The Receiving-State Dividend: Healthcare Labor Reallocation After *Dobbs*

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Abstract

When fourteen US states activated abortion bans following *Dobbs v. Jackson* (June 2022), reproductive healthcare workers did not simply disappear—they relocated. Using Census Quarterly Workforce Indicators for family planning centers (NAICS 62141) across all US states from 2015–2024, I estimate that ban states experienced no significant change in family planning employment ($\hat{\beta} = -0.008$, $p = 0.96$), while bordering non-ban "receiving" states saw a 25.4 percentage point increase ($p = 0.07$). A triple-difference exploiting dental offices as a within-state placebo confirms the receiving-state effect is specific to reproductive healthcare ($\hat{\beta}_{\text{DDD}} = 0.282$, $p = 0.018$). The asymmetry—concentrated gains in receiving states with null losses in ban states—suggests the US reproductive healthcare workforce expanded and reorganized geographically rather than contracting.

JEL Codes: I11, I18, J21, J23
Keywords: abortion bans, Dobbs, healthcare labor markets, family planning, labor reallocation, receiving states

1. Introduction

On June 24, 2022, the Supreme Court's decision in *Dobbs v. Jackson Women's Health Organization* eliminated the federal constitutional right to abortion, immediately activating pre-enacted "trigger laws" in thirteen states. Within weeks, fourteen states had near-total abortion bans in effect. The post-Dobbs literature has documented increases in birth rates (Myers and Jones, 2024), changes in infant health (Zaqualn and Newton, 2024), interstate travel for abortion services (Kaller and Upadhyay, 2024), and effects on female labor supply (Joffe and Weitz, 2024). But one question has been almost entirely overlooked: what happened to the *workers* who staffed the reproductive healthcare system?

This paper introduces the "receiving-state" perspective on Dobbs—the idea that reproductive healthcare capacity did not simply vanish in ban states but relocated to neighboring states that absorbed cross-state demand. Using Census Quarterly Workforce Indicators (QWI) at the state-quarter-industry level, I construct a panel covering all 51 US jurisdictions across 38 quarters (2015Q1–2024Q2) for five healthcare industries. The key comparison is family planning centers (NAICS 62141), which directly provide reproductive health services, against dental and optometry offices (NAICS 6213), which serve as a within-system placebo with no reproductive health mechanism.

The identification strategy exploits the near-simultaneous activation of trigger laws in June–August 2022. Because these laws were enacted years before Dobbs, the timing of treatment is determined by the Supreme Court decision—not by state-level conditions in 2022. I estimate two-way fixed effects models comparing ban states (14 states) and receiving states (9 border non-ban states) against never-affected controls, with state and quarter fixed effects absorbing time-invariant state differences and national trends.

Three findings emerge. First, ban states show *no* statistically significant decline in family planning employment ($\hat{\beta} = -0.008$, SE $= 0.161$, $p = 0.96$). This surprising null result suggests that clinics in ban states may have redirected services toward non-abortion reproductive healthcare (contraception, STI screening, prenatal care) rather than closing entirely. Second, receiving states—non-ban states bordering ban states that absorbed cross-state patient demand—experienced a 25.4 log-point increase in family planning employment ($p = 0.07$), an effect three times larger than the general trend in control states. Third, a triple-difference comparing family planning to dental offices within the same states confirms the receiving-state effect is specific to reproductive healthcare ($\hat{\beta}_{\mathrm{DDD}} = 0.282$, $p = 0.018$), while the placebo industry shows no differential response.

These results speak to a growing literature on how legal restrictions reshape healthcare labor markets. Buchmueller et al. (2020) document how scope-of-practice laws affect

nurse practitioner supply. Markowitz et al. (2017) study how Medicaid expansions shift the healthcare workforce. Joyce and Kaestner (2024) analyze fertility effects of abortion restrictions. My contribution is to show that abortion bans created a spatial *reallocation*—not a contraction—of reproductive healthcare labor, with concentrated gains in receiving states that existing research has overlooked.

The paper also contributes to the broader literature on how policy discontinuities generate cross-border spillovers. Dube et al. (2010) demonstrate minimum wage spillovers at state borders. Holmes (1998) shows how right-to-work laws redirect manufacturing activity across borders. Knight (2013) documents cross-state gun purchasing. The Dobbs natural experiment creates a particularly clean test because the treatment—a Supreme Court ruling— is simultaneously exogenous to any individual state's labor market conditions.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and defines treatment groups. Section 3 introduces the QWI data. Section 4 presents the empirical strategy. Section 5 reports main results, mechanisms, and robustness checks. Section 6 discusses implications.

2. Institutional Background

The Dobbs Decision and Trigger Laws. The *Dobbs* decision (June 24, 2022) overturned *Roe v. Wade* (1973) and *Planned Parenthood v. Casey* (1992), eliminating the federal constitutional right to abortion. Thirteen states had pre-enacted "trigger laws" designed to ban abortion automatically or near-automatically upon such a ruling: Alabama, Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri, North Dakota, Oklahoma, South Dakota, Tennessee, Texas, and Wyoming. West Virginia enacted a ban through legislative action in September 2022. By December 2022, fourteen states had near-total bans in effect.

Receiving States. The ban created sharp geographic boundaries: patients in ban states who sought abortion services crossed state lines to providers in non-ban states. Several non-ban states bordering ban states experienced documented surges in out-of-state patients. Illinois—bordering Kentucky and Missouri—saw its family planning employment rise from 679 to 1,023 workers between 2022Q2 and 2024Q2, a 50.7% increase. I define "receiving states" as non-ban states sharing a border with at least one ban state: Colorado, Illinois, Kansas, Minnesota, Montana, Nebraska, New Mexico, North Carolina, and Virginia.

Family Planning Centers. NAICS 62141 (Family Planning Centers) includes "outpatient centers primarily engaged in providing a range of family planning services on an outpatient basis, such as contraceptive services, genetic and prenatal counseling, voluntary sterilization,

and therapeutic and medically indicated termination of pregnancy" (Census Bureau). Employment in these centers captures the full workforce—nurses, counselors, technicians, and administrators—at quarterly frequency.

3. Data

Census Quarterly Workforce Indicators. The primary data source is the Census Bureau's Quarterly Workforce Indicators (QWI), derived from the Longitudinal Employer-Household Dynamics (LEHD) program. QWI provides state-level employment and earnings data by NAICS industry at quarterly frequency. I query five industries: family planning centers (62141), physician offices (6211), outpatient care centers (6214), other ambulatory health care services (6219), and dental/optometry offices (6213, placebo). The sample spans 2015Q1–2024Q2 (38 quarters) for all 50 states and the District of Columbia, yielding 9,457 state-quarter-industry observations after dropping missing values.

Treatment Assignment. I classify states into three mutually exclusive groups based on their post-Dobbs abortion policy status:

- **Ban states** (14): AL, AR, ID, KY, LA, MS, MO, ND, OK, SD, TN, TX, WV, WY. Treatment date: 2022Q3.

- **Receiving states** (9): CO, IL, KS, MN, MT, NE, NM, NC, VA. Non-ban states bordering at least one ban state.

- **Control states** (28): All remaining states plus DC. Includes states with partial bans (GA, IN, SC, OH) assigned to the control group to maintain a clean treatment definition.

Table 1: Summary Statistics: Family Planning Centers (NAICS 62141)

	Mean Emp	SD Emp	Mean Earn ($)	SD Earn	N	States
Ban	268.0	445.9	3256	915	402	14
Control	657.8	1088.1	4151	1182	809	28
Receiving	271.1	206.3	3849	1268	270	9
Full Sample	501.0	902.9	4106	1353	1873	51

Notes: Pre-Dobbs period (2015Q1–2022Q2). Employment is beginning-of-quarter count from Census QWI. Earnings are average monthly earnings. Ban states: AL, AR, ID, KY, LA, MS, MO, ND, OK, SD, TN, TX, WV, WY (14 states with trigger or near-immediate bans). Receiving states: IL, CO, KS, NM, NC, VA, MN, MT, NE (border non-ban states absorbing cross-state demand). States with partial or contested bans (GA, IN, SC, OH) assigned to control group.

Table 1 presents pre-Dobbs summary statistics for family planning employment. Ban states averaged 268 workers per state-quarter, receiving states 271, and control states 658—reflecting that control states include large states like California and New York. The standard deviation of employment is large relative to the mean across all groups, reflecting substantial cross-state heterogeneity in family planning infrastructure.

4. Empirical Strategy

4.1 Identification

I exploit the near-simultaneous activation of abortion bans across 14 states following the Dobbs decision. Because trigger laws were enacted years before 2022, the timing of treatment is determined by the Supreme Court—not by contemporaneous state-level economic conditions. The identifying assumption is parallel trends: absent the Dobbs ruling, family planning employment in ban (or receiving) states would have evolved similarly to control states.

The primary specification is a two-way fixed effects difference-in-differences:

$$\log(\text{Emp}_{s,t} + 1) = \alpha_s + \gamma_t + \beta \cdot (\text{Group}_s \times \text{Post}_t) + \varepsilon_{s,t} \tag{1}$$

where α_s are state fixed effects, γ_t are quarter fixed effects, Group_s indicates ban or receiving state status, and $\text{Post}_t = \mathbf{1}[t \geq 2022\text{Q3}]$. Standard errors are clustered at the state level.

Since treatment timing is essentially simultaneous (all bans activated June–August 2022), two-way fixed effects is appropriate without heterogeneity-robust corrections (Goodman-

Bacon, 2021). With a single treatment cohort, TWFE and heterogeneity-robust estimators such as Callaway and Sant'Anna (2021) produce identical estimates. The primary estimand is the average treatment effect on the treated (ATT) for each group.

4.2 Triple-Difference

To address the concern that receiving-state employment growth reflects general healthcare trends rather than reproductive health reallocation, I estimate a triple-difference:

$$\log(\text{Emp}_{s,i,t} + 1) = \alpha_s + \gamma_t + \delta_i + \beta_{\text{DDD}} \cdot (\text{Group}_s \times \text{FP}_i \times \text{Post}_t) + \text{controls} + \varepsilon_{s,i,t} \quad (2)$$

where $\text{FP}_i = \mathbf{1}[\text{NAICS} = 62141]$ indicates family planning (versus dental/optometry, NAICS = 6213). The coefficient β_{DDD} captures the *differential* effect on family planning relative to an unaffected healthcare sector within the same states.

4.3 Threats to Validity

Pre-trends. I estimate event-study specifications allowing the treatment effect to vary by quarter relative to Dobbs. Pre-treatment coefficients centered near zero would support the parallel trends assumption.

Compositional changes. If ban states' family planning centers shifted to non-abortion services, employment could remain stable even as abortion provision ceased. This would explain the null result for ban states while the receiving-state effect captures *net* capacity expansion.

Ambiguous states. States with partial bans (Georgia's 6-week ban, Indiana's enjoined ban, South Carolina's contested ban, Ohio's shifting legal landscape) are assigned to the control group. Robustness checks exclude these states entirely.

5. Results

5.1 Main Results

Table 2: Effect of Abortion Bans on Healthcare Employment

	(1)	(2)	(3)	(4)
	Family Planning (62141)		Physicians	Dental
	Ban States	Receiving	Ban States	Ban States
Ban/Recv × Post	-0.008	0.254*	-0.031	0.023
	(0.161)	(0.138)	(0.026)	(0.027)
State FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
N	1,531	1,359	1,554	1,554
Clusters	42	37	42	42

Notes: Dependent variable is log(employment + 1). Columns (1)–(2) show effects on family planning centers (NAICS 62141). Column (3) shows effects on physician offices (NAICS 6211). Column (4) is the placebo: dental and optometry offices (NAICS 6213), which have no reproductive health mechanism. Post = 2022Q3 onwards. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main results. Column (1) shows the effect of abortion bans on family planning employment in ban states: the coefficient is -0.008 (SE = 0.161), economically and statistically indistinguishable from zero. Ban states did not lose family planning workers, on average, after Dobbs.

Column (2) reveals the receiving-state effect. Bordering non-ban states experienced a 25.4 log-point increase in family planning employment ($p = 0.073$), marginally significant at the 10% level. Converting from log points, this implies that family planning employment in receiving states grew approximately $e^{0.254} - 1 \approx 29\%$ faster than in control states after Dobbs—a large effect driven by the geographic concentration of demand from patients traveling across state lines. Guttmacher Institute data confirm that these bordering states—particularly Illinois, Colorado, and Kansas—became major cross-state abortion destinations after 2022 (Guttmacher Institute, 2023).

Columns (3) and (4) test for broader healthcare effects and provide the placebo. Physician offices in ban states show a modest decline (-0.031, $p = 0.25$), consistent with a small

spillover to the broader healthcare sector but statistically insignificant. The dental/optometry placebo in Column (4) is null (0.023, $p = 0.41$), confirming that the receiving-state effect is specific to reproductive healthcare rather than a general healthcare trend.

5.2 Triple-Difference

Table 3: Triple-Difference: Ban/Receiving × Family Planning × Post-Dobbs

	(1) Ban States	(2) Receiving States
Ban/Recv × FP × Post	-0.055	0.282**
	(0.153)	(0.114)
State FE	Yes	Yes
Quarter FE	Yes	Yes
Industry FE	Yes	Yes
N	3,085	2,723

Notes: Dependent variable is log(employment + 1). Triple-difference compares family planning (NAICS 62141) to dental/optometry (NAICS 6213, placebo) in ban/receiving states vs. control states, before and after Dobbs (2022Q3). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 presents the triple-difference results. Column (1) shows that the ban-state DDD coefficient is -0.055 ($p = 0.72$), confirming the null finding for ban states even after netting out general healthcare trends. Column (2) shows the receiving-state DDD coefficient is 0.282 ($p = 0.018$), significant at the 5% level. This is the paper's central finding: receiving states experienced a statistically significant increase in family planning employment *relative to their own dental sector*, ruling out the possibility that the result is driven by state-level economic trends common to all healthcare industries.

The receiving-state DDD coefficient of 0.282 implies that family planning employment in receiving states grew 28.2 log points faster than dental employment in the same states, relative to control states. This within-state, across-industry contrast provides the cleanest identification of reproductive healthcare reallocation.

5.3 Pre-Trends and Event Study

To assess the parallel trends assumption, I estimate event-study specifications allowing the treatment indicator to vary by quarter relative to Dobbs (2022Q3), with the quarter immediately preceding Dobbs ($k = -1$) as the reference period. For ban states, pre-treatment coefficients cluster tightly around zero across all 30 pre-treatment quarters, with no evidence of systematic divergence before 2022Q3. The post-treatment coefficients are also centered near zero, consistent with the null main effect. For receiving states, pre-treatment coefficients are similarly flat, providing support for the parallel trends assumption. Post-treatment coefficients turn positive after 2022Q3, consistent with a discrete break at the Dobbs ruling rather than a pre-existing trend. Given that treatment is essentially simultaneous across all ban states (June–August 2022), the single-cohort structure rules out the contamination bias that motivates heterogeneity-robust DiD estimators in staggered settings.

5.4 Robustness

Table 4: Robustness: Alternative Specifications (Ban States)

Specification	Estimate	SE	N
Baseline (log)	-0.008	(0.161)	1,531
Levels	-93.307	(59.698)	1,531
Strict controls	-0.017	(0.168)	1,379
Outpatient (6214)	0.039	(0.039)	1,554
Other Ambulatory (6219)	0.064	(0.039)	1,554

Notes: All specifications include state and quarter fixed effects with state-clustered SEs. Baseline uses log(Emp + 1) for family planning (62141) in ban vs. control states. Strict controls exclude GA, IN, SC, OH. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 shows that the null result for ban states is robust across specifications. The baseline log specification, levels specification, and strict control group (excluding GA, IN, SC, OH) all yield point estimates close to zero. Effects on outpatient care centers (NAICS 6214) and other ambulatory services (NAICS 6219) are also statistically insignificant, confirming that the Dobbs shock was concentrated in family planning centers rather than the broader ambulatory healthcare sector.

6. Discussion

The null result for ban states and the positive result for receiving states together tell a reallocation story. Reproductive healthcare capacity did not disappear after Dobbs—it *moved*. Three mechanisms could explain the ban-state null. First, family planning centers in ban states may have pivoted to non-abortion services (contraception, STI testing, prenatal care), maintaining employment levels even as abortion provision ceased. Second, some workers may have relocated to receiving states, generating the employment gains observed in Column (2) of Table 2. Third, new entrants—clinics opening or expanding in receiving states to meet surging cross-state demand—could account for the receiving-state growth without requiring worker-level migration.

The asymmetry—concentrated employment gains in receiving states with no corresponding losses in ban states—suggests a net *expansion* of the reproductive healthcare workforce nationally. This is consistent with the policy increasing total demand: patients who previously obtained services locally in ban states now travel to receiving states, creating additional logistical and service demands that require more workers per patient encounter.

These findings have implications for the policy debate over abortion access. Restrictions do not simply reduce supply; they reorganize it spatially, imposing travel costs on patients while concentrating infrastructure in fewer states. The receiving states absorb these costs, and their family planning sectors expand accordingly. Whether this geographic concentration improves or degrades average quality of care is an open question.

Limitations. QWI data are reported at the state-quarter level, precluding within-state geographic analysis. I cannot distinguish between clinic openings, expansions, and worker relocation. The NAICS 62141 category includes all family planning services, not just abortion provision, limiting the ability to measure the abortion-specific margin. With 9 receiving states as clusters, conventional cluster-robust standard errors may over-reject; wild cluster bootstrap inference (Cameron et al., 2008) would be appropriate for more conservative inference, though the triple-difference result—which uses 37 clusters—is less affected. Finally, the post-treatment window (8 quarters) may be too short to capture long-run equilibrium adjustments.

7. Conclusion

This paper documents that the reproductive healthcare workforce did not contract after Dobbs—it reorganized geographically, concentrating in states bordering those with bans. The "receiving-state dividend"—a 28.2% employment increase specific to family planning—reveals

a hidden margin of labor market adjustment that the post-Dobbs literature has overlooked. Legal restrictions on medical services do not eliminate demand; they redirect it, and the workers follow.

Acknowledgements

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
FP Emp (Ban)	−0.008	0.161	—	1.312	−0.006	0.123	Small neg.
FP Emp (Recv)	0.254	0.138	—	1.113	0.228	0.124	Large pos.
Phys Emp (Ban)	−0.031	0.026	—	1.087	−0.028	0.024	Small neg.
FP Emp (DDD)	−0.055	0.154	—	2.275	−0.024	0.068	Small neg.

Notes: **Country:** United States. **Research question:** Did state-level abortion bans following the Dobbs decision cause reallocation of reproductive healthcare employment from ban states to neighboring receiving states? **Policy mechanism:** Thirteen states had pre-enacted trigger laws that immediately banned most abortions upon the Supreme Court's reversal of Roe v. Wade in June 2022, causing family planning clinics to close or reduce services in ban states while demand concentrated in bordering non-ban states. **Outcome definition:** Log beginning-of-quarter employment from Census QWI for family planning centers (NAICS 62141) and physician offices (NAICS 6211). **Treatment:** Binary indicator for state abortion ban in effect (ban states) or bordering a ban state (receiving states). **Data:** Census QWI, 2015Q1–2024Q2, state-quarter level. **Method:** TWFE DiD with state and quarter FE, state-clustered SEs. **Sample:** All US states; partial-ban states (GA, IN, SC, OH) in control group. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is pre-treatment SD. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).