

Family matters: Parental health insurance and teen contraception

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Abstract

Does expanding adults' eligibility for public insurance increase adolescents' reproductive health care use? Using the universe of Medicaid and CHIP claims linked to a new index of contraceptive National Drug Codes for 2010-2020 and difference-in-differences models, we examine three margins of eligibility change: adult-expansion only, children-expansion only, and simultaneous expansion. Expanding adult eligibility increases both long-acting reversible and short-acting contraceptive rates among Medicaid-enrolled teens by 2.8 and 6.6 per 1,000 teens, respectively. Child-only expansions have no statistically significant effects on contraception rates, while simultaneous expansions generated changes in contraceptive rates of even higher magnitudes than adult-expansion only. These findings support that adult coverage shapes adolescents' access to reproductive health care, suggesting that the benefits of adult insurance extend to teens.

1 Introduction

Health in childhood is a key determinant of health, educational attainment, and earnings in adulthood, and a channel through which economic disadvantage is passed from one generation to the next (Currie, 2009; Currie and Almond, 2011). Because health investments early in life can have large returns to human capital (Currie and Gruber, 1996; Heckman, 2006; King, 2016), expanding children's access to health care has been a central goal of U.S. health policy. Beginning in 1984 with Medicaid and in 1997 with the Children's Health Insurance Program (CHIP),¹ states expanded eligibility so that children could obtain affordable coverage even when their parents' income exceeded the threshold for a standard Medicaid plan. These expansions increased *child* coverage and health care utilization, but led to limited changes in short-run health, with improvements coming primarily from reductions in child mortality (Currie and Gruber, 1996; Howell and Kenney, 2012; Hamersma et al., 2019). Their largest effects emerged

¹Originally this was referred to as State Children's Health Insurance Program, or SCHIP. CHIP/SCHIP is different from the Separate Child Health Insurance Program (Separate CHIP), which we discuss in more detail in the Background section.

later: children who gained coverage experienced better health and labor market outcomes as adults (Currie et al., 2008; Cohodes et al., 2016; Wherry and Meyer, 2016; Brown et al., 2020).

One drawback of child-targeted health insurance expansions is that these expansions can increase insurance discordance among families (Hamersma et al., 2026). How important is it that all family members have the same health insurance? Recent work has shown that spillover (welcome mat) effects of parental coverage increase child insurance enrollment and also increase the likelihood that parents and children share the same insurance (Hudson and Moriya, 2020; Lombardi et al., 2022; Hamersma et al., 2026).

Evidence on whether the spillover effect of expanded parental insurance coverage translates into increased child use of health care is more limited. Cross-sectional work provides suggestive evidence that even when a child is insured, if a parent does not share the same insurance type, the child is less likely to have a regular source of care or seek preventive care (King, 2016). Causal analyses support these findings, as they show that when parents gain Medicaid coverage, children are more likely to use preventative care (Venkataramani et al., 2017), increase use of dental care (Lipton, 2021), and experience decreases in emergency room visits and hospital utilization (Marcus and Zhang, 2026). However, these studies focus on health services that parents typically initiate for young children. In adolescence, decisions about care are increasingly shared between parents and teens: some services are sought by parents on their child’s behalf, while others might be sought by teens themselves. As a result, evidence from younger children may not extend to teens, and whether parental coverage shapes care remains an open question.

Adolescence is a period in which both health care needs and the way care decisions are made change substantially. As children age, they may take a more active role in their own care, while parents continue playing an active role in access through insurance provision, scheduling visits, and covering costs. At the same time, adolescence brings new health needs, and among the most consequential is reproductive health care: many teens become sexually active during these years or require hormonal contraception for health conditions arising during adolescence, and most teen pregnancies are unintended (Finer and Zolna, 2016). Reproductive health care is therefore both a central health need during adolescence and a setting in which the roles of parents and adolescents are especially intertwined. Parents may bring their children to a provider or raise contraception needs during a routine visit, while teens may seek care on their own and place a high value on confidentiality for sensitive services (Reddy et al., 2002). Parental coverage could increase adolescent contraceptive use if parents who gain coverage become more connected to providers and more likely to seek care for their children, or if teens find care easier to navigate and afford. Alternatively, parental coverage could have little effect, or even discourage use, if shared coverage raises teens’ concerns that parents will learn about visits they would rather keep private. Because these channels may operate in opposite directions, the net effect of parental coverage on adolescent reproductive health care use is theoretically ambiguous and must be determined empirically.

In this paper, we ask whether parental health insurance eligibility increases adolescent reproductive health care utilization. We focus on teen women living in low-income households because they are at high risk of unintended pregnancy and birth (Dehlendorf et al., 2010; Finer and Zolna, 2016), a gradient that is also evident among teens (Kearney and Levine, 2012), and because they have historically experienced high unmet need for reproductive health services (Dehlendorf et al., 2010; Frost et al., 2019). We test whether adolescent contraceptive utilization increases once parents become eligible for public health insurance.

We rely on data from the universe of Medicaid claims,² which is combined with our newly created comprehensive index for all contraceptive National Drug Codes during the period 2010 to 2019. Using our novel dataset, we estimate the effects of expansion in parental insurance eligibility on the contraceptive provision by age group and by type of contraceptive.

We examine three margins of adjustment in child and adult Medicaid eligibility. These include: 1) the Medicaid adult expansion starting in 2014, among states whose child Medicaid eligibility already exceeded the ACA’s floor and, therefore, does not directly affect teen eligibility, 2) the January 2014 ACA’s child eligibility floor or “stairstep” increase, which directly changes teens’ Medicaid eligibility, and 3) the simultaneous adult and child expansion in January 2014, in states where neither had previously occurred.

We exploit plausibly exogenous variation in Medicaid eligibility induced by these three margins of adjustment on the number of contraceptives per 1,000 teens among Medicaid-enrolled teens in difference-in-differences settings. The first margin of adjustment relies on staggered treatment, while the remaining two experience a single treatment time in 2014. To examine the first margin of adjustment, we rely on Callaway and Sant’Anna (2021) estimator by comparing Medicaid adult-expansion states with states that did not implement adult and child Medicaid expansions (i.e., never treated) and aggregate these comparisons into overall and event-time effects, avoiding “forbidden” comparisons with already-treated units as controls (Borusyak et al., 2024). For the child and simultaneous margins, we rely on two-way fixed effects models comparing treated states with states treated by neither policy change, as in margin 1.

Our findings indicate that the overall contraception rate among Medicaid-enrolled teens was not statistically different between states with Medicaid adult expansion vs. states without adult expansion and stairstep leveling. However, long-acting reversible contraception (LARC) rates increased by about 2.8 additional LARCs per 1,000 teens. Similarly, short-acting contraception increased by 6.6 additional contraceptives per 1,000 teens, both statistically significant at conventional levels.

In contrast, child eligibility leveling does not show statistically significant effects at conventional levels for any contraceptive rate. Nonetheless, for short-acting contraceptives, the point estimate is similar in magnitude to the one for adult expansion, preventing us from ruling out a potential effect of child

²These data include the combination of Medicaid and CHIPS claims.

eligibility leveling on short-acting contraceptive rates.

The adult and child simultaneous expansions caused statistically significant and larger effects in magnitude in the overall contraceptive rate and both LARC and short-acting contraceptive rates of 25.6, 3.1, and 19.6 additional contraceptives per 1,000 teens, respectively. These findings suggest that adult eligibility matters for teens' contraceptive take-up. Furthermore, when both adult and child eligibility is expanded, these expansions complement each other and cause even larger effects on teen contraceptive rates.

We validate our findings with a number of robustness checks, including variations in the sample of treated and control states, measurement of the outcome variable, and alternative specifications. Our findings remain robust to these analyses. We also examine the validity of our findings to the presence of family planning waivers at the time of the insurance expansions. We observe that adult Medicaid expansion in the absence of these waivers yields larger treatment effects in comparison to states with pre-existing waivers.

We examine the dynamic treatment effects of each margin of adjustment by relying on event study analyses, which confirm that adult expansion and simultaneous adult and child expansion changed teen contraceptive rates. In both cases, we find no evidence of differential trends before policy implementation, supporting the validity of our underlying assumption, parallel trends. In contrast, for child-only expansion, we are unable to rule out pre-existing differential trends between expansion and control states.

Our work contributes to several areas of the literature. First, we add to the literature working to understand the spillover (welcome mat) effects of parental insurance eligibility on child health insurance enrollment and child health care utilization. Second, we contribute to the literature on health disparities as we study a low-income population that has historically high unmet health care need. Last, we contribute to the broad literature on the effects of the ACA on children and, more specifically, teenagers. We elaborate on this in section 2.

The remainder of the paper is structured as follows: Section 2 describes the differences in insurance coverage across family members, Medicaid policy, and teenage contraceptive use. Section 3 describes the sources of information we rely on for our analyses. Sections 4 and 5 discuss our identification and the empirical strategy we employ. Section 6 presents our findings. Finally, section 7 concludes.

2 Background

2.1 Mixed Insurance Coverage Within the Family

Within the U.S. Health Care system, it is common, although not the norm, for individuals in the same family to be insured by different entities. Mixed insurance coverage can occur if everyone is insured but with a mix of public and private carriers, or if children have insurance but parents do not or vice versa. Prior to the ACA, between 14% and 16% of families were classified as mixed

insurance, and most of these cases arose from one of the following situations: a parent had private insurance through a job but could not afford private insurance for the children; a parent did not have insurance, but the children had insurance through CHIP; or, one family member had disability-related public insurance, and the other members had a different forms of (or no) insurance at all (Hanson, 2001; King, 2016). The ACA both expanded the possibility of family members having either the same *private coverage* through the robust private marketplaces, or the same *public coverage* through the higher income thresholds that increased the number of families eligible for Medicaid insurance. Figure 1 shows that children living in the poorest families (with income below 200% of the federal poverty line) experienced decreases in insurance discordance after 2013, particularly in Medicaid-expansion states.^{3,4} Furthermore, Figure 2 shows for this same period, children were less likely to live in a home with an uninsured parent and that this happens due to increases in public as well as private coverage.⁵ In our analysis, we focus on Medicaid-covered individuals. We are specifically interested in whether a parent gaining Medicaid insurance changes adolescent use of medical care, even if the teen is already eligible prior to the parent eligibility.

2.2 Medicaid

The ACA implemented sweeping changes to U.S. healthcare policy beginning in 2010. Among the most well-known changes was the expansion of the Medicaid eligibility threshold to cover all adults who had incomes under 133% of the federal poverty line, including adults without children who were previously ineligible. Although the ACA Medicaid expansion was slated to occur in 2013, a 2012 U.S. Supreme Court ruling determined that states had the right to choose whether or not to expand Medicaid as originally legislated in the ACA (Rosenbaum and Westmoreland, 2012). This led to a staggered adoption of the ACA Medicaid expansion, with 26 states expanding in 2013 and 38 by 2020, which is one source of variation we utilize in our identification strategy.

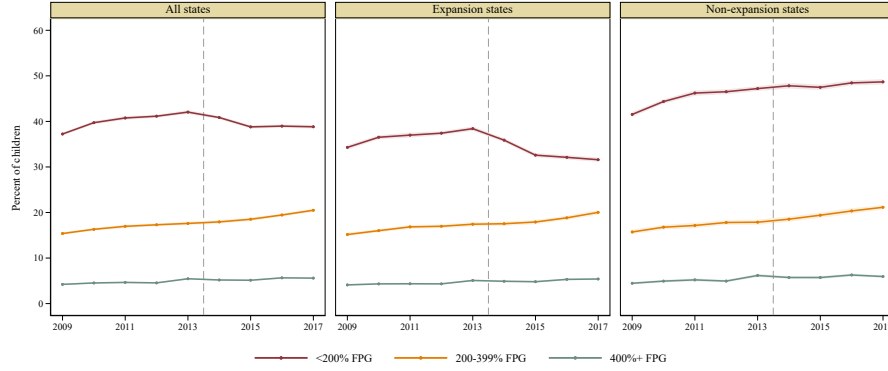
A less broadly known change resulting from ACA legislation affected child eligibility for subsidized health insurance. Prior to the ACA, children could have been eligible as a dependent of a parent who was a Medicaid recipient, or eligible even if their parent was not eligible via the Separate Child Health Insurance Program (Separate CHIP), which often had higher income thresholds than parent Medicaid. Additionally, at this time in some states, Separate CHIP had different income thresholds for younger (ages 0-5) versus older (ages 6-18) children, which meant that sometimes within a family only the youngest children were eligible for Separate CHIP. The ACA raised the federally mandated income threshold for Separate CHIP eligibility from 100% to 133% of the federal

³The trends presented here complement the findings of Hamersma et al. (2026), which uses CPS data to construct family trends.

⁴The figures we produce with ACS data use the federal poverty guideline, which we refer to as the federal poverty line (in keeping with typical use in the Medicaid policy literature).

⁵Figure A1 demonstrates this pattern over type of insurance mix.

Figure 1: Discordant within-family health insurance coverage, by family income and state Medicaid expansion status.



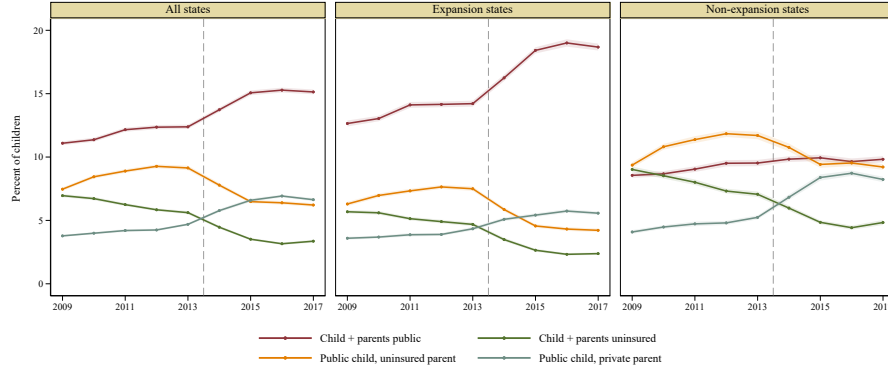
Notes: Children aged 6–18 living with a co-resident parent aged 19–64, American Community Survey 1-year files, 2009–2017. A child is counted as discordant when the child’s coverage type differs from that of the co-resident parents, where each person’s coverage is classified as private, public or uninsured using the State Health Access Data Assistance Center (SHADAC) primary-source hierarchy. In two-parent families the child is discordant unless both parents share the child’s type. Income is health insurance unit income as a percent of the federal poverty line, with the health insurance unit defined following the SHADAC construction rather than the Census family. The vertical line marks the January 2014 ACA coverage provisions. Expansion states: the 32 with the ACA Medicaid expansion in effect before 2018 are AK, AZ, AR, CA, CO, CT, DE, DC, HI, IL, IN, IA, KY, LA, MD, MA, MI, MN, MT, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA and WV. Non-expansion states: the 19 expanding in 2018 or later or not at all are AL, FL, GA, ID, KS, ME, MS, MO, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI and WY.

poverty line for children between the ages of 6 and 18. This was done to align eligibility for all children in the family with the legislated parent eligibility.⁶ This is a nuanced but important change since without also changing the income limit for child eligibility, children of different ages in the same family potentially faced different eligibility thresholds in addition to the potential different parental eligibility thresholds. Furthermore, Separate CHIP plans often offer less generous insurance relative to standard Medicaid since they may entail premiums, cost-sharing, benefit restrictions, and lockout periods (MACPAC, 2021).

The harmonization of child thresholds to 133% was mandated to go into effect in 2014. Critically, it was not reversed by the 2012 U.S. Supreme Court Ruling that relaxed the requirement that all states expand standard Medicaid (Prater and Alker, 2013). Consequently, as a result of the ACA and the decision by some states not to expand their Medicaid program according to the original ACA legislation, there could have been children newly eligible at the ACA-legislated 133% (ACA-CHIP expansion) with parents whose Medicaid eligibility

⁶Practically, the child threshold was 138% since they were given an additional 5% disregard over the 133%. This is a separate change relative to any changes in health insurance coverage accessed through changes in parental coverage directly.

Figure 2: Selected within-family health insurance coverage patterns, by state Medicaid expansion status.



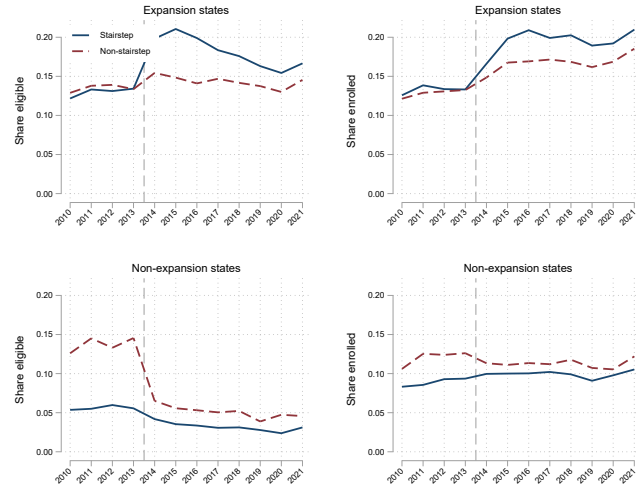
Notes: Children aged 6–18 living with a co-resident parent aged 19–64, American Community Survey 1-year files, 2009–2017. Each panel plots the percent of children in the named parent–child coverage combination. The vertical line marks the January 2014 ACA coverage provisions. Patterns requiring “all parents” to share a coverage type place two-parent families whose parents differ from one another into a residual mixed category, which is not shown. The four patterns displayed therefore do not sum to 100 percent: the remaining patterns are omitted from the figure but retained in the denominator. Expansion states: the 32 with the ACA Medicaid expansion in effect before 2018 are AK, AZ, AR, CA, CO, CT, DE, DC, HI, IL, IN, IA, KY, LA, MD, MA, MI, MN, MT, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA and WV. Non-expansion states: the 19 expanding in 2018 or later or not at all are AL, FL, GA, ID, KS, ME, MS, MO, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI and WY.

did not change. We refer to states that were required to increase their minimum threshold to 133% as stairstep states. Separately, we classify states as expansion (expanded Medicaid according to ACA legislation) versus non-expansion states as of 2019, the end of our study period.

Figure 3 shows parental Medicaid eligibility (left panel) and enrollment (right panel) in expansion (top panel) versus non-expansion (bottom panel) states for stairstep versus non-stairstep states. This visual shows that there is a relatively large increase in eligibility and to a lesser extent enrollment by parents in expansion states and if anything a decrease in eligibility and enrollment by parents in non-expansion states that are not stairstep states.

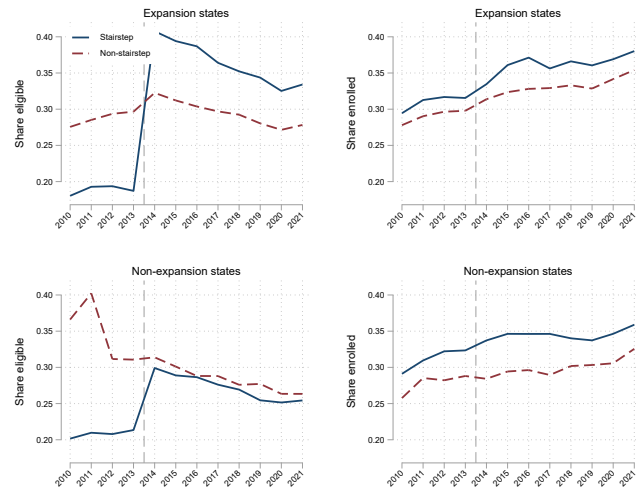
Figure 4 provides similar visual evidence for teens. The increases in eligibility are more dramatic for stairstep states, especially for expansion states. However, the increase in enrollment is again muted relative to the eligibility change.

Figure 3: Parental eligibility and enrollment by staircase and expansion states



Source: American Community Survey, 2010-2021.

Figure 4: Teen eligibility and enrollment by staircase and expansion states



Source: American Community Survey, 2010-2021.

Adolescents newly eligible for Medicaid would have had contraceptives covered as a result of ACA-induced coverage changes, either through their parent’s new Medicaid eligibility, or through ACA-expansion CHIP eligibility.

An additional consideration when evaluating contraceptive provision, our health care use outcome, is that even before the ACA Medicaid expansion, teens in some states also had access to Medicaid coverage for contraceptives through an additional pathway: Medicaid family planning waivers, which provided reproductive health care to low-income women and adolescents, often starting at age 15, and typically with maximum eligibility thresholds exceeding those of full Medicaid eligibility programs.

Table 1 characterizes how these three policies apply to each state, illustrating the policy variation our study relies on. There are two important takeaways from the policies we examine that are important when we consider our identification strategy. First, family planning waivers and child Medicaid eligibility represent pre-existing interventions, where teens were exposed to greater eligibility for Medicaid at baseline. Most such policies were established years before the beginning of our study period in 2010. In states with these existing policies, every teen below 133% of the FPL was already eligible for Medicaid coverage of contraceptives. Second, nearly all states that increased child eligibility thresholds and increased adult eligibility thresholds during the period for which we have data did so *at the same time*. Only two states implemented an adult expansion that followed the child expansion (PA and UT). We discuss the implications of this point for identification of treatment effects in Section 4.

2.3 Health Insurance Coverage and Contraceptive Use

Theoretically, obtaining health insurance should lead to greater usage of health care, including contraception.⁷ Even a small increase in enrollment and use of Medicaid insurance could be important for adolescents since they, and especially individuals who would qualify for Medicaid insurance, have high unmet reproductive health care needs. In fact, adolescents have the highest rates of unmet needs for contraception and unintended pregnancy (Rossen et al., 2023). They have also been historically less likely to receive person-centered contraceptive counseling—the recommendation or provision of a method that best aligns with their own preferences, values, and reproductive goals, supported by unbiased, complete provider information and free from financial, structural, or coercive barriers—and more vulnerable to physician reluctance to prescribe Intrauterine Devices (IUDs) based on misperceptions (Dehlendorf et al., 2014; Espey et al., 2019; Whitfield et al., 2025; Gomez et al., 2024).

When the ACA was enacted, nearly 50 percent of young women ages 15 to 24 were using some form of birth control (Bailey and Lindo, 2018). Prior research has shown the ACA expansion to be associated with a decrease in the rate of uninsured, and an increase in health care utilization, including an increase in

⁷Prior research has shown teens to be responsive to changes in insurance coverage of contraception. For example, family planning waivers enacted during the late 1990s and early 2000s have been shown to increase contraceptive use (Kearney and Levine, 2009).

Table 1: State Classification by Pre-ACA Child Eligibility, Medicaid Expansion Timing, and Family Planning Waiver Status

	Stairstep (Child Medicaid $\leq$ 100% FPL in 2013)	Non-Stairstep (Child Medicaid $>$ 100% FPL in 2013)
No expansion	<i>FPW</i> (5): AL, GA, MS, NC, TX <i>No FPW</i> (4): FL, KS, TN, WY	<i>FPW</i> (4): MO, OK ^c , SC, WI ^e <i>No FPW</i> (1): SD
Expanded January 2014	<i>FPW</i> (2): CA ^b , OR <i>No FPW</i> (5): AZ, DE ^a , ND, NV, WV	<i>FPW</i> (11): AR ^f , CT, IA, IL, MD, MI ^f , MN, NM, NY ^a , OH ^f , WA <i>No FPW</i> (8): CO ^d , DC ^a , HI, KY, MA ^a , NJ, RI, VT ^a
Expanded post-2014	<i>FPW</i> (1): PA (2015) <i>No FPW</i> (1): UT (2020)	<i>FPW</i> (5): IN, LA, MT, NH ^d , VA <i>No FPW</i> (4): AK, ID, ME, NE

Notes: States shown by postal abbreviation. “Stairstep” states had Medicaid child eligibility at or below 100% of the federal poverty level (FPL) in January 2013 and were required to raise coverage to the ACA’s 133% floor by January 2014. “Expansion” states adopted the ACA Medicaid expansion extending coverage to non-elderly adults below 138% FPL. “FPW” states operated a Section 1115 family planning waiver covering contraceptive services for low-income women as of January 2013 (measured at baseline); waiver start dates from CMS. Child eligibility thresholds from KFF (*Medicaid Income Eligibility Limits for Children Ages 6–18*); expansion dates from KFF State Health Facts. Stairstep states that expanded Medicaid post-2014 (PA, UT; expansion year in parentheses) had a child-channel-only window between January 2014 and their adult expansion date and contribute to identification of the direct child eligibility channel alongside the non-expansion stairstep states. ^aExcluded from all regression samples (early Medicaid expansion predating the ACA). ^bCalifornia: child eligibility transition date ambiguous due to phased Healthy Families → Medi-Cal transfer in 2013–2014; assigned January 2014 in main specification, sensitivity check drops CA. ^cOklahoma expanded July 2021, after the main sample period (2010–2020); never-treated control in all samples. ^dNH and CO completed the child eligibility transition in 2011 and 2013 respectively; excluded from the parental-channel robustness check. ^eWisconsin did not adopt the ACA expansion but raised BadgerCare eligibility for childless adults to 100% FPL in 2014; partially treated, and therefore excluded from the primary control pool (retained in a keep-WI robustness check). ^fFPW at baseline but waiver terminated within the sample period (AR 2013; MI and OH 2016); the FPW-arm heterogeneity results report these states separately.

contraceptive use by adult women (Abramowitz, 2018; Becker, 2018; Gruber and Sommers, 2019). Yet, no work has documented whether ACA-induced parental coverage has affected teen contraceptive use. The first study to propose this channel shows that teen fertility responds to changes in parental insurance coverage, but it does not examine contraceptive use directly (Rosenberg, 2026).

It is an open question whether changes in adult coverage lead to changes in teen contraceptive use, especially if the adolescents may have already had access to publicly funded contraception either through SCHIP or through a family

planning waiver prior to the ACA. Our novel data set, which we constructed from administrative data, allows us to examine contraceptive use for adolescent Medicaid recipients.

3 Data

3.1 Medicaid data claims

The main data we use is administrative enrollment and claims records for the universe of Medicaid enrollees in the Medicaid Analytic eXtract (MAX) and Transformed Medicaid Statistical Information System (T-MSIS) data from the Centers for Medicare and Medicaid Services (CMS) from 2010 to 2020. The data contain individual-level insurance claims for all individuals enrolled in standard Medicaid and Medicaid Expansion CHIP (M-CHIP).⁸

We identify contraceptive claims across the prescription, inpatient, and outpatient files to construct a comprehensive measure of contraceptive utilization. This multi-file approach is necessitated by substantial cross-state variation in how Medicaid programs classify and reimburse contraceptives. While some states reimburse contraceptive drugs and devices as a pharmacy benefit, others bill the same products as a medical benefit through the inpatient or outpatient system (DiAntonio, 2015). Relying on any single file would systematically undercount contraceptive utilization in states where the dominant billing channel differs, introducing bias that varies across states and over time.

In the prescription files, contraceptive claims are identified using National Drug Codes (NDCs). NDCs are product-specific identifiers assigned to all commercially marketed drug products. NDCs allow for the precise identification of contraceptive drugs and devices dispensed through a pharmacy.

In the inpatient and other therapy files, contraceptive claims are identified using a combination of diagnosis and procedure codes. Claims dated before October 1, 2015, use ICD-9 diagnosis codes, while claims dated October 1, 2015, and later use ICD-10 codes (CMS, 2015). In addition, HCPCS and CPT-4 procedure codes are used throughout the study period to identify contraception billed as medical claims, mainly IUDs and implants. To avoid double-counting, claims appearing in multiple files for the same enrollee on the same service date are de-duplicated. After identifying the contraceptive claims, we construct state-age-group-year patient counts by contraceptive type. The two types analyzed are long-acting reversible contraceptives (LARCs) and short-acting hormonal contraceptives (SAHCs).⁹

⁸There are a handful of state-years missing from the data due to reporting issues. Kansas and Maine are missing in 2010, and Idaho is missing outpatient claims in 2011 (Mathematica Policy Research, 2014, 2017).

⁹TO COMPLETE: WHAT ARE THE SPECIFIC CONTRACEPTIVE METHODS INCLUDED IN EACH CATEGORY?

3.2 Contraceptive coding

To identify contraceptives by NDC throughout the study period, we construct a comprehensive list of NDC codes for prescription contraceptives from 2005 to 2020. A challenge of constructing such a comprehensive list is that available contraceptives have changed over time (due to approvals or withdrawals by the FDA, and termination or reformulation by manufacturers). Even for the same contraceptives, NDC codes change over time due to manufacturers changing ownership, changing formulation, or changing packaging.

As a first reference point, we begin with two reliable sources for NDC codes from recent years, going back to 2017: a list of NDCs for contraceptive pills and names of prescription contraceptives from Sumarsono et al. (2021) and annual lists by NDC, proprietary name, and non-proprietary name from the Department of Health and Human Services () back to 2017. We match the codes from these sources to annual historical snapshots of the National Drug Code Database (accessed using the Internet Archive’s Wayback Machine) over the years 2005-2020.

Next, we identify all current NDCs using RxClass based on the Anatomical Therapeutic Chemical Classification System. This classification excludes injections, copper IUDs, and diaphragms.

After deduplicating the resulting list of contraceptive NDCs produced from these sources, we then run each NDC code through RxNorm to identify all associated drug concept identifiers (RxCUI). Using these unique drug concept identifiers, we can search for all associated NDCs among historical NDCs stretching back to 2007.

Since this generates additional matches on both NDC and nonproprietary drug names, we run an additional round of matching with NDC databases back to 2005. Finally, we collect all matches and deduplicate at the 9-digit product level, storing active ingredients, product names, and type of contraceptive.

4 Identification

Our question of interest is how do ACA Medicaid expansion for adults and children affected contraceptive provision to teens within Medicaid? We first describe our estimands of interest in light of the policy variation we presented in section 2.2. We target three distinct average treatment-on-the-treated effects:

θ_1 (**adult margin**): the effect of adult Medicaid expansion in states whose child Medicaid eligibility already exceeded the ACA’s floor (“non-stairstep” states), so that the adult expansion arrived against a fixed child-eligibility environment;¹⁰

¹⁰Treated states: Alaska, Arkansas, Colorado, Connecticut, Hawaii, Idaho, Indiana, Iowa, Illinois, Kentucky, Louisiana, Maine, Michigan, Minnesota, Montana, Nebraska, New Hampshire, New Jersey, New Mexico, Ohio, Rhode Island, Washington, Virginia.

θ_2 (**child margin**): the effect of the ACA’s child eligibility floor (the “stairstep” increase from $\leq 100\%$ to 133% FPL) in states that never expanded adult eligibility during the sample period (2010 to 2010);¹¹

θ_3 (**joint expansion**): the effect of simultaneous adult and child expansion in January 2014, in states where neither had previously occurred¹²

The first margin entails staggered treatment, while the second and third both have a single treatment time in 2014. The three estimands differ not only in which policy varies but in how the sampled teens are exposed to it. The child eligibility floor changes the sampled teens’ own Medicaid eligibility directly: the age groups we study fall within Medicaid’s child eligibility categories (15 to 17 years old; the relevant Medicaid age category covers 6 to 18 years), so the stairstep states’ floor increase extended coverage to them. Adult expansion, by contrast, never changes a teen’s own eligibility; any effect must operate through indirect channels—the salience and outreach surrounding expansion (a “welcome mat” for already-eligible but unenrolled teens) or enrollment spillovers from newly eligible parents. Thus θ_1 isolates the indirect channels, θ_2 captures the direct eligibility change, and θ_3 combines the two.

Given the policy variation available—nearly all states that adopted both expansions did so simultaneously in January 2014—we cannot separately identify the marginal effect of the adult or the child expansion for states that adopted both: for those states the two treatments switch on at the same moment, so only their bundled effect is observed.¹³ Accordingly, θ_1 , θ_2 , and θ_3 are defined as policy-relevant bundle effects for three distinct populations of states, and differences among them (for example, θ_3 exceeding $\theta_1 + \theta_2$) are suggestive of complementarity but confound interaction effects with treatment-effect heterogeneity across populations.

Identifying assumptions

The key identifying assumptions are parallel counterfactual trends and no anticipation of treatment. For the adult margin, parallel trends requires that, absent expansion, treated cohorts’ outcomes would have evolved in parallel with those of the comparison states—non-stairstep states that never adopt the expansion during the sample period—and no anticipation requires that there is no treatment effect prior to treatment. Under these assumptions, each treatment effect is identified compared to the same pool of never-treated states.

Concretely, the shared control pool consists of the non-stairstep states that never adopted the adult expansion during the sample period—Missouri, Okla-

¹¹Treated states: Alabama, Florida, Georgia, Kansas, Mississippi, North Carolina, Tennessee, Texas, Wyoming.

¹²Treated states: Arizona, California, Oregon, North Dakota, Nevada, West Virginia.

¹³Pennsylvania and Utah are the exceptions: both raised child eligibility in 2014 but expanded adult eligibility later (2015 and 2020), creating a child-only window followed by joint treatment. With only two such states, no credible cluster-based inference is available. Therefore, we do not make use of this variation to attempt to separate individual from combined marginal effects.

homa, South Carolina, and South Dakota—together with Nebraska.¹⁴ The same five states serve as the comparison group for all three estimands. Wisconsin, which did not adopt the ACA expansion but raised its BadgerCare eligibility for childless adults to 100% FPL in January 2014, is excluded from the comparison pool as partially treated; we return to this choice in the robustness analysis. We further assume no interference across states: treatment is assigned at the state level, and one state’s expansion is assumed not to affect contraceptive provision in another.

These targeted treatment effects focus on the ACA-era Medicaid policies that affect teens. As previously discussed, pre-existing family planning waiver policies are present in numerous states, as is pre-existing coverage of children. The validity of our identification thus also rests on these pre-existing policies being stable over the sample period: their level effects are absorbed by state fixed effects, and we assume they induce no differential trends in counterfactual contraceptive outcomes. Three states—Arkansas, Michigan, and Ohio—are exceptions to within-window stability, terminating family planning waivers during the sample period; all three are treated states, and the heterogeneity analysis below reports them separately.

5 Estimation and Inference

5.1 Samples

All estimation uses the state-by-year panel of contraceptive utilization rates for 2010–2020 described above, separately for each of the two teen age groups. States that expanded Medicaid before the ACA (Delaware, the District of Columbia, Massachusetts, New York, and Vermont) are excluded from every sample. For the adult-margin sample (θ_1), the treated group includes 23 non-stairstep states adopting between 2014 and 2020. The child-margin sample (θ_2) includes the nine stairstep never-expanders as the treated group. The joint-expansion sample (θ_3) includes six states that simultaneously increase adult and child eligibility.

All three margins are estimated against the same comparison pool of five never-treated states. Four of them—Missouri, Oklahoma, South Carolina, and South Dakota—did not adopt the adult expansion during the sample period and already had child eligibility at or above 133% FPL, so they are untreated on both margins. The fifth is Nebraska, whose adoption in October 2020 falls outside the estimation window and which therefore contributes no treated periods. Holding the comparison pool identical across the three margins is deliberate: it means the three estimands are differenced against the same counterfactual, so that comparisons among them reflect the treated populations and not the comparison group. We report as a robustness check in section 5.5, the alternative for the adult margin, in which not-yet-treated cohorts also serve as comparisons before

¹⁴Nebraska adopted expansion in the fourth quarter of 2020; since it is untreated for three-quarters of the last year of our sample year we treat it as a never-treated state.

their own adoption. The resulting cluster counts are 28 states for θ_1 , 14 for θ_2 , and 11 for θ_3 . These small cluster counts—not sampling error within states—are our binding constraint on inference throughout, and, therefore, our estimator and inference choices below are made with them in mind.

5.2 Aggregation

To make θ_1 , θ_2 , and θ_3 comparable, we choose to construct the ATT by averaging over the six post-treatment periods estimated from the event-study specifications for each. This reconciles differences in estimation: θ_1 represents a treatment that is staggered, where we use Callaway and Sant’Anna (2021) estimator, which aggregates group-time treatment effects and is not directly comparable to estimation by OLS for θ_2 and θ_3 , which have only a single treatment time/group. We discuss weighting choices for each treatment estimate below.

5.3 Adult margin (θ_1): staggered adoption

Because adult expansion is staggered across 2014–2020 cohorts, we estimate the group-time effects of equation (4) using the Callaway and Sant’Anna (2021) estimator, implemented without covariates (so the doubly robust, outcome-regression, and inverse-probability-weighted versions coincide) and with the never-treated comparison group. The reported $\hat{\theta}_1$ aggregates the group-time estimates as follows:

$$\hat{\theta}_1 = \frac{1}{7} \sum_{e=0}^6 \sum_g \omega_{g,e} ATT(g, g+e), \quad (1)$$

This aggregation first constructs an average at each relative event time, where the group-time ATTs are weighted by the sample share they represent of treated cohorts observed at relative time e . Then, these relative event-time estimates are averaged over the seven post-treatment periods.¹⁵

Inference relies on Callaway and Sant’Anna’s clustered multiplier bootstrap with 4,999 draws, clustering at the state level. Event-study figures display simultaneous (uniform) confidence bands at the 95% and 90% levels, which are wider than pointwise bands and appropriate for assessing the pre-treatment path as a whole. For the aggregated $\hat{\theta}_1$ we report the bootstrap standard error with a normal-approximation confidence interval and p -value.

¹⁵Note that this is distinct from an alternative aggregation that is often reported when using Callaway and Sant’Anna (2021) estimator, which effectively reverses the order of the inner and outer sums, and weights the latter. Our choice of aggregation results in each post-treatment horizon being given equal weight but compositionally over-weighting early-treated cohorts because they are present in more periods; the alternative overrepresents early treatment horizons but avoids over-representing the early-treated cohorts. We choose the former to be comparable with aggregation in θ_2 and θ_3 , where the natural aggregation equally weights each relative event time.

5.4 Child and joint margins (θ_2, θ_3): single cohort

The child and joint margins involve a single 2014 treatment cohort compared with states never treated on either margin. In this two-group, common-timing setting, the two-way fixed-effects event-study regression is free of the negative-weighting problems that arise under staggered adoption (Goodman-Bacon, 2021) and coincides with the Callaway–Sant’Anna estimand; we, therefore, estimate:

$$Y_{st} = \alpha_s + \lambda_t + \sum_{\substack{e=-4 \\ e \neq -1}}^6 \beta_e \mathbf{1}\{t - 2014 = e\} T_s + \varepsilon_{st}, \quad (2)$$

where T_s indicates treated states and the omitted category is $e = -1$, and report

$$\hat{\theta}_j = \frac{1}{7} \sum_{e=0}^6 \hat{\beta}_e, \quad j \in \{2, 3\}, \quad (3)$$

which represents the analog of the aggregated group-time treatment effect for $\hat{\theta}_1$.

Estimating the event-study coefficients via OLS rather than Callaway and Sant’Anna’s approach allows us to use wild cluster bootstrap inference, which is an appropriate inference approach given the 14 and 11 clusters we have in each analysis, respectively.

Inference on $\hat{\theta}_j$ uses the restricted (null-imposed) wild cluster bootstrap on the linear contrast in equation (3), with Webb six-point weights—at fewer than roughly a dozen clusters the two-point Rademacher distribution supports too few distinct bootstrap samples (Cameron et al., 2008; Roodman et al., 2019; Webb, 2023)—and 9,999 replications.

Reported standard errors, and the pointwise confidence intervals in event-study figures, use the bias-reduced (CR2) cluster-robust covariance estimator with Satterthwaite degrees of freedom (Pustejovsky and Tipton, 2018).

5.5 Robustness checks

We conduct several robustness checks, as follows:

Policy sensitivity. Given the complex Medicaid policy environment, we assess several variations on how the treatment and control pools are defined. Specifically, we conduct checks that include Wisconsin in the comparison pool and remove Nebraska from it. We also drop California (for the dual-treatment arm, as child eligibility was phased in 2013-14). In the heterogeneity analysis, we also consider sensitivity to inclusion or exclusion of the three states that drop family planning waivers during the study period.

Estimation approach. We report simple pooled TWFE estimates for each treatment margin, for the sake of comparison. We also report aggregate estimates in line with our main aggregation but restricting through the fourth

post-treatment year, which reduces the compositional over-weighting of the earliest treated cohort for θ_1 .

Data encoding. In the Medicaid claims data, values are suppressed if the true values are between 1 to 10. To bound the extent to which our results are sensitive to the choice to assume a uniform underlying distribution of these values, we report replacing all suppressed values with both 1 or 10.

5.6 Heterogeneity by family planning waiver status

We also assess heterogeneity by family planning waiver status. We only conduct this heterogeneity analysis for the adult-only treatment arm, as the other margins include too few states.

The presence of family planning waiver policies generates predictions on heterogeneity in treatment effects: effects should be smaller where a family planning waiver policy was already in place. The hypothesized channels require a teen to gain, or perceive gaining, new coverage. In states whose family planning waivers already covered contraceptive services for women with incomes well above Medicaid thresholds (typically to roughly 185% FPL), expansion opened little new eligibility margin, and treatment effects should be attenuated regardless of the size of the policy change—although the strength of this attenuation depends empirically on the extent to which waiver eligibility had been taken up.

Importantly, in this heterogeneity analysis we split only the treated side: the comparison pool is held fixed across arms and is not split by waiver status, which assesses whether the treatment magnitude is different without simultaneously shifting the counterfactual. We also re-estimate the non-waiver arm against only the non-waiver comparison states, which addresses the fixed-pool assumption directly; and re-estimate while removing the three waiver-terminating states (Arkansas, Michigan, and Ohio).

6 Results

Our results highlight the importance of expansions for adults in increasing contraceptive prescriptions for teens within Medicaid, although we cannot fully rule out that the child-only expansions may have also led to economically meaningful increases. The results are reported by estimand and contraceptive outcomes. All results should be interpreted in terms of the number of additional contracepting patients per 1,000 females ages 15 to 17 in a given state.

6.1 Main results

Table 2 presents the aggregated ATT estimates over the first four post-treatment years. Column 1 shows the results for the adult-only expansion treatment group. The estimate indicates an increase of 7.05 per 1,000 teens, although this estimand is not statistically significant at conventional levels ($p = 0.12$). In contrast,

the LARC estimate is significant at the 1% level, with an estimate of 2.80 additional contraceptives per 1,000 teens. Similarly, the estimate for short-acting methods shows an increase of 6.59 contraceptives per 1,000 teens and it is significant at the 5% level. While the short-acting contraception represents the largest contribution to the overall contraceptive increase, the LARC estimate represents a larger increase in proportional terms: an increase of one-third, whereas the short-acting contraceptives increase by less than one-tenth.

Column 2 presents estimates for the child eligibility increase, whereby some states needed to increase the level of eligibility for teens. States considered as controls in this specification already had eligibility for teens at this higher level. None of the estimates for this treatment group are significant at conventional levels, and for any contraception and LARCs the point estimates are close to zero. For short-acting contraceptives, the point estimate is nearly identical in magnitude to that of column 1, but the standard error is nearly twice as large. This comparison suggests caution in concluding that the lack of significance is indicative of a meaningful null effect of the increase in child eligibility on its own.

Column 3 presents the estimates for the simultaneous expansion of adult and child eligibility. Strikingly, the point estimates for any contraceptives and for short-acting contraceptives are much larger in magnitude: 25.58 and 19.56, respectively. The LARC estimate is similar in magnitude to that of the adult-only expansion in column 1, with an increase of 3.09 versus 2.80, although for the simultaneous treatment the LARC estimate is only statistically significant at the 10% level. A notable difference for this group is much lower initial baseline levels of contraceptive prescription rates through Medicaid, meaning that effects are not only larger in absolute magnitude but also proportionally.

These main results suggest that adult eligibility does matter for teens' contraceptive take-up, and they are suggestive that expanding eligibility for adults and children together is complementary, yielding larger effects than the sum of either intervention in isolation, at least for short-acting contraceptives.

6.2 Robustness checks

To probe whether these results hold up to numerous robustness checks, Table 3 presents a series of alternative specifications for each estimand, with the original main estimates presented under the label "Primary." In some cases, due to the differences in treated states or technical limitations, certain robustness checks are not possible to implement across all estimands.

It is important to note that not all checks are expected to produce a result that is similar to the original result. Panel (a) represents variations in the sample and measurement of the outcome variable, for which we expect estimates to be stable. Panel (b), by contrast, presents alternative specifications where we expect that the estimate may change in a particular way, and differences are informative in motivating our choices.

For Panel (a), estimates are largely stable in both magnitude and significance. For the child-only expansion, represented in columns 4 to 6, all esti-

Table 2: Average treatment on the treated by treatment type and contraceptive outcome

	Adult expansion (E1, CS)	Child channel (E2, TWFE)	Simultaneous (E3, TWFE)
Any contraception	7.05 (4.54)	2.23 (7.25)	25.58** (9.43)
Pre-period mean	97.5	95.4	67.4
LARC	2.80*** (0.77)	0.48 (0.61)	3.09* (1.67)
Pre-period mean	8.1	7.2	4.0
Short-acting hormonal	6.59** (2.97)	6.63 (5.19)	19.56** (6.86)
Pre-period mean	74.4	66.3	55.6
Treated states	23	9	6
Control states	5	5	5
Years	2010–2020	2010–2020	2010–2020

mates remain non-significant and similar in magnitude to the main estimates with moderate variations. The simultaneous expansion is likewise robust across the alternative sample and measurement specifications.

Panel (b) must be considered one specification at a time. Wisconsin is excluded from the main set of controls; although it did not adopt the ACA Medicaid expansion, it did choose to increase state program Medicaid eligibility levels to 100% of the FPL around the same time. Thus, if we hypothesize that these eligibility thresholds matter, its inclusion as a control should have a negative effect on all estimates. Indeed, parsing across the row, this is precisely the effect observed, supporting the informed choice of excluding Wisconsin entirely.

Not-yet-treated controls are only an option when there is staggered treatment, that is to say, for the adult-only expansion. A key motivation for using never-treated controls is consistency of comparison groups across the different treatment types. Employing not-yet-treated observations as the control groups requires more stringent assumptions on the parallel counterfactual trends, as the composition of the control group changes throughout the treatment period. The advantage is often considered to be that this choice can improve precision, yet the opposite occurs in our case, with standard errors instead increasing, even as point estimates attenuate towards zero and both the overall and LARC estimates become insignificant, while the short-acting estimate is significant only at the 10% level.

Table 3: Robustness Checks

	Adult expansion (E1, CS)			Child channel (E2, TWFE)			Simultaneous (E3, TWFE)		
	Any contra.	LARC	Short- acting	Any contra.	LARC	Short- acting	Any contra.	LARC	Short- acting
Primary	7.05 (4.54)	2.80*** (0.77)	6.59** (2.97)	2.23 (7.25)	0.48 (0.61)	6.63 (5.19)	25.58** (9.43)	3.09* (1.67)	19.56** (6.86)
<i>Panel A: Sample and measurement robustness (stability expected)</i>									
Drop NH & CO	5.69 (4.53)	2.68*** (0.85)	5.77** (2.89)	—	—	—	—	—	—
Drop CA	—	—	—	—	—	—	29.79** (9.90)	3.59* (1.92)	22.88*** (7.08)
Drop NE from controls	—	—	—	3.94 (7.72)	0.76 (0.62)	7.57 (5.54)	27.29** (9.79)	3.37* (1.67)	20.49** (7.13)
Suppressed cells = 1	7.09 (4.54)	2.80*** (0.77)	6.64** (2.94)	2.25 (7.24)	0.48 (0.61)	6.66 (5.18)	25.72** (9.41)	3.12* (1.67)	19.68** (6.84)
Suppressed cells = 10	7.02 (4.55)	2.80*** (0.77)	6.57** (2.97)	2.22 (7.26)	0.48 (0.61)	6.62 (5.20)	25.60** (9.41)	3.06* (1.67)	19.61** (6.85)
<i>Panel B: Sensitivity to identification and estimation choices (see text for expected direction)</i>									
Keep WI in controls	3.52 (5.50)	1.37 (1.77)	4.16 (3.59)	-1.95 (8.01)	-1.28 (1.84)	3.71 (5.73)	21.40* (10.03)	1.33 (2.41)	16.64** (7.28)
Not-yet-treated controls	5.10 (4.40)	0.99 (1.13)	5.97* (3.16)	—	—	—	—	—	—
TWFE + wild cluster bootstrap	4.45 (4.85)	1.11 (1.03)	5.29 (3.11)	-3.10 (9.47)	-1.00 (0.76)	3.12 (6.70)	20.09** (8.03)	2.77 (1.80)	15.75** (5.59)
Anticipation (1 year)	7.12 (5.49)	2.05* (1.05)	7.92** (3.70)	-2.12 (8.78)	-0.68 (0.91)	3.28 (6.70)	16.15* (8.02)	2.25 (1.63)	13.54* (6.51)

The application of a static TWFE in the staggered treatment setting is expected to attenuate estimates towards zero; indeed, this is precisely what occurs. The pooled static TWFE estimate is also presented for the other two treatments, to contrast with the primary specification, which estimates the event-study coefficients separately instead of pooling and averaging over the six post-treatment periods. The coefficients for the simultaneous treatment in columns 7 to 9 are relatively robust, though they decrease in magnitude and the LARC coefficient becomes insignificant.

Anticipation effects are a concern either if there is an increase in the outcome for treated states immediately prior to official treatment; if this were the case, then shifting the reference period one year earlier should increase the treatment estimates. Instead, all but one estimate decreases in magnitude, suggesting that this is not a major cause for concern.

6.3 Event Studies

We also present event studies for each of the treatment types and outcomes to examine dynamic treatment effects and the presence of differential trends prior to treatment implementation.

Figure 5 presents the event study estimates for the adults-only expansion. One point that is immediately evident upon examining the graphs is that the period-specific coefficients are in many cases fairly imprecise, owing to the small number of observations and states. In line with the aggregated estimates, effects on any contraception are largely not statistically different than zero, with the exception of one period. In contrast, the LARC and short-acting contraception rates show clearer patterns of positive and significant treatment effects, though little evidence of a dynamically increasing treatment effect—the first year of treatment ($t = 0$) yields small treatment effects across outcomes, but after an initial jump in the second period the point estimates appear to stabilize. The pre-trends for each series are largely flat, though early pre-periods especially are fairly imprecise.

Figure 6 presents relative-time coefficients for the child-only expansion. These coefficients align with the main estimates in Table 2 in that none of the post-treatment estimates across any outcome appear significantly different than zero, and there is no evident trend in the post-treatment period. However, there is a notable and concerning declining and seemingly linear trend across all outcomes in the pre-treatment period. Although only one of these coefficients is significantly different at the 5% level, the visually differential pre-treatment trends suggest another point of caution in interpreting the null estimates of the increase in child eligibility levels as a valid estimate of its true treatment effect.

Finally, Figure 7 exhibits the event-study estimates for the simultaneous changes in eligibility for children and adults. For any contraception and by type, we observe evidence of non-differential trends before policy implementation, providing support for our identification assumption. Moreover, the estimated effects are statistically significant in most periods. Comparing the findings in this Figure relative to Figures 5 and 6 provides important insights into how

Figure 5: Event-Study: Adults only expansion

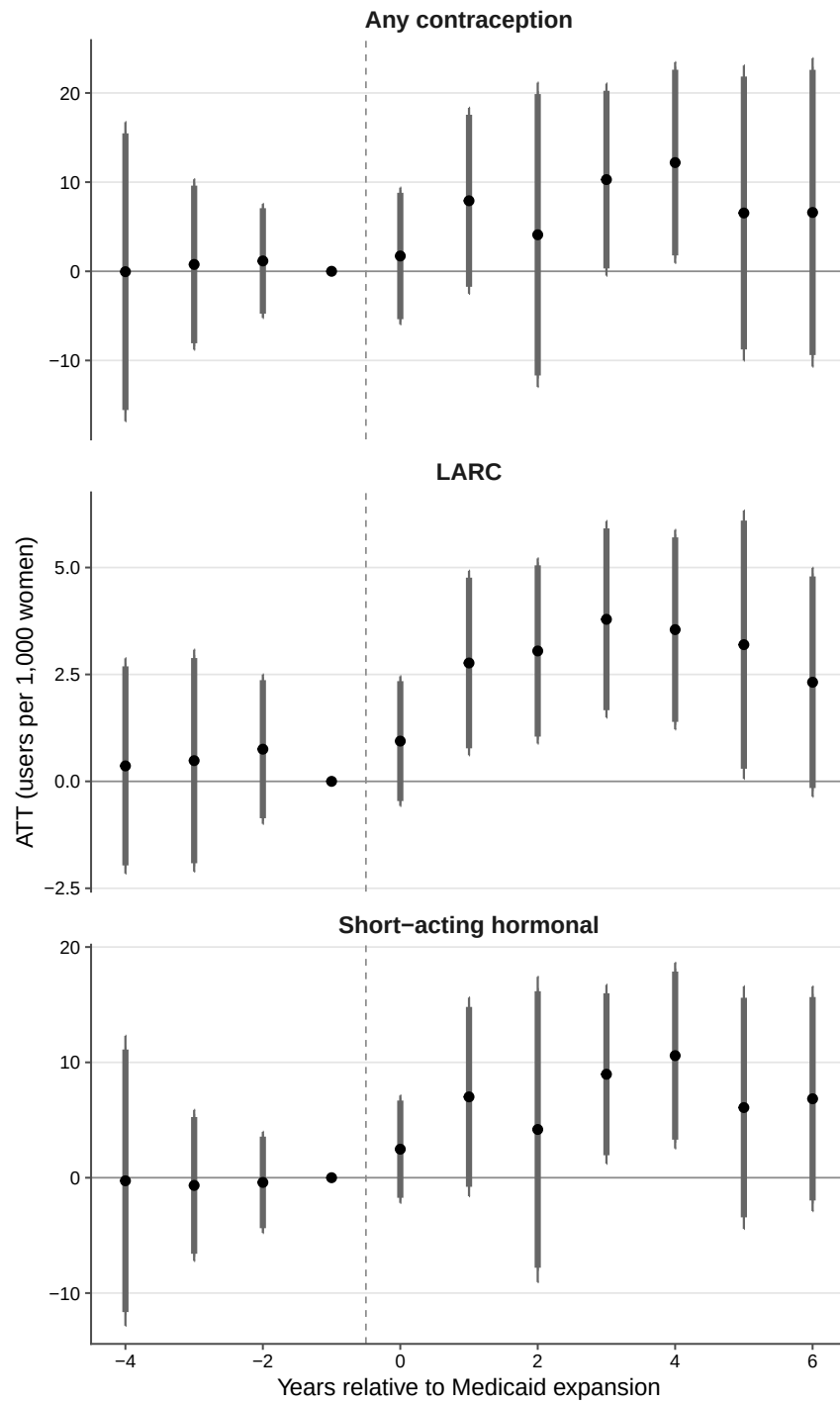
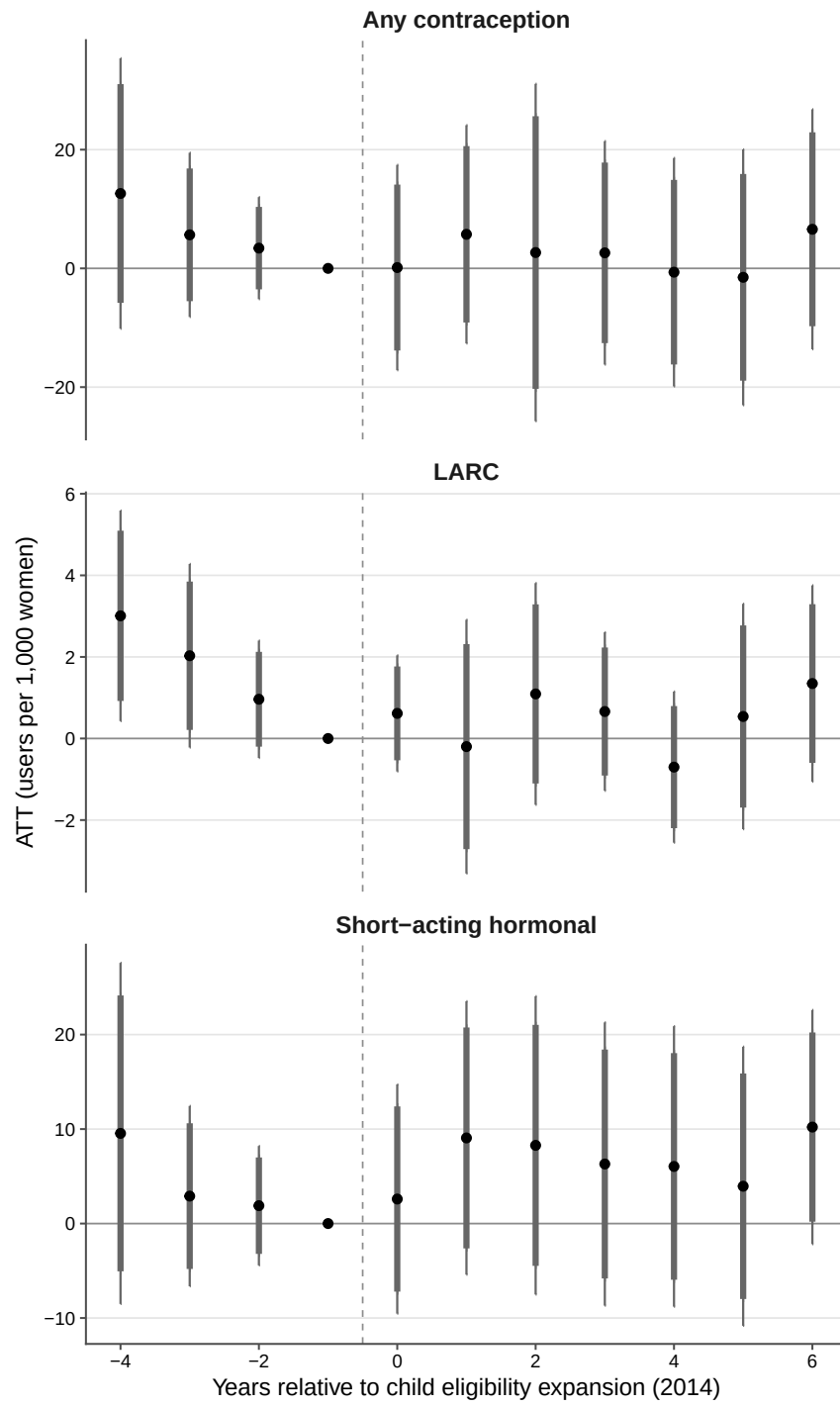


Figure 6: E2 Event-Study: Child-only expansion



the impacts of simultaneous treatments compare to those of each treatment in isolation. As for the adults-only expansion, estimates exhibit some “ramp up” in the first year, with point estimates subsequently stabilizing. As for the child-only treatment, visual examination of the pre-treatment point estimates is suggestive of a downward linear trend for treatment compared to control units. However, to the extent that this represents divergent trends that would have continued in the absence of treatment, they would bias our estimated treatment effects towards zero. Thus, for the simultaneous treatments, we are more confident that our estimates are indicative of a true positive treatment effect, although the magnitude may be underestimated.

6.4 Heterogeneity by family planning waiver status

In addition to child and adult eligibility for full Medicaid coverage, we have also highlighted a third type of narrower Medicaid eligibility that may affect outcomes in our analysis: family planning waiver programs.

Many of these waiver programs were instituted many years before our study period, so the primary concern is not necessarily in contaminating estimates with previously treated units. Instead, there may be a ceiling effect: if treated states already had such programs and there was meaningful take-up, the share of the population that gains eligibility and also may newly take up contraception under Medicaid would be smaller. Thus, we expect that states that already had family planning waivers in place should exhibit smaller treatment effects, while states that had not implemented them would experience larger treatment effects. There is a third, special case of three states (AR/MI/OH) that had waiver programs prior to the start of our study period, but *eliminated* them during the study period. The effect of including or excluding these three states is somewhat more ambiguous since initially they operate like an “already-treated” unit, while later on some fraction of the population (those above 133% of the FPL, who are not eligible for full Medicaid but were previously eligible for the family planning waiver coverage) lose eligibility for contraceptives.

Given the ongoing limitation of the small groups of states for each treatment type, we only carry out this heterogeneity analysis for the adult-only ACA expansion, since it includes the largest number of states.

Table 4 makes these differences by family planning waiver status explicit. Dropping AR, MI, and OH increases the magnitude of estimates across all three outcomes, and likewise all estimates are significantly different from zero. In contrast, when the family planning waiver states are isolated as the treated group, the estimates for any contraceptives and short-acting decrease markedly in magnitude and become insignificant, though for LARCs they are essentially unchanged (2.68 vs 2.80).

When treated states without prior family planning waiver programs are isolated, in contrast, the estimate for any contraceptives nearly doubles (from 7.05 to 13.45), mostly driven by short-acting contraceptives (6.59 in the primary pooled specification and 10.54 in the non-FPW specification). LARC estimates remain largely unchanged, at 3.06. Appendix Figure A2 presents event stud-

Figure 7: Event-Study: Adult and child expansion

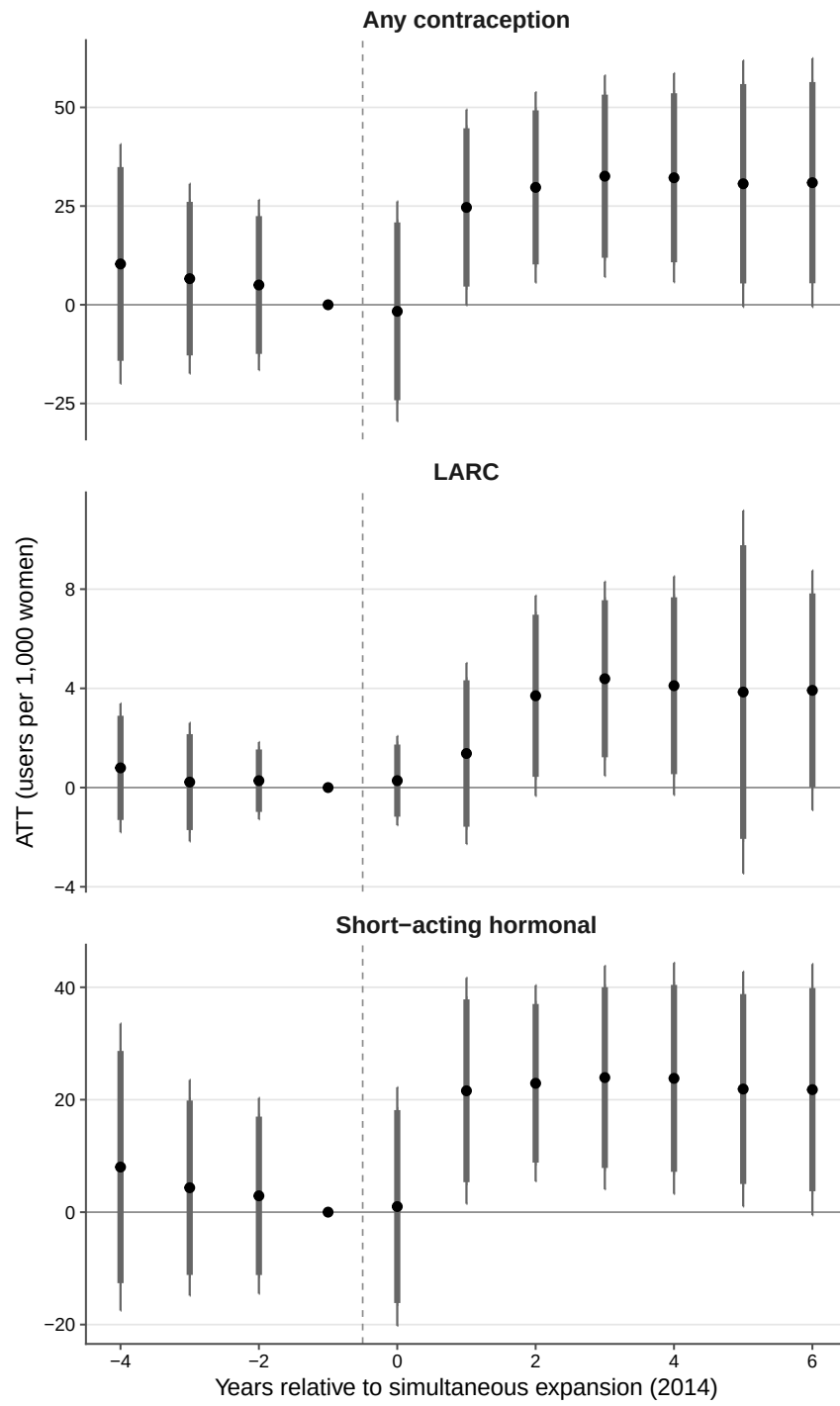


Table 4: Heterogeneity by family planning waiver status

	Any contraception	LARC	Short-acting hormonal	Treated states
All treated (pooled)	7.05 (4.54)	2.80*** (0.77)	6.59** (2.97)	23
Drop AR/MI/OH (waiver losses)	9.94** (4.60)	2.98*** (0.87)	8.28*** (2.95)	20
FPW treated (15)	4.15 (4.98)	2.68*** (0.80)	4.82 (3.02)	15
less AR/MI/OH (waiver losses)	7.84 (5.09)	2.91*** (0.91)	6.95** (3.30)	12
Non-FPW treated (8)	13.45** (6.15)	3.06* (1.61)	10.54** (4.26)	8
keeping WI in controls	9.82 (7.01)	1.57 (2.13)	8.04* (4.64)	8
non-FPW controls only	7.62 (5.65)	-0.49 (2.16)	9.03* (5.01)	8

ies for each outcome for states without pre-ACA family planning waivers and supports these findings.

This heterogeneity analysis highlights that family planning waivers do appear to interact with treatment estimates in expected ways, and that expanding adult Medicaid in the absence of prior family planning coverage likely yields larger treatment effects than indicated in our main estimates. One subtlety is that family planning programs appear to matter more for short-acting contraceptives, whereas the effects on LARCs are more stable across family planning waiver status.

7 Conclusion and Discussion

We study whether expanding adults' eligibility for public health insurance affects teens' contraception use. We rely on the universe of Medicaid and CHIP claims and a novel comprehensive index for all contraceptive National Drug Codes for 2010-2020.

We examine three margins of eligibility change: adult-expansion only, expansions that affect children only, and simultaneous expansions. Our findings show that adult eligibility matters for teens. In adult-expansion-only states, LARC rates among teenagers increased by 35% relative to the pre-expansion mean. Similarly, short-acting contraception rates increased by 8% relative to the pre-treatment mean. When adult and child eligibility are expanded simultaneously, the effects on contraception rates are even larger— by almost 40% relative to the pre-expansion mean. We do not find statistically significant effects of child expansion only. However, the imprecision of these estimates and evidence of

pre-existing trends in event study analyses prevent us from interpreting this evidence as a no effect.

Our findings are consistent with prior research showing that teens are responsive to changes in the cost of and access to contraception. Kearney and Levine (2009) find that Medicaid family planning waivers that expanded eligibility to women of higher income reduced teen births, which the authors attributed to greater contraceptive use. We show evidence that the effects of adult insurance and simultaneous expansions are even stronger in places with no Medicaid family planning waivers, supporting the fact that these waivers brought access to contraception to some of the teens that would have benefited from insurance expansions. In this sense, our estimates for states with waivers may understate the effect that broader adult coverage can have where other reproductive health programs are absent. Studies of the Colorado Family Planning Initiative, which expanded access to LARCs through Title X clinics, also show that improving access to these methods reduced teen birth rates (Lindo and Packham, 2017; Kelly et al., 2020). Conversely, cuts to family planning funding have increased teen childbearing (Packham, 2017). Most closely related to our study, Rosenberg (2026) shows that teen births respond to changes in parental insurance coverage. Our findings support that contraceptive access could be a mechanism behind this change.

Taken together, our findings indicate that the benefits of health insurance coverage for adults extend to teenagers, when reproductive health needs mainly emerge. Evaluations of adult coverage policies that focus only on the adults gaining coverage likely miss the gains arising from these spillover effects.

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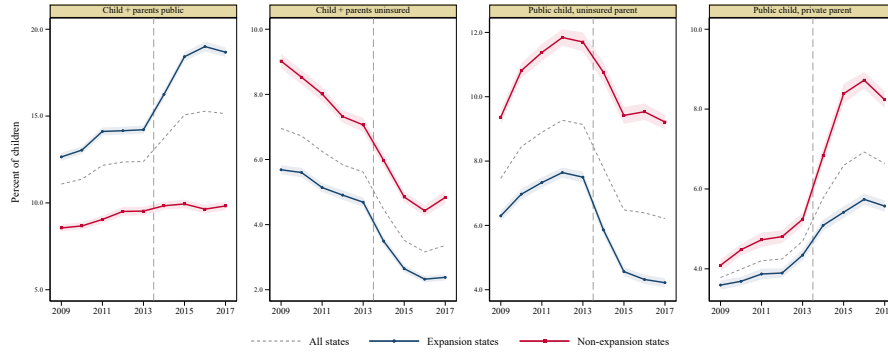
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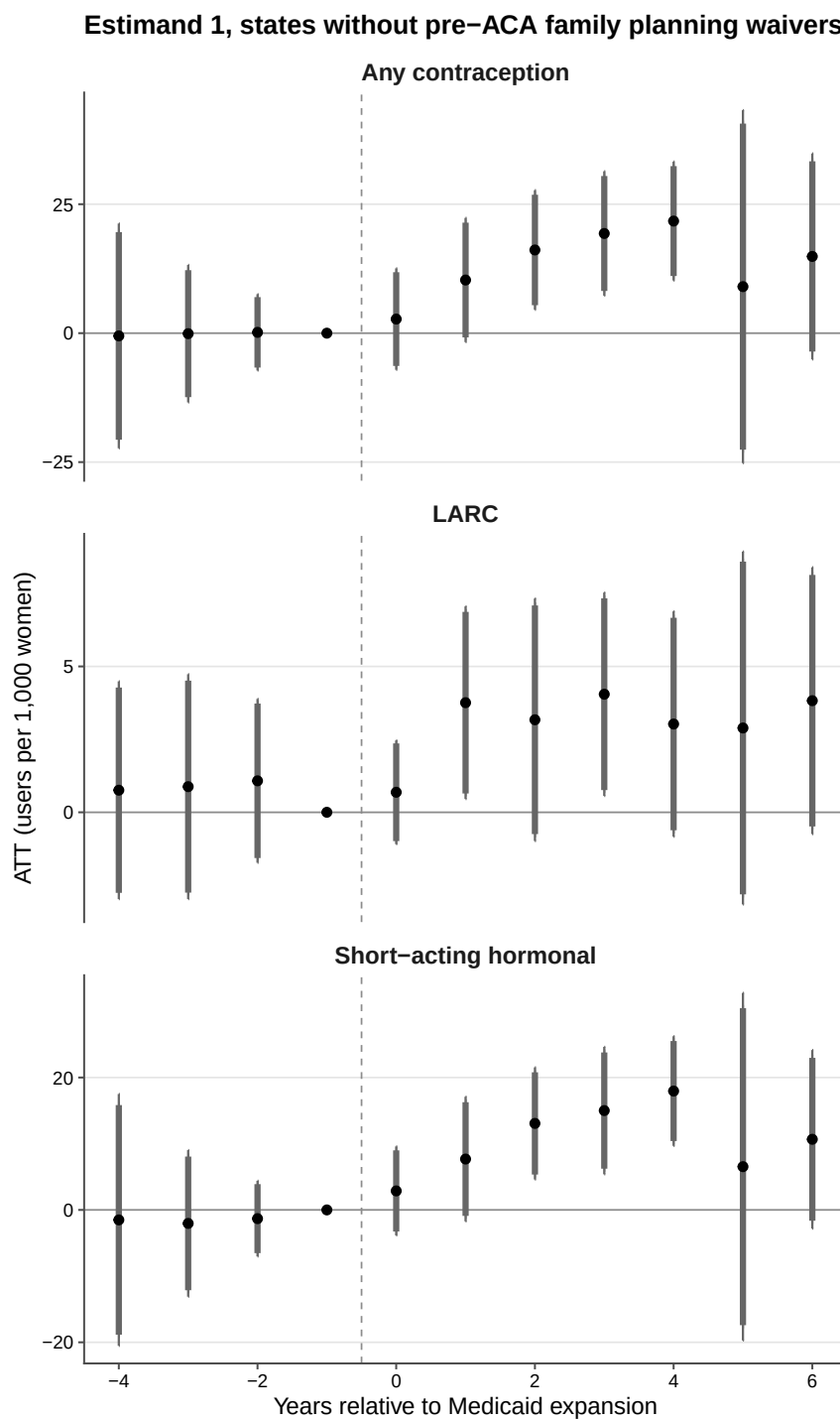
Appendix A

Figure A1: Selected within-family health insurance coverage patterns, by coverage combination and state Medicaid expansion status.



Notes: Children aged 6–18 living with a co-resident parent aged 19–64, American Community Survey 1-year files, 2009–2017. Each panel plots the percent of children in one parent–child coverage combination, separately for all states, for the 32 states with the ACA Medicaid expansion in effect before 2018, and for the 19 expanding in 2018 or later or not at all; the all-states series is the composite of the other two, not a third group. Each panel is scaled independently, so the shapes of the series may be compared across panels but their heights may not. Shaded bands are 95% confidence intervals for the two state groups, computed from the 80 successive difference replicate weights. The vertical line marks the January 2014 ACA coverage provisions. Patterns requiring “all parents” to share a coverage type place two-parent families whose parents differ from one another into a residual mixed category, which is not shown; the four patterns displayed therefore do not sum to 100 percent, and the remaining patterns are omitted from the figure but retained in the denominator. Expansion states are AK, AZ, AR, CA, CO, CT, DE, DC, HI, IL, IN, IA, KY, LA, MD, MA, MI, MN, MT, NV, NH, NJ, NM, NY, ND, OH, OR, PA, RI, VT, WA and WV. Non-expansion states are AL, FL, GA, ID, KS, ME, MS, MO, NE, NC, OK, SC, SD, TN, TX, UT, VA, WI and WY.

Figure A2: Event-Study: Adult-only expansion accounting for family planning waivers



Appendix B

Notation and target parameters

Let s index states and $t \in \{2010, \dots, 2020\}$ calendar years, and let Y_{st} denote a contraceptive utilization rate (users per 1,000 women) for a fixed teen age group. Two policy margins vary. First, adult expansion: let $G_s \in \{2014, \dots, 2020\} \cup \{\infty\}$ be the year state s adopted the ACA adult expansion, with $G_s = \infty$ for states that did not adopt during the sample period. Second, the child floor: let $S_s = 1$ indicate a staircase state (child Medicaid eligibility $\leq 100\%$ FPL in January 2013), all of which raised child eligibility to the ACA floor in January 2014. Denote by $Y_{st}(a, c)$ the potential outcome under adult-expansion status $a \in \{0, 1\}$ and child-floor status $c \in \{0, 1\}$, so that, for example, $Y_{st}(1, 0)$ is the outcome under adult expansion alone and $Y_{st}(0, 1)$ under the child floor alone, with $(0, 0)$ the no-ACA-treatment regime. Observed treatment states are $a_{st} = \mathbf{1}\{t \geq G_s\}$ and $c_{st} = S_s \cdot \mathbf{1}\{t \geq 2014\}$.

All three estimands are reported as the average treated effect over the first seven post-treatment years (event times $e = 0, \dots, 6$), the full post-treatment horizon the data afford for every margin. This window is common to the three estimands, so that every reported number targets the same functional of the treatment-effect path. We report a narrower window, $e = 0, \dots, 4$, alongside it throughout, and the sensitivity analysis of the estimation section is conducted on that narrower window; the reasons for reporting both are estimability considerations discussed there.

For the adult margin, following ?, define group-time average treatment effects on the treated among non-stairstep states,

$$ATT_1(g, t) = E[Y_{st}(1, 0) - Y_{st}(0, 0) \mid G_s = g, S_s = 0], \quad (4)$$

and aggregate across the staggered cohorts at each event time and then across the reported window:

$$\theta_1 = \frac{1}{7} \sum_{e=0}^6 \sum_g \omega_{g,e} ATT_1(g, g+e), \quad (5)$$

where $\omega_{g,e}$ is the share of cohort g among treated cohorts observed at event time e . The inner sum is Callaway and Sant’Anna’s event-study aggregation of the group-time effects; the outer equal-weighted average over $e = 0, \dots, 6$ is our reporting choice of summary. Because later cohorts are observed for fewer post-treatment years, cohort composition shifts across e within the window ($g+e \leq 2020$), and at the top of the window only the 2014 cohort remains. The narrower $e = 0, \dots, 4$ window we report alongside limits, but does not eliminate this compositional turnover; we return to it in the estimation section.

The child and joint margins involve a single treatment cohort (2014), so no

cohort aggregation is needed:

$$\theta_2 = \frac{1}{7} \sum_{e=0}^6 E[Y_{s,2014+e}(0,1) - Y_{s,2014+e}(0,0) \mid S_s = 1, G_s = \infty], \quad (6)$$

$$\theta_3 = \frac{1}{7} \sum_{e=0}^6 E[Y_{s,2014+e}(1,1) - Y_{s,2014+e}(0,0) \mid S_s = 1, G_s = 2014]. \quad (7)$$

Note that θ_3 is the effect of the policy bundle; the decomposition of θ_3 into an adult margin, a child margin, and their interaction is not identified for these states, per the discussion above.