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The Impact of Political Party Control on Education Finance and Outcomes: Evidence from U.S. States

Mark J. Chin
Harvard University

Lena Shi
Harvard University

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The Impact of Political Party Control on State Education Finance and Outcomes

Mark J. Chin & Lena Shi*

ABSTRACT:

Given states' balanced budget requirements, investment decisions often involve trade-offs between policymakers' budget priorities. How does political party control affect investment decisions and outcomes? Using a regression discontinuity design based on close state elections between 1984-2013, we find that marginally Democratic legislatures spend more on higher education in states with higher unemployment and poverty rates. However, Democrats spend less on K-12 education, particularly in more liberal states. Democrats do not appear to decrease K-12 spending to increase higher education, but rather, to fund welfare. Gains in local revenue offset party differences in K-12 spending such that current expenditures in K-12 increase and student-teacher ratios decrease under Democrats, though not enough to change attendance rates. Increased welfare spending under Democrats appear consistent with their redistributive goals that also benefit their constituents, as health insurance coverage for non-White children also expand. Altogether our results indicate that policymakers make spending decisions while considering constituents' needs and the availability of external budget sources.

*Mark J. Chin and Lena Shi are co-equal first authors. Chin: Harvard Graduate School of Education, 13 Appian Way, Cambridge, MA 02138 (mark_chin@g.harvard.edu); Shi: Harvard Graduate School of Education, 13 Appian Way, Cambridge, MA 02138 (email: yulenashi@g.harvard.edu) We thank David Deming, Chris Avery, Marty West, Bridget Terry Long, Larry Katz, Kosuke Imai, Tom Kane, Josh Goodman, AEFP conference participants, and PIER seminar participants for valuable guidance and feedback. We thank David Deming for access to Grapevine Survey data and gratefully acknowledge Kirsten Slungaard Mumma, Rafael Treviño and David Manikas for the teamwork in transcribing the data.

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Research shows that education funding is critical for improving student outcomes (Abott et al. 2020; Deming and Walters 2017; Candelaria and Shores 2017; Hyman 2017; R. Johnson and Jackson 2017; Jackson, Johnson, and Persico 2016; Bound and Turner 2007). Voters also recognize the importance of education: they have ranked education as a top policy priority and 73 percent of the public opposes cutting education to balance the budget (Gallup 2020; Pew Research Center 2010). Yet states' education is one of the most common line items to get cut when budgets are tight (Delaney and Doyle 2011; Johnson, Oliff, and Williams 2011; Kane, Orszag, and Apostolov 2005; McGranahan 1999). Spending cuts may exacerbate variation between states, as per-pupil education investments in education differ by as much as 313% for K-12 or 668% for higher education (Tandberg and Laderman 2018; Annual Survey of School System Finances 2016).

States' level of education investment depends not only on tax revenues available, but also on legislators' preferences for how much to elevate education spending relative to other competing budget priorities. Given states' restrictions against running deficits, policymakers may confront difficult trade-offs when balancing the state budget (Bohn and Inman 1996; Poterba 1994). Legislators may rely on their party affiliation to make decisions on how to tax and spend; Democrats have a stronger reputation than Republicans for increasing taxes, spending, and redistribution (Sieg and Yoon 2017; Reed 2006; Besley and Case 2003; Alt and Lowry 1994). In 1999, education was the top policy priority for both parties but is no longer on Republicans' top five issues today (Pew Research Center 2019). The significant increase in partisanship since the 1980s (Caughey, Warshaw, and Xu 2017) makes understanding the role of party control on investments and trade-offs particularly relevant.

In this study, we investigate the effect of state legislatures' party control on investments in different government functions between 1984-2013. We also explore potential explanations for any partisan effects on spending decisions. We focus primarily on education, the largest state budget item until Medicaid surpassed it in 2015 (Urban Institute, 2017). State education finance is also critical to K-12 schools and colleges because states provide the largest source of their revenue (NCES 2018 Table 235.10; NCES 2018 Table 333.10).

On average, Democrat-controlled states in our sample spend more per pupil than Republican-controlled states on higher education but less on K-12, at least until about 2010 (Figure 1). Simple OLS estimates show that Democrats spend substantially more on higher

education and less on K-12, even after controlling for economic and demographic factors (Appendix Table 1). However, these differences may still be biased because the party in control of state legislatures is not random. Party control may relate to economic conditions, education quality, voter preferences, and other confounding factors that also affect education spending and outcomes. In the ideal experiment to isolate the effect of political party, we would compare appropriations between states identical on all characteristics except party control of the legislature. Given the infeasibility of randomly assigning political party to states, we overcome bias by using a regression discontinuity (RD) design.

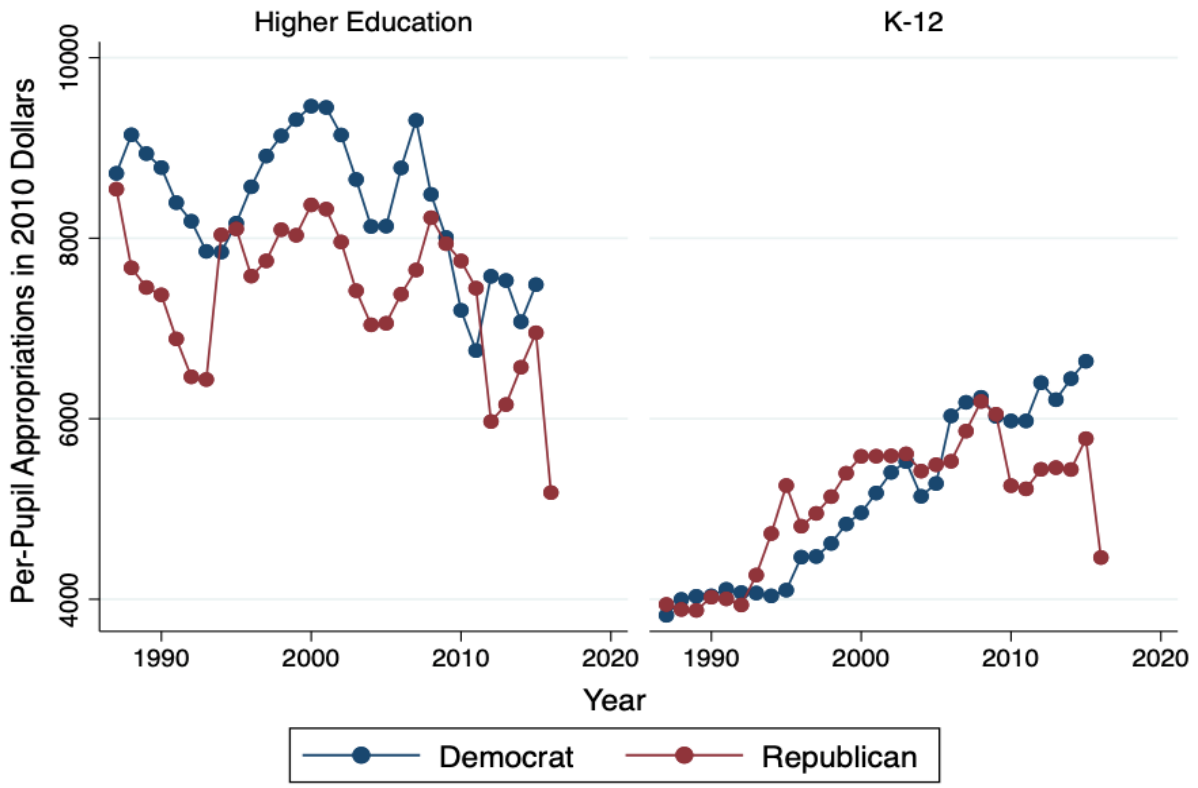

FIGURE 1: AVERAGE STATE APPROPRIATIONS FOR HIGHER EDUCATION (LEFT) AND K-12 (RIGHT) BY PARTY IN CONTROL OF STATE HOUSE OVER TIME

Our RD design leverages close legislative elections between 1984 and 2013 to identify the causal effects of state legislatures' political party on education finance, other budget priorities, and downstream outcomes. To do so, for each state we aggregate voting results across electoral districts to calculate the strength and margin of Democratic victory (or loss) in the

legislature, which we then use as the RD running variable. Our measure of Democratic control is therefore a function of both the number of seats needed to secure majority control and the margins of victory for these seats. We then identify the effect of political party on spending decisions by comparing states with narrow Democratic wins or losses, controlling for state and year fixed effects.

Using data from the State Higher Education Executive Officers and Common Core of Data, we compare total and per-student education spending and outcomes between states just above and below the cut-off for securing majority Democratic party status. We then explore the effects of state party control on other budget categories using historical U.S. Census Bureau data. The main threat to identification is that other treatments occur at the cut-off for Democratic control. We provide evidence of the internal validity of our estimates below using covariate balance tests and a McCrary test for changes in density around the cut-off. Furthermore, state-level elections are less susceptible to factors that affect validity, such as manipulation from pre-election behavior or the characteristics of candidates (de la Cuesta and Imai 2016).

Our RD estimates show that marginally Democratic state legislatures spend 6.5 percent less than Republicans in K-12 education appropriations per pupil, two years after an election. This result is statistically significant and robust to various regression specifications and samples. Despite less state support under state legislatures with slim Democratic victories, districts raise revenues to counteract the party difference such that K-12 current expenditures still increase on net by 1.5 percent, consistent with other findings that local spending can offset state funding (Baicker and Gordon 2006; Card and Payne 2002). Our results imply that under states legislatures with weak Democratic control, increases in non-state revenue sources such as local property taxes help stabilize education expenditures over time. However, the small difference in K-12 expenditures between parties may explain why we observe just slight decreases to student-teacher ratios under Democrats and no differences in attendance rates, consistent with Dynes and Holbein (2020).

In contrast, legislatures under marginal Democratic control increase higher education appropriations by 3.6 percent total and 2.3 per-pupil. Estimates for this category of state educational spending, however, are smaller in magnitude than those for K-12, are more sensitive to a variety of robustness tests, and are not accompanied by any significant changes in

enrollment[1] to public postsecondary institutions after correcting for bias and accounting for serial correlation of errors within states over time. States in our sample with higher baseline unemployment and poverty rates appear to drive the partisan effects on higher education expenditures, which suggests that Democrats may at least view college investments as one way to improve economic mobility and bolster a lagging economy.

We find other evidence that Democrats' spending decisions depend on economic circumstances and competing priorities, rather than trade-offs within the education budget. We show that investment in higher education, a discretionary item that occupies a smaller share of the budget, does not alone offset the K-12 decrease. Instead, Democratic-controlled legislatures appear to trade-off K-12 spending for welfare spending, which primarily consists of Medicaid. Because of who benefits from welfare spending, our results are consistent with evidence highlighting that Democrats legislators are more likely to vote congruently with lower income voters than Republicans (Brunner, Ross, and Washington 2013). Welfare spending, in other words, may be a more targeted form of funding that Democrats use to achieve redistributive goals that also help their constituents more effectively than K-12 education spending. The magnitude of Democratic increases to welfare spending in our data completely offset Democratic decreases to K-12 spending. Furthermore, the effects on K-12 spending are consistently negative over time and frequently occur alongside positive increases to welfare spending in the same election cycles (particularly starting in the 1980's) or contexts.

One possible explanation for our results is that a rise in Medicaid and welfare spending, which receive federal funding matches based on caseloads, crowds out education, which lacks this budget structure. However, this explanation alone is unlikely to explain why we observe partisan differences. Our heterogeneity analysis instead reveals the possibility that Democrats make spending trade-offs by considering constituents' ideologies and needs; Democratic decreases in K-12 education are driven by states with relatively more liberal citizens. Furthermore, the stabilizing effect of local appropriations to K-12 suggest that Democratic state legislatures may be aware that they can address poverty through health and welfare investments while also maintaining education spending levels using other funding sources. For example, K-12 and welfare effects are largest after elections occurring in the 1980s and 1990s, when changes to welfare policy are more likely to have increased public healthcare eligibility specifically for

[1] Undergraduate and graduate enrollment

pregnant women and children (Centers for Medicare & Medicaid Services 2015). Indeed, we find that, in the years after Democrats secure a narrow majority in state houses, health insurance coverage increases for non-White children in particular. Together with evidence that states with higher unemployment and poverty drive the increases in Democrats' higher education spending, our results provide more nuance to the common perception that Democrats spend more on public education overall. Policymakers likely consider how different state spending choices serve redistributive and economic goals—and the availability of external budgets.

Our paper contributes to three literatures. This is the first paper, to our knowledge, to evaluate party differences on all major state spending categories, ranging from education to welfare. The question of whether and when party differences exist remains an active area of research without consensus. Recent studies showing no or few differences in policy outcomes by party for mayors (Ferreira and Gyourko 2009) and state offices (Dynes and Holbein 2020; Leigh 2008) contradict other studies documenting party differences on fiscal policy (Reed 2006; Besley and Case 2003; Poterba 1994; Alt and Lowry 1994), environmental enforcement (Konisky 2007), incarceration rates and sentencing (Cohen and Yang 2019; Yates and Fording 2005), and fair employment legislation (Chen 2007). While policymakers may not always be able to control the effects of their policies, they do have direct authority over budgetary decisions. Studies suggest that policymakers make budget decisions based on their constituents and election pressures (Bertocchi et al. 2020; Bostashvili and Ujhelyi 2019; Chatterji, Kim, and McDevitt 2018; Aghion et al. 2009; Brender and Drazen 2008). Therefore, this paper's evaluation of budget and spending choices thus provides direct insight into parties' policy priorities and trade-offs.

Second, our paper builds on literature documenting determinants of education finance and state spending by describing conditions when partisanship affects different categories of state spending. Notably, this paper identifies how Democrats do not necessarily treat K-12 and higher education spending the same. Even though education budgets share similar functions, party effects for K-12 and higher education show different signs and magnitudes under various economic and political conditions. Evaluating the effects of partisanship on spending and when partisanship matters builds on the growing literature of political and economic conditions affecting state investments (Perez, Benitez, and Seiber 2018; Clemens and Miran 2012; Cohen, Coval, and Malloy 2011). Studies have documented how education and other budget categories

can crowd out one another in federal, state, or local budgets (Baicker and Gordon 2006; Gordon 2004; Baicker 2001), but less is understood about the political conditions when these offsets occur. Furthermore, an established body of literature evaluates changes to investments and student outcomes under school finance reforms (Lafortune, Rothstein, and Schanzenbach 2018; Jackson, Johnson, and Persico 2016; Card and Payne 2002; Hoxby 2001) but few studies have documented the role of political party in influencing educational outcomes (Berry 2010).

Finally, the finding that marginal Democrats increase education investments, raise welfare funding, and increase children's health insurance coverage may offer a mechanism to explain differences in economic growth by party. Studies have shown that Democratic control contributes to higher GDP growth, lower unemployment rates, and decreased income inequality by race (Blinder and Watson 2016; Beland 2015; Leigh 2008). A robust literature documents the relationship between human capital investment and wages, as well as human capital and economic growth (Zimmerman 2014; Aghion et al. 2009; Hoekstra 2009; Goldin and Katz 2008). Furthermore, policies that increase health insurance coverage for children and mothers and invest in schools pay for themselves over time (Hendren and Sprung-Keyser 2020). This paper's findings may help explain why economic outcomes improve under Democrats.

The rest of the paper is organized as follows. Section 1 describes the role of state legislators in education finance. Section 2 describes our data and provides summary and descriptive statistics. Section 3 describes the empirical strategy. Section 4 presents the main results along with robustness checks. Section 5 is our conclusion where we discuss these findings.

1. The Role of State Legislators in Education Finance

1.1 State Legislators

Elected by citizens from their districts, state legislators are responsible for budgets, legislation, and responding to constituents. State legislatures except for Nebraska's are composed of two separate legislative chambers. The lower (and typically larger) chamber is the house. With over 7,000 representatives in U.S. state houses, partisanship matters more in the lower chambers because coalitions play a larger role for their governance (Balla et al. 2002). Notably, parties

with majority control can set the legislative agenda to achieve their policy goals (Bianco and Sened 2005; Lee, Moretti, and Butler 2004; Wright and Schaffner 2002), control more discretionary funding decisions (Curto-Grau and Zudenkova 2018), and receive more financial support from regional and federal counterparts when political parties align (Curto-Grau, Solé-Ollé, and Sorribas-Navarro 2018; Albouy 2013).

Making budget appropriations is one of state legislatures' primary responsibilities, and often the main focus of legislative sessions (Abney and Lauth 1987). Depending on the state, the governor may propose the initial budget, but the legislature is responsible for reviewing, amending, and approving the state budget. Studies suggest that legislatures possessed equal budgetary influence as governors in the 1990's and potentially even more influence in the 1970's and 1980's (Abney and Lauth 1998; Dometrius and Wright 2010). But despite the importance of state legislatures and their partisanship, regression discontinuity studies evaluating partisanship effects on education spending have focused solely on governors (Ortega 2020; Beland and Oloomi 2017; Hill and Jones 2017).

Because state legislature elections typically occur in November with legislators beginning their first term the following January, the first budget cycle that legislators can influence typically begins two years after they take office (National Conference of State Legislatures 2008).[2] Notably, though house representatives' terms usually last just two years, studies show that marginally Democratic state legislatures have a lower probability of holding onto their party control long-term (Feigenbaum, Fouirnaies, and Hall 2017). This highlights the importance of the short-term budgetary decisions made by legislatures under weak political party control following elections.

1.2 State Education Finance

Investments in public K-12 and higher education together make up the largest state budget item. K-12 education spending was the single largest budget item from 1977 to 2015, until it was outpaced by Medicaid. States contribute the largest source of revenue for K-12 and higher education during our panel (NCES 2018 Table 235.10; NCES 2018 Table 333.10). States' most common and largest revenue sources are income and sales taxes (CCRSL 2017).

[2] Most states' fiscal years run from July 1 through June 30.

From 1972 to 1987, states' investments in K-12 grew from approximately 10 percent of schools' total revenues to 49 percent, with one study attributing this growth to finance reforms that held states responsible for ensuring equitable and adequate spending across districts (Murray, Rueben, and Rosenberg 2007). Before 1970, local property taxes provided most of the funding for K-12 but were surpassed by states as the main revenue source in 1979 (NCES 2018 Table 235.10; Loeb 2001; Hoxby 1996). However, policymakers can still leverage local property taxes and other sources. Iowa school districts, for example, raised property taxes by 8.8 percent in 2010, two years after Democrats won back the house for the first time in 18 years. Local schools raised property taxes when Democrats, responding to recessionary pressures, decreased state aid to local schools rather than raising taxes. The choices of Iowa's legislators to cut spending rather than raise taxes is also a common way for states to respond when facing budget shortfalls (McGranahan 1999) (see Appendix A for another case study of Michigan on how political parties influence states' school finance).

Colleges and universities have relied on states as their largest source of revenue until tuition and fees revenues surpassed state revenues in 2013-14 (NCES 2018; Table 333.10). The exception is community colleges, which relies primarily on state revenue; states make up 29 percent of their revenues compared to 17 from tuition and fees.

2. Data and Descriptive Statistics

2.1 Data

We investigate the impact of party control of state houses on education finances and outcomes. We build a panel dataset that spans legislative elections occurring between 1984 and 2013 and covers four key categories: election data, state appropriations, downstream outcomes, and other state-level data.

The first category comprises state legislative election data compiled by Klarner (2018) on all 50 states' legislative general election results. These data describe, at the election district level, election type, candidates, vote share, margin of victory, and outcome. For our analyses, we exclude state-election observations with data abnormalities (e.g., missing vote counts; Klarner 2018), primary elections, non-partisan and/or unicameral legislatures (i.e., Nebraska), and state

chambers with multimember districts, following Eggers and Fouirnaies (2014). We also focus on state house elections because many states do not elect every senate seat each election cycle and, as noted above, political partisanship often matters more in these lower chambers. We aggregate district-election results into a single state-level metric describing the Democratic legislative control and the strength of this control in any given year. We describe this measure, which contains information on both the number of seats needed to flip party control of state legislatures and the margins of victory for these seats, in more detail below.

The second category of data captures appropriations. K-12 appropriations come from the National Center for Education Statistics' Common Core of Data (CCD; National Center for Education Statistics, 2020), including various measures of public-school revenue and spending at the state level. The other data source for school spending includes public higher education appropriations from the State Higher Education Executive Officers (SHEEO; State Higher Education Executive Officers Association, 2019). Together, these sources provide information on state funding to K-12 and higher education for every state and year of our panel except Virginia in the 1986-1987 and 1987-1988 school years, for which we only have the latter. Both data from the CCD and SHEEO include student enrollment data at the state level, allowing us to calculate per-pupil finance measures. For non-education budget categories, we use the Historical Database on Individual Government Finances (INDFIN; U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Government, 1967-2017) data from the U.S. Census Bureau. We focus specifically on states' per-capita spending on the largest non-educational budget items, which include highways, public welfare, police, corrections, and health and hospitals.

Third, to investigate impacts on downstream outcomes, we use state-level K-12 data from the CCD on student-teacher ratios and average daily attendance rates. We similarly investigate effects of political party on college undergraduate plus graduate enrollment using data from SHEEO. As we describe in our results, Democratic legislatures may be spending less on K-12 in order to spend more on welfare. To evaluate impacts on welfare-related health outcomes, we supplement these data on downstream educational outcomes with information from the Annual Social and Economic Supplement of the Current Population Survey (CPS). Specifically, we

leverage CPS information on health insurance coverage for different populations across U.S. states.[3]

Our final data sources describe states' economic and demographic conditions. Specifically, in analyses we follow Ortega (2020) and control for other branches of state government's party control and a state's demographic characteristics, unemployment rate, and per-capita personal income using data from the University of Kentucky's Center for Poverty Research (2019). Because in heterogeneity analyses, we explore if results vary by state's political ideology and whether the state's school finance system had experienced significant reform, we control for these measures in analyses as well. For political ideology, we use Berry et al.'s (1998) state-level citizen ideology measure. To identify the presence of significant school finance reform, we flag elections occurring after the year of each state's first substantial legislative reform or first court case overturning the constitutionality of their school finance systems. We collate this information for the years spanning 1970 through 2014 using research conducted by Corcoran and Evans (2015), Downes and Shah (2006), LaFortune, Rothstein, and Schanzenbach (2018), and Shores, Candelaria, and Kabourek (2020).

2.2 Sample and Descriptive Statistics

Because we hypothesize that state legislators may make budget trade-offs between higher education spending, K-12 spending, and spending on other areas, our main analytic sample includes state-election observations without any missing appropriations data. For K-12 appropriations, higher education appropriations, and the INDFIN state government spending outcomes, we have data beginning in the 1986-1987 school year, the 1979-1980 school year, and the 1966-1967 school years, respectively. We focus on elections impacting school years beginning in 1986-1987, when appropriations data on all three sources are available.

Below we explore impacts of Democratic control on school finance and education outcomes one and two academic years after elections, when state legislatures can theoretically influence budgets before the subsequent election. The earliest possible election we consider in

[3] Following Cohodes, Grossman, Kleiner, and Lovenheim (2016), we construct three-year moving averages of health insurance coverage because small sample sizes in the CPS for different subgroups may lead to measurement error.

our data occurs on November 1984.[4] Of the remaining 482 state-elections occurring between November 1984 and November 2013, we then exclude the one observation with missing spending data: Virginia in November 1985, which lacks information on K-12 state appropriations. Our final sample includes 481 state-election-cycles.

TABLE 1. SUMMARY STATISTICS

	Full sample			Close election sample		
	Democratic house (1)	Republican house (2)	Difference p-value (3)	Democratic house (4)	Republican house (5)	Difference p-value (6)
Panel A. Baseline characteristics						
Proportion unemployed	0.060 (0.018)	0.057 (0.019)	0.182	0.060 (0.018)	0.058 (0.018)	0.577
Proportion in poverty	0.132 (0.036)	0.118 (0.029)	0.000	0.125 (0.028)	0.116 (0.028)	0.103
Income per capita (Log)	10.474 (0.197)	10.498 (0.141)	0.141	10.461 (0.119)	10.463 (0.127)	0.943
Citizen liberalness (z-score)	0.463 (0.967)	-0.211 (0.653)	0.000	0.258 (0.628)	0.164 (0.504)	0.376
Democratic governor	0.513	0.422	0.048	0.526	0.533	0.940
Democratic senate	0.751	0.211	0.000	0.456	0.400	0.544
Post first major School Finance Reform	0.726	0.667	0.164	0.561	0.500	0.510
Panel B. Education Spending & Outcomes two years post-election						
Higher education enrollment	234967.22 (293415.28)	196390.41 (194747.48)	0.103	234664.66 (196697.44)	180184.23 (197042.80)	0.137
Higher education state appropriations per pupil	8341.59 (2284.97)	7416.77 (2627.96)	0.000	7297.40 (1589.32)	7249.94 (2197.23)	0.894
K-12 enrollment	1198214.50 (1348139.38)	945981.63 (995438.88)	0.024	1207036.13 (1047752.19)	876980.81 (1005800.25)	0.085
K-12 state appropriations per pupil	5002.43 (1821.15)	5383.45 (1890.55)	0.026	4604.30 (1365.73)	5484.15 (1671.09)	0.002
N	277	204		57	60	

Note: All dollars are in 2010 dollars. Close elections include those where fewer than five seats determine party control of the house, and the margins of victory for each of these seats is less than ten percentage points. Standard deviations in parentheses. *P*-values describe two-sample t-tests comparing means between Democratic and Republican houses.

[4] Assuming the first post-treatment time period is the 1985-1986 school year and the second post-treatment time period if the 1986-1987 school year.

In Table 1, we provide descriptive statistics separately for Democrat-controlled and Republican-controlled houses for all 481 state-election observations in our panel dataset. We also provide these statistics for a subsample of state-election observations with "close elections"—defined as elections where majority control of the house was determined by fewer than five seats, with each legislator of these determining seats winning with a vote margin of less than 10 percentage points. We describe baseline economic and demographic characteristics for these observations in the year of the election and state appropriations to public education two years after elections. Although Democrat-controlled states have a higher poverty rate and are more liberal on average in the full sample, there are no statistically different baseline characteristics in the close-election sample, as seen in Table 1 column 6. Comparing more similar states in the close-election sample provides more confidence in the use of an RD to help identify unbiased effects of party control. In Appendix Figure 1, we document the states that appear in our RD samples and their frequency of appearance. Notably, we find these states to be distributed across the country, assuaging any concerns that our findings may be driven by party control effects in particular states or regions of the U.S.

3. Empirical Methodology

3.1 Estimation Specification

To identify the impact of state legislatures' party affiliation, we compare marginally elected Democratic and Republican majority state legislatures. However, party control of state legislatures likely correlates with other observable and unobservable state characteristics that influence school finance and education outcomes. Thus, to identify causal impacts, we leverage variation in house party control resulting from close elections. Our preferred regression specification is:

$$(1)\quad Y_{it} = \alpha + \beta_1 DemocratMargin_{it} + \beta_2 Democrat_{it} + \beta_3 (DemocratMargin_{it} \times Democrat_{it}) + \delta_i + \gamma_j + \varepsilon_{it}$$

Y_{it} captures inflation-adjusted appropriations to public K-12 or higher education institutions from the state government (or other state-level finance and downstream outcomes) in state i either one

or two school years following election year t. Predictors in this model include $Democrat_{it}$, which indicates a majority Democratic state house, and the running variable, $DemocratMargin_{it}$.

Existing studies arrive at plausibly causal estimates of party control using an RD design, with the Democratic margin of victory, $DemocratMargin_{it}$, serving as the running variable. When electing officials for most levels of executive office (i.e., governor, mayor, president), the margin of victory from a single election determines party control. However, party control of state houses instead depends on winning enough district-level elections.

We therefore calculate $DemocratMargin_{it}$ by aggregating multiple election results based on the share of seats and vote percentages that Democrats need to secure majority control. First, we identify the number of district-level elections, n, required to flip the majority party status of each state house. We then identify the margins of victory (in cases where Democrats have a majority) or loss (in cases where Democrats are in the minority) for the winning (losing) Democratic candidate in the n closest district-level elections. To arrive at state-level estimates for $DemocratMargin_{it}$, we then follow Folke (2014) and sum these n margins to calculate the minimum rectilinear distance to flip party control of the house (i.e., $\sum_1^n(DemocratDistrictMargin)$). We rescale this vote margin or margin of victory measure (the L_1 Norm or the "Manhattan Distance") to be negative when Democrats are in the minority.[5] As such, states with more positive (negative) estimates of $DemocratMargin_{it}$ have state houses that are more strongly Democratic (Republican) because vote margins of victory (loss) across the closest Democratic winners (losers) are large and/or Democrats need to flip more seats to secure the majority. Alternatively, states with $DemocratMargin_{it}$ estimates near zero are not only closer to flipping from Democratic to Republican house control (or vice versa)—they are more plausibly comparable.

In Equation (1), β_1 captures the underlying relationship between Democratic margin of victory in state house elections and education outcomes; β_3 captures any difference in this relationship between majority and minority Democratic state houses; and β_2 captures the effect of interest—the impact of "just barely" achieving a Democratic majority—by leveraging the

[5] In Appendix Tables 2, 4, and 5, we provide results using running variables used in other studies investigating impacts of party control of state legislatures (Feigenbaum et al. 2017). Our results are generally robust to using other measures such as Uniform Swing and Euclidean Distance. In Appendix B, we provide more detail on the estimation of all of these running variables.

quasi-random variation in house control resulting from a series of close district-level elections. The estimated model above also includes state fixed effects, δ_i, and election cycle fixed effects, γ_j, which allow us to account for differences in outcomes across contexts or election cycles.

When estimating the RD model represented in Equation (1), we focus on marginally Democratic or Republican houses close to the threshold for majority party control. This limits the influence of potential outliers in party control on estimates and reduces the chance that results are sensitive to how we model the relationship between the running variable and outcomes. Specifically, we use a local linear regression with triangular kernel weights and an empirically determined optimal "bandwidth" for $DemocratMargin_{it}$ to make these exclusions (Calonico, Cattaneo, and Farrell 2020) but show that our results are robust to different bandwidths (Appendix Tables 2, 4, and 5). The *rdrobust* command in Stata allows us to implement these decisions for our RD models and to also arrive at robust, bias-corrected point estimates.[6]

Finally, we show that the direction of impact of a Democratic house majority on outcomes generally does not change when we employ simpler models (that require stronger assumptions to arrival at causal conclusions); we describe these models and the qualitatively similar results in Appendix C, Appendix Table 1, and Appendix Figure 2.

3.2 Internal Validity of RD Estimates

To further support the internal validity of our RD estimates of state legislative partisanship's impact on education finance and outcomes, we investigate whether state-elections with margin of victory scores just above zero (barely majority Democrat houses) are similar to those with margin of victory scores just below zero (barely majority Republican houses) on observable characteristics. The key identifying assumption is that other plausible determinants of education appropriations and outcomes evolve smoothly across the cut-off. This is necessary in order to attribute any observed discontinuities in outcomes to the "treatment" of Democratic party control, rather than other confounding variables. Though we cannot test for equivalence on

[6] Because the *rdrobust* command does not currently support heterogeneity analyses, when investigating the impacts of Democratic majority control on state legislatures by subgroup, we do not employ *rdrobust* and manually calculate triangular kernel weights for use in regular regression analyses.

unobservable characteristics, the absence of substantial discontinuities on other observable characteristics may assuage concerns that RD estimates may be biased by factors besides Democratic control.

To perform this test, we estimate Equation (1), replacing our outcome measures with state-level characteristics measured at baseline, right before or concurrent to when elections occur. Results from estimation of this model, seen in Table 2, show that though some significant differences emerge, we do not find convincing evidence that state-election observations on either side of the margin of victory threshold differ substantially from one another at baseline.[7] For example, marginally Democratic houses preside over states with higher baseline unemployment and poverty, and lower income per capita. These absolute differences by party, however, are very small in magnitude. We also observe that barely Democratic houses are more likely in state-election observations that also have Democratic senates. Thus, in our preferred models, we control for these factors and in sensitivity analyses we also explore whether results differ when we omit observations around the margin of victory threshold (i.e., a donut RD). Results omitting covariates and/or observations are largely consistent with those from our preferred models, as seen in Appendix Tables 2, 4, and 5.

[7] We will often present the conventional, bias-corrected, and robust point estimates and standard errors resulting from the *rdrobust* command, but our preferred results (and the ones we refer to) are the most conservative set—the robust (which are also bias-corrected) point estimates and standard errors.

TABLE 2. IMPACT OF DEMOCRATIC MAJORITY ON COVARIATE BALANCE

	Proportion unemployed	Proportion in poverty	Income per capita (Log)	Democratic Governor	Democratic Senate	Citizen liberalness (z-score)	Post School Finance Reform
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conventional	0.00261**	0.00407***	-0.0158***	0.0299	0.106**	0.0522	0.0214
	(0.00102)	(0.00136)	(0.00353)	(0.0564)	(0.0474)	(0.0359)	(0.0192)
Bias-corrected	0.00275***	0.00415***	-0.0165***	0.0380	0.0899*	0.0506	0.0197
	(0.00102)	(0.00136)	(0.00353)	(0.0564)	(0.0474)	(0.0359)	(0.0192)
Robust and Bias-corrected	0.00275**	0.00415***	-0.0165***	0.0380	0.0899*	0.0506	0.0197
	(0.00114)	(0.00152)	(0.00389)	(0.0620)	(0.0524)	(0.0382)	(0.0205)
N Left of 0	147	145	130	124	122	123	127
N Right of 0	160	157	137	133	132	133	135
BW Left of 0	1.441	1.318	0.997	0.916	0.864	0.906	0.940
BW Right of 0	1.441	1.318	0.997	0.916	0.864	0.906	0.940

Note: All dollars are in 2010 dollars. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. column 7 documents the first major School Finance Reform in that state. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. *$p<.1$, **$p<.05$, ***$p<.01$.

Next, we demonstrate that the partisanship of states' houses is independently determined from the margin of victory threshold. In order to interpret β_2 as a causal estimate, there should be no manipulation of the margin of victory score, which determines party control in the state legislature. Some may question the validity of using close elections to identify quasi-random variation in party control of different branches of government more broadly because incumbents tend to have advantages in terms of political relationships and resources that may confound estimates. Our specific RD design may alleviate those concerns because our running variable depends on multiple separate close district-level elections occurring at the same time (Grimmer, Hersh, Feinstein, and Carpenter 2011). By leveraging results from several elections, we are less concerned about manipulation related to pre-election behavior or characteristics of candidates, such as resource advantages, and post-election advantages, such as vote-tallying (de la Cuesta

and Imai 2016). Extant evidence also suggests that these challenges are less relevant for state legislative legislations, further supporting their use in RD approaches (Eggers et al. 2015). When we conduct a McCrary Test, we find that the design satisfies this identifying assumption. In Figure 2 we show visual and empirical evidence that there is no bunching in observations above or below the cut-off, as might occur if vote margins scores can be manipulated to ensure securing (or losing) majority Democratic status.

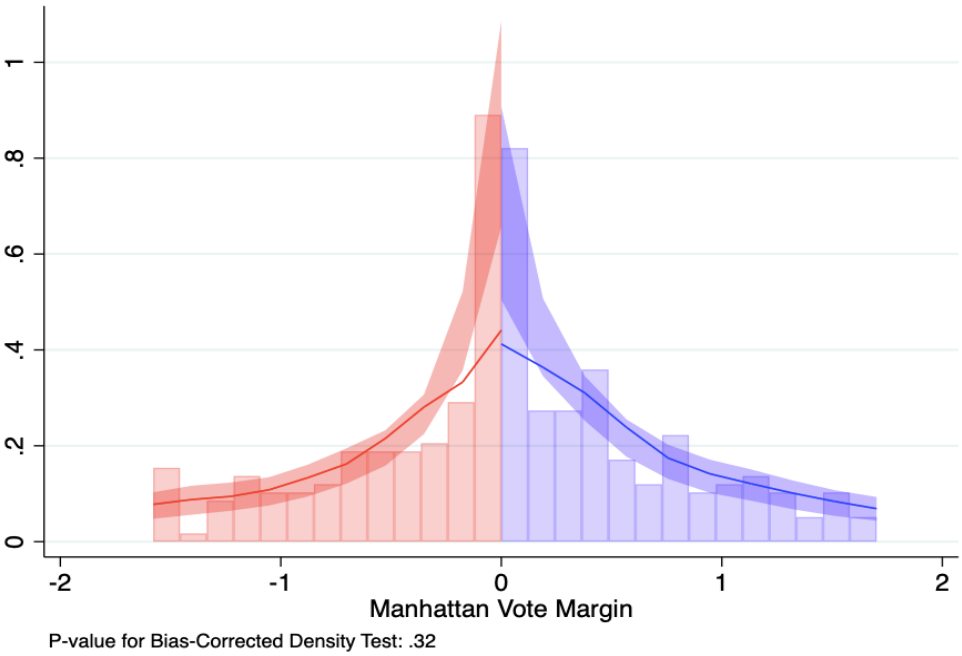

FIGURE 2: DISTRIBUTION OF THE DEMOCRATIC MARGIN OF VICTORY MEASURE AT THE STATE-ELECTION LEVEL

Note: Vote margins above zero indicate that Democrats control the state house. Local linear approximation and triangular weights are used to construct the density estimator.

4. Results

4.1 The Impact of States' Party Control on Education Finance

Weakly Democratic states spend more on higher education but less on K-12 funding than Republicans. In Table 3, we present our main results for the impact of Democratic control of state houses on state appropriations to public higher and K-12 education. Each column displays estimates of β_2 from the estimation of Equation (1) with baseline controls. From Table 3 panel

B, we find that two years after elections, Democratic houses appropriate to public higher education 3.6 percent more overall to public post-secondary institutions. When investigating other commonly used higher education funding measures suggested by Tandberg and Griffith (2013) in columns 2 and 3, we also find that two years post-election, Democrats appropriate 2.3 percent more per pupil and 2.2 percent more per $1000 dollars of state-level personal income. Though all measures and coefficients are positive and similar in magnitude, the per-pupil (a need-based measure of funding) and income-referenced (an effort-based measure of funding) estimates are not statistically significant at the 5% level after correcting for bias and accounting for the serial correlation of errors (Calonico, Cattaneo, and Farrell 2020). In the table we present all three measures of higher education spending but highlight total levels moving forward, as this measure has the advantage of offering direct insight into Democrats' spending decisions. Unlike in K-12, where enrollment is compulsory and thus more stable, policymakers may also concurrently affect higher education enrollment through legislative and budgetary actions like financial aid or college preparation programs.

Table 3 column 4 shows that Democratic houses appropriate less per pupil to K-12 by approximately 3.4 percent and 6.5 percent one- and two-years post elections relative to Republicans, respectively. These results two years later are robust to different RD modeling decisions: across specifications and samples, we generally observe statistically significant negative impacts of Democratic control on K-12 funding; for higher education spending, estimates are positive but not always significant (Appendix Table 2). Estimates from simple OLS and fixed effects models also consistently show positive effects of Democratic control for higher education spending and negative effects in K-12 spending (Appendix Table 1).

TABLE 3. IMPACT OF DEMOCRAT MAJORITY ON K-12 AND HIGHER EDUCATION STATE APPROPRIATIONS

	Higher education appropriations (1)	Higher education appropriations per pupil (2)	Higher education appropriations per $1000 in income (3)	K-12 appropriations per pupil (4)
Panel A. One year post election				
Conventional	-0.000434	-0.0119	-0.00346	-0.0300
	(0.0110)	(0.0116)	(0.0108)	(0.0217)
Bias-corrected	-0.00315	-0.0112	-0.00128	-0.0344
	(0.0110)	(0.0116)	(0.0108)	(0.0217)
Robust and Bias-corrected	-0.00315	-0.0112	-0.00128	-0.0344
	(0.0122)	(0.0133)	(0.0119)	(0.0233)
N Left of 0	122	125	122	140
N Right of 0	127	129	127	152
BW Left of 0	0.959	1.002	0.945	1.399
BW Right of 0	0.959	1.002	0.945	1.399
Panel B. Two years post election				
Conventional	0.0368**	0.0205	0.0195	-0.0644**
	(0.0151)	(0.0133)	(0.0125)	(0.0256)
Bias-corrected	0.0363**	0.0233*	0.0221*	-0.0648**
	(0.0151)	(0.0133)	(0.0125)	(0.0256)
Robust and Bias-corrected	0.0363**	0.0233	0.0221	-0.0648**
	(0.0165)	(0.0148)	(0.0134)	(0.0275)
N Left of 0	124	120	121	122
N Right of 0	133	127	131	133
BW Left of 0	0.915	0.817	0.850	0.881
BW Right of 0	0.915	0.817	0.850	0.881

Note: All dollars are in 2010 dollars. All models include covariates: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. *$p<.1$, **$p<.05$, ***$p<.01$.

To evaluate whether decreases in K-12 and increases in higher education accompany changes in alternative budget resources, we identify Democrats' impacts on other revenue sources. We focus specifically on impacts two years after elections, where we saw significant changes in state support for both higher education and K-12, and find that Democratic control of the state house also leads to increases in local appropriations in K-12, which contribute to net positive K-12 current expenditures under Democrats. However, we observe no party differences

in net tuition per pupil in higher education and federal support for K-12. Table 4 column 1 shows that under Democrats there are small null effects on net tuition per pupil at public institutions—suggesting that the increases in appropriations to higher education seen in Table 3 most likely translated to school investments rather than reduced tuition prices. Columns 2 through 4 show the effects of a marginal Democratic majority on the other K-12 finance outcomes. Column 2 show that local appropriations, two years post elections, increase by approximately 9.3 percent under Democrats.

One possible interpretation is that Democrats change K-12 funding formulas so that state funding decreases and local funding increases, but we do not find any party differences in major legislative changes to these formulas under marginally Democratic legislatures (results available on request). Instead, local districts may be raising more revenue to neutralize the difference in K-12 state spending between parties. Column 3 shows that federal financial support to public K-12 does not significantly differ by party two years post elections. Overall for this specific time period, current expenditures increase slightly by 1.5 percent, likely due to the observed increases in local spending offsetting decreases to state spending under Democrats, consistent with other studies documenting how local and state funding may counteract one another (Baicker and Gordon 2006; Card and Payne 2002). Simpler models with stronger assumptions again generally show similar effects specifically for local funding for K-12 (Appendix Table 1).

TABLE 4. IMPACT OF DEMOCRAT MAJORITY ON OTHER EDUCATION FINANCE OUTCOMES

	Higher education net tuition per pupil (Log)	K-12 local appropriations per pupil (Log)	K-12 federal appropriations per pupil (Log)	K-12 current expenditures per pupil (Log)
	(1)	(2)	(3)	(4)
Conventional	-0.0116	0.0934***	0.00558	0.0139**
	(0.0194)	(0.0221)	(0.0133)	(0.00560)
Bias-corrected	-0.0125	0.0925***	0.00459	0.0150***
	(0.0194)	(0.0221)	(0.0133)	(0.00560)
Robust and Bias-corrected	-0.0125	0.0925***	0.00459	0.0150**
	(0.0217)	(0.0227)	(0.0150)	(0.00614)
N Left of 0	111	141	127	114
N Right of 0	115	153	135	118
BW Left of 0	0.662	1.220	0.951	0.739
BW Right of 0	0.662	1.220	0.951	0.739

Note: All dollars are in 2010 dollars. All models include covariates: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. *$p<.1$, **$p<.05$, ***$p<.01$.

In Figure 3, we show the impact of Democratic house control visually by plotting, against the Democratic margin of victory, the residuals from a regression predicting average education finance outcomes two years after elections. These regressions include state and election-year fixed effects and our covariates (for similar plots that use raw outcome data instead of residualized measures, see Appendix Figure 2). At the margin of victory threshold for achieving party control—centered at zero—we observe discontinuities in average outcomes for the four measures that Democratic house control significantly affect.

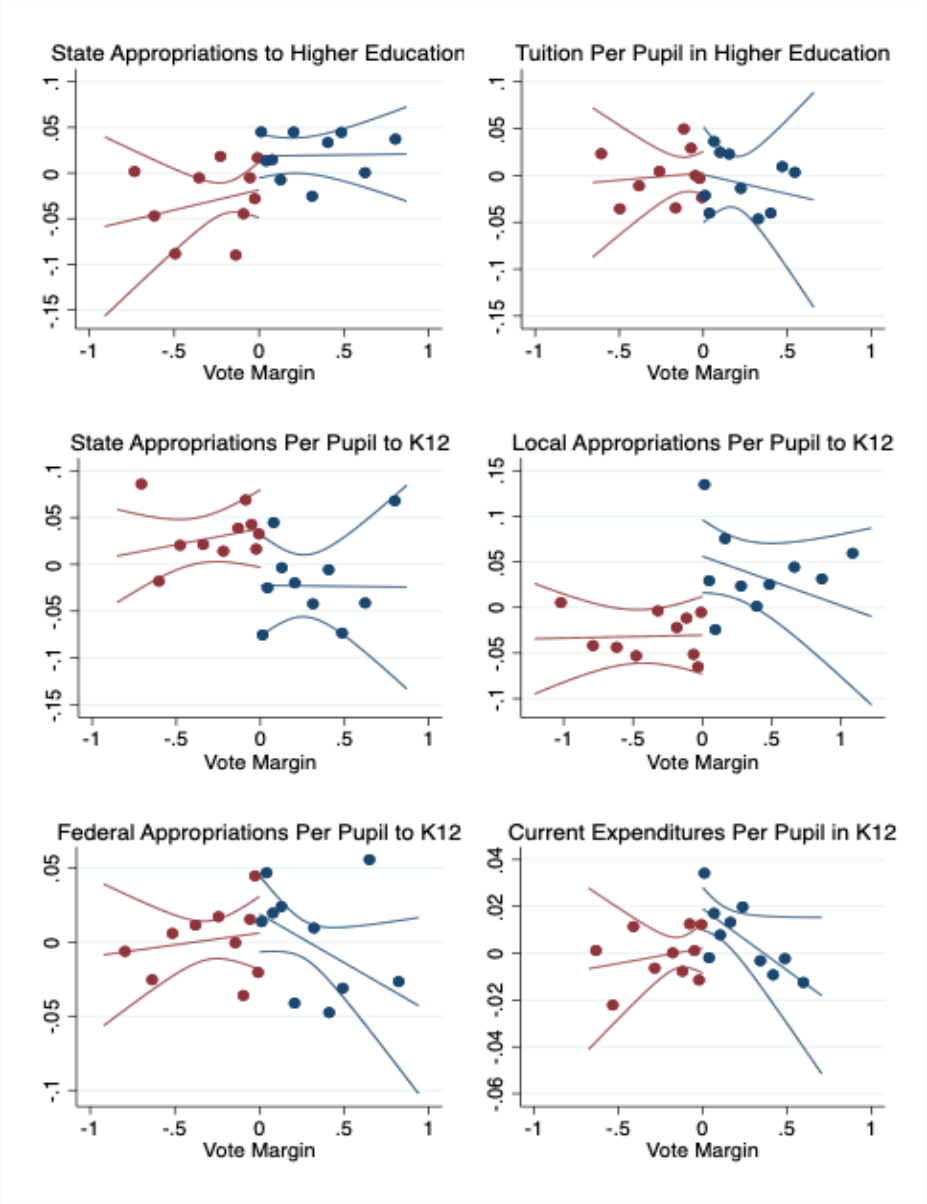

FIGURE 3: DEMOCRATIC MARGIN OF VICTORY AND ADJUSTED EDUCATION FINANCE OUTCOMES TWO-YEARS POST ELECTION

Note: Binned (20 bins) scatter plots with line of best fit for residualized education finance outcomes (adjusting for baseline covariates, state fixed effects, and election year fixed effects) against the Democratic margin of victory measure. 95% confidence intervals with standard errors clustered at the state level also shown. Triangular kernel weights from the preferred regression discontinuity models are used to estimate lines of best fit. Observations in blue indicate Democrat-controlled houses. All appropriation outcomes are in 2010 dollars. All outcomes are log transformed.

4.2 The Role of Party Control on Budget Trade-offs with Competing Priorities

Do policymakers make trade-offs within the education budget or across budget priorities? So far, we have shown that Democrats increase spending to higher education but decrease spending for K-12. Because the former holds a much smaller share of the overall state budget, it is unlikely that education-specific party differences explain our results. With this in mind, we then explore whether other budget priorities can explain lower K-12 spending. We do so by evaluating how state spending changes under Democratic houses for the following areas: public welfare (which primarily consists of Medicaid), highways, police, corrections, and health and hospitals. In Table 5, we present results from estimating Equation (1) but replace the outcome with state spending on these categories from the INDFIN data in addition to education spending data from the CCD and SHEEO. To make estimates comparable and to ease interpretation, we calculate per-capita state revenue, expenditures, and spending—without log transforming these per capita estimates. Each column of Table 5 shows the RD impact of Democratic majority control on different per capita state finance outcomes. Our preferred specification is the third row, showing robust and bias-corrected estimates. We focus on outcomes two years after the election, consistent with the past education finance specifications.

TABLE 5. IMPACT OF DEMOCRAT MAJORITY ON OTHER BUDGET PER-CAPITA SPENDING

	Revenue	Expenditures	K-12	Higher Education	Highways	Welfare	Police
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conventional	113.4	37.51	-38.58**	5.260*	-9.816	43.40***	-3.295**
	(142.0)	(43.71)	(18.25)	(2.833)	(8.909)	(16.30)	(1.532)
Bias-corrected	143.9	43.36	-36.33**	5.938**	-7.884	45.02***	-3.692**
	(142.0)	(43.71)	(18.25)	(2.833)	(8.909)	(16.30)	(1.532)
Robust and Bias-corrected	143.9	43.36	-36.33*	5.938*	-7.884	45.02***	-3.692**
	(162.3)	(47.34)	(18.83)	(3.169)	(9.689)	(16.76)	(1.642)
N Left of 0	129	121	120	109	95	127	122
N Right of 0	137	129	128	113	107	133	133
BW Left of 0	0.988	0.840	0.822	0.646	0.522	0.922	0.890
BW Right of 0	0.988	0.840	0.822	0.646	0.522	0.922	0.890

Note: All dollars are in 2010 dollars. All models include covariates: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. *$p<.1$, **$p<.05$, ***$p<.01$.

As seen in the table, a clear trade-off pattern in state spending by political party control emerges. First, we find that under weakly Democratic controlled houses, overall revenues and expenditures are higher, but these effects are insignificant. Second, per capita K-12 spending significantly decreases and per capita higher education spending increases, consistent with our original findings. However, since K-12 occupies a larger share of the overall budget, its decrease is substantially larger in absolute dollars than the increase in higher education, indicating that Democrats are not necessarily trading off on spending within public education. Third, Democrats appropriate less to police by 3.7 dollars per-capita; this effect is statistically significant, but small on an absolute level. Most notably, we see a large increase in per capita welfare spending under Democrats of 45 dollars—a change that by itself is greater than the per capita decrease for K-12. Since Medicaid represents the largest welfare budget item, the possible crowd-out effects of Medicaid on education is consistent with other studies (Kane, Orszag, and Apostolov 2005; Okunade 2004). Altogether, these findings offer compelling evidence that Democrats may trade-off K-12 and welfare spending in particular. In the heterogeneity analyses that follow, we further investigate this hypothesis.

4.3 Heterogeneous Treatment Effects: Contexts and Conditions for Party Effects

Although both contribute to states' education budgets, K-12 education investments under marginal Democratic house control goes in the opposite direction of higher education investments. The per-capita impact of Democratic control, however, is much smaller for higher education, which provides one piece of evidence that policymakers do not trade-off between K-12 and higher education (Table 5). The opposing effects for K-12 and higher education instead may indicate that Democrats' spending decisions depend on budget structures and how perceptions about how different investments accomplish their priorities. For example, when exploring state spending on other areas besides education in Table 5, we found suggestive evidence that Democrats may be prioritizing welfare spending in lieu of investments in K-12.

We hypothesize specifically that differences in the economic health of states—which can translate to subsequent shifts in state government revenues and spending decisions—may influence variation in the impact of Democratic house control on spending, and that results would be strongest when policymakers' political ideology aligns with that of their voters. We

thus explore heterogeneous treatment effects to understand the different factors affecting K-12 and higher education spending—specifically how the effects of political party on state finance outcomes vary across different settings and economic conditions.

The effects of Democratic control on K-12 education spending is strongest when liberal policymakers' party affiliation align with citizens' liberal ideology. This is consistent with findings that states are more likely to implement a more liberal policy such as increased spending on higher education if they have more policy support (Lax and Phillips 2012). Table 6 panel A shows that Democrats in more liberal states reduce state appropriations to K-12 by 11 percent (column 2); in more conservative states, party differences are approximately 1.4 percent. This party difference is mirrored when considering changes to local appropriations by state liberalness. In panel A we also show that Democrats' effects for higher education are also larger in liberal states, providing more suggestive evidence that policymakers are responsive to their constituents (Lee, Moretti, and Butler 2004). However, only for K-12 state appropriations are differences statistically significant by state political ideology type.

TABLE 6. HETEROGENEOUS DEMOCRATIC MAJORITY EFFECTS

	Higher education appropriations (Log) (1)	K-12 state appropriations per pupil (Log) (2)	K-12 local appropriations per pupil (Log) (3)	K-12 current expenditures per pupil (Log) (4)	K-12 state appropriations per capita (5)	Welfare state appropriations per capita (6)
Panel A. Heterogeneity by state liberalness						
Democratic Majority X	0.0344	-0.0140	0.0434**	0.00940	11.58	25.77
Conservative	(0.0335)	(0.0294)	(0.0206)	(0.0135)	(25.91)	(35.12)
Democratic Majority X	0.0413**	-0.107*	0.134***	0.0143	-78.63**	56.78*
Liberal	(0.0175)	(0.0535)	(0.0446)	(0.0111)	(38.21)	(29.14)
Effect difference p-value	0.836	0.0871	0.0555	0.768	0.0503	0.468
Panel B. Heterogeneity by baseline unemployment						
Democratic Majority X	0.000144	-0.0653	0.0955***	0.0121	-56.74*	-7.967
Low unemployment	(0.0316)	(0.0410)	(0.0335)	(0.0116)	(30.99)	(28.12)
Democratic Majority X	0.0854***	-0.0634	0.0907**	0.0156	-16.64	105.5***
High unemployment	(0.0276)	(0.0382)	(0.0379)	(0.0129)	(28.19)	(37.49)
Effect difference p-value	0.0357	0.941	0.900	0.841	0.139	0.0148
Panel C. Heterogeneity by baseline poverty						
Democratic Majority X	0.00466	-0.0453	0.0713**	0.0126	-33.35	28.97
Low poverty	(0.0313)	(0.0369)	(0.0261)	(0.0109)	(27.92)	(33.30)
Democratic Majority X	0.0787***	-0.0914**	0.119***	0.0159	-46.29	64.66*
High poverty	(0.0237)	(0.0443)	(0.0406)	(0.0105)	(30.48)	(35.17)
Effect difference p-value	0.0431	0.157	0.113	0.801	0.571	0.467
Panel D. Heterogeneity by whether first School Finance Reform case occurred before election						
Democratic Majority X	0.0642*	-0.113	0.148**	0.0194	-69.57	68.71
Pre SFR	(0.0352)	(0.0728)	(0.0681)	(0.0120)	(51.29)	(47.45)
Democratic Majority X	0.0113	-0.0186	0.0460	0.00844	-8.991	19.78
Post SFR	(0.0252)	(0.0372)	(0.0412)	(0.0104)	(33.44)	(33.73)
Effect difference p-value	0.229	0.279	0.274	0.443	0.362	0.443
N	246	244	283	220	236	249

Note: All dollars are in 2010 dollars. All models include covariates: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from regressions using triangular kernel weights and the default bandwidth selection process as determined by Stata command *rdrobust*. *$p<.1$, **$p<.05$, ***$p<.01$.

Table 6's panel B and panel C column 1 shows that effects for state higher education funding are primarily driven by changes in state-election observations with baseline unemployment and poverty rates above the state median. In these contexts, Democratic houses appropriate approximately eight percent more overall to higher education; for observations with lower baseline unemployment and poverty, we observe a null effect—and this effect difference is statistically significant. These differences are not evident for any of the K-12 finance outcomes. Economic and unemployment conditions may therefore provide Democrats with stronger policy justifications or serve as a clearer policy tool to increase economic mobility through college investments. Democrats' preferences of investing in the human capital of those in postsecondary institutions to increase economic mobility may help explain why Democrats have higher GDP growth and lower unemployment rates when they occupy office as president and governor (Blinder and Watson 2016; Beland 2015; Leigh 2008).

In panel D of Table 5, we show results from heterogeneity analyses investigating whether party differences in education finance and outcomes vary for state-election observations occurring after significant court-mandated and/or legislative school finance reform (SFR) in K-12. We find limited evidence of pre-post reform differences for higher education appropriations. We find slightly stronger evidence that Democratic houses appropriated less to K-12 public schools prior to significant SFR. State funding to K-12 decreased by approximately 11 percent and local funding increased by 15 percent in observations before SFR, but these estimates attenuate towards zero (and are insignificant) post-reform. Differences in impacts by whether states faced SFRs, however, are insignificant. The direction of this result is expected if we assume that certain parties have less flexibility in significantly altering public primary and secondary schools appropriations after implementing state funding formulas reforms.

As noted above, in separate analyses we find no Democrat-Republican differences in the likelihood of passing significant SFR legislation following close house elections. From this we conclude that it is unlikely that the observed overall negative Democratic effect on states' K-12 spending and compensatory increase in local revenue is driven by substantial changes to school funding formulas under Democrats. Two other explanations may explain why we do not see SFRs or formula changes driving the compensatory increases in local revenue sources under Democrats. First, prior research shows that expenditures per student increased in states with and without courts overruling their state finance formulas (Card and Payne 2002). Second, the most

common type of state formula reforms (foundation allowances) through the mid-1990s did not necessarily increase overall per-pupil spending. Instead, the most common SFRs primarily increased (rather than decreased) revenues and spending in the lowest-income districts with limited property tax bases—and may have even reduced state-level spending long-term (Lafortune, Rothstein, and Schanzenbach 2018; Jackson, Johnson, and Persico 2016; Hoxby 2001).

In the last two columns of Table 6, we explore impact heterogeneity of Democratic control on per-capita spending for K-12 and welfare specifically. We hypothesize that, if Democrats are indeed trading off between these two substantial budget items, we would tend to observe RD effects in the same contexts—which we do. For example, in more liberal states, marginal Democratic houses appropriate significantly less to K-12 and significantly more to welfare; this effect is attenuated and insignificant in both cases for more conservative states. Similarly, in higher poverty observations, impacts are larger in magnitude for both K-12 and welfare. The one exception is when investigating heterogeneity by baseline unemployment. Democrats spend significantly less per capita on K-12 when there is lower unemployment but not when there is higher unemployment, whereas they spend significantly more per capita on welfare when there is higher unemployment and not when there is lower unemployment.

We also do not find convincing evidence that our results are necessarily driven by specific years or states, as depicted in Figure 4 and Appendix Figure 1, respectively. To show temporal heterogeneity, we plot coefficients from our RD model represented by Equation (1) estimated for groups of election cycles. Because we cannot fully account for state- or election-level differences when using RD models that subset data from only a few election cycles, we use residualized state finance outcomes as our dependent variables. Specifically, as we do earlier, we regress these outcomes on our baseline controls, state fixed effects, and election fixed effects, and then use residuals from these full panel models in models where our outcomes come from only a subset of elections.[8] Figure 4 plots these specific impacts of Democratic house control.

[8] Most state house elections occur in even years. A few occur in odd years. We group odd year elections with the prior even year elections to improve precision (e.g., elections occurring in 1987 are considered concurrently with elections occurring in 1986 for our within-year RD models).

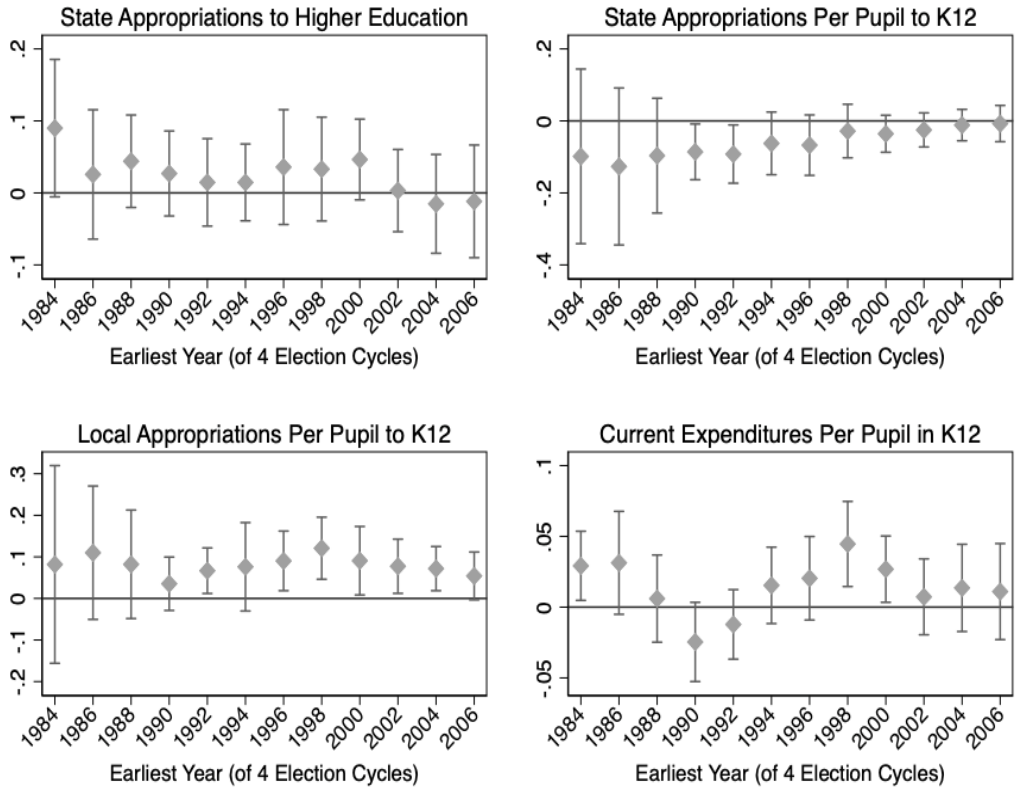

FIGURE 4. DEMOCRATIC CONTROL REGRESSION DISCONTINUITY IMPACTS ON OUTCOMES ACROSS ELECTION CYCLES

Note: These graphs plot treatment effects of Democratic house control on residualized two-years post election outcomes (from models using the whole panel of data, adjusting for baseline covariates, state fixed effects, and election year fixed effects) for subsets of election cycles (i.e., those occurring between election year *n* and election year *n+6*). 95% confidence intervals with standard errors clustered at the state level also shown. Triangular kernel weights from the preferred regression discontinuity models are used to estimate impacts. All appropriations are in 2010 dollars. All outcomes are log transformed.

From the figure, we observe several patterns that provide evidence to earlier conclusions and hypotheses. First, the impact of Democratic control on state revenues to public K-12 is negative over time, though more substantial earlier on in our panel. The impact of Democratic control on local revenues in public K-12 is similarly positive over time, supporting the conclusion that local revenues increase to offset decreases in state support to elementary and secondary education. Finally, the effects of Democrats on total higher education spending are positive for most elections in our sample.

Finally, when performing the same temporal comparisons of impacts for the Democratic control of houses on welfare and K-12, we again find evidence that supports our hypotheses. Specifically, in Figure 5, we show that decreases to K-12 spending under Democrats largely

occur alongside increases to welfare spending in the same election cycles. Interestingly, we also see that the positive welfare effects—and the negative K-12 impacts—generally appear most strongly following elections in the 1980s and early 1990s. During this time period, many states were expanding eligibility for Medicaid, and in particular for children and pregnant mothers (Centers for Medicare & Medicaid Services 2015). Thus, in the next section, we investigate whether these differences in finance outcomes by party translate not only to improvements to educational outcomes, but also outcomes like health insurance coverage, which would follow from these investments in welfare.

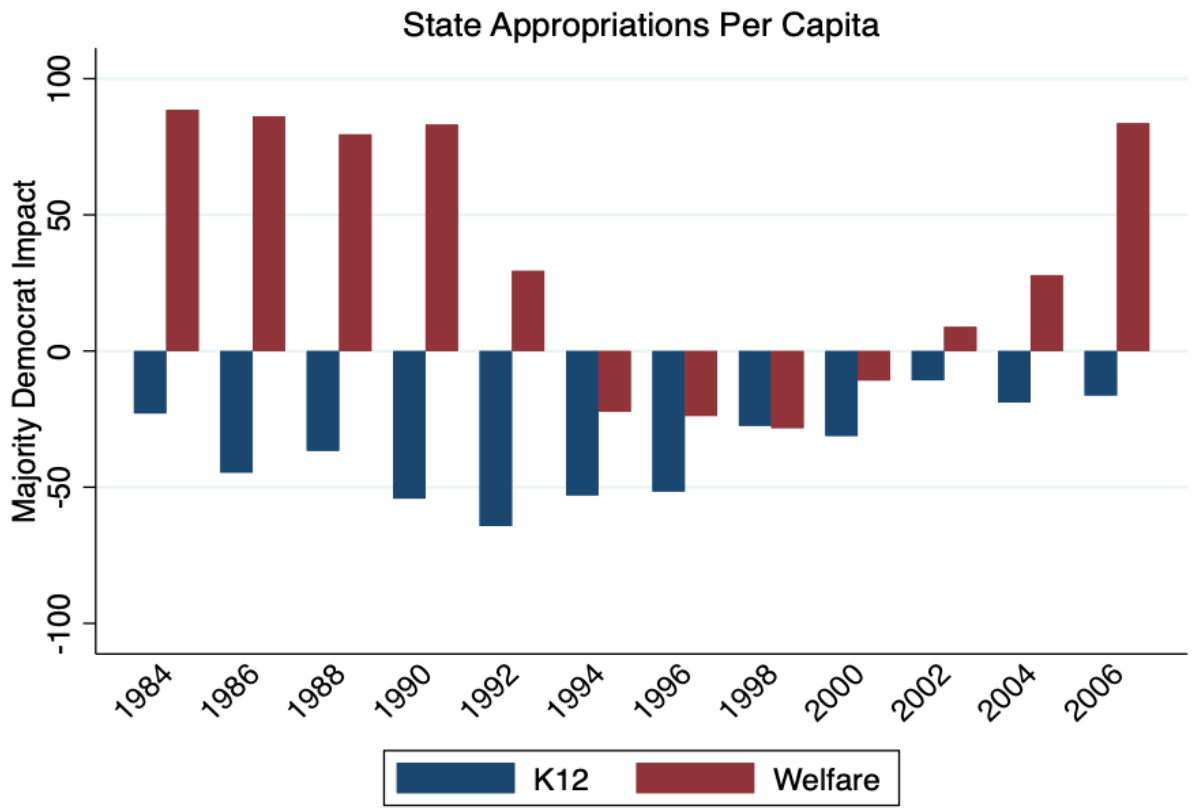

FIGURE 5. DEMOCRATIC CONTROL REGRESSION DISCONTINUITY IMPACTS ON STATE K-12 AND WELFARE APPROPRIATIONS ACROSS ELECTION CYCLES

Note: These graphs plot treatment effects of Democratic house control on residualized two-years post election outcomes (from models using the whole panel of data, adjusting for baseline covariates, state fixed effects, and election year fixed effects) for subsets of election cycles (i.e., those occurring between election year *n* and election year *n+6*). Triangular kernel weights from the preferred regression discontinuity models are used to estimate impacts. All appropriations are per-capita and in 2010 dollars.

4.4 The Impact of State's Party Control on Downstream Outcomes

In our main analyses, we investigate how marginal Democratic majority control of state houses affects key measures of education finance, and then explore how impacts on other state spending areas might explain these results. We now investigate whether party differences emerge in downstream outcomes related to these spending areas.

We find that the positive effects of Democratic control on higher education investments from state governments do not translate to increased enrollment into public postsecondary institutions. We also find that party differences in local and state funding to K-12 is reflected in marginal decreases in student-teacher ratios, but differences are small enough such that another K-12 educational outcome—student attendance—is not affected. Finally, the increase in welfare spending we see under Democrats do appear to lead to key measures of welfare improvement, including increases in health insurance coverage for non-White children by 2 percent (Table 7 panel B). This is consistent with findings that health care spending after 1980 increased most for children (Mayer and Lopoo 2008).

We show all these results in Table 7. To arrive at these majority Democrat impacts, we replace our state finance outcomes in Equation (1) with these downstream outcomes. We focus on data specifically collected two, three, and four years post elections (as opposed to just two years post) because we evaluate the follow-on effect from the trade-off between K-12 and welfare, which may take time to emerge.

As seen in panel A of Table 7, party differences in K-12 and higher education spending at the state level do not substantially alter students' educational outcomes. We observe null point estimates for full-time enrollment into public postsecondary institutions. Similarly, though K-12 student-teacher ratios are 0.2 to 0.4 points lower under Democrats two to four years post elections, party differences in average attendance rates also approximate zero.

On the other hand, we find that majority Democratic houses preside over two percentage-point increase in health insurance coverage for non-White children three- and four-years post elections. As seen in panel B of Table 7, increases to public health insurance coverage (i.e., Medicaid and SCHIP) largely account for these part differences. These results converge with our positive RD impacts of Democratic control on welfare following elections in the 1980s and 1990s described earlier. During this time period, many states were expanding Medicaid

eligibility for children, in particular, which led to positive effects on affected individuals' long-term educational attainment (Cohodes et al. 2016; Miller and Wherry 2019; Brown, Kowalski, and Lurie 2019).

TABLE 7. IMPACT OF DEMOCRATIC MAJORITY ON DOWNSTREAM OUTCOMES

	Two years post	Three years post	Four years post
Panel A. Downstream education outcomes			
Higher education full-time enrollment	0.00937	0.00420	-0.00584
	(0.0102)	(0.0123)	(0.00946)
K-12 student-teacher ratio	-0.440***	-0.223**	-0.240**
	(0.122)	(0.0943)	(0.118)
K-12 average daily attendance rate	0.00132	0.000688	0.000170
	(0.00248)	(0.00173)	(0.00220)
Panel B. Downstream welfare outcomes			
Any health insurance (White adult)	-0.00181	0.00441**	0.00655***
	(0.00187)	(0.00203)	(0.00200)
Any health insurance (Non-White adult)	0.00275	0.00233	0.00307
	(0.00555)	(0.00486)	(0.00472)
Any public health insurance (White adult)	0.000899	0.00205	0.00269
	(0.00241)	(0.00307)	(0.00275)
Any public health insurance (Non-White adult)	0.00284	-0.00960	-0.00516
	(0.00770)	(0.00703)	(0.00600)
Any health insurance (White child)	-0.00197	0.00424*	0.00626***
	(0.00315)	(0.00247)	(0.00183)
Any health insurance (Non-White child)	0.00544	0.0171***	0.0197***
	(0.00559)	(0.00484)	(0.00615)
Any public health insurance (White child)	0.00242	-0.00285	-0.00391
	(0.00348)	(0.00378)	(0.00379)
Any public health insurance (Non-White child)	0.0156	0.0198***	0.0238***
	(0.0114)	(0.00742)	(0.00759)

Note: All dollars are in 2010 dollars. All models include covariates: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. Only bias-corrected robust estimates and standard errors are reported. *$p<.1$, **$p<.05$, ***$p<.01$.

4.5 *Robustness Checks and Placebo Tests*

We test whether our results are robust to different specifications in Appendix Tables 2, 4, and 5. In these tables, we explore whether the results are robust when employing running variables used in other studies investigating impacts of party control of state legislatures (Feigenbaum et al. 2017; Fiva et al. 2014) and when we employ different polynomial specifications (i.e., quadratic and cubic), compared to the local linear regression in our default model. We then investigate the sensitivity of our results to varying the sample size. Specifically, we test whether using an optimal bandwidth that is 80 percent of the optimal bandwidth calculated following (Calonico, Cattaneo, and Farrell 2020) or whether using a specification with 120 percent of the optimal bandwidth changes the results. Similar we run a model that excludes observations immediately to the right and left of the margin of victory cut-off for Democratic control using a "donut-hole" RD. For our final robustness check, we present estimates of the RD model omitting baseline covariates. Overall, our findings for state investments in K-12 are consistently negative and significant; for higher education, however, Democratic impacts are less consistently significant though always positive. Per-capita impacts on welfare are similarly robust to modeling decisions, as are our results for the downstream outcomes of K-12 student-teacher ratios and health insurance coverage for non-White children.

We also conduct a series of placebo tests to explore whether a Democratic majority in state houses affects outcomes that it should not be affecting. Specifically, we estimate our primary RD models but use as outcomes K-12 and higher education finance in years prior to Democratic control. In Appendix Table 3 we show that we generally do not find strong evidence of impacts on these placebo outcomes.

5. Conclusion

State education appropriations and outcomes vary by state and over time. Does political party affect how policymakers invest in resources, particularly education? This question of whether elected officials' policy agendas and values matter has remained under debate. Evaluating party effects on spending offers a direct view of whether and when party differences exist. Yet naïve estimates of the effects of party control may be biased because political

affiliations may reflect voters' ideology, political and economic climate, leadership quality, and other state characteristics that may also affect education outcomes. Our paper offers new evidence that party affiliation affects education spending using a regression discontinuity that leverages close elections based on the margin of victory for party control in the state house between 1984-2013.

We find that state with a weak Democratic majority in the legislature spend 6.5 percent less than Republicans on K-12 education state appropriations. However, increases in local revenue to K-12 districts more than offset this difference by party such that current expenditures still increase by 1.5 percent. These findings are robust to various specifications. Heterogeneous treatment effects suggest that differences by party are largest in states with more liberal citizens. Democrats appropriate 3.6 percent more to higher education overall, but this result is driven almost entirely by states with higher baseline unemployment (8.5 percent) and higher baseline poverty rates (7.8 percent). Under constrained economic conditions, Democrats may be more likely than their Republican counterparts to advocate for higher education spending rather than cuts.

It may be surprising to observe K-12 and higher education budget items with opposing trends even though both relate to education. We considered two primary explanations for why Democrats decrease K-12 education but increase higher education. The first is that policymakers trade off K-12 education spending for increased higher education spending. However, we find little evidence in support of this: the Democrat-Republican per-capita difference in states' appropriation to higher education is much smaller than the difference for K-12. That leads to our next possibility: that policymakers trade off K-12 spending with spending based on other priorities while satisfying balanced budget requirements. We show that the Democratic impacts on welfare more than offsets all of the decrease in K-12 funding. Democrats increasing higher education to improve economic outcomes and trading off welfare for K-12 for redistributive goals are consistent with past findings on Democrats' policy priorities. As expected, we see that public health care insurance coverage increases for non-white children by a statistically significant two percent starting three years after an election, which is one year after observing partisan differences in K-12 spending.

Though our findings accord with studies that show that the party affiliation of legislators affect their policy and budget decisions, our results paint a more nuanced picture of how

partisanship influences policy agendas and budgets. Common perception might be that Democrats would always increase funding to public education. However, policymakers' spending choices depend largely on their own (and alternative) revenue sources, competing or complementary policy priorities, and economic conditions. Thus, policymakers must balance constituents' expectations with their ability to justify spending decisions in meeting those priorities. One interpretation is that Democrats may perceive some budget items (e.g., welfare) as more effective means to achieve redistributive goals than others (e.g., K-12 education). The need to make trade-offs may be particularly true in a political environment of increasing partisanship and decreasing government unification (Caughey, Warshaw, and Xu 2017; Ansolabehere, Rodden, and Snyder Jr. 2006). That local revenues to K-12 districts increase enough to offset decreases in state support suggests that, these trade-offs may not necessarily lead to negative short-term consequences.

Even with many states setting minimum per-pupil K-12 spending requirements, without consistent state support, districts may find it difficult to raise expenditures to a level that guarantees a high-quality education for all students. Similarly, without significant state support, the finances of public institutions of higher education have shown to be more vulnerable to political and economic fluctuations in recent years. As close elections become increasingly common in today's polarized political climate, education appropriations may be more exposed to political priorities. Other budget items such as health care and welfare have distinct budget processes and structures that tie it to federal matches, caseloads, and labels as mandatory budget items that offer a higher degree of protection against political and economic changes. Without efforts to standardize budget formulas or offer federal matching support, education appropriations may be more exposed to political fluctuations than other large budget priorities.

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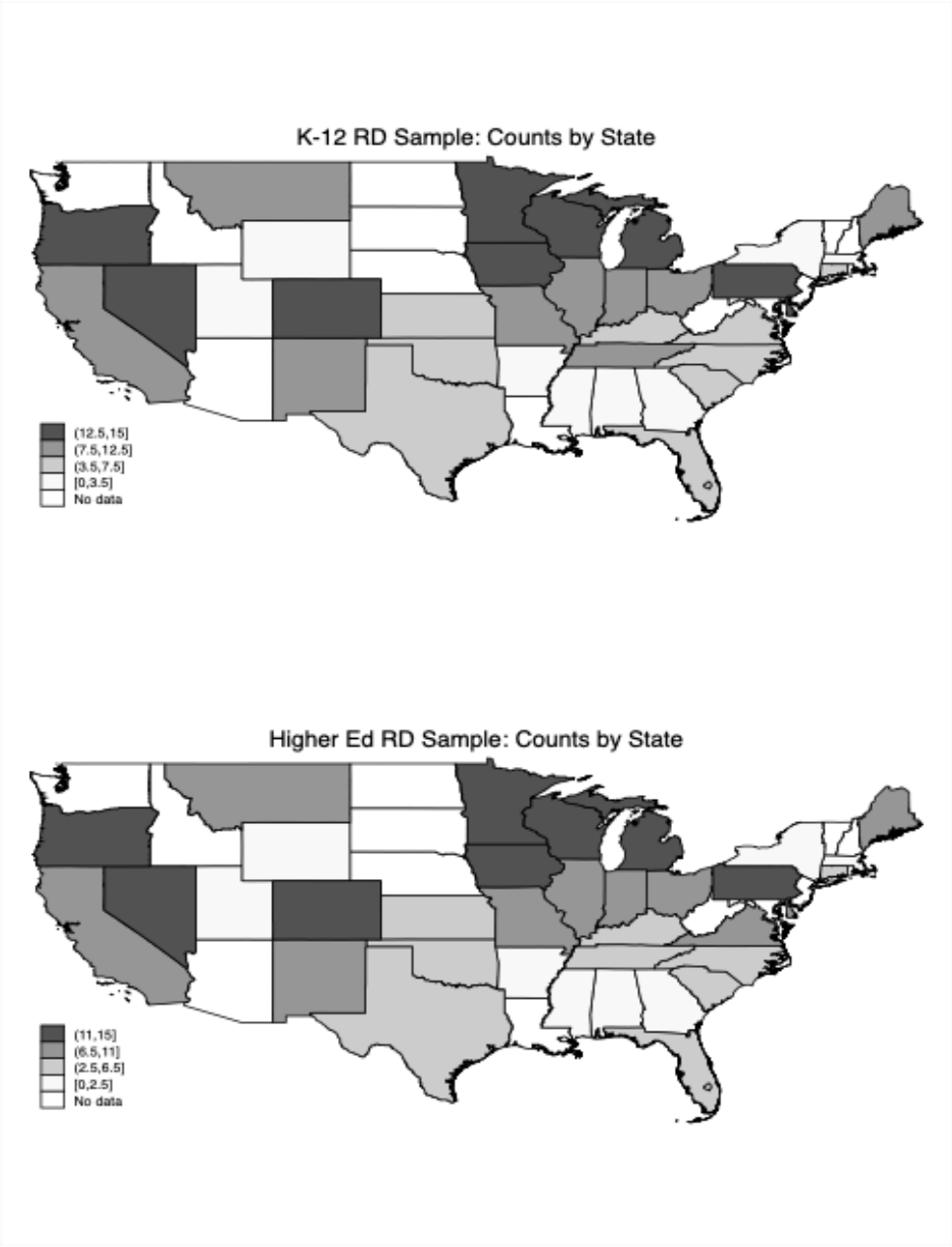
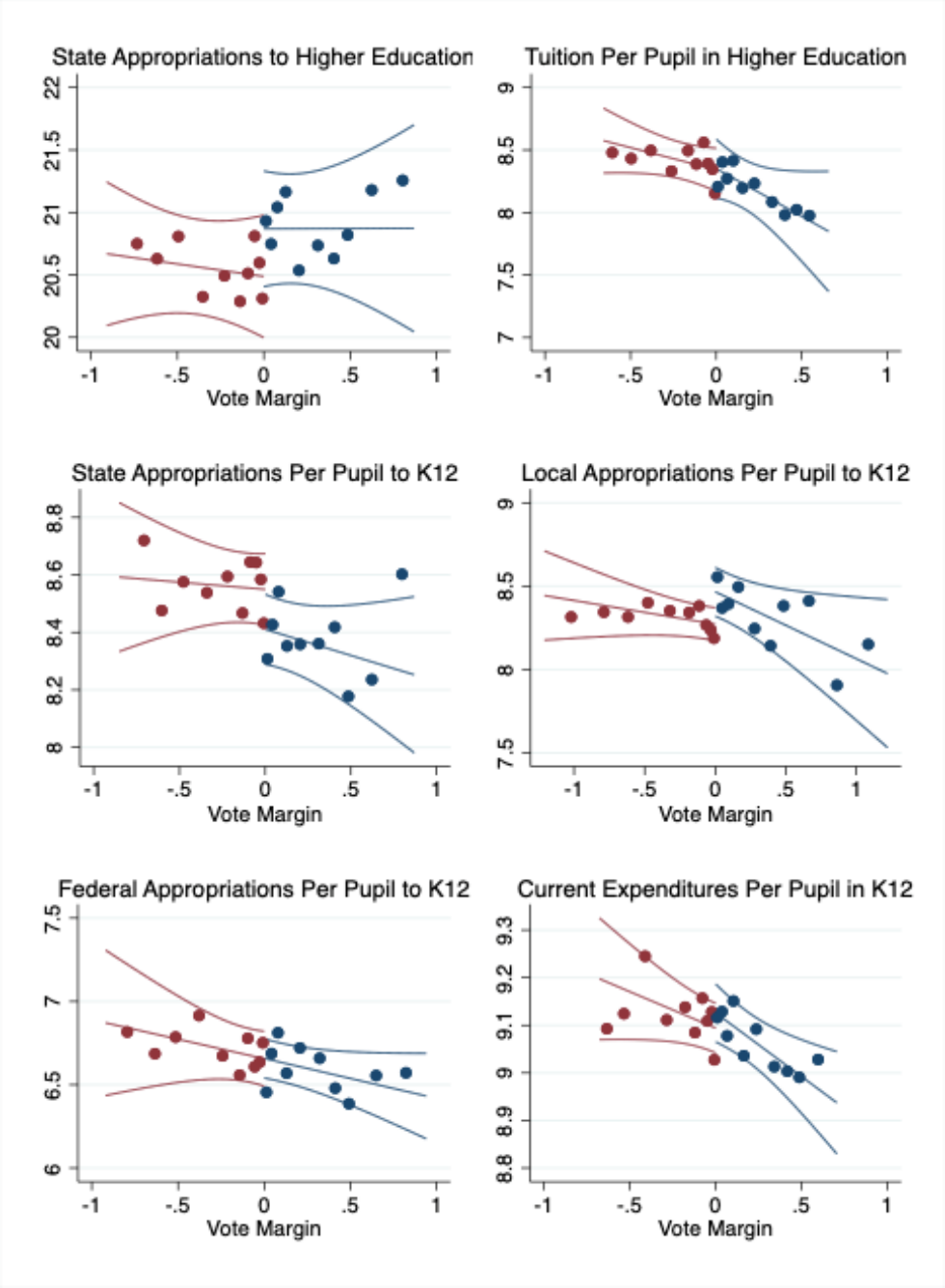
Online Appendix Tables and Figures



APPENDIX FIGURE 1. REGRESSION DISCONTINUITY SAMPLE FOR K-12 (TOP) AND HIGHER EDUCATION (BOTTOM): COUNTS BY STATE

Note: Two maps for the inclusion of different states in the RD samples. The top map shows the sample of states and their frequency of inclusion for the RD model using higher education total state spending outcomes, two years post elections. The bottom map shows the sample of states and their frequency of inclusion for the RD model using K-12 per-pupil state spending outcomes, two years post elections.



APPENDIX FIGURE 2. DEMOCRATIC MARGIN OF VICTORY AND ADJUSTED EDUCATION FINANCE OUTCOMES TWO-YEARS POST ELECTION

Note: Binned (20 bins) scatter plots with line of best fit for raw education finance outcomes against the Democratic margin of victory measure. 95% confidence intervals with standard errors clustered at the state level also shown. Triangular kernel weights from the preferred regression discontinuity models are used to estimate lines of best fit. Observations in blue indicate Democrat-controlled houses. All appropriation outcomes are in 2010 dollars.

APPENDIX TABLE 1. IMPACT OF DEMOCRATIC MAJORITY ON EDUCATION FINANCE OUTCOMES – ALTERNATIVE REGRESSION SPECIFICATIONS

	Higher education appropriations (Log) (1)	Higher education net tuition per pupil (Log) (2)	K-12 state appropriations per pupil (Log) (3)	K-12 local appropriations per pupil (Log) (4)	K-12 federal appropriations per pupil (Log) (5)	K-12 current expenditures per pupil (Log) (6)
Simple Ordinary Least Squares (OLS)	0.446**	-0.249**	-0.170**	0.122	-0.193**	-0.0277
	(0.206)	(0.109)	(0.0772)	(0.0852)	(0.0751)	(0.0348)
State & Election Year Fixed Effects	0.0423	0.0226	-0.0290	0.0902**	-0.00863	0.0191
	(0.0259)	(0.0318)	(0.0358)	(0.0355)	(0.0167)	(0.0135)
State & Election Year Fixed Effects using RD Sample	0.0450	0.00822	-0.0625*	0.0692**	-0.00675	0.00124
	(0.0290)	(0.0270)	(0.0342)	(0.0315)	(0.0148)	(0.0109)

Note: All dollars are in 2010 dollars. Covariates included in all models. Covariates include proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. Simple Ordinary Least Squares (OLS)=no state or election fixed effects, all observations. State & Election Year Fixed Effects=state and election fixed effects, all observations. State & Election Year Fixed Effects using RD Sample=state and election fixed effects, only observations in the RD sample determined by *rdrobust* optimal bandwidth. Standard errors clustered at the state level in parentheses. *$p<.1$, **$p<.05$, ***$p<.01$.

APPENDIX TABLE 2. ROBUSTNESS CHECKS FOR IMPACT OF DEMOCRATIC MAJORITY ON EDUCATION FINANCE OUTCOMES

	Higher education (Log)	Higher education net tuition per pupil (Log)	K-12 state per pupil (Log)	K-12 local per pupil (Log)	K-12 federal per pupil (Log)	K-12 current expenditures per pupil (Log)
	(1)	(2)	(3)	(4)	(5)	(6)
Euclidean RV	0.0290	-0.00788	-0.0746**	0.130***	0.00914	0.0199**
	(0.0230)	(0.0255)	(0.0337)	(0.0269)	(0.0200)	(0.00800)
Uniform RV	0.00715	-0.0184	-0.0861*	0.196***	0.00387	0.0338***
	(0.0274)	(0.0327)	(0.0464)	(0.0417)	(0.0284)	(0.0101)
Quadratic	0.0190	0.00450	-0.0648**	0.116***	0.0141	0.0202***
	(0.0209)	(0.0275)	(0.0304)	(0.0264)	(0.0166)	(0.00743)
Cubic	0.0152	0.0165	-0.0525	0.130***	0.0227	0.0228**
	(0.0229)	(0.0340)	(0.0360)	(0.0297)	(0.0201)	(0.00923)
BW * .8	0.0285	0.00397	-0.0700*	0.128***	0.0123	0.0114
	(0.0239)	(0.0286)	(0.0414)	(0.0294)	(0.0189)	(0.00980)
BW * 1.2	0.0288	0.00531	-0.0644*	0.112***	0.00790	0.0145*
	(0.0195)	(0.0295)	(0.0357)	(0.0274)	(0.0187)	(0.00847)
Donut-hole	0.0458	0.129*	-0.0706	0.0331	0.0170	0.0335*
	(0.0592)	(0.0684)	(0.0546)	(0.0475)	(0.0403)	(0.0189)
No covariates	0.0418***	-0.00967	-0.0859***	0.103***	0.0129	0.00354
	(0.0151)	(0.0205)	(0.0261)	(0.0228)	(0.0149)	(0.00657)

Note: All dollars are in 2010 dollars. Covariates included in all models except when specified. Covariates include: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. Only bias-corrected robust point estimates and standard errors are reported. *$p<.1$, **$p<.05$, ***$p<.01$.

APPENDIX TABLE 3. PLACEBO TESTS FOR IMPACT OF DEMOCRATIC MAJORITY ON EDUCATION FINANCE OUTCOMES

	Higher education appropriations (Log) (1)	Higher education net tuition per pupil (Log) (2)	K-12 state appropriations per pupil (Log) (3)	K-12 local appropriations per pupil (Log) (4)	K-12 federal appropriations per pupil (Log) (5)	K-12 current expenditures per pupil (Log) (6)
Baseline year	0.0301	0.00368	-0.0311	0.0127	0.0166	-0.00230
	(0.0186)	(0.0205)	(0.0212)	(0.0235)	(0.0151)	(0.00473)
Baseline year - 1	0.0165	0.00135	0.0179	-0.00834	-0.0139	-0.00302
	(0.0198)	(0.0198)	(0.0222)	(0.0159)	(0.0148)	(0.00605)

Note: All dollars are in 2010 dollars. Covariates included in all models except when specified. Covariates include: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. Only bias-corrected robust point estimates and standard errors are reported. *$p<.1$, **$p<.05$, ***$p<.01$.

APPENDIX TABLE 4. ROBUSTNESS CHECKS FOR IMPACT OF DEMOCRATIC MAJORITY ON OTHER BUDGET PER-CAPITA SPENDING

	K-12	Higher Education	Welfare	Police
	(1)	(2)	(3)	(4)
Euclidean RV	-34.06	8.340**	52.14**	-4.733**
	(22.72)	(3.853)	(21.10)	(1.870)
Uniform RV	-36.81	15.16***	10.89	-3.885*
	(32.88)	(5.712)	(30.02)	(2.324)
Quadratic	-28.34	5.770	43.75**	-3.338*
	(21.72)	(3.532)	(19.62)	(1.915)
Cubic	-28.13	5.281	39.47	-3.544*
	(27.68)	(4.150)	(24.22)	(2.095)
BW * .8	-42.08	8.299*	30.64	-4.279**
	(28.80)	(4.546)	(28.74)	(2.036)
BW * 1.2	-41.62	6.850	41.94*	-4.042**
	(26.82)	(4.884)	(25.03)	(1.945)
Donut-hole	-30.75	-9.533	148.5**	-5.451
	(64.29)	(14.71)	(71.73)	(5.063)
No covariates	-51.73***	3.350	32.39*	-3.645**
	(18.45)	(3.389)	(19.10)	(1.634)

Note: All dollars are in 2010 dollars. Covariates included in all models except when specified. Covariates include: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. Only bias-corrected robust point estimates and standard errors are reported. *$p<.1$, **$p<.05$, ***$p<.01$.

APPENDIX TABLE 5. ROBUSTNESS CHECKS FOR IMPACT OF DEMOCRATIC MAJORITY ON DOWNSTREAM OUTCOMES

	K-12 student-teacher ratio	Any health insurance (Non-White child)	Any public health insurance (Non-White child)
Euclidean RV	-0.348**	0.0192***	0.0245**
	(0.151)	(0.00697)	(0.0106)
Uniform RV	-0.560***	0.0202**	0.0216*
	(0.194)	(0.00807)	(0.0125)
Quadratic	-0.245*	0.0177**	0.0324***
	(0.139)	(0.00770)	(0.0103)
Cubic	-0.234	0.0187**	0.0387***
	(0.163)	(0.00795)	(0.0118)
BW * .8	-0.234	0.0205***	0.0337***
	(0.161)	(0.00706)	(0.0104)
BW * 1.2	-0.196	0.0182**	0.0293***
	(0.140)	(0.00711)	(0.00941)
Donut-hole	-0.0684	0.0464***	0.0139
	(0.241)	(0.0152)	(0.0140)
No covariates	-0.243*	0.0124**	0.0232***
	(0.127)	(0.00606)	(0.00805)

Note: All dollars are in 2010 dollars. Covariates included in all models except when specified. Covariates include: proportion unemployed, proportion in poverty, income per capita (logged), citizen liberalness, and dummy variables for: the election occurring after the states' first school finance reform; and being under Democratic senate and/or governor control. State and election fixed effects included in all models. Standard errors clustered at the state level in parentheses. Results reported come from Stata command *rdrobust* after specifying a local linear regression discontinuity with triangular kernel weights and the default bandwidth selection process. Only bias-corrected robust point estimates and standard errors are reported. *$p<.1$, **$p<.05$, ***$p<.01$.

Appendix A. Case Study: Michigan State House

Michigan presents a useful case study to evaluate the effects of political party in the state legislature. In 1994, Michigan was the first state to end its 100-year reliance on local property tax to fund public education. At that point, Michigan ranked sixth in the nation in its reliance on property taxes. Following this shift away from local funding, the state's share in educational investment increased from 29 percent to 78 percent. Republicans who took control of the House, and shortly after the Senate, helped pass the policy change to eliminate property tax reliance after securing party control of the legislature for the first time in 27 years. Michigan eliminated its reliance on local property taxes after public schools in Kalkaska shut down during the school year in 1993 because voters rejected three property tax increases to stabilize education funding.

As part of the reforms to change education finance in Michigan, Republicans leveraged increases to the sales and cigarette taxes to augment state education budgets by about 6 percent. Michigan then used a state formula to distribute aid to districts based on a foundation allowance, which primarily increased funding for low-funded districts. Following this formula, Michigan paid for the difference between the total foundation allowance (set at $5,000 per-pupil) and the revenue that local districts collect from non-homestead property taxes to businesses, industries, and second homes. Although basic foundation allowances were the most common type of school finance reform, Michigan did not face any court mandates to implement the foundation allowance. Nothing guided aid distribution to institutions of higher education until 1998, when Michigan developed a community college funding formula.

After regaining control of the House in 1996, Democrats increased funding for public universities by 4 percent in the 1997-1998 budget and 2.8 percent in the 1998-99 budget, despite lower state revenues resulting from the phase out of their Single Business Tax. They also appropriated a 2.3 percent increase in need-based financial aid and support for independent colleges, as well as an additional $2 million to provide a tuition incentive program for Medicaid-eligible students to attend college.

Under Republicans in 2004, general fund revenues fell to the available revenue amount in 1993. Republicans chose to cut spending by 10 percent and maintain K-12 education at the existing level. In the next year's 2005-2006 budget, community college funding decreased 3.7 percent but higher education overall increased 1.3 percent. Once Democrats regained control in

2006, they increased higher education appropriations by 18 percent and community college appropriations by 29 percent while keeping K-12 funding constant.

Between the Michigan House's 1999-2000 budget and 2008-2009 budget, the fraction of adjusted gross appropriations devoted to education finance changed very little. The fraction of education finance made up approximately 35.5 percent in both periods. Similarly, the share appropriated to transportation, the social safety net, and corrections were nearly identical over the course of this time period. The only difference was the fraction devoted to Medicaid, health care, and aging, which increased by 5 percentage points. One possible explanation is that Medicaid, along with welfare, were designated as "matching federal funds," which provided Michigan (and other states) more incentive to maintain or increase these programs' investments compared to education finance.

Appendix B. Estimating a state-level margin of victory measure for Democratic house control

As noted in the main text, most RD studies identifying the effects of political party control of different governmental branches in the U.S. use voting results from a single election. Party control of the presidency, governorships, and mayorships, for example, depend only on the vote share of the Democratic candidate relative to that of the Republican candidate (or second highest vote receiving non-Democratic candidate). When vote shares between Democratic and Republican candidates running for these elected positions are similar, any observed effects of party control are less likely to be biased by omitted variables.

Identifying a state-level vote margin variable that can similarly both identify party control of state houses and the "strength" of this control is slightly more difficult. This is because party control is determined not by a single election. Instead, party control of houses depends on a party winning a majority of district-level elections, or house seats.

Ultimately, the dependency of "treatment" (i.e., Democratic party control of the house) on several "running variables" (i.e., Democratic vote margins across several district elections) calls for a variation of the traditional RD approach to identify causal effects. In our study, we focus on one such variation, where we reduce multiple running variables into a single metric. We consider three vote margin metrics in the main text and in appendices, following Feigenbaum et al. (2017): a Manhattan distance metric, a Euclidean distance metric, and a Uniform distance metric.

To estimate each of these three metrics, we first identify the n number of seats in each state house that would need to switch party hands to flip the majority status of the house. We then identify the individual district-level elections that would have been most likely to switch party hands in the appropriate direction: when the house is majority Democrat, we identify the n closest elections where the Democrat won; when the house is majority Republican, we identify the n closest elections where the Democrat lost. We leverage the absolute value of all n district-level vote margins, $DemDistrictMargin$, to calculate each state-level vote margin metric.

For the Manhattan distance, we simply add all n $DemDistrictMargin$ values (i.e., $\sum_1^n DemDistrictMargin$). For the Euclidean distance, we calculate the "straight-line" distance between the n closest elections and zero (i.e., $(\sqrt{\sum_1^n (DemDistrictMargin)^2}$), where the seat would have thus flipped party control. We multiply these distance measures so that Democrat

majority houses have positive values and Democrat minority houses have negative values (i.e., going over the zero threshold indicates the house party affiliation flipping). The intuition behind these first two metrics is that Democratic and Republican state houses with Manhattan or Euclidean distance scores closer to zero are more similar to one another than those with more extreme values. Larger values (more negative or more positive) indicate that more individual seats need to change hands to flip party control of the house and/or that the seats that would need to change hands are more strongly Republican or Democrat.

To arrive at the final metric, the Uniform distance score, we simply take the greatest $DemDistrictMargin$ value across the n closest district-level elections, again rescaling the score to be positive if the house is majority Democratic and to be negative if the house is majority Republican. The intuition behind the Uniform distance metric is that all n elections would have flipped party control if the seat in this set with the greatest vote margin flipped party control.

Appendix C. Estimating the impact of Democratic house control on outcomes using different models

Our preferred model to estimate the impact of a majority Democratic house on education finance and outcomes is the RD model represented in Equation (1). The RD identification strategy allows us to plausibly infer the causality of impact estimates. However, we also estimate simpler models with more stringent assumptions to infer causality to help demonstrate the consistency of our results.

First, we estimate a simple OLS model, where we predict our education finance and outcomes with just our primary covariates and an indicator variable for whether or not the state house was under Democratic control.

Second, we estimate a fixed effects model ("State & Election Year Fixed Effects") where in addition to covariates and the Democratic majority indicator, we also include state and election year fixed effects. This model specifically allows us to account for persistent context-specific differences in outcomes as well as contemporaneous trends in outcomes when identifying the effect of Democratic control of the house.

The final simpler model we estimate builds on this second model by using the same controls (and fixed effects) but by also restricting the sample of observations in the analysis. Specifically, we include only state-elections in analyses that we would also be including in the RD sample ("State & Election Year Fixed Effects RD Sample"). By restricting the analytic sample, we reduce the influence of potential outliers in terms of Democratic strength (or weakness) in the house on observed relationships between Democratic control of the house and education finance and outcomes.

In Appendix Table 2, we document the point estimates across models for majority Democratic control on our primary outcomes. We find that results are fairly consistent between the RD model and the simpler models.