# Unionization and Local College Enrollment: Evidence from Close Union Elections*

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**Abstract**

Union wage premia have historically been larger for workers with less education, potentially changing their incentive to attend college. In this paper I study whether local college enrollment changes in a county after unions win elections there. I combine data from NLRB certification elections held between 1959 and 2024 and a panel of institution-level college enrollment, which includes information from IPEDS and HEGIS, along with newly digitized printed reports of the U.S. Office of Education for the 1947 to 1967 period. I use the elections that unions won or lost by a narrow margin as a source of variation in new unionization at the county level, following the regression discontinuity aggregation framework. Four to seven years after a wave of close union wins, community-college enrollment in the county is 2.9 percent lower for a one-standard-deviation increase in new unionization, and the decline reaches 4.7 percent after twelve years. The number of associate's degrees granted at these institutions also falls a few years after enrollment does. I also find some weaker evidence that the share of men with a bachelor's degree declines.

**Keywords:** labor unions, college enrollment, community colleges, opportunity cost of schooling, regression discontinuity, NLRB elections.
**JEL codes:** J51, I26, J24, R23.

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# 1 Introduction

Unionization may affect schooling by changing the relative payoff to education. Farber et al. (2021) document a union family-income premium of 10 to 20 log points over the last eighty years and find that this premium is larger among less-educated households. A similar change in the payoff to education happened during the housing boom of the 2000s, when college attendance fell in the places where construction jobs paid well, mostly at two-year colleges (Charles et al., 2018). Fewer young men finished high school during the coal boom in Appalachia (Black et al., 2005), and the same happened during the fracking booms in shale counties (Cascio and Narayan, 2022).

In this paper I ask whether new unionization has a similar effect on college enrollment, given that a union win in a county makes more union jobs available to the young people who live there, some of whom may decide to work instead of going to college. Even though there is a large literature on teacher unions and the supply side of education (Hoxby, 1996; Lovenheim, 2009; Lovenheim and Willén, 2019), and on the effects of unionization on firms using close elections (DiNardo and Lee, 2004; Lee and Mas, 2012; Frandsen, 2021), I am not aware of work connecting union strength to the schooling decisions of young people in the surrounding labor market.

To study this question, I collected and harmonized the certification elections run by the National Labor Relations Board (NLRB) between 1959 and 2024, from four sources that each cover a different period. This gives a panel of 242,592 elections, and I was able to locate 98.5 percent of them in a county. The other part of the data is a panel of college enrollment by institution that runs from the fall of 1947 to the fall of 2023. From 1968 on, I use the federal surveys of colleges, the Higher Education General Information Survey (HEGIS) and its successor, the Integrated Postsecondary Education Data System (IPEDS). Before 1968, the U.S. Office of Education printed every fall a table with the enrollment of each college, and to my knowledge these tables have not been available as data. I digitized them for the falls of 1947 and 1949 to 1967, and linked each printed line to the institutions of the later surveys. In this paper I use the years before 1968 to measure the changes in enrollment before the elections of the earliest windows.

The main idea of the empirical strategy, following DiNardo and Lee (2004), is to compare elections that unions won or lost by a few votes. As an example, a plant where the union got 49 percent of the votes is presumably very similar to one where it got 51 percent, but only the workers at the second plant end up being represented by a union. Since young people decide whether to go to college based on the jobs available in the local labor market, and not in a single plant, I add up close elections at the county level, using the regression discontinuity aggregation framework of Borusyak, Kolerman-Shemer, and Lei (2026) and Kolerman-Shemer (2024). In this framework I control for the number of workers who voted in close elections in the county and for their vote margins, so that the variation that is left comes from which side

of 50 percent each of these elections falls on.

The main outcome is fall enrollment at the community colleges located in each county, which I define as the public two-year colleges. I group the elections into four-year windows and measure the change in enrollment from the last year of each window to four, eight and twelve years later, so that enrollment four years after the window is enrollment four to seven years after the elections. I find that community-college enrollment in the county falls after a wave of close union wins. For a one-standard-deviation increase in new unionization, enrollment is 2.9 percent lower four years after the window, and 4.2 and 4.7 percent lower after eight and twelve years. Enrollment at the other colleges of the county does not change, and neither does total enrollment at all of its colleges. The community colleges of the county also award fewer associate's degrees a few years later, 6.7 percent fewer eight years after the window for one standard deviation of new unionization. In the main specification the estimates for the years before the window are close to zero. The decline is similar with county fixed effects, even though it is a quarter to a third smaller at eight and twelve years with county trends or state-by-window fixed effects.

Since the enrollment data count students at the college where they enroll, and not where they live, a decline at the community colleges of a county does not necessarily mean that fewer young people from the county go to college. Some of them may enroll in other counties instead, or fewer colleges may open in the county, and several of my results are consistent with this. The estimates are close to zero when I do not weight the counties by private employment, or when I weight them by community-college enrollment, which suggests that the decline is proportional and comes from counties with many private-sector workers relative to their community-college students. At the level of the commuting zone the estimates are also close to zero, even though they are imprecise, and in the window that ends in 1970 enrollment at the community colleges of the other counties of the commuting zone goes up when it falls in the county where unions won. In my view, part of the response then seems to be a change in where students enroll, and I do not read these results as evidence of a national decline in college enrollment.

I also estimate the main specification separately by period, and the results are quite different. In the window that ends in 1970, when many counties were opening their first community college, 65 percent of the decline eight years after the window comes from colleges that enter the county's data later. Counties where unions won close elections in these years seem to open fewer community colleges afterward. In the windows that end between 1974 and 1982 I find no effect, and in those that end between 1986 and 2022 almost all of the decline is at colleges that already existed. At every horizon, I reject that the estimates are the same in the three periods.

With County Business Patterns, I find that employment and the number of establishments in the county do not change after the elections, and that payroll per worker rises slightly, by 0.51 percent per standard deviation of new unionization. In my view, the enrollment response most likely comes from union jobs, which are rationed (Abowd and Farber, 1982), becoming

easier to get for the young people who live nearby as the plants where unions won hire new workers. Using the decennial Census, I also find some evidence that new unionization lowers the share of men in the county with a bachelor's degree. Since this estimate appears only in logs and in the same decade as the elections, I see it as weaker evidence than the results for community colleges.

The source of variation in this paper is different from the one in the work on local labor-demand shocks and schooling. The booms in housing, coal and shale gas that I mention above created jobs for young people without a degree, and so did the export factories in Mexico studied by Atkin (2016), where students left school at the grades in which these jobs became available to them. The papers on the China shock look at the case of negative shocks (Autor et al., 2013; Greenland and Lopresti, 2016). A close union win instead changes the pay and the conditions of jobs that already exist. Unionization is also directly affected by labor law, which makes it more relevant for policy than commodity or housing booms. Even so, the response that I find is again concentrated at two-year colleges, as Charles et al. (2018) find for the housing boom.

Unions compress wages and raise pay in the lower-middle part of the distribution, and their decline explains an important part of the rise in male wage inequality (Card et al., 2004). An 18-year-old who does not go to college is likely to end up in this part of the distribution, working at a plant, a warehouse, or a construction site. The papers that use close NLRB elections mostly look at the firms where unions win. DiNardo and Lee (2004) find that union wins have essentially no effect on wages, employment, output, or the probability that the firm survives. With better data, Frandsen (2021) finds that they reduce payroll, employment, and survival, and with stock prices Lee and Mas (2012) find that they lower the value of the firm for shareholders. Other papers look at what unions do beyond wages, such as fringe benefits and safety enforcement (Knepper, 2020; Sojourner and Yang, 2022). Holmes (2006) had pointed out that the effects of unionization can go beyond the plant that is organized, and Kolerman-Shemer (2024) adds up close elections in commuting zones to study the effects of unions on local politics. I add up close elections in a similar way to look at college enrollment. The panel of elections I put together, which covers six decades and is located in counties, may also be useful for other questions about the local effects of unions.

Community colleges are at the center of the literature on access to college. Distance to the nearest college, or the presence of a college in the county, has long been used as a source of variation in schooling (Card, 1995; Kane and Rouse, 1995), and Rouse (1995) and Mountjoy (2022) ask whether community colleges raise educational attainment or divert students from four-year colleges. College openings have also been used to study the effects of education (Currie and Moretti, 2003), and Bleemer and Quincy (2026) study the openings of junior colleges in the first half of the twentieth century and their effects on college-going and on the local economy. Public two-year colleges compete with other schools for the same students, and in California

the passage of local bonds for community colleges diverted students from private schools to the public colleges (Cellini, 2009). My results suggest that enrollment at community colleges responds to unionization in the local labor market, and that in the late 1960s unionization may have affected whether a county opened one. The college panel I use, which extends enrollment by institution back to 1947, two decades before the federal surveys start, may also be useful for work on the expansion of community colleges in the 1950s and 1960s.

The remainder of the paper proceeds as follows. Section 2 describes the data, and Section 3 presents the research design and the evidence on its validity. Section 4 reports the results, and Section 5 concludes.

## 2 Data

### 2.1 NLRB representation elections, 1959–2024

In the private sector, a union usually gains the right to represent a group of workers by winning an election run by the NLRB, in a bargaining unit that the NLRB defines after the union or the workers file a petition. If the union gets more than half of the valid votes, the NLRB certifies it as the representative of every worker in the unit, including those who voted against it.

I put together these elections from four sources. For 1959 to 1979 I use the file compiled from the records of the AFL-CIO and distributed by J.P. Ferguson, and for 1980 to 1999 the series of Holmes (2006). For 2000 to 2008 I use the NLRB's Case Activity Tracking System (CATS), and from 2009 on the case pages that the NLRB publishes on its website. The Holmes file is organized by the fiscal year in which a case closed, and it misses part of the elections held in 1998 and 1999. I add the elections of these two years that are in the Ferguson file or in CATS and not in the Holmes file.[1] For each election I have the employer's name and location, the number of eligible voters, the votes for and against the union, and whether the union won.

I keep only certification elections, in which a union seeks to represent workers who are not yet represented (RC cases, in the NLRB's terms). The files for 1959 to 1999 also include decertification elections, in which the workers vote on removing a union, and elections petitioned by employers. In a decertification election a union win only keeps an existing union, and I drop both types in every year, so that the sample has the same kind of election throughout. The CATS and NLRB files that I use for the later years contain certification elections only. The assembled panel has 242,592 elections, and I locate 98.5 percent of them in a county, using the county codes of the older files and the employer's address in the newer ones.[2]

For the instrument I keep the units with at least 20 eligible voters, and, following Frandsen

[1] These are 728 elections of 1998 from the Ferguson file and 1,162 elections of 1998 and 1999 from CATS. Appendix B describes how I matched them to the Holmes file.

[2] I map every county code to the county definitions of the 1990s, which are also the ones of the commuting-zone crosswalk of Autor et al. (2013). Some of the county codes of the Holmes file are not valid in any vintage, and I place most of these elections using the city.

**Table 1:** NLRB certification elections by source period.

| Source (years) | Elections | Close (±10 points) | Union win rate | Win rate (close) | Located in a county (%) |
|---|---|---|---|---|---|
| AFL-CIO / Ferguson (1959–1979) | 68,300 | 19,169 | 0.50 | 0.44 | 99.8 |
| Holmes (1980–1999) | 39,030 | 10,455 | 0.44 | 0.42 | 99.5 |
| NLRB CATS (2000–2008) | 10,775 | 2,827 | 0.53 | 0.45 | 92.7 |
| NLRB filings (2009–2024) | 11,637 | 2,609 | 0.67 | 0.54 | 93.1 |
| Total (1959–2024) | 129,742 | 35,060 | 0.50 | 0.44 | 98.5 |

*Notes.* The instrument sample keeps NLRB certification (RC) elections with at least 20 eligible voters and drops ties and elections decided by one vote. Close elections are those within 10 points of a 50 percent union vote share, including exact 40–60 and 60–40 splits. Win rates are shares of elections the union won. The last column is the percent of elections located in a county. The 1980–1999 row includes 728 elections of 1998 from the Ferguson file and 1,162 CATS elections of 1998 and 1999 that the Holmes file does not contain. Before the sample restrictions the assembled panel has 242,592 elections, a union win rate of 0.55 and 98.5 percent located in a county. By period it has 131,058 (1959–1979), 70,036 (1980–1999), 19,682 (2000–2008), 21,816 (2009–2024). Elections in the instrument sample located in a county enter the treatment and the instrument (127,840 elections, 34,599 close). The instrument sample also drops 63 CATS elections without a recorded count of votes against the union. The Ferguson file covers elections from mid-1961. It has 1 election in 1959, 2 in 1960 and 3,233 in 1961, of which 49 were held before June and 3,020 from July on.

(2017), I drop ties and elections decided by a single vote. This leaves 129,742 elections, which Table 1 summarizes by source period. Of these elections, 35,060, or 27.0 percent, are close, with a union vote share within 10 points of 50 percent, including the exact 40 to 60 splits. Unions win half of the elections in the instrument sample, from 0.44 in 1980 to 1999 up to 0.67 since 2009, and 44 percent of the close ones, which is below one half (Section 3.4).

Figure 1 shows the number of elections in each year. There were 7,229 elections in 1965 and 6,963 in 1975, and the number then fell sharply in the early 1980s, from 5,809 in 1981 to 3,315 in 1983. By 2010 there were 1,707. The number of close elections that enter the instrument follows the same path at a lower level. The figure also shows the counts that the NLRB published in its annual reports, by fiscal year, and the panel follows them closely. It is a few percent below them before 1980, and above them after 2011, when the published reports count elections in a different way. The sharp fall of the early 1980s is also in the published counts, so it does not come from the change of source in 1980 (Appendix B).

Figure 2 shows the number of elections in each county. There was at least one election in 2,841 of the 3,137 counties, and at least one close election that enters the instrument in 2,231 of them. Close elections took place in almost every county of the Northeast, and in about two thirds of the counties in the rest of the country. Appendix Figures A3 and A4 show the same maps by decade.

For each county and window of years, I count the newly unionized workers, which are the eligible voters of the bargaining units where the union won. I also count the subset of those workers who were organized in close elections, which is the instrument.[3] I divide both

[3]I count all the eligible voters of the unit, and not only those who voted for the union. The aggregation controls

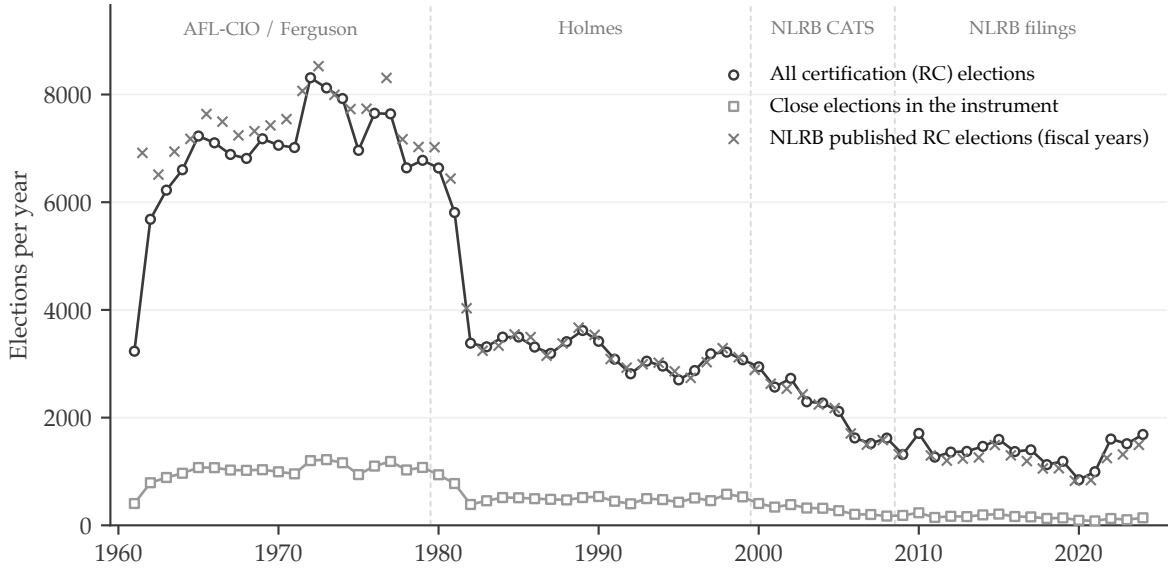



**Figure 1:** NLRB certification elections per year, 1961–2024. Circles count all the certification (RC) elections in the panel, squares the close elections of the instrument sample, and crosses the counts that the NLRB published for each fiscal year. Dashed lines mark the changes of source.

counts by private employment in the county at the start of the window, from County Business Patterns, and multiply them by 100, so that one unit corresponds to one new union member per 100 private-sector workers. I call the first variable NewU and the second $Z$. As an example, suppose a county has 50,000 private-sector workers and only one close election in the window, in a unit with 500 eligible voters. If the union wins, NewU and $Z$ for that window are both $100 \times 500/50{,}000 = 1$, and if the union loses, both are zero.

## 2.2 College enrollment, 1947–2023

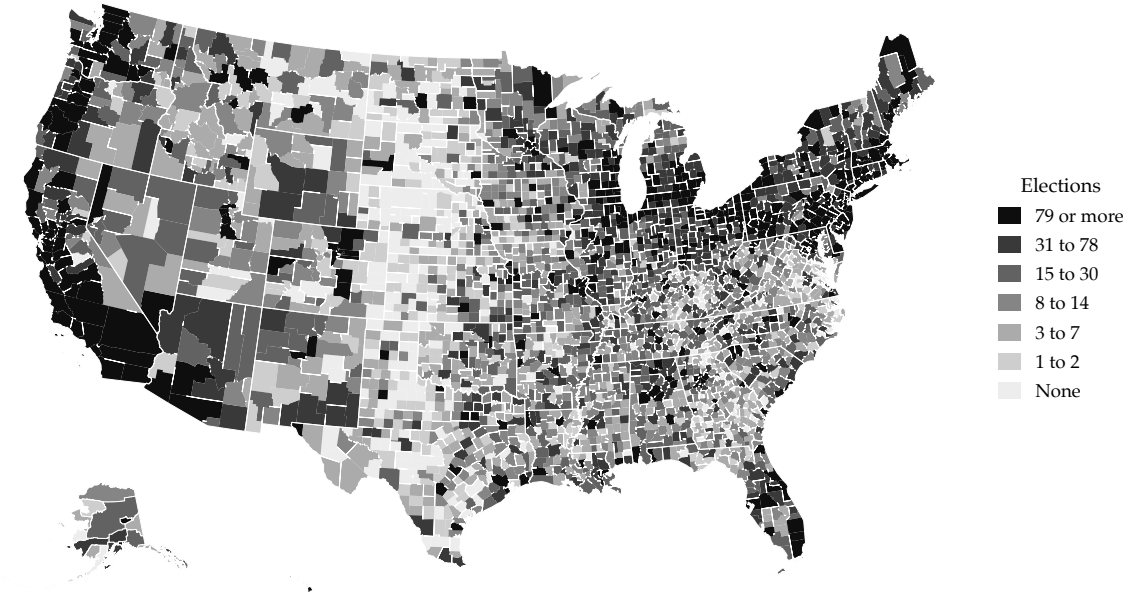
Since 1968, the federal government has collected the fall enrollment of every college in the country, first in HEGIS, from 1968 to 1985, and then in IPEDS, from 1986 on (Fuller, 2011; NCES, 1984–2023). Before HEGIS, the U.S. Office of Education ran an annual survey of fall enrollment and printed its results in circulars and reports, with one line per institution (U.S. Office of Education, 1947–1969). These printed tables are the only annual source I know of with the enrollment of every college before 1968, and I digitized them for this paper.[4]

I digitized the falls of 1947 and 1949 to 1967 from 20 publications scanned by HathiTrust, which are listed in Appendix Table A1. No institution table exists for the falls of 1946 and 1948, for which the Office of Education printed only national totals. Each number on a printed line

of Section 3 count voters in the same way.

[4]For the years before 1968, most work in economics has used national or State totals of enrollment (Bound and Turner, 2007), or institution data from private college guides that cover four-year colleges (Hoxby, 1997). Bleemer and Quincy (2025) digitize the College Blue Books for several years between 1923 and 1962.

A. Certification elections




B. Close elections in the instrument

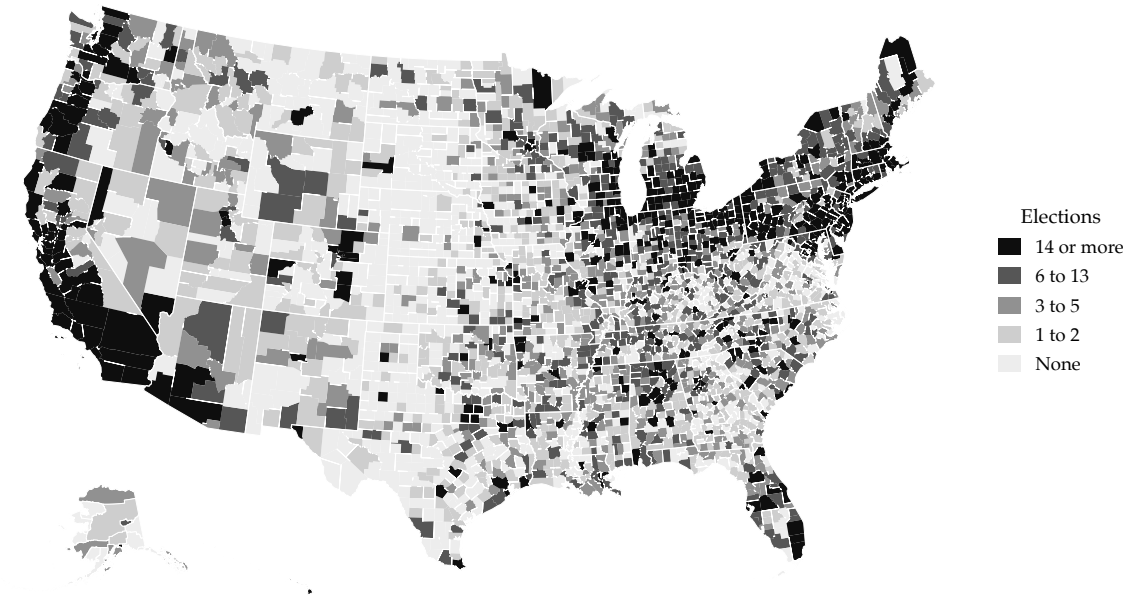



**Figure 2:** NLRB certification elections by county, 1959–2024. Panel A counts all the elections located in the county, and Panel B the close elections that enter the instrument. The shades are quantiles of the counties with at least one election.

was read at least twice by machine, once from the text layer of the scan and once by optical character recognition of the cell. A line was accepted when the readings agreed and its own identities held, such as the total being equal to the sum of men and women, and the lines that did not pass these checks were read by eye from the scan. Appendix Figure A1 shows two excerpts of the printed tables next to the digitized lines. In every year, the 39,970 digitized lines for the 50 States and DC add up to the State totals printed in the same tables and to the published national total.[5]

What the Office of Education counted as a student changed several times before 1968. From 1947 to 1952 the tables count resident students, which leaves out students only in extension or correspondence courses. From 1953 to 1955 they count students in courses of college grade, including those at extension centers. From 1956 to 1962 they count degree-credit students, whose programs lead mainly to a bachelor's or higher degree, which leaves out technical institutes and occupational programs. From 1963 on they count all students, as HEGIS and IPEDS do. Given these changes, I never compare enrollment levels across concepts, and each county's series is chained from changes between consecutive falls that count the same colleges under the same concept. The report for 1963, for example, prints both degree-credit students and all students, so I use degree-credit students for the change from 1962 to 1963 and all students after it. The report for 1953 has no such overlap, and for the change from 1952 to 1953 I leave out 36 of the 254 community colleges, those whose enrollment moved far more than in earlier years (Appendix Table A2).

To place the printed lines in counties, I linked them by name within State to HEGIS, starting from the table printed for 1968, whose 2,775 lines are identical to the HEGIS file of that year, and going back one year at a time. Between 92 and 98 percent of the lines in each year, with at least 96.8 percent of the students, are linked. Most of the rest are colleges that closed or merged before 1968, and the University of California, which the tables print as a single line until 1957. A few others never entered HEGIS.

From 1968 to 1985 I use the institution files of HEGIS, most of them distributed by ICPSR (NCES, 1966–1985), and from 1986 to 2023 the IPEDS Fall Enrollment files (NCES, 1984–2023), which I link through the FICE codes of HEGIS. Since IPEDS also covers schools that do not award degrees, the change from 1985 to 1986 uses only the institutions that were in the HEGIS universe in both years. I place each institution in the county where it is most often located in the IPEDS files from 2009 on and keep it there in every year, since in the raw files many institutions switch county for a year or two, often because the reported address changes, and each switch would create a fake change in enrollment. When the move is real, as when Kirtland Community College moved from Roscommon to Grayling in 2019, I keep it and drop the change of that year. I also drop the change of the year in which a community college is absorbed by an institution

[5]The exception is a few lines that are hidden or faded in the scans of 1947, 1953 and 1965. In 1947, for example, a library slip hides some lines of the District of Columbia, with 15,165 students. I leave these lines missing and drop the changes of their counties in those years (Appendix A).

that is not a community college, and I link the records of colleges that change their identifier.

From the 2000s on, some institutions enroll most of their students at a distance, often in other States. Their enrollment has little to do with the county where they are located, so I remove them in every year. I define an institution as online when at least half of its students were enrolled only in distance courses and lived in another State or abroad, in at least two of the falls from 2012 to 2019 and 2022 to 2023. I measure this with the distance-education counts of IPEDS. This rule flags the large online universities, such as Western Governors University and Southern New Hampshire University.[6]

I define community colleges as public two-year colleges. Before 1968 these are the public institutions that the Office of Education coded as two-year, and from 1968 on they are the institutions in the public two-year sector of HEGIS and IPEDS. The tables before 1963 print no codes, so each line takes the earliest code of its institution in the tables of 1963 to 1967.[7] A college's classification can change over time, and the number of public two-year colleges in the panel falls from 963 in 1986 to 791 in 2023, in part because colleges merged or moved to the four-year sector. In the last two decades many community colleges started to award a few bachelor's degrees and moved to the four-year sector of IPEDS, even though most of their students are still in associate's and certificate programs. Each of these moves would otherwise show up as a fake change in the enrollment of the county, and for this reason I fix the set of community colleges at the start of each annual change, with Miami Dade College, which moved to the four-year sector in 2003, in the change from 2002 to 2003 but not in the changes after it. I add a new college to the series of its county in the first fall in which it appears, and in that fall its students count as an increase in the county's enrollment. I call all the other institutions of the county other colleges.

Figure 3 shows the number of institutions and their enrollment in each fall from 1947 to 2023, together with the national totals of the Digest of Education Statistics (NCES, 2023). The panel has 1,749 institutions in 1947, 2,647 in 1967 and 3,844 in 2023, and the number of public two-year colleges goes from 235 in 1947 to 543 in 1967 and 950 in 1985. In every year from 1963 to 2022, national enrollment in the panel is within 1.4 percent of the Digest's total (Appendix Table A3). To compare the panel with the Digest, from 1986 on the figure keeps only the institutions that award an associate's or higher degree, as the Digest does. The county series that I use in the estimates also keeps the ones that do not award degrees, and leaves out those without a local campus, such as the online universities. Its national enrollment is 99 percent of the total in the figure in 1985, 102 percent in 2000 and 95 percent in 2020, and its enrollment at public two-year colleges is between 100 and 102 percent of the one in the figure in these years.

Figure 4 shows the counties with a community college in some of these years. In 1950 only

[6]The rule does not use the falls of 2020 and 2021. I also leave out institutions outside the 50 States and DC, the Community College of the Air Force and correspondence schools.

[7]This misses public junior colleges that became four-year colleges before 1963, which are then coded as four-year in every year.

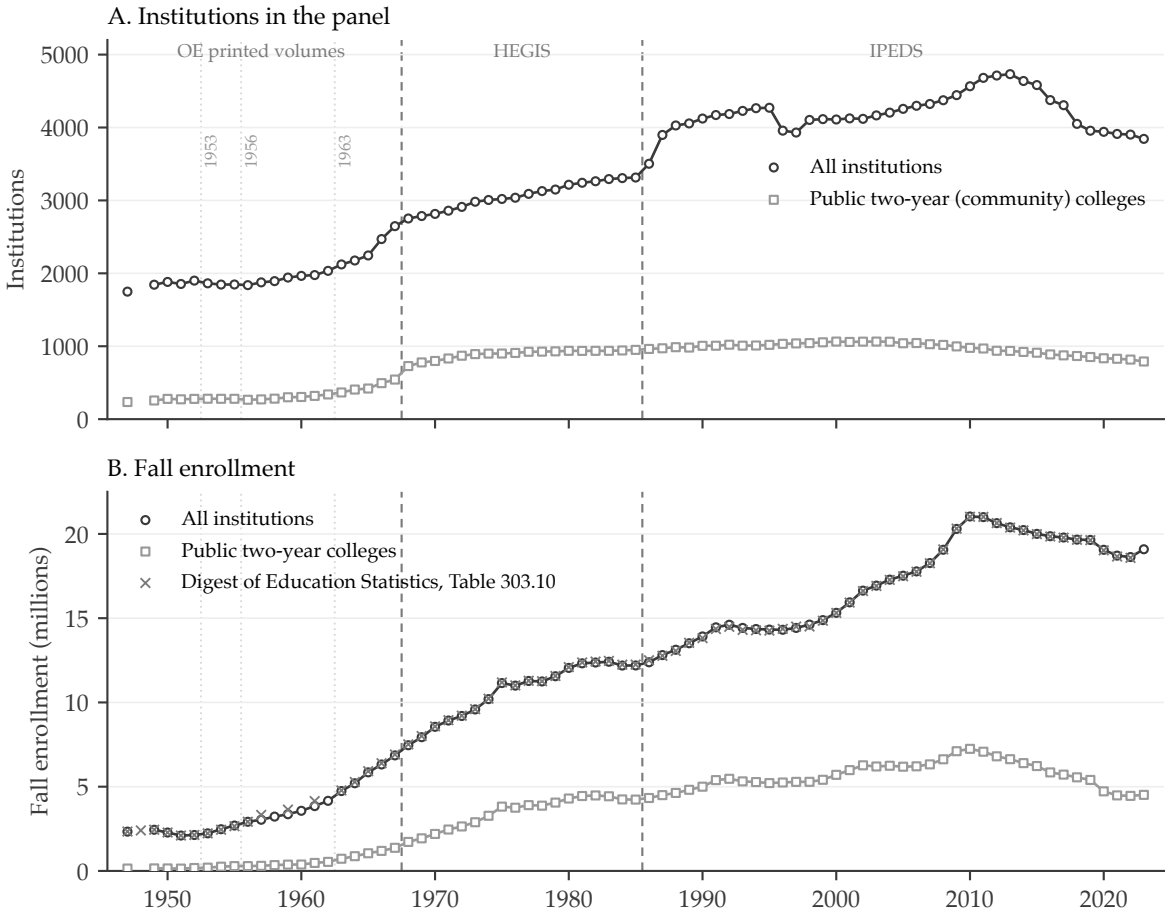



**Figure 3:** The college panel, fall 1947–2023, national totals. Panel A shows the number of institutions and Panel B their fall enrollment, for all institutions and for public two-year colleges. Crosses are the national totals of the Digest of Education Statistics. Dashed lines mark the changes of source, and dotted lines the changes in what the printed volumes count as a student.

235 counties had one, 27 of them in California and 25 in Texas. Many counties opened their first one in the 1960s and 1970s, and by 1980 there were 748 counties with a community college. The number rises to 912 in 2000 and falls to 747 in 2020, as some colleges merged or moved to the four-year sector.

The changes in enrollment after each window use only the falls from 1968 on. For the changes before and during the window I also use the digitized falls, so that the first windows have these changes as well. For the window that ends in 1970, for example, the change during the window goes from the fall of 1966 to the fall of 1970, and the two changes before the window go back to the fall of 1958. For the other colleges of the county I use the digitized falls only from 1966 on. I do not use the earlier data to add windows that end before 1970, since these windows would have fewer falls of data before the elections than the later ones.

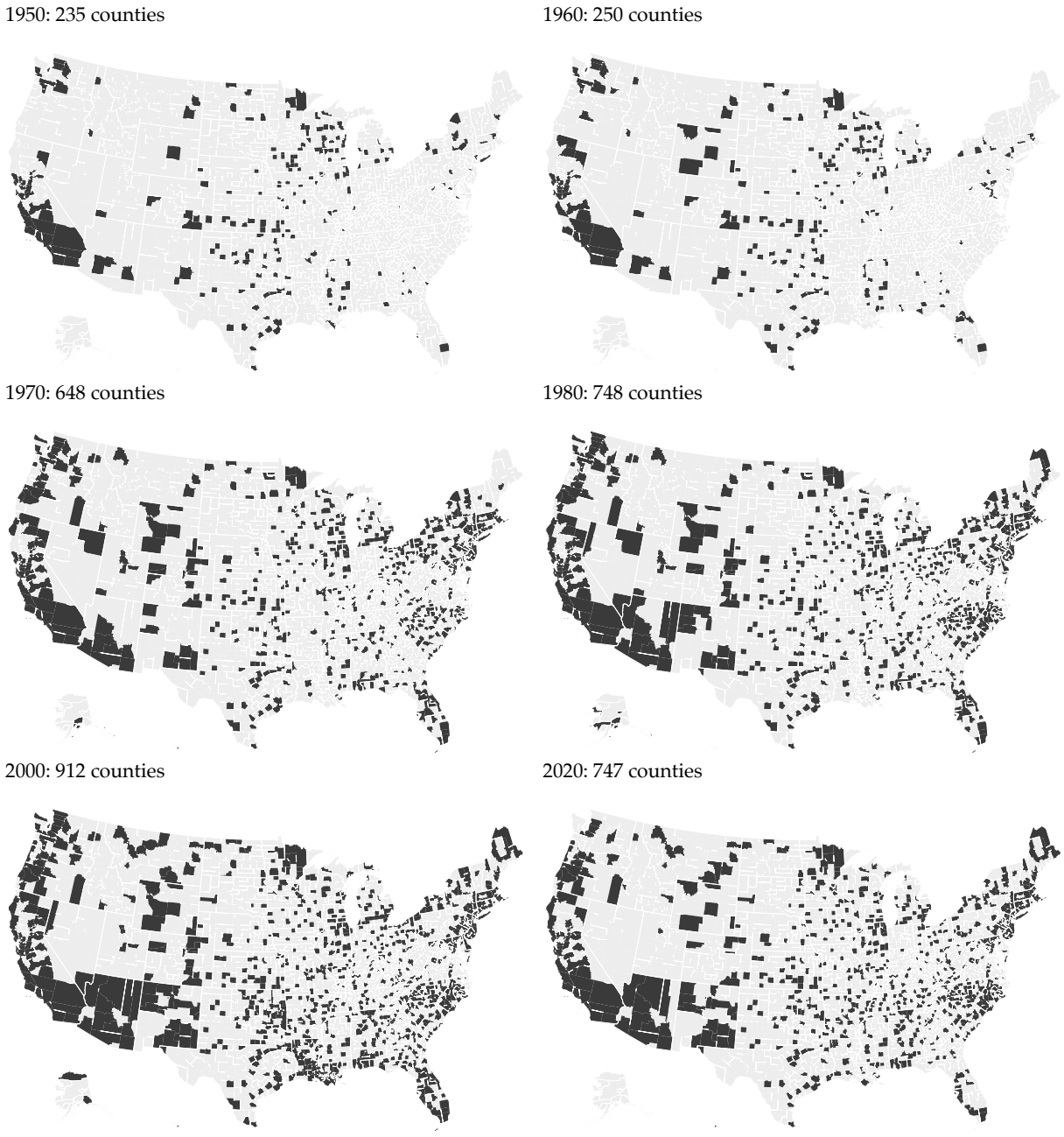



**Figure 4:** Counties with a community college, fall 1950–2020. Dark counties have at least one public two-year college in the fall shown.

### 2.3 Degrees awarded

For degrees, I use the HEGIS surveys of degrees and other formal awards for the academic years 1965–66 to 1981–82, and the completions files of NCES, which include IPEDS, for 1983–84 to 2022–23 (NCES, 1966–1985, 1984–2023). The files for 1969–70 and 1982–83 are missing. I add up the associate's degrees awarded by the community colleges of each county, with the same definition and county rules as for enrollment, and since a small college awards few of them, the count of a single year is noisy. For each year I then use the mean of the awards in the academic years that end in that year, the year before and the year after, or of the other two when 1969–70 or 1982–83 is one of them. I also look at the certificates of community colleges, at the bachelor's and other awards of the other colleges, and at all the awards of the county. The definition of sub-bachelor's awards changes across the HEGIS and IPEDS files, in 1967, 1971 and 1984, so for the outcomes that count certificates I drop the changes that span two definitions.

### 2.4 Employment and the analysis sample

I measure private employment with County Business Patterns (CBP), which reports mid-March employment in each county. I put together a county series from 1962 on and harmonized it to the county definitions used throughout.[8] This series scales NewU and $Z$ and gives the regression weights. For payroll per worker and the number of establishments I use the Census Bureau's county files for 1986, 1990, and every four years up to 2022.

The unit of analysis is a county and a four-year window of elections, with windows that end in 1970, 1974, and so on up to 2022 (Figure 5, Panel A). A county-window enters the community-college regressions when the county has private employment at the start of the window and community-college enrollment at both ends of the change in the outcome. Four years after the window, the sample has 10,050 county-windows in 1,035 counties and 13 windows. The employment-weighted mean of NewU in this sample is 0.42 new union members per 100 private workers, its standard deviation is 0.83, and 45 percent of the county-windows have at least one close election.

### 2.5 Educational attainment

I measure attainment with the decennial Census tabulations of 1970, 1980, 1990 and 2000 distributed by NHGIS, and the outcome is the change in the share of residents aged 25 and older with a bachelor's degree over the decades that end in 1980, 1990 and 2000. For this outcome I scale NewU by the population aged 16 and older.[9]

[8] The sources are the Census Bureau's county files, the files for the 1960s and 1970s distributed by ICPSR, and, for some years, the imputations of suppressed cells of Eckert et al. (2020). There is no CBP file for 1963. When a year is missing for a county, I use the nearest earlier year up to four years back, and otherwise the nearest later year. Counties whose total employment is suppressed are left missing.

[9] The share counts the degree-holders who live in the county, including those who studied in other counties. The censuses of 1970 and 1980 measure four or more years of college instead of a bachelor's degree, so the change from

# 3 Research design

## 3.1 Close elections aggregated to counties

As in DiNardo and Lee (2004), I use elections that were decided by a narrow margin. If an election is decided by a few votes, whether the union ends up with 49 or 51 percent should have nothing to do with local economic conditions, and for this reason I treat the new unionization that comes from these elections as exogenous. Schooling decisions respond to the local labor market and not to a single plant, so I aggregate close elections at the county and window level, following Borusyak et al. (2026) and Kolerman-Shemer (2024). The instrument for each county and window is the number of workers in the units where unions won a close election, scaled as in Section 2.1. Counties where more workers vote in close elections will tend to have a larger instrument even if the results are random, so I control for the aggregation terms described below.

Formally, let $j$ index the elections held in county $c$ during window $k$, with $n_j$ eligible voters and a union vote share $v_j$. The running variable is the margin $r_j = v_j - 0.5$, $D_j$ indicates a union win, and $C_j$ indicates a close election, $|r_j| \leq 0.10$. With $L_{ck}$ the private employment of the county at the start of the window,

$$\text{NewU}_{ck} = \frac{100}{L_{ck}} \sum_j n_j D_j, \qquad Z_{ck} = \frac{100}{L_{ck}} \sum_j n_j D_j C_j.$$

The aggregation controls, scaled in the same way, are the voters in close elections, their margins, and the margins on the winning side,

$$S_{ck} = \frac{100}{L_{ck}} \sum_j n_j C_j, \qquad SR_{ck} = \frac{100}{L_{ck}} \sum_j n_j C_j r_j, \qquad SR'_{ck} = \frac{100}{L_{ck}} \sum_j n_j C_j D_j r_j.$$

Once these terms are held fixed, the remaining variation in the instrument comes from which side of 50 percent each close election falls on.

## 3.2 Estimating equation, outcomes and timing

For a county $c$ and a window $k$ that ends in year $t$, I estimate

$$Y_{ck} = \beta \, \text{NewU}_{ck} + \gamma_1 S_{ck} + \gamma_2 SR_{ck} + \gamma_3 SR'_{ck} + \lambda_k + \varepsilon_{ck} \tag{1}$$

by two-stage least squares, with $\text{NewU}_{ck}$ instrumented by $Z_{ck}$ and with window fixed effects $\lambda_k$. As the outcome $Y_{ck}$ I use the change in log enrollment in the county over a span of years

1970 to 1980 is approximate. County boundaries are harmonized to the same county definitions as the rest of the data.

counted from $t$, so that for the span from $t + a$ to $t + b$ it is

$$\log E_{c,t+b} - \log E_{c,t+a}.$$

After the window I use the cumulative changes from $t$ to $t + 4$, $t + 8$ and $t + 12$, which I call 0 to 4, 0 to 8 and 0 to 12, and before and during the window the changes from $t - 12$ to $t - 8$, from $t - 8$ to $t - 4$ and from $t - 4$ to $t$.

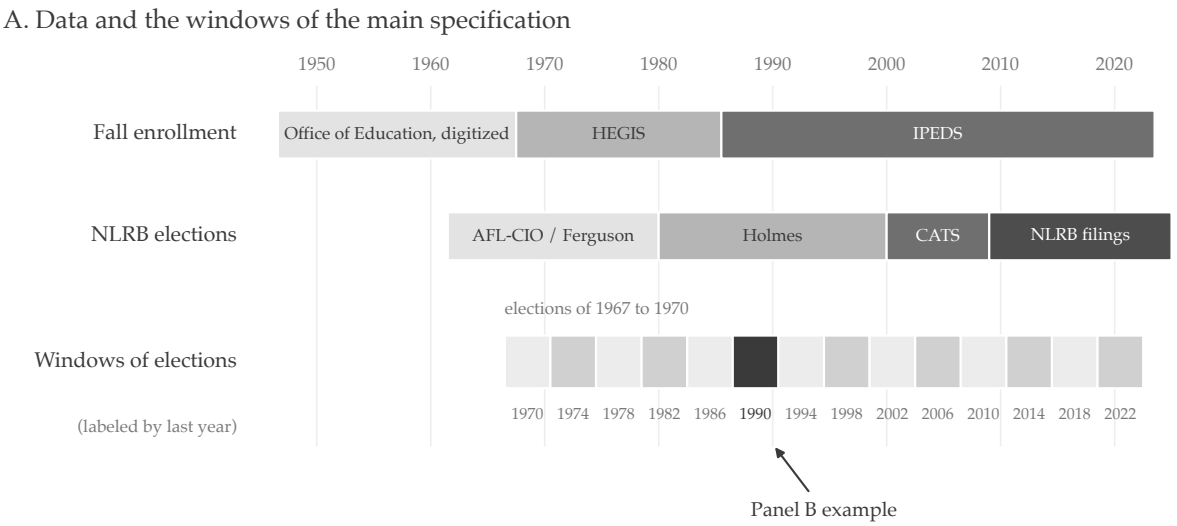
As an example, take the window that ends in 1990, which contains the elections held between 1987 and 1990. For this window, the span from 0 to 4 is the change in log enrollment between the fall of 1990 and the fall of 1994. The fall of 1994 is four years after the last elections of the window and seven years after the first ones. So when I say that enrollment falls four years after the window, I mean that it falls at some point between the fourth and the seventh year after the elections. The span from $-4$ to 0 is the change between the falls of 1986 and 1990, during the elections. The spans from $-8$ to $-4$ and from $-12$ to $-8$ are the changes between the falls of 1982 and 1986 and between the falls of 1978 and 1982, which end before any of the elections. NewU and $Z$ for this window are scaled by private employment in 1986. Panel B of Figure 5 shows the window and the six spans for this example.

I measure the changes after the window from its end so that all of them come after all of the window's elections. The cost is that each span after the window is spread over the four years of elections. If unions were winning close elections in counties where enrollment was already changing, the changes before the window would be different from zero. Later windows can also affect the outcomes of a window. For example, the span from 0 to 8 for the window that ends in 1990 covers the same years as the span from 0 to 4 for the window that ends in 1994. If the results of close elections are as good as random, the elections of later windows add noise to the outcome but are not correlated with the instrument.

Given that the outcome is already a change within the county, I include window fixed effects only, and no fixed effects for the county. Borusyak et al. (2026) also leave out fixed effects for the unit in their application, and they add other fixed effects only to gain precision, since in their framework the design with the aggregation controls should be enough to identify the effect. In Section 4.3 I add county fixed effects, county trends and state-by-window fixed effects. I weight each county-window by the same private employment that scales NewU, which for the window that ends in 1990 is employment in 1986, so that each county counts in proportion to the workforce whose jobs the elections can change, and I cluster the standard errors by county.

### 3.3 Lower-level specification

The specification above is what Borusyak et al. (2026) call the upper-level estimator, with one observation per county-window. They also propose a lower-level, or stacking, estimator, which



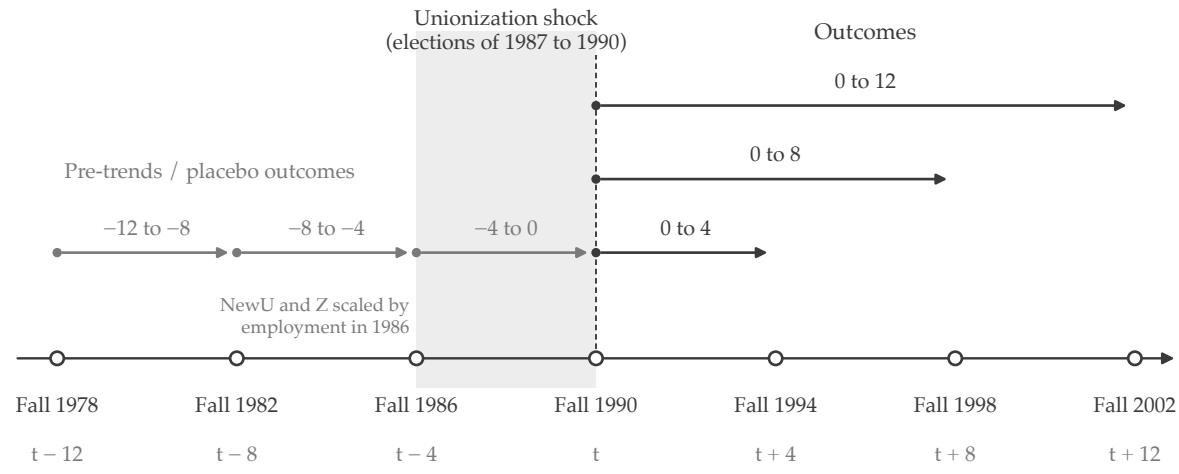




**Figure 5:** Data, windows and outcomes. Panel A shows the years covered by the enrollment and election data and the four-year windows of elections, labeled by their last year. Panel B shows the window that ends in 1990 and the six changes in log fall enrollment used as outcomes.

consists of a fuzzy regression discontinuity on the stacked sample of close elections. Each close election $j$ is an observation. Its running variable is the margin $r_j$, its instrument is the indicator $D_j$ for a union win, and its treatment and outcome are those of the county-window $i(j)$ in which the election took place:

$$Y_{i(j)} = \beta \, \text{NewU}_{i(j)} + \gamma_1 r_j + \gamma_2 r_j D_j + \lambda_{k(j)} + \varepsilon_j, \qquad |r_j| \leq 0.10, \tag{2}$$

with the observations weighted by the number of eligible voters $n_j$. A county with five close elections in a window then has five rows in the stacked sample, all with the same treatment and outcome, and each with the margin and the result of one of its elections. Borusyak et al. (2026) show that the two estimators are equal when the outcome and the treatment of the county-window are first residualized on the aggregation controls and the fixed effects. In other words, the only difference between the two is whether this residualization is done. The advantage of the lower-level version is that I can use the usual RD plots at the election level. Its disadvantage in my setting is a weaker first stage, since a single close win changes the treatment of a county only when the bargaining unit is large compared with local employment. For this reason I keep the upper-level specification as the main one.

### 3.4 Density of the vote margin and covariate balance

It is well documented that NLRB elections are not perfectly random at the threshold, since unions lose very close elections more often than they would if votes were random (Frandsen, 2017; McCrary, 2008). Besides applying the standard exclusions, I check the density of the vote margin and the balance of covariates in my own sample.

Figure 6 plots the density of the vote margin in the instrument sample. Since I exclude ties and one-vote margins, few elections fall within one point of 50 percent, given that a margin of two votes is that close only when at least 100 votes are cast. On the other hand, there are 1,066 losses and 884 wins at exact 40 to 60 splits, and in my view these spikes come from the many small units, where a split of 40 to 60 can come from very few votes. Even with these exclusions, unions win only 44.5 percent of the elections decided within 2 points.[10] If I kept the elections decided by a single vote, this share would fall to 31.6 percent, so most of the deficit of union wins comes from these elections, even though, as in Frandsen (2017), part of it remains after I drop them. By source period, the share of union wins within 2 points is 0.440 in the Ferguson period and 0.424 in the Holmes period, where the deficit is precisely estimated, 0.451 in the CATS period, where it is similar but imprecise, and 0.536 in the most recent period.

The aggregated design needs the instrument to be unrelated to the characteristics of the county that matter for schooling, once the aggregation controls are included. Following

[10] There are 2,825 such elections, and an exact binomial test rejects a win share of one half at the 1 percent level. Within 5 points, unions win 45.2 percent of 14,039 elections.

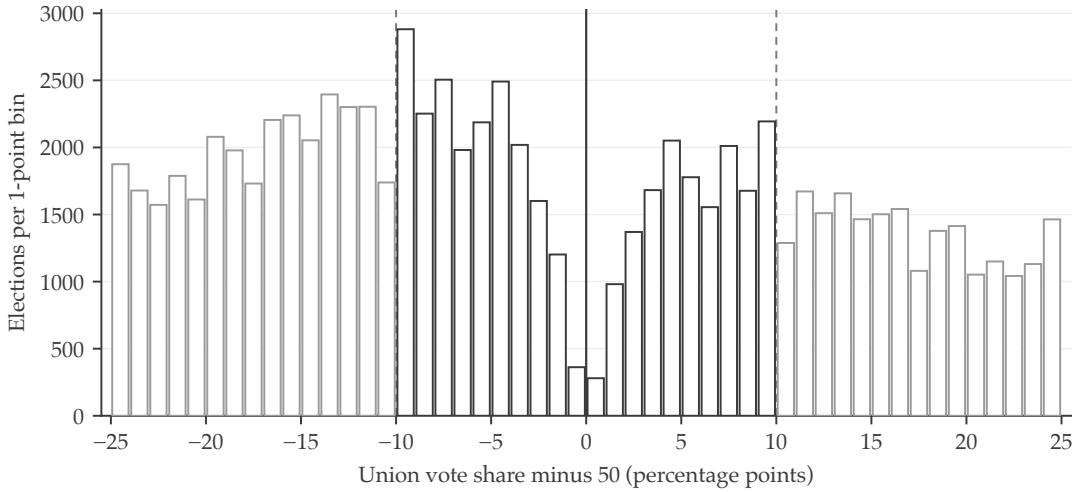



**Figure 6:** Density of the vote margin, instrument sample. Elections within 25 points of a 50 percent union vote share (84,956 elections), in 1-point bins. The dashed lines mark the close elections, within 10 points.

Borusyak et al. (2026), I regress NewU and $Z$ on 11 characteristics of the county measured before the window, each standardized, with window fixed effects and employment weights (Table 2). They cover the size of the county and its growth, its community colleges, whether it is in the South and its history of elections. Appendix Table A4 repeats the test with larger sets of 17 and 19 covariates.

Without controls, both new unionization and the instrument are strongly related to the county. In the regression of $Z$, 8 of the 11 covariates are significant at the 5 percent level, and the instrument is larger in counties with a smaller private workforce and in counties with more elections, both in the previous window and over the whole period. Controlling for $S$, the voters in close elections, is not enough, since 4 covariates are still significant at the 5 percent level and the covariates are still jointly significant. With $S$, $SR$ and $SR'$, as in the main specification, only one covariate is significant at the 5 percent level, and the covariates explain 0.4 percent of the variation in the instrument that is left after the fixed effects and the controls. The joint test then does not reject at the 10 percent level, with any of the three sets of covariates. This is the same pattern that Borusyak et al. (2026) find in their application, and as they do, I read it as evidence in favor of the design.

At the level of the elections, Appendix Table A5 compares the close elections on the two sides of the threshold, controlling linearly for the margin on each side. The elections that unions barely won were held somewhat later, by 0.79 years, and in counties with somewhat fewer elections over the whole period, by about 10 log points. The manufacturing share is also lower on the winning side, by 2.4 points, which is significant at the 10 percent level, and a joint test of the four covariates that I observe for every election rejects balance at the 5 percent level. The

**Table 2:** Balance of the instrument on pre-window county covariates.

| | NewU | $Z$ | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Any CC enrollment at $t-4$ | $-0.018$ | $-0.011^{**}$ | $-0.004$ | 0.000 |
| | (0.017) | (0.004) | (0.003) | (0.002) |
| Public two-year college in 1967 | 0.008 | $0.006^{**}$ | $0.005^{**}$ | 0.001 |
| | (0.010) | (0.003) | (0.002) | (0.001) |
| log private employment at $t-4$ | $-0.339^{***}$ | $-0.134^{***}$ | $-0.043^{**}$ | $-0.001$ |
| | (0.076) | (0.011) | (0.017) | (0.007) |
| Employment growth, $t-8$ to $t-4$ | $-0.034^{***}$ | $-0.002$ | 0.002 | $0.003^{**}$ |
| | (0.010) | (0.004) | (0.003) | (0.001) |
| log population (last census) | 0.083 | $0.033^{**}$ | $0.026^{**}$ | 0.008 |
| | (0.070) | (0.015) | (0.012) | (0.006) |
| Population growth, previous decade | $-0.016$ | $-0.001$ | $-0.005$ | $-0.003^{*}$ |
| | (0.017) | (0.005) | (0.003) | (0.002) |
| NewU, previous window | $0.115^{*}$ | $-0.005$ | 0.007 | 0.005 |
| | (0.060) | (0.005) | (0.006) | (0.004) |
| $S$, previous window | $-0.012$ | $0.030^{***}$ | $-0.025^{***}$ | $-0.008$ |
| | (0.018) | (0.006) | (0.008) | (0.006) |
| log(1 + elections), previous window | $0.128^{***}$ | $0.044^{***}$ | 0.014 | $-0.007$ |
| | (0.024) | (0.008) | (0.009) | (0.004) |
| log elections in the county, 1959–2024 | $0.164^{***}$ | $0.056^{***}$ | 0.004 | $-0.004$ |
| | (0.036) | (0.010) | (0.010) | (0.004) |
| South | $-0.003$ | $0.007^{**}$ | 0.001 | $-0.002^{*}$ |
| | (0.008) | (0.003) | (0.002) | (0.001) |
| Covariates significant at 5 percent | 4 | 8 | 4 | 1 |
| Partial $R^2$ | 0.057 | 0.029 | 0.010 | 0.004 |
| Partial $F$-statistic | 39.4 | 29.0 | 3.0 | 1.3 |
| Partial $F$-test, $p$-value | $< 0.001$ | $< 0.001$ | $< 0.001$ | 0.197 |
| Joint $p$-value, set B (17 covariates) | $< 0.001$ | $< 0.001$ | $< 0.001$ | 0.251 |
| Joint $p$-value, set C (19 covariates) | $< 0.001$ | $< 0.001$ | $< 0.001$ | 0.396 |
| Aggregation controls | None | None | $S$ | $S$, $SR$, $SR'$ |
| County-windows | 9,885 | 9,885 | 9,885 | 9,885 |
| Counties | 1,008 | 1,008 | 1,008 | 1,008 |

*Notes.* Each column is a weighted least squares regression, by county and 4-year window, of the variable at the top on the covariates listed: NewU (new union members per 100 private workers in the window) in column (1) and $Z$ (the members added in close union wins) in columns (2) to (4). Column (3) controls for $S$ (voters in close elections) and column (4) also for $SR$ and $SR'$, as the main specification does. All regressions include window fixed effects and are weighted by CBP private employment at the start of the window. Standard errors clustered by county are in parentheses. Covariates are measured before the window and standardized (employment-weighted mean zero and standard deviation one), so each coefficient is the change in NewU or $Z$ for one standard deviation of the covariate. The sample is the county-windows of the main estimate from $t$ to $t+4$ (Table 3) with every covariate of the set observed. Set A has the 11 covariates shown (windows ending 1970–2018). Set B adds CC enrollment growth from $t-12$ to $t-8$ and from $t-8$ to $t-4$, public two-year colleges, the manufacturing share of employment, the share Black and the share of adults with a BA (6,970 county-windows, windows ending 1982–2018). Set C adds the poverty and unemployment rates (5,556 county-windows, windows ending 1982–2010). Appendix Table A4 reports their coefficients. The partial $R^2$ and $F$ statistic refer to the covariates jointly, net of the fixed effects and controls, and $F$ is the cluster-robust Wald statistic divided by the number of covariates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

difference in the election year comes from pooling elections over six decades, since within each source period it is small and not significant, except from 2009 on, where it goes in the opposite direction. The share of close elections won by unions changes over time (Appendix Figure A2), so a later year on the winning side seems to reflect this trend, which the window fixed effects absorb. The number of elections in the county is one of the covariates of Table 2, and with the aggregation controls its coefficient is close to zero.

Appendix Table A6 uses each of 22 covariates measured before the window as the outcome of the main IV regression. None of the estimates is significant at the 5 percent level. Three are significant at the 10 percent level, the growth of enrollment at the other colleges of the county before the window and two measures of the elections of the previous window. The joint tests for each of the three groups of covariates do not reject either, at the 10 percent level. When I test all 22 jointly, however, the test rejects at the 5 percent level. In my view, this result, together with the deficit of union wins next to the threshold and the gaps at the level of the elections, is a reason for some caution. Still, the estimates before and during the window, which are close to zero in the main specification (Table 3), give some reassurance that the elections that unions won were not in counties with different trends in enrollment.

# 4 Results

## 4.1 Community-college enrollment

Table 3 reports the estimates of equation (1) for enrollment at all the colleges of the county, at its community colleges and at its other colleges, with one column for each span, and Figure 7 plots them for community colleges and for the other colleges. For community colleges, the first-stage $F$ statistics are between 219 and 330, and the first-stage coefficient is close to one in every column (Appendix Table A7), which is what I would expect if close union wins add to new unionization one for one.

For community colleges, the three estimates before and during the window are close to zero in the main specification. The estimate is 0.015 from 12 to 8 years before the end of the window, $-0.013$ from 8 to 4 years before it, and $-0.008$ during the window. After the window, enrollment falls. Four years after the end of the window, which is four to seven years after the elections, the estimate is $-0.035$, and it grows to $-0.048$ after eight years and $-0.051$ after twelve, with all three estimates significant at the 5 percent level.

Since the outcomes after the window are cumulative changes from its end, these estimates say that most of the decline happens in the first four to seven years after the elections, and that enrollment does not recover over the following eight years. A delay of a few years seems reasonable to me, given that it probably takes some time for a wave of union wins to turn into union jobs that a prospective student can see. The coefficients are per new union member per 100 private workers. The employment-weighted standard deviation of NewU in the sample

**Table 3:** Close-election unionization and college enrollment.

| | Before the window | | During | After the window | | |
|---|---|---|---|---|---|---|
| | −12 to −8 | −8 to −4 | −4 to 0 | 0 to 4 | 0 to 8 | 0 to 12 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. IV estimates, change in log fall enrollment* | | | | | | |
| All colleges | 0.003 | 0.003 | 0.005 | 0.001 | −0.001 | 0.003 |
| | (0.018) | (0.014) | (0.008) | (0.008) | (0.009) | (0.013) |
| Community colleges | 0.015 | −0.013 | −0.008 | −0.035** | −0.048** | −0.051** |
| | (0.040) | (0.027) | (0.026) | (0.015) | (0.021) | (0.025) |
| Other colleges | −0.008 | 0.010 | −0.005 | 0.004 | −0.009 | −0.013 |
| | (0.021) | (0.012) | (0.011) | (0.010) | (0.014) | (0.018) |
| *Panel B. Community colleges* | | | | | | |
| Kleibergen–Paap $F$ | 330 | 286 | 290 | 245 | 225 | 219 |
| County-windows | 9,380 | 10,018 | 10,585 | 10,050 | 8,934 | 7,892 |
| Windows ending | 1970–2022 | 1970–2022 | 1970–2022 | 1970–2018 | 1970–2014 | 1970–2010 |
| Effect of one SD of NewU (%) | 1.0 | −0.8 | −0.6 | −2.9 | −4.2 | −4.7 |

*Notes.* Each coefficient comes from a separate regression. The unit is a county and a 4-year window of elections, with windows ending in 1970, 1974, …, 2022. The outcome is the change in log fall enrollment in the county. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: before the window ($t-12$ to $t-8$ and $t-8$ to $t-4$), during it ($t-4$ to $t$) and after it ($t$ to $t+4$, $t+8$ and $t+12$). Column (4) compares fall $t+4$ with fall $t$, so it measures enrollment 4 to 7 years after the elections of the window. Columns (4) to (6) use falls from 1968 on. Columns (1) to (3) also use the falls before 1968 from the digitized Office of Education reports, from 1958 for community colleges and from 1966 for the other colleges, so that they include the windows ending 1970 to 1978. Each column uses the windows for which both falls are observed (Panel B). NewU, new union members per 100 private workers in the window, is instrumented by the members added in close union wins (within 10 points of a 50 percent vote share). All regressions control for the aggregation terms $S$ (voters in close elections), $SR$ (their vote margins) and $SR'$ (the margins on the winning side) and include window fixed effects. NewU is scaled by, and the regressions are weighted by, CBP private employment at the start of the window. Community colleges are public two-year colleges, with the set fixed at the start of each annual change. Other colleges are all other colleges in the county, and all colleges is their total. The effect of one SD is 100 times the coefficient times the employment-weighted SD of NewU in the column's sample. Standard errors clustered by county are in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

from 0 to 4 is 0.83, so a one-standard-deviation increase lowers community-college enrollment by about 2.9 percent four to seven years after the elections, and by 4.2 and 4.7 percent after eight and twelve years.

The response is at community colleges, where a job and school are the closest substitutes, as Charles et al. (2018) also find for housing booms. Enrollment at the other colleges of the county does not respond. After the window their estimates are 0.004, −0.009 and −0.013, and none of the six estimates in this row is significant. Enrollment at all the colleges of the county does not change at any span either, with estimates between −0.001 and 0.005. This row uses every county-window with college enrollment, 16,667 four years after the window, and not only those with a community college (Appendix Table A7). Community-college enrollment falls for both men and women, by 3.8 and 4.0 log points four years after the window (Appendix Table A8).

The lower-level estimates of equation (2), which stack the close elections of the same county-

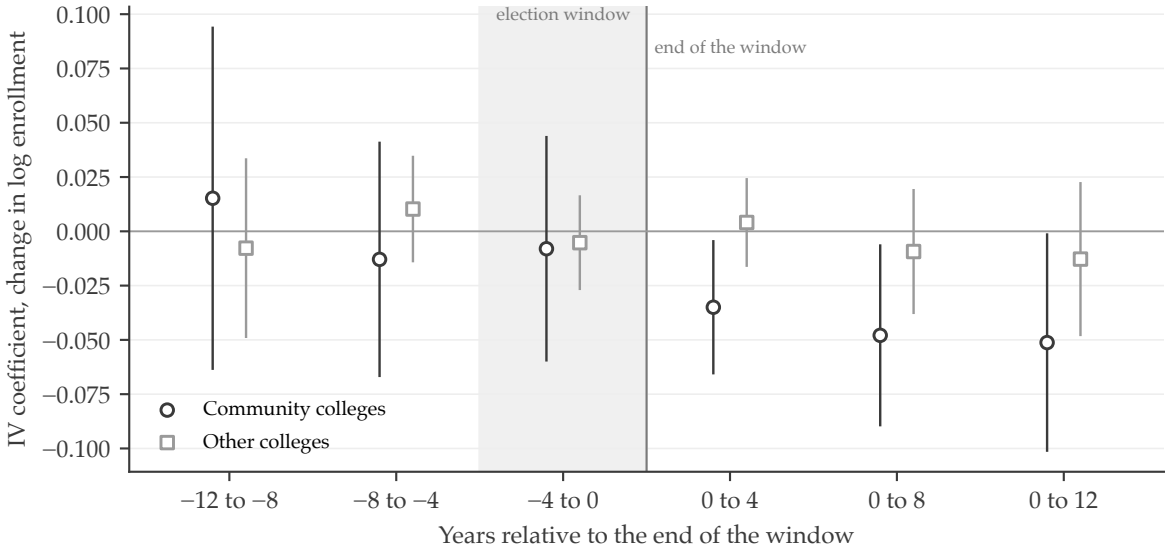



**Figure 7:** Enrollment response by span. IV coefficients of Table 3 with 95 percent confidence intervals, clustered by county. Each span is a change in log fall enrollment counted from the last year of the window, which is shaded.

windows, are −0.032, −0.044 and −0.049 after the window, similar to the main ones but less precise (Appendix Table A9). Figure 8 shows the RD plots of the stacked sample, without fixed effects. At the threshold, NewU jumps by 0.96 new union members per 100 workers (Panel A). The change in log community-college enrollment from 8 to 4 years before the end of the window changes by only −0.006 (Panel B). The change from the end of the window to four years later drops by 0.041, and the change to eight years later by 0.064 (Panels C and D). The ratio of the jumps in Panels C and A is the lower-level estimate without fixed effects in Appendix Table A9.

## 4.2 Associate's degrees

If fewer young people enroll at community colleges, the colleges should also award fewer associate's degrees some years later. Table 4 has the same spans as Table 3, with the log number of awards as the outcome, for all the awards of the county and, separately, for the associate's degrees and certificates of community colleges and the bachelor's and other awards of the other colleges.

For associate's degrees, the estimates before the window are 0.024 and 0.012, and neither is significant. During the window associate's degrees rise, with an estimate of 0.055 that is significant at the 10 percent level, and after the window they fall. Even though enrollment has already fallen four years after the window, the estimate for degrees at that point is −0.035 and not significant. After eight years it is −0.078, significant at the 5 percent level, and after twelve years it is −0.077, significant at the 10 percent level, so that one standard deviation of

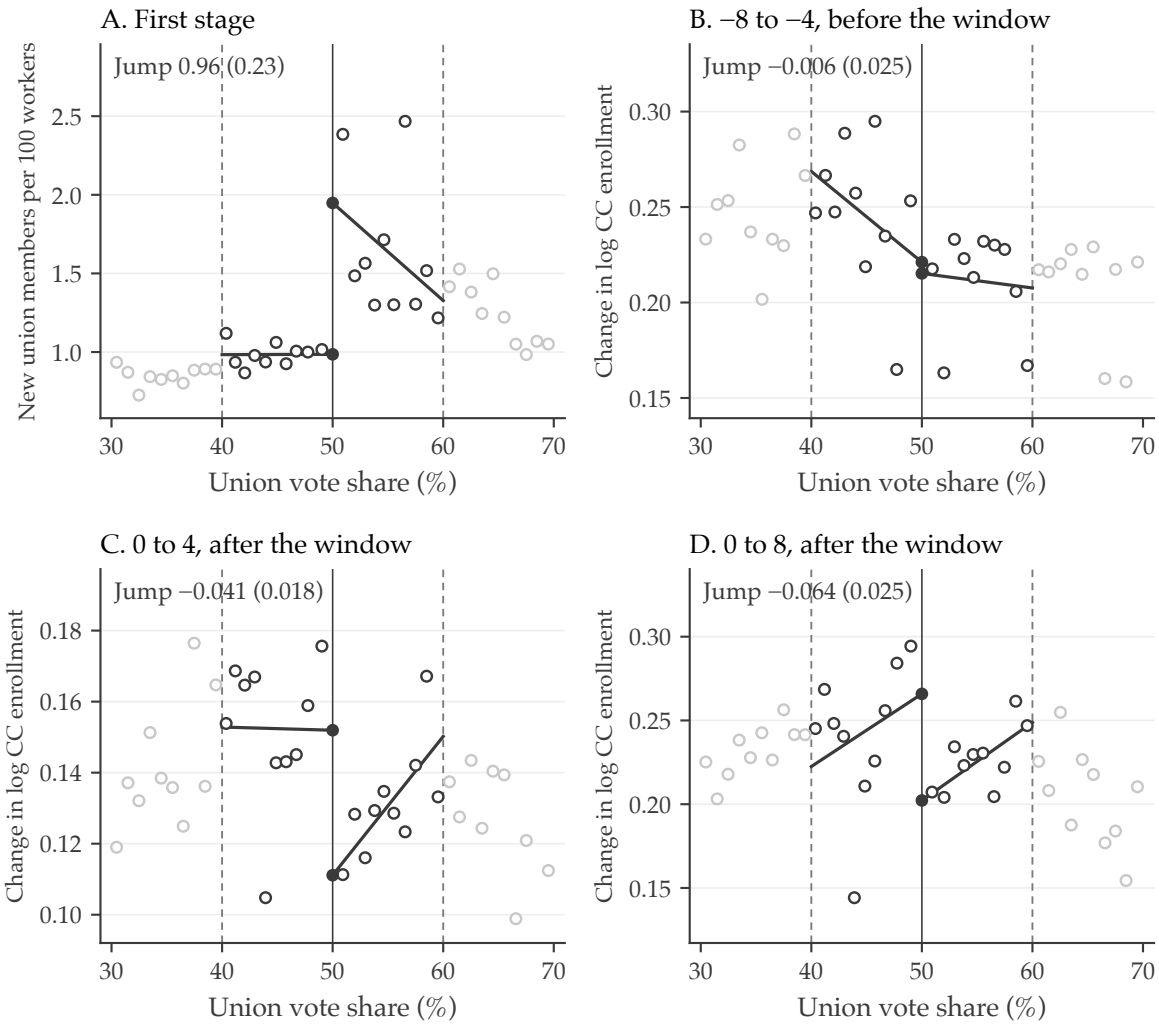



**Figure 8:** Lower-level RD plots. Each observation is a close election in a county-window of the main sample, with the county-window's NewU (Panel A) or its change in log community-college enrollment over the span of the panel (Panels B to D). Dots are means in bins, weighted by eligible voters, and lines are weighted linear fits on each side of the cutoff within the dashed lines, without fixed effects. Each panel prints the jump at the cutoff, with its standard error clustered by county.

**Table 4:** Close-election unionization and college awards.

| | Before the window | | During | After the window | | |
|---|---|---|---|---|---|---|
| | $-12$ to $-8$ | $-8$ to $-4$ | $-4$ to 0 | 0 to 4 | 0 to 8 | 0 to 12 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. IV estimates, change in log awards* | | | | | | |
| All awards, all colleges | $-0.012$ | 0.025 | $-0.014$ | $-0.009$ | $-0.003$ | 0.012 |
| | (0.021) | (0.017) | (0.012) | (0.009) | (0.015) | (0.043) |
| Community colleges | | | | | | |
| Associate's degrees | 0.024 | 0.012 | 0.055* | $-0.035$ | $-0.078$** | $-0.077$* |
| | (0.045) | (0.029) | (0.033) | (0.030) | (0.036) | (0.042) |
| Other awards | $-0.035$ | 0.097 | $-0.077$ | $-0.006$ | $-0.001$ | 0.093 |
| | (0.086) | (0.094) | (0.064) | (0.052) | (0.081) | (0.176) |
| Other colleges | | | | | | |
| Bachelor's degrees | $-0.001$ | $-0.010$ | 0.004 | $-0.014$ | $-0.024$ | $-0.037$ |
| | (0.021) | (0.018) | (0.013) | (0.012) | (0.018) | (0.026) |
| All other awards | 0.044 | $-0.013$ | $-0.031$ | $-0.060$* | $-0.011$ | 0.113 |
| | (0.081) | (0.072) | (0.045) | (0.033) | (0.052) | (0.107) |
| *Panel B. Community colleges: associate's degrees* | | | | | | |
| Kleibergen–Paap $F$ | 319 | 367 | 310 | 279 | 266 | 257 |
| County-windows | 8,435 | 9,202 | 9,939 | 9,508 | 8,519 | 7,580 |
| Windows ending | 1978–2022 | 1974–2022 | 1970–2022 | 1970–2018 | 1970–2014 | 1970–2010 |
| Effect of one SD of NewU (%) | 1.1 | 0.6 | 4.0 | $-2.9$ | $-6.7$ | $-6.9$ |

*Notes.* Main specification of Table 3. The outcome is the change in the log number of awards. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: before the window ($t-12$ to $t-8$ and $t-8$ to $t-4$), during it ($t-4$ to $t$) and after it ($t$ to $t+4$, $t+8$ and $t+12$). Awards in year $y$ are the mean of the awards in the academic years ending in $y-1$, $y$ and $y+1$ (a 3-year sum divided by three). No award files exist for 1970 and 1983. When the 3-year span includes one of them, the mean uses the other two years. Award years run from 1966 to 2023. Community colleges are public two-year colleges, with the set fixed at the start of each annual change. Their other awards are certificates, the sub-bachelor's awards that are not associate's degrees. All awards counts every award of every college in the county, and the other colleges' other awards are their sub-bachelor's and graduate awards. The definition of sub-bachelor's awards changes across the HEGIS and IPEDS files (in 1967, 1971 and 1984), so changes that span a definition change are dropped from the three rows that include certificates (all awards, community colleges' other awards and other colleges' other awards). These rows use fewer windows: windows ending 1974–2018 at 0 to 4 years and 1986–2010 at 0 to 12 years. Standard errors clustered by county are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

new unionization lowers associate's degrees by about 6.7 percent eight years after the window. In Appendix Table A8, associate's degrees are 10.4 log points lower for women and 7.0 for men at that point, and the estimate for women is also more precise.

The certificates of community colleges are noisy, and none of their estimates is significant. For the other colleges, the estimates for bachelor's degrees are small and not significant, and their other awards fall by 0.060 four years after the window, which is significant at the 10 percent level, with no significant estimate at the other spans. The total number of awards in the county does not change at any span, with estimates between $-0.014$ and 0.025.

### 4.3 Robustness

Appendix Table A10 reports the community-college estimates when I change one element of the main specification at a time. Panel A of the table repeats the main specification.

**Fixed effects.** With county and window fixed effects (Panel B), the estimates are $-0.033$, $-0.047$ and $-0.053$, almost the same as the main ones. I also add state-by-window fixed effects, which absorb shocks common to the counties of a State, such as a change in State funding for community colleges. With them, the estimate at four years is similar, at $-0.031$, but the estimates at eight and twelve years shrink to $-0.031$ and $-0.032$, and the last one is not significant. Adding county trends to the county fixed effects gives estimates of $-0.026$, $-0.036$ and $-0.036$, and only the eight-year estimate is significant, at the 10 percent level. In both cases the estimates at longer horizons are about a quarter to a third smaller than the main ones.

**Definitions.** If I count as community colleges the public colleges that award mainly associate's degrees, with or without the public two-year colleges, the estimates are very close to the main ones (Panel C). If I decide the public two-year status year by year, without fixing the set of colleges at the start of each change, the estimates are $-0.027$, $-0.035$ and $-0.047$. These are somewhat smaller and less precise, which seems consistent with the noise that reclassifications add to the outcome.

**Scaling and weights.** Panel D changes the scaling, the weights and the grid of windows. Scaling NewU by employment at the end of the window, and weighting by it, gives slightly larger estimates. However, the estimates are close to zero when the county-windows are not weighted, at $-0.003$, $-0.001$ and 0.011, and also when they are weighted by community-college enrollment. In Panel E, the same happens when each change is weighted by enrollment at its start, and with a Poisson regression of enrollment at the end of the change, which gives more weight to the counties with more students.

These results say that the decline is proportional and that it comes from a particular set of counties. With employment weights, the estimate averages the proportional change in enroll-

ment across counties in proportion to their private workforce. Without weights every county counts the same, and with weights equal to community-college enrollment the estimate is close to the proportional change in the total number of students across counties. The decline is then concentrated in counties with many private-sector workers relative to their community-college students, and it does not show up as a change in the number of students when the counties are added up. I keep employment weights in the main specification because the treatment is a share of private employment, but these results limit what the estimates can say about how many students the elections keep out of college.

**Grid of windows.** The main grid uses windows that end in 1970, 1974, and so on. Pooling the four possible grids of four-year windows gives estimates of $-0.031$, $-0.046$ and $-0.038$. On its own, the grid with windows that end in 1972, 1976, and so on, gives weaker estimates, at $-0.024$ and $-0.035$ after four and eight years, and $-0.004$ after twelve years.

**Functional form.** Panel E changes the outcome. A log change computed with one set of colleges fixed at the end of the window, the growth rate of Davis et al. (1996), which is defined even when enrollment starts at zero, and the change as a percent of the starting level all give negative estimates. The last one is larger than the main estimates at eight and twelve years, at $-6.48$ and $-9.24$ percent per unit of NewU. The share of community colleges in the county's college enrollment falls by 0.89 percentage points four years after the window, which is another way of seeing that the other colleges do not share the decline.

**Geography.** To compare counties with commuting zones, in Panel F I use chained four-year changes, which I can compute for both. For counties these changes give estimates of $-0.031$, $-0.040$ and $-0.048$, close to the main ones. For commuting zones the estimates are close to zero and positive, at 0.018, 0.007 and 0.021, so part of the decline in the county seems to be offset by enrollment in the other counties of the same labor market. For the window that ends in 1970, I also estimate the main specification with the enrollment of the other counties of the commuting zone, or of the whole commuting zone, as the outcome. Community-college enrollment in the other counties rises when the county's own enrollment falls, and the total for the commuting zone barely changes.[11] In the windows that end in 1986 or later, enrollment in the other counties does not move.

**Balanced sample.** In Panel G I keep the 5,927 county-windows observed at every point from twelve years before to twelve years after the window, in the windows that end between 1970 and 2010. On this balanced sample the estimates are $-0.007$, $-0.012$ and $-0.012$. On the county-windows of the same windows that are left out of it, the estimates are $-0.049$, $-0.080$ and

[11] I cluster the standard errors of these estimates by commuting zone. They are not reported in the tables.

$-0.093$. On all the county-windows of these windows, they are $-0.032$, $-0.045$ and $-0.051$, very similar to the main ones. The county-windows left out are those where community-college enrollment is missing at some point in these 24 years, which happens, for example, when the county's first college opened during this period or when one of the annual changes is dropped. This suggests that a large part of the decline comes from counties where the set of community colleges changed around the time of the elections, which is also what the results by period suggest.

### 4.4 Differences across periods

The windows of the main sample cover very different years for community colleges. In 1967 there were 543 public two-year colleges in the panel, and by 1985 there were 950, after a period in which many counties opened their first one. After the mid-1980s the number changed little until the 2000s, when many colleges merged or moved to the four-year sector. Appendix Table A11 estimates the main specification separately for the window that ends in 1970, the windows that end between 1974 and 1982, and those that end between 1986 and 2022.

The three periods give very different estimates. For the window that ends in 1970, the estimates are $-0.099$, $-0.164$ and $-0.222$ at four, eight and twelve years. This window is a single cross-section of counties, its first stage is much weaker than in the other periods, with an $F$ statistic of 17, and new unionization was much more variable, with a standard deviation of 2.55. For the windows that end between 1974 and 1982 the estimates are close to zero and precise. For the windows that end between 1986 and 2022 the estimates are again negative, at $-0.081$, $-0.117$ and $-0.101$. I reject that the coefficients are equal across the three periods at every horizon, at the 5 percent level. Enrollment at the other colleges does not respond in any period.

In Panel B, I split the change over eight years into the change at the colleges that already existed at the end of the window and the contribution of the colleges that enter the county's data later. In the window that ends in 1970, the colleges that enter later account for 65 percent of the decline. In Panel C the outcome is the number of community colleges that open in the county. In the same window, one unit of NewU lowers the number of openings over the next eight years by 0.138, against a mean of 0.421, and it does not predict openings before the window. Many counties were deciding whether to open a community college in the late 1960s, and close union wins seem to have delayed or prevented some of these openings. As the results by commuting zone suggest, some students also seem to have enrolled at colleges in nearby counties instead.

In the windows that end between 1986 and 2022, the colleges that enter later account for 3 percent of the decline, and the effect on openings is close to zero. By then most counties that would have a community college already had one, and almost all of the decline is at the existing colleges, with no change in enrollment in the other counties of the commuting zone. In

the windows that end between 1974 and 1982, the effect on openings is smaller, at $-0.032$, and the enrollment estimates are precise and close to zero. I do not have an explanation that I can test for this absence of an effect.

### 4.5 Payroll, employment and establishments

So far I have taken from the literature the idea that unionization raises the return to work without a degree. In this section I look at it with County Business Patterns, which report payroll, employment and the number of establishments in each county. Table 5 uses the main specification with these outcomes, in Panel A for all the county-windows with data and in Panel B for the county-windows of the community-college regression. Payroll and establishments come from the Census Bureau's county files, so their changes start in 1986.

Payroll per worker rises slightly after the window. Four years after it, the estimate for all county-windows is 0.010, significant at the 5 percent level, which is an increase of 0.51 percent per standard deviation of new unionization. After eight years the estimate is similar, at 0.012, but not significant, and the estimate before the window, from $-8$ to $-4$, is 0.009 and not significant either. In the community-college sample the estimates are slightly larger and less precise.

Employment and the number of establishments do not change after the window. The estimates for employment are 0.000 and 0.004 at four and eight years, and those for establishments are 0.003 at both spans. Before the window, from $-8$ to $-4$, the estimate for employment is positive and significant, at 0.031. The county files only allow this change for the windows that end in 1994 or later, since it has to start in 1986. The harmonized series that scales NewU covers every window from 1970, and with it the estimate is 0.005 and not significant. Given that this series also gives 0.031 in the windows that end between 1994 and 2022, and 0.001 in those that end between 1970 and 1990, the difference comes from the years and not from the series. I do not have an explanation for the positive estimate before the window in the later windows, so I read the employment results for these years with caution.

In my view, these results are consistent with union jobs becoming easier to get for young people without a degree. One standard deviation of NewU adds 0.83 new union members per 100 private workers, and even with a union premium of 20 percent, these jobs alone would raise average pay in the county by much less than the 0.51 percent I estimate, so some other employers may also raise pay (Holmes, 2006), or the composition of jobs in the county may change. With employment flat, the decline in enrollment does not seem to come from a local boom or bust.

Given that DiNardo and Lee (2004) find no effect on wages at the establishment level, it may seem surprising to find an effect on schooling at the county level. Even so, I think the two results are consistent with each other. A close union win changes the pay of the workers of one employer, and these workers are a small part of the county's workforce, so the average

**Table 5:** Payroll, employment and establishments.

| | Before the window | After the window | |
|---|---|---|---|
| | $-8$ to $-4$ | 0 to 4 | 0 to 8 |
| *Panel A. All county-windows* | | | |
| Payroll per worker | 0.009 | 0.010** | 0.012 |
| | (0.007) | (0.005) | (0.008) |
| Employment | 0.031*** | 0.000 | 0.004 |
| | (0.012) | (0.008) | (0.014) |
| Establishments | 0.007 | 0.003 | 0.003 |
| | (0.007) | (0.006) | (0.010) |
| Employment, all windows from 1970 | 0.005 | 0.004 | 0.006 |
| | (0.004) | (0.004) | (0.006) |
| County-windows (payroll) | 11,397 | 12,670 | 11,264 |
| Windows ending (payroll) | 1994–2022 | 1986–2018 | 1986–2014 |
| County-windows (full grid) | 19,290 | 18,105 | 16,705 |
| *Panel B. County-windows of the CC regression at 0 to 4* | | | |
| Payroll per worker | 0.009 | 0.014* | 0.018 |
| | (0.010) | (0.008) | (0.013) |
| Employment | 0.045** | $-0.007$ | $-0.004$ |
| | (0.018) | (0.014) | (0.023) |
| Establishments | 0.016 | 0.003 | 0.003 |
| | (0.011) | (0.010) | (0.016) |
| Employment, all windows from 1970 | 0.008 | 0.009 | 0.015 |
| | (0.006) | (0.006) | (0.010) |
| County-windows (payroll) | 5,737 | 7,307 | 6,576 |
| Windows ending (payroll) | 1994–2018 | 1986–2018 | 1986–2014 |
| County-windows (full grid) | 9,984 | 10,046 | 9,317 |

*Notes.* Main specification of Table 3 with County Business Patterns outcomes. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: $t-8$ to $t-4$, before the window, and $t$ to $t+4$ and $t+8$, after it. Payroll per worker is annual payroll over mid-March employment. Payroll, employment and establishments come from the Census county files for 1986, 1990, …, 2022, so their changes start in 1986 and their change from $t-8$ to $t-4$ uses windows ending 1994–2022. The row for all windows from 1970 uses the CBP employment series that scales NewU, which reaches back to 1962 and gives the same changes as the county files on every county-window where both exist. The two employment rows therefore differ at $-8$ to $-4$ only in their windows. With the full series the change from $t-8$ to $t-4$ gives 0.031 (0.012) on windows ending 1994–2022 and 0.001 (0.004) on windows ending 1970–1990 in Panel A, and 0.045 (0.018) and 0.001 (0.006) in Panel B. Panel B keeps the county-windows of the community-college regression from $t$ to $t+4$. First-stage $F$ statistics range from 131 to 561. Standard errors clustered by county are in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

wage in the county can barely move even when the union raises pay at the plant. Besides, Frandsen (2021) finds effects at the firm level with better data, so a null wage effect is no longer the consensus. Finally, young people may be reacting to how easy it looks to get a well-paid job without a degree, and a wave of union wins in the county could make these jobs look easier to get even if measured wages change little.

### 4.6 Bachelor's attainment

The last outcome is the share of residents aged 25 and older with a bachelor's degree, over the decades that end in 1980, 1990 and 2000 (Section 2.5). Take the change from 1980 to 1990. People who turned 25 between 1981 and 1990 were 18 sometime between 1974 and 1983, which is when they decided whether to enroll. I measure new unionization in three ten-year windows, from 1981 to 1990 (L0), from 1976 to 1985 (W) and from 1971 to 1980 (L10).

In Panel A of Appendix Table A12 the outcome is the change in the log share, and the estimate for men in L0 is $-0.0277$, significant at the 5 percent level, with a first-stage $F$ statistic of 164. For women it is $-0.0163$ and not significant, and for both sexes it is $-0.0222$, also significant at the 5 percent level. None of the estimates for W and L10 is significant.

However, the result depends on measuring the outcome in logs. In Panel B the outcome is the change in the share in percentage points, and the estimate for men in the same decade is $-0.09$, which is close to zero and not significant, with estimates that are also close to zero for women and for both sexes.

Even though the cohorts that turn 25 between 1981 and 1990 decided whether to enroll around the years of the window W, from 1976 to 1985, the decline shows up only for the union wins of L0, between 1981 and 1990, when most of these young people were already past the usual age of enrollment. In my view, the estimate of $-0.0277$ for men is then some evidence that new unionization lowers the share of men with a bachelor's degree, even though it is weaker than the evidence for community colleges, since it appears only in logs and only for the union wins of the same decade, and since the census data do not tell me whether a lower share comes from men who left college without a degree or from changes in who lives in the county.

### 4.7 Magnitudes

I describe magnitudes in terms of one standard deviation of the treatment. In the four-year community-college sample, the employment-weighted mean of NewU is 0.42 new union members per 100 private workers and its standard deviation is 0.83. In a county with 50,000 private-sector workers, for example, one standard deviation corresponds to about 415 new union members over the four years of the window, which is a large wave of organizing for a county, given that only 45 percent of the county-windows have a close election at all.

One standard deviation of NewU lowers community-college enrollment by 2.9 percent four

to seven years after the elections, and by 4.2 and 4.7 percent after eight and twelve years. Per unit of NewU, the decline after four years is 3.4 percent. For associate's degrees, one standard deviation lowers the number awarded by 6.7 percent after eight years. In the attainment sample, one population-weighted standard deviation of NewU is 0.88 new union members per 100 residents. For men, the estimate in logs then implies a decline of about 2.4 percent in the share with a bachelor's degree, which was 23.5 percent at the start of the decade, on average.

My estimates are local average treatment effects for the counties where close NLRB elections take place, weighted by private employment, and in these counties union work is probably a closer alternative to college than in the average county. The results of Section 4.3 also mean that the estimates are proportional changes in the counties with the most private-sector workers relative to their community-college students. Added up over counties, the change in the number of students is close to zero, and part of the decline in a county seems to be offset in nearby counties. For this reason, I do not think the estimates can be used to compute how much of the national change in college enrollment comes from the decline of unions, even though, as for the housing booms of Charles et al. (2018), the response is concentrated at two-year colleges.

# 5 Conclusion

This paper studied whether new unionization affects college enrollment in the surrounding labor market, using close NLRB elections held between 1959 and 2024. For enrollment, I put together a panel by county from 1947 to 2023, which I extended to the years before 1968 by digitizing the printed reports of the Office of Education. Once the aggregation controls are included, the characteristics of the county that I observe before the elections explain almost none of the variation in the instrument, even though the elections that unions barely won were held somewhat later and in counties with fewer elections. Before the elections, the estimates are close to zero in the main specification.

As a consequence of a wave of close union wins, community-college enrollment in the county falls, by 2.9 percent per standard deviation of new unionization four to seven years after the elections and by 4.7 percent after twelve years, and associate's degrees fall a few years later. Enrollment at other colleges does not change, and neither does total enrollment at all the colleges of the county. The decline is similar for men and women. However, the decline is proportional and concentrated in counties with many private-sector workers relative to their community-college students, and it does not show up at the level of the commuting zone. It is also concentrated in the window that ends in 1970, where most of it comes from fewer new colleges, and in the years after 1986, where it is at existing colleges. In my view, part of the response is a change in where young people enroll, and the estimates do not imply a large change in the number of students.

Payroll per worker rises by 0.51 percent per standard deviation of new unionization four

years after the window, and employment and the number of establishments do not change. This seems consistent with union jobs becoming more valuable for young people without a degree, through the union premium itself and perhaps through other employers raising pay. I also find some evidence that new unionization lowers the share of men with a bachelor's degree, but I give it less weight because it appears only in logs.

With data on where students come from, it would be possible to separate the young people who do not enroll from those who enroll in another county.

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# A Construction of the college panel

## A.1 The printed reports

In the years after the Second World War, the Office of Education surveyed the institutions of higher education every fall and published the results in a circular, later a report, with the title changing a few times (Table A1). From 1947 the reports print a table with one line per institution, grouped by State, with fall enrollment by sex and, depending on the year, first-time students, veterans, and full- and part-time students. The reports for the falls of 1946 and 1948 have only national totals. I digitized the institution tables of the falls of 1947 and 1949 to 1967, which come from 20 publications, and the table of 1968, which the Office of Education printed from HEGIS. The table of 1968 is not part of the panel, and I use it to check the link between the printed lines and HEGIS.

Each of the 39,970 lines for the 50 States and DC was read at least twice, first by placing the text layer of the HathiTrust scan on the grid of the table, and then by optical character recognition of the image of each cell. For the typewritten and line-printer volumes I also added a third reading, from a classifier of digits that I trained on the digits of the same volume. I accepted a line when the readings agreed and its identities held, as when the total is equal to the sum of men and women, or, from 1963 on, when all students are equal to degree-credit plus other students, and when the readings disagreed the identities usually left only one combination that was consistent. I read the remaining lines by eye from the scan. Figure A1 shows two excerpts of the printed tables, for 1947 and 1967, next to the same lines in the panel.

In every year, the digitized lines of each State add up to the State total printed in the same table, and the States add up to the published national total, except for a few lines that are hidden or faded in the scans. In 1947 a library slip hides some lines of the District of Columbia, with 15,165 students, a few lines are also hidden in the scan of 1953, and a page is faded in 1965. The totals of these lines are known from the published State tables, and with these lines set to the published residual every volume adds up to its printed total, with a difference of 3 students in 1965 (Table A3). In the county series of this paper I leave these lines missing and drop the changes of their counties in those years.

## A.2 Linking and counties

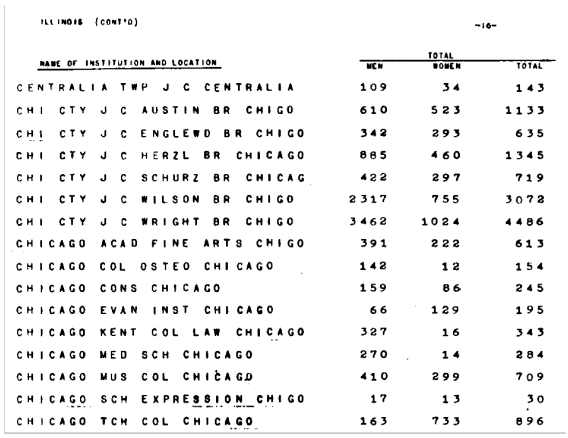
The printed table of 1968, which the Office of Education produced from HEGIS, equals the HEGIS file of 1968 for each of its 2,775 lines. I linked the printed lines of 1967 to those of 1968, and then each year to the next one, backward to 1947. Links are made by name within State, with abbreviations expanded and with more weight on distinctive words, and every link that is not an exact match was reviewed and recorded with a note, for example for renamings, mergers, closures, moves and splits. Each line then takes the HEGIS unit of its institution and, through it, the IPEDS unit and the county. In each year, between 92 and 98 percent of the lines,

**Table A1:** The digitized Office of Education fall enrollment volumes.

| Fall | Publication and table | Concept | Institutions | National total | HathiTrust id |
|---|---|---|---|---|---|
| 1947 | Circular 238 (1947), Table VI | Resident | 1,749 | 2,338,226 | uc1.10053703369 |
| 1948 | Circular 248 (1948) | – | no table | 2,408,249 | osu.32435020204749 |
| 1949 | Circular 264 (1949), listing | Resident | 1,844 | 2,456,841 | umn.31951d03530829u |
| 1950 | Circular 281 (1950), listing | Resident | 1,882 | 2,296,592 | umn.31951d03530821a |
| 1951 | Circular 328 (1951), Table 10 | Resident | 1,854 | 2,116,440 | osu.32435020204152 |
| 1952 | Circular 359 (1952), Table 10 | Resident | 1,900 | 2,148,284 | osu.32435020203907 |
| 1953 | Circular 382 (1954), Table 9 | College grade | 1,863 | 2,250,701 | osu.32435020203683 |
| 1954 | Circular 419 (1955), Table 9 | College grade | 1,846 | 2,499,750 | osu.32435020200127 |
| 1955 | Circular 460 (1956), Table 12 | College grade | 1,847 | 2,720,929 | osu.32435020203261 |
| 1956 | Circular 496 (1957), Table 17 | Degree-credit | 1,838 | 2,946,985 | osu.32435020203055 |
| 1957 | Circular 518 (1958), Table 13 | Degree-credit | 1,876 | 3,068,417 | ufl.31262096120091 |
| 1958 | Circular 544 (1959), Table 1 | Degree-credit | 1,893 | 3,258,556 | ufl.31262083437615<br>umn.31951t00252148p |
| 1959 | Circular 606, OE-54003 (1959), Table 1 | Degree-credit | 1,942 | 3,402,297 | mdp.39015043572299 |
| 1960 | Circular 637, OE-54003-60 (1960), institutional table | Degree-credit | 1,965 | 3,610,007 | ufl.31262083436633 |
| 1961 | Circular 675, OE-54003-61 (1961), listing | Degree-credit | 1,976 | 3,891,230 | umn.31951t00251186m |
| 1962 | Circular 697, OE-54003-62 (1962), listing | Degree-credit | 2,034 | 4,206,672 | ufl.31262083437086 |
| 1963 | Circular 728, OE-54003-63 (1963), Tables 6–7 | All students | 2,122 | 4,800,332 | ufl.31262083436468 |
| 1964 | Circular 762, OE-54003-64 (1964), Table 7 | All students | 2,176 | 5,320,294 | uc1.b4737808 |
| 1965 | Circular 796, OE-54003-65 (1966), Table 9 | All students | 2,246 | 5,967,411 | uc1.10053412250 |
| 1966 | OE-54003-66 (1967), Table 15 | All students | 2,470 | 6,438,477 | osu.32435051468981 |
| 1967 | OE-54003-67 (1967), Table 28 | All students | 2,647 | 6,963,687 | mdp.39015035873556 |
| 1968 | OE-54003-68, Part B (1969), Table 1 | All students (HEGIS) | 2,775 | – | osu.32435026670810 |

*Notes.* Falls 1947 and 1949–1967 were digitized from the printed institutional tables of the U.S. Office of Education (20 publications, scanned by HathiTrust). Titles are *Fall Enrollment in Higher Educational Institutions* (1947–1954), *Opening (Fall) Enrollment in Higher Educational Institutions* (1955–1957), *Opening (Fall) Enrollment in Higher Education: Institutional Data* (1958–1962) and *Opening Fall Enrollment in Higher Education* (1963–1968). The year in parentheses is the year of publication. Institutions are the printed lines in the 50 States and DC, including separately printed branch campuses and the system lines that a volume counts in its State totals (University of California 1947–1957, Minnesota 1953–1957, Wisconsin 1956). The national total is the one printed in the volume. The 1948 circular is a four-page summary without an institution table. The 1968 Part B table, printed from HEGIS, was digitized to check the 1967 to 1968 link and is not part of the panel. The 1958 table was read from two scans of the same print.

A. Fall 1947: Circular 238, Table VI (Illinois), and the same lines in the panel

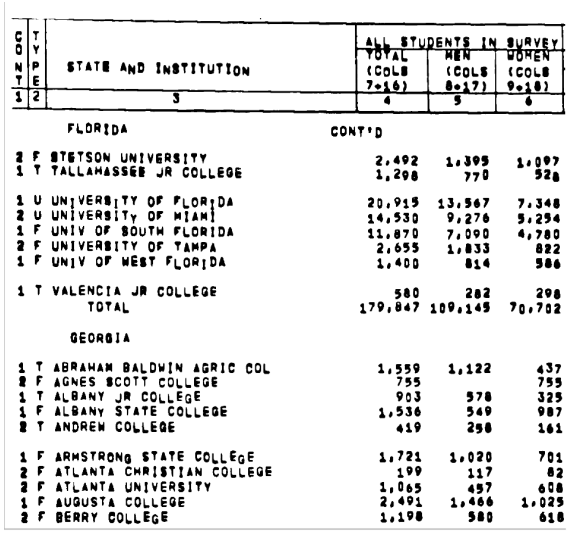


ILLINOIS (CONT'D) -16-

| NAME OF INSTITUTION AND LOCATION | TOTAL MEN | TOTAL WOMEN | TOTAL |
|---|---|---|---|
| CENTRALIA TWP J C CENTRALIA | 109 | 34 | 143 |
| CHI CTY J C AUSTIN BR CHIGO | 610 | 523 | 1133 |
| CHI CTY J C ENGLEWD BR CHIGO | 342 | 293 | 635 |
| CHI CTY J C HERZL BR CHICAGO | 885 | 460 | 1345 |
| CHI CTY J C SCHURZ BR CHICAG | 422 | 297 | 719 |
| CHI CTY J C WILSON BR CHIGO | 2317 | 755 | 3072 |
| CHI CTY J C WRIGHT BR CHIGO | 3462 | 1024 | 4486 |
| CHICAGO ACAD FINE ARTS CHIGO | 391 | 222 | 613 |
| CHICAGO COL OSTEO CHICAGO | 142 | 12 | 154 |
| CHICAGO CONS CHICAGO | 159 | 86 | 245 |
| CHICAGO EVAN INST CHICAGO | 66 | 129 | 195 |
| CHICAGO KENT COL LAW CHICAGO | 327 | 16 | 343 |
| CHICAGO MED SCH CHICAGO | 270 | 14 | 284 |
| CHICAGO MUS COL CHICAGO | 410 | 299 | 709 |
| CHICAGO SCH EXPRESSION CHIGO | 17 | 13 | 30 |
| CHICAGO TCH COL CHICAGO | 163 | 733 | 896 |

| Name in the panel | City | County | Type | Students |
|---|---|---|---|---|
| CENTRALIA TWP J C CENTRA. | Centralia | 17027 | Pub. 2-yr | 143 |
| CHI CTY J C AUSTIN BR CH. | Chicago | 17031 | Pub. 2-yr | 1,133 |
| CHI CTY J C ENGLEWD BR C. | Chicago | 17031 | Pub. 2-yr | 635 |
| CHI CTY J CC HERZL BR CH. | Chicago | 17031 | Pub. 2-yr | 1,345 |
| CHI CTY JC SCHURZ BR CHI. | Chicago | 17031 | Pub. 2-yr | 719 |
| CHI CTY J C WILSON BR CH. | Chicago | 17031 | Pub. 2-yr | 3,072 |
| CHI CTY J C WRIGHT BR CH. | Chicago | 17031 | Pub. 2-yr | 4,486 |
| CHICAGO ACAD FINE ARTS C. | Chicago | 17031 | Priv. 2-yr | 613 |
| CHICAGO COL OSTEO CHICAGO | Chicago | 17043 | Priv. 4-yr | 154 |
| CHICAGO CONS CHICAGO | Chicago | 17031 | Priv. 4-yr | 245 |
| CHICAGO EVAN INST CHICAGO | University P | 19123 | Priv. 4-yr | 195 |
| CHICAGO KENT COL LAW CHI. | | 17031 | Priv. 4-yr | 343 |
| CHICAGO MED SCH CHICAGO | Chicago | 17097 | Priv. 4-yr | 284 |
| CHICAGO MUS COL CHICA GO | Chicago | 17031 | Priv. 4-yr | 709 |
| CHICAGO SCH EXPRESSION C. | | | Priv. 4-yr | 30 |
| CHICAGO TCH COL CHICAGO | Chicago | 17031 | Pub. 4-yr | 896 |

B. Fall 1967: OE-54003-67, Table 28 (Florida and Georgia), and the same lines in the panel



| CONT | TYPE | STATE AND INSTITUTION | ALL STUDENTS IN SURVEY TOTAL (COLS 7+16) | MEN (COLS 8+17) | WOMEN (COLS 9+18) |
|---|---|---|---|---|---|
| 1 | 2 | 3 | 4 | 5 | 6 |
| | | FLORIDA CONT'D | | | |
| 2 | F | STETSON UNIVERSITY | 2,492 | 1,395 | 1,097 |
| 1 | T | TALLAHASSEE JR COLLEGE | 1,298 | 770 | 528 |
| 1 | U | UNIVERSITY OF FLORIDA | 20,915 | 13,567 | 7,348 |
| 2 | U | UNIVERSITY OF MIAMI | 14,530 | 9,276 | 5,254 |
| 1 | F | UNIV OF SOUTH FLORIDA | 11,870 | 7,090 | 4,780 |
| 2 | F | UNIVERSITY OF TAMPA | 2,655 | 1,833 | 822 |
| 1 | F | UNIV OF WEST FLORIDA | 1,400 | 814 | 586 |
| 1 | T | VALENCIA JR COLLEGE | 580 | 282 | 298 |
| | | TOTAL | 179,847 | 109,145 | 70,702 |
| | | GEORGIA | | | |
| 1 | T | ABRAHAM BALDWIN AGRIC COL | 1,559 | 1,122 | 437 |
| 2 | F | AGNES SCOTT COLLEGE | 755 | | 755 |
| 1 | T | ALBANY JR COLLEGE | 903 | 578 | 325 |
| 1 | F | ALBANY STATE COLLEGE | 1,536 | 549 | 987 |
| 2 | T | ANDREW COLLEGE | 419 | 298 | 121 |
| 1 | F | ARMSTRONG STATE COLLEGE | 1,721 | 1,020 | 701 |
| 2 | F | ATLANTA CHRISTIAN COLLEGE | 199 | 117 | 82 |
| 2 | F | ATLANTA UNIVERSITY | 1,065 | 457 | 608 |
| 1 | F | AUGUSTA COLLEGE | 2,491 | 1,466 | 1,025 |
| 2 | F | BERRY COLLEGE | 1,198 | 580 | 618 |

| Name in the panel | City | County | Type | Students |
|---|---|---|---|---|
| STETSON UNIVERSITY | Deland | 12127 | Priv. 4-yr | 2,492 |
| TALLAHASSEE JR COLLEGE | Tallahassee | 12073 | Pub. 2-yr | 1,298 |
| UNIVERSITY OF FLORIDA | Gainesville | 12001 | Pub. 4-yr | 20,915 |
| UNIVERSITY OF MI AM | Coral Gables | 12025 | Priv. 4-yr | 14,530 |
| UNIV OF SOUTH FLORIDA | Tampa | 12057 | Pub. 4-yr | 11,870 |
| UNIVERSITY OF TAMPA | Tampa | 12057 | Priv. 4-yr | 2,655 |
| UNIV OF WEST FLORIDA | Pensacola | 12033 | Pub. 4-yr | 1,400 |
| VALENCIA JR COLLEGE | Orlando | 12095 | Pub. 2-yr | 580 |
| ABRAHAM BALDWIN AGRIC COL | Tifton | 13277 | Pub. 2-yr | 1,559 |
| AGNES SCOTT COLLEGE | Decatur | 13089 | Priv. 4-yr | 755 |
| ALBANY JR COLLEGE | Albany | 13095 | Pub. 2-yr | 903 |
| ALBANY STATE COLLEGE | Albany | 13095 | Pub. 4-yr | 1,536 |
| ANDREW COLLEGE | Cuthbert | 13243 | Priv. 2-yr | 419 |
| ARMSTRONG STATE COLLEGE | Savannah | 13051 | Pub. 4-yr | 1,721 |
| ATLANTA CHRISTIAN COLLEGE | East Point | 13285 | Priv. 4-yr | 199 |
| ATLANTA UNIVERSITY | Atlanta | 13121 | Priv. 4-yr | 1,065 |
| AUGUSTA COLLEGE | Augusta | 13245 | Pub. 4-yr | 2,491 |
| BERRY COLLEGE | Rome | 13115 | Priv. 4-yr | 1,198 |

**Figure A1:** Scan excerpts and the digitized rows. Left: the printed tables as scanned by HathiTrust (Circular 238, Table VI, printed page 16, and OE-54003-67, Table 28, printed page 66). Right: the same lines in the panel with the city, county, control and level assigned to them and the enrollment counted.

with at least 96.8 percent of the students, are linked.

Since the printed tables give no county, a linked line takes the county of its HEGIS unit. Public two-year colleges that closed before 1968, and so have no HEGIS unit, are placed in a county from the city printed for them. A college that moved before 1968 is therefore placed where it was later. From 1968 on, each institution is placed in the county where it is most often located in the IPEDS files from 2009 on, and, for institutions that are not in these files, in its most frequent county over all years, with ties going to the latest year. When the move to another county is real, as when Kirtland Community College moved from Roscommon to Grayling in 2019, I keep the move and drop the change of the year in which it happens. I also drop the change of the year in which a community college is absorbed by an institution that is not a community college, since the absorbed students would otherwise look like a decline. In 1986, some IPEDS records are imputations, and I interpolate the enrollment of the institutions in the HEGIS universe between 1985 and 1987.

### A.3 Concepts and chaining

Table A2 lists the falls in which what the Office of Education counted as a student changes. Between 1952 and 1953 the reports give no overlap, since extension students enter the count in 1953. For this change I leave out 36 of the 254 community colleges, those whose own change lies outside the range from the 5th to the 95th percentile of the changes of 1949 to 1952. When the Office of Education restated 1955 on the degree-credit basis, it dropped 19 two-year technical institutes, and I leave them out of every fall up to 1955. In New York, the degree-credit count of the locally sponsored junior colleges goes from 6,022 students in 1959 to 2,052 in 1960, and I drop the change of the New York counties in that year.

### A.4 Validation

In Table A3 I compare the national totals of the panel with those printed by the Office of Education and with Table 303.10 of the Digest of Education Statistics. Before 1957 the Digest reports the totals printed in the volumes, and the panel is within 1.6 percent of them. For 1957, 1959 and 1961 the Digest does not report the degree-credit students of the printed tables but a broader count, which is 6 to 9 percent larger, and in these years the panel is 7.2 to 8.6 percent below the Digest. Between 1963 and 2022 the panel is never more than 1.4 percent away from the Digest, and from 2001 on never more than 0.3 percent. In the HEGIS years the panel is 0.3 percent below the Digest on average, mostly because it leaves out the service academies.

**Table A2:** Concept seams in the college panel and how changes are chained.

| Step | What changes | How the step is computed |
|---|---|---|
| 1947 to 1949 | No institution table for 1948. | Two-year change 1947 to 1949, same resident concept. |
| 1952 to 1953 | Extension students counted from 1953 (resident to college grade). No overlap year. | Stable-college bridge: community colleges whose 1952 to 1953 change lies outside the 5th to 95th percentile range of college-level changes in 1949–1952 (36 of 254) are left out of this step. |
| 1953 to 1955 | Junior colleges include two-year technical institutes. OE's later restatement drops them. | The 19 institutions dropped by the restatement are left out of every fall up to 1955. |
| 1955 to 1956 | Degree-credit concept from 1956. OE restated 1955 on the new basis. | The step uses the restated 1955 basis. |
| 1959 to 1960 | The degree-credit count of New York's locally sponsored junior colleges breaks (6,022 students in 1959, 2,052 in 1960). | New York counties are masked on this step. |
| 1962 to 1963 | All students from 1963. The 1963 volume prints degree-credit and other students separately. | The 1962 to 1963 step uses degree-credit students. Later steps use all students. |
| 1967 to 1968 | Printed volumes to HEGIS. | The printed 1968 table (all students) equals HEGIS 1968 total enrollment for each of its 2,775 lines, and the step from 1967 to 1968 links the printed lines of 1967 to the HEGIS units. |
| 1985 to 1986 | HEGIS to IPEDS, which adds schools that award no degree and schools of less than two years. | Only the institutions in the HEGIS universe in both years enter this step. |

*Notes.* The community-college enrollment of each county is chained from its changes between consecutive falls, and levels are never compared across concepts, with the set of colleges of each step fixed at its start so that both falls count the same colleges.

# B Election data

## B.1 Sources

The election panel joins four sources. The file for 1959 to 1979 was compiled from the records of the AFL-CIO and distributed by J.P. Ferguson. It has very few elections before 1961, and it has no official NLRB case number, so I build an identifier for each case. The file of Holmes (2006) covers 1980 to 1999. It is organized by the fiscal year in which a case closed and ends with the fiscal year 1999, so it misses the elections of 1999 whose cases closed later, and part of those of 1998. I add the elections of 1998 in the Ferguson file that are not in the Holmes file, and the elections of 1998 and 1999 in CATS that are in neither, after dropping duplicates by case number and by the tally. For 2000 to 2008 I use the NLRB's CATS records, with the initial tally of each election, and from 2009 on the case pages of the NLRB website.

**Table A3:** National fall enrollment in the panel and in published sources.

| Fall | Source | Concept | OE published | Digest 303.10 | Panel total | Panel vs. Digest (%) |
|---|---|---|---|---|---|---|
| 1947 | OE | Resident | 2,338,226 | 2,338,226 | 2,332,814 | −0.2 |
| 1950 | OE | Resident | 2,296,592 | 2,281,298 | 2,278,925 | −0.1 |
| 1953 | OE | College grade | 2,250,701 | 2,231,054 | 2,229,961 | 0.0 |
| 1955 | OE | College grade | 2,720,929 | 2,653,034 | 2,694,359 | 1.6 |
| 1956 | OE | Degree-credit | 2,946,985 | 2,918,212 | 2,917,828 | 0.0 |
| 1957 | OE | Degree-credit | 3,068,417 | 3,323,783 | 3,037,464 | −8.6 |
| 1959 | OE | Degree-credit | 3,402,297 | 3,639,847 | 3,364,945 | −7.6 |
| 1961 | OE | Degree-credit | 3,891,230 | 4,145,065 | 3,847,253 | −7.2 |
| 1963 | OE | All students | 4,800,332 | 4,779,609 | 4,740,892 | −0.8 |
| 1965 | OE | All students | 5,967,411 | 5,920,864 | 5,850,908 | −1.2 |
| 1967 | OE | All students | 6,963,687 | 6,911,748 | 6,857,185 | −0.8 |
| 1968 | HEGIS | All students | | 7,513,091 | 7,459,518 | −0.7 |
| 1970 | HEGIS | All students | | 8,580,887 | 8,552,287 | −0.3 |
| 1975 | HEGIS | All students | | 11,184,859 | 11,150,024 | −0.3 |
| 1980 | HEGIS | All students | | 12,096,895 | 12,049,669 | −0.4 |
| 1985 | HEGIS | All students | | 12,247,055 | 12,194,584 | −0.4 |
| 1986 | IPEDS | All students | | 12,503,511 | 12,383,781 | −1.0 |
| 1990 | IPEDS | All students | | 13,818,637 | 13,915,813 | 0.7 |
| 2000 | IPEDS | All students | | 15,312,289 | 15,317,312 | 0.0 |
| 2010 | IPEDS | All students | | 21,019,438 | 21,035,697 | 0.1 |
| 2020 | IPEDS | All students | | 19,027,410 | 19,058,590 | 0.2 |
| 2022 | IPEDS | All students | | 18,580,026 | 18,627,326 | 0.3 |

*Notes.* National totals, falls. OE published is the national total printed in each Office of Education volume. The panel total is the total of Figure 3: institutions in the 50 States and DC, without the service academies and the Community College of the Air Force, and from 1986 the IPEDS institutions that award an associate's or higher degree. The county series used in the estimates has a different universe (Section 2.2). In every volume the digitized lines, with the few hidden or faded lines imputed to the published residual, add up to the printed total (a difference of 3 students in 1965). Digest is Table 303.10 of the Digest of Education Statistics. Its totals for 1957, 1959 and 1961 are 6 to 9 percent above the degree-credit totals printed by OE for those falls. The last column is the panel total relative to the Digest. From 1963 to 2022 the largest absolute difference is 1.4 percent and from 2001 to 2022 it is 0.3 percent.

## B.2 Certification elections

All four sources identify the type of case, either in a type variable or in the case number. I keep the certification elections, in which a union petitions to represent workers who have no union (RC cases). I drop the decertification elections (RD cases), in which workers vote on removing a union, and the elections petitioned by employers (RM cases). These two types appear only in the files for 1959 to 1999.

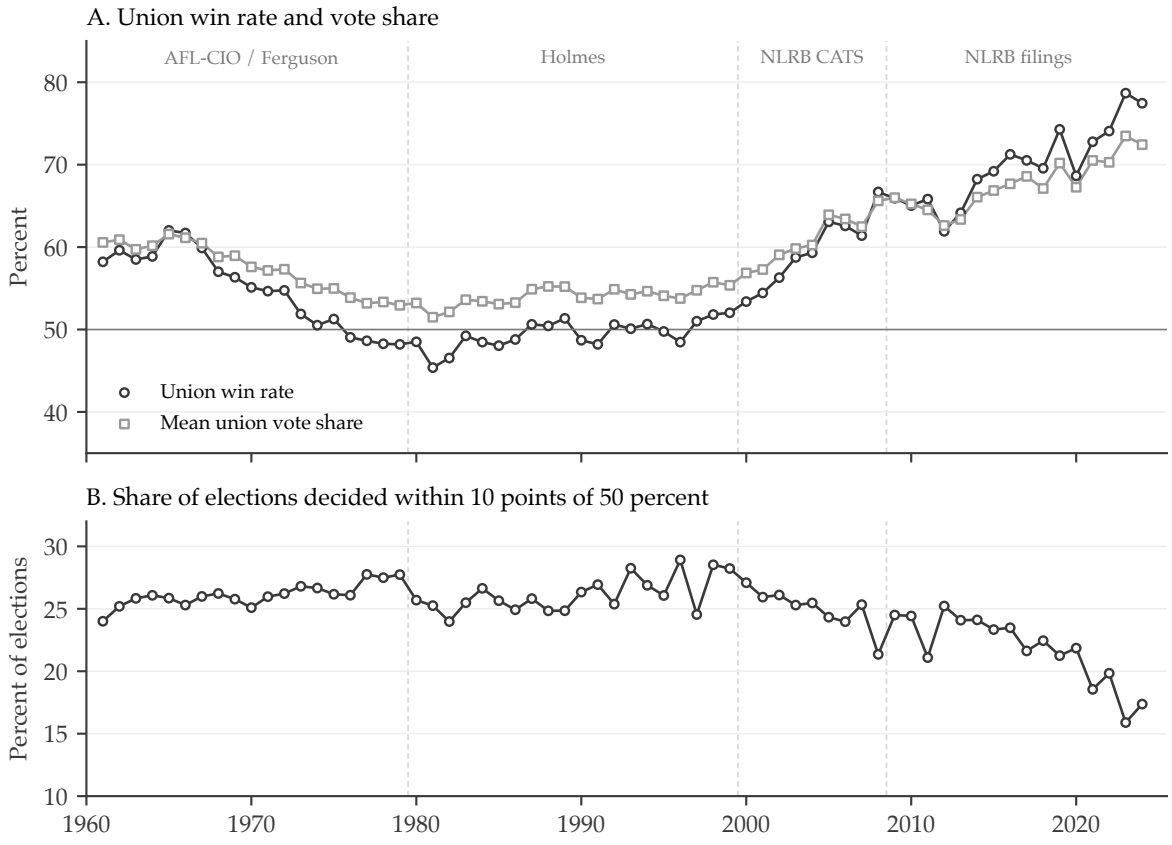



**Figure A2:** Union win rate, vote share and close elections by calendar year, 1961–2024. Panel A shows the share of certification (RC) elections that the union won and the mean union share of valid votes. Panel B shows the share of elections decided within 10 points of 50 percent. Dashed lines mark the changes of source.

## B.3 Published counts

The NLRB published, every fiscal year, the number of certification elections it held, in its annual reports and, from 2011, in its election reports. Figure 1 compares these counts with the panel. The two series follow each other closely, including the fall of the early 1980s. Before 1980 the panel is a few percent below the published counts, and the first year, 1961, covers only part of the year. After 2011 the panel is above them, and in my view this comes mostly from the way the reports count elections, and partly from the timing of each report. In Figure A2, unions won 62 percent of the elections in 1965, 49 percent in 1980 and 69 percent in 2020. Over the same years the share of elections decided within 10 points of 50 percent changed less, with 26 percent in 1965 and in 1980, 27 percent in 2000 and 22 percent in 2020.

### B.4 Elections by county and decade

Figures A3 and A4 show the maps of Figure 2 separately for each decade, with the same shades in every panel. The number of counties with at least one election falls from 2,472 between 1970 and 1979 to 1,196 between 2010 and 2019. Over the same years, the number of counties with a close election that enters the instrument falls from 1,601 to 485. The decline is smaller in the Northeast, where 82 percent of the counties still had an election between 2010 and 2019, against 30 to 45 percent of the counties in the other regions.

## C Additional results

The first three tables complement the balance tests of Section 3.4. In Table A4 I repeat the regressions of Table 2 with 17 and 19 covariates, adding the growth of community-college enrollment before the window, the number of public two-year colleges, the manufacturing share of employment, the share Black and the share of adults with a bachelor's degree, and in the second set the poverty and unemployment rates, and with the three aggregation controls the joint test does not reject with either set. Table A5 compares the 34,599 close elections that enter the instrument on the two sides of the threshold, and Table A6 uses each of 22 covariates measured before the window as the outcome of the main IV regression, with joint tests by group of covariates and for all of them.

Table A7 reports the first stage of the regressions of Table 3 for each span, separately for the samples of all colleges, community colleges and other colleges. Table A8 and Figure A5 report the estimates for enrollment and for associate's degrees by sex. Table A9 reports the lower-level estimates on the stacked close elections, with window fixed effects for the three rows of Table 3, and without fixed effects for community colleges, which corresponds to the RD plots of Figure 8.

Table A10 reports the robustness checks of Section 4.3, Table A11 the estimates by period of Section 4.4, and Table A12 the estimates for bachelor's attainment of Section 4.6.

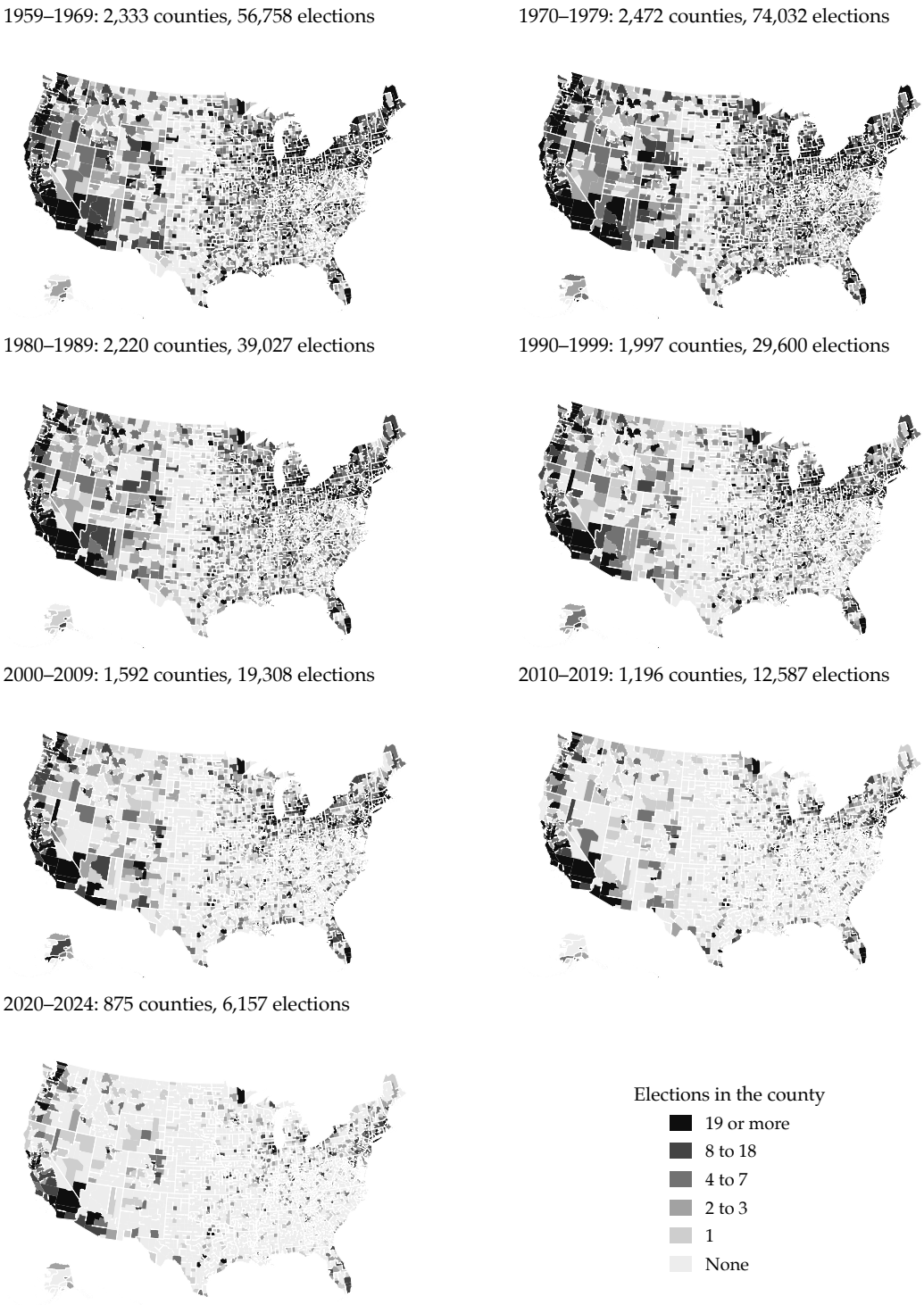



**Figure A3:** NLRB certification elections by county and decade. Each panel counts the elections located in the county in the years shown, with the same shades in every panel.

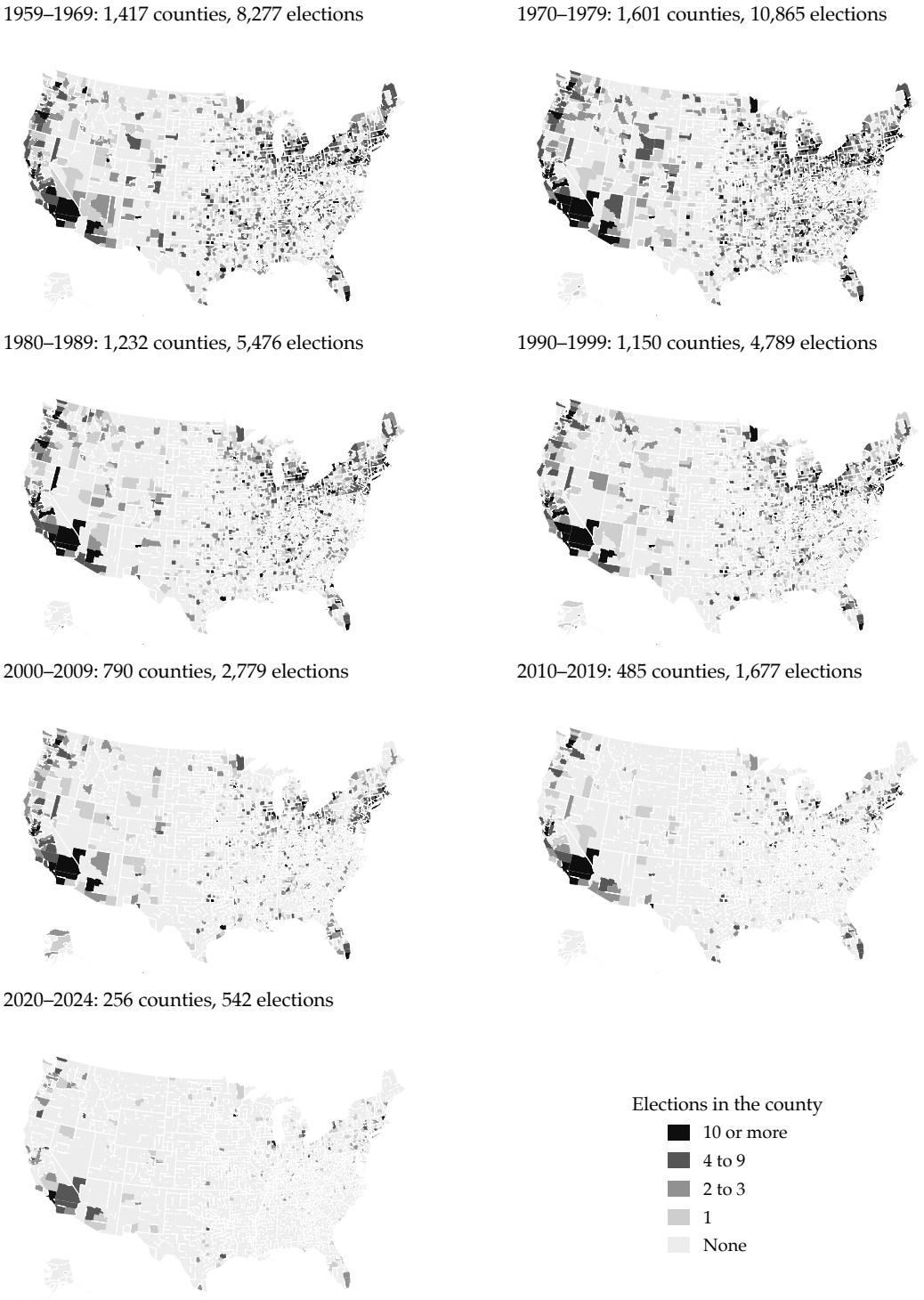



**Figure A4:** Close elections in the instrument by county and decade. Each panel counts the close elections that enter the instrument in the years shown, with the same shades in every panel.

**Table A4:** Balance of the instrument, covariate sets B and C.

| | NewU | $Z$ | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A. Set B (17 covariates)* | | | | |
| Any CC enrollment at $t-4$ | 0.0009** (0.0004) | 0.0003** (0.0001) | 0.0001 (0.0001) | 0.0000 (0.0000) |
| Public two-year college in 1967 | 0.0136* (0.0078) | 0.0049* (0.0026) | 0.0017 (0.0020) | 0.0009 (0.0008) |
| log private employment at $t-4$ | −0.3792*** (0.1036) | −0.1124*** (0.0170) | −0.0278 (0.0185) | 0.0002 (0.0069) |
| Employment growth, $t-8$ to $t-4$ | −0.0211*** (0.0079) | −0.0036 (0.0045) | −0.0013 (0.0030) | 0.0024 (0.0015) |
| log population (last census) | 0.1893** (0.0955) | 0.0415*** (0.0157) | 0.0229** (0.0116) | 0.0025 (0.0047) |
| Population growth, previous decade | −0.0047 (0.0094) | 0.0026 (0.0038) | −0.0018 (0.0031) | −0.0019 (0.0015) |
| NewU, previous window | 0.0228*** (0.0085) | −0.0004 (0.0041) | 0.0070** (0.0034) | 0.0018 (0.0013) |
| $S$, previous window | 0.0000 (0.0073) | 0.0116** (0.0056) | −0.0094 (0.0064) | −0.0033 (0.0023) |
| log(1 + elections), previous window | 0.1143*** (0.0185) | 0.0251*** (0.0056) | 0.0024 (0.0054) | −0.0028 (0.0033) |
| log elections in the county, 1959–2024 | 0.0925*** (0.0284) | 0.0439*** (0.0085) | 0.0033 (0.0104) | −0.0015 (0.0034) |
| South | −0.0310*** (0.0109) | −0.0044 (0.0030) | −0.0033 (0.0025) | −0.0008 (0.0010) |
| CC enrollment growth, $t-8$ to $t-4$ | −0.0091 (0.0072) | −0.0001 (0.0021) | −0.0024 (0.0019) | 0.0010 (0.0009) |
| CC enrollment growth, $t-12$ to $t-8$ | 0.0034 (0.0089) | −0.0017 (0.0030) | −0.0019 (0.0030) | 0.0000 (0.0011) |
| Public two-year colleges at $t-4$ | 0.0169 (0.0163) | 0.0020 (0.0036) | 0.0009 (0.0024) | 0.0009 (0.0008) |
| Manufacturing share of employment | −0.0115 (0.0110) | 0.0056 (0.0041) | −0.0056 (0.0037) | −0.0007 (0.0014) |
| Share Black | 0.0279*** (0.0106) | 0.0022 (0.0032) | 0.0008 (0.0022) | −0.0004 (0.0010) |
| Share of adults 25+ with a BA | 0.0417*** (0.0108) | 0.0067 (0.0044) | 0.0023 (0.0033) | 0.0005 (0.0013) |
| Covariates significant at 5 percent | 10 | 6 | 2 | 0 |
| Partial $R^2$ | 0.109 | 0.029 | 0.008 | 0.003 |
| Partial $F$-statistic | 35.4 | 21.3 | 3.5 | 1.2 |
| Partial $F$-test, $p$-value | $< 0.001$ | $< 0.001$ | $< 0.001$ | 0.251 |
| County-windows | 6,970 | 6,970 | 6,970 | 6,970 |
| Windows ending | 1982–2018 | 1982–2018 | 1982–2018 | 1982–2018 |
| *Panel B. Set C (19 covariates)* | | | | |
| Any CC enrollment at $t-4$ | 0.0014*** (0.0002) | 0.0005*** (0.0001) | 0.0000 (0.0001) | 0.0000 (0.0000) |
| Public two-year college in 1967 | 0.0062 (0.0079) | 0.0036 (0.0031) | 0.0011 (0.0024) | 0.0009 (0.0010) |
| log private employment at $t-4$ | −0.4423*** (0.1003) | −0.1296*** (0.0194) | −0.0409** (0.0196) | −0.0030 (0.0069) |
| Employment growth, $t-8$ to $t-4$ | −0.0235*** (0.0089) | −0.0036 (0.0053) | −0.0009 (0.0034) | 0.0029* (0.0017) |
| log population (last census) | 0.2352*** (0.0852) | 0.0479*** (0.0168) | 0.0375*** (0.0131) | 0.0078 (0.0059) |
| Population growth, previous decade | −0.0039 (0.0118) | 0.0019 (0.0045) | −0.0038 (0.0037) | −0.0032* (0.0018) |
| NewU, previous window | 0.0237*** (0.0091) | −0.0023 (0.0051) | 0.0088** (0.0042) | 0.0017 (0.0015) |
| $S$, previous window | −0.0033 (0.0082) | 0.0125* (0.0066) | −0.0107 (0.0074) | −0.0035 (0.0027) |
| log(1 + elections), previous window | 0.0996*** (0.0198) | 0.0284*** (0.0077) | 0.0007 (0.0065) | −0.0021 (0.0042) |
| log elections in the county, 1959–2024 | 0.1175*** (0.0346) | 0.0491*** (0.0110) | 0.0044 (0.0126) | −0.0033 (0.0040) |
| South | −0.0266* (0.0141) | −0.0017 (0.0035) | −0.0027 (0.0031) | −0.0001 (0.0011) |
| CC enrollment growth, $t-8$ to $t-4$ | −0.0166** (0.0080) | −0.0005 (0.0026) | −0.0020 (0.0024) | 0.0017 (0.0011) |
| CC enrollment growth, $t-12$ to $t-8$ | 0.0029 (0.0102) | −0.0025 (0.0036) | −0.0027 (0.0038) | 0.0001 (0.0013) |
| Public two-year colleges at $t-4$ | 0.0203 (0.0233) | 0.0006 (0.0046) | 0.0002 (0.0036) | −0.0003 (0.0014) |
| Manufacturing share of employment | −0.0039 (0.0126) | 0.0099* (0.0054) | −0.0036 (0.0045) | 0.0003 (0.0018) |
| Share Black | 0.0039 (0.0139) | −0.0045 (0.0039) | −0.0022 (0.0029) | −0.0021 (0.0015) |
| Share of adults 25+ with a BA | 0.0493*** (0.0137) | 0.0135** (0.0060) | 0.0067 (0.0042) | 0.0026 (0.0017) |
| Share below the poverty line | 0.0324* (0.0195) | 0.0098 (0.0070) | 0.0063 (0.0051) | 0.0019 (0.0020) |
| Unemployment rate | 0.0158 (0.0147) | 0.0033 (0.0060) | −0.0038 (0.0050) | 0.0005 (0.0020) |
| Covariates significant at 5 percent | 9 | 6 | 3 | 0 |
| Partial $R^2$ | 0.116 | 0.029 | 0.008 | 0.004 |
| Partial $F$-statistic | 52.7 | 22.0 | 2.9 | 1.1 |
| Partial $F$-test, $p$-value | $< 0.001$ | $< 0.001$ | $< 0.001$ | 0.396 |
| County-windows | 5,556 | 5,556 | 5,556 | 5,556 |
| Windows ending | 1982–2010 | 1982–2010 | 1982–2010 | 1982–2010 |
| Aggregation controls | None | None | $S$ | $S$, $SR$, $SR'$ |

*Notes.* The regressions of Table 2 with the covariates of sets B and C. Each column is a weighted least squares regression of NewU (column 1) or $Z$ (columns 2 to 4) on the standardized covariates, with window fixed effects, weights equal to CBP private employment at the start of the window and the aggregation controls shown in the last row. Each cell reports the coefficient and, in parentheses, its standard error clustered by county, with four decimals because many coefficients in columns 3 and 4 are below 0.001. The sample is the county-windows of the main estimate from $t$ to $t+4$ with every covariate of the set observed. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table A5:** Covariate balance of close ($\pm 10$ point) elections at the threshold.

| | Mean (union lost) | Mean (union won) | RD diff. | SE | $p$ | $N$ |
|---|---|---|---|---|---|---|
| log(eligible voters) | 4.383 | 4.257 | 0.005 | (0.023) | 0.83 | 34,599 |
| Election year | 1981.41 | 1982.19 | 0.79 | (0.41) | 0.06 | 34,599 |
| South (Census region) | 0.234 | 0.240 | 0.010 | (0.012) | 0.41 | 34,599 |
| log(county elections) | 5.248 | 5.266 | −0.099 | (0.049) | 0.04 | 34,599 |
| Manufacturing (SIC 20–39) | 0.605 | 0.574 | −0.024 | (0.013) | 0.07 | 29,327 |
| Trade (SIC 50–59) | 0.126 | 0.123 | −0.004 | (0.009) | 0.61 | 29,327 |
| Services (SIC 70–89) | 0.145 | 0.176 | 0.009 | (0.010) | 0.39 | 29,327 |
| Joint test, first four covariates ($\chi^2_4 = 10.50$) | | | | | 0.03 | 34,599 |
| Joint test, all seven covariates ($\chi^2_7 = 13.86$) | | | | | 0.05 | 29,327 |

*Notes.* Close elections (within 10 points of a 50 percent union vote share) of the instrument sample that are located in a county, the elections that enter the instrument (34,599 elections in 2,300 counties). The RD difference is the coefficient on a union-win indicator in a regression of the covariate on that indicator, the vote margin and their interaction. Standard errors are clustered by county. The county election count is the number of instrument-sample elections located in the county in 1959–2024. Trade is wholesale and retail trade. Industry shares use the SIC code, which only the 1959–1999 sources report. The joint tests regress the union-win indicator on the standardized covariates, the vote margin and the margin on the winning side, and test that the covariate coefficients are zero.

**Table A6:** Pre-window covariates as outcomes of the main IV.

| Covariate | Coefficient | SE | $p$ | Standardized effect | $N$ |
|---|---|---|---|---|---|
| *Enrollment and colleges* | | | | | |
| CC enrollment growth, $t-8$ to $t-4$ | $-0.005$ | (0.026) | 0.858 | $-0.016$ | 8,073 |
| CC enrollment growth, $t-12$ to $t-8$ | $-0.009$ | (0.035) | 0.789 | $-0.031$ | 7,124 |
| Other-college enrollment growth, $t-8$ to $t-4$ | $0.061^{*}$ | (0.036) | 0.095 | 0.205 | 4,722 |
| log CC enrollment at $t-4$ | 0.017 | (0.069) | 0.805 | 0.010 | 9,432 |
| Public two-year colleges at $t-4$ | 0.096 | (0.176) | 0.585 | 0.016 | 9,432 |
| Any CC enrollment at $t-4$ | 0.003 | (0.017) | 0.839 | 0.019 | 10,050 |
| Public two-year college in 1967 | 0.028 | (0.030) | 0.350 | 0.051 | 10,050 |
| *Economy and census* | | | | | |
| log private employment at $t-4$ | $-0.066$ | (0.077) | 0.390 | $-0.039$ | 10,050 |
| Employment growth, $t-8$ to $t-4$ | 0.008 | (0.006) | 0.182 | 0.063 | 10,047 |
| Manufacturing share of employment | $-0.007$ | (0.012) | 0.556 | $-0.056$ | 8,061 |
| log population (last census) | $-0.038$ | (0.057) | 0.504 | $-0.025$ | 10,041 |
| Population growth, previous decade | $-0.004$ | (0.010) | 0.672 | $-0.024$ | 10,013 |
| Share Black | $-0.010$ | (0.009) | 0.269 | $-0.066$ | 9,425 |
| Share below the poverty line | 0.004 | (0.004) | 0.300 | 0.067 | 7,926 |
| Share of adults 25+ with a BA | 0.004 | (0.006) | 0.555 | 0.028 | 9,425 |
| Unemployment rate | 0.001 | (0.002) | 0.665 | 0.028 | 7,926 |
| South | $-0.032$ | (0.033) | 0.341 | $-0.057$ | 10,050 |
| *Election history* | | | | | |
| NewU, previous window | 0.101 | (0.158) | 0.522 | 0.073 | 10,047 |
| $Z$, previous window | $-0.072^{*}$ | (0.038) | 0.060 | $-0.131$ | 10,047 |
| $S$, previous window | $-0.298$ | (0.248) | 0.229 | $-0.291$ | 10,047 |
| log(1 + elections), previous window | $-0.155^{*}$ | (0.086) | 0.072 | $-0.087$ | 10,050 |
| log elections in the county, 1959–2024 | $-0.113$ | (0.074) | 0.125 | $-0.064$ | 9,922 |
| *Joint tests* | | | | | |
| All 22 covariates ($\chi^2_{22} = 35.6$) | | | 0.033 | | |
| Enrollment and colleges, 7 covariates ($\chi^2_7 = 4.6$) | | | 0.709 | | |
| Economy and census, 10 covariates ($\chi^2_{10} = 14.0$) | | | 0.173 | | |
| Election history, 5 covariates ($\chi^2_5 = 8.8$) | | | 0.116 | | |
| All 22, without $S$, $SR$, $SR'$ ($\chi^2_{22} = 120.9$) | | | $< 0.001$ | | |

*Notes.* Each row is a separate regression of the main specification of Table 3 with a pre-window covariate as the outcome: an IV regression of the covariate on NewU, instrumented by $Z$, with the controls $S$, $SR$ and $SR'$ and window fixed effects, weighted by CBP private employment at the start of the window. Standard errors are clustered by county. The sample is the county-windows of the main estimate from $t$ to $t+4$ (10,050 county-windows, windows ending 1970–2018) where the covariate is observed, and the last column gives their number. The column of standardized effects is the coefficient times the employment-weighted standard deviation of NewU (0.83) divided by the standard deviation of the covariate. The joint tests are Wald tests that the coefficients of the listed covariates are all zero, with the covariance across the regressions estimated from their county-level influence functions. The last test drops the aggregation controls from every regression. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table A7:** First stage by outcome sample.

| | Before the window | | During | After the window | | |
|---|---|---|---|---|---|---|
| | $-12$ to $-8$ (1) | $-8$ to $-4$ (2) | $-4$ to 0 (3) | 0 to 4 (4) | 0 to 8 (5) | 0 to 12 (6) |
| *All colleges* | | | | | | |
| First-stage coefficient | 0.966 | 0.940 | 0.991 | 0.999 | 1.005 | 1.004 |
| | (0.034) | (0.041) | (0.044) | (0.042) | (0.044) | (0.044) |
| Kleibergen–Paap $F$ | 790.2 | 524.4 | 503.3 | 555.5 | 531.0 | 522.2 |
| County-windows | 14,990 | 16,521 | 18,146 | 16,667 | 14,639 | 12,749 |
| Counties | 1,506 | 1,559 | 1,639 | 1,576 | 1,509 | 1,472 |
| Windows ending | 1978–2022 | 1974–2022 | 1970–2022 | 1970–2018 | 1970–2014 | 1970–2010 |
| Mean of NewU | 0.288 | 0.335 | 0.393 | 0.429 | 0.454 | 0.470 |
| SD of NewU | 0.626 | 0.746 | 0.934 | 0.958 | 1.035 | 1.064 |
| With a close election (%) | 33.1 | 34.6 | 35.9 | 38.0 | 39.3 | 40.5 |
| Close elections | 16,025 | 19,970 | 23,423 | 23,283 | 20,282 | 16,754 |
| *Community colleges* | | | | | | |
| First-stage coefficient | 0.972 | 0.931 | 0.977 | 0.996 | 0.979 | 0.984 |
| | (0.054) | (0.055) | (0.057) | (0.064) | (0.065) | (0.066) |
| Kleibergen–Paap $F$ | 329.9 | 285.5 | 290.0 | 245.0 | 224.7 | 218.9 |
| County-windows | 9,380 | 10,018 | 10,585 | 10,050 | 8,934 | 7,892 |
| Counties | 1,038 | 1,048 | 1,060 | 1,035 | 989 | 952 |
| Windows ending | 1970–2022 | 1970–2022 | 1970–2022 | 1970–2018 | 1970–2014 | 1970–2010 |
| Mean of NewU | 0.320 | 0.342 | 0.377 | 0.424 | 0.446 | 0.474 |
| SD of NewU | 0.630 | 0.656 | 0.742 | 0.825 | 0.870 | 0.918 |
| With a close election (%) | 40.3 | 41.2 | 42.3 | 45.0 | 47.2 | 49.6 |
| Close elections | 15,843 | 17,728 | 19,555 | 20,269 | 18,918 | 17,447 |
| *Other colleges* | | | | | | |
| First-stage coefficient | 0.944 | 0.909 | 0.986 | 1.000 | 1.015 | 1.012 |
| | (0.044) | (0.054) | (0.058) | (0.056) | (0.058) | (0.060) |
| Kleibergen–Paap $F$ | 457.4 | 287.3 | 286.1 | 316.2 | 301.4 | 282.4 |
| County-windows | 10,944 | 12,078 | 13,289 | 12,249 | 10,526 | 9,000 |
| Counties | 1,227 | 1,285 | 1,369 | 1,306 | 1,195 | 1,130 |
| Windows ending | 1978–2022 | 1974–2022 | 1970–2022 | 1970–2018 | 1970–2014 | 1970–2010 |
| Mean of NewU | 0.289 | 0.334 | 0.390 | 0.422 | 0.450 | 0.470 |
| SD of NewU | 0.599 | 0.709 | 0.829 | 0.869 | 0.942 | 1.025 |
| With a close election (%) | 37.5 | 39.3 | 40.6 | 43.1 | 44.7 | 46.2 |
| Close elections | 14,643 | 18,295 | 21,462 | 21,389 | 18,446 | 14,991 |

*Notes.* First stage of the regressions in Table 3, by the sample of each column. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: before the window ($t-12$ to $t-8$ and $t-8$ to $t-4$), during it ($t-4$ to $t$) and after it ($t$ to $t+4$, $t+8$ and $t+12$). The first-stage coefficient is the coefficient on close-election union members per 100 workers ($Z$) in the regression of NewU on $Z$, the controls $S$, $SR$, $SR'$ and window fixed effects, weighted by CBP employment at the start of the window. Its standard error is clustered by county. The $F$ statistic is the Kleibergen–Paap Wald statistic. Means and SDs of NewU are employment-weighted. The last two rows count the county-windows with at least one close election and the close elections in the sample.

**Table A8:** Enrollment and associate's degrees by sex.

| | Before the window | | During | After the window | | |
|---|---|---|---|---|---|---|
| | $-12$ to $-8$ | $-8$ to $-4$ | $-4$ to 0 | 0 to 4 | 0 to 8 | 0 to 12 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Community-college enrollment* | | | | | | |
| Men | 0.002 | 0.016 | 0.014 | $-0.038^{**}$ | $-0.044^{**}$ | $-0.043^{*}$ |
| | (0.033) | (0.028) | (0.018) | (0.016) | (0.022) | (0.025) |
| Women | $-0.015$ | $-0.023$ | 0.003 | $-0.040^{**}$ | $-0.065^{**}$ | $-0.071^{**}$ |
| | (0.046) | (0.028) | (0.020) | (0.018) | (0.026) | (0.030) |
| *Panel B. Community-college associate's degrees (3-year sums)* | | | | | | |
| Men | 0.021 | 0.021 | $0.072^{**}$ | $-0.030$ | $-0.070^{*}$ | $-0.069$ |
| | (0.044) | (0.031) | (0.036) | (0.035) | (0.040) | (0.047) |
| Women | $-0.003$ | $-0.004$ | 0.021 | $-0.051^{*}$ | $-0.104^{***}$ | $-0.104^{**}$ |
| | (0.040) | (0.033) | (0.039) | (0.029) | (0.038) | (0.044) |

*Notes.* Main specification of Table 3, separately by sex. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: before the window ($t-12$ to $t-8$ and $t-8$ to $t-4$), during it ($t-4$ to $t$) and after it ($t$ to $t+4$, $t+8$ and $t+12$). Associate's degrees are those awarded by the community colleges of the county, averaged over three award years centered on each year (award years 1966–2023). Standard errors clustered by county are in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

**Table A9:** Lower-level estimates on stacked close elections.

| | Before the window | | During | After the window | | |
|---|---|---|---|---|---|---|
| | $-12$ to $-8$ | $-8$ to $-4$ | $-4$ to 0 | 0 to 4 | 0 to 8 | 0 to 12 |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| *Panel A. Window fixed effects (counterpart of the main specification)* | | | | | | |
| All colleges | 0.000 | 0.002 | 0.007 | 0.001 | $-0.001$ | 0.003 |
| | (0.015) | (0.012) | (0.008) | (0.007) | (0.009) | (0.013) |
| Community colleges | 0.003 | $-0.017$ | $-0.004$ | $-0.032^{*}$ | $-0.044^{*}$ | $-0.049^{*}$ |
| | (0.040) | (0.026) | (0.027) | (0.016) | (0.023) | (0.027) |
| Other colleges | $-0.005$ | 0.007 | $-0.006$ | 0.002 | $-0.009$ | $-0.012$ |
| | (0.013) | (0.010) | (0.010) | (0.010) | (0.015) | (0.019) |
| First-stage $F$ (community colleges) | 8.6 | 10.9 | 15.5 | 20.5 | 19.2 | 19.1 |
| Close elections (community colleges) | 15,843 | 17,728 | 19,555 | 20,269 | 18,918 | 17,447 |
| Counties (community colleges) | 754 | 788 | 816 | 816 | 792 | 766 |
| *Panel B. No fixed effects (the RD plots)* | | | | | | |
| Community colleges | 0.024 | $-0.008$ | $-0.007$ | $-0.042^{*}$ | $-0.063^{**}$ | $-0.044$ |
| | (0.046) | (0.033) | (0.041) | (0.022) | (0.031) | (0.033) |
| First-stage $F$ | 8.6 | 10.5 | 14.1 | 17.2 | 16.4 | 17.1 |

*Notes.* Lower-level IV estimates of Borusyak, Kolerman-Shemer and Lei (2026). Each observation is a close election (within 10 points of a 50 percent union vote share) held in a county-window of the corresponding regression of Table 3, and it carries that county-window's outcome and NewU. Each column is the change in the log outcome between two years, counted in years relative to $t$, the last year of the window: before the window ($t-12$ to $t-8$ and $t-8$ to $t-4$), during it ($t-4$ to $t$) and after it ($t$ to $t+4$, $t+8$ and $t+12$). NewU is instrumented by the union-win indicator. The controls are the vote margin and its interaction with the win indicator. Observations are weighted by eligible voters. Panel A adds window fixed effects. Standard errors clustered by county are in parentheses. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

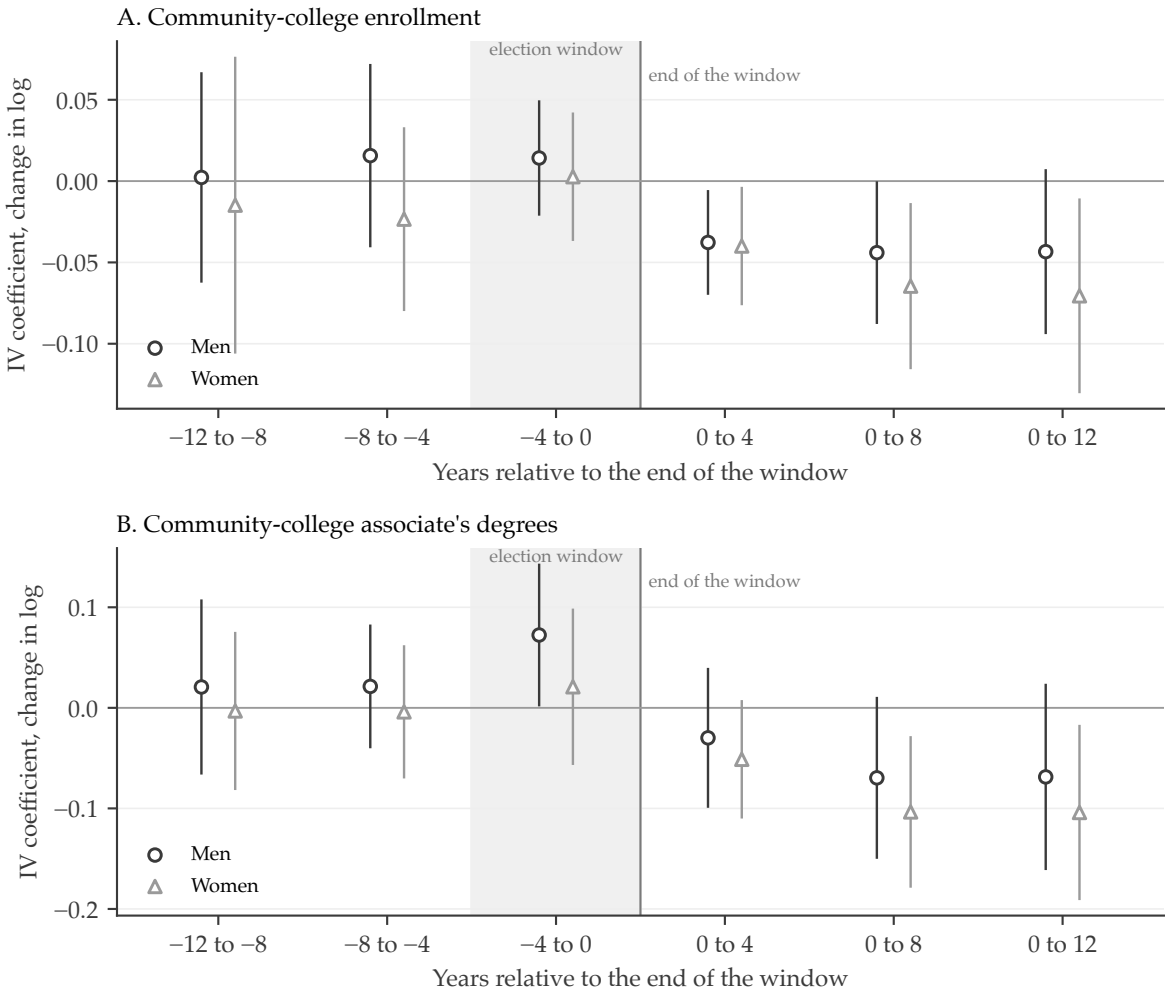



**Figure A5:** Responses by sex. IV coefficients of Appendix Table A8 with 95 percent confidence intervals, by span counted from the last year of the window, which is shaded.

**Table A10:** Community-college enrollment under alternative specifications.

| | 4 years | 8 years | 12 years | $N$ (4 years) |
|---|---|---|---|---|
| *A. Main specification* | | | | |
| Main specification | −0.035** (0.015) | −0.048** (0.021) | −0.051** (0.025) | 10,050 |
| *B. Fixed effects* | | | | |
| State × window fixed effects | −0.031** (0.014) | −0.031* (0.019) | −0.032 (0.022) | 10,010 |
| County and window fixed effects | −0.033** (0.015) | −0.047** (0.020) | −0.053** (0.023) | 10,004 |
| County and window FE, county trends | −0.026 (0.018) | −0.036* (0.019) | −0.036 (0.022) | 10,004 |
| *C. Community-college definition* | | | | |
| Public, two-year or mainly associate's | −0.032** (0.015) | −0.044** (0.020) | −0.048* (0.025) | 10,540 |
| Public, mainly associate's | −0.031** (0.015) | −0.043** (0.020) | −0.049** (0.025) | 10,153 |
| Public two-year, status year by year | −0.027* (0.016) | −0.035 (0.024) | −0.047* (0.028) | 10,002 |
| *D. Scaling, weights and window grid* | | | | |
| Employment at the window end | −0.039** (0.018) | −0.054** (0.024) | −0.058** (0.029) | 10,050 |
| Unweighted | −0.003 (0.009) | −0.001 (0.010) | 0.011 (0.012) | 10,050 |
| Weights: enrollment at public colleges awarding mainly associate's degrees, fall before the window | 0.001 (0.005) | 0.005 (0.008) | 0.010 (0.011) | 9,432 |
| Four 4-year grids pooled | −0.031** (0.013) | −0.046** (0.020) | −0.038 (0.024) | 39,303 |
| Windows ending 1972, 1976, …, 2020 | −0.024* (0.014) | −0.035* (0.019) | −0.004 (0.022) | 9,343 |
| *E. Functional form and estimator* | | | | |
| Log change, one set fixed at $t$ | −0.031** (0.015) | −0.036* (0.020) | −0.044* (0.024) | 10,293 |
| Davis–Haltiwanger growth | −0.040** (0.020) | −0.060** (0.025) | −0.033 (0.029) | 10,624 |
| Percent of baseline (winsorized) | −3.39* (1.95) | −6.48** (3.21) | −9.24* (4.73) | 10,463 |
| CC share of enrollment (points) | −0.89** (0.41) | −0.83* (0.47) | −1.04** (0.53) | 10,233 |
| Weights: CC enrollment at the start of each change | 0.004 (0.005) | 0.008 (0.008) | 0.013 (0.011) | 10,049 |
| Poisson, control function | 0.004 (0.006) | 0.003 (0.008) | 0.005 (0.011) | 10,463 |
| *F. Geography* | | | | |
| County, 4-year changes chained | −0.031** (0.015) | −0.040** (0.020) | −0.048* (0.025) | 10,282 |
| Commuting zone, 4-year changes chained | 0.018 (0.031) | 0.007 (0.040) | 0.021 (0.050) | 5,624 |
| *G. Samples* | | | | |
| Windows ending 1970–2010 | −0.032** (0.015) | −0.045** (0.021) | −0.051** (0.025) | 8,549 |
| Balanced sample | −0.007 (0.018) | −0.012 (0.023) | −0.012 (0.027) | 5,927 |
| Complement in windows ending 1970–2010 | −0.049* (0.026) | −0.080** (0.034) | −0.093** (0.042) | 2,622 |

*Notes.* Each cell is the coefficient on NewU in a separate IV regression, with the standard error clustered by the geographic unit in parentheses. The outcome is the change in log community-college enrollment from the end of the window to 4, 8 or 12 years later. The main specification is that of Table 3. Each other row changes one element. Panel C changes the set of colleges counted as community colleges: public colleges that are two-year or award mainly associate's degrees, public colleges that award mainly associate's degrees, and public two-year colleges with the status decided each year instead of at the start of each change. In Panel D the first row scales NewU by and weights by employment at the end of the window, the second weights all county-windows equally and the third weights by the county's enrollment at public colleges awarding mainly associate's degrees in the fall before the window. The pooled row stacks the four grids of 4-year windows. The 1972–2020 grid starts with enrollment observed in fall 1968. Panel E changes the outcome or the estimator. The percent and share rows are in percent and percentage points. The row weighted by CC enrollment weights each change by the county's community-college enrollment at its start. The Poisson row reports the coefficient of a Poisson regression of enrollment at the end of the change on NewU with enrollment at the start as exposure, a first-stage residual control and window fixed effects, unweighted, with a standard error that treats the first stage as known. Panel F chains 4-year changes of enrollment, with the set of colleges fixed at the start of each 4-year change, instead of annual changes. The commuting-zone row clusters by commuting zone. In Panel G the balanced sample keeps the windows ending in 1970–2010 and the county-windows observed at every point from 12 years before to 12 years after the end of the window. The last column is the number of observations at 4 years. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table A11:** Community-college enrollment by period.

| | Window ending 1970 | Windows ending 1974–1982 | Windows ending 1986–2022 | All |
|---|---|---|---|---|
| *Panel A. IV estimates, change in log enrollment* | | | | |
| CC, 4 years | −0.099* | 0.001 | −0.081** | −0.035** |
| | (0.056) | (0.012) | (0.033) | (0.015) |
| CC, 8 years | −0.164** | 0.004 | −0.117** | −0.048** |
| | (0.076) | (0.014) | (0.050) | (0.021) |
| CC, 12 years | −0.222** | 0.010 | −0.101* | −0.051** |
| | (0.093) | (0.020) | (0.053) | (0.025) |
| Other colleges, 8 years | −0.003 | −0.006 | −0.035 | −0.009 |
| | (0.016) | (0.018) | (0.051) | (0.014) |
| Kleibergen–Paap $F$ (CC, 8 years) | 17 | 220 | 214 | 225 |
| County-windows (CC, 8 years) | 603 | 2,034 | 6,297 | 8,934 |
| SD of NewU (CC, 8 years) | 2.55 | 0.97 | 0.41 | 0.87 |
| Equal across periods, $p$ (4 / 8 / 12 years) | 0.014 / 0.006 / 0.007 | | | |
| *Panel B. Components of the 8-year change in CC enrollment* | | | | |
| Total (decomposition sample) | −0.165** | 0.004 | −0.125** | −0.050** |
| | (0.075) | (0.014) | (0.050) | (0.020) |
| Incumbent colleges | −0.057 | 0.005 | −0.121** | −0.029* |
| | (0.050) | (0.014) | (0.052) | (0.018) |
| Colleges entering after $t$ | −0.108* | −0.001 | −0.004 | −0.021* |
| | (0.059) | (0.003) | (0.026) | (0.012) |
| New colleges' share (%) | 65 | – | 3 | 42 |
| County-windows | 596 | 2,029 | 6,273 | 8,898 |
| *Panel C. Community-college openings in the county (number)* | | | | |
| Openings in the 8 years after $t$ | −0.138* | −0.032* | 0.038 | −0.052** |
| | (0.073) | (0.019) | (0.044) | (0.025) |
| Mean | 0.421 | 0.187 | 0.105 | 0.136 |
| Openings from $t-8$ to $t-4$ (placebo) | −0.016 | 0.007 | 0.032 | 0.012 |
| | (0.041) | (0.029) | (0.035) | (0.018) |
| Mean | 0.638 | 0.352 | 0.060 | 0.126 |
| County-windows | 1,807 | 3,604 | 11,298 | 16,709 |

*Notes.* Main specification of Table 3, estimated separately on the windows ending in each period. The window ending in 1970 covers elections in 1967–1970. Its regressions have one window, so the window fixed effect is a constant. The test of equal coefficients across the three periods comes from the stacked regressions with all coefficients interacted with the period and county-clustered errors. In Panel B the change in log community-college enrollment from $t$ to $t+8$ is split exactly, step by step, into the chained log change of the colleges already in the series (incumbents) and the contribution of the colleges that enter the county's series after $t$ (their entry and later growth relative to the incumbents). Panel B leaves out the county-windows where all the incumbent colleges close. The new colleges' share is their coefficient divided by the total, shown only when the total is significant at the 10 percent level. In Panel C an opening is a college's first appearance in the county's community-college series, and the sample is every county-window of the period, including counties without a community college at $t$. Standard errors clustered by county are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

**Table A12:** Close-election unionization and bachelor's attainment, 1980–2000.

| | Same decade L0 | Cohort window W | Previous decade L10 |
|---|---|---|---|
| *Panel A. Change in the log share with a bachelor's degree* | | | |
| Men | $-0.0277^{**}$ | $-0.0075$ | 0.0082 |
| | (0.0113) | (0.0096) | (0.0078) |
| Women | $-0.0163$ | $-0.0064$ | 0.0126 |
| | (0.0126) | (0.0101) | (0.0090) |
| Both sexes | $-0.0222^{**}$ | $-0.0075$ | 0.0099 |
| | (0.0104) | (0.0089) | (0.0072) |
| First-stage $F$ (men) | 164 | 137 | 124 |
| County-decades | 9,385 | 9,385 | 9,385 |
| *Panel B. Change in the share with a bachelor's degree (percentage points)* | | | |
| Men | $-0.09$ | $-0.19$ | $-0.13$ |
| | (0.19) | (0.19) | (0.14) |
| Women | 0.02 | $-0.07$ | 0.06 |
| | (0.14) | (0.12) | (0.10) |
| Both sexes | $-0.02$ | $-0.13$ | $-0.03$ |
| | (0.15) | (0.14) | (0.11) |
| County-decades | 9,394 | 9,394 | 9,394 |

*Notes.* County changes over the decades ending in 1980, 1990 and 2000 in the share of residents aged 25 and older with a bachelor's degree (decennial Census, NHGIS). NewU counts new union members per 100 residents aged 16 and older in a 10-year window and is instrumented by the members added in close union wins. L0 is the decade of the outcome, $(D-9, D]$. W is $(D-14, D-5]$, centered on the years in which the cohorts that turn 25 during the decade were 18. L10 is the previous decade, $(D-19, D-10]$. All regressions control for $S$, $SR$ and $SR'$, include county and decade fixed effects and are weighted by the population aged 16 and older. Standard errors clustered by county are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.