

# Registering Returning Citizens to Vote: A Field Experiment in North Carolina\*

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## Abstract

Millions of people in the United States are eligible to vote despite past felony convictions, but their voter participation rates are extraordinarily low. In this study, we report the results of a randomized controlled trial (RCT) of mail-based interventions aimed at encouraging people with a felony conviction to register to vote in North Carolina. We use a novel approach to identify and contact this population, using a combination of administrative data and data from a commercial vendor. In a field experiment conducted in the fall of 2020, we find that, on average, our mailers increased voter registration by 0.8 percentage points (12%), and voter turnout in the general election by 0.5 percentage points (11%). By contrast, the treatment has no effect on a comparison group of people without known felony convictions who live in the same neighborhoods. We find suggestive evidence that treatment effects vary across demographic groups and with the content of mailers. Overall, we demonstrate that it is possible to identify, contact, and mobilize a marginalized group that is not effectively targeted by existing outreach efforts. Our results show how organizations can increase voter registration and turnout among people with past criminal records, without necessarily changing laws to broaden eligibility.

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# Introduction

Millions of people in the United States with felony convictions are currently eligible to vote (Manza and Uggen, 2008). Only eleven states permanently disenfranchise certain people with felony convictions, most states do not permanently bar anyone from voting, and since 2016 seventeen states and the District of Columbia have expanded access to the franchise for people involved in the criminal legal system (*Felon voting rights*, 2021; Uggen et al., 2022). North Carolina is one of them, having restored voting rights for people serving time on probation and parole in the fall of 2022.<sup>1</sup> Researchers estimate that between the 2016 and 2020 general elections, over a million people gained the right to vote (Uggen et al., 2020). Yet, formally regaining eligibility does not mean that one will turn out, and researchers estimate that less than 30% of people with a conviction are registered voters – compared to 73% of eligible Americans who were registered as of the November 2020 election (Current Population Survey, 2021; Gerber et al., 2015; Burch, 2011). Given the wave of franchise expansions, we ask: can traditional voter mobilization tools be leveraged to identify, contact and mobilize people with felony convictions who are eligible to vote?

This question is deceptively hard to study. Since there is no universal list of unregistered people, most of what we know about voter mobilization comes from studies that begin with lists of registered voters who scholars then try to turn out through various means (Mann and Bryant, 2020; Jackman and Spahn, 2021). This focus on registered people will miss many people with convictions, who are already much less likely to be registered compared to the overall population. People with felony convictions may also be uniquely unlikely to participate in electoral politics, where confusion around eligibility, cumulative disadvantage that precedes and follows carceral contact, and declining trust in the government all contribute to low levels of electoral engagement among justice-impacted people (Meredith and Morse, 2015; White, 2019; Pettit and Western, 2004). Crucially, for all of these rea-

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<sup>1</sup>This study took place prior to the restoration of rights to people serving time on probation and parole, and thus was restricted to those people had completed the terms of their sentence.

sons, justice-impacted people are almost never the target of traditional “get-out-the-vote” (GOTV) outreach efforts (Owens and Walker, 2018). Institutional neglect likely compounds chronic non-voting among this group. Accurate samples of returning citizens are hard to construct, further deterring scholars and advocates from pursuing mobilization efforts among justice-impacted people (Gerber et al., 2015; Burch, 2011). We therefore know very little about how to identify and reach unregistered people with felony convictions who retain the right to vote, how to characterize the differences between registered and unregistered people, and whether traditional methods of voter mobilization—mailers, phone calls, or in-person efforts—are effective.

In this study, we develop knowledge around how to identify and reach people with convictions who are eligible voters, and whether baseline electoral engagement among this group can be improved through traditional outreach strategies. We identify individuals with felony convictions who are eligible but unregistered by bringing together administrative records on sentencing and conviction and voter registration files. We then work with a commercial data vendor to obtain contact information for eligible but unregistered people with convictions. This constitutes our list of individuals to target for outreach efforts. After several pilot studies refining our approach, evaluating various modes of contact, we developed a mail-based intervention informing people of their right to vote and how to register that we evaluate against an uncontacted control group in an experimental setting. Ultimately, we are able to find and contact approximately one in three eligible North Carolinians with a past felony conviction, and we see substantively large effects of these mailers on registration and turnout. On average, the mailers sent as part of the main experiment in fall 2020 increased voter registration by 0.8 percentage points (12%,  $p < 0.05$ ), and voter turnout by 0.5 percentage points (11%,  $p < 0.10$ ).

Although we tested variations in message wording, we find no differences of note across treatment arms, leading us to conclude that the simple provision of information about eligibility to register and the means to do so is sufficient to increase baseline engagement among

justice-impacted people. We find no differences by party of registration that follow from our mailers. We find suggestive evidence that some treatments may be less effective at mobilizing Black individuals, relative to their white counterparts. Supplemental tests suggest that this pattern is not due to differences in database address quality, but some other unobserved factor. Finally, a parallel study targeted to similarly situated individuals who do not have a felony conviction had no impact on turnout among the non-conviction group, suggesting that efforts targeted to justice-impacted people hold unique promise for electoral expansion.

One major contribution of this project is methodological. We develop a process for reaching difficult-to-contact populations, and we contact a much broader sample of people with convictions (an estimated 30%) than the most relevant previous registration study focused on this population (an estimated 5%, [Gerber et al., 2015](#)).<sup>2</sup> Further, we develop this list without working in collaboration with a state agency (as scholars have done in the past), important for a population whose experiences with the state lead to declining trust in government ([Lerman and Weaver, 2014](#)). Moreover, the various benchmarks we provide suggest that barriers to targeting unregistered citizens for mobilization can be overcome. Since returning citizens are often on the margins of a variety of systems and institutions (employment, education, social services)—in part because of their records—it has been unclear how to identify them for outreach. Even the data we use from commercial vendors was of previously-unknown quality for this group. We show that it is possible to find and contact a meaningful share of these individuals, and that the resulting samples are similar to the underlying population.

Substantively, this project contributes to a small but growing body of work suggesting that returning citizens are not lost to the polity. Instead, they are a latent political force. Simply reaching out, providing information about eligibility and inviting participation can improve the baseline registration of a group scholars have largely regarded as lacking in civic capacity. Many formerly-incarcerated people have spoken about the personal significance of reclaiming citizenship rights ([Owens, 2014](#)). Researchers have further pointed out that

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<sup>2</sup>The details of these coverage estimates appear in the “Sample Construction” section of the Supporting Information (SI).

carceral contact itself means that custodial citizens are policy stakeholders across a number of issue areas (Owens and Walker, 2018). There are democratic and practical benefits to re-integrating people into political life after criminal convictions, and this study contributes to our understanding of how to do so.

## Background

The majority of people with past felony convictions regain their right to vote upon completion of all or part of their sentence. Despite fairly widespread rights restoration, scholars have documented very low rates of registration and voting, even among those who are eligible (Lerman and Weaver, 2014; Gerber et al., 2015; White, 2019; White and Nguyen, forthcoming). Some work suggests that people with felony convictions face barriers to participation that will be hard to overcome. Many did not participate before their conviction, and continue to have low rates of registration and voting afterward (Burch, 2013; Lerman and Weaver, 2014; White, 2019). Burch (2011) estimates that in North Carolina – the state we focus on in this study – 36% of residents with felony convictions were registered to vote in 2008, and 24% turned out in that election.<sup>3</sup>

People with felony convictions do not exhibit high levels of electoral participation, and as such they are often overlooked by the kinds of organizations central to mobilization efforts during elections: campaigns, political parties, and related interest groups. These groups are incentivized to spend their limited resources ensuring that people who are likely to vote do in fact cast a ballot. Most often, this means targeting people who are already registered and have a demonstrated track record of voting. Organizations with the strongest incentives to turn people out on election day may therefore neglect individuals who are eligible but who exist at society’s margins and are chronically absent from the electorate, including people with felony convictions, further inhibiting their full incorporation into the polity (Owens,

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<sup>3</sup>By comparison, 70% of the full adult citizen population in North Carolina was registered to vote as of the November 2020 election (Current Population Survey, 2021).

2014).

There are many reasons that people with felony convictions are unlikely to vote. Custodial citizens often contend with multiple sources of material disadvantage that predate criminal justice contact, and which criminal legal involvement exacerbates (Lerman and Weaver, 2014; Gerber et al., 2017). Contact with the criminal justice system can erode attitudes important to participation, like trust in government and a belief in the value of one's political voice (Lerman and Weaver, 2014; Weaver and Lerman, 2010; Weaver, Prowse and Piston, 2020). Thus, returning citizens are unlikely to vote, which renders them undesirable targets for institutions traditionally interested in mobilizing individuals to the polls. This institutional neglect compounds chronic non-voting in this group.

There is some evidence, however, that returning citizens are not only not lost to political life, but that contact itself may render them newly compelled to civically engage. Owens (2014) highlights that a felony conviction can make individuals stakeholders in new policy areas, since they may lose access to important social welfare goods as a consequence of a conviction. Walker (2020) argues that contact can lead individuals to develop a politicized identity, and when they view their experiences as unjust or inhumane, individuals may be compelled to protest and engage in community organizing. In terms of voting, Laniyonu (2019) notes that when issues related to criminal justice are on the ballot individuals in highly policed communities turn out at higher rates, and Ang and Tebes (2021) find that exposure to apparently-unjust police violence in one's neighborhood also promotes turnout. It is unclear from this work whether people with convictions themselves participate – it may be that the loved ones of justice-impacted people vote at higher rates. These findings are nevertheless encouraging, especially alongside Meredith and Morse (2015), who found that informing returning citizens that they were eligible to vote after the law changed in Iowa improved their overall participation. Thus, a small collection of work suggests that many justice-impacted people are ripe for mobilization, and other kinds of barriers, including neglect by organizations traditionally engaged in voter mobilization efforts, prevent them

from becoming active members of the electorate.

Researchers recognize, moreover, that requirements that one register in order to vote place an additional burden on voters, and that those who have overcome this precondition are also more likely to turn out.<sup>4</sup> A small collection of studies have made inroads into understanding how to mobilize other groups of marginalized people – highlighting, for example, the value of delivering messages in Spanish when targeting Latinx people (Michelson, 2006; Bedolla and Michelson, 2012), and of working with community-based organizations (Sinclair, McConnell and Michelson, 2013; Kammerer and Michelson, 2022; Grumbach, Han and Warren, 2022). Yet, despite the recognition that getting registered is a major step often out of reach for marginalized people, most research around voter mobilization still focuses on convincing already-registered voters to turn up at the polls (Mann and Bryant, 2020). Evaluating how to encourage registration is more difficult, because there is no universal database of unregistered people (Mann and Bryant, 2020). Researchers interested in voter registration must, instead, rely on other readily-available lists of individuals, often targeting known groups, like college students, or engage in a more general, neighborhood-focused door-knocking approach (Bennion and Nickerson, 2016; Mann and Bryant, 2020; Nickerson, 2015). Such approaches are challenging to apply to people with criminal legal contact, who may be less likely to have stable addresses, be listed on utility bills or issued credit cards, or appear in consumer or other commercial datasets. Returning citizens are therefore a hard-to-reach population, and little is known about how to effectively encourage their civic engagement when they are not already registered.

Only one study of which we are aware examines the responsiveness of formerly incarcerated people to messages encouraging them to register and vote. Gerber et al. (2015) find that a basic mailer targeted to recently-released people improved turnout by 1.8 percentage points relative to the control group, suggesting that people can be re-incorporated into polit-

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<sup>4</sup>One of the only electoral reforms researchers have identified that effectively enhances turnout among low propensity voters after the passage of the Voting Rights Act is same-day registration (Grumbach and Hill, 2022).

ical life if they can be found and asked. This study, however, focused on a narrow subgroup of recently-released, non-violent offenders who served relatively short stints in prison. The researchers partnered with the state government, which provided release-address information for those included in the study. In short, they focused on an easy-to-identify, easy-to-contact and potentially easy-to-mobilize group that is not necessarily representative of the larger population of people with felony convictions who are eligible to vote. We estimate that [Gerber et al. \(2015\)](#) reached about 5% of the total population of individuals with convictions who are also eligible voters.<sup>5</sup> This work is groundbreaking insofar as it demonstrates that simply asking custodial citizens to participate can boost their engagement. Yet, [Gerber et al. \(2015\)](#) use a strategy that: 1) cannot be applied to identify and contact the millions of people who completed their sentences years or decades ago and thus do not have a current address on file with correctional agencies; 2) may not work outside of Connecticut, in places where state agencies may be reticent to cooperate with researchers for this purpose; and 3) may be less effective than strategies that do not work with state institutions, since increased fear and skepticism towards the state is often a direct consequence of contact with the system for returning citizens ([Lerman and Weaver, 2014](#)). Finally, the restricted and relatively small sample precludes analysis of the intervention’s effectiveness among population subgroups.

Considering the broad impact of the criminal legal system and the variety of effective interventions identified in the voter mobilization literature, we know relatively little about the baseline capacity for mobilization among justice-involved individuals and how it compares to other marginalized people without convictions ([Uggen, Manza and Thompson, 2006](#)). On one hand, criminal legal entanglement and its consequences are associated with many barriers to voting. On the other hand, carceral contact itself creates policy stakeholders, and some research suggests that individuals can be compelled to participate under the right conditions ([Owens, 2014](#)). Yet, data limitations and difficulty identifying unregistered voters has hindered the development of knowledge around how to effectively mobilize this group.

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<sup>5</sup>See SI Section [A.2](#) for the details of this calculation.



Serious questions remain around the capacity to develop a representative list of people with felony convictions to target for intervention.

We investigate whether voting-eligible people with criminal records can be mobilized to register to vote if given information about eligibility and the registration process. Our first-order task is to develop an effective method for building a list that is broadly representative of people with felony convictions who are eligible to vote, and to do so without working together with a state agency. We therefore take great care to evaluate the quality of our constructed sample, who we reach, and the eligible voters we successfully register and turn out. Our second-order task is to see whether we can increase registration and turnout in this group. Recognizing the attitudinal obstacles to participation people with felony convictions face, we partner with a well-known North Carolina nonprofit organization focused on voter mobilization. Acknowledging the material and informational barriers to voting that returning citizens face, we aim to reduce barriers to registration with the provision of information about eligibility and the means to register. We describe our data, interventions and analytic strategy in detail below.

## Experimental Design

### The List

This project focuses on North Carolina, using a combination of state administrative data and information from a commercial data vendor to identify unregistered, voting-eligible people with past criminal convictions. As of 2020 (when the intervention was fielded), people who had been convicted of felonies in North Carolina were temporarily ineligible to vote, but their eligibility was automatically restored after completion of their sentences (including probation or parole).<sup>6</sup> Individuals can register by mail, online, or in person. North Carolina

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<sup>6</sup>In 2022, North Carolina amended its law, expanding access to the franchise to people still serving time on probation and parole.

is an appropriate site for our study, because we can easily obtain both the criminal justice records necessary to identify many people with felony convictions who are eligible to vote and the voter registration files that allow us to identify eligible voters with convictions who are not already registered. In addition, North Carolina’s rules around access to the vote for people with convictions are fairly similar in severity to many other states, neither barring people indefinitely nor allowing them to vote from prison. Rules around registration are similarly middling, where individuals may register online, but access to the ballot box has recently been contested in the wake of *Shelby County v. Holder* (Herron and Smith, 2015; Shepherd et al., 2021). North Carolina is therefore a useful site for understanding how to build a representative list of people with convictions who are eligible to vote and establishing effective strategies for mobilizing returning citizens.

To build our list of voting-eligible individuals with felony convictions, we started with publicly available data from North Carolina’s Department of Public Safety (DPS) to identify people convicted of a felony and sentenced to DPS custody (incarceration or supervision) who had completed the terms of their sentence.<sup>7</sup> We also used the state’s publicly available voter file data to identify people who appeared to be registered to vote already and omitted them from the study sample. We then contracted with a commercial data vendor to find current mailing addresses for as many people from our list of eligible voters as possible.

In the interest of providing benchmarks for future research and validating the demographic composition of our final list, we provide a brief overview of the technical details of the data construction process (described in full in the SI). We removed the following groups of people from the sentencing records data: those who were still under supervision at the time of the study (9% of the sentencing records); those who did not clearly have a felony conviction and thus may never have lost the right to vote (64%); individuals who were deceased or over 70 years old (almost 3%); individuals who were non-citizens and those who appeared to be duplicate entries (roughly 1%).

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<sup>7</sup>North Carolina is one of the few states that make this kind of data publicly available. Scholars and advocates are likely to have to employ public records requests to obtain these data in other states.

Prior to launching this study, we conducted three pilot studies during 2020 to streamline our approach and develop a preliminary sense of the best way to contact justice-impacted people. Ultimately, we developed a list of over 70,000 people who we determined were eligible to vote and for whom we obtained address information. We estimate that this represents approximately one-third of all voting-eligible unregistered North Carolinians with a felony conviction (as of 2020). It also represents a dramatic improvement over the estimated 5% reached in previous work. We included 35,249 records in the main experiment, presented here, and the remainder were used in the pilot experiments.

The NC DPS data includes some covariates we use to benchmark the final sample of individuals for whom we found addresses against the overall list of individuals sentenced in North Carolina, including race, gender, age and time since release. We do not observe much difference in the demographic composition of the final mailable list compared to the full set of people we sought to find. Slightly less than half of the full list is Black, and this is true of the final list of people for whom we obtained addresses. About 76% of individuals sentenced overall are men, relative to 75% in our list of those with addresses. The mean age of the sample of those for whom we obtained addresses is slightly lower than in the sentencing records overall (42 compared to 50) but this is likely due to the omission of those who are deceased or over the age of 70. The removal of over-70 records likely also contributes to a notable difference in time since release. Individuals in the full list of sentencing records had been released for 17 years on average, relative to nine years among those for whom we obtained addresses. This pattern may also have to do with residential mobility and the potential that more recently released individuals may have served time on supervision, which tethered them to the state.

## **The Intervention**

Over the course of 2020, we ran three pilot studies (described in the SI) and one main study (described here). From the pilots we determined that we are able to obtain higher quality

address information than phone numbers for our sample, and that mailers are more effective than phone calls or text messages. Following from this, we designed a large field experiment to be run in fall 2020. As noted above, it is not clear from the literature how to effectively mobilize returning citizens into the electorate. A large body of research has investigated why individuals do not participate – including resource barriers to participation, lack of knowledge about eligibility, and alienation from the state – and the research on mobilization among this group simply indicates that sometimes people with criminal justice contact still do vote, especially when they view themselves as policy constituents (which can be triggered by negative experiences with enforcement policy). Following from these findings, we partnered with the organization *You Can Vote*, a well-known non-profit in North Carolina focused on voter mobilization.<sup>8</sup> We also crafted a mailer that provided information about eligibility and instructions on how to register. In order to test the premise that it is important to help people overcome logistical barriers to registration, we randomized whether individuals received a registration form and a pre-addressed, pre-stamped return envelope with their mailer.

Research on how to mobilize marginalized people suggests it is important to work with organizations known to members of the impacted group and to deliver information that speaks to the specific nature of one’s marginalized status. To the first point, we worked with *You Can Vote*. We also varied two other pieces: whether the informational mailer included introductory language explicitly focused on eligibility and felony convictions (several sentences relative to a brief note that one must be *off papers* to be eligible); and the inclusion of language connecting the current election to civil rights and related issues faced by returning citizens. In sum, we randomly assigned people to one of five treatment conditions:

1. a control group that was not contacted at all;
2. a group sent an informational letter about eligibility to vote signed by a local non-profit

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<sup>8</sup>In earlier pilots, we observed a slightly larger registration effect when mailers came from a non-profit rather than a generic project name, although the effect was not distinguishable from zero.

organization, along with a registration form with pre-addressed, postage-paid envelope (“basic mailer”);

3. a group sent the basic mailer package, without highlighted information about eligibility among people with felony convictions (“no criminal record framing”);<sup>9</sup>
4. a group sent the basic mailer, with no registration form or pre-addressed, postage-paid envelope included (“no registration form”); and
5. a group sent the basic mailer package, with additional messaging about how issues related to civil rights are on the ballot and the importance of voting (“extra civil rights framing”).<sup>10</sup>

All mailers were developed in conjunction with *You Can Vote*, and are shown in Section A.3 of the Supporting Information. We randomly assigned individuals in the sample across these five groups with equal probability. The SI presents descriptive statistics for this sample and tests illustrating covariate balance across treatment arms, as well as a discussion of ethical considerations we took into account in sample construction and treatment design.

All mailers were sent in fall 2020, landing in mailboxes in early October (before the state registration deadline). After the November 2020 presidential election, we used the state’s publicly-available voter file to observe voter registration and turnout among people in the sample. All analyses presented are of intent-to-treat effects based on assignment to each treatment arm, as we cannot observe who actually opened or read the mail we sent them.

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<sup>9</sup>Note that these mailers still included a list of eligibility criteria, including information relevant to those with criminal records. But they do not include an opening paragraph highlighting this information.

<sup>10</sup>The closing paragraph of the “extra civil rights framing” mailer reads as follows: “Criminal Justice and Civil Rights are on your ballot. Members of Congress and the state legislature decide what is a crime and how it should be punished. They make rules on how our courts, prisons, and jails are managed and how people should be treated when they are in custody. Judges decide who gets detained and for how long, and who goes to prison and for how long. Elected officials have an impact on how equal protection is enforced and are responsible for ensuring freedom of speech, assembly and religion, and specific rights including voting rights. Find out what’s on your ballot and *why your vote matters* at [url].”

## Results

Table 1 presents regression estimates of the experimental treatment effects. First, in Panel A, we consider the combined effect of any treatment, relative to the control group. Column 1 shows that sending any mailer increased registration by November 2020 by 0.8 percentage points (12%,  $p < 0.05$ ). Column 2 shows that including background covariates has no effect on this estimate. Columns 3 and 4 show the effect of any treatment on voter turnout, without and with covariates, respectively. We find that sending any mailer increased voter turnout by 0.5 percentage points (11%,  $p < 0.10$ ). This implies that a substantial fraction of people who were induced to register by our treatment mailers ultimately voted in the next election.

Table 1: Effects on Voter Registration and Turnout

	<i>Dependent variable:</i>			
	Voter Registration		Voted November 2020	
	(1)	(2)	(3)	(4)
<b>Panel A: All Arms Combined</b>				
Any Treatment	0.008** (0.003)	0.008** (0.003)	0.005* (0.003)	0.005* (0.003)
<b>Panel B: Separate Treatment Arms</b>				
Basic mailer	0.008* (0.004)	0.008* (0.004)	0.008** (0.004)	0.007** (0.004)
No crim. record framing	0.011** (0.004)	0.011** (0.004)	0.007* (0.004)	0.007* (0.004)
No registration form	0.008* (0.004)	0.007* (0.004)	0.003 (0.004)	0.003 (0.004)
Extra civil rights framing	0.006 (0.004)	0.006 (0.004)	0.004 (0.004)	0.004 (0.004)
Control Mean	0.066	0.066	0.046	0.046
Covariates		X		X
Observations	35,245	35,245	35,245	35,245

*Notes:* This table shows the effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter registration by November 2020 and subsequent turnout. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

Panel B of Table 1 presents these results separately by treatment arm. We focus on Columns 1 and 3 (estimates without covariate controls), but estimates with controls (Columns 2 and 4) are nearly identical. Column 1 shows the effect on voter registration. The basic mailer increased voter registration by 0.8 percentage points (12%,  $p < 0.10$ ). The mailer with no criminal record framing increased registration by 1.1 percentage points (17%,  $p < 0.01$ ). The mailer with no registration form increased registration by 0.8 percentage points (12%,  $p < 0.10$ ). And the mailer with extra civil rights framing increased registration by 0.6 percentage points (9%, n.s.). We cannot statistically distinguish the effects of these different treatment arms from each other, partly because this study is only powered to detect relatively large differences across arms (on the order of a percentage point). However, the similarity across arms is also consistent with findings from [Green and Gerber \(2019\)](#), who observe that subtle differences in treatment wording rarely yield large voter-mobilization effects. Even the treatment condition that omitted the paper registration form did not produce a substantially-smaller treatment effect, a pattern that we note could be due to North Carolina’s online voter registration platform; in states requiring paper forms, the inclusion of a form could be more important. Though none of the treatment arms are statistically distinguishable, the point estimates suggest that a simply-worded informational mailer that does not lead with language about felony convictions can be as effective (and possibly more effective) than mailers that begin with a focus on past criminal legal contact.

Column 3 shows the mailers’ effects on voter turnout in the November 2020 general election. The basic mailer increased voter turnout by 0.8 percentage points (17%,  $p < 0.05$ ). The mailer with no criminal record framing increased voter turnout by 0.7 percentage points (15%,  $p < 0.10$ ). The mailer without a registration form increased turnout by 0.3 percentage points (7%, n.s.). The mailer with extra civil rights framing increased voter turnout by 0.4 percentage points (9%, n.s.). Again, we cannot reject the null hypothesis that these mailers all had equivalent effects.

Figure 1 visualizes the effects on voter registration, showing treatment effects (of receiving

any mailer relative to control) by week. In this figure, all treatment arms are pooled and compared with the control group. The dashed vertical line shows the week that mailers were scheduled to land in mailboxes. The coefficients are derived from an interaction between receiving any mailer and an indicator for calendar week, where the comparison week is the week before the treatment. The coefficients therefore reflect the difference between the pooled treatment and control in a given week, relative to the week prior to fielding the experiment. The weekly estimates illustrate that the impacts of these mailers are concentrated in the few weeks after mailers were delivered, as expected.

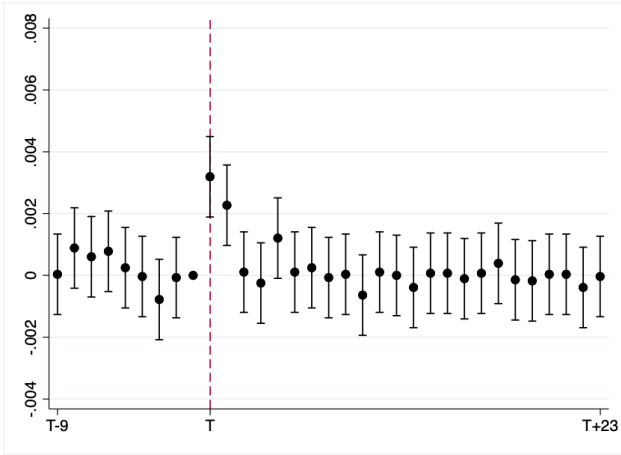


Figure 1: Treatment effects by week. This figure shows treatment effects of sending a mailer (all treatment arms combined), relative to the control group. The x-axis shows the week since mailers were scheduled to arrive in mailboxes. The y-axis shows effect on registering to vote.

On the whole, it appears that sending mailers with basic information about voter eligibility can substantially increase voter registration and turnout among this group of people with low propensity to register and vote. The size of these effects on registration and turnout may appear modest relative to the effects—1.8 and .9 percentage point increases in registration and turnout—of a previous intervention targeting those with felony convictions (Gerber et al., 2015). However, that study targeted a narrow and easy-to-find group of recently released non-violent offenders who had served minimal time (recall that we estimate they reached about 5% of voting-eligible former felons in Connecticut). By comparison, we



contacted a broad group of individuals with past felony convictions who are likely more residentially-mobile and contend with multiple forms of disadvantage. Until this study, it was unknown whether people in this group could even be identified and found (as noted, we estimate that we reached one third of this population in North Carolina). For these reasons, we are encouraged by the positive and significant findings we observe here, even as the size of the effect is comparatively modest.

Given ongoing debates about the partisan implications of expanding the electorate, we also consider party registration among those who registered to vote. Table A2 in the Supporting Information shows the number of people registering with each party. Overall, the distribution of party registrations appears similar for the treatment and control groups in each sample, with nearly equal proportions of Democratic and Republican registrants as well as many non-party registrants in both groups. That is, it appears that our intervention was not disproportionately effective among people inclined to vote for one party over another.

## **Effect Heterogeneity**

Given Black Americans' disproportionate exposure to the criminal legal system, we are curious about whether this intervention is equally effective for Black potential voters. Table 2 thus presents regression results showing how the experimental treatment effects vary across race. Columns 1 and 2 present the effects for Black and white individuals, separately. Column 3 combines these groups and interacts "Black" with the treatment indicator to formally test for differential effects by race. Panel A shows the effects of any treatment; Panel B shows effects separately by treatment arm.

The results suggest some differences in the effectiveness of the mailers across racial groups. We see consistently large and positive treatment effect estimates for white mailer recipients across the four different types of mailers. The point estimates among Black mailer recipients are smaller, not always positive, and never statistically distinguishable from zero. These differences are not simply due to a lack of statistical power or higher baseline rates of reg-

istration among Black mailer recipients: Column 1 has a similar number of observations to Column 2, and the control-group registration rates differ by less than one percentage point. In Panel A we see that being in any treatment group increased voter registration by 0.3 percentage points (5%, n.s.) for Black individuals vs. 1.3 percentage points (18%,  $p < 0.05$ ) for White recipients. This difference is not statistically distinguishable from zero, however.

Turning to Panel B: Our basic mailer increased voter registration for Black individuals by 0.7 percentage points (11%) vs. 0.8 percentage points (11%) for white individuals. The mailer with no criminal record framing increased registration by 0.2 percentage points (3%) for Black individuals vs. 1.8 percentage points (25%) for white individuals. The mailer with no registration form increased registration by 0.3 percentage points (5%) for Black individuals vs. 1.3 percentage points (18%) for White individuals. And the mailer with extra civil rights framing *reduced* registration by 0.1 percentage points (2%) for Black individuals vs. a 1.2 percentage point (17%) increase for White individuals. As shown in column 3, only for the “no criminal record framing” mailer is the cross-race difference in effects significant. We note that the basic mailer appears to have performed equally well across these groups; the apparent gap in effectiveness is driven by the other mailer types, which themselves are not statistically distinguishable from one another. Thus, we prescribe caution when interpreting these coefficients. The suggestive racial differences observed here raise the need for further research on how best to reach nonwhite justice-impacted people.

In addition to these differential effects by race, we test for heterogeneous effects using a machine-learning approach developed by Wager and Athey (2018).<sup>11</sup> This approach allows us to identify subgroups with larger treatment effects in a principled way that minimizes concerns about data mining. The full results of this analysis appear in Table A3 in the Supporting Information. Briefly, those in the highest treatment effect bin – where we see the biggest positive effects on voter registration – are, on average, more likely to be male and more likely to have a history of incarceration (as opposed to supervision). We do not observe

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<sup>11</sup>Both the explicit test for racial heterogeneity and this machine-learning-based approach were pre-registered.

Table 2: Study 4: Racial Heterogeneity

	<i>Dependent variable:</i>		
	Voter Registration		
	Black	White	Both
	(1)	(2)	(3)
<b>Panel A: All Arms Combined</b>			
Treatment (Any mailer)	0.003 (0.005)	0.013** (0.005)	0.013*** (0.005)
Black			-0.008 (0.006)
Treatment * Black			-0.010 (0.007)
<b>Panel B: Separate Treatment Arms</b>			
Basic mailer	0.008 (0.006)	0.008 (0.006)	0.008 (0.006)
No criminal record framing	0.002 (0.006)	0.018*** (0.006)	0.018*** (0.006)
No registration form	0.003 (0.006)	0.012* (0.006)	0.012** (0.006)
Extra civil rights framing	-0.001 (0.006)	0.012* (0.007)	0.012** (0.006)
Black			-0.008 (0.006)
Basic mailer * Black			-0.001 (0.009)
No criminal record framing * Black			-0.016* (0.009)
No registration form * Black			-0.009 (0.009)
Extra civil rights framing * Black			-0.013 (0.009)
Control Mean	0.063	0.071	0.067
Observations	15,279	17,660	32,939

*Notes:* This table shows the effect of the treatment (sending a mailer) on voter registration by race group. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

such clear patterns when it comes to age and time since release, although individuals in the highest treatment effect bin are less likely to be over the age of 55.

Finally, in order to learn about address quality and experimental noncompliance in the form of undelivered mailers, we ran a followup study to measure address quality. We sent a postcard that invited people to take a survey – although the primary purpose of the study was to assess whether postcards landed in individuals’ mailboxes and did so differentially by observable characteristics. This study is described in detail in the SI. Postcards were more likely to bounce when sent to younger people, and less likely to bounce among those who had been released for longer periods of time. This makes some sense – it may be harder to obtain addresses for people who have been out for long periods of time, but those for whom we do obtain an address are more residentially stable. Crucially, there are no differences in bounce rate by race and gender. It does not appear that racial differences in the effect of the mailer were driven by lower-quality address data for Black individuals relative to their white counterparts. More research is needed to understand how to find people who have been released for a significant amount of time, and to understand how to more effectively mobilize different racial groups into the electorate.

## **Comparison to People Without Felony Convictions**

A final substantive query raised by this project is whether there is added value to targeting justice-impacted people specifically for mobilization efforts. We have argued that two things are likely true of many returning citizens: 1) they are overlooked by other agents traditionally engaged in voter mobilization efforts ([Owens and Walker, 2018](#)); and 2) they can be as interested in politics as other individuals, and perhaps even more so given their experiences with the criminal legal system ([Owens, 2014](#); [Walker, 2020](#)). If either or both of these things are true, there should be added value to targeting them specifically for outreach. If not, we should not see much difference in the effectiveness of outreach between justice-impacted people and similarly situated people without criminal legal involvement.

In order to evaluate this argument, we constructed an approximate comparison group of individuals without felony convictions and fielded a parallel experiment in North Carolina during the 2020 election. We contracted with the same commercial data vendor who provided the addresses for the main study, this time to purchase a list of people from their address database who were not registered to vote and who did not appear in the sentencing records. We focused this list on the six North Carolina zipcodes with the largest numbers of people from our main study sample, to limit the possibility that differences between the two samples were due to socioeconomic differences or different mobilization efforts across neighborhoods. This process yielded a sample of 35,708 people. We randomly assigned people in this second, no-felony-conviction sample to either an uncontacted control group or a treatment group that would be sent a basic mailer without extra conviction framing (the same mailer as those in the “no criminal record framing” treatment arm of our main study). In other words, individuals in the treatment group were simply provided with information about eligibility and how to register, inclusive of a registration form and pre-addressed, pre-paid return envelope.

The results of this parallel experiment are shown in Table S8 in the SI. They are precise null effects that are statistically distinguishable from the treatment effects observed in our main study sample (restricting the main study to the control group and the group that received the same treatment). It appears that our experimental treatment was uniquely effective among justice-impacted people, compared even to people living in the same neighborhoods during the same election season. This pattern could be due to differences in existing mobilization efforts: if people in our main sample have been missed by other outreach efforts, the information we offer them about eligibility may have an especially large effect, whereas people without felony convictions who are unregistered may have already received this information about registration in the past and nevertheless chosen to remain unregistered. It is plausible that our outreach to unregistered people with past convictions is finding those who have not been contacted in the past, and is targeted to a group especially receptive to

mobilization efforts.<sup>12</sup>

## Conclusion

Millions of people with felony convictions are eligible to vote ([Manza and Uggen, 2008](#)). In fact, individuals with felony convictions regain their rights at some point in the vast majority of states – and this is true for more and more individuals every day, as states continue to pass expansive reforms around felony disenfranchisement. North Carolina, where we have focused this study, is a prototypical example: at the time of the study, individuals regained access to the vote upon the completion of their sentence, and in the fall of 2022 the state expanded voting rights to people serving time on probation and parole ([Uggen et al., 2022](#)). Yet, participation rates for people with felony convictions are low and traditional mobilization campaigns tend not to prioritize returning citizens, because they are hard to reach, may lack valid mailing addresses, and are largely understood to be unlikely to participate in the electorate ([Owens and Walker, 2018](#)).

At the same time, a nascent line of research suggests that, under the right circumstances, people with felony convictions can be politically mobilized. Though they remain low-propensity voters, this research largely finds that they can become mobilized into other, non-voting activities. Whether the political energy observed among justice-impacted people can transform into political power via the vote remains an outstanding question. In this project, we asked: can we identify returning citizens who are nevertheless eligible to vote, find them, contact them, and convert them into active, registered voters? With a combination

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<sup>12</sup>The Voter Participation Center, a large voter-registration organization that blanketed North Carolina with registration-focused mailers in 2020, compared their mailing lists to the list used for our main experiment and found that fewer than one-fourth of the people in our sample were included in their list of unregistered people to contact. This pattern suggests that many people in the study sample may not have received other eligibility information during the 2020 cycle. Voter mobilization organizations often target select groups of voters based on characteristics such as race or age, and many people included in this study do not fall into the categories often targeted. This disparity in outreach may explain the differential impact of our mailer by race for the same reason our mailer is most effective for people with felony convictions: when individuals are not contacted by organizations traditionally engaged in outreach efforts, a light touch mailer can be particularly effective at mobilizing them.

of administrative and private data we developed a method for identifying difficult-to-reach potential voters, and we show through randomized control trials that it is possible to increase registration and voting among this population.

One contribution of this project is the method by which we constructed the sample. Diverging from strategies employed in previous research which relied on state agencies, we leveraged publicly-available administrative data and voter files to identify members of the population of interest, their voting eligibility status, and whether they were already registered. A data vendor sourced valid mailing addresses from commercial data. In sum, we have found a way to construct a list that endeavors to be broadly representative, where past work was exclusive and narrow, raising questions around the generalizability of the findings to a more deeply marginalized group of individuals.

Methods previously employed, moreover, relied on a privileged relationship with the state – a relationship not necessarily available to the kinds of organizations interested in mobilizing justice-impacted people, and potentially undesirable to returning citizens themselves. We worked together with a non-state actor to field the intervention. We have offered, at every stage in the process, benchmarks for both the sample and the impact (or lack thereof) of the various interventions (including those fielded in the pilots). We have developed a process that could be replicated by non-state actors and a roadmap to effective interventions for those interested in the political welfare of justice-impacted people. In so doing, we have tried to reorient questions of mobilization among people with felony convictions around service to the marginalized, and we can imagine this approach being of use being useful for research on mobilizing other difficult-to-reach or under-mobilized populations.

The second contribution of this study is the experimental results, which show that a light-touch, mail-based intervention that provides information about how to register and the means to do so increases registration and turnout for people with past felony convictions. We find suggestive evidence that our treatment effects vary across demographic groups and with the content of our mailers. Perhaps learning about eligibility requirements and the registration

process is particularly costly for people with past criminal justice contact; reducing these costs (with a simple mailer, or in other ways) can thus be effective. Returning citizens are not lost to political life. Instead, they hold a stake in politics and many retain an interest in participating, and are thus ripe for mobilization.

The findings presented in this paper also suggest several areas for future research. While we test several variations in message wording, we do not find substantial differences across mailers. We conclude that the simple provision of information and resources to register is enough to improve registration among returning citizens. Perhaps a more tailored form of contact would further improve registration. We worked together with a known organization in North Carolina, *You Can Vote* – but research about mobilizing marginalized people emphasizes the importance of trusted messengers grounded in the communities targeted for mobilization. Future research might consider employing such a trusted messenger or working with institutions even more deeply embedded in heavily policed communities. With respect to mode of contact, our pilots suggest that phone and text message are not particularly effective, but other research suggests that members of marginalized groups may be more receptive to strategies that establish a personal connection through conversation – such as neighborhood-intensive door-knocking efforts. Finally, important questions remain about how to more effectively mobilize members of different racial groups.

This project, which establishes a means of identifying and contacting returning citizens, lays the logistical groundwork for future studies. We have shown that simply reaching out and asking individuals to participate and giving them the means to do so can improve the baseline electoral engagement of a group that scholars and campaigns have, otherwise, largely treated as lacking in civic capacity. We have shown that returning citizens can be effectively incorporated into the electorate.



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# Appendices

## Supplemental Information

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# A More Details on Main Experiment

## A.1 Descriptive Statistics and Balance

Table A1 below shows descriptive statistics and tests of covariate balance for the main study described in the paper. The first column shows the control group mean for each covariate at baseline, the next columns show differences between that control group mean and the mean for each treatment group, and the last column shows the p-value from a joint F-test testing that the group means are different.

## A.2 Sample Construction

This study sought to find and contact people in North Carolina who were eligible to vote after a past felony conviction, but who were not registered to vote. Available estimates put this group of people at approximately 167,000-216,000 in North Carolina.<sup>13</sup> Across the pilots and main experiment, we found a sample of 72,213 people for whom we could determine voting eligibility and send mail to a current mailing address, representing over one-third of the population we sought to target in NC.

This coverage is remarkably high given the difficulty of finding and contacting this population. A previous study that collaborated with Connecticut’s secretary of state’s office to send out official letters informing people of their eligibility to vote reached a much smaller share of the state’s eligible unregistered population.(Gerber et al., 2015) That study reached approximately one of every twenty eligible Connecticut voters with past felonies<sup>14</sup>, both

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<sup>13</sup>The full population of people with past felony convictions still living in North Carolina in 2010, who had completed their custodial sentences and supervision (and thus were eligible to vote) was about 295,000-335,000 [(Shannon et al., 2017)Online Appendix Table S9]. Burch estimated that 35.5% of eligible voters in North Carolina with past felony convictions were eligible to vote, implying a 64.5% non-registration rate.(Burch, 2011) Thus, we estimate that the target population in North Carolina is between approximately  $295,000 \cdot 0.645 = 167,055$  and  $335,000 \cdot 0.645 = 216,075$  people.

<sup>14</sup>The full population of people in Connecticut with felony convictions (including people who were ineligible to vote while in custody or on parole) was estimated to be 223,000-266,000 in 2010 (Shannon et al., 2017). The ineligible population in CT in 2010 was estimated at 22,215, putting the range of eligible voters with past felonies at 200,785-243,785 (Uggen, Shannon and Manza, 2012). Gerber et al estimate that 40.2 % of a sample of eligible voters with past felonies in Connecticut were registered, implying a 59.8% non-

because of government decisions to carve out people with specific conviction types as well as limitations of government address records: because that study relied on state address records, it was constrained to contacting recently-released people for whom those address records were up-to-date. Our approach allows us to find and contact people who may have been convicted years or even decades earlier, thus reaching a much broader swath of the relevant population. It also provides a roadmap to organizations that may seek to help people register in states where government is not interested in collaborating in registration efforts.

### **A.2.1 Building the List for Main Experiment**

As described in the main paper, we began with a database from NCDPS (North Carolina Department of Public Safety) of records for people who had been sentenced to DPS custody in North Carolina since the 1970’s. This 1.2-million-row file contained many duplicates, along with many people who were not in our target population either because they were not eligible voters or because they had not been convicted of a felony. Figure A1 illustrates the process of paring the DPS dataset down to a list of people who appeared to be eligible voters who should be included in our study. We omitted people who were still serving sentences or who were known to be deceased, as well as people without clear felony records (denoted on the figure as “felony status unclear”: some of this is due to unclear recordkeeping, but for most people this is because they only had misdemeanor convictions). We also omitted apparently duplicated records and those missing name information, as well as people over 70 (out of concern that many people would be deceased) and those whose records indicated they were not citizens. This left us with about a quarter-million people who we believed to be eligible voters in NC with past felony convictions; about half of this list had already been used in our pilot studies earlier in the year (this number is larger than our eventual pilot sample size because many people were already registered or did not match to addresses). Removing data that had been used for pilots left us with a list of 136,268 potential voters to look for.

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registration rate (Gerber et al., 2017). Thus, we estimate that the target population in Connecticut was between approximately 120,069 and 145,783.

We worked with a commercial data vendor to find addresses for as many people on this list as possible while omitting people who were potentially already registered to vote, measuring existing registration quite conservatively.<sup>15</sup> This process yielded a main experimental sample of 35,245 people in North Carolina.

The process of paring down the DPS records and finding address matches involved trimming down the dataset in multiple stages, and we were curious about how the resulting final sample compared to the original DPS records or the full list of eligible voters we sought to find and contact. Figure A2 below traces the racial and gender composition of the dataset at various stages of sample construction. They are remarkably stable: the composition of the final sample is not dramatically different from the original DPS list or the list of eligible voters for whom we sought addresses. There are several expected differences based on our design choices: the decision to omit people over 70 means that the age composition of our final sample is slightly younger than the original DPS dataset and also that the median length of time since release is slightly shorter in our sample than in the full DPS dataset. But on the whole, this approach appears to yield a surprisingly representative sample of the overall population, at least on visible characteristics.

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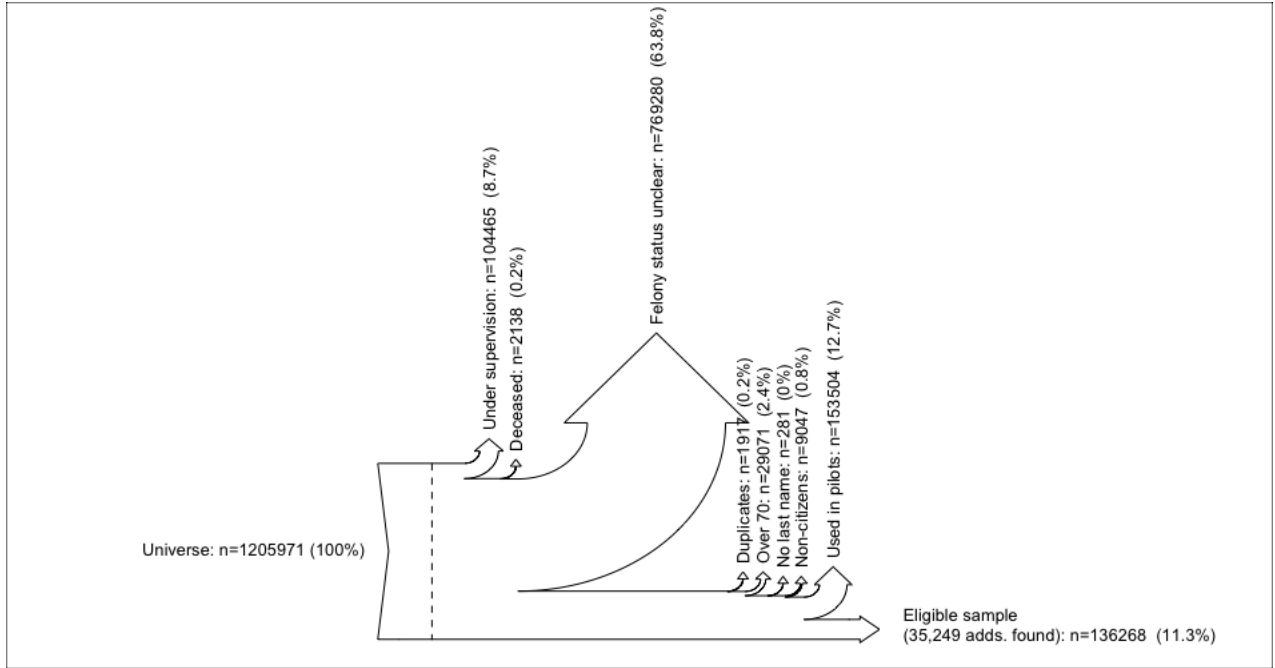
<sup>15</sup>Because the state’s publicly-available voter files only include voter year of birth rather than exact date of birth, it can be difficult to be sure whether an apparent voter-file match (say, ”John Smith” born in 1950) actually represents a voter registration for the person in question or a false positive. We erred on the side of being conservative and excluded from our study sample anyone for whom it appeared that they could possibly be registered to vote already, in order to avoid confusing people about their registration status.



Table A1: Main Study: Covariate balance across treatment arms

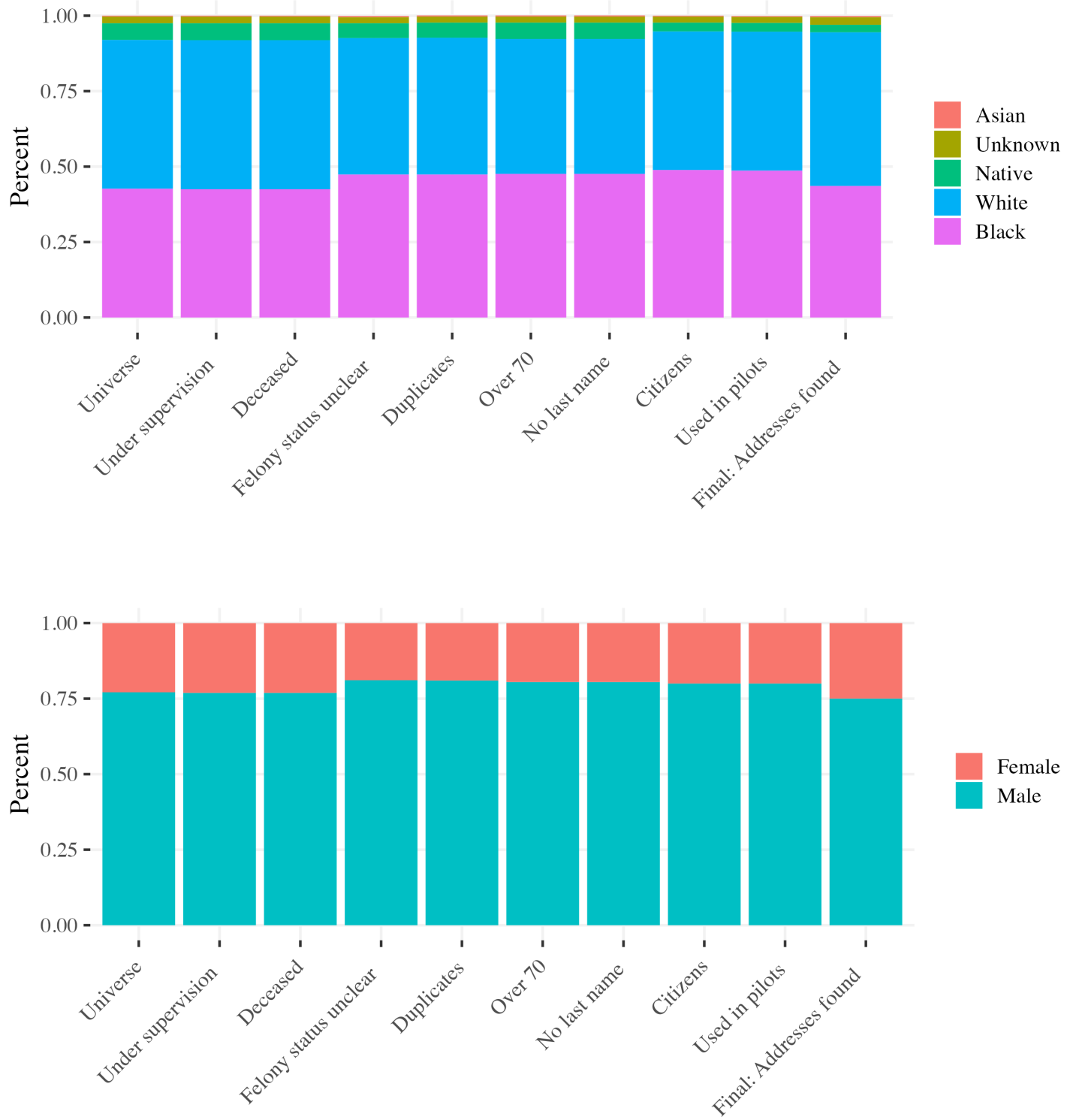
	Control Mean	Difference from Control				Joint F-test p-val
		Basic Mailer	No CR Framing	No Reg. Form	Civil Rights Framing	
Male	0.75	0.00 (0.01)	-0.00 (0.01)	0.00 (0.01)	-0.00 (0.01)	0.89
Age	43.75	0.25 (0.19)	0.18 (0.19)	0.23 (0.19)	0.38 (0.19)	0.40
Black	0.43	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	0.02 (0.01)	0.18
Past Incarc.	0.57	0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.56
Days since release	3474.65	3.27 (75.51)	58.59 (75.84)	-49.04 (75.83)	16.84 (75.72)	0.73
Observations	7049	7049	7049	7049	7049	

Figure A1: Construction of Sample



Notes: This figure shows the process of paring down from North Carolina's DPS database to the sample of voting-eligible unregistered people with past felony convictions for the main study sample.

Figure A2: Describing Data Loss: Race and Gender



Notes: This figure shows how the racial (top panel) and gender (bottom panel) composition of the dataset changed as we omitted DPS records to arrive at our final experimental sample.

### A.3 Experimental Mailers

Figure A3 below presents all four letter variations associated with the treatment arms described in the main paper.

### A.4 Partisanship

Table A2 below shows the number of people registering with each party among those who registered to vote. Column 1 shows the number of new registrants by party, for those assigned to a treatment group from our main study sample. Column 2 shows the equivalent numbers for those assigned to the control group in the main study. Columns 3 and 4 show the numbers for people from our no-criminal-record comparison group (parallel experiment described below). Overall the distribution of party registrations seems similar for the treatment and control groups in each sample. That is, it appears that our intervention was not disproportionately effective for people inclined to vote for one party over another. These numbers also tell us about the political leanings of people with felony convictions. In North Carolina in 2020, 36% of new registrants registered as Democrats, 35% registered as Republicans, 0.7% registered as Libertarians, and 28% registered as unaffiliated.

Table A2: Party of Registration for Registrants in Main Study and Comparison Group

	Criminal Record Sample		Comparison Group	
	Any Treatment	Control	Treatment	Control
Democratic	752	192	371	392
Republican	767	167	203	199
Libertarian	16	3	7	8
Not Affiliated	630	122	266	253
Total new registrants	2165	484	847	852

### A.5 Effect Heterogeneity

As discussed in the main paper, we examine effect heterogeneity in two ways, both pre-registered. First, we examine possible differences in treatment effects by the race of the person contacted, shown in the main paper.



**Do you or a loved one have a criminal record? You may still have the right to vote. Know your rights!** There are many misconceptions about the right to vote for North Carolina citizens. The 2020 Election will be historic and understanding your rights means you have the choice to **make your voice heard** in 2020 and beyond. Citizens are eligible to vote as soon as they have completed the terms of their felony conviction. This means if you are **off papers, your right to vote has been automatically restored.**

**WHO IS ELIGIBLE TO REGISTER AND VOTE IN NORTH CAROLINA?**

- You must be a US Citizen AND
- You must be 18 years old by Election Day AND
- You must be a resident of NC for at least 30 days by Election Day
- You must not be **currently serving** jail time for a felony conviction OR **currently** be on probation or parole for a felony.

**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

**STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS. A registration form is included in this letter.**

1. Complete ALL required sections (in pink) and provide EITHER your driver's license number OR the last four digits of your social security number **if you are able to.**
2. If you don't get mail where you live, enter a valid mailing address in Section 5.
3. Sign and date the form, and include a phone number so the Board of Elections can contact you if they have questions.
4. Mail or deliver the registration form to your local county Board of Elections. Find your local Board of Elections Office at [youcanvote.org/BOE](http://youcanvote.org/BOE).

You can also register online, if you have a North Carolina DMV ID card or license. Visit [youcanvote.org/register](http://youcanvote.org/register) to access the online voter registration portal!

**STEP 2: CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE (Once you are registered.)**

1. Vote EARLY at any early voting site in your county—October 15-31 OR
2. Vote on Election Day—Nov 3rd at your assigned polling location, OR
3. Vote by Mail. Visit [youcanvote.org/voting](http://youcanvote.org/voting) to request to vote by mail, look up your polling location, and more!

The 2020 Election is right around the corner. This year we will vote for local, statewide, and federal offices including US President and US Senate, NC Governor, statewide and district court judges, and many more elected offices. These office-holders make decisions that directly impact you and those you care about. By voting for people who care about the issues you do, you help shape your future. **Your vote actually does matter.**

Thank you,  


Kate Fellman  
Executive Director

You Can Vote is a North Carolina nonprofit, nonpartisan 501(c)(3) organization that works to educate, register, and empower NC citizens to cast their vote.



You are receiving this letter because we would like to encourage you to register and use your voice in upcoming elections! If you think you may be registered already, or if you would like to verify your voter registration status, you can check it here: [youcanvote.org/register](http://youcanvote.org/register).

**WHO IS ELIGIBLE TO REGISTER AND VOTE IN NORTH CAROLINA?**

- You must be a US Citizen AND
- You must be 18 years old by Election Day AND
- You must be a resident of NC for at least 30 days by Election Day
- You must not be **currently serving** jail time for a felony conviction OR **currently** be on probation or parole for a felony.

**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

**STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS. A registration form is included in this letter.**


1. Complete ALL required sections (in pink) and provide EITHER your driver's license number OR the last four digits of your social security number **if you are able to.**
2. If you don't get mail where you live, enter a valid mailing address in Section 5.
3. Sign and date the form, and include a phone number so the Board of Elections can contact you if they have questions.
4. Mail or deliver the registration form to your local county Board of Elections. Find your local Board of Elections Office at [youcanvote.org/BOE](http://youcanvote.org/BOE).

You can also register online, if you have a North Carolina DMV ID card or license. Visit [youcanvote.org/register](http://youcanvote.org/register) to access the online voter registration portal!

**STEP 2: CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE (Once you are registered)**

1. Vote EARLY at any early voting site in your county—October 15-31 OR
2. Vote on Election Day—Nov 3rd at your assigned polling location, OR
3. Vote by Mail. Visit [youcanvote.org/voting](http://youcanvote.org/voting) to request to vote by mail, look up your polling location, and more!

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**Do you or a loved one have a criminal record? You may still have the right to vote. Know your rights!** There are many misconceptions about the right to vote for North Carolina citizens. The 2020 Election will be historic and understanding your rights means you have the choice to **make your voice heard** in 2020 and beyond. Citizens are eligible to vote as soon as they have completed the terms of their felony conviction. This means if you are **off papers, your right to vote has been automatically restored.**

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**USE THE GUIDE BELOW TO ENSURE YOUR VOICE COUNTS**

**STEP 1: GET REGISTERED AT YOUR CURRENT ADDRESS.**

1. Register entirely online if you have a North Carolina DMV ID card or license. If you don't have an NC ID, start the form online and you'll be mailed a form to sign, date and return to the Board of Elections. Visit [youcanvote.org/register](http://youcanvote.org/register) to access the online voter registration portal!

**STEP 2: CHOOSE THE BEST VOTING OPTION FOR YOU & VOTE (Once you are registered.)**

1. Vote EARLY at any early voting site in your county—October 15-31 OR
2. Vote on Election Day—Nov 3rd at your assigned polling location, OR
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**Criminal Justice and Civil Rights are on your ballot.** Members of Congress and the state legislature **decide what is a crime and how it should be punished.** They make rules on how our courts, prisons, and jails are managed and how people should be treated when they are in custody. Judges decide who gets detained and for how long, and who goes to prison and for how long. Elected officials have an impact on how equal protection is enforced and are responsible for ensuring freedom of speech, assembly and religion, and specific rights including voting rights. Find out what's on your ballot and **why your vote matters** at [youcanvote.org/wob](http://youcanvote.org/wob).

Thank you,  


Kate Fellman  
Executive Director

You Can Vote is a North Carolina nonprofit, nonpartisan 501(c)(3) organization that works to educate, register, and empower NC citizens to cast their vote.

Figure A3: All Mailer Variations from Fall 2020 Experiment

In addition to these differential effects by race, we test for heterogeneous effects using a machine-learning approach developed by Wager and Athey (Wager and Athey, 2018) and applied in Davis and Heller (Davis and Heller, 2017). The goal of this approach is to identify subgroups with larger treatment effects in a principled way that minimizes concerns about data mining. This allows us to consider subgroups that more standard binary comparisons might miss (for instance, Black men in their 40s with a history of incarceration). The approach uses separate training and testing samples: we randomly selected a portion of the sample to be excluded from the training data, and use it to test the predictions made based on the training sample. In this way, we use machine learning to generate hypotheses about which subgroups are most affected by our mailers, then test those hypotheses in the holdout sample. This helps us avoid concerns about overfitting and multiple hypothesis testing.

We use the following characteristics to examine heterogeneity: gender, race/ethnicity, past supervision, age (binned into quintiles), and time since release (binned into quintiles; missing for people never incarcerated).

There are numerous possible ways of visualizing the results of this approach. Table A3 presents variable importance from this exercise, highlighting that gender as well as certain age groupings were particularly predictive of variation. Figure A4 presents subgroup estimates of treatment effects on registration, illustrating both the racial differences presented in the paper as well as some potential heterogeneity by age (bin 5 includes people above age 55, who are less likely to show large treatment effects)

## A.6 Address Quality Followup Study

As noted in the main paper, we observed suggestive evidence of racial differences in the effectiveness of the mailer treatments. One possible reason for such differences could be address quality differences: if the addresses we found for Black residents in the sample were especially likely to be out-of-date or simply wrong, we could see smaller effects for Black than white letter recipients even if the letters were equally effective once delivered. We explore

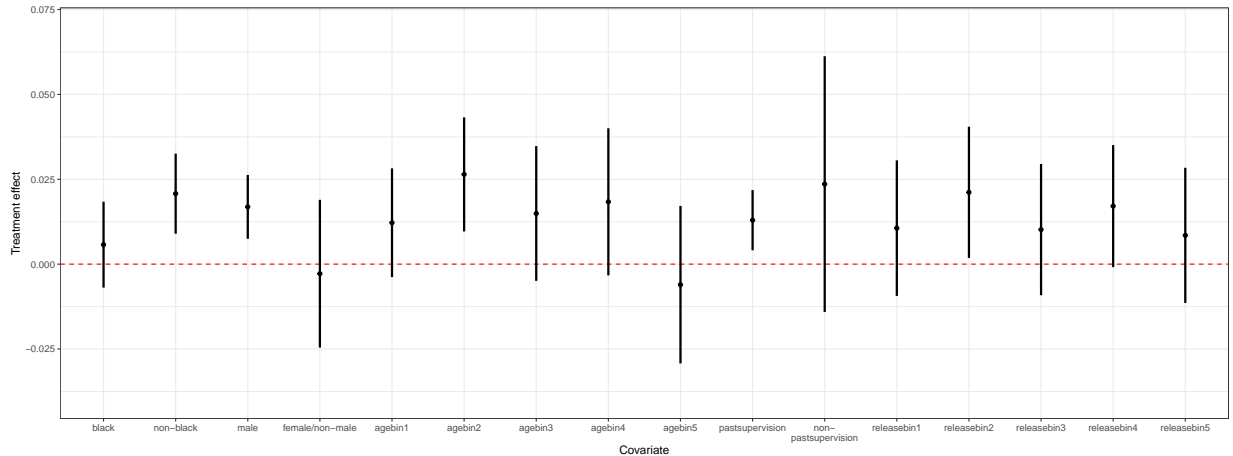


Figure A4: Subgroup treatment effect estimates from ML approach

var	importance
agebin3	0.20
male1	0.19
agebin4	0.11
wrublack1	0.10
releasebin3	0.07
agebin5	0.07
releasebin2	0.06
releasebin5	0.05
releasebin4	0.05
pastsupervision1	0.04
agebin2	0.04
wruhispanic1	0.01

Table A3: Variable importance from causal-forest approach to effect heterogeneity

this possible explanation with a followup study fielded in summer 2021.

To explore this possibility, we ran a small followup study. We mailed postcards to the sample from Study 4 and used postal-service tracking tools to observe whether the postcards were successfully delivered. This allowed us to assess the quality of the addresses we obtained from our data vendor. (Note that we sent these postcards in the summer of 2021. People may have moved during the year between our main study and this follow-up postcard study, so the results likely underestimate the accuracy of addresses at the time of the main study.)

Table A4 shows how demographic characteristics correlated with whether the postcard “bounced” (that is, whether it was not successfully delivered). We interpret a bounce as

an indicator of having an incorrect address. Overall, 86% of the postcards were successfully delivered — a high success rate for a population that is relatively transient and difficult to reach. Postcards were less likely to bounce if they were sent to men. The probability of bouncing increased with the intended recipient’s age and decreased with their time since release. Race does not predict whether a postcard bounced. It thus appears that the racial disparities in our estimates, described above, are not driven by racial differences in address quality.

Table A4: Postcard Followup: Predicting Bounced Mailers

	<i>Dependent variable:</i>
	Bounced
Male	−0.011 (0.007)
Black	0.004 (0.005)
Age	0.001*** (0.0003)
Time since Release	−0.002*** (0.0003)
Constant	0.123*** (0.013)
Observations	19,656

*Notes:* This table shows the relationship between individual characteristics and whether a mailer “bounced” (was returned to sender) – a proxy for a wrong address. Specifically, it shows the results of a regression with “mailer bounced” on the left-hand side, and individual characteristics on the right-hand side. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01



## A.7 Texas Sample

The study described in the main paper originally included a component in Texas, with the same treatment conditions as in North Carolina. However, we confronted a series of challenges implementing the project which lead us to seriously doubt the validity of the outcome. Primarily, we faced issues with mail delays as we fielded the experiment, such that the mailing of letters from the vendor was delayed and then they faced further delays due to USPS issues affecting the entire country in fall 2020. In Texas, individuals must return a registration form post-marked by 30 days prior to the election (November 3, 2020). The registration deadline for the 2020 general election was thus October 3. A sample piece of mail addressed to one of the PIs landed in their Texas mailbox on October 1, much later than originally planned. A voter receiving a mailer on October 1 (and many likely received them even later) would have had less than 48 hours to open the mailer, fill out the registration form, and get it into the mail. For this reason, in addition to some sample construction problems,<sup>16</sup> we are extremely doubtful about the treatment implementation. It seems highly likely that the mailers arrived too late to meaningfully affect registration or turnout in the November election. This makes the results of the Texas arm of the experiment unhelpful for determining whether our intervention affects those outcomes. Nevertheless, Table A5 below presents estimates from Texas side-by-side with the main paper’s estimates from NC.

## A.8 Ethical Considerations

We made numerous decisions throughout the design and implementation of this study aimed at minimizing potential harms that could be caused by our study. We knew that some jurisdictions are imposing criminal penalties on people who are ineligible to vote but mistakenly register. This informed several decisions.

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<sup>16</sup>There were errors in the processing of the administrative data. Due to these issues, we inadvertently included some individuals whose status was unclear and thus may have not been eligible to vote (28% of the Texas sample), who were still under supervision and were definitely not eligible to vote (2.5% of the Texas sample), or alternatively who were already registered and thus outside the target population of the study (at least 11% of the Texas sample).

First, we avoided states where either we or potential registrants might find it difficult to assess their eligibility status. For example, in Florida, voters who have completed their sentences and paid all fines and fees were eligible to vote – but the state provided no mechanism for researchers, voter registration organizations, or even voters themselves to check whether they had unpaid fines and fees. We did not conduct research in Florida because we did not want to risk encouraging someone to register when we could not be sure of their eligibility, and they might be criminally penalized for erroneously registering.<sup>17</sup>

Second, we were extremely conservative in selecting our sample. We excluded people with incomplete information, because we did not want to risk encouraging someone to register when they were not eligible. Third, we recognized that there would always be a degree of uncertainty. We therefore designed our mailers to avoid telling people that they personally were or were not eligible to register and vote. Instead, we informed them about the eligibility requirements for registration, including the requirements related to criminal legal system involvement.

This final ethical choice – designing our mailers to avoid determinations of individual status – also addressed privacy concerns for people who received the mailers. We knew that mail addressed to one person might end up in other hands. Thus, we designed our mailers to avoid disclosing any information about the recipient’s criminal legal system involvement. Taken in total, these choices reflect our commitment to foreseeing and avoiding unintentional harm to mailer recipients. They also highlight the role of state sanctions in the ethics of voter registration for returning citizens. Our results are conservative: if states provided clear individual eligibility determinations and removed criminal punishments for good faith errors, the reachable population would likely be significantly larger.

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<sup>17</sup>Indeed, the state of Florida has recently begun prosecuting people who inadvertently voted while still owing legal financial obligations, including some who had been told by local election officials that they were eligible.

Table A5: Effects on Voter Registration and Turnout in NC and TX

	<i>Dependent variable:</i>					
	Voter Registration			Voted November 2020		
	NC	TX	All	NC	TX	All
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: All Arms Combined</b>						
Any Treatment	0.008** (0.003)	-0.002 (0.003)	0.001 (0.002)	0.005* (0.003)	0.001 (0.002)	0.002 (0.002)
<b>Panel B: Separate Treatment Arms</b>						
Basic mailer	0.008* (0.004)	0.001 (0.004)	0.003 (0.003)	0.008** (0.004)	0.003 (0.003)	0.004* (0.002)
No crim. record framing	0.011** (0.004)	-0.002 (0.004)	0.002 (0.003)	0.007* (0.004)	0.002 (0.003)	0.004 (0.002)
No registration form	0.008* (0.004)	-0.005 (0.004)	-0.002 (0.003)	0.003 (0.004)	-0.002 (0.003)	-0.001 (0.002)
Extra civil rights framing	0.006 (0.004)	-0.001 (0.004)	0.001 (0.003)	0.004 (0.004)	-0.0003 (0.003)	0.001 (0.002)
Control Mean	0.066	0.146	0.123	0.046	0.079	0.069
Observations	35,245	89,750	124,995	35,245	89,750	124,995

*Notes:* This table shows the effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter registration by November 2020 and subsequent turnout, both in our main NC sample and in a sample in TX where we encountered implementation problems. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## B Pilot Studies

Prior to our main experiment in fall 2020, we conducted three smaller pilot experiments earlier in 2020 to test out various components of the design. All the pilots followed a similar structure to the main experiment in terms of sample construction (using administrative data to find eligible unregistered voters, then commercial address data to find current contact information) and random assignment to treatment, but they vary in the exact treatment conditions included. Those pilots are described briefly here.

Pilot 1 took place in January 2020, in time for letter recipients to register and vote in the March 2020 primary election. With a sample of 8,621 people, we randomly assigned people with equal probability to an uncontacted control condition or to a control group that would be mailed a simple informational mailer about registration and a registration form with postage-paid reply envelope.<sup>18</sup>

Pilot 2 was fielded in March 2020. We assigned a sample of 6,584 people to three treatment arms: (1) an uncontacted control group, (2) a group who received the basic mailer (replicating the first study), and (3) a group who received the basic mailer followed by a text message. The inclusion of a text-message followup made this sample slightly different from those used in Pilot 1 since we restricted not only to eligible people for whom our data vendor could find a mailing address but also to those with available telephone numbers. Readers will notice that Pilot 2 took place just as most of the country went into lockdown due to the COVID pandemic, potentially explaining the null effects seen in this pilot relative to the other three studies reported here.

Pilot 3 was fielded in June 2020, with the goal of testing the impact of partnering with a local organization already involved in voter outreach, since some research suggests that members of marginalized groups are more receptive to organizations rooted in their communities (Sinclair, McConnell and Michelson, 2013; Michelson, 2006). We partnered with a

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<sup>18</sup>The text of this mailer was relatively similar to the “basic mailer” in our main study, but included information about the primary and was not branded with the logo of the nonprofit organization with whom we collaborated on the main study.

North Carolina organization, You Can Vote (YCV), to refine the text of our mailer and craft the treatments. You Can Vote wished to execute follow up calls. Our third pilot therefore assigned a sample of 21,763 across four treatment arms: (1) a control group, (2) a group who received a basic mailer without YCV branding (again replicating the first study), (3) a group who received the mailer with YCV branding, and (4) a group who received both the YCV-branded mailer and a follow-up call from YCV staff and volunteers.

Table [B6](#) presents estimates of each pilot study’s effects on voter registration as of 30 days after the mailers were sent. Panel A presents effects of assignment to any treatment arm (relative to control), while Panel B separates out each treatment arm for studies with multiple arms. Columns 1-3 present each pilot study separately, while Column 4 pools all three studies together.

On the whole, the pilot studies showed the effectiveness of this mailer-based approach to registration, with the exception of pilot 2 (which was fielded during the COVID lockdown period, when we imagine people may have had other things on their minds besides voter registration). In Pilot 2, we did not see any indication that adding a followup phone call increased mailer effectiveness, potentially because many of the phone numbers used seemed to be out of date (they no longer belonged to the person we were trying to reach), so we did not include followup calls in the main study design. In Pilot 3, we could not statistically distinguish between the different treatment arms, but the point estimates suggested that the mailer produced in collaboration with a non-profit partner organization (and bearing their logo) might be more effective than a simple mailer without that branding. We continued this collaboration for the main experiment (as seen in the mailer images below).

Table [B7](#) presents estimates of each pilot study’s effects on voter turnout. Column 1 examines Pilot 1’s effect on March 2020 primary voting, since that pilot occurred before the primary. Columns 2-4 look at each pilot’s effect on eventual turnout in the November 2020 general election, and Column 5 pools all three studies together to look at November turnout. Pilot 1 had a small but significant effect on primary turnout shortly after the mailers went

out. Long-run effects of the pilot studies on eventual general-election turnout are less clear, with largely positive but non-significant point estimates. The pooled estimate in Panel A Column 5 suggests a combined effect of about .3 percentage points' increase in turnout, slightly smaller than the .5 percentage-point effect seen in our main experiment and not statistically distinguishable from zero. These pilot studies were not designed or powered to measure long-run turnout effects, and we interpret these with caution: it is hard to know whether the effects of the intervention are short-run effects that “wash out” with time as other outreach efforts find some people in the control group, or whether these are simply noisily-estimated positive effects.

Table B6: Studies 1-3: Effects on Voter Registration

	<i>Dependent variable:</i>			
	Registration			
	Pilot 1	Pilot 2	Pilot 3	All
	(1)	(2)	(3)	(4)
<b>Panel A: All Arms Combined</b>				
Treatment (Any mailer)	0.013*** (0.003)	0.002 (0.004)	0.009*** (0.002)	0.009*** (0.002)
<b>Panel B: Separate Treatment Arms</b>				
Basic Mailer	0.013*** (0.003)	-0.0005 (0.004)	0.007*** (0.003)	0.008*** (0.002)
Basic Mailer + Text		0.005 (0.004)		0.009** (0.004)
Branded Mailer			0.010*** (0.003)	0.011*** (0.002)
Branded Mailer + Call			0.010*** (0.003)	0.010*** (0.002)
Study Fixed Effects				X
Control Group Mean	0.007	0.021	0.011	0.01
Observations	8,621	6,584	21,763	36,968

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table B7: Studies 1-3: Effects on Voter Turnout

	<i>Dependent variable:</i>				
	Voted in March 2020	Voted in November 2020			
	Study 1	Study 1	Study 2	Study 3	All Studies
	(1)	(2)	(3)	(4)	(5)
<b>Panel A: All Arms Combined</b>					
Treatment (Any mailer)	0.002* (0.001)	-0.0002 (0.005)	0.007 (0.006)	0.004 (0.004)	0.003 (0.003)
<b>Panel B: Separate Treatment Arms</b>					
Basic Mailer	0.002* (0.001)	-0.0002 (0.005)	0.007 (0.007)	0.007 (0.005)	0.004 (0.003)
Basic Mailer + Text			0.008 (0.007)		0.007 (0.006)
Branded Mailer				0.001 (0.005)	-0.0003 (0.004)
Branded Mailer + Call				0.004 (0.005)	0.003 (0.004)
Study fixed effects					X
Control Group Mean	0.002	0.056	0.055	0.06	0.055
Observations	8,621	8,621	6,584	21,763	36,968

*Notes:* This table shows the effect of each treatment (relative to the control), as well as pooled treatment arms relative to control, on voter turnout. \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## C Comparison-Group Study Fall 2020

As mentioned in the main paper, we fielded a parallel experiment simultaneously with our main study in fall 2020, this one focused on unregistered people who to our knowledge did not have past felony convictions. Table C8 below presents estimated treatment effects from that experiment and compares them to the main experimental effects presented in the main paper. Column 1 shows the effect of our treatment on voter registration for the no-felony-record comparison group. The coefficient is near-zero and statistically insignificant. Column 2 shows the treatment effect from the comparable treatment arm in the main study; our mailers increased voter registration among people with felony convictions by 1.1 percentage points (16%;  $p < 0.05$ ). Column 3 combines both experimental datasets to test for different effects across the two samples, finding that that the treatment is significantly more effective among the main-study sample of people with past records.

Columns 5-8 of Table C8 show the effects on turnout in the November 2020 general election. Column 4 indicates that our treatment had no effect on turnout among the comparison group, while Column 5 shows the comparable treatment effect from the main study, for individuals with a felony conviction, is 0.7 percentage points (15%;  $p < 0.10$ ). As shown in Column 6, the difference in the effects across these groups is marginally significant.



Table C8: Treatment Effects for Comparison Group Versus Main Study Group

	<i>Dependent variable:</i>					
	Voter Registration			Voter Turnout		
	Comparison	Main Study	All	Comparison	Main Study	All
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment mailer	-0.0003 (0.002)	0.011** (0.004)	-0.0003 (0.002)	-0.0003 (0.002)	0.007* (0.004)	-0.0003 (0.002)
Criminal record group			0.021*** (0.003)			0.011*** (0.003)
Treatment * Record			0.011** (0.005)			0.008* (0.004)
Control Mean	0.048	0.069	0.054	0.037	0.048	0.04
Observations	35,708	14,098	49,806	35,708	14,098	49,806

*Note:*

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01