The Long-Term Impact of Restricted Access to Abortion on Children's Socioeconomic Outcomes

Gábor Hajdu[a], Tamás Hajdu[b]

[a] Institute for Sociology, Centre for Social Sciences, Hungary (hajdu.gabor@tk.mta.hu)
[b] Institute of Economics, Research Centre for Economic and Regional Studies, Hungary (hajdu.tamas@krtk.mta.hu)

Extended abstract

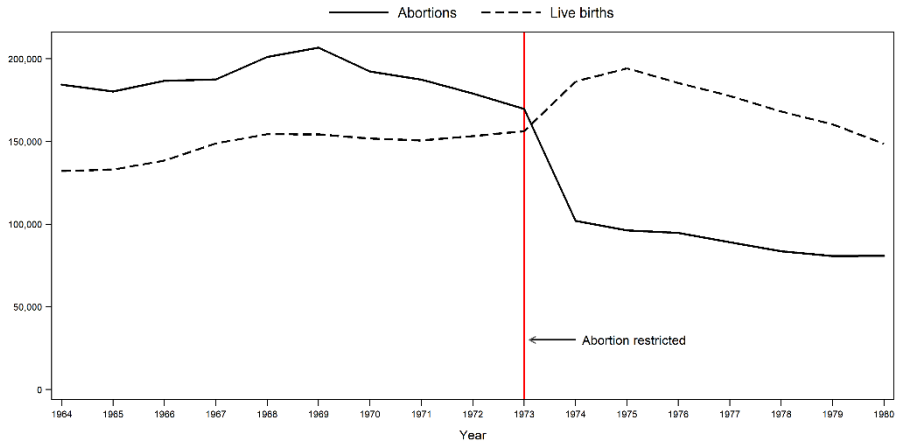
In this research, we examine the long-term consequences of the restrictive Hungarian abortion policy introduced in 1974. We analyze the long-term effects of the restrictive abortion policy on the affected children's socioeconomic outcomes (educational attainment, labor market participation, teen fertility).

Background

On January 1, 1974, new abortion rules were introduced in Hungary. They were justified as intended to protect women's health, but the implicit goal was to reduce the number of abortions and to increase fertility(*1*). The new regime allowed access to legal abortion only for targeted groups. The exceptions to the ban were unmarried women, those who had at least three children, those who were at least 35 years old, had serious housing problems or lived in poverty, and cases when pregnancy would cause serious health hazards for the mother (*1–3*).

The law change had immediate and substantial impacts. Between 1973 and 1974, the number of induced abortions decreased from 169 650 to 102 022 (Figure 1). At the same time, the number of live births increased by 30 000: from 156 224 to 186 288. These changes mean that the number of induced abortions per 100 live births decreased by 50 percent: from 108.6 to 54.8.

Figure 1: Number of induced abortions and live births between 1964 and 1980



Source: Hungarian Central Statistical Office (http://www.ksh.hu/docs/eng/xstadat/xstadat_long/h_wdsd001a.html and http://www.ksh.hu/docs/eng/xstadat/xstadat_long/h_wdsd001b.html)

Mechanisms

There are three main mechanisms through which a restrictive abortion policy might affect the outcomes of children (*4*, *5*). Since restrictive abortion laws make abortion less easily available,

women have three options: using other birth control technologies, remaining abstinent or giving more births. Therefore, reduced access to abortion increases the number of unplanned, mistimed, or unwanted children, and this unwantedness might have negative impacts on the child (*unwantedness effect)*. The composition of women carrying pregnancies to term might also be different after changes in abortion policy *(composition effect)*. The direction of this effect is ambiguous both theoretically and empirically. A negative *crowding effect* might emerge due to changes in cohort size. A larger cohort shares the same resources, which might affect negatively the outcomes of children.

Methods

We use matched large-scale, individual-level administrative datasets of the Hungarian Central Statistical Office (Population census of 2011 and live birth register). We use eight outcome variables. First, educational achievement is measured by (1) having a university degree, (2) having only primary education, (3) years of education completed. Second, labour market activity is measured by (4) being not in employment (by ILO definition), (5) working (self-categorization), and (6) being unemployed (self-categorization). We also study (7) whether the child became a teen parent, and (8) whether she/he or her/his family is the owner of the dwelling where she/he lives.

We estimate the effects of the law change by comparing children born just before and after the new law. In this way, we are able to rule out the effect of other (unobserved) time trends and other potential behavioral responses to the law change, and we can draw causal inference. We compare children born between July and September 1974 to children born between April and June 1974.

We also utilize the fact that the new rules permitted abortion to women who were at least 35 years old. We use these women's newborns as control groups and create treatment groups as similar as possible to these groups. The treatment and control groups are the following: women who were just below 35 years at the conception vs. women above 35 years at the conception. We use a ±1.5-year time range and we exclude women of around 35 years as there is no information about the exact decision process of the abortion committees, and we do not know how they treated the abortion requests of women around this age limit. Using these treatment and control groups, we can apply a difference-in-differences framework to estimate the impact of the abortion restriction.

We estimate the following equation:

$$Y_i = \beta_0 + \beta_1 Below35_i + \beta_2 After_i + \beta_3 Below35_i \times After_i + \beta_4 X_i + \varepsilon_i \quad (1)$$

where Y_i is an outcome of interest for child *i*, $Below35_i$ is a dummy that takes the value of 1 if the child is born to a mother who were below 35 years at the conception, and 0 if the child is born to a mother who were above 35 years at the conception. $After_i$ is a dummy that takes the value of 1 if the child is born between July and September 1974, and 0 if the child is born between April and June 1974. X_i is a vector of control variables that includes the newborn's sex, characteristics of the mother (in 1974), characteristics of the father (in 1974), and interaction terms for some of the parents' characteristics. Although the composition of women carrying pregnancies to term might be different after changes in the abortion policy, with the rich set of control variables we can control for a substantial part of this composition effect.

Since with the difference-in-difference framework, the crowding effect is less of a concern, this empirical strategy is assumed to estimate the unwantedness effect. The key coefficient is β_3 that captures the unwantedness effect.

Results

Table 1 shows the results from estimating Equation (1). Each row shows the result for different outcome variables. Regarding educational outcomes (Row 1-3), there is a negative effect. Children born after the law change in the treatment group are less likely to have a university degree in 2011, they have a higher probability of finishing only primary school, and they completed 0.7 less school years. The results also suggest a negative effect on the labour market outcomes (Row 4-6). Children born after the law change are more likely to be not in employment and to classify themselves as unemployed. Finally, we see a sizable increase in the probability of having a child before age 18 (Row 7) and a decrease in the probability of being the owner of the dwelling (Row 8).

Table 1: The effect of the abortion restriction on socioeconomic outcomes

	Below35 × After (β_3)	Robust SE	p	N
(1) University degree	-0.046	0.025	0.066	1124
(2) Primary education	0.112	0.055	0.041	1124
(3) Years of education completed	-0.699	0.330	0.034	1124
(4) Not in employment (ILO)	0.104	0.058	0.074	1124
(5) Working	-0.074	0.059	0.210	1124
(6) Unemployed	0.077	0.042	0.069	1124
(7) Teen parent	0.060	0.029	0.042	1124
(8) Owner of the dwelling	-0.090	0.043	0.034	1124

Robustness of the results

First, we estimate a triple difference model by using data of children born in 1973. In this way, we can control for seasonal differences that might affect the treatment and the control groups differently.

We estimate the following equation:

$$Y_i = \beta_0 + \beta_1 Below35_i + \beta_2 After_i + \beta_3 Y74_i + \beta_4 Below35_i \times After_i + \\ + \beta_5 Below35_i \times Y74_i + \beta_6 After_i \times Y74_i + \beta_7 Below35_i \times After_i \times Y74_i + \beta_8 X_i + \varepsilon_i \quad (2)$$

where Y_i and $Below35_i$ to Equation (1). $After_i$ is a dummy that takes the value of 1 if the child is born between July and September, and 0 if the child is born between April and June. $Y74_i$ is a dummy that takes the value of 1 if the child is born in 1974, and 0 if the child is born in 1973. In this specification, β_7 captures the unwantedness effect.

Table 2 shows the results. In general, the size of the estimated coefficients are similar to the main results in Table 1, but coefficients on the labour market outcomes lost their significance.

To verify that the results are not due to coincidence or model misspecification, two additional placebo tests are performed, using placebo treatment groups and placebo law changes. First, treatment and control groups are changed to mothers who were identically affected by the restricted abortion rules. We compare mothers below 32 and mothers over 32 years. No sizable

impacts can be observed in these estimations, which suggests that the main models capture the effect of the change in the law.

Table 2: The effect of the abortion restriction on socioeconomic outcomes, triple differences

	Below35 × After × Y74 (β_7)	Robust SE	p	N
(1) University degree	-0.088	0.039	0.024	2150
(2) Primary education	0.143	0.073	0.052	2150
(3) Years of education completed	-0.844	0.453	0.063	2150
(4) Not in employment (ILO)	0.084	0.081	0.296	2150
(5) Working	-0.047	0.082	0.565	2150
(6) Unemployed	0.094	0.060	0.117	2150
(7) Teen parent	0.084	0.038	0.028	2150
(8) Owner of the dwelling	-0.132	0.055	0.017	2150

Next, in order to check that the estimated impacts do not merely reflect a general trend in these years, a placebo reform test is performed. We use data from other years between 1971 and 1979, and we assume that the new law was introduced one or more years before or one or more years later than 1974. We estimate the effect of placebo law changes in these years, applying an empirical approach identical to what was used before. We expect to see insignificant coefficients for the years before and after 1974. For every year, we count the number of significant coefficients with the expected sign. In the benchmark year of 1974, seven coefficients are significant at the 10% level (Table 3). In other years, the coefficients are hardly significant, which confirms that the baseline results are not driven by any general trend in the outcomes or by standard seasonal differences.

Table 3: The result of the placebo law changes

	1971	1972	1973	1974	1975	1976	1977	1978	1979
Number of significant coefficients	1	0	0	7	0	0	1	0	0

Lastly, we change the treatment period. The pre-change and the post-change periods are extended to 6 months (July-December vs. January-June) and to 12 months (July 1974-June 1975 vs. July 1973-June 1974). In these estimations, the coefficients are smaller and lose their significance, which suggests that some quick adaptation occurred in 1974. This adaptation process might include increased use of available legal birth control technologies (*6*) or resorting to an illegal or semi-illegal abortion (*7, 8*). It is also possible that women became familiar with the decision process of the abortion committees, and were able to argue convincingly (*1*, *9*).

Conclusion

These results suggest that the restrictive Hungarian abortion policy introduced in 1974 had a negative long-term impact on the socioeconomic outcomes of the affected children. Significant changes in abortion laws are rare, and the effects of the restrictions in abortion legislation are even more rarely analyzed, hence these results give important information about the consequences of access to abortion and, in a wider perspective, to family planning. Since abortion policy is still an emerging issue in many countries' public debates, these results could provide significant information for an evidence-based policy.

Bibliography

1. S. Gal, Gender in the Post-socialist Transition: the Abortion Debate in Hungary. *East Eur. Polit. Soc.* **8**, 256–286 (1994).
2. L. Haney, *Inventing the Needy: Gender and the Politics of Welfare in Hungary* (University of California Press, Berkeley, 2002).
3. J. Sándor, From Ministry Orders towards Constitutional Debate: Lessons Drawn from the Past 50 Years of Abortion Laws in Hungary. *Med. Law*. **18**, 389–408 (1999).
4. A. Mitrut, F.-C. Wolff, The impact of legalized abortion on child health outcomes and abandonment. Evidence from Romania. *J. Health Econ.* **30**, 1219–1231 (2011).
5. C. Pop-Eleches, The Impact of an Abortion Ban on Socioeconomic Outcomes of Children: Evidence from Romania. *J. Polit. Econ.* **114**, 744–773 (2006).
6. Z. Makay, Contraceptive use in Hungary: Past trends and actual behaviour. *Demogr. Engl. Ed.* **58**, 65–90 (2016).
7. Z. Bognár, A. Czeizel, Mortality and morbidity associated with legal abortions in Hungary, 1960-1973. *Am. J. Public Health*. **66**, 568–575 (1976).
8. E. Czeizel, A terhességmegszakítások orvosi javallat alapján történő engedélyezéseinek az elemzése. *Orv. Hetil.* **124**, 1297–1302 (1983).
9. J. Szalai, Abortion in Hungary. *Fem. Rev.* **29**, 98–100 (1988).