Do Donors Flee the Sunshine? Transparency Laws and Public University Foundation Finances

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Abstract

Independent foundations play an increasing role in funding higher education, leading to calls for transparency regarding their donors and operations. Some have opposed such efforts, arguing that disclosure requirements may discourage donations and reduce endowment growth. Using panel data on public institutions from IPEDS and the foundations associated with them from IRS disclosures, we employ the difference-in-differences estimator of Callaway and Sant'Anna (2021) and estimate the causal effect of state transparency laws on both the stock of foundation assets and the flow of gifts to foundations. We also test for changes in the net price paid by in-state students and institutional expenditures. We find no statistically significant effect on any outcome, a result robust to alternative treatment definitions, sample restrictions, control compositions, and an augmented synthetic control estimator. Our design rules out severe sustained donor flight—the strongest version of the policy concern—but cannot reject more modest gift reductions. We interpret these results as evidence that transparency mandates do not produce detectable medium-run harm to foundation finances.

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1 Introduction

A significant feature of the higher-education landscape in the United States is the presence of substantial endowment funds associated with major colleges and universities. These funds, which in some cases control assets several times larger than the associated institution's annual revenue, serve several purposes, including operational support, insurance, autonomy, academic quality, and scholarships (Hansmann, 1990; Lerner et al., 2008; Meyer & Zhou, 2017). Endowments are generally invested in capital markets, and thus their growth can be a significant factor in the financial health of an institution. Endowment growth can broaden access to elite institutions, while poor endowment performance can lead to cuts in instructional staff and educational programs (Brown et al., 2010; Gilbert & Hrdlicka, 2015).

A primary function of an endowment is to be an instrument through which funds may be donated to an institution in a way that is separate from the day-to-day operations or governance of that institution. While endowments are generally managed by a foundation established and controlled directly or indirectly by their associated university or institution (Dimmock, 2012), they exist as a separate legal entity that often shields the endowment from regulatory restrictions placed on the institution itself. Donors may give anonymously, and may place restrictions on how funds may be used. For example, a donor may only give money on the condition that it be used exclusively for athletic rather than academic programs. This has led to a growing debate surrounding the transparency of endowment operations and the way in which donors may influence institutions through restricted donations. On one hand, students, faculty, and other advocates have tended to support an increased level of transparency. On the other, many university administrators have opposed efforts to increase foundation transparency, fearing that disclosure may reduce the rate at which donors are willing to support the university. With no federal law in place, this debate is left to state legislators (Jung, 2024).

We seek to inform this debate by estimating the effects of state-level foundation transparency regulations on the financial health of institutions of higher education. The empirical question can be framed at three levels: do transparency laws reduce *gifts* to university foundations; do they reduce the *stock* of foundation endowment assets; and do they reduce the *institutional resources* available for student services and instruction?

We investigate the first question using data from IRS filings from the non-profit foundations associated with universities. We investigate the second and third using data from the Integrated Postsecondary Education Data System (IPEDS) on endowment values, net prices, and instructional expenditures from over 1,200 institutions over 2004–2024. We employ the staggered difference-in-differences framework of Callaway and Sant'Anna (2021), which accounts for variation in treatment timing across states. We find no evidence that transparency laws reduce the flow of gifts to endowments or the assets of those endowments, nor that they increase the net price paid by in-state students or reduce instructional spending.

While, to the best of our knowledge, we are the first to measure the impacts of these rules on institutional foundation finances, there is a more general literature on university foundations and their relationships with their associated institutions. One major concern in this literature is the growing concentration of wealth to a small number of elite institutions. In 2013, among the 120 U.S. universities with the largest endowments, the top 10% of institutions collectively held approximately the same value in endowments as the bottom 90% combined. Meyer and Zhou (2017) argue that while large endowments have historically improved public welfare by fueling academic freedom and innovation, a shift towards a "winner-takes-all" market has led to endowments being used more as a means to cushion top universities from the need to compete. Bulman (2022) provides evidence that larger endowments are associated with higher selectivity as universities are able to increase spending on instruction and student services, resulting in fewer low-income students and persons of color enrolling. Thus, Libby (2025) argues that university endowments should be taxed to align incentives with the public good.

Our work connects to the broader literature on transparency and accountability in higher education (McLendon & Hearn, 2006). This literature has often focused on the competing objectives of public accountability, individual privacy, and institutional autonomy (Cleveland, 1985). In this context, calls for foundation transparency generally come from those seeking to enhance accountability to the public, particularly after perceptions of unethical spending or other behaviors (Nicholson, 2023). Opposition generally comes from concerns about privacy (Mitchell, 2011) leading to a donation-chilling effect (McGuigan, 2021). That said, it is possible that mandatory disclosure may *increase* giving through increasing donor trust (Barber et al., 2022).

Our work also connects to the literature on transparency in the public sector writ large, which generally finds that greater disclosure leads to more desirable social outcomes. For example, there is evidence that disclosing salaries for public sector employees may reduce the gender pay gap by as much as 40% (Baker et al., 2019). In the context of government spending, increased transparency is correlated with more equal political competition and power sharing, while decreased transparency is correlated with more political polarization (Alt et al., 2006). However, in the private sector the effects of transparency may not be so desirable. Some scholars argue that increased transparency in corporate governance can harm a firm's profitability while leading to higher executive compensation, and may incentivize firms to distort information (Hermalin & Weisback, 2007). Still, even in the private sector there is evidence that foundations which rely on donations may benefit from transparency, as donors demand to know how their money is actually being spent (Rey-Garcia et al., 2012). These findings paint a somewhat conflicting picture of how transparency regulation may affect higher education.

A separate strand of the philanthropy literature speaks more directly to why foundation transparency might affect giving in either direction, and offers some predictions about which donors should be most responsive. Glazer and Konrad (1996) model charitable contributions as a signal of unobservable income or wealth, observing that charities routinely publish donor names in rank-ordered threshold categories ("contributor," "benefactor," "patron," and so on) precisely because such recognition allows donors to distinguish themselves to peers. Harbaugh (1998a, 1998b) develops a complementary framework in which donors derive utility from the prestige conferred by public acknowledgment, and shows empirically – using data on alumni donations to a prestigious law school – that donors disproportionately bunch at category thresholds, consistent with a substantial prestige motive. In Harbaugh's framing, universities are an especially clean case: alumni donations to one's alma mater purchase a form of prestige and reputational capital among fellow alumni that gifts to substitute charities cannot replicate, giving the institution a degree of monopoly power over the recognition donors seek.

These models have an ambiguous, but illuminating, implication for our setting. To the extent that the marginal donor to a public university foundation is motivated by prestige and recognition, foundation transparency laws should have little deterrent effect – and may even increase giving, since broader public visibility magnifies the prestige value of a recognized gift. The donor-flight argument implicitly assumes a different marginal donor: one who values anonymity, perhaps because the gift is intended to influence institutional decisions, to avoid public scrutiny of the source of wealth, or to attach restrictions that the donor would prefer not be publicly known. Whether transparency laws meaningfully reduce giving therefore depends on the relative size of these two donor populations and on which group is marginal at the institutions in our sample. Our null aggregate results are consistent with the prestige-motive donors dominating in this market, though we cannot directly observe the composition of giving.

2 Data

Our analysis combines two primary administrative sources. We collect institutional characteristics (including spending) and endowment stocks from the Integrated Postsecondary

Education Data System (IPEDS). We also collect data on foundations directly through IRS Form 990 filings as digitized and harmonized by the National Center for Charitable Statistics (NCCS). Because the foundation is where donors give and the institution is where money is eventually spent, these two sources allow us to more fully test the donor-flight hypothesis along the chain from gifts to assets to spending.

2.1 Institutional finances: IPEDS

We collect institutional characteristics from the IPEDS from 2004 through 2024. IPEDS includes every college, university, and technical and vocational institution in the United States that receives federal student financial aid.

Our primary IPEDS outcome is the beginning-of-year (BOY) endowment value, which measures the stock of endowment assets at the start of each fiscal year.[1] IPEDS collects financial data on separate forms for public institutions (GASB standards) and private nonprofit institutions (FASB standards). Endowment values are reported on each form, and we use the form-specific measure throughout; private nonprofit institutions, which these laws do not reach, enter only as a placebo.

To test whether transparency laws affect institutions through channels beyond the endowment stock, we follow the money down the chain to the activities donor support funds. We examine four spending outcomes—research, scholarship, and instructional expenditures, and the gross book value of buildings—together with the average in-state[2] net price paid by Title IV financial aid recipients (reported from 2009).[3] If transparency requirements reduced foundation support, institutions might compensate by raising the net cost to students, cutting instruction or student aid, or curtailing construction. Buildings is drawn from the GASB capital-assets schedule and is therefore reported by public institutions only.[4] Research expenditures are examined only at research-active institutions, defined as a median annual research expenditure of at least $1 million.

Finally, we also collect a decomposition of within-year endowment changes into gifts to endowment, investment returns, spending distributions for current use, and other changes.

[1] We focus on BOY rather than end-of-year (EOY) endowment because it is predetermined with respect to within-year investment and spending decisions. We have repeated our analysis using EOY endowment values with similar qualitative results.

[2] IPEDS does not report a similar measure for out-of-state students at public institutions.

[3] Net price warrants a caveat. Several treated states enacted "free community college" programs inside their post-treatment windows—Tennessee Promise (2014), the New Jersey Community College Opportunity Grant (2018), Virginia G3 (2021), the New Mexico Opportunity Scholarship (2022), and California AB 19 (2017)—which mechanically reduce in-state net price at two-year institutions regardless of any foundation channel. The estimate we report should be read with that coincidence in mind.

[4] This stands in for the donor-funded construction channel, as construction in progress has extremely high variance.

As IPEDS only began collecting these sub-components in 2020, there is insufficient data to use as DiD outcomes. They do, however, provide a useful benchmark for the scale of gift flows: from 2020–2024 gifts to endowment are a median 3% of beginning-of-year endowment value annually.

2.2 Foundation finances: IRS Form 990

The independent foundations associated with many public institutions are organized as 501(c)(3) entities, meaning they fall under the general non-profit transparency regulations enforced by the IRS. Each year, they must file IRS Form 990 and report several financial outcomes, including gifts. We construct a panel of foundation finances from NCCS repositories of Form 990 data. Our primary outcome is total contributions received (Form 990, Part VIII, line 1h). We also observe total revenue (which can include investment earnings), end-of-year total assets, and the rolling five-year gift total reported on Schedule A.

We build the panel from the NCCS Core public-charity files, which cover both Form 990 and Form 990-EZ filers and extend back to 1989. The year in an NCCS file name is a release vintage rather than a fiscal year, and the same return can appear in several vintages, so we date every return by its tax-period end date and retain one observation per entity-fiscal year, keeping the latest vintage in which it appears. We exclude the 2022 release, which is a known partial extract whose incompleteness is correlated with filer size. The resulting panel extends through fiscal year 2021; we estimate on fiscal years 1990 onward, dropping fiscal 1989 because only about fifty foundations report it. Because roughly three-quarters of university foundations close their books on June 30, a fiscal year labeled t typically spans July $t-1$ through June t; we carry each entity's modal fiscal-year-end month and use it in the treatment-timing sensitivity checks described below.

Linking institutions to foundations. The principal measurement challenge is that no crosswalk from institutions to foundation Employer Identification Numbers (EINs) exists, and the mapping is not one-to-one. Some foundations serve entire multi-campus systems; some institutions operate several vehicles at once (a general-purpose foundation alongside real-estate, research, or athletic entities); and a number of large public universities—Penn State, the University of Michigan, the academic campuses of the University of Texas—hold their endowments through governmental entities and have no affiliated foundation at all. The mapping also changes over time, as campuses consolidate and foundations merge, dissolve, and reincorporate.

We resolve each public institution in our sample frame to its affiliated foundation EIN(s) in three stages: a national candidate pool built from the IRS Business Master File; independent review of every state's institutions and candidate foundations by two large language models; and human adjudication of all cases where the two reviews disagreed or either flagged uncertainty. Section A describes the procedure and its validation in full. Against an independently hand-verified reference set, the automated stages agree at 96.7 percent before adjudication, and all flagged cases were adjudicated by hand.

Of the 2,253 public institutions examined, 1,898 (84 percent) match to at least one foundation and 355 have no separate foundation; the latter hold endowments through state entities and lie outside the population this outcome is defined on.

Foundation lineages. Because foundations reorganize, our unit of analysis is the foundation *lineage*: several EINs summed into a single synthetic entity, so that a consolidation does not appear as a donation collapse at one EIN alongside a windfall at another. Where one foundation serves many campuses—statewide community-college system foundations, for instance—it enters the panel once, as a single lineage. Each lineage inherits treatment status from the state of the campuses it supports, which is the state whose transparency law binds it. The crosswalk yields roughly 1,650 EINs organized into 1,637 lineages, of which 1,580 report financial data in at least one year and enter the panel; the panel contains 38,092 lineage-year observations.

2.3 Transparency laws and treatment definition

We obtain state-level transparency regulation data through a comprehensive review of state public records laws. Public records laws—the state-level equivalents of the federal Freedom of Information Act—control access to documents generated by government institutions and, depending on the state, private organizations which receive public funds or perform a government service. Public higher education foundations are typically private 501(c)(3) charities legally distinct from their associated institutions, but they exist for the purpose of financially supporting a public body. Whether these foundations are subject to public records laws depends on the particulars of each state's statutory language and, in some cases, on how interwoven foundation and institution operations are.

To perform our review, we checked the text of each state's public records statute for its definition of "public body" or "agency" and whether it included mention of "quasi-governmental bodies." We also checked for statutory exemptions, searched court case tabulations in legal literature and news coverage of legal disputes over access to foundation records (Capeloto, 2015), and consulted attorney general opinions and public records committee orders where available (Nicholson, 2023; Reinardy & Davis, 2005; Student Press Law Center, 2010).

Using the totality of this evidence, we classified each state, and the District of Columbia, into one of four categories. We found 20 states that *explicitly* require foundations associated with their public higher education institutions to be subject to public records law, 4 states where foundations are *directly* subject to additional government scrutiny but not full public records access, 12 states that explicitly *shield* foundations from public records law, and 14 states—plus the District of Columbia—where the legal status is *unknown* or unsettled. Figure 1 displays the geographic distribution of these classifications. Twenty-two states have an identifiable year in which the transparency requirement was enacted; of these, 9 were enacted during our panel period (2004–2024), providing the variation that identifies our treatment effects. The transparency regulations in the remaining 13 states were enacted prior to our panel and are classified as "always treated."

Those nine adoptions do not look the same in our two panels, because the panels end in different years. New Mexico's 2023 law falls inside the IPEDS window but after the last Form 990 fiscal year we observe, so the institution panel has six adoption cohorts (2005, 2011, 2012, 2017, 2020, and 2023) while the foundation panel has five, with New Mexico's lineages entering there as never-treated controls. Figure 2 plots the rollout of the laws against both observation windows.

Our primary treatment definition uses Explicit states only, with None states as the control group. We exclude Direct states (whose foundations face government scrutiny but not full public records access), Unknown states, and states whose transparency laws predate the panel (enacted before 2004). The last exclusion ensures that control institutions are truly never-treated; including "always-treated" states as controls would risk using institutions that have already adjusted to transparency requirements. Our robustness checks vary these treatment definitions.

2.4 Sample definition

The transparency laws we are concerned with govern *public* university foundations, so our analysis focuses on public institutions. We include both two-year and four-year institutions, as transparency laws apply to foundations regardless of the level of instruction offered, and many community colleges maintain foundations with substantial endowments.

That said, our two data sources face unique challenges with respect to reporting and coverage. To ensure that these features of the data do not drive our results, we explore a variety of sample definitions and restrictions for both the IPEDS and Form 990 data. In addition to the range of definitions we explore for each data source individually, we report a common-sample specification on each side: the institutions in the primary IPEDS sample that are served by a foundation surviving the Form 990 estimation frame, and those foundations.

Institution sample (IPEDS). Endowment reporting in IPEDS is far from universal. Among public institutions in Explicit or None states without a pre-panel law, 600 ever report a positive beginning-of-year endowment and only 389 report one in every year of the panel. Requiring a complete series is nonetheless the wrong screen, for two separate reasons. First, it excludes institutions for missing *data* rather than for missing endowments: the Urbana-Champaign campus of the University of Illinois reports in 19 of 21 years against a median reported endowment of \$1.28 billion, and Pennsylvania State University (\$1.77 billion, 19 years), Rutgers–New Brunswick (\$683 million, 20 years), and the Colorado School of Mines (\$208 million, 20 years) are excluded on the same grounds. Thirty-six of the 49 institutions in that band sit in treated states. Second, and separately, completeness does nothing about institutions with no meaningful endowment to measure: Nashville State Community College reports a median of \$4,739, contributing enormous proportional changes from trivial absolute ones.

We therefore screen on reporting coverage and on size separately. An institution enters the primary sample if it reports a positive beginning-of-year endowment in at least 18 of the 21 panel years and holds a median endowment of at least \$1 million over a fixed 2004–2010 baseline window. The baseline window is pre-treatment for every in-panel cohort except 2005, so the size screen cannot condition on post-treatment outcomes.[5] We additionally exclude the small number of institutions that switch private/public classifications during the panel as their financial reporting shifts between GASB and FASB standards.

Of the 600 institutions that ever report an endowment, 468 clear the coverage rule and 361 also clear the baseline size floor without switching sector. That is our primary sample: 361 public institutions—210 in treated states and 151 in never-treated states, 149 two-year and 212 four-year—contributing 7,570 institution-years. Our robustness checks restrict to four-year institutions only, impose alternative endowment floors, vary the reporting requirement in both directions, and estimate the same specifications on private nonprofit institutions as a placebo.

Foundation sample (Form 990). Restricting the lineage panel to Explicit and None states without a pre-panel law leaves 700 lineages. From these we exclude three categories whose Part VIII line 1h is contaminated by government grant flows rather than private giving: research foundations, hospital and medical foundations, and the small number of institutions

[5] Institutions that first report an endowment after 2010 have no baseline median and are excluded.

that file a Form 990 for themselves. This removes 23 lineages. A further 2.0 percent of lineage-years report zero or negative contributions and cannot enter a log specification, leaving an estimation sample of 666 lineages—405 treated in eight states, and 261 never-treated. Including the excluded types, or narrowing the sample to general-purpose foundations only, leaves the results unchanged. The panel carries no balance requirement: NCCS vintage coverage is uneven—fiscal 1999 is a near-total gap, with 76 lineages reporting against roughly 500 in adjacent years—and only two lineages report in all 32 years, so we estimate on the unbalanced panel and report a near-balanced restriction as a robustness check.

Summary statistics for our primary sample are shown in Table 1. Treated and control institutions are broadly comparable in terms of endowment size, net price, and building values; though treated institutions have larger mean expenditures across all categories, reflecting the concentration of large state university systems in treated states such as California and Pennsylvania.

Comparing the two panels of Table 1, the average foundation appears to hold fewer assets than the average institution reports as endowment—the opposite of what one would expect if foundations were the primary vehicle through which institutional wealth is held. This is an artifact of composition rather than a failure of coverage. The foundation panel imposes no size floor, so it includes many small community-college foundations whose institutions do not clear the endowment screens described above; only 56 percent of the foundation lineages in Panel A serve an institution that appears in Panel B. Restricting to the matched set reverses the comparison. Across the 4,738 institution-years in which we observe both a single-campus foundation and its institution's reported endowment, the median ratio of foundation assets to institutional endowment is 1.52, foundation assets exceed the reported endowment in 89 percent of cases, and the log correlation between the two is 0.81. The two series track the same underlying institutional scale while measuring different objects: a foundation's balance sheet includes operating reserves, pledges receivable, real estate, and campaign funds held for construction alongside the endowment corpus it manages, whereas the IPEDS figure reports only the endowment stock at the beginning of the year.

3 Empirical Strategy

We employ the difference-in-differences estimator of Callaway and Sant'Anna (2021), which is designed for settings with staggered treatment adoption. Recent work has shown that the conventional two-way fixed effects (TWFE) estimator can produce biased estimates in such settings due to "negative weighting" of heterogeneous treatment effects across cohorts (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). The Callaway and Sant'Anna

(2021) estimator avoids this problem by separately estimating group-time average treatment effects for each treatment cohort:

$$ATT(g, t) = E[Y_t(g) - Y_t(0) \mid G = g]$$

where g indexes the treatment cohort (defined by the year a state's transparency regulation took effect), t indexes the calendar year, and $Y_t(g)$ and $Y_t(0)$ denote potential outcomes under treatment and no treatment, respectively. These group-time effects are then aggregated into an overall average treatment effect on the treated (ATT).

Identification relies on a parallel trends assumption: absent treatment, the evolution of outcomes for treated institutions would have followed the same trajectory as control institutions. We assess this assumption through event-study plots that display pre-treatment estimates alongside post-treatment effects. Comparisons are built from not-yet-treated units, which in our sample consists of None states (never treated) plus Explicit states in their pre-treatment years, providing a larger and more stable control pool than never-treated units alone.

Our primary outcome variables are measured in logs, so the ATT estimates are interpretable as approximate percentage effects. Because transparency laws target public university foundations, our primary specification uses public institutions only. We also report results for private nonprofit institutions as a placebo check.

4 Results

Table 2 reports our main results in order of the flow of resources from contribution flows to endowment stocks and then spending flows. The first two columns report average treatment effects and standard errors estimated under a standard asymptotic assumption. However, since only nine states adopt transparency laws during our panel, we report three alternative inference measures rather than a traditional asymptotic p-value. The first is the minimum detectable effect (MDE) at 80% power. The second is a 95% confidence interval computed using the wild-cluster bootstrap procedure over state clusters (Cameron et al., 2008). The third is a randomization-inference p-value from placebo reassignments of the sequence of observed treatments (MacKinnon & Webb, 2020). We discuss the results of each step in the flow in sequence, though we note up front that all results are null: we can't reject the hypothesis that transparency laws do not affect university or foundation finances.

Gifts. The contributions received by foundations are the beginning of the resource flow, and the outcome perhaps most directly linked to the decisions of donors. The point estimate is positive, suggesting that, if anything, transparency laws increase contributions. The wild cluster bootstrap results indicate that we can rule out declines larger than 10%.

Figure 3 illustrates these results in event-study form. Contributions vary within roughly ±0.2 log points of the mean during the pre-period with no immediate anticipation or adoption effect. The mild positive drift beyond five fiscal years is generated by the two earliest cohorts, and its confidence band contains zero at every time period.

Assets. Our analysis of the endowment stock yields similar results. Again, the point estimate is positive, though the confidence interval is wide. Figure 4 displays the dynamics. The pre-treatment estimates show no systematic trend. Post-treatment estimates begin near +0.04 and rise to roughly +0.10 by the ninth year, never separating from zero and widening steadily as cohorts drop out of the longer horizons. We read this as a null with a mildly positive point estimate, not as evidence of a delayed increase.

Spending. None of the spending outcomes reach significance. In fact, other than the book value of buildings, the point estimates for each outcome suggest an *increase* in flows. Similarly, the point estimate for in-state net price suggests an improvement in resources, not a decline. The largest point estimate is for research spending at research universities, though we interpret this too as a noisy null, not as a rejection of the null.

Statistical power. Every estimate above is a null, so what the design and data can rule out matters as much as what it finds. The MDE column of Table 2 reports, for each outcome, the effect a 5%-level test would detect 80% of the time, $(z_{0.975} + z_{0.80}) \times \text{SE} = 2.80 \times \text{SE}$. These range from 10% on net price to 63% on scholarship expenditures. We note that clustering at the state level, as the assignment of treatment requires, roughly doubles the standard error relative to the institution-level default.

On the other hand, the wild-cluster bootstrap intervals are tighter than the MDEs alone would suggest—for example, we obtain intervals of $[-10\%, +6\%]$ on contributions and $[-6\%, +15\%]$ on the endowment—because they come from a stacked event study that weights cohorts differently and restricts attention to a $t \in [-5, +10]$ window.

5 Robustness

We report robustness separately for the two panels of Table 2, since they rest on different data and face different threats to identification. Table 3 varies the foundation contributions specification along four dimensions—treatment timing, sample, outcome definition, and inference. Table 4 varies the institutional specifications, reporting the endowment stock alongside the three institutional outcomes broad enough to survive the full battery.[6]

Foundation contributions. Panel A of Table 3 addresses timing. Because most foundations close their books on June 30, a law enacted in calendar year t binds only part of fiscal year t. Dating treatment one fiscal year later lowers the standard error but does not change the qualitative null result.

Panel B varies the sample definition. In separate specifications, we add back the grant-administering entities the primary specification screens out, narrow to general-purpose foundations only, impose a near-balanced-panel requirement, drop the five lineages with the largest single-year windfalls, restrict to foundations whose institutions also appear in the IPEDS sample, and collapse the panel to state-by-fiscal-year aggregates so that each of the eight treated states contributes a single series. Point estimates range from +2.6 to +14.8 log points. All are positive, but we are not able to reject the null hypothesis under any specification.

Panel C changes the outcome. Replacing the log of the contributions with an inverse hyperbolic sine, which retains the zero-contribution observations the log drops, yields a larger point estimate and a p-value of 0.06, which merits additional investigation. The zero-contribution years are concentrated in real-estate vehicles—university-affiliated entities funded by rent rather than by gifts—which report zero contributions in most years, and the asinh transformation converts each zero-to-positive transition into a jump of roughly fourteen points. Treated lineages happen to have proportionally more such transitions after adoption than before. Therefore, we interpret this estimate primarily as a mechanical artifact of the transformation. We also explore using the Schedule A five-year gift total as an outcome. This estimate is also uninformative.

Panel D explores inference results under different assumptions. Clustering on the lineage rather than the state cuts the standard error by a third and the MDE from 41% to 25%. We also estimate a stacked event study which imposes a common effect across cohorts with $t \in [-5, +10]$. While this approach results in a negative point estimate and the tightest MDE, we still cannot reject the null hypothesis of no effect.

Institutional outcomes. Panel A of Table 4 varies the treatment and control definitions. Broadening treatment to include Direct states, broadening controls to include Unknown

[6] Research expenditures and the book value of buildings are omitted from Table 4 because each carries a sample restriction of its own—research-active institutions and GASB filers respectively—that cannot be varied coherently alongside the sample perturbations in Panel B.

states, doing both, or adding back the always-treated states whose laws predate the panel all leave the endowment estimate between +8.3 and +10.0 log points, though the estimates are never significant. Importantly, the null result does not depend on how we classify the ambiguous states.

Panel B varies the sample definition. We change the reporting requirement in both directions, redefine the size floor over the whole panel rather than the 2004–2010 baseline, move the floor to $500,000, $5 million, and $10 million, impose strict balance, restrict to four-year institutions, and restrict to institutions matched into the Form 990 sample. Across all alternative specifications the endowment estimate ranges from −5.5 to +17.0 log points and, again, is never statistically significant. The other three columns report similar results.

Panel C conditions the parallel trends assumption on baseline log enrollment and log tuition, addressing the concern that treated and control institutions differ in size or pricing. The endowment estimate is +7.4 log points, essentially unchanged from the unconditional specification.

Panel D reports a placebo test using private nonprofit institutions as a "treated" group even though they should not be affected by the transparency laws. The results confirms that they are not: endowment value is +1.6 log points (SE 0.126), instructional expenditures +1.8 (SE 0.169), and scholarship expenditures +16.2 (SE 0.398), all far from significance and all far less precisely estimated than their public counterparts.

An alternative estimator. Finally, we estimate the endowment effect using the augmented synthetic control method of Ben-Michael et al. (2021), which constructs for each treated institution a weighted combination of control institutions matching its pre-treatment endowment trajectory. This substitutes a fit requirement for the parallel trends assumption: identification rests on the synthetic control reproducing the treated unit's pre-treatment outcomes rather than on the two evolving in parallel absent treatment. The estimator yields an overall ATT of +5.4 log points (SE = 0.094), with a confidence interval of $[-13\%, +24\%]$—consistent with the CS DiD null in both sign and magnitude. Figure 6 displays the dynamic estimates, which show pre-treatment balance by construction and no post-treatment estimate distinguishable from zero. We note that this estimator is not exactly like-for-like: the augmented synthetic control estimator requires a strictly balanced panel, so we use the balanced subset of our primary sample—157 treated institutions against 210 in Table 2. It also requires sufficient pre-treatment periods to construct a synthetic control, so we restrict it to cohorts adopting in 2011 or later, dropping the 2005 cohort entirely.

6 Conclusion

As quasi-independent foundations play a greater role in the financial landscape of higher education in the United States, concerns have arisen about the potential for undue influence of donors on academic freedom, particularly if foundations are able to conduct operations completely opaquely. Proponents of transparency argue that such rules would breed greater accountability, while skeptics worry about the potential for such regulations to discourage donors and thus reduce endowment growth and ultimately harm academic operations.

We test for this possibility in this paper. Using a modern difference-in-differences framework that accounts for staggered treatment timing, we find no evidence that state transparency laws reduce gifts to foundations or endowment assets. We also find no evidence of downstream harm: transparency laws do not increase the net price paid by in-state students or reduce instructional expenditures. This null result holds across multiple treatment definitions, across a range of sample restrictions and control group compositions, and under an alternative augmented synthetic control estimator. Our findings are consistent with the view that donor behavior is not substantially altered by foundation transparency requirements, though we cannot rule out modest reductions in giving that would accumulate over horizons longer than our panel permits.

These results have a measured policy implication. States considering whether to subject university foundations to public records requirements can do so without expecting sharp, detectable harm to foundation endowment assets, net prices, or instructional spending in the medium run. The strongest version of the "donor flight" argument—a large, sustained reduction in gifts that would meaningfully erode endowment stocks within a decade—finds no empirical support in our data. We are more cautious about smaller-magnitude or longer-horizon effects on giving, which our design cannot detect.

Several limitations warrant mention. First it is possible that transparency laws shift the composition of endowment flows without affecting the aggregate flow, but we cannot test this with available data. Second, our treatment is a binary indicator that does not capture heterogeneity in the stringency or scope of transparency requirements across states. Third, some treatment cohorts (e.g., Virginia, treated in 2020) have only a few post-treatment years, limiting our ability to detect long-run effects, particularly modest reductions in gifts that would accumulate slowly relative to the size of endowment stocks. Future work with longer panels may shed light on whether transparency affects endowment trajectories over horizons longer than we can currently observe.

SAY SOMETHING ABOUT HETEROGENEITY, CHANGE IN MIX OF DONORS. CAN'T IDENTIFY THAT YET.

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7 Tables and Figures

Table 1: Summary Statistics

	Treated		Never-Treated	
	Mean	SD	Mean	SD
Panel A: Foundation lineages (Form 990, FY1990–2021)				
Contributions received (2024 $M)	9.4	37.7	10.1	35.9
Total revenue (2024 $M)	13.4	48.9	16.3	59.8
Total assets, end of year (2024 $M)	71.8	279.8	111.5	419.1
Campuses served	1.23	1.30	1.53	2.30
Foundation lineages	405		261	
Lineage-years	10,044		6,301	
Treatment cohorts	5		—	
Panel B: Public institutions (IPEDS, 2004–2024)				
Endowment Value (2024 $M)	185.5	686.0	157.8	483.0
Research Expenses (2024 $M)	64.1	201.1	46.7	138.7
Scholarship Expenses (2024 $M)	21.3	30.8	12.1	15.9
Instructional Expenses (2024 $M)	144.3	263.1	98.7	151.6
Net Price, in-state (2024 $)	13,837	6,278	13,431	4,567
Buildings, gross book value (2024 $M)	416.2	666.5	405.8	671.0
Institutions	210		151	
Institution-years	4,399		3,171	
Treatment cohorts	6		—	

Notes: All dollar figures are in constant 2024 dollars, deflated by the annual-average CPI-U. Panel A covers the affiliated 501(c)(3) foundations of public institutions, at the level of the foundation lineage (see text for details); contributions are Form 990 Part VIII line 1h. Panel B covers public institutions (2- and 4-year) reporting a positive endowment in at least 18 of the 21 panel years and holding a median 2004–2010 endowment of at least $1 million, in Explicit or None states only, excluding states with pre-panel transparency laws. The two panels describe overlapping but distinct populations and different time periods; the difference in treatment cohorts comes from New Mexico's 2023 law, which falls inside the IPEDS data but outside the Form 990 data.

Table 2: Effect of Transparency Laws on University and Foundation Finances

	ATT	(SE)	MDE	WCB 95% CI	RI p
Panel A: Foundation lineages (Form 990, FY1990–2021)					
Contributions received	0.059	(0.124)	41%	[−10, +6]	0.50
Panel B: Institutions (IPEDS, 2004–2024)					
Endowment Value	0.071	(0.091)	29%	[−6, +15]	0.58
Research Expenses	0.174	(0.103)	34%	[−7, +91]	0.26
Scholarship Expenses	0.008	(0.174)	63%	[−17, +26]	0.95
Instructional Expenses	0.046	(0.038)	11%	[−3, +7]	0.35
Net Price (in-state)	−0.030	(0.033)	10%	[−9, +2]	0.53
Buildings (book value)	−0.015	(0.042)	12%	[−8, +9]	0.79

Notes: Each row reports the ATT from a separate Callaway and Sant'Anna (2021) DiD estimation on public institutions, with the state-clustered standard error in parentheses. All outcomes are in logs. MDE is the effect detectable with 80% power at 5% size. The last two columns report a 95% confidence interval from a wild-cluster bootstrap over state clusters (Cameron et al., 2008), computed from a stacked event study with a $t \in [-5, +10]$ window, and a randomization-inference p-value from 999 placebo reassignments of the observed adoption history across sample states (MacKinnon & Webb, 2020). See Section 2.4 for sample construction details. Panel A reports donations received by the affiliated foundations from IRS Form 990 Part VIII line 1h; Panel B the institution outcomes, led by the endowment stock. The research-expense row is restricted to institutions with median annual research expenditure ≥ $1 million. Buildings is the gross book value of buildings (GASB form F1A) and net price is the average in-state price paid by Title IV aid recipients (reported from 2009).

Table 3: Robustness of Foundation Contribution Results

	ATT	(SE)	p	MDE	Treated
Primary specification	+0.059	(0.124)	0.64	41%	405
Panel A: Treatment definition					
Treatment dated one fiscal year later	+0.010	(0.087)	0.91	28%	405
Panel B: Sample					
Including grant-administering entities	+0.033	(0.135)	0.81	46%	417
General-purpose foundations only	+0.026	(0.142)	0.85	49%	378
Near-balanced (≥28 of 32 years)	+0.107	(0.114)	0.35	38%	184
Dropping five largest windfall lineages	+0.034	(0.126)	0.79	42%	401
Common sample with IPEDS	+0.049	(0.173)	0.78	62%	216
State × year aggregate (states)	+0.148	(0.173)	0.39	62%	8
Panel C: Outcome definition					
asinh, zero-contribution years retained	+0.500	(0.266)	0.06	111%	409
Schedule A five-year gifts (2010+)	+0.032	(0.616)	0.96	462%	338
Panel D: Inference					
Lineage-clustered standard error	+0.059	(0.080)	0.46	25%	405
Stacked event study, state CRVE	−0.020	(0.038)	0.61	11%	395
Stacked, wild-cluster bootstrap	−0.020	—	0.62	[−10, +6]%	395

Notes: The outcome is log total contributions received (Form 990, Part VIII, line 1h) at the foundation-lineage level. Each row is a separate Callaway and Sant'Anna (2021) estimation. Standard errors are clustered on the state whose transparency law binds the foundation. MDE is the effect detectable with 80% power at 5% size. In Panel C, the asinh row retains zero-contribution observations, which the log specification drops (1.8 percent of observations). In Panel D, the stacked specification restricts attention to a $[-5, +10]$ window and imposes a common effect across cohorts.

Table 4: Robustness of Selected Institutional Results

Specification	Endowment Value	Scholarship Expenses	Instructional Expenses	Net Price (in-state)
Primary (Explicit vs None)	0.071 (0.092)	0.008 (0.163)	0.046 (0.036)	−0.030 (0.031)
Panel A: Treatment and control definitions				
Explicit+Direct vs None+Unknown	0.100 (0.085)	−0.033 (0.160)	0.033 (0.033)	−0.011 (0.036)
Explicit+Direct vs None	0.096 (0.089)	−0.028 (0.197)	0.035 (0.038)	−0.006 (0.044)
Explicit vs None+Unknown	0.090 (0.089)	−0.027 (0.145)	0.033 (0.030)	−0.020 (0.017)
Include always-treated states as controls	0.083 (0.106)	−0.019 (0.172)	0.036 (0.038)	−0.018 (0.022)
Panel B: Sample				
All 21 reporting years required	0.057 (0.107)	−0.000 (0.177)	0.049 (0.040)	−0.044 (0.034)
No reporting requirement	0.111 (0.104)	0.037 (0.168)	0.049 (0.043)	−0.026 (0.035)
Size floor on whole-panel median	0.058 (0.109)	0.013 (0.140)	0.041 (0.034)	−0.052 (0.038)
Size floor at $500K	0.069 (0.080)	0.011 (0.147)	0.038 (0.035)	−0.048 (0.033)
Size floor at $5M	0.143 (0.127)	0.071 (0.141)	0.068 (0.041)	0.014 (0.041)
Size floor at $10M	0.170 (0.131)	−0.010 (0.154)	0.050 (0.051)	0.001 (0.048)
Balanced panel, no size floor	−0.005 (0.107)	−0.010 (0.196)	0.033 (0.045)	−0.064 (0.038)
4-year institutions only	−0.055 (0.224)	0.080 (0.162)	0.043 (0.052)	−0.011 (0.035)
Common sample with Form 990	0.062 (0.094)	0.012 (0.162)	0.053 (0.036)	−0.025 (0.036)
Panel C: Covariates				
With covariates (enrollment, tuition)	0.074 (0.101)	−0.003 (0.206)	0.060 (0.040)	−0.030 (0.038)
Panel D: Placebo				
Private nonprofit institutions	0.016 (0.126)	0.162 (0.398)	0.018 (0.169)	—

Notes: Each cell reports the overall ATT from a separate Callaway and Sant'Anna (2021) estimation, with the state-clustered standard error in parentheses. All outcomes are in logs. The top row reproduces the primary specification from Table 2. The in-state net price measure has no observations for private nonprofit institutions.

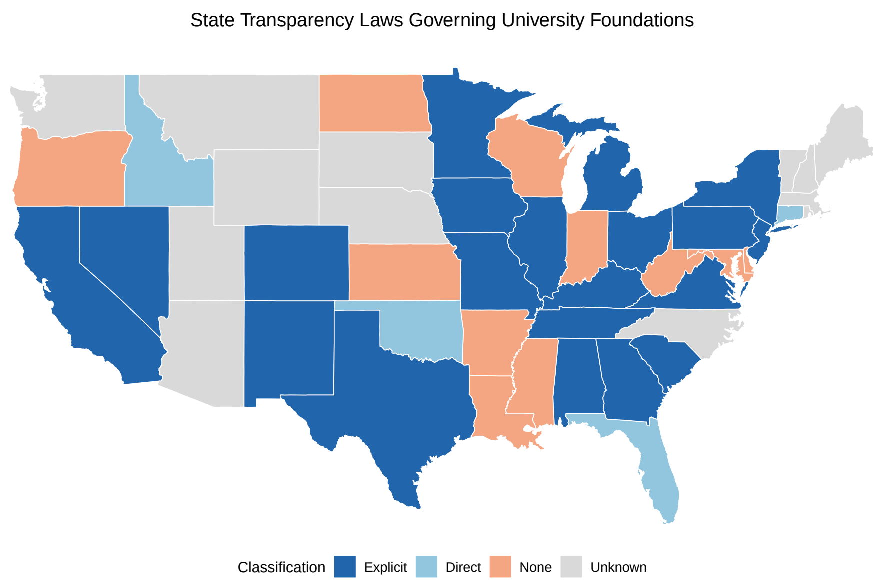

Figure 1: State Transparency Law Classifications

Notes: States are classified based on whether their public records laws apply to university-affiliated foundations. “Explicit” states require foundation compliance with public records requests. “Direct” states subject foundations to government scrutiny without full public records access. “None” states explicitly shield foundations. “Unknown” states have unsettled or ambiguous legal status.

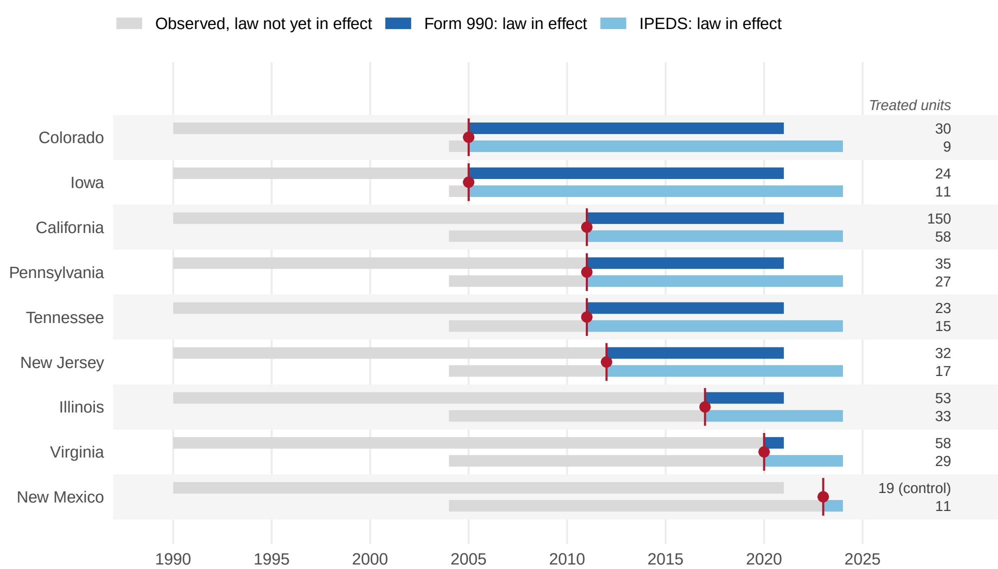

Figure 2: Rollout of State Transparency Laws

Notes: Each row is a state whose transparency law takes effect during at least one of the two analysis panels. The red rule marks the year the law takes effect; it is drawn once per state because the adoption date is a property of the state and is common to both data sources. Nested within each row are the two observation windows, which is where the data sources differ: the upper bar is the Form 990 foundation panel (FY1990–2021) and the lower bar the IPEDS institution panel (2004–2024). Grey marks years that are observed but precede the law; colour marks the years in which the law is in force. The numbers at the right are the treated units the state contributes to each panel, aligned with the corresponding bar. Nine states adopt during the study period, forming six treatment cohorts in the IPEDS panel but only five in the Form 990 panel. New Mexico shows why: its 2023 rule falls to the right of the end of its Form 990 bar, so its lineages are never treated within that window and enter as controls there, while the same law leaves a single treated year in IPEDS. The never-treated comparison group comprises 151 institutions and 261 foundation lineages, the latter including New Mexico's 19.

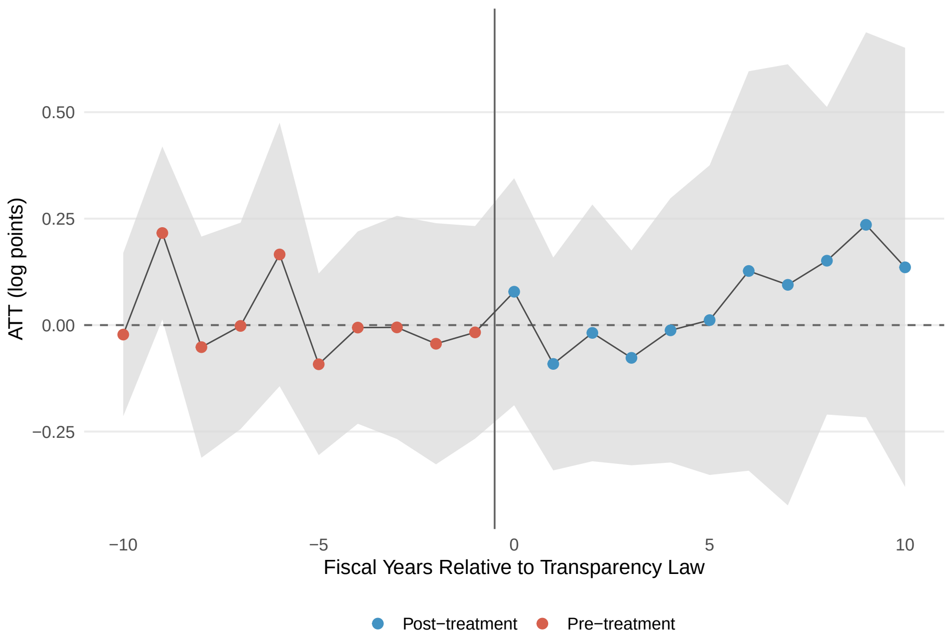

Figure 3: Event Study: Effect of Transparency Laws on Foundation Contributions

Notes: Dynamic treatment effect estimates from Callaway and Sant'Anna (2021) DiD on the foundation-lineage panel. The outcome is log total contributions received (Form 990, Part VIII, line 1h). Sample: 405 treated and 261 never-treated foundation lineages in Explicit vs. None states, excluding always-treated. Event time is measured in fiscal years relative to the year the law was enacted; because most lineages have June-30 fiscal year ends, fiscal year zero is largely pre-law. Shaded area shows the 95% confidence interval from state-clustered standard errors. The vertical line marks treatment onset. The display is trimmed to $[-10, +10]$, the same window as Figure 4, so the two event studies are read on comparable axes; outside that range only one or two small cohorts contribute. The estimation itself is not trimmed.

Figure 4: Event Study: Effect of Transparency Laws on Endowment Value

Notes: Dynamic treatment effect estimates from Callaway and Sant'Anna (2021) DiD. Sample: public institutions (2- and 4-year), Explicit vs. None states only, excluding always-treated. Shaded area shows 95% confidence interval. The vertical line marks treatment onset.

A Appendix: Linking Institutions to Their Affiliated Foundations

Our foundation outcomes are drawn from the Form 990 filings of the private, tax-exempt entities that raise and hold gifts on behalf of public institutions. The Form 990 files contain many education-related entities that are not institutional foundations, and in some specifications, we use a subset of observations that are limited to the IPEDS coverage. These two features of the analysis mean we require a crosswalk from the 'Unit ID's of each IPEDS institution to the Employer Identification Number or numbers of its affiliated foundations. No such crosswalk is published, and the mapping is not one-to-one. This appendix documents the challenges involved in creating such a crosswalk, the procedure we use, and what we can say about how well that procedure works.

A.1 Challenges in linking institutions and foundations

Not every institution has a separate foundation. Several large public universities hold their endowments on the institution's own books, through a governmental entity or a university-controlled investment function, rather than in a separately incorporated 501(c)(3). A few examples: the Regents of the University of Michigan hold that system's endowment as a governmental instrumentality; gifts to the academic campuses of the University of Texas System flow to the institutions, with the endowment pooled through UTIMCO; Pennsylvania State University is a governmental unit that files no standard Form 990, and the "Corporation for Penn State" is an assetless shell; gifts to the University of Missouri flagship campuses flow to the Curators of the University of Missouri. These institutions have no foundation observation at all.

Some institutions operate several vehicles at once. Many institutions maintain a general-purpose foundation alongside a real-estate foundation, a research foundation, an alumni association, a housing or student-life foundation, and, at some public HBCUs, a separate educational trust. Our procedure must find all organizations that support the operations of each institution.

A literal name match can lead to errors. Is the "ASU Foundation" associated with Alabama State University or Arizona State University? Indeed, at Alabama State University the "ASU Foundation" holds roughly $12 million while the "ASU Trust for Educational Excellence" holds roughly $113 million. We must be confident that we are associating foundations with the actual institutions that they support.

One foundation often serves many campuses. A single foundation frequently supports an entire system. The University of Illinois Foundation covers Urbana-Champaign, Chicago, and Springfield; the University System of Maryland Foundation, the Rutgers University Foundation, the Alamo Colleges Foundation, and the Kentucky Community and Technical College System Foundation each serve many campuses, and in some systems a campus foundation operates as a trade name of the system foundation with no EIN of its own. A single EIN therefore can map to many institutions. Table 5 reports that 177 of the 1,580 lineages entering our panel support more than one campus, the largest serving 35.

The mapping changes within the panel. Public systems consolidated campuses repeatedly throughout our study period: including the University System of Georgia's mergers, Pennsylvania's 2022 creation of Commonwealth University and Pennsylvania Western University, Connecticut's 2023 consolidation of its community colleges, the 2020 unification of the Dallas County community colleges, and numerous Louisiana technical-college mergers around 2012. Foundations also reorganize on their own schedule: Western Kentucky University's foundation merged into the College Heights Foundation effective 2023, and the University of Arkansas–Pulaski Technical College foundation was absorbed when the campus joined the UA System. When campuses merge, pre-merger foundations often persist, sometimes under old names and sometimes folded into a successor, so the correct match for a now-closed campus is frequently a foundation named for a different institution. Institutions and foundations also rename, and foundation legal names lag, with some entities still filing under former "Junior College" or "Technical College" names.

Observability varies across foundations. Small foundations file the Form 990-N e-postcard, which carries no financial detail; they appear in the IRS Business Master File but not in the financial-return files. Dissolved and merged foundations drop out of the current Business Master File while retaining a historical filing record. We record each matched EIN's coverage status: of the 1,691 EINs we verify, 1,511 file full returns through the end of the panel, 72 file only the e-postcard, and 96 appear only in historical returns through 2011.

A.2 Matching procedure

The crosswalk is built in three stages from the IRS Business Master File, the Form 990 files (both from NCCS), and IPEDS.

(a) Candidate generation. For each institution we draw a set of candidate foundations from the Business Master File, restricted to education-related 501(c)(3)s in the institution's state. Candidates are scored on a blend of name similarity—a Jaro–Winkler distance computed over an alias set that absorbs abbreviations, acronyms, former names, and campus qualifiers—together with the ratio of foundation assets to institutional endowment, Form 990 filing history, and same-city co-location, with penalties for entity types that are usually the wrong answer, such as alumni associations and medical, athletic, or research affiliates. A curated alias file supplies nicknames, such as the Capstone Foundation for the University of Alabama.

(b) LLM review. Since ultimately the crosswalk has a many-to-many structure, we must review each potential match. Because the review covers thousands of such potential matches and benefits from broad knowledge of institutional histories, we conduct it with the assistance of large language models. We provide Claude and ChatGPT with the candidate list for a single state at a time and prompt it with a set of decision rules: prefer the general-purpose institutional foundation over narrow affiliates; disambiguate same-named foundations of sibling campuses by city; assign the system foundation when a branch campus has none of its own; select the larger asset-holder when two vehicles exist; and bridge nicknames and former names. Each model is explicitly instructed that the candidate list may not be exhaustive and is asked to identify, from external public sources, foundations missing from it. Each review records an EIN and Unit ID for each match, whether the match came from the candidate list or from an external source, a confidence level, and a free-text note explaining any choices made.

We treat this output as a search-and-screening aid and never as an authority. Every EIN proposed, whether drawn from the candidate list or found externally, is independently verified against IRS administrative data: we confirm deterministically that the EIN exists in the Business Master File or has a Form 990 filing history, and we record its name, state, assets, and coverage window. Any EIN appearing in no IRS source is rejected as unverifiable.

(c) Consensus and human adjudication. Step (b) gives us two sets of matches. We compare the two sets and classify each as full agreement, partial agreement, genuine disagreement about the entity, or a disagreement about whether any foundation exists. Every case short of full agreement, along with any case flagged as low-confidence or unverifiable, is routed to human adjudication.

A.3 From EINs to foundation lineages

Because foundations merge, dissolve, and reincorporate within the panel, the EIN is not a stable identifier. A 2023 foundation merger recorded at the EIN level appears as a donation collapse at one identifier alongside a windfall at another, neither of which reflects donor behavior. We therefore aggregate EINs into *lineages*: multiple identifiers combined into a single synthetic entity that persists across the reorganization. Flows are summed across a lineage's member EINs within a fiscal year. The crosswalk yields 1,650 EINs organized into 1,637 lineages; 9 of those lineages are assembled from more than one EIN, absorbing 22 member identifiers between them.

Finally, not every affiliated 501(c)(3) is appropriate for testing the donor-flight hypothesis. We classify each lineage by type and exclude from the primary specification those whose contributions line is contaminated by government grant flows rather than private giving: research foundations, medical and hospital foundations, and the small number of institutions that file a Form 990 for themselves. This screen removes 23 lineages in the estimation frame; including them, or narrowing further to general-purpose foundations only, leaves the results unchanged.

A.4 Reliability and validation

The verification step lets us quantify the reliability of the assisted matching rather than assert it. Table 5 reports the full set of statistics; we summarize the four that matter most.

Agreement against an independent hand-verified reference. We hold a crosswalk of major public university foundations built by hand, without reference to the assisted review, covering 91 institutions. The consensus pick agrees with it for 88, or 96.7 percent, before any adjudication.

Cross-model agreement. On all 2,253 institutions, the two models select the same matches for 92.8 percent. The residual disagreements are concentrated in exactly the hard cases catalogued above: 127 are disagreements over whether a foundation exists at all, which is the recurring question of whether a branch campus should inherit its system's foundation, and only 35 are disagreements over which of two real entities is correct.

Fabricated identifiers, and why consensus is not enough. Twelve of the 1,691 proposed EINs, 0.71 percent, correspond to no organization in any IRS source. All twelve were caught by the verification step. Two of them were nominated *independently by both models* for the same institution from a common erroneous web source. This is the central methodological point of the design: a procedure that accepted cross-model consensus as sufficient would have admitted those two, and only verification against administrative data rejects them.

Structural no-matches. Of the 2,253 institutions in the frame, 355—15.8 percent—match to no foundation. These are predominantly the governmental-endowment flagships discussed above and non-degree vocational, technical, and nursing units with no institutional foundation. Both independent reviews agree on the great majority of them, which is the pattern one expects if these are correct exclusions rather than search failures.

Taken together, 97.2 percent of the 2,221 edges in the final crosswalk come from a combination of the LLM model outputs and our deterministic triage scripts. 2.8 percent of the edges come from a human reviewer's decision.

Table 5: Reliability of the Institution–Foundation Crosswalk

	Count	Share (%)
Panel A: Scale of the review		
Institutions in the matching frame	2,253	—
Distinct EINs proposed across all reviews	1,691	—
Reviews performed	4,506	—
Panel B: Agreement, before human adjudication		
Two models select the same match	2,091	92.8
Disagree: different entities	35	1.6
Disagree: one finds a foundation, one does not	127	5.6
Institutions with a hand-verified reference match	91	—
Consensus agrees with hand-verified reference	88	96.7
Flagged for human adjudication	174	7.7
Panel C: EIN verification against IRS records		
Distinct EINs submitted to verification	1,691	—
Found in IRS records (BMF or 990 filing history)	1,679	99.3
Found in no IRS source (fabricated identifier)	12	0.7
Present in the current Business Master File	1,647	97.4
Files Form 990 through FY2021/22	1,511	89.4
Files through 2011 only (dissolved/merged)	96	5.7
In BMF, files 990-N only (no full return)	72	4.3
Institutions where both models proposed the same unverifiable EIN	2	—
Panel D: Final crosswalk, after adjudication		
Institutions matched to at least one foundation	1,898	84.2
Institutions with no affiliated foundation	355	15.8
Institution-EIN edges	2,221	—
Distinct foundation EINs in the crosswalk	1,650	—
Foundation lineages	1,637	—
Lineages assembled from multiple EINs (successor groups)	9	0.5
Member EINs in those successor groups	22	—
Lineages with usable Form 990 data (the analysis panel)	1,580	96.5
Analysis lineages serving more than one campus	177	11.2
Edge from model review or automated triage	2,158	97.2
Edge decided by a human reviewer	63	2.8

Notes: Panels A–C describe the automated matching stages before the human adjudication of flagged cases; Panel D describes the crosswalk that enters the analysis, after adjudication. An agreement rate computed after adjudication would be circular. Panel B shares, and the first two rows of Panel D, are of the 2,253 institutions in the matching frame, every one of which was reviewed independently by both models; the exception is Panel B's two reference-set rows, whose denominator is the 91 institutions covered by the hand-verified crosswalk. Two models are counted as selecting the same match when both name the same foundation, when both conclude that no foundation exists, and when both nominate the same pair of vehicles for an institution that runs two; the last case is not separated out because the analysis sums an institution's vehicles regardless of which was nominated first. Panel C shares are of the 1,691 distinct EINs submitted to verification. In the rest of Panel D, the lineage rows are shares of the 1,637 crosswalk lineages, the multi-campus row of the 1,580 lineages with usable Form 990 data, and the edge-source rows of the 2,221 institution–EIN edges. An EIN counts as verified if it appears in the IRS Business Master File or has a Form 990 filing history.

B Appendix: Event Studies for Secondary Outcomes

Figure 5 displays event-study estimates for the four secondary institutional outcomes—research expenditures, scholarship expenditures, instructional expenditures, and in-state net price—analogous to Figure 4 for the primary endowment outcome. Pre-treatment estimates fluctuate around zero with no systematic trend in any of the four, supporting the parallel trends assumption across the board. Post-treatment estimates for scholarships and net price are centered on zero. Research and instructional expenditures both drift positive at longer horizons; in both cases the confidence band widens with the drift and contains zero throughout.

Figure 5: Event Studies: Secondary Outcomes

Notes: Dynamic treatment effect estimates from Callaway and Sant'Anna (2021) DiD. Sample: public institutions (2- and 4-year), preferred sample definition, Explicit vs. None states only, excluding always-treated. Outcomes are in log points. Shaded areas show 95% confidence intervals. Vertical lines mark treatment timing.

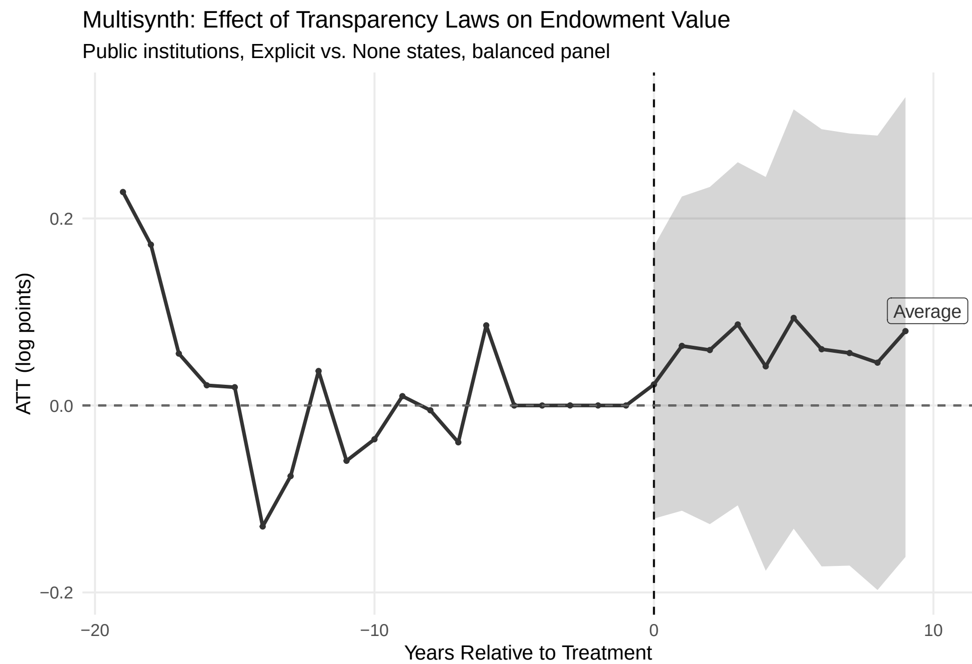

Figure 6: Augmented Synthetic Control: Effect of Transparency Laws on Endowment Value
Notes: Multisynth estimates from Ben-Michael et al. (2021). Sample: public institutions (2- and 4-year), balanced panel, Explicit and None states only, excluding always-treated, cohorts treated 2011 or later. Shaded area shows 95% confidence interval.

C Appendix: Randomization Inference

With nine treated states, the standard errors in Table 2 rest on an approximation that is unreliable in the few-clusters regime. Figure 7 reports a procedure that makes no asymptotic appeal at all: holding the observed set of adoption years fixed, we randomly reassign which sample states receive which adoption year and re-estimate the headline specification, 999 times per outcome. The actual estimate falls unremarkably inside its own placebo distribution for every outcome.

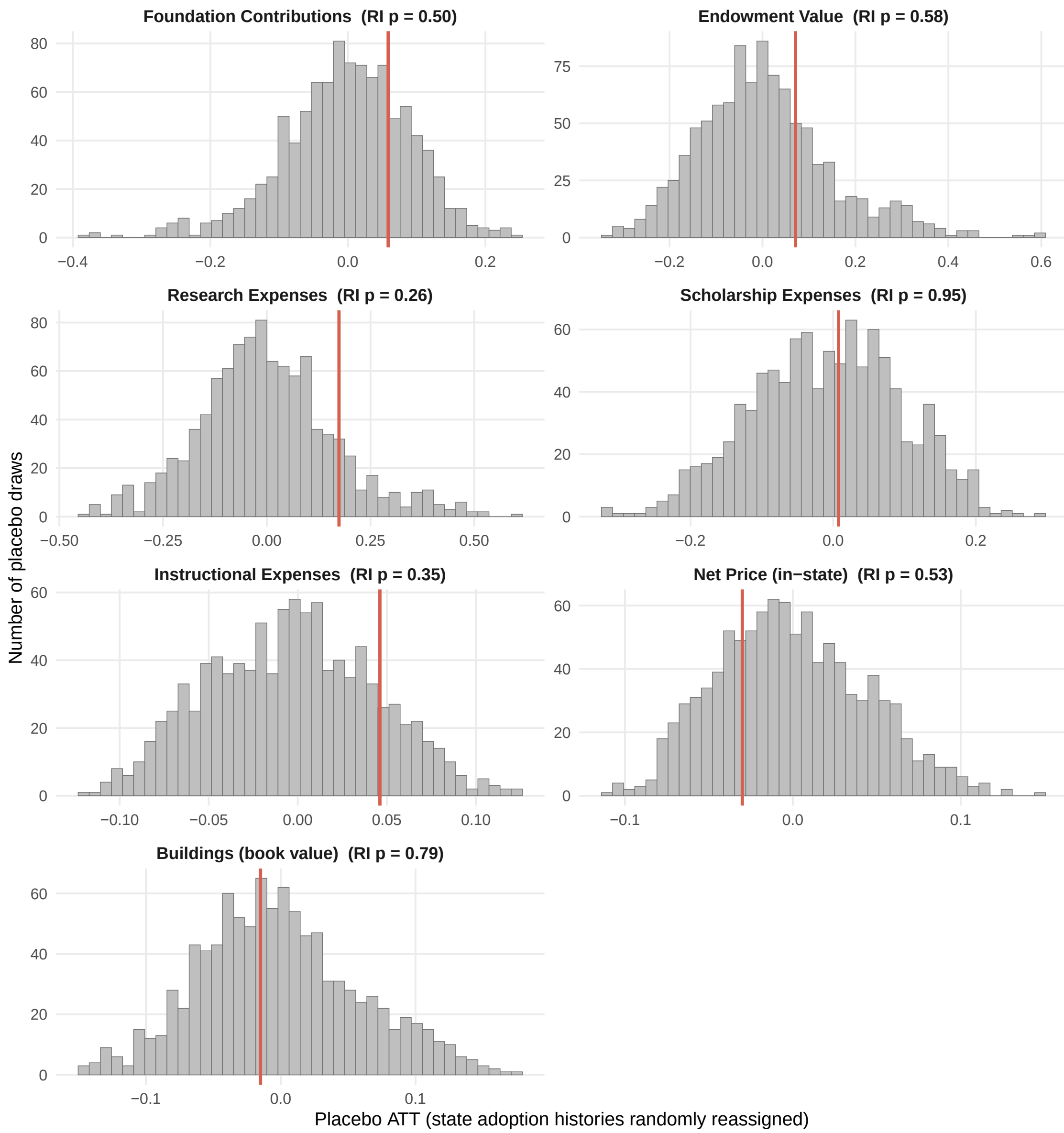

Figure 7: Randomization Inference: Actual Estimates against Placebo Distributions

Notes: Each panel shows the distribution of 999 placebo average treatment effects. Each placebo draw holds the observed multiset of adoption years fixed and randomly reassigns which of the sample states receives which adoption year, then re-estimates the headline Callaway and Sant'Anna (2021) specification for that outcome. The red line marks the estimate from the actual assignment. The randomization-inference p-value in each panel heading is the share of placebo draws whose absolute effect exceeds the absolute actual effect (MacKinnon & Webb, 2020). Draws are common across outcomes.