

# Medicaid Spillovers on Commercially Insured Patients: Evidence from Postpartum Depression Screening

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## Abstract

This paper examines how targeted Medicaid reimbursement affects screening practices across payer types. In January 2014, Colorado Medicaid began reimbursing pediatric providers for postpartum depression (PPD) screening during infant well-child visits. Using linked birth records and All-Payer Claims data from 2012–2019, I find that practices with greater pre-policy Medicaid exposure increased screening more after the policy, primarily among commercially insured patients. Screening for Medicaid patients rose broadly across all practices, consistent with practice-wide adoption rather than payer-specific targeting. Leveraging physician moves across practices, I show that physicians adjust their screening behavior toward the norms of their new practice, indicating that organizational systems play a central role in shaping provider behavior. These findings show that payer-specific incentives can influence care delivery beyond the targeted payer through practice-level mechanisms.

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# 1 Introduction

Understanding spillovers from public to private insurance is central to evaluating the broader impact of health policy. When providers respond to payment incentives for publicly insured patients, their behavior may extend to privately insured patients as well, amplifying the reach of targeted programs. Prior work has documented such spillovers in hospital spending (Baicker et al., 2013), physician pricing (Clemens and Gottlieb, 2017), and bundled payment reforms (Einav et al., 2020; Barnett et al., 2020). Yet little is known about whether similar effects arise in mental health care, where detection and treatment gaps remain large.

Postpartum depression (PPD) provides a particularly useful case for studying these dynamics. PPD affects 10–20% of mothers in the United States (Khadka et al., 2024), yet often goes undiagnosed in 50–70% of cases and untreated in nearly 85% (Dagher and Bair-Merritt, 2021). Untreated PPD harms maternal well-being and can lead to developmental delays and higher healthcare utilization in early childhood (Slomian et al., 2019). Improving detection and treatment has therefore become a national public health priority. Medicaid, one of the largest payers of maternity care, offers a natural policy lever for addressing these gaps. Because Medicaid covers only a subset of the population, policies targeting its enrollees may leave out the commercially insured. Spillovers through provider behavior can bridge this divide: if practices adopt new screening protocols in response to Medicaid incentives, these workflows may extend to all patients, regardless of payer.

This paper investigates how pediatric practices responded to a targeted Medicaid reimbursement policy for postpartum depression screening and whether these changes affected care for commercially insured mothers. In January 2014, Colorado implemented a policy allowing pediatric providers to bill Medicaid for screening mothers for PPD during infant well-child visits. While the policy aimed to increase screening among Medicaid-enrolled mothers, many pediatric practices serve both Medicaid and commercially insured patients. The policy thus created heterogeneous incentives across providers, depending on their baseline Medicaid patient share, and introduced the possibility that practices exposed to stronger Medicaid incentives would adopt screening workflows that spill over to commercial patients.

To test these hypotheses, I use the Colorado All-Payer Claims Database (APCD) linked to statewide birth records from 2012–2019. A key feature of the analysis is the use of practice-level variation in Medicaid patient share to measure financial incentives directly. I define each practice’s pre-policy Medicaid exposure as the average share of patients enrolled in Medicaid in 2012–2013 and implement a difference-in-differences design using this exposure as a continuous treatment variable. I restrict the analytic sample to practices with at least one pediatric or primary care physician in the pre-policy period. To assess whether practices applied screening uniformly across patient groups, I estimate a triple-difference model comparing Medicaid and commercially insured mothers within the same practices. Finally, to distinguish physician

behavior from practice-level effects, I implement a physician-movers design following the same clinician as their primary practice changes over time. I identify moves using NPES primary-practice listings, validate timing with billed well-child and screening claims, and estimate an event study around the move to test whether a physician’s screening behavior converges toward the destination practice’s environment.

The results show that screening rose more for Medicaid overall, but practices with higher Medicaid exposure saw especially large gains for commercially insured patients. Among commercially insured mothers, practices with full pre-policy Medicaid exposure experienced large increases in both the share screened and the share of well-child visits that included screening, relative to practices with little or no Medicaid exposure. Screening adoption for both patient groups was concentrated in high-Medicaid-share practices early in the policy period and persisted thereafter. Triple-difference estimates confirm that providers applied screening protocols uniformly across Medicaid and commercial patients, consistent with practice-level workflow changes rather than payer-targeted responses. The physician-movers analysis shows that when a clinician moves to a higher-Medicaid-share practice, their screening behavior shifts toward the destination practice’s screening environment by roughly 70%, similar to other estimates of place effects in health care (Molitor, 2018). These findings suggest that financial incentives at the practice level, rather than fixed physician propensities, are the main drivers of post-policy adoption.

This paper contributes to three strands of research. First, I contribute to the literature on spillovers from public to private insurance. A large body of work shows that changes in public payment systems can influence care delivered to privately insured patients through shared providers and market interactions. For example, Baicker et al. (2013) show that greater Medicare Advantage penetration reduces hospital spending for commercially insured patients; Clemens et al. (2025) demonstrate that the introduction of new Medicare billing codes led to gradual but heterogeneous take-up across physicians, highlighting how payment design and administrative frictions shape provider adoption; Clemens and Gottlieb (2017) document that Medicare fee changes spill over to private-sector physician prices; and Einav et al. (2020) and Barnett et al. (2020) find that Medicare bundled payment reforms altered treatment intensity beyond the targeted population. I extend this literature by focusing on mental health care, an area rarely studied in this context, and by using a direct, practice-level measure of Medicaid exposure that captures the financial incentives faced by providers rather than relying on geographic or population-level proxies. This approach allows me to identify how targeted Medicaid reimbursement can influence practice-wide adoption of screening protocols that reach commercially insured patients as well.

Second, I contribute to work on the organizational drivers of care delivery and the diffusion of clinical practices across payers. Providers operating under multiple payer arrangements often standardize workflows to reduce administrative burden and ensure compliance with quality met-

rics (Glied and Zivin, 2002; Baicker and Robbins, 2015; Barnett et al., 2020). By documenting uniform adoption of postpartum depression screening across payer types following a Medicaid reimbursement change, I show how organizational routines can translate payer-specific incentives into system-wide improvements in care delivery. This perspective highlights the importance of practice-level decision-making and offers a broader interpretation of how targeted public incentives can reshape clinical standards.

Finally, I contribute to the literature on physician behavior and the role of place versus person effects. Prior studies separate individual provider preferences from local practice environments by tracking patients or physicians who move across regions or institutions (Finkelstein et al., 2016; Molitor, 2018). These studies show that much of observed variation in treatment intensity reflects local environments rather than fixed provider propensities. I adapt this framework to a new setting, maternal mental health screening in pediatric care, and demonstrate that roughly half of a physician’s screening behavior adjusts to the destination practice after a move. This provides novel evidence that organizational context, rather than stable physician traits, drives much of the observed response to financial incentives.

The rest of the paper proceeds as follows. Section 2 provides institutional background on the Colorado Medicaid reimbursement policy and the clinical context for postpartum depression screening. Section 3 describes the linked Colorado All-Payer Claims Database and Vital Records data, outlines the construction of key variables, and summarizes sample characteristics. Section 4 examines the effect of Medicaid reimbursement on screening rates, first estimating the overall impact using a difference-in-differences framework and then extending the analysis to assess spillovers across payers through a triple-difference design that compares Medicaid and commercially insured patients within the same practices. Section 5 investigates mechanisms of adoption using a physician-movers design that distinguishes physician behavior from practice-level effects. Section 6 concludes with policy implications and directions for future research.

## 2 Background

Postpartum depression (PPD) is a prevalent and serious maternal health condition that often goes unrecognized in routine care. Obstetric care typically concludes after a single postpartum visit around six weeks after delivery, and as many as 40% of women do not attend that visit (American College of Obstetricians and Gynecologists, 2018). By contrast, mothers are far more likely to accompany their infants to regularly scheduled pediatric visits, creating repeated opportunities for early identification of depressive symptoms (Liberto, 2012; Earls et al., 2010). In recognition of this, the American Academy of Pediatrics (AAP) recommends that pediatricians screen for maternal depression during early infant well-child visits (Earls et al., 2010). Yet despite these guidelines, fewer than half of mothers with postpartum depressive symptoms are

identified in clinical settings, with major barriers including lack of reimbursement, limited referral pathways, and uncertainty about billing procedures (Yawn et al., 2012; Liberto, 2012).

In January 2014, Colorado implemented a policy authorizing pediatric and family medicine providers to bill Medicaid for postpartum depression screening conducted during infant well-child visits. Providers could bill under either the mother’s or the infant’s Medicaid identification number, and state guidance outlined validated screening tools and referral expectations. The policy aimed to increase screening and follow-up treatment among Medicaid-enrolled mothers, who face particularly high barriers to accessing postpartum mental health services. A recent evaluation by Gordon et al. (2025) found that the policy raised screening among Medicaid mothers by 9.6 percentage points relative to commercially insured mothers and also increased diagnosis and treatment rates. Similarly, Currie and Malinovskaya (2025) show that a comparable policy adopted in Michigan in 2018 roughly doubled screening rates, though treatment gains were concentrated in higher-income areas.

At the national level, the U.S. Preventive Services Task Force (USPSTF) issued a Grade B recommendation in 2016 for universal depression screening, including postpartum women. Under the Affordable Care Act, this required most commercial insurers to cover screening without cost sharing beginning in 2017. Because this change may have independently affected screening rates, I later verify that my results are robust when restricting the analysis to the pre-2017 period.

Colorado’s 2014 Medicaid policy thus provides a valuable setting to examine whether targeted reimbursement can generate broader changes in provider behavior. Once screening tools such as the Edinburgh Postnatal Depression Scale (EPDS) are integrated into practice workflows, the marginal cost of extending them to all patients is low, especially in practices serving both Medicaid and commercially insured families. This institutional feature allows for a clean test of whether Medicaid-specific payment incentives can reshape practice-wide screening routines and produce spillovers across payer types.<sup>1</sup>

## 3 Data

### 3.1 Data and Variable Definition

This study links multiple administrative and survey-based data sources to construct a comprehensive mother–infant panel spanning 2012–2019. Each dataset contributes distinct information on screening behavior, provider characteristics, and the socioeconomic environment of practice locations. The Colorado All-Payer Claims Database (APCD) serves as the primary source of information on utilization and billing, including detailed medical and pharmacy claims, enrollment, and payment data across Medicaid and commercial insurers. I use it to measure infant well-child

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<sup>1</sup>Appendix Figure E.2 reproduces the standard Edinburgh Postnatal Depression Scale (EPDS) screening form used in pediatric and primary care settings.

visits, postpartum depression (PPD) screening claims, and maternal insurance coverage. The Colorado Vital Records provide official mother–infant linkages and detailed maternal demographics and birth characteristics, allowing claims to be matched to maternal profiles. The National Plan and Provider Enumeration System (NPES) supplies information on each provider’s National Provider Identifier (NPI), primary practice address, and organizational affiliation, which I use to define consistent practice identifiers and track providers over time. Finally, I link area-level socioeconomic and demographic characteristics from the American Community Survey (ACS) to each practice location using ZIP Code Tabulation Areas (ZCTAs) to account for differences in neighborhood context.

Each claim in the APCD lists both a billing and a service provider National Provider Identifier (NPI). I link these NPIs to the NPES primary practice location file and assign a consistent practice identifier based on the billing provider’s primary address, which best reflects the locus of financial incentives and billing decisions. When multiple billing NPIs share the same address, I treat them as a single practice. The billing and service provider practice locations coincide for approximately 75% of well-child visits; in the remaining cases, billing is often centralized through larger networks or affiliated systems. To capture changes in provider location over time, I use the last-month NPES file from each quarter to extract quarterly practice addresses. If a provider’s practice location is missing in a given quarter, I impute it using the closest available quarter’s address. As a robustness check, I replicate key results using the service provider’s practice location, which yields similar estimates. To characterize provider types, I link taxonomy codes reported in the APCD to the National Uniform Claim Committee (NUCC) taxonomy crosswalk, which maps each NPI to a standardized specialty category. This classification identifies pediatric and primary care providers (internal medicine, family medicine, or general practice) and distinguishes organization types such as hospitals, ambulatory facilities, nursing facilities, agencies, and managed care organizations. The analytic sample is restricted to practices that include at least one pediatric or primary care provider in the pre-policy period.

The unit of analysis is a practice–year–quarter cell  $(p, t)$ . I examine two main outcomes. The first, Any screening, is an indicator equal to one if at least one postpartum depression screening was billed at practice  $p$  in quarter  $t$ . The second, Percent of visits screened, is the percentage of eligible well-child visits at practice  $p$  in quarter  $t$  with a billed screening claim. Eligible visits are those occurring within the infant’s first year of life, consistent with the policy’s coverage window. The Any screening outcome is analyzed without weights to capture adoption at the practice level, while the Percent of visits screened outcome is weighted by the number of well-child visits in the corresponding practice–quarter to reflect visit volume. For descriptive analyses and robustness checks, I also use visit-level data to visualize adoption dynamics.

A key variable in the analysis is each practice’s pre-policy Medicaid exposure, denoted  $\text{MedicaidShare}_p$ . This measure captures the extent of financial incentives each practice faced

when Colorado introduced Medicaid reimbursement for postpartum depression screening in 2014. To construct  $\text{MedicaidShare}_p$ , I use claims from 2012–2013 and assign each patient a primary payer at practice  $p$  in each year based on the majority of that patient’s claims at practice  $p$ . Among patients with any Medicaid or commercial claims at practice  $p$ , a patient is classified as Medicaid if Medicaid pays for the majority of their claims in that practice-year and commercial otherwise. Patients with only non-Medicaid or non-commercial payers are excluded, and exact ties are dropped. For each practice-year, I compute the share of classified patients who are Medicaid, then average the 2012 and 2013 shares to obtain a time-invariant baseline  $\text{MedicaidShare}_p$ . Practices are identified by the billing practice ID.

This measure reflects the proportion of a practice’s patient volume financed by Medicaid prior to the policy and therefore its relative financial exposure to the new reimbursement. Practices with higher pre-policy Medicaid shares faced stronger incentives to adopt screening once reimbursement became available. In all regression specifications, I control for the total number of infant well-child visits billed by each practice in a given year to account for differences in practice size and patient volume that may correlate with both payer mix and adoption behavior. For robustness, I confirm that the results are consistent when using the service provider’s practice identifier instead of the billing provider’s and that the two measures yield a highly correlated distribution of Medicaid shares.

Maternal insurance status is derived from the APCD enrollment file and classified as Medicaid or commercial based on the majority of coverage during the calendar year. This variable is used for subgroup analyses and as the Medicaid indicator in the triple-difference specification.

Covariates include patient, practice, and neighborhood characteristics that may influence screening rates. Maternal covariates, aggregated to the practice and quarter level as visit-weighted means, capture demographic and health attributes such as age, education, race and ethnicity, marital status, nativity, and indicators for chronic or pregnancy-related conditions. Practice-level controls include the number of physicians, total patient volume, the composition of pediatric, primary care, and OB/GYN physicians, and indicators for practice type, such as hospital-based, ambulatory, or managed care organization.

To account for local socioeconomic context, I link practice ZIP codes from NPPES to ZIP Code Tabulation Areas (ZCTAs) and merge year-specific American Community Survey (ACS) 5-year estimates from 2012 to 2019. From the ACS DP02, DP05, and S1901 tables, I extract measures of educational attainment, household income, language proficiency, nativity, fertility, and racial and ethnic composition. These ZIP-level covariates enter all regressions as time-varying controls to capture demographic and socioeconomic variation in practice surroundings. A full list of variable definitions appears in Appendix B., and the CPT and HCPCS codes used to identify PPD screening and well-child visits are detailed in Appendix C..

Unless otherwise noted, regressions are estimated at the practice-year-quarter level without

weights. Observations missing outcomes, insurance assignment, or key identifiers are excluded from the analytic sample.

### 3.2 Sample and Summary Statistics

I form practice–payer–quarter cells and compute two outcomes: an indicator for any PPD screening in the cell and the share of well-child visits with a billed screen. The baseline uses all practice–quarter observations. As summarized in Table 1, the analytic sample retains practices observed both pre-policy (2012–2013) and post-policy (2014–2019), yielding 658 practices, 5,838 pre-policy and 18,117 post-policy practice–quarter cells, and a total of 1,088,807 well-child visits. The share of practice–quarters with any PPD screening increased substantially after the policy, from 4.4 to 18.5 percent among Medicaid practices, and also rose among commercial practices, from 1.3 to 14.5 percent, despite no contemporaneous changes in federal or state reimbursement guidelines for commercial plans. This parallel rise motivates the analysis of potential spillover effects in subsequent sections.

Table 1: Sample and Summary Statistics

	All		Commercial		Medicaid	
	Pre	Post	Pre	Post	Pre	Post
Practices (count)	658	658	536	536	532	532
Practice–quarter observations	5,838	18,117	2,768	8,526	3,004	8,500
Well-child visits	219,102	869,705	52,244	257,665	166,746	603,919
Any screening (%)	2.9	15.9	1.3	14.5	4.4	18.5
	(16.8)	(36.5)	(11.5)	(35.2)	(20.6)	(38.8)
% WCVs screened (mean)	0.3	7.6	0.1	4.3	0.3	9.1
	(1.8)	(14.9)	(1.9)	(10.7)	(1.8)	(16.2)

*Notes:* Pre = 2012–2013; Post = 2014–2019. “Any screening” is the share of practice–payer–quarter cells with at least one PPD screen. “% WCVs screened” is the visit-weighted mean share of well-child visits with a billed PPD screen. Practices are restricted to those observed in both periods. Standard deviations are in parentheses below the mean. Source: Author’s calculations using Colorado All-Payer Claims Database (APCD) 2012–2019.

**Pre-policy Medicaid exposure.** Figure 1 plots the distribution of pre-policy Medicaid share across practices, averaged over 2012–2013. Practices vary widely in the proportion of their patients covered by Medicaid: some serve predominantly commercially insured families, while others draw mostly from Medicaid, with the median practice having roughly half of its patients enrolled in Medicaid. This variation captures meaningful differences in financial exposure to the 2014 policy, which reimbursed postpartum depression screening only for Medicaid-covered mothers. The dispersion in pre-policy Medicaid share provides the identifying source of cross-practice variation in the difference-in-differences design, where practices with higher baseline Medicaid shares faced stronger incentives to adopt screening.

Figure 2 presents the same distribution separately for mothers covered by commercial insurance and Medicaid, based on their infants’ visits to the same set of pediatric practices. Practices serving commercially insured mothers tend to have lower Medicaid shares on average, while



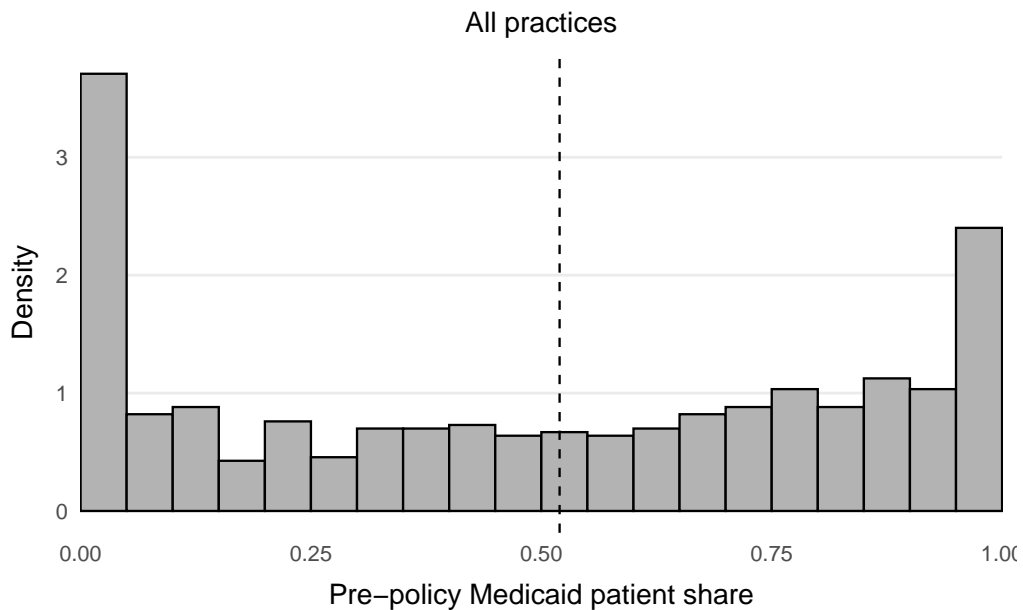


Figure 1: Distribution of Pre-Policy Medicaid Share.

*Notes:* Each bar shows the density of practices by their average share of Medicaid patients across 2012–2013. The measure is based on all claims at the billing-practice level and reflects the proportion of patients with Medicaid coverage among those with either Medicaid or commercial insurance. The dashed line marks the median practice. Number of practices: 658.

those serving Medicaid mothers have higher shares, yet there is substantial overlap across the two distributions. This overlap is central for identification: within the same statewide policy environment, both groups of mothers are seen at practices with differing levels of pre-policy Medicaid exposure, allowing comparisons of how screening behavior evolved across payer types while holding practice characteristics and broader policy shocks constant.

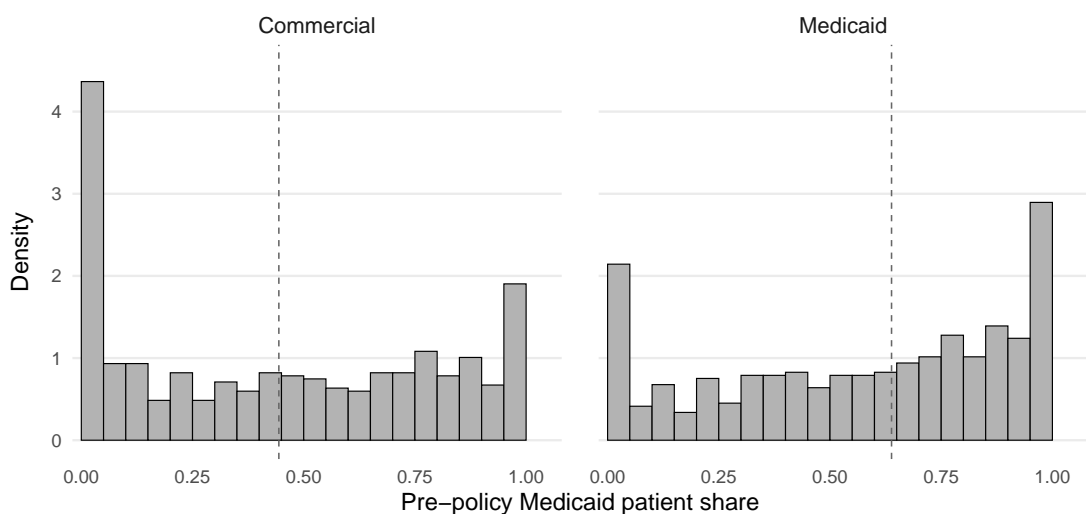


Figure 2: Distribution of Pre-Policy Medicaid Share by Payer Group.

*Notes:* Each panel shows the distribution of practices by pre-policy Medicaid patient share for visits billed to commercially insured (left) and Medicaid (right) mothers. Both panels use the same horizontal and vertical scales. Dashed lines indicate the median practice within each group. Distributions overlap substantially, supporting within-state comparisons across payer types. Number of practices: 536 (Commercial) and 532 (Medicaid).

Some commercially insured mothers appear in high-Medicaid-share practices, and conversely, some Medicaid-enrolled mothers visit low-Medicaid-share practices. This pattern arises because a mother’s insurance classification is based on her majority enrollment during the calendar year, whereas a practice’s Medicaid share is defined using the share of total billed claims across all patients seen in that practice in a given year. The two measures therefore capture related but

distinct aspects of coverage. Insurance coverage is also not always consistent over time: mothers can hold both Medicaid and commercial coverage during the postpartum year, and I classify each mother based on her majority coverage during that period. In addition, infant and maternal coverage can diverge. Infants born under Medicaid are automatically enrolled in Medicaid for their first year of life, but maternal eligibility changes shortly after delivery. During the study period, pregnant individuals in Colorado qualified for Medicaid up to 265 percent of the federal poverty level (FPL), while eligibility for low-income adults was limited to 138 percent FPL under the state’s Medicaid expansion in 2014.

## 4 Effect of Medicaid Reimbursement on Screening Rates

### 4.1 Impact on Overall Screening Rate

#### 4.1.1 Empirical Strategy

This section estimates how the 2014 Medicaid reimbursement policy for postpartum depression (PPD) screening affected screening rates at pediatric and primary care practices in Colorado. The empirical strategy exploits cross-practice variation in pre-policy Medicaid exposure as a continuous measure of treatment intensity under a single statewide policy change.

I examine two outcomes. The first is a binary indicator for whether a practice–payer–quarter cell recorded any PPD screening (Any screening). The second is the percentage of well-child visits in that cell that included a billed PPD screen (Percent screened). The first outcome captures the extensive margin of adoption—whether a practice ever screens—while the second measures the intensity of screening among practices that do.

I estimate the following difference-in-differences (DiD) model:

$$Y_{pt} = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{MedicaidShare}_p + \beta_3 (\text{Post}_t \times \text{MedicaidShare}_p) + X_{pt}\gamma + \epsilon_{pt}. \quad (1)$$

Here,  $Y_{pt}$  represents one of the two outcomes for practice  $p$  in year–quarter  $t$ ;  $\text{Post}_t$  equals 1 for quarters after January 2014 and 0 otherwise; and  $\text{MedicaidShare}_p$  is the practice’s pre-policy Medicaid patient share, defined using all patient visits in 2012–2013. The interaction term  $\text{Post}_t \times \text{MedicaidShare}_p$  captures the differential post-policy change in screening rates across practices with higher versus lower baseline Medicaid exposure.

The vector  $X_{pt}$  includes both quarterly and annual practice-level covariates. Quarterly covariates summarize the average patient characteristics for well-child visits in that practice–quarter, including maternal age, education, and area-level income and rurality. Annual covariates include practice-level characteristics constructed from provider taxonomies and patient volume, such as the total number of physicians, the counts of pediatric, primary care, OBGYN, and mental health providers, and the number of infants under age one visiting the practice in that year.

ZIP-code-level characteristics from the American Community Survey are also merged annually by practice location to capture local socioeconomic and demographic context, including income, education, race, and household composition. These covariates vary by year but are constant within each year-quarter.  $\epsilon_{pt}$  is the error term.

The coefficient of interest,  $\beta_3$ , measures how screening outcomes changed in the post-policy period as a function of a practice’s pre-policy Medicaid share. A positive  $\beta_3$  indicates that practices with a higher share of Medicaid patients experienced larger increases in screening following the policy. Coefficients are interpreted as the effect of a one-unit (100 percentage point) increase in pre-policy Medicaid share on the post-policy change in screening. Standard errors are clustered at the practice level to account for serial correlation, and all regressions are restricted to practices observed in both the pre- and post-policy periods. I estimate the model separately for Medicaid and commercially insured visits to assess whether the increase in screening extended beyond the directly targeted population.

#### 4.1.2 Identification

The identification strategy relies on two conditions. First, parallel trends: absent the policy, screening rates for practices with different pre-policy Medicaid shares would have evolved similarly. Second, the absence of sorting or compositional changes: the mix of patients visiting a given practice should not change systematically in ways correlated with pre-policy Medicaid exposure after the policy was introduced. Both conditions are supported by the institutional setting. The policy was implemented statewide with no phase-in or differential eligibility across practices.

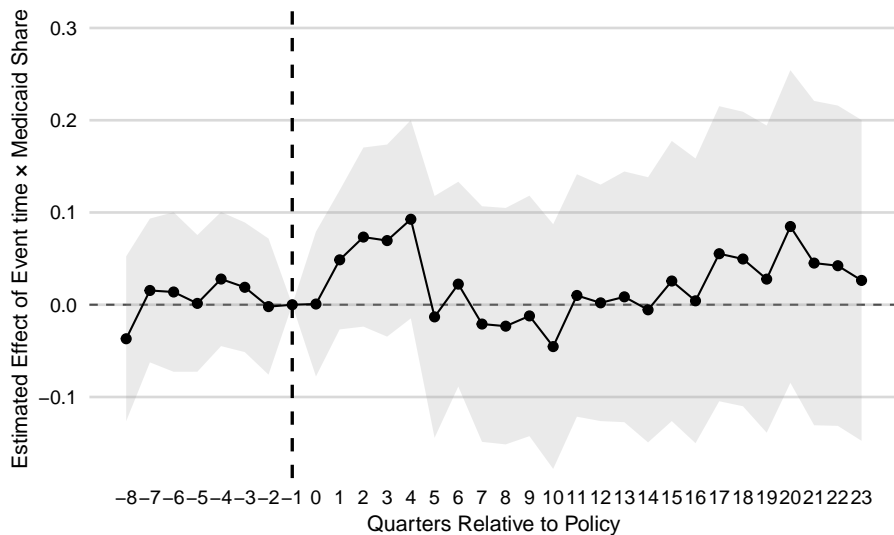
To assess the parallel trends assumption, I estimate an event-study specification:

$$Y_{pt} = \alpha_p + \lambda_t + \sum_{k \in \mathcal{K}, k \neq -1} \beta_k (D_k(t) \times \text{MedicaidShare}_p) + X_{pt}\gamma + \epsilon_{pt}. \quad (2)$$

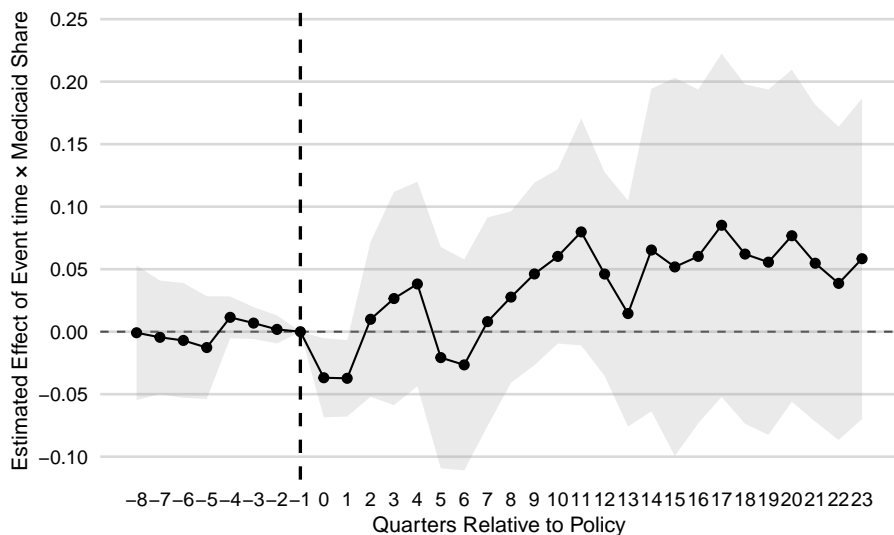
Here,  $D_k(t) = \mathbb{1}\{t - t_0 = k\}$  are event-time indicators relative to the policy quarter  $t_0 = 2014\text{Q1}$ , with  $k = -1$  (2013Q4) omitted as the reference period. The coefficients  $\beta_k$  trace the dynamic evolution of screening rates before and after the policy by pre-policy Medicaid share. Practice fixed effects  $\alpha_p$  absorb time-invariant differences across practices, while year-quarter fixed effects  $\lambda_t$  capture aggregate time shocks common to all practices.  $X_{pt}$  includes both quarterly and annual time-varying covariates as described above, and standard errors are clustered at the practice level.

Under the identifying assumptions, a positive and statistically significant  $\beta_3$  in the DiD specification, or upward shifts in post-2014 coefficients  $\beta_k$  in the event study, indicate that Medicaid reimbursement increased PPD screening, with stronger effects among practices that were more financially exposed to Medicaid before the policy.

To examine the dynamic effects of the policy and assess the identifying assumption of parallel trends, I estimate Eq. (2) separately for Medicaid and commercially insured patients. The outcomes are 1) an indicator for any screening during well-child visits and 2) the percentage of well-child visits screened. The first captures the extensive margin of adoption, while the second reflects screening intensity among visits. The coefficients  $\hat{\beta}_k$  trace how screening changed over time in practices with higher pre-policy Medicaid shares, relative to those with lower shares.



(A) Medicaid: Any screening



(B) Medicaid: Percentage of well-child visits screened

Figure 3: Event-study coefficients for Medicaid patients

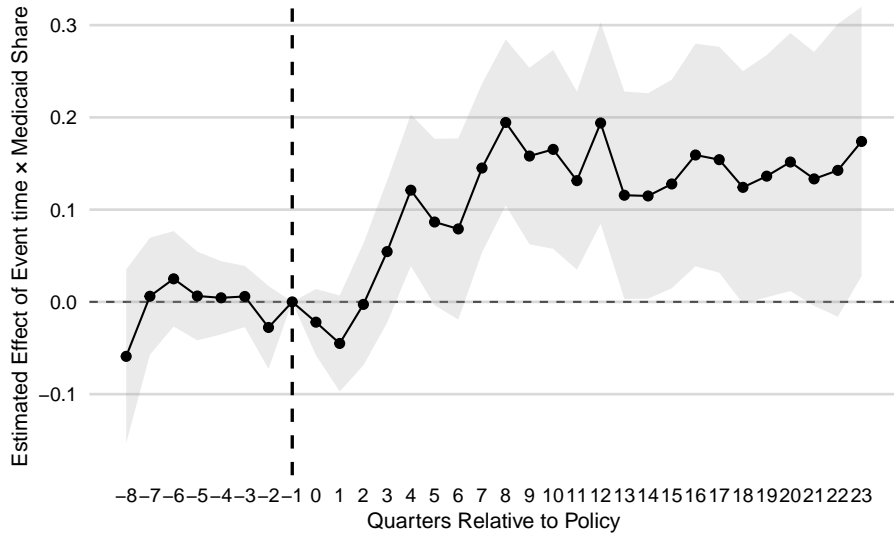
*Notes:* Each panel plots coefficients  $\hat{\beta}_k$  from Eq. (2), where  $D_k(t)$  are event-time indicators relative to the first quarter of 2014 and  $k = -1$  (2013Q4) is the omitted reference period. Shaded regions show 95% confidence intervals clustered by practice. The specification includes practice and year-quarter fixed effects and controls for time-varying ZIP-code and practice-level covariates  $X_{pt}$ . The vertical dashed line marks the quarter prior to the policy implementation, and the shaded area indicates the pre-policy period. Number of practices: 658. Source: Author's calculations using Colorado All-Payer Claims Database (APCD) 2012–2019.

**Joint pre-trend tests.** Figure 3 presents event study estimates for Medicaid patients, and Figure 4 shows corresponding estimates for commercially insured patients. For each insurance group  $g \in \{\text{Medicaid}, \text{Commercial}\}$ , I jointly test whether all pre-policy event-time coefficients

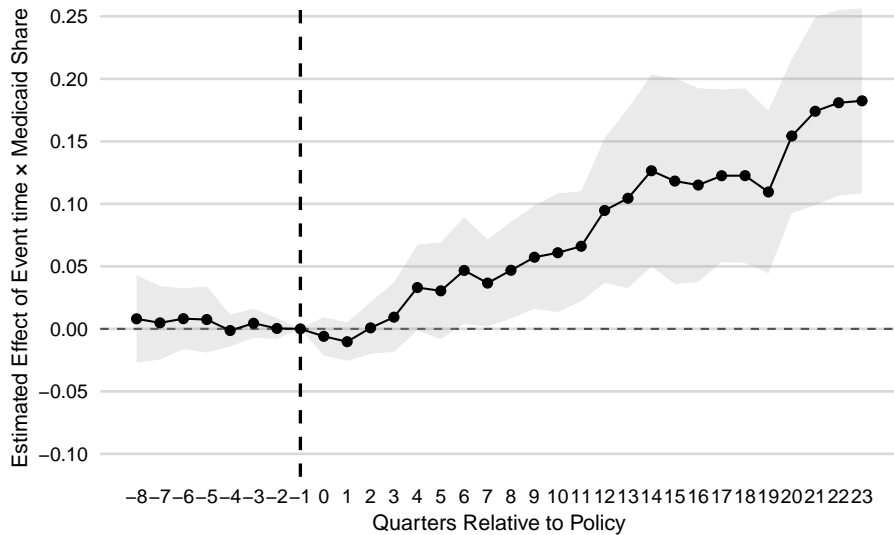
are equal to zero,

$$H_0 : \beta_k = 0 \quad \text{for all } k \in \{-8, -7, -6, -5, -4, -3, -2\},$$

with  $k = -1$  (2013Q4) omitted. Cluster-robust Wald tests (clustered by practice) fail to reject the joint null of no pre-policy effects at conventional significance levels for both Medicaid and commercial samples, providing evidence of parallel pre-trends prior to the 2014 policy.



(A) Commercial: Any screening



(B) Commercial: Percentage of well-child visits screened

Figure 4: Event-study coefficients for Commercial patients

*Notes:* Defined as in Fig. 3. 95% confidence intervals clustered by practice. Same sample, controls, and fixed effects as the Medicaid panel.

Across both insurance groups, pre-policy coefficients are small and jointly insignificant, indicating no systematic differences in screening trends across practices with different baseline Medicaid exposure prior to the policy. After 2014, screening rates among commercially insured patients rise more in practices with higher pre-policy Medicaid shares, and the event-study coefficients become positive and statistically distinguishable from zero in the commercial sample for both outcomes. In the Medicaid sample, post-policy coefficients are generally positive but imprecise and not statistically different from zero, consistent with the payer-specific DiD estimates that show statistically insignificant gradients for Medicaid. These event study analyses support

parallel pre-trends for both payer groups and indicate that the strongest post-policy divergence occurs for commercial patients. This is with practice-wide workflow adoption that spills over to commercially insured mothers rather than a Medicaid-only response especially for practices with higher pre-policy Medicaid patient share.

**Compositional change by pre-policy Medicaid exposure.** To assess whether observable maternal characteristics changed differentially across practices with varying pre-policy Medicaid exposure, I estimate difference-in-differences regressions at the practice-quarter level. Each regression interacts the post-policy indicator (2014–2019) with the practice’s pre-policy Medicaid share, estimated separately for the Medicaid and commercial samples. All models include practice and year-quarter fixed effects, cluster standard errors at the practice level, and weight by the number of eligible well-child visits in each cell. To adjust for multiple hypothesis testing across covariates, I report Benjamini–Hochberg false discovery rate (FDR)  $q$ -values within payer. Tables 3 and 2 report, for each covariate and payer, the estimated coefficient on the interaction term, with its standard error shown in parentheses below, and the corresponding  $p$ -value and FDR-adjusted  $q$ -value in separate columns.

After accounting for multiple testing, most maternal covariates show no differential changes by pre-policy Medicaid exposure between the pre- and post-policy periods. In the commercial sample (Table 3), a small number of socioeconomic and health characteristics remain statistically significant after FDR adjustment, but the estimated magnitudes are minor. In the Medicaid sample (Table 2), only shifts in racial composition reach statistical significance, with a slight decline in the share of White mothers and a corresponding increase among those classified as Other race. Overall, the magnitudes of these effects are small, suggesting that patient composition across practices remained largely stable before and after the policy.

These results suggest that the estimated effects of the policy are not driven by differing compositional changes across practices with varying pre-policy Medicaid shares. Combined with the event-study evidence of parallel pre-trends, this supports the validity of the identification strategy used in the main difference-in-differences analysis.

### 4.1.3 Results

Table 4 reports the difference in differences estimates for the impact of the Medicaid reimbursement policy on postpartum depression screening, estimated separately for Medicaid and commercially insured patients. Each specification includes practice and year-quarter fixed effects, with standard errors clustered at the practice level.

Screening rates rose more in practices with higher pre-policy Medicaid exposure, though the pattern differs across payer groups. Among Medicaid patients, post-policy increases were broad-based and did not vary by baseline exposure: the slope in pre-policy Medicaid share is small

Table 2: Maternal Covariate DiD by Pre-Policy Medicaid Share: Medicaid Sample

Covariate	Coefficient	p-value	FDR-adjusted $q$
Age, mean (years)	0.064 (0.45)	0.89	0.97
Number of prenatal visits, mean	-0.61 (0.38)	0.11	0.46
Income: \$0–14,999	-0.011 (0.022)	0.62	0.83
Income: \$15,000–24,999	0.024 (0.012)	0.046	0.17
Income: \$25,000–34,999	0.031 (0.053)	0.56	0.96
Income: \$35,000–49,999	0.0010 (0.0090)	0.97	0.97
Income: \$50,000–74,999	-0.022 (0.025)	0.38	0.82
Income: \$75,000+	-0.011 (0.010)	0.31	0.72
Urban	-0.00084 (0.0041)	0.84	0.90
Rural	0.023 (0.019)	0.23	0.71
Rural (isolated)	-0.099 (0.090)	0.27	0.72
Diabetes	-0.011 (0.010)	0.31	0.72
Hypertension	-0.022 (0.025)	0.38	0.82
Any chronic condition	0.029 (0.037)	0.45	0.84
Cesarean-section births	0.063 (0.040)	0.12	0.46
Pregnancy and delivery complications	-0.046 (0.025)	0.076	0.46
Hispanic	-0.023 (0.022)	0.30	0.72
Education: High school	0.090 (0.053)	0.091	0.46
Education: College	-0.022 (0.028)	0.43	0.84
White	-0.37 (0.11)	0.0013	0.036
Black	0.048 (0.038)	0.22	0.71
Asian	-0.023 (0.022)	0.30	0.72
Other race	0.35 (0.12)	0.0028	0.039
Married	0.0017 (0.0076)	0.82	0.89
Prenatal care initiated in first 3 months	-0.099 (0.090)	0.27	0.72
Preterm birth	-0.0041 (0.0069)	0.55	0.80
Born outside US	-0.011 (0.010)	0.31	0.72

*Notes:* Each row reports a separate difference-in-differences regression of the listed maternal covariate on the interaction  $\text{Post (2014–2019)} \times \text{pre-policy Medicaid share}$ , estimated for the Medicaid sample. Coefficients are reported with standard errors in parentheses below. Except for age and number of prenatal visits (reported in their natural units), all covariates are measured as proportions (0–1), and coefficients are expressed in percentage points. Each coefficient reflects the change in the outcome from the pre- to post-policy period associated with a 100% (one-unit) increase in a practice’s pre-policy Medicaid share. All models include practice and year-quarter fixed effects, are weighted by the number of eligible well-child visits, and cluster standard errors by practice. p-values are unadjusted; FDR-adjusted  $q$  values follow the Benjamini–Hochberg procedure. Source: Author’s calculations using Colorado All-Payer Claims Database (APCD) 2012–2019.

Table 3: Maternal Covariate DiD by Pre-Policy Medicaid Share: Commercial Sample

Covariate	Coefficient	p-value	FDR-adjusted $q$
Age, mean (years)	4.3 (1.8)	0.02	0.12
Number of prenatal visits, mean	0.94 (0.82)	0.25	0.47
Income: \$0–14,999	0.13 (0.086)	0.12	0.34
Income: \$15,000–24,999	-0.11 (0.090)	0.21	0.46
Income: \$25,000–34,999	0.092 (0.028)	0.0012	0.023
Income: \$35,000–49,999	-0.24 (0.17)	0.15	0.34
Income: \$50,000–74,999	0.15 (0.073)	0.042	0.17
Income: \$75,000+	0.046 (0.31)	0.88	0.91
Urban	-0.0095 (0.065)	0.88	0.91
Rural	-0.078 (0.083)	0.35	0.56
Rural (isolated)	0.0032 (0.026)	0.90	0.91
Diabetes	-0.0080 (0.014)	0.58	0.74
Hypertension	0.00074 (0.0068)	0.91	0.91
Any chronic condition	0.16 (0.11)	0.14	0.34
Cesarean-section births	0.086 (0.098)	0.38	0.65
Pregnancy and delivery complications	0.059 (0.030)	0.050	0.18
Hispanic	0.045 (0.12)	0.70	0.82
Education: High school	-0.061 (0.10)	0.56	0.74
Education: College	0.18 (0.080)	0.029	0.13
White	-0.031 (0.042)	0.46	0.64
Black	0.045 (0.019)	0.021	0.12
Asian	0.037 (0.041)	0.37	0.56
Other race	-0.051 (0.054)	0.35	0.56
Married	-0.12 (0.10)	0.24	0.47
Prenatal care initiated in first 3 months	0.12 (0.076)	0.12	0.34
Preterm birth	0.17 (0.056)	0.0026	0.024
Born outside US	0.030 (0.081)	0.71	0.83

*Notes:* Each row reports a separate difference-in-differences regression of the listed maternal covariate on the interaction  $\text{Post (2014–2019)} \times \text{pre-policy Medicaid share}$ , estimated for the commercial sample. Coefficients are reported with standard errors in parentheses below. Except for age and number of prenatal visits (reported in their natural units), all covariates are measured as proportions (0–1), and coefficients are expressed in percentage points. Each coefficient reflects the change in the outcome from the pre- to post-policy period associated with a 100% (one-unit) increase in a practice’s pre-policy Medicaid share. All models include practice and year-quarter fixed effects, are weighted by the number of eligible well-child visits, and cluster standard errors by practice. p-values are unadjusted; FDR-adjusted  $q$  values follow the Benjamini–Hochberg procedure. Source: Author’s calculations using Colorado All-Payer Claims Database (APCD) 2012–2019.



Table 4: Difference-in-Differences Estimates by Payer

	Medicaid				Commercial			
	Any Screening (1)	(2)	% WCVs Screened (3)	(4)	Any Screening (5)	(6)	% WCVs Screened (7)	(8)
Post-policy (2014+)	0.0856*** (0.0274)		0.0334 (0.0226)		0.0305* (0.0156)		-0.0144* (0.00765)	
Pre-policy Medicaid share	0.101*** (0.0324)		0.0667** (0.0296)		0.0576** (0.0250)		0.0102 (0.0149)	
Post $\times$ Medicaid share (DiD)	0.0124 (0.0412)	0.0192 (0.0415)	-0.0206 (0.0301)	0.0226 (0.0329)	0.0767** (0.0320)	0.109*** (0.0331)	0.0628*** (0.0180)	0.0696*** (0.0177)
Practice fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	11,336	11,332	11,336	11,332	11,144	11,143	11,146	11,145

*Notes:* Each column reports a separate regression at the practice-year-quarter level using billing-practice IDs. “Any screening” equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. “% WCVs Screened” is the share of well-child visits with a PPD screen. Standard errors in parentheses, clustered at the practice level. Percent-screening regressions are weighted by the number of well-child visits at that practice-quarter for the corresponding payer. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

and statistically indistinguishable from zero for both outcomes (columns 2 and 4). This suggests that once reimbursement became available, most practices adopted screening for their Medicaid patients regardless of their initial share of Medicaid enrollees.

For commercially insured patients, screening rose more sharply in high-Medicaid-share practices. The estimated gradients are positive and statistically significant for both outcomes (columns 6 and 8), implying increases of roughly 1.1 percentage points in the share screened and 0.7 percentage points in the share of well-child visits screened for every 10 percentage point higher pre-policy Medicaid share. Because baseline screening rates were near zero, these effects represent substantial post-policy gains concentrated among practices more financially exposed to Medicaid incentives.

The evidence suggests that practices serving more Medicaid patients before the policy were more likely to implement screening after reimbursement became available, and that this implementation extended to commercially insured visits within the same practices. The absence of a differential gradient among Medicaid visits indicates that screening for Medicaid patients increased broadly across practices, while commercial patients benefited more in high Medicaid share settings. This pattern supports the interpretation that the policy induced practice-level adoption rather than payer-specific behavior, consistent with the event study evidence of parallel pre-trends and post-policy divergence.

#### 4.1.4 Robustness

I assess the sensitivity of results to practice definition and sample period. The All-Payer Claims Database records both billing and service provider identifiers, which align for roughly 75 percent of well-child visits based on the NPES practice address. The main analysis uses billing identifiers, reflecting the locus of financial decision-making and reimbursement incentives. As a robustness check, I replicate the difference-in-differences model using the service-provider identifier, defined by the service provider’s primary practice address in the NPES. This alternative

definition more directly reflects where care is delivered, rather than where claims are processed or aggregated for billing purposes.

Table 5: Difference-in-Differences Estimates by Payer (Service-Provider ID)

	Medicaid				Commercial			
	Any screening (1)	(2)	% WCVs screened (3)	(4)	Any screening (5)	(6)	% WCVs screened (7)	(8)
Post-policy (2014+)	0.0757*** (0.0254)		0.0334 (0.0226)		0.0444** (0.0190)		-0.00521 (0.00807)	
Pre-policy Medicaid share	0.0524** (0.0243)		0.0667** (0.0296)		0.0205 (0.0224)		0.0189 (0.0138)	
Post $\times$ Medicaid share (DiD)	0.0580 (0.0431)	0.0651 (0.0430)	-0.0206 (0.0301)	0.0384 (0.0320)	0.111*** (0.0366)	0.116*** (0.0379)	0.0697*** (0.0185)	0.0696*** (0.0177)
Practice fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12,918	12,917	12,918	12,917	11,296	11,296	11,296	11,296

Notes: Each column reports a separate regression at the practice-year-quarter level using service-provider practice IDs. Any screening equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. % WCVs screened is the share of well-child visits with a PPD screen; those regressions are weighted by the number of eligible visits at that practice-quarter for the corresponding payer. Practice and year-quarter fixed effects absorb the main effects of Post and pre-policy Medicaid share in FE specifications, so those rows are blank in FE columns by design. Standard errors in parentheses, clustered at the practice level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

Table 5 reports the corresponding estimates. The results are highly consistent across specifications: practices with higher pre-policy Medicaid exposure continue to exhibit larger post-policy increases in screening, particularly among commercially insured patients. The estimated coefficients are similar in magnitude to those from the billing-provider specification, and the patterns remain statistically significant for the commercial sample but small and imprecise for Medicaid. This pattern suggests that Medicaid screening increased broadly across practices after the policy, while diffusion to commercial patients was concentrated among practices more exposed to Medicaid incentives. The consistency of these findings under both definitions indicates that the results are not driven by billing structure or attribution but reflect true behavioral responses within practices to the introduction of Medicaid reimbursement.

I also re-estimate the difference-in-differences models restricting the sample to the 2014–2016 period, before the USPSTF recommendation took effect in 2017. As discussed in Section 2, this update required commercial insurers to cover postpartum depression screening without cost sharing, potentially diluting the identifying variation in later years. The pre-2017 restriction yields similar results: the post-by-Medicaid-share coefficient remains positive and significant for commercially insured patients, with magnitudes comparable to the baseline estimates (Table 6). If commercial plans began reimbursing screening after 2017, increased screening among low-Medicaid-share practices would bias the DiD estimates toward zero, implying that the main estimates are, if anything, conservative. The persistence of the effect in the pre-2017 sample supports the interpretation that the observed increases reflect behavioral responses to the Medicaid reimbursement policy rather than subsequent documentation or coverage changes.

Table 6: Difference-in-Differences Estimates by Payer (Pre-2017 Sample)

	Medicaid				Commercial			
	Any screening (1)	(2)	% WCVs screened (3)	(4)	Any screening (5)	(6)	% WCVs screened (7)	(8)
Post-policy (2014+)	0.0759*** (0.0184)		0.0279 (0.0173)		0.0314*** (0.0108)		-0.00553* (0.00388)	
Pre-policy Medicaid share	0.0329 (0.0219)		0.0195 (0.0160)		0.0278 (0.0200)		0.000766 (0.00540)	
Post $\times$ Medicaid share (DiD)	0.0449 (0.0338)	0.0374 (0.0343)	-0.00227 (0.0237)	0.00950 (0.0149)	0.0670*** (0.0245)	0.0844*** (0.0265)	0.0372*** (0.0115)	0.0301*** (0.00792)
Practice fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Observations	7,592	7,590	7,593	7,590	7,123	7,122	7,130	7,122
$R^2$	0.008	0.399	0.017	0.611	0.011	0.387	0.024	0.431

Notes: Each column reports a separate regression at the practice-year-quarter level using billing-practice identifiers. Any screening equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. % WCVs screened is the share of well-child visits with a PPD screen; those regressions are weighted by the number of eligible visits at that practice-quarter for the corresponding payer. Fixed effects columns omit the main effects of Post and pre-policy Medicaid share by construction. Standard errors in parentheses, clustered at the practice level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

## 4.2 Impact Across Payers

### 4.2.1 Empirical Strategy

In the previous section, I examined how screening rates changed across practices with differing baseline Medicaid exposure using a difference-in-differences framework. Those results showed that screening rose more in high-Medicaid-share practices, especially among commercially insured patients, suggesting that the policy’s influence extended beyond directly affected Medicaid claims. To test whether this pattern reflects payer-specific responses or practice-wide adoption, I estimate a triple-difference (DDD) model for two outcomes: (i) whether a practice-quarter has any PPD screening (any screening) and (ii) the share of well-child visits that included a PPD screening (percent screened).

This specification compares changes in screening for Medicaid versus commercially insured patients within the same practice, before and after the policy, and across practices with different pre-policy Medicaid shares:

$$\begin{aligned}
Y_{pgt} = & \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{MedicaidShare}_p + \beta_3 \text{Medicaid}_g \\
& + \beta_4 (\text{Post}_t \times \text{MedicaidShare}_p) + \beta_5 (\text{Post}_t \times \text{Medicaid}_g) \\
& + \beta_6 (\text{MedicaidShare}_p \times \text{Medicaid}_g) + \beta_7 (\text{Post}_t \times \text{MedicaidShare}_p \times \text{Medicaid}_g) \\
& + X_{pgt} \gamma + \varepsilon_{pgt}.
\end{aligned} \tag{3}$$

Here,  $Y_{pgt}$  represents either (i) an indicator for any screening or (ii) the percentage of well-child visits at practice  $p$  for insurance group  $g$  in quarter  $t$  that included a screening.  $\text{Medicaid}_g$  equals 1 for Medicaid and 0 for commercial.  $\text{Post}_t$  and  $\text{MedicaidShare}_p$  are defined as before, and  $X_{pgt}$  includes time-varying practice-level and patient composition covariates.

The coefficient of interest,  $\beta_7$ , measures whether practices with higher baseline Medicaid

exposure experienced larger post-policy increases in screening for Medicaid patients relative to commercially insured patients. A positive and significant  $\beta_7$  would indicate a payer-targeted response, where financial incentives led practices to expand screening primarily for Medicaid patients. Conversely, an insignificant  $\beta_7$  coupled with a positive  $\beta_4$  (the DiD interaction) would imply practice-wide adoption—once screening was introduced in response to Medicaid reimbursement, practices applied it broadly across payer groups. This design thus distinguishes financial targeting from organizational spillovers within practices.

#### 4.2.2 Results

I extend the difference-in-differences analysis by comparing Medicaid and commercially insured patients within the same practice. The goal is to test whether practices with higher pre-policy Medicaid exposure expanded screening primarily for Medicaid patients (a payer-targeted response) or more uniformly across all patients (a practice-level adoption).

Table 7 compares within-practice changes for Medicaid versus commercially insured patients and allows the post-policy effect to vary with pre-policy Medicaid share. In the fixed-effects specification, the triple interaction  $\text{Post}_t \times \text{MedicaidShare}_p \times \text{Medicaid}_g$  is negative and statistically significant for the extensive margin (any screening), approximately  $-0.080$  with a standard error of about  $0.031$  (column 2), but is close to zero and statistically insignificant for the intensive margin (percent of well-child visits screened), roughly  $-0.0049$  with a standard error of about  $0.0089$  (column 4). The negative extensive-margin estimate indicates that, in higher-Medicaid-share practices, the post-policy increase in the probability that a quarter has any screening is smaller for Medicaid patients relative to commercial patients. Combined with the near-zero intensive-margin estimate, this pattern is consistent with practices adopting uniform workflows once screening is in place: the share of visits screened within screening-active quarters moves similarly across payers, while the expansion at the extensive margin is relatively larger for commercial patients in higher-Medicaid-share practices. This aligns with the DiD findings that show stronger post-policy gradients for commercial patients and supports a practice-level adoption story rather than payer-targeted implementation.

Across specifications, the evidence points to a practice-level behavioral response to Medicaid reimbursement rather than a payer-specific one. Practices with higher pre-policy Medicaid exposure were the most likely to increase screening following the introduction of reimbursement, and these gains extended to commercially insured patients. The absence of a positive triple-difference effect and the similar pre-policy trends across payers suggest that screening adoption reflected a change in clinical workflow rather than differential targeting of Medicaid patients. The results imply that financial incentives for one payer can influence broader provider behavior, generating cross-payer spillovers that improve care delivery beyond the directly affected population.

Table 7: Triple Difference-in-Differences Estimates

	Any Screening		% WCVs Screened	
	(1)	(2)	(3)	(4)
Post-policy (2014+)	0.0314** (0.0156)		-0.00384 (0.0117)	
Pre-policy Medicaid share	0.0307 (0.0240)		0.0283 (0.0185)	
Post $\times$ Medicaid share	0.0709** (0.0302)	0.110*** (0.0324)	0.0218 (0.0195)	0.0570** (0.0224)
Medicaid $\times$ Medicaid share	0.0751*** (0.0223)	0.182*** (0.0254)	0.0171* (0.00931)	0.0244*** (0.00884)
Post $\times$ Medicaid	0.0385** (0.0160)	0.0520*** (0.0173)	0.0154** (0.00632)	0.0203*** (0.00638)
Post $\times$ Medicaid $\times$ Medicaid share	-0.0422 (0.0287)	-0.0795** (0.0312)	-0.00174 (0.00936)	-0.00488 (0.00886)
Practice fixed effects	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes
Observations	23,589	23,586	23,589	23,586

*Notes:* Each column reports a separate regression at the practice-year-quarter level. “Any screening” equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. “% WCVs screened” is the share of well-child visits with a PPD screen; these regressions are weighted by the number of eligible visits at that practice in that quarter. Standard errors in parentheses, clustered at the practice level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

#### 4.2.3 Robustness

As a robustness check, I re-estimate the triple-difference model using the practice location linked to the service provider’s NPI rather than the billing provider’s address. This approach attributes screening activity to the physical site of care, capturing potential diffusion through co-located providers rather than shared billing systems.

Table 8 reports the results. The triple interaction term,  $\text{Post}_t \times \text{MedicaidShare}_p \times \text{Medicaid}_g$ , remains negative and statistically significant for the any-screening outcome in the specification with fixed effects ( $-0.069$ , s.e.  $0.029$ ), but close to zero and insignificant for the percent-screening outcome. The direction and magnitude of the coefficients closely match the billing-based estimates, indicating that the observed pattern is not sensitive to how practices are defined. The results again suggest that high-Medicaid-share practices did not disproportionately expand screening for Medicaid patients relative to commercially insured patients, consistent with practice-wide adoption and diffusion of screening protocols once reimbursement was introduced.

Restricting the sample to the pre-2017 period, before the USPSTF’s B-grade recommendation for postpartum depression screening took effect, produces consistent results. The triple-difference estimates in Table 9 mirror the main findings: the triple interaction term,  $\text{Post}_t \times \text{MedicaidShare}_p \times \text{Medicaid}_g$ , remains negative and statistically significant for the any-screening outcome in the specification with fixed effects ( $-0.073$ , s.e.  $0.031$ ) and close to zero for the percent-screening outcome. These results imply that, as in the main analysis, practices with greater Medicaid exposure continued to expand screening broadly across payer types, rather than disproportionately for Medicaid patients.

Because the USPSTF update in 2016 required commercial insurers to cover postpartum de-

Table 8: Triple Difference-in-Differences Estimates (Service-Provider ID)

	Any Screening		% WCVs Screened	
	(1)	(2)	(3)	(4)
Post-policy (2014+)	0.0174 (0.0163)		0.00630 (0.0127)	
Pre-policy Medicaid share	0.0129 (0.0226)		0.0296* (0.0166)	
Post $\times$ Medicaid share	0.0681** (0.0318)	0.0984*** (0.0330)	0.0268 (0.0228)	0.0420* (0.0251)
Medicaid $\times$ Medicaid share	0.0743*** (0.0219)	0.153*** (0.0241)	-0.00631 (0.00979)	0.0233*** (0.00691)
Post $\times$ Medicaid	0.0362** (0.0172)	0.0431** (0.0183)	0.00189 (0.00745)	0.0143** (0.00659)
Post $\times$ Medicaid $\times$ Medicaid share	-0.0451* (0.0264)	-0.0692** (0.0291)	0.0215* (0.0113)	0.00894 (0.0103)
Practice fixed effects	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes
Observations	25,134	25,132	25,134	25,132

Notes: Each column reports a separate regression at the practice-year-quarter level using service-provider practice identifiers. “Any screening” equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. “% WCVs screened” is the share of well-child visits with a PPD screen; these regressions are weighted by the number of eligible visits at that practice in that quarter. Standard errors in parentheses, clustered at the practice level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

pression screening without cost sharing starting in 2017, any resulting spillover would bias the difference-in-differences and triple-difference estimates toward zero. The persistence of the effects when restricting to 2014–2016 therefore reinforces that the observed screening increases were driven by the 2014 Medicaid reimbursement policy rather than by later coverage mandates or changes in coding and documentation practices.

Table 9: Triple Difference-in-Differences Estimates (Pre-2017 Sample)

	Any Screening		% WCVs Screened	
	(1)	(2)	(3)	(4)
Post-policy (2014+)	0.0341** (0.0150)		0.00844 (0.00578)	
Pre-policy Medicaid share	0.0326* (0.0188)		0.000118 (0.0108)	
Post $\times$ Medicaid share	0.0669** (0.0315)	0.109*** (0.0332)	0.0128 (0.0110)	0.0196*** (0.00706)
Medicaid $\times$ Medicaid share	0.0682** (0.0274)	0.175*** (0.0298)	0.0180*** (0.00587)	0.00915** (0.00393)
Post $\times$ Medicaid	0.0392** (0.0168)	0.0543*** (0.0180)	0.0122*** (0.00404)	0.0123*** (0.00361)
Post $\times$ Medicaid $\times$ Medicaid share	-0.0395 (0.0295)	-0.0725** (0.0308)	-0.000709 (0.00620)	0.00151 (0.00543)
Practice fixed effects	No	Yes	No	Yes
Year-quarter fixed effects	No	Yes	No	Yes
Observations	15,460	15,458	15,460	15,458
R-squared	0.194	0.501	0.194	0.501

Notes: Each column reports a separate regression at the practice-year-quarter level using billing-practice IDs, restricted to 2014–2016. “Any screening” equals 1 if at least one well-child visit at the practice in quarter  $t$  included a PPD screen. “% WCVs screened” is the share of well-child visits with a PPD screen; these regressions are weighted by the number of eligible visits at that practice in that quarter. Standard errors in parentheses, clustered at the practice level. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

## 5 Mechanism for Screening Adoption: Individual Physicians versus Practices

### 5.1 Empirical Strategy

The preceding results show that the Medicaid reimbursement led to broad increases in postpartum depression (PPD) screening across patients, consistent with practice-wide adoption. To further assess whether this diffusion reflects changes in individual physician behavior or institutional systems at the practice level, I adapt a movers framework following Finkelstein et al. (2016) and Molitor (2018). The analysis exploits physicians who switch billing practices within Colorado, allowing comparison of screening behavior before and after the move.

The intuition follows migration-style research designs that distinguish individual from place effects. If screening adoption reflects practice-level systems, such as EMR prompts, nurse-administered checklists, or standardized protocols for well-child visits, then a physician who moves from a low-screening to a high-screening billing practice should increase their screening rate after the switch. If screening reflects persistent physician-specific preferences or beliefs, the rate should remain unchanged. Observing how screening behavior changes around a move therefore helps identify whether adoption is driven by institutional incentives or individual persistence.

For each move episode  $m$  of physician  $j$ , I compute two measures of the difference in screening environments between the destination and origin billing practices:

$$\Delta_{jm}^{\text{pre}} = \text{Rate}_{\text{dest}(jm),\text{pre}}^{(-j)} - \text{Rate}_{\text{orig}(jm),\text{pre}}^{(-j)}, \quad \Delta_{jm}^{\text{post}} = \text{Rate}_{\text{dest}(jm),\text{post}}^{(-j)} - \text{Rate}_{\text{orig}(jm),\text{post}}^{(-j)},$$

where each rate is averaged over four quarters before and after the switch, excluding the moving physician (leave-one-out). These two measures capture the pre- and post-move differences in practice-level screening intensity, providing a concise summary of the screening environments experienced by movers. Calculating these practice rates in a leave-one-out manner ensures that a physician’s own screening behavior does not mechanically affect the measured screening rate of the origin or destination practice, isolating the practice-level environment they are exposed to.

The distribution of  $\Delta_{jm}^{\text{post}}$  is centered near zero, with substantial variation in both directions: many movers transition to practices with modestly higher or lower screening rates (for example,  $|\Delta^{\text{post}}| \leq 0.10$ ), while others experience much larger changes. The presence of both positive and negative gaps indicates that movers are exposed to a wide range of screening environments, providing identifying variation in cross-practice exposure.

I estimate a stacked event study at the provider–practice–quarter level, controlling for the same practice-level, ZIP-code, and maternal covariates used in the main difference-in-differences

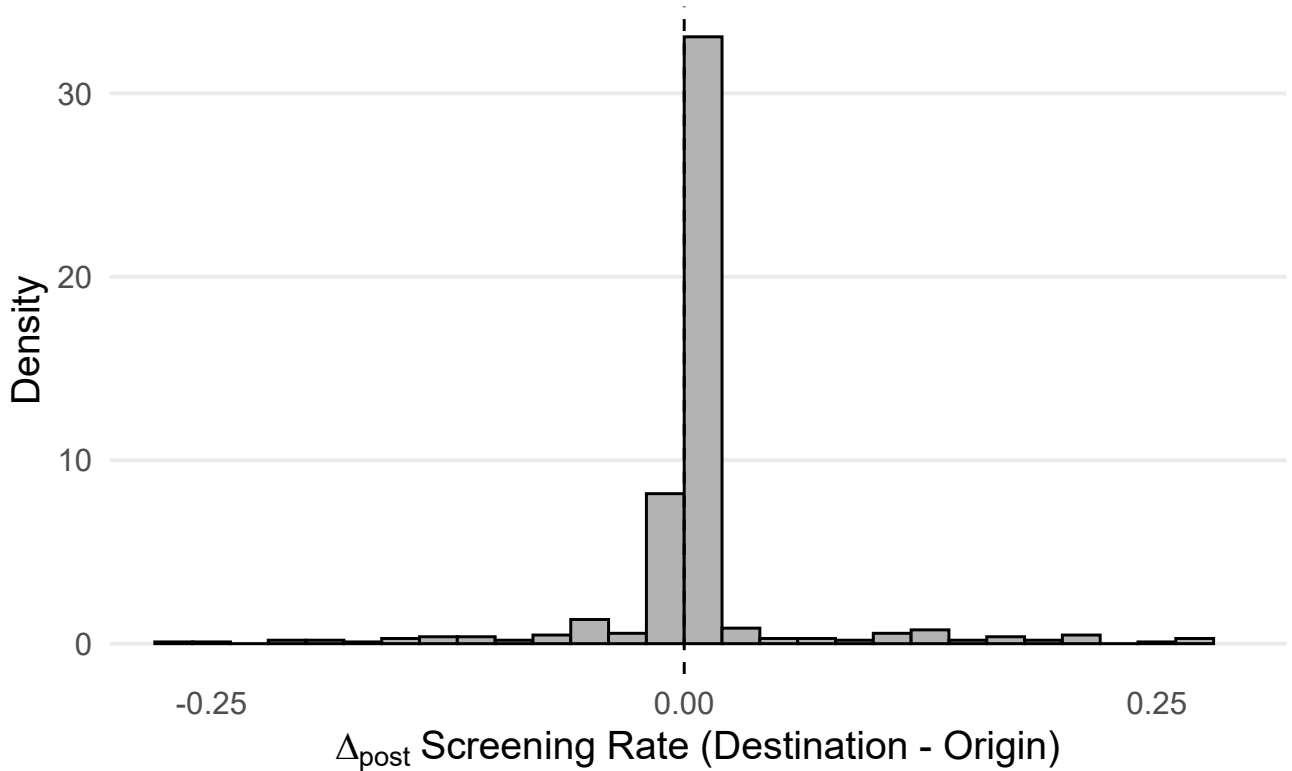


Figure 5: Distribution of Post-Move Differences in Screening Rates Across Moves

Notes: Histogram shows the distribution of post-move differences in average postpartum depression (PPD) screening rates between destination and origin billing practices,  $\Delta_{jm}^{\text{post}}$ . Each observation represents one physician move. The sample includes physicians observed for at least two quarters before and after a switch. Bars to the right (left) of zero indicate moves to practices with higher (lower) screening rates. Source: Author's calculations using Colorado All-Payer Claims Database (APCD) 2012–2019.

analysis:

$$Y_{jpmt} = \eta_{o(jm)} + \lambda_t + \sum_{k \in \mathcal{K}, k \neq -1} \theta_k \left[ \mathbf{1}\{\tau_{jmt} = k\} \times \Delta_{jm}^{\text{pre/post}} \right] + X_{pt}\gamma + \varepsilon_{jpmt}. \quad (4)$$

Here,  $Y_{jpmt}$  is the share of well-child visits with a PPD screen for physician  $j$  at practice  $p$  in quarter  $t$ . The interaction term uses  $\Delta_{jm}^{\text{pre}}$  for quarters before the move ( $\tau_{jmt} < 0$ ) and  $\Delta_{jm}^{\text{post}}$  for quarters after the move ( $\tau_{jmt} \geq 0$ ), corresponding to the leave-one-out difference in average screening rates between the destination and origin billing practices during the pre- and post-switch periods. The vector  $X_{pt}$  includes the same practice-, ZIP-code-, and maternal-level covariates as in the main DiD specification. All regressions are weighted by the number of well-child visits in each physician–practice–quarter cell. Origin-practice fixed effects  $\eta_{o(jm)}$  absorb baseline differences across billing systems, while  $\lambda_t$  controls for quarter-specific shocks common to all physicians. Standard errors are clustered by both physician and practice. This is the main specification. Appendix D. presents the event-study results estimated with provider fixed effects, showing that the pattern of adjustment is similar under this alternative specification.

The coefficients  $\theta_k$  trace how physician screening behavior responds to differences in screening intensity between destination and origin practices. Pre-move coefficients ( $k \leq -2$ ) should be close to zero if movers are not differentially trending before the switch. Positive post-move coefficients ( $k \geq 0$ ) indicate convergence toward the destination practice's screening rate per



unit of  $\Delta_{jm}^{\text{post}}$ , consistent with institution-driven changes in behavior. To summarize these effects, I also estimate a single difference-in-differences specification that interacts a post-move indicator with  $\Delta_{jm}^{\text{post}}$  ("Post  $\times \Delta^{\text{post}}$ ").

The movers sample includes physicians with at least two consecutive quarters at their origin billing practice and two at their destination, ensuring sufficient pre- and post-move observations. Moves are identified using the NPES primary billing practice address, which reflects the financial and administrative entity responsible for claims submission. The analysis is restricted to allopathic and osteopathic physicians whose primary specialty is pediatrics or primary care, as these providers are eligible to bill for postpartum depression screening under Medicaid guidelines. This design provides a direct test of whether the diffusion of screening practices occurred through billing-level organizational systems or through individual physician persistence.

Table 10 compares physicians who switched primary billing practices ("movers") to those who remained at the same practice ("non-movers") during the post-policy period (2014–2019). For movers, practice characteristics are measured based on their origin billing practice prior to the switch. For each physician, I compute the yearly average ZIP-code and practice-level characteristics of their primary billing location between 2014 and 2019, then take the mean across years to obtain post-policy averages. These physician-level averages are subsequently averaged across movers and non-movers. Because practices with more pediatric or primary care physicians contribute more individual observations, they implicitly receive greater weight in the comparison.

Movers and non-movers exhibit broadly similar ZIP-level and household characteristics, including education, income, and racial composition, suggesting that mobility is not systematically concentrated in distinct socioeconomic areas. This similarity supports the credibility of the movers framework by showing that differences in community context are unlikely to bias within-physician estimates of behavioral change.

The main differences appear at the practice level. Movers tend to come from smaller practices, with fewer total physicians and infants served on average, and are somewhat less likely to be affiliated with hospitals or managed care organizations. This pattern suggests that mobility is more common among physicians in smaller or independent practices, where structural changes and practice consolidation occur more frequently. While movers may not represent all physicians statewide, the event study isolates within-physician adjustments in screening behavior, holding constant individual characteristics through fixed effects. The comparison therefore supports that the observed convergence toward destination practice behavior reflects place-based adaptation rather than selection into systematically different environments.

Table 10: Movers vs. Non-Movers: Post-Policy Practice and ZIP Characteristics (2014–2019)

	Non-movers	Movers
<b>ZIP-level characteristics (%)</b>		
High school or higher	91.42 (6.83)	90.85 (6.34)
Bachelor's degree or higher	42.06 (17.35)	41.22 (17.61)
Foreign-born	10.13 (6.45)	10.05 (5.39)
English only	83.09 (10.17)	82.48 (9.05)
Spanish spoken	10.85 (9.27)	12.05 (8.92)
Non-English spoken	16.91 (10.17)	17.52 (9.05)
White	83.30 (10.21)	84.36 (9.78)
Black	4.45 (6.44)	3.57 (5.61)
Asian	3.33 (2.90)	2.98 (2.61)
Other race	4.22 (3.49)	4.44 (3.74)
Female	50.48 (2.95)	50.42 (2.49)
Age 20–64	62.72 (7.47)	62.31 (7.07)
Total households	11,177 (5,401)	11,561 (5,472)
Average household size	2.44 (0.35)	2.47 (0.34)
Total population	28,144 (14,583)	29,574 (14,610)
Mean household income (\$)	82,765 (26,896)	83,363 (26,737)
Median household income (\$)	63,655 (21,760)	63,370 (19,479)
<b>Practice characteristics</b>		
Number of infants	1,549 (3,610)	1,006 (2,995)
Total physicians	61.75 (115.58)	35.80 (74.65)
Hospital (%)	11.8 (32.3)	6.8 (25.2)
Ambulatory facility (%)	33.2 (47.1)	33.6 (47.3)
Agency (%)	1.0 (10.1)	1.5 (12.1)
Managed care organization (%)	6.6 (24.8)	2.2 (14.5)
Nursing facility (%)	0.2 (4.4)	0.0 (0.0)
Number of physicians	3,739	404

*Notes:* Means with standard deviations in parentheses. ZIP and practice characteristics are averaged over 2014–2019 (post-policy period). Each observation corresponds to a physician's primary billing practice; for movers, characteristics are measured using their origin billing practice prior to the switch. Movers are physicians who switched primary billing practices during 2014–2019 with at least two consecutive quarters observed at both the origin and destination. Non-movers remained in the same practice over the same period.

## 5.2 Results: Physician Movers

This section examines whether the observed practice-level adoption of postpartum depression (PPD) screening reflects physician-specific behavior or adaptation to practice-level systems. Figure 6 plots the event-study coefficients  $\theta_k$  from equation (4), which interact relative event time with the difference in screening intensity between the physician’s destination and origin practices. The estimates are centered at the quarter of the move ( $\tau = 0$ ), with  $\tau = -1$  omitted.

The event-study results indicate minimal movement in screening behavior before the switch. Pre-period coefficients are close to zero and not statistically different from one another, suggesting that movers and non-movers followed similar trajectories prior to changing practices. After the switch, screening rates among movers rise and begin to align with the average level of their destination practice. The increase stabilizes within a few quarters, suggesting that physicians adapt quickly once they are exposed to new practice environments. This pattern supports the interpretation that institutional features such as electronic medical record prompts, billing templates, and staff-administered checklists play a central role in shaping screening practices, independent of individual physician preferences.

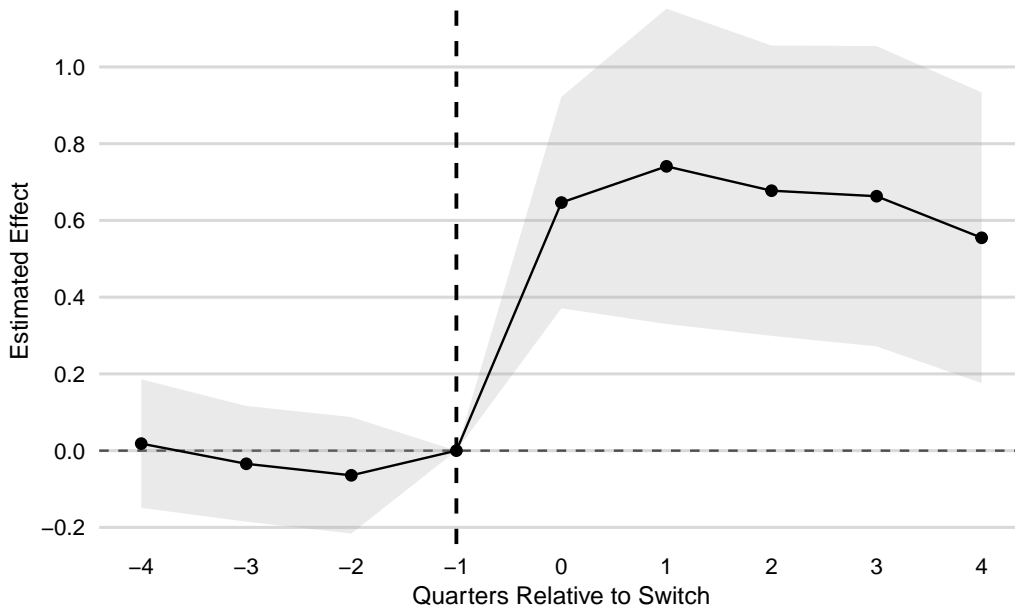


Figure 6: Physician Movers Event Study: 4-Quarter Window

*Notes:* Points plot coefficients from a stacked event study regression of the physician–practice–quarter screening rate on event-time indicators interacted with the leave-one-out difference in average screening rates between the destination and origin billing practices.  $\Delta_{jm}^{\text{pre}}$  and  $\Delta_{jm}^{\text{post}}$  represent average screening gaps in the four quarters before and after each move. Event time is measured in quarters relative to the provider switch. The model includes origin-practice and quarter fixed effects and controls for the same practice-, ZIP-code-, and maternal-level covariates as in the main difference-in-differences specification. Standard errors are clustered by physician and practice. The vertical dashed line marks the quarter before the move ( $t = -1$ ), and shaded areas show 95% confidence intervals.

Table 11 presents the corresponding DiD results. Across all specifications, the interaction term between post-switch and the destination–origin screening gap is positive and significant, with an estimated coefficient around 0.7. This implies that when a physician moves to a practice with a 10–percentage-point higher screening rate than their origin (measured excluding their own behavior), their own screening rate rises by roughly 7 percentage points. The estimates are

stable across specifications that include origin-practice, physician, and year-quarter fixed effects.

Overall, the results indicate that the diffusion of screening practices operates partly through physician adaptation to new organizational environments. When physicians move to practices with higher baseline screening rates, their own screening behavior rises proportionally, suggesting that institutional factors such as billing protocols, EMR prompts, or standardized workflows play a substantial role in shaping individual practice patterns. The magnitude of the response, roughly 70 percent convergence to the destination practice’s screening level, points to an important role for practice-level systems in driving adoption, rather than screening behavior being solely determined by physician-specific preferences or beliefs.

Table 11: Physician Movers: Difference-in-Differences Estimates

	(1)	(2)	(3)
Post-switch	-0.006 (0.007)	-0.016* (0.009)	-0.010 (0.009)
$\Delta$ Screening (dest – origin)	0.021 (0.118)	-0.238 (0.194)	-0.472*** (0.147)
Post-switch $\times$ $\Delta$ Screening	0.677*** (0.163)	0.700*** (0.158)	0.695*** (0.165)
Origin practice FE	Yes		Yes
Provider FE		Yes	Yes
Year-quarter FE	Yes	Yes	Yes
Observations	3,314	3,301	3,301

*Notes:* Outcome is the share of well-child visits with a billed PPD screen in the physician–practice–quarter cell. “Post-switch” equals 1 for quarters after the physician changes billing practices.  $\Delta$ Screening is the leave-one-out difference in average screening rates between destination and origin billing practices in the post period. Models include the same maternal, practice-level, and ZIP-code covariates as in the main specification. Standard errors are clustered by physician and practice. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .

### 5.3 Robustness

**Alternative event windows.** As a robustness check, I extend the event-study window to include eight quarters before and after each physician move. This specification allows for a longer adjustment period and provides an additional test of pre-trend stability. The estimation follows the same specification as in equation (4), interacting event-time indicators with the corresponding pre- and post-move leave-one-out screening gaps. For this version,  $\Delta_{jm}^{\text{pre}}$  and  $\Delta_{jm}^{\text{post}}$  are computed as the average differences in screening rates between the destination and origin billing practices over the eight quarters before and after each move, respectively. The regression includes the same set of practice-, ZIP-code-, and maternal-level covariates and fixed effects as in the main specification.

The results, shown in Figure 7, confirm the main findings. Coefficients in the pre-period remain close to zero, suggesting no anticipatory changes in screening behavior prior to the switch. Post-move coefficients rise sharply in the first few quarters after the move and then stabilize, consistent with convergence toward the destination practice’s screening rate. The magnitude of the post-move response is somewhat larger than in the four-quarter specification, likely reflecting

the use of longer averaging windows that smooth quarter-to-quarter noise and capture more sustained changes in physician behavior. Overall, the extended event window reinforces the conclusion that the adoption of postpartum depression screening is largely driven by practice-level systems that shape physician behavior after switching billing environments, rather than by persistent physician-specific screening preferences.

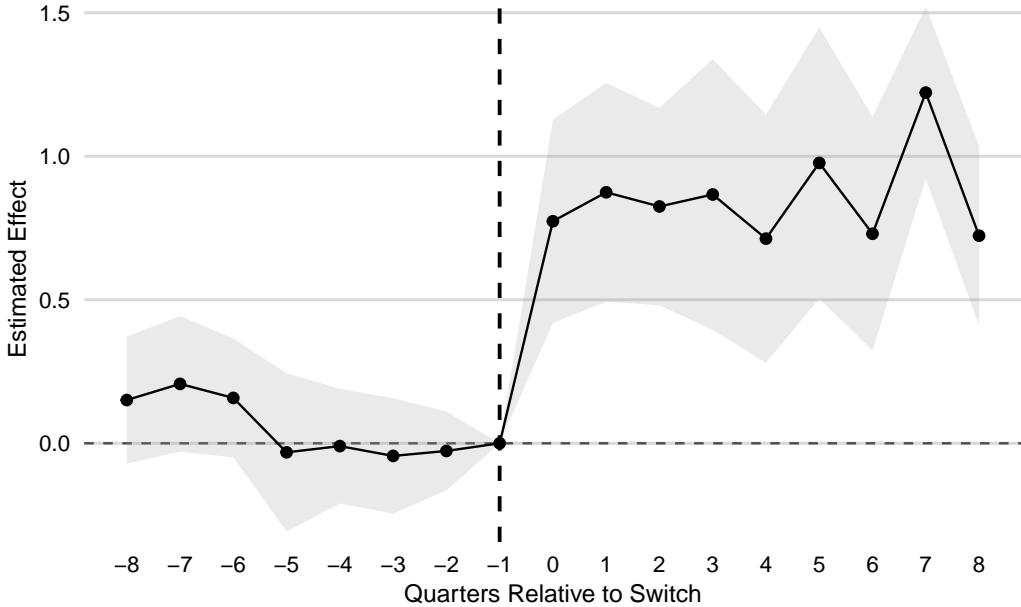


Figure 7: Physician Movers Event Study: Extended 8-Quarter Window

Notes: Points plot coefficients from a stacked event study regression of the physician–practice–quarter screening rate on event-time indicators interacted with the leave-one-out difference in average screening rates between the destination and origin billing practices.  $\Delta_{jm}^{\text{pre}}$  and  $\Delta_{jm}^{\text{post}}$  represent average screening gaps in the eight quarters before and after each move. Event time is measured in quarters relative to the provider switch. The model includes origin-practice and quarter fixed effects and controls for the same practice-, ZIP-code-, and maternal-level covariates as in the main difference-in-differences specification. Standard errors are clustered by physician and practice. The vertical dashed line marks the quarter before the move ( $t = -1$ ), and shaded areas show 95% confidence intervals.

Overall, the extended event window reinforces the conclusion that the adoption of postpartum depression screening is largely driven by practice-level systems that influence physician behavior after switching billing environments, rather than by persistent physician-specific screening preferences.

**Restricting to moves involving ZIP code changes.** As an additional robustness test, I restrict the physician movers sample to switches in which the billing practice’s ZIP code changes between the origin and destination practices. This restriction helps ensure that the identified moves correspond to genuine practice transitions rather than administrative reassignments within the same location or billing network. The specification follows equation (4), interacting event-time indicators with the leave-one-out differences in screening rates between the origin and destination practices. The regression includes the same set of practice-, ZIP-code-, and maternal-level covariates and fixed effects as in the main event study.

The results, presented in Figure 8, remain consistent with the baseline analysis. Screening rates show no differential trends prior to the move, and post-switch coefficients display a similar pattern of rapid convergence toward the destination practice’s screening rate. The close

correspondence between these estimates and the main specification indicates that the observed changes are not driven by physicians who remain within the same geographic or administrative system. Instead, the results reinforce that practice-level systems, rather than individual preferences, drive the observed behavioral adjustments.

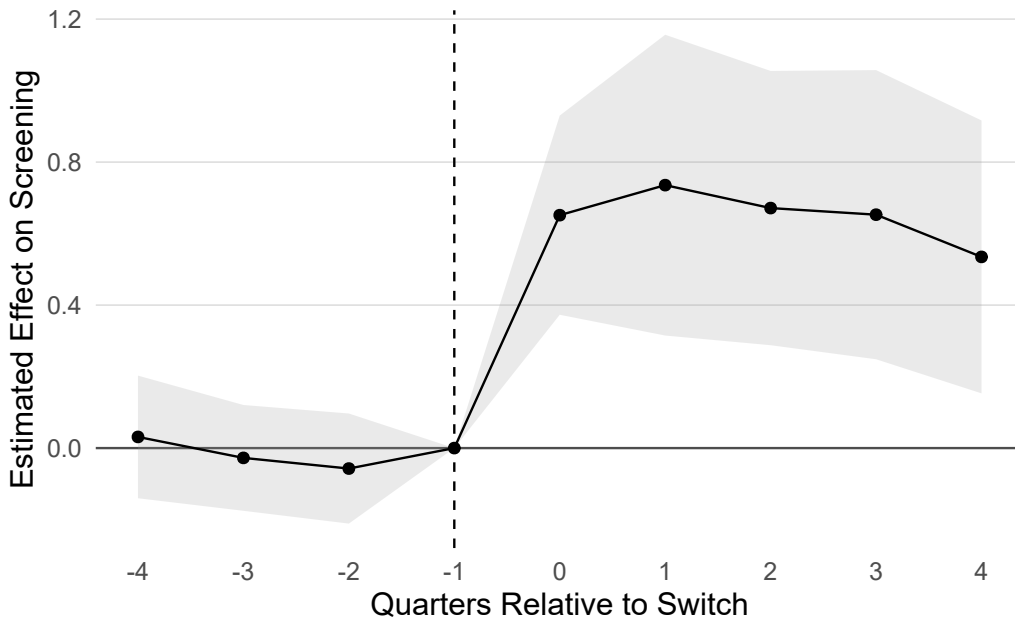


Figure 8: Physician Movers Event Study: Restricting to Moves with ZIP Code Change

Notes: Points plot coefficients from a stacked event study regression of the physician–practice–quarter screening rate on event-time indicators interacted with the leave-one-out difference in average screening rates between the destination and origin billing practices. The sample is restricted to physicians whose billing practice ZIP codes differ between origin and destination. Event time is measured in quarters relative to the provider switch. The model includes origin-practice and quarter fixed effects and controls for the same practice-, ZIP-code-, and maternal-level covariates as in the main difference-in-differences specification. Standard errors are clustered by physician and practice. The vertical dashed line marks the quarter before the move ( $t = -1$ ), and shaded areas show 95% confidence intervals.

Appendix Table D.7 reports the corresponding difference-in-differences results. The estimates remain highly consistent with the main specification: the post-switch interaction between the leave-one-out screening gap and the post indicator is positive and statistically significant, confirming that the convergence in screening behavior reflects genuine practice transitions rather than administrative changes within billing systems.

## 6 Conclusion

This paper examines how targeted Medicaid reimbursement for postpartum depression (PPD) screening shaped provider behavior and generated cross-payer spillovers. Using linked birth records and the Colorado All-Payer Claims Database from 2012–2019, I find that the introduction of Medicaid reimbursement for an existing screening code led to broad increases in PPD screening among Medicaid patients across all practices, consistent with widespread adoption of reimbursable screening. Consequently, the difference-in-differences estimates for the Medicaid group are not statistically significant, reflecting uniform improvements across practices regardless of their baseline Medicaid exposure. In contrast, screening among commercially in-

sured mothers rose more sharply in high-Medicaid-share practices, indicating that providers in Medicaid-exposed settings adopted standardized screening workflows and applied them to all patients, not just those covered by Medicaid. The triple-difference estimates further confirm this interpretation: conditional on a practice’s baseline Medicaid share, post-policy gains were not disproportionately concentrated among Medicaid patients, consistent with payer-neutral, practice-wide adoption.

The physician movers analysis provides complementary evidence on mechanisms. When physicians switched billing practices, their screening behavior adjusted sharply toward the destination practice’s norms, converging by roughly 70 percent within the first year after the move. The extended eight-quarter event study confirms this pattern: no pre-trends are observed, and screening rates rise immediately following the move, consistent with physicians adopting the institutional routines of their new billing environment. These findings suggest that practice-level systems, rather than fixed physician preferences, drive much of the observed diffusion in screening behavior.

The results have several policy implications. First, targeted public payment reforms can reshape provider behavior more broadly than intended when they alter organizational routines that affect all patients. Designing reimbursement incentives with practice-wide implementation in mind may thus amplify their total impact. Second, because physicians adapt rapidly to new organizational environments, interventions that reach provider groups or practices, rather than individual clinicians, may be more effective in achieving sustained behavioral change. Finally, in maternal mental health care, where screening can be easily integrated into existing well-child workflows, such practice-level diffusion offers a scalable mechanism for improving detection and treatment of postpartum depression.

Overall, the evidence shows that targeted Medicaid reimbursement not only increased screening among publicly insured mothers but also elevated screening standards across payer types. In this setting, a modest payment policy catalyzed organization-wide behavioral change, illustrating how public insurance reforms can diffuse through shared delivery systems to enhance maternal and child health at a population level.

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## Appendix A. Sample and Variable Construction

Data for this study were drawn from the Colorado All-Payer Claims Database (APCD), which includes a supplemental file containing Marketplace plan information and birth records for all Colorado births. The APCD is maintained by the Center for Improving Value in Health Care (CIVHC), a non-profit organization that administers the database on behalf of the State of Colorado. Birth records were obtained from the Colorado Department of Public Health and Environment (CDPHE). The Colorado APCD captures approximately 70 percent of insured lives statewide and includes claims from Health First Colorado (the state’s Medicaid program), the Children’s Health Insurance Program (CHIP), the state’s largest commercial insurers, and both ERISA and non-ERISA self-insured employers that submit data voluntarily. Roughly one-quarter of ERISA self-insured employers and about one-half of all self-insured employers in Colorado participate in the APCD. Because ERISA plans cannot be legally required to report data under the *Gobeille v. Liberty Mutual* (2016) Supreme Court decision, the APCD contains all Medicaid-financed births but only a subset of commercially insured births.

The linkage between the Colorado APCD and state Vital Records was conducted by CDPHE using a deterministic matching process based on unique identifiers available in both datasets: social security number, date of birth, and first and last name. CDPHE has used the same procedure to link other administrative datasets to Vital Records and achieved a 96.7 percent match rate for births identified in the APCD.

The National Plan and Provider Enumeration System (NPES) is a national registry maintained by the Centers for Medicare & Medicaid Services (CMS) that contains information on all providers and organizations issued a National Provider Identifier (NPI) in the United States. The NPES includes each provider’s unique NPI, taxonomy classification, primary practice address, and organizational affiliation. I use the NPES to define consistent practice identifiers across years and to map billing and service provider NPIs in the APCD to physical practice locations. When multiple NPIs share the same primary practice address, they are grouped into a single practice identifier.

Provider specialty information in the NPES was supplemented with taxonomy definitions from the National Uniform Claim Committee (NUCC), which maintains the standardized health care provider taxonomy code set used across public and private insurance billing systems. The NUCC taxonomy crosswalk allows consistent identification of pediatric, family medicine, internal medicine, and obstetrics and gynecology providers within the APCD.

To capture neighborhood socioeconomic context, I linked each practice location to ZIP Code Tabulation Areas (ZCTAs) using a ZIP–ZCTA crosswalk and merged in ZIP-level measures from the American Community Survey (ACS) 5-year estimates (2012–2019). The ACS, administered by the U.S. Census Bureau, provides detailed information on income, educational attainment, language proficiency, nativity, fertility, and racial and ethnic composition. These measures are

merged as time-varying contextual covariates that account for socioeconomic differences across practice locations.

To evaluate the representativeness of the analytic sample, I compare the racial and ethnic composition of linked APCD–Vital Records births to all statewide births reported in the Colorado Vital Records. The analytic sample includes all Medicaid-financed births and approximately 28 percent of privately insured births in Colorado between 2012 and 2019. Coverage of Medicaid births is nearly complete because the APCD includes all Health First Colorado claims, whereas coverage of privately insured births is partial due to the exclusion of self-insured ERISA plans. As shown in Tables A.1 and A.2, the analytic sample closely mirrors the statewide distribution of births by race and ethnicity for both payer groups.

Table A.1: Comparison with Statewide Private Insurance Births (2012–2019)

Race/Ethnicity	All Private Insurance Births (CO Vital Records)		Analytic Sample (Linked APCD + Birth Records)	
	Number of births	Percent	Number of births	Percent
White	239,702	88.2	66,063	86.7
Black	7,563	2.8	2,195	2.9
Asian	14,516	5.3	3,761	4.9
Other or Unknown	10,008	3.7	4,127	5.4
<b>Total</b>	271,773	100.0	76,145	100.0
Hispanic (any race)	38,799	14.3	12,861	16.9

*Notes:* Race categories are not mutually exclusive with Hispanic ethnicity. The analytic sample is restricted to infants whose well-child visits occurred at practices with claims in the pre-policy period (2012–2013) and at least one pediatric or primary care provider. These practices account for approximately 28% of all privately insured births in Colorado. Source: Author’s calculations using Colorado Vital Records and Colorado All-Payer Claims Database (APCD) 2012–2019.

Table A.2: Comparison with Statewide Medicaid Births (2012–2019)

Race/Ethnicity	All Medicaid Births (CO Vital Records)		Analytic Sample (Linked APCD + Birth Records)	
	Number of births	Percent	Number of births	Percent
White	150,528	76.4	148,377	76.0
Black	18,957	9.6	18,075	9.3
Asian	7,398	3.8	6,325	3.2
Other or Unknown	23,756	12.1	22,476	11.5
<b>Total</b>	196,925	100.0	195,253	100.0
Hispanic (any race)	92,772	47.1	88,556	45.4

*Notes:* Race categories are not mutually exclusive with Hispanic ethnicity. The analytic sample includes 98.6% of all Medicaid-financed births in Colorado between 2012 and 2019. Source: Author’s calculations using Colorado Vital Records and Colorado All-Payer Claims Database (APCD) 2012–2019.

The merged dataset provides a longitudinal mother–infant panel spanning 2012 to 2019. The analytic sample includes births with a valid mother–infant linkage and at least one well-child visit in the first year of life. Claims from both Medicaid and commercial payers are included, while births financed through other sources such as self-pay or TRICARE are excluded.

Covariates include maternal, practice, and ZIP code-level characteristics aggregated to the practice–quarter level as described in Appendix B. Definitions of the outcome measures and code lists are provided in Appendix C.

# Appendix B. Covariate Definitions

Maternal-level covariates capture demographic characteristics, socioeconomic status, and health indicators that may influence both access to care and screening rates. These include age, race and ethnicity, nativity, education, marital status, income, and indicators of rural or urban residence, as well as measures of prenatal care utilization, chronic health conditions, delivery complications, and prior diagnosis of mood or anxiety disorders. Practice-level covariates reflect organizational characteristics and provider composition that may shape screening behavior, including practice size, physician mix, and facility type. Finally, ZIP code-level characteristics derived from the American Community Survey (ACS) control for neighborhood socioeconomic and demographic context, such as educational attainment, income, language use, nativity, and racial and ethnic composition. All covariates are aggregated to the practice-quarter level and included as time-varying controls in the regression analyses.

Table B.3: Maternal Covariates

Descriptive Characteristic	Description and Units
Age	Age of mother, measured in years (continuous).
<b>Race and Ethnicity, %</b>	
White	Mother’s race is coded as White.
Black	Mother’s race is coded as Black.
Asian	Mother’s race is coded as Asian.
Hispanic	Mother is identified as Hispanic.
Other Race	Mother’s race is coded as neither White, Black, nor Asian.
Born outside U.S., %	Mother’s birthplace is coded as outside of the United States.
<b>Education, %</b>	
High School	Mother completed high school.
College	Mother completed any college degree (Associate’s or higher).
Married, %	Mother is reported as being married and living with their spouse.
Rural	Mother resides in a small or large rural area according to RUCA assignment.
Urban	Mother resides in an urban area according to ZIP Code RUCA assignment.
Income	Household income categories: (1) \$0–24,999; (2) \$25,000–34,999; (3) \$35,000–49,999; (4) \$50,000–74,999; (5) \$75,000 and up.
Number of Prenatal Visits, Mean	Continuous count variable; number of prenatal care visits billed to Medicaid during pregnancy.
Prenatal Care Initiated in First 3 Months, %	Indicator equal to one if a prenatal care visit was billed to Medicaid during the first 3 months of pregnancy.
Pre-Existing Chronic Conditions, %	Mother had pre-existing hypertension, diabetes, or obesity prior to pregnancy.
Preterm Birth, %	Birth occurred prior to 37 weeks of gestation.
Pregnancy and Delivery Complications, %	Mother experienced any of the following: gestational hypertension, gestational diabetes, eclampsia, HELLP syndrome, multiple births, ICU admission, blood transfusion, 3rd or 4th degree perineal laceration, ruptured uterus, or unplanned hysterectomy.
Cesarean-Section Birth, %	Delivery method reported as Cesarean-section.

*Notes:* RUCA refers to Rural–Urban Commuting Area classification codes. All variables are aggregated to the practice-quarter level as visit-weighted means unless otherwise noted.

Table B.4: Practice- and ZIP Code-Level Characteristics and Definitions

Variable	Description and Units
<b>Practice-Level Characteristics</b>	
Number of infants	Count of unique infants with at least one well-child visit billed at the practice in a given year.
Total physicians	Total number of unique physician NPIs billing at the practice in a given year.
Primary care physicians	Number of physicians with primary care taxonomy codes (family medicine, internal medicine, or general practice).
Pediatricians	Number of physicians with pediatric taxonomy codes.
OB/GYNs	Number of physicians with obstetrics and gynecology taxonomy codes.
Hospital	Indicator equal to one if the practice organization type is a hospital, based on NPES classification.
Ambulatory facility	Indicator equal to one if the practice is classified as an ambulatory health care facility.
Agency	Indicator equal to one if the practice is classified as a health agency.
Managed care organization	Indicator equal to one if the practice is classified as a managed care organization.
Nursing facility	Indicator equal to one if the practice is classified as a nursing facility.
Total patient volume	Total number of unique patients (all ages and payers) with any visit billed to the practice during the year.
<b>ZIP Code-Level Characteristics (from ACS 5-Year Estimates)</b>	
High school or higher (%)	Share of the population aged 25 or older with at least a high school diploma.
Bachelor's degree or higher (%)	Share of the population aged 25 or older with a bachelor's degree or higher.
Foreign-born (%)	Share of the population born outside the United States.
English only (%)	Share of households that speak only English at home.
Spanish spoken (%)	Share of households that primarily speak Spanish at home.
Non-English spoken (%)	Share of households that speak a language other than English at home.
White (%)	Share of the ZIP code population identifying as White.
Black (%)	Share of the ZIP code population identifying as Black or African American.
Asian (%)	Share of the ZIP code population identifying as Asian.
Other race (%)	Share of the ZIP code population identifying as a race other than White, Black, or Asian.
Female (%)	Share of the population identifying as female.
Age 20–64 (%)	Share of the population between ages 20 and 64.
Total population	Total number of residents living within the ZIP Code Tabulation Area (ZCTA).
Total households	Total number of households within the ZIP Code Tabulation Area (ZCTA).
Average household size	Average number of persons per household in the ZIP Code Tabulation Area (ZCTA).
Mean household income (\$)	Mean household income in current U.S. dollars.
Median household income (\$)	Median household income in current U.S. dollars.

*Notes:* Practice-level characteristics are derived from APCD billing records and NPES organization classifications. ZIP code-level characteristics are merged from the American Community Survey (ACS) 5-year estimates (tables DP02, DP05, and S1901) using ZIP–ZCTA crosswalks. All measures are aggregated to the practice–year level and used as controls in regression models.

# Appendix C. Screening and Well-Child Visit Codes

Codes used to identify maternal depression screening during well-child visits varied over time to account for changes in covered services, billing guidance, and the transition from ICD-9 to ICD-10 during the study period. Prior to January 2014, Colorado reimbursed for postpartum depression screenings only for Medicaid enrollees aged 20 and younger. In January 2014, new guidance introduced additional codes specific to postpartum depression screenings conducted during infant well-child visits, and in August 2014, new codes were added to allow this service to be billed under the infant’s Medicaid ID.

Code lists were provided by the Colorado Department of Health Care Policy and Financing (HCPF), which administers Health First Colorado, the state’s Medicaid program. In October 2015, diagnosis codes were updated from ICD-9 to ICD-10, and in September 2017, HCPF revised the guidance to consolidate the list of Healthcare Common Procedure Coding System (HCPCS) codes.

To ensure comparability between Medicaid and commercially insured populations, I applied the full list of Medicaid-specific screening codes to the commercial sample and included all standard depression screening and mental health assessment codes used in commercial insurance claims to the Medicaid sample. This harmonized approach avoids conflating policy-driven changes in postpartum screening with broader national changes, such as the Affordable Care Act’s 2014 introduction of annual depression screening as a mandated preventive service.

Table C.5: Codes to Identify Maternal Depression Screenings

Code Type	Codes
ICD-9	V40.9, V79.8, V79.0
ICD-10	Z13.32, Z13.39, Z13.30
CPT	99420 (with HD modifier), 96127, 96160, 96161
HCPCS	S3005, G0444, G8431, G8510, G8511

*Notes:* Codes are drawn from billing guidance issued by the Colorado Department of Health Care Policy and Financing (HCPF) between 2013 and 2017. The list combines Medicaid-specific codes with standard commercial insurance screening codes to maintain consistent measurement of postpartum depression screening across payer types.

Table C.6: Codes to Identify Well-Child Visits for Infants Under Age 1

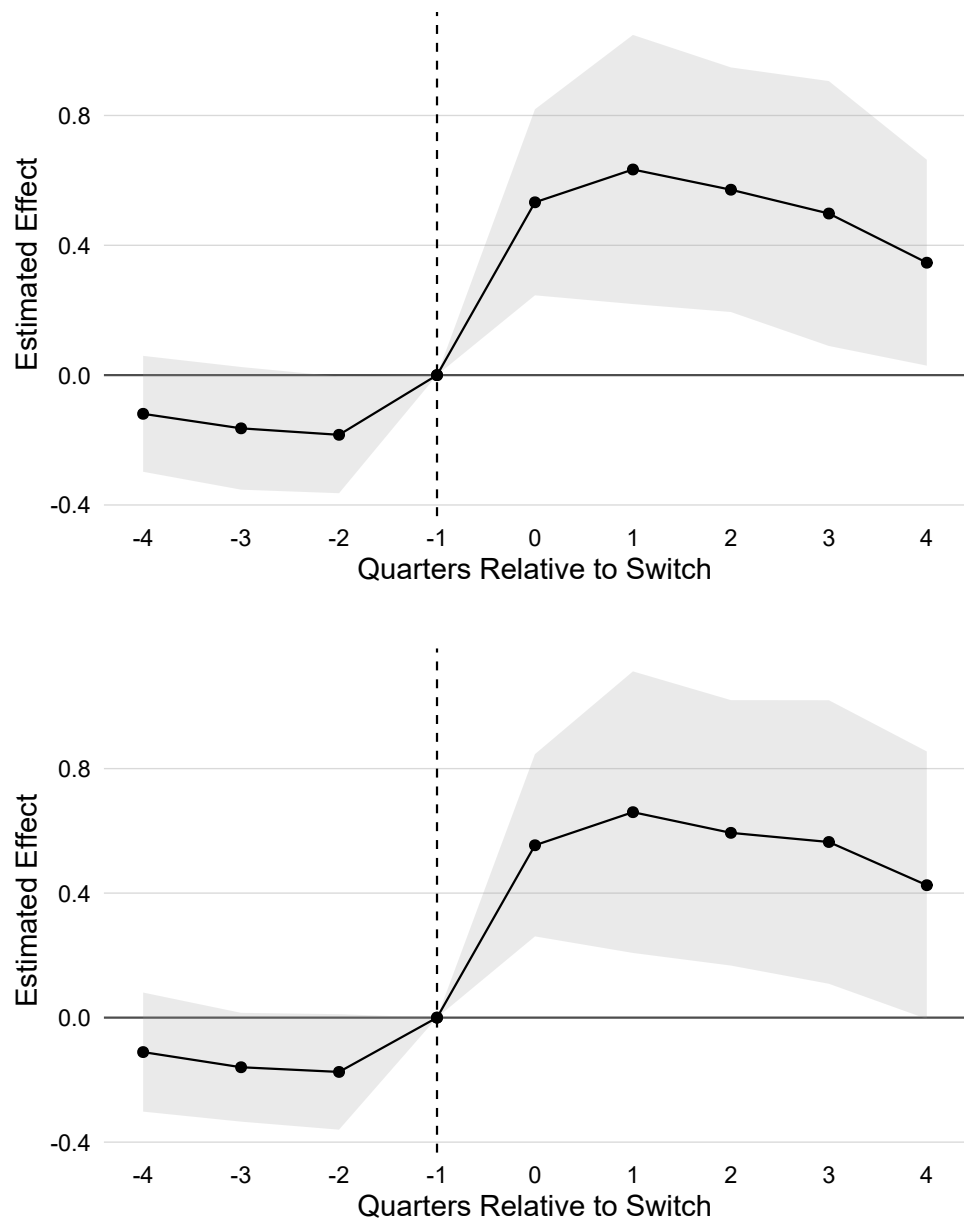
Code Type	Codes
ICD-9	V20.31, V20.32, V20.2
ICD-10	Z00.110, Z00.111, Z00.121, Z00.129
CPT	99381, 99391

*Notes:* For diagnosis codes V20.2, Z00.121, and Z00.129, corresponding CPT codes must also be present on the same date of service to confirm a well-child visit for an infant under age one. Codes were used consistently across payer types to ensure comparability between Medicaid and commercial samples.

# Appendix D. Additional Figures and Tables

Figure D.1 presents supplementary event study estimates from the main physician movers analysis. The top figure includes provider and quarter fixed effects, while the bottom one adds origin practice fixed effects to account for baseline practice-level differences in screening intensity. The results are similar across specifications, showing no differential pre-trends prior to the move and a convergence in screening behavior following the switch. The magnitude and timing of adjustment are consistent with the interpretation that practice systems, rather than physician-specific preferences, drive screening behavior.

Figure D.1: Event Study Estimates for Physician Movers



Notes: Each panel plots coefficients from stacked event study regressions of the physician–practice–quarter screening rate on event-time indicators interacted with the leave-one-out difference in average screening rates between the destination and origin billing practices. The top panel includes provider and quarter fixed effects, while the bottom panel adds origin-practice fixed effects. Event time is measured in quarters relative to the provider switch. The vertical dashed line marks the quarter before the move ( $t = -1$ ), and shaded areas show 95% confidence intervals. The sample is limited to physicians observed for at least two quarters before and after a switch.

Table D.7 presents the difference-in-difference analysis using an additional robustness check restricting the sample to physicians who switched billing practices and also changed ZIP codes between the origin and destination practices. This restriction ensures that observed changes in

screening behavior are not driven by administrative or within-system billing changes. The estimated post-switch interaction remains positive and highly significant, with a magnitude similar to the baseline results, indicating that convergence in screening behavior persists even among physicians who move to entirely new practice locations.

Table D.7: Physician Movers with ZIP Code Change: Difference-in-Differences Estimates

	(1)	(2)	(3)
Post-switch	-0.018* (0.010)	-0.014 (0.011)	-0.014 (0.011)
$\Delta$ Screening (dest – origin)	-0.225 (0.183)	-0.435*** (0.145)	-0.435*** (0.145)
Post-switch $\times$ $\Delta$ Screening	0.678*** (0.157)	0.673*** (0.164)	0.673*** (0.164)
Origin practice FE	Yes		Yes
Provider FE		Yes	Yes
Year-quarter FE	Yes	Yes	Yes
Observations	2,722	2,722	2,722

*Notes:* Outcome is the share of well-child visits with a billed postpartum depression (PPD) screen in the physician–practice–quarter cell. The sample is restricted to physicians who switched billing practices and also changed ZIP codes between the origin and destination practices. “Post-switch” equals 1 for quarters after the physician changes billing practices.  $\Delta$ Screening is the leave-one-out difference in average screening rates between destination and origin billing practices in the post period. Models include the same maternal, practice-level, and ZIP-code covariates as in the main specification. Standard errors are clustered by physician and practice. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.10$ .



Appendix E. EPDS Screening Form

Edinburgh Postnatal Depression Scale<sup>1</sup> (EPDS)

Name: \_\_\_\_\_

Address: \_\_\_\_\_

Your Date of Birth: \_\_\_\_\_

\_\_\_\_\_

Baby's Date of Birth: \_\_\_\_\_

Phone: \_\_\_\_\_

As you are pregnant or have recently had a baby, we would like to know how you are feeling. Please check the answer that comes closest to how you have felt **IN THE PAST 7 DAYS**, not just how you feel today.

Here is an example, already completed.

I have felt happy:

☐ Yes, all the time

☒ Yes, most of the time

☐ No, not very often

☐ No, not at all

This would mean: "I have felt happy most of the time" during the past week.

Please complete the other questions in the same way.

In the past 7 days:

1. I have been able to laugh and see the funny side of things

☐ As much as I always could

☐ Not quite so much now

☐ Definitely not so much now

☐ Not at all

2. I have looked forward with enjoyment to things

☐ As much as I ever did

☐ Rather less than I used to

☐ Definitely less than I used to

☐ Hardly at all

\*3. I have blamed myself unnecessarily when things went wrong

☐ Yes, most of the time

☐ Yes, some of the time

☐ Not very often

☐ No, never

4. I have been anxious or worried for no good reason

☐ No, not at all

☐ Hardly ever

☐ Yes, sometimes

☐ Yes, very often

\*5 I have felt scared or panicky for no very good reason

☐ Yes, quite a lot

☐ Yes, sometimes

☐ No, not much

☐ No, not at all

\*6. Things have been getting on top of me

☐ Yes, most of the time I haven't been able to cope at all

☐ Yes, sometimes I haven't been coping as well as usual

☐ No, most of the time I have coped quite well

☐ No, I have been coping as well as ever

\*7 I have been so unhappy that I have had difficulty sleeping

☐ Yes, most of the time

☐ Yes, sometimes

☐ Not very often

☐ No, not at all

\*8 I have felt sad or miserable

☐ Yes, most of the time

☐ Yes, quite often

☐ Not very often

☐ No, not at all

\*9 I have been so unhappy that I have been crying

☐ Yes, most of the time

☐ Yes, quite often

☐ Only occasionally

☐ No, never

\*10 The thought of harming myself has occurred to me

☐ Yes, quite often

☐ Sometimes

☐ Hardly ever

☐ Never

Administered/Reviewed by \_\_\_\_\_

Date \_\_\_\_\_

<sup>1</sup>Source: Cox, J.L., Holden, J.M., and Sagovsky, R. 1987. Detection of postnatal depression: Development of the 10-item Edinburgh Postnatal Depression Scale. *British Journal of Psychiatry* 150:782-786 .

<sup>2</sup>Source: K. L. Wisner, B. L. Parry, C. M. Piontek, Postpartum Depression N Engl J Med vol. 347, No 3, July 18, 2002, 194-199

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Figure E.2: Standard Edinburgh Postnatal Depression Scale (EPDS) Screening Form

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