Restoring Rights, Restoring Trust:
Evidence that Reversing Felony Disenfranchisement Penalties Increases Both Trust and Cooperation with Government

Victoria Shineman
Assistant Professor
Department of Political Science
University of Pittsburgh
shineman@pitt.edu

Abstract: More than six million American citizens were denied the right to vote in the 2016 Presidential Election because they had been convicted of a felony crime. Beyond the effects of these laws on voter turnout and electoral outcomes, how do felony disenfranchisement laws affect the citizens who are being disenfranchised? This study estimates the effects of restoring voting rights on the level of political trust among citizens who were formerly disenfranchised. Two field experiments are embedded within panel surveys conducted before and after statewide elections in Ohio and Virginia. The survey population is composed of American citizens with a felony conviction who were once disenfranchised, but now are either eligible to vote, or are eligible to have their voting rights restored. Both experiments leverage misinformation about voting rights policies as an opportunity to estimate how people respond when they receive new information about more lenient voting rights restoration policies. Experimental treatments randomly increase awareness about restored voting rights, along with varying encouragements and assistance with registration and voting. In both experiments, treated subjects report stronger trust in government and the criminal justice system, perceive government as being more fair and representative, and report an increased willingness to cooperate with law enforcement. The results suggest that restoring voting rights to disenfranchised citizens helps those citizens develop the types of pro-democratic attitudes commonly associated with successful post-prison re-entry, reduced tendencies to commit crime, and lower rates of recidivism.

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More than six million American citizens were denied the right to vote in the 2016 Presidential Election because they had been convicted of a felony crime. Forty-eight states (plus the District of Columbia) do not allow American citizens to vote while they are in prison, and more than thirty states continue to disenfranchise citizens after they have been released from prison. Some of those states automatically restore voting rights after citizens complete parole and/or probationary periods. In other states, citizens must also pay off all legal financial obligations (including court fines and fees) before their right to vote is restored. In three states – Iowa, Virginia, and Kentucky – a citizen with a felony conviction is permanently disenfranchised for the rest of their life, unless the Governor of their state intervenes on their behalf. In an international comparison of criminal disenfranchisement policies, Ewald and Rottinghaus conclude that “the United States is almost certainly the only country in the world that disenfranchises a significant number of people who are either no longer incarcerated or were never in prison at all” (2009; p 9-10).

A large literature has explored many causes and consequences of felony disenfranchisement policies. Previous research has explored the possible racialized motivations behind crime-based disenfranchisement (Behrens, Uggen, and Manza 2003; Ewald and Rottinghaus 2009), the disproportionate number of non-white citizens who are disenfranchised (Manza and Uggen 2008; Uggen, Larson, and Shannon 2016), the elements affecting public opinion on these laws (Pinaire, Heumann, and Bilotta 2002; Manza, Brooks, and Uggen 2004; Manza and Uggen 2008; Wilson, Owens, and Davis 2015; Shineman 2020a), the effects of disenfranchisement policies on voter registration and turnout (Meredith and Morse 2014; Meredith and Morse 2015; Gerber et al 2015; Hjalmarssson and Lopez 2010; Haselswerdt 2009;
Miles 2004), and the effects these laws might be having on electoral outcomes (Uggen and Manza 2002; Manza and Uggen 2008; Burch 2011).

Another important question to ask is: How do felony disenfranchisement laws affect the citizens who are being disenfranchised? Substantial evidence suggests that the experience of being incarcerated generates a number of unintended negative consequences – including increased levels of stress, negative effects on physical and mental health, and weakened social bonds (Wildeman and Wang 2017; Sugie and Turney 2017). Other stages of carceral interaction – including police stops, arrest, and conviction – have also been linked to detrimental effects on mental health and social attitudes (Lerman and Weaver 2014). This study builds on prior research about the effects of carceral experiences and punishments, to estimate the effects of a civil penalty commonly levied within the American criminal justice system – namely, the effects of electoral disenfranchisement on the citizens who are being disenfranchised. More specifically, this study estimates the effects of restoring voting rights on the level of political trust among citizens who were formerly disenfranchised.

There are a number of reasons to expect that being disenfranchised would cause a citizen to develop lower trust in government, and that restoring voting rights would cause pro-social and pro-democratic attitudes like political trust to increase – even if newly eligible citizens choose not to vote. Qualitative interviews reveal numerous instances where individuals report that disenfranchisement makes them feel excluded, isolated, discouraged, and as if they are not treated like full citizens (Cardinale 2004; Manza and Uggen 2008; Uggen, Manza, and Behrens 2002; Pinkhard 2013; Miller and Spillane 2012; Miller and Agnich 2016). The stigma generated by electoral disenfranchisement has been theorized as one of many elements that can make it
difficult for a formerly incarcerated individual to develop a positive pro-social self-identity and fully reintegrate into society after being released from prison.

The anecdotal evidence is rich. However, empirically estimating the effects of disenfranchisement (or the effects of restoring voting rights) is a challenging task. Disenfranchisement and restoration policies are both affected by the severity of the crime committed, the severity of the sentence issued, the citizen’s progress through the criminal justice system, and in some cases – also by whether the citizen applies to have their rights restored and whether the Governor approves the request. None of these elements are random. Thus the population of citizens who are disenfranchised is very different from the population of citizens who are not. Similarly, formerly disenfranchised citizens who have their rights restored are different from the citizens who remain disenfranchised. Although numerous theorists, policy-makers, and legal scholars have proposed that the relationship between voting rights and political trust exists – no empirical study has properly identified and estimated the effects of voting rights on pro-social and pro-democratic attitudes. This study seeks to fill that gap.

This study includes two field experiment embedded within panel surveys conducted before and after statewide elections in Ohio and Virginia. All subjects in both experiments are American citizens with a felony conviction who were once disenfranchised, but now are either eligible to vote or are eligible to have their voting rights restored.

Because voting rights policies vary so widely between the states (and also change frequently within states), there is a high degree of misinformation about eligibility (Drucker and Barreras 2005; Ewald 2005; Manza and Uggen 2008; Allen 2011). Some people think they have the right to vote when in fact they are legally disenfranchised. And there are many other citizens who believe they are disenfranchised, when in fact their right to vote has been restored. This
study selects two cases where the latter type of misinformation is likely to be high – where many people will have the right to vote, but will be unaware that their voting rights have been restored. The design leverages that misinformation as an opportunity to study how providing new information about more lenient voting rights policies affects the citizens whose voting rights are positively affected by such laws.

In both experiments, subjects are randomly assigned to receive varying levels of information about voting rights policies, encouragements to participate, and assistance with the restoration of voting rights and voter registration. By randomly increasing awareness about voting rights among some subjects, the experiments are able to estimate how citizens change when information about more lenient voting rights policies is provided to them.

As a quick preview of the results, both experiments find that treated subjects report stronger trust in government and the criminal justice system, are more likely to perceive government as being fair and representative, and report an increased willingness to cooperate with law enforcement. A big upshot of this result is that these pro-social and pro-democratic attitudes are common predictors of an individual’s ability to successfully re-enter society after being released from prison. Furthermore, because respect for the rule of law is also a predictor for not breaking the law, the results suggest that restoring voting rights causes newly-enfranchised individuals to increase the very types of attitudes and behaviors that make a return to crime less likely.

Section 1 details the predicted effects of restoring voting rights and mobilization on different dimensions of political trust. Section 2 presents the design and results from the first experiment, which was conducted in Akron, Ohio in November 2014. Section 3 presents the
design and results from the second experiment which was conducted in Richmond, Virginia in November 2017. Section 4 discusses the inferences that can be made from the results as a whole.

**Section 1: Theory and Hypotheses**

**What is Political Trust?** Political trust, or trust in government, characterizes individuals’ trust toward government institutions and actors. Political trust is typically characterized as either a rational utility-seeking prediction that the government is indeed looking after the public interest, or as an affective trait representing a feeling of attachment to the political system (see Maloy 2008). Political trust is considered to be a prerequisite for a stable and efficient democracy. The process of democratic governance negotiates between diverse preferences, sometimes resulting in winners and losers. If the government is perceived as legitimate and trustworthy, electoral losers are more likely to accept outcomes without resisting, enabling democracy to continue. Putnam (1993) also describes trust as one of two facets of social capital. Low trust in government increases the probability that citizens will engage in non-electoral types of participation – like protest – and also increases the likelihood that citizens will support anti-system candidates and political parties when they do vote (Levi and Stoker 2000; Hooghe and Marien 2013; Hooghe, Marien, and Pauwels 2011). High trust in government increases support for stronger government involvement (Hetherington and Husser 2012; Hetherington 2005), and leads to higher levels of government efficiency (Hetherington and Rudolph 2015; Marien and Hooghe 2011) and higher investment in public goods (Hetherington 1999).

There are two primary pathways by which reversing felony disenfranchisement laws should generate an increase in political trust among formerly disenfranchised citizens: (1)
through the effect of restoring voting rights on voter turnout; and (2) through the restoration of voting rights on its own.

First Pathway – The Effect of Political Participation on Political Trust: Democratic theorists contend that the act of engaging in political participation can play an educative and transformational role – promoting civic engagement and democratic values, and creating a self-reinforcing cycle of continuing participation (see Pateman 1970). Deliberative theorists (e.g. Fishkin 1991) similarly argue that participation in deliberation and decision-making transforms individuals, increasing civic-mindedness and political trust.

Engaging in the act of participation might also increase political trust because the act of participation increases the perceived legitimacy of government. Participating in a system can make an individual more likely to approve of the institutions, norms, and values associated with that regime, leading to an increase in satisfaction with the system as a whole (Finkel 1987; Thompson 1970). Research on public concepts of justice suggest that the notion of legitimacy is linked more to the perceived fairness of the decision-making process, as opposed to approval of particular outcomes (Gamson 1968; Grimes 2006; Tyler 2011; Marien 2011). Individuals who take part in the electoral decision-making process report higher levels of efficacy and political trust (Shineman 2018) and are more likely to view electoral outcomes as legitimate, increasing their willingness to acquiesce to government authority (Finkel 1985; 1987; Dryzek 2000).

There is some disagreement as to how frequently disenfranchised citizens would actually vote if their rights were restored (Meredith and Morse 2014; Meredith and Morse 2015; Manza and Uggen 2008; Gerber et al 2015; Hjalmarsson and Lopez 2010; Haselswerdt 2009; Miles 2004). Although the exact rate of expected participation among currently disenfranchised citizens (if their rights were restored) is typically lower than the rate of voter turnout in the rest
of the population, restoring voting rights to the disenfranchised population would increase turnout among at least some members of this population (Manza and Uggen 2008; Meredith and Morse 2015; Gerber et al. 2015). Since restoring voting rights would increase voter turnout, restoring voting rights should also increase political trust and the perceived legitimacy of law.

**Second Pathway – The Effect of Voting Rights on Political Trust:** The experience of being disenfranchised also likely causes a direct decrease in trust in government – independent from whether or not the citizen would vote if allowed. A disenfranchised citizen is deliberately disconnected from the democratic process, creating both a psychological stigma and a rational belief that the system is non-inclusive and non-responsive.

Numerous international court decisions have suggested that disenfranchisement is likely to hurt respect for democracy and the rule of law. For example, the Supreme Court of Canada ruled: “denying penitentiary inmates the right to vote is more likely to send messages that undermine respect for the law and democracy than enhance those values” (Sauvé v Canada (Chief Electoral Officer) 2002 3 SCR 519 [30]); and the European Court of Human Rights wrote that disenfranchisement is “more likely to undermine the authority of the law than enhance it” (Hirst v. United Kingdom 2005 [42]). For further examples, see Hill (2000), Hill (2009), and Ewald and Rottinghaus (2009).

The literature on prisoner re-entry emphasizes the importance of self-identity. Early studies on the ritual of criminal justice argue that the process of being convicted and sentenced changes a person’s identity: “a person starts out as one status (presumably a ‘person’ or ‘citizen’), and emerges at the other end as a different entity altogether – an ‘offender’ or ‘criminal’” (Garfinkel 1956, p 420). When individuals see themselves in the role of a criminal, they are more likely to commit further crimes; whereas, if they are able to transition from the
stigmatized identity of a criminal to the pro-social identity of an adult law-abiding citizen, they are more likely to successfully re-integrate into society post-release (Maruna 2001; Uggen, Manza, and Behrens 2004; Uggen, Manza, and Thompson 2006; Maruna 2011).

Further, *Social bond theory* (Hirschi 1969) argues that attachments to pro-social values, people, and institutions prevent citizens from committing crimes. The “belief” component of social bond theory argues that the more a person embraces the moral validity of laws, the more likely that person is to follow those laws. If restoring voting rights does indeed make the democratic system feel more fair and inclusive, one can thus derive a theoretical link between disenfranchisement and both cooperation with government and respect for law.

**Hypotheses**: Given the very strong reasons to expect voting rights to be linked with trust in government, the following hypotheses are proposed. Informing formerly disenfranchised citizens about a policy that increases access to restored voting rights in advance of a statewide election will cause those individuals to:

(H1) …Become more trusting of the government elected in that election. Being eligible to vote in an election should make that electoral process feel more fair and legitimate, increasing the level of trust in the representatives elected through that process.

(H2) …Become more trusting of political actors not elected in that election. Learning that one’s rights have been restored might also increase the perceived trustworthiness of government actors elected in other contests in which the individual is now eligible to vote. The entire political system might appear more fair, inclusive, and representative;

(H3) …Increase the degree of fairness and representation that they perceive from the government. Learning that access to restored voting rights is more inclusive should make the system feel both more fair and more representative;
(H4) …Become more trusting of and willing to cooperate with law enforcement. Trust in the police might increase when one’s right to vote is restored because the penalty imposed based on an arrest from a police officer is being reversed. As individuals transform from an identity of “criminal” to an identity of “citizen” they might also see police as more of a protective force, rather than as a threat. Furthermore, willingness to cooperate with law enforcement should increase if the laws being enforced by the police are seen as more legitimate;

(H5) …Become more trusting of the (criminal) justice system. When one’s right to vote is restored, this might increase their trust in the actors perceived as responsible for taking away that right in the first place. Additionally, as one transitions toward an identity as a citizen and active equal member of democratic society, the institutions in that democracy will feel more inclusive, fair, and legitimate;

(H6) …Become more trusting of non-political actors. Finally, some scholars model trust as a more general feeling, which can spill over and between several dimensions. If the restoration of voting rights causes citizens to become more trusting of government actors, that increase in trust might spillover and also affect other dimensions of trust beyond the electoral realm.

(H1.B – H6.B) Similarly, the combination of information about more inclusive voting rights along with assistance with voter registration and an encouragement to vote in the upcoming statewide election should also cause individuals to increase their trust in all the dimensions specified above. A combined treatment should generate these effects due to both the effects of voting rights (as discussed above), and because adding a mobilization element further increases the probability that a person will register and vote. A comparison of effects between the two treatments can isolate whether information about voting rights alone is sufficient to
increase political trust, or if restoring voting rights is only effective when it is also combined with mobilization to register and vote.

Section 2. Experiment #1: Akron, Ohio (November 2014 General Election)

The first experiment embedded two “get out the vote” (GOTV) mobilization treatments into a panel survey conducted before and after the November 2014 General Election. All subjects were adult American citizens with a felony conviction living in Ohio. In the state of Ohio, a citizen loses the right to vote when they are imprisoned for a felony conviction, and any existing voter registration is (supposed to be) purged. The right to vote is automatically restored after a person is released from prison, but citizens who were incarcerated must re-register. Thus any citizen in Ohio who is not in prison has already had their right to vote restored. However, because so many other states continue to disenfranchise citizens after they are released from prison, many eligible citizens in Ohio might not be aware that their right to vote has been restored.

The study recruited a convenience sample of 52 subjects, by advertising in places where potential subjects would be likely to see the announcement. Potential subjects were invited to participate in a research study, consisting of taking two surveys about 5-6 weeks apart. Subjects completed the first survey in person at the Akron Urban League, a non-partisan non-profit organization well-known for offering assistance with job training and community development.

After completing the pre-election survey, subjects were randomly assigned to receive one of three different treatments in a private face-to-face setting.¹ One treatment (Generic GOTV) provided subjects with a generic appeal to register and vote, along with assistance with voter

¹ Complete details on all experimental protocols, treatments are provided in Appendix A1.
registration, and a reminder of the upcoming election. In the generic treatment, the subject’s criminal history was never mentioned during the delivery of treatment – though all subjects knew that they needed a felony record to participate in the study. A second mobilization treatment (Felony-Specific GOTV) added additional felony-specific information. Subjects were provided with a handout displaying the variation in voting rights by state, informed that they did have the right to vote in Ohio, and received a scripted verbal appeal urging subjects to take advantage of their right to vote. This information was intended to correct potential misinformation among subjects who might not know their right to vote had been restored, and to provide subjects with a personalized appeal for why they should be involved in politics. The Placebo treatment borrowed and expanded the idea of a recycling placebo, which has been used frequently in the mobilization literature (e.g. Nickerson 2008). The placebo treatment was designed to provide subjects with an equivalent level of personalized attention and a positive pro-social message, without any reference to elections or voting.

The post-election survey began on November 5th, 2014 – one day after the November 2014 General Election. Attrition was low; of the 52 subjects who took the first survey, 41 also completed the second survey (78.9%).² All subjects who completed both surveys were sent a $25 postal money order through the mail.

Both treatments generated increases in voter registration and turnout. The rate of new voter registrations increased by 13.3 percentage points (pp) in the Generic GOTV treatment (p = 0.07), and by 9.5 pp in the Felony-Specific GOTV treatment (p = 0.11), increasing by 11.1 pp

² Attrition was comparable across treatment groups: completion rates were 75% (Placebo), 80% (Generic GOTV), and 81% (Felony-Specific GOTV).
overall across both treatments (p = 0.09). Voter turnout was also higher in both treatment groups in the 2014 election – increasing by 20.4 pp in the generic treatment (p = 0.07), by 8.0 pp in the felony-specific treatment (p = 0.22), and by 13.2 pp overall (p = 0.12).

**Results – Treatment Effects on Political Trust:** The pre and post-treatment surveys both asked subjects to indicate their level of trust regarding various institutions and groups of people. The effects of each treatment on political trust are estimated using ordinary-least-square (OLS) regression, including controls for each pre-treatment value of trust along with a set of covariates intended to decrease noise and increase the accuracy and precision of the estimates (Pocock, Assmann, Enos, and Kasten 2002).

Figure 1 displays the predicted values for post-treatment (and post-election) levels of trust for a typical subject in the sample, comparing the control group to each of the two treatments groups, as well as the pooled estimate of receiving either GOTV treatment. In order to easily facilitate comparisons of magnitude, all estimates of trust are re-scaled from 0-100, with higher numbers indicating higher trust. The effects of the two treatments were very similar. Although all three estimates are presented in the figure below, the discussion focuses on the pooled estimate from both treatments.

FIGURE 1 ABOUT HERE

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3 An additional 9.0% of treated subjects filled out a registration form because they said they did not think they were registered to vote, but they were already registered (and did not know it). Another 16.7% of treated subjects filled out a registration form for some other purpose – such as updating their address, or to request an absentee ballot.
The first experiment provides initial support for the hypotheses. The mobilization treatments significantly increased trust in the local and state government (by 19.5 and 14.7 percentage points (pp), respectively), and marginally increased trust in the federal government (+16.8 pp, p = 0.07). Trust in specific governmental actors (Congress and the President) was not affected. Although subjects increased their trust in government, trust in other non-political actors (the Fire Department, Media, Neighbors, and Co-workers) was not affected. This selective pattern of increases in trust lends support to the theory that mobilization affects political trust in particular, rather than overall trust in or positive feelings toward all types of actors. However, the treatments did increase trust in the criminal justice system. On average, the mobilization treatments increased trust in the police (by 36.8 pp) and trust in the criminal courts (by 30.1 pp). Trust in the Supreme Court was also 13.1 pp higher on average among treated subjects (though this last difference was not statistically significant; p = 0.13).

Overall, the results suggest that both mobilization treatments increased trust in government in a general sense, and that these effects did not spillover to also increase trust in non-political actors. The significant positive effects of mobilization on trust in the criminal justice system were not originally predicted. But these effects seem intuitive in retrospect. Learning that one’s voting rights have been restored is effectually reversing some of the penalty assigned to each subject by the criminal justice system.

The results from the first experiment cannot speak to whether these effects were caused by learning that one’s rights had been restored, or by being encouraged to register and vote. The Felony-Specific GOTV treatment was designed to engage the subject’s criminal history in a positive light – to clarify misinformation, and to provide a positively-framed felony-specific appeal to participate. However, the Felony-Specific GOTV treatment might have accidentally
introduced negative elements, both by raising the salience of the subject’s criminal record (increasing stigma), and by possibly correcting misinformation in the opposite direction as intended. Because Ohio’s felony disenfranchisement policy is one of the most lenient in the country, some subjects might have been unaware that disenfranchisement policies were so much more strict in other states. Additionally, although the Generic GOTV treatment was intended to be devoid of references to the subject’s criminal record, because the recruitment materials (and several survey questions) mentioned the subject’s criminal record, the connection to felony disenfranchisement and eligibility might have been obvious to some subjects. These patterns can help explain the similarity between the effects of the treatments – as the positive message from the Felony-Specific GOTV treatment was likely weaker than intended, and the Generic GOTV treatment likely implied more information about eligibility than intended.

Given the promising results from the first experiment and the overlapping boundaries between the two initial mobilization treatments, a second experiment is warranted. The second experiment was designed to clearly differentiate between the effects of increasing information about restored voting rights and the effects of being mobilized to register and vote. Because the effects of mobilization on trust in the criminal justice system were discovered during exploratory analysis, but seem theoretically justifiable, the second study was also designed to deliberately test this relationship as a predicted effect. The second experiment also tests the robustness of all other observed effects. The survey was lengthened, to allow for a more detailed analysis of the different dimensions of political trust, and how each is affected by information about the restoration of voting rights. Finally, the second experiment identifies a population of citizens who are even more likely to be unaware that their voting rights have recently been restored – thus increasing the ability for treatments to correct misinformation in a strictly positive direction.
3. Experimental #2: Richmond, Virginia (November 2017 Statewide Election)

Electoral Setting: The second experiment was conducted during the November 2017 Virginia Statewide General Election, during which the state of Virginia elected its Governor, Attorney General, and the State Legislative Assembly.

In the state of Virginia, a citizen loses the right to vote when they are convicted of a felony. But unlike in Ohio, Virginia has no automatic process for the restoration of voting rights. A citizen convicted of a felony crime in Virginia is permanently disenfranchised for the rest of their life, unless the Governor intervenes and signs an executive order restoring that citizen’s voting rights. Given the ubiquitous and permanent nature of this penalty, all citizens with a felony record in Virginia were very likely to know that they were disenfranchised (whereas in Ohio, some citizens might have been unaware that they were disenfranchised while in prison).

Traditionally, citizens had to apply to have their rights restored, and the Governor’s office would evaluate each application individually. But then, in 2016, Virginia Governor Terry McAuliffe decided to proactively restore voting rights to every citizen who had completed their probation. In April 2016, McAuliffe signed a single executive order restoring voting rights to 206,000 disenfranchised citizens. More than 13,000 newly enfranchised citizens registered to vote that summer. But in July 2016, the VA Supreme Court overturned McAuliffe’s executive order (in a 4-3 split decision). The Court agreed that McAuliffe had the power to restore voting rights to all 206,000 citizens – but they ruled that he couldn’t restore voting rights to all those citizens in a single executive order. Instead, the Court said, McAuliffe would have to issue 206,000 separate executive orders – one for each individual. McAuliffe’s original executive order was overturned. The 13,000 new voter registrations were all canceled, and all 206,000 citizens were once again disenfranchised.
McAuliffe began to issue the individual executive orders one-at-a-time, but it was a time-consuming process. He restored voting rights to 70,000 citizens before the November 2016 Presidential Election, and to more than 150,000 citizens before the November 2017 Statewide General Election. The Governor’s office sent a certificate of restored civil rights to the last known address for each citizen after their rights were restored. However, many of these addresses were no longer current, and some people never got the letter. Among those who did receive the letter, some weren’t sure if it was real, because they heard that the Supreme Court had declared McAuliffe’s executive order to be unconstitutional. Thus many of these newly eligible citizens were unaware or unsure as to whether or not their rights had been restored.

Given this set of events, the Virginia 2017 election was an ideal setting for increasing information about newly restored voting rights. More than 150,000 citizens had just had their right to vote restored, but the confusing process by which those restorations were issued caused short-term uncertainty about who was eligible. The experiment leveraged this confusion and misinformation as an opportunity to randomly increase information about a new voting rights policy that was more inclusive than the previous status quo that people were accustomed to.

**Experimental Design:** The experiment embedded two treatments into a panel survey conducted before and after the November 2017 Virginia Statewide Gubernatorial Election. Recruited subjects (n = 98) were adult citizens with a felony conviction, currently living in Virginia, who had completed probation – individuals who were once disenfranchised, but were also eligible to have their voting rights restored by the Governor. A convenience sample was
recruited by advertising in places where potential subjects would be likely to see the invitation to participate.4

Subjects completed the first survey in a private office suite located in downtown Richmond, VA about one month before the November 2017 election. After completing the first survey, subjects were randomly assigned to receive one of three treatments. All treatments were delivered one-on-one in a private face-to-face setting, after subjects completed the first survey.5

In order to avoid accidentally activating the stigma-ridden identity of a criminal, all treatments in the Virginia experiment were tailored to avoid references to the subject’s criminal record, and instead focused on the subject’s completion of probation as a success story. None of the treatments or survey questions used the word “felon” as a noun, following guidance from Assistant Attorney General Karol Mason’s directive to the Office of Justice Programs to stop using “unnecessarily disparaging labels” like “felon” and “convict” (Manson 2016). Treatments referred to subjects as “citizens” and none of the survey questions asked about criminal history.

(Treatment #1) Restoration Only: The Restoration Only treatment provided subjects with information about the Governor’s new initiative regarding the restoration of voting rights – specifically, that he was proactively restoring rights for all citizens who had completed probation, one at a time. Subjects were told “there’s a pretty good chance your voting rights have already been restored,” and then the researcher offered to look up the subject’s voting rights

4 Recruitment ads were placed in a variety of newspapers and job search websites. Flyers were distributed at street fairs, public transportation hubs, bodegas, and with organizations that provided public services. Recruitment materials did not mention voting rights or politics.

5 Full details on experimental protocols, treatments, and survey questions are in the Appendix.
status, a process that “only takes 15 seconds.” If the subject agreed to let the researcher look up their voting rights, the researcher then looked up the subject’s voting rights status in front of them on an official government website. If the subject’s voting rights had been restored, the researcher would confirm the date on which the subject’s voting rights had been restored. If the subject’s voting rights had not yet been restored, the researcher offered to assist the subject in submitting a request to restore their voting rights. The restoration request was a simple single page application. Submitting that application would move the subject’s name to the top of the Governor’s list, so that their name would be the next name processed (a process expected to take about three weeks). In order to separate the effects of voting rights from the effects of mobilization, the Restoration Only treatment only provided information about the new voting rights restoration policy. This treatment did not mention the upcoming election or offer any assistance with voter registration.

(Treatment #2) Restoration with Mobilization: The Restoration with Mobilization treatment provided subjects with all elements of the Restoration Only treatment. Additionally, subjects were told that a statewide general election was coming up on November 7th. Subjects were given information about how to register to vote and how to look up other information about the election. The researcher also offered to look up the subject’s registration status (which also only takes about 15 seconds through an official government website), and did so only if the subject agreed. If the subject was not registered to vote, the researcher offered to assist the subject in registering to vote.

Placebo Treatment: The Placebo treatment provided subjects with an energetic personalized appeal to volunteer and be active in their communities, along with a calendar list of upcoming volunteer opportunities in the area. The goal of the placebo treatment was to parallel
the same degree of personal contact and connection provided in the two voting rights treatments, and also to provide a similar pro-participatory message encouraging community involvement, without any reference to voting rights or the upcoming election.\textsuperscript{6}

The post-election survey began two days after the election, and could be completed by phone, online, or through the postal mail. Attrition was extremely low: of the 98 subjects who completed the first survey, 93 also completed the second survey. All subjects who completed both surveys were provided with a $25 gift card to a location of their choice.

\textbf{Treatment Effects on Voting Rights, Registration, and Turnout:} In total, 42\% of treated subjects allowed the researcher to look up the status of their voting rights. Most who declined stated that they already knew their rights had been restored and did not need to look it up. Of those who looked up their voting rights during the treatment delivery, about half (21.4\% of all treated subjects) learned that their right to vote had been restored on the spot. Another 16\% of treated subjects verified that their right to vote had \textit{not} yet been restored, and then filled out the request form to move their name to the top of the Governor’s list.

Among subjects who received the \textit{Restoration with Mobilization} treatment, 20.7\% filled out a voter registration form – 17.2\% to register to vote for the first time, and 3.5\% to update

\textsuperscript{6} This \textit{Placebo} treatment was modified from the original recycling template used in the first experiment in order to make it feel more believable to subjects that the placebo message was the intended purpose of the study. The pre-treatment survey also ended with a series of questions about volunteering, to further increase the naturalness of delivery of the placebo treatment. A pro-volunteer message also better mimics the pro-participatory content of the treatments, allowing more direct isolation of the voting rights content.
their address. The *Restoration with Mobilization* treatment increased new voter registrations by 13.8 pp (p = 0.01) – though one subject registered after the deadline had passed to vote in the November 2017 election. Although subjects in the *Restoration Only* treatment were not offered assistance with voter registration, 14.8% of those subjects registered to vote on their own shortly after the treatment was delivered – though notably only one of these subjects registered before the deadline to vote in the November 2017 Election. In comparison, no new subjects registered to vote in the *Placebo* group. The treatments increased the percent of subjects who had the right to vote, the percent of subjects who knew they had the right to vote, and the percent of subjects who were registered to vote. However, neither treatment generated a significant increase in voter turnout in the November 2017 election.

**Methods for Analysis:** The surveys asked 26 different questions estimating multiple dimensions of trust in a variety of government and non-government actors. Each estimate of trust is assessed individually, and presented in categories (corresponding to the six hypotheses): Trust in the government actors elected in the November 2017 Election (Models 1A – 1C); Trust in government actors not associated with the November 2017 Election (Models 2A – 2F); Perceptions regarding the fairness and representativeness of government (Models 3A – 3F); Trust and cooperation with the police (Models 4A – 4C); Trust in the (Criminal) Justice System (Models 5A – 5C); and Trust in Non-Political Actors (Models 6A – 6E).

The analysis uses OLS regression to estimate the effects of receiving each treatment (as well as the pooled effects of both treatments) on each estimate of political (and non-political)
trust. As in the previous experiment, all models control for the pre-treatment value of each dependent variable and also include a pre-specified set of covariates. To easily facilitate comparisons of magnitude, all estimates of trust are re-scaled to range from 0-100, with higher numbers indicating higher levels of trust. As specified in the pre-analysis plan, given the directional nature of all hypotheses, all models are evaluated with a 1-tailed (directional) significance test within a 95% confidence interval. Effects within a 90% confidence interval are noted as marginally significant.

**Results – Treatment Effects on Political Trust:** Figure 2 displays the estimated effects of both treatments on all 26 estimates of political trust, including 2-tailed 90% and 95%

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7 Although estimating a complier average causal effect (CACE) might seem compelling, it is not appropriate in this case. Firstly, the treatments aimed to increase awareness of restored voting rights, increase new restorations of voting rights, and also to increase voter registration and voter turnout. Thus, the criteria as to what it would mean to be a complier is unclear. Moreover, for any category of compliers, the exclusion restriction would be violated, which means that any CACE model would over-estimate the average treatment effects. Thus, all estimates present the average effect of receiving each treatment on the sample as a whole.

8 As specified in the pre-analysis plan, covariates include age, age\(^2\), gender, race, education, employment, and number of years at current address. In the interests of space (and as specified in the pre-analysis plan), the body of the manuscript only presents the estimates from the OLS models including covariates. Coefficients for all models are included in the Appendix. Models estimating the change (pre v. post treatment) are also presented in the Appendix. The results are robust across models, and the substantive interpretation of the results is the same across all specifications.
confidence intervals. The first two columns display the effect of each treatment individually, and the third column presents the pooled effects of receiving either restoration treatment.

FIGURE 2 ABOUT HERE

To place the magnitude of each estimate within the context of varying baseline values, Figures 3 through 8 (below) display the predicted post-treatment levels of political trust generated by each model, comparing the predicted value in the placebo group to the predicted value among subjects who received either of the two restoration treatments. Because both treatments performed comparably (in all but one case), the discussion focuses primarily on the pooled results.

Figure 3 displays the predicted values generated from Models 1A – 1C. The first hypothesis is largely supported. Both treatments caused subjects to become more trusting of the government elected in that election. On average, the treatments generated a significant increase in trust in the newly elected Governor (+15.1 pp), the newly elected State Assembly (+15.5 pp), and the Virginia State Government as a whole (+16.9 pp).

FIGURE 3 ABOUT HERE

Figure 4 displays the predicted values generated from Models 2A – 2F. The second hypothesis is not supported. Although trust in the old Governor (McAuliffe) increased significantly (+18.2 pp), this is likely because subjects inferred that McAuliffe was the one who restored so many people’s voting rights. Trust in the previous State Assembly, the President, the US Congress, and the US Federal Government were not affected – though trust in the local government did increase by 8.1 pp on average (p < 0.10).

FIGURE 4 ABOUT HERE

Models 3A-3F find strong support for the third hypothesis. Figure 5 displays the predicted values. On average, receiving either treatment caused a significant increase in the perceived
fairness (+24.6 pp) and representativeness (+19.8 pp) of the Virginia State Government.

Perceived fairness of the federal government also increased (+14.9 pp), as did the perceived representativeness of the local government (+12.2 pp). The remaining two categories increased on average, but these effects were not significant within a 95% confidence interval. Overall, this pattern of results reflects those observed in the first two hypotheses. Treated subjects increased their positive evaluations of the government that was elected during the course of the experiment (the Virginia State Government), but experienced fewer changes in their evaluations of other levels of government.

FIGURE 5 ABOUT HERE

Models 4A-4C find strong support for the fourth hypothesis. Figure 6 displays the predicted values. The restoration treatments significantly increased trust in the police (+19.2 pp). Subjects were also asked if they would be willing to contact the police for help to protect themselves or to protect someone else. Reported willingness to cooperate with the police increased significantly when acting to protect oneself (by 17.6 pp), and also when evaluating a scenario where one would be acting to protect another person (by 11.0 pp).

FIGURE 6 ABOUT HERE

The fifth hypothesis predicts that both treatments will increase trust in the (criminal) justice system. Unlike all the other models where the two treatments performed remarkably similar to each other, the effects of the Restoration Only treatment were different from the effects of the Restoration with Mobilization treatment in Models 5A-5C (the area of criminal justice). Because of this difference, discussing pooled effects is less accurate. Figure 7 displays the predicted values for typical subjects in the placebo group compared to typical subjects in each treatment group individually. The Restoration Only treatment did not significantly affect trust in any area
of the justice system; whereas the Restoration with Mobilization treatment increased trust in the criminal courts (+15.7 pp), the US Supreme Court (+24.7 pp), and the VA State Supreme Court (+25.2 pp).

**FIGURE 7 ABOUT HERE**

Finally, Models 6A-6E do not support the sixth hypothesis. Figure 8 displays the predicted values. Neither treatment caused a significant increase in trust in the fire department, media, one’s neighbors, one’s co-workers, or the subject’s peer group – citizens with felony convictions. As in the Ohio experiment, the selective pattern of increases in trust serves to bolster support that restoring voting rights causes increases in trust in the actors and institutions associated with the denial of voting rights along with the actors involved in the election in which citizens are now eligible to vote.

**FIGURE 8 ABOUT HERE**

**Section 4. Discussion of Results**

Overall, this study provides strong evidence that restoring voting rights to citizens with felony convictions will cause those citizens to develop stronger levels of trust in government, as well as stronger trust and cooperation with the police and (criminal) justice system more generally speaking. Treated subjects reported significantly higher levels of trust in political institutions and elected representatives, and also strengthened their evaluations of the level of fairness and representation within the government.

This study makes several important contributions. First, the results find that all three treatments including a mobilization element generated a significant increase in political trust. Previous research conducted on a sample of citizens without felony convictions found that
mobilizing people to vote caused those people to become more trusting of government and electoral institutions (Shineman 2018; Shineman 2020b). The current study adds further confidence to the overall theory that increasing participation increases trust, by replicating this previous finding with two different subject populations in two different electoral settings. Replicating the effect of participation on political trust among a population of subjects who have a felony record also adds new normative implications to the relationship between participation and political trust.

This study also makes two additional novel contributions. First, both experiments find evidence that mobilizing citizens with felony convictions to participate in politics causes them to become more trusting of not just the government actors elected in that contest – but to also become more trusting of the police and the (criminal) justice system more generally speaking. This relationship was unexpectedly observed in the Ohio experiment, and was then replicated as a formal hypothesis in the Virginia experiment. These results have striking implications, as the population being studied has notoriously low levels of trust in the criminal justice system.

The final novel contribution of this study is providing evidence that voting rights alone (even without mobilization) lead to higher levels of trust in both government and law enforcement. Subjects who only received information about the new protocol for restoring voting rights – with no assistance with voter registration or mention of the upcoming election – also became significantly more trusting of the newly elected Virginia State Government, more trusting of the police, more willing to cooperate with law enforcement, and they perceived government as more fair and representative. These results suggest that felony disenfranchisement laws are directly decreasing trust through the act of disenfranchisement – not only through their effect on turnout. In all but one case, the marginal effect of adding mobilization – after the voting
rights content had already been provided – was non-significant. Some argue that the effects of disenfranchisement are less severe because of the low voter turnout among eligible ex-offenders (e.g. Miles 2004). While other estimates of the electoral power of the ex-offender population refute this critique (e.g. Manza and Uggen 2008), the current study offers an additional response. Regardless as to whether citizens choose to exercise their voting rights, the act of restoring rights alone causes newly enfranchised citizens to feel more trusting of the democratic system.

**Characterizing the Treatment:** One might ask what exactly the average treatment effect is measuring. The answer to this question is that the treatment experience varied widely across subjects. Some subjects learned their right to vote had been restored. Some subjects were still disenfranchised, and initiated a request for their voting rights to be restored. Some subjects registered to vote, or looked up the location of their polling site. One subject walked in believing they were disenfranchised, learned their right to vote had been restored, registered to vote during the treatment delivery, and then also voted in the November 2017 Election. Others declined to look up their voting rights status, and left with only the information about the Governor’s new protocol for restoring voting rights. There were many different levels of engagement, and these varied based on each subject’s actual voting rights status, what the subject initially believed their voting rights status to be, and whether or not the subject was interested in receiving any information or assistance. Notably, there were no subjects who believed their right to vote had been restored and learned that they were still disenfranchised through any of the treatments.

In the Ohio experiment, although the felony-specific treatment was framed as a positive encouragement to participate, it is possible that that treatment provided new negative information about felony disenfranchisement polices to some subjects. In the Virginia experiment, however, no matter where a subject was when they entered the experiment, the treatment provided them
with nothing but positive information about a new more lenient voting rights protocol, and sometimes also provided additional positive information and/or assistance that was directly relevant to the subject. Thus the overall effects of the treatment can be considered as the effects of learning about a policy that increases access to restored voting rights in a more general sense.

**The Magnitude of the Effects:** The sample size does place some limitations on the level of confidence we can place in the magnitude of these estimates. Small samples create a more challenging barrier to revealing existing effects, increasing the risk of false negatives (Vadillo, Konstantinidis, and Shanks 2016). Thus null effects should not be considered to be conclusive evidence that no effect is taking place. Additionally, small samples are also more susceptible to over-estimating effect sizes, so we should also approach the magnitude of the significant findings with caution.

However, if one wanted to estimate the effect of learning that one’s right to vote has been restored, there are a few reasons to suspect that the observed treatment effects are an under-estimate of this value. Firstly, about 65% of subjects in the treatment groups reported that they thought their voting rights had been restored (before they received the treatment). Several subjects recounted about when they received the official restoration notice from the Governor, talked about how they had it framed, or said they had been excited they were able to vote for the first time in the 2016 Presidential election. These subjects had already experienced the positive effects of learning their rights had been restored – and sometimes also the positive effects of being registered and voting. As such, the estimates only capture the full changes in trust among the subset of the subjects who received new information during the treatments (about 21% of treated subjects who learned their rights had been restored, 16% whose rights were not yet restored but requested restoration, and about 20% - in the mobilization treatment only – who
either registered to vote or updated their address for the first time). Additionally, among the 16% of treated subjects whose rights had not yet been restored that requested the restoration of their voting rights – although voting rights were eventually restored to nearly all of these subjects, all of the restorations were processed on or after November 15th (after the second survey had been completed). Thus although these subjects had been told that their rights would be restored, they had not received confirmation that this restoration had actually been processed until after they completed the second survey. For example, one subject sent a text message on November 30th (weeks after he completed the second survey), after he received confirmation from the Governor that his voting rights had been restored, saying: “Thank you guys very so much..Now I feel as a American citizens.. [sic] Thanks again..” It is likely that this subject would have reported higher levels of trust, had he been surveyed after the restoration of his voting rights had been processed and confirmed.

The Duration of the Effects: The treatment effects on trust were estimated in the short-term, about 2-7 days after the election took place (and 4-6 weeks after treatments were delivered). Subjects were not contacted again after the post-election survey, so there is no formal estimate regarding the long-term duration of these effects. While the initial magnitude of the effect size is likely to decrease in the weeks and months following the election – as many short-term effects have been found to do – there is also reason to believe that the positive effects of the treatments will be both enduring and reinforcing over time. Subjects whose rights were restored will always continue to know that their rights have been restored, and the feelings generated from this knowledge will likely be re-activated during future elections and political discussions. As such, although the magnitude of the treatment effects is likely to fall in the short-term, these effects should not diminish completely, and should also rise again during political events.
Michelitch and Utych (2018) find that the strength of partisan identity rises and falls with electoral cycles. Similarly, the effects of being eligible to vote will likely rise and fall with electoral cycles – with a gradual increase over time, as the identity as a citizen and represented member of society continues to be reinforced.

Furthermore, as we know that participation and turnout are habit forming (Gerber, Green, and Shachar 2003; Meredith 2009; Davenport et al. 2010), the effects on voter registration and voter turnout are likely to persist, continuing to cause subjects to feel more included and represented in the political system. As noted in the analysis of the Virginia experiment, treated subjects continued to increase their rate of both new restorations of voting rights and new voter registrations after the second survey was conducted. These delayed effects suggest that not only will registration and turnout effects continue to persist where they were observed, but we might also see other new subjects register and vote in response to the treatments in future elections.

**Generalizability**: Both experiments recruited subjects who either already had their right to vote restored, or who were eligible to request the restoration of their voting rights. One might naturally ask whether the effects demonstrated in these studies can be generalized to all citizens with felony convictions who might have their rights restored. In short, there is no way to answer such a question with the available data. Because felony disenfranchisement laws vary so widely between states, the population of eligible subjects varied between the Ohio and Virginia experiments, and the eligible population could vary even more across some other states.

Both experiments were also conducted in a single city in each state, and should not be presumed to be representative of the entire state population they were recruited from. Nor should either sample be considered to be a representative sample of the full population of potential subjects in either Akron or Richmond. Both studies recruited a convenience sample among a
very hard-to-reach population, and deliberately avoided recruiting from the voter registration list or from consumer databases. Thus we cannot infer from either study that all citizens who are eligible to vote in Akron or who are eligible to have their voting rights restored in Richmond would experience effects of similar magnitude in response to these treatments.

However, the results do show that the citizens in this study did increase their political trust in response to the treatments – which means at least some members of this population would experience these effects. The replication of the effects across two different samples in two different states provides robustness to the notion that these effects are not limited to citizens from a single state. The significant positive effects on trust in government, the police, and the criminal justice system were also replicated in all four treatment conditions – further adding to the robustness of the interpretation of the results. The average effect across all possible subjects nationwide might be larger, or might be smaller, compared to the effects documented among this particular sample. Future studies can repeat this template in future elections, in order to test the robustness of these effects across varying subject populations and electoral settings. Furthermore, there might be interesting variations in the treatment effects across types of citizens (e.g. by age, by gender, by race). What types of citizens might experience the highest and lowest increases in trust when their rights are restored?

**CONCLUSION**

Felony disenfranchisement laws currently restrict the voting rights of more than four million American citizens. Much attention has been paid to whether these blocked votes might be affecting electoral outcomes. A full examination of the effects of these laws should also account for the effects of disenfranchisement on the individuals who are being disenfranchised. The re-
entry literature suggests that successful post-prison re-entry is best supported by targeting criminogenic characteristics – traits that are dynamic, and directly linked to recidivism – including anti-social attitudes and beliefs. Models of successful re-entry stress the importance of community ties, feeling vested, and prosocial attitudes (Gendreau, Little, and Goggin 1996).

Scholars and policymakers have long suspected that the stigma of disenfranchisement might cause a feeling of otherness that decreases trust and social bonds and ripples into disengagement beyond the electoral arena. Many scholars have endorsed civic engagement and reintegration as key elements of successful post-prison re-entry (Orr 1998; Bazemore, Gordon, and Stinchcomb 2004; Uggen, Manza, and Thompson 2006). Though these theories are long-standing, the best existing evidence in support of these effects has been from anecdotes or based on correlations gathered from observational data that is highly subject to endogeneity.

By leveraging the high misinformation about eligibility, this study is able to estimate the effects of exogenous shocks in information about restored voting rights. This study provides the first empirical evidence of a causal relationship between the restoration of voting rights and political trust. The results suggest that the right to vote doesn’t only affect whether a person registers and votes; the right to vote also increases trust in democratic actors and institutions. Thus the results from this study also suggest that disenfranchisement policies might inadvertently be preventing citizens with felony convictions from developing the types of attitudes that would help them successfully reintegrate into society after being released from prison – potentially making a return to crime more likely.

The implications of this study are widespread. More than 95% of citizens incarcerated in state prisons will eventually be released. Currently, more than 75% of those released are re-arrested within 5 years (Bureau of Justice Statistics 2018). Although felony disenfranchisement
has traditionally been a partisan issue (Yoshinaka and Grose 2005), bipartisan alliances have recently found common ground on concerns regarding criminal justice reform and developing successful re-entry programs. For example, a super-majority of Florida voters passed a constitutional amendment in 2018 that could restore voting rights to up to 1.4 million formerly disenfranchised citizens. Similar reforms are being actively debated in numerous other states and have also been highlighted in national political campaigns. Given the historically low trust in government and police among the population being studied – and the links between pro-social attitudes and recidivism – evidence that restoring voting rights increases political trust could have a wide impact on the discussion of disenfranchisement policies nationwide.
Works Cited


Ewald, Alec C., and Brandon Rottinghaus, eds. Criminal disenfranchisement in an international perspective. Cambridge University Press, 2009


Meredith, Marc, and Michael Morse. "Do voting rights notification laws increase ex-felon turnout?" The ANNALS of the American Academy of Political and Social Science 651.1 (2014): 220-249


Appendixes:

All Appendixes for Akron, Ohio Experiment

https://drive.google.com/file/d/0B8ncWLLzADEac1h1SUs3R1FnVTQ/view?usp=sharing

Appendixes for the Richmond, VA Experiment:

Content of Sample Recruitment Ad:
https://drive.google.com/file/d/1Uoqp9MrwmgR3tj-TymMPYrn6D0p-080m/view?usp=sharing

Pre-Election Survey:
https://drive.google.com/file/d/1IRGD9zrRlwmpGmWSD_o5IPNL0kn0-ceR/view?usp=sharing

Post-Election Survey Questions:
https://drive.google.com/file/d/16cjOiesrITAWukhmAAxXrrDyiL7JuVSLy/view?usp=sharing

Placebo Treatment:
https://drive.google.com/file/d/13LhSGZFjplG4GXdXH1ZD5jtck9stTvIF/view?usp=sharing

Restoration ONLY Treatment:
https://drive.google.com/file/d/1e1yCugbhA__h0hNqSCtzfIBtvU0bWHog/view?usp=sharing

Restoration + Mobilization Treatment:
https://drive.google.com/file/d/1CdqGcV0ckF6JPWhCUd-TBvwZwsukK1ws/view?usp=sharing

Pre-Analysis Plan (Registered on EGAP #20171109AA)
https://drive.google.com/file/d/1xs1hZwHmUYbb-6tT6Y7d42sl10M6zxA/view?usp=sharing
Figure 1: Average Treatment Effects on 12 Estimates of Political Trust (Ohio 2014)

1A. Trust: Local Government

1B. Trust: State Government

1C. Trust: Federal Government

1D. Trust: Congress

1E. Trust: President

2A. Trust: Fire Department

2B. Trust: Media

2C. Trust: Neighbors

2D. Trust: Coworkers

3A. Trust: Police

3B. Trust: Criminal Courts

3C. Trust: Supreme Court

\[ ^* = p < 0.1 \quad ^{**} = p < 0.05 \quad ^{***} = p < 0.01 \]
Figure 2: Average Treatment Effects on 26 Estimates of Political Trust (Virginia 2017)

Figure displays average treatment effect on each estimate of political trust, including 90% and 95% confidence intervals. Estimated with OLS regression with pre-registered covariates.
Figure 3: Average Treatment Effect on Trust in Newly Elected Government (Virginia 2017)

1A. Trust: New Governor

+15.1*

Placebo 72.1
Restoration (Pooled) 57.0

1B. Trust: New State Assembly

+15.5**

Placebo 58.9
Restoration (Pooled) 43.5

1C. Trust: VA State Government

+16.9**

Placebo 56.9
Restoration (Pooled) 43.0

^ = p < 0.1  * = p < 0.05  ** = p < 0.01

Figure 4: Average Treatment Effect on Trust in Previously Elected Government (Virginia 2017)

2A. Trust: Old Governor

+18.2**

Placebo 70.6
Restoration (Pooled) 52.4

2B. Trust: Old State Assembly

+7.2

Placebo 49.3
Restoration (Pooled) 42.2

2C. Trust: Local Government

+8.1^

Placebo 67.2
Restoration (Pooled) 46.1

2D. Trust: President

+1.1

Placebo 19.8
Restoration (Pooled) 18.7

2E. Trust: Congress

-4.9

Placebo 31.3
Restoration (Pooled) 36.2

2F. Trust: Federal Government

-2.9

Placebo 33.3
Restoration (Pooled) 36.2

^ = p < 0.1  * = p < 0.05  ** = p < 0.01
Figure 5: Average Treatment Effect on Perceived Fairness and Representativeness of Government (Virginia 2017)

3A. State Government is Fair
- Placebo: 36.1
- Restoration (Pooled): 60.7
+24.6**

3B. Federal Government is Fair
- Placebo: 37.1
- Restoration (Pooled): 52.0
+14.9**

3C. Local Government is Fair
- Placebo: 47.1
- Restoration (Pooled): 54.4
+7.3

3D. State Government Represents Me
- Placebo: 41.6
- Restoration (Pooled): 61.4
+19.8**

3E. Federal Government Represents Me
- Placebo: 35.2
- Restoration (Pooled): 43.6
+8.4

3F. Local Government Represents Me
- Placebo: 40.7
- Restoration (Pooled): 53.0
+12.2*

^ = p < 0.1  * = p < 0.05  ** = p < 0.01

Figure 6: Average Treatment Effect on Trust and Cooperation With Police (Virginia 2017)

4A. Trust: Police
- Placebo: 31.4
- Restoration (Pooled): 50.6
+19.2**

4B. Would Call Police (Protect Myself)
- Placebo: 46.7
- Restoration (Pooled): 64.3
+17.6**

4C. Would Call Police (Protect Other)
- Placebo: 67.5
- Restoration (Pooled): 78.5
+11.0*

^ = p < 0.1  * = p < 0.05  ** = p < 0.01