

“No longer a number”: Testing new ways to find and engage newly-reenfranchised people in New Jersey *

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Abstract

In 2019, New Jersey expanded access to the franchise for people with criminal convictions. In this study, we partnered with New Jersey Institute for Social Justice (NJISJ) to develop a strategy to encourage newly-reenfranchised individuals to register and vote. Because little is known about how to mobilize justice-impacted people, we use a multi-method strategy to develop messaging which we then test through a randomized controlled trial. In-depth interviews highlight the importance of trusted messengers in registration efforts, as well as specific messaging themes. We worked with an organizer who was formerly incarcerated to craft an outreach message based on his own story, drawing on themes from the interviews. The mailer informed by qualitative research had a substantial effect on registration and turnout in the next election, outperforming a basic informational mailer.

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Introduction

In 2019, New Jersey expanded the right to vote, re-enfranchising people still serving probation or parole sentences. In so doing, New Jersey became one of 17 states (and the District of Columbia) to restore the right to vote for people with felony convictions between 2016 and 2022 (Uggen et al., 2022). Nationally, the Sentencing Project estimates that one and half million people with felony convictions became newly eligible to vote between the 2016 and 2022 elections (Uggen et al., 2022). Indeed, few other efforts to reform criminal justice policy have met with such stunning success. With groups like the Florida Rights Restoration Coalition in Florida,¹ the Community Success Initiative in North Carolina,² the Voice of the Experienced in Louisiana,³ and the New Jersey Institute for Social Justice in New Jersey⁴ leading rights restoration campaigns, credit for the movement's success lies squarely with formerly incarcerated people themselves who lead these organizations' efforts.

Existing research on the political consequences of the carceral state did not predict such mobilization by custodial citizens to have their rights restored, nor does it predict that rights restoration will meaningfully improve registration and turnout among justice-impacted people.⁵ Voting participation among this group is very low and researchers estimate that fewer than a third of people with convictions who are voting eligible are actually registered (Burch, 2011; Gerber et al., 2015; White and Nguyen, 2022). Criminal legal contact can compound challenges already faced by marginalized people prior to conviction, including housing and job instability, and can introduce new challenges like those associated the logistical problems of re-registering, questions about eligibility, and doubts about whether one is seen as a valid participant (Lerman and Weaver, 2014; Gerber et al., 2017; White, 2019). There are therefore many reasons we might not expect justice-impacted people to become mobilized around felon disenfranchisement, and why low levels of

¹<https://floridarrc.com/policy/>

²<https://www.communitysuccess.org/our-team/>

³<https://www.vote-nola.org/>

⁴<https://www.njisj.org/>

⁵Throughout this paper, we use the term “justice-impacted” to describe people who have had contact with the criminal legal system ranging from criminal convictions to supervision (probation or parole) to incarceration.

engagement might endure even after rights are restored.

Even as much of what we know about the participation of justice-impacted people is focused on civic deficits, a handful of works ask how and whether experiences with the criminal legal system can drive mobilization. Contact with the legal system makes people stakeholders in new policy areas, given that a conviction can render individuals ineligible for certain goods and services (Owens, 2014). People who view their experiences as systemically unjust may be spurred to politically engage, especially in activities that are clearly linked to reforming criminal justice policy (Walker, 2020; Laneyonu, 2019). Yet, by scholarly accounts, when mobilization occurs, it almost never manifests in electoral politics. Instead, custodial citizens and their loved ones engage in protesting and community-focused activities, even as they withdraw from the state. There is thus a disconnect in the literature between electoral and non-electoral participation that leaves scholars wondering how to explain the current movement for voting rights restoration, led as it is by the formerly incarcerated, and organizers wondering how to channel political energy among justice-impacted people that is evident in the streets into the ballot box as well. We therefore ask: Under what conditions can justice-impacted people become active voters after deeply disempowering carceral experiences?

To answer this question, we build on Gerber et al. (2015) and Doleac et al. (2022), who contacted people with felony convictions via mailers informing people of their eligibility and how to get registered. These ground-breaking studies demonstrate that simply reaching out and asking justice-impacted people to participate can increase their baseline rates of registration and turnout. Yet, how to overcome barriers to participation that develop from and are exacerbated by carceral contact is an outstanding question.⁶ In order to understand how conventional voter mobilization tools can be applied to build power among deeply and uniquely marginalized constituents, we consult expertise often overlooked by political science: justice-impacted people who are themselves politically mobilized, and who engage in the everyday work of organizing their communities. To

⁶As we discuss below, the literature has struggled to precisely measure the causal effects of imprisonment on voting, but has identified a range of attitudes that tend to be held by people with past carceral contact. We are interested in how outreach can address the specific concerns of justice-impacted people to help them participate.

this end, we begin by developing an organizational partnership with the New Jersey Institute for Social Justice (NJISJ), who in 2019 spearheaded the 1844 No More Campaign to expand access to the franchise for people with felony convictions. In the wake of the campaign's success, NJISJ's staff and membership undertook efforts to register the newly enfranchised. NJISJ's approach centers the voices of justice-impacted people. Their efforts are led by a staff member and a council of individuals who were themselves formerly involved in the criminal legal system.

We use qualitative methods to develop theoretical insight into how to best connect with and engage justice-impacted people. As described below, we observed organizing meetings and conducted interviews with justice-impacted people with varying degrees of political involvement. This process identified several factors important to engaging justice-impacted people, one consistent with prior research and two unique to this project: (1) the importance of a trusted, credible messenger in delivering mobilization messages, (2) that messages should lift up narratives that connect individuals' personal experiences to a larger collective struggle for justice, and (3) that strategies should aim to restore belief in the value of voting by casting voting as a collective action well positioned to speak to that larger, collective struggle. In particular, how to channel political interest individuals already hold and express through participation in community-based activities into registering and voting via group-based narratives is a key insight developed from the qualitative portion of the project.

Working together with the team at NJISJ, we developed a mail-based randomized control trial (RCT) to test the efficacy of outreach approaches that apply these themes. One mailer version incorporated themes developed from the qualitative work. We evaluated the effectiveness of this research-informed mailer relative to a basic informational mailer of the sort employed by Gerber et al. (2015), as well as Doleac et al. (2022) and an uncontacted control group. Using administrative records of criminal legal involvement and commercial address information, we identified 23,768 individuals eligible to receive our mailer, which amounts to about one-third of the newly-enfranchised population in New Jersey. We find that the research-informed mailer was effective at spurring participation, increasing voter registration by .5 percentage points (and yielding many

more updated registrations among people in the sample who were already registered) and voter turnout by .6 percentage points in the November 2021 election relative to an uncontacted control group. These effects are substantial, and they are larger than the effects of a simpler informational mailer that did not incorporate the insights of our qualitative research.

These findings help us understand how people come to participate in civic life after conviction, and point to ways that community organizations could help reincorporate people into electoral politics. This civic rebirth stands to benefit both the people engaged in politics—making them feel part of a broader community (Owens, 2014)—and also their families and neighborhoods. Voting participation has been associated with reduced rates of future crime and arrest (Uggen and Manza, 2004). And given the geographic concentration of exposure to the legal system (Burch, 2013), increased participation by justice-impacted people could mean that neighborhoods otherwise overlooked by vote-seeking politicians would receive more political attention.

In what follows, we begin by reviewing existing research on voter registration, highlighting how little political science knows about how to mobilize justice-impacted people. We turn to research on the mobilization of other historically marginalized groups in order to identify basic strategies that may likewise succeed with our population of interest. We then describe the New Jersey context and the 1844 No More Campaign, and our research strategy. We highlight relevant themes developed from the qualitative data, outline our experimental design, and review the findings from the RCT. We conclude with directions for future research.

Background

Much of what scholars know about the political lives of justice-impacted people focuses on why such individuals do not participate. People with past criminal convictions have very low rates of registration and voting, even when they are eligible, and even in comparison to demographically-similar people who live near them (Burch, 2011, 2012). Examining the most permissive contexts, where people with felony convictions never lose the right to vote even while incarcerated, scholars

find they do so at very low rates (White and Nguyen, 2022). Studies estimate that in many states baseline registration among people with felony convictions hovers around 35% (Burch, 2011).

Reasons for low levels of engagement among this group are many. People with past criminal legal contact were often deeply marginalized prior to contact, and as such had limited political involvement before those experiences (Gerber et al., 2017). A conviction can negatively impact access to material resources important to participation, compounding barriers to engagement faced prior to criminal legal entanglement (Pettit and Western, 2004; Western, 2006; Burch, 2013; White, 2019). Likewise, scholars have written powerfully about the political lessons conveyed to justice-impacted people about their value as democratic citizens, the trustworthiness of state institutions, and the efficacy of political engagement overall (Lerman and Weaver, 2014; Weaver, Prowse and Piston, 2019, 2020).⁷

Yet some work raises the possibility that experiences with the criminal legal system can provide the basis for mobilization (Walker, 2020; Owens, 2014; Anoll, Epp and Israel-Trummel, 2022; Morris, 2021). Detailing the organizing of people with felony convictions around rights restoration in Rhode Island, Owens (2014) recognizes that a conviction uniquely renders justice-impacted people policy constituents, as they are barred from accessing a variety of goods and services as a consequence of a criminal record (Owens, 2014). Walker (2020) argues that criminal justice contact can sometimes give way to a politicized identity. When individuals view their experiences as inhumane or discriminatory, and connect those experiences to a larger collective struggle for justice, those same experiences can promote political action (Walker, 2020).

However, studies linking criminal justice contact to heightened political engagement find that

⁷We note that scholars have struggled to causally identify the impacts of imprisonment on downstream political outcomes, especially voting (Gerber et al., 2017). Yet, the full impact of the criminal justice system on attitudes likely extends beyond the identified experience of imprisonment to include quotidian experiences with police on the street, vicarious experiences via a loved one, and so forth. As such, *experiences with the criminal justice system* are akin to a bundled treatment and it is difficult to parse out the precise impact of any one experience (like conviction or incarceration) on the subsequent attitudes individuals hold about politics. Moreover, a rich body of work documents how experiences with the system are reflected in political attitudes held by justice-impacted people, very often in their own words (Weaver, Prowse and Piston, 2020, 2019; Lerman and Weaver, 2014). Scholars at least agree that criminal justice contact exacerbates the barriers to participation faced by many marginalized people. Our goal here is to understand the attitudes about participation that are related to criminal justice contact and which may function to inhibit electoral participation among this group, even as a precise causal relationship has eluded scholars.

some people become more likely to engage in activities like protesting and community organizing – but not more likely to participate in elections. In keeping with this pattern, state programs informing individuals of their eligibility have limited impact on voter engagement (Meredith and Morse, 2014, 2015). Scholars have argued that because contact can heighten alienation from the state, justice-impacted people who are politicized by their experiences may withdraw from formal political institutions, even as they lean in to activities that build power within their communities (Owens and Walker, 2018). Indeed, Walker (2020) has little insight to offer about how to channel political agency held by justice-impacted people into formal electoral power.

Two studies of which we are aware ask how to recover *electoral* participation among people with felony convictions (Gerber et al., 2015; Doleac et al., 2022). These studies evaluate the effectiveness of encouragements to register within the context of mail-based RCTs. Gerber et al. (2015) partner with a state agency to obtain contact information for a subset of recently released people. Doleac et al. (2022) construct a list using publicly available conviction records, and obtain contact information through a commercial data vendor. Both studies employ a basic mailer informing people of their rights and how to register, and both succeed in boosting registration and turnout among people with convictions. These studies help solve important logistical problems around how to identify and contact justice-impacted people, who are residentially mobile, hard-to-reach, and deeply marginalized. They also demonstrate that simply reaching out, providing individuals with information about eligibility and how to register and inviting them to do so can boost baseline engagement among a group scholars have otherwise largely treated as lacking in civic capacity.

At the same time, neither of these studies employ a strategy designed to overcome barriers to voting specific to people with past criminal legal contact. Instead, they primarily offer information about eligibility and how to register. Research around what compels justice-impacted people to politically engage presents a tension for scholars and advocates, where scholars note that non-electoral engagement is often driven by different factors than is voting. Yet, justice-impacted people do respond to basic invitations to register and vote (Owens and Walker, 2018; Walker, 2020; Weaver, Prowse and Piston, 2020). Moreover, Doleac et al. (2022) found that their mailers

were *more* effective among justice-impacted people than among a comparison sample of people without records, suggesting that electoral engagement is not beyond reach for this group. What does a mobilizing strategy that is designed to overcome the barriers to participation introduced (or exacerbated) by criminal justice contact look like? How might advocates and organizers tap into the experiences of justice-impacted people to mobilize them into electoral engagement?

Given that we know very little about how to electorally engage justice-impacted people, we might try to draw insight from other marginalized people. Knowledge about how to electorally engage other chronic non-voters who are not justice-impacted is also thin (Green and Gerber, 2019). It is difficult to identify and find eligible voters who are not listed on the voter rolls, and doing so can be costly without clear political payoff, so political actors are less likely to do it than to do “GOTV” work among already-registered voters (Jackman and Spahn, 2021). A small literature on registration has identified some techniques that can increase registration—personalized actions like door-knocking—and some that can actually backfire, such as email outreach (Bennion and Nickerson, 2011; Nickerson, 2015). Many of the extant studies of voter registration have focused on specialized populations such as college students, since class lists can provide a starting point for registration efforts that would otherwise be difficult to conduct among the general public (Bennion and Nickerson, 2021).

Moving beyond how to find chronic non-voters, research about how to engage members of historically marginalized groups identifies three key factors to success: 1) culturally competent methods of outreach, including making materials available in languages other than English (García Bedolla and Michelson, 2012; Michelson, 2003; Michelson and García Bedolla, 2014; Valenzuela and Michelson, 2016); 2) personalized interactions that establish a connection between the individual engaging in outreach and those they are recruiting (Ramirez, 2005; Malhotra et al., 2012; García Bedolla and Michelson, 2012); and related to this, 3) the employment of a trusted and credible messenger in recruitment efforts (Kammerer and Michelson, 2022; Scott, Michelson and DeMora, 2021; Green and Michelson, 2009; Grumbach, Han and Warren, 2022; Han, McKenna and Oyakawa, 2021). In sum, research points to the general importance of targeted, culturally

competent messages that center the voices of individuals who themselves come from the community that is targeted for recruitment; at the same time, we know little about what an appropriately targeted outreach strategy should look like for justice-impacted people.

In order to develop knowledge around how to mobilize justice-impacted people, we began by developing a partnership with an organization in New Jersey already heavily involved in the process of recruiting them to register and vote. Working together with advocates and organizers, we developed a broad outreach approach and tested its effectiveness using a randomized controlled trial conducted via mailers sent in the lead up to the 2021 statewide election. We opt to test our approach using mailers because they are a cost-effective way of reaching people on a large scale; we do not face issues with building a list faced by other registration campaigns because our approach is targeted to people with convictions, who we can identify using administrative records; and because other work has found that mailers are an effective way of engaging unregistered people, including those impacted by the criminal justice system, in the electoral process (Haenschen and Mann, 2022; Doleac et al., 2022; Gerber et al., 2015). Below we outline why this partnership in the state of New Jersey provided a fruitful setting for this project.

New Jersey's Re-enfranchisement Process

Until 2020, New Jersey was among the states that restored the right to vote only after an individual had served any sentence of incarceration as well as any probation or parole time. The New Jersey Institute for Social Justice (NJISJ) is a non-profit organization located in Newark, New Jersey focused on the promotion of racial and economic justice.⁸ In 2017, NJISJ spearheaded the 1844 No More campaign to highlight the historical racial injustices represented by felony disenfranchisement in the state and to press for the restoration of the vote to all justice-impacted New Jerseyans. They launched the campaign with the release of the 1844 No More report, and, centering the work and voices of people affected by disenfranchisement, partnered with state legislators to introduce

⁸<https://www.njisj.org/>

and promote legislation restoring the right to vote for all justice-impacted people.⁹

In 2019 New Jersey’s legislature passed a bill that would restore the right to vote for people currently on probation and parole. In March 2020, the law went into effect. State agencies were tasked with informing people on supervision that they had the right to vote, but NJISJ and other community organizations reported hearing from recently-released people that they were not given accurate information about voting eligibility. NJISJ and their partners formed the Reform Alliance for Civic Engagement (RACE) Council with the goal of bringing together formerly-incarcerated people both to advocate for further expanding voting rights to currently-incarcerated people, and to ensure that justice-impacted people newly eligible to vote were informed about and could exercise their right to vote. The RACE Council was led by NJISJ’s Democracy Fellow, Ron Pierce.¹⁰ Mr. Pierce is himself formerly incarcerated. The RACE Council is likewise comprised of formerly-incarcerated people and advocates who work closely with justice-impacted people.

In early 2020, we developed a partnership with NJISJ to test new ways of finding and engaging newly-enfranchised people in New Jersey. The goal of the research partnership was to develop a method of contacting and recruiting justice-impacted people to register and vote that was informed by the experiences of justice-impacted people, reflected strategies and tactics NJISJ and partners already viewed as successful, and to systematically evaluate the efficacy of those same strategies and tactics. Our project therefore used participant observation and qualitative interviews to inform methods of contact and messaging, which we then tested through a mail-based approach. New Jersey holds its legislative and gubernatorial elections in off-election cycles. We therefore carried out the RCT in the lead up to the fall 2021 statewide election. In what follows, we detail the research approach and findings.

⁹Find the “1844 No More” report here: <https://www.njisj.org/1844nomorereport2017>; and the “Value to the Soul” report, documenting the importance of voting to justice-impacted people here: https://d3n8a8pro7vhmx.cloudfront.net/njisj/pages/1360/attachments/original/1570569487/Value_to_the_Soul_10-08-19_FIN_WEB.pdf?1570569487

¹⁰Mr. Pierce now holds the title of Policy Analyst at NJISJ.

Qualitative Data Collection

Method

Best practices for establishing organizational relationships that lead to RCTs include working with organizations that already have the resources and infrastructure required by the design, and that have experience working with the target population (Levine, Druckman and Green, 2021). NJISJ's work on the 1844 No More Campaign was spearheaded by formerly incarcerated people and actively engaged justice-impacted people to testify before the state legislature. After the law was adopted, NJISJ and the RACE Council undertook efforts to inform newly-enfranchised people about their rights and to help them register and vote. NJISJ's work on this topic made them an ideal partner for this research, insofar as their efforts are led by justice-impacted people, they are already engaged in the kind of work we hope to evaluate and augment with an experimental intervention, and they wished to systematically evaluate some of the lessons they had learned through their efforts.

Lessons from community-based participatory research efforts emphasize the importance of approaching organizational partnerships from the perspective that there are multiple and diverse kinds of expertise beyond those scholars bring to the table, and that the knowledge held by organizational and target group members themselves should inform every aspect of the research process (Gabbidon and Chenneville, 2021; Pk, 2018; Hardy et al., 2016; Levine, Druckman and Green, 2021). Allowing the knowledge of impacted people to guide the research process engages participants as active agents in the construction of knowledge around their experiences.

Our project was informed by extensive cooperation with community experts from the RACE Council. First, we attended a handful of meetings of the RACE Council to participate as active listeners while members of the RACE Council discussed efforts to reach and register newly-enfranchised people. The purpose of participating in RACE Council meetings was to build trust between the researchers and the organizers and to begin to identify common challenges faced by organizers in their efforts to register people. Much of these conversations revolved around efforts

to help individuals newly released from jail or prison navigate the reentry process, and the details of those individuals' struggles. We then engaged in a more directed conversation with members of the RACE Council, in order to outline the intent behind the research project and solicit feedback on how to best conduct the project. Best practices for engaging in organizational partnerships for the purposes of community-engaged research include deferring to participants when assessing burdens incurred within the context of the study and appropriate ways of alleviating said burdens (Gabbidon and Chenneville, 2021). We therefore worked with the RACE Council to vet our recruitment method, materials, and incentives to participate.

Our aim was to learn about the attitudes held by individuals who were politically mobilized and those who were not active, with special attention to attitudes towards voter participation, and to identify themes that could inform future mobilization efforts among justice-impacted people. We also wanted to make sure we interviewed a group of people diverse in terms of race, gender and length of experiences with the criminal justice system. Following recommendations we received from the RACE Council, we began by interviewing several individual members of the Council who were actively involved in registering newly-enfranchised people to vote. Then, we employed a snowball sampling strategy to further interview individuals not immediately involved with NJISJ, but who may have had a wider variety of criminal legal experiences. After developing this snowball sample, we observed that our interviewees were disproportionately male and many were actively politically engaged. In order to develop a sample more broadly representative of justice-impacted people in New Jersey, we contacted another re-entry organization not at all focused on voter registration.¹¹

In total, we conducted 29 in-depth interviews. We interviewed 17 RACE Council members and individuals connected to Council members and 12 people identified through the second organization. Participants were reimbursed \$50 for their time. Twelve of our interviewees were women, and 17 were men; 20 were Black, three were white, three were Latino, and three did not share their racial identity. While the exact details of individuals' criminal legal involvement were often

¹¹This organization prefers to not be identified.

unclear (and not directly queried), a number of individuals indicated they had experienced very long prison sentences; several more suggested that they had repeated, shorter experiences with incarceration; seven individuals indicated they had been released within the last few years, five indicated they had been out for several years, and the remainder did not indicate any detail about time since release.

The interviews were semi-structured and open-ended. Participants were first asked to discuss how they heard about the interview and why they agreed to speak to us. When interviews did not naturally progress towards issues related to politics, the interviewer would prompt participants to reflect on how they felt about contemporary politics, as well as whether they were registered to vote, and why or why not. When interviewing individuals who were actively involved with efforts to register and turn out newly-enfranchised people, the interviewer prompted participants to reflect on that effort, successful strategies, and obstacles faced to registration. Interviews were completed over the phone or via Zoom. Interviews ranged from as short as 10 minutes to just over an hour.

The interviews were transcribed and evaluated to identify common themes that characterized the views of participants who were actively engaged in politics relative to those who were not. We enlisted the assistance of two graduate research assistants (GRAs) to evaluate the transcripts. The GRAs were instructed to carefully review each interview and to identify common themes among those who were highly engaged, relative to those who were not at all engaged. They were instructed not to count the number of times certain themes emerged. They were advised that there was no predetermined idea about how many themes they should look for. Instead, they were instructed that the interviews themselves should dictate the number of themes and relevant content. The principal investigators, already intimately familiar with the interview content, each selected a sample of interviews on which to perform the same task. We then compared the analyses to identify themes that commonly emerged.¹²

¹²In what follows, we summarize themes that emerged from the interviews. In certain instances, we include direct quotes. All names of individuals interviewed are pseudonyms, and have been changed to protect the privacy of the interviewee.

Findings

Three factors central to promoting electoral engagement among justice-impacted people developed from the qualitative work: 1) the importance of trusted or credible messengers to deliver information about voter eligibility and how to navigate the electoral process, 2) tapping into a collective identity held by justice-impacted people, and 3) recovering a belief in the value of voting through casting it as a collective (as opposed to individual) act. It is worth noting that the importance of these factors, particularly that of a trusted messenger, hold for other historically marginalized groups as well, even as the specific content of the mobilizing messages may be particular to justice-impacted people (García Bedolla and Michelson, 2012; Walker, 2020; Green and Gerber, 2019).

Our qualitative research underscored the importance of employing a trusted, credible messenger to deliver information about eligibility to vote and navigating the electoral process, in order to overcome fear and skepticism of the state. Throughout RACE Council meetings, organizers reported that formerly incarcerated people expressed uncertainty about whether they could vote and fear about what might happen if they voted but were ineligible. In response, organizers would sometimes accompany people to the polls to help them navigate the process. The importance of a trusted messenger with similar life experiences recurred across interviews as well. For example, Ivy is a formerly incarcerated Black woman who became active when she was invited to share her story through the 1844 No More Campaign, and who registered people after the campaign's success. She emphasized that similar life experiences can establish credibility, even when one does not personally know the individual they are recruiting. Specifically, Ivy shared the story of how she recruited another justice-impacted individual, Ray Ray, saying, "Getting Ray Ray to register to vote because he was selling drugs for life, because this is what happened in his life. But Ray Ray didn't think he could have, he had a chance to [vote]. So, I was here to tell Ray Ray like, hey, Ray Ray, I was, I was with you. I was out there too. But this is where I'm at today. And you have a right to vote, but guess what – I voted, too. He's like, what? So, it's, you know, those little experiences help other folks realize, like, no, I have a chance to."

Receiving information about eligibility and how to navigate the process from a trusted source

helps overcome a major barrier to participation faced by justice-impacted people: lack of trust in formal political institutions. The effectiveness of trusted messengers in prompting engagement is highlighted in past work (García Bedolla and Michelson, 2012; Malhotra et al., 2012). However, the idea that trusted messengers can effectively mobilize many different kinds of people raises questions about the particular importance to justice-impacted people, who face unique attitudinal and material challenges to participation. Are some messages especially effective for this group?

Interviews suggest that a trusted messenger is necessary but not sufficient for increasing electoral engagement among justice-impacted people. In addition, people need to have a reason to engage, and they need to believe voting, specifically, is worth their time. Recovering the belief that voting is worthwhile is particularly important, since organizers reported both during RACE Council meetings and in interviews that they confronted deep disaffection towards voting even as individuals may express an interest in other kinds of collective action. How, then, to recover a belief in the value of the vote is a primary challenge within the context of this project.

The findings from the qualitative interviews chart a path forward via group-based narratives that connect individuals' personal experiences to a larger set of grievances held by justice-impacted people. An example of this kind of narrative comes from Eric. Eric is very active in organizing in his community, connecting newly released individuals with jobs. Reflecting on his own reasons for organizing in this particular way, he said:

I started to see that, okay. You have a certain group of people that can actually get loans, get assistance, get this, get that. But then you have another segment of people that don't worry about them, they don't vote, they don't count. Forget about them, or, you know, they only want social services, they don't want to work for themselves. I knew that to be not true. Because from my experience of trying to gain and gather economics off the street, I knew what it was, it wasn't that I wanted to hurt my people or harm my people in regard to what I was doing. I wanted economics, I wanted to have more, I wanted to have more than one bathroom. And 10 people using it, you know what I mean? So, what it was is that I started to see the difference. I started to see that, wait a minute, somebody wants you to land in their prison system, somebody wants you to be bad because jobs are created off of social situations.

Eric uses a group-based narrative about the political economy of the criminal justice system,

and identifies this view as providing a catalyst to act. Walker (2020) refers to this kind of narrative as *a sense of systemic injustice*. This sense of systemic injustice is a politicized identity that locates individual grievances in a larger set of institutional biases disproportionately impacting a group with which one identifies, and accordingly indicates a group with whom to organize. Yet, Walker offers no insight into how this kind of collective narrative promotes *electoral engagement*, specifically. Instead, in Walker's story a politicized identity accompanies low trust in government to promote other kinds of community-focused activities. In-depth interviews conducted for this project help diagnose the symptom of chronic non-voting further, and highlight that it may be easier to recover voting by tapping into the same collective identity that promotes other kinds of engagement than to do so by trying to improve trust in government directly. This can be done by casting voting as one kind of collective act available to the group to forward their collective goals.

Interviews suggest that in addition to holding generalized distrust towards the state, people often view voting as a highly individualized activity, where a single vote in any given election does not hold much capacity for change. For example, Henry is a young man of color sentenced as a juvenile and recently released from prison. He has become involved in various groups, but does not see value in voting. In talking through his position, he emphasized the individual nature of the activity, saying, "What does one [vote] do? You know... it's not that I'm like pessimistic about it. It's more like if I can get like five or six hundred people to vote with me, then we're gonna go vote together. I don't want to vote by myself. Like, what impact am I making?"

Other individuals who are themselves formerly incarcerated but who are actively engaged in efforts to get newly enfranchised folks registered offered further insight. Ivy, who highlighted the importance of a trusted messenger above, provides an example of the attitudinal transformation that can occur when people begin to understand voting as collective, and thus as a vehicle for creating change for one's community. Recall that Ivy became active when she was invited to participate in the 1844 No More Campaign. Referencing that experience, she says:

"So as a result of the 1844 No More Campaign, which was led by amazing people like Ron at the Institute, I had my vote restored last December. I once again felt proud, I

felt like I belonged, like I could finally contribute my essential voice to policies, that would affect my community and bring change, the change that I needed for myself... It has really brought home how much voting means and what impact it can have within a community, individually and collectively.”

For Ivy, voting is meaningful not only because it offers a way to make change not just for herself, but for the community of which she is a part. She came to understand voting in this way through first organizing collectively with other justice-impacted people as part of the 1844 No More Campaign. Together, Ivy and Henry’s comments suggest that activating attitudes like a sense of community belonging and a desire to create change for that community, which research elsewhere shows promotes non-electoral engagement, could also be a route to eventual voter participation. For example, Abel, also active with 1844 No More and in community organizing in Camden, offered an example of community engagement as a route to voting:

You just have to be patient with them and pretty much try to galvanize them in some way. So, you just have to basically just find the shiny item to dangle in front of them. During Christmas, a guy who ran for mayor in Camden. . . had a Christmas – a gift drive, you pull up and get a gift. And usually he asks [us] to assist in that. And the Attorney General was there, the state police and Camden County police, you know, brought gifts and everything to hand out to the kids. So, [we] took two guys who didn’t want to participate in the political process and just asked them to help us, you know, hand out toys. It was COVID friendly, you know, you run up to the car, you know, find out how many kids they got and their ages. And then you go get a gift, you know, in the age range. And once they participated in that. . . it had such an impact on them. They just wanted, they wanted more. . . I guess you could say indoctrinated into the community activism arena through their own work. So, it’s like, alright, this felt good. And now what more should we do? We go back to the issue that we had a debate about the vote, you know. I’m gonna give it a try, you know, so trick people into working for themselves.

In sum, strategies to boost electoral engagement among people with past convictions should first tap into a group-based identity that develops from criminal justice experiences, and second, cast voting as a collective tool with the potential to benefit the group. Rather than focusing solely on recovering trust in political institutions, outreach programs should move through community both in developing engagement strategies, and with respect to who executes the strategy. Even

eager voters focused on the importance of community and belonging when discussing why voting was valuable to them. Tasha, an older Black woman who cycled in and out of prison and jail for most of her adult life and cast a ballot for the first time in 2020 remarked, “So I was happy that I could vote when I got to this point where I was pleased. I was a member of society. And I’m no longer a number.” Thus, how to recover electoral engagement in the face of deep disaffection towards the state held by justice-impacted people is the final piece of the mobilization puzzle developed from the qualitative portion of the project.

From the qualitative research, we have developed the components of a strategy that we think is likely to boost electoral engagement among justice-impacted people. This trusted-messenger, group-based approach is consistent with previous scholarship on how to engage marginalized people via get-out-the-vote campaigns, as well as research on why justice-impacted people participate in non-electoral activities. But it is unclear from the literature how to put these pieces together to promote electoral engagement among a group of people who deeply distrust the state. From this exercise, we have developed theory around how to use a group-based approach to promote electoral activities. However, leveraging a trusted-messenger, group-based approach to improve registration and voting has not been systematically tested, and many theoretically-derived interventions do not work in the field (Doleac et al., 2022). We thus turn our attention to testing whether these observed themes can be effectively incorporated into a real-world effort to increase participation. The next section describes that test.

Before turning to the actions inspired by these findings, we briefly report one other pattern observed in the interviews. Beyond the attitudinal patterns described above, interviewees also reported facing real material barriers to voting: they described needing to prioritize finding housing, jobs, or their next meal over registering to vote. Some also reported logistical costs of voting, such as needing to figure out eligibility and registration processes. Some of these barriers may be addressed as part of a voting outreach program (such as one where credible messengers talk people through the registration process or accompany them to needed appointments), but others are on a scale that is unlikely to be addressed by civic-engagement programs. While we proceed to an

intervention based on some of the attitudinal findings laid out above, we acknowledge that many justice-impacted people face voting barriers that are a result of high-level policy choices rather than messaging decisions.

Mailing Study

We developed a randomized controlled trial (RCT) that sought to contact as many newly-enfranchised voters in New Jersey as possible, and to test the effectiveness of a strategy incorporating the insights from our qualitative research. The RCT used mailers about registration and voting fielded in advance of New Jersey's statewide (gubernatorial and legislative) elections in November 2021.

We began by developing two mailers in cooperation with Ron Pierce of NJISJ and his colleagues. The first mailer included a one-page letter from Mr. Pierce inviting newly-enfranchised people to register and to vote. The letter highlighted his own story of losing the right to vote after a conviction, his belief in the importance of voting, an understanding of felony disenfranchisement as a specific means of disempowering historically marginalized people, and the success story of the 1844 No More Campaign. The mailer also included instructions on how to register and a paper registration form (New Jersey's registration forms include pre-paid postage on a detachable return envelope). We refer to this package as the *research-informed mailer*. The second mailer only provided information on how to register and vote alongside a registration form; the letter included in this mailer was generically signed by NJISJ and included no reference to Mr. Pierce or the 1844 No More Campaign. We refer to this as the *information-only mailer*.

To find a pool of newly-reenfranchised people for the RCT, we relied on government records of probation and parole acquired through records requests to state agencies; these records allowed us to find people who were or had recently been on probation or parole in the state, even if they had not come into contact with our partner organization in the past. Then, we worked with a commercial data vendor to find current addresses for people we wanted to contact, a crucial step since this population has high rates of residential mobility and even relatively recent addresses

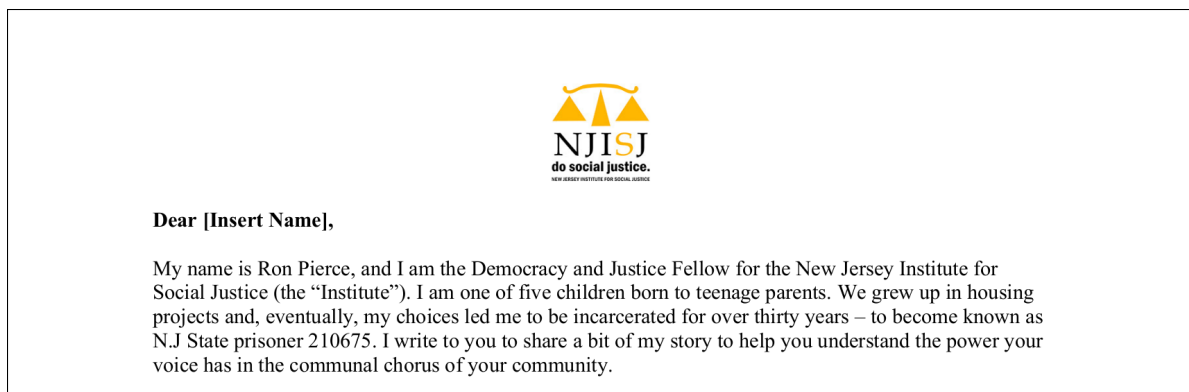


Figure 1: Top portion of the letter from Ron Pierce included in the “Research-Informed” mailer. Full copies of both letter versions appear in the SI.

found in government records could be out of date.

Sections A.2 of the SI describes the construction of the mailing lists in detail. Briefly, we requested lists of people on parole as well as sentencing records that would allow us to identify people on probation, and ultimately found identifiable records for more than 39,000 recently-reenfranchised people. A commercial data vendor was able to find current mailing addresses for nearly two-thirds of them, and this group of people with mailing addresses formed the sample for the RCT. The pool thus represented nearly one-third of the newly-reenfranchised residents of New Jersey, with data loss due both to unclear sentencing records (described further in the SI) and to unavailable mailing addresses. It is broad in its coverage, containing people who have experienced both probation and parole, and it is demographically similar to the original list of 39,000 people we sought to find.

Once we had built this list of 23,768 people with addresses, they were randomly divided into three groups: an uncontacted control group, a group that would receive the Information-Only mailer, and a group that would receive the Research-Informed mailer with the letter written and signed by Ron Pierce. Due to budget constraints for the mailing, the probability of assignment to the control group was slightly higher than for the two treatment arms. Treatment assignment was blocked on available covariates.¹³ In a small number of cases where multiple people from the

¹³Covariates used were: age, gender, race, data source, pre-treatment match status to the voter file (based on a match conducted by a partner organization), and voter registration status as found in that voter-file match.

sample lived at the same address, we constrained those households to all receive the same treatment condition; all regression analyses include standard errors clustered at the household level to reflect this structure. The sample was not restricted to unregistered people, as some people with out-of-date voter registrations might need to update their registration in order to vote in the upcoming election and thus could still benefit from the letter.

We worked with a mail vendor to print and send out treatment letters in early September, about five weeks in advance of New Jersey’s voter-registration deadline. Although we do not know how many people actually opened and read the letters, USPS mail tracking indicated that over 98% were successfully delivered to mailboxes. To estimate treatment effects on registration and voting, we use snapshots of the New Jersey voter file requested from the state (see Section A.3 for a description of record linkage).

In accordance with the pre-analysis plan we filed before the RCT was fielded, we examine intent-to-treat effects on three main outcome measures: voter registrations, updated registrations, and voter turnout. First, we examine voter registration: did the person appear on the state’s voter file in time to be eligible to vote in the fall 2021 election? Second, because some people included in the pool were already registered prior to the RCT, we also measure updates to voters’ registration records during the period we were in the field.¹⁴ Voters who newly registered between August 2021 (the date of our pre-treatment voter snapshot) and the November election, or those who were already registered but updated key registration fields (name, address, party registration) are recorded as having updated their registrations. Finally, we measure whether each person in the pool turned out to vote in the November 2021 general election, unconditional on registration (Nyhan, Skovron and Titiunik, 2017). Overall, 34% of people in the sample were registered to vote as of the November 2021 election, 2% either newly registered or updated their registration during the RCT period, and 6% voted.

¹⁴Nearly one third of the sample were already registered to vote at the time mailers were sent; many of these registrations had long-ago registration dates or inactive status and appeared to be due to people not having been removed from the voter file at the time they lost the right to vote.

Effects of Treatment Conditions on Registration and Voting

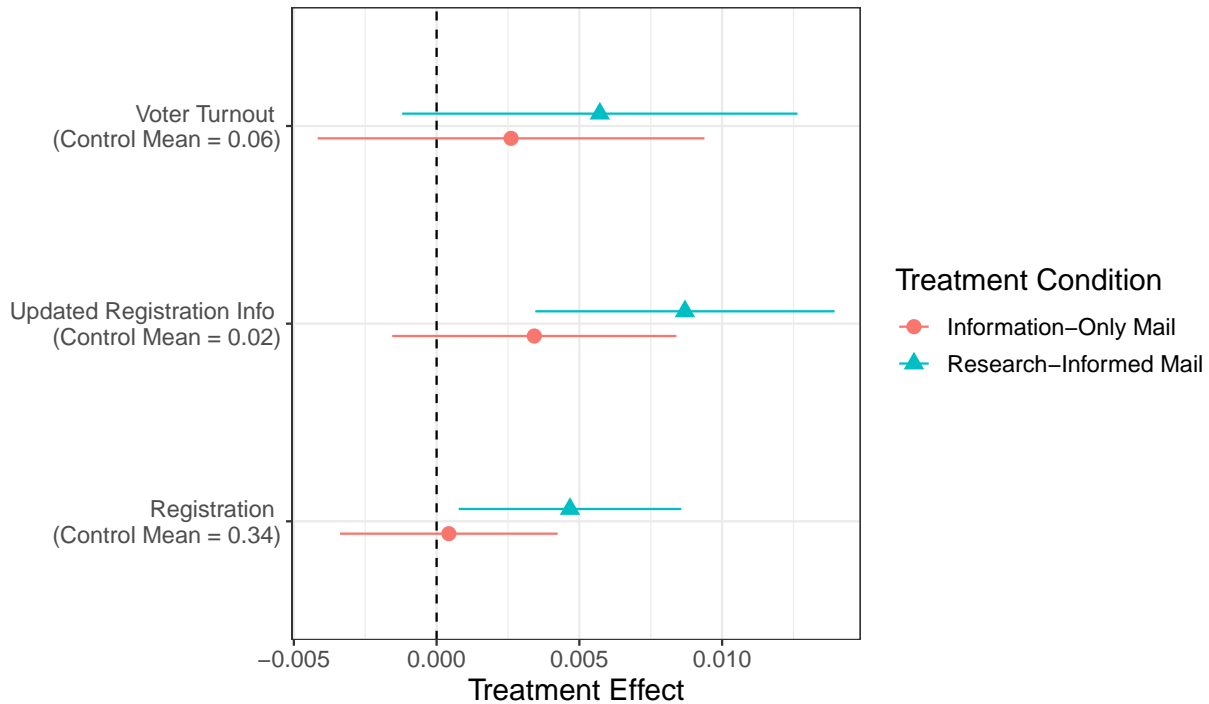


Figure 2: Treatment effects of both mailer types on voter registration and November 2021 voting

Findings

Table 1 and Figure 2 present the effects of each treatment arm on our outcomes of interest.¹⁵ The research-informed mailer had a substantial and statistically-significant effect on voter registration, increasing registration by .5 percentage points relative to control. In contrast, the effect of the information-only mailer on registration appears minuscule (less than one-tenth of a percentage point) and cannot be statistically distinguished from 0. The research-informed mailer's effect on registration is statistically distinguishable from the information-only mailer's effect, further demonstrating the importance of tailoring the message to the unique experiences of justice-impacted people.

Next, we turn to our measure of registration updates, which includes both individuals who

¹⁵All estimates presented in the main paper include an indicator for pre-treatment registration (whether the person already appeared on the voter file before the mailers were sent) due to chance imbalance on this highly-prognostic covariate; Table 5 in the SI presents the same estimates without any covariates and supports the same conclusions (and statistical significance is unchanged from the results shown in the main paper).

Table 1: Registration and Voting by Treatment Arm

	Voter Registration	Updated Registration Info	Nov 2021 Turnout
Information-Only Mail	0.000 (0.002)	0.003 (0.003)	0.003 (0.003)
Research-Informed Mail	0.005* (0.002)	0.009* (0.003)	0.006 (0.004)
Diff: Research-Informed - Info-Only	0.004* (0.002)	0.005 (0.003)	0.003 (0.004)
Num.Obs.	23 768	23 768	23 768
Control Mean	0.34	0.02	0.06

All models include pre-treatment registration status as a covariate

* $p < 0.05$

newly registered and those who updated information on existing registration records during the RCT period. On this outcome measure, the research-informed mailer increased (re)registration by .9 percentage points (statistically-significant), while the basic information-only mailer increased it more modestly by .3 percentage points (non-significant). With respect to new and updated registrations that occurred during the RCT period, the impact of the research informed mailer is substantively large, increasing this measure by 45% relative to the control. The difference between the research-informed and information-only mailers is just shy of conventional levels of significance ($p=.06$).

Estimates for November 2021 voter turnout show a similar pattern, though the estimates are noisier. The research-informed mailer appears to have increased turnout by .6 percentage points (a 10% increase over control), while the information-only mailer appears to have increased turnout by about .3 percentage points; neither estimate is statistically distinguishable from zero or from each other.

Effect Size

It is difficult to compare these effect sizes to the broader experimental literature on voter registration (or on registration of people with past criminal legal contact) given the limited number of

papers on these topics and the wide range of intervention styles and sample characteristics included therein. For example, Gerber et al. (2015) partnered with the Connecticut Secretary of State’s office to send official correspondence to unregistered people recently released from custody after a felony conviction assuring them of their right to vote and found registration effects of 1.8 percentage points and turnout effects of .9 percentage points during the 2012 presidential election cycle. Doleac et al. (2022) partnered with a non-profit organization to send out mailers to unregistered North Carolinians with past felony sentences during the 2020 presidential election, finding average effects on registration of .8 percentage points and on turnout of .5 percentage points. These previous studies found slightly larger effects than those reported here, particularly when focusing on our basic “information-only” mailer that is most comparable to the mailers used in those experiments. These studies differed from the present study on a variety of dimensions that may have implications for estimated effect size.

First, we note that the RCT pool for this project included some people who were already registered to vote, rather than subsetting to those without a voter record on file. This was a decision that helped ensure as many people as possible received information needed to help them cast a valid vote, given that some people’s registrations may have been out of date and needed updating. But it also limits registration effects mechanically, in that people who are already registered cannot be induced to register. If focusing on only unregistered people, as in previous studies, the registration effects of the intervention look larger. Table 2 presents registration and turnout effects among people who were not already registered to vote when the RCT mailers went out, finding substantially larger registration effects of .7 percentage points from the research-informed mailer and .3 percentage points (non-significant) for the information-only mailer.¹⁶ Comparatively, Ger-

¹⁶These analyses should be considered exploratory, as they were not pre-registered. We do not present the updated-registration outcome measure for this group because for previously-unregistered people, it is exactly the same as the voter-registration measure. We did, however, examine the moderating effects of pre-registration on the impact of the treatment on all three outcomes of interest. Evaluating the data in this way reveals that the impact of the research informed mailer was only statistically significant for registration and turnout among previously unregistered people. For those who were already registered, the mailers modestly increased the rate at which one updated their registration and ultimately voted, but these effects are not distinguishable from zero. We note that previously-registered people account for about a third of the overall sample, meaning that statistical power may be lower when examining this group. This analysis is located in Section B of the Supporting Information.

Table 2: Registration and Voting by Treatment Arm (Among those not already registered)

	Voter Registration	November 2021 Turnout
(Intercept)	0.018* (0.002)	0.003* (0.001)
Information-Only Mail	0.003 (0.003)	0.002 (0.001)
Research-Informed Mail	0.007* (0.003)	0.004* (0.001)
Num.Obs.	15 760	15 760

* $p < 0.05$

ber et al. (2015) evaluate the effects of their treatment among individuals who voted in earlier elections (which assumes they must have been registered even if later purged from the voter rolls due to a felony conviction) and those who were eligible but did not vote. The size of their effects are driven by those who previously voted. Among those who had not previously voted (perhaps more comparable to those not previously registered in our study), they improve registration by .7 percentage points, which is closer to the effect sizes in the present study.

Second, the present study entered a context in which the law had just changed: 2021 was the first election in which people on probation and parole in New Jersey could register and vote. As such, there was ongoing news coverage of the eligibility change, as well as other efforts by NJISJ and other organizations to ensure that newly-eligible people would be able to register and vote. Given this flurry of activity, it is perhaps especially likely (compared to past experimental contexts) that some people in the “uncontacted control” group nevertheless were encouraged to register and vote via other forms of contact, thus attenuating the treatment effects seen here.

Finally, we note the election context: Gerber et al. (2015) and Doleac et al. (2022) conducted their studies during high-salience presidential elections, while ours was conducted during an off-year gubernatorial election. This pattern is somewhat in contrast to voter mobilization RCT’s conducted with other populations, where those conducted during presidential elections are least likely to generate clear effects, and the quieter context of non-presidential elections minimizes contamination of the control group (Green and Gerber, 2019; García Bedolla and Michelson, 2012).

Instead, lower-salience elections may be more challenging for voter registration efforts, in contrast to get-out-the-vote efforts targeting already-registered voters. It is possible that registration efforts are more difficult when elections do not feel salient to the potential voters being contacted. Nevertheless, even under these challenging circumstances, where the traditional information-only mailer approach struggled relative to other contexts in which it has been used, we see substantial increases in registration and turnout from the qualitative-research-informed mailer.

Effect Heterogeneity

The pre-analysis plan for this project included examining effect heterogeneity by race and by type of system contact (probation or parole). We present both these analyses in section C of the Supporting Information; these are generally unable to statistically distinguish between treatment effects for different racial groups or those with different types of records. Although we examined heterogeneous effects among racial subgroups, we cannot offer firm conclusions with any kind of confidence, given the poor quality and coverage of available race data. Only one of the lists used for this project included data on race from administrative records, and imputation methods will often miss Black racial identity in heterogeneous neighborhoods.¹⁷ Moreover, the findings are sensitive to alternative coding schemes and modeling choices. We encourage future researchers on this topic to be attentive to the possibility of differential effects of interventions.

Discussion and Conclusion

We began by asking: under what conditions do justice-impacted people become active voters, and how can we craft an engagement strategy that speaks to the unique challenges faced after criminal legal entanglement? Our goals were two-fold. We wished to develop knowledge around how to channel political agency held by justice-impacted people and expressed through protesting and community organizing into electoral politics as well. On one hand, we endeavored to resolve a dis-

¹⁷Further, we note that even the administrative data we have on race may be based on government officials' perceptions of people's race, not their own self-identification.

connect in existing literature, which has observed (and theorized about) non-electoral engagement among justice-impacted people, but sees little hope for improved engagement with the state via voting. On the other hand, the current movement for rights restoration is led by justice-impacted people themselves, and we wished to develop a study with practical applications to their efforts. We focused our work in New Jersey, driven by an expansion of the franchise that occurred in 2019 as a consequence of organizing led by NJISJ. We began by consulting the expertise of justice-impacted people, which we then systematically assessed in the context of a randomized controlled trial (RCT).

Specifically, we began with a qualitative inquiry designed to understand the unique challenges to voting faced by justice-impacted people, and the strategies those already involved in registering this group in New Jersey felt were or could be effective. Participant observation in RACE Council meetings and in-depth interviews with justice-impacted people from across the state highlighted the importance of employing a trusted and credible messenger; developing group-based messages that tap into one's experiences as a justice-impacted person; and recovering the belief in the value of voting through casting it as one action available to justice-impacted people to forward their collective goals. Working with NJISJ and Ron Pierce, we helped develop a letter that told Mr. Pierce's story, was personally signed by him, and was threaded through with a story of injustice that connected contemporary disenfranchisement to historic efforts to marginalize Black and poor voters. We evaluated the effectiveness of a mailer that included that letter against a basic information-only version, akin to those employed by previous studies and against an uncontacted control group. Overall, we found that the research-informed mailer was more effective across the outcome measures examined.

This project contributes to a small but growing body of work that demonstrates that justice-impacted people are not lost to political life. On the contrary, because they are considered unlikely to vote they are neglected by campaigns and parties that traditionally engage in voter registration and turnout efforts. This widespread neglect means that any mobilization efforts targeted to this group have the potential to yield significant gains in terms of electoral expansion. From this study,

specifically, we have developed an understanding of the kind of outreach strategy that may be uniquely important to justice-impacted people. This project demonstrates that centering the voices of justice-impacted people when developing an outreach strategy is more effective than simply engaging traditional GOTV messaging and tactics. More to the point, justice-impacted people themselves are already engaged in removing barriers to their own participation, and in turn, they are uniquely effective at mobilizing other justice-impacted people into the electorate. Confirming the value of their work, qualitative research led us to conclude that employing a trusted, credible messenger when reaching out to justice-impacted people, delivering a message that casts voting as a collective act, points to past successes, and invokes group membership through connecting individual experiences to a larger collective struggle are likely to be particularly effective.

This project is not beyond critique. The letter we developed is a bundled treatment, including information about eligibility alongside a narrative of injustice and a recounting of the successful collective action efforts undertaken by NJISJ, and delivered by a trusted messenger. Centering concerns about identification and causal mechanisms might lead one to view this as a limitation of the project. Certainly, further research should engage questions of the importance of a trusted messenger relative to other aspects of the outreach strategy. However, the bundled treatment was developed from the expertise of justice-impacted people themselves. That is, the empirical strength of the paper is twofold. We deployed qualitative methods to build theory around how and when justice-impacted people exercise political agency, given the dearth of attention from political scientists on this topic, which we then tested through an RCT. That qualitative inquiry, where we sought the unique (and not often recognized) expertise of justice-impacted people themselves, led us to conclude that a multi-pronged message was both appropriate and akin to the kind of messaging that would have developed without researcher involvement.

Readers may likewise have questions about scope conditions. The project in New Jersey developed in the wake of a major campaign by NJISJ to restore voting rights to people with convictions. It is possible that the major change in the law, the media attention it drew, and the activism that followed combined to attenuate the effects of our efforts. At the same time, our partnerships with

NJISJ, a respected organization well known to justice-impacted people in the state, may have contributed to the strength of our findings and the credibility of the research-informed mailer. While we do not think that simply partnering with NJISJ itself drove the results, given that the comparison group also received an informational mailer with NJISJ's branding, it could be the case that we were especially effective in a context where activists undertook a concerted effort to bring awareness to the issues impacting people with felony convictions. However, these findings are consistent with those observed in North Carolina, where scholars partnered with an organization not strictly focused on issues related to criminal justice (Doleac et al., 2022); and Connecticut, where scholars partnered with a state agency (Gerber et al., 2015), even as the size of the effect of the basic informational mailer (most comparable to those used in previous studies) was smaller. We suspect the effectiveness of our outreach strategy is generalizable beyond this specific context. However, given the added uniqueness of New Jersey's electoral cycle, where major statewide elections are held in off years (only four states similarly hold such major elections), we think this is an important area for future research. We suspect that the effectiveness of our outreach strategy is generalizable beyond the context of New Jersey and the partnership with NJISJ, but this is an empirical question – one our project demonstrates is worth further attention and investment.

Although we set out to evaluate heterogeneous effects by race, we were unable to observe differences by racial group with any confidence, largely due to the poor quality of the data on race. This is a pressing issue for this line of research, because other studies have likewise found effects that suggest white individuals are more responsive to outreach efforts, even as people of color are disproportionately impacted by the system (Doleac et al., 2022). As such, it is paramount to understand how best to reach and mobilize Black and Latinx people with convictions. Researchers have further work to do in terms of developing strategies to overcome the limitations of administrative records, which make list-based field experiments like this one possible.

Finally, we have employed mailers to reach justice-impacted people. This makes sense insofar as we precisely identify unregistered individuals with convictions using administrative records, and we were able to reach a statewide list. This process would have been far more challenging

(and costly) using neighborhood-focused canvassing. At the same time, our mail intervention provides a hard test of the theoretical insight developed from the qualitative interviews. The trusted messenger, group-based approach developed here suggests that the most successful strategies to engage justice-impacted people are grounded in longer-term efforts at community organizing. We have tried to induce voting through a comparatively light touch mailer that incorporates a number of factors. That we did so successfully suggests the great potential of more intensive mobilization efforts.

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Supporting Information for “No Longer a Number”

A More RCT Detail

This section provides more detail about the mail study described in the main paper. Table 3 shows descriptive statistics for the sample by treatment arm. The rightmost column displays the p -value from a joint hypothesis test of no difference across the three arms of the study (control, information-only mailer, research-informed mailer).

Table 3: Covariate balance across treatment arms

	Control Mean	Info-Only Mail	Research-Informed Mail	Joint F-test p-val
Age	40.90	-0.04 (0.20)	0.06 (0.20)	0.90
Female	0.04	0.00 (0.01)	0.00 (0.01)	0.83
Probation	0.64	-0.00 (0.01)	0.00 (0.01)	0.95
White	0.66	-0.01 (0.01)	0.01 (0.01)	0.15
Latinx	0.17	0.01 (0.01)	0.00 (0.01)	0.61
Black	0.15	-0.00 (0.01)	-0.01 (0.01)	0.11
Already Registered	0.33	0.00 (0.01)	0.01 (0.01)	0.16
Observations	8838	7476	7454	

Table 4 reports estimates from a pooled analysis that combines both treatment groups and tests for differences between that “any-treatment” group and the control group in voter registration and turnout. Table 5 includes versions of the main analysis with and without the inclusion of an indicator for pre-experiment registration status.¹⁸

Section A.1 below includes the text of the two treatment letters sent as part of the study. Each letter also included a simple voter guide on the reverse with more details about how to register and/or verify eligibility, as well as a registration form. Section A.2 describes the process of putting together the mailing lists for the project and compares the study sample to the entire universe of

¹⁸As discussed below, an error in the initial merge code used by a partner organization meant that the study was not properly blocked on pre-existing registration as planned. This issue allowed for a small amount of imbalance on pre-existing registration, and thus we include pre-existing registration as a covariate in most analyses. Table 5 illustrates the reason for this analytical decision: if we do not include this extremely-prognostic covariate, even the small amount of chance imbalance observed across treatment conditions leads to inflated treatment effect estimates on registration. As shown in Table 3, the “research-informed mailer” group has slightly (1 percentage point) higher rates of previous registration than the other treatment groups, leading to higher estimates of the registration effect of this treatment in column 1 of Table 5 than in column 2 (again, about 1 percentage point higher).

Table 4: Pooling both treatment arms

	Voter Registration	Updated Registration Info	November 2021 Turnout
(Intercept)	0.019* (0.001)	0.016* (0.002)	0.002 (0.002)
Any Treatment Arm	0.003 (0.002)	0.006* (0.002)	0.004 (0.003)
Already Registered	0.973* (0.001)	0.026* (0.003)	0.162* (0.004)
Num.Obs.	23 768	23 768	23 768

* $p < 0.05$

Table 5: Registration and Voting by Treatment Arm

	Voter Reg	Voter Reg	Updated Reg	Updated Reg	Voted	Voted
(Intercept)	0.342* (0.005)	0.019* (0.001)	0.025* (0.002)	0.016* (0.002)	0.055* (0.002)	0.002 (0.002)
Info-Only Mail	0.002 (0.007)	0.000 (0.002)	0.003 (0.003)	0.003 (0.003)	0.003 (0.004)	0.003 (0.003)
Research-Informed Mail	0.018* (0.008)	0.005* (0.002)	0.009* (0.003)	0.009* (0.003)	0.008* (0.004)	0.006 (0.004)
Already Registered		0.973* (0.001)		0.026* (0.003)		0.162* (0.004)
Num.Obs.	23 768	23 768	23 768	23 768	23 768	23 768

* $p < 0.05$

re-enfranchised people. Section A.3 describes the process of merging the study sample to the voter file to measure treatment outcomes.

A.1 Mailer text



Dear [Insert Name],

Are you registered to vote? On **November 2**, the state legislature and the governor are up for re-election, as are many local officials. Don't forget to register, cast a ballot, and ensure your voice counts!

Your voice starts with your vote. Voting is one of the most important ways citizens have to make their voices heard. By taking the time to do their civic duty, voters ensure that elected leaders know what they think and how they feel. We encourage you to take the time to fulfill your civic duty by voting this November.

Use the following step-by-step instructions to register and vote. We have included a paper registration form with this letter, and the instructions on the back of this page explain how to fill it out as well as how to register online if you prefer. We have also answered commonly-asked questions about who is eligible to vote and what is needed to cast a ballot. If you have more questions, you can email vote@njisj.org or you can find more information at vote.nj.gov.

Sincerely,

New Jersey Institute for Social Justice



Dear [Insert Name],

My name is Ron Pierce, and I am the Democracy and Justice Fellow for the New Jersey Institute for Social Justice (the “Institute”). I am one of five children born to teenage parents. We grew up in housing projects and, eventually, my choices led me to be incarcerated for over thirty years – to become known as N.J State prisoner 210675. I write to you to share a bit of my story to help you understand the power your voice has in the communal chorus of your community.

My father taught me voting was our most important right and my duty to family and community. He taught me that voting has value to the soul and it brings a connectedness to it. After my conviction, I received a letter stating I could no longer vote. This disconnected me, not only from society, but from my family and community. The years I spent incarcerated reinforced my understanding of the importance of voting. While my father raised me to understand the importance of voting, I did not fully grasp the power of it until I took an NJ Step Political Science Class. This course focused on historical voter disenfranchisement of Black Americans. I learned felony disenfranchisement is not about the purity of the ballot, but a means to wrench power from marginalized communities, specifically Black communities. I learned not only about efforts to suppress the vote of Black people, but how it also affects generational disenfranchisement.

Even after being released from prison in 2016, I still did not have the right to vote because I was on parole. In 2018, the Institute began a campaign to restore the vote to people with criminal convictions. I explained to lawmakers and the public how essential the right is. Incarcerated individuals also lent their voices for our report, *Value to the Soul: People with Convictions on the Power of the Vote*. As one incarcerated person said: “I have come to understand the importance of my vote through my children’s eyes, to know if I do not get to choose who represents me, someone else makes that choice and my representatives owe their allegiance to them.”

Returning citizens gave commanding testimony to the legislature about their experiences and how they wanted to use their votes to help their communities. Lawmakers do not always do things because it is the right thing to do, they do things when feeling pressure from the community. Hearing from returning citizens, their families and community members was key. Our efforts were too much for those in power to ignore and in December 2019, lawmakers restored the right to vote for people on probation and parole, enfranchising 83,000 people.

Now, in New Jersey, unless you are currently incarcerated for an indictable offense, every citizen has the right to vote. Lawmakers cannot ignore us. Our votes matter. We can use this power to fight for things that are important to our communities – jobs, better schools, changing our criminal justice system and much more. The election on **November 2** is an opportunity to make our voices heard in a big way. The Governor and all state legislators are up for election, as are many local officials who can impact things like policing and access to public housing. A vote in November is a vote on improving the lives of incarcerated people, formerly incarcerated people and their communities.

I urge you to register and vote. This right is precious. Join me in using this power to help make this state better. We fought for the right to vote for people with felony convictions. When we fight together, we win. Now it is time for us to use our political voices to achieve a more just world. Any questions, email vote@njisj.org or contact your county: vote.nj.gov.

Honor & Loyalty,
Ron Pierce

A.2 Building mailing lists

This section discusses the mailing lists built for the RCT described in the main paper and how they relate to the full universe of re-enfranchised people in New Jersey.

In 2019, New Jersey’s governor signed a bill into law that would restore voting rights to people on parole or probation in New Jersey. At the time, the governor said the bill would re-enfranchise “over 80,000 residents on probation or parole.”¹⁹. We have been unable to find the exact source of this number used by the governor, but we note that a BJS publication about supervision counts as of January 1 2020²⁰ listed the state as having 15,194 people on parole and 135,020 on probation; assuming 39% of people on probation were there for felony-level offenses (as is BJS’ national estimate) yields a felony probation estimate of 52,658, suggesting a total re-enfranchised population of $15,194 + 52,658 = 67,852$, not far short of Gov. Murphy’s statement.²¹

We sought out information about people on both probation and parole via records requests to state agencies in early 2021, then worked with a commercial data vendor to append current mailing address for as many people as possible. Finding parole records was relatively straightforward: we requested data from the state parole board on everyone currently supervised by that board, and received a list in January 2021 containing 15,812 records (similar to estimates of the state’s full parole population). We removed apparent duplicate observations and those with extreme missingness to produce a list of 15,782 people for whom we sought mailing addresses. In April 2021, our data vendor returned a list of 9,038 mailing addresses, covering over 57% of the list we had sent them. We used 372 of these addresses for a springtime pilot to test out mailing logistics. In summer 2021, while preparing for our main RCT, we asked the data vendor to update the remaining list of 8,666 addresses to reflect any residential moves that had occurred between April and July. The vendor updated several thousand of the addresses (reflecting relatively high rates of residential mobility among this group) and also flagged several hundred people whose primary addresses appeared to be out of state; we removed cases with out-of-state addresses and were left with a list of 8,394 addresses for the fall 2021 study. To this list we added a small list of new records based on an updated parole records request we filed in July 2021: using these updated parole records, we identified an additional 758 people we thought had been newly added to the parole rolls since our January request, and sent them to the data vendor for matching. The vendor found addresses for 431 of them, leaving us with a total list of 8,825 addresses for people on parole.

Finding people on probation was a more challenging process, as probation is administered through the state courts system rather than a separate agency and we were not able to request and receive a list of people currently on probation. Instead, we had to request years of sentencing records and use them to construct a list of people likely to still be on probation (or to have recently finished it) as well as not currently incarcerated. In building this list, we made some relatively conservative choices, such as removing any observations that appeared to be duplicates or where we thought someone might still be serving a sentence of incarceration, as well as focusing on people sentenced in recent years (between 2015 and March 2020, when the law changed). These data limitations and this cautious approach meant that we could not reconstruct a full list of the

¹⁹<https://www.cnn.com/2019/12/19/politics/new-jersey-voting-rights-felons-phil-murphy/index.html>

²⁰<https://bjs.ojp.gov/content/pub/pdf/ppus20.pdf>

²¹Further, NJ’s supervision populations have dropped dramatically in recent years, so using several-year-old numbers to do this calculation would likely yield a number much closer to Murphy’s guess.

upwards of 50,000 people who were likely on probation and newly-reenfranchised; our cautiously-constructed list of eligible people on probation contained 22,219 records. Our data vendor was able to match 15,176 of them (over two-thirds) to mailing addresses.

Across the parole and probation system, we were able to identify nearly 39,000 people who had recently regained the right to vote in New Jersey, likely over half of the entire re-enfranchised population. We were able to find mailing addresses for over 24,000 of them, a substantial fraction of the residents who had recently regained the right to vote.²² In contexts where state agencies were more cooperative about sharing probation records, or where a funder were prepared to send many mailers to people who might not yet be eligible to vote, this approach could likely reach an even larger share of this population.

Though we have limited demographic data available to assess representativeness, we note that available evidence suggests that our final sample (of people with commercially-available mailing addresses) looks quite demographically similar to the full list of people we were trying to contact. The probation/sentencing data has almost no demographic information, so we focus here on parole data. The set of 15,782 people we sent to the vendor for address matching was 96.6% male, 29.5% Black, and 28.3% Hispanic according to the fields provided in the parole records.²³ The set of 9,038 people for whom the vendor was able to provide addresses are relatively similar in composition: 96.1% male, 31.6% Black, 18.8% Hispanic/Latino. It is certainly possible that the final sample differs on unobservable characteristics (such as residential mobility) from the full targeted list, but we note that available variables look similar.

A.3 Merging to the voter file

The outcome measures for the mailer study are drawn from the New Jersey voter file, which lists all registered voters and also includes records of their voter turnout in recent elections. For this paper, we merge the RCT dataset with three different snapshots of the voter file, following the same merge procedure in all three cases. The three snapshots are as follows:

1. **Pre-study snapshot:** First, we use a snapshot of the voter file from August 2021, just before our study was fielded, to measure pre-treatment registration.²⁴
2. **Election snapshot:** Second, we use a snapshot of the voter file from just before Election 2021 (October 26, 2021) to observe which voters were registered in time to vote in the 2021 New Jersey general election. Given the voter registration deadlines in New Jersey, this snapshot should include anyone who was eligible to cast a ballot in November 2021. We use this snapshot for our registration and updated-registration outcomes.
3. **Post-election snapshot:** Finally, we use a snapshot of the voter file collected in spring 2022 (May 2022), after vote history information had been updated on the voter file, to observe

²²This number includes the 372 addresses used in the spring pilot, in addition to the 23,768 included in the main study.

²³As discussed elsewhere in the paper, there is a great deal of missingness on race in these records. The figure of 29.5% Black reported here is calculated based on the full list, not only people who have race information included; this is why it seems so low relative to the racial composition of NJ's criminal legal system.

²⁴As noted in Section D below, this measure was initially constructed inaccurately by a project partner and thus we have sourced an old voter snapshot from the state to get a complete picture of pre-treatment registration.

voter turnout in the 2021 general election. We use this snapshot for our voter-turnout outcome.

We link all voter-file snapshots to the experimental dataset using the same approach, which relies on both names and dates of birth but allows for some variation in names across datasets to ensure that small transcription differences do not result in missed matches.²⁵ First, the voter file snapshot and study data are merged solely on dates of birth, requiring an exact match on this field. Then, the many resulting potential matches are pared down based on the similarity of the first-name and last-name fields across the datasets, using the `stringdist()` package in R (van der Loo, 2014).²⁶ Finally, merges with conflicting middle names or initials (where both datasets have some middle-name information) are discarded.

This approach finds matches that might have otherwise been missed by an exact-name-match approach, either because of transcription differences across the datasets (“O’BRIEN”/ “OBRIEN”/ “O BRIEN”) or because of slight differences in name usage across fields (“CHRISTOPHER”/ “CHRIS”). However, it does not appear to introduce many false-positive matches. We follow Meredith and Morse (2015) in performing a birthdate-permutation test to get a sense of false positive match rates. We permute the dates of birth in the study data by adding 35 days to all of them, and then follow the same voter-file merge approach described above using real names and these permuted dates of birth. The intuition of this test is that most of these permuted records do not correspond to any real person in New Jersey, so any matches found via this approach should be considered false positives and can give an idea of the false-positive match rate for this approach. This permutation test finds relatively few matches: for the match to the post-election snapshot, for example, we find just 20 matches to the voter file from the permuted dataset, or a match rate of less than one-tenth of one percent.

The “updated-registration” outcome measure used in the paper is intended to capture any updates to a voter’s registration record during the study period. Unlike the “registration” measure, which could only change for people who were unregistered before the study, this measure captures any updates to a registration and thus can change both for people who were registered or unregistered prior to the study. It is constructed by comparing voter records from the pre-experimental snapshot and the election snapshot: for everyone registered in time to vote on election day 2021, we go back and compare their voting record from that snapshot to their record in the pre-experimental snapshot. Anyone who has newly registered between those two snapshots is recorded as having an “updated registration,” as is anyone who saw changes to key registration fields between the two snapshots: first or last name, street number or street name, or party registration.

B Effects by Registration Status

²⁵Other approaches, such as FastLink’s probabilistic approach or a simpler manual merge approach that requires exact matches on DOB and name, yielded slightly higher or lower total counts of registrants but similar estimates of the treatment effects; as described below, we use this approach because it appears to reduce false negatives relative to a strict-name-match approach, without introducing as many false positives as more probabilistic approaches did.

²⁶We rely here on the Jaro-Winkler metric, which places extra emphasis on similarity between the beginning of strings (making it good for comparing names), and use a threshold of .15 to identify reasonable matches (based on a manual inspection of some matched names).

Table 6: Registration and Voting by Treatment Arm and Prior Registration

	Registration	Updated Registration	Turnout
(Intercept)	0.018* (0.002)	0.017* (0.002)	0.003* (0.001)
Information-Only Mail	0.003 (0.003)	0.003 (0.003)	0.002 (0.001)
Research-Informed Mail	0.007* (0.003)	0.007* (0.003)	0.004* (0.001)
Already Registered	0.977* (0.002)	0.023* (0.004)	0.159* (0.007)
Information-Only x Already Registered	-0.006 (0.004)	0.003 (0.006)	0.003 (0.010)
Research-Informed x Already Registered	-0.006 (0.003)	0.006 (0.006)	0.006 (0.010)
Num.Obs.	23 768	23 768	23 768

* $p < 0.05$

C Effect Heterogeneity

The pre-analysis plan for this project included examining effect heterogeneity by race and by type of system contact (probation or parole).

Racial classification The administrative records used for this project contained many odd or missing values in the race field²⁷, so we present a version of the analysis based on modeled race using name and census geography (using the “WRU” package of Imai and Khanna (2016)). This approach is imperfect, but it provides more complete coverage than the government records (which also may not correspond to people’s racial self-identification). However, it also struggles to distinguish between Black and white people who live in similar areas and do not have racially-distinctive names. For example, of 2623 people in our sample listed as Black or African American in the parole records, our modeled-race approach only identified 1007 of them as Black (most of the remainder were classified as white). As such, we present heterogeneity analyses below using three different approaches to measuring race, each of which has some problems.

Racial heterogeneity estimates Table 7 presents estimates of treatment effects on voter registration by race, relying on the modeled approach (using the `wru()` package) described above. The first three columns present estimates for people classified as white, Latinx, or Black, while the fourth column combines all non-white people from the sample. The final column presents estimates that interact treatment indicators with an indicator for “non-white” to see whether the study treatments appear to have a larger or smaller effect among non-white individuals. Table 8 presents equivalent estimates for effects on voting in the 2021 election. Together, these tables present a mixed pic-

²⁷Only the parole data we obtained (not probation) contained race fields.

ture, with apparently-smaller registration effects of both treatment arms among non-white people but a split on voter turnout effects (one treatment arm apparently working better among non-white people and one working worse). None of these differences are statistically distinguishable from 0. We note that (as mentioned above) this group of people who are classified as non-white is likely missing many people who are wrongly being included in the "white" group in ways that could bias the estimates presented.

Given concerns that our race-modeling approach could be misclassifying many non-white individuals who do not have racially distinctive names,²⁸ we also present heterogeneity estimates using two other ways of measuring race. Our first alternative measure relies on a combination of modeled race and administrative data where administrative data on race is available: if both sources indicate that a person is white, then they are included as white, but if either source indicates a person belongs to some other group, we rely on that classification. This "backstopped-with-administrative-data" approach finds more people of color in the sample: 9,759 people are counted as nonwhite in this measure, compared to 8,034 under the modeled approach. However, this approach also carries some strange compositional patterns: because we only have administrative data on race from parole records, people with parole histories are more likely to be classified as people of color than those with probation histories, making the comparison of treatment effects by race one that now also carries with it a comparison across program types. Nevertheless, we present these estimates in Table 9, with column 1 presenting voter registration effects and column 2 presenting turnout effects. The point estimates on the interactions between treatment indicators and "non-white" suggest that our mailers might be less effective among people of color, especially in stimulating voter turnout (though they vary in significance).

Finally, we present a version of these estimates that relies solely on administrative data on race, which is only available for people with parole records (and is missing even for some of them). Table 10 presents these estimates, which are based on a sample of only 7,041 people. These estimates suggest an even more pronounced difference in mailer effectiveness by race, with non-white people showing treatment effects several percentage points smaller than those for white people (implying null or in some cases even negative effects among non-white people), though these interactions again vary in statistical significance.

We have presented several sets of estimates of racial heterogeneity in treatment effects, each with caution given the drawbacks of each of these approaches to racial classification. We are hesitant to draw firm conclusions about the effectiveness of these treatments among non-white New Jerseyans with past criminal legal contact, but we note that the most complete records we have (the parole data) indicate concerning differences in effectiveness by race, and we encourage future researchers to be attentive to this possibility.

Effect heterogeneity by record source Our pre-analysis plan also indicated that we would present estimates of effect heterogeneity by list source: that is, whether people entered the study sample by way of parole or probation records.²⁹ Table 11 presents these estimates for voter registration (column 1) and voter turnout (column 2). The interactions suggest that both treatment

²⁸We note, for example, that the race-modeling approach only identifies 3,456 people in our sample as Black, an astonishingly low share given the rate of Black overrepresentation in New Jersey's criminal legal system.

²⁹A small number of people in the sample appeared in both probation and parole records; most of those people will be grouped with the parole list because they will have entered the sample via parole records (since we received those records first, and excluded apparent duplicates from the probation records when they arrived).

Table 7: Registration Effects by Race

	White	Latinx	Black	All nonwhite	All (Interacted)
(Intercept)	0.021*	0.016*	0.015*	0.016*	0.020*
	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)
Information-Only Mail	0.002	-0.005	0.002	-0.002	0.002
	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)
Research-Informed Mail	0.005*	0.003	0.003	0.004	0.005*
	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)
Already Registered	0.969*	0.980*	0.976*	0.979*	0.972*
	(0.002)	(0.003)	(0.004)	(0.002)	(0.001)
Nonwhite					-0.003
					(0.003)
Information-Only Mail * Nonwhite					-0.004
					(0.004)
Research-Informed Mail * Nonwhite					-0.002
					(0.004)
Num.Obs.	15 734	4177	3456	8027	23 761

* $p < 0.05$

Table 8: Turnout Effects by Race (November 2021 Election)

	White	Latinx	Black	All nonwhite	All (Interacted)
(Intercept)	0.003	-0.003	0.001	-0.002	0.013*
	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)
Information-Only Mail	0.001	0.007	0.002	0.007	0.001
	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)
Research-Informed Mail	0.006	0.011	-0.004	0.004	0.007
	(0.005)	(0.006)	(0.007)	(0.005)	(0.005)
Already Registered	0.186*	0.097*	0.107*	0.104*	0.160*
	(0.005)	(0.009)	(0.009)	(0.006)	(0.004)
Nonwhite					-0.031*
					(0.004)
Information-Only Mail * Nonwhite					0.005
					(0.007)
Research-Informed Mail * Nonwhite					-0.004
					(0.007)
Num.Obs.	15 734	4177	3456	8027	23 761

* $p < 0.05$

Table 9: Registration and Turnout Effects by Race (alternative measure)

	Voter Registration	November 2021 Turnout
(Intercept)	0.020*	0.013*
	(0.002)	(0.003)
Information-Only Mail	0.004	0.004
	(0.003)	(0.005)
Research-Informed Mail	0.007*	0.011*
	(0.003)	(0.005)
nonwhite_alt	0.000	-0.025*
	(0.003)	(0.004)
Already Registered	0.972*	0.160*
	(0.001)	(0.004)
Information-Only Mail * nonwhite_alt	-0.009*	-0.003
	(0.004)	(0.007)
Research-Informed Mail * nonwhite_alt	-0.006	-0.014*
	(0.004)	(0.007)
Num.Obs.	23 761	23 761

* $p < 0.05$

Table 10: Registration and Turnout Effects by Race (alternative measure, using parole data only)

	Voter Registration	November 2021 Turnout
(Intercept)	0.019*	0.038*
	(0.004)	(0.009)
Information-Only Mail	0.011	0.025
	(0.007)	(0.014)
Research-Informed Mail	0.015*	0.030*
	(0.006)	(0.015)
nonwhite_alt2	0.003	-0.053*
	(0.005)	(0.011)
Already Registered	0.970*	0.209*
	(0.003)	(0.008)
assign_treatInformation-Only Mail:nonwhite_alt2	-0.017*	-0.029
	(0.008)	(0.016)
assign_treatResearch-Informed Mail:nonwhite_alt2	-0.012	-0.035*
	(0.008)	(0.017)
Num.Obs.	7041	7041

* $p < 0.05$

Table 11: Registration and Turnout Effects by Program Contact

	Voter Registration	Updated Registration	November 2021 Turnout
(Intercept)	0.021* (0.002)	0.020* (0.003)	0.015* (0.004)
Information-Only Mail	-0.001 (0.004)	0.003 (0.004)	0.007 (0.006)
Research-Informed Mail	0.005 (0.004)	0.005 (0.005)	0.010 (0.006)
Probation	-0.003 (0.003)	-0.006 (0.004)	-0.020* (0.005)
Already Registered	0.973* (0.001)	0.026* (0.003)	0.162* (0.004)
Information-Only Mail * Probation	0.002 (0.004)	0.000 (0.005)	-0.007 (0.008)
Research-Informed Mail * Probation	-0.001 (0.004)	0.006 (0.006)	-0.006 (0.008)
Num.Obs.	23 768	23 768	23 768

* $p < 0.05$

arms may have been more effective at encouraging people from the parole list to vote than among people on the probation list, though none of these coefficients are distinguishable from zero.

D Pre-Analysis Plan

This appendix includes a copy of the pre-analysis plan we filed when pre-registering this study with the AEA.

Here, we also note the few places we depart from this pre-analysis plan:

1. The PAP says that outcome variables will be drawn from “Targetsmart’s copy of the NJ voter file”; we found it easier to simply request a copy of the voter file directly from the state of New Jersey, so we instead draw outcomes from the state’s copy of the voter file as detailed in Section A.3.
2. The PAP states (page 4) that if a large fraction of our mailers do not land, we will construct CACE estimates using two-stage least squares. As noted in the paper, mail delivery rates were extremely high in this study so we do not conduct this analysis.
3. Due to an error in the voter-file merge code used by a partner organization, the measure of pre-treatment registration status used as a covariate for blocked random assignment was incomplete (some people who were registered to vote prior to the study were not recorded as such at the time random assignment was conducted). The incompleteness of this variable should not impact the validity of the study, but for any analyses that consider prior registration we substitute in a newly-constructed measure of pre-treatment registration status.³⁰ Given some chance imbalance on this variable (and how predictive previous registration is of current registration), we include pre-treatment registration as a covariate in our main analyses. See Table 5 for versions of the main analyses without this covariate included.

³⁰We produce this variable by requesting a snapshot of the NJ voter file from August 2021, before our mailers went out, and merging it into our main study dataset using the same merge approach as we use for the post-experimental voter file snapshot when constructing our main outcome variables. See Section A.3 above for merge details.

A Field Experiment Encouraging People on Probation or Parole to Register and Vote in New Jersey

October 2021

1 The Project

We are partnering with the New Jersey Institute for Social Justice to measure the effectiveness of an intervention that will contact people who have experienced contact with the criminal legal system and encourage them to register and vote.

2 Experimental Design

This experiment builds on a qualitative research process in which the academic team observed meetings that NJISJ held about civic engagement efforts and interviewed people with past convictions about their political attitudes and engagement as well as their efforts to mobilize others. This process yielded some insights about what might help encourage people to register and vote, and we seek to test those with this experiment.

This experiment will be run in fall 2021, with the goal of encouraging people to register and vote in the November 2021 (municipal and state-level) elections in New Jersey. The sample includes people who are currently or have recently been on supervision (probation or parole) in NJ, since NJ has recently restored the right to vote to people on supervision and NJISJ is interested in finding and informing people about that right. The sample was constructed through the following process:

1. *Find lists of people on supervision.* The researchers filed public records requests for lists of people on parole or sentenced to probation in recent years, receiving lists of people's names and dates of birth from the relevant offices in NJ. We removed records missing information in key fields (names, dates of birth), those we believed to be ineligible to vote due to additional incarcerations, as well as people who had been included in a small spring pilot mailing used to test our mail setup. We also removed apparently duplicated observations.
2. *Match names/DOBs to commercial addresses.* We then contracted with a commercial address vendor to have them find current addresses for people on the list from Step 1,

using names, dates of birth, and (where available from sentencing data) prior addresses. They were able to find current New Jersey addresses for over half the list we sent them.

This process yielded a sample of 23,768 people with current addresses in NJ who are currently or have recently been on supervision (probation or parole) in NJ.

People in the sample have been randomly assigned to one of three conditions: an uncontacted control group, a “basic informational” mailer treatment, and a “personal appeal” mailer treatment. The uncontacted control group will not be sent any mail at all. The “basic informational mailer” group will be sent mail with information about eligibility to register, steps to do so, and a paper copy of the state’s registration form. The “personal appeal” mailer will include all the information of the basic mailer, but will also include a letter from an NJISJ staff member who has experienced incarceration and supervision and who helped lead NJISJ’s advocacy for re-enfranchising people who are on supervision in the state. This letter will include themes identified from our qualitative research: a credible messenger who shares experiences with the person receiving the letter, a focus on the impact of political action (highlighting NJISJ’s success in restoring the right to vote and the centrality of affected people in that success), and a personalized appeal to get involved in issues that matter to you.

Treatment assignment was conducted via blocked random assignment, with blocks constructed based on age, gender, race, data source, match status to the voter file, and voter registration status. Treatment probabilities are the same across all blocks. There are a small number of people on our list who live at the same address as someone else on the list. We constrain households to fall into the same treatment group so different people at the same address do not receive different mailers. Our analyses, as discussed below, will take into account this clustering of treatment assignment. This preregistration document is being filed after treatment assignment has occurred and mailers sent out but before outcome measures have been collected.

3 Hypotheses

First, we expect that our mailings will have a positive effect on voter registration and turnout: that is, each treatment arm, or both treatment arms combined, will have higher levels of registration and 2021 turnout than the control group.

Second, we hypothesize that the “personal appeal” mailer will have a larger mobilizing effect than the “basic informational” mailer, though we are only statistically powered to

detect relatively large differences in these treatment effects.

It is also possible that there could be heterogeneity in these treatment effects along characteristics like race or program involvement (probation versus parole), but we do not have such clear directional hypotheses for these possibilities. In the case of race, for example, we could imagine that the personal-appeal letter’s focus on the injustice of the criminal legal system could resonate more with people of color and drive larger treatment effects among them. But we could also imagine that there would be smaller treatment effects among people of color because of higher barriers to voting or possible differences in compliance (if address data were lower-quality in minority neighborhoods, for example, and more people in our sample did not receive the mailers). We describe possible approaches to exploring effect heterogeneity for a few key variables below, but we do not register directional hypotheses.

4 Analysis

1 Outcome Measures

We will focus on two main outcome measures: voter registration and turnout. Both will come from Targetsmart’s copy of the NJ voter file, collected after the 2021 statewide elections (once vote history has fully updated). Because our sample includes people who may already be registered, we will measure registration two ways. First, we will simply look at the share of each group that is registered to vote. Second, provided the voter data allows us to see this, we will look at registration changes/updates: the share of people who either newly registered or newly updated their registration between our mailer delivery date and the election. Our measure of turnout will be constructed unconditional on registration, and will simply measure whether the person voted in the November 2021 election; people not matched to a voter registration record will be treated as non-voters.

2 Main Comparisons

Our main analyses will focus on the two hypotheses specified above. First, we will test whether our mailers worked compared to the no-contact control condition: we will compare registration and turnout rates for each treatment arm, as well as the two treatment arms combined, to the uncontacted control arm. We will present the simplest possible regression specification, and then may also report a specification that incorporates pretreatment covari-

ates as available in order to improve precision.¹ In all analyses, we will adjust the standard errors to account for the clustering of treatment assignment at the household level. We will then compare the treatment effects of the two different arms to see whether the personal appeal mailer has a larger effect than the simpler basic-informational mailer as hypothesized above.

3 Extensions

Heterogeneity As noted above, we have limited information about the people in the study sample, and we want to avoid multiple testing that could lead to a high false discovery rate. However, we think that consumers of our research will naturally wonder whether treatment effects differ for particular groups in the population. So we are prespecifying a few variables we will use to look for effect heterogeneity, with the understanding that we have limited power for these interactive models and that these measures may also be imperfect. The two characteristics we will focus on when looking for effect heterogeneity are program involvement (did the person come to the sample due to their involvement with probation or with parole) and race. We will proceed with caution particularly when presenting analyses by race, since there is substantial missingness on this variable in the administrative records we are using and we will supplement them by imputing race with the `WRU()` package in R.

Non-compliance We are also using USPS mail tracking to see whether many of the mailers sent out “bounce” or are returned as undeliverable. Our main analyses are intent-to-treat estimates based on treatment assignment, but if a large fraction of our mailers do not land, we will also construct CACE (complier average causal effect) estimates using two-stage least squares.²

¹We note, of course, that having blocked on the pretreatment covariates listed above means that such a specification is unlikely to be much more precise than the specification without prognostic covariates.

²We note that bounced mailers are only one possible form of noncompliance and that we cannot observe whether mailers reach the specific people to whom they are addressed or whether they are actually read. Nevertheless, mail tracking provides us some information about a particular way that people may not receive the treatment messages as assigned.