



Registration costs and voter turnout

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ARTICLE INFO

Article history:

Received 20 February 2019

Revised 20 March 2020

Accepted 15 April 2020

Keywords:

Registration costs

Voter turnout

Elections

ABSTRACT

We exploit a natural experiment in Massachusetts in 2012 to estimate the causal effect of lowering voter information and registration costs on: voter registration, turnout and voting behavior in presidential elections. Both a within-Massachusetts specification and a cross-state specification (utilizing Vermont, Maine and New Hampshire data) show a statistically significant effect on voter registration and turnout that is of a material magnitude. However, conditional on registration we find no material difference in turnout. Finally, we find a large treatment effect on Democrat voteshare. Our results highlight the importance of voter registration and information costs for electoral participation, especially for citizens from lower socioeconomic backgrounds.

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1. Introduction

Since at least Hegel it has been recognized that, in a large electorate, the probability of being pivotal in an election is small.² Some estimates put the chance at around one in 89,000 in a US Congressional election (Mulligan and Hunter, 2003). There may be other benefits of voting such as fulfilling a sense of civic duty, but Hegel's calculus still suggests that even relatively small costs of voting can have a material effect on turnout. Of course, in the United States, one cannot vote if one is not registered.³ Getting voters to the polls is thus a two-part problem: they must be registered and, conditional on that, they must decide to turnout.

From both a practical and a policy standpoint the questions of what affects turnout and how this varies by demographics are very important, and, as we will discuss below, a large literature has addressed these questions. There has been significantly less attention paid to the determinants of the *registration* of voters. This is despite the perceived importance of registration, as reflected in the *National Voter Registration Act* of 1993 ("NVRA").⁴ Moreover, there are large differences in

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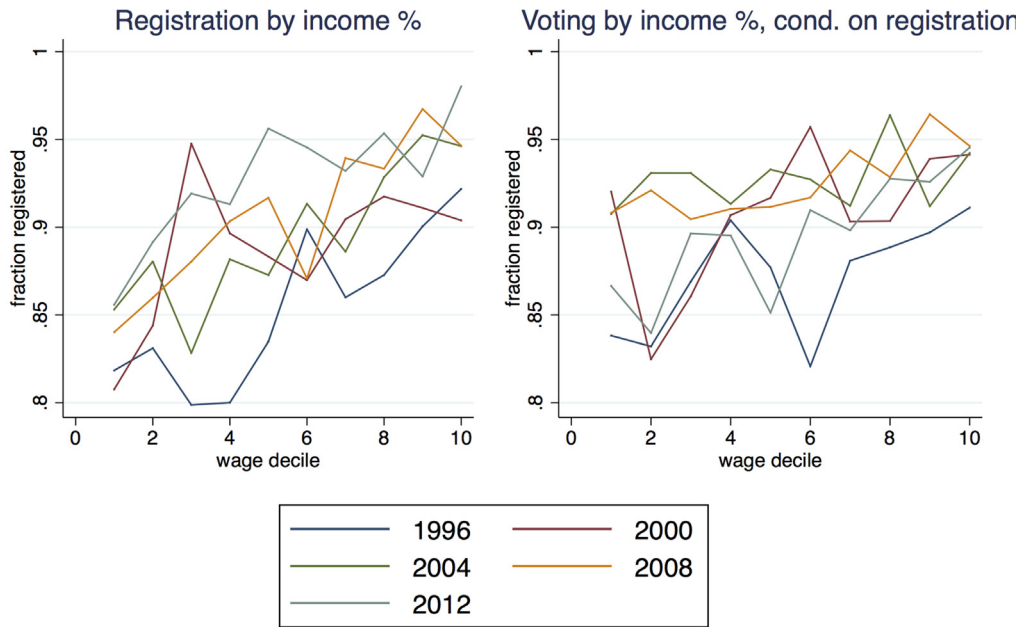
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¹ We thank seminar participants at the Melbourne Institute of Applied Economic and Social Research (University of Melbourne), Australian National University and University of New South Wales for valuable feedback. We also thank the Editor and two anonymous referees for helpful comments and feedback. Richard Holden acknowledges support from the Australian Research Council (ARC) Future Fellowship FT130101159.

² For example, Buchanan (1974) cites a translation of Hegel in 1821 as writing: "As for popular suffrage, it may be further remarked that especially in large states it leads inevitably to electoral indifference, since the costing of the single vote is of no significance where there is a multitude of electors. Even if of voting qualifications highly valued and esteemed by those who are entitled to, they still do not enter the polling booth. As the result of an institution of this kind is more likely to be opposite of what was intended; election actually falls into the power of a few, of a caucus, and so the particular and contingent interest which is precisely what was to have been neutralized."

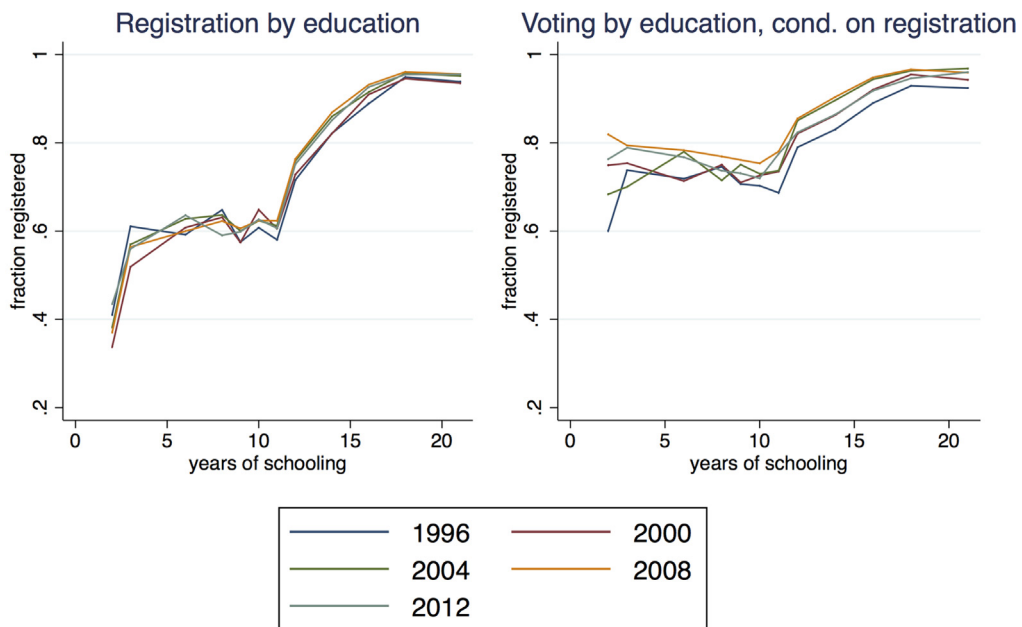
³ The one exception is North Dakota.

⁴ 42 U.S.C. §§1973gg - 1973gg-10.



Source: CPS November supplements, 1996, 2000, 2004, 2008, 2012

Fig. 1. Voter registration by income deciles.



Source: CPS November supplements, 1996, 2000, 2004, 2008, 2012

Fig. 2. Voter registration by years of schooling.

registration based on socioeconomic, racial, and other demographic factors that are well known to be associated with voting preferences. Thus, registration *per se* can have a significant effect on electoral outcomes and on the degree of representation of various groups within the electorate.

Figs. 1 and 2 show the differences in registration and turnout conditional on registration across income and schooling levels in the United States. There are large differences in registration rates. Lower socioeconomic status is associated with low registration rates. However, conditional on registration the differences in voting behavior are much smaller.

The relative paucity of causal evidence⁵ on voter registration is, perhaps, because it is difficult to randomly register voters and hence uncover the causal effect or registration drives. To circumvent this difficulty, we exploit a large scale natural experiment in Massachusetts in 2012 which allows us to observe an exogenous decrease in the costs of voter registration and increase in information about registration and elections.

The NVRA was designed to increase voter registration by requiring state governments to offer voter registration to eligible persons under a variety of circumstances: notably, if a person applies for or renews a driver's license, or applies for or renews public assistance. The NVRA requires, *inter alia*, that all public assistance offices in all states offer clients the opportunity to register to vote with every new or renewal application for benefits and change-of-address transaction (NVRA Section 7, 1993).

On May 15, 2012 the Secretary of the Commonwealth of Massachusetts and the Massachusetts Department of Transitional Assistance ("DTA") were sued for failing to comply with the NVRA requirements in Massachusetts public assistance offices.⁶ The DTA administers Supplemental Security Income ("SSI"), Public Assistance ("SSI"), and Supplemental Nutrition Assistance Program ("SNAP", i.e., food stamps) applications. On July 5th 2012, a settlement agreement was reached under which the Commonwealth and DTA agreed to improve voter registration services at public assistance offices, conduct community voter registration outreach activities in areas with high concentrations of "DTA clients", and send mailings to all Massachusetts residents who had been clients of the DTA from June 2011 to May 2012.⁷ These mailings provided information about registration deadlines, election dates, and included a voter registration application with a pre-paid return envelope.

We treat the actions outlined in the settlement agreement as a large-scale natural experiment in lowering registration and information costs. We exploit variation in treatment intensity, specifically, the exposure to mailings and outreach activities that results from cross-municipality differences in poverty levels and public assistance participation. We examine registration and election outcomes in 2012 and 2008, in 2008 no comparable policy affecting registration and information costs was in place.

We find that the intervention had a positive effect on registration and turnout rates. The turnout rate in the overall population increases in 2012 in the affected municipalities by more than in those which are not affected. The higher turnout is consistent with the higher relative registration rates in these communities in November 2012. On the other hand, conditional on registration we find no effect of the policy on turnout. Our results suggest that among low income populations, registration and/or information costs are a more important barrier to eventual turnout than the costs and benefits associated with the actual voting.

There is a large literature on the determinants of voter turnout. Moreover, randomized field experiments have often been utilized, allowing the genuine causal effect of a particular intervention to be obtained (see for example, Gerber and Green, 1999; Gerber and Green, 2000; Gerber et al., 2003; Nickerson, 2006; Michelson, 2006; Arceneaux and Nickerson, 2009; Dale and Strauss, 2009; Braconnier et al., 2017; Bertocchi et al., 2017; and very early work by Gosnell, 1927). These experiments, however, focus on how to get more registered voters to turnout, rather than getting non-registered voters to register. Notable exceptions are Bertocchi et al. (2017), Nickerson (2015) and Braconnier et al. (2017). Bertocchi et al. (2017) utilizes the policy of preregistration as a natural experiment and shows that it increases registration and turnout for younger individuals. Nickerson (2015) randomly assigns a face-to-face registration drive across 620 streets in six cities and finds a 4.4% increase in registered, of whom 24% turned out to vote. The registration effect is larger on poorer streets and turnout effect is larger on more affluent streets. Braconnier et al. (2017) conducts an extended version of this experiment in France and shows that registration requirements have significant effects on turnout and disproportionately discourage marginalized citizens on the left of the political spectrum.

Both Nickerson (2015) and Braconnier et al. (2017) conduct field experiments to examine the effect of reducing voter registration costs on registration and turnout. Both studies also face similar implementation issues. First, there are potential sample selection concerns associated with the probability of finding the respondent at home during canvassing hours. The survey is more likely to underrepresent individuals who have more activities outside of home, for example those who work longer hours.⁸ Second, individuals who live in apartment buildings, in less populated areas or individuals in severely disadvantaged neighborhoods (which could be perceived as dangerous) were less likely to be included in the pool. Third, the treatment received by each respondent is not necessarily uniform. For example, Braconnier et al. (2017) describes how time spent with each participant may vary when they providing information about the coming elections. Fourth, canvassers

⁵ As we discuss below, notable recent exceptions are field experiments by Nickerson (2015) and Braconnier et al. (2017) and the work by Bertocchi et al. (2017).

⁶ From the case summary: "This voting registration case was filed on May 15, 2012, by a consortium of public interest lawyers, as part of a national campaign... They sued the state of Massachusetts in the U.S. District Court for the District of Massachusetts. The plaintiffs alleged that Massachusetts was systematically failing to comply with the National Voter Registration Act of 1993 (NVRA), section 7, 42 U.S.C. 1973gg-5(a), under which public assistance offices must provide voter registration services to assistance recipients, with each application, recertification, renewal, or change of address. The plaintiffs were an assistance recipient and two voter registration organizations. They sought an injunction requiring implementation and enforcement of appropriate practices and policies to ensure compliance with Section 7 of the NVRA, along with reporting and monitoring of that plan." Source: The Civil Rights Litigation Clearinghouse, the University of Michigan Law School. For more details, see Case No. 1:12-cv-10872-DJC.

⁷ We use the term "DTA clients" as it is defined in the settlement agreement, "any individual who is applying for Public Assistance Benefits, including an individual who has one or more authorized representatives acting on his or her behalf, or any individual who is receiving Public Assistance Benefits".

⁸ Nickerson (2015) states that canvassers spoke to someone at 30–50% of the doors knocked on a given street.

themselves may have their own agenda and spend more time with some participants than with others.⁹ These four issues may have an effect on the generality and replicability of the outcomes. Finally, the implemented method of increasing registration, door-to-door canvassing, could be prohibitively expensive at the state or national level, although there are no estimates of such costs.

Our study uses a natural experiment that implements a uniform treatment to all affected individuals. All treated individuals receive a letter and to register they need to fill out the enclosed form and send it back using a provided prepaid envelope. It is a state-wide experiment, covering individuals even in the most remote and disadvantaged locations of Massachusetts. A priori it is not clear which approach to reducing registration costs is more effective. Face-to-face interactions may speed up the process but may also elicit false information from a participant with regards to their registration status (in case they prefer not to interact with the canvasser). On the other hand, using the “letters method” reduces the costs of interaction with the canvasser but may increase the likelihood of procrastination and non-registration. However, the cost of the “letters method” is substantially lower than the cost of intervention implemented by Nickerson (2015) and Braconnier et al. (2017). The disadvantage of our approach is data limitations. We analyze municipality-level data, whereas Nickerson (2015) and Braconnier et al. (2017) use street level data. As all related studies serve the purpose of informing policy makers about potential effects of voter registration and turnout interventions, the comparison of findings from different experiments provides further guidance about potential outcomes of alternative policies.¹⁰

Both approaches have similar caveats. Most notably, it is not possible to separate the reduction in registration costs and the reduction in information costs under either approach. Information costs are reduced because individuals who are asked to register also receive further information about upcoming elections, or can be encouraged to seek more information about the upcoming elections. Such effects are documented in Braconnier et al. (2017).

Another strand of related literature considers what proportion of (newly) registered voters turnout to vote. Papers that utilize variation in legal rules over time or across political jurisdictions yield varied estimates. For instance Martinez and Hill (1999) find that very few such registrants vote, while Brown et al. (1999), Mitchell and Wlezien (1995), and Timpone (1998) find large effect. Other papers find more intermediate effects (see, for instance: Knack, 1995 and Ansolabehere and Konisky, 2006). This wide range of estimates is unsurprising given that election law changes are typically not random, and thus selection and treatment effects are conflated.¹¹

We contribute to both strands of the literature. Our results provide evidence on the “registration elasticity” of lowering both the direct and informational costs of voter registration. Because we have plausibly causal estimates we also speak to efficacy of the ultimate goal of registration – getting people to vote. The fact that, conditional on registration, turnout is not materially different suggests that the registration margin is a particularly important driver of overall electoral participation.

Finally, our results speak in part to the impact of policies that may affect the costs of voter registration. An example of such policies are voter-ID laws that have been recently enacted in several states. In an economic sense, voter-ID laws are an increase in the cost of voter registration, in particular, for those individuals who do not have a valid ID, who would need to obtain one which involves both financial and transaction costs. Our findings suggest that individuals from the more disadvantaged background need a further reduction in registration costs to boost their participation in elections, whereas a further increase in costs may have an opposite effect. Whatever the benefits – in terms of reduced electoral fraud – maybe be, our results suggest that additional registration costs may have a significant negative effect on registration and turnout, as well as affect election outcomes.¹²

The paper proceeds as follows. Section 2 describes the data. In Section 3 we articulate our empirical strategy and report the results. Section 4 contains some concluding remarks.

2. Data

There are two main components of our data: (i) registration, turnout and voting outcomes, and (ii) demographic, reciprocity and poverty indicators.

The voter registration and turnout data are from the relevant state departments of Election Services. There are 351 municipalities in 14 counties in Massachusetts, we collect information on the number of registered voters in 2008 and 2012 for

⁹ In Braconnier et al. (2017), canvassers are described as volunteers and a large proportion of them is associated with a specific political wing. This issue is alleviated in Nickerson (2015)’s experiment, where canvassers are not local and have less personal interest in election outcomes.

¹⁰ Technically, the two approaches may deliver different results because the direct engagement with potential voters may produce the Local Average Treatment Effect estimates, whereas our strategy allows to estimate the Intention to Treat Effect. See Gonzales et al. (2019) for further discussion.

¹¹ Another series of papers utilize plausibly exogenous shifts in information to assess the effect of information on turnout. Stromberg (2004) finds that areas with a higher share of radio ownership, where information about elections would have been broadcast, had higher voter turnout during the 1920s–1930s. On the other hand, Gentzkow (2006) finds that substitution away from media outlets with higher levels of political coverage reduces turnout. Lassen (2005) finds that individuals that are more informed about the issues being voted on are more likely to vote.

¹² There are a few studies that evaluate the effects of voter-ID laws on registration and turnout. Most studies find either no effect on turnout (e.g., Mycoff et al., 2009; Highton, 2017; Cantoni and Pons, 2019) or negative effect on turnout (e.g., Alvarez et al., 2011; Hajnal et al., 2017). Most studies use an aggregate state level analysis, with the exception of Cantoni and Pons (2019) that uses individual level data. They show that there is no aggregate effect on registration or turnout but there is a positive effect of turnout among Hispanics, possibly driven by compensating efforts to boost turnout in these communities in the affected states.

the months of January (in 2008), February (in 2012) and October (in 2008 and 2012).¹³ Presidential elections took place in November 2008 and 2012; we collect the number of votes for each presidential candidate in 2008 and 2012 as well as the overall turnout numbers.

Our analysis uses data for three control states, Vermont, New Hampshire and Maine.¹⁴ There are 233 municipalities in Vermont in 14 counties; registration information is available for March and November in 2008 and 2012. There are 240 municipalities in 10 counties in New Hampshire, with registration data available for January and November in 2008 and 2012. There are 490 municipalities in 16 counties in Maine, with registration data available for June and November in 2008 and 2012. For all control states we collect data on votes for each presidential candidate in 2008 and 2012 as well as the overall turnout numbers.

We define treatment intensity in accordance with the settlement agreement reached in the law suit against Massachusetts DTA and Secretary of the Commonwealth (SOC). On July 5, 2012, the SOC and DTA agreed to do the following in the next four months: (1) Send a mailing with an enclosed voter registration application to all persons who were “DTA clients” within the last year and may have been denied the opportunity to register; (2) Conduct or participate in at least three community voter registration outreach activities in areas with high concentrations of “DTA clients”; (3) Arrange for broadcasts of public service announcements publicizing the availability of voter registration forms and registration assistance; (4) Provide training, modify procedures, and increase oversight regarding the required voter registration services. Actions described in (1) and (2) are the relevant policies we utilize as treatment in the empirical analysis. The intensity of the treatment is defined as the fraction of “DTA clients” in a given municipality.

We identify the fraction of the “DTA clients” using the following measures at the municipality level: (i) Proportion of households receiving Supplemental Security Income (SSI) and/or Public Assistance (PA). SSI provides monthly payments to certain eligible, low-income individuals who are 65 or older, blind, or disabled; PA is combined of Transitional Aid for Families with Dependent Children (TAFDC), Emergency Aid to the Elderly, Disabled and Children (EAEDC), and Temporary Assistance for Needy Families (TANF). (ii) Proportion of households receiving Supplemental Nutrition Assistance (SNAP) (food stamp benefits); (iii) Proportion of individuals receiving SNAP; (iv) Proportion of population below the federal poverty line.

The DTA reciprocity, poverty and demographic information are at the municipality level for each state, drawn from the American Community Survey (ACS). We use the 2005–2009 5 Year sample to obtain demographic measures for 2008 and the 2007–2011 5 Year sample to obtain demographic measures for 2012. We use normalized DTA reciprocity and poverty measures in all estimations, such that the relevant sample mean is zero and the standard deviation is one. Reciprocity and poverty measures described in (i), (ii) and (iv) are recorded by the ACS. To construct (iii), proportion of individuals receiving SNAP, we use the information on the number of families that receive SNAP which is reported by family type, we assign the number of persons in the family according to the reported family type (two persons in married couple families, one person in husband only or wife only family, and one person in non-family households). Demographic data utilized in our analysis include: proportion of women, proportion of black, proportion of Hispanic, median age, proportion of population over 18 years old, proportion of population with with a BA degree or higher, proportion of population with less than high school education, proportion of foreign-born population, and the proportion of population who lived in a different county one year ago.

Our constructed sample excludes municipalities with high (above 50%) reported margin of error in the population variable (where the margin of error is a measure of the possible variation of the reported value around the actual population value). The excluded observations mostly include very small municipalities. Therefore, our final dataset includes 350 municipalities in Massachusetts, 233 municipalities in Vermont, 463 municipalities in Maine and 233 municipalities in New Hampshire.¹⁵

Appendix Tables 1 and 2 report descriptive statistics of these data by state, for 2008 and 2012.

3. Empirical strategy and results

3.1. Empirical strategy

Our empirical strategy utilizes actions taken by the Commonwealth of Massachusetts and DTA under the settlement agreement starting July 5th 2012 as a large scale natural experiment in lowering registration and information costs. The Commonwealth of Massachusetts and DTA agreed to improve voter registration services at public assistance offices, conduct community voter registration outreach activities in areas with high concentrations of “DTA clients”, and send mailings to all Massachusetts residents who had been clients of the DTA from June 2011 to May 2012. We use municipality level data and exploit variation in treatment intensity using the three measures of poverty, (i) proportion of population receiving SSI plus percent receiving PA, (ii) proportion of population receiving SNAP, (iii) proportion of individuals receiving SNAP, and

¹³ Voter registration data are snapshots of relevant statistics before the presidential preference primary and presidential elections, statistics for other dates are not available.

¹⁴ The choice of control states is due to data availability and relative geographic proximity to Massachusetts.

¹⁵ Difference-in-differences estimations use 700 observations for Massachusetts municipalities. Difference-in-difference-in-differences estimations use all states data and include 2558 estimations. Measure of proportion of population below poverty line is missing for four municipalities in Vermont, New Hampshire and Maine, therefore the number of observations in these specifications is 2550.

Table 1

Percentage change in number of registered voters, October 2010 – February 2012, Massachusetts, N=350.

	% below poverty line				% below poverty line			
		% SSI+PA	% SNAP	% SNAP (ind)		% SSI+PA	% SNAP	% SNAP (ind)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DTA clients	-0.0020 (0.002)	-0.0026 (0.003)	-0.0024 (0.002)	-0.0026 (0.002)	0.0012 (0.003)	0.0009 (0.005)	0.0016 (0.005)	0.0017 (0.004)
Median age					-0.0003 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)	-0.0003 (0.000)
% Black					-0.0002 (0.000)	-0.0002 (0.000)	-0.0002 (0.000)	-0.0002 (0.000)
% Hispanic					0.0001 (0.000)	0.0001 (0.000)	0.0001 (0.000)	0.0001 (0.000)
% Less than High School					-0.0001 (0.001)	-0.0001 (0.001)	-0.0002 (0.001)	-0.0002 (0.001)
% BA or higher					0.0002 (0.000)	0.0002 (0.000)	0.0002 (0.000)	0.0002 (0.000)
% lived in diff county					-0.0006 (0.001)	-0.0006 (0.001)	-0.0005 (0.001)	-0.0006 (0.001)
Population					-0.0001* (0.000)	-0.0001* (0.000)	-0.0001* (0.000)	-0.0001* (0.000)
% Women					-0.0005 (0.000)	-0.0005 (0.000)	-0.0005 (0.000)	-0.0006 (0.000)
% Noncitizens					0.0264 (0.043)	0.0268 (0.045)	0.0287 (0.046)	0.0288 (0.046)
County FE					yes	yes	yes	yes
const	-0.0153*** (0.002)	-0.0153*** (0.001)	-0.0155*** (0.002)	-0.0156*** (0.002)	0.0493 (0.029)	0.0478 (0.029)	0.0500 (0.029)	0.0504 (0.029)
R2 adj.	0.002	0.004	0.003	0.004	0.079	0.079	0.079	0.079

Note: MA observations for 350 municipalities in 14 counties. Change in registration is measured by percentage change in registered voters between fall 2010 and spring 2012. "DTA clients" indexes: "% below poverty" measures % of families below poverty line; "% SSI+PA" measures % of households receiving Supplemental Security Income or Public Assistance; "% SNAP" measures % of households receiving Supplemental Nutrition Assistance; "% SNAP (ind)" measures % of persons receiving Supplemental Nutrition Assistance. Other controls are county fixed effects. Standard errors are clustered at the county level. Statistical significance is denoted as *10%, **5%, and ***1% levels.

(iv) proportion of population below the federal poverty line. The exposure to mailings and outreach activities should be highly correlated with these measures. We provide empirical evidence on post-settlement registration rates that supports this assumption.

Our analysis uses two empirical approaches to identify the effect of settlement agreement on registration and voting outcomes. Under both strategies we consider three dependent variables of interest: (i) the change in voter registration rates, (ii) voter turnout, and (iii) the Democratic voteshare. First, we analyse the relative outcomes in 2008 and 2012, before and after the settlement, in Massachusetts municipalities based on their exposure to the settlement actions which we measure using a range of "DTA clients" measures. If the settlement actions were effective, we should see larger changes in registration and voting behavior in municipalities with higher proportions of "DTA clients" in 2012 compared to 2008. Second, we compare changes in registration and voting behavior in Massachusetts municipalities with changes in comparison states of Maine, New Hampshire and Vermont. These estimates are robust to specific shocks and differential trends in registration and voting behavior associated with socio-economic background.

3.1.1. Within massachusetts analysis

We evaluate changes in registration and voting behavior across Massachusetts municipalities. Appendix Tables A.1 and A.2 show that there is a sufficient variation in the utilized poverty indicators across municipalities which provides variation

Table 2

Within-MA estimates, effects of DTA actions on voter registration, N=700

	% below poverty			
	line	% SSI+PA	% SNAP	% SNAP (ind)
	(1)	(2)	(3)	(4)
2012*DTA clients	0.0080*** (0.002)	0.0106*** (0.002)	0.0088* (0.004)	0.0067 (0.005)
DTA clients	-0.0042* (0.002)	-0.0059*** (0.002)	-0.0087** (0.004)	-0.0055** (0.002)
2012	0.0186 (0.029)	0.0089 (0.030)	0.0178 (0.028)	0.0167 (0.026)
cons	0.0192 (0.028)	0.0229 (0.027)	0.0123 (0.032)	0.0142 (0.035)
R2 adj.	0.208	0.210	0.207	0.204

Note: MA observations for 350 municipalities in 14 counties, 2008 and 2012. "DTA clients" indexes: "% below poverty" measures % of families below poverty line; "% SSI+PA" measures % of households receiving Supplemental Security Income or Public Assistance; "% SNAP" measures % of households receiving Supplemental Nutrition Assistance; "% SNAP (ind)" measures % of persons receiving Supplemental Nutrition Assistance. Change in registration is measured by percentage change in % of registered voters between fall and spring in 2008 and 2012. Other controls are county fixed effects and socio-economic characteristics at the municipality level, including % of women, % of black, % of hispanics, % with BA or more, % with less than HS, % inter-county movers, population size, and iterations of these variables with 2012 indicator. Standard errors are clustered at the county level. Statistical significance is denoted as *10%, **5%, and ***1% levels.

in the exposure to treatment for the empirical estimations.¹⁶ Our identification strategy relies on the assumption that, if there were no settlement agreement, municipality levels of DTA reciprocity rates should not explain the differential changes in registration and voting behavior in 2012 compared to 2008. We provide evidence to support this assumption.

The first strategy is to compare the 2008 municipality level outcomes (pre DTA settlement/treatment) with the 2012 outcomes (post DTA settlement/treatment) in Massachusetts. This has the virtue of being a clean test of the treatment effect of a lowering of voter registration and information costs. A potential drawback, however, is that it does not account for other factors occurring across states, targeting specific socio-economic backgrounds, which could affect registration and turnout.

Our specifications control for a range of other socioeconomic factors at the municipality level that may affect voting behavior. The estimations control for average age, proportion of women, proportions of blacks and hispanics, proportion of individuals with low and high education (less than high school and college degree or higher), proportion of foreign-born population, proportion of inter-county movers (such move requires a new voter registration). The estimations also include county fixed effects to control for any ballot differences at the county level.

First, we compare the change in voter registration across Massachusetts municipalities between October 2010 and February 2012.¹⁷ This estimation allows to detect any pre-existing trends in changing registration rates by socio-economic background. For example, due to a given candidate campaign that targets a specific group of individuals to increase their registration. The settlement agreement and its consecutive actions took place starting July 2012 and it should not affect registration trends before July 2012, however other policies or specific candidates could affect registration rates prior to the settlement agreement. We do not detect such pre-existing trends. We estimate the following equation,

$$\Delta \text{register}_{mc}^{\text{Feb12, Oct10}} = \beta_0 + \beta_2 \text{reciprocity}_{mc} + X_{mc}\gamma + \epsilon_{mc}, \quad (1)$$

where $\Delta \text{register}_{mc}^{\text{Feb12, Oct10}}$ is the rate of change in registered voters between October 2010 and February 2012. The subscripts m and c denote municipality and county, respectively. The variable reciprocity_{mc} measures the exposure to treatment using each of the indicators for "DTA clients" levels in 2012. The vector X_{mc} includes demographic and socioeconomic characteristics of each municipality in 2012 as well as county fixed effects.

Table 1 reports the results. Estimations show no statistically significant relationship between each of the four reciprocity measures and change in voter registration between October 2010 and February 2012. The coefficients of reciprocity

¹⁶ Percent of public assistance recipients (% receiving SSI + % receiving Public Assistance) ranges between 0% and 21% in 2008 and between 0% and 24% in 2012. Below poverty line rates are in the 0%-35% range in 2008 and in 0%-38% range in 2012. Proportion of SNAP recipients is in 0%-30% range in 2008 and in 0%-33% range in 2012.

¹⁷ Registration rates in October 2010 and February 2012 are selected due to data availability.

measures are small and statistically not different from zero, suggesting that the pre-existing trends in registration rates are independent of reciprocity measures and therefore not correlated with the intensity of treatment.

The next step of our analysis is to evaluate the effects of settlement agreement on registration rates. Presidential elections took place in November 2008 and November 2012. We use municipality level voter registration data at the end of February and mid-October in 2008 and 2012 to evaluate whether the change in registration rate between spring and fall varies with the exposure to the treatment induced by settlement actions. We estimate the following equation,

$$\Delta \text{register}_{mct} = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciprocity}_{mct} + \beta_3 (Y2012_t \times \text{reciprocity}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (2)$$

where $\Delta \text{register}$ is the rate of change in voter registration between February and October for 2008 and 2012. The variable $Y2012$ takes the value 1 if year is 2012 and 0 if year is 2008. The vector X_{mct} includes demographic and socio-economic characteristics for each municipality in 2008 and 2012 as well as county fixed effects. Standard errors are clustered at the county level to account for correlation in error terms within counties over time. The parameter of interest is β_3 , the coefficient of interaction between the reciprocity index and 2012 indicator.

Table 2 reports estimation results of Eq. (2). We report β_0 , β_1 , β_2 and β_3 .¹⁸ We interpret the coefficients of the interaction between reciprocity measures and 2012 indicator as the effect of settlement agreement on outcomes of interest (changes in registration rates and voting behavior). The size of reciprocity measures in 2012 varies across municipalities and is interpreted as the intensity of policy and exposure to treatment. Higher levels of reciprocity (or, higher proportion of “DTA clients”) are negatively correlated with the change in registration rates between Spring and Fall. This is consistent with findings in the existing literature that documents negative correlation between the likelihood of voter registration and socio-economic background. However, there is a positive correlation between the reciprocity indicators in 2012 and the change in registration rates. For example, one standard deviation increase in the %SSI+PA is associated with a 1.1 percentage points increase in the registration rate.¹⁹ The coefficients of year 2012 indicator have large standard errors and are not statistically different from zero, suggesting no difference between 2008 and 2012 overall rates of growth in voter registration between Spring and Fall in Massachusetts. Assuming that there were no other policies that could differentially affect registration rates within Massachusetts (this assumption is supported by results reported in Table 1), the 2012 increase in registration growth in areas with higher proportions of “DTA clients” can be attributed to the treatment effect. Appendix Table A.3, Panel A, shows similar results in the estimation of Eq. (2) controlling for municipality fixed effects.

Having established that there was a positive effect of the settlement agreement on registration rates, we estimate its effect on voter turnout and Democrat voteshare. We estimate the following specifications.

$$\left(\frac{\text{turnout}_{mct}}{\text{population}_{mct}} \right) = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciprocity}_{mct} + \beta_3 (Y2012_t \times \text{reciprocity}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (3)$$

$$\left(\frac{\text{turnout}_{mct}}{\text{registered}_{mct}} \right) = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciprocity}_{mct} + \beta_3 (Y2012_t \times \text{reciprocity}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (4)$$

where $\left(\frac{\text{turnout}_{mct}}{\text{population}_{mct}} \right)$ is the proportion of voters out of the relevant age population and $\left(\frac{\text{turnout}_{mct}}{\text{registered}_{mct}} \right)$ is the proportion of voters out of the registered to vote. Right-hand-side variables are defined as in Eq. (2); all specifications include county fixed effects and standard errors are clustered at the county level.

Panel A in Table 3 presents the results for voter turnout. Columns (1)–(4) show estimation results for $\left(\frac{\text{turnout}_{mct}}{\text{registered}_{mct}} \right)$. Columns (5)–(8) show results for $\left(\frac{\text{turnout}_{mct}}{\text{population}_{mct}} \right)$. Municipalities with higher rates of “DTA clients” have lower election turnout rates, out of registered and out of population. However, the negative correlation between reciprocity and turnout/population is mitigated in 2012, β_3 is positive and statistically significant for all measures of reciprocity; we attribute this outcome to the policy effect. For example, a one standard deviation increase in the %SSI+PA is associated with a 2.4% increase in turnout out of population. Columns (1)–(4) show that there is no significant effect of the policy in 2012 on voter turnout conditional on registration. The findings suggest that the decision to vote of those who have registered due to the policy in 2012 is similar to those of similar socio-economics background who would have registered regardless of the policy. This result suggests that registration, and possibly information costs, is the main impediment to voting for the treated population.

Finally, to estimate the effect of settlement on Democrat voteshare we estimate the following specifications.

$$\left(\frac{\text{democrat}_{mct}}{\text{population}_{mct}} \right) = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciprocity}_{mct} + \beta_3 (Y2012_t \times \text{reciprocity}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (5)$$

¹⁸ Complete estimation outcomes are available in appendix tables B.1–B.6 in the Online Appendix.

¹⁹ Appendix Tables A.1 and A.2 report the size of one standard deviation for each reciprocity measure; for %SSI+PA in MA in 2008–2012 it is 3.6%. The size of population over 18 years old in MA in 2008–2012, also reported in Appendix Table A.1 and A.2, is 14472. This implies that an average municipality has received 521 letters.

Table 3

Within-MA estimates, effects of DTA actions on voter registration, N=700

	% below poverty line				% below poverty line			
	% SSI+PA	% SNAP	% SNAP	(ind)	% SSI+PA	% SNAP	% SNAP	(ind)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
	turnout/registration				turnout/population			
2012*DTA clients	0.0061 (0.804)	0.0519 (0.959)	0.4506 (0.714)	0.3986 (0.787)	1.6311* (0.816)	2.4153** (1.019)	2.5630* (1.330)	1.0622 (0.856)
DTA clients	-1.3515* (0.670)	-1.5793** (0.604)	-2.9798*** (0.751)	-3.6962*** (0.845)	-1.8707* (0.962)	-2.3964** (0.970)	-3.7047** (1.441)	-3.4085** (1.509)
2012	-8.2665 (6.412)	-6.4817 (5.908)	-6.8510 (5.616)	-6.5004 (7.474)	0.6347 (12.956)	0.0817 (12.167)	2.4038 (12.371)	1.0292 (12.131)
cons	79.993*** (6.009)	80.463*** (5.973)	76.499*** (6.368)	71.8255*** (7.350)	59.726*** (8.471)	60.943*** (8.584)	56.049*** (8.376)	61.3347*** (6.302)
R2 adj.	0.688	0.689	0.696	0.646	0.734	0.734	0.735	0.736
Panel B								
	democrat votes/registration				democrat votes/population			
2012*DTA clients	0.3414 (0.336)	-0.2643 (0.780)	0.8174 (0.579)	1.2089* (0.620)	1.5606** (0.703)	1.5183 (1.203)	2.5284* (1.234)	2.0608* (1.045)
DTA clients	0.1356 (1.163)	1.8687 (1.082)	0.8061 (1.210)	0.3631 (1.373)	-0.4426 (1.596)	1.0150 (1.143)	0.0279 (1.761)	-0.0133 (1.567)
2012	-8.2392 (9.613)	-9.7946 (7.988)	-7.5046 (8.980)	-6.9041 (8.742)	-1.6234 (15.404)	-4.6750 (12.897)	-0.2670 (14.419)	-0.9068 (14.297)
cons	14.2373* (7.734)	15.7610** (7.293)	15.4097* (7.815)	14.9582* (8.161)	11.6885 (9.808)	13.8090 (9.637)	12.5767 (10.314)	12.5882 (10.677)
R2 adj.	0.742	0.746	0.744	0.743	0.709	0.712	0.711	0.710

Note: MA observations for 350 municipalities in 14 counties, 2008 and 2012. "DTA clients" indexes: "% below poverty" measures % of families below poverty line; "% SSI+PA" measures % of households receiving Supplemental Security Income or Public Assistance; "% SNAP" measures % of households receiving Supplemental Nutrition Assistance; "% SNAP (ind)" measures % of persons receiving Supplemental Nutrition Assistance. Change in registration is measured by percentage change in % of registered voters between fall and spring in 2008 and 2012. Other controls are county fixed effects and socio-economic characteristics at the municipality level, including % of women, % of black, % of hispanics, % with BA or more, % with less than HS, % inter-county movers, population size, and iterations of these variables with 2012 indicator. Standard errors are clustered at the county level. Statistical significance is denoted as *10%, **5%, and ***1% levels.

$$\left(\frac{\text{democrat}_{mct}}{\text{registered}_{mct}} \right) = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciency}_{mct} + \beta_3 (Y2012_t \times \text{reciency}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (6)$$

where $\left(\frac{\text{democrat}_{mct}}{\text{population}_{mct}} \right)$ is the proportion of Democrat votes out of the voting age population and $\left(\frac{\text{democrat}_{mct}}{\text{registered}_{mct}} \right)$ is the proportion of Democrat votes out of the registered to vote. Right-hand-side variables are defined as in Eq. (2); all specifications include county fixed effects and standard errors are clustered at the county level.

Panel B in Table 3 highlights our main findings from these specifications. Columns (1)–(4) show estimation results for $\left(\frac{\text{democrat}_{mct}}{\text{population}_{mct}} \right)$, Columns (5)–(8) show results for $\left(\frac{\text{democrat}_{mct}}{\text{registered}_{mct}} \right)$. We do not find any statistically significant relationships between reciprocity indicators and political preferences, β_2 coefficients are not statically significant in any specification. In Columns (1)–(4), the estimates of β_3 , that measures the effect of policy on democrat voteshare out of registered, are not statistically significant. This result suggests that the political preferences of those who have registered due to the policy in 2012 are similar to those of voters from similar socio-economic background who would have registered regardless. On the other hand, in Columns (5)–(8), there are positive and statistically significant relationships between the reciprocity measures in 2012 and the Democrat voteshare out of population, summarized by the β_3 coefficients in Eq. (5). For example, one standard deviation increase in the %SNAP+PA is associated with a 1.5% increase in the democrat voteshare out of population. The

results in Columns (5)–(8) suggest that the majority of those who registered due to the policy and voted have a preference for the Democratic candidate.²⁰

Appendix Table A.4, Panel A, shows similar results in the estimation of Eqs. (3)–(6) controlling for municipality fixed effects.

3.1.2. Cross-state analysis

To control for additional sources of variation in registration, turnout and voteshare outcomes, we expand our analysis to include the three surrounding states of Vermont, New Hampshire and Maine. The comparison of Massachusetts and surrounding states outcomes controls for contemporaneous changes in registration, turnout and democrat voteshare across municipalities with high reciprocity rates. If the settlement agreement affected registration and election outcomes, we should observe these effects only in Massachusetts 2012.

We find that the within-Massachusetts and across-state empirical strategies produce similar results in terms of the effect of the treatment on: voter registration, voter turnout, and Democrat voteshare.

For the change in registration outcome variable we estimate the following equation,

$$\begin{aligned} \Delta \text{register}_{mcs t} = & \alpha_0 + \alpha_1 MA_s + \alpha_2 Y2012_t + \alpha_3 \text{reciprocity}_{mcs t} + \alpha_4 MA_s \times \text{reciprocity}_{mcs t} \\ & + \alpha_5 Y2012_t \times MA_s + \alpha_6 Y2012_t \times \text{reciprocity}_{mcs t} + \alpha_7 MA_s \times Y2012_t \times \text{reciprocity}_{mcs t} \\ & + \gamma X_{mcs t} + \eta X_{mcs t} \times MA_s + \lambda X_{mcs t} \times Y2012_t + \mu X_{mcs t} \times Y2012_t \times MA_s + \nu_{mcs t}, \end{aligned} \quad (7)$$

where the notation is essentially the same as in the previous specifications, other than the necessary state subscripts, s . The indicator MA_s takes the value of 1 for Massachusetts municipalities and zero for control states. We estimate similar specifications to examine effects on turnout and Democrat voteshare. In those specifications the left-hand-side variables are $\left(\frac{\text{turnout}_{mcs t}}{\text{population}_{mcs t}}\right)$, $\left(\frac{\text{turnout}_{mcs t}}{\text{registered}_{mcs t}}\right)$, $\left(\frac{\text{democrat}_{mcs t}}{\text{population}_{mcs t}}\right)$ and $\left(\frac{\text{democrat}_{mcs t}}{\text{registered}_{mcs t}}\right)$, defined as in Eqs. (3)–(6). All specifications include county fixed effects and standard errors are clustered at the county level. Our main focus is the α_7 coefficient, where $MA_s \times Y2012_t \times \text{reciprocity}_{mcs t}$ measures the intensity of the treatment (strictly positive in most municipalities of Massachusetts in 2012 and zero in the control states).

Table 4 reports results for change in registration rates between Spring and Fall. The estimates of α_7 are positive and statistically significant. This suggests a statistically and economically significant positive relationship between the change in registration rate in Massachusetts in 2012 and treatment intensity. For example, one standard deviation increase in the %SSI+PA recipients is associated with a 1.9% increase in change in registration rate between Spring and Fall in Massachusetts 2012. Appendix Table A.3, Panel B, shows similar results in the estimation of Eq. (2) controlling for municipality fixed effects. To further examine the robustness of the findings, we estimate Eq. (2) for each comparison state. Appendix Table A.5 reports these results and shows no significant change in registration rates in the comparison states, most coefficients of DTA reciprocity rates in 2012 are negative or very small and not statistically significant.

Panel A in Table 5 presents results for the turnout outcomes. Columns (1)–(4) show estimation results for $\left(\frac{\text{turnout}_{mcs t}}{\text{registered}_{mcs t}}\right)$. Columns (5)–(8) show results for $\left(\frac{\text{turnout}_{mcs t}}{\text{population}_{mcs t}}\right)$. Similar to the results that use only Massachusetts municipalities, our findings suggest that the settlement agreement had a large effect on turnout out of population, α_7 is positive and statistically significant in Columns (5)–(8). For example, a one standard deviation increase in the %SSI+PA is associated with a 4.0% increase in turnout out of population. Turnout out of registration does not appear to be affected by the policy, α_7 coefficients in Columns (1)–(4) are not statistically significant. Appendix Table A.4, Panel B, shows positive and significant α_7 coefficients in estimations that control for municipality fixed effects.

Appendix Tables A.6 shows turnout results for each of the control states. Panel A reports results for turnout conditional on registration, $\left(\frac{\text{turnout}_{mcs t}}{\text{registered}_{mcs t}}\right)$, and Panel B reports results for turnout out of population, $\left(\frac{\text{turnout}_{mcs t}}{\text{population}_{mcs t}}\right)$. Reciprocity rates are negatively associated with turnout, however, in the control states, we do not find a trend in turnout in municipalities with higher representation of “DTA clients” between 2008 and 2012.

Panel B in Table 5 reports results for the Democrat voteshare outcomes. Columns (1)–(4) show estimation results for $\left(\frac{\text{democrat}_{mcs t}}{\text{registered}_{mcs t}}\right)$, Columns (5)–(8) show results for $\left(\frac{\text{democrat}_{mcs t}}{\text{population}_{mcs t}}\right)$. These results are also similar to the results that use only Massachusetts municipalities. Reciprocity indicators that measure the intensity of the treatment are positively correlated with the Democrat voteshare, and the effects are more pronounced when considering the proportion of Democrat votes out of population. For example, a one standard deviation increase in the %SSI+PA or %SNAP is associated with a 2.2% and 3.3% increase in the Democrat voteshare out of population. We also document some increase in Democrat voteshare out of registered in 2012 in Massachusetts, but these increases are less pronounced. For example, a one standard deviation increase in the %SNAP is associated with a 1.3% increase in Democrat voteshare out of registered. Appendix Table A.4, Panel B, shows similar results in the estimation of cross-state specifications controlling for municipality fixed effects.

Appendix Table A.7 reports results for the Democrat voteshare out of population and registration, for each of the control states. Municipalities with higher concentration of DTA recipients in comparison states in 2012 do not exhibit important changes in political preferences in 2012 compared to 2008. We observe declines in the Democrat votes out of population in

²⁰ It should be noted that the positive relationship between democrat voteshare out of population and treatment intensity is potentially Massachusetts specific. Similar policies in other states could result in different voteshare outcomes.

Table 4
Cross-state estimates, effects of DTA actions on voter registration, N=2558

	% below poverty line (1)	% SSI+PA (2)	% SNAP (3)	% SNAP (ind) (4)
2012 * MA* DTA clients	0.0073** (0.003)	0.0188*** (0.004)	0.0165*** (0.006)	0.0147** (0.007)
DTA clients	-0.0048** (0.002)	0.0031 (0.003)	-0.0069 (0.005)	-0.0043 (0.004)
2012*DTA clients	0.0007 (0.003)	-0.0082** (0.003)	-0.0074 (0.005)	-0.0079 (0.005)
MA*DTA clients	0.0003 (0.003)	-0.0094*** (0.003)	-0.0033 (0.006)	-0.0032 (0.005)
2012	-0.0726 (0.046)	-0.0666 (0.049)	-0.0556 (0.049)	-0.0585 (0.049)
MA	-0.1030** (0.050)	-0.0975* (0.052)	-0.1060* (0.056)	-0.1070* (0.056)
MA*2012	0.0917 (0.058)	0.0766 (0.062)	0.0748 (0.060)	0.0774 (0.059)
R2 adj.	0.118	0.117	0.125	0.121

Note: Observations for 1279 municipalities in 54 counties in MA, VT, NH and MN, in 2008 and 2012. "DTA clients" indexes: "% below poverty" measures % of families below poverty line; "% SSI+PA" measures % of households receiving Supplemental Security Income or Public Assistance; "% SNAP" measures % of households receiving Supplemental Nutrition Assistance; "% SNAP (ind)" measures % of persons receiving Supplemental Nutrition Assistance. Change in registration is measured by percentage change in % of registered voters between fall and spring in 2008 and 2012. Other controls are county fixed effects and socio-economic characteristics at the municipality level, including % of women, % of black, % of hispanics, % with BA or more, % with less than HS, % inter-county movers, population size, and iterations of these variables with 2012 indicator. Standard errors are clustered at the county level. Statistical significance is denoted as *10%, **5%, and ***1% levels.

2012 in some of the comparison states, some of which are statistically significant at the 10% level. This result is in contrast to the statistically significant increase in Democrat voteshare in the more intensively treated municipalities of Massachusetts in 2012 compared to 2008. Democrat votes out of registered do not show any pronounced trend in control states in 2012 compared to 2008.

To further assess whether the results are not driven by a candidate campaign that targets the more disadvantaged communities to increase their registration and turnout, we perform estimations that distinguish between poverty rates and "DTA clients" rates. We use the municipality level data and exploit the variation in treatment intensity using, (i) proportion of households receiving SSI or PA, (ii) proportion of households receiving SNAP; in estimations that control for poverty rates differences across time and across states. These estimations control for the possibility that candidates were targeting specific communities differentiated by levels of poverty, additionally to race, age, education, gender distributions, proportion of county movers, and proportion of foreign-born population (for which we control in all specifications), during their campaigns in 2008 or 2012. Appendix Table A.8 reports results of the within-Massachusetts specification, Columns (5)–(8) also control for municipality fixed effects. We find the coefficients of %SSI or %SNAP in 2012 to be positive in regressions of the change in voter registration, turnout out of population and democrat voteshare out of population, but not always tightly estimated. Appendix Table A.9 reports the results for the cross-state specification, Columns (5)–(8) also control for municipality fixed effects. (The empirical specification is similar to that in Eq. (7) with additional controls for poverty rates and its interactions with indicators for MA and 2012.) These results are similar to the findings reported in Tables 4 and 5, we find statistically significant relationships between registration and voting outcomes variables and the treatment intensity measured by the fraction of DTA recipients at the municipality level.

To further explore the robustness of our findings we collect data on polling stations, assuming that changing access to polling stations can affect the cost of voting and change voting behavior. Systematic change in the presence of polling stations may suggest that alternative policies were implemented to influence turnout and election outcomes.²¹ We use the

²¹ For example, Stein and Vonnahme (2008) document a positive correlation between the presence of more polling stations and voter turnout in the United States.

Table 5

Cross-state estimates, effects of DTA actions on elections turnout, N=2558

	% below poverty (1)	% SSI+PA (2)	% SNAP (3)	% SNAP (ind) (4)	% below poverty (5)	% SSI+PA (6)	% SNAP (7)	% SNAP (ind) (8)
Panel A	turnout/registration				turnout/population			
2012*MA*DTA clients	-0.2135 (0.804)	0.2885 (0.971)	1.2433 (0.785)	0.3766 (1.047)	1.0987 (0.946)	4.0361*** (1.021)	3.8943** (1.493)	0.3457 (1.199)
DTA clients	-0.1195 (0.259)	-0.8879*** (0.307)	-1.1462*** (0.408)	-1.5005*** (0.480)	-0.9713 (1.011)	-0.9361 (0.578)	-1.8899** (0.860)	-3.3970*** (1.066)
2012*DTA clients	0.2179 (0.374)	-0.2683 (0.369)	-0.8430 (0.553)	-0.5367 (0.530)	0.5342 (0.558)	-1.5856*** (0.500)	-1.2555* (0.644)	0.7347 (0.886)
MA*DTA clients	-1.1111 (0.672)	-0.5594 (0.610)	-1.5390* (0.835)	-1.2537 (0.985)	-1.0276 (1.348)	-1.6072 (1.048)	-2.2587 (1.440)	-0.4799 (1.553)
2012	-9.1855* (5.389)	-9.6641* (5.205)	-8.3918 (5.126)	-8.9046* (5.240)	-11.7249 (13.499)	-12.2959 (13.414)	-9.9361 (13.862)	-11.4151 (13.839)
MA	17.6164* (8.893)	18.1127** (8.891)	15.3072* (9.056)	1.6446 (7.952)	8.3286 (15.118)	8.0997 (15.669)	3.8589 (15.557)	1.9012 (14.637)
MA*2012	0.7378 (8.253)	2.8467 (7.534)	1.2642 (7.421)	2.4426 (7.386)	12.5517 (16.379)	12.7511 (15.540)	12.7565 (16.652)	12.8933 (15.947)
R2 adj.	0.377	0.382	0.388	0.391	0.419	0.425	0.428	0.431
Panel B	democrat votes/registration				democrat votes/population			
2012*MA*DTA clients	0.7330* (0.421)	-0.1723 (0.885)	1.2969* (0.655)	1.6385** (0.699)	1.6813** (0.819)	2.2370* (1.181)	3.2640** (1.246)	1.9964* (1.142)
DTA clients	0.5802 (0.385)	-0.0378 (0.338)	0.2632 (0.634)	-0.3286 (0.530)	0.2927 (0.880)	-0.0603 (0.452)	-0.0466 (0.668)	-1.1720* (0.634)
2012*DTA clients	-0.3981 (0.281)	-0.2164 (0.297)	-0.6960* (0.376)	-0.4826 (0.338)	-0.1250 (0.325)	-0.8006** (0.338)	-0.8737** (0.434)	0.0311 (0.475)
MA*DTA clients	0.0232 (1.352)	2.4251* (1.210)	1.8112 (1.644)	2.0618 (1.697)	-0.4265 (1.896)	1.4167 (1.268)	0.8838 (1.976)	2.0186 (1.809)
2012	0.5513 (4.240)	0.8054 (4.511)	1.0876 (4.274)	1.2312 (4.348)	2.0684 (5.227)	2.1431 (5.523)	2.8918 (5.354)	2.6528 (5.473)
MA	-4.5794 (12.582)	-4.5233 (12.139)	-2.6981 (12.026)	-2.5527 (11.902)	-6.9108 (12.533)	-6.8561 (12.492)	-6.2915 (12.795)	-6.1615 (12.911)
MA*2012	-9.4916 (10.805)	-11.9182 (9.344)	-9.7824 (10.081)	-9.4484 (9.921)	-4.1546 (14.924)	-7.6862 (12.622)	-3.9183 (14.090)	-4.3837 (13.769)
R2 adj.	0.553	0.552	0.552	0.553	0.504	0.505	0.504	0.506

Note: Observations for 1279 municipalities in 54 counties in MA, VT, NH and MN, in 2008 and 2012. "DTA clients" indexes "% below poverty" measures % of families below poverty line; "% SSI+PA" measures % of households receiving Supplemental Security Income or Public Assistance; "% SNAP" measures % of households receiving Supplemental Nutrition Assistance; "% SNAP (ind)" measures % of persons receiving Supplemental Nutrition Assistance. Change in registration is measured by percentage change in % of registered voters between fall and spring in 2008 and 2012. Other controls are county fixed effects and socio-economic characteristics at the municipality level, including % of women, % of black, % of hispanics, % with BA or more, % with less than HS, % inter-county movers, population size, and iterations of these variables with 2012 indicator. Standard errors are clustered at the county level. Statistical significance is denoted as *10%, **5%, and ***1% levels.

following specifications to evaluate whether the changing access to polling stations could drive our findings,

$$\text{Polling Stations}_{mct} = \beta_0 + \beta_1 Y2012_t + \beta_2 \text{reciency}_{mct} + \beta_3 (Y2012_t \times \text{reciency}_{mct}) + X_{mct} \gamma + (X_{mct} \times Y2012_t) \delta + \epsilon_{mct}, \quad (8)$$

$$\text{Polling Stations}_{mct} = \alpha_0 + \alpha_1 MA_s + \alpha_2 Y2012_t + \alpha_3 \text{reciency}_{mct} + \alpha_4 MA_s \times \text{reciency}_{mct} + \alpha_5 Y2012_t \times MA_s + \alpha_6 Y2012_t \times \text{reciency}_{mct} + \alpha_7 MA_s \times Y2012_t \times \text{reciency}_{mct} + \gamma X_{mct} + \eta X_{mct} \times MA_s + \lambda X_{mct} \times Y2012_t + \mu X_{mct} \times Y2012_t \times MA_s + \nu_{mct}, \quad (9)$$

where $Polling\ Stations_{mct}$ is the number of polling stations at the municipality level.²² Appendix Tables A.10 and A.11 report estimation results, Columns (5)–(8) in each table also control for municipality fixed effects. The results show no systematic relationship between the treatment intensity and the presence of polling stations, i.e., β_3 in Eq. (8) and α_7 in Eq. (9) are not statistically significant.

4. Concluding remarks

Our empirical results point to the importance of voter registration and information costs for electoral participation—especially for lower socioeconomic-status citizens.

The main contribution of our paper is to obtain plausibly causal inferences about the impact of lowering registration and information costs. Precisely because such costs are amenable to policy interventions to lower them, and because of the importance of voting, our results highlight a significant role for potential policy interventions. In particular, our paper speaks to the effectiveness of large-scale and low-cost interventions that could potentially extend to other forms of mass communication in the current digital era.

More broadly, our results have implications for the effectiveness of voter registration drives targeted at low-income voters.

Our findings also suggest that field experiments on registration rather than turnout may be particularly valuable in providing confirmatory evidence and also broader findings than are possible from a given natural experiment. As we mentioned earlier, Nickerson (2015) and Braconnier et al. (2017) are notable, but somewhat lonely examples of voter registration field experiments. Those experiments also have certain limitations—particularly concerning selection into the treatment group—that we are able to avoid using our natural-experiment approach.

A final, but less upbeat observation is that there are policies which raise, rather than lower, registration costs. Voter identification laws, for example, can most usefully be seen as an increase the registration costs. Voters need to obtain not only a valid registration, but also a driver's license or other government-issued ID. The courts, including the United States Supreme Court, are in the midst of assessing the constitutionality of such voter ID laws. The empirical evidence assembled in this paper suggests that, whatever else they do, such laws materially reduce actual voting, and that there is a potential partisan skew to this reduction.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:[10.1016/j.jebo.2020.04.017](https://doi.org/10.1016/j.jebo.2020.04.017)

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²² The data on polling stations at municipality level are from Election Administration and Voting Surveys, The U.S. Election Assistance Commission.

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