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# The Short-Term Fertility Impact of Abortion Law Restrictions: A Research Note

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**Abstract:** We examine the short-term fertility effects of Poland's 2020 Constitutional Tribunal (CT) ruling, which declared abortions on the grounds of fetal anomaly unconstitutional. The decision effectively outlawed nearly all legal abortions, as over 97% had been conducted on this ground. Using vital statistics and interrupted time series analysis, we find a significant and immediate decline in births of around 6.6%. The fertility response was strongest among younger women and first-time mothers, suggesting heightened sensitivity to the increased risks of pregnancy. Contrary to expectations, highly educated women did not significantly adjust fertility, likely due to greater access to abortion services abroad. Our findings demonstrate that abortion bans may lower fertility when they substantially increase the perceived costs and risks of childbearing, particularly in societies with widespread contraceptive use. These results provide insights relevant to current debates in the United States, where overturning of Roe v. Wade may also reshape fertility patterns.

**Keywords**: abortion, reproductive rights, fertility, Poland

**JEL codes**: J10, J11, J12, J13

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## 1. Introduction

Many nations have liberalized their abortion laws and increased women's reproductive rights since the 1994 International Conference on Population and Development (ICPD) (Center for Reproductive Rights, 2024). Few countries, including the US and Poland, have recently deviated from this worldwide trend, however. The Dobbs v. Jackson decision of the US Supreme Court in 2022 overturned Roe v. Wade, allowing states to limit abortion, with many states passing nearly complete prohibitions. Similarly, a 2020 Constitutional Tribunal (CT) ruling made abortions due to fetal abnormalities illegal, effectively limiting legal abortions to cases of rape, incest, or threats to the mother's life. These changes sparked massive social protests and reinvigorated scholarly debates about the socioeconomic consequences of restricted access to abortion, including effects on women's autonomy and health as well as well-being of children (Adkins et al., 2024; Fuentes, 2023; Gemmill et al., 2024; Stevenson, 2021).

While much of the literature on abortion restrictions has focused on the United States, this study examines the fertility effects of Poland's 2020 CT ruling. Poland has long exhibited low fertility, with a total fertility rate between 1.3 and 1.4 since the early 2000s, and teenage fertility rates among the lowest in Europe (Sobotka et al., 2015). These patterns coexisted with restrictive abortion laws enacted in 1993, which permitted terminations only in cases of rape, threats to the mother's life or health, or severe fetal anomalies. Although women seeking abortions were not criminalized, physicians performing or assisting abortions faced legal penalties. On October 22, 2020, the CT declared abortions due to fetal defects unconstitutional, effectively eliminating nearly all legal abortion access, as these cases accounted for 97% of terminations between 2017 and 2020 (Rada Ministrów, 2020). The ruling contributed to several maternal deaths linked to delayed medical interventions (Pamula 2023) and triggered widespread protests and intensified scrutiny of abortion enforcement practices.

Abortion restrictions generally tend to increase fertility unless offset by high rates of contraceptive use. Studies from the United States, Uruguay, and Mexico have shown that expanding abortion access reduces fertility, particularly among teenagers (Ananat et al., 2007; Cabella & Velázquez, 2022; Clarke & Mühlrad, 2021; Guldi, 2008; Levine et al., 1999). Similarly, the recent *Dobbs v. Jackson* decision of the U.S. Supreme Court resulted in increases in pregnancies in states that imposed abortion restrictions (Dench et al., 2024), particularly among younger, less-educated, and unmarried individuals, as well as racial minorities (Bell et al., 2025). A fertility increase was also observed in Romania following the 1966 abortion ban, though it was temporary and was followed by a fertility decline as people turned to illegal methods of birth control (Kulczycki et al., 1996).

However, abortion restrictions can also lead to lower fertility if women respond by avoiding pregnancy altogether due to concerns about the risks and costs of childbirth (Levine, 2007). In the United States, both abortions and pregnancies declined following cuts to Medicaid abortion funding (Levine et al., 1996) and vasectomy-related Google searches spiked after the overturning of *Roe v. Wade (Sellke et al., 2022)*. We hypothesize that Poland's 2020 ruling produced a similar effect: fears about carrying pregnancies involving severe fetal anomalies, stillbirth, or life-threatening complications—exacerbated by concerns over delayed or denied medical care—may have prompted many women to avoid pregnancy altogether, contributing to a decline in fertility.

We assess the impact of the CT ruling using an interrupted time series analysis, focusing on births occurring 37 weeks after the decision. Our findings indicate a significant decline in births, exceeding the number of legal abortions performed in previous years. This pattern suggests that many women opted to avoid pregnancy altogether rather than face restricted abortion access. The decline was most pronounced among younger women and those without previous children, consistent with heightened sensitivity to the increased risks associated with pregnancy following the ruling.

## 2. Data and Methods

To examine the fertility effects of the tightening of abortion law, we use data on births from publicly available vital statistics. This database includes daily counts of births by place of birth (urban versus rural location). We complement this with data on the weekly number of births by mothers' age, education level, and birth order, obtained upon request from the Central Statistical Office of Poland[1]. To ensure consistency across all analyses and comparability between data sources, we aggregate all birth records to the weekly level.

We analyze these data using an Interrupted Time Series (ITS) approach (Bernal et al. 2016, McDowall et al. 2019). This method resembles a Regression Discontinuity Design in that it

[1] Weekly data by parity, education and mother's age are not entirely consistent with weekly totals aggregated from daily data for some periods, particularly towards the end and at the beginning of each calendar year. In these cases, we imputed births in each subgroup (education, age, parity) using proportions from weekly data, and total number of births from aggregated daily data.

compares observations immediately before and after an intervention, assuming that conditions would have otherwise remained similar across the cutoff. Consequently, differences in fertility emerging between the pre- and post-ruling periods can be attributed to the change in abortion legislation.

ITS analysis involves two main steps. First, the pre-intervention period is modeled, carefully accounting for both stationary and non-stationary components of the time series. Second, the analysis evaluates whether the intervention produces significant deviations from the counterfactual trajectory—what would have been expected had prior trends continued. In our case, modeling weekly birth counts requires adjusting for strong seasonal patterns. We do so by regressing the number of births in week w and year y on a set of week-of-the-year fixed effects, which capture systematic seasonal variation. The estimated seasonal component represents the average number of births for each week across years. Subtracting this component from observed birth counts yields a deseasonalized series. Because these residuals are centered around zero, we add back the average weekly number of births from the pre-intervention period to restore a meaningful baseline. This adjustment anchors the series at the pre-ruling fertility level, facilitating meaningful comparisons over time.

Having deseasonalized the birth data, we estimate the effects of the CT ruling by fitting time series models where the running variable t indicates the number of weeks since the CT ruling:

$$Y_t^d = \beta_0 + f(t) + \beta_1 After + g(t) * After + \epsilon_t \quad (1)$$

where $Y_t^d$ is the deseasonalized number of births in Poland in week t; After is a binary indicator equal to 1 following the CT ruling, shifted forward by the mean time since conception; and β1 is the parameter of interest, capturing the immediate change in births attributable to the ruling. The functions f(t) and g(t) model the time trends before and after the intervention, respectively. In principle, these trends could differ and involve higher-order polynomials; however, given the relatively short time series, we primarily employ second-order polynomials. Because the data are deseasonalized, β0 represents the average number of births during the pre-ruling period. We estimate this model first for the total number of births, and then separately by maternal age, place of residence, education, and parity.

Applying interrupted time series analysis in our context requires three decisions. First, we had to establish the timing of the treatment. Under Polish law, a Constitutional Tribunal ruling does not take binding effect until published in the Official Bulletin, a process overseen by the Prime Minister's Office. Although publication usually occurs within 14 days, in this case the ruling was not published until January 27, 2021—approximately three months after the initial announcement—likely due to widespread protests. Nevertheless, we treat the announcement date, October 22, 2020, as the intervention point, as both women and medical professionals anticipated the ruling's eventual enforcement and adjusted their behavior accordingly.

Second, we determined the gestational lag between conception and birth. In Poland, the mean gestational age is 275.2 days (approximately 39 weeks and 2 days), measured from the first day of the last menstrual period (LMP) (Kajdy et al., 2024). Since conception typically occurs about two weeks after the LMP, we assume a mean time since conception of 37 weeks. It is, however, possible that some of the births that we ascribe to the treatment group, i.e. those occurring 37 weeks after the initial announcement of the decision, could have been conceived before. Analogously, some births from the control group might be early deliveries from pregnancies conceived after the announcement. Both errors bias our estimates towards zero, independent of whether the new ruling results in fewer or more births.

Finally, we selected the appropriate time frame for analysis. Although birth data are available from the early 2010s, we restrict our sample to the period immediately preceding the ruling. This decision reflects the nature of interrupted time series analysis, which, like regression discontinuity designs, is designed to estimate local treatment effects. Including earlier years could reduce standard errors but risk misrepresenting trends near the cutoff. Moreover, extending the series before 2016 would introduce confounding from the introduction of the Family 500+ cash transfer program, which temporarily boosted fertility rates. To avoid these issues, we limit the analysis to births from 2018 to 2022, and for parity-specific analyses, from 2019 onward (due to data availability).

## 3. Results

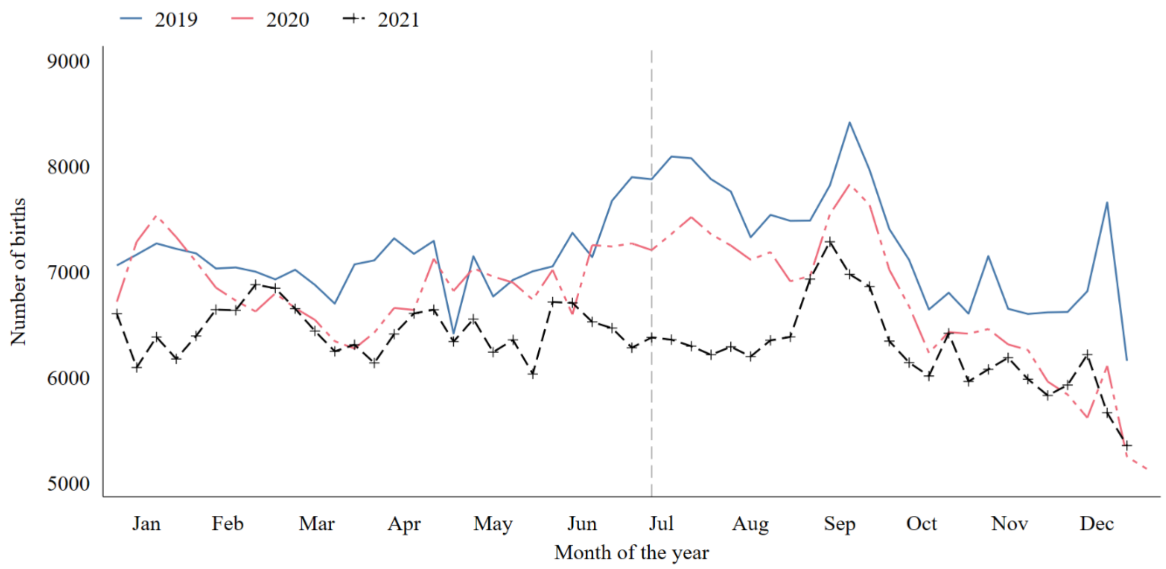
### *3.1 Descriptive analysis*

We begin by presenting descriptive patterns in the raw data. Figure 1 plots the weekly number of births by week of the year. The dashed vertical line indicates the first week in which births could have been affected by the CT ruling.

Several patterns emerge. First, there is a general downward trend in births, with more recent years consistently showing lower levels than earlier years. Second, the data exhibit strong seasonality, with births typically peaking in July, September, and late December or early

January. Third, within these seasonal patterns, births in 2021 deviate notably from prior trends. The July spike, prominent in previous years, is absent in 2021, and the number of births during this period is markedly lower than expected based on earlier patterns. This timing aligns with the first cohort of births conceived after the CT ruling. However, the decline appears temporary: by September, the gap between 2021 and 2020 narrows, and by mid-November, the number of births across the two years converges. Caution is warranted in interpreting late 2020 data as a reference point, however, as births in November and December 2020 would have been conceived during the onset of the COVID-19 pandemic.

**Figure 1.** Weekly number of births, Poland 2019-2021




Source: based on data from Central Statistical Office, retrieved from https://demografia.stat.gov.pl/bazademografia/Tables.aspx

### *3.2 The effect of CT ruling*

Table 1 presents the estimates from the model specified in equation (1). Column 1 shows that the CT ruling resulted in an immediate decline of approximately 220 births per week, corresponding to a 3.4% reduction relative to the average number of weekly births prior to the ruling (the relative effect is reported at the bottom of Table 1). Allowing for greater flexibility in the time trend specification by increasing the polynomial order leads to larger estimated effects. Using a second-order polynomial, the estimated decline increases to 421 births per week, or 6.6% of the pre-ruling average. The estimated effect approximately doubles when a third-order polynomial is employed. However, given the greater risk of overfitting with higher-order polynomials, we select the second-order specification as the preferred model for subsequent analysis.

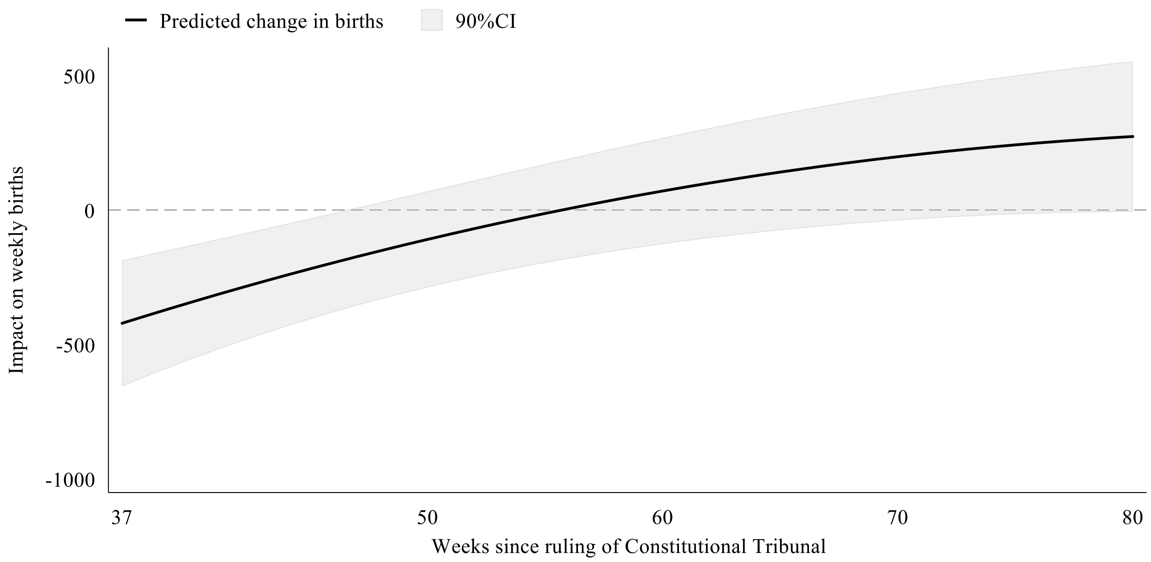
**Table 1:** Consequences of abortion ban on births, estimates of model (1)

| | | | |
|---|---|---|---|
| After=1 | -220.4** | -421.3*** | -861.1*** |
| | (108.2) | (142.5) | (161.6) |
| Weeks since | -6.746*** | -11.67*** | 1.135 |
| | (0.500) | (1.938) | (5.484) |
| Weeks since$^2$ | | -0.0376*** | 0.208** |
| | | (0.0135) | (0.0911) |
| Weeks since$^3$ | | | 0.00125*** |
| | | | (0.000432) |
| After X Weeks since | -0.860 | 27.41*** | 59.73*** |
| | (2.130) | (7.566) | (18.02) |
| After X Weeks since$^2$ | | -0.262*** | -1.944*** |
| | | (0.0927) | (0.543) |
| After X Weeks since$^3$ | | | 0.0110** |
| | | | (0.00468) |
| Constant | 6464.0*** | 6357.4*** | 6494.5*** |
| | (40.63) | (60.16) | (89.82) |
| N | 209 | 209 | 209 |
| $R^2$ | 0.781 | 0.804 | 0.822 |
| Relative effect | -0.034 | -0.066 | -0.133 |
| P-value | 0.041 | 0.003 | 0.000 |
| BIC | 2960 | 2947 | 2938 |

Notes: Heteroskedasticity consistent standard errors in parentheses. Data are from authors' calculations. * p<0.1, ** p<0.05, *** p<0.01. The row "Relative effect" presents the effect as a percentage of average births in the period before the ruling of the CT.

Estimates for the interaction between time and the post-ruling period suggest that the decline was temporary. By the 55$^{th}$ week after the CT ruling, the number of weekly births returned to pre-ruling levels (see Figure 2). Note, however, that the ITS analysis allows only for assessing local effects, shortly after the ruling, which means that we are less able to conclude about the long-term effects which might have been already influenced by other external factors.

**Figure 2.** Projected Deviation in Birth Numbers from the Counterfactual Scenario (no Abortion Ban), 37 Weeks Post-Ruling Onward




### *3.3 Heterogeneity analysis*

We also examined whether the effects of the CT ruling varied by mothers' socio-demographic characteristics, including place of residence (urban vs. rural), age, education, and birth order. Urban, younger, and more educated women—who tend to hold more progressive views and be less aligned with Catholic values—may have responded with stronger fertility declines. However, these groups may also have had better access to abortion abroad due to greater financial and social resources. Age may additionally shape risk perception: while younger women face lower risks of fetal abnormalities, they may also be more willing to challenge restrictive policies. Lastly, women with more children, being typically older and already mothers, may have been especially cautious about continuing pregnancies under the new legal constraints.

With a few exceptions, we find limited heterogeneity in the magnitude of the CT ruling's impact on fertility (Table 2). The decline in births 37 weeks after the ruling was similar across urban and rural areas, amounting to 6.6% in both settings. The impact was also relatively consistent across maternal age groups, although it was slightly larger among mothers under age 25 (8.1%) compared to those aged 25–35 (6.4%) and over 35 (6.6%). Differences by parity were somewhat more pronounced: the number of first births declined by 10%, whereas second births fell by 4.7% and third births by 7.6% relative to the average weekly number of births of the same parity. These patterns suggest that childless women may have chosen to further postpone parenthood in response to the ruling, while women who had already had at least one child were somewhat less deterred.

Finally, we find some differences by mothers' education. The decline in births was stronger among women with low and medium education levels (reductions of 6.5% and 7.7%, respectively) than among those with tertiary education, for whom we do not observe a statistically significant decline. The absence of a significant effect among highly educated women is unexpected. Given that highly educated women in Poland tend to hold more progressive values, are less religious,

**Table 2: Heterogeneity of effects across subpopulations**

| | All births | By mother's place of residence | | By mother's age | | | By mother's birth order | | | By mother's education | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Urban | Rural | Age<25 | 25-35 | Age>35 | Parity 1 | Parity 2 | Parity 3+ | Low | Medium | High | Unknown |
| after=1 | -421.3*** | -245.5*** | -171.7*** | -67.94*** | -265.5*** | -87.83** | -283.0*** | -101.3* | -105.7** | -21.22* | -185.8*** | -113.5 | -100.7*** |
| | (142.5) | (81.77) | (64.77) | (19.89) | (98.80) | (38.08) | (52.52) | (59.52) | (45.26) | (10.95) | (60.87) | (86.45) | (22.21) |
| Weeks since | -11.67*** | -6.134*** | -5.529*** | -2.293*** | -7.603*** | -1.769*** | -4.512*** | -5.708*** | -0.533 | -0.477** | -6.944*** | -4.647*** | 0.403 |
| | (1.938) | (1.202) | (0.839) | (0.343) | (1.291) | (0.578) | (0.795) | (0.844) | (0.568) | (0.210) | (0.860) | (1.343) | (0.814) |
| Weeks since$^2$ | -0.0376*** | -0.0156* | -0.0219*** | -0.00124 | -0.0257*** | -0.0106*** | -0.0220*** | -0.0171*** | 0.00824* | 0.00250 | -0.0190*** | 0.00169 | -0.0227*** |
| | (0.0135) | (0.00835) | (0.00600) | (0.00263) | (0.00903) | (0.00399) | (0.00545) | (0.00588) | (0.00432) | (0.00161) | (0.00626) | (0.00991) | (0.00662) |
| After X Weeks since | 27.41*** | 14.61*** | 12.70*** | 4.740*** | 17.52*** | 5.153*** | 15.01*** | 8.171** | 5.492** | 1.076* | 15.07*** | 5.351 | 5.916*** |
| | (7.566) | (4.376) | (3.428) | (1.293) | (5.138) | (1.902) | (2.964) | (3.285) | (2.354) | (0.617) | (3.355) | (4.479) | (1.107) |
| After X Weeks since$^2$ | -0.262*** | -0.154*** | -0.106** | -0.0504*** | -0.161** | -0.0502** | -0.152*** | -0.0573 | -0.0825*** | -0.0168** | -0.117*** | -0.0826 | -0.0454*** |
| | (0.0927) | (0.0532) | (0.0422) | (0.0172) | (0.0623) | (0.0218) | (0.0384) | (0.0403) | (0.0281) | (0.00770) | (0.0426) | (0.0524) | (0.0112) |
| Constant | 6357.4*** | 3723.4*** | 2609.7*** | 841.4*** | 4179.5*** | 1336.6*** | 2828.4*** | 2150.2*** | 1389.6*** | 328.9*** | 2402.5*** | 3153.7*** | 472.3*** |
| | (60.16) | (37.50) | (24.81) | (9.415) | (39.70) | (18.41) | (24.88) | (25.22) | (15.50) | (5.736) | (25.17) | (38.57) | (17.89) |
| N | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 | 209 |
| R-squared | 0.804 | 0.807 | 0.768 | 0.870 | 0.790 | 0.518 | 0.788 | 0.820 | 0.555 | 0.738 | 0.838 | 0.802 | 0.793 |
| Relative effect | -0.066 | -0.066 | -0.066 | -0.081 | -0.064 | -0.066 | -0.100 | -0.047 | -0.076 | -0.065 | -0.077 | -0.036 | -0.213 |
| P-value | 0.003 | 0.00234 | 0.00746 | 0.001 | 0.007 | 0.019 | 0.000 | 0.086 | 0.019 | 0.049 | 0.002 | 0.186 | 0.000 |

Notes: Heteroskedasticity consistent standard errors in parentheses. Data are from authors' calculations. * p<0.1, ** p<0.05, *** p<0.01. The row "Relative effect" presents the effect as a percentage of average births in the period before the ruling of the CT.

and face higher opportunity costs associated with raising a child with disabilities due to strong labor market attachment, we anticipated a stronger fertility response. One possible explanation is that highly educated women were better able to circumvent the restrictions by seeking abortion services abroad, facilitated by greater financial resources and stronger social networks.

## 4. Robustness checks

We conducted several robustness checks, starting with alternative assumptions about the mean time since conception. While our main specification uses 37 weeks—aligned with Poland's average gestational age—we also tested values from 36 to 40 weeks (Table A1, Appendix). The effect of the CT ruling remains negative and significant for mean times of 36–38 weeks but diminishes and becomes insignificant at 39 weeks. Notably, this corresponds to a gestational age of 41 weeks, which is already one standard deviation above the national average and thus is relatively unlikely.

Next, we assessed whether the observed decline in births is not due to the pandemic rather than the CT ruling. To this end, we extend equation (1) with a variable that captures the number of new Covid cases in a given week lagged by 37 retrieved from John Hopkins Coronavirus Resource Center (Dong et al., 2022). We further include different lags of the number of cases, to account for uncertainty on the time of contagion. The inclusion of this additional variable leads to a negligible decline in point estimates (up to 5 of the initial coefficient, or around 23 births per week, Table A2 in the Appendix). In all cases, the effect of the CT ruling decision remains statistically significant and negative.

Finally, we experiment with different windows around the event. By selecting narrower windows, estimates better reflect changes around the cutoff, but these estimates lose precision. Table A3 in the Appendix shows that focusing on observations around the event results in larger estimates. In almost all cases, point estimates fall within a 95% confidence interval from our baseline specification. The only exception is when attention is narrowed to 30 weeks on both sides of the cutoff. In spite of larger confidence intervals, all coefficients remain statistically significant.

## 5. Conclusions

This study investigates the impact of Poland's 2020 Constitutional Tribunal (CT) ruling, which banned abortions on the grounds of fetal defects, on fertility. Using an interrupted time series analysis, we document a sharp and statistically significant decline in the number of births, emerging approximately 37 weeks after the announcement of the ruling. The estimated reduction—around 6.6% of the average weekly number of births—exceeded the number of legal abortions performed in prior years, suggesting that the ruling did not merely alter abortion practices but led many women to avoid pregnancy altogether. This highlights an important mechanism: in contexts with widespread contraceptive use and severe reproductive constraints, abortion bans may lower fertility by increasing the perceived physical, emotional, and legal risks associated with pregnancy.

Subgroup analyses reveal some notable heterogeneity. The decline was more pronounced among younger women and first-time mothers, suggesting that individuals facing higher opportunity costs or heightened uncertainty about childbearing were more sensitive to the increased legal risks. Surprisingly, no significant fertility decline was observed among highly educated women. Although this group tends to hold more progressive views and might be expected to respond most strongly to reproductive rights violations, their greater financial means and access to abortion services abroad may have buffered them from the immediate consequences of the ruling.

Robustness checks confirm the stability of our main results across different assumptions about gestational age and after adjusting for COVID-19 infection rates. However, a key limitation of the interrupted time series design is its local focus: it captures only short-term effects and cannot assess longer-term demographic consequences. Although our findings suggest that the fertility decline was temporary— with the number of weekly births returning to pre-ruling levels within approximately 20 weeks—the lasting impacts of the ruling cannot be evaluated within this framework. Events such as the deaths of pregnant women due to complications from fetal anomalies (Pamula, 2023), may have further heightened public awareness of the risks associated with the new legal environment. Future research using longer time series data and alternative identification strategies will be necessary to fully assess the sustained impact of abortion restrictions on fertility and reproductive behavior in Poland.

Overall, our findings challenge the conventional wisdom that abortion bans uniformly raise fertility. Instead, they demonstrate that in highly contraceptive societies, restrictions that amplify the perceived costs and risks of pregnancy may paradoxically lead to short-term declines in fertility by discouraging conception itself.

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## Appendix

**Table A1** Robustness check: Alternative assumption about the mean time to conception

| | 36 weeks | 37 weeks | 38 weeks | 39 weeks | 40 weeks |
|---|---|---|---|---|---|
| after=1 | -508.4*** | -421.3*** | -357.9** | -246.7 | -123.8 |
| | (134.4) | (142.5) | (151.4) | (152.5) | (143.7) |
| Weeks since | -10.46*** | -11.67*** | -12.53*** | -13.78*** | -15.02*** |
| | (1.804) | (1.938) | (1.918) | (1.997) | (2.051) |
| Weeks since^2 | -0.0303** | -0.0376*** | -0.0425*** | -0.0499*** | -0.0571*** |
| | (0.0129) | (0.0135) | (0.0132) | (0.0135) | (0.0136) |
| After X Weeks since | 27.99*** | 27.41*** | 27.10*** | 25.28*** | 22.69*** |
| | (7.191) | (7.566) | (8.080) | (8.163) | (7.749) |
| After X Weeks since^2 | -0.282*** | -0.262*** | -0.250** | -0.215** | -0.170* |
| | (0.0882) | (0.0927) | (0.0990) | (0.101) | (0.0986) |
| Constant | 6402.9*** | 6357.4*** | 6322.1*** | 6274.4*** | 6226.1*** |
| | (54.40) | (60.16) | (60.42) | (64.45) | (67.08) |
| N | 209 | 209 | 209 | 209 | 209 |
| R^2 | 0.807 | 0.804 | 0.803 | 0.804 | 0.805 |
| Relative effect | -0.079 | -0.066 | -0.057 | -0.039 | -0.020 |
| P-value | 0.000 | 0.003 | 0.017 | 0.103 | 0.387 |
| BIC | 2945 | 2947 | 2948 | 2945 | 2942 |

Notes: Heteroskedasticity consistent standard errors in parentheses. Data are from authors' calculations. * p<0.1, ** p<0.05, *** p<0.01

**Table A2:** Robustness check: accounting for Covid cases

| | | (Log) Covid -19 cases lagged by *t* periods | | | |
|---|---|---|---|---|---|
| | Base | 36 | 37 | 38 | 39 |
| after=1 | -421.3*** | -410.2*** | -403.4*** | -396.6*** | -398.5** |
| | (142.5) | (146.6) | (149.2) | (152.1) | (155.8) |
| Weeks since | -11.67*** | -9.463** | -9.227** | -9.050** | -9.651** |
| | (1.938) | (4.074) | (3.970) | (3.868) | (3.831) |
| Weeks since^2 | -0.0376*** | -0.0254 | -0.0240 | -0.0230 | -0.0263 |
| | (0.0135) | (0.0246) | (0.0242) | (0.0237) | (0.0236) |
| After X Weeks since | 27.41*** | 23.84** | 23.28** | 22.82** | 23.78** |
| | (7.566) | (10.53) | (10.57) | (10.55) | (10.73) |
| After X Weeks since^2 | -0.262*** | -0.257*** | -0.254*** | -0.251** | -0.252** |
| | (0.0927) | (0.0958) | (0.0967) | (0.0972) | (0.0984) |
| Covid cases | | -8.281 | -9.388 | -10.31 | -8.118 |
| | | (14.01) | (13.90) | (13.77) | (13.83) |
| N | 209 | 209 | 209 | 209 | 209 |
| R^2 | 0.804 | 0.805 | 0.805 | 0.805 | 0.805 |

Notes: Heteroskedasticity consistent standard errors in parentheses. Data are from authors' calculations. * p<0.1, ** p<0.05, *** p<0.01.

**Table A3:** Estimations from trimming the sample around the event

| | | Observation within … weeks from the event | | | |
|---|---|---|---|---|---|
| | Base | 76 | 50 | 30 | 10 |
| after=1 | -421.3*** | -516.7*** | -564.5*** | -922.9*** | -396.4* |
| | (114.0) | (138.0) | (172.8) | (203.6) | (210.2) |
| Weeks since | -11.67*** | -5.712 | -7.017 | -65.91*** | -149.0** |
| | (2.378) | (5.758) | (10.79) | (20.52) | (53.17) |
| Weeks since^2 | -0.0376** | 0.0183 | -0.0220 | -2.180*** | -10.54* |
| | (0.0176) | (0.0733) | (0.209) | (0.661) | (5.121) |
| After X Weeks since | 27.41*** | 21.88*** | 25.89 | 201.1*** | 65.73 |
| | (5.939) | (8.329) | (15.79) | (30.77) | (90.86) |
| After X Weeks since^2 | -0.262*** | -0.324*** | -0.260 | -1.810* | 28.85*** |
| | (0.0699) | (0.105) | (0.303) | (0.976) | (8.297) |
| Constant | 6357.4*** | 6448.7*** | 6443.7*** | 6200.7*** | 6084.8*** |
| | (67.41) | (94.67) | (116.6) | (133.0) | (114.3) |
| N | 209 | 153 | 101 | 61 | 21 |
| R^2 | 0.804 | 0.692 | 0.455 | 0.601 | 0.901 |

Notes: Heteroskedasticity consistent standard errors in parentheses. Data are from authors' calculations. * p<0.1, ** p<0.05, *** p<0.01.

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