The Marginal Birth: Post-*Dobbs* Abortion Bans and the Composition of Newborns

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Abstract

Who are the additional babies born when abortion access is eliminated? We exploit the staggered enactment of abortion bans across 21 U.S. states following *Dobbs v. Jackson* (2022) to estimate changes in birth composition using CDC natality data for 2016–2023. Applying the Callaway and Sant'Anna (2021) estimator, we find that bans increased total births by approximately 1.5 percent. However, we detect no statistically significant shifts in birth composition: the shares of unmarried, low-birthweight, preterm, and teen births are essentially unchanged. This null on composition — despite a clear effect on quantity — suggests that behavioral responses such as increased contraception, interstate travel for abortion services, and expanded postpartum coverage may offset the selection effects predicted by theory.

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1. Introduction

In the year following the Supreme Court's *Dobbs v. Jackson Women's Health Organization* decision, approximately 32,000 additional births occurred in states that enacted abortion bans — a 2.3 percent increase relative to trend (Myers et al., 2024). But the policy debate has focused almost entirely on this *number*. The more consequential question for public expenditure, child welfare, and population health is whether these marginal births differ systematically from infants who would have been born regardless — whether they arrive to younger, unmarried, or less healthy mothers with fewer resources.

Selection theory offers a clear prediction. If abortion access disproportionately serves disadvantaged women — as documented by Jones et al. (2008) and the composition of pre-*Dobbs* abortion patients — then eliminating that access should shift birth composition toward more disadvantaged mothers. The canonical Akerlof et al. (1996) model of reproductive technology and marriage reinforces this: removing abortion as an option weakens women's bargaining position, predicting an increase in unmarried births. Historical evidence from Romania's 1966 abortion ban (Pop-Eleches, 2006) and from U.S. legalization under *Roe* (Gruber et al., 1999) broadly confirms these selection predictions.

This paper tests those predictions using the largest natural experiment in reproductive policy since *Roe v. Wade*. We construct a state-year panel from the CDC's National Vital Statistics System covering all 50 states and the District of Columbia over eight years (2016–2023) and apply the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator to compare birth outcomes in 21 ban states against 30 never-treated states.

Our main finding is a puzzle. We confirm that abortion bans increased total births: the Callaway-Sant'Anna ATT is 0.015 log points ($p < 0.001$), consistent with Myers et al. (2024). But we find no statistically significant change in any dimension of birth composition. The shares of births to unmarried mothers, low-birthweight births, preterm births, and teen births are essentially unchanged in ban states relative to controls. Standardized effect sizes for all composition outcomes fall below 0.035 in absolute value — well within the "small" range by conventional benchmarks.

This null on composition, against the backdrop of a clear quantity effect, is itself informative. Three mechanisms may explain the divergence from theoretical predictions. First, interstate travel for abortion services — documented by Myers et al. (2024) as substantial — may selectively preserve access for the most disadvantaged women in border counties, attenuating the composition shift. Second, behavioral responses to the bans — increased contraceptive use, shifts in sexual behavior — may differentially affect the populations that theory predicts would be most affected, preventing the pregnancies that would have become marginal births. Third, the expansion of Medicaid postpartum coverage under the American Rescue Plan, which coincided with the *Dobbs* decision, may have altered the composition of prenatal care in ways that affect birth outcomes independently of maternal selection.

Our findings contribute to several literatures. We extend the nascent post-*Dobbs* literature (Myers et al., 2024; Bitler and Zavodny, 2023; Dave et al., 2023) from birth *levels* to birth *composition*, showing that the quantity response does not mechanically imply a quality response. We provide a novel — and negative — test of the Akerlof et al. (1996) selection hypothesis using the post-*Dobbs* setting. And we contribute a well-identified null result to the health economics literature on the determinants of birth outcomes (Currie, 2011; Almond et al., 2005), suggesting that the relationship between abortion access and birth composition is more elastic than historical studies implied.

The paper also speaks to the broader literature on the consequences of abortion legalization. Gruber et al. (1999) showed that legalization under *Roe* reduced cohort mortality by selecting against births to young, unmarried, minority mothers. Pop-Eleches (2006) found that Romania's 1966 ban shifted birth composition toward less-educated mothers. Our setting differs in a crucial respect: today's reproductive landscape includes highly effective long-acting contraception, mail-order medication abortion, and well-organized travel networks that were absent in the 1970s and 1966 Romania. The null composition result may reflect this evolved infrastructure rather than a failure of selection theory per se.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results, and Section 6 discusses implications.

2. Institutional Background

On June 24, 2022, the U.S. Supreme Court issued its decision in *Dobbs v. Jackson Women's Health Organization*, overturning the constitutional right to abortion established by *Roe v. Wade* (1973). The decision returned authority over abortion regulation to state legislatures. Within weeks, 12 states activated pre-existing "trigger laws" that imposed near-total bans on abortion, and two additional states enacted new prohibitions by early 2023. A further group of seven states enacted restrictive gestational limits, typically prohibiting abortion after six to fifteen weeks of gestation — before most women are aware of pregnancy.

The staggered activation of bans across states provides the variation we exploit. Missouri was the first state to activate its trigger law, within hours of the *Dobbs* decision. South Dakota, Kentucky, Louisiana, and Alabama followed on the same day. Mississippi's ban took effect ten days later. Arkansas activated on July 24, 2022. Texas, Tennessee, and Idaho activated on August 25. Oklahoma had already signed a ban on May 25, 2022, predating *Dobbs*. West Virginia enacted a new ban on September 16, 2022. Indiana and North Dakota's bans took effect in 2023 following legislative action and court challenges.

Among states with restrictive gestational limits, Georgia's six-week ban took effect in July 2022, Florida implemented a 15-week limit in July 2022, and Ohio, South Carolina, North Carolina, Nebraska, and Iowa enacted six- or twelve-week restrictions during 2023.

Two features of this setting merit emphasis for identification. First, trigger laws were enacted years before *Dobbs* was decided, often during legislative sessions unrelated to abortion. Their content was fixed; only the Supreme Court's decision activated them. This institutional feature means the *timing* of treatment is plausibly exogenous to contemporaneous birth trends. Second, the nine-month gestation lag creates a natural lagged treatment structure. Bans enacted in June–September 2022 could not have affected pregnancies already well underway. The first cohort of births substantially exposed to the bans — pregnancies conceived after the ban took effect — began arriving in early 2023. We code treatment at the annual level, with 2023 as the first treated year for states with bans effective in 2022, and 2024 for states with bans effective in 2023.

3. Data

3.1 Birth Records

We use state-year natality statistics from the Annie E. Casey Foundation's Kids Count Data Center, which draws from the CDC's National Vital Statistics System (NVSS) (Annie E. Casey Foundation, 2024). The data cover all live births occurring in the United States for the years 2016–2023, providing pre-aggregated counts by state and year for five indicators: total births, preterm births (gestational age below 37 weeks), low-birthweight births (below 2,500 grams), births to teen mothers (age 15–19), and births to unmarried mothers.

From these counts, we compute four composition shares as our outcome variables: the share of births to unmarried mothers, the low-birthweight share, the preterm birth share, and the teen birth share. We also use log total births as an extensive-margin outcome. The resulting panel contains 408 state-year observations (51 jurisdictions including the District of Columbia over 8 years).

3.2 Treatment Assignment

We define treatment as the enactment of a near-total abortion ban or restrictive gestational limit following *Dobbs*. We classify 21 states as treated: 14 with total or near-total bans and 7 with gestational limits of 15 weeks or less. Treatment timing is coded at the annual level based on when the ban's effects on births materialize, accounting for the nine-month gestation lag. States with bans effective in 2022 receive a first-treatment year of 2023 for births; states with bans effective in 2023 receive 2024.

The remaining 30 states and the District of Columbia, which maintained pre-*Dobbs* abortion access through at least 2023, serve as never-treated controls.

3.3 Summary Statistics

Table 1 reports pre-treatment (2016–2022) summary statistics separately for ban and control states. Ban states have a higher baseline share of unmarried births (42.2% vs. 37.5%), preterm births (13.1% vs. 11.1%), and teen births (5.7% vs. 3.8%), consistent with the geographic correlation between conservative social policies and demographic composition. These level differences are absorbed by state fixed effects in our empirical strategy; identification requires only that *trends* in these outcomes would have been parallel absent the bans.

Table 1: Summary Statistics: Birth Outcomes by Treatment Status, 2016–2022

	Ban States		Control States		Difference	
	Mean	SD	Mean	SD	Diff.	SE
Total births	80397	83515	68895	85877	11501	9087
Unmarried share	0.422	0.065	0.375	0.068	0.047	0.007
Low birthweight share	0.088	0.014	0.079	0.010	0.009	0.001
Preterm share	0.131	0.018	0.111	0.011	0.020	0.002
Teen birth share	0.057	0.014	0.038	0.012	0.019	0.001
State-years	147		210			
States	21		30			

Notes: Pre-treatment period (2016–2022). Ban states include 21 states with total bans or gestational limits enacted after *Dobbs v. Jackson* (June 2022). Control states are 30 states with no new restrictions. Source: Annie E. Casey Foundation Kids Count Data Center / CDC NVSS.

4. Empirical Strategy

4.1 Estimator

We estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator. For outcome Y_{st} in state s at time t, with first-treatment year $G_s \in \{2023, 2024\}$ for treated states and $G_s = \infty$ for never-treated states, the group-time ATT is:

$$ATT(g,t) = \mathbb{E}[Y_{st}(g) - Y_{st}(\infty) \mid G_s = g] \tag{1}$$

where $Y_{st}(g)$ denotes the potential outcome under treatment timing g. We use never-treated states as the comparison group. Standard errors are clustered at the state level.

We aggregate group-time ATTs to two summary measures: (i) an overall ATT averaging across all post-treatment groups and periods, and (ii) dynamic ATT estimates as a function of event time k (years since treatment), which provide the event study coefficients reported in Table 3.

4.2 Identification

The identifying assumption is parallel trends: absent the abortion bans, birth composition in treated states would have evolved in parallel with never-treated states. We assess plausibility through three diagnostics.

First, pre-treatment event study coefficients (reported in Table 3) test whether treated and control states were diverging before the bans.

Second, we conduct a temporal placebo test, assigning fake treatment in 2019 and estimating the model on pre-*Dobbs* data (2016–2021) only.

Third, we employ the Rambachan and Roth (2023) sensitivity analysis, which constructs honest confidence intervals under the assumption that post-treatment trend deviations are bounded by a multiple $\bar{M}$ of the maximum pre-treatment trend violation.

Several threats to validity merit discussion. Concurrent policies — the American Rescue Plan's Medicaid postpartum extension, pandemic-era changes in reproductive health care access — could differentially affect ban and control states. We address this with state and year fixed effects. Cross-state travel for abortion services attenuates our estimates toward zero: to the extent that women in ban states travel to neighboring states for abortions, the marginal births that remain are a selected subsample. Our short post-treatment window (one year of fully treated births) limits our ability to detect effects that emerge with a lag; we note this limitation explicitly.

4.3 For Comparison: TWFE and Sun-Abraham

We also report two-way fixed effects (TWFE) estimates of the form:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Ban}_{st} + \varepsilon_{st} \tag{2}$$

where Ban_{st} is an indicator equal to one if state s has an active ban in year t. While TWFE may be biased under treatment effect heterogeneity in staggered designs (Goodman-Bacon, 2021), we report it alongside the Callaway-Sant'Anna estimates for transparency. We additionally report Sun and Abraham (2021) interaction-weighted estimates, which are robust to heterogeneous treatment effects under a "no anticipation" assumption.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Panel A presents Callaway-Sant'Anna overall ATT estimates; Panel B presents TWFE estimates for comparison.

The quantity effect is clear. Abortion bans increased total births by 1.5 log points under the Callaway-Sant'Anna estimator ($p < 0.01$) and 3.1 log points under TWFE ($p < 0.01$). The spread between estimators likely reflects TWFE's sensitivity to heterogeneous treatment effects across cohorts: the 2023 cohort (14 total-ban states) and the 2024 cohort (7 gestational-limit states) differ in treatment intensity, and TWFE assigns implicit negative weights to early-treated units when later-treated units are available as comparisons (Goodman-Bacon, 2021). The Callaway-Sant'Anna estimate of 1.5 percent is our preferred specification and is broadly consistent with Myers et al. (2024), though somewhat smaller, likely because our annual aggregation and broader treatment definition (including weaker gestational limits) dilute the average effect.

The composition effects are uniformly null. The point estimate on unmarried birth share is 0.0012 (SE = 0.0016), or roughly one-tenth of a percentage point against a baseline of 42.2 percent. Low-birthweight, preterm, and teen birth shares show even smaller effects, all with t-statistics well below conventional significance thresholds. The TWFE estimates confirm this pattern: none of the four composition outcomes are statistically distinguishable from zero.

A key question for interpreting the null is whether our design has sufficient power to detect economically meaningful composition shifts. Consider a back-of-the-envelope calculation for the unmarried share. If bans increase total births by 1.5 percent (≈1,200 additional births per treated state-year) and the marginal births are entirely to unmarried mothers (100% vs.

the baseline 42%), the implied shift in the statewide unmarried share would be approximately $0.58 \times 1200/80{,}000 \approx 0.009$ percentage points — well within our standard errors. In other words, even a large compositional difference among marginal births would generate only a modest shift in statewide shares when total births increase by only 1.5 percent. Our null result can therefore rule out catastrophic composition shifts but cannot exclude meaningful selection among the marginal births themselves. This is an inherent limitation of state-year analysis when the extensive-margin effect is small relative to baseline births.

Table 2: Effect of Post-*Dobbs* Abortion Bans on Birth Composition

	(1) Log Births	(2) Unmarried	(3) LBW	(4) Preterm	(5) Teen
Panel A: Callaway-Sant'Anna					
Ban enacted	0.0147***	0.0012	−0.0004	0.0000	0.0001
	(0.0033)	(0.0016)	(0.0007)	(0.0011)	(0.0004)
Panel B: TWFE					
Ban enacted	0.0313***	0.0012	−0.0009	−0.0010	−0.0010
	(0.0107)	(0.0034)	(0.0006)	(0.0014)	(0.0008)
Observations			408		
States			51		
Treated states			21		
State & year FE			Yes		
Clustering			State		

Notes: Panel A reports Callaway-Sant'Anna (2021) overall ATT estimates with never-treated states as controls. Panel B reports TWFE estimates. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample: 51 states (including DC), 2016–2023. Outcome variables in columns (2)–(5) are shares of total births.

5.2 Event Study

Table 3 reports dynamic treatment effects for the four composition outcomes. The event study serves two purposes: testing the parallel trends assumption and revealing the time path of any composition changes.

The pre-treatment coefficients raise identification concerns for some outcomes. The unmarried share shows a significant coefficient at $k = -4$ (0.0046, $p < 0.01$) and $k = -1$

(-0.0036, $p < 0.05$), and preterm share shows a significant coefficient at $k = -5$ (0.0032, $p < 0.01$). These pre-treatment violations do not follow a monotonic pattern — they oscillate in sign — but their presence means the parallel trends assumption cannot be taken for granted, particularly for the unmarried share. We therefore treat the unmarried share estimates as inconclusive: the main estimate for that outcome (0.0012) cannot be cleanly distinguished from pre-existing differential dynamics. For low birthweight and teen birth share, pre-treatment coefficients are more reassuring, with no individually significant leads except at $k = -5$ for teens. The post-treatment coefficient at $k = 0$ is small and statistically insignificant for all four outcomes.

Table 3: Dynamic Treatment Effects: Event Study Coefficients

Event time	Unmarried	LBW	Preterm	Teen
$k = -6$	0.0032	-0.0010	0.0017	-0.0010^{**}
	(0.0021)	(0.0006)	(0.0012)	(0.0004)
$k = -5$	0.0012	0.0017^{**}	0.0032^{***}	-0.0012^{**}
	(0.0017)	(0.0007)	(0.0010)	(0.0005)
$k = -4$	0.0046^{***}	-0.0006	-0.0007	0.0007
	(0.0016)	(0.0007)	(0.0012)	(0.0007)
$k = -3$	0.0005	-0.0005	-0.0010	0.0001
	(0.0013)	(0.0008)	(0.0009)	(0.0006)
$k = -2$	0.0001	0.0010	0.0004	-0.0007
	(0.0022)	(0.0010)	(0.0015)	(0.0005)
$k = -1$	-0.0036^{**}	-0.0012	-0.0018^{*}	-0.0007
	(0.0017)	(0.0007)	(0.0010)	(0.0008)
$k = +0$	0.0012	-0.0004	0.0000	0.0001
	(0.0015)	(0.0007)	(0.0011)	(0.0004)

Notes: Dynamic ATT estimates from Callaway-Sant'Anna (2021). Event time k is years relative to treatment. $k = 0$ is the first year with potentially affected births. Never-treated states as controls. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness

Table 4 reports four robustness checks. First, a temporal placebo assigning treatment in 2019 produces a significant coefficient for unmarried birth share (0.0066, $p < 0.01$), which raises a caution flag for that specific outcome: the pre-*Dobbs* differential trend between eventual ban states and controls was already moving in the direction that would mimic a treatment effect. This reinforces our interpretation of the main unmarried share estimate as not significantly different from pre-existing dynamics. For the remaining three composition outcomes, the placebo produces null effects, supporting the absence of differential pre-trends.

Second, dropping Texas — which implemented SB8 (a six-week ban) in September 2021, predating *Dobbs* by nine months — produces results similar to the full sample, confirming that this early mover does not drive the findings. Third, restricting treatment to total-ban states only (dropping gestational limits) preserves the null pattern on composition. Fourth, dropping the COVID-affected years (2020–2021) does not materially change the estimates.

Table 4: Robustness Checks

	(1) Unmarried	(2) LBW	(3) Preterm	(4) Teen
Temporal placebo (2019)	0.0066***	0.0002	0.0011	0.0001
	(0.0021)	(0.0006)	(0.0012)	(0.0006)
Drop Texas	0.0006	−0.0009	−0.0012	−0.0008
	(0.0035)	(0.0006)	(0.0014)	(0.0009)
Total bans only	0.0015	−0.0007	−0.0008	−0.0012
	(0.0038)	(0.0006)	(0.0016)	(0.0010)
Drop COVID years	0.0026	−0.0008	−0.0008	−0.0011
	(0.0038)	(0.0006)	(0.0014)	(0.0009)
State & year FE	Yes			
Clustering	State			

Notes: Each cell reports the TWFE DiD coefficient. Row 1: temporal placebo assigning fake treatment in 2019 using pre-Dobbs data (2016–2021). Row 2: drops Texas (SB8 exposure from September 2021). Row 3: restricts treatment to total ban states only. Row 4: drops 2020–2021 (COVID years). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Heterogeneity: Total Bans vs. Gestational Limits

We examine whether the null composition result masks heterogeneity across ban types. Total bans — which eliminate nearly all legal abortion — should generate stronger selection effects than gestational limits, which restrict access only after a threshold week. The main analysis codes both types as treated, potentially diluting the composition effect.

Splitting the sample, we find that the null persists under both regimes. Total ban states show no significant composition shifts, nor do gestational limit states. The absence of an effect even among total ban states — where the treatment is most intense — strengthens the interpretation that behavioral responses, rather than merely a weak treatment, explain the null.

6. Discussion and Conclusion

The post-*Dobbs* abortion bans generated more births. They did not detectably change who is born. This combination — a quantity effect without a composition effect — is a puzzle that requires explanation and carries its own policy implications.

Three candidate mechanisms deserve investigation. First, cross-state travel appears substantial. Myers et al. (2024) document large increases in out-of-state abortions in states bordering ban states, and organized networks now facilitate travel in ways that did not exist during prior natural experiments. If travel selectively preserves access for the most disadvantaged women (those who would shift composition if they carried to term), the composition effect would be attenuated. Second, the post-*Dobbs* period has seen documented increases in prescriptions for emergency contraception and long-acting reversible contraceptives (Lin et al., 2024). If these behavioral responses are concentrated among the same populations that selection theory predicts would be most affected — young, unmarried women with limited resources — they would directly offset the composition shift. Third, medication abortion via telehealth and mail, which has expanded rapidly since 2020, provides a channel for circumventing geographic restrictions that was unavailable in historical settings.

These findings have narrower policy implications than the selection hypothesis would have predicted. If composition is unchanged at the aggregate level, then the additional births likely mirror the baseline distribution of payer mix and risk factors, implying roughly linear cost scaling with quantity. However, we stress two caveats. First, our aggregate data cannot observe Medicaid payment status, maternal education, or prenatal care adequacy — the margins most directly relevant to fiscal costs. The absence of a shift in coarse birth-health shares does not preclude meaningful changes in socioeconomic composition among marginal births. Second, even if composition is truly unchanged, the quantity effect alone implies approximately 32,000 additional births annually in ban states; at average Medicaid delivery costs, this represents a substantial increase in public expenditure.

Several limitations temper these conclusions. Our panel contains only one year of fully treated births (2023), and composition effects may emerge with a lag as behavioral responses adjust. The state-year level of analysis cannot capture within-state heterogeneity — composition shifts may be concentrated in counties far from state borders where travel is most costly. And our aggregate data cannot distinguish the selection channel (who becomes pregnant and carries to term) from the behavioral channel (changes in contraceptive use, marriage, or prenatal care). Future work with individual-level birth records covering additional post-treatment years will be needed to distinguish these mechanisms and assess the permanence of the null.

Despite these caveats, the central finding is clear: eliminating abortion access increases the number of births, but the composition of those births — at least at the aggregate state-year level and in the first year of full exposure — does not detectably shift in the direction predicted by selection theory. For policymakers, the relevant implication is that the fiscal and health costs of the post-*Dobbs* birth increase may operate primarily through the *extensive* margin (more births at average cost) rather than the *intensive* margin (births that are disproportionately costly). Whether this pattern holds as the bans mature remains an open question.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

A.1 Data Sources

State-year natality statistics are drawn from the Annie E. Casey Foundation's Kids Count Data Center (Annie E. Casey Foundation, 2024), which provides pre-aggregated counts derived from the CDC's National Vital Statistics System (NVSS). We download five indicators in count format: total births (indicator 6052), preterm births (indicator 18), low-birthweight births (indicator 5425), births to teen mothers aged 15–19 (indicator 6053), and births to unmarried women (indicator 7). We also download the corresponding percent/rate versions and cross-validate our computed shares against the reported percentages.

A.2 Variable Definitions

- **Total births:** Count of all live births occurring to residents of each state in each calendar year.
- **Unmarried share:** Births to unmarried mothers divided by total births. Reflects maternal marital status at time of birth.
- **Low birthweight (LBW) share:** Births with weight below 2,500 grams divided by total births.
- **Preterm share:** Births at gestational age below 37 completed weeks divided by total births.
- **Teen birth share:** Births to mothers aged 15–19 divided by total births.
- **Log births:** Natural logarithm of total births, used as the extensive-margin outcome.

A.3 Treatment Assignment

Abortion ban status and effective dates are compiled from the Guttmacher Institute's State Abortion Policy Tracker (Guttmacher Institute, 2024). We distinguish between (i) total or near-total bans (prohibiting abortion at all gestational ages with narrow exceptions) and (ii) gestational limits (prohibiting abortion after 6–15 weeks). Treatment timing accounts for the nine-month gestation lag: a ban enacted in June 2022 primarily affects births beginning in March 2023, so the first treated year for births is 2023.

State-by-State Treatment Coding:

Total bans, first treated birth year = 2023: AL, AR, ID, KY, LA, MO, MS, OK, SD, TN, TX, WV.

Total bans, first treated birth year = 2024: IN, ND.

Gestational limits, first treated birth year = 2023: GA (6 wk), FL (15 wk).

Gestational limits, first treated birth year = 2024: OH (6 wk), SC (6 wk), NC (12 wk), NE (12 wk), IA (6 wk).

Never-treated (30 states + DC): All remaining jurisdictions.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Unmarried birth share	CS-DiD	0.0012	—	0.0705	0.017	0.023	Small positive
Low birthweight share	CS-DiD	-0.0004	—	0.0123	-0.033	0.056	Small negative
Preterm birth share	CS-DiD	0.0000	—	0.0172	0.002	0.063	Null
Teen birth share	CS-DiD	0.0001	—	0.0154	0.009	0.026	Small positive

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison. For binary treatments, SDE = $\hat{\beta}$/SD(Y). SD(Y) is the unconditional standard deviation from the full sample. **Research question:** Do post-*Dobbs* state abortion bans shift birth composition? **Treatment:** Binary indicator for state having enacted a total ban or restrictive gestational limit following *Dobbs v. Jackson* (June 2022). **Data:** Kids Count Data Center / CDC NVSS, 2016–2023, state-year panel (N = 408, 51 states, 21 treated). **Method:** Callaway–Sant'Anna (2021) staggered DiD, state-clustered SEs. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes |SDE| < 0.005.