

A Police Stop Is Enough to Make Someone Less Likely to Vote

New research shows how the communities that are most heavily policed are pushed away from politics and from having a say in changing policy.

[Jonathan Ben-Menachem](#) | February 1, 2023



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Florida Governor Ron DeSantis grabbed headlines throughout 2022 for practices that weakened democracy—from [creating](#) a police force to monitor voting to coordinating the [arrests of people who allegedly voted illegally](#) after the state told them they were eligible. In August, he [suspended](#) Tampa’s elected prosecutor, Democrat Andrew Warren, over his stated refusal to prosecute cases relating to abortion and trans rights, overriding voters’ decision.



But a host of more routine decisions made by Florida officials may be undermining the health of the state’s elections as well, when they don’t seem directly related to voting rights.

the conservative Federalist Society. One of Lopez's first decisions was to rescind a policy implemented by Warren to not prosecute bicyclists and pedestrians for certain traffic charges. A 2015 *Tampa Bay Times* report exposed the Tampa police department's relentless ticketing of Black cyclists for things like having inadequate lighting, or riding on handlebars, a dynamic local organizers have labeled "bicycling while Black." The report catalyzed a Justice Department investigation which ultimately confirmed the disproportionate enforcement.

New research shows how such low-level interactions with the police can undercut our democracy by reducing the number of people who participate in elections. A study I co-authored with fellow researcher Kevin Morris, published in December in the *American Political Science Review*, finds that traffic stops by police stops in Hillsborough County reduced voter turnout in 2014, 2016, and 2018 federal elections.

Our study compared the voter turnout of Hillsborough motorists who were stopped by police shortly before and after each election. Drawing on information about each person's turnout in past cycles, we found that these stops reduced the likelihood that a stopped individual turned out to vote by 1.8 percentage points on average. The effect held when accounting for characteristics like race, gender, party affiliation, past turnout, and prior traffic stops to improve our comparisons. The discouraging effect of stops was slightly higher in 2014 and 2018.

These results make clear that the collateral consequences of policing—including worsening outcomes for economic security, educational attainment, and health—also extend to political participation. If the communities who are most frequently subjected to policing are also discouraged from voting as a result, it could create a vicious feedback loop of political withdrawal.

Why would traffic stops make people less likely to show up to the polls? Past research has already established that the most disruptive forms of criminal legal contact, like arrest and incarceration, discourage people from voting. Our study shows that low-level police contact matters in the same way. If a traffic stop makes a motorist fear that the government will harm them, it can prompt a withdrawal from civic life that political scientists call "strategic retreat." Motorists might worry that a routine traffic stop could escalate into police violence, a more common outcome for Black people in particular. Beyond justified fears of violent victimization, voters might also bristle at the perception of being targeted to raise revenue through excessive ticketing. Accordingly, if incarceration 'teaches' would-be voters that their government is an alienating and harmful force in their lives, traffic stops could catalyze a similar form of 'learning.'

"I think that people see police as a part of the government," Bernice Lauredan, director of voter engagement at Dream Defenders, an organization that champions voting rights in Florida, told *Bolts*. "I don't believe any interaction with police is safe for people of color—having any interactions with police gives them a negative image of the government. And it may give them a negative idea of voting."

And while millions of white Americans have also been swept up in municipal ticketing efforts, the fines and fees in Florida as elsewhere disproportionately affect Black communities.

On average, we found that the deterrent effect was smaller for Black drivers: It reduced their likelihood to vote by 1 percentage point, compared to 1.8 for the overall population. We went further and looked at when voters had been stopped. If they had been stopped in the six months before the election, stops discouraged Black people from voting more than non-Black people. But as the time between a stop and the election increased, the effect weakened. That averaged out to a comparatively smaller effect over the whole two-year period.

We think that this counterintuitive result might be a mix of two things: on one hand, Black Americans probably have less to "learn" about government from a traffic stop, considering that Black Americans are more likely to have a family member in jail than other Americans. On the other hand, Black Americans probably know that a traffic stop is more likely to turn deadly for them compared to white drivers, which could cause "anticipatory stress" that reduces willingness to vote in the short term.

"Black folks and other people of color are criminalized in Tampa," Lauredan says.

While Florida Republicans have dialed up the use of criminalization to maintain political power, deep-blue urban dwellers also face the political ramifications of policing in their own backyards.

In New York City, for example, Mayor Eric Adams has dramatically increased police presence and encouraged police to be more proactive in punishing behaviors ranging from public drinking and dice games to carrying unlicensed firearms. New York Governor Kathy Hochul has also announced plans to beef up a "hot spots" policing initiative that focuses on gun violence—quite similar to the Memphis police squad ("SCORPION") that killed Tyre Nichols in January. Gun control policing efforts in New York could be driving a dynamic similar to the "strategic retreat" that our research demonstrated in Tampa—another study found that NYPD stop and frisk practices, which expanded significantly under Mayor Michael Bloomberg, may have reduced voter turnout in the 2006 and 2010 midterm elections.



ballooned from 80,000 to 578,000 between 2015 and 2021. In addition to boosting city revenues through regressive taxation, these traffic stops also function as a pipeline for gun possession arrests (which have been steadily increasing over time, despite criticisms from local prosecutor Kim Foxx).

The civic consequences of criminalization don't stop at voting, either. Research also shows that Americans who have been stopped by police, arrested, or incarcerated become less likely to engage with a range of public institutions that they perceive as surveilling them. Sociologist Sarah Brayne calls this phenomenon "system avoidance," and argues that the record-keeping practices of institutions like hospitals, schools, and banks—and the ability of state actors to surveil data from these institutions— justify why criminalized people withdraw from them. It's an ugly realization—harsh punishments and increased carceral surveillance are causing lasting damage to the social fabric of criminalized communities.

"The more communities are abused by the system, the more natural it is for them to feel alienated from it," said Yannick Wood, director of the criminal justice reform program at the New Jersey Institute for Social Justice, an organization that advocates reducing the interactions between the criminal legal system and democracy in New Jersey. "They don't feel like the system serves them, and they don't feel like their voices are represented, or even respected."

This is the most important takeaway from our research: American communities most likely to oppose "tough on crime" policy (thanks to their personal experience) are being pushed away from politics and from opportunities to steer policy change.

In Tampa, ticketing practices work in tandem with an extremely harsh regime of felony disenfranchisement that drives Floridians away from politics more explicitly. Almost one-quarter of the 4.6 million Americans barred from voting due to felony convictions live in Florida. The Florida Rights Restoration Coalition (FRRRC) led the successful 2018 campaign to pass a state constitutional amendment restoring voting rights to Floridians with felony convictions, though their victory was diminished by subsequent state legislation requiring fines and fees payments before voting rights were restored, leaving more than 1 million people without access to the ballot. Traffic stops affect an even larger share of Florida residents.

"Criminalizing any kind of behavior can have unintended consequences," FRRRC deputy director Neil Volz told *Bolts*. "Voting is a reflection of our belief that we're part of the system, that our voice matters, that we can take that past pain and turn it into something productive."

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The Impact of Voter Suppression on Communities of Color



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Studies show that new laws will disproportionately harm voters of color. Federal legislation is necessary.

PUBLISHED: January 10, 2022



**Ensure Every
American Can Vote**

■ **Vote Suppression**

Over the past decade, scholars have studied myriad ways in which certain state voting rules make participation disproportionately difficult for Americans of color — including strict voter ID laws, lines faced

on Election Day, and other facets of our election system. This analysis catalogs some of the most prominent research findings on the negative impact of voting restrictions on voters of color.

There is a large and growing pile of evidence that strict voter ID laws disproportionately impact voters of color.

- **Using county-level turnout data** around the country, researchers demonstrated that the racial turnout gap grew when states enacted strict voter ID laws.
- Researchers have also **looked specifically** at the turnout of individuals in North Carolina without proper identification, and they found that the enactment of the law reduced turnout. The turnout effects continued *even after* the strict voter ID law was repealed.
- **Another study** shows that voters in Texas who would be barred from voting absent the state's "Reasonable Impediments Declaration" (a court-ordered remedy allowing voters without proper IDs to participate) are disproportionately Black and Latino. The study argues that its "findings indicate that strict identification laws will stop a disproportionately minority, otherwise willing set of registered voters from voting."
- **An article** using a similar methodology and administrative records found that voters of color in Michigan were more likely to show up to the polls without proper identification.
- **Yet another study** used survey data to demonstrate that voters of color in states across the country lacked access to the needed IDs to vote in their state.
- While some studies have argued that voter IDs have little effect on *overall* turnout, it is clear that voters of color are less likely to have the IDs needed to participate.

Restrictions on Sunday voting — such as those proposed last year in Georgia and Texas — would fall disproportionately on voters of color.

- **Our research** showed that voters of color were substantially more likely to vote on Sundays in Georgia than white voters.
- **Another study** argues that these Sunday voters do not seamlessly transition to other days after cuts are made. For example, when Sunday voting was outlawed in Florida in 2012, Black voters who voted on Sunday in 2008 were especially likely to abstain from voting.

Voters of color consistently face longer wait times on Election Day — lines that would be exacerbated by cutting alternative options, such as vote-by-mail or expansive early voting hours.

- **Our report** from 2020 indicates that voters of color around the country reported longer wait times in the 2018 midterms, using self-reported wait times from a national survey.
- **Other researchers** have used cellphone data to demonstrate the same thing: waits are longer in neighborhoods with more racial and ethnic minorities.

- **Other research** — including **work from the Brennan Center** — has also used administrative data to show that polling places with fewer white voters have more slowdowns.

Even vote-by-mail options, however, don't completely level the playing field. Voters of color face more difficulties voting by mail, too.

- **Our research shows** that mail ballots were rejected at much higher rates than those of white voters in the Georgia primary in 2020.
- **Other studies** have found that this was true in Georgia and Florida's 2018 general elections, too.

Polling place consolidation is also especially harmful for the turnout of racial and ethnic minorities.

- The Brennan Center authored the **first academic study** documenting the turnout effects of the Covid-19 pandemic. We showed that polling place consolidation severely depressed turnout in Milwaukee's presidential primary — and that the effects were even larger for Black than white voters.
- This joins **other research** showing that voters of color are disproportionately impacted by polling place closures. This may be due to worse transportation access.



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The Civic Voluntarism of “Custodial Citizens”: Involuntary Criminal Justice Contact, Associational Life, and Political Participation

Michael Leo Owens and Hannah L. Walker

A growing body of research explores the influence of involuntary criminal justice contact on political participation, demonstrating that all types of contact weaken political participation. We posit, however, that personal connections to civil society organizations (CSOs) moderate the negative effects of involuntary criminal justice contact on political participation, particularly political activism beyond registering to vote and voting. We test this proposition with individual-level and aggregate-level data from metropolitan and municipal Chicago. Our findings confirm a paradox of participation by custodial citizens. One, we demonstrate positive, statistically significant, and substantive effects of personal connections to CSOs on *nonvoting political participation* by custodial citizens. Two, the negative effects of involuntary criminal justice contact on *voting participation* among individuals and communities may endure, despite personal connections to CSOs, even in a state where the franchise is restored immediately after incarceration. Our study suggests that an associational account of political participation deepens our understanding of the political behavior of custodial citizens and their communities in the age of mass incarceration.

In the United States, citizens (and immigrant denizens) may experience unwanted, even unwarranted, contact with the criminal justice system. Categories of such criminal justice contact¹ include police stops of drivers and frisks of pedestrians, arrests without formal charges and convictions, diversion from court convictions via “problem-solving” or specialized courts such as drug courts, and court convictions for misdemeanors and

felonies, accompanied by sentences of incarceration, parole, or probation, along with the imposition of fees and fines by the courts.² Criminal justice contact also includes unwanted interactions between youth and “resource officers” (i.e., police working in elementary and secondary schools), arrests and detentions of youth in juvenile or adult correctional facilities, and community supervision by and beyond “youth diversion programs.”³ The myriad of

A list of permanent links to Supplementary Materials provided by the authors precedes the References section.

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moments for unwanted contact between citizens and the criminal justice system in the United States multiplies membership in the group scholars call “custodial citizens.”⁴ They are the growing set of citizens law enforcement agencies have detained for questioning, arrested, charged, convicted, or placed under some form of correctional control for suspicion of or actual criminal behavior.

The number and rate of Americans who are custodial citizens are great, even as violent and property crime rates fall and public punitiveness abates.⁵ There are 110,235,200 people in the criminal history files of the 50 states and U.S. territories, an increase of 130% from 1993.⁶ Nearly one in every three adults has a record of criminal arrest.⁷ While approximately 9% of adults have felony convictions, we generally know that the percentage of adults with misdemeanor convictions is many times larger.⁸ Nearly one in 34 Americans is under some form of correctional control.⁹ Millions of adults and juveniles who were once under correctional control for criminal convictions are now “off paper” but not necessarily fully (re) integrated into society.

Even when citizens are no longer in the unwanted grasp of carceral government, deep imprints of their criminal justice contact remain. Law enforcement agencies retain custody of, often publicize, and may permit others to monetize all information about criminal justice contact, whether slight or severe. It is contained in a vast assortment of “criminal intelligence databases, police blotters, rap sheets, court records, presentence reports, prosecutors’ files, probation files, and jail and prison databases,” inclusive of photographs and residential addresses.¹⁰ Thus, the half-life of criminal justice contact is immeasurable. Its effects, however, are not.

Custodial citizenship, whether from short detentions for questioning at police departments and county jails to long stretches in state or federal prisons, produces a range of negative social and labor effects for those who have had criminal justice contact. They include stigma, depression, lost earnings, unemployment, and homelessness.¹¹ Custodial citizenship also has concomitant negative consequences for the children and families of the citizens whom law enforcement agencies have detained for questioning, arrested, charged, convicted, or placed under some form of correctional control. Examples include weakened family ties and increased behavioral problems for children.¹² Custodial citizenship may even have negative effects on communities with greater residential densities of such citizens, measured for instance by residential churn, social control, and crime.¹³

What about the polis and politics? Custodial citizenship affects government and governance at every rung of the federalist ladder. It shapes the demographics and decisions of the legislative, executive, and judicial branches of localities, states, and the nation.¹⁴ It influences legislative reapportionment and redistricting;¹⁵ juries

and electorates;¹⁶ political candidacies;¹⁷ vote margins in elections and referendums;¹⁸ distributive politics (e.g. siting of prisons and allocation of some intergovernmental transfers);¹⁹ mundane matters of public policy (e.g., measuring labor force participation and unemployment);²⁰ and municipal revenue, particularly when city managers, police, and courts collude for exploitative revenue.²¹

Furthermore, custodial citizenship is consequential to civic voluntarism by adults, particularly political participation.²² As criminal justice contact increases, intensifies, and lengthens at the individual and community levels, scholars generally observe that custodial citizens and communities with residential concentrations of them participate less in political activities, especially voting.²³ Declines in voting by custodial citizens and their communities are not a function of imprisonment and felony disenfranchisement alone. Citizens who police officers have detained for questioning, for instance, report voting less, too.²⁴ Because criminal justice contact reduces the number of people participating as voters, as well as possessing positive civic sentiments,²⁵ it is “an important force in shaping American mass politics.”²⁶

Rightly, scholars of political behavior, institutions, and normative theory focus on the existence, size, and causal mechanisms of the negative democratic effects of custodial citizenship.²⁷ The study of the civic consequences of criminal justice contact is valuable—theoretically, empirically, and substantively. It increases, in particular, our understanding of the anti-democratic consequences of custodial citizenship for political attitudes and behaviors. It brings into better focus the “second face of the American state,” revealing the punitive profile of “the governing institutions and officials that exercise social control and encompass various modes of coercion, containment, repression, surveillance, regulation, predation, discipline, and violence” that condition civic voluntarism while influencing other political phenomena.²⁸ Nonetheless, as important, we argue, is the need to identify the set of factors that may attenuate the negative influence of “the state’s more controlling ‘second face’”²⁹ on civic voluntarism.

While the study of the negative democratic effects of criminal justice contact invites an assortment of scholarly interventions, we widen here the disciplinary lens of the politics of criminal punishment to give greater attention to civil society. This is necessary. Deliberate consideration of civil society is atypical for political penologists interested in the democratic consequences of the American carceral state. Scholars of punishment and politics—be they Americanists, normative theorists, or comparatists—tend to neglect the potential and limits of civil society organizations (CSOs) to influence political participation by custodial citizens.³⁰ Even when they do not neglect civil society, studies of the relationship between criminal justice contact and political behavior and attitudes are limited.

For example, they overemphasize “ex-felon serving institutions,” which likely lack capacity for political mobilization.³¹ Yet there is reason to suspect that civil society and a variety of CSOs may mitigate the negative effects of criminal justice contact on civic voluntarism.

A large body of scholarship exists about the potential of CSOs to shape political behavior through civic skills development, community organizing, services provision, and opportunities for activism and mobilization, inclusive of participation.³² We leverage it to deduce a proposition about how and why personal connections to CSOs may positively influence political participation by adult custodial citizens, just as personal connections to CSOs routinely do for adult *noncustodial* citizens.³³ Taking what Han calls “an organizational approach to understanding [political] activism”³⁴ and applying it to custodial citizens, we explore whether personal connections to one set of varied CSOs—formal, tax-exempt nonprofit organizations—are associated with weaker negative effects (or positive ones) of criminal justice contact on political participation. We employ individual-level data from metropolitan Chicago on criminal justice contact, connections to CSOs, and participation and aggregate data on conviction rates, CSO densities, and political participation (i.e., voting and citizen-initiated contact with government) in municipal Chicago. Our data permit us to estimate relationships among custodial citizenship, civil society, and political forms of civic voluntarism.

Four findings are key: 1) independent of personal connections to CSOs, criminal justice contact is neither associated with voting nor non-voting; 2) custodial citizens *without* connections to CSOs participate less in politics via voting *and* nonvoting; 3) personal connections to CSOs are associated with *increased nonvoting political participation* among those who have had the heaviest criminal justice contact (i.e., correctional control via incarceration and community supervision); and 4) however, personal connections to CSOs may only increase *some* forms of political participation among custodial citizens.

Criminal Justice Contact and Political Participation

People, as the aphorism states, “participate when they can, when they want to, and when they are asked.”³⁵ Resources, orientations, and recruitment, along with “rewards, interests, and beliefs,” influence political participation.³⁶ Institutions and policies affect political participation, too. As Mettler and Soss³⁷ stress, “living under a given policy regime affects citizens’ goals, beliefs, and identities—and hence, the possibilities and limits for future political action.” However, individual-level factors associated with the likelihood of criminal justice contact build resource, efficacy, and recruitment barriers to political participation by custodial citizens. Also, the American carceral regime, buttressed primarily by white public support for punitive

policies,³⁸ including policies that exclude custodial citizens from the ballot box and public office, supplemented by punitive policies that the state, market, and civil society coproduce,³⁹ prevents many custodial citizens from thinking and behaving as full political members of society. It is unsurprising then that research on the democratic effects of the American carceral state draws the same conclusion as scholarship on the democratic effects of the American welfare state—“some policies draw citizens into public life and others induce passivity.”⁴⁰

Aside from executing the administration of public safety and criminal corrections, criminal justice contact as a “political learning situation”⁴¹ for custodial citizens negatively affects the political attitudes, ambitions, and activities of many custodial citizens, arguably by socializing them to see themselves as citizens with less liberty, equality, dignity, and regard than other citizens.⁴² Additionally, scholars observe that the relationship between criminal justice contact and *electoral* participation, particularly voting, is negative and the negative relationship strengthens as the grasp of criminal justice contact tightens.⁴³ The most cited empirical study of the political attitudes and behavior of custodial citizens in the United States, for instance, estimates the likelihood of decline in voting by persons with arrests as 7%, criminal convictions as 10%, short stints in jail or prison as 17%, and long stints of incarceration at almost one-third.⁴⁴ Furthermore, in communities where the residential density of custodial citizens is higher and where more residents are removed from them through imprisonment as “coercive mobility”⁴⁵ voting is lower, too.⁴⁶

Yet even when we observe that custodial citizens participate electorally at lower levels than their non-custodial counterparts, their probabilities of voting or participating politically in other ways are never zero. Additionally, some custodial citizens “double down on democratic values and practices,”⁴⁷ despite punitive policy designs and negative feedback of the carceral state.⁴⁸ Acting as if their political participation matters and to make it so, many custodial citizens participate, defying low expectations of collective action improving their lives.⁴⁹ Furthermore, criminal justice contact may reduce voting *without* affecting other political activities (e.g., contacting government officials, signing petitions, and demonstrating).⁵⁰ Plus, because elections are infrequent and often noncompetitive—limiting their service as a strong means of democratic accountability and civic engagement—custodial citizens, like noncustodial ones, could perceive participation outside the voting booth as a better means for sharing preferences with policymakers and for achieving policy responsiveness than participation in elections. And some people prefer to participate via nonvoting political activities instead of by electoral ones.⁵¹ Therefore, borrowing Han’s caution about political activism by noncustodial citizens, generally, when it comes to our

knowledge about the political participation of custodial citizens, “we cannot assume that findings related to voting import directly to other forms of activism.”⁵²

Civil Society Organizations and Custodial Citizens

Civil society is “the primary agent of political dialogue and citizen influence” when people participate politically in the United States, particularly in cities and metropolitan areas.⁵³ It has been since the nineteenth-century travels of Alexis de Tocqueville, who was one of the first empiricists of civil society *and* criminal justice contact in America.⁵⁴ CSOs, as de Tocqueville observed then, pave the way for political participation. It is truer in the twenty-first century, where “every day thousands of nonprofit organizations around the country are busy organizing and creating opportunities for new associations” for individual and collective efficacy to solve public problems by (and beyond) voting and nonvoting political participation.⁵⁵

Civil Society Organizations as Hindrances to Participation

Although we expect CSOs to positively affect the political participation of custodial citizens connected to them, reasons exist to be pessimistic about their influence. There has been a steady growth in the number of charitable nonprofit organizations and nonprofit advocacy organizations, which often do not mobilize clients and rely little on memberships to advance their interests. The growth of CSOs focused on charitable purposes and without mass memberships (e.g., American Society for the Prevention of Cruelty to Animals or the American Civil Liberties Union) shrinks stores of social capital for conversion to political capital.⁵⁶ Such organizations “focus on maximizing the number of people involved without developing their capacity for civic action.”⁵⁷ It likely undermines the political utility of CSOs for citizens,⁵⁸ be they custodial or noncustodial.

Also, custodial citizens, generally, are likely to reach out to and be contacted by *social services* CSOs that focus on meeting immediate needs (e.g., shelter, substance abuse recovery, employment).⁵⁹ Such organizations seek to support their own persistence first and foremost. Fearing that mobilization of clients could negatively affect their finances, and misunderstanding government regulations of political activities by nonprofits, social services CSOs tend to avoid politics, limiting their level of political engagement.⁶⁰

Even when CSOs foster political engagement, many bias their mobilization. CSOs tend to target individuals who demonstrate greater activism⁶¹ and who possess more positive social constructions and political capital.⁶² Given the central tendencies of the racial and class demographics of custodial citizens, the political strategies of many CSOs disregard custodial citizens and their communities, preferring to mobilize the less marginalized.⁶³

Civil Society Organizations as Pathways to Participation

Those realities notwithstanding, it is plausible that civil society positively affects the participation of custodial citizens. Political participation has associational anchors and “organizational roots” in civil society.⁶⁴ Routine functions and incentives of charities, associations, and other forms of CSOs strengthen the anchors and roots for political participation.⁶⁵ Moreover, many CSOs engage in a variety of activities consequential for the political behavior of clients, volunteers, members, and other stakeholders (figure 1).⁶⁶

Many CSOs, inclusive of many faith-based/religious CSOs like black churches⁶⁷ and secular CSOs like labor unions, educate, train, and socialize individuals for political participation.⁶⁸ They teach people to develop opinions and perceive their interests, form group consciousness and identify shared grievances, speak their concerns, and amplify their voices.⁶⁹ They cultivate personal commitments to public issues and collective problem solving.⁷⁰ They help individuals develop political efficacy, education, and civic skills for participation. Political engagement by individuals connected to charitable CSOs can increase from their participation in the “ordinary and routine” practices and activities of the organizations—“activity that has nothing to do with politics or public issues, can develop organizational and communications skills that are relevant for politics and thus can facilitate political activity.”⁷¹ For instance, the one-on-one conversations, opportunities for public speaking, and collective problem solving of sacred and secular CSOs influence civic development, even when they are indirectly political.⁷² As a result, connections to CSOs by custodial citizens should reduce barriers to participation, including resource, efficacy, and recruitment barriers.⁷³

Beyond dispositional and institutional reasons, people participate politically because others recruit and mobilize them,⁷⁴ and CSOs can channel people into opportunities for political participation. Personal connections to CSOs increase the likelihood that “political leaders” will attempt to mobilize them for political influence: “First, organizations mobilize their own members, often explicitly Second, organizations expose their members to mobilization by sympathetic politicians, activists, and other organizations.”⁷⁵ Thus, CSOs create bridges that connect the civic and the political spheres, which can positively affect resources, orientations, and recruitment for political participation through mobilization.⁷⁶ Hence, “people who belong to associations are more likely to be mobilized and more likely to participate than people who do not belong.”⁷⁷ As well, some CSOs are capable of activating and mobilizing clients, constituents, and members for political participation because of “reciprocal service

Figure 1
Civil society organizations pave ways for political participation

| Political Foundation-Building | Educate for Political Participation | Enable & Organize Political Participation | Lobby, Advocate, & Litigate for Political Change | Electioneer to Influence Votes for Candidates & Referendums |
|--|---|--|---|---|
| Framing and problem definitions | Notification of political, social, and civil rights (e.g., right to vote or to receive public benefits) | Voter registration | Contact policymakers (elected and appointed) to oppose or support existing or new legislation, laws, regulations, rules, ordinances, services, directives, etc. via direct and grassroots lobbying and advocacy | Recruit candidates |
| Dialogues to identify shared interests, values, concerns | Voter education guides | Voter mobilization (e.g., Get Out the Vote or “Souls to the Polls” events) | File court cases | Appeal to voters to retain or change who holds public office |
| Activities to foster identities, group consciousness, solidarity | Candidate forums | Organize or publicize legislative lobby days | | Create PACs, parties, and other formal political associations |
| Participatory skills-building (e.g., public speaking, collective critique, canvassing, etc.) | Policy discussion groups | Organize or publicize petition campaigns | | “Express advocacy” and “Issue Advocacy” |
| Community problem solving activities via committees and associations | Town halls with policymakers | Organize or publicize protests | | |
| Enrollments in public programs | Decennial Census education | Certification as an official polling station for voting | | |
| | | Promote referendums | | |
| Perceived as least political | | ←—————→ | | Perceived as most political |

provision” that produces patron/client relationships that benefit organizations, their clients, and even political elites.⁷⁸

Evidence is strong and consistent that CSOs, even human services CSOs, can inform, activate, and mobilize *marginalized* people. CSOs help them overcome the greater resource, efficacy, and recruitment barriers to participation that they face relative to people who are not marginalized.⁷⁹ The provision and shaping of routine opportunities for engagement often allow CSOs to demonstrate that collective action produces symbolic and substantive rewards for marginalized people.⁸⁰ Accordingly, civic voluntarism among lower SES individuals is higher in communities with more and stronger CSOs than in neighborhoods with fewer and weaker ones.⁸¹

Furthermore, CSOs are central to social welfare delivery in communities with high rates of custodial citizens. Again, it is unusual for human services CSOs to explicitly emphasize political action and mobilization. Nevertheless, some CSOs deliberately enable custodial citizens to develop and practice political resistance.⁸² Moreover, some CSOs exist to politically activate and mobilize custodial citizens and their communities.⁸³ They provide custodial citizens with a greater sense of civic and political worth for fostering “new citizenship” and political participation *through* associational life (see figure 2).⁸⁴

In sum, CSOs can perform multiple roles that bear on political participation. From fostering group consciousness, solidarity, and social capital to spending resources to influencing elections, CSOs can develop the democratic capacities, sentiments, and activities of citizens, custodial

or otherwise. Accordingly, there are more reasons than not to expect that personal connections between custodial citizens and CSOs mitigate—and possibly reverse—the negative effects of criminal justice contact on political participation by custodial citizens. Therefore, we predict that *custodial citizens with personal connections to CSOs are more likely to participate by voting and nonvoting political activities than custodial citizens without personal connections to CSOs.*

Data, Measures, and Methods

We test our prediction about the influence of criminal justice contact and CSOs on the political participation of custodial citizens with individual and aggregate-level data from the Chicago metropolitan area. Chicago is more alike than different from many major metropolitan areas and central cities. “Chicago is not absolutely average, to be sure But Chicago has faced the dynamics that have confronted old major cities in the country—growth, decline, crime, and boom times. In this sense Chicago [is] both unique and broadly representative, grounded in a thoroughly documented history and context that helps us understand key patterns.”⁸⁵ Moreover, trends in criminal justice contact in Illinois and Chicago “are broadly consistent with trends in crime and incarceration throughout the United States.”⁸⁶

Illinois is neither high nor low regarding correctional control: The rate of adults incarcerated, paroled, or on probation is 1:38, placing Illinois thirty-fourth among the fifty states. Chicago, its key metropolitan area and central city, has a large subpopulation of custodial

Figure 2
Redeeming citizenship by/for people with criminal justice contact

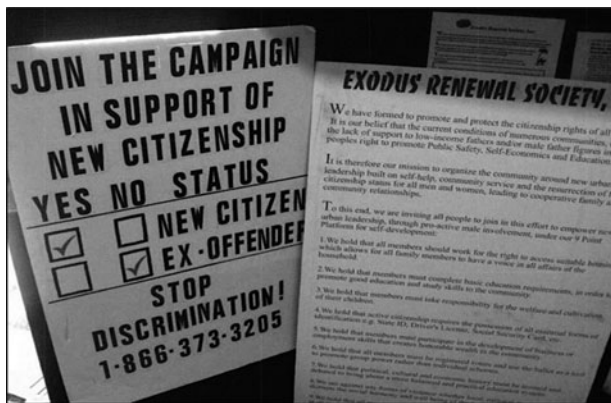


Photo credit: Michael Leo Owens

citizens and is the primary destination for the return of persons released from correctional institutions, inclusive of the local Cook County Jail. For instance, of the approximately 39,000 people annually released from prison in Illinois, 51% of them reside in the city of Chicago, mainly in seven of 77 neighborhoods, marked by “concentrated disadvantage” in terms of poverty and unemployment rates.⁸⁷

Additionally, restrictions on voting by people under correctional control via prison, jail, parole, or probation with felony convictions in Chicago are comparable to those covering custodial citizens in most metropolitan areas (and states) in the United States. Under Illinois law, custodial citizens are only disenfranchised while incarcerated. Like those in fifteen other states and the District of Columbia, custodial citizens in Chicago who are on parole or probation (i.e., most Chicagoans under correctional control) may vote. Custodial citizens in Illinois may vote, too, while *awaiting* trial, be they jailed or bonded.

Most important, because of the 2014 Chicago Area Study (CAS), there is adequate survey data permitting estimates of the relationship among criminal justice contact, personal connections to CSOs, and political participation. The CAS, which our online appendix details, surveyed 1,294 respondents, including an oversample of blacks and Latinos, living in the central city and suburbs of metropolitan Chicago.⁸⁸ Of course, we know the statistical limits of self-reported data relative to strengths of administrative data on criminal justice contact and political participation.⁸⁹ We lack the latter, however, for Chicago or Illinois, particularly for measures of personal connections to CSOs and participation beyond registering to vote and voting.

In addition to the CAS, we collected aggregate data on criminal justice contact, CSOs, and political participation

in the city of Chicago at the level of neighborhood beats of the Chicago Police Department. We matched a set of measures of criminal justice contact, CSO density, and voting and nonvoting political participation to each police beat. Police beats, which are our units of supplemental analysis (N = 270), allowed us to leverage an important form of nonvoting political participation in Chicago, namely attendance at police beat meetings⁹⁰ (and to address concerns about selection and response bias associated with surveys like the CAS).⁹¹

Generally, Chicago is a city of public meetings, as is true of many cities.⁹² In particular, it is a city that uses public meetings as vehicles for citizen contact with government officials. The police beat meetings of the Chicago Alternative Policing Strategy (CAPS) are one important set of meetings in Chicago.⁹³ At least 53,000 police beat meetings were held from 1995 through 2016, with total annual attendance ranging from 21,000 to approximately 60,000 people.⁹⁴ The meetings occur at least once each quarter, offering residents opportunities for face-to-face interaction with law enforcement and other government officials, where they make requests related to policing and other municipal services. The meetings allow municipal agencies and citizens to collaborate to coproduce improved police-community relations and greater public safety. “With the exception of elections, it is difficult to identify a municipal activity of any kind attracting similar levels of civic participation—anywhere in the country.”⁹⁵ Furthermore, “Chicago’s policing program has helped to even out the opportunities to participate in community governments, with the greatest increase in collective participation by African-Americans.”⁹⁶

CAS Survey—Criminal Justice Contact and Personal Connections to Civil Society Organizations

Our key independent variables are criminal justice contact and personal connections to CSOs. Our measures of criminal justice contact come from a CAS question: *We are interested in how much contact people have had with the police. In the past five years, have you . . . (please select all that apply)—been questioned by the police for any reason, been on probation or parole, served time in jail and/or prison.* Eleven percent of respondents indicated that the police had questioned them and five percent reported being under correctional control in the past. We model self-reported levels of criminal justice contact in the past five years with dummy variables (Yes = 1, 0 = No) for *detained* (i.e., questioned by the police) and *correctional control* (i.e., prison, parole, or probation). The CAS data do not permit a refined assessment of the length of time either under correctional control or length of time since criminal justice contact.

The CAS queried respondents about their personal connections to CSOs: *Some people participate in groups and organizations while others do not. Do you currently belong to,*

volunteer with, attend meetings of, or pay dues for any of the following types of groups? Types of groups included: (1) religious group; (2) neighborhood or community organization; (3) labor union; (4) a professional organization; (5) an ethnic/cultural organization; (6) political organization that focuses on a specific cause; (7) political party; (8) civic organization (e.g., Rotary Club); (9) other; and (10) none of these. Initially, we coded the types of CSOs that respondents identified having personal connections to into six dummy variables: *religious, community, ethnic, labor, political, and other*. We tested the independent effects of these dichotomous variables. Ultimately, we exclude them because the CAS provided insufficient data for rigorous tests and robust results. Consequently, our measure of personal CSO connections is a dichotomous variable, measuring personal or direct participation in any type of group (Yes = 1, No = 0). Forty-two percent of respondents reported connections to CSOs. Although we know that higher levels of personal involvement in organizations produce greater personal involvement “in governmental politics” and other arenas of political participation such as elections,⁹⁷ the CAS did not measure the degree of personal involvement in CSOs.

We use two measures of *voting* from the CAS—whether a respondent was registered to vote and whether they reported voting in the 2012 presidential election. Among registered voters in the CAS, 89% reported voting in 2012.⁹⁸ We measure *nonvoting political participation* by self-reports of seven political activities, inclusive of a few electoral activities besides voting, respondents performed within 12 months prior to the survey: (1) signed a petition; (2) shared political information via social media; (3) attended a protest; (4) wrote a letter to an elected official; (5) donated to a political cause; (6) volunteered for a political campaign; or (7) issued a political opinion publicly in the form of an op-ed or calling into a radio show. We scaled the items, creating a conventional nonvoting political participation index. It ranges from zero to seven, with a mean of 1.53 activities.⁹⁹ The variety of activities in our index raises the possibility that one activity (e.g., protesting) drives observed relationships between criminal justice contact and nonvoting political participation. To evaluate the appropriateness of the use of the index we model the independent effects of each type of criminal justice contact and CSO connections on each item in the index. Figure 3 displays the marginal effects.

There is no evidence that correctional control negatively correlates with any item in the *nonvoting political participation* index. Instead, we observe that criminal justice contact positively correlates with contacting a government official, volunteering, and donating money to a cause. It is otherwise unrelated to nonvoting political participation. Figure 3 also does not reveal that any one activity or type of activity underlies a positive association

between police detentions for questioning and nonvoting political participation. Finally, figure 3 illustrates that CSO connections statistically increase participation in all activities but one—sharing opinions on TV, radio, or in a newspaper. Given the general absence of a pattern of criminal justice contact affecting nonvoting political participation, we retain the seven-item scale as our measure of political participation beyond voting and registering to vote.

We control for race, gender, age, education, income, political interest, efficacy, party identification, marriage, and unemployment. The CAS stratified its sample by race. It did not stratify by other characteristics co-occurring with criminal justice contact (e.g., income and education). There may be an underrepresentation of respondents with criminal justice contact because of under-sampling some subpopulations or respondents concealing criminal justice contact. To mitigate potential data biases arising from sampling error, we apply weights to the CAS data where possible, using U.S. Census Bureau estimates of the demographics of the Chicago metropolitan area.¹⁰⁰

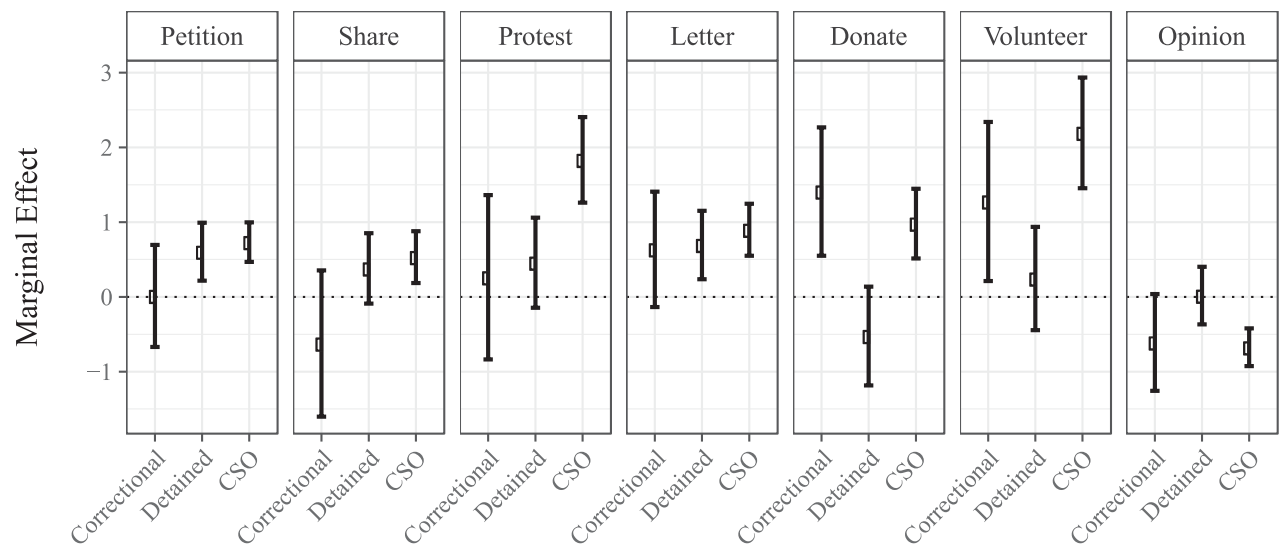
Aggregate Data—Criminal Justice Contact and Density of Civil Society Organizations

We rely on a unique set of public records of criminal convictions to measure criminal justice contact at the community level. Our data from the Chicago Justice Project (CJP), originally collected by the Office of the Chief Judge of the Circuit Court of Cook County (CCCC), includes records of criminal convictions and sentences by the Criminal Division of the CCCC between 2005 and 2009.¹⁰¹ The records are cases the State’s Attorney brought against 173,204 individuals it charged with felonies. For our measure and analysis, we retained felony conviction records for residents of the city of Chicago, relying on physical home address the court provided for each defendant. We successfully geocoded 90% of the 42,200 cases in the dataset (N=37,980). We corrected for an extremely right-skewed raw rate of felony convictions by logging conviction rates ($\log(\text{Convictions})$).¹⁰²

We also measure density of CSOs at the community level, which somewhat parallels or proxies for personal connections to CSOs. Extant research on civil society, especially in Chicago, tells us important things about the measure, beyond the fact that it is an indicator of the organizational foundation and milieu of communities.¹⁰³ First, CSO densities are relatively stable by decade. Second, CSO density is positively correlated with events for civic engagement and action, especially “charity events, community festivals, public meetings, recreational activities, and workshops,” and political protests and rallies.¹⁰⁴ Third, CSO density is a strong predictor of the propensity of “collective action” in or by neighborhoods in Chicago.¹⁰⁵

We constructed our CSO density measure from the Exempt Organizations Business Master File Extracts¹⁰⁶ of

Figure 3
Marginal effects of criminal justice contact on participation



Notes: The figure reflects the marginal effect of correctional control, being detained by the police, and CSO connections on each item in the nonvoting participation index. Coefficients reflect fully specified models, located in tables A6 and A7 of the online appendix.

the Internal Revenue Service (IRS). The IRS identifies all active, tax-exempt, CSOs in the United States with annual incomes greater than \$25,000 that are registered with the federal government and are required to file annual financial disclosure reports. Also, the IRS dataset classifies all nonprofit organizations by missions, describes the types of services they provide, and identifies their physical addresses. We chose all CSOs operating in Chicago that the IRS categorized as 501(c)(3), 501(c)(4), 501(c)(5), 501(c)(7), and 501(c)(9) nonprofit organizations (N=13,932).¹⁰⁷ We aggregated CSOs to police beats, standardizing counts by rates of CSOs per 1,000 people. Like the convictions data, densities of CSOs skewed rightward,¹⁰⁸ requiring logging CSO densities ($\log(CSO)$).

We use two measures of nonvoting political participation that involve contacting public officials, either of a community or particularistic bent.¹⁰⁹ The first measure is a multiyear variable of public attendance at police beat meetings, using proprietary CAPS data on the number of civilian attendees per beat per month from 2013 through 2015.¹¹⁰ We regard attendance at police beat meetings as an aggregate equivalent of survey items measuring attendance at community or political meetings. We calculated both the mean average attendance across all years by police beat and rates of meeting attendance per 1,000 people. Attendance rates ranged from .55 to 36.2, with a mean of 3.6.

Influenced by a resurgence in the study of nonvoting political participation, particularly citizen-initiated contact with government,¹¹¹ our second measure of contact

operationalizes nonvoting political participation by instances of requests for nonemergency services from the city of Chicago via its 24-hour municipal 311 call system.¹¹² We treat a 311 call as an analog to the conventional survey measure of contacting a public official. Furthermore, 311 callers demonstrate “custodianship” for the commons, contribute to the maintenance of public goods, and reveal a “civic disposition—that is manifest in a broader pattern of political participation, including behaviors like voting and volunteering.”¹¹³ Theoretically, variation in criminal justice contact and CSO densities should influence 311 calls. If higher levels of criminal justice contact, for instance, degrade trust in government, whereby less engagement with political life results, eroded trust should diminish the propensity of communities to contact governments to address neighborhood issues.¹¹⁴ We geocoded all 311 calls in Chicago in 2014 (N = 584,644) and calculated calls per 1,000 people at the level of police beats. Rates ranged from 1.9 calls to 221.1 calls, averaging 87.5 calls.¹¹⁵

Turning to voting participation in the municipality of Chicago, our two measures are turnout by registered voters in the 2014 Illinois general election (*2014 voter turnout*) and voter turnout in the 2015 mayoral election (*2015 voter turnout*), derived from electoral precinct-level data from the municipal Board of Election Commissioners. Estimated mean voter turnout for the 2014 election is 47% and 38% for 2015. Descriptive statistics and correlation matrices for all variables in our individual-level and community-level analyses are available from tables A1–A4 of the online appendix.¹¹⁶

Table 1
Effects of criminal justice contact on political participation

| | Registered to Vote ^a | Voted in 2012 | Nonvoting Political Participation ^b |
|----------------------|---------------------------------|--------------------|--|
| Correctional control | -0.131 (0.586) | -0.881 (0.485) | 0.061 (0.122) |
| Detained by police | 0.754* (0.372) | 0.099 (0.345) | 0.191** (0.068) |
| CSO connection | 0.683** (0.234) | 0.549* (0.245) | 0.293*** (0.050) |
| Black | 0.289 (0.396) | 0.439 (0.383) | -0.077 (0.075) |
| Latino | -0.599* (0.265) | -0.120 (0.307) | 0.098 (0.066) |
| Other race | -0.572 (0.342) | -0.578 (0.395) | -0.078 (0.098) |
| Political interest | 0.053 (0.108) | 0.321** (0.116) | 0.166*** (0.026) |
| Political efficacy | 0.472** (0.151) | 0.261 (0.161) | 0.106** (0.037) |
| Education | -0.009 (0.130) | 0.195 (0.136) | 0.069* (0.030) |
| Female | 0.075 (0.224) | 0.111 (0.239) | -0.007 (0.049) |
| Age: 18-34 | -0.525* (0.249) | -0.126 (0.272) | 0.147* (0.061) |
| Age: 65+ | 1.273* (0.506) | 0.873* (0.423) | -0.067 (0.067) |
| Democrat | 0.308 (0.332) | 0.489 (0.335) | -0.014 (0.066) |
| Independent | -0.572 (0.301) | -0.463 (0.307) | -0.055 (0.066) |
| Income | 0.163* (0.079) | 0.267** (0.089) | -0.015 (0.017) |
| Married | -0.347 (0.250) | 0.142 (0.274) | 0.039 (0.056) |
| Unemployed | -0.199 (0.275) | -0.608* (0.289) | 0.132 (0.074) |
| Constant | 0.546 (0.609) | -0.925 (0.678) | -0.749*** (0.152) |
| Observations | 1,229 | 1,140 | 1,229 |
| Log Likelihood | -335.595 | -293.608 | -1,699.86 |
| AIC | 707.191 | 623.216 | 3,435.71 |

Notes: ^aWe model registered to vote and voted in 2012 with logistic regression.

^bWe model nonvoting political participation with Poisson regression. A dispersion test, using the R package "AER," yielded an estimate of .662, suggesting the data are not burdened by overdispersion.

*p<.05; **p<.01; ***p<.001.

Empirical Analyses and Results

Individual-Level Analysis

To get a baseline estimate of the relationship between criminal justice contact and political participation, we assess the relationships of criminal justice contact and measures of voting participation and nonvoting participation at the individual level. We would expect, based on extant studies, that voting participation by custodial citizens would be lower than voting participation by noncustodial citizens, with more intense criminal justice

contact being associated with greater reductions in voting. The results in table 1 partially support the expectations.

More intense criminal justice contact, measured by correctional control via imprisonment or jailing or community supervision through parole or probation, is negatively associated with individuals being registered to vote and having voted. However, the relationship is not statistically significant. Conversely, and curiously, being detained by the police for questioning is associated with an increased likelihood of voter registration. Yet, like having been under correctional control, we failed to

observe any association between police detentions for questioning and voting in 2012.

Turning to the effect of contact on nonvoting participation, we would expect a positive relationship, as most previous studies conclude. As table 1 shows, the results from Chicago partially support the expectation.¹¹⁷ Respondents who report that the police had detained them for questioning are likely to have greater levels of nonvoting political participation. The finding confirms results from other studies that show less intense criminal justice contact may increase nonvoting participation.¹¹⁸ Yet there is no evidence from Chicago that incarceration and community supervision increase or decrease nonvoting participation. This, too, is relatively consistent with prior research.¹¹⁹

So far, we have evidence that criminal justice contact may have varied effects on voting and nonvoting political participation. The positive associations we observe for civic voluntarism through personal connections to CSOs—controlling for other factors, including correctional control—are, however, consistent for voting and nonvoting participation (table 1). The relationship achieves statistical significance across all three models. Personal connections to CSOs are associated with increased participation, as we theorized. This strengthens our expectation that personal connections to CSOs are positively associated with voting and nonvoting political participation by custodial citizens.

Turning our attention to examining the moderating effect of personal connections to CSOs on the political participation of individuals with criminal justice contact, we explore the relationship between criminal justice experiences with personal connections to CSOs. The results in table 2 suggest that personal connections to CSOs correlate with a greater propensity of *nonvoting* participation by custodial citizens, relative to those with criminal justice contact who lack CSO connections. The interaction of correctional control and personal connections to CSOs produced results that accord with our hypothesis about political participation beyond registering to vote and voting. Personal connections to CSOs are positively related to greater nonvoting political participation by people who have been under correctional control, after controlling for factors that conventionally correlate with civic voluntarism.

To better interpret the relationships among criminal justice contact, connections to CSOs, and *nonvoting* political participation, we calculated the expected score on the nonvoting political participation index by degree of criminal justice contact. We did this for custodial citizens with and without connections to CSOs. Regardless of the degree of contact, personal connections to CSOs improve nonvoting participation by custodial citizens relative to those lacking connections to CSOs (figure 4). Absent CSO connections, criminal justice contact diminishes the expected level of nonvoting political participation. Those

without criminal justice contact and without CSO connections have an expected score of 1.25 activities on the index. The expected score on the index shrinks to one activity for those who have been under correctional control and lack CSO connections. Among similarly situated custodial citizens with CSO connections who have been under correctional control, however, the expected score on the index *increases* to 2.25 activities.¹²⁰

Remarkably, the size of the positive association of personal connections to CSOs on nonvoting political participation is larger for those with criminal justice contact than for those without it. For noncustodial citizens, personal connections to CSOs improve nonvoting political participation by about .3 political activities. Among Chicagoans the police have detained, CSO connections improve the expected score on the index by about .6 political activities. Personal connections to CSOs also increase the expected score by 1.25 political activities among respondents who have experienced some form of correctional control. Moreover, the expected value of nonvoting participation for custodial citizens with personal ties to CSOs *exceeds* that of their counterparts without criminal justice contact.

Nevertheless, personal connections to CSOs appear to *not* moderate the depressive effects of criminal justice contact on voting. Our finding, which is important, may reflect that the material and attitudinal barriers to voting custodial citizens face remain high despite any capacity of CSOs to pave the way for voting. Even in places like metropolitan Chicago, where *all* custodial citizens are eligible to vote, and even as their connections to CSOs may ease participation in other forms of civic engagement, barriers to voting may remain. Lastly, the results reveal that personal connections to CSOs by custodial citizens the police have detained for questioning may not be statistically significant for voting *or* nonvoting political participation.¹²¹

One implication of our findings is that the empirical claims of the literature about the negative democratic effect of the carceral state are too general to adequately describe civic voluntarism by custodial citizens. Certainly, custodial citizens may withdraw from or neglect voting. Nonetheless, many continue to participate outside the ballot box, especially when they are connected to CSOs. Moreover, some custodial citizens will begin participating beyond voting because they have personal connections to CSOs. Such connections may make their participation more likely and frequent because the CSOs create opportunities for education, activation, and mobilization, as they do for noncustodial citizens.

Aggregate-Level Data

Based on our results from the individual-level analysis, combined with the broader empirical findings in the literature on the political behavior and attitudes of

Table 2
The interactive effects of contact and CSO connections on forms of participation

| | Registered to Vote ^a | Voted in 2012 | Nonvoting Political Participation ^b |
|-------------------------------------|---------------------------------|---------------------|--|
| Correctional control | -0.160 (0.662) | -1.565** (0.574) | -0.213 (0.196) |
| CSO connection | 0.601* (0.243) | 0.551* (0.273) | 0.255*** (0.054) |
| Detained by police | 0.533 (0.419) | 0.519 (0.471) | 0.131 (0.112) |
| Correctional control*CSO connection | 0.356 (1.483) | 2.518 (1.389) | 0.519* (0.249) |
| Detained*CSO connection | 1.027 (1.025) | -0.911 (0.676) | 0.109 (0.139) |
| Observations | 1,229 | 1,140 | 1,229 |
| Log Likelihood | -335.809 | -290.046 | -1,696.76 |
| AIC | 711.619 | 620.092 | 3,433.52 |

Notes: ^a We model registered to vote and voted in 2012 with logistic regression.

^bWe model nonvoting political participation with Poisson regression. A dispersion test, using the R package “AER,” yielded an estimate of .660, suggesting an absence of overdispersion.

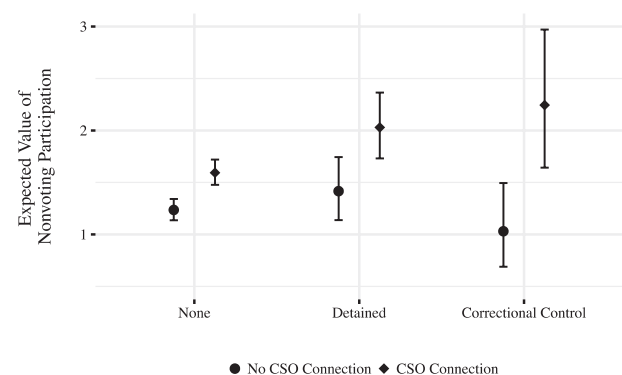
*p<.05; **p<.01; ***p<.001. Reported coefficients are from fully specified models, located in table A8 of the appendix.

custodial citizens, we explore a simple set of predictions about criminal justice contact at the community-level: greater criminal justice contact is associated with decreases in electoral participation, measured by voting and greater criminal justice contact is associated with increases in nonvoting political participation. Recall that our aggregate criminal justice contact measure is felony conviction rate. It is, as table 3 displays, statistically associated with lower turnout in the 2015 mayoral election. We logged the rate of felony convictions such that the coefficient estimate is interpreted as the absolute change in 2015 turnout given a percent change in felony convictions. Thus, a 5% increase in the felony conviction rate decreases expected voter turnout at the community-level in 2015 by approximately four percentage points. Also, while the relationship is not statistically significant, the felony conviction rate is negatively associated with the 2014 general election turnout.

Independent of CSO density, the rate of felony convictions is associated with increased nonvoting political participation, measured by 311 calls. Moving from the minimum value of felony conviction rate (.018) to its mean value (7.5) increases the rate of nonemergency calls in Chicago from 24 calls per 1,000 residents in police beats to just over 100 calls. Similarly, the felony conviction rate positively relates to meeting attendance, with the relationship approaching statistical significance. The positive effect that may exist between the felony conviction rate on both types of nonvoting political participation perhaps indicates neighborhood disorder and maybe lower (or higher) levels of community efficacy.¹²² That would mean felony conviction rates co-occur with needs for nonemergency assistance/antagonistic policing practices in need of redress.¹²³

Furthermore, one would expect that the scale of CSO presence in communities would particularly influence political participation by custodial citizens, whereby CSOs mitigate the demobilizing effects of criminal justice contact. We test this expectation at the community-level by interacting felony conviction rates and CSO density. Parallel to the results from our individual level analysis, political participation should be greater in communities where conviction rates are higher *and* the presence of CSOs are denser, compared to

Figure 4
The impact of criminal justice contact and CSO connections on participation



Notes: The figure reflects the interactive effect of criminal justice contact and CSO connections on participation among CAS respondents in the Chicago Metropolitan Area. Expected values derived from the nonvoting political participation model (refer to table 2).

Table 3
Effect of conviction rates and CSO densities on voting and nonvoting participation

| | 2014 Voter Turnout | 2015 Voter Turnout | Meeting Attendance | 311 Calls |
|--------------------|----------------------|----------------------|--------------------|-------------------------|
| Log (convictions) | -0.006 (0.003) | -0.008* (0.003) | 0.494 (0.306) | 11.137*** (2.662) |
| Log (CSOs) | 0.004 (0.004) | -0.002 (0.004) | 0.852** (0.325) | 9.391** (3.242) |
| % 18-34 | -0.347*** (0.062) | -0.296*** (0.063) | 0.196 (4.872) | 10.168 (48.693) |
| % 65+ | 0.297** (0.108) | 0.598*** (0.109) | -1.117 (8.442) | 23.671 (84.266) |
| % black | 0.082*** (0.018) | -0.029 (0.019) | -3.290* (1.438) | 24.780 (14.350) |
| % Latino | -0.053* (0.025) | 0.057* (0.025) | -4.978* (1.946) | 53.152** (19.354) |
| % College graduate | 0.287*** (0.039) | 0.175*** (0.040) | -7.714* (3.190) | -78.220* (30.894) |
| % Poor | -0.022 (0.055) | -0.087 (0.056) | 5.902 (4.334) | -298.645*** (43.081) |
| % Unemployed | -0.019 (0.064) | 0.090 (0.065) | -5.752 (5.120) | -9.664 (50.472) |
| % Owner occupied | 0.226*** (0.062) | 0.219*** (0.063) | 9.353 (4.881) | -30.243 (48.655) |
| Constant | 0.263*** (0.068) | 0.172* (0.069) | 0.413 (5.375) | 166.427** (53.611) |
| Observations | 270 | 270 | 268 | 270 |
| Adjusted R2 | 0.774 | 0.712 | 0.087 | 0.4 |

Note: All dependent variables are continuous, and are modeled using ordinary least squares regression.

*p<.05; **p<.01; ***p<.001.

communities with lower CSO densities. Comparatively, lesser CSO densities and lower conviction rates should matter less for political participation outcomes at the community-level. While CSO density and conviction rates independently increase requests for nonemergency assistance, we do not observe in table 4 a statistically significant interactive association with nonvoting political participation or with voting.

Figure 5 displays the marginal effects of CSO density, conviction rate, and the interaction between the two on each outcome of interest. These visualizations highlight that, independent of the felony conviction rate, the density of CSOs are positively related to nonvoting political participation. That is true, too, for the felony conviction rate in relation to 311 calls, regardless of CSO density. We interpret those results to mean that CSO density positively

Table 4
Interactive effect of conviction rates and CSO densities on voting and nonvoting political participation

| | 2014 Voter Turnout | 2015 Voter Turnout | Meeting Attendance | 311 Calls |
|-------------------|--------------------|--------------------|--------------------|----------------------|
| Log (convictions) | -0.005 (0.004) | -0.006 (0.004) | 0.567 (0.378) | 10.046*** (2.940) |
| log(CSOs) | 0.004 (0.004) | -0.003 (0.004) | 0.847** (0.326) | 9.568** (3.250) |
| Convictions*CSOs | -0.001 (0.002) | -0.002 (0.002) | -0.048 (0.145) | 1.117 (1.275) |
| Observations | 270 | 270 | 268 | 270 |
| Adjusted R2 | 0.773 | 0.713 | 0.076 | 0.4 |

Notes: All dependent variables are continuous, and are modeled using ordinary least squares regression.

*p<.05; **p<.01; ***p<.001. Coefficients reflect fully specified models, located in table A12 of the online appendix.

increases requests for nonemergency assistance in communities with either low *or* high felony conviction rates. Our interpretation is plausible, given that “organizational resources predict collective efficacy and [CSOs] produce externalities that foster collective action” in Chicago.¹²⁴

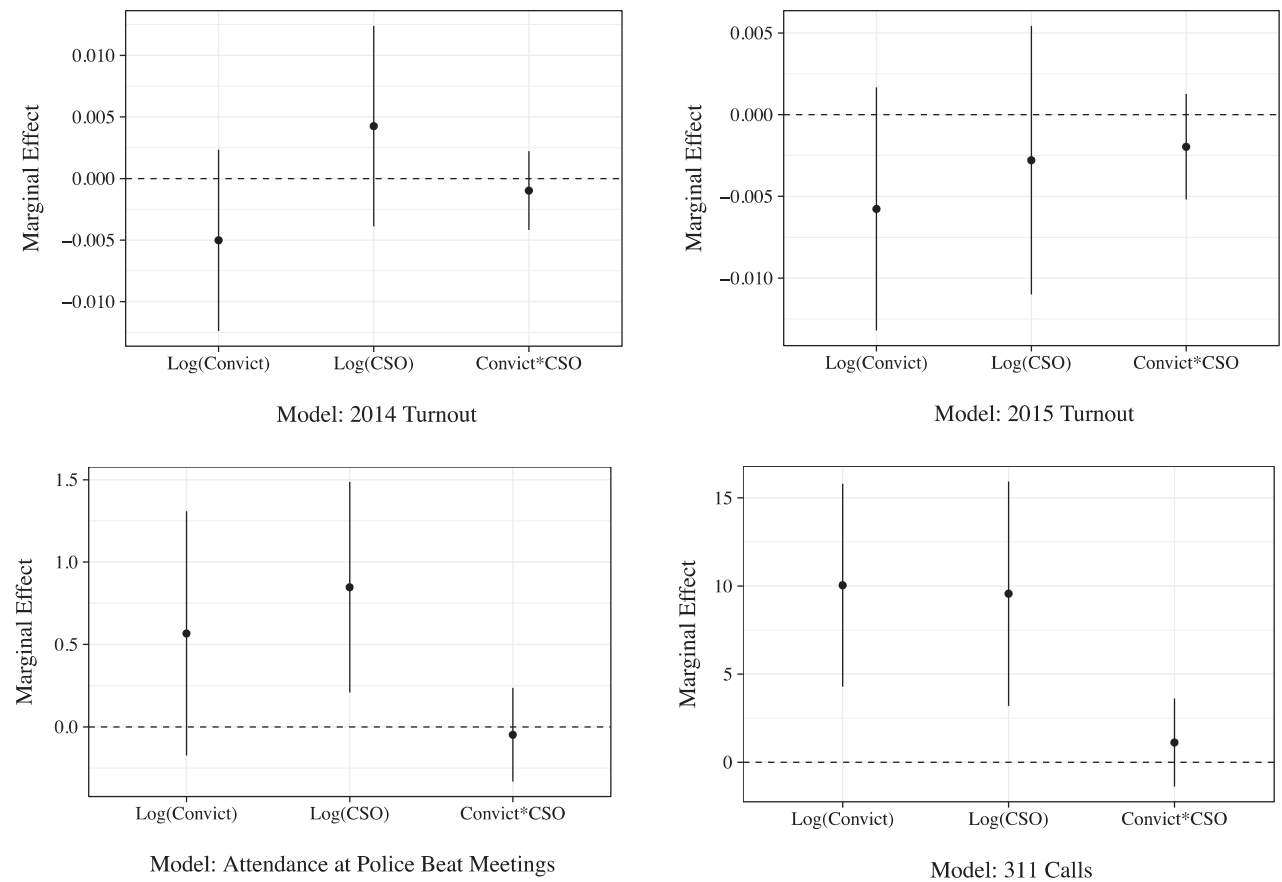
However, we must reiterate two points. First, higher rates of 311 calls may result from greater needs for nonemergency assistance in high criminal justice contact (i.e., high felony conviction rate) communities. Second, higher attendance at police beat meetings may stem from greater degrees of police-community antagonisms, if policing is more concentrated, “hot spot” oriented, and aggressive. Put another way, higher rates of citizen-initiated contact with government in one form or another in communities with more custodial citizens could result from a need in such communities for greater attention from public officials, a need that may be absent in communities with fewer custodial citizens.

To address those potentialities, we employed a matching causal inference strategy that allowed us to compare

the impact of CSO density among similarly situated communities that differ primarily by felony conviction rates. The strategy allowed us to compare low conviction rate communities to otherwise similarly situated high conviction rate communities. We describe the matching strategy and report results in full in the online appendix. Results from the matched analysis generally corroborate conclusions we drew from our analysis of the full sample before matching communities by rates of felony convictions. Specifically, greater CSO density is associated with greater requests for nonemergency assistance and attendance at police beat meetings among communities with either low *or* high felony conviction rates (refer to table 5 and figure 6).

To be clear, greater CSO density is associated with greater attendance at police beat meetings and requests for nonemergency assistance among low-conviction communities that are comparable in other ways to their high-conviction counterparts. Neither do conviction rates by themselves appear to impact nonvoting

Figure 5
Conviction rate, CSO density, and participation among Chicago police beats



Notes: The marginal effect of conviction rate, CSOs per 1,000 in the population and their interaction on voting, attendance at police beat meetings, and requests for nonemergency assistance. Coefficient estimates reflect models presented in table 4.

Table 5
Matched analysis: Interactive effect of conviction rates and CSO densities on voting and nonvoting political participation

| | 2014 Voter Turnout | 2015 Voter Turnout | Meeting Attendance | 311 Calls |
|--------------------------------|----------------------|----------------------|--------------------|---------------------|
| Log (convictions per 1000 pop) | -0.022* (0.010) | -0.042*** (0.010) | 0.610 (0.495) | -1.432 (7.991) |
| Log (CSO per 1000 pop) | 0.070*** (0.007) | 0.003 (0.008) | 0.880* (0.380) | 17.965** (6.087) |
| Convict*CSOs | -0.055*** (0.016) | -0.002 (0.017) | -0.386 (0.813) | -18.233 (13.096) |
| Observations | 88 | 88 | 88 | 88 |
| Adjusted R2 | 0.518 | 0.192 | 0.061 | 0.064 |

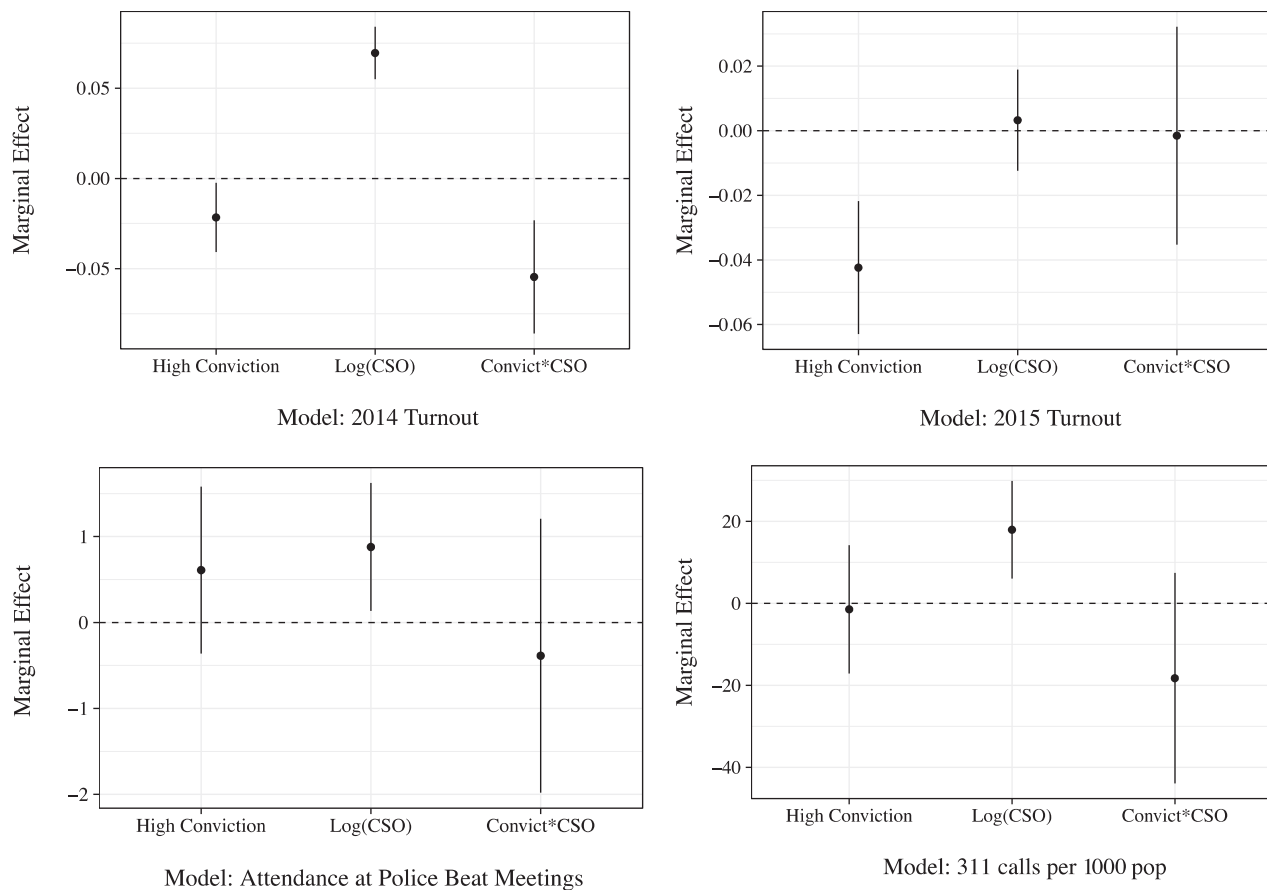
Notes: All dependent variables are continuous, and modeled using ordinary least squares regression.

*p<.05; **p<.01; ***p<.001.

political behavior, nor does CSO density operate differently for low- or high-conviction communities. The overriding factor influencing nonvoting political participation appears to be CSO density. Results from

the matching strategy, then, confirm that our broader findings about CSO density and nonvoting political participation are not due to need alone in those communities.

Figure 6
Conviction rate, CSO density, and participation among matched police beats



Notes: The marginal effects of conviction rate, CSOs per 1,000 in the population, and their interaction on voting, attendance at police beat meetings, and requests for nonemergency assistance, among matched police beats. Coefficient estimates reflect models presented in table 5.

Voting, however, is different. While increasing CSO density is associated with increasing voter turnout in high and low criminal justice contact communities in Chicago in 2014, the slope of increase is greater for low conviction communities than for high ones. This illuminates how we might interpret the effects of CSOs on nonvoting political participation in communities. Although the interaction term for convictions rates and CSO density does not achieve statistical significance for either meeting attendance or 311 calls per capita, as is the case with the results of the unmatched analysis presented earlier in table 4 and figure 5, the nature of the relationships are similar: *CSO density increases nonvoting political behavior in high-conviction and low-conviction police beats, even if the size of the effect is larger in low-conviction communities.*

Overall, CSOs are associated with more nonvoting political participation in high conviction communities, which parallels what we observed in the individual-level analysis, whereby personal connections to CSOs were associated with increased nonvoting political participation by custodial citizens. Also, contacting government, either through attendance at police beat meetings or making requests for nonemergency assistance, is about more than need. CSO density matters as there is an association between CSO density and increased nonvoting political participation, regardless of community-level conviction rates. However, as we generally observed in the individual-level analysis, where personal connections to CSOs were not associated with changes in voting, barriers may remain that hinder higher densities of CSOs from yielding greater degrees of voting by communities with high rates of criminal justice contact.¹²⁵

Discussion and Conclusion

Studies of the influence of involuntary criminal justice contact on political participation reveal how unwanted interactions with the carceral state negatively influence political behavior by individuals and communities. Such studies, which mainly report on voting, suggest that criminal justice contact shifts many custodial citizens from political engagement to political quiescence, assuming they ever were politically active. Hence, involuntary criminal justice contact may yield “diluted political engagement,”¹²⁶ which reduces the already low political influence of custodial citizens as individuals and collectives.¹²⁷ But there is a bifurcation of participation by custodial citizens: custodial citizens may engage less as voters but their nonvoting political participation may not decline. In fact, the nonvoting political participation by custodial citizens may increase.

Nonvoting political participation by people who have had criminal justice contact, as our results suggest, is strongly associated with their personal connections to civil society organizations. This association may result from causal mechanisms inherent in specific forms of

organizing and mobilization, but also service provision, by CSOs. When CSOs assist custodial citizens to solve problems arising from criminal justice contact (e.g., unemployment, precarious housing, lack of affordable legal assistance, etc.), they may implicitly or explicitly assist them—and their communities—to overcome efficacy and resource barriers to greater nonvoting (and sometimes voting) participation.¹²⁸ Although our study was not designed to identify causality, future research on political participation—including nonvoting political participation—by custodial citizens that applies careful causal identification strategies are warranted. Additionally, it may be the case that particular types of CSOs matter more to increased (or reduced) political participation by custodial citizens and communities where more of them reside. That is undetermined. Insights from qualitative studies of custodial citizenship and participation, however, hint that some types of CSOs may prove more influential than others in shaping the engagement of people who have had criminal justice contact.¹²⁹

Additionally, the observed relationship between personal connections to CSOs and increasing nonvoting political activities by custodial citizens, specifically individuals who experienced correctional control, is important to keep in mind. Nonvoting political participation is conventional political participation.¹³⁰ Depending on the measure, nonvoting participation is more common than casting a ballot in the United States.¹³¹ Hence, the strength of the association between personal connections to CSOs and increasing nonvoting political participation among custodial citizens reminds us that they—even those whom the state has held tightest and longest via correctional control—are not that much different from non-custodial citizens when it comes to nonvoting political participation. Custodial citizens volunteer with campaigns, attend public meetings, make public-regarding contact with government via nonemergency call systems or letters and emails, recruit others to participate, and engage via a variety of participatory modes.

Accordingly, nonvoting political participation by custodial citizens and its breadth of activities, inclusive of electoral ones (e.g., volunteering with campaigns) and nonelectoral ones (e.g., petitioning), deserves as much attention as registering to vote and voting. To be blunt, social scientists, particularly political scientists, are overly concerned with the effect of criminal justice contact on voting, paying too little attention to political participation beyond voting by custodial citizens. Yet nonvoting political participation is perhaps more likely to strengthen the voice of custodial citizens in relation to distributive politics that condition their full citizenship.

Furthermore, much of the CSO activity in relation to political organizing and mobilizing of custodial citizens is and will be for individual and collective goods, not the ballot. In increasing numbers of cities and states, the

restoration of social and civil rights to custodial citizens without them is the primary attention of CSOs concerned about the democratic effects of criminal justice contact. Their work is marked by advocacy and lobbying campaigns, not voter registration and turnout drives, to remove the bars on access to social welfare benefits (e.g., food stamps and public housing) and to open labor markets through “ban the box” (i.e., removal of screener questions from job applications that may reduce employment of people with criminal records).¹³² Additionally, a lot of organizing of custodial citizens is less about individuals and more about their communities in the name of universal improvement and community development.

Are we calling for political scientists and others to abandon the study of voting by custodial citizens? No. Normative, empirical, and substantive reasons remain for us to keep studying custodial citizens as slack resources before, during, and after elections.¹³³ Certainly, political mobilization of custodial citizens for greater democratic participation obligates attention to the restoration of voting rights and other electoral matters. There are, for example, significant activities underway in Florida to immediately re-enfranchise all custodial citizens without records of violent crime and to return the ballot to prison inmates in California, Massachusetts, and New Jersey.

Nevertheless, political participation of custodial citizens as *voters* may not increase, regardless of the success of those voting-rights restoration campaigns. It also may not increase despite personal connections to civil society organizations as we observed in our Chicago research. The results failed to show statistically significant relationships between criminal justice contact, personal connections to CSOs, and registering to vote and voting at both the individual and aggregate levels. More concretely, the negative relationship between criminal justice contact and voting may endure even by custodial citizens in communities where voting rights restoration happens immediately after incarceration *and* where CSOs are dense and personal connections to them are strong.

Before we conclude, it is worth reiterating the descriptive character of our study. We are not able to draw conclusions about the causal pathways of criminal justice contact, CSO connections, and political participation. Not only do we lack administrative data on individual level connections to CSOs and participatory activities beyond voting, our individual-level data is cross-sectional and from a survey subject to selection bias. It may be, for instance, that individuals who are connected to CSOs are also the sort of people likely to participate at high levels. Previous research suggests otherwise, however, and demonstrates that nonpolitical institutions are instrumental to cultivating the political skills and interests important to participation. Yet we are unsure about the exact nature of this relationship from cross-sectional, observational data.

While our use of aggregate data, which ameliorates sampling and response bias, produced results that generally support the claim that CSOs increase participation in all kinds of communities, we observed that the magnitude of the civil society potential for increased civic voluntarism in communities with lower rates of custodial citizens was greater than what we observed in communities with higher rates of custodial citizens. Cumulative disadvantage and institutional barriers to participation, which exacerbate each other, construct obstacles for custodial citizens and their communities to full participation in political life. In the absence of better data, however, we can only postulate about how CSO connections shape civic education, access, and engagement, and whether variation in civic voluntarism by custodial citizens is indicative of relative disempowerment or strength.

Still, our findings disrupt the characterization that communities where criminal justice contact is prevalent are beleaguered, possessing weak capacities for personal and collective efficacy, which undermine engagement of their custodial residents in the polis. For example, our study suggests that citizen-initiated contact with government can be greater in communities with higher rates of residents with criminal convictions than in communities with lower rates of residents with criminal convictions. Thus, communities with high rates of criminal justice contact may not participate less in all forms of action, at least when the civic voluntarism in question includes behaviors beyond the frequency of voting.

Also, our research encourages political scientists to rethink how we understand and measure the political lives of marginalized people and their communities. By widening our theoretical and empirical gazes beyond “the electoral-representative dynamics that have become the taken-for-granted object of our attention”¹³⁴ we can better observe how custodial citizens and their communities do politics, as well as better observe how their political behavior may bear on and be shaped by the institutions and distributive politics of the carceral state.

Finally, echoing Majic, “it is . . . imperative that we examine nonprofit organizations more closely and identify the ways they may engage in civic life, especially if we are concerned with expanding inclusion and justice through and in the democratic political process.”¹³⁵ This is especially true in the age of the carceral state and its expanding custodial citizenry. Research into the broader effects, particularly participatory paradoxes, that the American carceral state produces for civil society must continue, inclusive of the ways civil society organizations foster (or inhibit) the political participation of custodial citizens and their communities.

Notes

- 1 “Criminal justice contact” means here involuntary contact with the criminal justice system.

- 2 Gottschalk 2016.
- 3 Bruch and Soss 2018; Shedd 2015; Rios 2011.
- 4 Lerman and Weaver 2014a.
- 5 National Center for Victims of Crime 2016; Enns 2016.
- 6 U.S. Bureau of Justice Statistics 1993; U.S. Bureau of Justice Statistics 2018.
- 7 Clark 2018.
- 8 Jacobs 2015, 1 and n. 4.
- 9 U.S. Bureau of Justice Statistics 2012.
- 10 Jacobs 2015, xi.
- 11 Pager 2007; Travis and Waul 2003.
- 12 Comfort 2016; Travis and Waul 2003.
- 13 Clear 2007.
- 14 Gottschalk 2016.
- 15 Skocpol 2017.
- 16 Wheelock 2011; King and Mauer 2004; Smith 2004.
- 17 Steinacker 2003.
- 18 Manza and Uggen 2004, 2006.
- 19 Thorpe 2015; Walker et al. 2017.
- 20 Petit 2012.
- 21 Sances and You 2017; Harris 2016; Katzenstein and Waller 2015; U.S. Department of Justice 2015.
- 22 The democratic effects of juvenile criminal justice contact on the civic voluntarism of youth and during adulthood merit future investigation. Surveillance and policing of youth in the United States (e.g., police in schools) are common and youth interactions with the criminal justice systems for juveniles and adults are widespread; see Shedd 2015, Rios 2011. Juvenile criminal justice contact can affect subsequent adult criminal justice contact. It is plausible that youth contact with punitive institutions of juvenile criminal justice provides “formative political experience” as punitive institutions of schools do for youth; see Bruch and Soss 2018. If so, juvenile criminal justice contact may have negative consequences for juvenile civic voluntarism and subsequent adult civic voluntarism.
- 23 Weaver and Lerman 2010; Hjalmarsson and Lopez 2010; Lerman and Weaver 2014a; Kang and Dawes 2017; Laneyonu 2018.
- 24 Weaver and Lerman 2010; Lerman and Weaver 2014a, 2014b; Kang and Dawes 2017; Laneyonu 2018.
- 25 Some types of vehicle and pedestrian stops by police officers, as well as all types of correctional control, are associated with greater distrust and less confidence in government and lower perceptions of public institutions as legitimate; Clear 2007; Weaver and Lerman 2010; Lerman and Weaver 2014a; Epp, Maynard-Moody, and Haider-Markel 2014.
- 26 Weaver and Lerman 2010, 818.
- 27 E.g., Hull 2006; Manza and Uggen 2006; Katzenstein, Ibrahim, and Rubin 2010; Weaver and Lerman 2010; Burch 2013; Lerman and Weaver 2014a; Meredith and Morse 2014; Muller and Shrage 2014; Walker 2014; White 2015; Meredith and Morse 2015; Gerber et al. 2015, 2017; Howard 2017; Kang and Dawes 2017.
- 28 Soss and Weaver 2017, 567.
- 29 Ibid, 565.
- 30 See, e.g., Hull 2006; Howard 2017; Dilts 2014; Weaver and Lerman 2010; Lerman and Weaver 2014a, 2014b; White 2015; Burch 2013; Manza and Uggen 2006; Bruch, Marx Ferree, and Soss 2010; Lee, Porter, and Comfort, 2014; Muller and Schrage 2014; Walker 2014; Gerber et al. 2017; Kang and Dawes 2017; Laneyonu 2018.
- 31 Burch 2013; Owens 2014.
- 32 E.g., Cohen and Rodgers 1995; Warren 2001; Putnam 2000; Fung 2003; Majic 2011; LeRoux and Krawczyk, 2014; Han 2016; Fyall and Allard 2017.
- 33 Personal connections to CSOs by juveniles with criminal justice contact may also mitigate some of the negative political effects of juvenile criminal justice contact and influence civic voluntarism in adulthood. However, that is for future study.
- 34 Han 2016.
- 35 Schlozman 2002, 439; Verba, Schlozman, and Brady 1995.
- 36 Rosenstone and Hansen 2003, 2.
- 37 Mettler and Soss 2004, 56.
- 38 Peffley and Hurwitz 2010; Wilson, Owens, and Davis 2015; Enns 2016.
- 39 Owens and Smith 2012; Kaufman 2015; Miller 2014.
- 40 Mettler and Soss 2004, 56.
- 41 Fairchild 1977, 298.
- 42 Tyler, Fagan, and Geller 2014; Epp, Maynard-Moody, and Haider-Markel 2014; Justice and Meares 2014; Miller 2015.
- 43 Weaver and Lerman 2010; Lerman and Weaver 2014a; Kang and Dawes 2017; Haselswerdt 2009; Hjalmarsson and Lopez 2010; Meredith and Morse 2014, 2015; Gerber et al., 2015; White 2015. However, a provocative study using administrative data on pre-incarceration and post-release voting in Pennsylvania between 2008 and 2012 concludes “spending time in prison has almost no effect on voting”; Gerber et al. 2017, 1131–32. Nonetheless, as its authors note, “that incarceration per se does not appear to cause a large reduction in [voting] suggests that scholars should follow the path of recent research that examines how citizen preferences and behaviors are shaped by lower-level contact [e.g., detentions for questioning] with these other elements of the state.”

- 44 Lerman and Weaver 2014a, 220–21. Furthermore, there is evidence that noncustodial citizens in romantic relationships with custodial citizens are less likely to vote; Lee, Porter, and Comfort 2014; Sugie 2015.
- 45 Clear 2007.
- 46 Burch 2013; Lanionu 2018.
- 47 Owens 2014, 258.
- 48 Lerman and Weaver 2014a; Wildeman, Hacker, and Weaver, 2014; Burch 2013.
- 49 Owens 2014; Lerman and Weaver 2014a; Muller and Schrage 2014; Justice and Meares 2014.
- 50 Weaver and Lerman 2010; Lerman and Weaver 2014a; Walker 2014; Lee, Porter, and Comfort 2014.
- 51 Verba, Schlozman, and Brady 1995.
- 52 Han 2016, 296.
- 53 Berry, Portney, and Thomson 1993, 12.
- 54 De Tocqueville 2002; de Beaumont and de Tocqueville 1979.
- 55 Sampson 2012, 179.
- 56 Putnam 2000.
- 57 Han 2014, 8.
- 58 Putnam 2000.
- 59 Kaufman 2015; Miller 2014.
- 60 Fyall and Allard 2017; Berry 2003.
- 61 Han 2014; Rosenstone and Hansen 2003; Verba, Schlozman, and Brady 1995. Many individuals, however, involve themselves in organizations without “pressure” from CSOs. Instead of waiting on the organizing and mobilizing of CSOs, they rely on self-motivation. They may have orientations or predispositions to participate regardless of any activities by or engagement with CSOs. For them, recruitment by CSOs is not necessary to their participation, which is something the authors of the classic Civic Voluntarism Model acknowledge.
- 62 Schneider and Ingram 1997; Cohen 1999; Strolovitch 2007.
- 63 Burch 2013, ch. 6.
- 64 De Tocqueville 2002; Putnam 2000; Han 2016.
- 65 Marwell 2004; Berry, Portney, and Thomson 1993.
- 66 Harris 1999; Marwell 2004; Owens 2007; Fyall and Allard 2017.
- 67 Harris 2001; Owens 2007.
- 68 Levi 2004; Marwell 2004; Owens 2007.
- 69 Cohen and Rogers 1995; Warren 2000; Putnam 2000.
- 70 Han 2009, 2014, 2016.
- 71 Verba, Schlozman, and Brady 1995, 17–18.
- 72 Harris 2001; Wong 2009; Graauw 2016.
- 73 Skocpol 1990, 456.
- 74 Verba, Schlozman, and Brady 1995; Rosenstone and Hansen 2003; Han 2009, 2014.
- 75 Rosenstone and Hansen 2003, 83–84.
- 76 Berry, Portney, and Thomson 1993; Verba, Schlozman, and Brady 1995; Han, 2009, 2014, 2016; Harris, 2001.
- 77 Rosenstone and Hansen 2003, 32; Verba, Schlozman, and Brady 1995.
- 78 Marwell 2004; Owens 2007.
- 79 Wong 2006; Barreto et al. 2009; Majic 2011; LeRoux and Krawczyk 2014; Graauw 2016; García Bedolla and Michelson 2012; Warren 2001.
- 80 Harris 2001; Wong 2009; Majic 2011; García Bedolla and Michelson 2012; LeRoux and Krawczyk 2014; Graauw 2016.
- 81 Berry, Portney, and Thomson 1993, 95–97.
- 82 Fung 2003; Cohen 2004; Majic 2011.
- 83 Examples include EPOCA (Ex-Prisoners and Prisoners Organizing for Community Advancement) in Worcester, MA; V.O.T.E. (Voice of the Ex-Offender) in New Orleans, EXPO (Ex-Prisoners Organizing) in Milwaukee, Wisconsin; and the California, Texas, and North Carolina chapters of All of Us or None; Owens 2014; Kaufman 2015; Flores and Cossyleon 2016.
- 84 Owens 2014; Williams 2015; Flores and Cossyleon 2016.
- 85 Sampson 2012, 77.
- 86 *Ibid.*, 21.
- 87 Sampson and Loeffler 2010; Visher, Yahner, and La Vigne 2010.
- 88 Commonly used datasets such as the National Longitudinal Study of Adolescent Health and the Fragile Families and Child Well Being Study are two-decades old and include truncated age cohorts. More recent data from the 2006 African American Men’s Survey and the 2016 American National Election Studies include measures of criminal justice contact but the former only includes voter registration as political participation and the latter excludes measures of connections to CSOs.
- 89 Gerber et al. 2017.
- 90 Skogan 2016.
- 91 Gerber et al. 2017; Weaver and Lerman 2010; Lerman and Weaver 2014a; Lee, Porter, and Comfort 2014; Walker 2014.
- 92 Sampson 2012, 190.
- 93 Skogan and Hartnett 1997.
- 94 Skogan 2016.
- 95 *Ibid.*
- 96 Sampson 2012, 208.
- 97 Rosenstone and Hansen 2003, 86.
- 98 That is higher than the turnout of registered voters (73%) in Cook County elections that year, according to the Illinois State Board of Elections. CAS respondents may have over-reported voting, which is common in public opinion surveys because of the social desirability associated with voting. Selection bias may also explain the high rate of reported voting political participation in the CAS. Individuals who chose to participate in the CAS could be more

- civic-minded—and more likely to vote—than the general population. Either type of bias could skew empirical results in our favor. As we describe in our appendix, we took steps to address issues related to selection bias in survey data. While we cannot correct for response bias, abstaining from other types of political activities, like attending a protest or signing petitions, does not carry with it the stigma of failing in one’s duty as a citizen associated with nonvoting; Holbrook et al 2010, Persson and Solevid 2013.
- 99 Descriptive statistics for all variables in both datasets are in the online appendix, which reports the descriptive statistics for the CAS (tables A1–A2) and the aggregate dataset (tables A4–A5).
- 100 Sampling bias in surveys leads to samples unrepresentative of the overall population. Sampling weights are constructed to address this issue. However, individuals with criminal justice contact often are unlike the general population, differing, for instance, on dimensions of race, gender, and age. The subsample of Chicago Area Study respondents with criminal justice contact likely differs from the general population under correctional contact in the Chicago metropolitan area. Exact, current and complete demographic information on metropolitan Chicago’s custodial citizenry, especially members of the correctional population, is unavailable. In lieu of better demographic data, we compare Chicago Area Study respondents with criminal justice contact to Cook County Jail admissions in 2011 and the Illinois prison population in 2010. The subsample of custodial citizens in the Chicago Area Study is less black, more Latino, more female, slightly older, and more likely to be married than Cook County jailees and Illinois prison inmates. Full comparisons of their characteristics are available in the online appendix. The difference in characteristics likely biases statistical findings in favor of our prediction. This may be a limitation of our analysis. Barring better sampling strategies for social surveys of the behavior of custodial citizens, which we strongly advocate, there is little that political scientists can do to alter the nature of our samples.
- 101 For more information about the data and its collection, see <http://convictions.smartchicagoapps.org/#five-years-of-data>. Additionally, the online appendix describes the felony conviction data and other data we considered using to complement or substitute felony convictions as our aggregate measure of criminal justice contact.
- 102 The distribution of felony convictions had a minimum value of zero and a maximum value of 33. The lower quartile fell below 2.5 felony convictions.
- 103 Sampson 2012, ch. 8.
- 104 Ibid.
- 105 Ibid.
- 106 See <http://nccs-data.urban.org/data.php?ds=bmf> and <https://www.irs.gov/charities-non-profits/exempt-organizations-business-master-file-extract-eo-bmf>.
- 107 For more information on the IRS classifications, see <https://www.irs.gov/charities-non-profits/types-of-tax-exempt-organizations>.
- 108 The minimum was .032 CSOs and the maximum was 152 CSOs.
- 109 “More than any kind of activity, contacting [government and public officials] is distinguished by the control the participant can exercise over timing of the activity and the content of the message”; Verba, Schlozman, and Brady 1995, 89.
- 110 Although the creation of CAPS increased the opportunity structure for political engagement, our time frame corresponded with “the period of the greatest decline in overall attendance levels” at police beat meetings due to municipal budget cuts of CAPS and the redeployment of police officers. Skogan 2016, 7. Thus, the CAPS meeting data provides a hard test for our theory.
- 111 Lerman and Weaver 2014b; O’Brien 2016; White and Trump, 2016; O’Brien et al. 2017.
- 112 The 311 call systems are a simple way of citizen-initiated contact with government, especially to report public problems such as potholes and property blight. They provide a “low-transaction-cost access to government that have been shown to instill a sense of trust and offer the promise of increased bureaucratic responsiveness,” while fostering individual and collective efficacy; Minkoff 2016, 212.
- 113 O’Brien et al. 2017, 323.
- 114 Lerman and Weaver 2014b.
- 115 The online appendix details our construction of the aggregate dataset.
- 116 Although our key independent and dependent variables include both conventional and novel ones, some are imperfect. We use voter turnout estimates within police beats, calculated by simple area weighting. Felony convictions exclude criminal justice contact that does not result in arrests, charges without conviction, and convictions for misdemeanors. The CSO density measure includes a variety of nonprofit organizations, inclusive of those that do and do not target custodial citizens. They range from religious congregations like the Chicago Foursquare Church to the local affiliate of federated community organizing groups such as the Gamaliel Foundation, to labor organizations such as Women in Aviation International. The CSO density measure also excludes organizations that possess tax-exempt status but are not required to register with the IRS

(e.g., small religious congregations). While our CSO density measure is blunt, its noise should bias empirical analyses toward null results. To control for various sources of bias that may influence conviction rates, civic engagement, and CSO density, we leverage data on housing tenure, age and racial compositions, educational attainment, poverty, and unemployment from the U.S. Census Bureau.

- 117 We model nonvoting behavior with Poisson regression, as noted in table 1. If the distribution of the dependent variable is over dispersed, it is appropriate to evaluate the data with an alternative model (e.g., negative binomial regression or a quasi-Poisson model). Tests for dispersion, noted in table 1 and table 2, resulted in no evidence of data overdispersion. For more on this point, refer to the online appendix.
- 118 Walker 2014; Lee, Porter and Comfort 2014.
- 119 Lerman and Weaver 2010; Lee, Porter and Comfort 2014; Walker 2014.
- 120 Tests of the robustness of our models are available from the online appendix; See figure A1 and tables A9–A11.
- 121 Our null findings may result from our measure of voting, which is self-reported. Because of social desirability, respondents often misreport their voting histories on surveys; Holbrook and Krosnick 2010. Even when self-reported voting is not correlated with criminal justice contact and positively correlated with nonvoting political participation, contact may be negatively associated with voting when measured by validated voter records; Walker 2014. We may also have null findings because voting is modeled among registered voters. Thus, individuals in the sample with connections to the criminal justice system have already overcome a significant barrier to voting. We are cautious in interpreting our null result that criminal justice contact is not associated with voting in Chicago.
- 122 Sampson and Loeffler 2010.
- 123 White and Trump 2016; Lerman and Weaver 2014b.
- 124 Sampson 2012, 208.
- 125 Burch 2013.
- 126 Weaver and Lerman 2010, 824; Burch 2013, 2014; Lerman and Weaver 2014a, 2014b; Lanisonu 2018.
- 127 Owens 2014; Miller and Stuart 2017.
- 128 Owens 2014; Flores and Cossyleon 2016; Williams 2015; Kaufman 2015; Miller 2014.
- 129 Maruna 2001; Owens 2014; Flores and Cossyleon 2016; Williams 2015; Kaufman 2015; Miller 2014.
- 130 Verba, Schlozman, and Brady 1995; Han 2016.
- 131 Verba, Schlozman, and Brady 1995.
- 132 Owens 2014; Owens and Smith 2012.

- 133 E.g., Katzenstein, Ibrahim, and Rubin 2010; Dilts 2014; Lerman and Weaver 2014a; Kang and Dawes 2017; Gerber et al. 2015, 2017.
- 134 Soss and Weaver 2016, 75.
- 135 Majic 2011, 832.

Supplementary Materials

Appendix

- Description of the Chicago Area Study Data
- Description of the Aggregate Data
- Analysis of the Chicago Area Study
- Analysis of the Supplemental Aggregate Data

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Ticketing and Turnout: The Participatory Consequences of Low-Level Police Contact

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The American criminal legal system is an important site of political socialization: scholars have shown that criminal legal contact reduces turnout and that criminalization pushes people away from public institutions more broadly. Despite this burgeoning literature, few analyses directly investigate the causal effect of lower-level police contact on voter turnout. To do so, we leverage individual-level administrative ticketing data from Hillsborough County, Florida. We show that traffic stops materially decrease participation for Black and non-Black residents alike, and we also find temporal variation in the effect for Black voters. Although stops reduce turnout more for Black voters in the short term, they are less demobilizing over a longer time horizon. Although even low-level contacts with the police can reduce political participation across the board, our results point to a unique process of political socialization vis-à-vis the carceral state for Black Americans.

INTRODUCTION

Fines and fees are increasingly recognized as a form of racialized revenue extraction connected to marginalized communities' alienation from government (McCoy 2015; Sanders and Conarck 2017; Shaer 2019). After Michael Brown was killed by the Ferguson Police Department in 2014, a US Department of Justice investigation into the city's police and courts demonstrated that the municipality was engaged in a practice that advocates now refer to as "policing for profit." The city's reliance on fines and fees to fund government functions grew from 13% to 23% of the total budget between fiscal years 2012 and 2015. From 2012 to 2014, the Department of Justice found that 85% of vehicle stops, 90% of citations, and 93% of arrests targeted Black people. In contrast, just two-thirds of Ferguson's residents are Black (United States Department of Justice Civil Rights Division 2015).

It wasn't just a Ferguson problem, or even a Missouri problem. American cities' reliance on fines and fees revenue increased significantly following the 2008 recession—as local tax revenues dropped and tax increases became less politically viable, jurisdictions increased the amounts of fines and fees and imposed them more frequently in order to fund government services (Harris, Ash, and Fagan 2020; Harris et al. 2017; Singla, Kirschner, and Stone 2020).

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Given that American jurisdictions are increasing their reliance on fines and fees revenue—and that police are the government officials charged with generating revenue—it stands to reason that more low-level police contact has occurred, and often with blatantly extractive intent. Although scholars have examined the collateral consequences of this increased reliance on fines and fees (Pacewicz and Robinson 2020; Sances and You 2017), comparatively few have explored the moment during which such revenue-raising actually occurs—namely, in the individual interactions between residents and the police via the issuance of a ticket. This "moment" of low-level contact has also been relatively understudied by scholars investigating the participatory consequences of contact with the criminal legal system. Work exploring how criminalization directly and indirectly influences political participation has exploded in recent years. Scholars have found that criminal legal contact (i.e., arrest, conviction, incarceration) consistently discourages voting (Burch 2011; Weaver and Lerman 2010; White 2019b). Such work has largely focused on the effects of highly disruptive contact with the criminal legal system such as incarceration and felony convictions (Burch 2014; Lee, Porter, and Comfort 2014). Although ticketing involves potentially negative interactions with the state, it does not necessarily carry the disruptive consequences of a felony conviction and might thus politicize Americans in unique ways. This paper theorizes how local police practices affect voting behavior among stopped individuals and provides precisely estimated evidence of a causal effect.

Our project represents the first use of individual-level administrative data to identify the causal effect of traffic stops on voter behavior. The use of administrative data marks an important step forward in our understanding of how low-level contact with the criminal legal system

structures political participation. Past work looking at the individual-level effects of low-level contact has relied on survey or interview data (e.g., Walker 2014; Weaver and Lerman 2010). Existing research allows for the testing of specific psychological mechanisms and personal interpretations of criminal legal contact but does not allow us to generalize more broadly. As Weaver and Lerman (2010, 821) note, it may also introduce measurement bias. Our analysis investigates actual voting behavior following actual traffic stops, not reported voting behavior or reported exposure to a traffic stop. The administrative data therefore allow us both to sidestep reporting error and to observe the behavior of a quarter-million individuals stopped over a six-year period—a far larger pool than even the most robust surveys.

We use individual-level traffic stop data from Hillsborough County, Florida, to identify the turnout patterns of voters who were stopped between the 2012 and 2018 elections. By matching individual voters who were stopped to similar voters who were stopped at later points and running a difference-in-differences model, we estimate the causal effect of these stops on turnout. This borrows from the logic of regression discontinuities in time: conditional on observable characteristics *and* unobservable factors associated with being ticketed, the timing of the stop on either side of election day is essentially as-if random. We find that being stopped reduces the chance that an individual will turn out in the subsequent election but that this effect is smaller for Black voters in the long run.

We demonstrate that traffic stops—the most widespread form of police contact in America—substantially reduce the turnout of non-Black American voters but reduce Black voter turnout to a smaller degree. More specifically, we find temporal variation in the effect of stops on Black voter turnout: Black voters stopped shortly before an election are demobilized to a *greater* extent than are non-Black voters, but as more time passes between stops and the election of interest, the treatment effect becomes comparatively smaller for Black voters. Our findings complicate existing theories of how criminalization politically socializes Americans, and Black Americans in particular (Weaver and Lerman 2010). Additionally, although many forms of criminalization have been found to contribute to a well-documented subjective experience of alienation or group-level exclusion among Black Americans (Ang et al. 2021; Bell 2017; Desmond, Papachristos, and Kirk 2016; 2020; Stuart 2016; Zoorob 2020), our contribution emphasizes the need for further research regarding how different forms of criminalization affect group-level perceptions of government and resultant political behaviors. Our findings are relevant for interdisciplinary scholars of crime, race, politics, municipal finance, and policing.

THEORY

How Police Stops Might Influence Turnout

Learning about one's "place in the system" takes place over long periods. Could isolated police stops that do

not require sustained contact with the criminal legal system affect the political behavior of Americans? To ground our expectations, we turn first to recent work exploring the effect of high-level contact with the criminal legal system on political behavior. We then consider what this literature can and cannot say about expected effects of police stops on voting.

A growing body of work has explored the effects of criminal legal contact on political participation. Some scholars find large depressive effects from incarceration (Burch 2011), whereas others argue that any negative effects are smaller or mixed (e.g., Gerber et al. 2017; White 2019b). Other work has explored the "spillover" effects of incarceration, finding that the political behavior of family members (Walker 2014; White 2019a) and neighbors (Burch 2014; Morris 2021b) can be influenced by indirect contact with incarceration, and these effects might be quite durable (Morris 2021a). The one project that has used administrative data to explore the political implications of low-level police contact is Laniyonu (2019), which finds mixed effects of the Stop-Question-and-Frisk practice on neighborhood-level turnout in New York City, though the strength of the causal design is limited. Thus, the literature generally agrees that contact with the criminal legal system reduces political participation.

The existing literature broadly groups the depressive mechanisms into two categories: "resource" and "political socialization" (see White 2019b, 312). Classic political science literature indicates that citizens with more resources are more likely to participate (Brady, Verba, and Schlozman 1995); these resources are undermined by the time and financial resources individuals and family members devote to dealing with a felony conviction. Although higher-level contacts come with higher costs than an average police stop, the resource story could extend to some of these less-disruptive contacts with the criminal legal system. If a ticket leads to a suspended driver's license, the initial stop can snowball into a much bigger life event that could jeopardize employment or lead to shorter stints of incarceration. Searches conducted during traffic stops may also lead to arrest if a police officer finds contraband in the vehicle. These cases might have consequences more akin to those associated with a brief period of incarceration that can also threaten employment. Nevertheless, the average traffic stop is certainly less disruptive than the average period of incarceration, likely demanding fewer resources than other forms of contact.

Literature on political socialization argues that citizens' perceptions of and behavior with respect to government are heavily determined by routine interactions with state apparatuses and government officials. As Soss and Weaver (2017) argue, "interviewees have looked, not to City Hall, Congress, or political parties, but rather to their direct experiences with police, jails and prisons, welfare offices, courts, and reentry agencies as they sought to ground their explanations of how government works, what political life is like for them, and how they understand their own

political identities” (Soss and Weaver 2017, 574). To that end, Lerman and Weaver (2014) found that citizens nearly uniformly react negatively to criminal legal contact: trust in government and willingness to vote decrease as individuals progress through increasingly intense levels of contact (questioned by police, arrested, convicted, incarcerated; Weaver and Lerman 2010). This withdrawal is not limited to political participation but extends to other forms of civic life as well (e.g., Brayne 2014; Remster and Kramer 2018; Weaver, Prowse, and Piston 2020). Weaver, Prowse, and Piston (2020) describe this form of self-preserving withdrawal from public institutions as a “strategic retreat.”

These findings can be situated in a process that sociologist Monica Bell (2017) calls “legal estrangement,” which captures criminalized Americans’ negative perceptions of government as well as the historical conditions that produced them. Research on legal cynicism has found that public perceptions of abusive police practices can reduce willingness to report crimes or cooperate with law enforcement (Tyler, Fagan, and Geller 2014). The “hidden curriculum” (Justice and Meares 2014; Meares 2017) of the criminal legal system thus teaches Americans about their identities as citizens—even parts of their identities that have little to do with policing or incarceration.

This literature has given scholars far greater insight into the participatory consequences of incarceration, but it says little about the effects of *lower-level* contact with the criminal legal system on political participation. Yet far more Americans have low-level contact with the police than will ever spend a night behind bars: just under 20 million Americans experience a traffic stop each year, whereas approximately 10 million Americans are arrested and jailed each year (Harrell and Davis 2020; Zeng and Minton 2021). A police stop might be among a voter’s first interactions with the criminal legal system; thus, stops may be important for political socialization precisely because they are an early stage in the criminalization process.

Recent work shows that when threats are made newly salient, individuals can update their behavior (Hazlett and Mildenerger 2020; Lujala, Lein, and Rød 2015; Mendoza Aviña and Sevi 2021; Skogan 2006). Thus, although humans are generally bad at incorporating new information into their worldviews (e.g., Lord, Ross, and Lepper 1979), police stops—which are often considered unfair (Snow 2019)—might provoke a rethinking of the police and government and a subsequent updating of political behavior. Gerber et al. (2017) note in their study that the participatory consequences of incarceration might be small because incarceration “is an outcome that often follows a long series of interactions with the criminal justice system” (1145). In other words, much of what the criminal legal system “teaches” might have already been learned by the time an individual is sent to prison. Someone who is stopped by the police, however, might have had fewer negative interactions with the state, resulting in comparatively

larger turnout effects relative to the size of the disruption.

Additionally, the fact that traffic stops affect a larger and systematically less marginalized group of Americans compared with incarceration could help explain the relationship between stops and voting.¹ Traffic stops might be the primary way some of these Americans learn about the criminal legal system. If these Americans have not already “learned” about the system from their neighborhoods or family members, the political consequences of such newly gleaned knowledge might be large.

In short, although past work has argued that criminal legal contact influences participation through both “resource” and “socialization” mechanisms, we contend that the latter are particularly important for our study. The relatively small resource disruptions coupled with outsized opportunities for new learning about the state likely means any turnout effects will operate primarily through avenues associated with socialization (that is, legal estrangement and strategic retreat). Unfortunately, our empirical approach cannot formally adjudicate between the relative importance of the mechanisms. Future work should take up this question.

Potential for Racially Disparate Effects

In addition to testing the potentially demobilizing effect of traffic stops on voter turnout, we ask whether this effect is different for Black voters, who are disproportionately subjected to traffic stops (see Table 1) as well as criminal legal contact more broadly.

We propose that two causal mechanisms could distinctly shape the treatment effects for Black voters. First, we expect that due to greater baseline criminal legal contact, Black voters could have “less to learn” from stops in our analysis, thus leading to a weaker overall turnout effect. Separate from this “learning” process, it’s possible that a comparatively stronger initial psychological salience of traffic stops could lead to a larger demobilizing effect for Black voters in the short term. Thus, as the short-term demobilizing effect of a stop fades, the treatment effect returns to a baseline of “less learning.”

The average Black American knows far more about the criminal legal system than the average non-Black American due to racial disparities in policing and incarceration (Lee et al. 2015). In the previous section, we argued that police stops might reduce turnout because motorists stopped by the police might gain “new” information about the police and government more generally from this stop. Given that Black Americans have higher baseline exposure to the criminal legal system, the modal police stop could result in less new

¹ For instance, whereas Rabuy and Kopf (2015) find that individuals sent to prison make less than \$20,000, our analysis of the 2018 Cooperative Election Study indicates that respondents issued a traffic ticket in the preceding year had an average family income in excess of \$70,000.

TABLE 1. Balance Table

| Variable | Treated voters | Control voters | Never stopped |
|---------------------|----------------|----------------|---------------|
| % White | 47.4% | 47.4% | 62.2% |
| % Black | 24.4% | 25.5% | 13.1% |
| % Latino | 19.0% | 18.7% | 16.0% |
| % Asian | 2.1% | 2.1% | 2.7% |
| % Male | 53.2% | 53.4% | 42.8% |
| % Democrat | 42.5% | 42.6% | 37.9% |
| % Republican | 23.7% | 23.6% | 31.3% |
| Age | 42.5 | 41.8 | 51.9 |
| Median income | \$62,836 | \$62,409 | \$67,897 |
| % with some college | 60.3% | 60.3% | 63.8% |
| Unemployment rate | 6.6% | 6.5% | 5.9% |
| Turnout $t=-3$ | 31.7% | 31.7% | |
| Turnout $t=-2$ | 29.6% | 29.6% | |
| Turnout $t=-1$ | 44.6% | 44.6% | |
| Stops in preperiod | 2.2 | 1.9 | |
| Paid money | 89.4% | 89.4% | |
| Civil stop | 82.6% | 82.6% | |
| Stopped by Tampa PD | 47.0% | 47.0% | |

knowledge and provoke a smaller reduction in political participation.

Still, traffic stops differ in meaningful ways for Black and non-Black Americans. These differences could increase the psychological salience of stops for Black voters, especially in the immediate aftermath of a stop. As Baumgartner, Epp, and Shoub (2018) note, Black Americans are more likely than are whites to receive both “light” (i.e., a warning without a ticket) and “severe” (i.e., arrest) outcomes from a traffic stop. Although this may seem paradoxical at first, the authors explain: “while many might rejoice in getting a warning rather than a ticket, the racial differences consistently apparent in the data suggest another interpretation for black drivers: even the officer recognized that there was no infraction” (88). Goncalves and Mello (2021) find that Florida Highway Patrol officers are more likely to give “discounted” tickets to white motorists than to Black or Hispanic motorists, and although Black drivers are also more likely to be searched and arrested, they are less likely to be found with contraband (Baumgartner, Epp, and Shoub 2018). Similarly, Epp, Maynard-Moody, and Haider-Markel (2014) argue that traffic stops are particularly instructive for Black Americans, as pretextual traffic stops politically socialize Black voters to the specific context of discriminatory police ticketing.

The Black Lives Matter movement has increased the salience of structural racism in policing across the country, as have the tragic stories of individuals like Philando Castile who was killed during a police stop. Increasing municipal reliance on fines and fees creates more opportunities for police violence, and routine interactions with the police are also more likely to turn deadly for Black Americans than for others (Brett 2020; Levenson 2021). Indeed, Alang, McAlpine, and McClain (2021) find that Black Americans

experience “anticipatory stress of police brutality” (i.e., symptoms of depression and anxiety) to a degree that white Americans do not. Thus, even if an individual police stop for a Black American is relatively unremarkable on its own, the background context that the interaction *could* have turned deadly is likely to increase the psychological salience of traffic stops for Black drivers. We expect that traffic stops that immediately precede an election should be more demobilizing.

These apparently competing mechanisms can be reconciled by examining temporal variation in the effect of traffic stops on voting. We expect to find that the psychological salience of a police stop will disproportionately reduce the turnout of Black Americans in the short-term. Over the longer-term—when the immediacy of the police stop fades—we expect smaller turnout effects for Black Americans, potentially because they have less to learn from a given stop (pushing the treatment effect toward zero).

DATA AND DESIGN

We estimate the causal effect of traffic stops on voter turnout using individual-level administrative data from Hillsborough County, Florida (home to Tampa). The empirical estimand is the turnout gap between registered voters in Hillsborough County who have recently been stopped and voters who will be stopped in a future period, conditional on similar turnout in past elections and similar demographic characteristics. We exploit unusually detailed public data, which allows for a precise causal analysis that cannot be conducted in counties that do not provide ticketing records with personally identifiable information or states that do not include self-reported race data in the voter file.

Replication materials are available in the American Political Science Review Dataverse (Ben-Menachem and Morris 2022). Out of concerns for privacy and due to the use of a proprietary geocoder, we do not post individually identifiable data.

Hillsborough County

The Hillsborough County Clerk makes information publicly available about every traffic stop in the county going back to 2003. These data include the name and date of birth of the individual stopped, the date of the offense, and other information.²

Beyond the uniqueness of this dataset, Hillsborough County is a jurisdiction of substantial theoretical interest. The county is home to Tampa, where the Tampa Police Department has maintained “productivity ratios” for officers since the early 2000s (Zayas 2015a). Each officer’s number of arrests and tickets was divided by their number of work hours, and this ratio was used in performance evaluations. In 2015, written warnings were added to this ratio, and scrutiny from the *Tampa*

² See <https://publicrec.hillsclerk.com/Traffic/>.

Bay Times may have reduced the importance of the ratio in officer evaluations. Regardless, the department's de facto ticketing quotas were active during our study period, and voters may have been aware of them as well. Earlier that year, the same newspaper reported on the police department's practice of relentlessly ticketing Black bicyclists (Zayas 2015b). This investigation catalyzed a US Department of Justice investigation and report, requested by Tampa's mayor and police chief.

Ticketing has also been expressly politicized in Tampa: Jane Castor, who was elected mayor in 2019, was Tampa's police chief until 2015 and publicly defended her department's disproportionate ticketing of Black bicyclists before retracting her defense ahead of her mayoral campaign (Carlton 2018). Her opponent, banker and philanthropist David Straz, campaigned against red-light cameras and focused his outreach in Tampa's Black communities (Frago 2019).³

Design and Identification Strategy

To identify stopped voters, we match the first and last names and dates of birth from the stop data against the Hillsborough County registered voter file. Meredith and Morse (2014) develop a test for assessing the prevalence of false positives in administrative record matching. We present the results of that test in section 1 of the Supplementary Materials (SM). We likely have a false-positive match of around 0.03%, a figure we consider too low to affect our results meaningfully.

Using a single post-treatment snapshot of the voter file can result in conditioning on a post-treatment status (see Nyhan, Skovron, and Titunik 2017). Instead, we collect snapshots of the voter file following each even-year general election between 2012 and 2018. We thus observe virtually all individuals who were registered to vote at any time during our period of study. Unique voter identification numbers allow us to avoid double-counting voters who are registered in multiple snapshots. We retain each voter's earliest record and geocode voters to their home census block groups. We remove tickets issued by red-light cameras, which Hillsborough County only begins including in the data toward the end of our study period.

By matching the police stop and voter records, we identify all voters who were stopped between the 2012 and 2020 general elections. Voters stopped between the 2018 and 2020 elections serve only as controls. We collect self-reported information regarding the race of each voter from Florida's public voter file rather than the police stop data. Voters are considered "treated" in the general election following their stop. Treated voters are then matched to a control voter using a nearest-neighbor approach, with a genetic algorithm used to determine the best weight for each

characteristic (Sekhon 2011).⁴ Control voters are individuals who are stopped within the two years following the post-treatment election of the treated voters. Put differently, if a voter is stopped between 2012 and 2014, their control voter must be an individual stopped between the 2014 and 2016 elections. A voter cannot both be a treated and control voter for the same election; therefore, someone stopped between the 2012 and 2014 elections and again between the 2014 and 2016 elections cannot serve as a control for anyone stopped between 2012 and 2014. We limit the target population to voters who are stopped at some point in order to account for unobserved characteristics that might be associated with both the likelihood of being ticketed and propensity to vote.

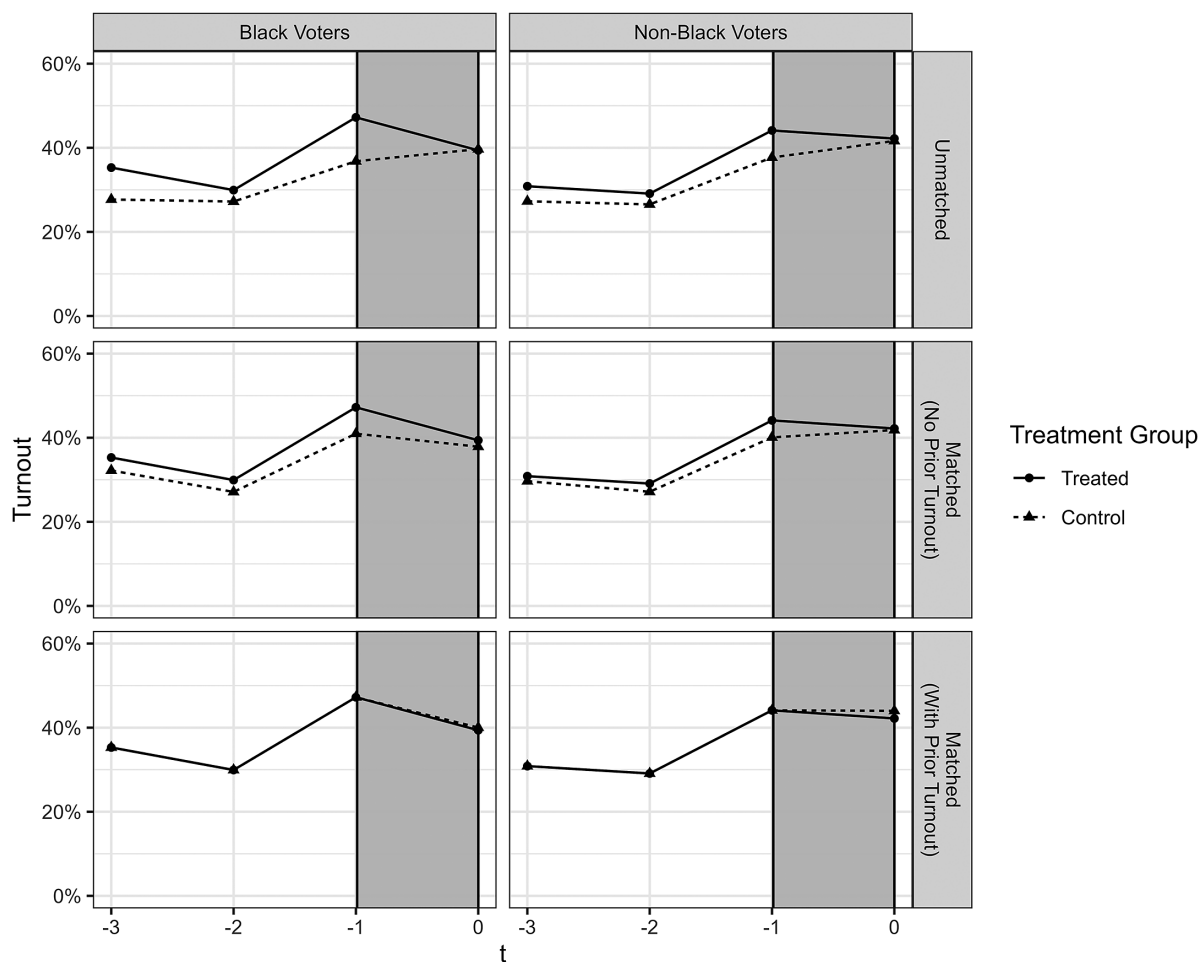
We match voters on individual-level characteristics (race/ethnicity, gender, party affiliation, age, and number of traffic stops prior to the treatment period) and block group-level characteristics from the 2012 five-year ACS estimates (median income, share of the population with some college, and unemployment rate). We match exactly on the type of ticket (civil/criminal infraction, whether they paid a fine, and whether they were stopped by the Tampa Police Department) to ensure that treated and control voters receive the same treatment. Finally, we match treated and control voters on their turnout in the three pre-treatment elections. Matching is done with replacement, and ties are not broken. This means that some treated voters have multiple controls; the regression weights are calculated to account for this possibility.

We assume that after controlling for observable characteristics, past turnout, *and* the unobservable characteristics associated with experiencing a traffic stop, the timing of the stop is effectively random. This is conceptually similar to the regression discontinuity in time framework, and we assume that any turnout difference between the treated voters and their controls is the causal effect of a police stop on turnout. Our overall turnout effects are robust to weaker assumptions: as we show, we uncover large, negative turnout effects even when we force voters stopped shortly before the election to match to voters stopped shortly afterwards.

Our analytical design incorporates matching in a traditional difference-in-differences model in order to improve the credibility of our identification assumptions. Leveraging pre-treatment turnout allows us to estimate the difference-in-differences model, and the matching procedure improves the plausibility of the parallel trends assumption by reducing salient observed differences between the treated and control voters. For a more detailed discussion of how matching can improve on traditional difference-in-difference approaches when using panel data, see Imai, Kim, and Wang (2021).

³ These facts would suggest the potential for a salient effect of ticketing on voter turnout in Tampa mayoral elections. We attempted this analysis, but voter turnout is too low in Tampa mayoral elections for our research design to produce an informative result.

⁴ Due to computing constraints, a 5% random sample stratified by treatment status is used to calculate the genetic weights. The full sample is used in the actual matching process.

FIGURE 1. Turnout, Treated and Control Voters


Note: Treatment occurs in the shaded band. The full regression tables are available in section 3 of the SM.

We then estimate the following equation:

$$v_{it} = \beta_0 + \beta_1 \text{Treated}_i + \beta_2 \text{PostTreatment}_t + \beta_3 \text{Treated}_i \times \text{PostTreatment}_t + \beta_4 \text{Year}_t + \delta Z_i + \varepsilon_{it} \quad (1)$$

Individual i 's turnout (v) in year t is a function of the year and whether they were stopped by the police. In the equation, β_1 measures the historical difference between treated voters and their controls, β_2 measures whether turnout increased for controls in the first election following the treated voter's stop, and β_3 tests whether turnout changed differently for treated voters than their controls in the election following their police stop. So, β_3 will capture the causal effect of a police stop on voter turnout; it is the unit-specific quantity measured in our empirical estimand (Lundberg, Johnson, and Stewart 2021). The term $\beta_4 \text{Year}_t$ captures year fixed-effects depending on the timing of the police stop, and the matrix δZ_i contains the individual- and neighborhood-level characteristics on which the match was performed, included in some of the models. In some models, we also interact the treatment and period

variables with a dummy indicating whether the voter is Black to determine race-specific treatment effects.

RESULTS

We begin by plotting the turnout of treated and control voters under different analytical approaches in Figure 1. The first row plots the turnout of all treated and control voters without any matching. In the second row, we plot the turnout of treated voters and matches selected when we exclude pre-treatment turnout from the matching procedure. In the final row, we present the controls selected when pre-treatment turnout is included in the match.⁵ The first election following a treated voter's stop is denoted as $t = 0$, and the years in which t is less than zero are the periods prior to the stop.

⁵ For a more thorough discussion of the trade-offs involved in including or omitting pre-treatment outcomes in matched difference-in-differences, see Lindner and McConnell (2019).

All three approaches demonstrate the same general treatment effect. In the first two approaches, treated voters consistently have slightly higher turnout rates than do the controls prior to the treatment; the difference between these two groups disappears in the election following the stop of the treated voter (visual indication of a negative treatment effect). Both the “raw” difference-in-differences approach and the approach excluding the pre-treatment outcomes from the match exhibit a potential violation of the parallel trends assumption (particularly for Black voters), so we adopt the final specification as our primary model. However, our negative treatment effects are not simply an artifact of our modeling decisions. The full specification for the first row of Table 1 (with and without matching covariates included) can be found in columns 1 and 2 of Table A7 in the SM, and those corresponding to the approach where prior turnout is not included can be found in columns 3 and 4 of the same table.

In Table 1 we present the results of the matching algorithm using our preferred specification incorporating pre-treatment turnout. As the table demonstrates, the selected control voters are very similar to the treated voters.

It is worth noting that voters who were stopped between 2012 and 2020 were far more likely to be Black and male than the general electorate and live in census block groups with moderately lower incomes.

Table 2 formalizes the final row of Figure 1 into an ordinary least squares regression. The full models from Table 2 with coefficients for the matched covariates can be found in Table A6 of the SM, and full specifications for 2014, 2016, and 2018 individually can be found in

Tables A3–A5, respectively. Models 1 and 2 show our overall causal effect, and models 3 and 4 allow for the possibility that a stop differentially mobilizes Black voters. In models 1 and 3, we include only the treatment, timing, and race dummies, whereas the full set of covariates used for the matching procedure are included in models 2 and 4. The empirical estimands are $Treated \times Post\ Treatment$ and $Treated \times Post\ Treatment \times Black$. In models 1 and 2, the coefficient on $Treated \times Post\ Treatment$ measures the overall treatment effect, and in models 3 and 4 it measures the treatment effect for non-Black voters. The coefficient on $Treated \times Post\ Treatment \times Black$ measures any effect for Black voters beyond the effect measured for non-Black voters. By multiplying the Black dummy through the treatment and timing dummies, models 3 and 4 become triple-difference (or difference-in-difference-in-differences) models. In Figure 2 we plot the coefficients for each of the individual years as well as the overall treatment effect. These models follow the same logic as Table 2, where we show the point estimates with and without the matched covariates included. The full models shown in Figure 2, with coefficients for the matched covariates, can be found in Tables A3–A6 of the SM.

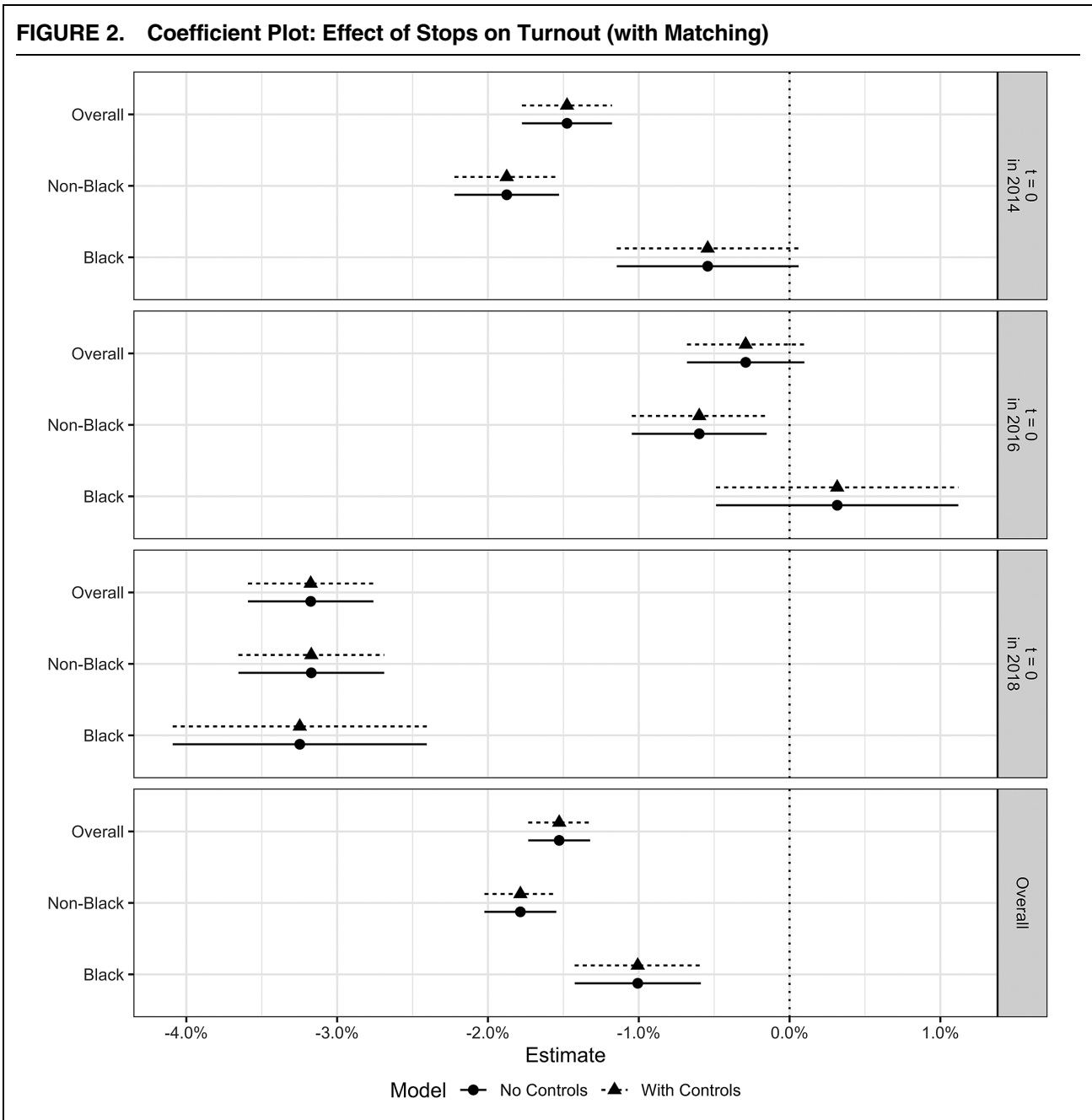
As both Figure 2 and Table 2 make clear, traffic stops meaningfully depressed turnout. In models 1 and 2, the estimated overall treatment effect is -1.5 percentage points (pp). In models 3 and 4, we can see that traffic stops were less demobilizing for Black individuals than for others—non-Black turnout was depressed by 1.8 percentage points, whereas the negative effect was just 1.0 for Black individuals.

TABLE 2. Overall Treatment Effect

| | Model 1 | Model 2 | Model 3 | Model 4 |
|--|----------------------|----------------------|----------------------|------------------------|
| Treated \times Post-treatment | -0.015*** (0.001) | -0.015*** (0.001) | -0.018*** (0.001) | -0.018*** 2 (0.001) |
| Treated \times Post-treatment \times Black | | | 0.008** (0.002) | 0.008** 1 (0.002) |
| Treated | 0.000*** (0.000) | 0.000 (0.000) | 0.000 (0.000) | 0.000* (0.000) |
| Post-treatment | 0.061*** (0.001) | 0.051*** (0.001) | 0.076*** (0.001) | 0.066*** 1 (0.001) |
| Black | | 0.006*** (0.001) | 0.026*** (0.002) | 0.020*** (0.001) |
| Treated \times Black | | | 0.002 (0.001) | 0.000 (0.000) |
| Post-treatment \times Black | | | -0.058*** (0.002) | -0.058*** (0.002) |
| Intercept | 0.393*** (0.001) | -0.015*** (0.001) | 0.386*** (0.001) | -0.019*** (0.001) |
| Year fixed effects | ✓ | ✓ | ✓ | ✓ |
| Matching Covariates Included | | ✓ | | ✓ |
| Num.Obs. | 2,349,808 | 2,349,808 | 2,349,808 | 2,349,808 |
| R^2 | 0.055 | 0.554 | 0.055 | 0.555 |
| R_2 Adj. | 0.055 | 0.554 | 0.055 | 0.555 |
| RMSE | 0.47 | 0.32 | 0.47 | 0.32 |

Note: Dependent variable: individual-level turnout; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

FIGURE 2. Coefficient Plot: Effect of Stops on Turnout (with Matching)



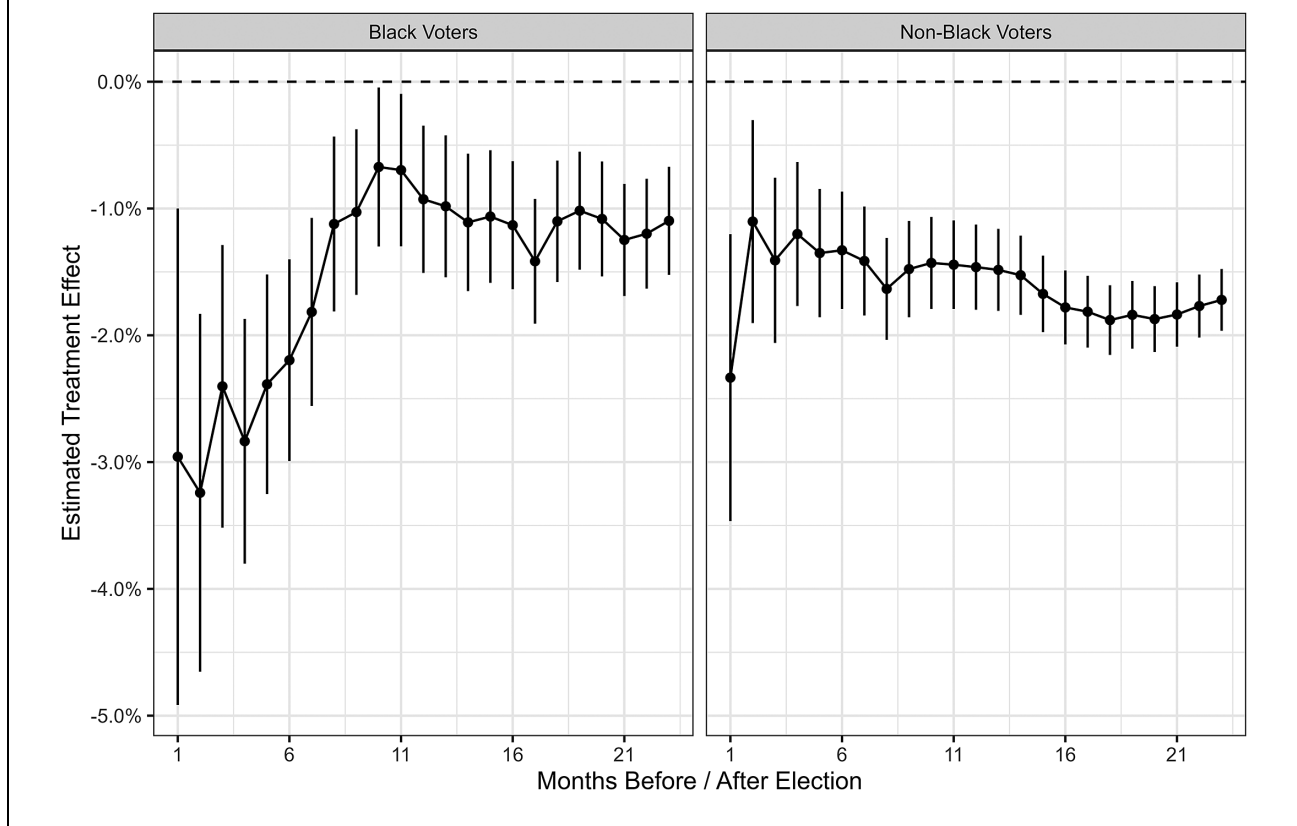
Although the treatment effect is still substantively quite large for Black individuals, Hillsborough County Black voters’ turnout in federal elections was not as negatively affected by police contact as that of non-Black individuals. It is also clear that midterm turnout is more affected by these stops. The negative effect is statistically significant in all years for non-Black residents but much smaller in 2016 (-0.6 pp) than in 2014 (-1.9 pp) or 2018 (-3.2 pp).

Testing the Temporal Durability of the Effect

In the section above, we present the average effect of a police stop on turnout for treated voters. This effect is averaged across all voters stopped in the two years

prior to a federal election. Although using such a large pool of treated and control voters allows for better covariate balance within pairs, such wide windows around each election give us no insight into the temporal stability or variability of the treatment effect. Moreover, treated and control pairs might have been stopped at very different points; a voter stopped almost two years before an election can be paired with someone stopped two years *after* that election, meaning there were four years between the police stops. These voters might differ in important, unobservable ways.

Here, we explore the temporal component of our primary results by rerunning our matching process on a variety of different windows around the elections. In

FIGURE 3. Treatment Effect over Time

the most conservative approach, we force voters stopped in the month before an election to match with voters stopped in the month after the election; we then gradually expand this window, allowing voters stopped in the two months before the election to match to those stopped in the two months afterwards until we reach the two-year period used in our main model. The left-hand side of Figure 3 plots the treatment effect for Black voters depending on the window used; the right-hand side shows these estimates for non-Black voters. The full regression outputs for these models can be found in Tables A8–A11 in the SM.

The treatment effects for Black voters show strong temporal variability. In fact, when looking at voters stopped shortly before an election, police stops are *more* demobilizing for Black than non-Black voters. This relationship flips by the time the full pool of voters is included. The treatment effect decreases from roughly -3 to -1 pp over the range of windows.

Although the administrative data prevent us from exploring the psychological mechanisms at play, and their temporal durability, this finding is consistent with our theoretical expectations: a police stop might be more psychologically salient—and thus more demobilizing—for Black voters in the *short term*. Once the immediate salience of the stop fades, it's possible that baseline knowledge about the criminal legal system mitigates longer-term effects, thus explaining the smaller effects in the models with longer windows. Of

course, future work should explore these possibilities directly.

The right-hand side of the plot shows far less temporal variation in the magnitude of the treatment effect for non-Black voters. Although non-Black voters are most demobilized if stopped in the month before the election, the overall trend is fairly stable (if moderately downward sloping).

DISCUSSION

Although existing sociological and political science literature has examined the rise and collateral consequences of criminalization on political socialization, no study has investigated the causal relationship between traffic stops and voter turnout using individual-level administrative data.

Given how widespread police stops are and their relationship to racial injustice, their political implications demand close study. What we find advances our understanding of how lower-level police contact affects political participation. We find that traffic stops reduce turnout among non-Black voters, with a smaller negative effect for Black voters. We also find substantial temporal variation in the treatment effect for Black voters: in the short term, stops appear to be more demobilizing, but as time passes they become comparatively less demobilizing. We conclude that the political

consequences of police stops are unique for Black Americans—and that they are, on balance, less demobilizing for Black Americans than for others. This joins other recent research finding that small-scale interventions like Get-Out-the-Vote encouragements have smaller effects on Black Americans (Doleac et al. 2022), perhaps because their opinions on the criminal legal system are more firmly set. Scholars ought to explore more specifically when and what sorts of interactions produce larger effects for Black Americans and when these effects are smaller.

Our findings have several implications for political science scholarship. Although existing literature suggests that the most disruptive forms of criminal legal contact (i.e., criminal convictions and incarceration) consistently discourage voting (Burch 2011; Lerman and Weaver 2014; White 2019b), research regarding police stops has produced more mixed results (Laniyonu 2019). We extend political socialization theory to traffic stops, the most common form of police contact in America, and find that police traffic stops generally reduce turnout. For Black voters, however, our findings suggest that traffic stops are less demobilizing, a contrast with existing scholarship wherein more disruptive forms of criminalization discourage Black voters more than non-Black voters. Our findings constitute new evidence in support of our theory that police stops are distinct from other forms of criminal legal contact and therefore catalyze different political behaviors among Black voters, who are disproportionately affected by both ticketing and criminalization in general.

It is worth considering the implications of a study focused only on the behavior of individuals who were registered to vote at some point during the study period. Registration is itself an act of political participation; therefore, our study population is systematically more engaged in electoral politics than the general population. This supports our argument that traffic stops are an important form of political socialization. More specifically, if voters in the target population already understood the ballot box as a tool they could use to change political outcomes or at least make their voices heard, structurally, it stands to reason that the effect of traffic stops is potent enough to overcome longer-term attitudes and behaviors with respect to government. In other words, even if the observed point estimates are small, the fact that registered voters' turnout is depressed by traffic stops justifies our contention that traffic stops are politically salient events. This focus on registered voters likely makes our results conservative: we cannot capture the lost participation of individuals who would have registered and voted if they were not stopped by the police.

Focusing on the turnout of registered voters also misses other important political behavior that future work should explore. As Walker (2020b) suggests, stopped Black individuals may be politically mobilized for activities other than voting not observed in this study, such as contacting elected representatives or volunteering for campaigns. The fact that we find that

stops produce a negative turnout effect for Black voters does not rule out the possibility that stopped Black motorists could be more likely to engage in nonvoting political activities. Christiani and Shoub (2022) also find that traffic stops and tickets can catalyze nonvoting political participation, but observe stronger positive effects among people who have better perceptions of police (i.e., white people).

Existing political science theory regarding “injustice narratives” could provide an alternate or complementary framework for interpreting our results. Recent work from Hannah Walker (2020a; 2020b) argues that police contact could lead to a *mobilizing* effect if voters understand criminal legal contact in the context of a narrative of racial injustice. Although she finds that this sense of injustice is especially likely to increase political participation in nonvoting ways (such as attending a protest or signing a petition) and particularly salient following proximal rather than personal contact, the injustice narrative mechanism could also affect voter turnout following personal contact. Thus, the temporal variation we found could occur because the experience of personal contact is eventually incorporated into an “injustice narrative” because Black Americans who are socially proximate to the stopped individual end up also being subjected to criminal legal contact between the stop and the election of interest, or both.

The injustice narrative mechanism could provide another justification for the reversal of the initially more demobilizing effect of stops on Black voter turnout—perhaps some subset of stopped Black voters end up affirmatively mobilized several months after the stop, thus explaining the overall comparatively smaller demobilizing effect observed in our results. Unfortunately, the administrative data do not allow for a compelling test of this hypothesis; most information about voters in our analysis is at the census tract level, not individual level, and we lack information about activities such as participation in community organizations that Walker suggests might mediate the relationship between criminal legal contact and political behavior. Ultimately, we are sensitive to the fact that although administrative data provide real-world evidence of actual behavior, such data limit our ability to understand the causal mechanisms at play. This means that although we demonstrate that police stops are demobilizing, future work must further investigate how stops are interpreted by individuals and translated into political behavior.

Future work should explore these and other questions. Particular attention should be paid to variation *within* the Black community. When is this sort of contact demobilizing? For whom? Can organizers build on this potential for broad-based political action? We were unable to test whether what we observed was simply *decreased* demobilization or whether some subgroups of the Black population were mobilized but others were demobilized. Scholars should also investigate the interactive effects of criminal legal contact, asking whether police stops result in different political behavior for formerly incarcerated

individuals than individuals with no other contact with the system. Finally, this project looks only at voting, so scholars should continue exploring whether low-level contacts also shape other sorts of engagements with the state.

Although we have contributed new evidence suggesting that police stops may not demobilize Black voters to the same extent as they do non-Black voters, we emphasize that this finding does not redeem or justify exploitative ticketing practices. Black Americans already suffer from disproportionate police contact and the racial wealth gap, and revenue-motivated ticketing only increases the burden on Black communities nationwide. Policy makers should work to ensure that Black Americans no longer have to struggle to enjoy the same political power as whites—to that end, the current trend of voting rights restriction policies across the country is especially pernicious. Even if some Black Americans understand the ballot box as one tool they can use to limit the state's power to exploit and harm them, policy makers should still feel an obligation to support voting rights protections and stop disproportionate ticketing in Black communities.

SUPPLEMENTARY MATERIALS

To view supplementary material for this article, please visit <http://doi.org/10.1017/S0003055422001265>.

DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/YGTFBW>. Limitations on data availability are discussed in the text.

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CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in their research.

ETHICAL STANDARDS

The author declares the human subjects research in this article was deemed exempt from review by the Graduate Center, City University of New York.

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