

The Introduction of Voter Registration and Its Effect on Turnout

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Studies of voter turnout across states find that those with more facilitative registration laws have higher turnout rates. Eliminating registration barriers altogether is estimated to raise voter participation rates by up to 10%. This article presents panel estimates of the effects of introducing registration that exploits changes in registration laws and turnout *within* states. New York and Ohio imposed registration requirements on all of their counties in 1965 and 1977, respectively. We find that the introduction of registration to counties that did not previously require registration decreased participation over the long term by three to five percentage points. Though significant, this is lower than estimates of the effects of registration from cross-sectional studies and suggests that expectations about the effects of registration reforms on turnout may be overstated.

1 Introduction

An extensive scholarly literature identifies registration as a substantial barrier to voting. Cross-sectional comparisons of states, the staple method in this literature, suggest that turnout is approximately 7–10 percentage points higher in states with lower registration hurdles. The logic is straightforward. People weigh the benefits of voting against the costs, and the higher the costs the lower the participation rate. For some people, voting is not a high priority, and small obstacles like registration are sufficient to keep them from participating. Others do not think to register until the final weeks of campaigning pique their interest in the election, long after registration deadlines have passed. Still others report that, although registered, they could not vote because of errors in the registration system.¹ This article demonstrates that common cross-sectional empirical estimates of the relationship between

Authors' note: The data we consider in this paper are available from the MIT Department of Political Science's website at: <http://web.mit.edu/polisci/research/representation.html>.

¹The Current Population Survey asks registered U.S. voters who did not vote why they did not vote. Approximately 8% state that there was a problem with their registration when they tried to vote or to obtain an absentee ballot (Caltech/MIT Voting Technology Project 2001).

registration laws and turnout overestimate the effects by a factor of two or three. Using panel methods, we find these effects to be on the order of three to five percentage points.

Easing registration requirements would seem an obvious remedy for low voter participation rates. In the United States, Congress and state governments have adopted several reforms over the past few decades for just this reason. At the federal level, Congress enacted the National Voter Registration Act (NVRA) in 1993 to ease the registration process and the Help America Vote Act in 2002 to correct problems in the registration system. Many states have also acted to liberalize their registration requirements. For example, there has been a long-term trend toward shortening registration closing dates. Several states now allow eligible voters to register through the mail. Perhaps the most popular, and far-reaching, reform is election day registration. In the mid-1970s, five states—Maine, Minnesota, Ohio, Oregon, and Wisconsin—adopted election day registration, although voters in Ohio (by constitutional amendment in 1977) and Oregon (by ballot initiative in 1975) repealed it soon after. Three other states—Idaho, New Hampshire, and Wyoming—adopted same-day registration in time for the 1994 election (to avoid having to implement the NVRA), and at least two other states—California and Colorado—considered but failed to adopt election day registration on ballot initiatives in 2002.

Empirical research has exploited differences and changes in registration laws to measure the magnitude of the effect of registration on the size of the electorate. One stream of research examines aggregate turnout rates for states or localities with different registration requirements (Kelley et al. 1967; Gilliam 1985; Rhine 1996). A second approach uses survey data to estimate the factors that predict voting, including measures of the stringency of state registration requirements. The survey data are then used to simulate the effects of a uniform liberalization of voter registration requirements to estimate the percentage of voters who might have voted had registration laws been more permissive (Rosenstone and Wolfinger 1978; Rosenstone and Hansen 1993; Mitchell and Wlezien 1995; Alvarez et al. 2002).

Nearly all of this research is based on differences in registration laws across states, which raises a couple of potentially important estimation problems. First, cross-sectional studies may suffer from an omitted variables problem. These studies generally do not control for variation in other state election laws that may also influence turnout, such as those regulating absentee ballots, as well as the simultaneous effects of other reforms. For example, the adoption of same-day registration in Minnesota in 1972 coincided with the state's shift from nonpartisan to partisan legislative elections, so teasing out the immediate effects of registration reform is difficult. Additionally, cross-sectional analyses do not control for unobserved state-level characteristics that also may affect voter participation rates. Specifically, states that have more liberal registration laws may also have a different sort of electorate—that is, one more inclined to vote. After all, that electorate or their representatives approved a more “civic-minded” or participatory way to run elections. To the extent that studies do not capture this difficult-to-measure concept, the registration law itself might. These state-level effects—observable and unobservable—are nearly impossible to hold constant in cross-state data analysis because there are only 50 states and a potentially large number of factors.

A second concern about cross-sectional studies (and panel analyses that rely on cross-sectional variation only) is their lack of statistical power. For example, studies comparing aggregate turnout rates using state-level differences in major registration laws such as election day registration (e.g., Knack 2001) give extraordinary explanatory weight to a small number of observations. In effect, the results of these studies depend on data from only a handful of states, often from just one or two elections. Survey analyses relying on the same source of variation share this problem. Without more observations, these studies

cannot easily rule out false positive results—that is, that there is actually no effect of registration on turnout despite evidence seemingly to the contrary. While several studies address the omitted variables problem by employing fixed effects in their regression analyses (Knack 1995; Rhine 1995; Brians and Grofman 2001) or through quasi-experimental research designs (Fenster 1994; Knack and White 2000; Knack 2001), these studies do not address this second problem.

In this article, we estimate the effect of registration within states by studying both the short-term and long-term differences in voter turnout in New York and Ohio, states that did not introduce statewide registration requirements until 1965 and 1977, respectively. Prior to these reforms, registration requirements in these states varied at the county level. The New York and Ohio experiences are interesting to study because the introduction of voter registration represents a big change in registration law (more so than, say, a move from a registration close date of 30 to 15 days before an election), and the circumstances in these states enable a direct comparison of cross-sectional and panel estimates. Moreover, studying the effect of registration on turnout below the state level enables us to address the omitted variables problem since, by the design of the study, we can control for the effects of state-level influences on turnout other than registration.² Finally, since we have data for New York and Ohio counties over a long time period—1954 to 2000—we have sufficient statistical power to rule out false positive results. In sum, these data provide an opportunity to sharpen estimates of the effect of registration on voter turnout.

Finally, our analysis provides for the direct comparison of methods, since we use both cross-sectional and panel tests. Our cross-sectional results suggest that registration has up to a 10 to 13 percentage point effect on voter turnout. However, our panel estimates—which substantially reduce confounds—indicate a long-term decline in turnout of just three to five percentage points in New York and Ohio counties. This suggests that the existing empirical literature that relies on cross-sectional research designs likely overestimates the impact of registration on turnout.

The article proceeds as follows. In the next section, we describe the New York and Ohio data and summarize our methodology. In the subsequent section, we present our estimates of the effects of registration on voter turnout. In the final section, we discuss the implications of our results.

2 Data and Methods

Voter registration evolved at the county and municipal level in the United States. While its development traces to the early nineteenth century, voter registration was not a universal requirement in the American states well into the twentieth century. To this day, the state of North Dakota still does not require voter registration, and the state of Wisconsin exempts its smallest towns from this requirement. Until the 1960s and 1970s, eight states—Iowa, Kansas, Minnesota, Missouri, Nebraska, New York, Ohio, and Wisconsin—used a “dual system” of voter registration: they imposed voter registration on some municipalities and counties but not on others. As a result, voters in about half of the counties in these states had to register in order to have access to the polls, while voters in the other half just showed up on election day.

In this article, we exploit the variation in registration rules across counties in states with dual registration systems. Three features of county-level registration rules are relevant. First, there is considerable variation in the use of registration across counties within these

²More specifically, we assume that these other state-level factors affect each state’s electorate uniformly.

states. All eight states in question allowed the counties the local option to use voter registration. Their state laws imposed voter registration on the largest counties and municipalities and allowed smaller counties to choose to run elections with registration or without. Second, state legislatures (exogenously) imposed registration on some counties. The first state registration laws at the beginning of the twentieth century in many states required registration in the largest cities. Third, in the 1960s and 1970s, the states—either through legislative action or initiatives—imposed registration on the counties that did not yet have it. As a result, many counties were forced to switch from no registration to some registration. We measure the change in turnout in these counties relative to the baseline case of counties that used registration before the state law created uniform registration. The change in voter turnout in the counties that were forced by state law to adopt registration is of particular interest. Did turnout drop in these counties? Did it fall faster than turnout changed in other counties in the states?

We focus on two states, New York and Ohio. The historical record in these two states permits us to identify which counties used voter registration and which counties did not. Certification of the vote in both states includes statements of registered voters by county in New York and by town in Ohio. This allows us to identify where registration existed and what fraction voted, as well as to measure the demographics of the populations affected by the counties' registration procedures.

The state laws in New York and Ohio specified which counties and towns had to use voter registration. In 1900, New York City became the first jurisdiction in the state to require personal voter registration. In 1920, the state enacted legislation that required voters living in cities or villages with populations of at least 5000 people to register annually. This law remained authoritative until 1965,³ when the state passed legislation requiring statewide voter registration for all New York voters.⁴ In Ohio, the state legislature enacted its initial voter registration law in 1885, but it applied only to Cincinnati and Cleveland. Over the next half century, the state gradually expanded registration requirements to other major cities and other parts of the state (determined by population size). Beginning in 1949, the state granted county election officials the option of requiring voters to register, and a number of counties began to do so. This remained the case until 1977, when the state enacted a law requiring voter registration statewide.⁵

Our panel estimates of the effects of registration on turnout in New York and Ohio exploit the dates of the statewide laws requiring registration in all counties. The key legal change of interest occurs in 1965 in New York and in 1977 in Ohio. We expect that counties on which registration was imposed should experience a significant drop in turnout relative to counties that already used registration. In addition, our analyses can exploit variation across counties, controlling for other relevant factors such as population, income, age, and education.

Unfortunately, the other six states in question—Iowa, Kansas, Minnesota, Missouri, Nebraska, and Wisconsin—did not keep sufficient records to allow us to identify the use of

³In 1954, the New York State Assembly enacted a law that provided counties with an option of setting up permanent personal registration systems. This legislation capped off 15 years of legislative effort to enact such a law, following an amendment to the state constitution approved by New York citizens in 1938, which authorized the state to design a permanent personal registration system. While this law change seems to have led to an increase in the number of citizens in New York required to register, only 20 counties required all of their voters to register in the 1966 general election, the last election before the statewide registration law took effect.

⁴We compiled this information from various editions of the *Manual for the Use of the Legislature of the State of New York*, from information provided by Nancy Horan at the New York State Library, from various editions of the *Laws of New York*, and from the *New York Times* historical archive.

⁵We compiled this information from various editions of *Ohio Election Statistics*, Martin (1998), and various editions of *Ohio Session Laws*.

voter registration before the state imposed a uniform requirement. We contacted state election officials, state research and law libraries, and other resources in the states in an effort to collect such information, but none of the states had compiled these data. We also reviewed past state statutory code, state legislature session laws, state publications, and the existing voter registration literature in an attempt to assemble a list of which counties and cities did and did not have registration requirements prior to the state making them mandatory. In every case, use of voter registration was left to the discretion of the local county office, and no state reports document where registration was used. On the whole, county and state election officers today seem unaware that their local election offices conducted elections without voter registration. One state election officer laughed in disbelief at the suggestion that elections could even be run without voter registration, let alone that this was the situation in some parts of her state as recently as the 1960s. As an aside, it is worth noting that academic studies of the effects of election day registration sometimes also err in this regard. Election day registration was a *more* restrictive system than existed in many Minnesota and Wisconsin towns—those towns had no registration system at all (Smolka 1977). Ultimately, it may be possible to reconstruct from records at each county courthouse the registration rules used, but such a task is beyond the scope of this article (and the resources of the authors).

New York and Ohio, fortunately, did compile information on the use of registration within the counties. The New York registration data are contained in past editions of a formerly annual publication, *The Manual for the Use of the Legislature of the State of New York*. From this volume, we recorded the number of election districts in each county that required personal registration and the number that did not. Registration data for Ohio come from the biannual publication *Ohio Election Statistics*. This publication contains county-by-county (and town-by-town) statistics on voter registration, including the number of precincts in each county that required personal registration. We recorded whether each county required, partially required (that is, required for only some towns in the county), or did not require personal registration in a given election year. For each state, we compiled these registration statistics for the years 1954 through the year that the state imposed mandatory registration—1965 in New York (effective 1967) and 1977 in Ohio (effective 1978).⁶

We construct the dependent variable, voter turnout, in the usual way by dividing the total number of votes in the highest office at stake in each election by the number of eligible voters. For presidential election years, we use total votes in the presidential race, while for nonpresidential years, we use total votes in senatorial elections, if available, and total votes in gubernatorial elections otherwise. For our vote data, we use county-level election returns compiled by Clubb et al. (1986) for the years 1954 to 1968. From the period beginning with the 1970 election, we use data from the *New York Red Book*, election returns provided by the New York State Board of Elections, and *Ohio Election Statistics*. To estimate the county-level voting-age population, we use data collected by the U.S. Census Bureau and reported in various editions of its *County and City Data Book*. While the Census Bureau regularly estimates voting-age population at the state level, it only does so at the county level for decennial census years.⁷ To estimate the number of eligible voters in each county in noncensus years, we use linear interpolation to impute the voting-age population. We also compiled county-level demographic data from various editions of the *County and City Data Book*.

⁶In the case of New York, there was an interim period from 1965 to 1966 in which counties had the option of requiring registration.

⁷The 1970s editions of the *County and City Data Book* do not provide an estimate of the voting-age population in 1970, but instead do for 1972.

It is important to emphasize the methodological advantage provided by these data. Across this time period, we can compare counties affected by the law change to those that were unaffected. Thus the counties with registration over the entire period provide a baseline, and our estimated effects of registration on turnout reflect changes in counties forced to require registration for the first time relative to those requiring registration throughout. Moreover, because we have county-level data, we are able to isolate the effects of the registration law change of interest from other state law changes (and other unobserved state-level phenomena). By studying within-state variation, we assume that any other factors besides registration influencing turnout, did so uniformly across all counties.⁸

Studying just two states does raise a question about the external validity of our results. *A priori*, we do not have reason to believe that New York and Ohio are exceptional. New York and Ohio are arguably less exceptional than the states with election day registration often studied to generate estimates of the effects of registration laws on turnout. In addition, the variability we exploit is at the county level, over time, and a wide range of counties are encompassed in the data examined. Finally, the point of the article is to compare cross-sectional and panel methods, and these data enable us to test the validity of widely cited cross-sectional results.

A further issue of validity concerns the potential uniqueness of the counties that did not use registration. Because of state laws, the counties required to use registration were, on the whole, more urban and populous than the counties allowed the option of no registration. We examine the data several ways to ensure that the effects of registration do not wholly reflect some confounding factor. The main insurance against confounds is the use of county fixed effects and regression of changes in laws on changes in turnout. Indeed, a subtle message of our analysis is that research that makes cross-sectional comparisons may suffer from such confounds. There are large differences in turnout between areas that use registration and those that do not. However, the within county drop in turnout with the imposition of registration, it turns out, is much smaller—just one-third the size of the cross-sectional effect. Population and urbanization percent are the most likely confounding factors, as those factors determine which counties must use registration. We control for county population and urbanization linearly, as well as other county-level demographic characteristics such as income, age, and education, which are the primary movers of voter turnout in cross-sectional studies (see, for instance, Rosenstone and Hansen 1993).

Population differences may still create bias if county population magnifies the effects of registration laws. For example, larger cities may have more registration problems than smaller communities. Such an interaction with population, however, has not been demonstrated in the existing literature. In addition, in the New York and Ohio data, we find no evidence of statistically significant interactions between registration laws and population in predicting turnout.

Table 1 compares the demographic characteristics of counties in states requiring personal registration with those that did not, using 1960 as a representative year.⁹ The most notable difference is in county population. This is not surprising, since both New York and Ohio election law based registration requirements on population criteria. The mean

⁸Numerous scholars have recognized differences in registration requirements within states, including Smolka (1977), Rosenstone and Wolfinger (1978), and Knack (2001), but few have explicitly studied them for this reason. Burnham (1974) examined this within-state variation in Missouri, New York, Ohio, and Pennsylvania for a period around the turn of the twentieth century. Two unpublished studies (Anderson 1983; Martin 1998) considered the Ohio case discussed in this paper, but they did not estimate the full panel model.

⁹1960 represents an approximate midway point in our dataset of the pre-statewide registration period for both New York and Ohio.

Table 1 Characteristics of counties with and without personal voter registration requirements in New York and Ohio, 1960

	<i>New York</i>		<i>Ohio</i>	
	<i>Registration</i>	<i>No registration</i>	<i>Registration</i>	<i>No registration</i>
Population	880,000	31,000	256,000	34,000
Median family income	\$7,200	\$5,200	\$6,200	\$5,200
% urban	87%	21%	62%	31%
% with h.s. education	45%	37%	41%	39%
% African American	7%	1%	6%	1%
Median age	32	31	28	29
Number of counties	15	6	28	47

population size of registration counties was nearly 30 and 8 times as large in New York and Ohio, respectively, as counties not requiring voters to register. The population differences in New York somewhat reflect the size of the counties comprising and immediately surrounding New York City. However, even omitting the five boroughs of New York City only marginally reduces the average population differences (they remain about 17 to 1). The same is true with Ohio. Excluding the largest counties in Ohio (all of which have had registration requirements since early in the twentieth century) also only marginally reduces the magnitude of the differences in the mean population between registration and nonregistration counties. The percent of the county populations that lived in urban areas closely parallels these differences in population size, as does the percent of the county population that was African-American, which coincides with the fact that urban areas generally had larger minority populations. Median family income and the percent of the population that had completed high school were somewhat larger in registration counties compared to nonregistration counties, with the differences of slightly more magnitude in New York than in Ohio. Finally, there were no meaningful differences in the median age of registration and nonregistration counties.

In terms of predictors of voter turnout, these differences suggest that turnout in counties requiring registration should be *higher* than in counties not requiring registration. That is, levels of education and income—socioeconomic characteristics established as positively predicting voter participation rates—are higher on average in registration counties. Thus, if anything, these county-level characteristics work against the presumed negative effect of registration on county-level turnout rates. However, as the analyses that follow demonstrate, county-level registration requirements did have an important effect on voter participation rates. To control for the potential confounds, we include these variables in the panel and regression analyses. Also, to test robustness of the results we omit the largest counties from the regressions. The results do not change.

We provide several different estimates of the effects of the introduction of registration on turnout. First, following much of the literature, we analyze the difference in turnout between counties that had registration and counties that did not. This serves as a base case with which we establish the result one would get from cross-sectional analysis. Second, we exploit the panel structure of the data. We take advantage of the pseudo-experimental circumstances provided by the state law changes by analyzing how the imposition of registration in 1965 in New York and 1977 in Ohio changed turnout in counties. We then introduce statistical controls by estimating a panel model with fixed effects for each county and year and a “differences-in-differences” model, each of

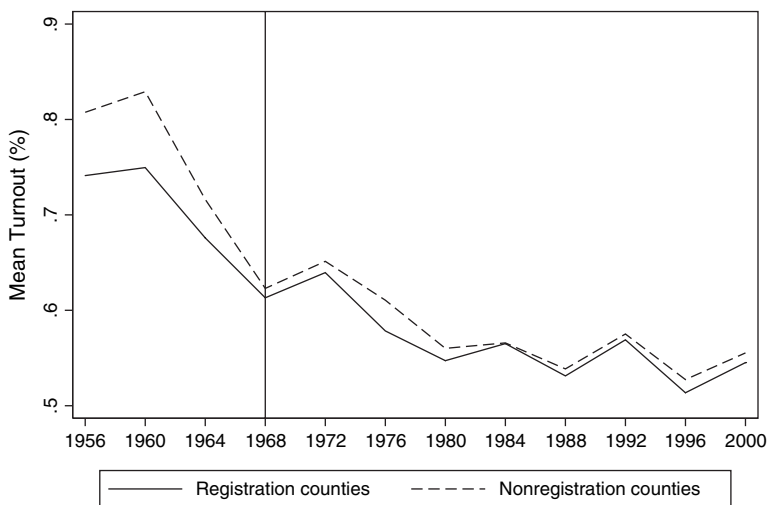


Fig. 1 New York: turnout in presidential elections.

which hold the county constant. We discuss these specifications in more detail in the next section.

3 The Effects of Registration on Turnout

Figures 1 and 2 present a first look at the county-level turnout data we compiled for New York and Ohio from 1956 to 2000. In each figure, we graph mean county-level turnout for registration counties (counties that required registration throughout the period) and nonregistration counties (counties that did not require registration in at least one election prior to the law change) in presidential year elections. The first feature of note is the overall decline in turnout in each state over the 50-year span. New York and Ohio reflect the well-known national pattern of declining voter turnout. Second, turnout rates in counties without registration are much higher on average than are turnout rates in counties with registration in the elections prior to the law change requiring statewide registration in each state (the year of the change in state law is indicated by the vertical line in each figure). Third, in the series of presidential elections after the law change, the difference in participation rates largely disappears. Voter turnout in previously nonregistration counties is generally the same as that in counties previously requiring registration, with the 1976 election in New York representing the only real deviation from this overall pattern.

In the analyses that follow, we more systematically examine these suggestive differences in voter turnout among counties with varying registration requirements. First, we discuss our estimates of the effects of the introduction of registration on turnout from our cross-sectional analysis, and then from our panel analyses.

3.1 Cross-Sectional Estimates

The cross-sectional analysis is presented in Table 2. Here we estimate the effect of registration on voter turnout by contrasting turnout in counties with registration and counties without registration from 1954 through the election prior to the change in state law.¹⁰

¹⁰We begin our analysis with 1954 because both the New York and Ohio state legislatures amended their election laws in this year, including sections pertaining to voter registration.

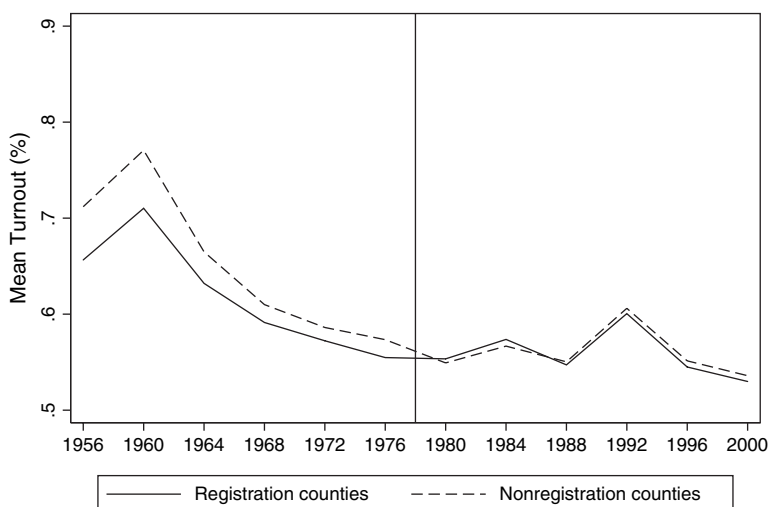


Fig. 2 Ohio: turnout in presidential elections.

The difference between these two types of counties is considerable.¹¹ In presidential-year elections, New York counties with registration had rates of turnout that were, on average, 13 percentage points lower than the New York counties without registration. In Ohio, the difference is approximately 10 percentage points. A difference of means test indicates that the differences are statistically significant. These estimates are on the high end of the range of estimates resulting from previous analyses. We find similar results for nonpresidential-year elections. Mean turnout in counties with registration was 10 and 5 percentage points lower than in nonregistration counties in New York and Ohio, respectively. A difference of means test indicates that these results are also statistically significant at conventional levels.

3.2 Panel Estimates

Our first set of panel estimates utilizes the natural experiment aspect of the data—that is, we study the turnout change shortly after the states' imposition of registration. Specifically, we compute the change in turnout from one election prior to the law change to one election after the law change for counties that did not have registration before it was imposed by the state governments. As a contrast or control group, we measure the change in turnout in counties that had registration throughout for the same set of elections. The estimated effect of the law is the change in turnout in the counties that previously had no registration minus the change in turnout in counties that had registration. As before, we make separate estimates for presidential and midterm elections.

As shown in Table 3, turnout did decline with the introduction of registration. In New York presidential elections, mean turnout in counties required to register their voters for the first time was seven percentage points lower than in the previous presidential election. The decline in turnout in the counties with registration was five percentage points, leaving a statistically significant net effect of two percentage points. In nonpresidential years, the effect is less than one percentage point, but it is not statistically distinguishable from zero.

¹¹A third type of county had partial registration (i.e., some, but not all, towns had registration). Predictably, mean turnout in partial registration counties rests between that of full and nonregistration counties.

Table 2 Cross-sectional results: Mean turnout across counties in period before statewide registration

	<i>New York</i> ^a	<i>Ohio</i>
<i>Presidential Years</i>	<i>1956–1964</i>	<i>1956–1976</i>
Full registration (NY=40; OH=245)	0.67 (0.014)	0.59 (0.004)
No registration (NY=16, OH=223)	0.80 (0.016)	0.69 (0.005)
<i>Difference of means t-test</i>	<i>Effect = 0.13*</i>	<i>Effect = 0.10*</i>
<i>Nonpresidential Years</i>	<i>1954–1966</i>	<i>1954–1974</i>
Full registration (NY=56; OH=221)	0.54 (0.012)	0.46 (0.004)
No registration (NY=20; OH=239)	0.64 (0.015)	0.51 (0.005)
<i>Difference of means t-test</i>	<i>Effect = 0.10*</i>	<i>Effect = 0.05*</i>

Note. * $p < .01$. Standard errors in parentheses. T-test is two-tailed.

^aExcludes Hamilton County and Putnam County for years 1954–1960 (see note 12).

In Ohio presidential elections, turnout declined 8% in counties without registration and grew 1% percent in counties with registration, for a net effect of nine percentage points. In nonpresidential elections in Ohio, turnout declined 11 percentage points in counties in which the state imposed registration but fell by four percentage points, on average, in counties that already had registration, with a net effect of seven percentage points.

We can introduce demographic controls and refine these estimates further by modeling the panel structure more explicitly. Two different sorts of panel estimates are possible. First, fixed effects regressions hold constant the unit of observation (i.e., a dummy variable for each county) by subtracting the unit-level average value of each variable. In essence, this estimator measures the effect of changing registration laws on long-term average turnout. Second, differences-in-differences regressions capture the immediate short-term effect of the policy innovation. The differences-in-differences model is a commonly employed approach for studying the effect of a major change in government policy and has been extensively used in the economics literature (Angrist and Krueger 1999). The differences-in-differences estimator models a treatment effect by estimating the difference between outcome measures at two points in time for both a treatment group (in this case, counties moving from no registration to registration) and a control group (in this case, counties requiring registration throughout the time period). The model then compares the differences between the groups to measure the effect of the treatment.

Our first regression model is a fixed effects specification that enables us to make use of both over-time and cross-sectional variation. The dependent variable is county-level turnout, and we consider elections from 1954 through 2000. Thus, for the 149 counties in our dataset (61 in New York and 88 in Ohio), we have 24 observations (i.e., turnout in the 24 elections from 1954 through 2000).¹² During this period, eight counties in New York and 52 counties in Ohio shifted from no registration to registration, and an additional 49 counties in New York and 14 counties in Ohio shifted from partial registration to full registration. Thus, in this model, we consider the long-term average difference in turnout between registration and nonregistration (or partial registration) counties.

¹²We exclude Hamilton County from the analysis because reported turnout is skewed by the fact that voting residence extended beyond the boundaries of residence defined by the U.S. Census Bureau, so data on voting-age population are too low (see *County and City Data Book 1977*, p. 336). We also drop observations for Putnam County for 1954–1960 due to the Census Bureau’s underestimation of the county’s voting-age population.

Table 3 Panel results: Comparison of mean turnout in election after law change to election before law change

	<i>New York</i> ^a	<i>Ohio</i>
<i>Presidential Years</i>	<i>1968 vs. 1964</i>	<i>1980 vs. 1976</i>
Counties impacted (NY=41; OH=26) ^b	−0.07 (0.003)	−0.08 (0.006)
Counties not impacted (NY=20; OH=62)	−0.05 (0.005)	0.01 (0.003)
<i>Difference of means t-test</i>	<i>Effect = −0.02*</i>	<i>Effect = −0.09*</i>
<i>Nonpresidential Years</i>	<i>1970 vs. 1966</i>	<i>1978 vs. 1974</i>
Counties impacted (NY=41; OH=26) ^b	−0.03 (0.003)	−0.11 (0.006)
Counties not impacted (NY=20; OH=62)	−0.03 (0.004)	−0.04 (0.003)
<i>Difference of means t-test</i>	<i>Effect = −0.001</i>	<i>Effect = −0.07*</i>

Note. * $p < .01$. Standard errors in parentheses. T-test is two-tailed.

^aExcludes Hamilton County and Putnam County for years 1954–1960 (see note 12).

^bIncludes counties in which some parts already had registration requirements in place (36 counties in New York and 4 counties in Ohio).

The specification is as follows:

$$T_{it} = \delta R_{it} + \beta X_{it} + y_t + \mu_i + \varepsilon_{it}, \quad (1)$$

where i indexes counties, t indexes years, T is turnout, R is the registration status of each county, X is a vector of demographic variables, y are year dummies, μ are county fixed effects, and ε is a stochastic error term. The independent variable of primary interest is the registration status of each county, R , by which we mean the percentage of election districts in each county requiring registration.¹³ Our expectation is that δ will be less than zero. This coefficient should be interpreted as the effect of a county without registration moving to registration. For counties already requiring registration for part of their electorates, the magnitude of the effect would, of course, be smaller.

Our model also includes a set of county-level demographic variables to control for other possible influences on county-level turnout, including population, median family income, the percentage of population with high school education, and the percentage of population that is African American.¹⁴ We also include year dummy variables to account for ballot effects (i.e., whether the election includes a presidential, senate, and/or gubernatorial election, electoral competitiveness) and other possible election year shocks. Finally, we employ county-level fixed effects, which enable us to remove any time-invariant, unobserved county-level factors that might also influence turnout. These fixed effects will

¹³We tried to isolate the effect of the registration close date by including a separate “days” variable for this effect in our model, in recognition of the consistent finding in the literature that registration close date is an important predictor of turnout (e.g., Wolfinger and Rosenstone 1980; Gilliam 1985). In both New York and Ohio, the registration close date varied during the period of our time series, and in New York the close date often varied across counties until the early 1990s. This variable, however, introduced multicollinearity into our model. Thus the coefficient on the registration variable likely also captures the effect of the registration close date found in other studies.

¹⁴Since these demographic data are available only at the county level for decennial-census years, we imputed the data for noncensus years using linear interpolation. Linear interpolation might introduce measurement error for these variables, which, if present, would attenuate the effects of our coefficients. We checked for this possibility by estimating the same fixed effects model using only those election years for which we have data from the Census Bureau (1960, 1970, etc.). The coefficient estimating the effect of registration on turnout was smaller (−0.02) but still statistically significant.

also capture other county-level demographic variables not included in the model. Descriptive statistics for the variables included in the model are presented in the top portion of Table A in the appendix.

The results of this fixed effects regression model are presented in Table 4. The first column of coefficients presents the results from estimating Eq. (1) for the years 1954 through 2000. The effect of registration on turnout is clear. The coefficient on the percent of county required to register is -0.039 , which suggests about a four percentage point decline in turnout for counties moving from no registration to registration. The effect for counties already requiring some voters to register would have been less.

We test the robustness of these results in several ways. First, we consider only “pure cases” by excluding counties with partial registration. As presented in the second column of Table 4, the decline in turnout for just those counties shifting from no registration to registration is also about four percentage points. Second, we estimate the same model using state-year dummy variables instead of year dummy variables, which allows us to control for the possibility that there are different confounding factors occurring within each state in each particular year. The effect of registration suggested by this model is reported in the third column of Table 4 and is slightly larger—about 5%. Third, we estimate the model for a period of time (1960–1986) more closely around the years of the state law changes of interest. The model yields similar coefficients for this shorter time series (not reported). The effect of registration on turnout is again just shy of four percentage points. Finally, we run the model separately for each state. New York and Ohio differed somewhat—the coefficient on registration is about -0.05 in Ohio and -0.03 in New York (not reported).¹⁵

Our second fixed effects model is identical to the first, with an additional variable introduced to measure any immediate, short-term impact of the change in state law. There is likely a learning curve when registration reforms are first implemented as poll workers and voters adapt to the new set of regulations. From the perspective of poll workers, unfamiliarity with new procedures may lead to the turning away of eligible voters. Voters, unaware of the newly established requirements, may not have registered in time to become eligible to vote. These types of effects, however, are likely a one-time phenomenon. To account for the short-term effect of the state law change, we create a new variable by multiplying a dummy variable indicating the first election in which the counties were required to register voters due to the change in state law (coded one for counties affected by the law change in first election after state law change, and zero otherwise) with the percentage of the county requiring registration before the law change. The coefficient on this term can be interpreted as the effect of registration specifically due to the law change, and thus it is separable from the impact of registration itself. We expect the coefficient on this variable to be negative, and it can be interpreted as an additional short-term “penalty” imposed on eligible voters required to register for the first time. For the impacted counties, the effect will vary in the aggregate, since counties ranged from having no election districts with prior registration to those having over 80% already requiring registration.

The results of this analysis are presented in the final three columns of Table 4. When we include the law change measure in the full panel—1954 to 2000—the coefficient on registration remains at about three to four percentage points. The coefficient of -0.02 for the law change variable indicates an additional effect of registration on turnout for those counties affected by the states’ adoption of statewide registration. This result remains

¹⁵We also estimated each of the above models excluding the largest New York and Ohio counties, and the results remain virtually unchanged (we do not report these results).

Table 4 Registration effects on turnout in New York and Ohio counties: Fixed effects model, 1954–2000

		<i>Dependent Variable = County-Level Turnout</i>					
		<i>Full sample</i>	<i>Excluding counties w/partial registration</i>	<i>Full sample w/state-year dummies</i>	<i>Full sample</i>	<i>Excluding counties w/partial registration</i>	<i>Full sample w/state-year dummies</i>
		(1)	(2)	(3)	(4)	(5)	(6)
13	% of county with registration	-0.039** (0.003)	-0.036** (0.003)	-0.051** (0.003)	-0.037** (0.003)	-0.034** (0.003)	-0.050** (0.003)
	Law change				-0.020** (0.005)	-0.018** (0.005)	-0.023** (0.006)
	Log population	0.048** (0.011)	0.036** (0.012)	0.017 (0.010)	0.047** (0.011)	-0.035** (0.021)	0.016 (0.010)
	Log median family income	-0.133** (0.013)	-0.142** (0.014)	0.050** (0.013)	-0.131** (0.013)	-0.139** (0.014)	-0.049** (0.013)
	% population with h.s. education	0.071* (0.028)	0.070* (0.029)	0.011 (0.024)	0.072* (0.028)	0.071* (0.029)	0.013 (0.024)
	% population African American	-0.795** (0.056)	-0.834** (0.059)	-0.532** (0.044)	-0.783** (0.055)	-0.822** (0.059)	-0.521** (0.044)
	Constant	1.47** (0.152)	1.70** (0.171)	0.775** (0.124)	1.45** (0.152)	1.68** (0.170)	0.819** (0.127)
	R ²	0.91	0.91	0.94	0.91	0.91	0.94
N	3572	3153	3572	3572	3153	3572	

Note. * $p < .05$, ** $p < .01$. Huber-White standard errors in parentheses. Year dummies and state-year dummies are not reported.

stable when we consider only pure cases. The effect of registration is about three percentage points, while the short-term effect of the law change is again about two percentage points. Finally, we reran the model using state-year dummy variables and find that the results remain stable. The effect of registration increases to about five percentage points, again with an additional short-term effect of approximately two percentage points. In sum, we find that in counties without registration prior to the law change, turnout suffered a cumulative decline of about five to seven percentage points in the first election after the law came into effect.¹⁶

The differences-in-differences model builds on the fixed effects specification by focusing on the short-term change in turnout in counties in which registration was imposed versus the short-term change in turnout among counties that already had registration. Taking first differences in Eq. (1) leads to the following equation:

$$T_{it} - T_{it-1} = \delta(1 - R_{it-1}) + \beta(X_{it} - X_{it-1}) + y_t + \varepsilon_{it}, \quad (2)$$

where i indexes counties, t indexes years, T is county-level turnout, R is the percentage of election districts in the county requiring registration, X is a vector of county-level demographic variables, y is a year variable to capture election-specific shocks, and ε is a stochastic error term. In the election following the imposition of registration, all counties required registration, so $R_t = 1$ and the difference is $1 - R_{t-1}$.

The variable of primary interest is the registration status of each county, R , which we again operationalize as the percentage of each county requiring registration. The model is constructed such that, in election year t , all counties required their voters to register. Thus, for counties requiring all their voters to register at $t-1$ (that is, before the state made it compulsory), the change in R will be zero. For counties that previously did not require registration, the change in the registration variable is $1 - R_{t-1}$, because once registration is required $R_t = 1$. Our expectation is that δ will be less than zero, because the introduction of registration is expected to lower turnout.

As in the fixed-effects model, we include a set of demographic control variables,¹⁷ as well as year dummies (at year t only) to control for ballot effects and other possible election-year shocks. Last, the first-differencing process removes any unobserved county-level effects that do not vary over time. Descriptive statistics for these variables for each set of elections we analyze below are presented in the bottom portion of Table A in the appendix.

We first consider the differences-in-differences model using data from the elections immediately before and after the law change in each state—that is, 1966 and 1968 for New York and 1976 and 1978 for Ohio. It does not matter that we mix presidential and congressional elections for these estimates, since they are similar from presidential election to presidential election and midterm election to midterm election. We present the pooled analysis here for simplicity.

¹⁶We also ran this model with an AR1 error process. One might argue that turnout is not completely independent from election to election. The likelihood that a voter participated in an election is likely related to whether he or she voted in the prior election. Therefore the errors in our regression model may be serially correlated, so we reestimated Eq. (1) with an AR1 process. The coefficient on the registration variable remained generally the same, fluctuating between four to five percentage points when the law change variable is excluded and three to four percentage points when the law change variable is included.

¹⁷In addition to the demographic variables included in the fixed effects model, we also include measures of the percent of county population that is urban and the median age of the county population, since these data are also available for these years.

Table 5 Registration effects on turnout in New York and Ohio counties:
Differences-in-differences model

<i>Dependent Variable = County-Level Turnout</i>		
	<i>One election before and after law change (1)</i>	<i>Two elections before and after law change (2)</i>
% of county with registration	−0.083** (0.007)	−0.049** (0.007)
Log population	0.237 (0.128)	−0.436* (0.009)
Log median family income	0.158 (0.276)	0.342** (0.401)
% population with h.s. education	0.238 (0.316)	−0.007 (0.125)
% population African American	−1.54** (0.582)	−0.260 (0.320)
% urban	−0.638** (0.231)	0.228* (0.097)
Median age	0.016 (0.011)	0.003 (0.004)
Year dummy	0.262* (0.024)	0.248** (0.019)
Constant	−0.192** (0.037)	−0.144** (0.034)
R ²	0.94	0.96
N ^a	149	149

Note. * $p < .05$, ** $p < .01$. Huber-White standard errors in parentheses. Independent variables represent Δ .

^aHamilton County is excluded (see note 12).

The results are presented in Table 5. The coefficient on the registration variable is -0.085 , which suggests that the states' imposition of registration requirements had a large effect on county-level turnout. This estimate is driven entirely by the Ohio data, for which the coefficient equaled -0.11 and was highly significant. The estimate for New York was not statistically different from zero, which might reflect idiosyncrasies in the specific elections at stake.

More reliable estimates emerge when comparing two elections before and after the law change. The second column of Table 5 presents the estimated effect of the registration law using a two-election window. The coefficient on the registration variable is approximately -0.04 , which suggests that the impact of New York and Ohio introducing voter registration requirements on a statewide basis led to about a four percentage point drop in voter turnout for counties not previously having personal registration laws in effect. The estimated effects for New York and Ohio separately in this model are similar—approximately -0.05 for Ohio and -0.03 for New York—so the estimates appear stable.

In sum, the estimates from the fixed effects and differences-in-differences models suggest a clear conclusion: the introduction of personal registration requirements led to a decline in county-level turnout in New York and Ohio. Our panel estimates suggest an effect of about three to four percentage points, with an additional one-time drop in turnout of about two to three percentage points in the first election for those counties required for the first time to register all of their voters.

4 Conclusion

Registration laws exist to ensure the integrity of the electoral process. In most states, they were introduced in the nineteenth century to combat widespread corruption, but they have also been used to exclude voters, as Key (1949) describes. Today, reforms such as same-day registration and later registration close dates seek to lessen the obstacle to voting posed

by registration. The critical questions are, how large is the effect of registration on participation, and how should we weigh that problem against the potential problem of vote fraud?

The relatively recent introduction of voter registration into all counties in Ohio and New York provides a powerful means through which to estimate the magnitude of the effect of registration itself on participation. As with many other studies, we have found a highly statistically significant drop in turnout in counties on which registration was imposed compared with counties that had already chosen to use registration. The effect we observe, however, is approximately half as large as often found, sitting at the low end of the range of past estimates.

The reason may be methodological. Cross-sectional studies find effects of registration on participation rates in the range of 5–10%. In New York and Ohio, we find that as well. During the 1950s and 1960s (and the early 1970s in Ohio), counties with registration had turnout rates roughly 10 percentage points lower than counties without registration. Half of that effect, though, seems to be due to features of the counties other than the registration law. When we use the panel of observations to estimate the effect, we find that the introduction of registration within counties dropped turnout immediately by four to seven percentage points and that over the longer term, the change in turnout attributable to the introduction of registration is approximately three to four percentage points. The larger short-term effect likely reflects a one-time additional penalty imposed on voters required to register for the first time. Unfamiliar with the new registration requirements, voters accustomed to simply showing up at their voting location to cast their ballots were likely turned away for having failed to meet the newly imposed registration deadline.

As a historical and political matter, it is noteworthy that we look at the introduction of registration in an era long after the fall of Tammany and other political machines widely alleged to have won elections by buying votes. The presumption today is that registration systems have been in place for a long time and were introduced to crush the corrupt practices of the nineteenth century. They may have done just that, but the effects of the introduction of registration in the late twentieth century, after machine politics had largely disintegrated, is similar in magnitude to the effects of the introduction of registration in urban areas in the nineteenth century. The effects in the nineteenth century are in the range of a seven percentage point drop in turnout. Converse (1972, 1974) and Rusk (1970, 1971, 1974) suggest that the introduction of registration and other institutional changes that decreased corruption account for the decline of turnout at the beginning of the twentieth century. Our findings from late in the twentieth century suggest that a substantial portion of the decrease in turnout attributable to registration at the beginning of the twentieth century likely reflected an actual decrease in voting among legally qualified voters, and not just the elimination of corrupt ballots.

Our findings also temper the contemporary debate over registration reform. Many observers have expressed disappointment with recent registration reforms, such as those in the National Voter Registration Act of 1993. The hope was to produce turnout in the range of 60–70%, rather than 50%. We study a large change in registration—the imposition of registration in counties without previous such requirements—yet find a small effect. Our analysis suggests that expectations should have been more modest, perhaps in the range of 53 or 55%. The reformers' disappointment may reflect biases in previous statistical estimates of the effects of registration on turnout, rather than failures of the laws. Registration does produce lower voting rates, though the effects are smaller than most prior research has indicated.

Appendix

Table A Descriptive statistics of variables included in regression models

<i>Variable</i>	<i>Mean</i>	<i>Standard Deviation</i>	<i>Minimum</i>	<i>Maximum</i>
<i>Fixed effects model (n = 3572)</i>				
County-level turnout	.53	.11	.17	.92
% of county with registration	.78	.39	0	1
Log population	11	1.1	9.2	15
Log median family income	9.6	.80	7.7	11
% population with h.s. education	.49	.11	.20	.87
% population African American	.04	.05	0	.38
Law change	.02	.14	0	1
<i>Differences-in-differences model (n = 149) One election before and after law change</i>				
County-level turnout	-.09	.13	-.36	.11
% of counties with registration	.37	.43	0	1
Log population	.03	.03	-.04	.22
Log median family income	.15	.04	.10	.25
% urban	-.002	.01	-.05	.08
% population with h.s. education	.005	.04	-.05	.08
% population African American	.002	.01	-.002	.05
Median age	-.20	.72	-1.9	.86
<i>Two elections before and two elections after law change</i>				
County-level turnout	.01	.14	-.23	.20
% of counties with registration	.39	.43	0	1
Log population	.02	.02	-.03	.13
Log median family income	.10	.01	.06	.13
% urban	-.01	.03	-.07	.12
% population with h.s. education	-.02	.08	-.15	.11
% population African American	.003	.01	-.01	.08
Median age	-.03	1.4	-4.3	2.6

Note. Hamilton County is excluded in differences-on-differences model (see note 12).

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