Reproductive Health Choices under Political Uncertainty

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Abstract

I study how contraceptive method choice responded to the 2016 U.S. presidential election among patients at Planned Parenthood of Northern New England (PPNNE), using de-identified encounter data on 172,744 patients and 536,588 visits across Maine, New Hampshire, and Vermont, 2012–2021. Because the data include uninsured and publicly insured patients, they capture a population excluded from claims-based studies. Long-acting reversible contraception (LARC) was already trending upward before the election; an interrupted time series that separates this secular trend from a discrete change estimates a discontinuous increase of 1.8 percentage points in the LARC share of visits at the election (from a 21 percent baseline), with an offsetting decline in short-acting methods. LARC use plateaued afterward rather than continuing to climb, consistent with a one-time precautionary response. The increase is concentrated among publicly insured patients, with suggestive but underpowered evidence of larger responses among Black and Hispanic patients.

Keywords: contraception; long-acting reversible contraception; reproductive policy; political uncertainty; precautionary demand

JEL codes: I1; J1; D8

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1. Introduction

Long-acting reversible contraceptives (LARCs), including intrauterine devices (IUDs) and contraceptive implants, are the most effective form of long-term birth control, with typical-use failure rates below 1 percent per year compared with approximately 9 percent for oral contraceptives (Trussell, 2011). Once inserted or implanted, they require little to no maintenance or user intervention, eliminating the need to remember daily pills or regular contraceptive appointments. In contrast, methods such as oral contraceptives and injections require routine attendance to doctors' appointments and consistent access to health insurance while in use (Cleveland Clinic, 2022). Following the Affordable Care Act, which eliminated contraceptive cost-sharing for most privately insured women (Health Resources and Services Administration, 2011), mean out-of-pocket costs for an IUD insertion fell from $262 to $84 between 2010 and 2013 (Becker and Polsky, 2015), so a repeal threatened a concrete, recent gain.

The November 2016 election generated immediate concern about reproductive health access. Women perceived Trump's platform as a direct threat along several dimensions: he campaigned on appointing Supreme Court justices who would overturn *Roe v. Wade* and publicly committed to defunding Planned Parenthood (Ballotpedia, 2016), and he opposed the Affordable Care Act and vowed to repeal it, threatening the contraceptive coverage mandate written into the law. In the hours after the election was called, Google searches for "IUD" spiked, with the top rising related queries being "IUD Trump" and "get an IUD now" (ABC News, 2016).

A survey of 2,158 women conducted in January 2017 found that 42 percent had concerns about future access to contraception following the election, and that 5.3 percent had already obtained a LARC (Judge et al., 2017). Pace et al. (2019) document a positive relationship between the election and insurance claims for LARC, but claims data leave out the uninsured and publicly insured and do not separate the post-election change from the secular upward trend in LARC use already under way. I use direct encounter data from a reproductive-health provider whose patients include these groups; the publicly insured, heavily exposed to federal policy through Medicaid and the ACA, turn out to be the most responsive.

This paper connects several literatures. Contraceptive access has large effects on women's economic lives (Goldin and Katz, 2002; Bailey, 2006; Bailey and Lindo, 2017), and cost and provider access shape method choice (Kearney and Levine, 2009; Kelly et al., 2020; Eeckhaut et al., 2021). A separate strand treats contraceptive choice as forward-looking: when future access is uncertain, longer-acting methods act as a hedge, a logic formalized as defensive investment in contraception under reproductive-policy shocks (Pennington and Venator, 2025) and foreshadowed by work on the insurance value of reproductive options (Levine and Staiger, 2002). LARCs are the natural margin for such a response, since a single insertion provides years of protection independent of continued coverage.

I find a discontinuous, trend-adjusted increase in LARC use at the 2016 election, offset by a decline in short-acting methods and shared across privately and publicly insured patients. The sample covers three northeastern states (Maine and Vermont reliably liberal, New Hampshire politically mixed); behavioral responses may differ in more restrictive policy environments, so the results speak most directly to this population rather than the nation.

2. Data

Data come from Planned Parenthood of Northern New England (PPNNE), an affiliate of Planned Parenthood Federation of America that operates in Maine, New Hampshire, and Vermont. The sample consists of all patients seeking birth control of some form over the period 2012–2021 at all PPNNE locations: four in Maine, five in New Hampshire, and thirteen in Vermont. Patient data include the date of the appointment, age, race, ethnicity, payer type, location, and birth control choice of each patient; all patients are women. Each patient has a unique de-identified identifier so patients can be followed over time, and all data were anonymized by PPNNE before transfer, so researchers received no identifying or confidential information. For LARC, a visit comprises the insertion plus subsequent follow-up or management visits rather than insertions alone, so the outcome measures the mix of contraceptive care delivered; because LARC generates few visits after insertion while short-acting methods generate recurring refill visits, visit-level shares do not map one-to-one onto the share of patients using each method.

The sample comprises 172,744 patients and 536,588 visits, with patients returning to Planned Parenthood an average of 3.1 times over the period. Patients paid using private insurance at 44.4 percent of visits, public insurance (Medicaid or Medicare) at 35.6 percent, and self-pay at 19.6 percent; the inclusion of publicly insured and self-pay patients is what distinguishes these data from claims-based samples. LARC accounts for 21 percent of visits at baseline, oral contraceptives for 26 percent, and barrier methods for 15 percent, and patients average 27 years of age. Patients are less likely to be white and more likely to be Latino than the general population of the three states. Summary tables appear in the Appendix.

2.1. Ethics and data security

The study was reviewed by the University of Pittsburgh IRB and determined exempt under 45 CFR 46.104(d)(4) (Protocol STUDY21020042), and separately reviewed by the Xavier University IRB. The exemption applies because the research is secondary analysis of records fully de-identified by PPNNE before transfer; no identifiable information was available to the research team at any stage. Because the data are not Protected Health Information under HIPAA, patient consent was not required. Data were stored on password-protected devices and all reported results are aggregated to preclude re-identification.

3. Empirical Strategy

LARC use was already rising before the election, by about 1.5 percentage points per year, so a before-after comparison would conflate this secular trend with any genuine election response. I therefore estimate an interrupted time series that separates the two:

$$\text{LARC}_{it} = \alpha + \beta_1 \,\text{month}_t + \beta_2 \,\text{post}_t + \beta_3 \,(\text{month}_t \times \text{post}_t) + X_{it}'\gamma + \varepsilon_{it}, \tag{1}$$

where month_t is the number of months relative to November 2016 (the running variable), post_t indicates the post-election period, and X_{it} comprises calendar-month (seasonal) dummies and state, payer-type, and race fixed effects. A separate year term is collinear with the linear trend and is not separately identified here; the local linear estimates reported below, which impose no global trend, do include year effects. The coefficient β_1 is the pre-election trend, β_2 the discontinuous level shift at the election (the estimate of interest), and β_3 the change in slope afterward. The regression is estimated at the visit level as a linear probability model on the $\pm$24-month window (238,140 visits), with standard errors clustered by month. As a complement I estimate an event study with six-month period indicators, referenced to the six months immediately preceding the election.

4. Results

Table I reports the interrupted time series estimates. The pre-election trend is positive and significant (LARC rising about 1.5 percentage points per year), and a discontinuous level shift of 1.8 percentage points ($p < 0.01$) remains after netting out this trend, from a baseline of 21 percent. A local linear regression-discontinuity estimator with a data-driven bandwidth yields a somewhat larger level shift of 2.9 percentage points (Appendix Table II); the two estimates differ in both trend model and control set (the local linear specification also includes year effects), with the interrupted time series the more conservative because it imposes a single linear trend across the full window. The post-election slope is negative and offsets the pre-trend, so LARC use plateaus rather than accelerating, the pattern expected of a one-time precautionary adjustment. The same specification applied to short-acting methods (oral contraceptives, injection, ring or patch, and barrier methods) yields a decline of 1.3 percentage points, consistent with substitution toward longer-acting methods; because this does not fully offset the 1.8-point LARC rise, the roughly half-point residual reflects a small shift out of other visit categories rather than a one-for-one swap. Because the outcome is a share of visits rather than of patients, I probe it two ways. First, monthly method counts: short-acting visits fell by about 6 percent at the election ($p < 0.10$), while LARC and total visit counts were statistically unchanged (Appendix Table VII). Second, and more directly, I measure LARC initiation at the patient level, using each patient's first LARC visit: the monthly rate at which active patients initiated LARC rose by 1.4 percentage points at the election, from an 11.7 percent baseline ($p < 0.05$; Appendix Table VIII). Because the number of active patients per month is itself statistically unchanged at the election (a -1.6 percent shift, $p = 0.56$), this rate increase is not driven by a shrinking denominator. The raw count of initiations rose by about 10 percent but is less precisely estimated ($p = 0.12$), so the patient-level evidence points to a real, if modest, increase in LARC initiation rather than a shift in visit composition alone. The event study in Figure 1 is consistent with this pattern but is not independent evidence: the pre-election coefficients rise toward the reference period, so the jump at the election is identified only relative to the extrapolated linear trend, while the post-election coefficients remain roughly flat and elevated. The level shift remains positive and significant across estimation windows, though it attenuates from 1.9 to 1.3 percentage points as the window widens from $\pm$18 to $\pm$36 months (Appendix Table V); the $\pm$12-month window is too short to identify it separately from the trend, and the gentle attenuation at wider windows reflects the post-election elevation being bounded rather than a growing effect. Three further checks address the concern that a linear trend understates an accelerating counterfactual. Allowing a quadratic pre-trend leaves a level shift of 2.7 percentage points ($p < 0.001$); a placebo interruption at a fake election date two years earlier (December 2014) yields a small, insignificant shift (-0.3 points, $p = 0.44$; Appendix Table VI); and coding the partial election month (November 2016) as pre-election, as here, or dropping it entirely leaves the shift essentially unchanged (1.6 and 1.8 percentage points). Across specifications the discontinuity ranges from 1.3 to 2.9 percentage points; I take the $\pm$24-month linear interrupted time series (1.8 points) as the primary estimate because it separates the secular trend explicitly and transparently while retaining a window wide enough to identify the level shift.

Table I: Interrupted time series estimates at the 2016 election

	LARC	Short-Acting
Level shift (post-election)	0.0181***	-0.0125**
	(0.00342)	(0.00522)
Linear trend	0.00126***	-0.00256***
	(0.000205)	(0.000275)
Post-election trend change	-0.00141***	0.00143***
	(0.000267)	(0.000340)
Observations	238140	238140

Standard errors in parentheses

Standard errors clustered by month.

Controls: month, state, payer type, race fixed effects.

Sample restricted to +-24 months around the November 2016 election (post period begins December 2016).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

"Level shift" is the discontinuous change at the election; "Linear trend" the monthly pre-election trend; "Post-election trend change" the change in slope afterward. Controls: calendar month, state, payer type, and race fixed effects (a separate year term is collinear with the trend). Standard errors clustered by month; sample restricted to $\pm$24 months around November 2016.

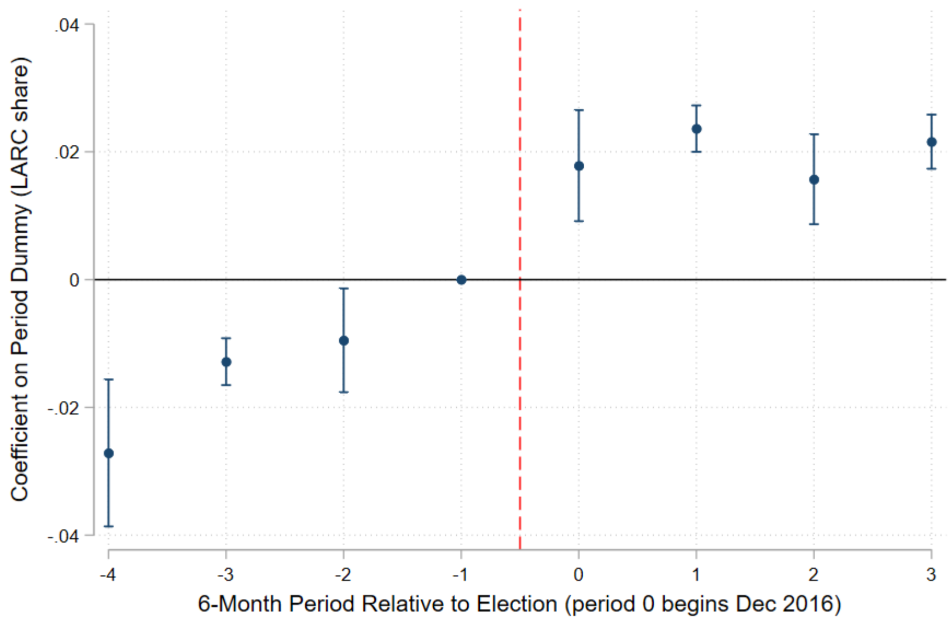

Figure 1: Event study: LARC use around the 2016 election

Coefficients from a regression of LARC use on six-month period indicators, with the six months preceding the election (period -1) as the reference; bars are 95% confidence intervals. Controls as in Table I.

Because identification rests on a single time-series break rather than on the number of visits, and inference relies on relatively few monthly clusters, I emphasize magnitude over statistical significance. The 1.8-percentage-point level shift is roughly a 9 percent increase in the share of visits involving the most durable methods; given LARC's far lower typical-use failure rate (Trussell, 2011), this mechanically raises the average effectiveness of the method mix delivered. I do not read it as a measured fertility effect: the share of visits at which a patient presented pregnant did not move detectably (Appendix Table II). The direction accords with Pennington and Venator (2025), who model contraceptive choice under reproductive-policy uncertainty, though their switching-probability object is not directly comparable to the share-of-visits change estimated here.

The increase is shared across insured patients. Estimating the same trend-adjusted specification within groups, the level shift is largest for the privately insured (3.1 percentage points) and positive for the publicly insured (1.4 points) and for white patients (1.9 points; Appendix Tables III–IV); estimates for self-pay and for Black and Hispanic patients are smaller and less precisely estimated. The response is thus broad-based across the insured population rather than confined to a single payer or racial group. Method switching provides scope for such substitution in this population: of about 91,000 patients who begin on short-acting methods and return, 28,612 switch to LARC at some point over the period, though this is a whole-period count rather than an election-specific response.

The same interrupted time series applied to two later federal events yields no comparable discontinuity: the level shift is -0.2 percentage points at the October 2018 Kavanaugh confirmation ($p = 0.65$) and 0.9 points at the September 2020 death of Justice Ginsburg ($p = 0.35$). Both followed the 2016 election, which appears to have captured the bulk of the precautionary response; a separate drop in early 2020 reflects the pandemic curtailment of non-emergency PPNNE services.

5. Conclusion

Using encounter data that include the uninsured and publicly insured, I find that the 2016 election was followed by a modest but robust trend-adjusted shift toward long-acting contraception, offset by a decline in short-acting methods and shared across insured patients. The pattern, a one-time jump that plateaus, is consistent with precautionary demand for contraception around a national election perceived as threatening reproductive-health access. In the appendix, LARC uptake declines modestly after protective laws in Maine and Vermont, while New Hampshire's more restrictive 2017 law shows no comparable change (Appendix A). The first pattern is loosely consistent with reduced precautionary demand once policy risk falls, but these are noisy binned series without controls, and the New Hampshire null does not display the increase a threat-based account would predict; I therefore read the state-policy evidence as suggestive at most. The results are descriptive and specific to three northeastern states.

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A. State Policies

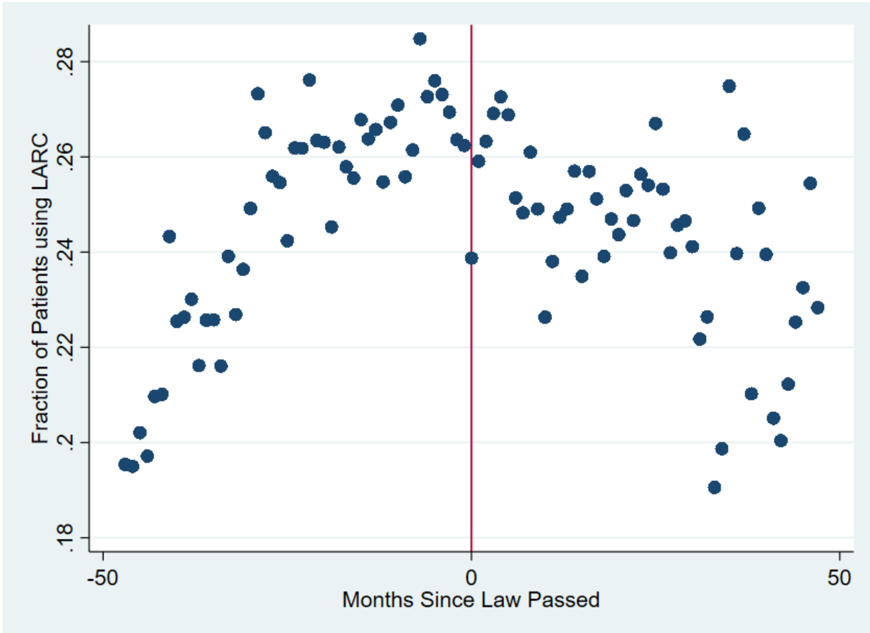

Figure 2: Binned LARC share around protective state laws (Maine and Vermont)

Binned means of the LARC share of visits (vertical axis) around Maine's June 2017 contraceptive-coverage mandate (Maine Legislature, 2017) and Vermont's May 2019 Freedom of Choice Act (Vermont General Assembly, 2019), with no controls or confidence intervals. Uptake declines modestly after both laws, but the series are noisy, pool two calendar dates, and overlap the 2020 pandemic period, so the pattern is descriptive only.

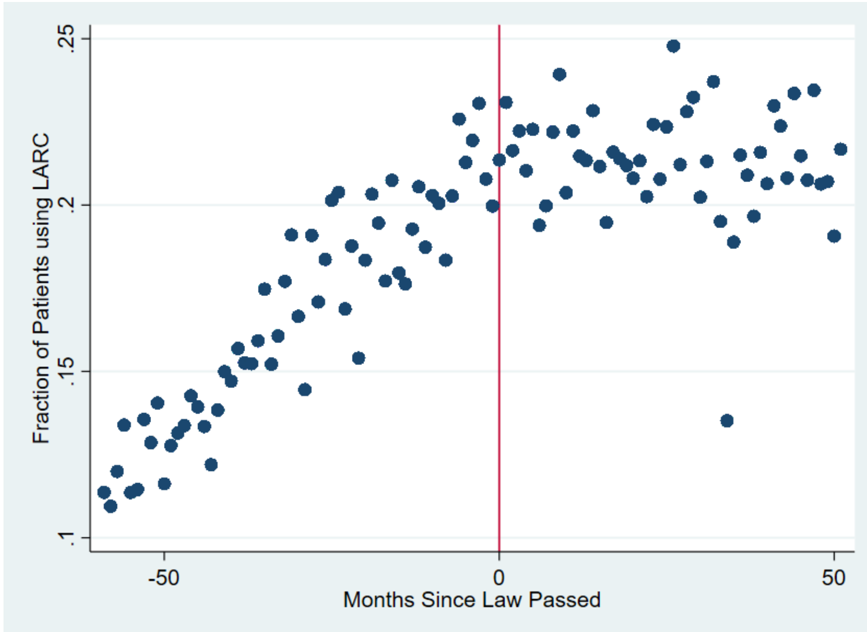

Figure 3: Binned LARC share around New Hampshire's 2017 fetal-homicide law

Binned means of the LARC share of visits (vertical axis) around New Hampshire's June 2017 fetal-homicide law (New Hampshire General Court, 2017), with no controls or confidence intervals; no comparable change follows the law. Descriptive only.

B. Additional Tables and Figures

Table II: Local linear regression-discontinuity estimates at the 2016 election

	LARC	Short-acting	Pregnant
Level shift	0.029***	−0.020**	0.001
	(0.007)	(0.009)	(0.004)
Observations	382,745	382,745	382,745

Local linear regression-discontinuity estimates (data-driven bandwidth) of the discontinuous change at November 2016 in the LARC share of visits, the short-acting share, and the share of visits at which a patient presented pregnant (the last reported for completeness). Covariates: calendar month, year, state, payer type, and race. ** $p < 0.05$, *** $p < 0.01$.

Table III: LARC response by payer type

	Public	Private	Self-pay
LARC level shift	0.014*	0.031***	−0.010
	(0.008)	(0.005)	(0.007)

Interrupted time series level shift, the same specification as Table I estimated separately by payer type on the ±24-month window. Standard errors clustered by month. * $p < 0.10$, *** $p < 0.01$.

Table IV: LARC response by race and ethnicity

	White	Black	Hispanic
LARC level shift	0.019***	−0.006	0.024
	(0.004)	(0.020)	(0.019)

Interrupted time series level shift, the same specification as Table I estimated separately by race/ethnicity on the ±24-month window. Standard errors clustered by month; the Black and Hispanic cells are small. *** $p < 0.01$.

Table V: Window-length sensitivity of the level shift

	±12 mo	±18 mo	±24 mo	±36 mo
Level shift (post-election)	n.i.	0.0190***	0.0181***	0.0125***
		(0.00319)	(0.00342)	(0.00355)
Linear trend	0.00232***	0.00113***	0.00126***	0.00187***
	(0.000210)	(0.000154)	(0.000205)	(0.000157)
Post-election trend change	-0.000605*	-0.00127***	-0.00141***	-0.00207***
	(0.000331)	(0.000305)	(0.000267)	(0.000196)
N	117896	177301	238140	359312

Standard errors in parentheses

LARC use. Standard errors clustered by month.

Controls: month, state, payer, race FE.

n.i. = not identified: the ±12-month window cannot separately identify the level shift from the calendar fixed effects and trend terms.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Interrupted time series estimates of the LARC level shift across analysis windows. At ±12 months the post indicator is nearly collinear with the calendar-month fixed effects and short trend, so the level shift is not separately identified (n.i.), though the trend terms remain identified.

Table VI: Robustness of the LARC level shift

Specification	Level shift
Baseline ITS (linear trend, ±24 months)	0.018***
Quadratic pre-trend	0.027***
Placebo interruption (December 2014)	−0.003

Level shift (β_2) in the LARC share of visits. The quadratic specification adds month_t^2 and its interaction with the post-election indicator. The placebo places a fake interruption 24 months before the election, using only pre-election data (±18 months around December 2014). *** $p < 0.01$.

Table VII: Monthly visit counts around the election

	ln(LARC visits)	ln(short-acting)	ln(total visits)
Level shift (post-election)	0.0540	-0.0572*	-0.0246
	(0.0429)	(0.0300)	(0.0319)
Linear trend	0.0124***	0.00230	0.00633***
	(0.00238)	(0.00156)	(0.00180)
Post-election trend change	-0.00991***	-0.000593	-0.00288
	(0.00272)	(0.00192)	(0.00206)
N	47	47	47

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Interrupted time series estimates on monthly log visit counts, with Newey–West standard errors (lag 3); coefficients approximate proportional changes. Sample ±24 months around November 2016.

Table VIII: Patient-level LARC initiation at the 2016 election

	Level shift
Initiation rate (initiators / active patients)	0.014**
	(0.006)
ln(monthly initiations)	0.096
	(0.061)

Interrupted time series estimates (Newey–West, lag 3) on monthly patient-level LARC initiation; a patient's first observed LARC visit counts as an initiation. The rate is initiators divided by unique active patients that month (baseline 11.7 percent). Sample ±24 months around November 2016. ** $p < 0.05$.