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

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Abstract

U.S. federal law mandates that most insurers offer contraceptives for free 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 and providers—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. Unlike OCs, LARCs require clinician insertion and upfront device stocking, making them especially vulnerable to supply-side forces. Using national Medicaid claims data from 2015–2019 and a movers design that separates place-based from patient-based variation, we find that local supply-side factors explain 58.5 percent of geographic variation in LARC use—substantially more than the 37.7 percent explained for OCs. We then estimate the causal effects of a supply-side policy that substantially lowers the cost of stocking LARCs for safety-net providers: the 340B Drug Pricing Program. Leveraging the staggered timing of program participation across clinics in an event-study design, we find that the arrival of the first participating clinic in an enrollee's ZIP code increases LARC use by 4.3 percent without displacing OC use or increasing LARC removals. Together, these findings identify the supply side as both a key source of unmet contraceptive need and a promising target for policy to improve access to the most effective forms of contraception.

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

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1 Introduction

More than half a century ago, the diffusion of the birth control pill transformed women's economic lives, allowing them to delay marriage and childbearing and to invest more in their education and careers (Goldin and Katz, 2002; Bailey, 2006; Guldi, 2008; Bailey et al., 2013; Bailey and Lindo, 2017). Contraceptive access continues to shape women's economic trajectories and the circumstances into which their children are born today (Bailey, 2020).[1] Yet access remains far from universal, and understanding which barriers prevent women from obtaining contraception—and which policies can lower them—is central to advancing both reproductive health and women's economic opportunity.

The 2022 *Dobbs v. Jackson Women's Health Organization* Supreme Court decision, which eliminated the federal constitutional right to abortion, has further raised the stakes of contraceptive access. With abortion banned or severely restricted in many states, contraceptive non-use or failure is now more likely to result in a birth, with recent evidence showing that post-*Dobbs* abortion bans have increased births by roughly two percent (Dench et al., 2024; Bell et al., 2025). Consistent with this, more than half of Obstetrician & Gynecologists (OBGyns) nationally report an increase in the share of patients seeking contraception since *Dobbs*, and especially long-acting options (Frederiksen et al., 2023).

Research on barriers to contraception use has focused overwhelmingly on the demand side: the costs, 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). But lowering demand-side barriers has not been enough. Although U.S. federal law requires insurers to cover contraception at zero cost to patients and about 90 percent of adult women are insured (KFF, 2026), nearly one in three reproductive-age women still report inadequate access to contraceptive services (Zapata et al., 2021). This paper examines the role of the "supply side"—the actions of healthcare facilities and providers, which can influence whether a service is available when a patient seeks it. A large literature shows that supply-side factors drive much of the geographic

[1] Contraception allows women to avoid unintended pregnancies, which are associated with a range of adverse outcomes, including domestic violence, poor maternal mental health, poor birth outcomes, and poverty (Cleland et al., 2012; Logan et al., 2007; Nelson et al., 2022).

variation in different types of health care use (Finkelstein et al., 2016; Molitor, 2018; Zeltzer et al., 2021; Ding, 2026), but their role in contraceptive access has received comparatively little attention.

We focus on long-acting reversible contraceptives (LARCs)—intrauterine devices (IUDs) and subdermal implants—which have the lowest typical-use failure rates of any reversible method (Guttmacher Institute, 2020). LARCs are an especially informative case because they depend on the supply side more than other methods do. Whereas oral contraceptives (OCs) require only a prescription, LARCs must be inserted at an in-person visit by a trained clinician, and facilities must stock the devices in advance at substantial upfront cost. Despite its effectiveness, LARC use is disproportionately low: among reproductive-age women enrolled in Medicaid, the main public insurance program for low-income Americans, fewer than 21 percent use LARCs, compared with 48 percent who use OCs (Frederiksen et al., 2022). Because Medicaid covers both methods without cost-sharing (Ranji et al., 2022; Centers for Medicare & Medicaid Services, 2026a), out-of-pocket costs cannot explain this gap. That leaves supply-side constraints and non-affordability-related demand-side factors (e.g., patient preferences and information gaps) as candidate explanations, and distinguishing between them is critical because each calls for a different policy response.

We use administrative national Medicaid claims 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 about a five-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 58.5 percent of the gap in LARC use between above- and below-median CZs, compared with only 37.7 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 33 to 55 percent larger than those for OCs.

Our event-study results indicate that moving from a CZ with zero LARC use to a CZ 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 at least three years following the move, suggesting that the local supply environment exerts persistent impacts on individuals' contraceptive utilization and that supply-side factors—rather than slow-moving demand-side adjustment—drive 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). Lastly, the effect on LARC insertions largely translates into lasting contraceptive use—we find that moving from a CZ with zero to 100 percent LARC use would lead to a 42 percentage point increase in the probability of getting a LARC that is retained for at

[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).

least a year, and the same move would lead to only a 14 percentage point increase in the probability of LARC removal.

Next, we examine 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 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 a supply-side policy that significantly reduced the cost of stocking and offering LARCs for providers serving low-income patients. The policy we study is the 340B Drug Pricing Program, which 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 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.[3] We use variation from the staggered timing in when existing safety-net clinics begin participating in the 340B program to study how provider access to the discounts affects Medicaid enrollees' LARC and OC use.[4] Using a Sun and Abraham (2021) event-study estimator, we find that clinic participation in the 340B program increases LARC use by 0.18 percentage points (4.3 percent relative to the pre-treatment mean) among Medicaid-enrolled women who reside in the same ZIP code as the clinic. We find no significant changes in OC use, implying that the 340B program is not causing a substitution away from oral contracep-

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

[4]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 versus 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; 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.

tives. We also show that the increase in LARCs is driven by those that are retained for at least a year and find no effect on LARC removals.[5]

This paper contributes to several 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, and *not* providers.[6] In the Contraceptive CHOICE Project, Secura 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-Affordable Care Act 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 a policy that reduces the providers' costs of offering LARCs without changing the patients' costs of obtaining them. Our analysis speaks directly to barriers that persist even when contraception is free to patients, as it is for the roughly 90 percent of American women who have health insurance (KFF, 2026).

Second, 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 spend-

[5] We also examine effects of the 340B program on the likelihood of pregnancy, but are underpowered to detect effects at conventional levels of statistical significance.

[6] 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).

ing, 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 extend this approach to contraceptive use, where prior work has focused largely on demand-side barriers, and complement the movers design with quasi-experimental evidence on how a policy that shifts local supply conditions affects reproductive health care.

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, while Section 4 provides the institutional details, empirical design, and results from our analysis of the

340B program. Section 5 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 (Stanford Center for Population Health Sciences, 2023a,b,c,d). We use the Transformed Medicaid Statistical Information System (T-MSIS) Analytic Files, or TAF, which are available for years 2015–2019.[7]

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.[8] 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 visits, emergency department encounters, and clinic-based care, along with associated ICD diagnosis and procedure codes.[9] 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

[7]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 E1 lists the MAX-to-TAF transition years for each state in the "TAF Transition" column.

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

[9]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.

organization), with usability recommendations published through the TAF Data Quality (DQ) Atlas.[10] We restrict our analysis to state-years rated as low or medium concern in the enrollment files. The "Low Data Quality" column of Appendix Table E1 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 empirical analyses, which we describe separately in each section below.

3 Place-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. Using data from the high-quality state-years 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 managed care or fee-for-service enrollment information are valid and non-missing, and (iii) residence state is the same as the enrollment state.[11] Appendix Table E2 reports the sample sizes that remain after applying each criteria. Our baseline analysis sample includes approximately 84.8 million beneficiary–year observations.

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

[11] 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.

Key Variables. Our main outcomes are indicators for two types of contraceptives: LARCs and OCs. We also create an indicator for LARC removal, as well as an indicator for a LARC insertion that is followed by no removal claim within the following 365 days to capture LARCs that are retained for at least one year. 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),[12] and identify claims containing these codes in the IP, OT, and RX files.[13] We merge these claims with the beneficiary–year sample using the unique beneficiary identifier, and create binary indicators capturing each outcome on an annual level.[14]

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).[15] CZs are geographical units created by the USDA's Economic Research Service that closely reflect where people live and work. We use the 2000 delineation, which follows the method of 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.

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

[12] See Appendix Table E3 for the exact list of codes.

[13] 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`.

[14] Only about 2.2 percent of beneficiary–years with a LARC insertion have it in an inpatient setting (most commonly, following childbirth); we restrict our attention to LARC insertions in outpatient settings.

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

observed period and move once.[16] 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.[17] "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 E2).

Descriptive Statistics. Table 1 presents the summary statistics for our sample, which is comprised of 449,897 movers and 22,094,922 stayers. Movers' characteristics are averaged over all their beneficiary–years in the sample. Across both groups, beneficiaries are on average 29 years old. About 19 percent of movers are non-Hispanic Black, 18 percent are Hispanic, and 46 percent are non-Hispanic white, while 21 percent of stayers are non-Hispanic Black, 21 percent are Hispanic, and 37 percent are non-Hispanic white. Movers and stayers have similar geographic distributions, although a higher share of movers reside in the West region. Movers are less likely than stayers to be enrolled in managed care plans. Lastly, movers are slightly more likely than stayers to have a LARC (5.7 percent compared to 4.5 percent) and an OC (12.5 percent compared to 12.0 percent). These patterns are consistent with documented contraceptive use in the KFF Women's Health Survey (Frederiksen et al., 2024).

[16] 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. Following Finkelstein et al. (2016), we use the location of the providers in the claims of movers to check that the moves are real and not due to data reporting issues. In the state–years with available information on provider location, the share of claims at providers in the destination CZ rises sharply in the move year (see Appendix A.2). 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). One concern is that moving to an area with higher or lower contraceptive use could itself affect the likelihood of enrolling in Medicaid, which would change the composition of our sample. We test this directly by using enrollment as an outcome and find no evidence of such a relationship (see below for more details).

[17] 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.

Figure 1 contains two maps that document geographic variation in LARC utilization across different CZs in our data.[18] Panel A shows the variation in raw LARC use rates 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 age-group and race/ethnicity fixed effects, using data from the movers, excluding the year of the move.[19] For each CZ, we then compute the average residual among the movers' beneficiary–years 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 1.8 percent in a Florida CZ to 9.4 percent in a CZ in Idaho, 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.[20]

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:

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

where y_{it} is a binary indicator equal to one if a mover beneficiary i has a LARC claim in year t, $\gamma_{j(i,t)}$ is a fixed effect for the CZ j in which mover i lives in year t, 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.

[18] While we drop some state–years due to poor data quality (see Appendix Table E1), no state is dropped entirely. Therefore, we have data for every (continental U.S.) state in these maps.

[19] We construct three indicators capturing ages 15–24, 25–34, and 35–49, and five indicators capturing race/ethnicity (Non-Hispanic White, Non-Hispanic Black, Hispanic, Other, and Missing).

[20] Appendix Figure E1 presents the analogous maps for oral contraceptive rates for comparison.

Variation and Identification. The coefficient of interest in equation (1) is $\gamma_{j(i,t)}$, which captures variation in LARC use after the move that is explained by differences across 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 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 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 0.62, suggesting differences in patient characteristics explain a little more than one third 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 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 E2 replicates this analysis for OCs. OC use exhibits substantial variation across CZs, and areas with higher LARC rates also tend to have higher OC rates (the correlation across CZs is 0.36). However, differences in patient characteristics appear to account for a larger share of the observed variation in OC use among movers, approximately 58 percent, compared to 38 percent for LARCs (Appendix Figure E2, 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.[21]

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 by studying a LARC removal indicator as well as an indicator for a LARC insertion that is retained for at least one year as separate outcomes.

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 above-median LARC rates, while group B includes CZs with below-median LARC rates),

[21] 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, the estimates from a single split vary a lot from split to split 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. A split that puts the woman with a LARC with one other woman yields $\frac{1}{4}$. The expected value across random splits is $\frac{1}{3}$, the rate in the full sample, so a single split is unbiased but noisy. Repeating the split many times and averaging across iterations reduces this noise.

then the mean difference in LARC use between the two groups can be written as:

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

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.[22]

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

[22] 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.

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, 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:

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

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).[23] The other variables and parameters are the same as in equation (1). Following Finkelstein et al. (2016), we also include relative-year fixed effects $\rho_{r(i,t)}$, which absorb changes in contraceptive use that occur around a move and are common to all movers regardless of their destination, such as a disruption of care during relocation. Without these fixed effects, the coefficients θ_s^m would load this common effect of moving whenever the average destination−origin difference among movers differs from zero.[24]

We estimate equation (5) separately for each of our key outcome indicators 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 dif-

[23] Specifically, to calculate CZ-level LARC and OC rates that we use to construct δ_i^m, we use data from the stayers across all sample years and the movers across all non-move years.

[24] Because the sample includes only movers, the individual, calendar-year, and relative-year fixed effects are jointly collinear with a linear trend. This does not affect the coefficients θ_s^m, which are identified from variation in δ_i^m across movers.

ference 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 rates of overall contraceptive use (LARC or OC), 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 58.5 and 60.3 percent, with patient effects explaining the remaining 39.7 to 41.5 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 20.8 percentage points higher for LARCs than for OCs and in column (4) the difference is 35.5 percentage points. In contrast, patient effects are substantially more important for OC use than for LARC use. In column (4), we find that 76.4 percent of the OC gap between Wisconsin and Arkansas is explained by patient effects compared to 40.9 percent for LARCs.

Appendix Table E4 assesses the sensitivity of our mean decomposition exercise to data quality issues. 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 when we only use state–years with high-quality OT and RX claims (rather than just high-quality enrollment files) from the DQ atlas in row 2. With this sample restriction, we find that the share of the mean difference in LARC use that is explained by place effects is 55.1 percent, in line with our baseline estimates.

Appendix C and Appendix Table E5 present a variance decomposition exercise in which we calculate by how much the variance in LARC and OC rates across CZs would be reduced if patient effects were made equal and if place effects were made equal. 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 59.9 percent compared to 52.6 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 the Medicaid program. 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, and the share of beneficiaries enrolled in managed care plans (rather than in fee-for-service Medicaid). 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

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

underlying the place effects we estimate, motivating our analysis of a distinct supply-side policy with a quasi-experimental design in Section 4 below.

Event-Study Results. Panel A of Figure 4 presents the event-study results from estimating equation (5), where the outcome is an indicator for a LARC insertion, and we study how a woman's propensity to get a LARC varies with the destination−origin difference in the LARC and OC rates, respectively, in the 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 the outcome an indicator for whether the woman has an OC prescription claim. Panel C uses an indicator for having a LARC removal as the outcome, while Panel D uses an indicator for having a LARC insertion that is retained for at least one year as the outcome. We report the estimated 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 49.6 percentage point increase in a woman's likelihood of getting a LARC (estimates in black). Put differently, LARC place effects explain about 50 percent of the individual LARC use propensity.[26] 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 zero to 100 percent OC use would increase individual LARC use propensity by about 5.2 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 a woman's probability of having an OC claim by 37.1 percentage points—a magnitude that is 25 percent smaller than the estimated 49.6 percentage point LARC effect in Panel A. Similar to LARCs, the OC place effect is stable throughout the post-move

[26] Note that this estimate is smaller than the effect implied by the 0.62 slope in Panel B of Figure 2, likely because here we include calendar year and relative year fixed effects along with controls for the origin to destination OC change.

period. The estimates in black in Panel B show that individuals' propensity to obtain OCs is uncorrelated with LARC place effects.

Panel C examines LARC removals. Moving from a CZ with zero to 100 percent LARC use raises the probability of a LARC removal claim by 13.9 percentage points. High-LARC-use areas therefore also increase removals, but by far less than they increase insertions (49.6 percentage points, Panel A).[27] Panel D confirms that most place-induced insertions are lasting. The same move in LARC use raises the probability of an insertion with no removal in the following year by 41.8 percentage points, implying that roughly $\frac{41.8}{49.6} = 84$ percent of place-induced insertions are retained a year later.

Heterogeneity by Age and Race/Ethnicity. Appendix Figure E3 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 from this sub-group analysis. 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.

Effects on Medicaid Enrollment. A potential concern is that local contraceptive access could affect who appears in our sample of Medicaid enrollees. This could happen because the adult Medicaid income eligibility threshold is higher during pregnancy, and supply-side contraceptive access may affect the likelihood of pregnancy. Thus, moving to an area with a higher contraceptive use rate could lower the probability of pregnancy and subsequently of Medicaid enrollment. To examine this issue, we use a sample of movers with no restriction on months enrolled before or after the move, and estimate our event-study model (5) using as the outcome an indicator for any Medicaid enrollment in a given year in Appendix Figure E4. We find that moving from a low- to a high-LARC CZ has

[27] The OC place effect on LARC removals is 3.8 percentage points, consistent with high-OC areas having small positive effects on both LARC insertions and removals, see the gray estimates in Panels A and C.

no detectable effect on enrollment, while moving from a zero to a 100 percent OC use CZ would increase the probability of enrollment by 9.4 percentage points (note, this effect goes in the opposite direction of the predicted mechanism where OC access would reduce the likelihood of pregnancy and thereby enrollment). These results suggest that our estimates of LARC place effects, which condition on enrollment both before and after the move, are unlikely to be driven by sample selection, but that our OC estimates could be partially impacted. To address this concern, we re-estimate the event-study models for the OC outcome without conditioning on enrollment in Appendix Figure E5, coding the outcome as a zero when a beneficiary is not enrolled. The OC place effect in this specification is 40.4 percentage points, close to our main estimate of 37.1 percentage points, suggesting that selection into enrollment does not drive the OC results.

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 how policy can influence the supply of LARCs. In the next section, we shed light on this issue by analyzing a policy that reduced the cost to healthcare providers of providing LARCs—the 340B Drug Pricing Program.

4 Supply-Side Policy: 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.[28] Eligible entities include disproportionate share hospitals (DSH) that serve a high percentage of low-income patients, Federally Qualified Health Centers (FQHCs), and other

[28]The program sets the price ceiling as the average manufacturer price (AMP) minus the unit rebate amount of the Medicaid Drug Rebate Program. The unit rebate amount is at least 23.1 percent of AMP for most branded drugs, 17.1 percent for clotting factors and drugs approved exclusively for pediatric indications, and 13 percent for generic drugs (Health Resources and Services Administration, Department of Health and Human Services, 2017). Every LARC in the sample (Mirena, Kyleena, Skyla, Liletta, ParaGard, and Nexplanon) is branded and classified as a single-source drug in the Medicaid Drug Rebate Program, so the 23.1 percent minimum applies to all LARCs (Centers for Medicare & Medicaid Services, 2023).

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, which was around $800 per device during our analysis period, not including insertion costs (Eisenberg et al., 2013). The discount thereby reduces a key financial barrier to stocking and offering these devices to patients.[29]

Once providers become eligible for the program (for instance, by receiving FQHC designation or Title X funding), they must register with Health Resources and Services Administration's (HRSA's) Office of Pharmacy Affairs, which verifies their grant status with the funding agency. Registration is accepted only during the first 15 days of each calendar quarter, and approved entities can begin purchasing at 340B prices on the first day of the following quarter. The timing of a safety-net clinic's entry into the 340B program therefore depends, first, on when it gains eligibility (e.g., when an existing community clinic becomes an FQHC) and, second, on when it chooses to register. That choice reflects both the clinic's expected savings and its capacity to meet the program's registration, verification, and contracting requirements. Participation grew steadily over our sample period. The number of safety-net clinics enrolled in the 340B program rose by about 30 percent from 5,270 in 2015 to 6,877 in 2019 (Appendix Figure E6), coinciding with Affordable Care Act-funded expansion of the health center program (U.S. Department of Health and Human Services, 2015). We leverage this staggered timing of entry into the program in our empirical design, as we discuss below.

Since our sample consists of Medicaid beneficiaries, it is also 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 HRSA Medicaid Exclusion File (see Appendix D

[29] Oral contraceptives also qualify for 340B discounts. However, because OCs are inexpensive and typically dispensed by pharmacies rather than purchased and stocked by clinics, providers face little or no financial barrier to offering them, so the discount is less likely to matter for OC provision. That said, we study the effects of the 340B program on OC use to directly examine this question.

for more details). We drop beneficiaries in state Medicaid models in which all drug claims are carved out from 340B: fee-for-service enrollees in New Hampshire and South Dakota, and managed care enrollees in Louisiana, Nebraska, New Hampshire, North Dakota, and Washington. We note that a provider barred from using 340B pricing on Medicaid claims can nonetheless procure and stock discounted devices through the program for use among other patients, meaning that 340B status remains relevant to contraceptive access even for carved-out enrollees.[30]

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 HRSA's Office of Pharmacy Affairs Information System (OPAIS).[31] For each entity, the directory reports its name, address, National Provider Identifier (NPI), and the years in which it began and, if applicable, stopped participating in the 340B program. Our main analysis focuses on 340B participants classified as safety-net clinics because they play a central role in contraceptive access for the Medicaid population (Frederiksen and Salganicoff, 2025).[32]

Since our Medicaid data include beneficiaries' residence ZIP codes as the smallest unit of geography, our main analysis leverages variation in the availability of 340B-participating safety-net clinics at the ZIP code level. Since patients may travel further than their own ZIP code to receive contraceptive services, we also test the sensitivity of our estimates to using county level variation.

We define treated ZIP codes as those whose first 340B participant is a safety-net clinic

[30] Consistent with this, our estimates are similar if we keep enrollees for whom drug claims are carved out from 340B (Panel F of Appendix Figure E9).

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

[32] To identify safety-net 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".

that joined the program after 2015. We then make three restrictions. First, we exclude ZIP codes first treated in 2019, since our data end in 2019: these ZIP codes have no post-treatment year, and their year of entry is only partially treated. Second, we only consider participants that remained in the program through at least 2019. Third, to avoid conflating 340B participation with new clinic openings (or with providers starting to accept Medicaid patients), we drop treated ZIP codes whose first clinic had no Medicaid claim in any year before joining the program. Control ZIP codes are those that either never had a 340B participant or had their first participant join after 2019. We drop all other ZIP codes from the analysis.

We then assign our Medicaid sample of female reproductive-age beneficiaries (described in Section 3.1) to either the treatment or control group based on their first observed residence ZIP code.[33] We only use beneficiary–year observations with continuous enrollment within the year (i.e., with annual enrollment spells lasting between 11 and 14 months, as explained above), but we do not require beneficiaries to be enrolled for the entire observation window in our main specification.

Descriptive Statistics. Appendix Table E6 reports summary statistics for the 340B analysis sample, comparing beneficiaries assigned to the treatment and control ZIP codes. The treatment group includes 444,198 beneficiaries in 275 ZIP codes and the control group includes 6,152,436 beneficiaries in 6,419 ZIP codes. Beneficiaries in both groups are on average 29.2 to 29.6 years old. Hispanic beneficiaries are overrepresented in treated ZIP codes (24.9 percent in treated vs. 20.7 percent in control ZIP codes), while non-Hispanic White beneficiaries are underrepresented (36.1 vs. 41.4 percent). Thirty-six percent of beneficiaries in treated ZIP codes live in the West, compared to 26.7 percent in the control ZIP codes. The rates of LARC use (4.6 vs. 4.1 percent) and oral contraceptive use (12.3 vs. 11.9 percent) are similar across groups.

[33] Assigning exposure based on the first observed ZIP code addresses potential endogenous mobility, such as beneficiaries systematically moving toward or away from areas with 340B participants. In a robustness check, we restrict the sample to beneficiaries who are only ever observed residing in one ZIP code and find very similar results, as discussed below.

4.3 Empirical Framework

Our empirical design compares within-person changes in contraceptive use following safety-net clinic 340B program participation among beneficiaries in treated ZIP codes relative to changes among beneficiaries in control ZIP codes using difference-in-differences and event-study designs. Since we find that the treatment ZIP codes have larger beneficiary counts on average than the control ZIP codes (see last row of Appendix Table E6), we implement a coarsened exact matching (CEM) approach (Blackwell et al., 2009) on state (exact) and ZIP code-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.[34]

Specifically, we estimate:

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

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 event-time 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 to control for all time-invariant individual-level characteristics and state–year fixed effects $\delta_{ts(i)}$ to account for any state-level policy changes and Medicaid data quality issues. The model is estimated using the 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 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 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.

[34] We also show results without CEM matching in Appendix Figure E9, as we discuss below.

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 LARC removal claim, and (D) a binary indicator for a LARC insertion retained for at least one year. 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 an existing safety-net clinic begins participating in the 340B program, the probability of a LARC insertion among women residing in the same ZIP code increases by 0.18 percentage points, or 4.3% relative to the pre-treatment mean. We do not find any evidence of significant effects on oral contraceptive use (Panel B), implying that program-induced LARC insertions do not reflect a substitution away from OCs. Panel D shows that the probability of a LARC insertion retained for at least one year increases by 0.22 percentage points, or 6.3% relative to the pre-treatment mean. The magnitude of the effect on persistent LARC use is larger in both absolute and relative terms than on LARC insertion, implying that program-induced LARC insertions are aligned with patients' long-term contraceptive preferences. Consistent with this, we also do not find any significant effect on LARC removals (Panel C).[35]

Appendix Figure E8 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 the 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.

[35] We also study the effect of the 340B program on the probability of pregnancy in Appendix Figure E7, using as the outcome an indicator that is equal to one if a beneficiary has any pregnancy-related claim. See Appendix Table E3 and the CMS Maternal and Infant Health codes in Centers for Medicare & Medicaid Services (2026b) for the exact codes. We do not detect any significant effects on pregnancy, likely due to a lack of statistical power given the low absolute effect size on LARC take-up.

Robustness. Appendix Figure E9 reports the sensitivity of our event-study estimates to alternative sample and treatment/control group definitions using the LARC insertion claim as the outcome of interest. We replicate the baseline result in Panel A for reference. Panel B expands the treatment group to include all providers participating in 340B rather than only safety-net clinics; Panel C restricts the sample to beneficiaries continuously enrolled throughout their state's sample period; Panel D uses all control ZIP codes without CEM matching; Panel E includes beneficiaries who are not continuously enrolled within a given year; Panel F adds states that carve out the 340B program for Medicaid beneficiaries; Panel G includes ZIP codes whose first 340B clinic had no Medicaid claims before joining the program in the treatment group; and Panel H drops beneficiaries who move ZIP codes over the sample period. Our estimates are remarkably robust across all of these alternatives. Appendix Figure E10 presents results in which we define treatment and control units at the county (rather than ZIP code) level. The resulting estimate of 0.21 percentage points is similar to our main estimate of 0.18 percentage points, but it is noisier because only 58 counties are treated, compared with 275 ZIP codes.

Lastly, Appendix Figure E11 assesses whether safety-net clinic participation in the 340B program affects selection into our analysis sample through changing who is enrolled in Medicaid. We collapse our data to the ZIP code–year level, and estimate a version of model (6) using the natural log of the number of female reproductive-age Medicaid enrollees as the outcome variable, with each ZIP code weighted by its 2010 population from the US Census (U.S. Census Bureau, 2010). In Panel A, we include all treatment and control ZIP codes. In Panel B, we restrict to treated ZIP codes that are observed in every year from two years before to two years after the treatment and use only these years in the estimation. We do not find any significant effects on Medicaid enrollment in either specification, implying that changes in sample composition are not biasing our main estimates of the effects of the 340B program on contraceptive use.

5 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. This paper shifts attention from the demand side—patient affordability and preferences—to the supply side, asking how much of the geographic variation in contraceptive use among Medicaid beneficiaries is attributable to local healthcare environments, and whether supply-side policies that lower the cost of offering expensive contraceptives to providers 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 58.5 percent of geographic variation in the use of long-acting reversible contraceptives—substantially more than the 37.7 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 a supply-side policy that reduced providers' financial barriers to offering LARCs. Participation in the 340B Drug Pricing Program by safety-net clinics increases LARC use by approximately 4.3 percent relative to pre-treatment levels, without displacing oral contraceptive use or encouraging removals. In fact, persistent LARC use—as measured by LARCs that are retained for at least a year—rises by 6.3 percent, implying that program-induced insertions are aligned with patient long-term preferences. These findings imply that the 340B program reduces 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.

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 a quasi-experimental analysis

of the 340B program provides a more complete picture than either approach alone: the movers design establishes that the supply-side matters, while the 340B analysis identifies a concrete causal mechanism by which the supply-side can shape contraceptive use.

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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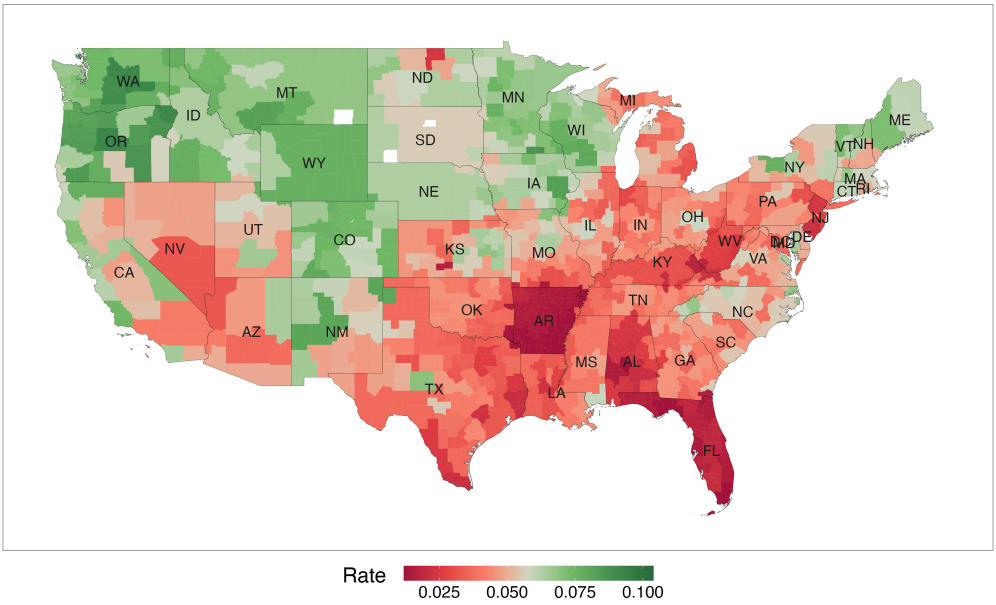
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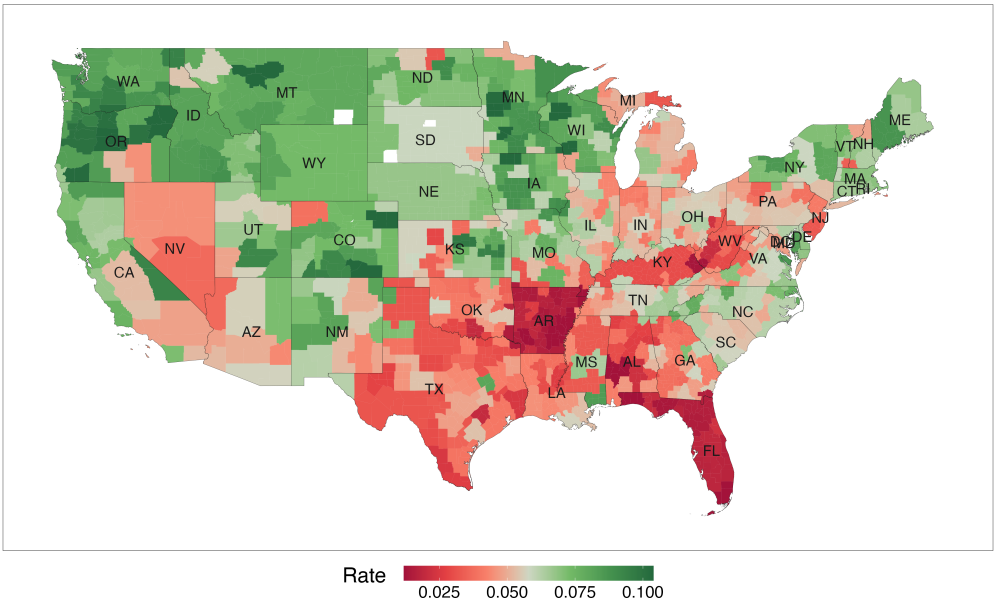
Figures & Tables

FIGURE 1: Geographic Distribution of LARC Use Rates

(A) Raw LARC Rates



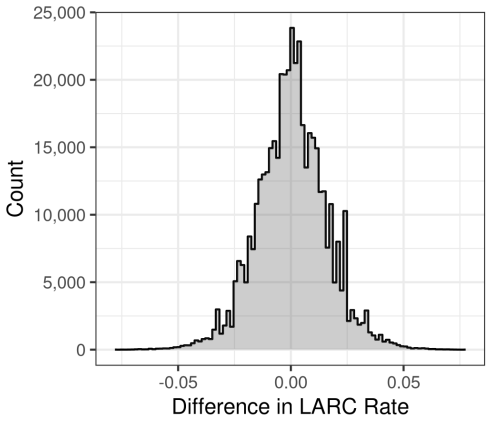
(B) Risk-adjusted LARC Rates



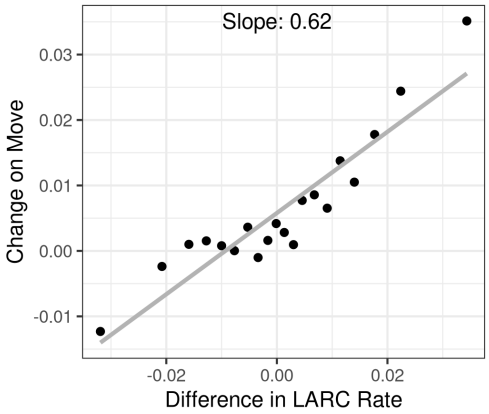
Note: This map shows average LARC use rates across commuting zones (CZs). The sample includes all movers and stayers (see Table 1 for more details). We present raw (unadjusted) averages in Panel A. In Panel B, we regress the LARC indicator on age-group and race/ethnicity fixed effects using only data from the movers (and excluding the year of the move), and then average the residuals in each CZ and re-center them to the sample mean.

FIGURE 2: Pre- and Post-Move LARC Use

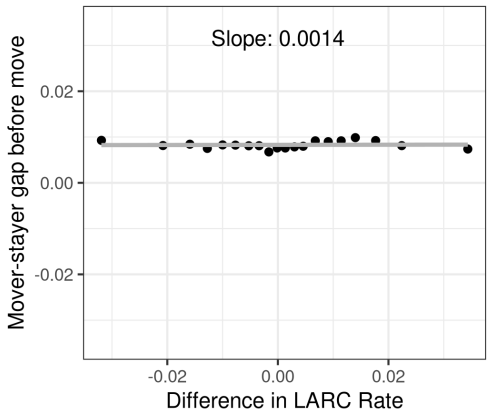
(A) Destination Minus Origin LARC Rate



(B) LARC Rate Change After Move

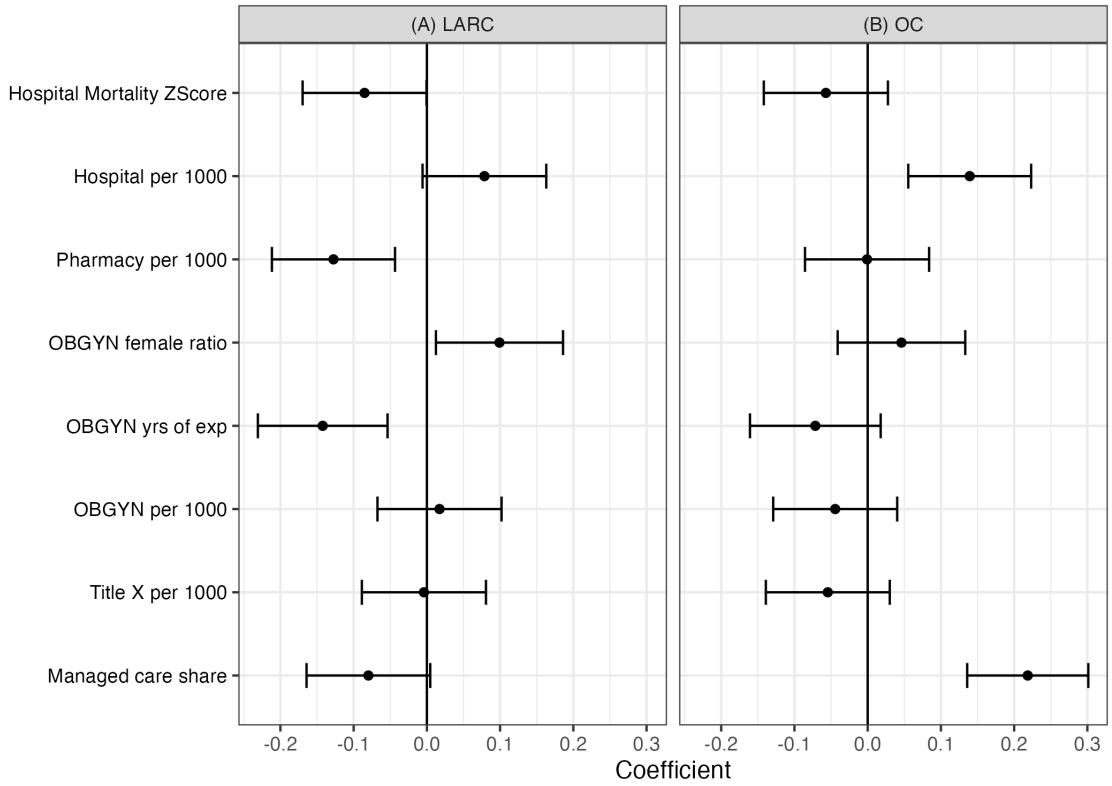


(C) LARC Rate Gap Movers vs. Stayers



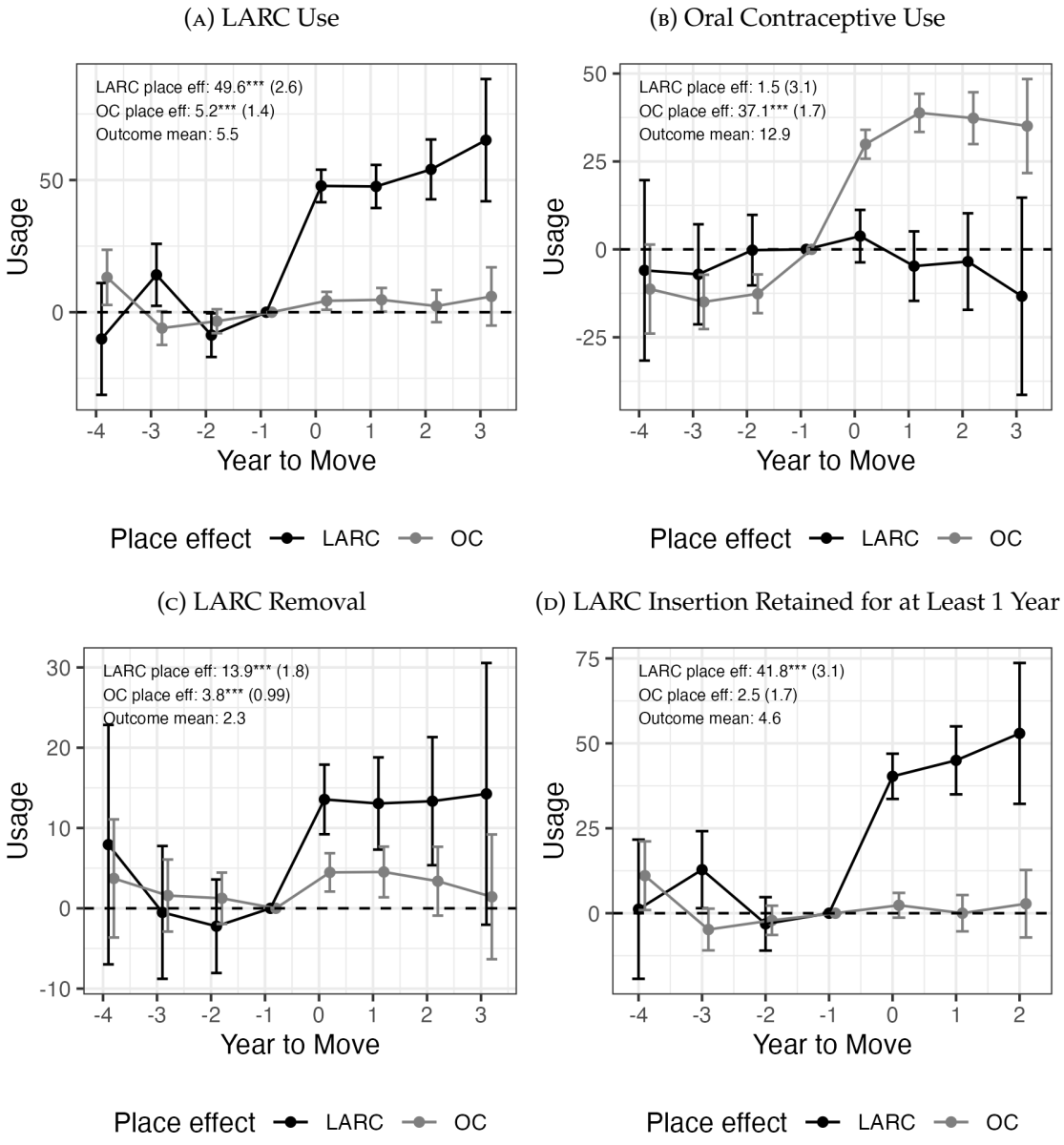
Note: Panel A shows the distribution across movers of the difference in LARC rates between their origin and destination CZs (the gap). Panel B shows the relationship between the destination–origin gap and the change in LARC use rate following the move. For each mover, we calculate the change in LARC rate from before to after the move. The points report the mean change in LARC rate within each ventile of the destination–origin gap. The line is obtained from a linear regression of the change in LARC rate after the move on the gap. Panel C shows the relationship between the destination-origin 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 report the mean of this difference within each ventile of the destination–origin gap. The line is obtained from a linear regression of this difference on the gap. The CZ LARC rates are raw rates, computed from the stayers in all sample years and the movers in all years except the year of the move.

FIGURE 3: Correlates of LARC and OC Place Effects



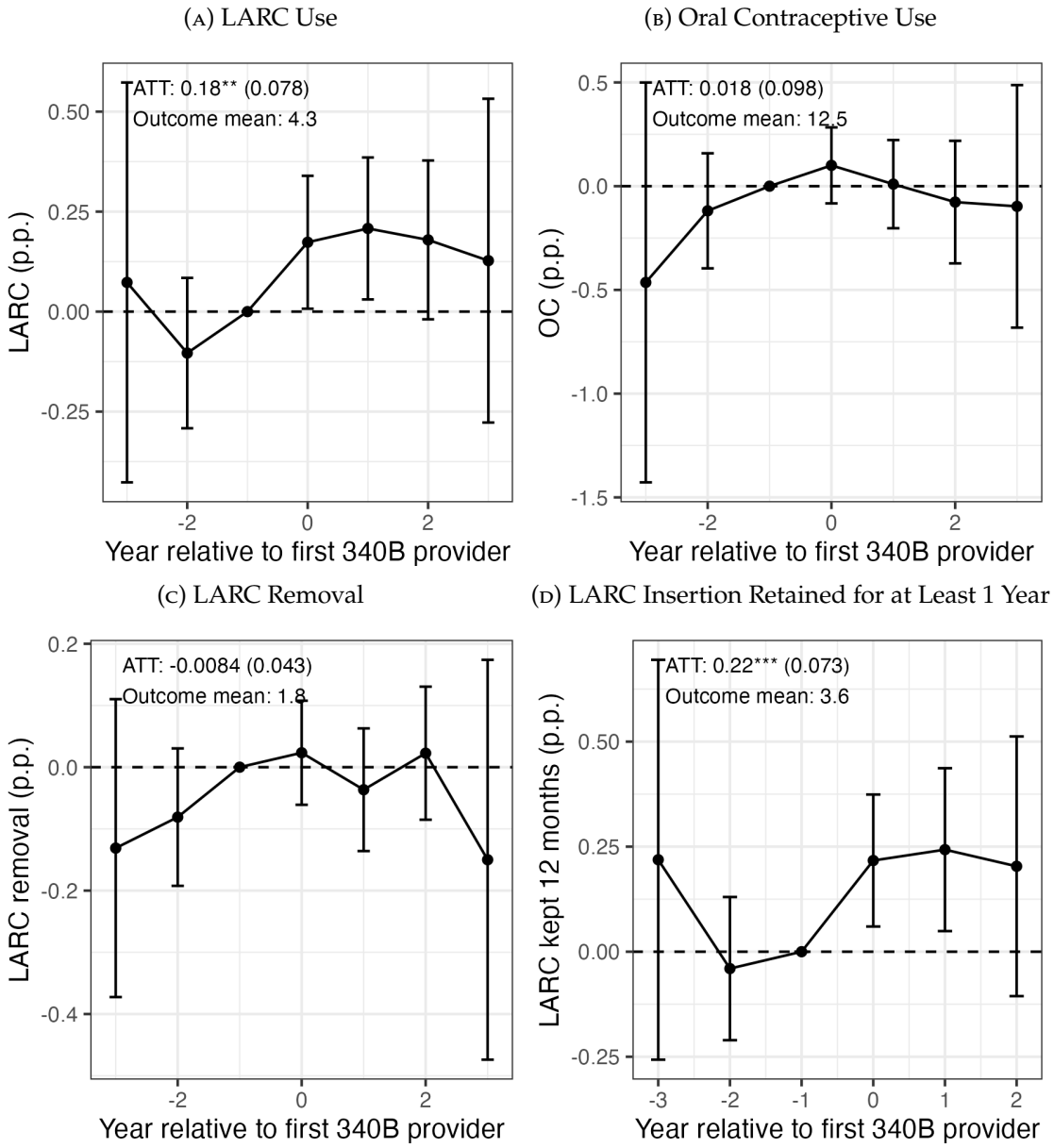
Note: Figure shows results from bivariate OLS regressions of CZ place effects of LARC (left panel) and oral contraceptives (right panel) on CZ characteristics. Both the place effects and the characteristics are standardized across CZs by subtracting the mean and dividing by the standard deviation, so each coefficient is the correlation between the place effect and the characteristic. 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, and "Managed care share" is the fraction of sample beneficiary–years in the CZ with full-year enrollment in a Medicaid managed care plan. See Appendix A for the data sources of these covariates.

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



Note: This figure presents event-study results from estimating model (5) using the following binary indicators as outcomes: (A) LARC insertion claim, (B) Oral contraceptive claim, (C) LARC removal claim, and (D) LARC insertion not followed by any removal within the following 365 days. This last outcome is not defined in 2019, so the event window of panel D ends one year earlier. The error bars report 95% 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



Note: This figure presents event-study results from estimating model (6), where treatment is defined by a ZIP code's first pre-existing safety-net clinic joining the 340B program (see Sections 4.2 and 4.3 for more details). We use the estimator from Sun and Abraham (2021), and include controls for individual and state–year fixed effects. We use the following binary indicators as outcomes: (A) LARC insertion claim, (B) Oral contraceptive claim, (C) LARC removal claim, and (D) LARC insertion not followed by any removal within the following 365 days. The outcome in panel D is not defined in 2019, so its event window ends one year earlier. The text reports the average treatment-on-the-treated effect and standard error. The error bars report 95% confidence intervals based on standard errors clustered at the ZIP level. The "mean" reports the 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	(9.0)	29.4	(9.9)
Non-Hispanic Black	0.19	(0.39)	0.21	(0.41)
Hispanic	0.18	(0.39)	0.21	(0.41)
Non-Hispanic white	0.46	(0.50)	0.37	(0.48)
Enrolled in managed care plan	0.44	(0.50)	0.55	(0.50)
Residence region				
Midwest	0.23	(0.42)	0.21	(0.41)
Northeast	0.10	(0.31)	0.17	(0.38)
South	0.29	(0.45)	0.32	(0.47)
West	0.37	(0.48)	0.29	(0.46)
Contraceptive use				
OC	0.13	(0.33)	0.12	(0.32)
LARC	0.057	(0.23)	0.045	(0.21)
LARC retained for at least one year	0.047	(0.21)	0.038	(0.19)
LARC removal	0.024	(0.15)	0.018	(0.13)
Number of Bene.	449,897		22,094,922	
Number of Bene-Years	1,578,876		56,551,190	

Note: This table presents the means and standard deviations (in parentheses) of selected characteristics and contraceptive outcomes of beneficiaries in our analysis sample for the movers design. Specifically, the sample is a beneficiary–year panel dataset consisting of female Medicaid enrollees ages 15 to 49, who are observed in the high-quality state–years (see Appendix Table E1), have valid and non-missing birth dates, ZIP codes, and managed care or fee-for-service enrollment information, and for whom residence state is the same as the enrollment state. Columns (1) and (2) report movers who moved between CZs exactly once. Columns (3) and (4) report stayers, who are beneficiaries who lived in the same CZ during the sample period. For movers, the means are over all beneficiary–years, including the year of the move.

TABLE 2: Mean Difference Decomposition of LARC and OC Rates

	Above - Below Median	Top - Bottom 25%	Top - Bottom 10%	WI-AR
Panel A: LARC				
Overall	0.037	0.060	0.085	0.066
Due to patients	0.015 (41.5%)	0.024 (39.7%)	0.035 (41.3%)	0.027 (40.9%)
Due to place	0.021 (58.5%)	0.036 (60.3%)	0.050 (58.7%)	0.039 (59.1%)
Panel B: OC				
Overall	0.065	0.107	0.150	0.130
Due to patients	0.041 (62.3%)	0.063 (58.2%)	0.084 (56.0%)	0.099 (76.4%)
Due to place	0.025 (37.7%)	0.045 (41.8%)	0.066 (44.0%)	0.031 (23.6%)

Note: Panel A reports the mean difference decomposition results with the LARC insertion indicator as the outcome, and Panel B reports the results for the OC claim indicator as the outcome. For each outcome, the table reports the estimates from equation (1). In columns (1)–(3), the two groups of CZs are defined by the rate of the outcome of the panel: the LARC rate in Panel A and the OC rate in Panel B. Wisconsin (Arkansas) is the state with the highest (lowest) rate of any contraceptive use (LARC, OC, or another method), and column (4) uses the same CZs in both panels. 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 the 2000 delineation of commuting zones (CZs), with ZIP codes mapped to CZs using the USDA crosswalk.[36] 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. Hospital Mortality Scores are from Chetty et al. (2016b). Provider characteristics are from the National Plan and Provider Enumeration System (NPPES) Registry[37] and the Medicare Provider National Downloadable File.[38] Hospitals per capita are from the American Hospital Association Annual Survey, 2015–2019. Pharmacies per capita count the NPPES providers with a pharmacy primary taxonomy, by the ZIP code of the mailing address, in the September 2024 NPPES file. Title X clinics are from the HHS Office of Population Affairs Title X directory of September 2017. The managed care share comes from the TAF data: it is the share of sample beneficiary–years in the CZ with full-year enrollment in a comprehensive Medicaid managed care plan. OB/Gyn years of experience are 2019 minus the graduation year in the Medicare Provider National Downloadable File. Per-capita measures use the 2000 county population.

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

[37] Accessed from https://download.cms.gov/nppes/NPI_Files.html

[38] Accessed from https://data.cms.gov/provider-data/dataset/mj5m-pzi6

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 Arizona, Georgia, and North Carolina) do not report the location of their providers, and 42.5 percent of the mover beneficiary-years are in these state-years. Therefore, we validate the moves only in the sub-sample with high-quality claim locations, and we do not use this validation to restrict the analysis sample.

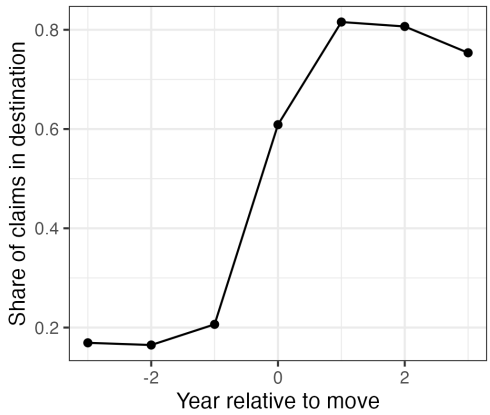
This algorithm closely follows Finkelstein et al. (2016), except that our criterion is at the state–year level, whereas theirs is at the individual level. For each person–year among the movers, we define their destination claim share as the ratio of claims (including all outpatient, inpatient, and prescription claims) made in their destination CZ to claims made in either their destination or origin CZ (the provider location is the practice ZIP code in the TAF annual provider file). 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 percentage points.[39] Appendix Figure A1 shows that in this sample, there is a sharp increase in the destination claim share in the move year.

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.

[39] 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%.

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).

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:

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

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 structural model implies a negative $\theta_{r(i,t)k}$, but the regression does not restrict its sign. 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:

$$\text{Var}(\overline{y}_j) = \text{Var}(\hat{\gamma}_j) + \text{Var}(\tilde{y}_j) + 2\text{Cov}(\gamma_j, \tilde{y}_j) \tag{7}$$

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:

$$S_{\text{patient}} = 1 - \frac{\text{Var}(\hat{\gamma}_j)}{\text{Var}(\overline{y}_j)} \tag{8}$$

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:

$$S_{\text{place}} = 1 - \frac{\text{Var}(\tilde{y}_j)}{\text{Var}(\overline{y}_j)} \tag{9}$$

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 follow Finkelstein et al. (2016). We assign the movers within each origin-destination pair randomly to two halves, estimate equation (1) separately in each half, and estimate each variance as the covariance between the estimates from the two halves. This split-sample covariance removes the estimation error of each half. We repeat the split 100 times and average each variance across the splits in which its split-sample covariance is positive.

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.[40]

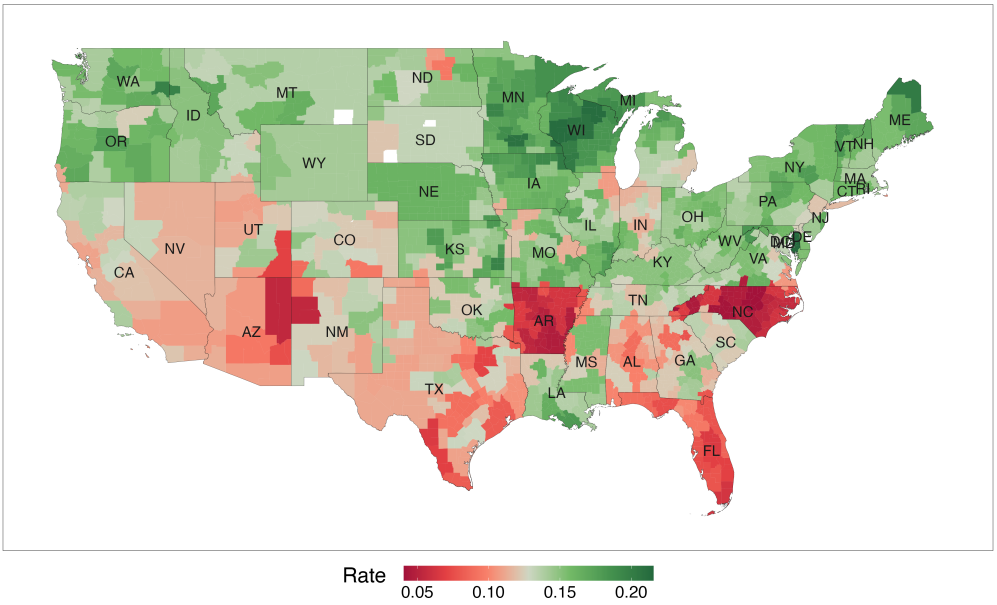
To account for carve-out, we drop beneficiaries in always-carve-out state-models (FFS in South Dakota and New Hampshire; MCO in Louisiana, Nebraska, New Hampshire, North Dakota, and Washington), and keep the remainder in the analysis.

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

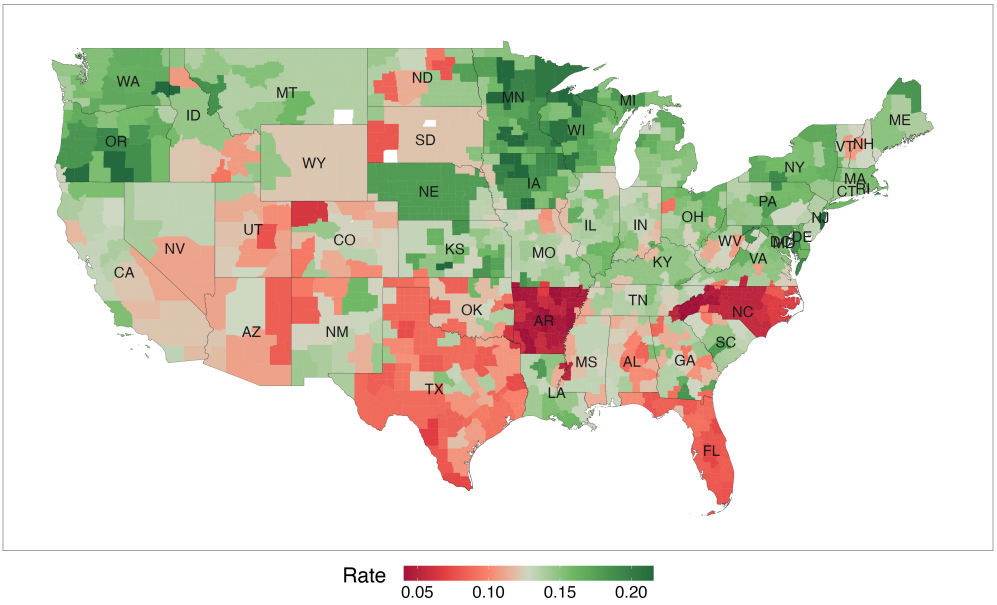
E Additional Results

FIGURE E1: Geographic Distribution of Oral Contraceptive Rates

(A) Raw Rates



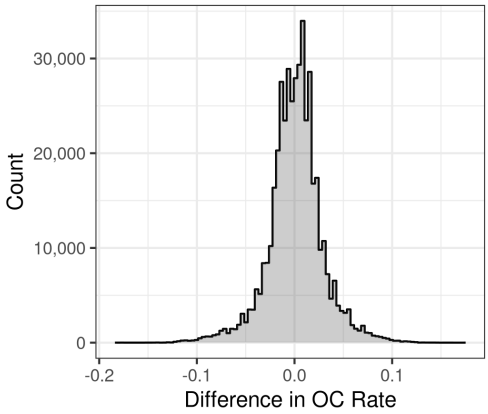
(B) Risk-adjusted Rates



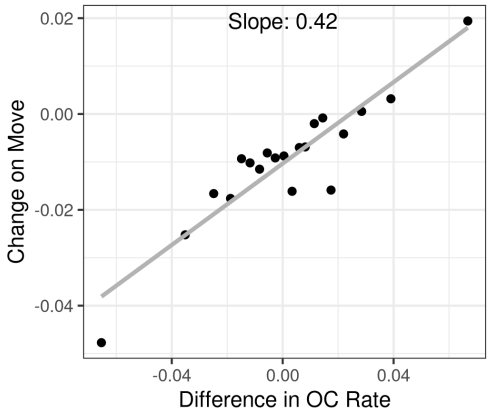
Note: This map shows average OC use rates across commuting zones (CZs). The sample includes all movers and stayers (see Table 1 for more details). We present raw (unadjusted) averages in Panel A. In Panel B, we regress the OC indicator on age-group and race/ethnicity fixed effects using only data from the movers (and excluding the year of the move), and then average the residuals in each CZ and re-center them to the sample mean.

FIGURE E2: Pre- and Post-Move Oral Contraceptive Use

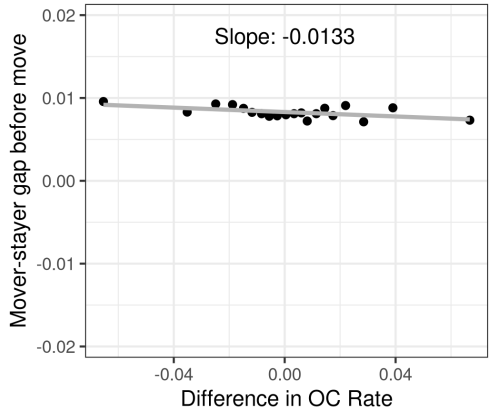
(A) Destination Minus Origin Oral Contraceptive Rate



(B) Oral Contraceptive Rate Change After Move



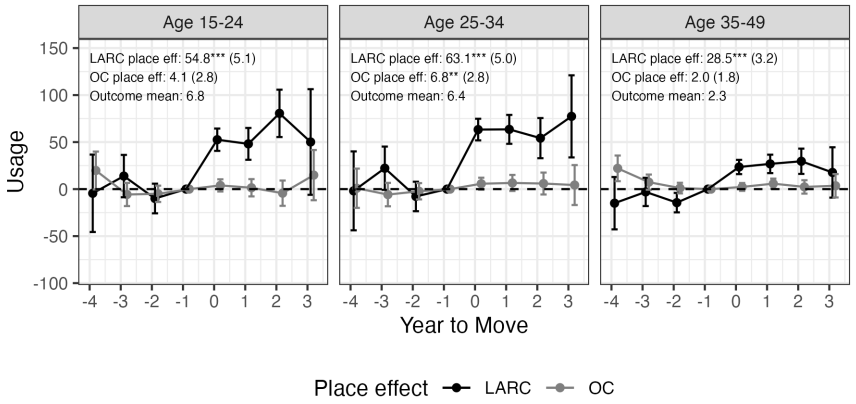
(C) Oral Contraceptive Rate Gap Movers vs. Stayers



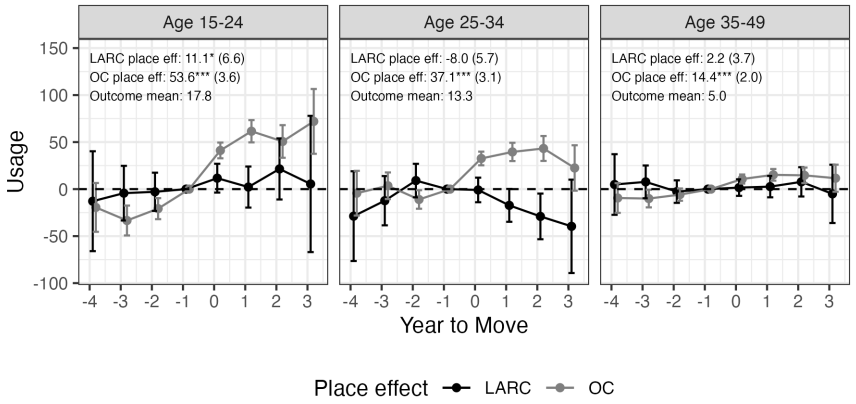
Note: Panel A shows the distribution across movers of the difference in OC rates between their origin and destination CZs (the gap). Panel B shows the relationship between the destination-origin gap and the change in OC use rate following the move. For each mover, we calculate the change in OC rate from before to after the move. The points report the mean change in OC rate within each ventile of the destination–origin gap. The line is obtained from a linear regression of the change in OC rate after the move on the gap. Panel C shows the relationship between the destination-origin 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 report the mean of this difference within each ventile of the destination–origin gap. The line is obtained from a linear regression of this difference on the gap. The CZ OC rates are raw rates, computed from the stayers in all sample years and the movers in all years except the year of the move.

FIGURE E3: Heterogeneity of Place Effects by Age and Race/Ethnicity

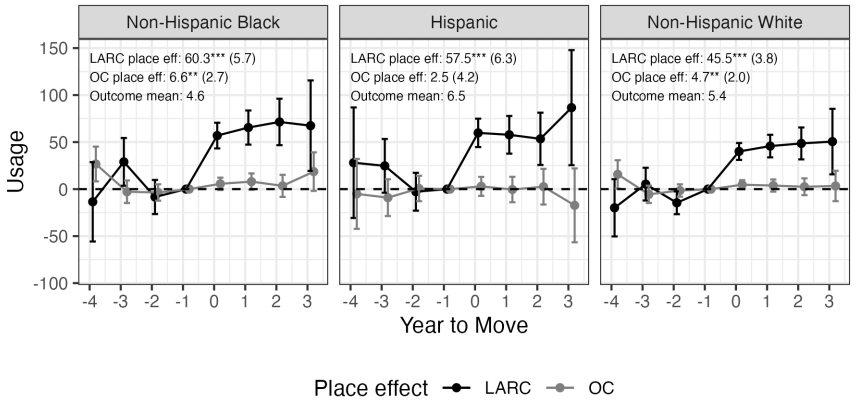
(A) Age group: LARCs



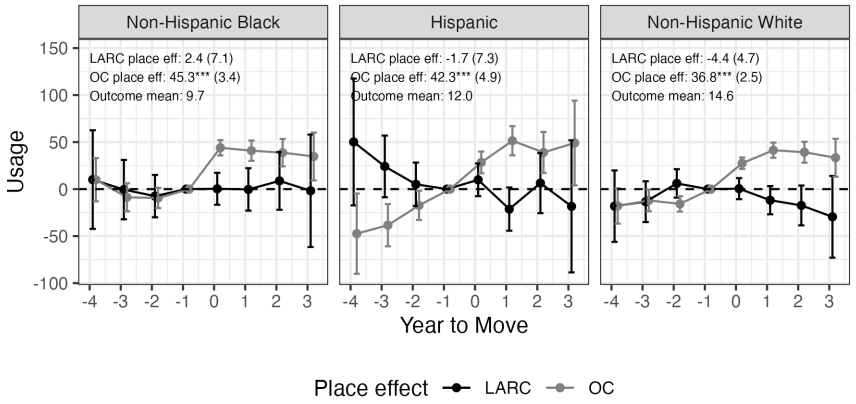
(B) Age group: Oral Contraceptives



(C) Race/Ethnicity: LARCs

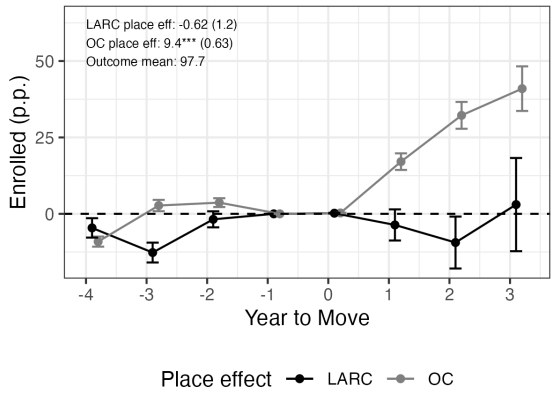


(D) Race/Ethnicity: Oral Contraceptives



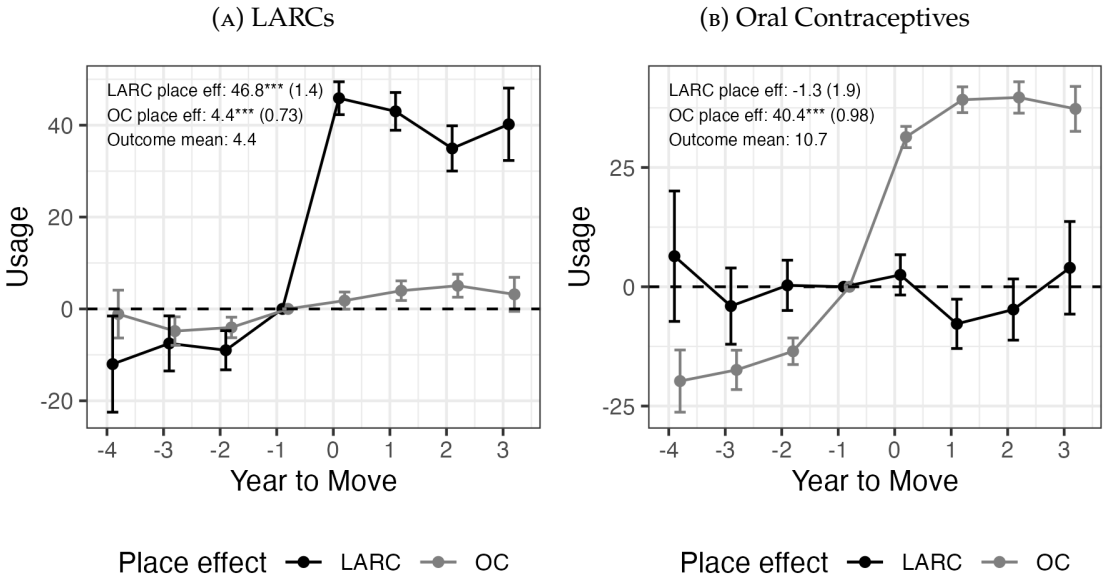
Note: These figures present event-study results from estimating model (5) for sub-groups defined by beneficiary age and race/ethnicity. Panels A and C report sub-group results for LARCs, while Panels B and D report sub-group results for OCs. The error bars report 95% confidence intervals based on standard errors clustered at the individual level. The "mean" reports average outcome among the movers in the year before the move for each sub-group.

FIGURE E4: Place Effects on Medicaid Enrollment



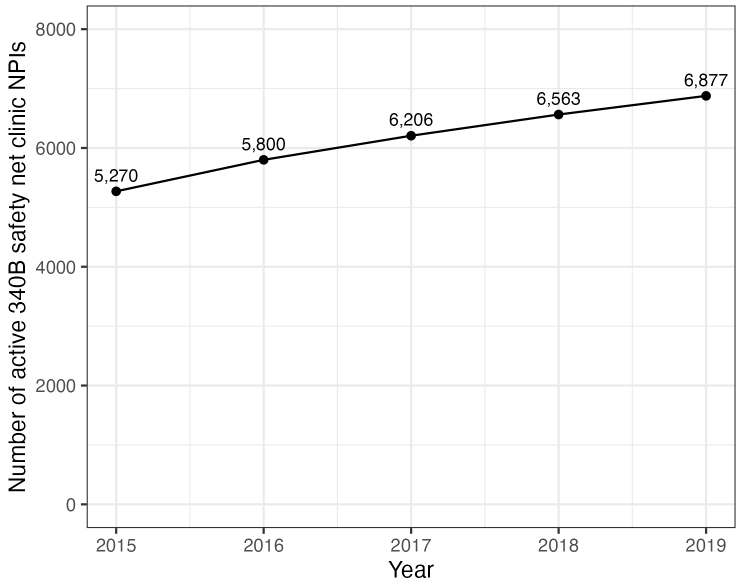
Note: This figure presents event-study results from estimating model (5) using the sample of movers without any restriction on enrollment either before or after the move and using an indicator for any enrollment in the year as the outcome. The error bars report 95% confidence intervals based on standard errors clustered at the individual level. The "mean" reports the average outcome among the movers two years before the move, because all movers are enrolled in the year before the move by construction.

FIGURE E5: Place Effects without Conditioning on Enrollment



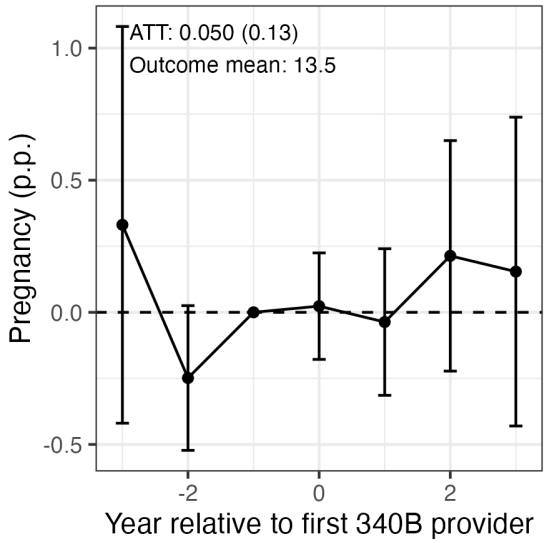
Note: This figure repeats Panels A and B of Figure 4 on the sample of movers without any restriction on enrollment either before or after the move. Each outcome is coded as a zero for a beneficiary who is not enrolled in a given year. The error bars report 95% confidence intervals based on standard errors clustered at the individual level.

FIGURE E6: Number of Safety Net Clinics in the 340B Program over Time: 2015–2019



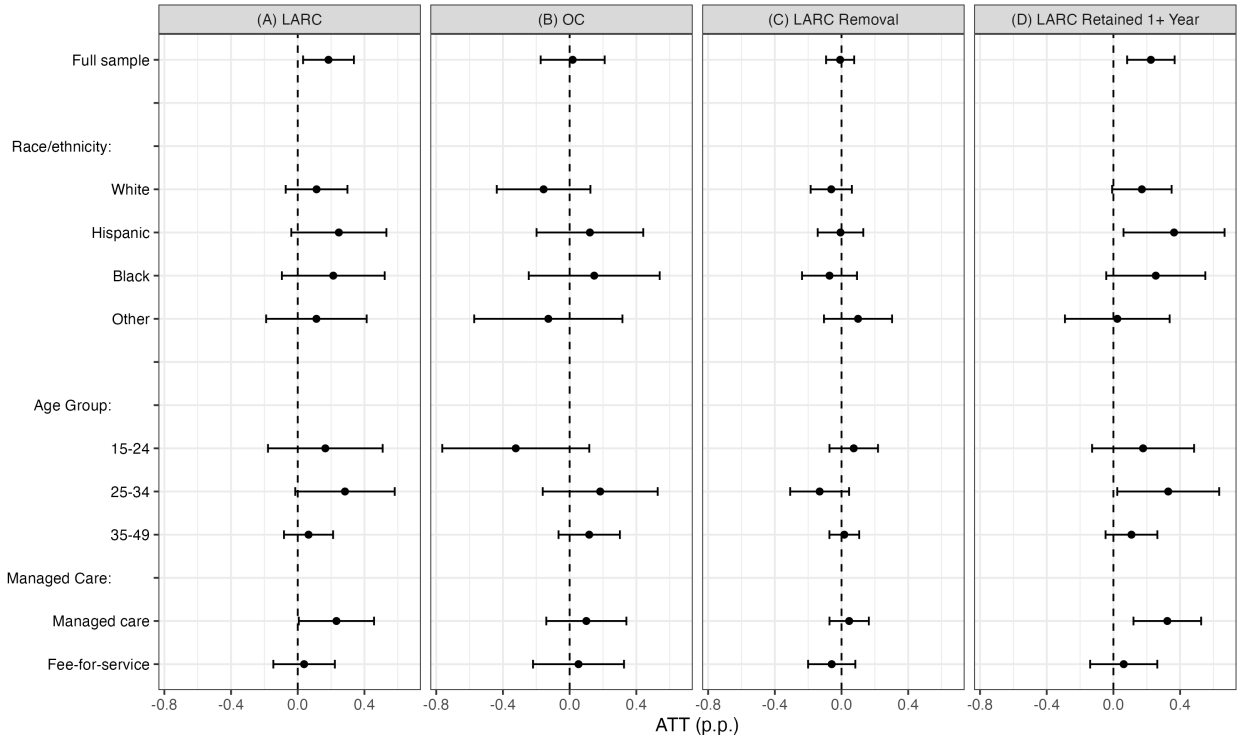
Note: This figure shows the number of safety-net clinics that participated in the 340B program in each year.

FIGURE E7: Effects of the 340B Program on Pregnancy



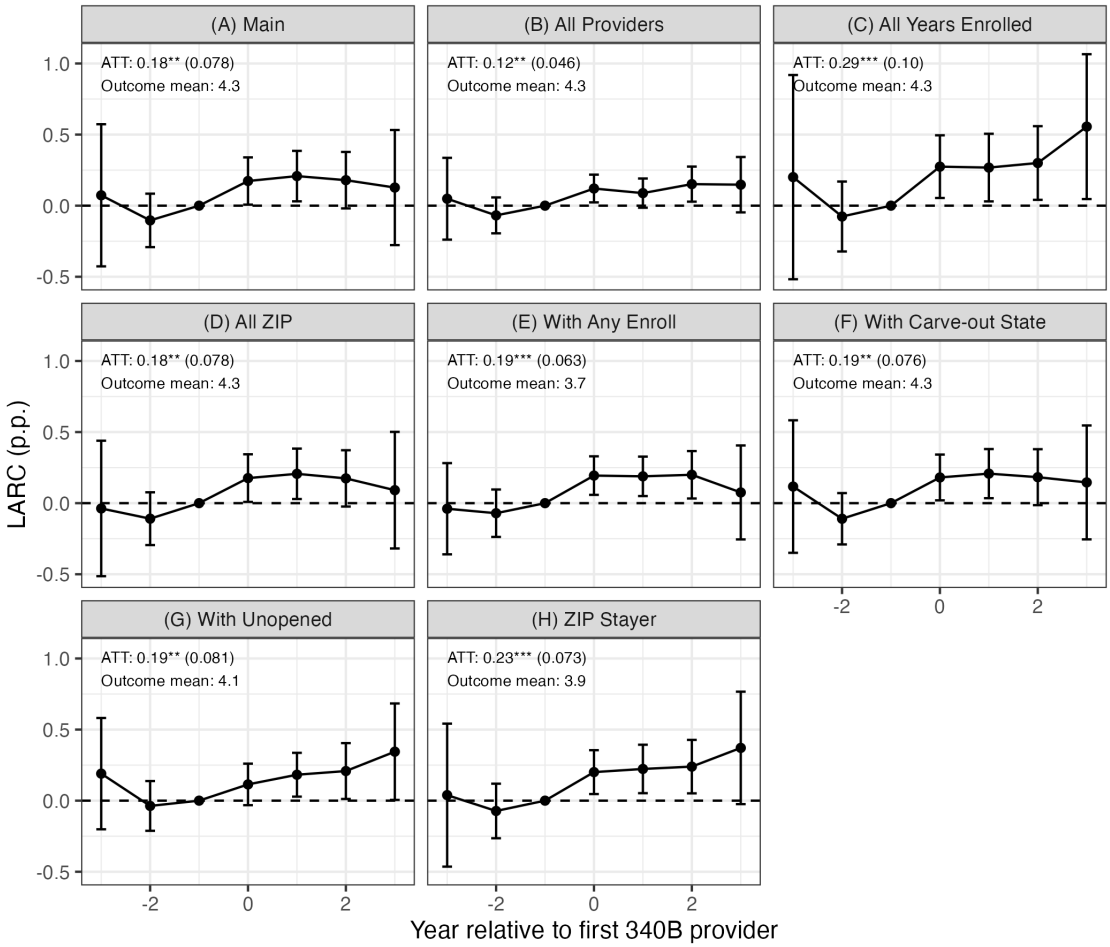
Note: This figure presents event-study results from estimating model (6), where treatment is defined by a ZIP code's first pre-existing safety-net clinic joining the 340B program (see Sections 4.2 and 4.3 for more details). We use the estimator from Sun and Abraham (2021), and include controls for individual and state–year fixed effects. We use an indicator for having a pregnancy-related claim as the outcome. The text reports the average treatment-on-the-treated effect and standard error. The error bars report 95% confidence intervals based on standard errors clustered at the ZIP level. The "mean" reports the average outcome among the treated group in the year before treatment.

FIGURE E8: Heterogeneous Effects of 340B Program



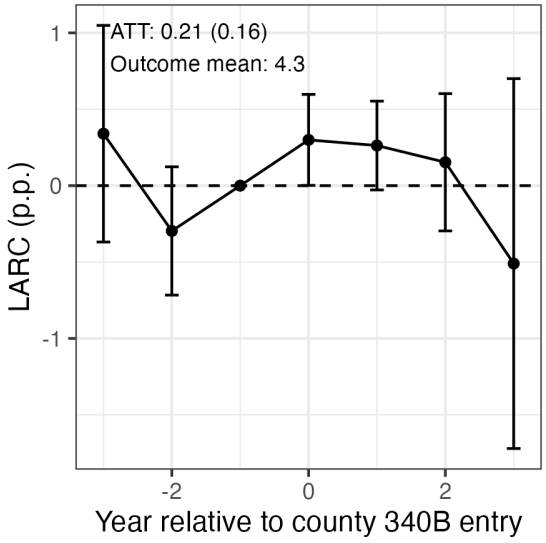
Note: This figure shows the coefficients and 95% confidence intervals from estimating the Sun and Abraham (2021) difference-in-difference models (Equation 6) on different sub-groups as specified on the left-hand side. We report the full-sample estimate in the top row for reference. We use the following binary indicators as outcomes: (A) LARC insertion claim, (B) Oral contraceptive claim, (C) LARC removal claim, and (D) LARC insertion not followed by any removal within the following 365 days.

FIGURE E9: Robustness of 340B Results



Note: Figure reports estimates from versions of model (6) across the following specifications: (A) "Main" reports our baseline result as in Figure 5; (B)"All Providers" includes all 340B providers (instead of just safety-net clinics) to define the treatment group; (C) "All Years Enrolled" limits the sample to beneficiaries who are enrolled in all years in which their state is included in the sample;(D) "All ZIP" uses all control ZIP codes without CEM matching; (E) "With Any Enroll" includes beneficiaries who are not continuously enrolled within the year; (F)"With Carve-out State" includes beneficiaries in state Medicaid models that carve out the 340B program; (G) "With Unopened" includes ZIP codes whose first 340B clinic had no Medicaid claim before joining 340B in the treatment group; (H) "ZIP Stayer" restricts the sample only to beneficiaries who are observed residing in one ZIP code over the analysis period.

FIGURE E10: 340B Results with County-Level Treatment

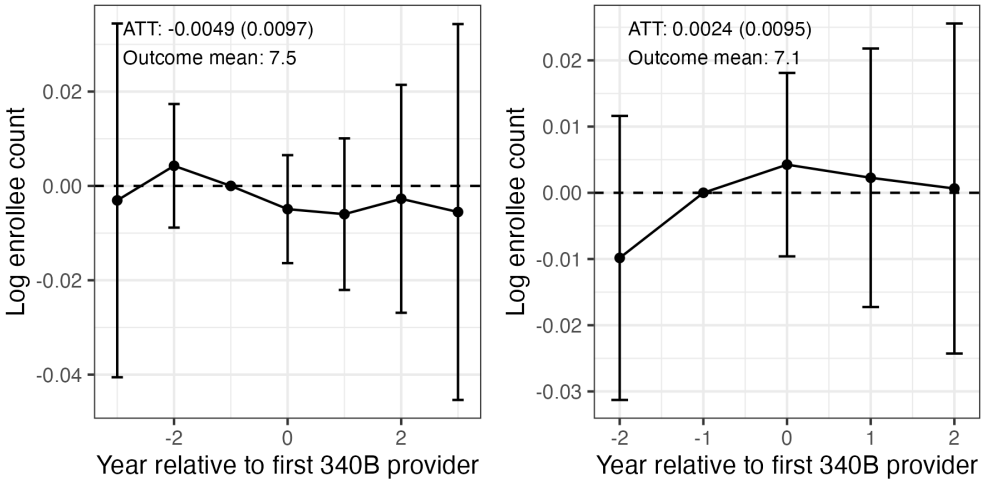


Note: This figure presents event-study results from estimating model (6), except we define treatment and control groups at the county rather than the ZIP code level. A county is treated in the year that its first 340B participant joins the program, if a safety-net clinic is among the participants of that year. Control counties are matched on terciles of the county beneficiary count within each state, and standard errors are clustered at the county level. The design has 58 treated and 464 control counties.

FIGURE E11: Effects of the 340B Program on the Number of Medicaid Enrollees

(A) All ZIP Codes

(B) Balanced Treated ZIP Codes



Note: This figure presents event-study results from estimating model (6) using data collapsed to the ZIP code–year level using as the outcome the natural log of the number of female age 15–49 Medicaid enrollees. Panel A uses all ZIP codes. Panel B keeps only the treated ZIP codes observed in every year from two years before to two years after treatment. The text reports the effect and standard error aggregated across the post periods. The error bars report 95% confidence intervals based on standard errors clustered at the ZIP level. The "mean" reports the average outcome among the treated group in the year before treatment.

TABLE E1: State-level Data Quality and MAX-to-TAF Transition Years

State	Low Data Quality	TAF Transition
Alabama		2014
Alaska		2013
Arizona		2014
Arkansas		2015
California		2015
Colorado	2016	2011
Connecticut		2015
Delaware	2016	2014
Florida		2013
Georgia		2015
Hawaii		2014
Idaho		2015
Illinois		2014
Indiana		2014
Iowa		2014
Kansas		2013
Kentucky	2015, 2017, 2018, 2019	2014
Louisiana		2015
Maine	2016	2014
Maryland		2014
Massachusetts		2014
Michigan		2015
Minnesota		2015
Mississippi	2016	2015
Missouri	2016	2015
Montana		2016
Nebraska	2017	2014
Nevada		2014
New Hampshire		2014
New Jersey	2016	2015
New Mexico		2014
New York	2016	2015
North Carolina		2013
North Dakota	2015	2014
Ohio		2014
Oklahoma	2019	2014
Oregon		2015
Pennsylvania		2014
Rhode Island	2016, 2019	2012
South Carolina		2014
South Dakota		2015
Tennessee		2015
Texas		2014
Utah		2015
Vermont		2015
Virginia		2014
Washington	2016	2015
West Virginia		2015
Wisconsin		2014
Wyoming	2016	2015

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 a rating of high concern, unusable, or unclassified on the DQ Atlas for total enrollment or for the number of enrollment spans, 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 sample does not include the District of Columbia.

TABLE E2: Sample Inclusion Criteria and Associated Sample Sizes

Criteria	Number of Bene-year (in millions)
All Bene.	434.1
High quality state-year	377.8
Reproductive women (age 15-49)	94.2 (100%)
Valid residence	85.7 (91%)
Enrollment equals residence	84.8 (90%)
Fully enrolled (11-14 months)	58.9 (63%)
Mover or stayer	58.1 (62%)

Note: This table reports the sample criteria and the associated sample sizes. The percentages are relative to the number of reproductive-age women (age 15–49). 'All Beneficiaries' refers to the count of all beneficiary–year observations in the full TAF Eligibility and Demographics base file. The 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.

TABLE E3: Code Set for Outcome Variables

Outcome	Source	Codes
Outpatient 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

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 3.1. 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 (2026b). The NDC codes are compressed for brevity.

TABLE E4: Proportion of Place Effects in Mean Difference of LARC and OC Rates between Above- and Below-Median CZs

Specification	LARC	OC	N
Main	0.585	0.377	1047371
Claim DQ	0.551	0.295	901619

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 column N reports the number of mover beneficiary-years, outside the year of the move, in the estimation sample of each row (the median across the 100 random splits). The N of the main row is smaller than the 1,578,876 mover beneficiary-years of Table 1 for two reasons. The estimation excludes the move year of each of the 449,897 movers. Each random split also keeps only the CZs that are connected in both halves, and it drops the beneficiaries and the CZs with one observation.

TABLE E5: Variance Decomposition Exercise

	LARC	OC
CZ variance of average:		
Utilization rate	0.00043	0.00146
CZ effects	0.00017	0.00029
Patient effects	0.00017	0.00069
Correlation:	0.266	0.539
Share variance reduced if:		
CZ effects made equal	0.59921	0.52596
Patient effects made equal	0.61088	0.80325

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 E6: 340B Analysis: Summary Statistics

	Treatment ZIPs		Control ZIPs	
	Mean	SD	Mean	SD
Demographics and enrollment				
Age	29.6	(9.9)	29.2	(10.0)
Non-Hispanic Black	0.19	(0.40)	0.19	(0.40)
Hispanic	0.25	(0.43)	0.21	(0.41)
Non-Hispanic white	0.36	(0.48)	0.41	(0.49)
Enrollment months	11.9	(0.27)	11.9	(0.27)
Residence region				
Midwest	0.18	(0.39)	0.25	(0.43)
Northeast	0.32	(0.47)	0.17	(0.37)
South	0.14	(0.34)	0.31	(0.46)
West	0.36	(0.48)	0.27	(0.44)
Contraceptive use				
OC	0.12	(0.33)	0.12	(0.32)
LARC	0.046	(0.21)	0.041	(0.20)
LARC retained for at least one year	0.038	(0.19)	0.035	(0.18)
LARC removal	0.018	(0.13)	0.016	(0.13)
Number of Bene.	444,198		6,152,436	
Number of Bene-Years	1,122,251		15,648,010	
Number of ZIPs	275		6,419	
Bene. per ZIP	1,615		958	

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. Columns (1) and (2) report beneficiaries who live in ZIPs with the first safety-net provider joining the 340B program between 2016 and 2018. Columns (3) and (4) report beneficiaries who live in ZIPs with the first provider joining the 340B program after 2019 or never. The sample is the matched sample after coarsened exact matching. Outcomes are averaged across the sample period. "Bene. per ZIP" is the mean number of sample beneficiaries per ZIP code, the count that the coarsened exact matching uses.