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

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Abstract

Democratic theory and prior empirical work support the view that political participation, by promoting social integration and pro-social attitudes, reduces one's propensity for anti-social behavior, such as committing a crime. Previous investigations examine observational data, which are vulnerable to bias if omitted factors affect both propensity to participate and risk of criminality or their reports. A field experiment encouraging 552,525 subjects aged 18-20 to register and vote confirms previous observational findings of the negative association between participation and subsequent criminality. However, comparing randomly formed treatment and control groups reveals that the intervention increased participation but did not reduce subsequent criminality. Our results suggest that while participation is correlated with criminality, it exerts no causal effect on subsequent criminal behavior.

Multiple strands of normative political theory provide detailed arguments about the ways that political engagement transforms the individual, thereby reducing the likelihood that she will break the law. The posited mechanisms are many and varied, and here we highlight several. For instance, it has been argued that political participation, by requiring individuals to weigh competing arguments about what choices are best, encourages them to consider the perspectives and interests of fellow citizens (de Tocqueville [1840] 1969; Mill [1861] 1978). Engagement through this and other avenues 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 behave anti-socially. Thus, participation may encourage a "democratic character," an enlightened self-interest incompatible with criminal acts that harm 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, participation may change attitudes toward the law if it serves to legitimate the state. When voting, all individuals are given the chance to express their views, in the process learning that those views are worthy and that legitimate avenues exist for expression of their 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 in which the particular outcome of governance is not preferred, one should still abide by it. 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 more law-abiding citizens, there is to date only modest supporting empirical evidence (Uggen and Manza 2004; Uggen and Schaefer 2005). Those who participate are much less likely than those who do not 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 differ from those who do not. And these differences, which might include greater engagement in community life, greater educational attainment, and so on, might also be directly related to the propensity to engage in criminal behavior.

In this paper we present evidence that allows us to estimate 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 in the United States. Specifically, we present results from a large-scale field experiment that permits us to address some of the challenges in assessing the causal effects of participation. The subject population is approximately 550,000 non-white young adults who were included in a randomized controlled trial conducted during the November 2010 election cycle in the United States. This design, in which some subjects were randomly chosen to receive a pre-election intervention aimed at increasing voting, overcomes the problems inherent in observational analysis, namely that individuals who vote do so for reasons (unobserved by the researcher) that might also be correlated with the probability of becoming involved with the criminal justice system. Further, unlike some prior work, we use administrative records to measure both participation and criminality—operationalized as being under state supervision—thereby avoiding concerns about systematic measurement error in survey reports of participation and criminality.

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. 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 (0.4% vs 0.9%).

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, the observed negative correlation between voting and criminality does not appear to represent a causal effect.

Finally, we discuss the limitations and implications of our analysis, as well as directions for future research. Substantively, it appears that simply casting a ballot is insufficient to reduce the propensity to engage in criminal behavior or to begin an immediate virtuous cycle that culminates in behaviors that make one less prone to incarceration (as would be predicted by the growing literature on the large effects of “minor” psychological interventions). Beyond this finding itself, this empirical pattern also suggests we should reconsider theoretical arguments (some of which may have been spurred in part by the robust observational correlation between voting and all sorts of pro-social outcomes) for why voting or other forms of civic engagement might produce pro-social behavior, as well as the possibility that voting might not be a meaningful enough form of participation to directly facilitate the development of better citizens. Methodologically, our work underscores the extreme challenges facing empirical researchers in testing normative theories about how political participation may alter individuals (a fact highlighted by the large differences between the observational and experimental results we present). When our prior beliefs about the effectiveness of interventions are based on designs for which there are reasonable concerns about fundamental sources of bias (such as unobserved heterogeneity or omitted variables that explain both treatment assignment and outcomes), then even the apparent precision of a highly significant regression result may be misleading. Consequently, we should not be surprised when improved research designs yield results far different from prior studies.

In summary, our credibly identified study, even after noting its limitations, allows us confidently to reject the large negative association between voting and subsequent incarceration found in prior observational research (and in our own data, when analyzed in that fashion). Furthermore, 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. 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 substantially reduce incarceration.

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 evidence. Several previous studies demonstrate strong associations between participation in pro-social behavior and reduced criminality after controlling for a number of known confounding factors (e.g., demographic and socioeconomic characteristics and prior anti-social and criminal behavior). 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 in lieu of prison are less likely to recidivate. In the U.S., 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 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 (e.g., if someone who has not voted and has not gone to prison is more likely to report that they voted than someone who has not voted but did go to prison), then measured participation may be negatively correlated with measured criminality.[1] Similarly, individuals who vote could underreport their criminality relative to those who do not. Fortunately, these threats to inference can be reduced through a randomized inducement of participation and use of administrative records rather than self-reports.

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 devoted to increasing the participation of historically underrepresented groups.[2] 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 increasing participation by underrepresented groups,

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

[2] See http://www.voterparticipation.org/about-us/, retrieved May 2016.

individuals were retained in their sample if they were identified as non-white.[3] Records were then 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[4] in their experimental population and sent them a non-partisan registration mailing informing them that they were eligible to register to vote.[5] The mailing included the relevant voter registration form with a postage-paid envelope for returning the signed document to the appropriate authority.[6] 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. At the same time, as with any experimental sample, our estimates are valid only for this particular sample. While about 86% of all individuals aged 17-18 attend school, for other groups (e.g., for those who have already

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

[4] 86% of cases are single-address records.

[5] Tests reported in the supplemental appendix reveal no imbalances across voter file covariates (race/ethnicity, gender, and state of residence) in the treatment and control groups.

[6] An example of the mailing appears in the supplemental appendix.

dropped out of school, for whites, and for those under state supervision for a juvenile offense) patterns could be different.

In 2011, the VPC's data were merged with voter file information by Catalist, a private list vendor, to obtain the (post-treatment) 2010 registration and voting behavior of these individuals. We obtained the data from the experiment at this point. Of the 664,000 records in the sample, about .3% had only a year of birth and another 10.8% of records had only a year and month of birth.[7] Because we require a valid date of birth for our matching procedure, 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 supplemental appendix Table S2. 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 the level of detail contained in the 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 either include information that allows us to avoid coding as incarcerated those whose offense likely took place prior to Election Day 2010 or a broader set of state supervision statuses (e.g., parole and/or probation). As noted below, all results are robust to analysis limited to these subsets of states. More generally, because we lack information about prior interactions with the criminal justice system, our estimates reflect a combination of reduced

[7] For records with only a month and year of birth, day of birth was often recorded by the original research team as the 1st of the month. For this reason, we exclude all cases with birthdays on the first of the month. These exclusions are implemented using pre-treatment measures.

criminality among those who have not previously interacted with the criminal justice system and diminished recidivism among those who already have. (As with any other source of unobserved heterogeneity, these potential differences will not bias the results of our experimental analysis.)

To merge the state supervision 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.[8]

[Insert Table 1 about here.]

Finally, in order to obtain additional information about the places in which these individuals lived, we successfully geocoded 97.3% of the 568,101 records shown in Table 1 to census block groups using ArcGIS 10.1.[9] We then merged in 2007-2011 American Community Survey (ACS) 5-year estimates for variables used in our statistical analysis to control for unobserved location-specific factors correlated with both the likelihood of voting and subsequent propensity to be under state supervision, as well as to compensate for the paucity of individual-

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[8] Although the process of matching voter file records to state supervision records is imperfect, the error rate should be the same in both the treatment and control groups.

[9] 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 the highest match score.

level covariates available in the original data files. Our final dataset consists of 552,525 records.[10]

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) provide face validity for our procedure matching subject records to the incarceration data (if the matched data produce the same observational correlation found in prior work, it is unlikely that we have failed to reliably locate youths who are later incarcerated). If we find that 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.

To begin, Table 2 reports OLS estimates 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. Because many of the coefficients are small in magnitude, we code this and the other binary dependent variables as 0=no, 100=yes (rather than 0,1) so that the coefficients have fewer leading zeros. Thus a coefficient of -2.0, for example, would mean that a unit change in that variable is associated with a 2 percentage point decrease in the probability of being under supervision. 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

[10] There are no covariate imbalances between the treatment and control groups for this final sample (see tests in the supplemental appendix).

than other nonwhites, women and those whose gender is unknown are less likely to be under supervision than males, and that standard measures of place-level disadvantage and crime (e.g., concentrated urban areas, etc.) are associated with increased risks of incarceration.

[Insert 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%, or from 0.88% to 0.40% ($p<.001$). This reduction is substantively large. Hypothetically, if all 18-22 year olds voted instead of none of them voting, and if the same relationship was observed for other demographic groups, it would correspond to almost 105,000 fewer individuals in prison.[11] Results are substantively the same when we instead estimate probit models (those complete results appear in the supplemental appendix, and average marginal effects estimates of changing from not voting to voting are reported in the bottom of the table). In column (3) we restrict our analysis to those cases for which a predicted risk of state supervision measure that we calculate is less than .01 (1%).[12] This

[11] According to 2015 Census estimates, there are a little over 21.8 million 18-22 year olds in the US. The coefficient is -0.48 percentage points. If all voted rather than none, the reduction in the number incarcerated is 104,785 individuals.

[12] This predicted risk score was obtained by estimating a logistic regression using records in the control group (those not sent a treatment letter in the field experiment) to predict the probability each individual in our dataset was under state supervision. That model, estimated separately for gender groups (male, female, or unknown, as reported by the list vendor), includes indicators for whether an individual is black or Hispanic (an exclusive coding, with all other races making up the excluded category), state fixed effects, and the various ACS survey measures in Table 2.

group comprises about 70% of the sample. For this sample, which we refer to as the low-risk sample, the average proportional effect estimate is comparable: voting reduces the risk of supervision by 57% (to .14% from .34%). 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 than those in the high risk sample who do not vote (0.98% vs. 2.45%).[13] We use this partitioning by predicted risk of supervision in the analysis that follows to demonstrate that it is not just individuals who appear, ex ante, to be at a low risk of being incarcerated who respond to the outreach encouraging participation. That is, the experimental intervention is effective in encouraging participation among individuals with both low and higher ex ante predicted rates of incarceration.

Using the coefficient estimates from that model, we predict the risk measure for the entire sample using each individual's observed (pre-treatment) characteristics. See the supplemental appendix for additional analysis and the underlying coefficient estimates.

[13] In the supplemental appendix, we show these results are robust to limiting the analysis to states where it is less likely that individuals were incarcerated for a crime they had committed prior to the 2010 election. In Ohio and Washington we observe when incarceration began and do not code as incarcerated individuals whose admission date is before June 1, 2011, while in Texas we observe date of offense and do not code as incarcerated individuals whose offense date precedes the 2010 election. In the supplemental appendix, we also show similar results in states where our records of supervision include more low-level punishments and 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 our strategy for measuring 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.[14] We first introduce some notation and then show that under standard assumptions an experiment can yield a consistent (asymptotically unbiased) estimate of the causal effect of voting.[15] Readers familiar with the technical assumptions of causal identification in the potential outcomes framework using randomized experiments may wish to skip ahead.

[14] We assume the effect of participation occurs via voting, rather than registration, since the prior theoretical literature stresses how deliberative and participatory activities—like voting—are transformative. In the supplemental appendix we present parallel analyses assuming that registration is the relevant participatory act. Note that our, or any other, experiment that perturbs both registration and voting, cannot be used to assess through which causal pathway these effects manifest—one reason that an explicit statement of the exclusion restriction must build on theory.

[15] Our technical framework 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 discussion of identification and estimation of causal effects in such circumstances, see Gerber and Green (2012), chapter 6.

Let $Y_i(V_i,Z_i)$ represent whether individual i is subsequently incarcerated ($Y_i = 1$ if incarcerated, 0 otherwise) given V_i (which is equal to 1 if the individual votes, 0 otherwise) and Z_i (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 Y_i may take on different values depending on V_i and Z_i. 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 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) $\mathrm{E}(Y_i(V_i{=}1)) - \mathrm{E}(Y_i(V_i{=}0)) = \mathrm{E}(Y_i(1) - Y_i(0))$

where the function E(X) represents the population average of X.[16]

[16] Our notation assumes that the consequential factor for a subject's treatment and outcome is her own treatment assignment and voting, and not that of the other subjects. This approach follows

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

$+ \mathrm{E}(Y(1)|\text{subject is } C)*\mathrm{P}(C)$,

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

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the assumptions in prior empirical and theoretical investigations, which articulate mechanisms for a direct effect of participation on an individual's mindset and habits.

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).[17] 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) $E(Y|Z{=}1) – E(Y|Z{=}0) =$

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

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

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

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

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

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

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

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 Y_i on Z_i, the subject's incarceration status regressed on her 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 who voted in the control group. We reiterate here that "treated" takes on a different meaning in this context than will be familiar to most readers. Treatment is defined not simply by 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 V_i, treatment status, on Z_i, 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 stages.[18]

[18] In the SA, we present parallel analyses without covariates.

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 for whom geocoding was successful for inclusion of the ACS measures).

Table 3 presents statistical analyses of the effect of the VPC's mailing efforts on voting in 2010.[19] These are the "first-stage" estimates in a two-stage least squares estimator.[20] 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), though, admittedly, the treatment effect is modest (see the next section for an extended discussion of the precision of our two-stage least squares estimates).

[Insert Table 3 about here.]

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)

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

[20] Parallel estimates using probit in the supplemental appendix yield similar results.

present this analysis for the low-risk sample. Among these subjects, 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 (columns [5] and [6], 12.9%, $p<.05$). This is a somewhat smaller absolute and proportional effect than for the entire sample, showing that more at-risk individuals are less likely to participate absent outreach and increase their participation less 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 subsequent incarceration? There are several ways to undertake this analysis, and so we present multiple approaches here. The key intuition that joins 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 allows us to assess the effect of participation on the risk of being incarcerated.

First, we present a reduced-form model estimating the relationship between being sent a 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 estimates 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 treatment 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 probit, which produces the same conclusions for all these columns in Table 4, appears in the supplemental appendix.

[Insert 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, i.e. the exclusion restriction 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 supplemental appendix, we report virtually identical results when we limit the analysis to states for which we can exclude those whose criminal behavior leading to incarceration likely pre-dates treatment implementation or have information on lower-level punishments (i.e., punishments other than time in prison).

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.[21]

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-form 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 interval for our experimental estimate includes Uggen and Manza's (2004) point estimate. Focusing only on the low risk sample, which is perhaps analogous to those who have never previously been arrested, our estimate is instead 11.6 points

[21] A potential concern is that one must vote multiple times to reduce the propensity for criminal behavior. Despite this possibility (discussed in the conclusion), our single election test is justified because prior work has operationalized and provided evidence for the relationship in this manner. In addition, because criminal behavior for those who engage in it generally begins around when they obtain the right to vote (i.e., turn 18), a single election must be sufficient time for the relationship to work if participation is to be an effective means to avert an initial turn to criminal behavior.

with a 95% confidence interval of 1.2 to 22.0 points. How should we think about what this 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 points 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 points to 1 point. In either case, the implications are qualitatively the same: On equal or even reduced footing, these results would lead 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 estimate 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 should instead be 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 control for observable 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, our best assessment of the evidence is that our data lead to an unbiased estimate that is statistically indistinguishable from 0.

What does this 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 in which 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-stage 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 estimate 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 differences, 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 correlation 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 inducing 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 an immediate 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, at least in the domain of criminality leading to incarceration. If such a cycle develops more slowly or over time, then it would need to be started earlier (given that the age range we examine generally corresponds to the period when individuals are first prone to criminality) and/or encompass additional opportunities to cast a ballot. 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 consistent 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 and others (e.g., Pitkin and Schumer 1982), the clearest case for the benefits of participation often seem to arise from forms of participation closer to governing 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 a richer account of citizenship, and then assess the causal effects of this deeper form of 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.[22]

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 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.

[22] For example, Wantchekon (2012) reports the effect on turnout of a large-scale deliberation experiment in Benin.

It is important to note that our experiment is limited in several important ways. First, our estimates, like all estimates, 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. 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 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 than the effect of the intervention we study here. 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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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 (100=yes)		Under State Supervision (100=yes), Low-Risk Sample	Under State Supervision (100=yes), High-Risk Sample
Voted in 2010 (1=yes)		-0.483***	-0.196***	-1.475***
		[0.044]	[0.034]	[0.156]
African American (1=yes)	0.688***	0.693***	0.316***	2.814**
	[0.030]	[0.030]	[0.027]	[1.202]
Hispanic (1=yes)	0.365***	0.363***	0.242***	1.204
	[0.036]	[0.036]	[0.032]	[1.211]
Female (1=yes)	-1.241***	-1.239***	-0.513***	-3.748***
	[0.028]	[0.028]	[0.025]	[0.149]
Gender Unknown (1=yes)	-0.709***	-0.711***	-0.178***	-1.733***
	[0.044]	[0.044]	[0.037]	[0.130]
Proportion Black	0.283***	0.282***	0.128**	0.121
	[0.066]	[0.066]	[0.050]	[0.188]
Proportion Hispanic	-0.640***	-0.639***	-0.077	-1.832***
	[0.078]	[0.078]	[0.061]	[0.316]
Proportion of Kids < 18	0.369***	0.364***	0.108**	0.960***
in Female Headed Household	[0.074]	[0.074]	[0.055]	[0.216]
Proportion of Families Below	0.375***	0.369***	-0.059	1.115***
the Poverty Rate	[0.116]	[0.116]	[0.081]	[0.326]
Proportion of Families Receiving	0.439	0.434	0.017	1.750**
Public Assistance	[0.296]	[0.296]	[0.217]	[0.784]
Proportion of Population Over 25	1.252***	1.243***	0.316***	3.223***
w/. < High School	[0.124]	[0.124]	[0.092]	[0.406]
Log Pop. Density	-0.022**	-0.022**	0.000	-0.076**
(1000 persons per sq mi.)	[0.010]	[0.010]	[0.007]	[0.039]
Constant	0.236***	0.253***	0.218***	-3.411***
	[0.034]	[0.034]	[0.031]	[1.224]
Observations	552525	552525	411477	141048
R^2	0.013	0.013	0.002	0.010
Mean of Outcome in Sample	0.880	0.880	0.343	2.446
Includes State Fixed Effects?	Yes	Yes	Yes	Yes
Average Marginal Effect of Voting in 2010 from Probit Model		-0.542	-0.204	-1.570

Note: Cell entries are OLS coefficient estimates with robust (Huber/White) standard errors in brackets. Dependent variable coded as 0=no, 100=yes. *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 (100=yes)		Voted in 2010 (100=yes), Low-Risk Sample		Voted in 2010 (100=yes), High-Risk Sample	
Treated (Sent Registration	0.488***	0.485***	0.552***	0.548***	0.301**	0.304**
Form 2010, 1=yes)	[0.071]	[0.071]	[0.084]	[0.084]	[0.135]	[0.135]
African American (1=yes)	1.014***		1.018***		1.941***	
	[0.084]		[0.091]		[0.107]	
Hispanic (1=yes)	-0.228**		-0.313***		1.352***	
	[0.091]		[0.095]		[0.180]	
Female (1=yes)	0.426***		0.413***		0.340**	
	[0.050]		[0.065]		[0.147]	
Gender Unknown (1=yes)	-0.367***		-0.523***		0.013	
	[0.064]		[0.083]		[0.118]	
Proportion Black	-0.191*		-0.166		-0.213	
	[0.111]		[0.139]		[0.197]	
Proportion Hispanic	0.208		0.337**		-0.232	
	[0.146]		[0.168]		[0.327]	
Proportion of Kids < 18	-1.024***		-1.107***		-0.785***	
in Female Headed Household	[0.121]		[0.148]		[0.211]	
Proportion of Families Below	-1.261***		-1.372***		-0.962***	
the Poverty Rate	[0.177]		[0.219]		[0.301]	
Proportion of Families Receiving	-1.011**		-1.439***		-0.537	
Public Assistance	[0.447]		[0.555]		[0.762]	
Proportion of Population Over 25	-1.794***		-1.735***		-1.955***	
w/. < High School	[0.201]		[0.243]		[0.374]	
Log Pop. Density	-0.094***		-0.058**		-0.233***	
(1000 persons per sq mi.)	[0.021]		[0.023]		[0.045]	
Constant	2.994***	2.511***	2.936***	2.574***	2.328***	2.326***
	[0.115]	[0.067]	[0.124]	[0.079]	[0.345]	[0.127]
Observations	552525	552525	411477	411477	141048	141048
R^2	0.006	0.000	0.007	0.000	0.005	0.000
F-test Statistic	121.03	46.09	102.77	42.63	63.46	5.06
F-test p-value	0.000	0.000	0.000	0.000	0.000	0.025
Mean of Outcome in Sample	2.511	2.511	2.574	2.574	2.326	2.326
Includes State Fixed Effects?	Yes	No	Yes	No	Yes	No
Average Marginal Effect of Treatment from Probit Model	0.481	0.485	0.549	0.548	0.285	0.304

Note: Cell entries are OLS coefficient estimates with robust (Huber/White) standard errors in brackets. Dependent variable coded as 0=no, 100=yes. *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 (100=yes)	Instrumental Variables Regression (2SLS), Under State Supervision (100=yes)	Under State Supervision (100=yes), low risk sample	Instrumental Variables Regression (2SLS), Under State Supervision (100=yes), low risk sample	Under State Supervision (100=yes), high risk sample	Instrumental Variables Regression (2SLS), Under State Supervision (100=yes), high risk sample
Treated (Sent Registration	0.045		0.064**		-0.006	
Form 2010, 1=yes)	[0.041]		[0.028]		[0.137]	
Voted in 2010 (1=yes)		9.183		11.598**		-1.924
		[8.440]		[5.352]		[45.531]
African American (1=yes)	0.688***	0.595***	0.314***	0.196***	2.785**	2.822*
	[0.031]	[0.091]	[0.027]	[0.062]	[1.202]	[1.478]
Hispanic (1=yes)	0.365***	0.385***	0.243***	0.279***	1.184	1.210
	[0.036]	[0.042]	[0.032]	[0.037]	[1.211]	[1.346]
Female (1=yes)	-1.241***	-1.280***	-0.514***	-0.562***	-3.753***	-3.746***
	[0.028]	[0.046]	[0.025]	[0.035]	[0.149]	[0.218]
Gender Unknown (1=yes)	-0.709***	-0.676***	-0.177***	-0.116**	-1.733***	-1.733***
	[0.045]	[0.055]	[0.037]	[0.048]	[0.130]	[0.130]
Proportion Black	0.283***	0.300***	0.128**	0.148***	0.124	0.120
	[0.066]	[0.069]	[0.050]	[0.054]	[0.189]	[0.211]
Proportion Hispanic	-0.640***	-0.659***	-0.078	-0.117*	-1.829***	-1.833***
	[0.079]	[0.082]	[0.061]	[0.066]	[0.317]	[0.332]
Proportion of Kids < 18	0.369***	0.463***	0.110**	0.238***	0.972***	0.957**
in Female Headed Household	[0.074]	[0.115]	[0.055]	[0.083]	[0.216]	[0.420]
Proportion of Families Below	0.375***	0.491***	-0.057	0.103	1.130***	1.111**
the Poverty Rate	[0.116]	[0.158]	[0.081]	[0.112]	[0.327]	[0.547]
Proportion of Families Receiving	0.440	0.533*	0.021	0.188	1.758**	1.747**
Public Assistance	[0.296]	[0.311]	[0.217]	[0.238]	[0.785]	[0.822]
Proportion of Population Over 25	1.252***	1.416***	0.319***	0.521***	3.251***	3.214***
w/. < High School	[0.124]	[0.196]	[0.092]	[0.134]	[0.406]	[0.976]
Log Pop. Density	-0.022**	-0.013	0.000	0.007	-0.073*	-0.077
(1000 persons per sq mi.)	[0.010]	[0.013]	[0.007]	[0.008]	[0.039]	[0.113]
Constant	0.196***	-0.079	0.154***	-0.187	-3.444***	-3.399**
	[0.050]	[0.292]	[0.040]	[0.187]	[1.233]	[1.720]
Observations	552525	552525	411477	411477	141048	141048
R^2	0.013		0.002		0.010	
Mean of Outcome in Sample	0.880	0.880	0.343	0.343	2.446	2.446
Includes State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Average Marginal Effect of Treatment from Probit Model	0.048		0.064		-0.026	

Note: Cell entries are OLS coefficient estimates with robust (Huber/White) standard errors in brackets. Dependent variable coded as 0=no, 100=yes. *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.