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Can Incarcerated Felons Be (Re)integrated into the Political System? Results from a Field Experiment

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Abstract

How does America's high rate of incarceration shape political participation? Few studies have examined the direct effects of incarceration on patterns of political engagement. Answering this question is particularly relevant for the 93% of formerly incarcerated individuals who are eligible to vote. Drawing on new administrative data from Connecticut, we present evidence from a field experiment showing that a simple informational outreach campaign to released felons can recover a large proportion of the reduction in participation observed following incarceration. The treatment effect estimates imply that efforts to reintegrate released felons into the political process can substantially reduce the participatory consequences of incarceration.

Word Count: 8041

Between 1980 and 2010, the proportion of the U.S. adult population that had been convicted of a felony increased by 140% (Shannon et al. 2011). One collateral consequence of this rapid growth of what scholars have termed the “carceral state” (e.g., Gottschalk 2006) is that millions of American citizens are ineligible to vote. Every state other than Maine and Vermont prohibits incarcerated felons from voting, with some continuing to restrict voting rights even after discharge from the criminal justice system. Seminal work by Uggen and Manza (2002) and Manza and Uggen (2006) argues that criminal disenfranchisement has important consequences for election outcomes because the criminally disenfranchised population disproportionately supports Democratic candidates (though see Burch 2012). Uggen and Manza primarily focus on the formally disenfranchised population, and thus their work may understate the electoral consequences of criminal disenfranchisement if its effects continue to suppress the political behavior of convicted felons who have completed their sentences and are eligible to vote. Approximately 93% of those who were formerly incarcerated, or about 14 million individuals, are eligible to vote (Shannon et al. 2011), but previous research documents that released felons¹ vote at low rates even in states where their voting rights are restored upon release. Are there interventions that can help the formerly incarcerated population become integrated into political life?

There are many reasons that incarceration might reduce political participation. Contact with the criminal justice system disrupts social capital, stigmatizes convicted criminals, and establishes negative associations with government. Additionally, even if the right to vote is

¹ Throughout the paper, we use the term “felon” to refer explicitly to those both convicted of a felony and incarcerated in a state correctional facility because of this conviction and “ex-felon” or “former felon” to refer explicitly to felons who have been discharged from both incarceration and parole. Not all convicted felons are incarcerated, but we limit our analysis to incarcerated individuals because confinement likely represents the largest detrimental effect of conviction and because incarceration is a pre-requisite for disenfranchisement in Connecticut, the site for our field experiment. Scholars have looked at other interactions with the criminal justice system but continue to find particularly negative effects associated with incarceration (e.g., Weaver and Lerman 2010).

restored upon release, individuals who lose voting rights while in prison must take the step of re-registering after their rights have been restored. Further, individuals who are actually eligible to vote may *believe* that they are legally disenfranchised, perhaps because election officials are ill-informed about how and when ex-felons' voting rights are restored (Drucker and Barreras 2005; Ewald 2009; Wood and Bloom 2008). At the same time, a lack of social capital and negative associations with government may produce individuals both unlikely to vote (Hjalmarsson and Lopez 2010; Weaver and Lerman 2010) and likely to end up in the criminal justice system in the first place (Buonanno et al. 2009), in which case incarceration may be a relatively minor factor in explaining low levels of participation by released felons.

In recent years, several states have undertaken reforms designed to increase engagement by released felons (Porter 2010). Several state legislatures have specifically debated legislation that would increase the state's responsibility to notify newly eligible ex-felons of their voting rights (Meredith and Morse 2014). Are such outreach efforts likely to be effective? Can eligible released felons be convinced to register and vote?

To answer this question, we present results from a novel field experiment conducted in Connecticut to assess the efficacy of efforts to reach out to released felons by encouraging them to register and vote. While the incarcerated population is disenfranchised in Connecticut, felons are free to register and vote upon discharge from both physical incarceration and parole.² We presently know little about whether direct outreach can increase this population's engagement with politics. On the one hand, felons vote at low rates *prior* to incarceration, which, when coupled with the negative consequences of being incarcerated, may make the recently enfranchised population difficult to mobilize. On the other hand, outreach may be particularly

² There is one exception: Those convicted of certain election-related crimes must also be discharged from probation. No such cases appear in the sample of released felons examined in our study.

effective for this population because ex-felons may be ignored by contemporary campaigns, in part because of registration purging, and also because some ex-felons incorrectly believe that they are ineligible to vote. Prior observational research on the effect of efforts to increase participation among eligible felons produces conflicting conclusions (Meredith and Morse 2014), showing the need for experimental tests of efforts to encourage participation by former felons. Successfully mobilizing released felons may narrow some of the important gaps in voting between key demographic groups, thereby reducing one of the many collateral consequences of incarceration. Further, if political participation molds individuals into virtuous citizens, outreach may also be a means to reduce recidivism (Uggen and Manza 2004; Uggen et al. 2004).

In October 2012, we worked with the Connecticut Secretary of State to contact a randomly selected subset of unregistered released felons who were eligible to register and vote. An outreach message informed these released felons of their eligibility to participate. Relative to an uncontacted control group, those sent a letter informing them of their eligibility to vote were 1.8 percentage points more likely to register and .9 points more likely to vote in the November 2012 election. These treatment estimates correspond to proportional increases in registration and voting rates of about 30% (the control group registration and turnout rates were 6% and 3%, respectively). Simple interventions designed to encourage released felons to participate in the political process therefore appear worthwhile. Given that even a single mailing generated these effects, this research suggests that more sustained outreach could perhaps be even more effective.

These results show the clear value of experimental tests of outreach to this important and growing population. Additionally, our results also have implications for ongoing research about the effects of felon disenfranchisement laws. Research that leverages differences across states to

compare felons who are and are not affected by these laws finds that they have modest effects on aggregate turnout (e.g., Miles 2004, which uses a pooled cross section design). A key assumption of such approaches is that previous disenfranchisement has no demobilizing effect on the currently enfranchised ex-felon population. If expanded outreach increased turnout among enfranchised felons, the negative consequences of formal prohibitions on voting in some states would become even more apparent.

The Criminal Justice System and Political Participation

Early research on the effect of the criminal justice system on political participation focused on the impact of formal post-incarceration disenfranchisement laws. Uggen and Manza (2002) estimated that those laws were responsible for altering several key election outcomes. They simulated the predicted voting rates of disenfranchised felons assuming that those rates, if their rights were restored, would be the same as the observed voting rates of non-felons with similar demographic characteristics (gender, race, age, education, marital status, income, and labor force status). Later scholars questioned this assumption and argued that those estimated voting rates overstated the effect of felon disenfranchisement because felons vote at low rates even when eligible to do so. Haselswerdt (2009), for example, found that in one New York county where released felons are eligible to vote, less than 10% did so, a rate much lower than comparable non-felons. Hjalmarsson and Lopez (2010) similarly used past felony conviction information matched to survey data to show that reenfranchised convicted felons are less likely to vote than individuals who have never been convicted of a felony, even when controlling for a host of demographic and related variables.³

It is unknown whether the low rates of participation by reenfranchised convicted felons

³ Miles (2004) examines how state-level turnout is affected by felon disenfranchisement laws and argues that those laws have minimal effects because the population most affected by them is already unlikely to vote.

reflect the effects of being incarcerated or the pre-existing characteristics of individuals who are later incarcerated. There are a variety of mechanisms by which a prison experience might reduce political involvement. For many citizens in urban communities, their primary experience with the state, a decidedly negative one for those who are incarcerated, may be through the criminal justice system (Weaver and Lerman 2010; Western 2006). These interactions may be essential in shaping their attitudes about political participation and trust in government. Lerman (2013), for example, argues that the social capital of formerly incarcerated individuals is degraded by their experiences in prison. Post-incarceration, former felons find it more difficult to obtain and maintain employment (Pager 2003), earn less than non-felons (Western 2002), are less residentially stable (Fleisher and Decker 2001), and are less likely to be married (Western 2006), all factors that likely depress subsequent participation.

On the other hand, the lower rate of ex-felon participation may be due to selection. In this context, selection refers to differences in the types of people who are convicted of a felony that predate their incarceration. Former felons may participate less not because they have been convicted of a crime, but because the same choices and circumstances that eventually lead to a felony conviction also explain reduced levels of political participation (Miles 2004). Uggen et al. (2006, 295) summarize these differences between felons and non-felons: “Compared to the nonincarcerated population, prisoners have long been undereducated, underemployed, relatively poor, and disproportionately nonwhite.” These differences extend to individual- and family-level measures. For example, having a father with an incarceration history is associated with a heightened likelihood a man becomes a felon (Murray and Farrington 2008; Roettger and Swisher 2009), as well as a greater probability of physical aggression and other actions correlated with criminal behavior (Wildeman 2010). Other familial factors (such as poor parental

supervision, large family size, violent parents, and young mothers; Farrington 1998) and tendencies toward antisocial behavior and impulsivity are key determinants of future criminal behavior (Farrington 1998; Gottfredson and Hirschi 1990). Cumulatively, these factors may explain why other individuals who are apparently identical in most ways, but who do not exhibit such tendencies, both do not enter prison and do vote (Gottfredson and Hirschi 1990).

Supporting this perspective, Burch (2011) uses administrative data on voting and incarceration in five states to show that people who were first incarcerated in 2009 voted at low rates in the 2008 presidential election.

In light of the low rates of participation by released eligible felons, a related question is whether they can be induced to become politically active. Incarcerated felons lose the right to vote in every state except Maine and Vermont. Once those formal prohibitions on participation are lifted, can these “former felons” be reintegrated into the democratic process? While low rates of pre-incarceration voting by felons prior to their incarceration may bound the potential effects of seeking to mobilize former felons, survey data suggest that released felons are often unaware of their rights to register and vote. Drucker and Barreras (2005), for example, observe that a majority of the convicted criminals they survey in New York, Connecticut, and Ohio are misinformed about their eligibility to vote. These findings comport with anecdotal evidence demonstrating that former felons are unaware of their voting rights (Uggen et al. 2005). Manza and Uggen (2006) show that former felons report that this misinformation comes from probation and parole officers as well as employees of correctional institutions. Election administrators may also contribute to misinformation, as surveys reveal that these officials often do not understand the details of criminal disenfranchisement policy in the jurisdictions they oversee (Allen 2011; Ewald 2005).

In light of this apparent lack of knowledge by former felons about their right to vote, a relevant question is whether seeking to correct these views may increase political participation. Two studies provide contrasting answers (CITATION DELETED; Meredith and Morse 2014). CITATION DELETED compare the participation in 2008 of two groups of Iowa former felons who were both eligible to register and vote: those released prior to July 4, 2005, who were retroactively made eligible to vote, and those released between July 4, 2005 and September 30, 2008, who were both eligible upon release and informed of this right in a letter sent by the governor. Using a quasi-experimental discontinuity design, they estimated that the combination of eligibility upon release and formal notification from the governor increased participation in the 2008 election by between 3.9 and 6.8 percentage points.

Meredith and Morse (2014) examine the effect of changes in notification laws in three states: New Mexico, New York, and North Carolina. In New Mexico, the new notification is a formal discharge certificate automatically presented when leaving state supervision, while in New York and North Carolina the new notifications were enhancements to the information in the discharge packets provided to those leaving state supervision. In these three states, they find no evidence that changes in notification affect the rates at which released felons register and vote.

In both studies, the key assumption is one of equivalence over time: felons released before and after the changes in notification must be identical for all reasons that could otherwise explain turnout, and there must be no other changes over time that would otherwise affect participation. Additionally, in the case of the Iowa study, the key question is whether it is the notification or changes in formal eligibility when first released that matters, because both policies change at the same time. This uncertainty suggests the value of conducting a more controlled evaluation of outreach to released felons.

Field Experimental Evidence on Inducing Released Felons to Participate

Given that felons vote at low rates after their release from incarceration, can these individuals be (re)integrated into the political system? Connecticut is one of a majority of states that disenfranchise incarcerated felons but automatically restores voting rights once a felon has completed certain parts of his or her sentence. In Connecticut, if a convicted felon has a voter registration record, this record is supposed to be purged from the statewide active voter registration database upon incarceration. But once convicted felons are discharged from both incarceration and parole, they are free to register as if they had never been convicted. The state notifies them of this right in the packet of materials given to felons being discharged from parole (see section A of the Supporting Information), so those who are interested in registering may already take this action. The types of people who are incarcerated for felony convictions and their experiences while incarcerated may also make few from this population amenable to mobilization efforts. On the other hand, ex-felons must know that they can reregister and must do so in order to be eligible to vote. Can additional outreach bring these individuals into the political process?

To answer this question, we worked with the Connecticut Secretary of State in the fall of 2012 to implement a field experiment designed to understand the effect of alerting unregistered members of the formerly incarcerated population of their rights to register to vote.⁴ A randomly selected subset of “former felons” were sent one of two letters on Secretary of State letterhead informing them that they were currently not registered to vote but were eligible to do so. This design allows us to indirectly test whether at least part of the explanation for depressed former felon participation rates derives from the formal requirement to re-register and/or confusion

⁴ All costs of the experiment were borne by the researchers.

about eligibility to do so. By comparing the registration and voting behavior of those sent a letter to those former felons not contacted, we can understand whether additional outreach can cause former felons to become politically active. Additionally, by leveraging additional information about their pre-incarceration participation, we can assess whether individuals who voted prior to being incarcerated are more amenable to these outreach efforts.

Experimental Design

To determine a population of former felons from which to sample, the Connecticut Secretary of State's office provided us with monthly records of all individuals convicted of a felony between July 1, 2009 and June 30, 2012, as well as lists of all individuals released from prison and discharged from parole over the same timeframe. Using the unique state inmate identifier, we first identified from these records 6,441 former felons released between these two dates who had been convicted of less serious crimes,⁵ were still out of prison, had valid mailing addresses at the last address reported to the Department of Corrections,⁶ would be at least 18

⁵ We excluded individuals from the sample who were incarcerated for more than three years, who were convicted of certain violent crimes, or who were convicted of an unusual crime. We excluded from the sample individuals who served a sentence of more than three years, which comprised about 12% of the initial population, because of concerns that we would be unable to obtain information about their pre-incarceration participation.

Individuals convicted of certain violent crimes—sexual assault, crimes with children as victims, or crimes resulting in the death of another person—were also removed. Additionally, cases for crimes in which serious bodily harm to the victim could have occurred were excluded if the felon served a sentence of more than a year. We chose the one year cutoff based on the advice of a Connecticut attorney that a sentence of more than one year was likely to indicate that the individual's crime involved serious bodily injury or the risk of such injury. In light of our resource constraints for this initial study, we removed these violent criminals (who comprised about 18% of the initial population, some of whom were also part of the 12% who served sentences of more than three years) because we believed they were less amenable to our treatment.

Finally, after eliminating cases of violent or potentially violent crime, in the remaining sample we also eliminated all crimes with fewer than ten instances in the corrections data, which removed a further 2% percent of the initial population.

A complete list of the crimes included in our analysis appears in Table SI.3.1 and a complete list of the violent or potentially violent crimes excluded appears in Table SI.3.2 of the Supporting Information. In Connecticut, the crimes committed by individuals included in our field experiment represent about 82% of the felony convictions for which individuals were sent to prison between July 1, 2009 and June 30, 2012. In the country as a whole, about 72% of felons were sentenced in 2011 for similar crimes.

⁶ The vendor hired to produce the mailings ran all of our records through the National Change of Address list and address standardization software, a procedure that flagged a number of entries as invalid. Some cases involved addresses that, as listed, did not exist, often due to spelling or punctuation errors the software could not decipher. We manually checked each address, making subjective decisions on whether we could identify the correct address.

years old on Election Day, and were not currently registered to vote.

To identify active registrants, we matched the list of former felons with a Connecticut voter file generated on August 22, 2012. In conducting this merge, which is explained in greater detail in Section 3 of the Supporting Information, we relied on the date of birth, name, and address information obtained from the Connecticut Secretary of State. Of note, we found (and excluded) 1,173 former felons who met the remaining criteria but had registration dates that *preceded* their release dates, which suggests they were not successfully purged from the voter rolls upon conviction.⁷ We also found and excluded 404 former felons who had already registered after their release. Cumulatively, this means that about 20% of our potential sample is excluded from the experiment for already being registered.

We then randomly assigned individuals to one of three groups; a control, which consisted of 50% of the sample (3,218 cases), and two different treatments (described below), with 25% of the sample receiving each treatment (1,611 and 1,612, respectively). Assignment was within blocks defined for individual crimes (e.g., burglary in the third degree) to ensure balance across conditions. Leftover cases that could not be blocked were independently and randomly assigned. Comparisons of control and treatment groups revealed no imbalances on age, time served in prison, time since release from prison, and 2008 turnout behavior.⁸ After assignment, but before the treatments were delivered, we received conviction records for July, August, and September

We erred on the side of caution in light of the nature of the reason for assignment into the treatment group (being a former felon). In all, this process excluded 745 individuals with inadequate mailing information.

⁷ Although the Help America Vote Act (HAVA) increased the importance of state actors in managing state voter rolls, a majority of states involve local officials in implementing purges of convicted felons (Ewald 2009). Our findings highlight the potential disparities created by using such a bottom-up system. In Connecticut, the Department of Corrections sends the Secretary of State information on individuals who were incarcerated for a felony in the previous month, which the Secretary of State in turn sends to a town's election administrators. It is then the responsibility of each town to remove newly disenfranchised voters from its voting rolls. That 18 percent of our potential sample still has a pre-conviction active registration record suggests there is substantial slippage in this process.

⁸ See Table SI.2.1 of the Supporting Information.

of 2012. This led us to remove an additional 161 individuals from the experiment who had been newly convicted of an additional felony (84 cases from the control and 37 and 40 from the two treatment groups, respectively). Final counts by assignment were 3,134 to the control and 3,146 to the two treatment groups (1,574 and 1,572). The total N for the experiment sample is 6,280.

In this final analysis sample, individuals were first sentenced between July 2006 and June 2012, and were released between July 1, 2009 and June 29, 2012. The average age of the released felons on Election Day in 2012 was 35, with a range from 18 to 89. These individuals had served an average sentence of 1 year, with a median sentence of approximately 11 months. Overall, this sample constitutes 53% of all felons released during this period. The five most frequent crimes in this sample are possession of narcotics (18%), sale of narcotics (16%), burglary in the third degree (13%), violation of protective order (9%), and larceny in the third degree (6%). A complete list of crimes appears in Table SI.3.1 of the Supporting Information.

Roughly a week before the deadline to register via mail (and two weeks before the in-person registration deadline), each ex-felon in the two treatment groups was sent a mailing in an envelope bearing the Secretary of State's seal and return address. Each mailing contained the official Connecticut mail-in voter registration card and one of two treatment letters on the Secretary of State's letterhead bearing the Secretary's signature. We label these letters as "assurance" and "assurance expanded." They provided the ex-felons with general information related to the upcoming election and informed them that they were not currently registered to vote. The letters also contained appeals to civic duties and responsibilities to vote, and detailed how to register before the deadlines.⁹

Passages in the letters were crafted to address the ambiguous signals received by former

⁹ The civic duty appeals mirror those employed in other successful GOTV field experiments (e.g., Gerber and Green 2000). See Section 4 of the Supporting Information for treatments.

felons regarding their ability to vote once released from state supervision (Drucker and Barreras 2005; Manza and Uggen 2006; Uggen et al. 2005). Both explicitly stated, “According to our records, **you are eligible to register and vote**” (boldface type in original). Additionally, the *assurance expanded* mailing contained a paragraph designed to alleviate any concerns that their felon status might cause them to be turned away at the polls, forced to explain their reason for being unregistered, or publicly embarrassed in any manner due to their previous incarceration.

The letters were identical except for this section, which read:

According to the current election rules, once you have successfully registered, you will not have to explain why you were previously unregistered when you show up to vote on Election Day. Election officials at the polling place will also not ask you any questions about your past. You will be treated with the same respect due to all of those who vote in Connecticut’s elections. (Boldface type in original.)

We note that both treatment letters are designed to be innocuous. That is, while they inform the recipients that the Secretary of State believes they are eligible to register to vote, they do not mention that the recipient is a newly eligible released felon. Thus, if the person continues to believe (incorrectly) that former felons cannot legally register, this letter does not directly address that false belief. Nonetheless, out of concern that treatment letters might be misdirected to someone other than the intended recipient, and that the person who opened the letter could otherwise be unaware that the intended recipient was a released felon, we chose not to mention former felon status in the treatment mailing. This may have limited the treatment’s potential effect.¹⁰

Our experimental design did not include a placebo treatment that excluded information about eligibility to register while retaining the general information related to the upcoming election and a civic duty appeal. Instead, we simply did not seek to contact those in the control

¹⁰ A substantial proportion of the incarcerated population reads at or below a grade-school level (Kozol 1985), which also likely limits the efficacy of our mailing.

condition. Thus, comparison of the behavior of those in the treatment conditions to those in the control condition estimates the total effect on released felons of an encouragement to register and vote, rather than the separate effects of these aspects of the treatment letter. Comparisons between the two treatment conditions, however, allow us to assess whether the expanded assurances in one condition might have larger effects on behavior.

We focus on two outcomes in our analysis: registration for the November 2012 election and voting in that election. All participants in the experiment were initially unregistered. We repeated the matching process used earlier to link individuals in the experimental population to a post-election voter file provided in January of 2013. Individuals successfully matched to the voter file as active registrants with registration dates at least one week before the election (the cutoff date in Connecticut) were coded as registered, and their turnout was measured as recorded in the voter file.¹¹ Individuals in the experiment who were not matched to the post-election voter file were coded as not having registered or voted.

Results

We begin with a simple comparison of average registration and turnout in the control and treatment conditions. These figures are provided in Table 1. In the non-contacted control group shown in column (1), the registration rate is 5.9% and the turnout rate is 3.0%.¹² First consider the effect of the mailing on registration. In the treatment groups, the registration rate is 7.6% in

¹¹ This date was chosen because all towns would have submitted registration updates by this date.

¹² If we group together individuals who were excluded from our experiment for prior registration (1173 with registration dates preceding incarceration and 404 after) along with those in the control group, the baseline rates of participation for uncontacted felons are higher: 37.3% registered and 8.4% voted. The large disparity between registration in the control group and registration in this expanded uncontacted group is due to the obvious fact that all individuals excluded due to prior registration are in fact registered. This registration rate is comparable to ex-felon registration rates reported in Burch (2011). The turnout figure, however, is slightly lower. In Florida, Georgia, Michigan, Missouri, and North Carolina, Burch (2011) reports registration rates of 23.7%, 34.5%, 58.9%, 35.1%, and 35.5%, respectively, and turnout rates of 11.1%, 22.7%, 34.7%, 19.4%, and 24.2%, respectively, among ex-felons in the 2008 general election.

We note that while Burch presents estimates for the entire ex-felon population of those states, the Connecticut estimates are for specific crimes and have also been recently released from prison.

Table 1: 2012 Registration and Turnout by Experimental Condition

	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Assurance	Expanded Assurance	Pooled Assurance	Pooled Assurance Mail Returned	Pooled Assurance Mail Not Returned
Mean Registration	0.059 [0.004]	0.076 [0.007]	0.077 [0.007]	0.077 [0.005]	0.056 [0.006]	0.091 [0.007]
Difference of Proportion Tests Relative to Control (Registration)		0.018 0.010	0.018 0.008	0.018 0.002	-0.003 0.347	
Mean Turnout	0.030 [0.003]	0.041 [0.005]	0.038 [0.005]	0.039 [0.003]	0.025 [0.004]	0.049 [0.005]
Difference of Proportion Tests Relative to Control (Turnout)		0.011 0.028	0.008 0.085	0.009 0.024	-0.005 0.167	
Number of Observations	3,134	1,574	1,572	3,146	1,258	1,888

Note: See text for details about dataset construction. Entries for means are means with standard deviations in brackets. Entries for differences of proportions tests are differences and their associated one-sided p-values. Difference of proportions test for Assurance treatment relative to the Expanded Assurance treatment has a p-value of .469. Using Bonferroni-corrected p-values for the three treatment group comparisons of Assurance relative to Control, Expanded Assurance relative to Control, and Expanded Assurance relative to Assurance, for Registration, the former two are significant at $p < .05$ and the latter is not significant. For turnout, Assurance relative to Control is significant at $p < .1$, while the latter two comparisons are not significant.

the assurance condition (column [2]) and 7.7% in the expanded assurance condition (column [3]). In difference of proportions tests, both figures are statistically distinguishable (at $p < .05$ and $p < .01$, one-tailed tests, respectively)¹³ from registration in the control condition. Substantively, the treatment letters are associated with modest absolute, but proportionally large, increases in registration of about 29% to 31%. Both estimates are highly similar, suggesting minor or no difference across treatment conditions. In column (4) we pool both treatments and estimate a registration rate of 7.7%, which is 1.8 points higher than in the control group and is distinguishable at $p < .01$.

For the November 2012 election, turnout is 4.1% in the assurance condition and 3.8% in the expanded assurance condition, representing proportional increases relative to the control condition of 37% and 27%, respectively. The difference between the assurance condition and the control group is 1.1 points and the difference between the pooled treatment groups and the control group is .9 points, both of which are significant at $p < .05$. The difference between the expanded assurance condition and the control is .8 points and is significant at $p < .1$.

These initial results have two important implications. First, the treatments had (proportionally) large effects on a population otherwise unlikely to register or vote. Second, there appears to be little difference between the two treatments in their effectiveness, although our modest sample sizes make comparisons of the two treatments difficult. In the remainder of our analysis we therefore pool the two treatments together (as in column [4]) and consider comparisons only between the pooled treatments and the control condition. Larger sample sizes and replication would be necessary to infer whether the estimated differences in the effectiveness of the different treatment letters are robust. (We discuss the analysis reported in columns [5] and

¹³ We present one-tailed p-values for comparisons of treatment to control because we predict that the treatments will increase registration and voting.

[6] later in this section.)

In Tables 2 and 3 we compare registration and turnout in the control and (pooled) treatment conditions using OLS regression. This approach allows us to account for additional covariates provided in the (pre-treatment) felon release files and the blocked (by crime) experimental design. In Table 2, the outcome of interest is registration for the 2012 election. We begin with a simple bivariate model with only a treatment indicator in column (1) and add crime-type fixed effects in column (2). These estimates are identical to each other and to the Table 1 differences of means, and imply that the treatment increased registration rates about 1.8 points on a base of 5.9%. In columns (3), (4), and (5), we add additional covariates, including prior voting behavior (the excluded category is those who were ineligible in 2008, either because they were younger than 18 or because they were incarcerated during that election), and the estimates are unchanged at 1.8 points and remain statistically significant.¹⁴

In Table 3 we focus on voting in the 2012 election. As with the estimates for registration, across all specifications we estimate the same statistically significant ($p < .05$) .9 point effect of being sent a treatment letter, which is the same difference we find in a simple difference of means test and represents a proportional increase in turnout of between 23 and 30%, depending on the specification.

How does this effect size compare to what we would expect if we sent a similar piece of mail to a member of the general population? Green et al.'s (2013) meta-analysis of 110 field experiments employing non-social pressure and non-advocacy mailings shows that being sent a mailing increases turnout by approximately 0.19 percentage points. Thus, this mailing has larger effects than normal, despite the much lower baseline rates of participation by this group.

¹⁴2008 voting behavior is collected from an early 2009 Connecticut voter file. Subjects in the experiment were matched to that voter file using the same multi-stage process detailed above.

Table 2: Effect of Outreach on 2012 Registration, Experimental Results

	2012 Registration (Yes = 1) among Experiment Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
SoS Letters Combined	0.018 [0.006]***	0.018 [0.006]***	0.018 [0.006]***	0.018 [0.006]***	0.018 [0.006]***	
Log Days Served			0.001 [0.002]	0.001 [0.003]	0.001 [0.003]	0.001 [0.003]
Age on Election Day (years)			-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.001]	-0.001 [0.001]
Age ²			0.000 [0.000]	0.000 [0.000]*	0.000 [0.000]	0.000 [0.000]
Time Since Release (years)			-0.027 [0.019]	-0.026 [0.023]	-0.027 [0.024]	-0.028 [0.024]
Time Since Release ²			0.004 [0.005]	0.004 [0.006]	0.004 [0.007]	0.004 [0.007]
Eligible and Voted, 2008					0.113 [0.026]***	0.074 [0.034]**
Eligible and Did Not Vote, 2008					-0.007 [0.006]	-0.012 [0.008]
SoS Letters Combined * Voted 2008						0.084 [0.047]**
SoS Letters Combined * Did Not Vote (but Eligible) 2008						0.019 [0.007]***
SoS Letters Combined * Did Not Vote (and Not Eligible) 2008						0.009 [0.009]
Constant	0.059 [0.004]***	0.059 [0.003]***	0.089 [0.047]*	0.083 [0.030]***	0.078 [0.030]**	0.083 [0.029]***
Observations	6,280	6,280	6,280	6,280	6,280	6,280
R ²	0.001	0.001	0.006	0.006	0.013	0.014
Number of Fixed Effects Units (Crimes)		37		37	37	37
R ² Within		0.001		0.006	0.013	0.014
Mean of DV in Control Group			0.059			

Note: Cell entries are OLS coefficients with robust standard errors in brackets, using fixed effects for crime in columns 2, 4, 5, and 6. *p<.1, **p<.05, ***p<.01; one-tailed tests for SoS Letters Combined in columns 1-5 and for all SoS Letters Combined interaction terms in column 6.

Table 3: Effect of Outreach on 2012 Voting, Experimental Results

	2012 Voting (Yes = 1) among Experimental Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
SoS Letters Combined	0.009 [0.005]**	0.009 [0.004]**	0.009 [0.005]**	0.009 [0.004]**	0.009 [0.004]**	
Log Days Served			0.002 [0.002]	0.002 [0.001]	0.003 [0.001]*	0.002 [0.001]*
Age on Election Day (years)			-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.001]	-0.001 [0.001]
Age ²			0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
Time Since Release (years)			-0.009 [0.013]	-0.007 [0.022]	-0.009 [0.022]	-0.010 [0.022]
Time Since Release ²			0.001 [0.004]	0.000 [0.006]	0.001 [0.006]	0.001 [0.006]
Eligible and Voted, 2008					0.110 [0.023]***	0.074 [0.031]**
Eligible and Did Not Vote, 2008					0.000 [0.004]	0.000 [0.004]
SoS Letters Combined * Voted 2008						0.077 [0.040]**
SoS Letters Combined * Did Not Vote (but Eligible) 2008						0.007 [0.004]*
SoS Letters Combined * Did Not Vote (and Not Eligible) 2008						0.006 [0.007]
Constant	0.030 [0.003]***	0.030 [0.002]***	0.039 [0.033]	0.039 [0.028]	0.030 [0.027]	0.032 [0.028]
Observations	6,280	6,280	6,280	6,280	6,280	6,280
R ²	0.001	0.001	0.005	0.005	0.017	0.018
Number of Fixed Effects Units (Crimes)		37		37	37	37
R ² Within		0.001		0.005	0.017	0.018
Mean of DV in Control Group			0.030			

Note: Cell entries are OLS coefficients with robust standard errors in brackets, using fixed effects for crime in columns 2, 4, 5, and 6. *p<.1, **p<.05, ***p<.01; one-tailed tests for SoS Letters Combined in columns 1-5 and for all SoS Letters Combined interaction terms in column 6.

What do these results imply about the importance of outreach for reintegrating released felons into the political process? If we focus on the proportion of our experimental sample that was eligible to vote in the November 2008 election (of age and out of prison), we can examine their pre- and post-incarceration participation to understand the practical significance of our treatment estimates. In the control group for this subset of the experimental population, 22.8% were registered in 2008 and turnout in that election was 5.0%. For these same individuals, 2012 registration was 6.0% and turnout was 3.3%, which means post-incarceration registration was reduced by 16.8 percentage points and turnout by 1.7 points.¹⁵ The estimates discussed above are 1.8 points for registration and .9 points for voting. This means that receiving a single letter from the Secretary of State is predicted to increase registration to 7.8% ($6.0 + 1.8$) and turnout to 4.2% ($3.3 + 0.9$). Thus, a single letter can boost post-incarceration registration from 26% to 34% of its pre-incarceration levels ($7.8/22.8$) and turnout from 66% to 84% of its pre-incarceration levels ($4.2/5.0$).

Which felons are mobilized?

In Tables 2 and 3 (column [5]), we find that the subset of felons who voted in 2008 (prior to being incarcerated) were substantially more likely to be registered and to have voted in 2012 than those who had not voted in 2008 or were ineligible to do so. This raises the question of which felons are most affected by our intervention. Does outreach generate new registrants and voters, or simply reengage those who were already voting prior to going to prison? To answer this question, in column (6) of each table we present estimates that interact the treatment variable with prior voting behavior (3.4% of the sample voted in 2008, 63.9% was eligible but did not

¹⁵ Overall Connecticut registration as a proportion of the voting age population decreased by 2.4 points between these two elections and turnout by 4.7 points, so the experimental population experiences a larger decrease in registration than the overall population but a smaller decrease in absolute turnout (but a drop that is larger in proportional terms).

vote, and the remaining 32.7% was ineligible in 2008).

For registration and voting, the estimated effects are largest for those who had previously voted. Focusing on registration (Table 2, column [6]), the point estimate for the interaction between treatment and voting in 2008 is 8.4 percentage points ($p < .05$, one-tailed), which is larger than either the 1.9 point effect ($p < .01$, one-tailed) for the interaction between the treatment and having been eligible but not voting in 2008 or the .9 point effect ($p = .17$, one-tailed) for the interaction between treatment and having been ineligible in 2008.¹⁶ These figures are indistinguishable from one another, however.

In the case of voting (Table 3, column [6]), the magnitudes of the estimates are ordered in the same way and, again, the interactions between treatment and either voting in 2008 or having been eligible but not voting both achieve conventional levels of statistical significance (at $p < .05$ and $p < .1$, one-tailed, respectively). For those who voted in 2008, we estimate that having been sent a treatment letter is associated with a 7.7 point increase in the probability of voting, which is about ten times larger than the estimate for the two other groups. This estimate is also distinguishable from the treatment effect estimates for the other two groups (at $p < .10$, two-tailed).¹⁷ Among those who voted in 2008, about 10% of the control group voted in 2012. For those sent a treatment letter, however, turnout was about 18%, or fully 64% higher than in the control group.¹⁸

¹⁶ Considering the family of hypotheses that each of the subgroups defined by the interaction terms in column (6) of Table 2 are significantly different than the control group, the Bonferroni-corrected p-value threshold for tests using a 10% alpha level is 0.033. Using the Bonferroni correction, we find that only the interaction between treatment and having been eligible but not voting in 2008 is statistically distinguishable from the control group at $p < .1$ using a one-tailed test.

¹⁷ We use a two-tailed test for comparisons among treatments, and one-tailed tests for comparisons to the control group.

¹⁸ Considering the family of hypotheses that each of the subgroups defined by the interaction terms in column (6) of Table 3 are significantly different than the control group, again the Bonferroni-corrected p-value threshold for tests using a 10% alpha level is 0.033. Using the Bonferroni correction, we find that only the interaction between treatment and voting in 2008 is statistically distinguishable from the control group at $p < .1$ using a one-tailed test.

In sum, these results are consistent with the notion that only a portion of the felon population is interested and easily mobilized to become involved in politics in this manner. Those who have previously voted are most responsive to outreach efforts. Holding fixed this interest, these results lend credence to the notion that the simple act of becoming unregistered because one is incarcerated, coupled with potential confusion about eligibility to vote, contributes to low rates of participation by released felons. With simple outreach reminding felons they are eligible to vote, formerly voting felons are much more likely to return to voting. These results are also highly encouraging, in that even this single and relatively bland intervention can substantially reduce the apparent negative effects of incarceration on political participation. Thus, despite all of the negative consequences of having been incarcerated, (some) felons appear responsive to outreach designed to (re)engage them into politics.

Adjusting Treatment Estimates for Returned Mail

One striking feature of our experiment is that large proportions of our treatment letters were returned as undeliverable. In both of the treatment conditions, 40% of the letters were returned. (Of course, the proportion that failed to reach their intended recipient was likely larger.) Given that those who cannot receive the mailing cannot be affected by it, the estimates of treatment effects presented so far, which are intent-to-treat (ITT) effects, are likely substantially lower than if we had been able to contact the entire treatment group. In cases where it is not possible to treat all subjects assigned to the treatment group, the effect of the treatment on the subset of the treatment group who can be treated, the compliers, can be estimated using quantities produced by the randomized intervention. In this context, we define compliance as

Further, considering the family of hypotheses that each of these three same subgroups are statistically distinguishable from one another, the Bonferroni-corrected p-value threshold is the same. Using this threshold, we find that none of the differences in turnout between subgroups is statistically significant at $p < .1$ using a two-tailed test.

being a member of the treatment group whose letter was not returned as undeliverable.

We can therefore estimate the complier average causal effect (CACE) by dividing the (ITT) estimates in Tables 2 and 3 by the contact (compliance) rate, which is .6 (see Gerber and Green 2012). This implies that receiving a single letter about voting eligibility increased registration by about 3 points ($1.8/.6$) and voting by 1.5 points. Table 4 presents a more formal estimation of the CACE using two-stages least squares regression, where we instrument for non-returned mail with assignment to the treatment group. We find that compliers' registration increased by 3.0 points (column (1), $p < .01$, one-tailed) and turnout by 1.5 points (column [4], $p < .05$, one-tailed).

We can also use this approach to estimate heterogeneous effects by past participation. Among the subset of the compliers who voted in the 2008 election, we find that registration increased by 11.6 points (columns [2], $p < .01$, and [3], $p < .05$, one-tailed) and turnout by 10.6 points (columns [5], $p < .001$, and [6], $p < .05$, one-tailed). The precision of these CACE estimates is affected by whether we implement robust clustered standard errors. The models in columns (3) and (6) show that the robust standard errors are substantially larger than the corresponding non-clustered estimates (columns [2] and [5], respectively). For 2008 non-voters, we estimate the CACE on registration of 3.1 points and 1.1 points on voting, while for those who were ineligible in 2008 the corresponding estimates are 1.6 and 1.1 points. Given the sensitivity of these estimates to clustering, it is not surprising that the larger CACE estimates for 2008 voters are distinguishable from the estimates for the other groups at $p < .05$ only when standard errors are not clustered. (The bottom rows of Table 4 report tests of the equality of CACE estimates across prior participation categories.)

Table 4: Complier Average Causal Effects (Adjusting for Returned Mail) of Outreach on 2012 Participation

	2012 Registration (Yes = 1)			2012 Voting (Yes = 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
SoS Letters Combined * Mail Not Returned	0.030 [0.010]***			0.015 [0.008]**		
Log Days Served	0.001 [0.003]	0.001 [0.003]	0.001 [0.003]	0.003 [0.002]	0.003 [0.002]	0.003 [0.002]
Age on Election Day (years)	-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.002]	-0.001 [0.002]
Age2	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
Time Since Release (years)	-0.026 [0.019]	-0.027 [0.018]	-0.027 [0.019]	-0.009 [0.014]	-0.010 [0.013]	-0.010 [0.014]
Time Since Release2	0.004 [0.005]	0.004 [0.005]	0.004 [0.005]	0.001 [0.004]	0.001 [0.004]	0.001 [0.004]
Eligible and Voted, 2008	0.111 [0.027]***	0.075 [0.026]***	0.075 [0.036]**	0.109 [0.024]***	0.074 [0.019]***	0.074 [0.031]**
Eligible and Did Not Vote, 2008	-0.007 [0.009]	-0.011 [0.012]	-0.011 [0.011]	0.000 [0.006]	0.000 [0.008]	0.000 [0.008]
SoS Letters Combined * Mail Not Returned * Voted 2008		0.116 [0.047]**	0.116 [0.072]*		0.106 [0.034]***	0.106 [0.064]**
SoS Letters Combined * Mail Not Returned * Did Not Vote (but Eligible) 2008		0.031 [0.013]**	0.031 [0.013]**		0.011 [0.009]	0.011 [0.009]
SoS Letters Combined * Mail Not Returned * Did Not Vote (and Not Eligible) 2008		0.016 [0.020]	0.016 [0.019]		0.011 [0.014]	0.011 [0.013]
Constant	0.076 [0.053]	0.079 [0.044]*	0.082 [0.053]	0.024 [0.036]	0.030 [0.032]	0.026 [0.036]
Observations	6,280	6,280	6,280	6,280	6,280	6,280
Number of Fixed Effects Units (Crimes)	37	37	37	37	37	37
P-value, test treatment 08 voters = treatment 08 non-voters		0.081	0.242		0.007	0.143
P-value, test treatment 08 voters = treatment 08 not eligible		0.047	0.173		0.010	0.147
P-value, test treatment 08 non-voters = treatment 08 not eligible		0.504	0.484		0.998	0.998

Note: Cell entries are second stage estimates from a two-stage least squares model, instrumenting for SoS Letters Combined * Mail Not Returned in columns (1) and (4) and for the interaction of that variable with past participation in the remaining columns. All variables shown are used as instruments, with treatment assignment the exogenous regressor. Fixed effects for crime are included in all columns. *p<.1, **p<.05, ***p<.01; one-tailed tests for SoS Letters Combined in columns (1) and (4) and for all SoS Letters Combined interaction terms in remaining columns. Robust standard errors in columns (1), (3), (4), and (6).

Discussion and Conclusion

The reach of the American criminal justice system has expanded dramatically in the last three decades, raising important questions about the effects of mass incarceration on patterns of political participation. Although a great deal of research has focused on the effect of post-incarceration felon disenfranchisement laws, relatively little research has examined the effect of conviction and incarceration on subsequent patterns of political engagement for the vast majority of released prisoners (those who are eligible to vote after release from state supervision). To understand whether participation by released felons can be increased, we designed and implemented a field experiment in which we randomly informed some released felons of their right to vote.

We find that even a single piece of mail reminding felons that they can now register is associated with a substantial increase in registration and can recover a large proportion of the 4 percentage point difference between pre- and post-incarceration voting rates. Most directly, this means that efforts to undo the participation effects of incarceration can be successful. Whatever the participatory consequences of incarceration, they are not in large part impossible to overcome.

Whether it is normatively desirable to increase ex-felons' political participation is, of course, a different question. In the 19th Century, felon disenfranchisement was seen as necessary to protect the integrity of elections (Keyssar 2000). In recent years, state criminal disenfranchisement policies have been quite fluid. Florida alone has substantially changed its restoration procedure three times in the last ten years. While many states have enacted policies that have made it easier for felons to restore their voting rights, other states have made it more difficult for released felons to vote (Porter 2010). For example, a number of states have enacted policies making the restoration of voting rights conditional on the payment of obligations like

court costs and restitution (Cammett 2012). Local officials in some jurisdictions have also been aggressive in prosecuting ineligible felons for voting (e.g., Foley 2014; Marley 2014), a move that may chill participation even in states where felons are eligible. Overall, this inconsistent pattern of reform shows that we are far from a normative consensus about whether ex-felons should be allowed to vote.

In light of this disagreement about whether felons should be eligible to vote, it is not surprising that there is conflict in state legislatures over policies that would mandate that the government notify discharged felons of their right to vote. Some argue that it is the individual ex-felon's responsibility to reintegrate into the political system. For example, in a statement accompanying his veto of a notification bill in Minnesota, then-Governor Tim Pawlenty wrote in 2009 that "...citizens should bear some responsibility for being informed about their own situation and rights."¹⁹ Others argue, however, that when the state takes away a citizen's voting rights, it has a responsibility to help ex-felons reintegrate into the political system when those rights are restored. This article provides the first piece of experimental evidence that policy-makers can use to help judge the efficacy of these sorts of notification efforts.

There are several areas in which researchers can build on these findings. Our experiment tests only the effect of a single letter sent by the Secretary of State for a particular subset of the felon population in Connecticut. Are felons in other states or those serving sentences for more serious crimes more or less amenable to being induced to participate? Among all U.S. states, Connecticut's rate of incarceration is middling (it ranked 25th out of 50 in 2008; Sabol et al. 2009). Assuming that rates of incarceration reflect differences in punishment rather than underlying differences in crime rates, the share of less serious offenders in the incarcerated population is likely increasing in the share of a state's population that is incarcerated. If outreach

¹⁹ Minnesota *Journal of the Senate, Permanent Journals of the 2009 Regular Session* (6736).

is more effective for less hardened offenders, then similar programs would likely have larger marginal effects in states where incarceration is more frequent and smaller effects in states where it is rarer. A similar logic suggests that targeting more serious offenders (those who had served longer prison sentences or committed more serious crimes) would be less effective. Of course, while these are plausible suppositions, only more expansive field testing can resolve the uncertainties.

More narrowly, a similar set of questions concerns the details of program implementation: Would more aggressive and sustained outreach, or outreach taking place at a different time (e.g., at the time of release), have a greater impact? Would face-to-face efforts, perhaps implemented through interactions that would take place at the same time as discharge from parole, prove more successful? These alternative outreach efforts might be particularly promising given the residential instability of this group, which makes contact by mail difficult.

As we note in the introduction, these results also have implications for efforts to assess the political consequences of post-incarceration felon disenfranchisement laws. When comparing participation by released felons in states that do and do not disenfranchise them, turnout could be much higher in states without such laws if outreach was expanded in the manner used in our field experiment. If more eligible felons voted in states that did not bar them from doing so, legal prohibitions on felon voting would have much clearer and comparatively larger effects.

A more general implication of our work is that it suggests another area in which beliefs may be an important determinant of participation (Gerber et al. 2013a, 2013b). Our work is motivated, in part, by prior research showing that released felons believe they are not allowed to vote (Drucker and Barreras 2005), which may explain their low rates of voting. Our experimental intervention attempts to address this misperception and is associated with an increase in

turnout. However, we have not measured directly whether our interventions increased ex-felons' awareness of their voting rights, an important next step for future research.²⁰ Similarly, future research can explore interventions designed to correct misperceptions about eligibility as well as other beliefs (e.g., a fear of being identified as an ex-felon to your neighbors) that might inhibit participation despite simple outreach efforts. Doing so will allow researchers to understand the degree to which low rates of participation reflect beliefs or simply a lack of systematic outreach.

Another implication of our research concerns efforts to understand the link between participation and recidivism. Other scholars have argued that the low political engagement of released felons may increase their risk of subsequent criminal behavior because individuals disconnected from civic life may see less to lose in violating social norms about appropriate behavior or view the state as less legitimate (Uggen and Manza 2004). Our experiment presents a design that can be used to exogenously increase the participation of some felons, which would allow for a test of the causal effect of political engagement on recidivism. To do so, however, one needs to undertake the design we present on a much larger scale.

Returning to our core findings, our experiment shows that efforts to reach out to felons can mitigate this disruption, addressing an important political consequence of felony conviction. Thus, in seeking to reduce the collateral consequences of incarceration for political participation, an overlooked but effective step is likely to be efforts to reach out to felons and encourage them to exercise a right that, in many places, they already hold. However, we need to be careful to not overstate the effect of receiving these mailings. Even those who received the mailing were substantially less likely to vote than the average person eligible to vote in Connecticut in 2012. Thus, a lack of outreach is likely only one item among many reasons that ex-felons vote at

²⁰ In addition, with a follow up survey, one could examine changes in other relevant political beliefs (e.g., alienation, efficacy, attitudes toward the state, etc.)

dramatically lower rates than the general population. While this intervention appears to reduce that gap, it does not eliminate it.

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