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Youth Enfranchisement, Political Responsiveness, and Education Expenditure: Evidence from the US[†]

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We examine the link between the political participation of the young and fiscal policies in the United States. We generate exogenous variation in participation using the passage of preregistration laws, which allow the young to register before being eligible to vote. After documenting that preregistration promotes youth enfranchisement, we show that preregistration shifts state government spending toward higher education, the type of spending for which the young have the strongest preference. A 1 percent increase in youth voter turnout generates a 0.77 percent increase in higher education spending. The results collectively suggest political responsiveness to the needs of the newly enfranchised constituency. (*JEL* D72, E62, H52, H75, I23, I28, J13)

In all modern states, a major function of the government is to allocate the public budget in response to the demands of socioeconomic groups. The government's choice of how much of the public budget to redistribute, and to which socioeconomic groups, is embedded within the political system. Since the seminal paper of Meltzer and Richard (1981), the political economy literature has been studying the role of electoral mechanisms in the determination of the level of government spending and the extent of redistribution. The main prediction of this literature is that groups of voters with greater political influence will have more success in diverting resources to policies that meet their needs.

Several contributions following Acemoglu and Robinson (2000) have established that conflict between rich and poor lies at the heart of the historical process extending the voting franchise and the consequent expansion of the welfare state. Yet conflict between different age groups and its implications for political participation and

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government spending in a modern setting have received far less attention. In the face of evolving demographic forces, public intervention in modern democracies favors pensions and health care (which benefit the old) over education expenditure (which benefits the young). The literature has yet to conclude whether this pattern relates to the reluctance of the young to vote.

This paper examines the link between the political participation of the young and policy decisions. For this purpose, the United States provides an ideal institutional setting. This is due to two reasons. First, even though the United States has long been a de jure full democracy with universal suffrage, various restrictions and extensions of political rights, which have affected the de facto ability of citizens to vote, exhibit rich variation across states and over time. Second, the United States is characterized by a peculiar two-step voting process that requires eligible voters to register as a prerequisite for casting their ballot. Voter registration entails a cost in terms of effort, time, and involvement, which is especially large for the young who must gather information and then show up at the voting stations for the first time.

We focus on preregistration, an electoral provision introduced at the state level with the aim of encouraging civic engagement among the young by reducing the burden of registration, in order to generate exogenous variation in turnout. Preregistration allows young individuals to register at a variety of locations that they frequent—such as schools, campuses, and motor vehicle bureaus—before becoming eligible to vote, regardless of whether they will reach voting age prior to the next election. Starting in 1993, 15 geographically dispersed states plus the District of Columbia introduced preregistration laws at various points in time. We exploit the variation generated by the staggered timing of voter registration reforms in order to empirically examine whether this policy accomplishes the intended task of boosting the political involvement of young citizens, and whether this de facto enfranchisement drives an increase in the public resources allocated to the newly enfranchised constituency.

We first study the electoral effects of preregistration. We use individual-level data on registration and voting records from the Voting and Registration Supplement of the Current Population Survey. We employ a difference-in-differences approach, as well as a triple-difference using older voters as a placebo (to remove state-years specific shocks), to exploit the differential timing of preregistration reforms. We find that preregistration reduces the voter registration gap between young and old by 2.3 percentage points and the turnout gap by 2.1 percentage points, relative to the pre-treatment means of 23.8 and 28.5 percentage points, respectively, in states that have introduced the law relative to those that have not. The results imply that on average over 20,000 additional young voters—who otherwise would have been without a political voice—are de facto enfranchised in every post-treatment election in each treated state.

We then apply the same difference-in-differences framework to estimate the effects of the introduction of preregistration on state spending. We use annual financial data for state governments gathered by the US Census Bureau. Evidence from survey data provided by the American National Election Studies indicates that young citizens have the strongest preference for state financial support of higher education. Therefore, we focus on higher education expenditure as the main

outcome and take advantage of the variation in the treatment dates among states in a difference-in-differences regression design. We also complement the state-level analysis by comparing institution-level higher education spending between matched county-border pairs employing an empirical strategy that exploits policy discontinuities at state borders.

We find that preregistration raises per capita higher education expenditure by 5.1 percent, which corresponds to an increase of about $25 per capita in those states that, at some point in time, adopted preregistration. The effect is economically substantial and supported by a set of validity tests, including the county-border pair design using institutional data.

Taken together, an IV-type interpretation of the results suggests that a 1 percent increase in young voter registration increases the allocation of state resources to higher education by roughly 0.84 percent while a 1 percent increase in young voter turnout increases it by 0.77 percent. Expressed in 2014 US dollars, these elasticities imply that, for every additional 1,000 young voters, governments respond by increasing higher education expenditure by approximately $1.25 per capita, which is 0.25 percent of per capita higher education spending in the average state. Although a number of complementary channels activated by registration reform may be operating simultaneously, the dynamic pattern of outcomes reinforces the hypothesis that changes in financial support for higher education are likely to partly reflect changes in the political participation of the young.

The paper contributes to the literature in three ways. First, a large literature analyzes democratization and de jure enfranchisement in the context of conflict between economic elites and the poor masses (see, for example, Acemoglu and Robinson 2000, 2006; Lizzeri and Persico 2004; and Llavador and Oxoby 2005). Lindert (1994), Aidt, Dutta, and Loukoianova (2006), and Acemoglu et al. (2019) empirically analyze the effect of enfranchisement on public spending. Other studies have addressed conflicts between socioeconomic groups along racial and gender lines.[1] In contrast, we study the implications of the enfranchisement of the young in the face of a potential conflict with the old, an issue that has not been addressed to date. Furthermore, we focus on a de facto enfranchisement episode in a developed economy where universal suffrage is already established, as opposed to most of this literature which focuses on de jure episodes and/or limited-suffrage contexts.

Second, a smaller literature studies the effects of preregistration laws. Based on the cases of Florida and Hawaii, McDonald and Thornburg (2010) and Holbein and Hillygus (2016) observe that increased preregistration exposure has a positive impact on the turnout of young voters. However, neither discusses the implications for government spending, which is the main contribution of the present paper. Moreover, both papers focus on an individual state, while we are able to generalize

[1] The implications of voting restrictions such as poll taxes and literacy tests, which were enacted in the US South following the Civil War and aimed at disenfranchising black populations, have been investigated by Naidu (2012) and Bertocchi and Dimico (2017). The removal of such restrictions with the passage of the 1965 Voting Rights Act and its influence on welfare policies are discussed by Husted and Kenny (1997); Besley, Persson, and Sturm (2010); and Cascio and Washington (2014). In a similar vein, the extension of suffrage to women and its impact on the size and composition of government spending is studied by Lott and Kenny (1999); Miller (2008); Carruthers and Wanamaker (2015); and Kose, Kuka, and Shenhavfor (2018) for the United States, and by Aidt and Dallal (2008) and Bertocchi (2011) for other Western countries.

the analysis by taking advantage of the fact that preregistration laws were passed in a large number of states. The impact of other laws aimed at easing the registration burden, such as the National Voter Registration Act and Election Day Registration, is analyzed by Highton (1997) and Besley and Case (2003).[2]

Third, we contribute to the macroeconomic literature on intergenerational conflicts over the financing and allocation of the public budget. By embedding electoral competition within models of dynamic government decision making, this literature predicts that intergenerational redistribution responds to shifts in political power across generations (see, for example, Tabellini 1991; Alesina and Rodrik 1994; Krusell, Quadrini, and Ríos-Rull 1997; Cooley and Soares 1999; Levy 2005; Song, Storesletten, and Zilibotti 2012; and Lancia and Russo 2016). A drawback of these models is their inability to quantitatively separate the effect of shifts in political power on government spending from the effect of changes in the demographic structure, since the median age of the electorate is generally the variable chosen to capture the political strength of old relative to young voters (see Strömberg 2006). Our contribution is to assess the impact of greater political engagement among the young on fiscal outcomes while isolating it from the impact of pure demographic forces.

The rest of the paper is organized as follows: Section I describes the institutional setting and historical background. Section II presents a conceptual framework. Section III describes the data. Section IV reports the estimation results for the impact of preregistration on the political participation of the young. Section V documents the effect of preregistration on government spending at the state level and at the level of higher education institutions. Section VI discusses the magnitude of the effects. Section VII concludes. The online Appendix includes: the figures and tables not presented in the text (online Appendix A); state-by-state information on the legislative process leading to the approval of a preregistration bill (online Appendix B); evidence for the divergence between young and old in terms of policy preferences (online Appendix C); evidence for the impact of preregistration on the identity of elected representatives (online Appendix D); an extended setup of the model (online Appendix E); and a more detailed description of the data (online Appendix F).

I. Institutional Setting and Historical Background

A. *The Electoral and Budgeting Processes*

The United States is a federal republic composed of 50 states plus the District of Columbia. The US Constitution establishes rules for federal elections, while state laws, controlled by state legislatures, regulate state and local elections. Since the separation of powers also applies at the state level, state legislatures and the executive are elected separately. In each state, voters elect the governor directly

[2] Cantoni and Pons (2019) analyze the effect of strict ID laws. The influence of voting reforms on voter turnout and policy outcomes in countries other than the United States is investigated by Baland and Robinson (2008) in the context of the secret ballot in Chile; by Fujiwara (2015) in the context of electronic voting in Brazil; by Hodler, Luechinger, and Stutzer (2015) in the context of postal voting in Switzerland; and by Fowler (2013), Hoffman, León, and Lombardi (2017), and León (2017) in the context of compulsory voting in Australia, Austria, and Peru, respectively.

for a four-year term except in New Hampshire and Vermont, where the length of a gubernatorial term is two years.[3]

US government spending is divided between the federal, state, and local levels. At the state level, the budget is proposed by the governor and then submitted for approval to the legislature. A budget proposal sets funding priorities and specifies the amounts to be allocated to various state agencies. The proposal is the most important means for a governor to influence the legislative process.

Among the various categories of state spending, higher education is the third largest.[4] It includes financial support for public universities, community colleges, and technical and vocational institutions and is primarily financed by broadly based state taxes. Funds allocated to an institution of higher education are managed by its Board of Trustees, which has the authority and responsibility to ensure the fulfillment of the institution's mission. To guarantee that they serve the public interest, many states have established independent coordinating agencies that oversee the Boards of Trustees and review budget requests submitted to the state.[5]

Unlike mandatory spending programs that dominate state budgets, higher education is a relatively flexible budget item.[6] The variation in state spending on higher education is largely driven by economic and demographic variables, such as per capita income and the size of the college-age population; fiscal variables, such as pressures to spend in other areas; and institutional factors, such as the political interests of governors. As a consequence, states differ markedly in their financial support for higher education.[7] In recent years, there has been a downward trend in state financial support overall. Higher education institutions have therefore converted their funding model from a state-subsidized model to a more self-financed one supplemented by financial aid, which has resulted in an increasing share of the cost burden being shifted from taxpayers to students through higher tuition rates.[8]

[3] Federal elections, as well as many state elections, are held on Election Day in November of even-numbered years, with the exception of Kentucky, Louisiana, Mississippi, New Jersey, and Virginia, which elect governors during odd-numbered years. The governors of 14 states can serve an unlimited number of terms, governors in the remaining states cannot be elected for more than two terms, and in some cases one.

[4] Elementary and secondary education was the largest category of general fund spending in fiscal 2014, accounting for 35 percent of the total. This category, combined with Medicaid (19.3 percent) and higher education (9.7 percent), accounts for nearly two-thirds of general fund spending. See NASBO (2014).

[5] According to the Education Commission of the States and the National Center for Higher Education Management Systems, independent coordinating agencies exist in 24 states. Their members are in part appointed by governors and in part nominated by the leadership of the two state chambers and the general public. Members usually serve an eight-year term to ensure independence from the state. Coordinating agencies have significant budgetary authority. See Fulton (2019).

[6] Delaney and Doyle (2011) show that higher education serves as a balance wheel, such that during economic upturns it is an attractive area for states to fund, while in downturns the reverse is true. This is partly due to the option of obtaining outside revenue by raising tuition.

[7] In fiscal 2014, state funding for higher education ranged from $3,660 per full-time equivalent (FTE) enrollment in New Hampshire to $18,550 per FTE in Alaska. Seven states provided less than $5,000 per student, while seven provided more than $10,000. See Baum and Johnson (2014).

[8] Between 2004 and 2014, per FTE state appropriation at public four-year institutions declined by $1,720, while net tuition revenue rose by $3,000. See Baum et al. (2018).

B. *Young Voter Turnout*

Voting is the most effective way to influence government decision making. In the 2012 presidential election, only 54.9 percent of Americans cast their ballot. Since the 1960s, turnout has been characterized by a consistent downward trend, decreasing by over 14 percentage points from its 1964 peak of 69.3 percent. Remarkably, there has always been a wide gap in voter turnout between different age groups. When 18-year-olds were first given the right to vote in the 1972 presidential election, following the passage of the Twenty-sixth Amendment to the Constitution, voter turnout was 52.1 percent in the 18–24 age group in comparison to 68.4 percent for citizens over 25. Since then, young voter turnout has consistently remained lower than that of other age groups. By the 2012 presidential election, the corresponding rates were 41.2 percent and 64.8 percent.[9]

The low level of participation by young Americans in the voting process has gained increasing attention. Several explanations for the persistence of low civic engagement among the young have been advanced, such as their limited resources and their inadequate knowledge of voting procedures and mechanisms. The fact that the young are more likely to move frequently because of education or work also makes it more difficult for them to collect information and establish connections, which lowers their participation rate.[10] Beyond these explanations, a peculiar feature of the US voting system that has been blamed for low turnout of the young is the two-step voting process, which forces eligible voters to register to vote in order to be able to actually cast their ballot.

Registration laws were introduced by most states in the nineteenth century to fight fraud and corruption, with the goal of ensuring the integrity of the electoral process.[11] The voter registration process is currently regulated by state law, with North Dakota being the only state not requiring it. Registration rules differ significantly across states in terms of deadlines, restrictions, and/or proofs required to register. Voter registration typically takes place between two and four weeks before an election and is organized at the county level.[12] Since registration in more than one place at a time is not permitted, moving permanently to a new county requires re-registration. The cost of registration includes the effort and time required to become familiar with the electoral process, which is especially large for first-time voters. Indeed, many newly eligible voters are unfamiliar with the registration system, including how and where to register, so that they more frequently miss voter registration deadlines. On the other hand, the share of young people who, once

[9] Young voter turnout rates are taken from the 2013 report of the Center for Information and Research on Civic Learning and Engagement, which is available at civicyouth.org/quick-facts/youth-voting.

[10] Other potential motives are linked to specific features of the US political context, such as the presence of a two-party system that limits the chances of third-party candidates, who are often supported by young people, and the funding system for electoral campaigns that relies heavily on large donors. On the demographics of voter turnout, see the classic text by Wolfinger and Rosenstone (1980) and the more recent account by Holbein and Hillygus (2016).

[11] Southern states introduced registration prerequisites involving poll taxes and literacy tests in order to curb the political power of the black population following the abolition of slavery in 1865. These were later abolished by the 1965 Voting Rights Act. On the history of registration laws, see Ansolabehere and Konisky (2006).

[12] On voter eligibility requirements and registration procedures, see usa.gov/register-to-vote.

registered, do actually vote is quite high.[13] The positive correlation between registration and voter participation suggests that the young are actually more likely to vote when given greater opportunities to register.

C. *Voter Registration Reforms*

To ease the burden of registration and encourage civic engagement, several reforms have been introduced with largely bipartisan support at the federal and state levels. The National Voter Registration Act (NVRA) is the most far-reaching federal intervention in the state and local registration systems in history. The act was signed into law by President Clinton in 1993 and is currently active in 44 states and the District of Columbia. The NVRA enabled any eligible voter to register either at state motor vehicle agencies, as part of a driver's license application or renewal, or at government offices for those requiring social assistance.[14]

In addition to the NVRA, three major voter registration reforms have been enacted at the state level: (i) Election Day registration (EDR), which allows eligible voters to register on Election Day;[15] (ii) online registration, which allows voters to submit their application over the Internet;[16] and (iii) preregistration, which enables citizens who are not yet 18 to register as pending voters, whether or not they reach voting age before the next election. Preregistration drives are organized at customary and frequent points of contact, such as schools, campuses, and motor vehicle bureaus, in order to make it easier for youths to register and automatically be ready to vote when they become eligible.[17]

The declared goal of preregistration is to encourage voting among the young. Congressman Markey, who introduced the Gateway to Democracy Act in 2004, appealed for a national preregistration law by declaring, "People need to exercise their right to vote. Unfortunately, young people consistently fail to turn out to the polls on voting day […]. It is in the best interest of the country to make it as easy as possible for the youth of our nation to go to the polls for the first time." Although attempts have been made to expand the law nationally, preregistration remains a state provision. Florida was the first state to extend voter registration to 17-year-olds in 1971, albeit conditional on reaching voting age by the upcoming election. In 2007, Florida introduced the preregistration option for all individuals aged 17 or younger with a driver's license. In 2008, Florida made it

[13] The percentage of registered voters under age 30 who cast their ballots in the 2000, 2004, and 2008 presidential elections was 74, 82, and 84, respectively. See File and Crissey (2012).

[14] Although the NVRA was intended to regulate only federal elections, it effectively changed the registration process for all elections by eliminating the inefficient practice of maintaining separate voting lists for different types of elections. Idaho, Maine, Minnesota, New Hampshire, Wisconsin, and Wyoming were exempted from the NVRA because by 1994 they had introduced Election Day Registration. North Dakota was also exempt since it has no registration requirements. There is no consensus as to the effectiveness of the NVRA in increasing voter turnout. Knack (1995) estimates that it has a positive effect, while Besley and Case (2003) find no significant effect.

[15] Starting with Maine in 1973, EDR has been introduced in 15 states, plus the District of Columbia. Highton (1997) and Besley and Case (2003) find evidence that EDR increases turnout.

[16] Starting with Arizona in 2002, 39 states plus the District of Columbia currently offer online registration. Quantitative investigations of the impact of online registration on voting have not yet been carried out.

[17] Preregistration laws differ from other state provisions that tie eligibility for early registration to attaining voting age prior to a specific election. In fact, preregistration operates on an ongoing basis, even when elections are not scheduled.

accessible to all 16-year-olds. Similarly, Hawaii permitted conditional registration as early as 1977 and introduced preregistration for all individuals over 16 in 1993. Other states later followed suit, often in response to a voter education campaign conducted by FairVote, a nonpartisan organization that has been promoting civic engagement and election reform since 2005.[18] Oregon enacted preregistration in 2007; California, North Carolina, and the District of Columbia in 2009; Delaware, Maryland, and Rhode Island in 2010; Maine in 2011; Colorado in 2013; Louisiana and Massachusetts in 2014; Utah in 2015; New Jersey in 2016; and Nevada in 2017. North Carolina later repealed the law in 2013.[19] The timeline of the preregistration legislations across US states is shown in Figure A1 in online Appendix A.

D. *Preregistration Legislation*

Understanding the legislative process that leads to the approval of a preregistration bill is important in order to evaluate the validity of our empirical strategy, which relies on the introduction of preregistration being an exogenous event with respect to a governor's budget decisions (examined in greater detail in Section V). We take advantage of the fact that the constitutional division of responsibilities between the executive and legislative branches has a major impact on the approval process of various types of bills. While budget bills are first promoted by the governor, then approved by the executive body, and eventually passed by the state legislature, electoral bills such as preregistration follow a reverse pattern. They are first sponsored by a member of the state legislature, then approved in the House and Senate, and finally signed into law by the governor. The opposite order of approval for electoral bills versus budget bills means that preregistration laws and fiscal policy decisions are distinct outcomes of two different games played between governors and legislatures. This argument is corroborated by Kousser and Phillips (2012), who document how state constitutions strip governors of their power over state lawmaking, while at the same time ensuring them an advantageous position over the legislature in approving the fiscal budget.[20]

A governor's restricted authority over state lawmaking is also reflected in her limited use of veto power. In principle, governors can exercise an executive veto in order to block the final approval of a bill or amendment. However, among the states where a preregistration bill has been approved, veto power has been exercised only in Rhode Island by Governor Carcieri in July 2009. An important feature of preregistration laws is that they have received *bipartisan* support, with California

[18] Representative Pacheco of Rhode Island, who sponsored House Bill 5005 with four cosigners from among both Republicans and Democrats, declared that: "FairVote is the major asset in the preregistration battle, doing crucial legwork and reaching out to local media." See fairvote.org.

[19] Currently, California, Colorado, Delaware, Florida, Hawaii, Louisiana, Maryland, Massachusetts, North Carolina, Oregon, Rhode Island, Utah, and the District of Columbia allow preregistration for 16-year-olds, while Maine, Nevada, and New Jersey allow it for 17-year-olds.

[20] Based on a sample of governors in 28 states during the 2001–2006 legislative sessions, Kousser and Phillips (2012) find that when governors propose changes to existing constitutional, fiscal, or electoral rules, only 27 percent of them pass, with another 6 percent ending in compromise. Along these lines, a long-standing strand of literature highlights the role of the state governor as an important actor in setting state policy agendas and influencing state spending priorities (see, e.g., Barrilleaux and Berkman 2003).

being the only exception.[21] In view of the broad and nonpartisan support for preregistration, state legislatures have had the ability to override an executive veto. This was the case in Rhode Island, where a veto override passed in both chambers of the state legislature in January 2010 and preregistration became law without the governor's signature. Remarkably, Delaware, Florida, Louisiana, Massachusetts, New Jersey, North Carolina, Utah, and the District of Columbia passed preregistration bills almost unanimously. North Carolina is perhaps the most noteworthy example of bipartisan approval of a preregistration bill. The bill was cosponsored in 2009 by four legislators, including the youngest Republican and Democrat in the General Assembly. The bill was approved by a state legislature controlled by Democrats although more than 88 percent of the Republicans voted in favor of it. It was finally signed into law by Democratic Governor Perdue. Since then, more than 150,000 teens have preregistered under the program. Of the 55,291 who preregistered in 2012, 41 percent choose to do so as unaffiliated, 33 percent as Democrats, and 26 percent as Republicans, making 2012 the first year that preregistered Democrats exceeded preregistered Republicans.[22] In reaction, the Republican-controlled state legislature rescinded voter preregistration in 2013. This is an enlightening example of how a preregistration law that initially has bipartisan support may have consequences that cause it to be repealed for partisan reasons.

Detailed information on preregistration legislation is provided in online Appendix B. The online Appendix also zooms in on political characteristics of preregistration states and shows that the bill's eventual approval appears to be independent of a governor's political affiliation, although in most cases the bill has been sponsored by a Democratic representative. Indeed, among the states that have passed the bill, eight had a Republican governor and seven a Democratic one. It is also worth noting that the success of a legislative process in introducing preregistration is not associated with the age and gender composition of the legislature. Thus, the adoption of preregistration is not more likely when political power is in liberal hands, as one might have thought.

II. Conceptual Framework

In this section, we present a conceptual framework for analyzing voting participation and policy formation and interpreting the estimation results. Traditional models of electoral competition predict that an increase in the participation of young voters will shift politicians' policy positions in order to better reflect young people's preferences, which are tilted toward higher education and away from pensions and health. The awareness of the young that certain types of government spending benefit them more than the old is supported by empirical evidence based on data provided by the American National Election Studies and reported in online Appendix C.

[21] Although Assembly Bill 30 was approved with a relative majority in both the Senate (22–15) and the Assembly (50–28) with Democratic support only, the bill was eventually signed into law in 2009 by Republican Governor Arnold Schwarzenegger.

[22] See Blythe (2014).

Existing theories have highlighted two alternative views of the role of elections in policy formation (see Lee, Moretti, and Butler 2004). According to the first, voters *elect* policies and elections are meant to decide which candidate's policy will be implemented. In this scenario, preregistration may help to elect representatives who are more likely to provide more education based on an ideology shared with young voters. In online Appendix D, we test this hypothesis by looking at changes in both the characteristics of state legislatures and the identity of elected governors, but do not find supporting empirical evidence. According to the second view, voters *affect* policies and elections have the effect of constraining candidates' choices. In this scenario, preregistration may encourage candidates to commit to higher education expenditure that caters to the needs of young voters. We adopt this second view and produce supporting empirical evidence. In online Appendix E, we present a formal political economy theory of fiscal policy that matches specific features of a preregistration system. The model is an adaptation of a probabilistic voting model à la Lindbeck and Weibull (1987) to an environment with individual cost of voting and intergenerational conflict over the allocation of the public budget. In what follows, we summarize the main results.

By lowering the cost of voting for the young relative to the old, the enactment of a preregistration law generates a de facto enfranchisement episode since a larger share of young voters register and cast their ballot. Rent-seeking politicians then respond by addressing the economic needs of the newly enfranchised constituency, namely by approving more expenditure on higher education. The model also illustrates how political competition and the demographics of the population mediate the impact of preregistration on education policy. When political competition becomes stiffer, the incumbent policymaker adapts her policy positions toward the preferences of young voters, thus sacrificing an electoral rent; on the other hand, the rival candidate, who advocates maximal public expenditure, will have an increased chance of winning. The resulting increase in education expenditure dampens the response to the introduction of preregistration. At the same time, an increase in the share of the young in the population raises the number of potential voters who will cast their ballot as a result of the voter registration reforms. Such an increase boosts the political incentives of candidates to target the young, as long as their share of the population is not that large to begin with and candidates have not already promised high education expenditure.[23]

As highlighted in Section I, preregistration is an electoral provision targeted at young soon-to-become voters that was implemented in a number of geographically dispersed states in different years. Hence, the theoretical predictions emerging from the model can be tested by leveraging the rich variation generated by the voter registration reforms in a flexible event study framework. The empirical strategy is based on the idea that units that do not experience events in a particular year form a useful counterfactual for those that do, as long as fixed differences and common time effects are taken into account. Hence, the key identification assumption underlying this strategy is that in the absence of treatment, the treated and untreated units would

[23] The US panel data show that during the period 1980–2014 the share of the 16–25 age group in the population was only 15 percent.

exhibit similar trends. An attractive feature of an event study approach is its ability to map out the time pattern of the effects and therefore to provide evidence on differential trends between treated and untreated units prior to event years as a direct validation of the identification assumption.

III. Data

Our goal is to ascertain how the enactment of preregistration laws affects the political participation of young individuals and the distribution of public resources. To accomplish this, we require both individual-level data on registration and voting across multiple elections and data on public expenditure at the state government level. We supplement these data with information on the timing of voter registration reforms across states and on relevant covariates collected from various sources. Online Appendix F provides detailed information on variable definitions, data sources, and summary statistics. In what follows, we summarize the main characteristics of the data.

Data on voting and registration at the individual level are obtained from the Voting and Registration Supplement of the Current Population Survey (CPS) carried out biennially after each November election by the US Census Bureau. We confine the sample to individual residents in the United States aged 18–90 who report whether they have voted or registered during the period 1980–2014. This delivers a stacked cross-section of 1,370,526 individuals. Respondents who report having voted but do not indicate whether they are registered are categorized as having registered. Recall that an individual is exposed to the preregistration law before becoming an eligible voter. We then classify respondents as being potentially affected by the law if their age is between 18 and 24 inclusively at the time of the first election after the law's passage.[24] CPS data are also used to construct young voter registration and turnout by state and year for the sample period. Registration and voting records are complemented with socioeconomic information for each respondent in the sample, which includes gender, race, educational attainment, family income, labor force status, and metropolitan city status.

Annual financial data for state governments are taken from the Annual Survey of State and Local Government Finances conducted by the US Census Bureau. The full sample includes all 50 state governments for the period 1980–2014. We consider direct expenditures for different categories of fiscal spending and state revenues. The main outcome of interest is current higher education expenditure as a measure of state financial support for higher education, which consists of current operating expenditures of degree-granting institutions operated by state governments that provide academic post-secondary training.[25] Since direct expenditure excludes intergovernmental expenditure, current elementary and secondary education expenditure is

[24] We explored alternative definitions of age groups, such as 18–25 and 18–23, with no significant impact on the results.

[25] Current higher education expenditure includes, among other things, activities for instruction, research, public service, libraries, student services, administration, plant maintenance, and auxiliary enterprises. See Tanberg and Griffith (2013) for a detailed examination of the expenditure composition of this variable.

taken from the Annual Survey of School System Finances.[26] To control for potential confounders, we collect state-by-year political and socioeconomic information from various data sources.

As validation of the state-level data, we utilize higher education institution-level panel data from the Integrated Postsecondary Education Data System (IPEDS) published by the Delta Cost Project Database. The database provides annual data, usually collected at the beginning of July, for individual colleges, universities, and technical and vocational institutions in the United States, whether public or private, for-profit or not-for-profit. To account for sample attrition in the database, we focus on the 2005–2015 wave and, as in all the other datasets, let the sample period run until 2014.[27] This results in a panel of 3,714 institutions distributed over 50 US states, plus the District of Columbia, which reports information on enrollment, institutional characteristics, and institutional finances, including revenues and expenditures by source. Within the IPEDS survey, we consider state appropriation, which is state transfers actually received by institutions to meet current operating expenses, as an alternative measure of state financial support for higher education. One strength of using state appropriation as an outcome variable is that the recipients of these transfers—that is, the institutions—are geographically identifiable. This makes the measure suitable for a test of distributive politics by exploiting a contiguous border-county pairs empirical strategy (as discussed in Section V). To operationalize this strategy, we first create pairs of contiguous border counties, as presented in online Appendix Figure F1, which make it possible to distinguish between counties belonging to states that have introduced preregistration and those belonging to states that have not, as of 2014.[28] We then use the 2010 USPS county zip code to georeference the panel of higher education institutions. The border-county pairs sample therefore contains a panel of 1,059 institutions located in 336 border counties, which yields 255 distinct border-county pairs. Of those, 99, formed by matching 123 counties, have different registration rules at some point in the sample.[29]

IV. Youth Enfranchisement

We begin the analysis by empirically examining the effect of preregistration on young voter registration and turnout. To this end, we take advantage of the fact that preregistration reduces the cost of registering and in turn the cost of voting for young relative to other age groups. Since the age of an individual is a dimension along which the treatment varies, along with time and space, we first split the set of

[26] According to the Census of Governments classification methodology, elementary and secondary education expenditure in the form of payments to public school systems is considered to be intergovernmental expenditure, that is, transfers from state governments to other government offices.

[27] The IPEDS consists of three matched datasets that cover the waves 1987–2015, 2005–2015, and 2010–2015. The number of institutions surveyed in each dataset grows in each subsequent wave. We focus on the 2005–2015 wave since it includes the largest number of states that have adopted preregistration. Furthermore, it is preferable to the 2010–2015 wave since it considers a longer pre-treatment period, and to the 1987–2015 wave since it suffers less from sample attrition related to the selective erosion of the initial sample over the waves.

[28] Alaska and Hawaii are dropped from the sample since they do not share a border with another state.

[29] Online Appendix Table F4 shows that the border-county pair sample (panel B) displays strong similarities with the all-county sample (panel A) in terms of state appropriations and institutional characteristics.

individuals into two age groups: the young and the old. For each of them, we then use a difference-in-differences (hereafter DD) regression design, which compares electoral outcomes for individuals in states with preregistration and states without before and after voting reform is introduced. We operationalize the empirical strategy employing the following event study model based on a DD estimator:

$$(1) \qquad Y_{i,s,t} = \delta_t + \delta_s + \pi \cdot X_{i,s,t} + \sum_{\tau=-5}^{3} \beta_\tau \cdot P_s \cdot \mathbf{1}(t - T_s = \tau) + \varepsilon_{i,s,t},$$

where $Y_{i,s,t}$ is an indicator variable set to 1 if individual i in state s in period t has registered or voted; δ_t denote year fixed effects and are meant to control for time shocks, while state fixed effects, denoted by δ_s, are meant to account for unobserved state characteristics; $X_{i,s,t}$ is a vector of time-varying individual characteristics; and $\varepsilon_{i,s,t}$ is the error term which we cluster by state since treatments vary at the state-year level.

We define event time in terms of election years, which occur every even year. The treatment variable is constructed by interacting the indicator variable P_s, which is set to 1 if state s has ever implemented preregistration, with the event-time dummy $\mathbf{1}(t - T_s = \tau)$, which is set to 1 if the observation time is $\tau = -5, \ldots, 0, \ldots, 3$ election years from T_s, the year of the first election after treatment initiation in state s. Observations more than 5 elections before or more than 3 elections after T_s are captured by $\mathbf{1}(t - T_s = -5)$ and $\mathbf{1}(t - T_s = 3)$, respectively. The year of the last election held before the treatment initiation $(\tau = -1)$ is the omitted election year.

Figure 1 plots the estimation results for the β_τs and the corresponding 95 percent confidence intervals using a specification of regression (1) that includes the state-specific time trends $\delta_s \cdot t$ as controls to capture differences in the trends of state-level voter participation. The x-axis measures the election window around the treatment initiation while the y-axis measures the estimated impact of the treatment on voter registration. Each dot represents the average registration rates for the young (panel A) and the old (panel B) in the treated and untreated states in a particular election relative to the same outcome in the election prior to treatment. As one would expect, the dynamic pattern exhibits an increase of registration outcomes for the young after treatment initiation (although it is not statistically significant at the 5 percent level) with a zero (placebo) effect for the old. Panels A and B of online Appendix Figure A2 repeat variants of regression (1) with voting outcomes for the young and the old, respectively, as the dependent variable. The dynamic pattern is similar to that in Figure 1.[30]

A potential shortcoming of regression (1) is that it does not allow for the inclusion of a full set of state-year interactions as controls, which can account for factors that may affect the political participation of individuals of all ages within a state in a particular election, such as, for example, a transitory increase in statewide electoral mobilization in close elections. To overcome this issue and at the same time increase the power of the estimates, we combine the DD models for the two age groups of

[30] Online Appendix Figure A2 also shows that the results are robust to dropping state-specific linear trends for both registration and voting outcomes of the young (panels C and E) and the old (panels D and F).

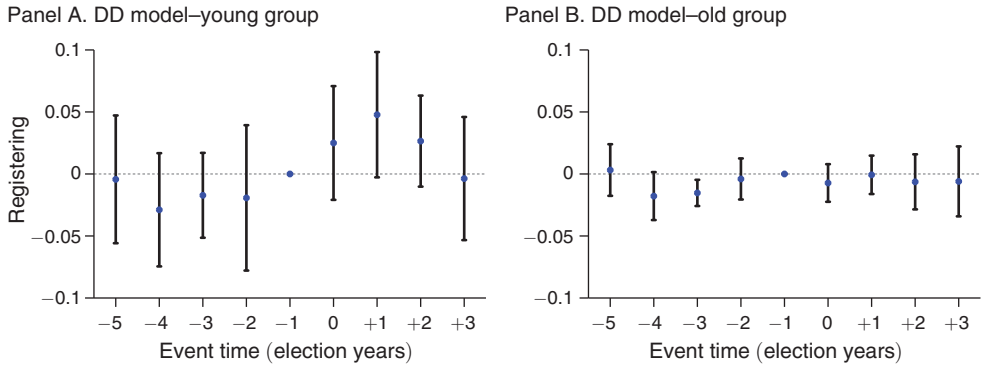

FIGURE 1. PREREGISTRATION AND POLITICAL PARTICIPATION OF YOUNG AND OLD VOTERS

Notes: The dependent variables are Registering for individuals aged 18–24 (panel A) and Registering for individuals aged above 24 (panel B). The coefficients are least-squares estimates of the β_τs with $-5 \leq \tau \leq 3$ in a specification of regression (1) that includes state-specific time trends. All specifications include state fixed effects, year fixed effects, and respondents' characteristics (dummies for gender, black, Hispanic, educational attainment, family income, labor force status, metropolitan city status, and self-respondent). Vertical lines represent 95 percent confidence intervals based on standard errors clustered at the state level. The unit of observation is at the individual level. The samples consist of 163,879 young individuals and 1,186,658 old individuals from all 50 states, plus the District of Columbia, who report whether they have registered over the period 1980–2014. Event time is defined in (biennial) election years and tracks the election window around $\tau = 0$, the year of the first election after treatment initiation. The omitted election year is $\tau = -1$. See online Appendix F for details on data sources and variable definitions.

voters and develop a triple-difference (hereafter DDD) regression design, using old voters as placebo.

Formally, the empirical model to be tested is as follows:

$$(2) \qquad Y_{i,a,s,t} = \delta_{s,t} + \delta_{a,t} + \delta_{a,s} + \pi \cdot X_{i,a,s,t} + \mathbf{1}(18 \leq a \leq 24) \cdot \sum_{\tau=-5}^{3} \beta_\tau \cdot P_s \cdot \mathbf{1}(t - T_s = \tau) + \varepsilon_{i,a,s,t},$$

where $Y_{i,a,s,t}$ is an indicator variable set to 1 if individual i belonging to age group a in state s in period t has registered or voted; $\delta_{s,t}$ denote state-by-year fixed effects and are meant to non-parametrically control for state-specific shocks over time; $\delta_{a,t}$ and $\delta_{a,s}$ include the full set of interactions between age-group fixed effects and time and state fixed effects and are meant to capture changes over time among the young nationwide and time-invariant characteristics of the young in the reform states, respectively; and $\varepsilon_{i,a,s,t}$ is the error term that we cluster by state. The treatment variable is constructed here by interacting $P_s \cdot \mathbf{1}(t - T_s = \tau)$ with the age-group dummy $\mathbf{1}(18 \leq a \leq 24)$, which is set to 1 if the respondent belongs to the young group. The identification assumption for consistency of the estimates now relies on the absence of shocks that differentially affect the political participation of the young only in the preregistration states during the sample period.

Figure 2 plots the estimation results for the β_τs and the corresponding 95 percent confidence intervals using a baseline specification of regression (2), which excludes individual characteristics as controls. Each dot represents the average registration (panel A) and voting gap (panel B) between young and old in the treated and

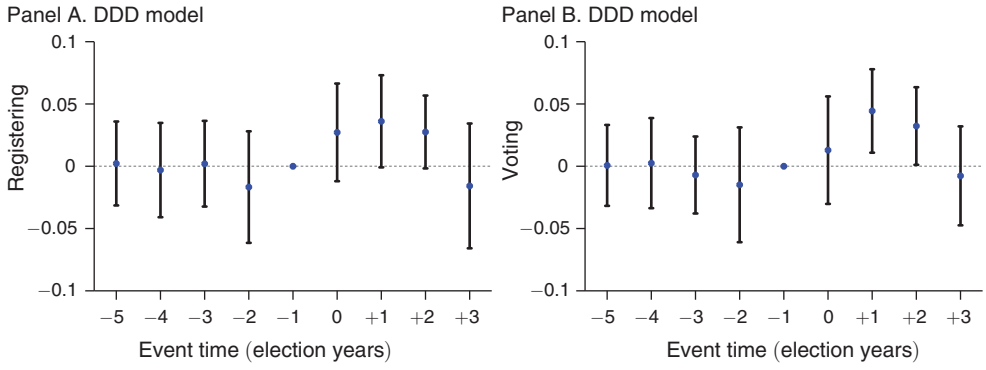

FIGURE 2. PREREGISTRATION AND POLITICAL PARTICIPATION OF YOUNG RELATIVE TO OLD VOTERS

Notes: The dependent variables are Registering (panel A) and Voting (panel B). The coefficients are least-squares estimates of the β_τs with $-5 \leq \tau \leq 3$ in a specification of regression (2) that excludes $X_{i,a,s,t}$ as controls. All specifications include state-by-year fixed effects, age-group-by-year fixed effects, and age-group-by-state fixed effects. Vertical lines represent 95 percent confidence intervals based on standard errors clustered at the state level. The unit of observation is at the individual level. The sample consists of 1,350,537 (1,358,545) individuals from all 50 states, plus the District of Columbia, who report whether they have registered (voted) over the period 1980–2014. Event time is defined in (biennial) election years and tracks the election window around $\tau = 0$, the year of the first election after treatment initiation. The omitted election year is $\tau = -1$. See online Appendix F for details on data sources and variable definitions.

untreated states in a particular election relative to the same gap in the election prior to treatment. As the pre-treatment dots indicate, the differential trends in the outcome of interest are statistically indistinguishable from one another in the election years leading up to the passage of the reform, which lends plausibility to the model's identifying assumption. The graphs also allow us to rule out unusual patterns of outcomes in the election years preceding the implementation of the reform, such as an Ashenfelter Dip originating from mean reversion at the onset of the preregistration reform, since the estimated coefficients remain almost unchanged during the pre-treatment period. After treatment initiation, we instead observe a significant change in political participation, with a reduction of the registration and voting gap between young and old voters, which is consistent with the DD estimates. The pattern of voter registration is remarkably similar to that of voter turnout, apart from the fact that the decline in the voter turnout gap is less pronounced in the first election after treatment and becomes larger in the second and third elections after treatment. A delay in the reform's effect is reasonable in this context, since preregistration is a provision targeted at 16- and 17-year-olds who become eligible voters and therefore are effectively exposed to the treatment more than a year after the implementation of the law.

Online Appendix Figures A3 and A4 report robustness checks for registration and voting respectively under the preferred event studies specification, which is based on the DDD estimator as follows: (i) including respondents' characteristics as controls (panel A) in order to address concerns of omitted variable bias; (ii) keeping only eventually treated states in the sample (panel B), thereby relying only on variation in the exact time of the law's passage; (iii) balancing the sample such that the treated states that implemented later in time and have less than two post-treatment

elections are excluded (panel C), since an unbalanced sample might pick up demographic changes from states exiting the event window; (iv) sequentially excluding each group of states that implemented preregistration in the same year (panel D–H), in order to check whether our findings are driven by only a few states; and (v) adding a set of interactions between event time and age-group dummies with indicators for EDR and online registration (panel I), in order to control for other state policies that may potentially impact on the political participation of the young and are contemporaneous with preregistration. Reassuringly, across all specifications, the magnitudes and standard errors remain similar, confirming the pattern in Figure 2.[31]

Table 1 summarizes the magnitude and the statistical significance of the DDD event study estimates for both voter registration and turnout. We refer to columns 1 and 4 for the baseline specification and to columns 2 and 5 for the baseline specification augmented with the full set of respondents' characteristics as controls. For the sake of brevity, even though the underlying model includes the pre-event interaction terms, we display only the β_τs for $\tau \geq 0$. Inspecting columns 2 and 5 reveals that the registration and voting gaps between young and old voters in treated states in the first post-treatment election decline by 2.7 and 1.4 percentage points, respectively. The initial effect is followed by an even larger reduction of 4 and 5.1 percentage points in the second post-treatment election, and 2.4 and 2.9 percentage points in the third. The fact that the effect lasts up to three elections is partly explained by the presence in the sample of a few treated states with such a long post-treatment exposure. In columns 3 and 6, we finally estimate the average changes in the outcomes following the event, controlling again for respondents' characteristics. To identify the post-treatment time, we estimate a specification of regression (2) that replaces $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$, an indicator variable set to 1 if individual i is resident in a state s that implements preregistration at some point and responds in any election year t after (and including) T_s. Hence, the treatment effect is captured here by the coefficient of the triple interaction term $\mathbf{1}(18 \leq a \leq 24) \cdot P_s \cdot \mathbf{1}(t \geq T_s)$. The estimated coefficients indicate that the implementation of preregistration reduces the voter registration and turnout gaps by 9.7 percent and 7.4 percent respectively on average, relative to the pre-treatment means, in states with preregistration compared to states without preregistration.[32]

To conclude, the analysis confirms that preregistration accomplishes the intended task of boosting the political involvement of young citizens by increasing their actual participation at the polls, with a consequent reduction in the registration and voting gap vis-à-vis old voters. In the next section, we turn to the main research question—whether this de facto enfranchisement episode is accompanied by an increase in public resources allocated to the newly enfranchised constituency.

[31] We also test for the sensitivity of the results to small changes in the first election after treatment initiation since in a few states preregistration is implemented shortly before the upcoming election. For example, in Delaware the law was introduced on September 8, 2010, while the first post-treatment election was on November 2, 2010. There is little effect on the results. These additional estimates are not reported for the sake of brevity.

[32] The estimation results are consistent with those obtained by Holbein and Hillygus (2016), who find a positive impact of about 8 percent for preregistration on the turnout of young voters in Florida.

Table 1—Preregistration and Political Participation of Young Relative to Old Voters

	Registering			Voting		
	(1)	(2)	(3)	(4)	(5)	(6)
Age-group indicator × P_s indicator × indicator for:						
$\tau = 0$	0.027	0.027		0.013	0.014	
	(0.020)	(0.019)		(0.021)	(0.022)	
$\tau = 1$	0.036	0.040		0.044	0.051	
	(0.018)	(0.019)		(0.017)	(0.018)	
$\tau = 2$	0.027	0.024		0.032	0.029	
	(0.015)	(0.014)		(0.015)	(0.016)	
$\tau = 3$	−0.016	−0.019		−0.008	−0.010	
	(0.025)	(0.023)		(0.020)	(0.020)	
Age-group indicator × P_s indicator × indicator for $t \geq T_s$			0.023			0.021
			(0.009)			(0.008)
State-by-year fixed effects	✓	✓	✓	✓	✓	✓
Age-group-by-year fixed effects	✓	✓	✓	✓	✓	✓
Age-group-by-state fixed effects	✓	✓	✓	✓	✓	✓
Individual controls		✓	✓		✓	✓
Mean at omitted time	0.183	0.183	0.238	0.230	0.230	0.285
R^2	0.051	0.118	0.118	0.082	0.156	0.156
Observations	1,350,537	1,350,537	1,350,537	1,358,545	1,358,545	1,358,545

Notes: State-level clustered standard errors are in parentheses. The dependent variables are Registering (columns 1–3) and Voting (columns 4–6). The coefficients are least-squares estimates of the β_τs with $-5 \leq \tau \leq 3$ in specifications that deviate from regression (2) as follows: columns 2, 3, 5, and 6 add respondents' characteristics (dummies for gender, black, Hispanic, educational attainment, family income, labor force status, metropolitan city status, and self-respondent); and columns 3 and 6 replace $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$ from columns 2 and 5. All specifications include state-by-year fixed effects, age-group-by-state fixed effects, and age-group-by-year fixed effects. The mean in the omitted time is averaged registering and voting gaps at $\tau = -1$ in columns 1, 2, 4, and 5, and at $t < T_s$ in columns 3 and 6. See the note to Figure 2 for details on sample size and estimation strategy and online Appendix F for details on data sources and variable definitions.

V. Political Responsiveness

In this section, we test the link between preregistration and government spending with particular focus on higher education expenditure, the type of policy for which the young have the strongest preference. To do so, we take advantage of the variation in treatment dates among states in a DD regression design. The identifying assumption underlying this approach is that unobserved state characteristics, which might have affected fiscal policies chosen by governors, are uncorrelated with the timing of preregistration. The fact that the timing of preregistration varies quite significantly across treated states and that governors exert limited authority over preregistration lawmaking lend plausibility to the identifying assumption. Nonetheless, the possibility that state-level reforms respond to state-specific dynamics remains a valid concern. To further investigate this issue, we show in online Appendix Table A1 that a large number of state characteristics fail to predict the timing of preregistration enactment. In addition, we show in online Appendix Table A2 that the same set of state characteristics fails to be predicted by preregistration. Exceptions

are personal income and the unemployment rate; however, this is not surprising since the majority of reform states implemented preregistration starting from 2007. Hence, an important factor contemporaneous to preregistration was the 2008 financial crisis, which had adverse and regionally diverse effects on per capita income and, in turn, on higher education expenditure. To account for these potential threats to internal validity, we include the logarithm of per capita income in our baseline specification.[33]

We directly test for the absence of differential pre-treatment trends in the outcome of interest between states with preregistration and states without by estimating the following event study model based on a DD estimator:

$$\ln(Y_{s,t}) = \delta_t + \delta_s + \pi \cdot X_{s,t} + \sum_{\tau=-10}^{4} \beta_\tau \cdot P_s \cdot \mathbf{1}(t - T_s = \tau) + \varepsilon_{s,t}, \tag{3}$$

where $Y_{s,t}$ is the per capita current higher education expenditure in state s in year t; δ_t and δ_s denote year and state fixed effects; $X_{s,t}$ is a vector of time-varying state characteristics; and $\varepsilon_{s,t}$ is the error term which we cluster by state to capture serial correlation within states.

Since data are annual, event time is defined here in terms of fiscal years. As previously defined, the treatment variable is constructed by interacting the indicator variable P_s with the event-time dummy $\mathbf{1}(t - T_s = \tau)$, where $\tau = -10, \ldots, 0, \ldots, 4$ and T_s is the year of the preregistration initiation in state s. We omit the fiscal year before the treatment initiation $(\tau = -1)$ and censor the endpoints of the event-time window, including an indicator for up to 10 fiscal years before and 4 fiscal years after treatment initiation. Using regression (3), fixed differences across states, common shocks varying nonlinearly over time, and observable confounding variables are all removed from the estimated effect of preregistration. As a result, the β_τs should capture trend breaks in the outcomes of interest that coincide precisely with the timing of preregistration initiation.

Estimation results for regression (3) and the associated 95 percent confidence intervals are shown in Figure 3. The x-axis measures the fiscal-year window around the treatment initiation while the y-axis measures the estimated impact of the treatment on higher education expenditure. Each dot then represents the average difference in higher education expenditure between treated and untreated states in a particular fiscal year relative to the same difference in the fiscal year prior to treatment. In panel A, we begin by estimating a baseline version of regression (3), which includes only the logarithm of per capita income in the vector $X_{s,t}$. The panel shows no differential trends in the outcome variable prior to the events. This suggests that higher education expenditure trends would have been the same in all states in the absence of the treatment. Following the reform, higher education ceases to trend similarly. In fact, the series begins trending noticeably upward starting from the second post-event fiscal year. Although there are no preexisting differential trends, one potential shortcoming of the baseline regression is that it does not allow for state-specific time trends. These might help in capturing omitted factors that may bias

[33] The unemployment rate is initially not included since it failed to predict higher education expenditure. Nonetheless, for completeness we include it as a regressor in a more saturated version of the model.

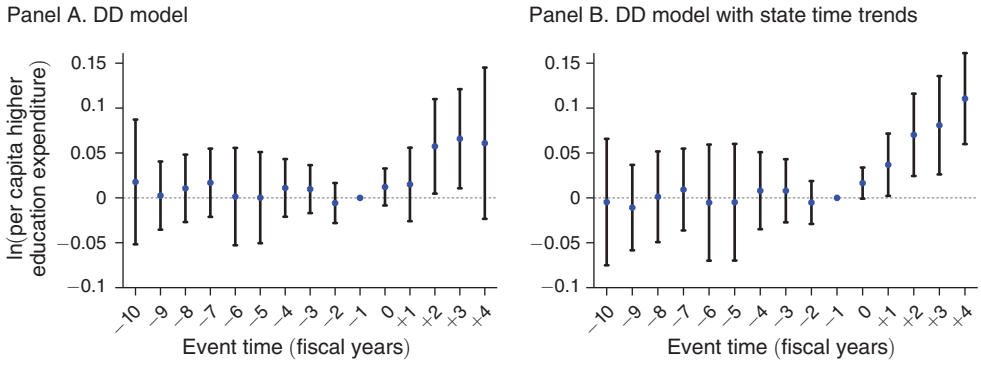

Figure 3. Preregistration and Higher Education Expenditure

Notes: The dependent variable is per capita current higher education expenditure. All regressions assign equal weight to each state and year. The coefficients are least-squares estimates of the β_τs with $-10 \leq \tau \leq 4$ in a specification of regression (3) that adds ln(per capita income) as a control in panel A and also state-specific time trends in panel B. All specifications include year fixed effects and state fixed effects. Vertical lines represent 95 percent confidence intervals based on standard errors clustered at the state level. The unit of observation is at the state level. The sample includes all 50 states over the period 1980–2014. Event time is defined in fiscal years and tracks the fiscal window around $\tau = 0$, the first fiscal year post-treatment initiation. The omitted fiscal year is $\tau = -1$. See online Appendix F for details on data sources and variable definitions.

the estimates when $\tau \geq 0$. Panel B in Figure 3 displays coefficients from an event study regression which also includes $\delta_s \cdot t$. As one would expect, the dynamic pattern is similar to the one reported in panel A prior to the events. In the post-treatment period, however, the positive impact of preregistration on higher education spending has a lag of one year and lasts through the end of the sample. Overall, the findings of Figure 3 suggest a large treatment effect even when accounting for unobserved trends in, for example, political activism and youth mobilization.

In online Appendix Figure A5, we perform robustness analyses that build on the preferred event study specification, which includes state-specific time trends. In a more flexible specification, we saturate the model by adding time-varying state confounders that reflect socioeconomic characteristics (panel A), political attributes (panel B), fiscal factors (panel C), other registration reforms implemented in the same period of preregistration, such as NVRA, EDR, and online registration (panel D), as well as all covariates simultaneously (panel E). The consistent finding across all specifications is that education expenditure increases after preregistration laws are enacted. The estimates are also robust to the inclusion of region-by-year fixed effects (panel F). Dropping the never treated states from the estimation sample produces very similar results (panel G), as does balancing the sample by considering only treated states with more than four post-treatment fiscal years (panel H). These results suggest that the effect on higher education expenditure is identified mainly from variation in the timing of preregistration reform among the states that implemented it.[34]

[34] In line with the analysis carried out in Section IV, panels I–O of online Appendix Figure A5 report event studies for robustness checks in which we sequentially exclude each group of states implementing the reform in the same year. Results are robust across all the different specifications. This suggests that the results capture a general relationship between preregistration provisions and fiscal policy outcomes, rather than the influence of only a small group of states.

Table 2—Preregistration and Higher Education Expenditure

	ln(per capita higher education expenditure)				
	(1)	(2)	(3)	(4)	(5)
P_s indicator × indicator for:					
$\tau = 0$	0.012	0.016	0.018	0.022	
	(0.010)	(0.009)	(0.009)	(0.010)	
$\tau = 1$	0.015	0.037	0.038	0.039	
	(0.020)	(0.017)	(0.015)	(0.017)	
$\tau = 2$	0.057	0.070	0.071	0.064	
	(0.026)	(0.023)	(0.022)	(0.022)	
$\tau = 3$	0.066	0.081	0.085	0.078	
	(0.027)	(0.027)	(0.027)	(0.028)	
$\tau = 4$	0.061	0.111	0.114	0.106	
	(0.042)	(0.025)	(0.025)	(0.027)	
P_s indicator × indicator for $t \geq T_s$					0.051
					(0.021)
State fixed effects	✓	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓	✓
State time trends		✓	✓	✓	✓
Other reforms			✓	✓	✓
State controls				✓	✓
R^2	0.935	0.972	0.972	0.975	0.975
Observations	1,750	1,750	1,750	1,750	1,750

Notes: State-level clustered standard errors are in parentheses. The dependent variable is per capita current higher education expenditure. The coefficients are least-squares estimates of the β_τs with $-10 \leq \tau \leq 4$ in specifications that deviate from regression (3) as follows: column 2 adds state-specific time trends; column 3 adds other registration reforms (dummies for NRVA, EDR, and online registration); column 4 adds state controls, including socioeconomic variables (population, median age, share of 16–25 age group, post-secondary enrollment, share of blacks, share of whites, inequality, and unemployment rate), political variables (dummies for gubernatorial election year, incumbent, year of term, governor runs in next election, governor not eligible to run again, Democratic governor, previous-term Democratic governor, political competition, and gubernatorial turnout rate), and fiscal variables (total expenditure, share of current expenditure, and total taxes); and column 5 replaces $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$ from column 4. All regressions include year fixed effects, state fixed effects, and ln(per capita income). See the note to Figure 3 for details on sample size and estimation strategy and online Appendix F for details on data sources and variable definitions.

To provide an idea of magnitudes, Table 2 reports estimates of the event study coefficients that were shown graphically. All models include state and year fixed effects and, for the sake of brevity, only display coefficients for $\tau \geq 0$. Because the dependent variable is in logarithmic form, the coefficient estimates can be interpreted roughly as percentage changes. Column 1 reports the estimates of the baseline specification. Column 2 adds state-specific time trends as controls. Columns 3 and 4 sequentially include other registration reforms and state-specific characteristics as additional regressors. The magnitudes and standard errors are relatively stable across specifications but somewhat larger once we control for state-specific characteristics. The impact of preregistration is stronger two years after treatment initiation. The dynamic effect therefore mirrors that of voter participation, as illustrated in Section IV. Since gubernatorial elections are held in reform states on average two years after the law's passage and in most cases are won by non-incumbents, the timing of the effects suggests that the decisions to approve preregistration and

the choice of how much to spend on education are unlikely to be codetermined, which provides further confirmation of the identifying assumption underlying the empirical strategy.[35]

Finally, column 5 presents estimates of the most saturated specification after replacing $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$ in order to evaluate the average post-treatment effect of preregistration on per capita higher education expenditure. We find a statistically significant increase of 5.1 percent in the outcome of interest. At the (pre-treatment) mean of around \$492.72, this corresponds to an increase of about \$25.13 per capita in higher education expenditure in those states that, at some point in time, adopted preregistration. To finance an equivalent increase by means of the income tax would require an average increase of 0.1 percentage points in the income tax rate, evaluated at the (pre-treatment) mean income of \$24,956.

We have so far focused on higher education expenditure since it directly affects the prospects of young soon-to-become voters. Since they have already graduated from high school or are near to doing so, we expect preregistration to have no effect on state spending on primary and secondary education. Panel A in online Appendix Figure A6 plots event study coefficients and confidence intervals from the estimation of regression (3) in its most saturated version, where we replace higher education expenditure with elementary and secondary education expenditure as the dependent variable. Reassuringly, the estimates confirm the zero (placebo) effect of preregistration on government school spending.[36]

Finally, we check the potential impact of preregistration on other categories of fiscal spending. To accomplish this, panels E–O of online Appendix Figure A6 repeat variants of regression (3) with all categories of current government expenditures other than higher education and with general revenue as the dependent variable. We detect variations of negative sign in the spending on employee retirement and financial administration. The former includes expenditures for which the young do not have a strong preference, while the latter reflects government spending overhead. Taken together, these results are consistent with governments becoming more accountable to the young, and the additional spending on higher education occurring at the expense of other fiscal categories.

A. *Heterogeneity*

The above results indicate that preregistration shifts government spending toward higher education. Guided by the theoretical predictions discussed in Section II, this section goes on to explore the nonlinear effects of preregistration on higher education expenditure by grouping reform states on the basis of variables capturing political competition and the demographics of the population. As a proxy for political competition, we utilize the electoral margin of victory. Smaller values of this variable correspond to gubernatorial elections with stiffer political competition. The

[35] Among the reform states, a non-incumbent candidate won the first post-treatment election in California, Florida, Hawaii, Louisiana, Massachusetts, North Carolina, Oregon, and Rhode Island.

[36] Panels B–D in online Appendix Figure A6 show zero (placebo) effects also on variables related to the young that should not be affected by the reform, such as the share of the young in population, educational attainment, and youth unemployment.

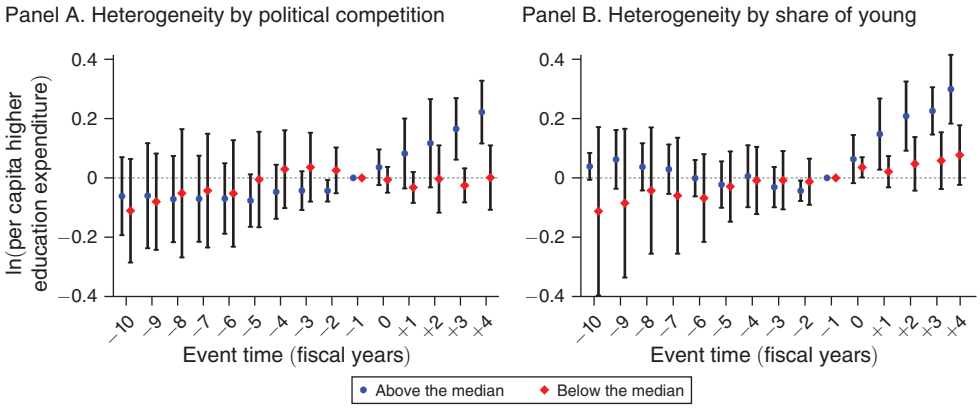

Figure 4. Preregistration and Higher Education Expenditure—Heterogeneity

Notes: The dependent variable is per capita current higher education expenditure. The coefficients are least-squares estimates of the β_τs with $-10 \leq \tau \leq 4$ in a specification of regression (3) that includes state-specific time trends and all covariates listed in the note to Table 2, other than political competition in panel A and the share of the 16–25 age group in panel B. All specifications include year fixed effects, state fixed effects, and state-specific time trends. Vertical lines represent 95 percent confidence intervals based on standard errors clustered at the state level. Specifications in panel A use eventually treated states where the initial level of the electoral margin of victory is either above or below the sample median (represented by circle and diamond dots, respectively). Specifications in panel B use eventually treated states where the initial share of the 16–25 age group is either above or below the sample median (represented by circle and diamond dots, respectively). The initial level of electoral margin of victory or share of the 16–25 age group is averaged over event time $-10 \leq \tau \leq -1$ while the sample median is the median of the initial level of electoral margin of victory or share of the 16–25 age group. See online Appendix F for details on data sources and variable definitions.

demographics of the population is captured by the share of the 16–25 age group in the population. In order to check for heterogeneity, we split the set of states that have adopted preregistration into two subsets, according to whether political competition and the share of the young are above or below the median in the pre-treatment period.

Figure 4 presents the estimation results and corresponding 95 percent confidence intervals for the event study regression based on (3).[37] The pre-treatment results show no evidence of differential trends in higher education expenditure in the fiscal years leading up to the adoption of preregistration. We observe that after treatment initiation reform states with weaker political competition (panel A) and a larger share of young individuals (panel B) show greater responsiveness to the introduction of preregistration. This is consistent with the hypothesis that voters affect policies, as rationalized in online Appendix E.

B. *Higher Education Institution-Level Analysis*

The state-level analysis employs higher education expenditure as a measure of state financial support for higher education. As an alternative measure, we now adopt state appropriation, that is, state funds actually *received* by higher education

[37] Given the small number of clusters, we check the robustness of the estimates using a wild bootstrap technique. The *p*-values found using the clustered standard errors and the wild bootstrap procedure are very similar.

institutions. The focus on institution-level data plays a key role by validating the state-level figures and therefore making it possible to test whether predictions for the provider of funding (i.e., a state) are mirrored by those for the recipient of that funding (i.e., a higher education institution).

As a starting point, we compare state-level and higher education institution-level data. To do so, we first aggregate state appropriations of all higher education institutions by state and year and then use the resulting aggregate measure as the dependent variable in the estimation of regression (3), with the goal of evaluating the average treatment effect of preregistration. Column 1 in online Appendix Table A3 reports the results. At the (pre-treatment) mean of $210.64, the increase in the outcome of interest of 12.2 percent corresponds to an increase of about $25.69 per capita in state appropriation in states that have adopted preregistration at some point in time. The magnitude of the effect is remarkably close to the estimates shown in column 5 in Table 2 with higher education expenditure as the main outcome.

The allocation of funds to higher education institutions is of course highly heterogeneous. Indeed, a myriad of time-varying spatial heterogeneous factors, such as local shocks to the demand and supply of education other than preregistration, may affect the distribution of funding (see Goldin and Katz 1999). Thus, the use of an empirical strategy that exploits all cross-state variation may be subject to omitted variable bias. In order to mitigate this bias, we take advantage of the disaggregated nature of IPEDS data and focus on a comparison of higher education institutions between contiguous counties that straddle a common state border. By using only variation in preregistration reform within US border-county pairs, we are able to exploit policy discontinuities at state borders and identify the effect of preregistration. This is an effective strategy because underlying economic fundamentals are expected to evolve in a more similar manner in contiguous counties than across states or randomly paired counties.[38]

Formally, the empirical model to be tested is as follows:

$$\ln\left(Y_{i,p,t}\right) = \delta_c + \delta_{p,t} + \pi \cdot X_{i,p,t} + \bar{\pi} \cdot Z_{\bar{\imath},p(c),t} + \sum_{\tau=-5}^{4} \beta_\tau \cdot P_s \cdot \mathbf{1}\left(t - T_s = \tau\right) + \varepsilon_{i,p,t}\,, \tag{4}$$

where $Y_{i,p,t}$ is state appropriation per FTE enrollment of higher education institution i in border-county pair p in year t. The key to identification in this approach is the border-county pair-year fixed effects, denoted as $\delta_{p,t}$. This term captures all possible spatially distributed yearly shocks that may jointly affect contiguous higher education institutions located in a border-county pair, such as the cross-border movements of students or spontaneous student activism. We also include county fixed effects denoted as δ_c in order to capture permanent unobserved county characteristics. Since counties can belong to multiple border-county pairs, which may induce

[38] The identification strategy is based on the assumption that higher education institutions in neighboring counties are more similar than two randomly chosen institutions due to the presence of cross-border spillovers and competition effects, which make them subject to similar shocks (see Dube, Lester, and Reich 2010).

a mechanical correlation in the unobservables across pairs and potentially along an entire border segment, we cluster the error term $\varepsilon_{i,p,t}$ by state and border segment.

The units of analysis are higher education institutions rather than counties. Thus, we include $X_{i,p,t}$, a vector of time-varying characteristics for higher education institution i in border-county pair p, to control for the potential confounding effects of higher education institution characteristics. Moreover, we also include $Z_{\bar{\imath},p(c),t}$, a vector of average time-varying characteristics of higher education institutions $\bar{\imath}$ located in a county adjacent to c in border-county pair p, denoted as $p(c)$. It is meant to control for local shocks that might affect the neighboring higher education institutions in the contiguous counties. Assuming that the allocation of education funding depends on, for example, the quality of the higher education institution, the demand for education, or the number of students, failure to control for them may lead to biased estimates if there is a shock in the neighboring higher education institution that affects one of these variables.

This rich set of controls should ensure that the β_τs capture the effect of the treatment variable (as previously defined) on the distribution of state funds actually received by higher education institutions, since they reflect only the within-pair variation in preregistration adoption between border-county pairs over time.[39] Furthermore, and as highlighted in Section IA, the fact that preregistration changes are exogenous from the point of view of an individual higher education institution—whose allocated funds are in many states managed by an independent state agency—makes it relatively straightforward to identify the effects of the voting reform.

Estimation results for regression (4) and the associated 95 percent confidence intervals are shown in panel A of Figure 5. The x-axis is the academic-year window around the treatment initiation while the y-axis measures the estimated impact of the treatment on per FTE state appropriation. Accordingly, each dot represents the average difference in the outcome of interest between treated and untreated higher education institutions in a border-county pair in a particular academic year relative to the same difference in the academic year prior to treatment. The panel does not indicate any differential trends in the outcome variable prior to the year of preregistration adoption. Following the adoption of preregistration, the series begins trending noticeably upward. The dynamic pattern of the effects is strikingly similar to that of Figure 3.[40] Furthermore, the average post-treatment increase of 18.8 percent reported in column 3 in online Appendix Table A3 is consistent with the increase estimated using the DD strategy in column 2 in online Appendix Table A3, in view of a likely downward bias due to omitted variables. We have therefore confirmed by means of an alternative identification strategy and a different sample that state financial support for higher education increases after the introduction of preregistration.

We now check the robustness of the institution-level estimates by constructing two placebo specifications. First, we randomly assign placebo preregistration laws to counties and match each state-border county with all other counties lying on the

[39] Given the ten-year data span, the event window runs from $\tau = -5$ years before T_s, the year of preregistration adoption in state s, to $\tau = 4$ years after.

[40] In online Appendix Figure A7, we show that results are robust to: (i) dropping institution-level covariates (panel A); (ii) adding region-by-year fixed effects (panel B); (iii) adding county-level characteristics (panel C); and (iv) adding all covariates simultaneously (panel D).

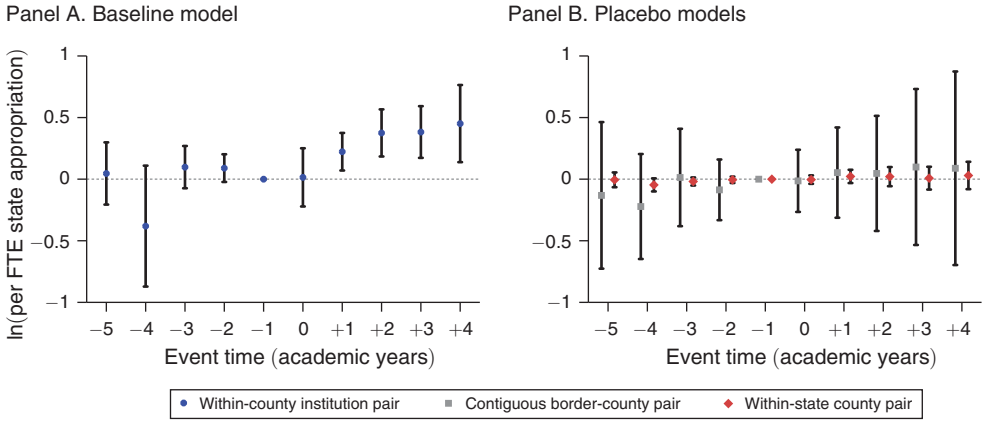

FIGURE 5. PREREGISTRATION AND STATE APPROPRIATION

Notes: The dependent variable is per FTE state appropriation. The coefficients are least-squares estimates of the β_τs with $-5 \leq \tau \leq 4$ from various specifications of regression (4). The vertical lines represent 95 percent confidence intervals. The unit of observation is higher education institutions and the sample period is 2005–2014. The specification in panel A employs a border-county pairs sample of 1,059 higher education institutions located in 336 counties and 255 border-county pairs. It adds border-county pair-year fixed effects and county fixed effects and clusters the standard errors by state and border segment. The specification in panel B, whose estimates are represented by diamond dots, employs a within-state county pairs sample of 1,308 higher education institutions located in 470 counties and 2,686 border-county pairs. It adds border-county pair-year fixed effects and county fixed effects and clusters the standard errors by county. The specification in panel B, whose estimates are represented by square dots, employs a within-county institution pairs sample of 2,826 higher education institutions forming 16,630 institution pairs. It adds higher education institution pair-year fixed effects and clusters the standard errors by county. All the specifications control for Carnegie classification, institutional level and sector, flagship, has hospital, and percent of fall cohort. Event time is defined in academic years and tracks the academic window around $\tau = 0$, the first academic year following treatment initiation. The omitted academic year is $\tau = -1$. See online Appendix F for details on data sources and variable definitions.

border within the same state. Second, we randomly assign placebo preregistration laws to higher education institutions and group them into pairs within the same county. The rationale behind these specifications is that in the absence of local shocks at state boundaries or at the institution level we should observe no difference in the impact of preregistration on state appropriations between a pair of counties within the same state or between a pair of higher education institutions within the same county. Panel B of Figure 5 presents the event study results of both specifications. Reassuringly, the estimates show a zero (placebo) effect for the treatment, confirming that the actual timing of preregistration is central to the inference we draw.

VI. Magnitude of the Effects

The empirical results paint a consistent picture, according to which preregistration leads to a shift in electoral composition toward greater representation of the young and in the distribution of state transfers toward the type of expenditure for which the newly enfranchised constituent group has the strongest preference—that is, financial support for higher education. While the overall pattern of effects suggests a causal chain linking the results, care must be taken in relying on them to

compute implied elasticities. Indeed, a number of complementary channels activated by registration reform may be operating at the same time.

Together with preregistration, some states signed bills into law to promote follow-up voter education programs in order to increase civic engagement among the young and to leverage the success of the reform.[41] Hence, preregistration may help the young become better informed about political issues and in turn encourage them to become politically active beyond just voting—for instance, by working in electoral campaigns, running for office themselves, or supporting lobbies for higher education.[42] Better-informed teens are also more likely to share electoral knowledge and opinions within a household, whose members may have similar preferences for education policy. Preregistration may then raise parental turnout, especially among those with children of college age, through peer pressure and in turn further incentivize politicians to pursue youth-targeted policies.[43] Finally, it has been claimed that first-time voters form voting-behavior habits that persist later in life.[44] In this context, preregistration may create an even greater incentive for politicians to attract young first-time voters who are likely to vote for them in the future as well.

With this caveat in mind, we now combine the results from Sections IV and V in a manner consistent with the mechanism highlighted in Section II and interpret the increase in spending on higher education in light of the increase in youth registration and voter turnout following the adoption of preregistration. This approach is in the spirit of Instrumental Variables (IV) estimation, even though the earlier discussion warned of potential violations of the exclusion restriction required for a strict interpretation of the results as causal. Therefore, the aim of the present section is to set ideas and offer a scaling of the coefficients.

We start with a "first-stage" estimation in order to demonstrate the effect of preregistration on youth enfranchisement. To do so, we first collapse individual-level registration and voting records for the 18–24 age group by state and year. We then use the resulting registration and voter turnout data as the dependent variables of a specification of regression (1) that replaces $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$ in order to evaluate the average treatment effect of preregistration. According to the results presented in panel A of Table 3, the estimated effect of preregistration on registration of young voters (column 1) is 3.5 and on young voter turnout (column 2) is 2.5 percentage points. It is worth noting that these estimates are consistent with those in Section IV, thus confirming the robustness of the results. To put the findings into perspective, multiplying the (pre-treatment) mean of 802,304 individuals aged 18–24 by the estimated increase in voter turnout of 2.5 percentage points shows

[41] In California, for example, Assembly Bill 700, 2013 and Assembly Bill 1817, 2014 provide channels through which communities and advocates can become involved in the schools.

[42] Among others, Tandberg and Griffith (2013) show that more intense lobbying tends to have a positive effect on state higher education budgets.

[43] DellaVigna et al. (2017) provide an estimate of the value of voting and a welfare evaluation of a get-out-the-vote campaign. They demonstrate that an important incentive for citizens to vote is to be able to show others that they have voted.

[44] As observed by Strate et al. (1989), the accumulation of political experience that comes with age leads to increasing levels of civic competence and voting participation. Fujiwara, Meng, and Vogl (2016) demonstrate that voting in a particular election has a positive effect on the probability of voting in subsequent ones.

Table 3—Preregistration and the Magnitude of the Effects

	Young registering (1)	Young voting (2)
Panel A. First stage		
P_s indicator		
$\times$ indicator for $t \geq T_s$	0.035	0.025
	(0.014)	(0.011)
Mean at omitted time	0.574	0.377
R^2	0.774	0.861
Panel B. Magnitudes		
Change in higher education expenditure	5.10%	5.10%
Change in electoral variables	6.10%	6.63%
Treatment per 1% registered/voting young	0.84%	0.77%

Notes: In panel A, state-level clustered standard errors are in parentheses. The dependent variables are Young registering (column 1) and Young voting (column 2). The average post-treatment coefficient is estimated using a specification of regression (1) that replaces $\mathbf{1}(t - T_s = \tau)$ with $\mathbf{1}(t \geq T_s)$. All regressions are weighted by cell size and include state fixed effects, year fixed effects, and state-specific time trends. District of Columbia is not part of the sample for consistency with Section V. In panel B, the first row reports the estimates of column 5 in Table 2. The second row reports the estimates of the first stage expressed as a percentage variation with respect to the pre-treatment sample mean. The third row provides the percentage treatment impact of a 1 percent increase in young registering and young voting, obtained by dividing the first row by the second.

that preregistration leads on average to more than 20,000 additional young voters in every post-treatment election in each treated state.

We combine the estimated impact of preregistration on electoral variables with the effect on higher education expenditure in panel B of Table 3. The first row reproduces the result from column 5 in Table 2, where preregistration raises per capita higher education expenditure by an estimated 5.1 percent. The second row shows the estimated impact of the reform on the registration and voting turnout of the young obtained by dividing the estimated coefficients of columns 1 and 2 in panel A by the (pre-treatment) mean of the dependent variable. The third row converts these effects into elasticity of higher education expenditure with respect to youth enfranchisement. Under the overly restrictive assumption that youth enfranchisement is the only operating mechanism through which the reform impacts the fiscal outcome, the IV-type interpretation of the results suggests that a 1 percent increase in youth registration and voter turnout increases the allocation of state resources to higher education by roughly 0.84 percent and 0.77 percent, respectively.[45] Expressed in 2014 US dollars, these elasticities imply that for every additional 1,000 young voters, governments respond by increasing higher education expenditure by $1.25 per capita, which is 0.25 percent of per capita higher education spending in the average state.

Whether or not our estimates of the strength of political responsiveness are inflated by the abovementioned complementary channels, they are nonetheless likely

[45] Similar results are obtained by formally estimating a two-stage least squares model in which youth registration and voter turnout are instrumented using preregistration and then regressing the fiscal variable on the predicted changes in youth enfranchisement.

to partly reflect a response to the increased political participation of the young. As a point of comparison, Cascio and Washington (2014) find that the cancellation of the literacy test in the United States increased the turnout in the presidential elections by 0.59 percent and per capita state transfers by 0.57 percent for each percentage point increase in the black share of the population. This implies an elasticity of transfers with respect to enfranchisement of approximately unity. Fujiwara (2015) finds that the introduction of electronic voting in Brazil increased the share of valid votes by 12 percent and spending on health care by 34 percent, with an implied elasticity of about 2.8. However, it is difficult to compare the conclusions of these studies to our own, since they only infer the identity of newly enfranchised voters based on local characteristics, while our empirical analysis makes it possible to identify the impact of the reform on the intentionally treated subgroup of the population—that is, the young—and then to provide evidence for preregistration's effects on fiscal policy by way of the treated subgroup.

VII. Conclusion

We investigate the effect of preregistration laws on political participation and government spending in the United States. Preregistration allows individuals to register before they reach voting age so as to be automatically added to the registration rolls once they come of age. By exploiting the variation in the timing of the passage of preregistration laws across states, the results collectively suggest that politicians responded to the change in electoral composition following the de facto enfranchisement event during the 2000s and in a manner consistent with the predictions of a political economy model of distributive politics.

A caveat to be considered is that the results may apply only to the United States and only to a specific time period. For example, the fact that political competition is strongly bipartisan, that voting is conditional on registration, and that the approval processes for electoral and budget bills involve a reverse legislative pattern are all features specific to the US context. Nonetheless, the analysis reinforces the common insight in political economics that increased electoral participation by a politically disadvantaged group is a precondition for the advancement of policies that benefit it. These findings should therefore inform the current debate on voting reform.[46] Recent attempts to roll back preregistration in some US states, which would make voting registration more restrictive, may be misguided not only because they tend to disenfranchise young voters, but also because they weaken the political incentive to implement fiscal policies that are to their benefit, such as the provision of public education.

Whether similar results can be replicated in different contexts is an open question, especially since youth disenchantment with the ballot is becoming a growing phenomenon across democracies. Many European countries, such as Austria, Germany, Norway, and the United Kingdom, are considering whether to lower the voting age from 18 to 16 as part of an effort to promote more active social and

[46] On the current debate over voter registration reform in the state of New York, see McKinley (2019).

political engagement among the young.[47] Our empirical results confirm that electoral reform will have a strong impact on public policy in countries where political competition is weaker and the share of the young in the population is larger. Future research should investigate these issues in different settings.

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