Youth Enfranchisement, Political Responsiveness, and Education Expenditure: Evidence from the U.S.*

Graziella Bertocchi  Arcangelo Dimico  Francesco Lancia  Alessia Russo

March 2018

Abstract

We examine the link between the political participation of the young and fiscal policies in the U.S. The focus is on preregistration laws, which allow the young to register before being eligible to vote. We establish that preregistration shifts state-level government spending toward higher education. The increase is larger when political competition is weaker and inequality higher. Moreover, we document a positive effect of preregistration on state-provided student aid and the number of its recipients. Finally, we show that preregistration promotes a de facto youth enfranchisement episode. The results collectively suggest political responsiveness to the needs of the newly-enfranchised constituency.

JEL Classification: D72, H52, P16.

Keywords: Education Expenditure, Political Responsiveness, Preregistration, Voter Turnout, Youth Enfranchisement.

*Graziella Bertocchi, University of Modena and Reggio Emilia, CEPR, DGI and IZA, email: graziella.bertocchi@unimore.it; Arcangelo Dimico, Queen’s University Belfast, email: a.dimico@qub.ac.uk; Francesco Lancia, University of Salerno and CSEF, email: flancia@unisa.it; Alessia Russo, BI Norwegian Business School, email: alessia.russo@bi.no. We would like to thank Erich Battistin, Raquel Fernández, Patricia Funk, Benny Geys, Marc Goïï, Marcus Hagedorn, Anirban Mitra, Espen Moen, Giacomo Ponzetto, Mathias Thoenig, Tom Vogl, and Fabrizio Zilibotti, as well as participants at the NBER Summer Institute on Income Distribution and Macroeconomics, the CEPR Annual Macroeconomics and Growth Meeting, the EJPE-IGIER-CEPR Conference on Political Economy, the MIPP Workshop in Political Economy & Political Science, and seminars at IMT and the Universities of Kent, Oslo, and Vienna for helpful comments. We also acknowledge the support of Unimore FAR Grant 2014.
1 Introduction

In all modern states a central activity of governments 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 which socioeconomic groups to target 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 greater 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. Surprisingly, conflict between different age groups and its implications for political participation and government spending have received far less attention. In the face of evolving demographic forces, the fact that in modern democracies public intervention favors pensions and health care – which benefit the old – over education expenditure – which benefits the young – has become the focus of the policy debate and has raised concern about the reluctance of the young to cast their ballot – still the most effective way to make politicians responsive to their demands.\(^1\)

The aim of the paper is to examine the link between the political participation of various age groups and policy decisions. For this purpose, the U.S. provides an ideal institutional setting. This is due to two reasons: First, even though the U.S. has been a de jure full democracy with universal suffrage for a long time, 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 U.S. 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.

The focus of the paper is on preregistration, an electoral provision introduced by individual states with the aim of encouraging civic engagement among the young by reducing the burden of registration.\(^2\) Preregistration allows young individuals to register

\(^1\)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 for the period 1984-2012. The estimation results show that the preferences for public goods vary markedly between age groups, with those of the young tilted toward higher education and away from pensions and health. See Appendix A.

\(^2\)In its report on Senate Bill 6340 concerning voter preregistration, the Senate Committee on
at a variety of locations that they frequent, such as schools, campuses, and motor vehicle bureaus, before becoming eligible to vote, independently of whether they will reach voting age prior to the next election. Starting from 1993, 13 states plus the District of Columbia have introduced preregistration laws at various points in time. Thus, the variation over time and space can be exploited in order to determine whether more convenient registration procedures can increase the voter turnout of the young and promote fiscal policies targeting their needs.

In order to interpret the empirical findings, we first develop a formal theory of electoral competition and voting participation which illustrates how preregistration can affect the allocation of the public budget. The theory is an adaptation of a probabilistic voting model to an environment with intergenerational conflict and individual cost of voting. Citizens are either young or old and differ in their wealth as well as in their preferences for education expenditure, since the young benefit from it while the old do not. The electoral competition takes place between two candidates, who run for office on a policy platform consisting of a wealth tax, education provision, and electoral rent. Voters cast their ballot or abstain after learning about the platform of each candidate and the realization of shocks affecting their voting behavior and cost. In this framework, the enactment of a preregistration law is interpreted as a decrease in the average cost of voting for the young and in the marginal electoral advantage of the old. The model predicts that following the introduction of the law politicians provide more education, which benefits the young, whose voter turnout increases relative to that of the old. Additional lessons of the model are that the link between preregistration and education expenditure is weaker when political competition is stiffer, the share of the young is larger, and inequality is lower.

The main contribution of the paper is to empirically test the predictions of the model. We use two complementary empirical strategies to identify the link between preregistration laws and economic outcomes. Since such laws have been introduced in several geographically dispersed states, and in different years, we first employ a difference-in-differences regression design and compare states with and without preregistration. Using annual financial data on the activity of state governments provided by the U.S. Census Bureau for the period 1980-2014, we find that per capita education expenditure is on average 6% higher in states that have adopted preregistration relative to states that have not. The effect is economically substantial and is supported by a set of informal validity

---

Government Operations & Security of the Washington State Legislature states: “This is a great opportunity to expand the franchise. We need to make sure people know they need to be registered before the election that occurs when they turn 18. This will expand access, increase engagement, and lead to more informed voters. This is a common ground that both sides can agree on.” See app.leg.wa.gov/billsummary?BillNumber=6340&Year=2015#documentSection.
tests that relate to concerns regarding the timing of preregistration adoption. Remarkably, the effect manifests itself after the first election following the passage of the law and regardless of whether the elected governor or the winning party at the time of budget approval was already in power in the reform year. This evidence shows that preregistration and education funding do not belong to a single youth-oriented reform package but rather are outcomes of distinct policy-making processes. A battery of additional results enriches the paper’s findings: (i) education expenditure does not respond to other registration reforms not specifically targeted at the young; (ii) the increase in education expenditure is larger in states where political competition is weaker and inequality higher, as predicted by the theory; and (iii) the effect of preregistration operates entirely through current expenditure on higher education, rather than elementary-secondary education or capital outlays.

The second empirical strategy exploits policy discontinuities at state borders to compare higher education institutions located in counties that border on each other but belong to different states. To this end, we use the Integrated Postsecondary Education Data System provided by the Delta Cost Project Database, which includes information on U.S. colleges, universities, and technical and vocational institutions for the period 2002-2012. This strategy serves a twofold purpose: First, it reduces the importance of unobservable heterogeneity in education funding, since underlying economic fundamentals are expected to evolve more similarly across contiguous counties than across states. Second, it tests whether predictions for the provider of funding, i.e., a state, are mirrored in the results for a recipient, i.e., a higher education institution. The results indicate that preregistration has a clear impact on state financial aid to higher education. Indeed, preregistration is associated with a 4.3 percentage point increase in the share of state grants within total student aid and a 7.1 percentage point increase in the number of recipients of state grants as a proportion of full-time first-time degree seekers. We find no placebo effect on other components of student financial aid, such as federal grants and grants financed by the institutions themselves. This result stands up to a wide variety of robustness checks. The beneficial effect of preregistration on government spending in favor of the young is therefore confirmed with a very different dataset and estimation strategy.

Finally, we investigate how voter turnout reacts to the introduction of preregistration. Since age is a dimension along which the preregistration treatment varies together with space and time, we employ a triple-differences regression design to further exploit between-age group and within-state variation. Using individual-level data on registration and voting records from the Voting and Registration Supplement of the Current Population Survey for the period 1996-2014, we find that preregistration positively affects the
participation of the young, especially those from low-income families, who turn out at elections at a higher rate than the rest of the electorate. The estimated increase in registration and voter turnout among the young is on average 6.4% and 8.2%, respectively, and it is shown to persist over time. The findings therefore suggest that a large number of young individuals, who otherwise would have been left without a political voice, are de facto enfranchised when they take advantage of preregistration. Taken together, the results reinforce the hypothesis that politicians respond to the electoral mobilization of young voters following the passage of preregistration by increasing the type of expenditures that target them. Indeed, current expenditure on higher education directly affects the college-age individuals in the electorate and more strongly so in the presence of higher inequality and when a larger share of them are in need of student financial aid.

This paper communicates with three strands of the literature. First, it is connected with the analysis of the determinants of democratization and de jure enfranchisement developed for the case of conflict arising between economic elites and poor masses (see, e.g., Acemoglu and Robinson, 2000, 2006; Lizzeri and Persico, 2004; and Llavador and Oxoby, 2005). Related empirical assessments of the enfranchisement effect for public spending include Lindert (1994), Aïdt, Dutta, and Loukoianova (2006), and Acemoglu, Naidu, Restrepo, and Robinson (2016). Parallel investigations have addressed conflicts arising between alternative socioeconomic groups along the racial and gender dimension. 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 occurring in a developed economy where universal suffrage is already established.

Second, the paper is also related to a small literature analyzing 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 registrants. However, neither paper discusses the implications for government spending, which is the main contribution of the present paper. 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). The influence of voting reforms on voter turnout and

---

3The implications of voting restrictions, such as poll taxes and literacy tests, enacted in the U.S. South after the Civil War and aimed at disenfranchising the blacks, 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) and Miller (2008) for the U.S. and by Aïdt and Dallal (2008) and Bertocchi (2011) for other Western countries.

Finally, the paper is closely related 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, e.g., Tabellini, 1991; Alesina and Rodrik, 1994; Krusell, Quadrini, and Rios-Rull, 1997; Cooley and Soares, 1999; Levy, 2005; Song, Storesletten, and Zilibotti, 2012; and Lancia and Russo, 2016). A major drawback of these models is their inability to quantitatively separate the effect of shifts in political power on governments 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 (Strömb erg, 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 2 describes the institutional setting and historical background. Section 3 presents the model. Section 4 reports the estimation results for the impact of preregistration on government spending at the state level. Section 5 documents the effect of preregistration on student financial aid at the level of higher education institutions. The implications of preregistration for the political participation of the young are presented in Section 6. Section 7 concludes. The Supplementary Material includes: evidence for the divergence between young and old in terms of policy preferences (Appendix A); the figures and tables not presented in the text (Appendix B); state-by-state information on the legislative process leading to the approval of a preregistration bill (Appendix C); proofs (Appendix D); a description of the data (Appendix E); and evidence for the impact of preregistration on the identity of representatives (Appendix F).

2 Institutional Setting and Historical Background

2.1 The Electoral and Budgeting Processes

The U.S. is a federal republic composed of 50 states plus the District of Columbia. The U.S. Constitution establishes rules for federal elections, while state laws regulate most
aspects of state and local elections. In each state, voters elect the governor directly. The length of a gubernatorial term is four years except in New Hampshire and Vermont, where it is two years. In 36 states, governors cannot be elected for more than two or even one term, while the governors of 14 states can serve an unlimited number of terms.

U.S. 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 indicates funding priorities and spells out the amounts that will be allocated to various state agencies. It is the most important means for a governor to influence the legislative process. The following sources of revenue are used to finance state spending: (i) General State Funds, which are the predominant means for financing state operations and are obtained via broadly-based state taxes; (ii) Other State Funds, which are restricted by law to be used for specific governmental functions and are obtained from tuitions and fees, provider taxes, donations, assessments, and local funds; (iii) Federal Funds, which are intergovernmental revenues received directly from the federal government; and (iv) Bonds. The use of funds and bonds to finance different functions of state spending varies across states.

Elementary-secondary education and higher education represent respectively the second and third largest components of total state spending and are financed from different sources. In most states, elementary-secondary education is considered a local function and is primarily financed by the local property tax. Spending on higher education, which includes financial support for public universities, community colleges, and vocational institutions, is in contrast primarily financed by broadly-based state taxes. However, the share of General State Funds spent on higher education has in recent years been declining, while spending on other programs, such as Medicaid, has been rising. As a consequence, an increasing share of the cost burden of higher education has been transferred from taxpayers to students through higher tuition rates. Funds allocated to an institution

---

4Federal as well as many state elections are held on Election Day in November of even-numbered years. Exceptions are Kentucky, Louisiana, Mississippi, New Jersey, and Virginia which elect their governors during odd-numbered years.

5The main functions that were funded at the state level in fiscal 2014 were: Medicaid (25.6% of State Funds), elementary-secondary education (19.8%), higher education (10.5%), transportation (7.9%), public assistance (1.5%), and other expenditures including economic development, environmental projects, housing, parks, and state police (31.5%). See nasbo.org/reports-data/state-expenditure-report/state-expenditure-archives.

6In fiscal 2013 45.3% of elementary-secondary education revenues came from localities (of which, property taxes represented 65%), while 45.6% came from the state and 9% from the federal government. The significance of local property taxes varies across states. See census.gov/content/dam/Census/library/publications/2015/econ/g13-aspef.pdf.

7General State Funds have decreased from 58.2% of total state spending on higher education in fiscal 1995 to 38.1% in fiscal 2014. As a result, Other State Funds have surpassed General State Funds as the single largest source of state spending on higher education, making up 47.4% of total state higher education expenditure. Average in-state tuitions and fees at public four-year institutions increased by
of higher education are managed by the Board of Trustees which has the authority and responsibility to ensure the fulfillment of an institution’s mission. To guarantee that institutions serve the public interest, however, many states have established independent coordinating agencies that oversee the Boards of Trustees and review budget requests submitted to the state.\textsuperscript{8}

### 2.2 Young Voter Turnout

Voting is the most effective way to influence government decision making. In the 2012 Presidential election, only 54.9\% of Americans cast their ballot. Since the 1960s turnout has been characterized by a persistent downward trend, decreasing by over 14 percentage points from its 1964 peak of 69.3\%. 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 26th Amendment to the Constitution, voter turnout was 52\% in the 18-24 age group in comparison to 68\% for citizens over 25. Ever since then, young voter turnout has persistently remained lower than that of other age groups. By the 2012 Presidential election, the corresponding figures were 41\% and 65\%.\textsuperscript{9}

The lack of participation by young Americans in the voting process has been the object of increasing attention, especially since low civic engagement among the young tends to persist later in life.\textsuperscript{10} Several explanations for the persistence of low civic engagement among the young have been advanced, such as the limited level of resources available to them and their inadequate knowledge of voting procedures and mechanisms. The fact that the young are more likely to move frequently for education or work also amplifies their difficulty in collecting information and establishing connections, which hinders their participation at the poll. Other potential motives are linked to specific features of the U.S. 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.\textsuperscript{11}

\textsuperscript{8}As documented by the State Constitutional Provisions and Higher Education Governance Policy, 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 8-year term to ensure independence from the state. Coordinating agencies have significant budgetary authority. See mhec.org/sites/mhec.org/files/20130516state-constitutional-provisions-highered-governance.pdf.

\textsuperscript{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/.

\textsuperscript{10}As observed by Strate, Parrish, Elder, and Ford (1989), the accumulation of political experience that comes with age leads to increasing levels of civic competence and voting participation.

\textsuperscript{11}On the demographics of voter turnout, see the classic text by Wolfinger and Rosenstone (1980) and

Beyond these explanations, a peculiar feature of the U.S. voting system that has been blamed for low turnout of the young is related to 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 purpose of ensuring the integrity of the electoral process. The voter registration process is currently regulated by state law, with North Dakota being the only state not requiring registration. Registration rules differ significantly across states in terms of deadlines, restrictions, and/or proofs required to register. Voter registration typically occurs between two and four weeks before each election and is organized at the county level. 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 registered, do actually vote is quite high. The positive correlation between registration and voter participation suggests that the young are actually more likely to vote when given greater opportunities to register.

2.3 Voter Registration Reforms

To ease the burden of registration and encourage civic engagement, particularly among socioeconomic groups that typically show lower turnout, 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. Although the act was initially 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. 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 public offices, for those requiring social the more recent account by Holbein and Hillygus (2016).

Southern states introduced registration prerequisites involving poll taxes and literacy tests in order to curb the political power of blacks 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).

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

The percentages of registered voters under 30 who cast their ballots in the 2000, 2004, and 2008 Presidential elections were 74, 82, and 84, respectively. See census.gov/prod/2010pubs/p20-562.pdf.
In addition to the NVRA, three main voter registration reforms have been enacted at the state level: (i) Election Day Registration (EDR) allows eligible voters to register on election day. Starting with Maine in 1973, 13 states, plus the District of Columbia, currently offer EDR. (ii) Online Registration allows voters to submit their application over the Internet. Starting with Arizona in 2002, 30 states plus the District of Columbia currently offer online registration. (iii) Preregistration 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 turn 18.

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 that: “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 individuals aged 15 or older with a driver’s license and in 2008 made it 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 non-partisan organization that has been promoting civic engagement and election reforms.

---

15 The NVRA is currently in force in 44 states and the District of Columbia. 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.


17 Quantitative investigations regarding the impact of Online Registration on voting have not been carried out as yet.

18 Preregistration laws differ from other state provisions that tie eligibility for early registration to attaining voting age prior to a specific election. Specifically, preregistration operates on an ongoing basis, even when elections are not scheduled. Together with preregistration, a few states have signed bills into law to promote follow-up voter education programs with the aim of increasing civic engagement among the young and to leverage the success of the reform. In California, for example, Assembly Bill 700, 2013, and Assembly Bill 1817, 2014, provide channels through which communities and advocates can work with schools.
since 2005.\textsuperscript{19} 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, and Utah in 2015. North Carolina later repealed the law in 2013.\textsuperscript{20} The timeline of the preregistration legislations across U.S. states is shown in Figure B1 in Appendix B.

2.4 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 as being an exogenous event with respect to a governor’s budget decisions (as probed in greater detail in Section 4). We take advantage of the fact that the constitutional division of responsibilities between the executive and the 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 like preregistration follow a reverse pattern. Thus, they are first sponsored by a member of the state legislature, then approved in the House and Senate, and finally signed by the governor to be converted into law. 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.\textsuperscript{21}

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

\begin{itemize}
  \item \textsuperscript{19}Representative Pacheco of Rhode Island, who sponsored House Bill 5005 with four co-signers from among both Republicans and Democrats, has declared that: \textit{“FairVote is the major asset in the preregistration battle, doing crucial legwork and reaching out to local media.”} See archive.fairvote.org/ncteenspreregister.
  \item \textsuperscript{20}Currently, California, Colorado, Delaware, Florida, Hawaii, Louisiana, Maryland, Massachusetts, Rhode Island, Utah, and the District of Columbia allow preregistration for 16-year-olds, while Maine and Oregon allow it for 17-year-olds.
  \item \textsuperscript{21}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, they are usually ignored by the legislature. Indeed, only 27\% of such proposals pass, with another 6\% ending in compromise.
\end{itemize}
is that they have received bipartisan support, with California being the only exception.\footnote{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 Schwarzenegger.} In view of a broad and non-partisan 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, North Carolina, Utah, and the District of Columbia passed the preregistration bill almost unanimously. North Carolina is perhaps the most notable example of bipartisan approval of a preregistration bill. The bill was co-sponsored in 2009 by four legislators who included the youngest Republican and Democrat in the General Assembly. The bill was approved by a state legislature controlled by Democrats although more than 88% of the Republicans voted in favor of it. It was finally signed into law by Democratic Governor Perdue. Since then, more than 150000 teenagers have preregistered under the program. Of the 55291 who preregistered in 2012, 41% choose to do so as unaffiliated, 33% as Democrats, and 26% as Republicans, making 2012 the first year that preregistered Democrats exceeded preregistered Republicans.\footnote{See charlotteobserver.com/news/politics-government/article9137564.html.} 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.

Further information on preregistration legislation is provided in Appendix C. The 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, six had a Republican governor and seven a Democratic one. It is also worth noting that the success of a legislative process to introduce preregistration is not associated with a higher rate of young and/or more female legislators, who may be more favorable to its introduction and at the same time more supportive of liberal fiscal policies. Thus, the adoption of preregistration is not more likely when political power is in liberal hands, as one might have thought.

3 Theoretical Framework

In this section, we present a simple model of electoral competition and voting participation which serves as a motivating theory for our estimation strategy and empirical results. The model is an adaptation of a probabilistic voting model à la Lindbeck and Weibull (1987)
to an environment with intergenerational conflict and individual cost of voting. We adopt the view that candidates commit to policies catered to the needs of the majority of voters. Hence, voters actually affect rather than elect policies. Although data limitations prevent us from testing this hypothesis, we present empirical evidence consistent with the predictions of the model and in support of this view.24

Consider a jurisdiction, such as a state, populated by a unitary mass of citizens. A fraction $\alpha$ of the population is young, denoted as $y$, whereas the remaining fraction $1 - \alpha$ is old, denoted as $o$. While the members in each group are identical, there is a different endowment of wealth in each group. Namely, a young individual has wealth $\omega^y$ which is less than $\omega^o$, the wealth of an old individual. Average wealth in the society is $\omega$. Endowments can then be conveniently rewritten as $\omega^y = \sigma \omega/\alpha$ and $\omega^o = (1 - \sigma) \omega/(1 - \alpha)$, where the parameter $\sigma \in [0, \alpha)$ provides an inverse measure of inequality, i.e., a higher $\sigma$ indicates less inequality.

Public decisions are made by a government that uses its fiscal authority to tax wealth at a rate $\tau \in [0, 1]$. The tax burden is borne by the entire population. Fiscal revenues can be used to finance public education, $e \geq 0$, but can also be diverted to finance an electoral rent, $R \geq 0$. We assume that governments are prevented from borrowing and lending. Thus, the government budget constraint is $(\tau - D(\tau)) \omega = e + R$, where $D(\tau)$ is an aggregate cost that captures the deadweight loss of taxation, with $D(0) = 0$, $D_\tau > 0$, and $D_{\tau\tau} > 0$. A fiscal policy platform is then a vector $q := (\tau, e, R)$.

An individual’s utility is influenced by government decision making. The utility of a young individual is $U^y(q) := (1 - \tau) \omega^y + (\lambda e/\alpha)$, where $\lambda > 0$ measures the marginal benefit from public education, and that of an old individual is $U^o(q) := (1 - \tau) \omega^o$. This formulation is flexible enough to capture, for example, the idea that the amount of education consumed when an individual is young affects her future income in the amount of $\lambda e/\alpha$. Education is traditionally seen as an expenditure that favors the young, due to its positive effect on future income or human capital, which the old cannot benefit from.25

Electoral Competition The government is democratically elected according to a

---

24Existing studies have highlighted two contrasting views of the role of elections in policy formation. In one view, voters affect policies and elections have the effect of constraining candidates’ policy choices. In the other view, voters merely elect policies and elections are meant to decide which candidate’s policy to implement. The empirical evidence is mixed. Lee, Moretti, and Butler (2004) use voting record data from the U.S. House to show that voters appear not to affect politicians’ choices, while Strömberg (2008) shows that a probabilistic voting setup can explain a candidate’s allocation of resources in U.S. Presidential elections fairly well.

25The utility function of the young can be seen as the reduced form of a utility function in a two-period model, where agents enjoy present as well as future consumption, which increases with current investment in education (see Lancia and Russo, 2016). The assumption that the young have a stronger preference for education spending than the old finds support in the evidence produced in Appendix A.
majority rule. The electoral competition takes place between two candidates, an incumbent and a challenger, denoted as $\zeta \in \{I, C\}$, who have the ability to non-cooperatively commit to a policy platform $q_{\zeta}$ before the election in order to maximize the expected rent from being in office. Thus, each candidate’s objective function is $p_{\zeta}(q_{I}, q_{C}) \cdot R_{\zeta}$, where $p_{\zeta}(q_{I}, q_{C})$ is the probability that candidate $\zeta$ defeats her opponent by proposing a policy agenda $q_{\zeta}$.

The electoral demand side is characterized by voters who derive benefits from voting regardless of whether they affect the electoral outcome. The individual benefits of voting depend on both the platform of each candidate and a popularity shock $\delta$. Such a shock captures the ex-post average success of candidate $I$ and is drawn from a uniform distribution on $[-(1/2) + \phi, (1/2) + \phi]$, with $\phi > 0$ measuring an incumbency advantage. Net of the popularity shock, citizens support the candidate whose proposed platform maximizes their utility. Formally, a citizen who belongs to age group $i \in \{y, o\}$ supports candidate $I$ if $V_{i}(q_{I}, q_{C}) := U_{i}(q_{I}) + \delta - U_{i}(q_{C}) \geq 0$ and candidate $C$ otherwise.

The act of voting imposes a cost $c$, which differs among individuals and is uniform on $[c, c']$. We assume that $c' > c$, reflecting the higher cost of voting for the young relative to that of the old. This may, for example, be because they are unfamiliar with registration procedures and voting requirements. Citizens therefore vote when the utility gains from voting outweigh its costs, i.e., $c \leq |V_{i}(q_{I}, q_{C})|$; otherwise they abstain.

**Political Economic Equilibrium** Candidates and voters move sequentially. First, candidates simultaneously announce their platform $q_{\zeta}$. Second, the shocks affecting individual voting behavior, i.e., the electoral advantage $\delta$ and the individual voting cost $c$, are realized. Third, the election is held and the citizens decide whether to vote and, if so, for which candidate. Finally, the winning candidate implements her political proposal. A political economic equilibrium is then defined as a vector of policy platforms and voter turnout and is characterized by solving the game via backward induction.

Two fundamental forces shape the equilibrium policy platform: (i) an intergenerational conflict over the allocation of the public budget and (ii) a political conflict over the size of the electoral rent. The intuition behind a candidate’s optimal tradeoff is as follows: Candidates must be attentive to the well-being of the young and the old since individuals in both groups can vote. The young are motivated to support high taxation to finance public education. The old dislike taxes since they derive no benefits from them. Candidates, therefore, set taxes in order to balance the marginal benefit of public

---

26By assuming that people get utility directly from voting, we are avoiding the issue of why people vote. A justification for this assumption is that voters decide emotionally, rather than based on any estimation of how their vote will influence the electoral outcome (see Schuessler, 2000).

27The fact that the party in power has a larger ex-ante probability of winning the election is confirmed in the empirical literature and can be microfounded (see Besley and Case, 1995).
education for the young against the marginal cost of public funds. Moreover, although a platform with a higher rent is attractive per se, it decreases the probability of being elected. Thus, candidates grab an amount of public resources in order to equate the return on an additional unit of electoral rent to the return on public spending in terms of a larger share of supporting voters. In sum, candidates propose an equilibrium platform in order to address the economic needs of their constituencies as well as to achieve their own political aspirations. This is proved in Appendix D.

**Preregistration** What does our model predict about the effects of preregistration on policy and voting outcomes? The enactment of a preregistration law can be simply modelled as a reduction of $c_y$, reflecting a smaller average cost of voting for the young as well as a smaller marginal electoral advantage for the old. The following proposition presents the key results linking a preregistration law to fiscal policies and voting participation.\(^\text{28}\)

**Proposition** If $\lambda > \lambda$ and $\phi < \phi$, an interior political economic equilibrium exists and the effect of preregistration on fiscal policy rules and voter turnout is as follows:

1. If $c_y$ decreases, (i) youth voter turnout increases and (ii) average public education expenditure increases;

2. The negative link between $c_y$ and average public education expenditure is stronger when (i) political competition is weaker, i.e., $\phi$ is larger; (ii) the share of young voters is smaller, i.e., $\alpha$ is smaller; or (iii) inequality is higher, i.e., $\sigma$ is smaller.

This proposition has several elements and provides a set of testable empirical predictions. In equilibrium, the education policy reflects the share of active voters within each age group and is limited by the size of the public budget. The model predicts that the level of education expenditure increases with the voter turnout of the group in favor of publicly provided education, i.e., the young. By lowering the cost of voting for the young, the enactment of a preregistration law generates a de facto enfranchisement episode, such that a larger share of young voters cast their ballot. All candidates then respond to higher voting participation by addressing the economic needs of the young, namely by providing more education expenditure (Point 1).

The model also illustrates how political competition, voter turnout demographics, and inequality mediate the impact of preregistration on education policy. Although the introduction of preregistration increases education expenditure, its effect is a non-linear function of political competition (Point 2.i). Intuitively, when political competition becomes stiffer, candidate $I$ adapts her policy towards the preferences of young voters, thus

\(^{28}\)The critical levels $\phi$ and $\lambda$ are defined in the proof of the Proposition in Appendix D. The conditions that public education spending be productive enough, i.e., $\lambda > \lambda$, and political competition be sufficiently stiff, i.e., $\phi < \phi$, are sufficient for the existence of an interior equilibrium outcome.
sacrificing electoral rent, while candidate C, who advocates maximal education expenditure, will have an increased chance of winning. The resulting increase in education expenditure dampens its responsiveness to the introduction of preregistration. As a result, we expect to observe a weaker link between preregistration and expenditure on education in states where political competition is more intense.

A similar intuition lies behind the result of Point 2.ii. The larger share of the young in the electorate, the more public resources will be committed to education in the candidates’ platforms. This in turn reduces the effect of preregistration reforms on the provision of education. Finally, since the young have less wealth than the old, they bear a smaller share of the fiscal burden and demand greater public expenditure through their vote. This implies that the link between preregistration and public education expenditure will be magnified in the presence of higher inequality (Point 2.iii). Our model captures a simple mechanism which helps in structuring our thinking about the driving forces behind the empirical results presented below for the U.S.

4 Preregistration and Education Expenditure

In this section, we empirically examine the effect of preregistration on U.S. government spending at the state level. Annual financial data on the activity of state governments is taken from the Annual Survey of State and Local Government Finances conducted by the U.S. Census Bureau. The full sample includes all of the 50 state governments for the period 1980-2014. We supplement this data with information collected from various sources regarding the timing of the introduction of the voter registration reforms across states and with a number of electoral and socioeconomic variables. Appendix E provides detailed information on variable definitions and data sources.

Summary statistics for the main variables are provided in Table B1 of Appendix B. Preregistration, which by 2014 has been introduced in twelve states, applies to 4% of the sample, while Online Registration, EDR, and the NVRA apply respectively to 4%, 11%, and 50% of the sample. The second set of variables consists of state-level electoral characteristics. On average, 50% of governors belong to the Democratic party, 43% are incumbent, 53% run in the next election, and 27% are not eligible to run again. The table also provides information on political competition, gubernatorial turnout rate, and the President’s party affiliation. The third set of variables consists of state-level fiscal characteristics (at constant 2014 U.S. dollars). The key variable is total education expenditure, which in per capita terms is equal on average to $776 and represents 14.5% of total expenditure. Disentangling education items shows that 79% of expenditure is allocated to higher education and 83.4% to its current component. Thus, the current
higher education component is $544, while elementary-secondary education expenditure accounts for $48. The table reports statistics also for all other categories of expenditure. Total taxes reflect state and local taxes and are on average $2421 per capita. The fourth set of variables is meant to capture the socioeconomic background of each state including, among others, the share of young and blacks in the population, post-secondary enrollment and educational attainment, personal income, inequality, and unemployment.

4.1 Empirical Strategy

We wish to test the link between preregistration and government spending with particular focus on education expenditure. We therefore compare states that have adopted preregistration to states that have not. Since preregistration laws have been introduced in different states in different years, these events have generated sufficient variation across space and over time. Hence, the theoretical predictions stemming from the model can be tested using a difference-in-differences regression design. Formally, the empirical model can be spelled out as follows:

\[ Edu_{s,t} = \beta \cdot Preg_{s,t} + \delta_t + \delta_s + \delta_s \cdot t + \pi \cdot X_{s,t} + \varepsilon_{s,t} \]

where \( Edu_{s,t} \) is the total per capita education expenditure in state \( s \) in year \( t \); \( Preg_{s,t} \) is a dummy variable which takes value 1 if state \( s \) has adopted preregistration in year \( t \), and 0 otherwise; \( \delta_t \) and \( \delta_s \) denote year and state fixed effects; \( \delta_s \cdot t \) represents state linear time trends; \( X_{s,t} \) are time-varying state characteristics; and \( \varepsilon_{s,t} \) is the error term which we cluster by state to capture serial correlation within states.\(^{29}\)

Year fixed effects are meant to control for time shocks, just as state fixed effects are meant to account for a state’s unobserved characteristics. State linear time trends are meant to capture differences in the trends of state-level outcomes and are added to separate out the effect of preregistration per se from the effect of, for example, FairVote programs to increase civic engagement, which advocate registration reforms and therefore may themselves be manifested in education expenditure trends. The vector \( X_{s,t} \) includes potential confounders reflecting the fiscal, political, and socioeconomic characteristics listed in Table B1. In this way, fixed differences across states, common shocks varying non-linearly over time (such as the 2008 financial crisis), observable confounding variables, and state-specific differences that vary linearly over time are all removed from the estimated effect of preregistration. As a result, the coefficient \( \beta \) should capture trend breaks in the outcomes of interest that coincide precisely with the timing of preregistration.

\(^{29}\)All variables that are neither shares nor categorical are log-transformed as \( \log(1+x) \) where \( x \) is the variable of interest, in order to retain observations equal to zero.
Figure 1: Parallel Trends (Panel a) and Residuals from Estimating the Empirical Model Without Preregistration (Panel b). Dotted lines refer to the 90% confidence intervals.

The main identifying assumption states that the timing of preregistration is exogenous to underlying factors that might have affected the fiscal policies chosen by governors, conditional on the controls. The facts that (i) the timing of preregistration varies quite significantly across treated states, and (ii) governors exert limited authority over preregistration lawmaking, lend plausibility to the identifying assumption. Nonetheless, the possibility that state-level reforms respond to state-specific political and socioeconomic dynamics remains a valid concern, even though it is partially addressed by the inclusion of state-specific time trends. To further dig into this issue, we first show that preregistration reforms are not related to education expenditure trends observed for the treated and control groups. Although a visual inspection of the parallel trends assumption is not straightforward in a context with multiple treatments and periods, we provide a graphical illustration in the spirit of an event study. Panel a of Figure 1 traces out the average year-by-year difference in education expenditure growth rates between treated and control states for the period leading up to the registration change. Formally, this is done by regressing education expenditure on year dummies and interactions between year dummies and a dummy for treated states. A treated state is dropped from the sample after the treatment for that state occurs. The figure shows no significant pre-treatment difference in education expenditure growth rates between the two groups of states. This suggests that education expenditure trends would have been the same in all states in the absence of the treatment.

Second, we show that the possible threat to identification originating from mean reversion at the onset of the preregistration reform, i.e., an Ashenfelter Dip, fails to be corroborated. To show this, we drop the never-treated states from the sample and check for shocks to education expenditure that may have hit the treated group just prior to the
preregistration year. Panel b of Figure 1 plots the coefficients obtained by regressing the residuals of the empirical model without the control \( Preg_{s,t} \) on each of the three years preceding and following the introduction of preregistration. The idea is that any shock affecting education expenditure prior to the adoption of preregistration should show up in a systematic pattern in the residuals. The figure, however, points to no significant variation in education expenditure prior to preregistration, followed by a significant increase that coincides with the enactment of a preregistration law. Overall, the evidence presented in Figure 1 indicates that the probability of shocks occurring at the same time as the registration change should be minimal.

Finally, we check that the timing of preregistration is orthogonal to a large number of predetermined state characteristics, thereby verifying that selection into treatment does not represent a threat to identification. With reference to the pre-treatment period, Table B2 of Appendix B reports point estimates, standard errors, and number of observations of pairwise correlations where the dependent variable corresponds to the preregistration year and the regressor of interest is defined by each row. Reassuringly, none of the correlations is significant suggesting that neither political nor fiscal or socioeconomic factors determined the timing of the preregistration reform.

### 4.2 Results

In order to examine changes in education expenditure at the state level during the period 1980-2014, we estimate variants of the empirical model. Because the dependent variable is in logarithmic form, the coefficient estimates can be interpreted roughly as percentage changes.\(^{30}\)

Table 1 reports the estimation results. In Model 1, we test for the direct effect of preregistration on total per capita education expenditure after controlling for fixed effects and state linear time trends. We find a statistically significant increase in the outcome of interest of 4.9%. In Model 2, we include a set of state-level variables as additional regressors in order to check for potential confounders that may have affected education expenditure through channels other than the lowering of registration costs. The coefficient of preregistration remains significant and equal to 6%. At the mean, this percentage variation corresponds to an increase of about $47 per capita in education expenditure in those states which at some point in time have adopted preregistration. To finance an equivalent increase by means of the income tax would require an average increase of 0.2 percentage points in the income tax rate (evaluated at mean income for the period

---

\(^{30}\)We first test for the effect on education expenditure of other registration reforms that aim to reduce the registration cost for all voters and not specifically youth. Table B3 in Appendix B shows that the introduction of NVRA, EDR, or Online Registration has no effect on the dependent variable.
Table 1: Preregistration and Education Expenditure

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Preregistration</strong></td>
<td>0.049**</td>
<td>0.060**</td>
<td>0.073***</td>
<td>0.026</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.023)</td>
<td>(0.016)</td>
<td>(0.016)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>F(1)Preregistration</td>
<td>-0.011</td>
<td>-0.006</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.010)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>F(2)Preregistration</td>
<td>-0.006</td>
<td>0.001</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.014)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>L(1)Preregistration</td>
<td>0.027*</td>
<td></td>
<td>0.020</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td></td>
<td>(0.016)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>L(2)Preregistration</td>
<td>0.032**</td>
<td>0.055***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.012)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>L(3)Preregistration</td>
<td>0.033*</td>
<td>0.047*</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.026)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

State-Level Controls
- No  Yes  Yes  Yes  Yes
- Yes Yes  Yes  Yes  Yes
- Yes Yes  Yes  Yes  Yes
- Yes Yes  Yes  Yes  Yes

R-squared 0.970  0.975  0.976  0.975  0.976
Observations 1750  1750  1650  1600  1500

Note: State-level clustered standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%. Regressions 2-5 also control for: 1) Registration Reforms: Online Registration, Election Day Registration, NVRA; 2) Electoral variables: Dummies for Gubernatorial and Presidential Election Year, Incumbent, Year of Mandate, Governor Runs Next Election, Governor Not Eligible to Run Again, Democratic Governor, Previous-Term Democratic Governor, Democratic President, Political Competition, Gubernatorial Turnout Rate; 3) Fiscal variables: Share of Current Expenditure, Total Taxes, Total Debt Outstanding, Total Federal Intergovernmental Revenue; 4) Socioeconomic variables: Population, Median Age, Share of 16-25, Post-Secondary Enrollment, Educational Attainment, Share of Blacks, Share of Whites, Personal Income, Inequality, Unemployment Rate.

In Model 3, we control for leads to test for potential changes in education expenditure that precede the preregistration year. The preregistration coefficient continues to be positive and significant, while anticipatory effects are not significant. Thus, we can reject the possibility of reverse causality running from education expenditure to preregistration. In Model 4, we include lags in order to evaluate the incremental effect on education expenditure over time during the post-treatment period. The point estimates are statistically significant and exhibit an increasing pattern during a three-year window.

31 Since Hawaii and Florida have a considerably different history with respect to the implementation of preregistration, and since California and North Carolina are the only states where the approval of the law happened to be respectively partisan and later repealed, in Table B4 of Appendix B we check that our findings are not driven by these states by sequentially excluding them from the sample and re-estimating Model 2 of Table 1. In each case, the estimated coefficient remains unchanged. This suggests that the results capture a general relationship between registration provisions and fiscal policy outcomes, rather than the influence of only a few states.

32 In Table B5 of Appendix B, we perform falsification tests by anticipating the date of adoption of preregistration to a random date within a 10-year window prior to the registration change. No statistically significant effects are observed.
after treatment at the rates of 2.7%, 3.2%, and 3.3% per year. Finally, in Model 5 we augment the base specification with both leads and lags. As in the previous models, the coefficients of the leads are close to zero and the coefficients of the lags show an increase in education expenditure following the preregistration year that lasts up to the third post-treatment year. This dynamic pattern is depicted in Panel a of Figure B2 of Appendix B.

We have so far focused on education expenditure since it is a category of spending directly targeted at the young. We now wish to check the potential impact of preregistration on other fiscal categories. Table B6 of Appendix B replicates the empirical specification of Model 5 of Table 1, except with general revenue and all categories of direct expenditures other than education as the dependent variable. We detect variations in the spending on health and hospital, employee retirement, and financial administration and general control, all of negative sign. The first two categories include expenditures for which the young do not have a strong preference, while the third accounts for overhead of government operations. Taken together, the reduction in these categories of expenditure is consistent with governments becoming more accountable to the young and more efficient, and the cuts are sufficiently large to cover the increase in education expenditure.

### 4.3 Heterogeneity of the Effects

Consistent with the predictions of the theoretical framework, the results in Table 1 show that candidates whose goal is to win elections dedicate more resources to the age group which is likely to hold more voting power after the registration reform. This section goes on to explore the non-linear effects of preregistration on education expenditure pointed out in Section 3 by interacting preregistration with variables measuring political competition, the age structure of the population, and inequality.

As a proxy for political competition, we utilize the index of political fractionalization. Higher values of this index correspond to states and periods with stiffer political competition. Since Louisiana is the only state with a so-called jungle primary system for gubernatorial elections, we drop it from the sample. In Model 1 of Table 2, the preregistration–political competition interaction coefficient is negative and significant, indicating that states with weaker political competition experience larger increases in education expenditure following the introduction of preregistration, which is consistent with the theory. The demographic composition of states appears not to affect the impact of preregistration on public education, as shown in Model 2 where we interact preregistration with the share of individuals aged 16-25. This lack of evidence, however, can be attributed to the limited variation in the explanatory variable relative to the mean in
Table 2: Preregistration and Education Expenditure - Heterogeneity of the Effect

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preg</td>
<td>0.261**</td>
<td>-0.233</td>
<td>-0.102</td>
<td>0.060**</td>
<td>0.061***</td>
<td>0.058**</td>
<td>0.063**</td>
<td>0.093***</td>
</tr>
<tr>
<td></td>
<td>(0.099)</td>
<td>(0.291)</td>
<td>(0.077)</td>
<td>(0.024)</td>
<td>(0.023)</td>
<td>(0.023)</td>
<td>(0.024)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Preg · Political Competition (Herfindahl Index)</td>
<td>-0.354**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.172)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Share of 16-25</td>
<td>1.645</td>
<td>(1.592)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Inequality (Theil Index)</td>
<td>0.198**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.087)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · First Year of Term</td>
<td>0.001</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Second Year of Term</td>
<td></td>
<td>-0.003</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Third Year of Term</td>
<td></td>
<td></td>
<td></td>
<td>0.013</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Fourth Year of Term</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.010</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preg · Previous-Term Democratic Governor</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>-0.045*</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

State FE: Yes Yes Yes Yes Yes Yes Yes Yes  
Year FE: Yes Yes Yes Yes Yes Yes Yes Yes  
State Time Trends: Yes Yes Yes Yes Yes Yes Yes Yes  

R-squared: 0.976 0.975 0.975 0.975 0.975 0.975 0.975 0.975  
Observations: 1715 1750 1750 1750 1750 1750 1750 1750

Note: State-level clustered standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%. All regressions also control for: 1) Registration Reforms: Online Registration, Election Day Registration, NVRA; 2) Electoral variables: Dummies for Gubernatorial and Presidential Election Year, Incumbent, Year of Mandate, Governor Runs Next Election, Governor Not Eligible to Run Again, Democratic Governor, Previous-Term Democratic Governor, Democratic President, Political Competition, Gubernatorial Turnout Rate; 3) Fiscal variables: Share of Current Expenditure, Total Taxes, Total Debt Outstanding, Total Federal Intergovernmental Revenue; 4) Socioeconomic variables: Population, Median Age, Share of 16-25, Post-Secondary Enrollment, Educational Attainment, Share of Blacks, Share of Whites, Personal Income, Inequality, Unemployment Rate.

The presence of an electoral cycle is tested for in Models 4-7 where we interact preregistration with each of the four years of a gubernatorial term. However, none of the interaction coefficients are statistically significant. Finally, in Model 8 we interact preregistration with a dummy which takes value 1 if in the previous term the governor was Democratic and we find the coefficient to be significant and negative. This implies that the impact of preregistration is stronger in states governed by Republicans in the past, the post-treatment period. Finally, in Model 3, we examine how the marginal impact of preregistration varies with inequality as measured by the Theil Index. In line with the theoretical predictions, we find that the effect of preregistration on education expenditure is magnified by higher inequality. This likely reflects the presence of a relatively larger share of poor young that need financial support in order to attend college and therefore demand more public education expenditure.33

In Table B7 of Appendix B, we test if the estimates are robust to (i) the winning margin and the Gini Index as alternative measures of political competition and inequality in Models 1 and 4; (ii) the inclusion of Louisiana in Model 2; (iii) the introduction of leads and lags of the treatment interacted with political competition and inequality in Models 3 and 5. The magnitude and statistical significance of the interaction coefficients are similar to those reported in Table 2. We also explore other sources of heterogeneity, such as the share of blacks, the governor’s party affiliation and whether the governor is term-limited. However, the results are not statistically significant and therefore not reported.
which were likely falling behind other states in terms of education provision on behalf of young voters.\textsuperscript{34}

### 4.4 Decomposing the Education Budget

The above results indicate that preregistration laws shift state-level government spending toward education. However, education expenditure includes components that may react differently to the registration change.

<table>
<thead>
<tr>
<th>Education by Function</th>
<th>Education by Character</th>
</tr>
</thead>
<tbody>
<tr>
<td>Elem.-Sec. (1)</td>
<td>Higher (2)</td>
</tr>
<tr>
<td></td>
<td>Total Cur. (3)</td>
</tr>
<tr>
<td></td>
<td>Total Cap. (4)</td>
</tr>
<tr>
<td></td>
<td>Higher Cur. (5)</td>
</tr>
<tr>
<td></td>
<td>Higher Cap. (6)</td>
</tr>
<tr>
<td>Preg</td>
<td>-0.249</td>
</tr>
<tr>
<td>(0.296)</td>
<td>0.053**</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State Time Trends</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.849</td>
</tr>
<tr>
<td>Observations</td>
<td>1750</td>
</tr>
</tbody>
</table>

Note: State-level clustered standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%. All regressions also control for: 1) Registration Reforms: Online Registration, Election Day Registration, NVRA; 2) Electoral variables: Dummies for Gubernatorial and Presidential Election Year, Incumbent, Year of Mandate, Governor Runs Next Election, Governor Not Eligible to Run Again, Democratic Governor, Previous-Term Democratic Governor, Democratic President, Political Competition, Gubernatorial Turnout Rate; 3) Fiscal variables: Share of Current Expenditure, Total Taxes, Total Debt Outstanding, Total Federal Intergovernmental Revenue; 4) Socioeconomic variables: Population, Median Age, Share of 16-25, Post-Secondary Enrollment, Educational Attainment, Share of Blacks, Share of Whites, Personal Income, Inequality, Unemployment Rate.

Table 3 disentangles education expenditure by function and character. In Models 1 and 2, we divide education expenditure by function, i.e., between elementary-secondary and higher education expenditure. While the coefficient of preregistration for elementary-secondary education is not statistically significant, we find that higher education expenditure is increased by 5.3% in states that adopt preregistration.\textsuperscript{35} In Models 3 and 4, we distinguish, by character, between current operating expenditure and capital outlays. The results indicate that the effect of preregistration obtained previously manifests itself in the current component, rather than the capital component. It is then natural to divide

\textsuperscript{34}State support for education has shown to be associated with shifts in the party affiliation of the governor and in the partisan control of the state legislature, with Republicans being associated with market-oriented policies and Democrats with larger state appropriations to education. See, e.g., Alt and Lowry (1994).

\textsuperscript{35}The high variability associated with the coefficient of preregistration in the case of elementary-secondary education is due to the presence of a substantial proportion of zero values, since this category of education is primarily financed by local taxes.
expenditure on higher education between its current and capital components. Models 5 and 6 show that only the current component is significantly affected by preregistration and the coefficient of 5% is in line with the previous findings. These results therefore demonstrate that the sizable effect of preregistration on education expenditure is predominantly by way of current spending on higher education, which is the component that directly affects the prospects of young soon-to-become voters who are enrolled in college or about to enroll.\(^{36}\)

### 4.5 Robustness

In this section, we provide additional evidence to alleviate the concern that preregistration and education policy might have been enacted as parts of a single youth-oriented policy package and therefore spuriously correlated. Since fiscal changes are predominantly associated with expenditure on higher education, in what follows we refer to this function of education spending. If education expenditure responds to the introduction of preregistration because initiatives in the legislature are strategically linked to a governor’s budget decision, we would expect to observe an increase in education expenditure during the four-year term of the legislature that approved the reform. However, when in Model 1 of Table 4 we interact preregistration with a dummy variable that takes value 1 in the years following the first election subsequent to the registration change, we find a significant and positive interaction coefficient, similar in magnitude to the coefficient of preregistration in Table 1, while the coefficient of preregistration is no longer significant. This confirms that the effect of preregistration on education expenditure manifests itself during a different gubernatorial term than the one in which the reform is passed.

There still may be concern that, although the fiscal adjustment and the reform take place in two different terms, the increase in education spending is carried out by the incumbent governors or by governors affiliated with the incumbent party. In both cases, we could not exclude that policies may have been conceived as parts of a youth-oriented reform package. However, when in Models 2 and 3 we interact the dummy for the first post-reform election with dummies meant to capture whether the elected governor or the party in power was incumbent, the interaction coefficients are not statistically significant, while the magnitude of the treatment effect after the first election remains unchanged. This provides reassurance that the results are not driven by spurious correlations.

There are some additional results worth noting. Models 4 and 5 show that the treatment effect after the first election following the passage of the law is not significantly

\(^{36}\)Panel b of Figure B2 in Appendix B displays the dynamic pattern of current higher education expenditure resulting from estimating an empirical specification augmented with leads and lags.
Table 4: Preregistration and Higher Education Expenditure - Robustness

<table>
<thead>
<tr>
<th></th>
<th>Higher Education Expenditure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Preg</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
</tr>
<tr>
<td>Preg · Post-First Election</td>
<td>0.072***</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
</tr>
<tr>
<td>Preg · Post-First Election · Incumbent Party</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
</tr>
<tr>
<td>Preg · Post-First Election · Incumbent Governor</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
</tr>
<tr>
<td>Preg · Post-First Election · Democratic Governor</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
</tr>
<tr>
<td>Preg · Post-First Election · Governor Not Eligible to Run Again</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
</tr>
<tr>
<td>State Time Trends</td>
<td>Yes</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.974</td>
</tr>
<tr>
<td>Observations</td>
<td>1750</td>
</tr>
</tbody>
</table>

Note: State-level clustered standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%. All regressions also control for: 1) Registration Reforms: Online Registration, Election Day Registration, NVRA; 2) Electoral variables: Dummies for Gubernatorial and Presidential Election Year, Incumbent, Year of Mandate, Governor Runs Next Election, Governor Not Eligible to Run Again, Democratic Governor, Previous-Term Democratic Governor, Democratic President, Political Competition, Gubernatorial Turnout Rate; 3) Fiscal variables: Share of Current Expenditure, Total Taxes, Total Debt Outstanding, Total Federal Intergovernmental Revenue; 4) Socioeconomic variables: Population, Median Age, Share of 16-25, Post-Secondary Enrollment, Educational Attainment, Share of Blacks, Share of Whites, Personal Income, Inequality, Unemployment Rate.

The results in Table 4 overall provide strong evidence on the timing of the effects. The fact that policy adjustments occur after the first post-reform election lines up with the hypothesis of voters actually affecting policies, since they suggest that policy makers are committed to their political platforms. Nonetheless, the possibility of voters electing policies in the context of preregistration remains a valid alternative hypothesis. Indeed, preregistration might have helped to elect representatives who are more likely to provide more education because of a shared ideology with young voters. In Appendix F, we address this issue in detail by looking at changes in both the characteristics of state legislatures and the identity of elected governors. However, we do not find empirical support for this hypothesis, since there is no evidence that voters elect representatives that are more liberal, younger, or female in states with preregistration.
5 Preregistration and Student Financial Aid

The state-level estimates provide empirical evidence of the effect of preregistration on government spending through changes in the current component of higher education expenditures. The higher education institution therefore becomes the relevant unit of observation to complement the state-level figures. This section employs the Integrated Postsecondary Education Data System (IPEDS) issued by the Delta Cost Project Database to test whether predictions for the provider of funding, i.e., a state, are mirrored by results for a recipient of that funding, i.e., a higher education institution.

For the period between the academic years 1987-1988 and 2011-2012, the Delta Cost Project Database provides annual data for individual colleges, universities, and technical and vocational institutions in the U.S., whether public or private, for-profit or not-for-profit. The data includes student financial aid, enrollment, and institutional and financial characteristics. The IPEDS consists of three matched datasets that cover the waves 1987-2012, 2002-2012, and 2007-2012. The number of institutions surveyed in each dataset grows in each subsequent wave. We focus on the 2002-2012 wave, which on average consists of 3968 institutions distributed over 50 U.S. states, plus the District of Columbia, and 1237 counties. To construct controls for the analysis, we supplement the IPEDS with data collected from various sources for a number of socioeconomic and geographic variables at the county level, including population, personal income, geolocalization, and surface area. Appendix E provides variable definitions and data sources.

Summary statistics for the main variables are reported in Panel 1 of Table B8 of Appendix B. Public financing varies widely across higher education institutions. Student financial aid includes institutional, state, and federal (or Pell) grants, which represent respectively 15%, 16%, and 60% of total student aid. The recipients of these grants represent respectively 30%, 25%, and 50% of the total number of full-time first-time degree seekers. For state grants, eligibility is tied to residency, as determined by requirements encoded in state statutes or established by state agencies of higher education. About 95% of total enrollment consists of state residents. Information is also provided on enrollment by race and gender, as well as characteristics of the institutions, such as qualitative ranking according to the Carnegie Classification, the types of degrees being offered, whether

---

37 We focus on the 2002-2012 wave since it includes the largest number of states which have adopted preregistration. In addition, it is preferable to the 2007-2012 wave since it considers a longer pretreatment period and to the 1987-2012 wave since it suffers less from sample attrition related to the selective erosion of the initial sample over the waves.

38 The sum of the percentages of types of grant within total student aid is less than 100% since total student aid also includes local grants, which account for only a small share and therefore are not reported here. The sum of the percentages of recipients is in contrast larger than 100%, since students can receive multiple grants. This is because federal grants are awarded on the basis of financial need, while institutional and state grants are provided also on the basis of academic merit.
the control is public or private, and the structure of tuition.

5.1 Empirical Strategy

The estimation of the effect of preregistration on education funding allocation at the level of the educational institution may be problematic due to the high level of heterogeneity among institutions, which is spatial in nature and tends to vary with the business cycle. Indeed, a myriad of time-varying spatial heterogenous factors, such as local shocks to the demand and supply of education, other than preregistration, may affect the distribution of funding.\footnote{A vast empirical literature highlights the spatial variation in education provision in the U.S. See Goldin and Katz (1999) for an overview.} Thus, the use of an empirical approach that exploits all cross-state variation and accounts for place and time fixed effects would ignore such spatial confounds and the estimator would be subject to an omitted variable bias.

In order to reduce the effect of unobservable heterogeneity in education funding, we focus on a comparison of institutions between contiguous counties that belong to different states. By using only variation in the voting reform within U.S. county pairs that straddle a common state border, we are able to exploit policy discontinuities at state borders and identify the effect of preregistration. This is beneficial because underlying economic fundamentals are expected to evolve more similarly in contiguous counties than across states or randomly paired counties.\footnote{A county-pair identification strategy has been used by, among others, Dube, Lester, and Reich (2010) to estimate the labor market effects of the minimum wage and by Naidu (2012) to estimate the effect of disenfranchising the blacks on economic outcomes.} Figure B3 of Appendix B displays the location of the border-county pair on a map of the U.S. which distinguishes between counties in states that have introduced preregistration and those in states that have not as of 2012.

Summary statistics for the border-county pair sample are reported in Panel 2 of Table B8 of Appendix B. Among the 3108 counties in the lower 48 states, 1139 lie along a state border. We have a full set of institutional data for 310 border counties.\footnote{If within a border-county pair there is no data available for any of the 11 years in one of the adjacent counties, then the pair is dropped from the sample. For this reason, Delaware is not part of the sample. Alaska and Hawaii are excluded from the lower 48, since they do not share a border.} This yields 226 distinct border-county pairs. Of those, 65, formed by matching 85 counties, have a different registration rule at some point in the sample. Although restricted, the border-county pair sample displays strong similarities with the all-county sample in terms of student financial aid, enrollment, and characteristics of the higher education institutions.

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

\[ G_{i,p,t} = \beta \cdot \text{Preg}_{s,t} + \delta_{c} + \delta_{p,t} + \theta \cdot X_{i,p,t} + \lambda \cdot Z_{i,p(c),t} + \epsilon_{i,p,t} \]
where \( G_{i,p,t} \) is the primary outcome for higher education institution \( i \) in border-county pair \( p \) in year \( t \); \( Preg_{s,t} \) is a dummy variable which takes value 1 if state \( s \) to which institution \( i \) belongs has enacted preregistration in year \( t \), and 0 otherwise; \( \delta_c \) denotes fixed effects for county \( c \) in which institution \( i \) is located; \( \delta_{p,t} \) represents border-county pair-year fixed effects; \( X_{i,p,t} \) are time-varying characteristics for institution \( i \) in border-county pair \( p \); and \( Z_{i,p(c),t} \) are average time-varying characteristics for higher education institutions \( \bar{t} \) located in a county adjacent to \( c \) in border-county pair \( p \), which is denoted \( p(c) \). We note that counties can belong to multiple border-county pairs, thereby inducing a mechanical correlation in the unobservables across pairs and potentially along an entire border segment.\(^{42}\) To account for this correlation as well as the serial correlation within a state, we cluster the error term \( \varepsilon_{i,p,t} \) by state and border segment.

The key to identification in this approach is the border-county pair-year fixed effect. This term captures all possible spatially distributed yearly shocks that may jointly affect contiguous institutions located in a border-county pair, such as the cross-border movements of students or spontaneous student activism. The vector \( Z_{i,p(c),t} \) controls for local shocks which may affect the neighboring higher education institutions in the contiguous counties. For instance, assuming that education fund allocation depends on the quality of the institution, the demand for education, the number of students, the size of ethnic minorities, female representation, etc., if there is a shock in the neighboring institution which affects one of these variables, failure to control for them may lead to biased estimates. Moreover, we include county fixed effects in order to absorb permanent unobserved characteristics of the county, such as local political preferences and specific labor market conditions, and also the vector \( X_{i,p,t} \) to control for the potential confounders reflecting the characteristics of educational institutions listed in Table B8.\(^{43}\)

This rich set of controls implies that the coefficient \( \beta \) should capture the average effect of preregistration on the distribution of public funding received by higher education institutions, since it reflects only the within-pair variation in preregistration adoption across border-county pairs over time. Furthermore, as highlighted in Section 2.1, the fact that preregistration changes are exogenous from the point of view of an individual institution, whose allocated funds are in many states managed by an independent state agency only partially linked to the government, makes it relatively straightforward to identify the effects of the registration reform. This empirical approach does of course have drawbacks. First, it exploits variation between contiguous counties straddling a common state boundary, thus capturing local average treatment effects. However, as shown in

\(^{42}\)A border segment is defined as the set of all counties on both sides of a given state border.

\(^{43}\)We also include time-varying observable characteristics for county \( c \) and the adjacent county \( p(c) \) in order to control for potential confounders associated with the demographic and socioeconomic features at the local level listed in Table B8.
Section 4, preregistration has an impact on national education expenditure, which also ensures the presence of average treatment effects nationwide. Second, the estimator is based on the assumption that 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. If this assumption is violated, then the strict exogeneity at the border-county pair level would be violated and the control groups would be improperly constructed. Even in this scenario, however, the size of the bias would likely be much smaller than the potential bias generated using an all-county sample and accounting for place and period fixed effects.

Furthermore, apart from local and geographically distributed shocks, there may be other specific individual shocks correlated with preregistration and education fund allocation, which may not have been controlled for and which may affect the estimation. We shall address this concern by means of a number of robustness checks.

5.2 Results

Table 5 presents the estimation results for the effect of preregistration on education fund allocation. The two primary outcome measures are state grants and the number of state grant recipients. We focus on these measures since they are related to the state-level budget and directly targeted at the young and, therefore, are potentially affected by the introduction of preregistration.

In Model 1, the dependent variable is the share of state grants within total student aid, while in Model 3 it is the share of state grant recipients within full-time first-time degree seekers. In both models, we find a statistically significant and positive effect, implying an average increase of 4.3 percentage points in the share of state grants and 7.1 percentage points in the share of state grant recipients in the post-reform period. In Models 2 and 4, we augment the basic specifications with two leads and one lag. The estimation results show that there are no pre-trends in state-provided student financial aid or in the number of recipients in the years leading up to preregistration. Moreover, the effect manifests itself in the year following the enactment of the law in a manner consistent with the prediction of the state-level analysis.

---

44 The presence of cross-border spillover includes the possibility of students leaving the untreated county for the within-pair treated county. However, this movement cannot be the result of larger grants allocated in the treated institutions since eligibility for state grants is tied to residency. Therefore, it is unlikely that the estimates are vulnerable to bias related to substitution effects due to treatment.

45 Figure B4 of Appendix B provides a visual inspection of the parallel trends assumption following an empirical strategy similar to that adopted for Panel a in Figure 1, with reference to state grants within total student aid and state grant recipients within full-time first-time degree seekers. The figure shows that the hypothesis of no difference in the annual change of the outcomes of interest between the treated and untreated states cannot be rejected at any level.
Table 5: Preregistration and State-Level Financial Aid

<table>
<thead>
<tr>
<th>% State Grants</th>
<th>% Recipients</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) (2) (3) (4)</td>
<td></td>
</tr>
<tr>
<td>Preregistration 0.043*** 0.028</td>
<td>0.071*** 0.019</td>
</tr>
<tr>
<td>(0.013) (0.018)</td>
<td>(0.032) (0.026)</td>
</tr>
<tr>
<td>F(1)Preregistration -0.011 -0.003</td>
<td>0.030</td>
</tr>
<tr>
<td>(0.026) (0.014)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>F(2)Preregistration 0.002 0.030</td>
<td>0.018</td>
</tr>
<tr>
<td>(0.018) (0.024)</td>
<td></td>
</tr>
<tr>
<td>L(1)Preregistration 0.038* 0.134***</td>
<td>0.134***</td>
</tr>
<tr>
<td>(0.020)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>County FE Yes Yes Yes Yes</td>
<td></td>
</tr>
<tr>
<td>Border-County Pair-Year FE Yes Yes Yes Yes</td>
<td></td>
</tr>
<tr>
<td>Dependent Variable Mean 0.151 0.151</td>
<td>0.237 0.237</td>
</tr>
<tr>
<td>R-squared 0.482 0.506</td>
<td>0.487 0.499</td>
</tr>
<tr>
<td>Observations 6945 4781</td>
<td>8380 5734</td>
</tr>
</tbody>
</table>

Note: State and border-segment level clustered standard errors are in parentheses. Significance levels: * 10%, ** 5%, *** 1%. All regressions also control for: 1) Institutional Variables: Full-Time First-Time Degree Seekers, Non-Resident Enrollment, White, Hispanic, and Black Enrollment, Share of Students with Loan, Carnegie Classification, Institutional Control and Sector, Medical Degree, Flagship, Has Hospital, Land Grant, and Hispanic Serving Institution, Historically Black College, Tuition Reliance, Total Expenditures, In-State and Out-of-State Tuitions; 2) Socioeconomic and Geographic Variables at the county level: Population, Personal Income, Latitude, Longitude, and Surface Area.

In order to ensure that the results in Table 5 are driven by increases in the numerators, rather than by variations in the denominators, in Table B9 of Appendix B we re-estimate the empirical model for the amount of state grants (Model 1) and the number of state grant recipients (Model 2). The introduction of preregistration has a statistically significant and positive effect equal to 42.8% and 38.6%, respectively. This implies that, at the mean, preregistration leads to an increase in state grants of about $1640 per full-time first-time degree seeker and 53 additional state grant recipients per institution.46 Moreover, the table repeats the estimation for federal (Models 3-4) and institutional (Models 5-6) grants and the corresponding number of recipients. These results can be interpreted as falsification tests for the estimates, since grants not related to the state budget should not be affected by preregistration. Reassuringly, the results show no significant effect for preregistration on the amount of the grants, nor on the number of recipients.47

46Table B10 of Appendix B presents the estimation results for the all-county sample, with county and year fixed effects and a state linear time trend. The coefficient for the impact of preregistration exhibits a downward bias in all the models and is not significant for the share of state grants in total student aid. The results reflect the presence of spatial heterogeneity in the distribution of education and the improper construction of the control group.

47Table B11 of Appendix B presents results for applications for admission. Model 2 reveals that the increase in the number of applications in the post-reform period works by way of its effect on the number of female applicants, which is 19.3% higher in treated institutions than in untreated ones. This is qualitatively consistent with the literature on the link between student aid and attendance rates (see, e.g., Epple, Romano, Sarpça, and Sieg, 2013, and Abbott, Gallipoli, Meghir, and Violante, 2016).

30
5.3 Robustness

The main identification assumption underlying the empirical approach is that there is no omitted variable affecting the outcomes in a similar manner to the introduction of preregistration. In order to confound the interpretation of the results as the effect of registration reform, in this section we check the robustness of the estimates by constructing two different placebo specifications. First, we match each state-border county with all its adjacent counties lying on the border within the same state. For each within-state county pair, one county is counterfactually assumed to be affected by preregistration, while the other is not. The rationale behind this is that in the absence of local shocks at state boundaries counties belonging to the same state should not differ in terms of student aid allocation.\footnote{When estimating the effect of the fictitious placebo preregistration on state grant allocation, we use the same specification of the main empirical model but employ a within-state county pair sample consisting of a full set of institutions located in 263 counties and 175 county pairs.} Second, we form pairs of institutions within the same county. For each within-county institution pair, we fictitiously assume that one institution is affected by preregistration while the other is not. Again, the rationale behind this is that in the absence of institution-specific shocks we should observe no difference in the effect of preregistration on student aid allocation between a pair of institutions within the same county.\footnote{Since matched institutions are located in the same county, we include here institution pair-year fixed effects, while disregarding controls at the county level. The within-county institution pairs sample consists of a full set of 1056 institutions and 6983 institution pairs.} Table B12 of Appendix B reports the estimation results for the effect of fictitious placebo preregistration on the shares of state grants and state grant recipients for both falsification exercises. Reassuringly, we do not find any significant effect of preregistration on state grants and the number of recipients in the case of county pairs in the same state (Models 1-2), nor in the case of matched institutions within the same county (Models 3-4). The results of both falsification exercises serve as compelling evidence that the actual timing of preregistration is central to our main empirical result since neither local nor institutional shocks impinge on the inferences we draw.

Collectively, these results paint a consistent picture. Preregistration laws have a sizable effect on the distribution of student aid through changes in state grant allocation across U.S. higher education institutions in view of a potential de facto youth enfranchisement episode.

6 Political Participation

The previous analysis indicates that the adoption of preregistration laws has raised expenditure on higher education by a non-trivial amount. We interpret these results as
reduced-form estimates. This leads us to shed light on possible mechanisms driving the effect of the registration reform on fiscal policies targeted at the young. We focus on the mechanism presented in Section 3, whereby politicians may have adopted policies that are more aligned with the preferences of the young in response to the increase in their political participation following the passage of the law. To test this hypothesis we use stacked cross-sections of individual-level voting records from the Voting and Registration Supplement of the Current Population Survey (CPS) carried out bi-annually by the U.S. Census Bureau during the period 1996-2014. The sample is confined to individuals aged 18-90 who report having voted and/or registered.\footnote{Available individual-level data provides information on voting and registering behavior for Congressional and Presidential elections. The assumption underlying the analysis is that changes in voting and registering patterns at the federal election level following registration reforms are mirrored in similar changes at the state and local levels. This is particularly plausible in those states where all types of elections are held on Election Day.}

Table B13 in Appendix B presents the summary statistics for the main variables. The residents of the states with a preregistration provision represent 11\% of the sample. On average, 63\% of the respondents report having voted and 79\% report having registered. Respondents are grouped into two age categories: young and old. Young respondents aged 18-24 represent 11\% of the sample, while women account for 53\% and blacks for 9\%.\footnote{As a robustness check, we use other age cutoffs, such as defining the youngest age group as 18-22 or as 18-29, with no substantive effect on the main insights of the analysis.} Average family income is between $35000 and $39999, while 33\% of the sample are not participating in the labor force. Data is also presented for siblings and children, metropolitan city status, and educational attainment. Appendix E provides detailed information on variable definitions and data sources.

6.1 Empirical Strategy and Results

Preregistration is an electoral provision targeted at young soon-to-become voters. Thus, the age of individuals is a dimension along which the treatment varies, in addition to space and time. It is then appropriate to employ a triple-differences (hereafter DDD) regression design in order to test the link between preregistration and political participation of the young. In this context, the DDD estimator is the difference-in-differences for the young and the old between states with preregistration and states without. In this way, both within-state and within-age group time trends are differenced out from the empirical results. Indeed, the DDD estimator is immune to both state-specific shocks – such as a transitory increase in the political participation of individuals of all ages in the preregistration states – and young-specific shocks – such as fluctuations in the political participation of young individuals across states. Hence, the identification assumption
for consistency of the DDD estimates relies on the absence of shocks during the sample period that differentially affect the political participation of the young only in the preregistration states.\footnote{Figure B5 of Appendix B provides visual evidence to assess the validity of the parallel trends assumption required for consistency of the DDD estimator. It traces out the average year-by-year difference in the voter turnout of the young between treated and untreated states in the pre-treatment period. To show this, we limit the sample to young respondents and regress voter turnout on year dummies and interactions between year dummies and a dummy for treated states, dropping observations once preregistration occurred. The figure suggests that the hypothesis of parallel trends in voter turnout cannot be rejected at any level.} Following Yelowitz (1995), the empirical model to be tested has the following specification:

\[ V_{i,a,s,t} = \beta \cdot Preg_{s,t} \cdot Young_{i,s,t} + \delta_{s,t} + \lambda_{a,t} + \theta_{a,s} + \pi \cdot X_{i,a,s,t} + \varepsilon_{i,a,s,t} \]

where \( V_{i,a,s,t} \) is a dummy variable that takes value 1 if individual \( i \) belonging to age group \( a \) in state \( s \) in year \( t \) has registered or voted, and 0 otherwise; \( Preg_{s,t} \) is a dummy variable for whether state \( s \) has adopted preregistration in year \( t \); \( Young_{i,s,t} \) is a dummy variable for whether individual \( i \) living in state \( s \) is aged 18-24 in year \( t \); \( \delta_{s,t} \) denote state-by-year fixed effects and are meant to control non-parametrically for state-specific shocks over time; \( \lambda_{a,t} \) and \( \theta_{a,s} \) 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 for the young nationwide and time-invariant characteristics of the young in preregistration states, respectively; \( X_{i,a,s,t} \) is a vector of time-varying individual respondent characteristics; and \( \varepsilon_{i,a,s,t} \) is the error term which we cluster by state. This rich set of controls implies that the parameter \( \beta \) should capture the average treatment effect.

Table 6 presents the estimation results. In Model 1, we test for the effect of preregistration on political participation of the young, as reflected in level of registration. We find a statistically significant increase of 3.8 percentage points in registration. This is equivalent to a 6.4% increase with respect to the sample mean. In Model 2, we run an analogous estimation to test for the effect of preregistration on young voter turnout. The point estimate is of similar magnitude to that in Model 1 and yields an increase of 8.2% with respect to the sample mean.\footnote{The estimation results are consistent with those obtained by Holbein and Hillygus (2016) who find a positive impact of about 8% for preregistration on the turnout of young voters in Florida.} Hence, preregistration accomplishes the task of boosting the political involvement of young citizens by enlarging the potential electorate and increasing their actual participation at the polls. In Model 3, we check for unusual patterns in the voter turnout that precede the preregistration year. To this end, we include a lead in the specification of Model 2.\footnote{Since data are biannual, each lead and lag corresponds to a shift of preregistration either backwards or forwards by two years. The estimation results are robust to the inclusion of additional leads.} Preregistration continues to exert a positive and significant effect, while the anticipatory effect is not statistically different...
from zero. Thus, we can confirm the absence of pre-trend differences in voter turnout between groups within states. In Model 4, we augment Model 3 with a lag in order to evaluate the incremental effect on young voter turnout during the post-treatment period. The point estimates are statistically significant and exhibit an increasing pattern during the first two elections after treatment in the order of 3 percentage points for the first election and 2.3 for the second. From a dynamic perspective, this suggests that the increase in political participation of the young following preregistration may continue into the future and thus reach a higher level than that found in Model 4. Such an incremental and persistent impact on voter turnout is likely to be taken into account by politicians when setting fiscal policies that target young voters’ needs.

While the results above indicate that preregistration mobilizes a large share of the young electorate to the polls, they are unable to identify the newly de facto enfranchised young voters. In order to do so, we interact $Preg_{s,t} \cdot Young_{i,s,t}$ with voter characteristics. Model 5 looks at the impact of family income and reveals a stronger effect of preregistration on voter turnout among young voters belonging to poor families.\(^\text{55}\) This result suggests

\(^{55}\)When we interact $Preg_{s,t} \cdot Young_{i,s,t}$ with other voter characteristics, such as gender, race, educa-
that preregistration reduces the costs of registration and thus makes voting easier for young individuals from low-income families who tend to exhibit low levels of political participation and are at the same time more in need of student aid. These voters are also more likely to vote for a governor supporting youth-oriented policies, regardless of her party affiliation, even when the fiscal shift towards their needs is not that large.\footnote{For an analysis of the link between income and voter turnout, see Leighley and Nagler (2013).}

The empirical results suggest overall that state-level politicians respond to change in electoral composition following the de facto enfranchisement event of the 2000s and in a manner consistent with the theoretical framework in Section 3. However, care must be taken in using the estimates to compute implied elasticities. Indeed, the change in young voter turnout cannot be interpreted as the only channel linking preregistration and the increase in spending on higher education. A number of other complementary channels activated by the registration reform may be operating at the same time. For example, preregistration may have induced not only the young to vote, but also their parents. Through voter education programs connected with preregistration, the young might receive information about registration and voting procedures and subsequently engage in political discussions with their parents and/or siblings at home. This information transmission may lead to an alignment of voting attitudes among family members. To test this hypothesis, we run a variant of the main specification, in which respondents with children in preregistration states are used as the treatment group, and we limit the sample to parents who are currently married.\footnote{To capture intra-household spillovers, we drop separated and divorced individuals from the sample since their inclusion would bias results downward if the child does not live with both parents.}

We replace $Young_{i,s,t}$ with $Parent_{i,s,t}$, where the latter is a dummy for whether individual $i$ living in state $s$ has children of any age in year $t$. Model 6 of Table 6 reports the estimation results, which show that preregistration increases the voter turnout of parents by 1.2 percentage points. Strikingly, parents with children of college age who are eligible for preregistration fully account for this increase, as shown in Model 7 where we interact preregistration with $Parent$ with College-Age Child,$_{i,s,t}$, a variable meant to capture whether the respondent has children aged 16-20. This result supports the hypothesis of intra-household spillovers associated with the transmission of information about the registration and voting procedures following preregistration, which may have increased parent turnout as well. Since parents with children of college age presumably have preferences for education expenditure that are similar to those of the young, this additional voter mobilization may have further encouraged politicians to pursue youth-oriented policies. Clearly, other transmission channels linking preregistration to political responsiveness, such as, for example, political activism...
or lobbying, may be at work here with similar effects.

7 Conclusions

This paper investigates the effect of preregistration laws on government spending and political participation in the U.S. Preregistration allows individuals to complete their registration application sometime 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 across states, we show that preregistration leads to a 6% increase in total per capita education expenditure at the state level and a 4.3 percentage point increase in the share of state grants within total student aid at the level of institutions of higher education. The results also produce evidence of a shift in electoral composition toward a greater representation of the young in the order of 8.2% in the post-reform period. Consistent with the predictions of a political economy model of distributive politics, the results collectively suggest political responsiveness to the needs of the newly-enfranchised constituent group.

A caveat to be considered is that the results may apply only to a specific country and 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 U.S. context. Nonetheless, the analysis reinforces a common insight from political economics, i.e., that increased electoral participation by a politically disadvantaged group is a precondition for the advancement of policies that benefit it. Thus, recent attempts to roll back preregistration in some U.S. 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 to their benefit, such as the provision of public education.

Whether similar results can be replicated in different contexts is a question ripe for investigation, especially since youth disenchantment with the ballot is becoming a growing phenomenon across democracies. Many European countries, such as Austria, Germany, Norway, and the UK, are considering whether to lower the voting age from 18 to 16 as part of an effort to promote more active social and political engagement among the young. Our empirical results confirm that we can expect a stronger impact for electoral reform on public policy in countries characterized by weak political competition, high inequality and, as predicted by the model, an aging population. Future research should

---

58 On the debate on whether to lower the voting age to 16, see economist.com/news/leaders/21716030-young-voters-are-becoming-disillusioned-elections-catch-them-early-and-teach-them-value.
investigate these issues in different settings.

References


