Supply-Side Drivers of Long-Acting Reversible Contraceptives: Evidence from Medicaid Beneficiaries*

Maya Rossin-Slater
Stanford University, NBER, IZA

Natalia Serna
Stanford University

Lin-Tung Tsai
UC Berkeley

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Abstract

In the U.S., federal law mandates that contraceptives are available at zero cost to patients, yet nearly one in three women of reproductive age still report inadequate access to contraceptive services. With demand-side affordability barriers largely removed, supply-side factors—the actions of healthcare facilities, providers, and insurers—may be critical in explaining this unmet need. This paper examines supply-side determinants of long-acting reversible contraceptive (LARC) use among Medicaid beneficiaries, a population for whom both LARCs and oral contraceptives (OCs) are covered at zero out-of-pocket cost. Using nationwide Medicaid claims data from 2015 to 2019 and a movers design that exploits cross-area migration to isolate supply-side from demand-side variation, we find that local supply-side factors explain 56 percent of geographic variation in LARC use—substantially more than the 35 percent explained for OCs. We then estimate the causal effects of two supply-side policies. Clinic participation in the 340B Drug Pricing Program increases LARC use by 4.6 percent without displacing OC use or increasing LARC removals. By contrast, Medicaid reimbursement changes for immediate postpartum LARC insertion increase uptake by 167 percent but are accompanied by a comparable rise in removals, raising concerns about whether induced insertions reflect informed patient choice. Together, these findings highlight that supply-side factors are critical determinants of LARC use, but the clinical context of insertion matters for whether expanded access translates into persistent, patient preference-concordant use.

Keywords: long-acting reversible contraceptives, 340B program, Medicaid

*Rossin-Slater: mrossin@stanford.edu; Serna: nserna@stanford.edu; Tsai: lintung_tsai@berkeley.edu. We thank Isabella Chu, Sun Young Jeon and David Rehkopf from the Stanford Center for Population Health Sciences (PHS) and Jens Hainmueller and the Stanford Immigration Policy Lab for assistance with accessing and using the Medicaid data at PHS. This research was supported by grant #25-15760 from the Laura and John Arnold Foundation administered via Arnold Ventures. The content is solely the responsibility of the authors and does not necessarily represent the official views of funder. All errors are our own.

1 Introduction

Access to contraception allows women to make informed decisions about childbearing, with significant physical, mental, and socioeconomic benefits (Cleland et al., 2012; Bailey, 2020).[1] Yet access remains far from universal, and understanding which barriers prevent women from obtaining contraceptive services—and which policies can overcome them—is essential for improving reproductive health and economic outcomes.

Existing research on barriers to contraception access has focused overwhelmingly on "demand-side" factors such as affordability, information, and preferences of patients (Lindo and Packham, 2017; Kelly et al., 2020; Bailey et al., 2023; Willage, 2020; Dupas et al., 2025; Bau et al., 2024; Miller et al., 2025; Kiser et al., 2026). The persistence of unmet need, however, suggests that reducing demand-side barriers is not sufficient: despite the Affordable Care Act's mandate that contraceptives are provided at zero out-of-pocket cost, nearly one in three U.S. women of reproductive age still report inadequate access to contraceptive services (Zapata et al., 2021). This paper focuses on the "supply side"—the actions of healthcare facilities, providers, and insurers—which has received comparably less attention in the existing literature.

Specifically, we examine how supply-side factors affect women's use of long-acting reversible contraceptives (LARCs), which include intrauterine devices (IUDs) and subdermal implants, and have the lowest typical-use failure rates among available contraceptive methods (Guttmacher Institute, 2020). Healthcare facilities and providers play a critical role in enabling access to LARCs since they must be inserted in-person during an outpatient or inpatient visit. However, compared to prescription oral contraceptives (OCs)—which is another method that requires some contact with a healthcare provider—LARC use rates are much lower. For instance, among reproductive-age females enrolled in Medicaid, the main public insurance program for low-income Americans, less than 21 percent use LARCs, while 48 percent use OCs (Frederiksen et al., 2022). Medicaid is

[1] Using contraceptives allows women to avoid unintended pregnancies, which have been found to be associated with a range of adverse outcomes, including domestic violence, poor maternal mental health, poor birth outcomes, and poverty (Logan et al., 2007; Nelson et al., 2022). Decades of research have also shown that access to contraception improves women's educational and labor market outcomes (e.g., Goldin and Katz, 2002; Bailey, 2006; Guldi, 2008; Bailey et al., 2013; Bailey and Lindo, 2017).

required to cover both types of contraceptives with zero cost-sharing (Ranji et al., 2022; Centers for Medicare & Medicaid Services, n.d.), removing demand-side affordability as an explanation for this difference.

We use administrative nationwide Medicaid data for years 2015–2019 and begin by documenting substantial variation in the rates of LARC use across different localities in the U.S., even after accounting for observable patient characteristics. Specifically, after adjusting for differences in race, ethnicity, and age among 15- to 49-year-old female beneficiaries living in different commuting zones (CZs), we find that there remains nearly a four-fold difference in LARC use rates between CZs at the 5th and 95th percentiles of the distribution.

To isolate the role of local supply-side factors from patient demand factors in explaining the cross-CZ variation in LARC use, we follow a growing literature that leverages patient migration (Doyle Jr, 2011; Finkelstein et al., 2016; Agha et al., 2019; Finkelstein et al., 2021b; Deryugina and Molitor, 2020, 2021; Zeltzer et al., 2021; Finkelstein et al., 2021a; Badinski et al., 2023; Fischer et al., 2025; Ding, 2026).[2] We use a sample of female beneficiaries who move across CZs, and estimate "AKM-style" (Abowd, Kramarz, and Margolis, 1999) models that include individual and CZ fixed effects, allowing us to decompose the variation in LARC rates into demand- and supply-side shares, respectively. We also estimate event-study models to measure how a mover's likelihood of LARC use changes as a function of the origin-to-destination change in the local LARC rate. We conduct the same analyses for OCs, allowing us to compare place and patient effects across the two forms of contraception.

We find that place effects explain about 56 percent of the gap in LARC use between above- and below-median CZs, compared with only 35 percent of the analogous gap in OC use. This pattern holds across different partitions of the distribution—e.g., top versus bottom 25 percent, top versus bottom 10 percent—with LARC place effects 40 to 70 percent larger than those for OCs.

Our event-study results indicate that moving from a CZ with zero LARC use to a CZ

[2]Outside of healthcare, studies have used "movers" designs to study the effects of place on educational and economic outcomes, as well as behaviors such as alcohol use (Chetty et al., 2016a; Chetty and Hendren, 2018; Chyn, 2018; Hinnosaar and Liu, 2022; Nakamura et al., 2022).

with 100 percent LARC use would lead to a 50 percentage point increase in a woman's likelihood of receiving a LARC, while moving from a zero to a 100 percent OC-use CZ would result in a 37 percentage point increase in the likelihood of OC uptake. Both place effects remain stable for three years following the move, suggesting that the local supply environment exerts persistent impacts on individuals' contraceptive utilization, consistent with supply-side factors—rather than slow-moving demand-side adjustment—driving the place effects (Finkelstein et al., 2016). Interestingly, we find little evidence of substitution between the two types of contraceptives—moving from a low- to a high-LARC CZ does not change the likelihood that a woman gets oral contraceptives, while moving from a low- to a high-OC CZ does not decrease the likelihood that a woman gets a LARC (if anything, there is a small positive effect that is one-tenth of the magnitude of the LARC place effect). Additionally, we study LARC removals, finding evidence of small but significant place effects. Moving from a CZ with zero to 100 percent LARC use would lead to a 14 percentage point increase in the probability of LARC removal, implying that roughly 28 percent of place-induced LARC insertions are subsequently removed,[3] consistent with other estimates of the LARC removal rate among low-income women (Darney et al., 2023).

Next, we examine the correlations between various supply-side factors and the LARC and OC place effects that we estimate, but do not find clear patterns that can pinpoint to concrete supply-side mechanisms. Moreover, correlational analyses of this type do not necessarily identify *causal* drivers of the supply of contraceptives.

Therefore, in the final part of the paper, we estimate the causal effects of two supply-side policies on contraceptive use among Medicaid beneficiaries. The first policy is the 340B Drug Pricing Program, which was established under the Veterans Health Care Act of 1992 and requires pharmaceutical manufacturers to provide discounted prices on outpatient drugs to eligible safety-net providers, including Federally Qualified Health Centers (FQHCs) and Title X family planning clinics (Health Resources and Services Administration, 2026). Because LARCs are device-intensive and carry significant upfront costs—during our analysis period, wholesale prices for IUDs and implants were around $800

[3]We arrive at this estimate by dividing the LARC removal place effect (14 percentage points) by the LARC insertion place effect (50 percentage points).

per device, not including insertion costs (Eisenberg et al., 2013)—these discounts can substantially reduce the financial barriers that clinics face to stocking and offering them.[4] We use the staggered variation in when safety net clinics first begin participating in the 340B program across ZIP codes to study how access to a 340B-participating provider affects Medicaid enrollees' LARC and OC use.[5] Using a Sun and Abraham (2021) event-study estimator, we find that the arrival of a first 340B clinic in a ZIP code increases LARC use by 0.19 percentage points (4.6 percent relative to the pre-treatment mean). We find no significant changes in OC use, implying that the 340B program is not causing a substitution away from oral contraceptives. We also examine effects on pregnancy and LARC removals, finding no significant impacts. The null pregnancy result likely reflects limited statistical power: given the modest effect on LARC uptake, downstream effects on pregnancy would be too small to detect at conventional levels of statistical significance.

The second policy focuses on immediate postpartum (IPP) LARCs, which are inserted during a childbirth hospital stay immediately following delivery. Until recently, state Medicaid programs reimbursed providers with a global maternity fee—a fixed payment for the episode of care related to childbirth—that did not separately cover LARC insertion, leaving providers to bear the full cost of the procedure and device. As of 2024, however, 25 states have modified their reimbursement schemes to provide additional payments for the inpatient procedure, the LARC device, or both (American College of Obstetricians and Gynecologists, 2024). We use a Callaway and Sant'Anna (2021) difference-in-differences estimator, comparing IPP LARC rates among beneficiaries who give birth before and after their state's reimbursement change, relative to beneficiaries in states that have not

[4]The Government Accountability Office estimates that 340B discounts reduce drug acquisition costs by between 20 and 50 percent (Government Accountability Office, 2011).

[5]Analyzing the 340B program with Medicaid data requires addressing the duplicate discount prohibition: Federal law bars manufacturers from paying the Medicaid Drug Rebate Program (MDRP) rebate on drugs sold at the 340B price, so each claim qualifies for one but not both (Romero et al., 2022). Which discount applies depends on a beneficiary's fee-for-service vs. managed care enrollment, state policy, and the provider's status in the Health Resources and Services Administration Medicaid Exclusion File. We drop beneficiaries whose state model carves out all drug claims and examine heterogeneity by provider carve-in/carve-out status; see Appendix D for details. Importantly, even when a provider cannot use 340B for a Medicaid claim, 340B participation still allows them to stock discounted contraceptives for all patients–so provider 340B status can affect contraceptive access even for Medicaid enrollees whose claims are carved out.

yet adopted such changes.[6] We find that the reimbursement policy change increases the probability that a new mother receives an IPP LARC by 0.94 percentage points—a 167 percent increase relative to the very low pre-treatment mean of 0.56 percent. We find no evidence of changes in oral contraceptive use among treated mothers, suggesting the policy did not substitute IPP LARCs for other forms of contraception observable in our data. However, we also find a large and significant increase in the probability of LARC removal, with a magnitude slightly exceeding that of the insertion effect.[7] This pattern suggests that the policy induced LARC insertions that women were more likely to subsequently remove—possibly because IPP LARCs are inserted before women have fully recovered from childbirth or had adequate time to consider their contraceptive preferences. Taken together, these results suggest that incentivizing IPP LARC insertions may not be an effective policy tool for encouraging persistent LARC use, and could raise concerns about whether all induced insertions reflect informed patient choice.

Overall, our results highlight that supply-side factors are critical for facilitating access to LARCs—far more so than for prescription oral contraceptives. Yet the setting of LARC insertion matters as much as access itself. A policy reducing providers' financial barriers to offering IPP LARCs successfully increases uptake, but the accompanying rise in removals suggests the induced insertions may not align with patient preferences. In contrast, a policy enabling family planning clinics to acquire and stock LARCs at a discount increases LARC use without any corresponding increase in removals, suggesting it reduces supply-side barriers in a way that is better aligned with patient need and preferences.

This paper contributes to four strands of literature. First, it adds to the literature on contraceptive access, which has largely focused on the effects of removing financial barriers faced by patients (rather than providers).[8] In the Contraceptive CHOICE Project, Secura

[6] When studying the 340B program, our treatment variable varies at the ZIP code × year level, and we therefore use the Sun and Abraham (2021) estimator because it allows for the inclusion of state×year fixed effects, which the Callaway and Sant'Anna (2021) is not well-equipped to handle. Our analysis of the IPP LARC reimbursement policy, however, uses the Callaway and Sant'Anna (2021) estimator, since we cannot control for state×year variation when the policy treatment varies at the state×year level.

[7] This is possible since some removals may involve LARCs inserted outside the immediate postpartum setting.

[8] Outside the US, research on contraceptive use in low-income countries has largely emphasized other demand-side factors such as mis-perceptions about pregnancy risk, contraceptive safety concerns, and restrictive social norms (Dupas et al., 2025; Bau et al., 2024; Miller et al., 2025).

et al. (2010) find that when reproductive-age women are given information regarding LARCs and offered a choice of contraceptives at no cost, two-thirds choose a LARC. Other studies evaluate state programs providing low-income women with free LARCs at family planning clinics. Lindo and Packham (2017) and Kelly et al. (2020) both study Colorado's Family Planning Initiative, which is the first large-scale program of this kind, pairing free LARCs for patients with clinic staff training and technical assistance. Lindo and Packham (2017) find a six percent reduction in teen birth rates in counties with funded clinics over the subsequent five years, while Kelly et al. (2020) show larger reductions in teen births among women living in close vicinity to the clinics, along with some spillover effects due to media coverage of the program. Kiser et al. (2026) evaluate a similar Virginia initiative, finding a more modest three percent reduction in birth rates, which they attribute to the post-ACA policy landscape and higher baseline LARC rates. Outside of policy evaluations, Bailey et al. (2023) conducted a large-scale randomized controlled trial offering patients at Title X clinics vouchers covering contraceptive costs, and found that eliminating out-of-pocket costs increases contraceptive use by 40 percent and LARC use by 324 percent. Our paper complements this work by focusing on *supply-side* factors, and isolating the effects of policies that reduce the providers' costs of offering LARCs without changing the patients' costs of obtaining them. Our nationwide Medicaid claims data also provide a large sample of low-income reproductive-aged women with comprehensive measurement of LARC use, oral contraceptive use, pregnancies, and LARC removals—a combination of outcomes rarely available in survey or birth records data used in prior studies.

Second, we build on a medical and health services research literature evaluating state Medicaid reimbursement policy changes for LARC insertions, typically using either single-hospital or single-state before-and-after designs or two-by-two difference-in-differences designs (Steenland et al., 2019, 2022a,b; Boudreaux et al., 2022; Caudillo et al., 2022; Gifford et al., 2024). The most closely related study is Rodriguez et al. (2024), who study the effects of the Medicaid reimbursement policy change in nine states using a difference-in-differences estimator, and find a 0.74 percentage point increase in the IPP LARC rate. We extend this literature in two key ways: by drawing on Medicaid data from 17 treatment states, and by studying LARC removals alongside insertions. The latter turns out to be

consequential—we find that removals rise as much as insertions, implying that not all induced IPP LARC placements reflect patients' long-term contraceptive preferences.

Third, we contribute to a growing literature on the 340B Drug Pricing Program. Most existing work has focused on cancer-related care, finding evidence of increased spending, higher prescription drug utilization, greater hospital-physician consolidation, and a shift of care from physician offices to hospital outpatient settings—with no meaningful improvements in patient outcomes such as survival (Desai and McWilliams, 2018; Jung et al., 2018; Bond et al., 2023; Chang et al., 2023; Smith et al., 2023; Horn, 2025; Horn and Schulman, 2026). A broad conclusion from this literature is that the program has increased hospital revenues without improving outcomes, particularly for the low-income patients it was designed to serve (Thomas and Schulman, 2020; Knox et al., 2023). Much less is known about how 340B affects LARC utilization, despite recognition that the program could be a lever for improving access in states that allow safety net providers to use 340B-purchased drugs and devices for Medicaid patients (Romero et al., 2022). To our knowledge, this paper provides some of the first evidence on the 340B program's effects on LARC utilization among Medicaid patients, suggesting that its implications for safety net family planning clinics may differ meaningfully from the limited welfare impacts documented for hospitals.

Finally, the paper contributes to a literature using movers designs to decompose geographic variation in healthcare utilization into patient and place components. Finkelstein et al. (2016) pioneered this approach in the context of overall Medicare spending, finding that roughly half of the geographic variation is attributable to place-specific supply factors. Subsequent work has extended this framework to study place effects on mortality (Finkelstein et al., 2021b; Deryugina and Molitor, 2020, 2021), mental health care (Ding, 2026), use of emergency department services (Zeltzer et al., 2021), fragmentation of care (Agha et al., 2019), opioid prescriptions (Finkelstein et al., 2021a), cesarean section deliveries (Fischer et al., 2025), and physician practice styles (Molitor, 2018; Badinski et al., 2023). We build on this work by studying place effects in the context of contraceptive use, for which the prior literature has mostly examined the role of demand-side barriers, and by pairing the movers design with an analysis of two distinct policies that influence local

supply conditions in the context of reproductive healthcare.

The rest of the paper proceeds as follows. Section 2 describes our Medicaid data. Section 3 presents our patient migration-based analysis that decomposes the geographic variation in LARC and OC use into demand and supply-side components. Section 4 provides the institutional details, empirical design, and results from our analysis of the 340B program, while Section 5 contains the institutional details, empirical design, and results from our analysis of the IPP LARC reimbursement policy changes. Section 6 offers concluding thoughts.

2 Data

Our primary source of data are administrative enrollment and claims files from the Medicaid program, which is the main public health insurance program for low-income individuals in the United States. These data are curated by the Centers for Medicare & Medicaid Services (CMS), and we access them through a data reuse agreement for the files stored at Stanford's Center for Population Health Sciences. We use the Transformed Medicaid Statistical Information System (T-MSIS) Analytic Files, or TAF, which are available for years 2015–2019.[9]

For each state and year, we have access to four TAF files. The enrollment file contains each enrollee's residential ZIP code, date of birth, race, ethnicity, sex, and eligibility category.[10] Inpatient (IP) files provide comprehensive records of hospital admissions for enrollees utilizing inpatient care, including International Classification of Diseases (ICD) diagnosis and procedure codes and service dates. Other services (OT) files capture a broad array of services delivered across multiple settings, including physician office

[9]Before the TAF system, states used the Medicaid Analytic eXtract (MAX) system, and different states transitioned from MAX to TAF at different times. Because there are known data quality issues in the transition period (Schpero et al., 2025), we only use state-years with complete TAF data and exclude the transition years. Appendix Table D1 lists the MAX-to-TAF transition years for each state in the second column.

[10]Enrollee's race and ethnicity are assigned using non-missing records; when multiple non-missing records exist, we use the modal value. Duplicated records at the ID-Year-State level are removed. Duplication at the ID-Year level across states is resolved by keeping the record in which the beneficiary was enrolled in January.

visits, emergency department encounters, and clinic-based care, along with associated ICD diagnosis and procedure codes.[11] The prescription drug (RX) files include records of filled prescriptions and the associated National Drug Codes (NDCs). To link between the enrollment, IP, OT, and RX files, we use a unique anonymized individual identifier generated by CMS (*BENEID*).

Data quality for the TAF files is assessed by CMS and Mathematica (a policy research organization), with usability recommendations published through the TAF Data Quality (DQ) Atlas.[12] We restrict our analysis to state-years rated as low or medium concern in the enrollment files. Column 1 of Appendix Table D1 lists the years with low data quality for each state, which we drop from our analysis.

We also bring in publicly available data on various characteristics aggregated at the Commuting Zone level. The data sources for these characteristics are detailed in Appendix A.1.

We construct different analytic samples for each of our three empirical analyses, which we describe separately in each section below.

3 Placed-Based Drivers of Contraceptive Use

We begin with a "movers" design to decompose the variation in contraceptive use into patient- and place-based factors. In this section, we describe the analytic sample construction, our key variables, the empirical approach, and the results.

3.1 Sample, Key Variables, and Descriptive Statistics

Main Sample. We construct a beneficiary-year panel dataset consisting of reproductive-age female Medicaid enrollees. Within the high-quality state-years as defined above, we apply the following sample inclusion criteria at the beneficiary-year level: (i) the beneficiary is female and between the ages of 15 and 49, (ii) birth date, ZIP code, and

[11] The Medicaid data encompass both fee-for-service (FFS) records, which include payment information, and encounter records submitted by Medicaid managed care (MMC) plans, which do not. We use the term "claims" to refer to both types throughout.

[12] Available at: https://www.medicaid.gov/dq-atlas/welcome.

managed care or fee-for-service enrollment information are valid and non-missing, and (iii) residence state is the same as the enrollment state.[13] Appendix Table D2, Panel A reports the sample sizes that remain after applying each criteria. Our baseline analysis sample includes approximately 84.8 million beneficiary-year observations.

Key Variables. Our main outcomes are indicators for two types of contraceptives: LARCs and OCs. We also create an indicator for LARC removal. To construct these outcomes, we obtain ICD (diagnosis), NDC (drug), and CPT/HCPCS (procedure) codes related to these contraceptive methods from Office of Population Affairs (2026),[14] and identify claims containing these codes in the IP, OT, and RX files.[15] We merge these claims with the beneficiary-year sample using the unique beneficiary identifier, and create binary indicators capturing the three outcomes on an annual level.[16]

Commuting Zones. We match the beneficiary's ZIP code of residence to their Commuting Zone (CZ) using a crosswalk from the U.S. Department of Agriculture (USDA).[17] CZs are geographical units created by the USDA's Economic Research Service that closely reflect where people live and work (Tolbert and Sizer, 1996). We consider CZs as the relevant geographical units because individuals tend to receive most of their healthcare around the areas where they live or work (Chetty et al., 2016b; Finkelstein et al., 2021b). To avoid having small units with few movers, we follow Finkelstein et al. (2021b) and merge CZs within a state that have a number of beneficiaries below the 25th percentile of the national distribution.

[13] Discrepancies between residence and enrollment state arise mainly from data quality issues (e.g., stale addresses, mailing vs. physical address, lagged updates after moves) and legitimate exceptions such as interstate foster care placements, out-of-state long-term care or psychiatric placements, border-area enrollment, students, and incarceration.

[14] See Appendix Table D3 for the exact list of codes.

[15] In the IP files, procedure codes are matched using `PRCDR_CD_1` through `PRCDR_CD_6`, diagnosis codes using `ICD_DGNS_CD_1` through `ICD_DGNS_CD_12`, and drug codes using `NDC`. In the RX files, drug codes are matched using `NDC`. In the OT files, procedure codes are matched using `LINE_PRCDR_CD`, diagnosis codes using `DGNS_1` or `DGNS_2`, and drug codes using `NDC`.

[16] While a small share of LARC insertions occur in inpatient settings—the focus of our analysis in Section 5—we restrict our attention to LARC insertions in outpatient settings in the analysis in this section.

[17] Accessed here: https://ers.usda.gov/sites/default/files/_laserfiche/DataFiles/48457/cz00_eqv_v1.xls

Mover and Stayer Samples. Our goal is to analyze how individuals' propensity to use contraceptives changes as they move between different locations with different LARC and OC rates. We define "movers" as beneficiaries who live in two different CZs during the observed period and move once.[18] To allow for complete observation of their contraceptive use in the years surrounding the move, we restrict our attention to beneficiary-years with continuous enrollment in the year before and the year after the move. Within each year, we allow for the length of an enrollment spell to be between 11 and 14 months, that is, we allow for at most one month of dis-enrollment and two months of enrollment overlap due to transitioning between states.[19] "Stayers" are defined as beneficiaries who live in the same CZ throughout the observation window. Combined, the "movers" and "stayers" yield about 58.1 million beneficiary-year observations (see Appendix Table D2, Panel B).

Descriptive Statistics. Table 1 presents summary statistics for movers (measured in the year prior to the move) and stayers. An observation corresponds to a beneficiary-year. The sample includes 449,926 movers and 22,094,817 stayers. Across both groups, beneficiaries are on average 29.1–29.4 years old; 19.2–21.0% identify as Black; 12.6–12.0% use oral contraceptives; and 5.9–4.6% have an OT LARC. These patterns are consistent with documented contraceptive use based on KFF reports (Frederiksen et al., 2024). Relative to stayers, white beneficiaries are slightly overrepresented among movers (46.3% – 37.5%). Movers are also less likely to be enrolled in MMC (48.8% – 61.6%) and exhibit slightly higher pregnancy rates (16.6% – 12.8%). Finally, movers are observed for an average of 42.1 months in the final dataset.

Figure 1 contains two maps that document geographic variation in LARC utilization across different CZs in our data.[20] Panel (A) shows the variation in raw LARC use rates

[18] A beneficiary-year is classified as moving if the CZ in the current year differs from the CZ in the previous year and both are non-missing. By focusing on individuals with active enrollment records in a given year, we guarantee that movers have Medicaid eligibility both in the origin and destination CZs even if the destination is in a different state (with potentially different Medicaid eligibility rules).

[19] When a beneficiary moves across CZs located in different states, it is possible that the enrollment files report some months of enrollment that overlap between the origin and destination state because of a lag between registering the move and updating their records. Of all the moves that we identify, 24.2% are across CZs located in different states. Of these cross-state moves, 63.0% are associated with an enrollment overlap of at most 2 months in the two states.

[20] While we drop some state-years due to poor data quality (see Appendix Table D1), no state is dropped

among all women in our analysis sample. Panel (B) presents variation in risk-adjusted LARC rates, which we obtain by regressing an indicator for having a LARC insertion claim on interactions between race/ethnicity and age categories using the sample of stayers.[21] For each CZ, we then compute the average residual among its residents and add it to the sample mean. The maps show that even after we account for differences in the age and racial/ethnic compositions of CZs, there remains substantial geographic variation in LARC use rates, ranging from 1.4 percent in an Arkansas CZ to 8.2 percent in a CZ in Oregon, the 5th and 95th percentiles of the distribution, respectively. However, unobservable differences in patient characteristics or preferences could still contribute to these gaps, motivating a movers design with individual and CZ fixed effects that can separate supply-side from demand-side factors.[22]

3.2 Empirical Framework

To decompose the variation in LARC use that is attributable to place versus patient effects, we use an empirical design that leverages variation from beneficiaries who move across CZs (Abowd et al., 1999; Finkelstein et al., 2016). We estimate the following model using the sample of movers:

$$y_{it} = \gamma_{j(i)} + \alpha_i + x_{it}\beta + \tau_t + \varepsilon_{it} \tag{1}$$

where y_{it} is a binary indicator equal to one if a mover beneficiary i has a LARC claim in year t, $\gamma_{j(i)}$ is a fixed effect for mover i's destination CZ j, α_i is an individual fixed effect, τ_t is a year fixed effect, and x_{it} includes observable individual-level time-varying characteristics: age and age squared. For comparison, we also estimate equation (1) using as the outcome an indicator for having an oral contraceptive claim.

Variation and Identification. The coefficient of interest in equation (1) is $\gamma_{j(i)}$, which captures variation in LARC use after the move that is explained by differences across

entirely. Therefore, we have data for every (continental U.S.) state in these maps.

[21] We construct three indicators capturing ages 15–24, 25–34, and 35–49, and four indicators capturing race/ethnicity (Non-Hispanic White, Non-Hispanic Black, Hispanic, and Other (including missing). Therefore, we include $3 \times 4 = 12$ race/ethnicity by age category indicators.

[22] Appendix Figure D1 presents the analogous maps for oral contraceptive rates for comparison.

CZs (i.e., the "place effects"). This regression recovers unbiased estimates of place effects under the assumption that individuals' choices of moving destinations are uncorrelated with underlying LARC use propensity. To assess the variation used in this estimation and the validity of the identifying assumption, Panel A of Figure 2 plots the distribution of the difference between destination and origin risk-adjusted LARC rates among movers. We observe a large number of moves in each direction of the LARC rate difference, implying that we are able to estimate place effects with sufficient statistical power. Additionally, the distribution is centered around zero and is largely symmetric, suggesting balance in the number of women who move to CZs with higher LARC rates and those who move to CZs with lower LARC rates relative to their origin.

Panel B of Figure 2 examines this symmetry more directly by presenting a scatterplot of the average change in movers' LARC use after the move against bins of the difference in risk-adjusted LARC rates between destination and origin CZs. If changes in women's LARC use could be entirely attributed to place effects, this figure would have a slope of one. Instead, we find a slope equal to 0.55, suggesting differences in patient characteristics explain a little less than half of the observed variation in LARC use among movers.

Panel C of Figure 2 explores whether movers differ systematically from stayers. It plots the difference in LARC use between movers (measured prior to the move) and stayers by bins of the difference in risk-adjusted LARC rates between destination and origin CZs. If there were selection into moving based on baseline LARC use, this figure would have a slope that is significantly different from zero. Instead, we find a slope that is very close to zero suggesting that the likelihood of being a mover is uncorrelated with baseline LARC use.

Appendix Figure D2 replicates this analysis for OCs. OC use exhibits substantial variation across CZs, and areas with higher risk-adjusted LARC rates also tend to have higher risk-adjusted OC rates. However, differences in patient characteristics appear to account for a larger share of the observed variation in OC use among movers, approximately 61 percent, compared to 45 percent for LARCs (Appendix Figure D2, Panel B).

Estimation. To estimate the parameters of equation (1), we implement the following procedure. First, we randomly split the sample of movers into two equal-sized sub-samples, stratified by CZ so that each CZ is represented in both sub-samples. Second, we estimate equation (1) separately within each sub-sample. Third, we repeat this random partitioning and estimation process 100 times. Finally, we average the resulting parameter estimates across the sub-samples and iterations.[23]

Importantly, our data allow us to estimate equation (1) because we observe sufficient *within-individual* variation in LARC use. Our main outcome captures the *incidence* of LARC insertions rather than the *prevalence* of LARC use: the indicator equals one if a beneficiary has a LARC insertion claim in year t and zero otherwise, including years in which she continues to use a LARC but has no new insertion. As a result, our place-effect estimates speak to supply-side determinants of LARC access rather than of the persistence of use once a LARC is obtained. We address the latter using a LARC removal indicator as a separate outcome.

Decomposition of Place and Patient Effects. Using the results of equation (1), we perform a mean decomposition exercise. We split our sample into two groups of CZs and compute the share of the difference in LARC use between the two groups that is explained by differences in patient versus place effects.

Formally, suppose A and B are two groups of CZs (e.g., group A includes CZs with below-median LARC rates, while group B includes CZs with above-median LARC rates), then the mean difference in LARC use between the two groups can be written as:

$$\overline{y}_A - \overline{y}_B = (\hat{\gamma}_A - \hat{\gamma}_B) + (\tilde{y}_A - \tilde{y}_B) \tag{2}$$

[23] Our method differs from standard mover designs in the third step. Existing applications typically rely on a single random split of the sample to estimate the parameters. In our setting, however, a single split would generate biased estimates because baseline LARC utilization is relatively low. To illustrate, consider a CZ with three movers, only one of whom gets a LARC after the move. In the full sample, the average LARC rate in that CZ is $\frac{1}{3}$. Suppose we perform a single split such that the woman with a LARC is assigned to sub-sample 1 and the other two women to sub-sample 2. The estimated CZ effect would then be equal to 1 in the first sub-sample and 0 in the second, yielding an average of $\frac{1}{2}$ across sub-samples, substantially larger than the true rate of $\frac{1}{3}$ in the full population. Repeating the split many times and averaging across iterations mitigates this small-sample distortion. In other words, our estimator is unbiased and consistent in the number of iterations.

where $\forall K \in \{A, B\}$, $\hat{\gamma}_K$ is a consistent estimate of γ_K and $\tilde{y}_K = \overline{\alpha_{i,K}} + \overline{x_{it,K}\hat{\beta}} + \overline{\tau_{t,K}}$, and variables with over-lines denote sample means.[24]

The share of the difference in LARC use between A and B that is explained by place effects is:

$$E_{\text{place}}(A, B) = \frac{\hat{\gamma}_A - \hat{\gamma}_B}{\overline{y}_A - \overline{y}_B} \tag{3}$$

and the share explained by patient effects is:

$$E_{\text{patient}}(A, B) = 1 - E_{\text{place}}(A, B) \tag{4}$$

By construction, the sum of $E_{\text{place}}(A, B)$ and $E_{\text{patient}}(A, B)$ equals 1, but each component need not be non-negative.

Importantly, the mean decomposition is valid under the assumption that place and patient effects are linearly separable. If patient preferences for LARCs change after the move or interact with place effects—for example, if after the move, peer effects in the destination CZ change the mover's preferences for LARCs—this decomposition would not accurately reflect the difference between place and patient effects. We follow Finkelstein et al. (2016) to assess the plausibility of the linear separability assumption by examining the persistence and stability of place effects after a move in an event-study framework. Stable estimates across all post-periods would argue against slow-moving demand-side adjustment—such as peer effects that gradually shift preferences—as the primary driver of the place effects.

Lastly, in order for us to be able to interpret $E_{\text{place}}(A, B)$ as the causal effect of place, we must assume that movers are not systematically accessing different types of supply-side factors from stayers. Reassuringly, Table 1 shows that movers and stayers are similar in terms of various characteristics including their healthcare utilization, while Panel C of Figure 2 documents that moving is not correlated with baseline LARC use, lending plausibility to this assumption.

For completeness, in Appendix C we also present a variance decomposition exercise,

[24] Note that in this decomposition, time effects τ_t are included in the patient effect to allow for time-varying preferences for contraception within individuals.

where we compute the share of the variation in LARC use that would be eliminated if patient effects and place effects were made equal across all CZs.

Event-Study Model. As noted above, we also use an event-study framework to analyze how a mover's propensity to obtain a contraceptive changes as a function of the difference between destination and origin rates of different types of contraceptives (LARCs and OCs). We use our sample of movers and estimate:

$$y_{it} = \alpha_i + \sum_{s=-4}^{3} \theta_s^{LARC} \mathbf{1}[s = r(i,t)]\delta_i^{LARC} + \sum_{s=-4}^{3} \theta_s^{OC} \mathbf{1}[s = r(i,t)]\delta_i^{OC} + \tau_t + \varepsilon_{it} \qquad (5)$$

where y_{it} is an outcome of interest, such as an indicator equal to 1 if mover i has a LARC insertion claim in year t; $\mathbf{1}[s = r(i,t)]$ is a set of indicator variables capturing years relative to the move, and δ_i^m is the destination−origin difference in the use of each contraceptive method m (LARC and OC) in the year before the move calculated using the stayers only. The other variables and parameters are the same as in equation (1). We estimate equation (5) separately for each of our key outcome indicators (LARC insertion, OC, and LARC removal), and cluster standard errors at the individual level. This model allows us to formally test whether women's propensity to get a contraceptive is correlated with their destination−origin difference in the years *before* the move, as well as to assess the stability of the post-move coefficients, as discussed above. Additionally, we use model (5) to assess substitution patterns between contraceptive methods since we include the destination−origin differences in both types of contraceptives as the explanatory terms interacted with relative time.

In Appendix B, we include a discussion of how the movers design can be interpreted through the lens of a structural demand model, and offer a mathematical framework to connect the event-study model to the baseline fixed effects model.

3.3 Results

Mean Decomposition Results. Table 2 presents the decomposition of mean differences in LARC rates into patient and place effects from our fixed effects model (1). We consider the mean difference between CZs in the top versus bottom 50 percent of the LARC rate distribution in column (1), top versus bottom 25 percent in column (2), top versus bottom 10 percent in column (3), and Wisconsin versus Arkansas in column (4)—the two states with the highest and lowest state-level LARC rates, respectively. Panel A presents the decomposition for LARC use, while Panel B presents the same exercise for OC use for comparison.

Across the different partitions of the data, we find that the share of the LARC rate difference explained by place effects ranges between 57.7 and 60.7 percent, with patient effects explaining the remaining 39.3 to 42.3 percent. Place effects are substantially larger for LARCs than for OCs, indicating that supply-side factors play a more prominent role for the former relative to the latter type of contraception. Specifically, in column (1) we find that place effects are 20.7 percentage points higher for LARCs than for OCs and in column (4) the difference is 36.4 percentage points. In contrast, patient effects are substantially more important for OC use than for LARC use. In column (4), we find that 77.8 percent of the OC gap between Wisconsin and Arkansas is explained by patient effects compared to 41.4 percent for LARCs.

Appendix Table D4 assesses the sensitivity of our mean decomposition exercise to two different approaches that address concerns about data quality. We replicate our main results that compare CZs in the top and bottom 50 percent of the LARC rate distribution in the first row for ease of comparison, and then show how our estimates vary with the following additional restrictions: (i) only using state-years with high-quality OT and RX claims (rather than just high-quality enrollment files) from the DQ atlas in row 2, and (ii) only using state-years with high-quality claim location patterns in row 3 (see Appendix Section A.2). Across both sample restrictions, we find that the share of the mean difference in LARC use that is explained by place effects ranges between 54.4 and 59.1 percent, in line with our baseline estimates.

Appendix C presents a variance decomposition exercise in which we calculate by how much the variance in LARC and OC rates across CZs that would be reduced if patient effects were made equal and if place effects were made equal, respectively. Our findings align with the mean difference decomposition results reported above: if place effects were made equal across CZs, the variance in LARC rates would fall by 60.6 percent compared to 53.2 percent for OCs, suggesting place effects play a larger role in the geographic dispersion of LARC use.

Correlates of Place Effects. What are the characteristics of CZs with larger LARC place effects? And do these characteristics differ from those associated with larger OC place effects? Figure 3 examines the correlates of LARC and OC place effects, focusing on various supply-side factors capturing aspects of healthcare delivery and the structure of Medicaid insurance. The figure reports coefficients and 95% confidence intervals of bivariate regressions of the place effect (for LARCs in the left panel and for OCs in the right panel) on each standardized characteristic.[25] We find that LARC place effects are positively correlated with the per-capita number of hospitals and the fraction of Obstetrician and Gynecologist (OB/Gyn) physicians who are female, but negatively correlated with hospital mortality z-score (a measure of hospital quality), the per capita number of pharmacies, the average number of years of experience (as of 2019) among OB/Gyns, the per capita number of clinics participating in the 340B Drug Discount Program, and the share of enrollees on managed care plans (rather than on fee-for-service). OC place effects are uncorrelated with many of these characteristics, except for positive associations with the per-capita number of hospitals and the managed care share. Overall, these correlational analyses do not pinpoint a clear supply-side mechanism underlying the place effects we estimate, motivating our analyses of two distinct supply-side policies with quasi-experimental designs in Sections 4 and 5 below.

Event-Study Results. Figure 4, Panel A presents the event-study results from estimating equation (5), where the outcome is an indicator for having a LARC insertion, and we study

[25] Each CZ characteristic in Figure 3 is standardized by subtracting the sample mean and dividing by the standard deviation.

how a woman's propensity to get a LARC varies with the destination−origin difference in the LARC and OC rates, respectively, in years before and after a move. The coefficients θ_s^{LARC} are shown in black, while the coefficients θ_s^{OC} are shown in gray. Panel B presents analogous results using as outcome an indicator for whether the woman has an OC claim. Panel C uses an indicator for having a LARC removal as the outcome. We report the estimated (summary) difference-in-differences coefficients, standard errors, and dependent variable means in the top left corner of each plot.

In Panel A, we find that moving from a CZ with zero LARC use to a CZ with 100 percent LARC use would lead to a 49.9 percentage point increase in an individual's likelihood of getting a LARC (estimates in black). Put differently, LARC place effects explain 50 percent of the individual LARC use propensity, consistent with the earlier statistic reported in Figure 2, Panel B. This LARC place effect is stable for three years after the year of the move, suggesting that it is unlikely to be driven by slow-moving demand-side factors or a correlation between patient and place effects. We also find that moving from a CZ with 0 to 100 percent OC use would increase individual LARC use propensity by about 5.4 percentage points (estimates in gray), consistent with OC and LARC place effects being positively correlated and implying that the two methods are not close substitutes. For both sets of event-study coefficients, the pre-move estimates are small in magnitude, mostly insignificant, and do not indicate any clear pre-trend.

In Panel B, we find that moving from a CZ with zero to 100 percent OC use would increase an individual's probability of having an OC claim by 37.1 percentage points—a magnitude that is 26 percent smaller than the estimated 50 percentage point LARC effect in Panel A. Similar to LARCs, the OC place effect is stable throughout the post-move period. The estimates in black in Panel B show that individuals' propensity to obtain OCs is uncorrelated with LARC place effects.

In Panel C, we observe that moving from a CZ with zero to 100 percent LARC use would raise the probability of having a LARC removal claim by about 14.1 percentage points, while moving from a CZ with zero to 100 percent OC use would increase this probability by 3.9 percentage points. These results imply that roughly 28 percent of

place-induced LARC insertions are subsequently removed,[26] in line with removal rates documented elsewhere for low-income women (Darney et al., 2023).

Appendix Figure D3 examines heterogeneity in LARC and OC place effects by estimating separate event-study models for different sub-groups of our sample by age and race/ethnicity. Two takeaways emerge with regard to LARC and OC place effects. First, we find that both LARC and OC place effects are stronger for women under age 35 than for older women (Panels A and B). Second, both LARC and OC place effects are larger for non-Hispanic Black and Hispanic women compared to non-Hispanic White women (Panels C and D). These results indicate that younger and racial/ethnic minority women may experience larger supply-side barriers to accessing contraceptives than their older and non-minority counterparts.

On the whole, our findings that supply-side factors play a key role in explaining the geographic variation in LARC rates among Medicaid beneficiaries—and a larger role than for OCs—raise important questions about the types of policies that can affect the supply of LARCs. In the next two sections, we shed light on this issue by analyzing two policies aimed at reducing the cost to healthcare facilities of providing LARCs: (1) the participation of safety-net clinics in the federal 340B Drug Pricing Program, and (2) the shift to separate reimbursement for LARC devices and insertion procedures under Medicaid.

4 Supply-Side Policy I: The 340B Drug Pricing Program

4.1 Institutional Background

The federal 340B Drug Pricing Program, established under Section 340B of the Public Health Service Act in 1992, requires pharmaceutical manufacturers to sell outpatient drugs to eligible providers serving low-income populations at significantly discounted prices.[27] Eligible entities include disproportionate share hospitals (DSH) that serve a high

[26] We arrive at the 28 percent estimate by dividing the LARC removal place effect (14 percentage points) by the LARC insertion place effect (50 percentage points).

[27] The program sets a price ceiling as the wholesale acquisition cost minus the rebate percentage. The percentage is generally 23.1 percent for brand drugs, 17.1 for clotting factors and drugs approved exclusively for pediatric indications, and 13 percent for generic drugs (Health Resources and Services Administration,

percentage of low-income patients, Federally Qualified Health Centers (FQHCs), and other safety-net providers including Title X family planning clinics that deliver reproductive services to low-income patients. LARC devices generally qualify for 340B discounts (Romero et al., 2022), meaning that participating providers can purchase them at a fraction of the market price, thereby reducing a key financial barrier to offering these devices.

Since our sample consists of Medicaid beneficiaries, it is worth explaining how the duplicate discount prohibition interacts with our analysis. Federal law prohibits manufacturers from using both the 340B discount and the Medicaid Drug Rebate Program (MDRP) discount on the same drug claim (Romero et al., 2022), and which of these applies to a given claim depends on the beneficiary's enrollment type, the state's Medicaid model, and whether the provider appears in the Health Resources and Services Administration (HRSA) Medicaid Exclusion File (see Appendix D for more details). We drop beneficiaries in states where all Medicaid drug claims are carved out from 340B. We note that a provider barred from using 340B pricing on Medicaid claims can nonetheless procure discounted devices through the program for use across all patients, meaning that 340B status remains relevant to contraceptive access even for carved-out enrollees.

4.2 340B Data and Analytic Sample

To identify providers participating in the 340B program, we use the publicly available directory of 340B Covered Entities from the HRSA Office of Pharmacy Affairs, which includes the name and address (including ZIP code) of each entity, the National Provider Identifier (NPI), the year when it began participating in the program, and the termination year (if applicable).[28] We restrict our attention to participating providers that have not been terminated before 2019. Since our Medicaid data include beneficiaries' residence ZIP codes as the smallest geographic units, we find the earliest participating clinic in each ZIP code and define the ZIP code's treatment year as that clinic's participation year. We exclude ZIP codes treated earlier than 2015 from our analysis.

We then use the main sample described in Section 3.1, and restrict our attention

Department of Health and Human Services, 2017).

[28] Available at https://340bopais.hrsa.gov/.

to beneficiary-year observations with continuous enrollment within the year (i.e., with annual enrollment spells lasting between 11 and 14 months, as explained above). For each beneficiary, we identify their first observed ZIP code of residence and assign their treatment cohort as the first year in which a safety net provider in their ZIP code started participating in the 340B program.[29]

In our main analysis, we restrict our attention to treated enrollees living in ZIP codes in which the first 340B participant is a safety net clinic, and use enrollees in ZIP codes that either never have any 340B participating provider or in which the first 340B participant begins after 2019 as the pool of controls. Safety net clinics include community health centers, family planning clinics, and public health departments that play an important role in providing contraceptive access for the Medicaid population (Frederiksen and Salganicoff, 2025).[30] As shown in Appendix Figure D4, there has been substantial growth in 340B participation among safety net clinics over our analysis period, from 1,458 new participants in 2015 to 11,500 new participants in 2019.

Lastly, we construct one more outcome for this analysis, which is an indicator that is equal to one if a beneficiary has any pregnancy-related claim, which we obtain from the CMS Maternal Infant Health code set Centers for Medicare & Medicaid Services (2026). See Appendix Table D3 for more details.

Descriptive Statistics. Appendix Table D6 reports summary statistics for the 340B analysis sample, comparing beneficiaries residing in ZIP codes whose first eligible provider began 340B participation between 2016 and 2019 (treated ZIP codes) to those in ZIP codes in which eligible providers either began participating later or never participated (control

[29]We use the first-observed ZIP code to account for potential endogenous mobility (i.e., beneficiaries systematically moving into or away from areas with 340B participating providers).

[30]To identify these clinics, we merge by NPI with National Plan and Provider Enumeration System (NPPES) to obtain each provider's business ZIP code and primary taxonomy code, and map the latter to the National Uniform Claim Committee (NUCC) taxonomy code set to determine the provider's category. We define community health centers as NPIs that either appear in HRSA's Health Center Service Delivery and Look-Alike Sites file, or have a specialization of "Community Health" or "Federally Qualified Health Center (FQHC)". Family planning clinics are defined as those whose business name contains "Planned Parenthood," or whose specialization is "Family Planning, Non-Surgical" or "Ambulatory Family Planning Facility". Public health departments are defined as those whose classification is "Public Health or Welfare," or whose primary specialization is "Public Health, Federal," "Public Health, State or Local," or "Rural Health".

ZIP codes). The treatment group includes 782,803 beneficiaries in 426 ZIP codes and the control group includes 6,152,200 beneficiaries in 6,418 ZIP codes. Beneficiaries in both groups are on average 29.4 to 29.2 years old. Hispanic beneficiaries are overrepresented in treated ZIP codes (31.4 –20.7 percent), while non-Hispanic White beneficiaries are underrepresented (31.4- 41.4 percent). 49.0 percent of beneficiaries in treated ZIP codes live in the West, compared to 26.7 percent in the control sample. Healthcare use such as LARC (4.6–4.1 percent), oral contraceptive (11.7–11.9 percent), and pregnancy rates (13.2–12.7 percent) are similar across groups.

Since we find that the treated ZIP codes have larger average population sizes than the overall pool of control ZIP codes, we implement a coarsened exact matching (CEM) approach on state (exact) and ZIP-level beneficiary count over all years in the data (coarsened into within-state ventiles of the distribution), with the matched sample defined as all ZIP codes in strata containing both treated and control units.[31]

4.3 Empirical Framework

We use the staggered participation of safety net clinics in the 340B program at the ZIP code level in difference-in-differences and event-study designs. Specifically, we estimate:

$$y_{it} = \sum_{k=-4, k\neq -1}^{3} \beta_k \mathbf{1}\{t - t_i^* = k\} + \gamma_i + \delta_{ts(i)} + \varepsilon_{it} \tag{6}$$

for every beneficiary i observed in year t. y_{it} is an outcome, such as an indicator for a LARC insertion claim. t_i^* denotes the year in which her residence ZIP code has a first safety net clinic participating in the 340B program, and $\mathbf{1}\{t - t_i^* = k\}$ is a set of indicator variables capturing years relative to this treatment year (always set to zero for beneficiaries in control ZIP codes). We include individual fixed effects γ_i and state-year fixed effects $\delta_{ts(i)}$. The model is estimated using heterogeneity-robust estimator from Sun and Abraham (2021), which can handle the inclusion of many fixed effects as required in our model. We cluster standard errors on the residence ZIP code level. The coefficients β_k allow us to assess the validity of the "parallel trends" assumption in the pre-period—that is, we can test

[31] We also show results without CEM matching in Appendix Figure D6, as we discuss below.

whether outcomes between beneficiaries in the treatment and control ZIP codes followed similar trends before 340B clinic participation—as well as dynamic treatment effects in the post-period.

4.4 Results

Figure 5 presents our results from estimating equation (6) using four outcomes as the dependent variables: (A) a binary indicator for an outpatient LARC claim, (B) a binary indicator for an OC claim, (C) a binary indicator for a pregnancy-related claim, and (D) a binary indicator for a LARC removal claim. We report the post-period average treatment effect on the treated (ATT), its standard error, and the mean in the treatment group in the year before treatment in the top left corner of each plot.

Panel A shows that when a safety net clinic begins participating in the 340B program, the probability of obtaining a LARC in an outpatient setting among women residing in the same ZIP code increases by 0.19 percentage points, or 4.1% relative to the pre-treatment mean. We do not find any evidence of significant effects on oral contraceptive use (Panel B), implying that the program-induced LARC insertions do not reflect a substitution away from OCs. We also do not find any significant effects on the probability of pregnancy (Panel C), but we are underpowered to detect effects on this outcome given the low absolute effect size on LARC take-up. Lastly, we also do not detect any significant effects on LARC removals (Panel D), which implies that the program-induced LARC insertions appear to be aligned with patients' long-term contraceptive preferences.

Appendix Figure D5 examines heterogeneity in the effects of the 340B program across beneficiary characteristics for each of our four main outcomes. For each sub-sample, we plot ATT estimates and 95% confidence intervals from model (6), estimated using the Sun and Abraham (2021) estimator. The full-sample estimates are shown at the top of each panel for reference. For all outcomes, confidence intervals across age and racial/ethnic sub-groups are largely overlapping, indicating no significant heterogeneity in treatment effects. Similarly, we find no significant difference in effects between beneficiaries enrolled in managed care plans versus fee-for-service. For oral contraceptives, we find no significant

effect for most sub-groups, with two exceptions: a marginally significant decline in use among women aged 15–24, and a significant increase among women aged 35–49. Because OC pills are substantially less expensive than LARCs, the 340B discount represents a smaller cost reduction for providers dispensing the former than the latter. Taken together, these patterns are consistent with substitution from OCs toward LARCs among younger women, and a modest increase in OC use among older women.

Appendix Figure D6 reports the sensitivity of our event-study estimates to alternative sample and treatment/control group definitions. We replicate the baseline result in panel A for reference. Panel B expands the treatment group to include all providers rather than only safety net clinics; Panel C replaces CEM-matched control ZIP codes with all never-treated and not-yet-treated control ZIP codes; Panel D restricts the sample to beneficiaries continuously enrolled throughout their state's sample period; Panel E restricts the treatment group to safety net clinics that were already open prior to 340B participation, ensuring the treatment captures program entry rather than clinic opening; Panel F includes beneficiaries who are not continuously enrolled within a given year; Panel G adds states that carve out the 340B program for Medicaid beneficiaries; and Panel H drops beneficiaries who change ZIP codes over the sample period. Our estimates are remarkably robust across all of these alternatives.

5 Supply-Side Policy II: Reimbursement for Immediate Postpartum LARCs

Immediate Postpartum (IPP) LARCs refer to insertions of long-acting reversible contraception in the hospital shortly after a patient gives birth. IPP LARCs are endorsed by the American College of Obstetricians and Gynecologists (ACOG) for their safety, potential savings for payers, healthy birth spacing, and alignment with women's preferences to delay and prevent childbirth (American College of Obstetricians and Gynecologists' Committee on Obstetric Practice, 2016), but they are a relatively infrequently used contraceptive choice (Rodriguez et al., 2024; Steenland et al., 2019). In this section, we study

how a change to Medicaid's reimbursement policy that provided hospitals with a stronger financial incentive to offer IPP LARCs to new mothers affects contraceptive outcomes.

5.1 Institutional Background

Until recently, most state Medicaid programs considered IPP LARCs as part of the maternity care bundle—which was reimbursed with a global maternity fee, or a fixed amount for the episode of care related to childbirth—and did not provide a separate reimbursement for LARC insertion during the inpatient stay. As a result, if the provider chose to offer an IPP LARC, they would bear any additional costs of this procedure and the device. However, over the last decade, many states have changed their Medicaid programs' reimbursement schemes to give additional payments to providers for either the inpatient procedure, the LARC device, or both (ACOG, 2017). We hand collected information on the year in which states implemented these IPP LARC reimbursement policy changes from multiple sources. Appendix E describes the data sources and algorithm used to assign each state's policy adoption year. Two states—Kentucky and Nebraska—have not changed their IPP LARC reimbursement scheme as of 2026, and we were not able to find the year of the policy change for four states (Alaska, Maine, Rhode Island, and South Dakota). Additionally, Idaho initially implemented the policy change but the reversed it. We exclude the states for which we could not find information and Idaho from our analysis.

5.2 IPP LARC Reimbursement Analysis Sample

To study the effects of this IPP LARC reimbursement policy change, we first identify all live births in our claims data. We use the OT and IP files, and identify all claims with relevant ICD and CPT codes listed on the `Medicaid.gov` website.[32] We then follow the preferred approach from Auty et al. (2024) to remove false positives and duplicates, as detailed in Appendix A.3. We restrict the sample to the first observed birth for each

[32] Accessed from https://www.medicaid.gov/medicaid/data-systems/macbis/medicaid-chip-research-files/transformed-medicaid-statistical-information-system-t-msis-analytic-files-taf.

woman in the data.

Our key outcome is an indicator for an IPP LARC claim, which we set equal to 1 if a woman has any LARC claim within seven days of the delivery. We use a seven-day window to allow for measurement error in the date of birth, which is imperfectly measured based on the date of admission to the hospital for labor/delivery.

We consider three additional outcomes. First, to study possible substitution from OCs toward IPP LARCs, we construct an indicator equal to 1 if a woman has an OC claim within three months of delivery. Second, since a central motivation for promoting IPP LARCs is to prevent short interpregnancy intervals (IPIs) (American College of Obstetricians and Gynecologists' Committee on Obstetric Practice, 2016), we construct an indicator equal to 1 if a woman has a subsequent birth within 460 days of the index birth.[33] Third, to study LARC persistence, we construct an indicator equal to 1 if a woman has any LARC removal claim within 365 days of delivery.

Descriptive Statistics. Appendix Table D7 reports summary statistics for our analysis sample. The treated group consists of beneficiaries who gave birth in states that implemented the IPP LARC reimbursement policy in 2016 or 2017 (2,221,097 births across 17 states), while the comparison group includes beneficiaries in states that adopted the policy later or never adopted it (630,984 births across 11 states). Beneficiaries are younger than in the samples used in our prior two analyses, averaging 27.0 to 27.0 years old. Demographics differ between the two groups: Hispanic representation is much higher in treated states (29.6% – 16.0%), while Black (18.9% – 24.6%) and white (33.3% – 43.5%) shares are lower. Treated-state beneficiaries are slightly more likely to be enrolled in managed care (30.2%–25.3%). LARC use within 12 months following childbirth is broadly similar across the two groups (12.5–13.3%).

[33]Short IPI is conventionally defined as a subsequent pregnancy within 180 days of delivery. Since we do not observe gestational length, we assume a gestational age of 280 days (40 weeks), implying a 460-day (approximately 15-month) window.

5.3 Empirical Framework

We use the staggered adoption of the IPP LARC reimbursement policy change across states in difference-in-differences and event-study designs. We estimate:

$$y_{ist} = \sum_{k=-3, k\neq -1}^{3} \beta_k \mathbf{1}\{t - t_s^* = k\} + \lambda' X_i + \gamma_t + \delta_s + \varepsilon_{ist} \tag{7}$$

for each new mother i giving birth in state s and year t. y_{ist} is an outcome of interest such as an indicator for an IPP LARC claim. t_s^* is the year in which the mother's state's Medicaid program implemented the IPP LARC reimbursement policy, and $\mathbf{1}\{t - t_s^* = k\}$ is a set of event-time indicators capturing the year relative to the policy change (all set to zero for never-treated states). We include a vector of individual-level control variables X_i for maternal race/ethnicity and age category indicators, year fixed effects γ_t, and state fixed effects δ_s. The model is estimated using the heterogeneity-robust estimator from Callaway and Sant'Anna (2021). We cluster the standard errors at the state level. As with our other event-study models, the pre-period coefficients allow us to assess the validity of the parallel trends assumption—that is, that outcomes in states that implemented the policy would have evolved similarly to those in states that did not or had not yet.

5.4 Results

Figure 6 presents our main results. We plot the event-study estimates and 95% confidence intervals for each of our four outcomes, and the ATT estimates, standard errors, and dependent variable means measured in the treated states in the year before treatment are printed in the top left corner of each graph.

Panel A shows that implementing a separate reimbursement for IPP LARCs significantly increases the probability that a new mother receives one by 0.94 percentage points, which is a 167 percent effect relative to the very low pre-treatment mean of 0.56 percent. These policy-induced LARCs do not appear to be displacing OCs—we do not find any evidence of reductions in OC claims among treated mothers (Panel B). We also do not detect effects on the probability of a short IPI (Panel C). One explanation for the lack of

impact on short IPI is the result in Panel D—we find that the policy increases the probability of LARC removal in the year following childbirth by 1.1 percentage points (68 percent relative to the pre-treatment mean).[34] This finding implies that the reimbursement policy induces marginal LARC insertions that women are more likely to have removed, possibly because the immediate postpartum period does not afford adequate time for recovery or deliberate contraceptive decision-making. More broadly, these results suggest that financial incentives for IPP LARC insertion may not translate into persistent use that is congruent with patients' preferences.

Appendix Figure D7 reports estimates of heterogeneous treatment effects of the policy by race/ethnicity, age group, and the presence of a 340B-participating provider. Overall, we do not detect evidence of significant heterogeneity—the policy appears to increase the probability of an IPP LARC insertion for all sub-groups.

Appendix Figure D8 assesses the sensitivity of our estimates to alternative samples and specifications. Specifically, in the top right panel, we show results when we use a balanced panel of treated cohorts that must appear in the data from two years before the policy change to two years after. In the bottom left panel, we only use beneficiaries giving birth in never-treated states in the control group. And in the bottom right panel, we use the Sun and Abraham (2021) estimator instead of the Callaway and Sant'Anna (2021) one. We replicate our main specification in the top left panel. Our results are robust across these specifications.

6 Conclusion

Access to contraception has profound consequences for women's health, economic, and social outcomes, yet nearly one in three U.S. women of reproductive age report inadequate access despite federal mandates requiring zero-cost coverage under the ACA. This paper shifts attention from the demand side—patient affordability and preferences—to the supply side, asking how much of the geographic variation in LARC use among Medi-

[34]The LARC removal rate is much higher than the IPP LARC rate since the former includes LARCs inserted outside the immediate postpartum period.

caid beneficiaries is attributable to local healthcare environments, and which supply-side policies can effectively increase usage.

Using nationwide Medicaid claims data from 2015 to 2019 and a movers design that exploits cross-area migration to separate supply from demand, we find that local supply-side factors explain approximately 56 percent of geographic variation in LARC use—substantially more than the 35 percent they explain for oral contraceptives. This asymmetry is consistent with the fact that LARCs require in-person insertion by a trained provider and depend on clinics maintaining costly device inventory, making them more sensitive to local supply conditions than prescription-based methods.

We then estimate the causal effects of two supply-side policies that reduce providers' financial barriers to offering LARCs. Participation in the 340B Drug Pricing Program by safety net clinics increases LARC use by approximately 4.6 percent relative to pre-treatment levels, without displacing oral contraceptive use or generating excess removals. This pattern is consistent with 340B reducing a significant supply-side barrier—the high upfront cost of stocking LARC devices—in a manner that expands access for patients who would have chosen a LARC had one been available. By contrast, Medicaid reimbursement changes for IPP LARC insertion produce a large 167 percent increase in uptake, but are accompanied by a comparably large rise in LARC removals within the year following delivery. This suggests that a meaningful share of policy-induced IPP LARCs may not reflect the sustained contraceptive preferences of the women who received them.

Taken together, our findings highlight an important distinction in supply-side policy design: expanding access is not equivalent to expanding preference-concordant use. The 340B result illustrates that reducing providers' costs of stocking and offering LARCs can translate into lasting uptake among patients. The IPP reimbursement result suggests that financial incentives tied specifically to the delivery hospitalization—when women may have limited capacity for deliberate decision-making—can induce insertions that are subsequently removed. Therefore, policymakers seeking to expand LARC access should consider the clinical context in which uptake occurs.

More broadly, our results contribute to a growing literature using movers designs to decompose geographic variation in healthcare utilization, extending this approach

to reproductive healthcare. Pairing the decomposition with quasi-experimental policy analyses provides a more complete picture than either approach alone: the movers design establishes that the supply-side matters, while the policy analyses identify two concrete mechanisms.

Our findings also point to several directions for future research. First, it is important to understand the roles of other supply-side factors, such as provider training, clinic capacity, Medicaid managed care contract structures, and telemedicine availability in contributing to the observed geographic variation in LARC use. Second, our data end in 2019, predating both the COVID-19 pandemic's disruption to healthcare utilization and the Supreme Court's 2022 decision in *Dobbs v. Jackson Women's Health Organization*, which introduced new uncertainty into the broader reproductive healthcare environment. Whether the supply-side dynamics we document persist in the post-2020 period is an important open question.

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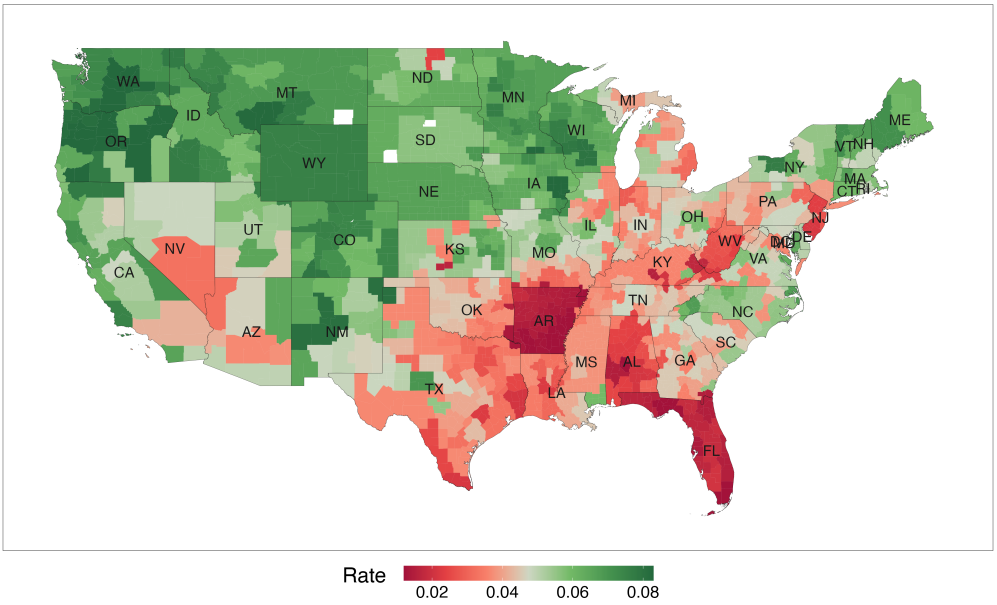
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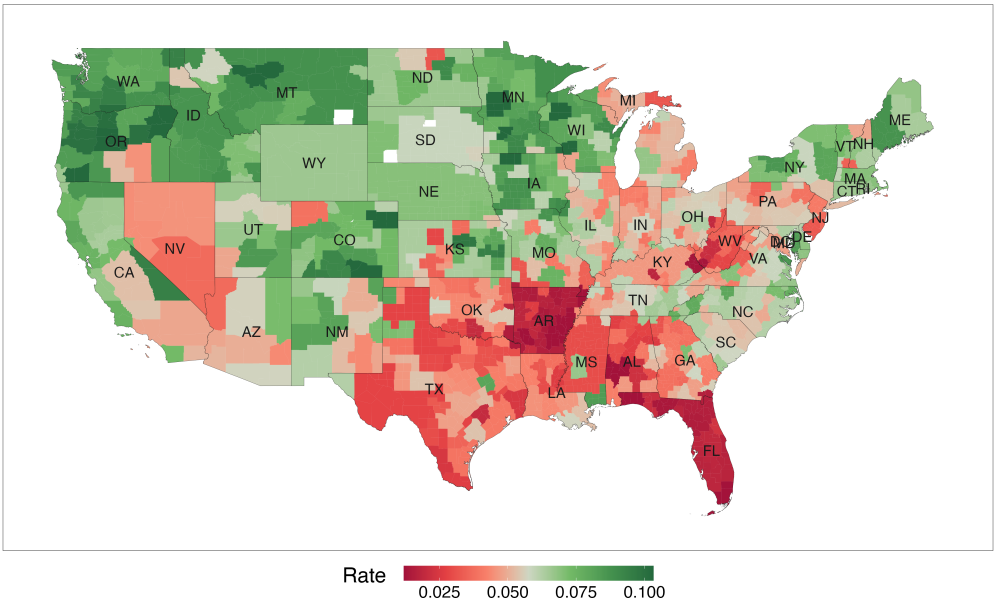
Figures & Tables

FIGURE 1: Geographic Distribution of LARC Rates

(A) Raw Rates



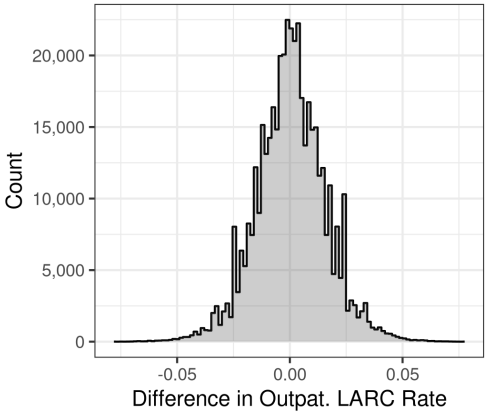
(B) Risk-adjusted Rates



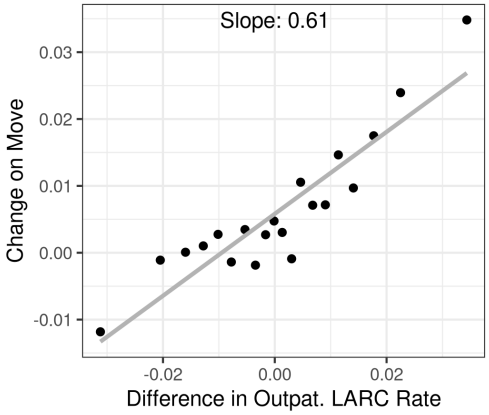
Note: This figure reports the rate of LARC usage across CZs. The sample is all movers and stayers. We first create indicators of outpatient LARC claims for each Bene-Year, then take the average within each CZ. Risk adjustment is performed by first regressing the outcome variable on age- group and and fixed effect, then average the residuals from the regression (re-centered to sample mean) by CZ.

FIGURE 2: Pre- and Post-Move LARC Use

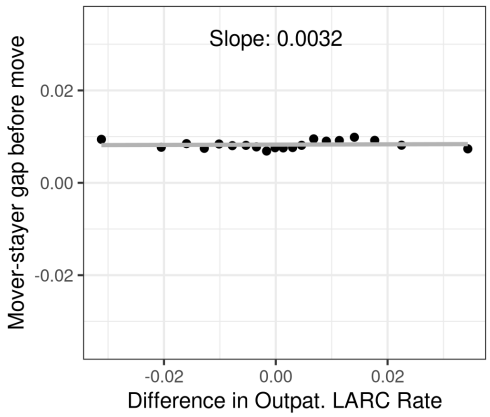
(A) Destination Minus Origin LARC Rate



(B) LARC Rate Change After Move

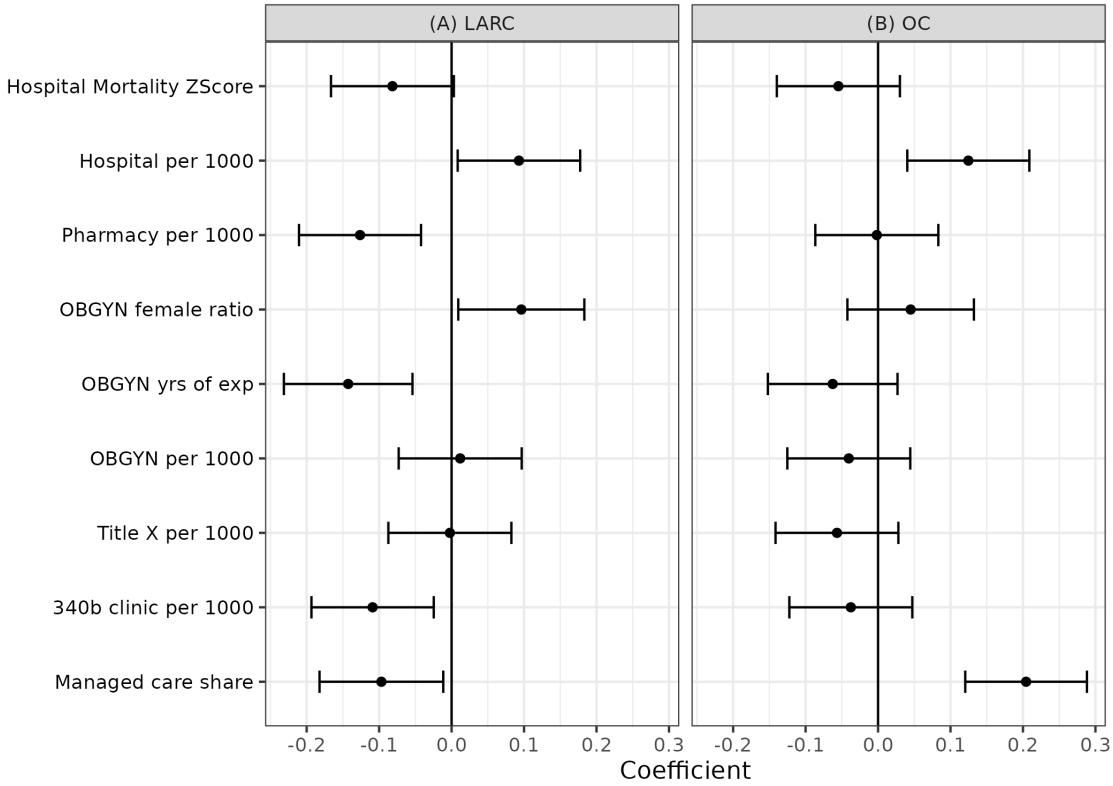


(C) LARC Rate Gap Movers vs. Stayers



Note: Panel (a) shows the distribution across movers of the difference in LARC rates between their origin and destination CZs (the gap). Panel (b) shows the relationship between the gap and changes in LARC rates. For each mover, we calculate the change in LARC rate before and after the move. The points report the mean change in LARC rate within each ventile of the gap. The line is obtained from a linear regression of the gap on the change in LARC rate after the move. Panel (c) shows the relationship between the gap and the difference in LARC rates between movers and stayers in their origin CZ. We first calculate the mover-stayer difference in LARC rate in each CZ, then assign it to movers according to their origin CZ. The points and lines are reported analogously.

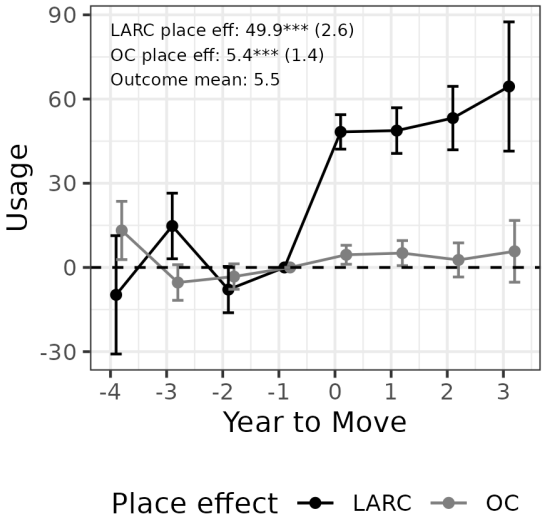
FIGURE 3: Correlates of LARC and OC Place Effects



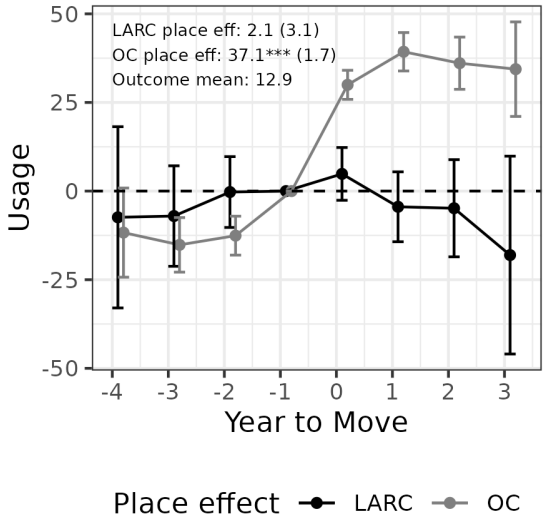
Note: Figure shows bivariate OLS regression results of CZ place effects of LARC (left panel) and oral contraceptives (right panel) on CZ characteristics. All covariates have been standardized by subtraction the sample mean and dividing by the standard deviation. Error bars show 95% confidence intervals. "Hospital Mortality ZScore" is the 30-day hospital mortality rate index, "Hospital per 1000" is the number of hospitals per 1000 CZ residents, "Pharmacy per 1000" is the number of pharmacies per 1000 CZ residents, "OBGYN female ratio" is the fraction of OBGYNs who are female, "OBGYN yrs of exp" is the average number of years of experience relative to 2019 among OBGYNs in the CZ, "OBGYN per 1000" is the number of OBGYNs per 1000 CZ residents, "Title X per 1000" is the number of Title X clinics per 1000 CZ residents, "340B clinic per 1000" is the number of safety net clinic per 1000 CZ residents, and "Managed care share" is the fraction of CZ residents enrolled with a Medicaid managed care plan. See Appendix A for the source of these covariates.

FIGURE 4: Event-Study Estimates of Place Effects On LARC and Oral Contraceptive Use

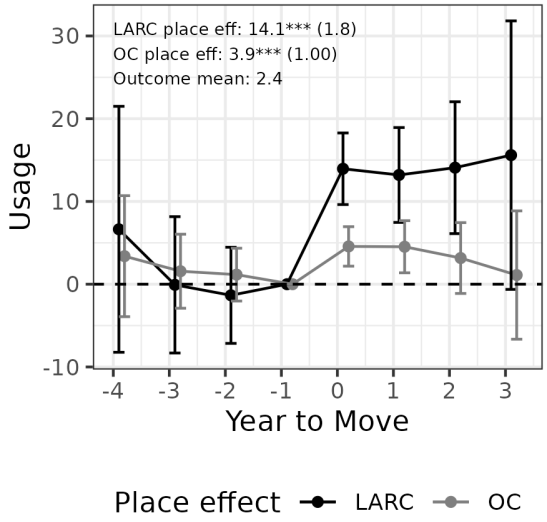
(A) LARCs



(B) Oral Contraceptives

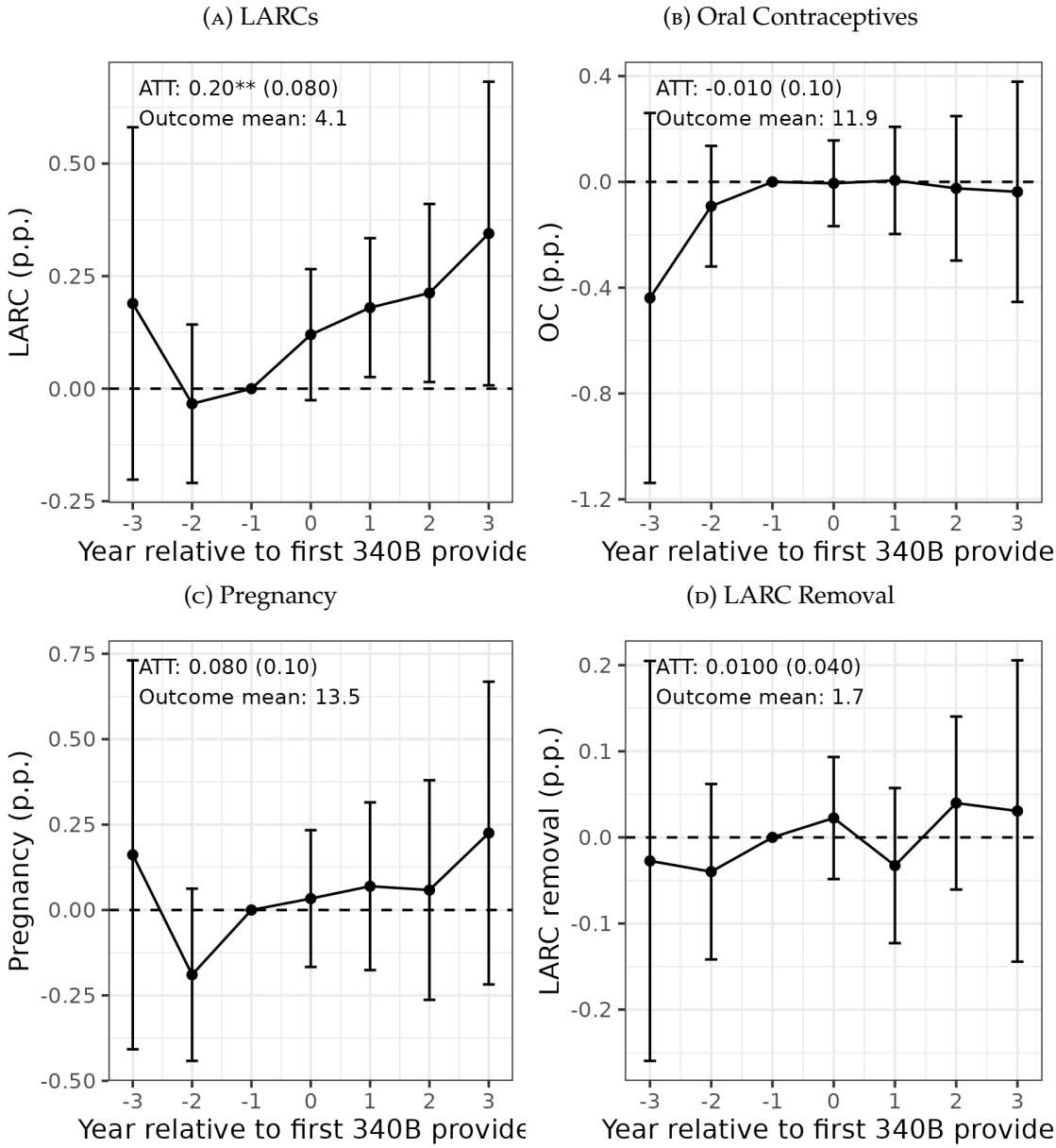


(C) LARC Removal



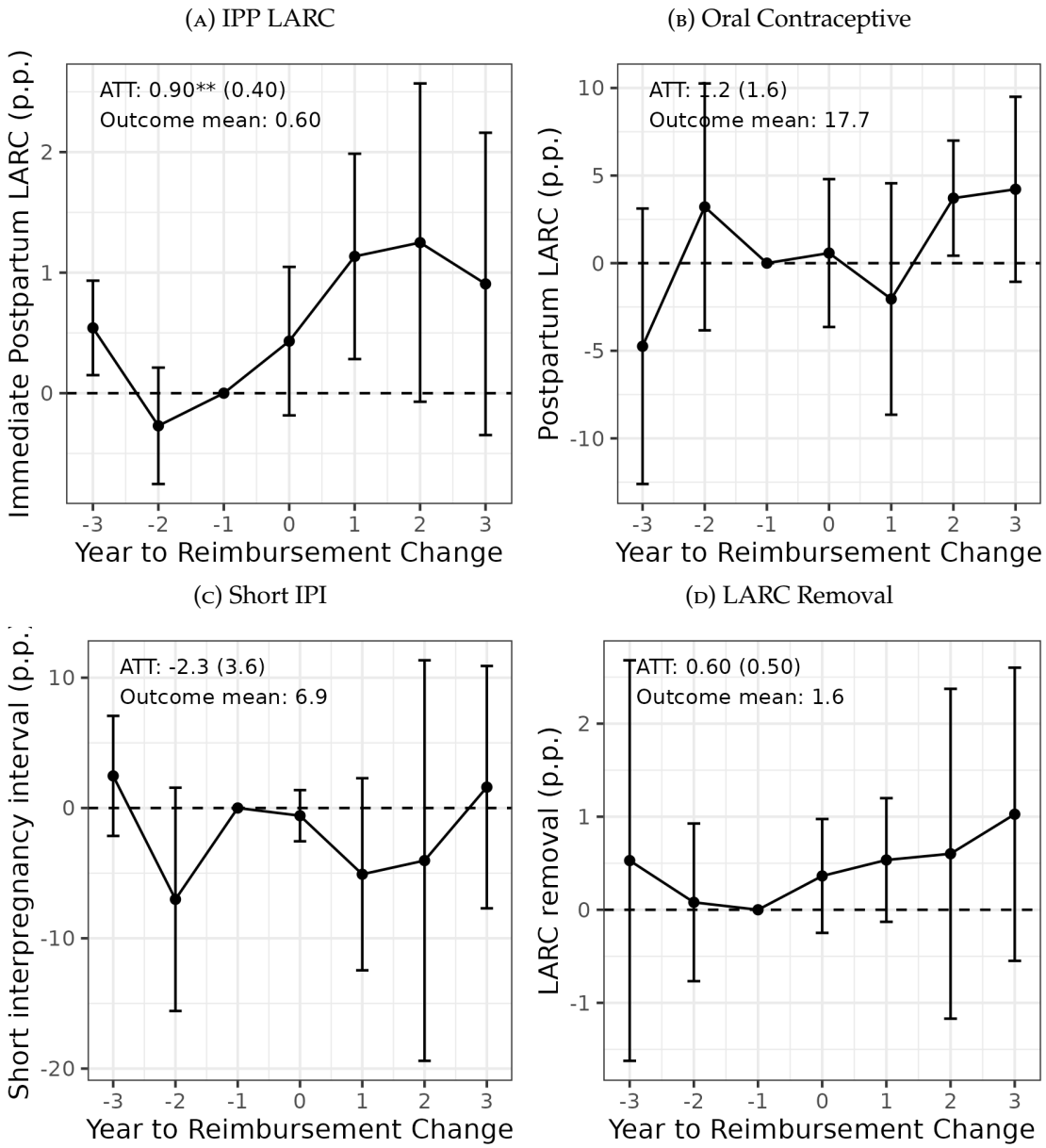
Note: This figure shows the dynamic effect of moving to CZs with higher LARC and oral contraceptives (OC) rate by interacting the difference in usage rate between the origin and destination CZ with indicators of year relative to the move (Equation B). Panel (a) report the effect on LARC usage. Panel (b) report the effect on oral contraceptives. The error bars report 95% percent confidence intervals based on standard errors clustered at the individual level. The "mean" reports average outcome among the movers in the year before the move.

FIGURE 5: Event-Study Estimates of the Effects of the 340B program on Contraceptive Use and Pregnancy



Note: This figure shows the dynamic effect of providers joining the 340B program on local contraceptive usage. The results are obtained with DiD estimator from Sun and Abraham (2021) controlling for state-year fixed effects (Equation 6). Panel (a) reports the effect on LARC usage, (b) on oral contraceptive usage, and (c) on pregnancy. The text report the averaged effect and standard deviation aggregated across the post periods. The error bars report 95% percent confidence intervals based on standard errors clustered at the ZIP level. The "mean" reports average outcome among the treated group in the year before treatment.

FIGURE 6: Event-Study Estimates of the Effects of the Medicaid IPP LARC Reimbursement Policy Change on Contraceptive Use and Short Inter-Pregnancy Intervals



Note: This figure shows the dynamic effect of state separately reimbursing immediate postpartum LARCs. The results are obtained with DiD estimator from Callaway and Sant'Anna (2021) using not-yet treated states as controls (Equation 7). Panel (a) reports the effect on immediate postpartum LARC, (b) on having any oral contraceptive claims in the next 365 days, (c) on short inter-pregnancy interval (starting pregnancy within 180 days after birth), and (d) on having any LARC removal claims in the next 365 days, (c). The text report the averaged effect and standard deviation aggregated across the post periods. The error bars report 95% percent confidence intervals based on standard errors clustered at the state level. The "mean" reports average outcome among the treated group in the year before treatment.

Table 1: Summary Statistics of Movers and Stayers

	Mover		Stayer	
	Mean	SD	Mean	SD
Demographics and enrollment				
Age	29.1	(9.045)	29.4	(9.937)
Black	0.192	(0.394)	0.210	(0.407)
Hispanic	0.185	(0.388)	0.208	(0.406)
White	0.463	(0.499)	0.375	(0.484)
MMC	0.488	(0.500)	0.616	(0.486)
Enrollment months	12.0	(0.432)	11.9	(0.260)
Residence region				
Midwest	0.232	(0.422)	0.213	(0.410)
Northeast	0.105	(0.306)	0.172	(0.377)
South	0.291	(0.454)	0.320	(0.467)
West	0.372	(0.483)	0.295	(0.456)
Healthcare use				
OC	0.126	(0.331)	0.120	(0.325)
LARC	0.059	(0.235)	0.046	(0.210)
LARC removal	0.024	(0.154)	0.018	(0.133)
Pregnancy	0.166	(0.372)	0.128	(0.334)
Number of Bene.	449,926		22,094,817	
Number of Bene-Years	1,578,903		56,551,358	

Note: This table presents the mean and standard deviation in parenthesis of characteristics of female Medicaid beneficiaries aged between 15 and 49. Column (1) includes movers beneficiaries who moved between CZs exactly once. Column (2) includes beneficiaries who lived in the same CZ during the sample period. Outcomes are measured in the year before the move.

Table 2: Mean Difference Decomposition of LARC Rates

	Above - Below Median	Top - Bottom 25%	Top - Bottom 10%	WI-AR
Outpat. LARC:				
Overall	0.036	0.060	0.084	0.066
Due to patients	0.015 (40.9%)	0.024 (39.3%)	0.036 (42.3%)	0.027 (41.4%)
Due to place	0.022 (59.1%)	0.036 (60.7%)	0.049 (57.7%)	0.039 (58.6%)
Oral contraceptive:				
Overall	0.065	0.108	0.150	0.130
Due to patients	0.040 (61.7%)	0.063 (58.4%)	0.083 (55.5%)	0.101 (77.8%)
Due to place	0.025 (38.3%)	0.045 (41.6%)	0.067 (44.5%)	0.029 (22.2%)

Note: The first panel reports the mean difference decomposition results with the indicator for LARC claims as the outcome, the second panel report the results with oral pills. For each outcome, the panel reports the estimates from equation 1 with the dependent variable y_{it} as the outcome and the age and age squared as the controls x_{it}. Each column defines two groups of CZs based on their LARC (oral pills) rates. Wisconsin (Arkansas) is the state with highest (lowest) overall contraceptive rate. The first row reports the difference in the average rates between the two groups. The second row reports the difference due to patient effects with its ratio relative to the total difference reported in parentheses. The third row reports the difference due to place effects.

ONLINE APPENDIX

Manuscript Title: Supply-Side Drivers of Long-Acting Reversible Contraceptives: Evidence from Medicaid Beneficiaries

Author Names: Maya Rossin-Slater, Natalia Serna, and Lin-Tung Tsai

A Additional Details on Data and Key Variables

A.1 CZ Characteristics

We use 2000 commuting zones (CZs), with ZIP codes mapped to CZs using the USDA crosswalk (USDA). Following Finkelstein et al. (2021b), we combine small CZs within the same state. Small CZs are defined as those with beneficiary counts below the 25th percentile of the CZ beneficiary count distribution in our sample.

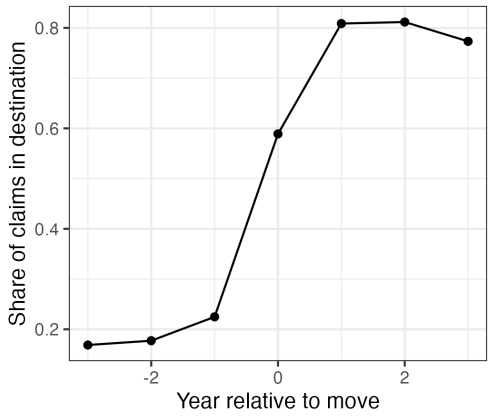
We combine various sources of data to obtain CZ characteristics. Socioeconomic variables (race share, uninsured rate, primary care visit rate, poverty rate, high school dropout rate, percent religious, total crime rate, unemployment rate) are from Chetty and Hendren (2018). Hospital Mortality Scores are from Chetty et al. (2016b). Provider characteristics are from the NPPES Registry[35] and the Medicare Provider National Downloadable File.[36] Newborn weight and infant mortality are from CDC WONDER.[37] Abortion facility access is from Myers (2024). Hospitals per capita are from the American Hospital Association Annual Survey. We calculate the birth rates based on our TAF data using the algorithm in Auty et al. (2024). For time-varying variables, we use the values in 2016.

A.2 Using Claim Location to Validate Moves

Finkelstein et al. (2016) used the location of claims beneficiaries made after their move to assess the validity of the moves. However, in our data, some states (such as California)

[35] Accessed from https://download.cms.gov/nppes/NPI_Files.html
[36] Accessed from https://data.cms.gov/provider-data/dataset/mj5m-pzi6
[37] Accessed from https://wonder.cdc.gov/controller/saved/D69/D452F649

FIGURE A1: Share of Claims in Destination CZ by Relative Move Year



Note: Figure shows the share of a mover's claims located in their destination CZ, among those in either their origin or their destination CZ. This figure uses a subset of the state-years with high-quality claim location (see Appendix A.2).

have a high missing rate in providers' location in the claims data. Therefore, we validate the moves in a sub=sample with high-quality claim locations.

This algorithm closely follows Finkelstein et al. (2016), except that our criterion is at the state-year level, whereas theirs is at the individual level. For each person-year among the movers, we define their destination claim share as the ratio of claims made in their destination CZ to claims made in either their destination or origin CZ (the provider location is based on the NPPES NPI Registry). Then, we calculate the change in this destination claim share before and after the move and aggregate it by origin state and move year. We then use only the state-years such that the average change in destination claim share is greater than 25%.[38] Appendix Figure A1 shows that in this sample, there is a sharp increase in the destination claim share in the move year.

A.3 Identifying Live Births in Medicaid Data

We follow the preferred approach in Auty et al. (2024) to identify live births in our Medicaid claims data and to remove false positives and duplicates. First, we remove OT claims with a null revenue code and a place of service other than a delivery room. Second, we remove

[38] For state-years with no post period, we keep them if the pre-period destination claim share is less than 25%. For state-years with no pre period, we keep them if the post-period destination claim share is greater than 75%.

claims with infant-related ICD codes outside of New Jersey and Tennessee. Third, we group birth claims into birth events by taking each beneficiary's first delivery claim as the first delivery date, removing subsequent claims within 180 days of this delivery, and counting the next claim (if any) as the subsequent delivery date, and repeating this process for up to five deliveries per person. Our analysis focuses on the first observed delivery for each beneficiary.

B Using a Structural Framework to Understand Movers Design

We can extend the model in equation (1) to allow for substitution between contraceptive methods. Specifically, suppose that in year t, woman i chooses a contraceptive method m from the set consisting of LARCs, OCs, and an outside option denoted by 0 that includes other methods (e.g., condoms, which are not observable in our data) as well as non-use. The indirect utility of woman i from choosing method m is given by:

$$U_{imt} = \underbrace{\alpha_{im} + \gamma_{j(i)m} + \tau_{tm}}_{V_{imt}} + \epsilon_{imt}$$

Assume that ϵ_{imt} follows a type-I extreme value distribution and that $U_{i0t} = \epsilon_{i0t}$. Then, the probability that woman i chooses method m in year t is:

$$p_{imt} = \frac{\exp(V_{imt})}{\sum_k \exp(V_{ikt})}$$

For each woman, the derivative of demand with respect to the mean utility V_{imt} is:

$$\frac{\partial p_{imt}}{\partial V_{ikt}} = \begin{cases} p_{imt}(1 - p_{imt}) & \text{if } m = k \\ -p_{imt}p_{ikt} & \text{if } m \neq k \end{cases}$$

Now, consider the case of a mover. Denote her origin CZ as $o(i)$ and her destination CZ as $d(i)$. For this woman, individual preferences given by α_{im} are fixed. But her mean

utility for method m changes after the move because of supply-side factors captured by $\gamma_{j(i)m}$. The change in mean utility is given by $dV_{imt} = \gamma_{d(i)m} - \gamma_{o(i)m}$ and the change in the woman's probability of choosing method m can be approximated by a first-order Taylor expansion as:

$$\Delta p_{imt} \approx p_{imt}(1 - p_{imt})dV_{imt} - p_{imt}p_{ikt}dV_{ikt}$$

Let $\delta_{im} = \overline{y}_{d(i)m} - \overline{y}_{o(i)m} = p_{d(i)m} - p_{o(i)m}$ and $E_{j(i)mt} = \frac{dV_{imt}}{\delta_{im}}$. Then, under a first-order approximation, δ_{im} is proportional to the change in mean utility, and we can rewrite the linear approximation above as:

$$\Delta p_{imt} \approx p_{imt}(1 - p_{imt})E_{j(i)mt}\delta_{im} - p_{imt}p_{ikt}E_{j(i)kt}\delta_{ik}$$

This equation shows two things about the movers design. First, it shows that a movers design can, in principle, be used to estimate demand responses to supply-side variation in a way that is grounded in the structural model (i.e., how changes in dV_{imt} translate into changes in choice probabilities Δp_{imt}). Movers designs provide quasi-experimental variation in local contraceptive supply conditions and therefore in the shadow prices of different contraceptive methods. By comparing the same woman before and after a move to areas with different method-specific utilization rates δ_{im}, we identify how demand responds to exogenous shifts in supply.

Second, the structural framework shows how our fixed-effects specification can be extended to approximate substitution between contraceptive methods in a reduced-form way. Specifically, we parameterize the interactions with δ_{im} to allow for separate coefficients by year relative to the move $r(i,t)$ as follows:

$$y_{imt} = \theta_{r(i,t)m}\delta_{im} - \theta_{r(i,t)k}\delta_{ik} + \alpha_{im} + \tau_t + \varepsilon_{imt}$$

The coefficients of interest $\theta_{r(i,t)m}$ capture the average reduced-form response of method m to destination-origin differences in method-specific utilization rates. The rest of variables and parameters are the same as in equation (5).

C Variance Decomposition

Consider the variance in our main regression model's prediction across CZs:

$$\text{Var}(\overline{y}_j) = \text{Var}(\hat{\gamma}_j) + \text{Var}(\tilde{y}_j) + 2\text{Cov}(\gamma_j, \tilde{y}_j) \tag{8}$$

If all CZs had the same patient characteristics, then $\text{Var}(\tilde{y}_j) = \text{Cov}(\gamma_j, \tilde{y}_j) = 0$, and the variance of predictions would be reduced by:

$$S_{\text{patient}} = 1 - \frac{\text{Var}(\hat{\gamma}_j)}{\text{Var}(\overline{y}_j)} \tag{9}$$

Similarly, if all CZs had the same place characteristics, then $\text{Var}(\hat{\gamma}_j) = \text{Cov}(\gamma_j, \tilde{y}_j) = 0$, and the variance of predictions would be reduced by:

$$S_{\text{place}} = 1 - \frac{\text{Var}(\tilde{y}_j)}{\text{Var}(\overline{y}_j)} \tag{10}$$

S_{patient} and S_{place} can alternatively be interpreted as the share of the variance of predictions that is explained separately by patient and place effects. Each of these components is non-negative by properties of the variance, however, their sum need not be equal to 1 due to the sample covariance between $\hat{\gamma}_j$ and $\tilde{y}_j$. To implement this decomposition, we split our sample of movers randomly into two groups of equal size, estimate equation (1) separately in each group, and then compute the sample analogs of variances and covariances.

D Duplicate Discount Prohibition for 340B Program

Federal law prohibits manufacturers from paying the Medicaid rebate on a drug already sold at the 340B ceiling price. Each Medicaid drug claim is therefore assigned to one channel: "carve-in" (340B price, no rebate) or "carve-out" (Medicaid rebate, no 340B), but not both.

Whether a claim qualifies for 340B depends on the beneficiary's enrollment (FFS or MCO), state policy, and the provider's status in HRSA's Medicaid Exclusion File (MEF).

For FFS beneficiaries, South Dakota and New Hampshire carve out all claims, so no 340B applies; in the remaining states, a claim qualifies for 340B if and only if the provider is listed in the MEF.

For MCO beneficiaries, Louisiana, Nebraska, New Hampshire, North Dakota, and Washington carve out all claims; states that cover prescriptions through FFS or extend the MEF to MCO follow the FFS rule; in all other states, 340B status is governed by the MCO contract, with each provider deciding separately whether to carve in.[39]

To account for carve-out, we drop beneficiaries in always-carve-out state-models (FFS in South Dakota and New Hampshire; MCO in Louisiana, Nebraska, New Hampshire, North Dakota, and Washington), and analyze the remainder separately by the provider's MEF status at the ZIP level.

E IPP LARC Reimbursement Policy Details

We collected information on the year of each state's Medicaid program's IPP LARC reimbursement policy change from a variety of sources, including state public health department websites and prior studies, CMS, and the American College of Obstetricians and Gynecologists' (ACOG) Medicaid reimbursement tracker (https://www.acog.org/programs/long-acting-reversible-contraception-larc/activities-initiatives/medicaid-reimbursement-for-postpartum-larc#maincontent). The last column of Appendix Table D1 lists the sources. To assign the policy year to each state we proceed as follows. First, we assign the CMS year when available; second, for states with missing year from step 1 we use information from Appendix Table D1; third, for states with missing policy year from steps 1 and 2, we use the ACOG's information.

Texas is assigned the year in Appendix Table D1 because the CMS entry reflects pharmacy rather than facility reimbursement. Several manual corrections are then applied: Idaho is coded as missing because it reversed its policy, Arkansas, Kentucky, and Nebraska are coded as never having implemented the policy, and Alaska, Maine, Rhode Island, and South Dakota are coded as missing because they have implemented the policy but no

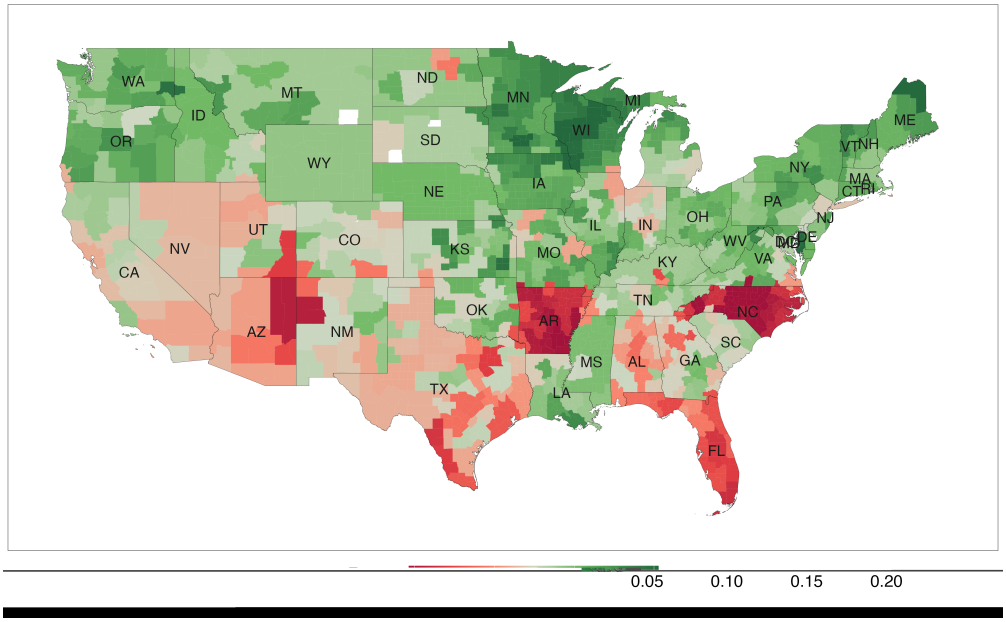
[39] We obtain information on these state policies from KFF (Gifford et al., 2020).

document confirming the exact year could be found.

F Additional Results

FIGURE D1: Geographic Distribution of Oral Contraceptive Rates

(A) Raw Rates

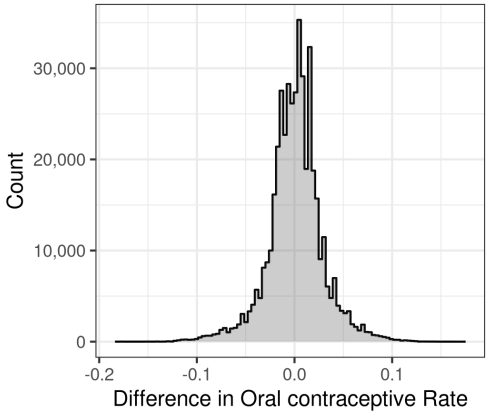


(B) Risk-adjusted Rates

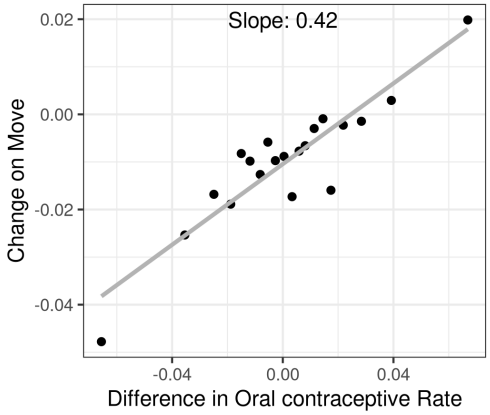
Note: This figure reports the rate of oral contraceptives usage across CZs. The sample is all movers and stayers. We first create indicators of outpatient oral contraceptives claims for each Bene-Year, then take the average within each CZ. Risk adjustment is performed by first regressing the outcome variable on age and fixed effect, then average the residuals from the regression (re-centered to sample mean) by CZ.

FIGURE D2: Pre- and Post-Move Oral Contraceptive Use

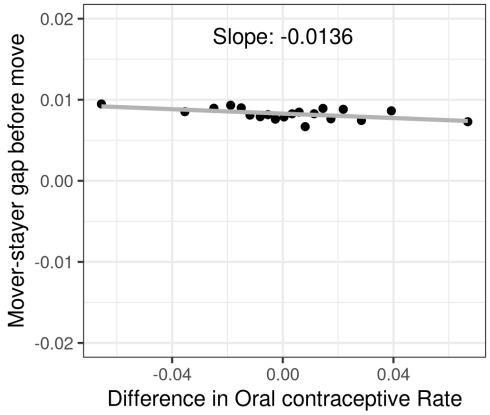
(A) Destination Minus Origin Oral Contraceptive Rate



(B) Oral Contraceptive Rate Change After Move



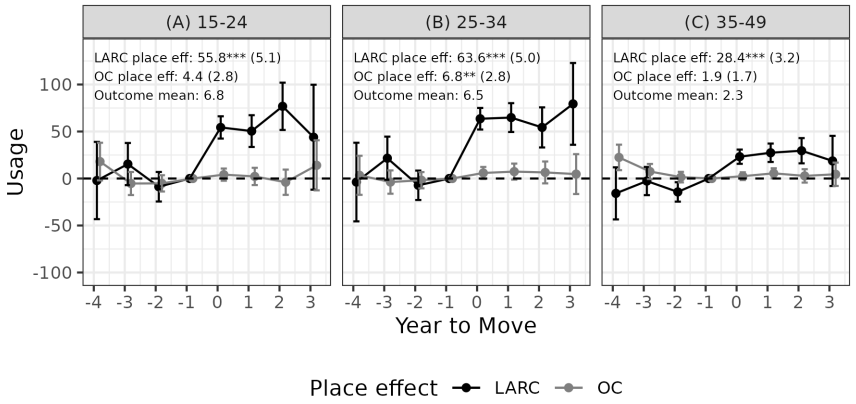
(C) Oral Contraceptive Rate Gap Movers vs. Stayers



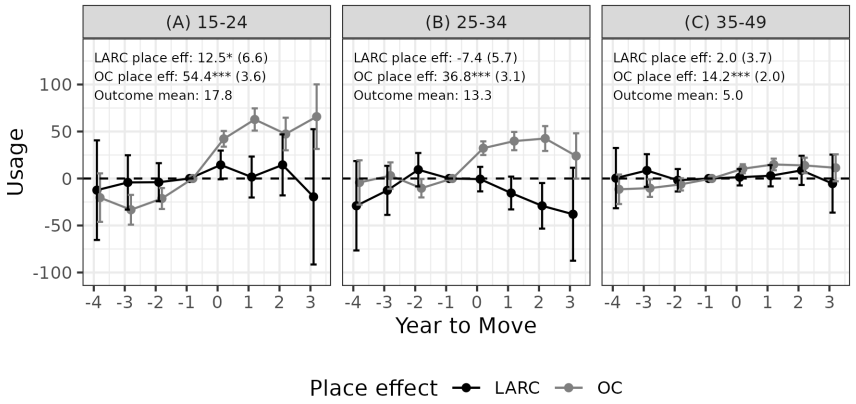
Note: Panel (a) shows the distribution across movers of the difference in oral contraceptives (OC) rates between their origin and destination CZs (the gap). Panel (b) shows the relationship between the gap and changes in OC rates. For each mover, we calculate the change in OC rate before and after the move. The points report the mean change in OC rate within each ventile of the gap. The line is obtained from a linear regression of the gap on the change in OC rate after the move. Panel (c) shows the relationship between the gap and the difference in OC rates between movers and stayers in their origin CZ. We first calculate the mover-stayer difference in OC rate in each CZ, then assign it to movers according to their origin CZ. The points and lines are reported analogously.

FIGURE D3: Heterogeneity of Place Effects by Age and Race

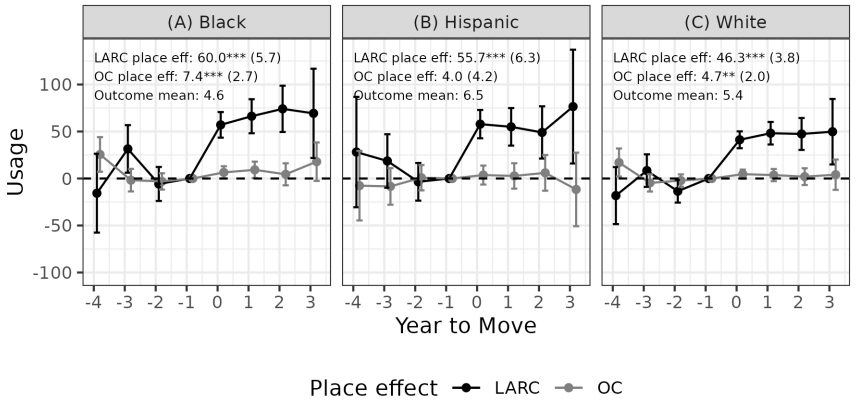
(A) Age group: LARCs



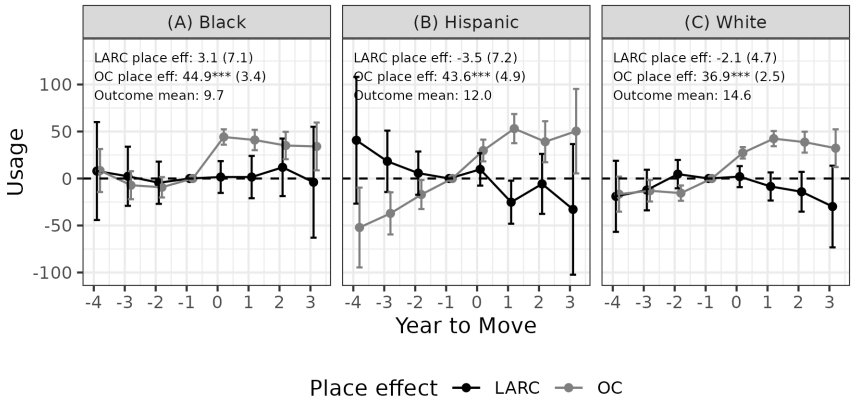
(B) Age group: Oral Contraceptives



(C) Race: LARCs

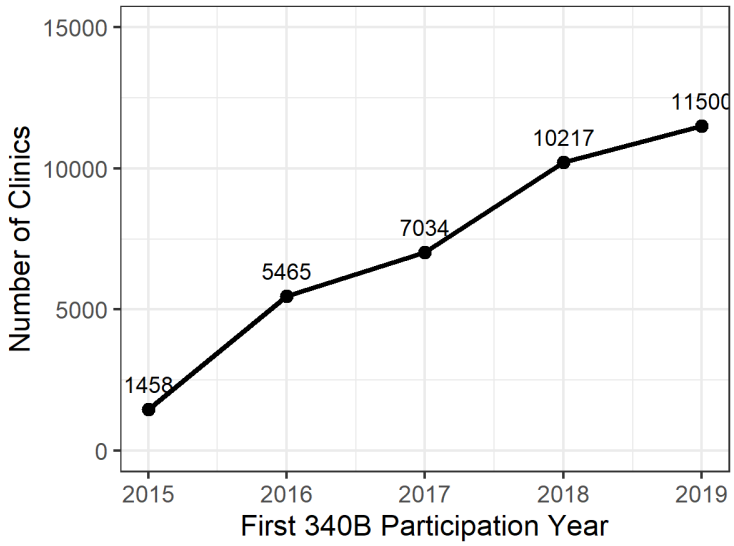


(D) Race: Oral Contraceptives



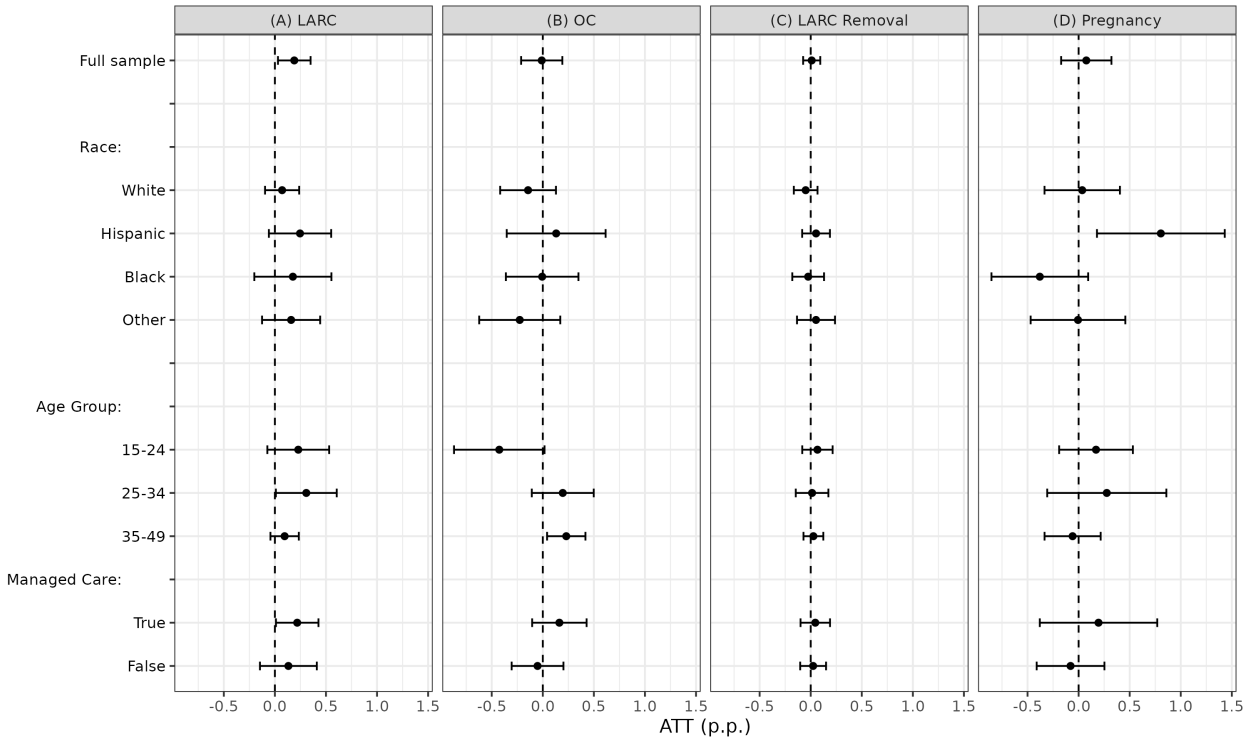
Note: These figures show the dynamic effect of moving to CZs with higher LARC and contraceptives rate from equation B. Panel (a) report the effect heterogeneity on LARC usage by age groups. Panel (b) report the effect heterogeneity on oral contraceptives usage by age groups. Panel (c) report the effect heterogeneity on LARC usage by race. Panel (d) report the effect heterogeneity on oral contraceptives usage by race. The error bars report 95% percent confidence intervals based on standard errors clustered at the individual level.

FIGURE D4: Number of Social Safety Net Providers by First 340B Participation Year



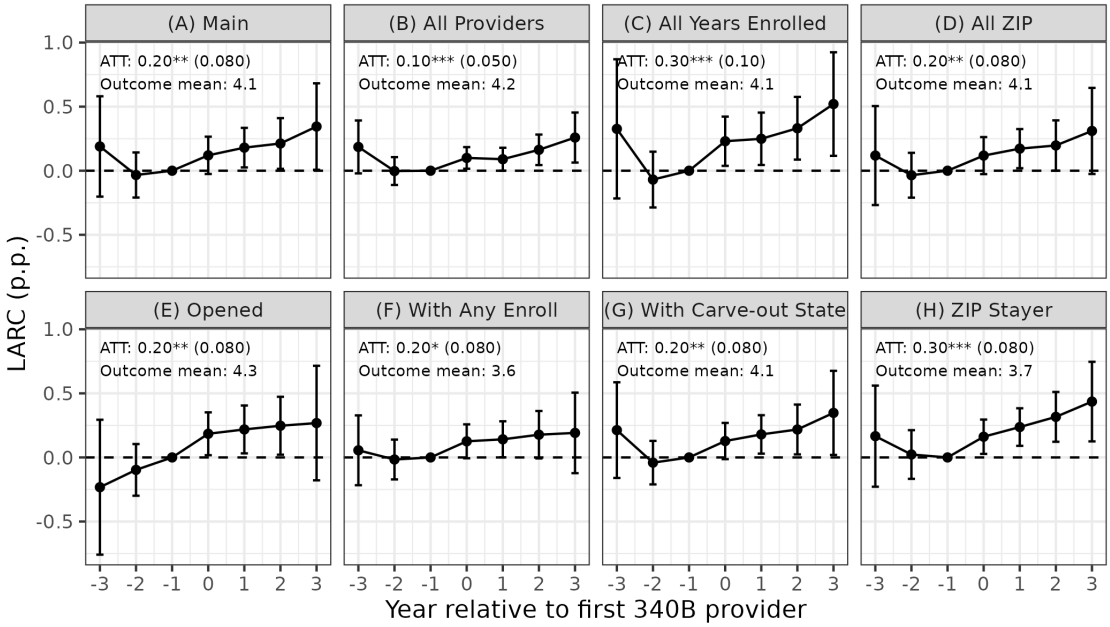
Note: This figure shows the total number of social safety net providers by year in which they started participating in the 340B program.

FIGURE D5: Heterogeneous Effects of 340B Program



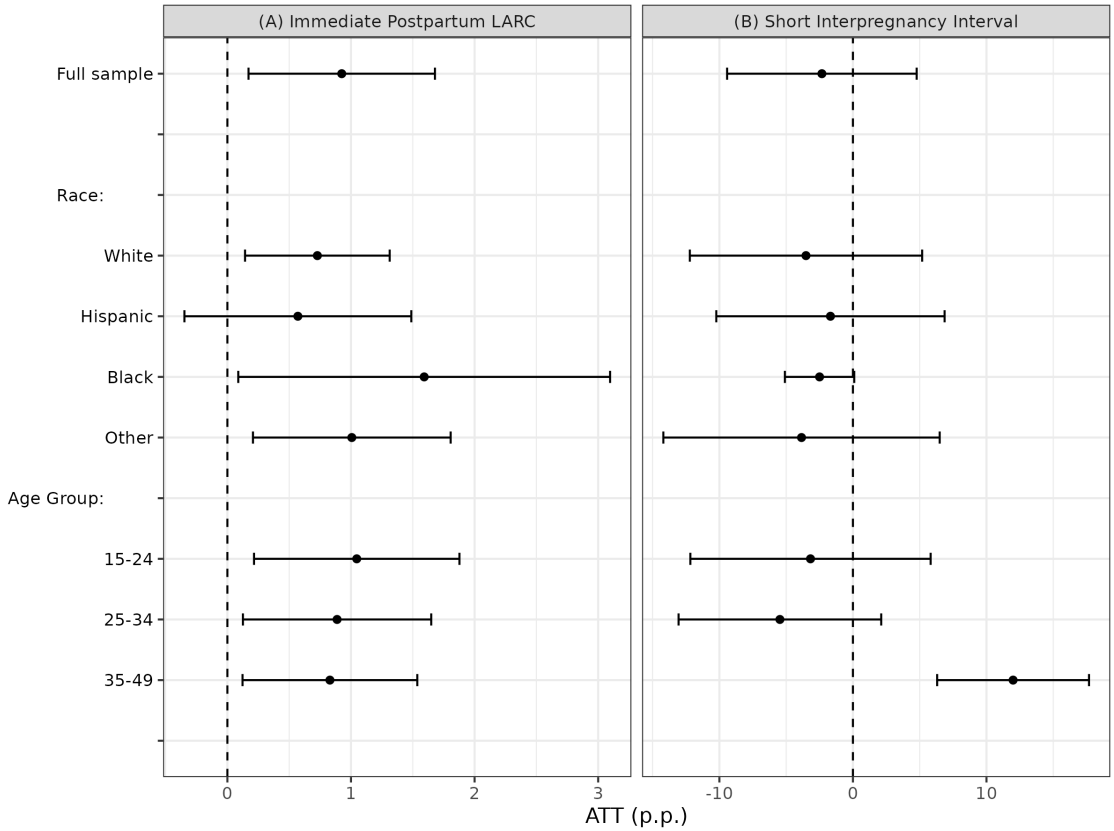
Note: This figure shows the heterogeneous effect of providers joining the 340B program on local contraceptive usage. The results are obtained with DiD estimator from Sun and Abraham (2021) controlling for state-year fixed effects (Equation 6), aggregated across the post periods. The left panel reports the effect on LARC usage, (b) on oral contraceptive usage. Each row reports the result estimated on the specified subsample. The error bars report 95% percent confidence intervals based on standard errors clustered at the ZIP level.

FIGURE D6: Robustness of 340B Results



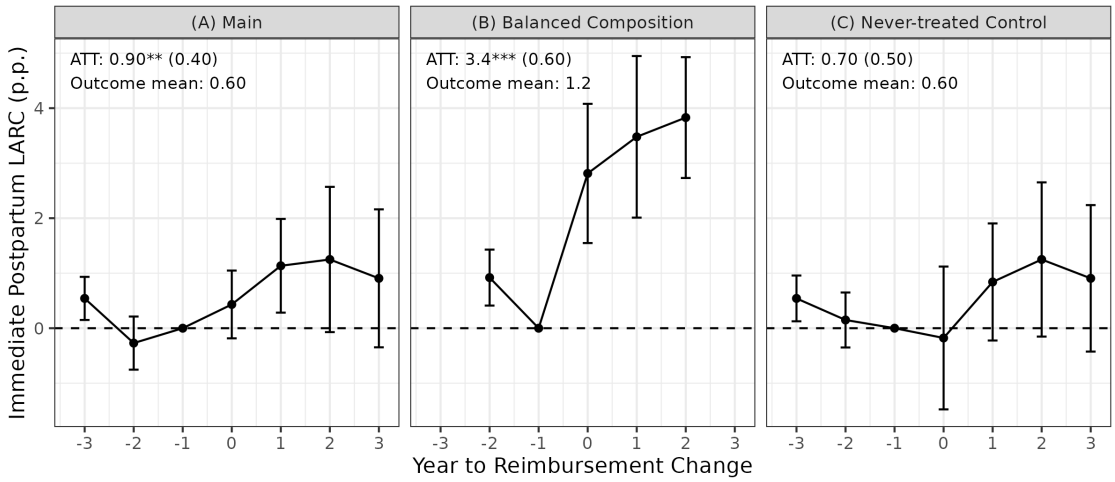
Note: Figure reports the robustness of the 340B DiD result to alternative sample construction choices. "Main" reports the result using the preferred specification as in Figure 5. "All Providers" includes all 340B providers as treatment group. "All Years Enrolled" limits the sample to beneficiary who are enrolled in all years in which their state is included in the sample. "All ZIP" includes other ZIPs discarded in population size matching. "Opened" conditions the treatment group to providers with claims in the year before joining 340B. "With Any Enrolled" relax the continuous enrollment criteria. "With Carve-out State" includes beneficiary living in states that carved-out the 340B program. "ZIP Stayer" filters to Bene. who only lived in one ZIP in the period.

FIGURE D7: Heterogeneous Effects of Medicaid's IPP LARC Reimbursement Policy Change



Note: This figure shows the dynamic effect of state separately reimbursing immediate postpartum LARCs. The results are obtained with DiD estimator from Callaway and Sant'Anna (2021) using not-yet treated states as controls (Equation 7), aggregated across the post periods. The left panel reports the effect on immediate postpartum LARC, (b) on short inter=pregnancy interval (starting pregnancy within 180 days after birth). Each row reports the result estimated on the specified subsample. The error bars report 95% percent confidence intervals based on standard errors clustered at the state level.

FIGURE D8: Robustness of IPP LARC Reimbursement Policy Change Results



Note: Figure reports the robustness of the IPP DiD result to alternative sample construction choices. "Main" reports the result using the preferred specification as in Figure 6. "Balanced Composition" limits the treatment group to cohorts continuously observed from two years before the policy to two years after. "Never-treated control" uses never-treated states as controls. "Sun and Abraham (2021)" uses the estimator from Sun and Abraham (2021).

TABLE D1: State-level Data Quality, MAX-to-TAF Transition Years, and Years of Immediate Postpartum LARC Reimbursement Changes

State	Low Data Quality	TAF Transition	IPP LARC Reimbursement	Source
Alaska		2013	Not Found	
Alabama		2014	2014	Alabama Medicaid
Arkansas		2015	Never	
Arizona		2014	2016	Arizona AHCCCS
California		2015	2015	LA County Public Health
Colorado	2016	2011	2013	Colorado HCPF
Connecticut		2015	2016	Connecticut DSS
Delaware	2016	2014	2015	NICHQ
Florida		2013	2016	USF Health
Georgia		2015	2014	
Hawaii		2014	2016	Hawaii Med-QUEST
Iowa		2014	2014	Steenland et al. (2022)
Idaho		2015	Reversed	
Illinois		2014	2012	Illinois PQC
Indiana		2014	2015	Indiana Medicaid
Kansas		2013	2018	Sunflower Health Plan
Kentucky	2015, 2017, 2018, 2019	2014	Never	
Louisiana		2015	2014	Louisiana DHH
Massachusetts		2014	2014	Massachusetts EOHHS
Maryland		2014	2013	Steenland et. al (2022)
Maine	2016	2014	Not Found	
Michigan		2015	2018	Michigan DHHS
Minnesota		2015	2024	Minnesota DHS
Missouri	2016	2015	2016	PubMed
Mississippi	2016	2015	2016	Mississippi Medicaid
Montana		2016	2015	Montana Medicaid
North Carolina		2013	2018	North Carolina DHHS
North Dakota	2015	2014	2024	North Dakota HHS
Nebraska	2017	2014	Never	
New Hampshire		2014	2018	New Hampshire Healthy Families
New Jersey	2016	2015	2018	New Jersey HCQI
New Mexico		2014	2014	
Nevada		2014	2016	Nevada DPBH
New York	2016	2015	2014	eMedNY
Ohio		2014	2017	Ohio Admin. Code
Oklahoma	2019	2014	2014	Oklahoma OKHCA
Oregon		2015	2017	Rodriguez et al. (2024)
Pennsylvania		2014	2016	Pennsylvania DHS
Rhode Island	2016, 2019	2012	Not Found	
South Carolina		2014	2012	South Carolina DHHS
South Dakota		2015	Not Found	
Tennessee		2015	2016	Hawaii Med-QUEST
Texas		2014	2016	Texas HHS
Utah		2015	2017	Utah Medicaid
Virginia		2014	2017	Virginia VDH
Vermont		2015	2016	Vermont AHS
Washington	2016	2015	2015	Washington HCA
Wisconsin		2014	2017	Kramer et. al (2021)
West Virginia		2015	2017	West Virginia DHHR
Wyoming	2016	2015	2022	Wyoming Medicaid

Note: Table reports state-specific information used in different parts of our analysis. The "Low Data Quality" column reports the years removed from the full sample due to having medium or high concern on the DQ Atlas, the "TAF Transition" column reports the year in which the state transitioned from MAX to TAF, which is removed from the full sample if it is in or after 2015. The "IPP LARC Reimbursement" column reports the year when the state's Medicaid program began reimbursing IPP LARC devices or insertion separately. Idaho reversed from de-bundled payment to bundled payment and is excluded in the IPP LARC analysis.

TABLE D2: Sample Inclusion Criteria and Associated Sample Sizes

Criteria	Number of Bene-Year Observations (in millions)
A. Full sample	
All Beneficiaries	434.1
High quality state-years	377.8
Reproductive-age women (age 15-49)	94.2
Valid residence	85.7
Enrollment equals residence state	84.8
B. Movers Analysis	
Continuously enrolled	58.9
Mover or stayer	58.1
C. 340B Analysis	
Continuously enrolled	58.9
ZIP Treated between 2016-2018 and matched controls	24.2
Non carve-out state	23.4
Safety net provider	17.7
D. IPP LARC Reimbursement Analysis	
Live Birth	6.4
First delivery	5.1
State treated later than 2016 and controls	2.8

Note: This table reports the sample criteria used in each empirical design and the associated sample sizes. 'All Beneficiaries' refers to the count of all beneficiary×year observations in the full TAF Eligibility and Demographics base file. The three design-specific samples are sub-samples of the full sample with further restrictions. See section 3.1 for the construction of the full and movers analysis sample, section 4.2 for the 340B analysis sample, and section 5.2 for the IPP LARC reimbursement analysis sample.

TABLE D3: Code Set for Outcome Variables

Outcome	Source	Codes
LARC	HHS OPA	**ICD**: 909, 996; T83; V25, V58; Z30; **CPT/HCPCS/PCS**: 119, 583, 697, 992, 996; J72–J73; Q00; S49; **NDC**: 20 codes
Oral contraceptive pill	HHS OPA	**ICD**: V25; Z30; **CPT/HCPCS/PCS**: S49; **NDC**: 1,542 codes
LARC removal	HHS OPA	**ICD**: V25; Z30; **CPT/HCPCS/PCS**: 119, 583, 977
Pregnancy	CMS MIH	**ICD**: 630–635, 638–679, 796; O00–O04, O07–O16, O20–O26, O28–O36, O40–O48, O60–O77, O80, O82, O85–O92, O94, O98–O99; V02, V22–V24, V27–V28, V30–V37, V39, V72, V89, V91; Z03, Z32–Z34, Z36–Z39; **CPT/HCPCS/PCS**: 050, 102, 109, 590–596, 598, 660, 690, 695, 699, 720, 722–724, 727–728, 730, 732, 735, 738–743, 747, 749, 751, 753–754, 759, 768, 800, 814–815, 964, 972, 995; H10
Live birth	Auty et al. (2024)	**ICD**: 650; O80; V27, V30–V37, V39; Z37–Z38; **CPT/HCPCS/PCS**: 594, 596

Note: This table reports the set of codes used in defining health and medical utilization variables. The codes listed here are used to match the claims data in the procedure described in Section 2. HHS OPA refers to code set from Office of Population Affairs (2026). CMS MIH refers to the Maternal and Infant Health code sets from Centers for Medicare & Medicaid Services (2026) and CMS PBHI to the Physical and Behavioral Health Integration codes. The live birth codes are as used in Auty et al. (2024). The ATC codes are translated to NDC codes using the NBER crosswalk. The NDC codes are compressed for brevity.

TABLE D4: Proportion of Place Effects in Mean Difference of LARC Rates between Above and Below Median CZ

Specification	Outpat. LARC	Oral contraceptive	N
Main	0.591	0.383	1047305
OT and RX Claim volume	0.552	0.298	901557
Claim Location Change	0.544	0.547	499291

Note: This table reports the share of place effect in the difference between CZs above and below median estimated with alternative samples. The first column report the results with LARC as the outcome, the second column report the results with oral contraceptives as the outcome. The first row reports the main estimates (row 3, column 1 in Table 2). The second row reports estimates with a sample restricting to the state-years with high-quality OT and RX claim volume patterns from the DQ atlas. The third row reports estimates with a sample restricting to the state-years with high-quality claim location patterns (see Appendix A.2). The thirdfourth row are estimated with a sample removing beneficiary with any LARC removal claims.

TABLE D5: Variance Decomposition Exercise

	Outpat. LARC	Oral contraceptive
CZ variance of average:		
Utilization rate	0.00042	0.00146
CZ effects	0.00015	0.00027
Patient effects	0.00017	0.00068
Correlation:	0.32486	0.59073
Share variance reduced if:		
CZ effects made equal	0.60571	0.53232
Patient effects made equal	0.63932	0.81510

Note: This table reports the variance decomposition results from equation 1. The first row reports variance of contraceptive rates across CZs. The second, third, and fourth rows report the variance of CZ effects, patient effects, and the correlation between the two fixed effects. The bottom panel report the share of variance across CZs reduced if CZ effects (patient effects) are made equal.

Table D6: 340B Analysis: Summary Statistics

	340B in 2016-2019		340B After 2019 or Never	
	Mean	SD	Mean	SD
Demographics and enrollment				
Age	29.35	(9.87)	29.25	(9.98)
Black	0.175	(0.38)	0.194	(0.396)
Hispanic	0.314	(0.464)	0.207	(0.405)
White	0.314	(0.464)	0.414	(0.493)
Enrollment months	11.95	(0.262)	11.95	(0.273)
Residence region				
Midwest	0.142	(0.349)	0.251	(0.433)
Northeast	0.198	(0.398)	0.168	(0.374)
South	0.17	(0.375)	0.314	(0.464)
West	0.49	(0.5)	0.267	(0.442)
Healthcare use				
OC	0.117	(0.321)	0.119	(0.324)
LARC	0.0464	(0.21)	0.0413	(0.199)
LARC removal	0.0181	(0.133)	0.0164	(0.127)
Pregnancy	0.132	(0.338)	0.127	(0.333)
Number of Bene.	782,803		6,152,200	
Number of Bene-Years	2,019,280		15,646,257	
Number of ZIPs	426		6,418	

Note: This table presents the mean and standard deviation in parenthesis of characteristics of the sample used in the 340B DiD design as defined in Section 4.2. Column (1) includes beneficiaries who live in ZIPs with the first safety net provider joining the 340B program between 2016 and 2019. Column (2) includes beneficiaries who live in ZIPs with the first provider joining the 340B program after 2019 or never. Outcomes are averaged across the sample period.

TABLE D7: IPP LARC Reimbursement Policy Analysis: Summary Statistics

	2016-2017		After 2018 or Never	
	Mean	SD	Mean	SD
Demographics and enrollment				
Age	27.0	(5.884)	27.0	(5.812)
Black	0.189	(0.361)	0.246	(0.391)
Hispanic	0.296	(0.361)	0.160	(0.353)
White	0.333	(0.438)	0.435	(0.467)
MMC	0.302	(0.384)	0.253	(0.293)
Enrollment months	9.550	(3.131)	10.2	(3.253)
Residence region				
Midwest	0.168	(0.374)	0.450	(0.497)
Northeast	0.106	(0.308)	0.119	(0.324)
South	0.579	(0.494)	0.422	(0.494)
West	0.147	(0.354)	0.009	(0.092)
Healthcare use				
LARC	0.125	(0.110)	0.133	(0.115)
Immediate Postpartum LARC	0.013	(0.105)	0.006	(0.071)
LARC removal	0.009	(0.091)	0.012	(0.105)
Live birth	1	(0)	1	(0)
Number of Bene.	2,221,097		630,984	
Number of Bene-Years	2,221,097		630,984	
Number of States	17		11	

Note: This table presents the mean and standard deviation in parenthesis of characteristics of the sample used in the IPP DiD design as defined in Section 4.2. Column (1) includes beneficiaries who live in states that de-bundled IPP LARC payment between 2016-2017. Column (2) includes beneficiaries who live in states that de-bundled IPP LARC payment after 2018 or Never. Since there is only 3 never-treated states, we include the late-treated states in the second column as they serve mostly as a control group in the estimation. Outcomes are measured in the year of the birth.