. Density (1000 persons per sq mi.)	-0.0041		-0.0039		-0.0058	
	[0.000416]***		[0.000478]***		[0.000839]***	
Constant	0.1901	0.1298	0.1910	0.1369	0.1281	0.1091
	[0.002457]***	[0.001437]***	[0.002675]***	[0.001701]***	[0.015574]***	[0.002639]***
Observations	552525	552525	411477	411477	141048	141048
R^2	0.019	0.000	0.019	0.000	0.011	0.000
F-test p-value	0.000	0.000	0.000	0.000	0.000	0.001
Mean of Outcome in Sample	0.130	0.130	0.156	0.156	0.117	0.117
Includes State Fixed Effects?	Yes	No	Yes	No	Yes	No

Note: Cell entries are OLS regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table S6: Robustness of Experimental Estimates: Effect of Outreach and Participation on Subsequent State Supervision

	(1)	(2)	(3)	(4)
	OH, TX, WA: Under state supervision (1=yes)	OH, TX, WA: Instrumental Variables Regression (2SLS), Under State Supervision (1=yes)	FL: Under State Supervision (1=yes), low risk sample	FL: Instrumental Variables Regression (2SLS), Under State Supervision (1=yes), low risk sample
Treated (Sent Registration Form 2010, 1=yes)	0.0006		0.0015	
	[0.000383]*		[0.001772]	
Voted in 2010 (1=yes)		0.1240		0.5039
		[0.079430]		[0.664813]
African American (1=yes)	0.0029	0.0015	0.0202	0.0158
	[0.000301]***	[0.000972]	[0.001315]***	[0.006039]***
Hispanic (1=yes)	0.0020	0.0023	0.0082	0.0099
	[0.000351]***	[0.000430]***	[0.001477]***	[0.002915]***
Female (1=yes)	-0.0051	-0.0055	-0.0312	-0.0328
	[0.000278]***	[0.000404]***	[0.001230]***	[0.002523]***
Gender Unknown (1=yes)	-0.0026	-0.0019	-0.0158	-0.0154
	[0.000459]***	[0.000631]***	[0.001748]***	[0.001984]***
Proportion Black	0.0021	0.0026	0.0043	0.0026
	[0.000791]***	[0.000880]***	[0.002844]	[0.003847]
Proportion Hispanic	-0.0013	-0.0018	-0.0124	-0.0160
	[0.000788]*	[0.000894]**	[0.003177]***	[0.006042]***
Proportion of Kids < 18 in Female Headed Household	0.0024	0.0039	0.0117	0.0168
	[0.000780]***	[0.001276]***	[0.003168]***	[0.007570]**
Proportion of Families Below the Poverty Rate	0.0002	0.0022	0.0103	0.0155
	[0.001143]	[0.001795]	[0.005443]*	[0.009089]*
Proportion of Families Receiving Public Assistance	0.0028	0.0052	-0.0047	0.0067
	[0.003367]	[0.003836]	[0.017368]	[0.024061]
Proportion of Population Over 25 w/. < High School	0.0033	0.0057	0.0188	0.0320
	[0.001157]***	[0.001937]***	[0.005501]***	[0.018318]*
Log Pop. Density (1000 persons per sq mi.)	0.0001	0.0002	-0.0021	-0.0005
	[0.000096]	[0.000114]	[0.000507]***	[0.002236]
Constant	0.0013	-0.0025	0.0181	0.0049
	[0.000491]***	[0.002825]	[0.002196]***	[0.019295]
Observations	237858	237858	84250	84250
R^2	0.003		0.013	
Mean of Outcome in Sample	0.004	0.004	0.026	0.026
Includes State Fixed Effects?	Yes	Yes	Yes	Yes

Note: Cell entries are regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01. In even numbered columns, these are second stage estimates from two-staged least squares estimates.

Table S7: Rare Events Logit Analysis versions of Tables 3 and 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Voted in 2010 (1=yes)		Voted in 2010 (1=yes), low risk sample		Voted in 2010 (1=yes), high risk sample		Under state supervision (1=yes)	Under state supervision (1=yes), low risk sample	Under state supervision (1=yes), high risk sample
Treated (Sent Registration Form 2010, 1=yes)	0.184	0.181	0.202	0.198	0.122	0.124	0.056	0.199	-0.001
	[0.028777]***	[0.028689]***	[0.032936]***	[0.032830]***	[0.058877]**	[0.058700]**	[0.049532]	[0.096748]**	[0.057807]
Race is African American (1=yes)	0.337		0.328		-156023.000		1.448	1.331	0.866
	[0.029633]***		[0.031143]***		[0.127184]***		[0.098003]***	[0.117546]***	[0.586391]
Race is Hispanic (1=yes)	-0.090		-0.121		-156024.000		0.956	0.964	0.278
	[0.034270]***		[0.035239]***		[0.151739]***		[0.106127]***	[0.119995]***	[0.591569]
Gender is female (1=yes)	0.144		0.135		0.154		-1.710	-1.781	-1.410
	[0.017297]***		[0.022007]***		[0.059771]***		[0.041575]***	[0.076964]***	[0.067015]***
Gender is unknown (1=yes)	-0.148		-0.206		0.018		-0.635	-0.650	-0.603
	[0.026540]***		[0.032953]***		[0.052496]		[0.039513]***	[0.078982]***	[0.049739]***
Prop. Black	-0.025		-0.005		-0.060		0.150	0.415	0.052
	[0.038597]		[0.046180]		[0.075893]		[0.070305]**	[0.140615]***	[0.082385]
Prop. Hispanic	0.105		0.141		-0.023		-0.539	-0.280	-0.601
	[0.051241]**		[0.057544]**		[0.128043]		[0.105840]***	[0.182770]	[0.135885]***
Prop. Kids < 18 in female headed hh	-0.351		-0.363		-0.304		0.418	0.315	0.434
	[0.044514]***		[0.052034]***		[0.086115]***		[0.076970]***	[0.149362]**	[0.090161]***
Prop. families below poverty rate	-0.479		-0.504		-0.405		0.292	-0.066	0.391
	[0.071168]***		[0.084123]***		[0.134443]***		[0.110871]***	[0.220927]	[0.128149]***
Prop. families getting public assistance	-0.527		-0.679		-0.349		0.666	0.469	0.661
	[0.191446]***		[0.229330]***		[0.349473]		[0.283965]**	[0.584437]	[0.327525]**
Prop. over 25 pop. < HS	-0.678		-0.648		-0.808		1.124	1.120	1.048
	[0.078325]***		[0.090780]***		[0.163493]***		[0.135825]***	[0.263448]***	[0.161764]***
Log Pop. density (1000 persons per sq mi.)	-0.032		-0.020		-0.084		-0.018	0.007	-0.026
	[0.006354]***		[0.007091]***		[0.014608]***		[0.012881]	[0.023644]	[0.015547]*
Constant	-3.522	-3.659	-3.540	-3.633	156020.000	-3.736	-6.714	-6.848	-6.094
	[0.042868]***	[0.027446]***	[0.046023]***	[0.031428]***	[0.000000]	[0.056009]***	[0.114939]***	[0.150853]***	[0.613398]***
Observations	552525	552525	411477	411477	141048	141048	552525	411477	141048
Mean of outcome in control group	0.025	0.130	0.026	0.026	0.023	0.117			
Mean of outcome in sample							0.009	0.003	0.024

Note: Cell entries are rare events logit coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01.

Table S8: Experimental Estimates: Effect of Outreach Instrumenting for Registration on Subsequent State Supervision

	(1)	(2)	(3)
	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes)	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes), low risk sample	Instrumental Variables Regression (2SLS), Under State Supervision (1=yes), high risk sample
Registered in 2010 (1=yes)	0.0246	0.0303	-0.0061
	[0.022435]	[0.013463]**	[0.143737]
African American (1=yes)	0.0063	0.0024	0.0282
	[0.000579]***	[0.000445]***	[0.014792]*
Hispanic (1=yes)	0.0039	0.0027	0.0121
	[0.000421]***	[0.000343]***	[0.013755]
Female (1=yes)	-0.0129	-0.0057	-0.0374
	[0.000507]***	[0.000357]***	[0.002434]***
Gender Unknown (1=yes)	-0.0069	-0.0013	-0.0173
	[0.000498]***	[0.000426]***	[0.001347]***
Proportion Black	0.0032	0.0021	0.0013
	[0.000756]***	[0.000636]***	[0.002280]
Proportion Hispanic	-0.0066	-0.0009	-0.0182
	[0.000803]***	[0.000623]	[0.004130]***
Proportion of Kids < 18 in Female Headed Household	0.0043	0.0019	0.0096
	[0.000953]***	[0.000667]***	[0.003937]**
Proportion of Families Below the Poverty Rate	0.0044	0.0003	0.0112
	[0.001299]***	[0.000914]	[0.003997]***
Proportion of Families Receiving Public Assistance	0.0040	-0.0008	0.0174
	[0.002991]	[0.002246]	[0.008897]*
Proportion of Population Over 25 w/. < High School	0.0146	0.0060	0.0322
	[0.002259]***	[0.001559]***	[0.009423]***
Log Pop. Density (1000 persons per sq mi.)	-0.0001	0.0001	-0.0008
	[0.000134]	[0.000088]	[0.000915]
Constant	-0.0027	-0.0042	-0.0337
	[0.004646]	[0.002849]	[0.023353]
Observations	552525	411477	141048
Mean of Outcome in Sample	0.009	0.003	0.024
Includes State Fixed Effects?	Yes	Yes	Yes

Note: Cell entries are regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01. These are second stage estimates from two-staged least squares estimates.

Table S9: Replication of Tables 2 and 4 using strict measure of matching to state supervision record

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Under state supervision, strict match (1=yes)	Under state supervision, strict match (1=yes), low risk sample	Under state supervision, strict match (1=yes), high risk sample	Under state supervision, strict match (1=yes)	Instrumental Variables Regression (2SLS), instrumenting for voting, Under state supervision, strict match (1=yes)	Under state supervision, strict match (1=yes), low risk sample	Instrumental Variables Regression (2SLS), instrumenting for voting, Under state supervision, strict match (1=yes), low risk sample	Under state supervision, strict match (1=yes), high risk sample	Instrumental Variables Regression (2SLS), instrumenting for voting, Under state supervision, strict match (1=yes), high risk sample
Treated (Sent Registration Form 2010, 1=yes)				0.0001		0.0003		-0.0006	
				[0.000376]		[0.000244]		[0.001288]	
Voted in 2010 (1=yes)	-0.0044	-0.0016	-0.0138		0.0165		0.0607		-0.2058
	[0.000369]***	[0.000266]***	[0.001349]***		[0.076964]		[0.045151]		[0.436853]
Race is African American (1=yes)	0.0056	0.0021	0.0216	0.0056	0.0054	0.0021	0.0015	0.0213	0.0253
	[0.000285]***	[0.000245]***	[0.012003]*	[0.000284]***	[0.000833]***	[0.000244]***	[0.000526]***	[0.012009]*	[0.014563]*
Race is Hispanic (1=yes)	0.0027	0.0015	0.0076	0.0027	0.0027	0.0015	0.0017	0.0074	0.0102
	[0.000325]***	[0.000286]***	[0.012085]	[0.000325]***	[0.000371]***	[0.000285]***	[0.000320]***	[0.012091]	[0.013338]
Gender is female (1=yes)	-0.0117	-0.0048	-0.0347	-0.0117	-0.0118	-0.0048	-0.0051	-0.0347	-0.0340
	[0.000261]***	[0.000229]***	[0.001366]***	[0.000261]***	[0.000422]***	[0.000229]***	[0.000302]***	[0.001365]***	[0.002068]***
Gender is unknown (1=yes)	-0.0077	-0.0022	-0.0183	-0.0077	-0.0076	-0.0022	-0.0019	-0.0183	-0.0182
	[0.000400]***	[0.000324]***	[0.001181]***	[0.000400]***	[0.000490]***	[0.000324]***	[0.000405]***	[0.001180]***	[0.001202]***
Prop. Black	0.0022	0.0008	0.0003	0.0022	0.0022	0.0008	0.0009	0.0003	-0.0001
	[0.000600]***	[0.000429]*	[0.001754]	[0.000600]***	[0.000617]***	[0.000429]*	[0.000445]**	[0.001755]	[0.002009]
Prop. Hispanic	-0.0059	-0.0010	-0.0172	-0.0059	-0.0060	-0.0010	-0.0012	-0.0171	-0.0176
	[0.000701]***	[0.000514]*	[0.002937]***	[0.000702]***	[0.000720]***	[0.000514]*	[0.000541]**	[0.002938]***	[0.003155]***
Prop. Kids < 18 in female headed hh	0.0033	0.0009	0.0087	0.0033	0.0035	0.0009	0.0016	0.0088	0.0072
	[0.000674]***	[0.000467]*	[0.002017]***	[0.000674]***	[0.001047]***	[0.000467]**	[0.000692]**	[0.002015]***	[0.004028]*
Prop. families below poverty rate	0.0031	-0.0005	0.0092	0.0032	0.0034	-0.0005	0.0004	0.0094	0.0074
	[0.001051]***	[0.000684]	[0.003024]***	[0.001050]***	[0.001431]**	[0.000684]	[0.000934]	[0.003021]***	[0.005219]
Prop. families getting public assistance	0.0055	0.0003	0.0207	0.0055	0.0057	0.0004	0.0012	0.0208	0.0197
	[0.002746]**	[0.001820]	[0.007465]***	[0.002744]**	[0.002849]**	[0.001819]	[0.001940]	[0.007461]***	[0.007964]**
Prop. over 25 pop. < HS	0.0108	0.0027	0.0284	0.0109	0.0112	0.0027	0.0038	0.0287	0.0247
	[0.001108]***	[0.000762]***	[0.003757]***	[0.001107]***	[0.001766]***	[0.000762]***	[0.001108]***	[0.003758]***	[0.009337]***
Log Pop. density (1000 persons per sq mi.)	-0.0002	0.0000	-0.0006	-0.0002	-0.0002	0.0000	0.0000	-0.0005	-0.0010
	[0.000089]**	[0.000062]	[0.000359]	[0.000089]**	[0.000114]	[0.000062]	[0.000068]	[0.000359]	[0.001079]
Constant	0.0029	0.0024	-0.0267	0.0027	0.0022	0.0021	0.0003	-0.0265	-0.0217
	[0.000308]***	[0.000270]***	[0.012191]**	[0.000456]***	[0.002659]	[0.000349]***	[0.001576]	[0.012277]**	[0.016849]
Observations	552525	411477	141048	552525	552525	411477	411477	141048	141048
R-squared	0.012	0.002	0.009	0.012		0.002		0.009	
Mean of outcome in sample	0.007	0.002	0.021	0.007	0.007	0.002	0.002	0.021	0.021

Note: Cell entries are regression coefficients with robust (Huber/White) standard errors in brackets. In columns (4)-(9), standard errors are clustered (at the household level). *p<.1; **p<.05; ***p<.01. In columns (5), (7) and (9) these are second stage estimates from two-staged least squares estimates.

Table S10: Experimental Estimates: Effect of Outreach on Subsequent State Supervision without Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
	Under State Supervision (1=yes)	Instrumental Variables Regression (2SLS), Instrumenting for Voting, Under State Supervision (1=yes)	Under State Supervision (1=yes), low risk sample	Instrumental Variables Regression (2SLS), Instrumenting for Voting, Under State Supervision (1=yes), low risk sample	Under State Supervision (1=yes), high risk sample	Instrumental Variables Regression (2SLS), Instrumenting for Voting, Under State Supervision (1=yes), high risk sample
Treated (Sent Registration Form 2010, 1=yes)	0.0004		0.0006		-0.0002	
	[0.000408]		[0.000280]**		[0.001373]	
Voted in 2010 (1=yes)		0.0901		0.1175		-0.0530
		[0.085352]		[0.054000]**		[0.463482]
Constant	0.0027	0.0005	0.0023	-0.0005	0.0100	0.0112
	[0.000388]***	[0.002457]	[0.000278]***	[0.001558]	[0.001941]***	[0.011916]
Observations	552525	552525	411477	411477	141048	141048
R^2	0.007		0.001		0.003	
Mean of Outcome in Sample	0.009	0.009	0.003	0.003	0.024	0.024
Includes State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes

Note: Cell entries are regression coefficients with clustered (at the household level) standard errors in brackets. *p<.1; **p<.05; ***p<.01. In even numbered columns, these are second stage estimates from two-staged least squares estimates.