Who Pays Attention?

Women in State Leadership, Partisanship, and School Finance, 1993–2024

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Abstract

Does the gender of state political leaders shape how states finance public schools? I build an annual index of state school finance quality for the 48 contiguous states, 1993–2024. It combines fiscal effort, progressiveness (equity) and wage-adjusted resources, drawn from the School Finance Indicators Database (SFID). I link it to the share of state legislative seats held by women (Center for American Women and Politics) and to governors' gender and party. Following the design of Oberfield and Baker (2022), I estimate two-way fixed-effects models with lagged ideology, demographic, court-reform and charter-sector controls. I add state-specific trends, annual-change models, placebo leads and a stacked event study of 182 gubernatorial transitions, and then test the estimates with sequential g-estimation, heterogeneity-robust difference-in-differences and sensitivity analysis for unobserved confounding. A second analysis covers 2009–2024 with an index that combines effort, equal opportunity and adequacy for all students and for those in high-poverty districts.

A 10-point rise in women's legislative share goes with a 0.065 SD higher relative index ($p < .05$), about a quarter of a typical within-state swing. That association runs mainly through effort, shrinks once demographic controls are added, and does not appear in annual changes. Over 2009–2024 the association with the adequacy-based index is about twice as large (0.12 SD, $p = .01$), holds with demographic controls and state trends, and runs through high-poverty adequacy and equal opportunity rather than overall adequacy; it too reflects cumulative rather than year-to-year change. Women governors show no reliable association in either period: levels and changes carry opposite signs, and the event study is flat before and after transitions to a woman. Republican governors are associated with lower index values (about −0.04 to −0.05 SD), concentrated in effort. Caughey and Warshaw's policy liberalism series (1937–2024) shows that both Republican control and women's legislative share move state policy liberalism, so part of each association runs through ideology. Causal-design checks qualify these associations. Holding policy liberalism fixed by sequential g-estimation leaves the 2009–2024 women's-share estimate essentially unchanged (0.12 SD), and an omitted confounder would need to be stronger than policy liberalism to erase it. But in a clean-control event study, large jumps in women's legislative share are not followed by gains in school finance; the index rises in the years before them. Heterogeneity-robust estimates confirm that women governors have no effect, while estimates for party become imprecise. Since 2009, states with more women legislators have funded high-poverty districts better, but these data do not show that electing more women caused the improvement.

Keywords: school finance; descriptive representation; women legislators; governors; fiscal effort; progressivity; policy liberalism
JEL codes: H75, I22, D72, J16

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1. Introduction

Public schooling is the largest single item in most state budgets, and in every state it rests on a constitutional education clause. How generously and how fairly states fund it is therefore a direct test of state political leadership. Over the last three decades that leadership has changed in a measurable way. Women held about 21% of state legislative seats in 1994 and about 34% by 2024, and the number of states with a woman governor rose from three to twelve.

A long literature argues that descriptive representation changes policy. Women legislators have been found to prioritize bills on children, families and education (Thomas, 1991; Swers, 2002). Outside the United States, quasi-experimental studies link women officeholders to more spending on child care and local public goods (Bratton & Ray, 2002; Chattopadhyay & Duflo, 2004; Clots-Figueras, 2011). Close-election studies of U.S. mayors, however, find little effect of gender on fiscal outcomes (Ferreira & Gyourko, 2014). Whether women in state office move school finance, the largest education lever states control, has not been tested with long-run fiscal data.

This paper asks whether, and by how much, gender balance in state legislatures and governors' gender are associated with improvement or decline in state school finance systems. It makes three contributions:

1. **A long, consistent outcome.** I construct an index of effort, equity and resources for 48 states from 1993 to 2024, using SFID measures that are comparable across states and years.
2. **New panel data on women legislators.** I assemble annual counts of women state legislators by state from the CAWP officeholder database and pair them with governors' gender and party.
3. **A design that takes the politics seriously.** I follow Oberfield and Baker (2022), who model the progressivity of state school funding as a function of ideology, court-ordered reform and demographic change, and add gender to that model. I then probe it with trends, changes, placebos and an event study, and test the policy liberalism control itself. Finally, I apply designs that relax the fixed-effects assumptions: sequential g-estimation of direct effects, heterogeneity-robust difference-in-differences, and sensitivity analysis for unobserved confounding.

The answer depends on the outcome and the period. Over 1993–2024, women's legislative share has a small, fragile positive association with the index. Over 2009–2024, when the index includes adequacy and equal opportunity, the association is nearly twice as large, survives demographic controls and state trends, and

is concentrated in high-poverty districts. Governor gender has no association in either period. Governors' party is the steadier signal for effort. Part of both the gender and the party associations runs through the broader liberalism of state policy. The causal-design checks set limits on interpretation. The 2009–2024 association holds when ideology is handled as a mediator and would take a strong omitted confounder to erase, but it does not appear after large jumps in women's seats, and school finance tends to improve before them. I therefore read the findings as robust associations rather than causal effects.

2. Literature and hypotheses

2.1 The politics of school finance progressivity

Most research on school finance reform asks whether reforms work. Court-ordered reforms raised spending in poorer districts (Murray, Evans & Schwab, 1998; Card & Payne, 2002), and the added money improved outcomes for low-income students (Jackson, Johnson & Persico, 2016; Lafortune, Rothstein & Schanzenbach, 2018). Fewer studies ask why states adopt and sustain progressive systems in the first place.

Oberfield and Baker (2022) take up that question with a state panel for 1995–2016. They test three explanations for changes in progressivity: court-ordered reform, the ideology and party of state government, and demographic change. Evidence that progressivity rose after court-ordered overhauls was mixed. The most consistent predictor was demographic: as students became poorer or more racially diverse, states built less progressive systems. That result ties school finance to work on racial heterogeneity and public goods (Alesina, Baqir & Easterly, 1999) and on the generational and racial gap between older voters and school-age children (Poterba, 1997).

Their measure of ideology is the policy liberalism index of Caughey and Warshaw (2016), a dynamic latent measure of how liberal a state's enacted policies are, now available for 1937–2024 (Caughey & Warshaw, 2022). Partisan control moves that index only incrementally (Caughey, Warshaw & Xu, 2017). This matters for the present design: policy liberalism is partly an outcome of who governs, not only a background condition, so controlling for it can absorb part of a party or gender effect. Section 7 tests this directly.

Gender does not appear in that framework. This paper adds it, keeping Oberfield and Baker's controls so that any gender association must hold net of ideology, courts and demographic change.

2.2 Women in office and public spending

Evidence that women's political power changes public spending runs from suffrage to modern legislatures. Women's suffrage expanded the size and scope of state government (Lott & Kenny, 1999) and redirected local

public health spending toward children, reducing child mortality (Miller, 2008). In legislatures, women members have given priority to bills on children, families and education (Thomas, 1991; Swers, 2002). Quasi-experimental studies abroad link women officeholders to more spending on child care, education and local public goods (Bratton & Ray, 2002; Chattopadhyay & Duflo, 2004; Svaleryd, 2009; Clots-Figueras, 2011).

Two qualifications shape expectations for American states. First, influence may depend on numbers. Critical-mass arguments hold that women's distinct priorities emerge only as they move from token presence to a substantial minority (Kanter, 1977; Dahlerup, 1988), although Bratton (2005) finds that even token women in state legislatures pursued distinctive agendas. Second, party conditions gender. Osborn (2012) shows that women legislators represent women's interests largely through their parties, so Democratic and Republican women act differently. Close-election evidence from U.S. mayors, where party and gender can be separated cleanly, finds little gender effect on spending (Ferreira & Gyourko, 2014).

2.3 Lessons from Kansas

Kansas shows how these forces can combine in a conservative state. Baker (2021) describes how Kansas built, and repeatedly defended, a stable and equitable school finance system through bipartisan coalitions, and devotes Chapter 6, "Women on the High Plains," to the women of Kansas who shaped that history.

The public record shows the pattern:

- **Joan Finney** was governor when the legislature enacted the 1992 School District Finance and Quality Performance Act, the foundation of the modern Kansas formula.
- **Rochelle Chronister**, the first woman in Kansas House leadership (assistant majority leader, 1983–1986), later led moderate Republicans opposing Governor Sam Brownback's cuts to school budgets.
- **Kathleen Sebelius** was governor during the *Montoy* litigation and the funding increases that followed.
- In **2016**, moderate challengers in suburban Johnson County made school funding a central issue in Republican primaries (Reuters, 2016).
- In late **2018**, three suburban Republican women legislators, **Stephanie Clayton, Dinah Sykes and Barbara Bollier**, left the party. Clayton cited party leaders' move to rewrite a school finance bill that moderates had passed with Democrats (NPR, 2019).
- **Laura Kelly**, a former state senator, became governor in 2019.

The data used here track that history. Women held 28.5% of Kansas legislative seats in 1993, against a national average of 20.8%, and 33.3% in 2000. Their share fell to 24.2% in 2016, below the national average,

before recovering after the 2016 elections. Kansas ranked 12th of 48 on the 1993–2024 index in 1996, 28th in 2012, and 8th in 2024. High-poverty adequacy fell from 1.06 in 2009 to 0.72 in 2019.

Kansas suggests a refinement of the national question. Women legislators may matter most where they anchor cross-party coalitions in conservative states, rather than through partisanship as such. If so, the association between women's share and school finance should be stronger in more conservative states.

2.4 Outcomes and hypotheses

A state can raise spending without targeting it, or target it while spending little. Effort captures how much of the economy a state devotes to schools; equity and equal opportunity capture who gets the money; resources and adequacy capture what it buys relative to need. Gender and party could plausibly move these differently, so each domain is tested on its own as well as in the combined indexes.

The hypotheses are:

- **H1.** A higher share of women in the legislature is associated with higher effort, equity and adequacy.
- **H2.** A woman governor is associated with improvement in the index during her tenure.
- **H3.** These associations hold net of governors' party, state ideology, court-ordered reform and demographic change.
- **H4 (placebo).** Future legislative composition does not predict current finance outcomes.
- **H5 (Kansas).** The association with women's legislative share is stronger in more conservative states.
- **H6 (ideology).** Estimates for gender and party do not depend on how policy liberalism is measured or whether it is controlled.

3. Data

The panel covers the 48 contiguous states from 1993 to 2024 (1,536 state-years). Alaska and Hawaii are excluded because their cost structures and single-district (Hawaii) systems make the wage and progressivity measures hard to compare.

3.1 School finance index

All finance measures come from the SFID state panel (Baker, Di Carlo & Weber, annual). I group eight measures into three domains (Table 1).

Table 1. Index components

Domain	Measure	SFID variable	Years
Effort	State and local education spending as share of gross state product	gsp_effort	1993–2024
Effort	State and local revenue as share of personal income	inc_effort	1993–2024
Equity	Progressiveness ratio, state and local revenue per pupil	fairness	1993–2024
Equity	Progressiveness ratio, current spending per pupil	fairness_curexpp	1993–2024
Equity	Progressiveness ratio, teachers per 100 pupils	fairness_tchph	1994–2024
Resources	Current spending per pupil, adjusted for regional wages (ECWI)	adj_ppcstot	1993–2024
Resources	Predicted teachers per 100 pupils at 10% poverty	predicted_tchph10_	1994–2024
Resources	Teacher salary parity with non-teachers (mean of ages 25, 35, 45, 55)	sal_parity25–55	2000–2024

Each measure is standardized two ways. The **relative** version is a z-score within each year, so a state's value says where it stands among states that year. The **absolute** version is a z-score pooled over all state-years, so movement over time is preserved. A domain score is the mean of its available standardized measures, and the index is the mean of the three domain scores. Before 2000 the resources domain has no wage measure, and before 1994 it also lacks staffing; the year fixed effects absorb the resulting shift in composition. SFID's adequacy measures, which begin later, are not used.

The relative index has a pooled standard deviation of 0.59 and a within-state standard deviation of 0.23. The absolute index has 0.58 and 0.28. In 2024 the highest-ranked states were Wyoming, Vermont, New Jersey, New York and Minnesota; the lowest were Tennessee, North Carolina, Arizona, Florida and Nevada.

3.2 Women in state legislatures

I count women serving in each state legislature in each year from the Center for American Women and Politics (CAWP) Women Elected Officials Database, querying all state legislative officeholders year by year. The share of women is that count over total seats in both chambers, with seat totals adjusted for chamber-size changes (North Dakota and Rhode Island in 2003; New York in 2003 and 2013). The series runs from 20.8% in 1994 to 33.5% in 2023, closely matching CAWP's published national figures. Within-state variation is substantial: the within-state standard deviation is 4.5 percentage points.

Two cautions apply. The database counts anyone who served at any point in the year, which slightly overstates mid-year composition. It also returns identical national totals for 1993, 1994 and 1995, which suggests coarser coding in those years; I report models starting in 1997 to address this.

3.3 Governors

Governors' names, parties and terms come from Kaplan's (2021) governors dataset, extended by hand through 2023 and corrected for December inaugurations (Kentucky, North Dakota) and mid-year transitions. Thirty-six women served as governor in the sample, covering 172 state-years. The share of states with a woman

governor rose from 6% in 1994 to 25% in 2024.

3.4 Timing and controls

SFID fiscal data for year *t* reflect the budget adopted in the prior year. Each fiscal year is therefore matched to the governor in office on July 1 of year $t-1$ and to the legislature serving in year $t-1$. Controls, lagged one year, follow Oberfield and Baker (2022):

- **Ideology:** state policy liberalism (Caughey & Warshaw, 2016, 2022), the median posterior estimate from the authors' 1937–2024 series, reversed so that higher values are more liberal.
- **Demographics:** the Black and Hispanic share of children aged 5–17 (available from 2000).
- **Public-sector coverage:** the share of children aged 6–16 enrolled in public school (from 2000).
- **Charter sector:** the statewide share of students in charter schools.
- **Courts:** the cumulative count of court-ordered school finance reform events.

4. Empirical strategy

4.1 Two-way fixed effects

The baseline model, for state *s* in fiscal year *t*, is:

$$Y_{st} = \beta_1 W_{s,t-1} + \beta_2 F_{s,t-1} + \beta_3 R_{s,t-1} + X'_{s,t-1}\gamma + \alpha_s + \tau_t + \varepsilon_{st}$$

Y is the index or one of its domains. *W* is women's share of legislative seats, in units of 10 percentage points. *F* and *R* indicate a woman governor and a Republican governor. *X* holds the lagged controls. State fixed effects remove stable differences such as constitutional language and political culture; year fixed effects remove national shocks such as recessions and federal stimulus. Standard errors are clustered by state.

Identification rests on within-state changes in leadership being unrelated to unobserved, time-varying shocks to school finance once controls are included. That assumption is strong. Voters who elect more women may also want more school spending, and a fiscal crisis may change who runs for office. The robustness checks below are built to probe it, and Section 8 applies designs that relax it.

4.2 Robustness

Robustness specifications

Spec	Change from baseline	What it addresses
A	Baseline, 1994–2024; ideology, charter and court controls	Longest sample
B	Add demographics and public enrollment coverage, 2001–2024	Oberfield & Baker's main confounder
C	Add a linear trend for each state	Slow-moving state trajectories that track both gender and finance
D	Start in 1997	Coarse CAWP coding for 1993–1995
E	Outcome is the annual change in the index	Whether leadership predicts movement, not just levels
F	4-year average of women's share	Cumulative exposure, smoothing election timing
G	Add women's share two years ahead	Placebo: future legislatures should not predict current budgets
H	Interact women's share with a woman governor	Complementarity between branches

4.3 Event study of gubernatorial transitions

Governor changes are discrete, so they allow a sharper design. I identify 182 changes of governor taking office between 1997 and 2021; 27 brought a woman into office, 24 of them succeeding a man. For each transition I take a window from three years before to four years after the new governor's first budget. I drop years after a further change of governor. The outcome is the change in the index relative to the year before the transition.

I regress that change on event-time indicators interacted with a woman successor. Event-time interactions with the incoming and outgoing governors' party, plus calendar-year fixed effects, are included. Comparing transitions to a woman against transitions to a man in the same event time removes the common effect of any change of administration. Flat pre-transition coefficients support the comparison; the post-transition coefficients estimate the effect of a woman governor's first four budgets.

5. Results

Across specifications, women's legislative share has a small, fragile positive association with the index, governor gender has none that is robust, and Republican governors are associated with lower values. Cells report coefficients with clustered standard errors in parentheses; * p<.10, ** p<.05, *** p<.01. Domain outcomes use the absolute (pooled) standardization. All models control for policy liberalism using Caughey and Warshaw's 1937–2024 series (Section 7).

5.1 Women in the legislature

Table 2. Women's legislative share (per 10 percentage points)

Spec	Index (rel.)	Index (abs.)	Effort	Equity	Resources	N
A Baseline, 1994+	0.065** (0.033)	0.037 (0.039)	0.140* (0.082)	−0.046 (0.058)	0.018 (0.048)	1,488
B + demographics, 2001+	0.023 (0.027)	−0.006 (0.030)	0.012 (0.061)	−0.059 (0.069)	0.028 (0.045)	1,152

Spec	Index (rel.)	Index (abs.)	Effort	Equity	Resources	N
C + state trends	0.043 (0.027)	0.027 (0.029)	0.015 (0.050)	0.008 (0.051)	0.058 (0.046)	1,488
D 1997+	0.039 (0.031)	0.006 (0.035)	0.055 (0.075)	−0.063 (0.063)	0.027 (0.044)	1,344
E Annual change	0.005 (0.014)	−0.006 (0.014)	−0.020 (0.021)	0.003 (0.025)	0.000 (0.012)	1,488
F 4-year average	0.055 (0.041)	0.012 (0.046)	0.112 (0.090)	−0.101 (0.098)	0.026 (0.058)	1,344
G Placebo: 2 yrs ahead	−0.026 (0.041)	−0.034 (0.046)	−0.007 (0.078)	−0.092 (0.064)	−0.005 (0.041)	1,392
H × woman governor	−0.083 (0.053)	−0.060 (0.058)	−0.036 (0.097)	−0.163 (0.115)	0.018 (0.049)	1,488

In the baseline, a 10-point higher share of women goes with a 0.065 SD higher relative index (p = .048). That is about 0.28 of the index's within-state standard deviation (0.23). The national rise of roughly 13 points since 1994 would imply about 0.08 SD. The association runs through effort (0.140, p = .09); equity coefficients are small and negative.

The association does not hold up well. Adding the demographic controls that Oberfield and Baker found most important cuts it by about two-thirds, and it loses significance with state trends or a 1997 start. In annual changes it is zero. The placebo passes: legislatures two years ahead do not predict current outcomes, and the contemporaneous coefficient rises to 0.084 (p < .01) in that model. The two branches are not complements: the interaction with a woman governor is negative and insignificant. H1 receives weak support in levels only. Section 7 shows that part of the total association runs through policy liberalism; without the ideology control the baseline estimate is 0.090.

5.2 Governors' gender and party

Table 3. Woman governor and Republican governor (indicators)

Spec	Term	Index (rel.)	Index (abs.)	Effort	Equity	Resources
A Baseline	Woman	−0.057 (0.037)	−0.065* (0.038)	−0.078 (0.076)	−0.020 (0.071)	−0.096** (0.042)
	Republican	−0.041 (0.026)	−0.044* (0.025)	−0.042 (0.058)	−0.030 (0.048)	−0.060* (0.031)
B + demog., 2001+	Woman	−0.034 (0.036)	−0.039 (0.037)	−0.027 (0.067)	0.004 (0.083)	−0.094** (0.042)
	Republican	−0.052** (0.024)	−0.054** (0.024)	−0.089** (0.036)	−0.028 (0.053)	−0.045 (0.034)
C + state trends	Woman	−0.048 (0.031)	−0.052* (0.031)	−0.069 (0.060)	−0.017 (0.065)	−0.070 (0.043)
	Republican	−0.033 (0.021)	−0.028 (0.020)	−0.022 (0.031)	−0.022 (0.039)	−0.042 (0.028)
D 1997+	Woman	−0.059 (0.037)	−0.068* (0.039)	−0.073 (0.076)	−0.017 (0.080)	−0.114*** (0.040)
	Republican	−0.049* (0.027)	−0.055** (0.025)	−0.079 (0.048)	−0.031 (0.051)	−0.056* (0.031)
E Annual change	Woman	0.013* (0.007)	0.008 (0.008)	−0.007 (0.017)	0.027 (0.017)	0.003 (0.015)
	Republican	−0.011 (0.009)	−0.013 (0.008)	−0.024** (0.012)	0.002 (0.015)	−0.016* (0.009)

In levels, years with a woman governor have slightly lower index values, mainly in resources (about −0.09 to −0.11 SD). In annual changes the sign flips: under women governors the relative index improves by 0.013 SD a year (p = .08), led by equity. Opposite signs in levels and changes are what selection produces. Women have more often taken office in states and years where resources were already below their state's norm, and those states then drifted slightly upward. H2 is not supported.

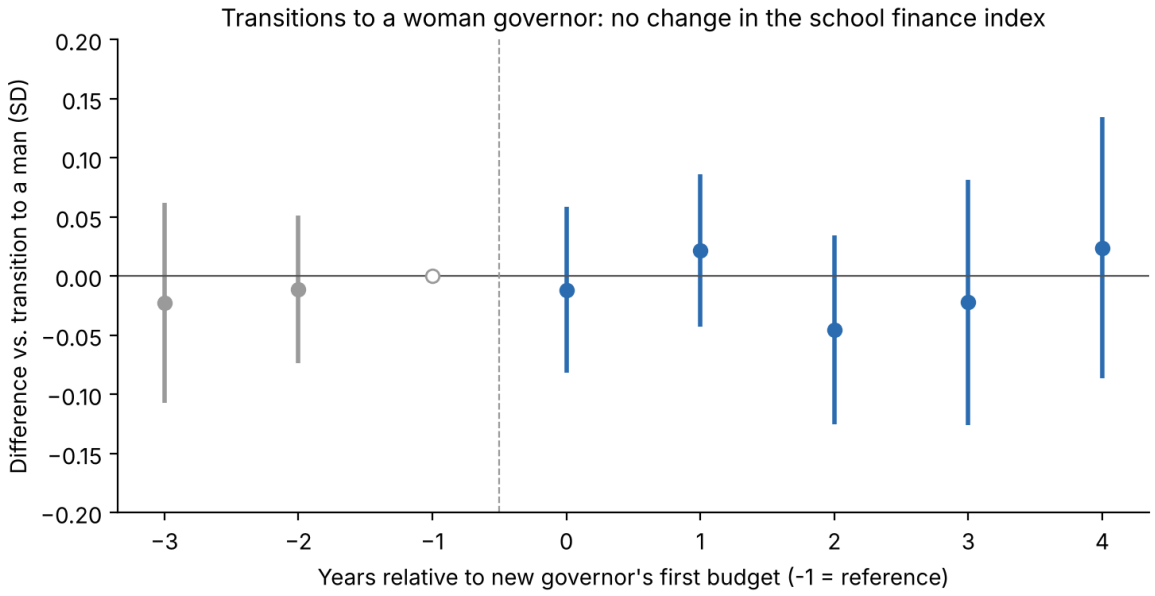


Figure 1. Transitions to a woman governor: event-study estimates, absolute index. Difference from transitions to a man, relative to the year before the new governor's first budget; 95% confidence intervals. Stacked event study of 182 gubernatorial transitions, 1997–2021 (27 to a woman). Controls: event time × incoming and outgoing party, year fixed effects; standard errors clustered by state. Sources: SFID; CAWP; Kaplan (2021).

Party is steadier than gender but smaller once ideology is controlled. Republican governors are associated with a 0.04–0.05 SD lower index in most specifications: −0.041 in the baseline ($p = .12$), −0.052 with demographic controls ($p < .05$) and −0.049 from 1997 ($p < .10$). The gap is concentrated in effort and resources rather than equity, and annual-change models show the same direction. Equity is the one domain where party shows no association, in line with Oberfield and Baker's emphasis on demographic change, rather than party, as the main correlate of progressivity. Without the ideology control the baseline party estimate is −0.063 ($p < .05$; Section 7).

5.3 Event study of transitions to a woman governor

The event study finds no effect. Before the transition, states that would get a woman governor track those that would get a man (pre-period average −0.017, SE 0.035). Over the first four budgets the difference averages −0.007 (SE 0.040). No single post-transition year differs from zero (Figure 1).

With only 27 transitions to a woman, the confidence intervals span roughly ±0.1 SD, so effects of that size cannot be ruled out. Domain-level event studies are likewise null, apart from one noisy dip in effort in the second year (−0.14, $p = .07$).

6. The adequacy era, 2009–2024

When outcomes include adequacy and equal opportunity, women's legislative share has a larger and steadier association with state school finance. A 10-point higher share goes with a 0.12 SD higher index (p = .01), about three-fifths of a typical within-state swing, carried mainly by high-poverty adequacy and equal opportunity.

6.1 Index and sample

SFID's adequacy measures begin in 2009. Each compares a district group's actual spending with the estimated cost of reaching national-average outcomes, by poverty quintile; a ratio of 1.0 means spending matches that cost. I build a second index for 2009–2024 from three equally weighted domains, standardized as in Section 3 (Table 4).

Table 4. Index components, 2009–2024

Domain	Measure	Construction
Effort	Spending as share of GSP; revenue as share of personal income	gsp_effort, inc_effort
Equal opportunity	High-poverty adequacy relative to low-poverty adequacy	pctadeq_1_5 / pctadeq_1_1
Adequacy	Overall adequacy (mean across poverty quintiles) and high-poverty adequacy	mean of pctadeq_1_1–1_5; pctadeq_1_5

The panel has 768 state-years. Connecticut lacks adequacy data for 2023–2024; its index in those years rests on effort alone, and it drops from the adequacy and equal-opportunity models (N = 766). Because demographic controls exist throughout this window, every model here includes them (the analogue of specification B). Specification D is not needed, since the CAWP coding problem predates 2009.

6.2 Women in the legislature

Table 5. Women's legislative share (per 10 percentage points), 2009–2024 index

Spec	Index (rel.)	Index (abs.)	Effort	Equal opp.	Adequacy	N
A Baseline + demog.	0.118** (0.047)	0.130** (0.052)	0.032 (0.058)	0.265* (0.155)	0.092 (0.060)	768
C + state trends	0.127* (0.076)	0.130* (0.079)	0.065 (0.060)	0.238 (0.179)	0.088 (0.059)	768
E Annual change	0.025 (0.036)	0.023 (0.035)	0.031 (0.027)	0.025 (0.078)	0.014 (0.028)	720
F 4-year average	0.259*** (0.076)	0.278*** (0.082)	0.121 (0.097)	0.540* (0.282)	0.167** (0.085)	624
G Placebo: 2 yrs ahead	−0.041 (0.063)	−0.036 (0.062)	−0.092 (0.058)	−0.006 (0.101)	−0.010 (0.067)	672
H × woman governor	−0.042 (0.053)	−0.048 (0.059)	0.015 (0.083)	−0.136 (0.108)	−0.026 (0.053)	768

The baseline estimate, 0.118 (p = .01), is nearly twice the 1993–2024 estimate and holds with the demographic controls that weakened the earlier one. It is about 0.6 of the index's within-state standard deviation (0.195). The cumulative measure is stronger still: a 10-point higher four-year average share goes with a 0.26

SD higher index. The placebo is clean, and adding state trends leaves the estimate essentially unchanged (0.127, p < .10).

In natural units the gain is in high-poverty districts. A 10-point higher share goes with high-poverty adequacy 0.043 higher (p = .02), meaning spending in the poorest fifth of districts covers about 4 more percentage points of the cost of national-average outcomes. The equal-opportunity ratio rises 0.039 (p = .09). Overall adequacy barely moves (0.007, SE 0.022). The typical state's share of women rose 9.8 points from 2009 to 2024, so the estimate implies high-poverty adequacy about 0.04 higher than with no change.

How robust. Dropping any single state leaves the estimate between 0.09 (without Nevada) and 0.13 (without Maine), and dropping the pandemic-relief years 2021–2022 leaves it at 0.12. The 1993–2024 index estimated over the same window gives 0.065 (p < .05), the same as over its full period, so the larger 2009–2024 estimate reflects the adequacy and equal-opportunity measures rather than the period. Two features call for caution. Annual changes show no association, and estimates within 2009–2019 (0.060) or 2020–2024 (0.039) alone are smaller and insignificant. The association therefore rests on slow, cumulative change across the full period, which is consistent with a policy effect but also with any slowly moving confounder that state trends do not capture. A long-difference model across states (change from 2009 to 2024, n = 48) is positive but imprecise: 0.035 SD per 10 points (SE 0.090).

6.3 Governors

Women governors again show no association with the 2009–2024 index (−0.023, SE 0.041) or any domain. An event study of 82 transitions with first budgets from 2012 to 2021, 13 of them to a woman, finds nothing before (−0.023, SE 0.039) or after (−0.015, SE 0.070). Republican governors are associated with a 0.05 SD lower index (p < .10), driven entirely by effort (−0.142, p < .01); their association with adequacy and equal opportunity is near zero.

7. Testing the policy liberalism control

Ideology matters: policy liberalism is a pathway for both party and gender, and the gender associations survive every treatment of it.

7.1 The measure

I use Caughey and Warshaw's (2022) state policy ideology series, which runs from 1937 to 2024 with posterior uncertainty in every state-year; the data were retrieved from the Dynamic Democracy data site on November 2, 2026. The series is scored so that higher values are more conservative; I reverse it so that

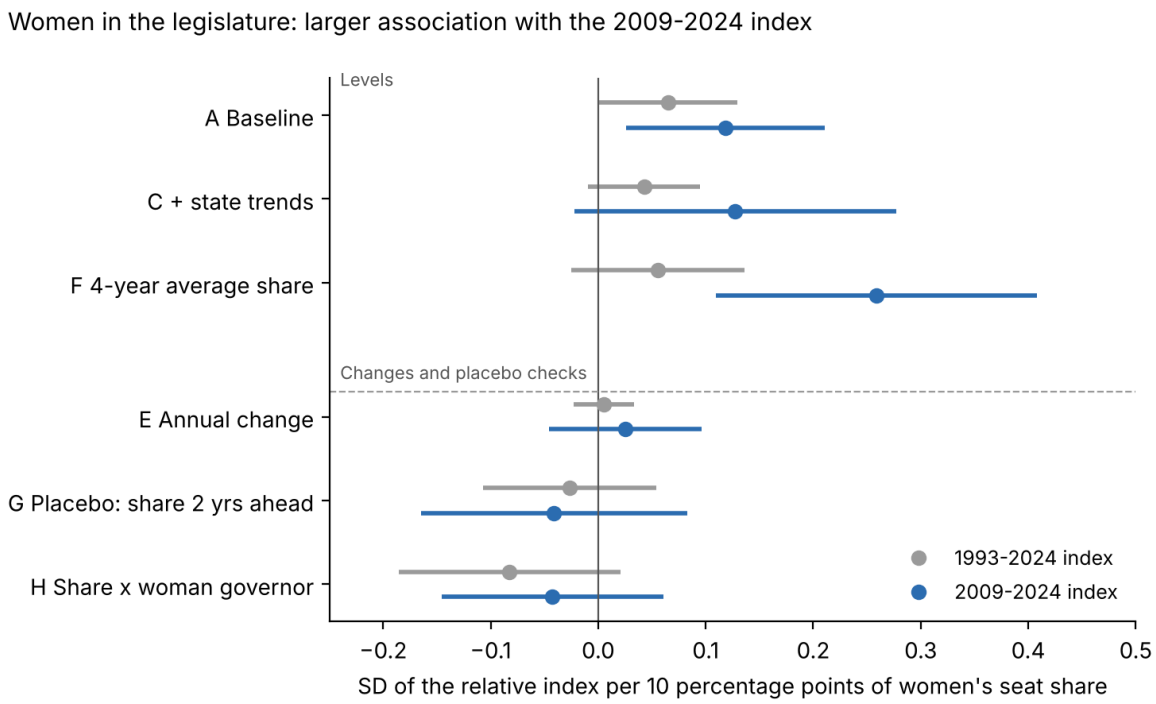


Figure 2. Coefficients on women's legislative share from Tables 2 and 5 (relative index), with 95% confidence intervals. The 1993–2024 baseline omits demographic controls; all 2009–2024 models include them. Placebo row shows the coefficient on the share two years ahead; interaction row shows share × woman governor.

higher values mean more liberal policy. Its within-state standard deviation over 1994–2024 is 0.30.

7.2 Sensitivity of the main estimates

I estimate the baseline models three ways: without any ideology control (P1); with the policy liberalism series, the baseline used throughout (P2); and averaging over 200 draws from each state-year's posterior distribution to account for measurement error (P3). A final model interacts women's share with a time-invariant measure of state conservatism, the state's standardized mean on the series over 1993–2024 (I).

Table 6. Sensitivity to the policy liberalism control (relative indexes)

Spec	Women, 93–24	Rep. gov., 93–24	Pol. lib., 93–24	Women, 09–24	Rep. gov., 09–24	Pol. lib., 09–24
P1 No ideology control	0.090** (0.035)	−0.063** (0.028)	—	0.153*** (0.050)	−0.055* (0.030)	—
P2 Policy liberalism (baseline)	0.065** (0.033)	−0.041 (0.026)	0.228** (0.095)	0.118** (0.047)	−0.047* (0.028)	0.263*** (0.100)
P3 Posterior draws	0.072** (0.033)	−0.046* (0.026)	0.170** (0.073)	0.131*** (0.048)	−0.050* (0.029)	0.165** (0.073)
I Women × conservatism	0.008 (0.029)			0.023 (0.042)		

N = 1,488 for 1993–2024 and 768 for 2009–2024. Women's share is per 10 percentage points; policy

liberalism is lagged one year. All models include the baseline controls and state and year fixed effects; standard errors clustered by state.

Policy liberalism predicts the index: a one within-state standard deviation (0.30) shift toward more liberal policy goes with about 0.07 SD higher index in 1993–2024 and 0.08 SD in 2009–2024. The women's-share estimate is significant in every specification. It is larger without an ideology control (0.090 and 0.153) than with it (0.065 and 0.118); accounting for measurement error moves it back up a little (0.072 and 0.131). H6 holds in the sense that the gender association survives any treatment of ideology, but its size depends on whether ideology is treated as a confounder or as a pathway.

7.3 Policy liberalism as a pathway

Both party and gender move policy liberalism. Republican governors shift their states to the right by 0.026 a year (SE 0.004, $p < .001$) and by 0.101 in level (SE 0.032, $p < .01$), consistent with the incremental partisan effects documented by Caughey, Warshaw and Xu (2017). Policy liberalism also responds to women's legislative share: a 10-point higher share goes with 0.109 more liberal policy (SE 0.044, $p = .01$) and with faster liberal drift (0.010 a year, $p = .07$). A woman governor does not predict it.

Controlling for policy liberalism therefore absorbs part of both associations. The estimates without the control (0.090 and 0.153 for women's share; −0.063 and −0.055 for Republican governors) are best read as total associations, and the controlled estimates as the part that does not run through the broader liberalism of state policy. H6 fails for party: its association with school finance depends substantially on how ideology is handled.

7.4 The Kansas hypothesis

Women's share is not more strongly associated with the index in conservative states. The interaction with state conservatism is small and insignificant in both periods (0.008 and 0.023). Whatever role women played in Kansas, the national data show no general pattern of larger associations in conservative states. H5 is not supported.

8. Causal-design checks

The estimates so far come from two-way fixed-effects models. They identify causal effects only if states' index values would have moved in parallel absent changes in leadership, and only if the controls are not themselves outcomes of leadership. This section relaxes both assumptions in three ways.

8.1 Direct effects holding policy liberalism fixed

Section 7 showed that policy liberalism is partly a result of who governs, so the baseline models control for a mediator. Charter-sector growth and court-ordered reform may also respond to leadership. Controlling for such post-treatment variables does not recover a well-defined effect. Following Acharya, Blackwell and Sen (2016), I estimate the average controlled direct effect of leadership, holding policy liberalism fixed, by sequential g-estimation. The first stage regresses the outcome on leadership, lagged policy liberalism, charter share, court reforms, the demographic controls (2009–2024) and state and year fixed effects, and recovers the effect of policy liberalism. The second stage subtracts that effect from the outcome and regresses the result on leadership and pre-treatment controls only. Standard errors come from 500 block-bootstrap resamples of states.

Table 7. Total, conventional and direct effects of leadership

Outcome	Leadership	Total	Conventional	Direct (ACDE)
Index, 1993–2024	Women's share	0.074* (0.041)	0.065* (0.035)	0.049 (0.037)
	Woman governor	−0.062 (0.041)	−0.057 (0.039)	−0.059 (0.040)
	Republican governor	−0.075*** (0.028)	−0.041 (0.026)	−0.052** (0.027)
Index, 2009–2024	Women's share	0.156*** (0.050)	0.118** (0.048)	0.121** (0.050)
	Woman governor	−0.044 (0.046)	−0.023 (0.044)	−0.034 (0.047)
	Republican governor	−0.064** (0.030)	−0.047 (0.029)	−0.056** (0.028)
High-poverty adequacy	Women's share	0.057*** (0.018)	0.043** (0.018)	0.051*** (0.019)
Equal-opportunity ratio	Women's share	0.044* (0.025)	0.039* (0.023)	0.047* (0.026)

Total omits policy liberalism, charter share and court reforms. *Conventional* includes all three as controls (the baseline model). *Direct (ACDE)* is the sequential g-estimate. Women's share is per 10 percentage points. Bootstrap standard errors (500 state resamples) in parentheses; * p<.10, ** p<.05, *** p<.01.

Since 2009, the direct effect of women's share is almost identical to the baseline estimate: 0.121 SD on the index ($p = .02$) and 0.051 on high-poverty adequacy ($p < .01$). Little of that association runs through ideology. Over 1993–2024 the direct effect is smaller, 0.049 SD, and not significant. For Republican governors the direct effect (−0.052 and −0.056, $p \approx .05$) is larger than the conventional estimate, which over-controls by holding charter growth and court reform fixed. Women governors' direct effects are small and insignificant. Because the total estimates here also omit charter share and court reform, they differ from P1 in Table 6.

8.2 Estimators robust to staggered and heterogeneous effects

When treatment switches on and off at different times, two-way fixed-effects coefficients are weighted averages that can put negative weight on some treated state-years and can be biased when effects vary over time (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2020).

Governors. In the baseline model no treated state-year receives a negative weight for a woman governor. For Republican governors, 105 of 833 treated state-years (1993–2024) and 118 of 433 (2009–2024) do, summing to −0.04 and −0.11. I therefore estimate the effect of each state's first switch in governor gender or party with de Chaisemartin and D'Haultfœuille's (2024) DID_1 estimator, which compares switchers with states that share their initial status and have not yet switched. Averaged over the first four budgets, the first switch to or from a woman governor has no effect (0.020, SE 0.030, 22 switches, 1993–2024; −0.041, SE 0.048, 19 switches, 2009–2024), and the placebos are flat. The first change in party has effects of −0.019 (SE 0.041) and −0.053 (SE 0.045): the same sign as the fixed-effects estimates, but imprecise. In the 2009–2024 window the party placebos are not flat. The index was already falling before switches to a Republican governor, and rising before switches away, by 0.20 SD three years before the switch ($p < .01$). Part of the party association may reflect the conditions under which party control changes.

Women's legislative share. Women's share changes in almost every election, so there are no states that never change to serve as comparisons. I instead use a local-projections difference-in-differences design with clean controls (Dube, Girardi, Jordà & Taylor, 2023). Treated cases are jumps of 4 or more points in a single legislature, with no jump of that size in the three prior years: 58 since 1994, averaging 5.7 points, and 41 in the 2009–2024 window, about half of them after the 2018 elections. Controls are state-years with no change of 2 points or more from three years before through four years after. The outcome is the change in the index from the year before the new legislature's first budget, with year fixed effects and standard errors clustered by state.

The index does not rise after jumps. The average over the first five budgets is −0.017 (SE 0.034) for the 1993–2024 index, −0.061 (SE 0.047) for the 2009–2024 index and −0.018 (SE 0.021) for high-poverty adequacy. It rises before them. Relative to the year before the new legislature's first budget, the index was 0.10 SD lower three years earlier for the 1993–2024 index and 0.12 SD lower for the 2009–2024 index (both $p \leq .01$). The pre-trend persists when treated states are required to have no earlier increases of 2 points or more, under thresholds of 3 or 5 points, and when the prior trend is controlled. Either states whose school finance is improving go on to elect more women, or both respond to a common influence. A sudden increase in women's seats is not followed by a measurable change within four budgets.

8.3 Sensitivity to unobserved confounding

Cinelli and Hazlett's (2020) robustness value asks how strong an omitted confounder would have to be to overturn an estimate. For the 2009–2024 baseline, a confounder would have to explain 16% of the residual variance of both women's share and the index to reduce the estimate to zero, and 9.5% to make it insignificant

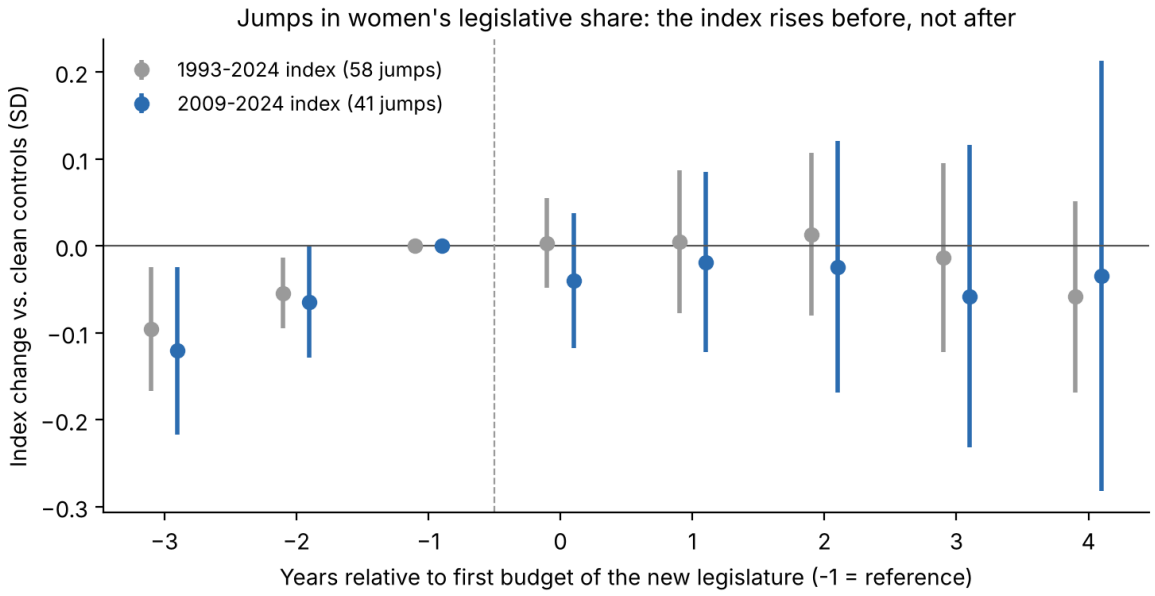


Figure 3. Jumps of 4+ points in women's legislative share: local-projections difference-in-differences estimates with clean controls. Change in the relative index from the year before the new legislature's first budget, treated vs. clean-control state-years; 95% confidence intervals. Year fixed effects; standard errors clustered by state.

at the 5% level (2% with clustered rather than conventional standard errors). Benchmarked against observed covariates, a confounder as strong as the racial composition of the school-age population leaves the estimate at 0.11 even at three times that strength. One as strong as policy liberalism reduces it to 0.08, twice as strong to 0.04, and three times as strong to zero. Oster's (2019) δ, with a maximum R^2 of 1.3 times the controlled R^2, is 4.7: unobservables would need to be nearly five times as important as the observed controls. The 1993–2024 estimate is more fragile. Its robustness value is 9% (4% for significance), with clustered errors it is already at the margin, and a confounder as strong as policy liberalism cuts it to 0.04.

8.4 What the checks imply

The three checks point the same way. The 2009–2024 association between women's legislative share and school finance, concentrated in high-poverty adequacy, is not an artifact of how ideology is controlled, and it would take a strong omitted factor to erase. But it is not a short-run response to electing more women. It does not appear after large jumps in women's seats, and the index tends to improve before those jumps. The association is best read as reflecting slow-moving differences between states that elect more women and states that do not, a cumulative effect that a four-year window cannot capture, or both; these designs cannot tell them apart. Women governors show no effect under any design. Republican governors' association with lower effort survives as a direct effect, but the robust event estimates are imprecise and, since 2009, show

pre-trends.

9. Discussion and conclusion

States with more women in their legislatures tend to make somewhat more effort for their schools. But the association is small, it weakens once demographic change is accounted for, and it does not show up as year-to-year improvement. Since 2009 the picture is sharper: states whose legislatures added women came closer to funding high-poverty districts adequately and narrowed the gap between high- and low-poverty districts, without raising overall adequacy. That association survives every treatment of ideology and moderate unobserved confounding. The causal-design checks in Section 8, however, do not support reading it as the effect of electing women: large jumps in women's seats are not followed by gains, and the index tends to rise before them. Women governors, despite strong priors from the representation literature, leave no detectable mark under any design. The steadiest political signal in these data is party: Republican governors preside over lower effort, though not lower progressivity, and move state policy to the right more broadly, although the heterogeneity-robust estimates for party are imprecise.

Several readings fit these results. First, gendered priorities may operate inside parties rather than across them; with party and ideology controlled, little gender-specific variation remains. Second, school finance is set by formulas, courts and long-run revenue structures that any one governor or legislature moves slowly. Third, the measure may be too coarse: women legislators may shape which programs are funded without changing aggregate effort or progressivity. Fourth, the timing suggests that improving school finance and the election of more women may both reflect a broader shift in state politics, such as suburban realignment, that fixed effects do not capture. Kansas points to a fifth possibility, that women matter as anchors of cross-party coalitions in particular states and moments; that pattern does not generalize into a larger association in conservative states nationally, but it may be visible only in case histories like Baker's (2021, ch. 6).

Limitations.

- **Selection into office.** Neither women's legislative share nor governor gender is randomly assigned. Fixed effects, trends and placebos reduce but cannot remove this concern, and the pre-trends in Section 8.2 show that it is real. Close mixed-gender legislative elections, in the spirit of Ferreira and Gyourko (2014), could supply variation in women's share that is unrelated to state trajectories; that requires district-level returns and candidate gender not assembled here.
- **Power.** With 27 transitions to a woman governor, effects smaller than about 0.1 SD cannot be distinguished from zero.

- **Robust designs have limited power.** The heterogeneity-robust estimators in Section 8.2 use only first switches (governors) or large jumps (legislatures), so they cannot detect small or slowly accumulating effects.
- **Measurement.** CAWP counts are service-during-year and coarse for 1993–1995; the index changes composition in 1994 and 2000; and the demographic controls exist only from 2000.
- **Aggregation.** A single legislature share ignores chamber, leadership positions, committee chairs and party. Women's share of the majority caucus or of education and appropriations committee seats may matter more than the total.

For voters and advocates, the implication is more measured than the descriptive pattern suggests. Governors' gender does not show up in these data. Their party does, though less cleanly under robust designs. And since 2009, states with more women in their legislatures have funded their highest-poverty districts better, a pattern that survives most checks but that these data cannot show was caused by electing women.

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Data and replication

All code and derived data are in the replication package (`redblue_gender_replication.zip`). Running `python run_all.py` rebuilds the CAWP legislature series, both finance indexes, the analysis panel, Tables 2–7 and Figures 1–3 from the SFID state panel, which is distributed separately by the School Finance Indicators Database. A Stata version (`redblue_gender_replication_stata.zip`, `do run_all.do`) reproduces the same results and checks them against the Python output.