



The impact of abortion on crime and crime-related behavior[☆]

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ABSTRACT

The 1966 abolition and 1989 legalization of abortion in Romania immediately doubled and decreased by about a third the number of births per month, respectively. Comparing birth month cohorts born on either side of the reform cut-offs allows us to cleanly identify the effect of abortion access on crime. For both the abolition and legalization of abortion, we find large and significant effects on the level of crime and risky-behavior related hospitalization, but insignificant effects on crime and hospitalization rates. We conclude with a discussion of what our results say about the mechanisms underlying the crime effects of abortion policy.

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1. Introduction

Abortion policy is actively debated in both the United States and around the world today. During the President Trump mandate, many US state legislators approved bills that made abortion policies more restrictive.¹ This contrasts many other countries: a 2018 Guttmacher report indicates that 27 countries liberalized their abortion laws since 2000.² Most of this debate focuses on the moral, religious, and constitutional legality of abortion, and pays little attention to the potential societal consequences of abortion laws. This paper contributes to this debate by studying the impact of abortion laws in Romania on one important societal outcome: crime and crime-related behaviors.³

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¹ See <https://www.vox.com/2018/3/22/17143454/trump-ohio-heartbeat-bill-abortion-ban-mississippi>, last accessed November 30, 2018, for a summary of recent US restrictions.

² See <https://www.guttmacher.org/report/abortion-worldwide-2017>, last accessed November 30, 2018.

³ There is a larger literature looking at the effects of abortion policies on other socio-economic outcomes of children, such as poverty, education, health or fertility. For the US, see Gruber et al. (1999), Charles and Stephens (2006), Joyce (1987) and Grossman and Joyce (1990). For Romania, see Pop-Eleches (2006), Mitrut and Wolff (2011) and Malamud et al. (2016).

Romania abruptly abolished abortion in October 1966 following the orders of the communist dictator Ceausescu and legalized it again in December 1989, a day after Ceausescu was killed. These events provide a unique opportunity to study the effect of abortion on crime. First, given that abortion was the main form of birth control in Romania, the magnitudes of the shocks are large: births *doubled* in the year after the 1966 ban and decreased by about a third after the 1989 legalization. These Romanian shocks are substantially larger than the estimated 4%-11% decrease in U.S. fertility rates (Levine et al., 1999) due to legalization. Second, the external validity of our analysis is increased by studying *both* the abolition and legalization of abortion; we reach the same conclusions despite the reforms occurring in two periods with different socio-economic-political environments and the fact that the fertility effect of the abolition was three times that of the legalization. Third, national administrative prison and hospital registers, which include individual month of birth, allow for a clean identification strategy that relies on the comparison of birth month cohorts on either side of the abortion regime. Since birth months on only one side of the cut-off are exposed to legalization, but cohorts on both sides are exposed to similar crowding effects resulting from changes in cohort sizes from these policies, we can isolate the link between abortion access and crime abstracting from such general equilibrium effects or any other common shocks that affect children born within the same age cohort.

The abortion-crime literature dates back to Donohue and Levitt's (2001) well known paper that puts forward the legalization of abortion as an explanation for up to 50% of the drop in U.S. crimes in the early 1990 s. They hypothesize that this is consistent with the timing of Roe v. Wade in 1973, which led to a substantial

increase in abortions – especially among poor, unmarried mothers, whose children would have been at an increased risk of crime. [Donohue and Levitt \(2001\)](#) find that crime decreased 15–25 years later, as these more ‘wanted’ cohorts reached the peak crime ages. Moreover, [Donohue and Levitt’s \(2019\)](#) recent evaluation using almost twenty additional years of data (1998–2014) re-affirms the predictions from their original work – namely that there would be a further decline of about 20% in US crime.

A number of channels could theoretically underlie the abortion legalization–crime relationship. The most straightforward channel is mechanical: a substantial reduction in cohort size – as a result of legalization – would imply lower crime levels. If this would be the only relevant channel, then crime rates should not change. However, one would also expect a decrease in the number of unwanted or unplanned children that would be accompanied by an increase in child quality ([Becker, 1981](#)). Moreover, legalization could help optimize the timing of childbearing (especially when access to birth control is difficult). In turn, mother’s education and labor market outcomes may improve, and have positive spillovers onto children ([Angrist and Evans, 1996](#)). Involuntary parenthood could also impact less tangible mother outcomes, e.g. physical and mental well-being, which could influence the development of the child. One would expect this channel to affect not only the number of crimes but also the crime rate, given the relationship between unwantedness and crime in the literature.⁴ A third potential channel is compositional changes in the socio-economic characteristics of women giving birth. The direction of this effect is theoretically ambiguous, and depends on who the marginal users of abortion are. All of these potential channels – cohort size, unwantedness, and compositional changes – can affect crime levels, while only unwantedness and compositional effects could potentially impact crime rates. Both theoretically and empirically, it is extremely difficult to clearly differentiate between the unwantedness and the compositional effects, and sometimes these two channels are referred to together as the selection channel ([Foote and Goetz, 2008](#)). Finally, beside the main channels mentioned above, general equilibrium effects (GE) of abortion reforms could also potentially occur at either the societal level (e.g. with respect to the resources of the criminal justice system or the number of children in a school classroom), or within household (e.g. upon abortion legalization, the first rank child is less likely to have younger siblings).⁵

[Donohue and Levitt’s \(2001\)](#) findings have been questioned in the literature on multiple grounds, including: (i) not accounting for unmeasured time effects like changes in crack cocaine ([Joyce, 2004](#)), (ii) specification sensitivity, especially with respect to within-state variation ([Foote and Goetz, 2008](#)), and (iii) the need for per capita crime data to assess whether there is a selection effect ([Foote and Goetz, 2008](#)). [Foote and Goetz \(2008\)](#) argue that when using such data, there is no evidence of a selection effect of abortion on crime. [Foote and Goetz \(2008\)](#) also highlight that using an abortion policy indicator rather than the abortion rate can help eliminate a mechanical negative relationship that can exist between the abortion rate (where the denominator is the number of births) and the number of arrests when there is no

selection or cohort size effects. [Donohue and Levitt’s \(2008\)](#) response highlights yet another challenge – measuring the extent to which an annual birth cohort is exposed to an abortion policy.⁶

The current paper sheds light on this important and controversial question using two national reforms in Romania, which despite the size and sharpness of the shocks, have not been directly used in studying the relationship between abortion and crime.⁷

Our research design is carefully informed by the academic debate. Specifically, we use individual register data containing month of birth, which allows for the explicit and clean measurement of abortion policy exposure (our main independent variable) around the 1966 abortion ban and the 1989 abortion legalization. We consider both levels and rates in our outcome variables. Our main crime measure for the two reforms is convictions resulting in a prison sentence. For cohorts around the 1989 reform, we can also expand the definition of conviction to include individuals held in preventative detention (and not sentenced to incarceration). Moreover, given that prison sentences and preventative detention represent relatively serious offenses, we complement our analysis with hospital admission data for mental health disorders (including mood disorders such as depression and substance – alcohol, drugs, other – related disorders) and risky behavior (such as transport related accidents or toxic effects of substances like drugs). The advantage of these data is that they measure behaviors related to crime but do not have the same age-crime profile;⁸ this is especially relevant since our conviction data does not measure crime at peak crime ages for cohorts around the 1966 reform.

Using data aggregated to the birth month cohort level, our baseline specification estimates the effect of being exposed to the 1989 and 1966 reforms (i.e. born after June 1990 and May 1967, respectively) when controlling for month of birth dummies (seasonality) and a time trend in month of birth. The latter captures any other national trends that may affect crime rates, even if they are unrelated to fertility behavior. We abstract away from GE related effects by using a short window of time around the abortion cut-off points (i.e. 18 months on either side). We also demonstrate the robustness of our findings to: (i) shortening or expanding this window, (ii) using a county by month of birth cohort panel, which leverages variation in the fertility impact of the reforms across counties, and (iii) estimating the results separately for each county, with various sized shocks to fertility.

Consistent with the large decrease (increase) in the number of births with the unexpected 1989 legalization (1966 ban) of abortion, we observe large and significant decreases (increases) in the level of each measure of crime (total, property, and violent) and its risk factors (hospitalization for mental health disorders and risky behavior, like drug abuse). But, all of these effects disappear when normalizing by the size of the birth month cohort. The crime rate does not change. This pattern of results is readily visible from both simple graphs and the specifications described above. Finally, the results are robust to controlling for observable cohort compositional characteristics.

⁷ The effect of the 1966 Romanian abortion ban on crime has been briefly addressed previously by [Pop-Eleches \(2006\)](#), though the emphasis of this work was actually on children’s socioeconomic outcomes, like education and labor market success. Using a similar research design to ours (normalizing by the size of the birth cohort), [Pop-Eleches \(2006\)](#) finds suggestive evidence that crime rates increased for cohorts born after the ban. The robustness and generalizability of these findings, however, are limited by: (i) only being able to measure crime using about 2,000 penal cases for the regional courts of one single county (out of 42), (ii) not having birth month but only year of birth, and (iii) not being robust to the inclusion of age-specific trends. These limitations – as well as the lack of study of the 1989 reform effect – emphasize the need and importance of the current paper.

⁸ This is consistent with a large literature that documenting a strong correlation between physical/mental health and crime. See [Piquero et al. \(2014\)](#) for a review. There is also a small but growing literature documenting a causal relationship between health care access and crime; see [Doleac \(2018\)](#) for a review.

⁴ [Doyle \(2008\)](#) finds that foster care in Illinois increased the arrest rate of the marginal child by 200–300%.

⁵ There is a clear link between child delinquency outcomes and the birth order across siblings. For example a recent paper by [Breining et al., \(2020\)](#) show that in families with two or more children, second-born boys are more likely to be disciplined in school and enter the criminal justice system compared to first-born boys.

⁶ [Donohue and Levitt \(2004 and 2008\)](#) further demonstrate the robustness of the results. For other abortion–crime papers, see [Kahane et al. \(2008\)](#) for the UK, [Sen \(2007\)](#) for Canada and [Pop-Eleches \(2006\)](#) for Romania. See also [Joyce’s \(2010\)](#) review.

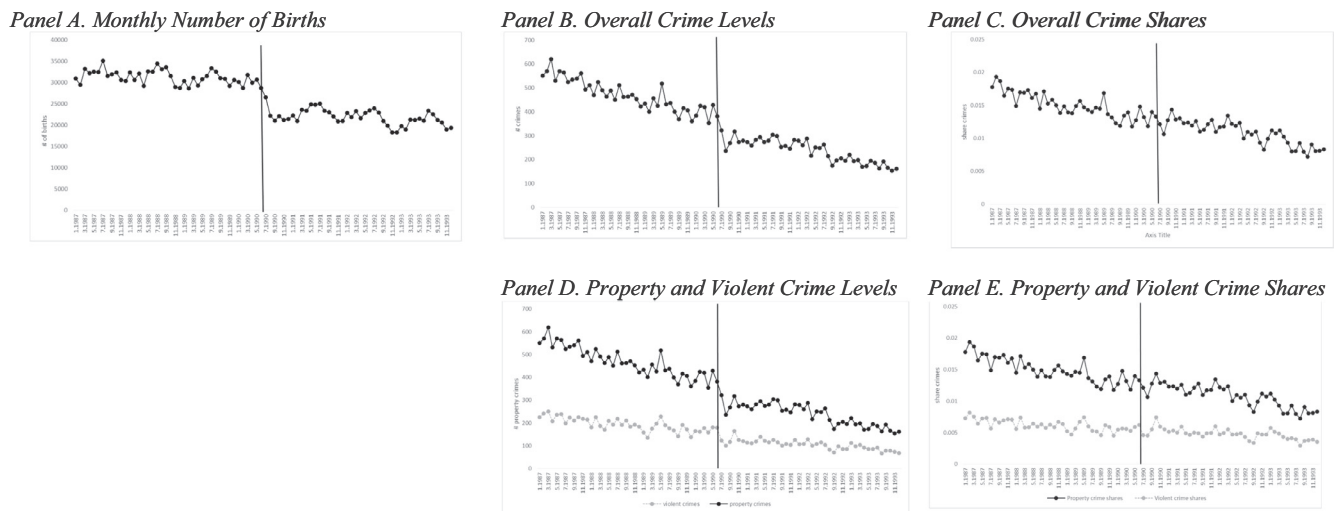


Fig. 1. The 1989 Legalization of Abortion: Number of Births, Crime Levels and Crime Shares by Crime Type. Note: This figure shows the effect of the 1989 legalization of abortion on the monthly # of births (Panel A), # of convictions - overall (Panel B), share of convicted individuals overall (Panel C), and # and share of convictions by crime type (Panels D and E). The solid black line is for property offenses and the dashed grey line for violent crimes. The Y-axis indicates the # or the share convicted, while the X-axis indicates the month and year of birth for the individuals born between January 1987 and December 1993 and observed in the crime registers from 1997 to September 2017. Data sources: Statistics Romania (birth data) and the National Penitentiary Administration (crime registers).

How should these results – significant changes in levels but not in rates – be interpreted? As highlighted earlier, cohort size effects would impact levels, while unwantedness and composition effects should affect rates. Can the null effect on rates be interpreted as evidence of no unwantedness or compositional changes effects of abortion reform? As in the previous empirical literature on abortion (see e.g., Donohue and Levitt, 2001; Joyce, 2004; Foote and Goetz, 2008), which has not conclusively been able to disentangle the compositional and unwantedness channels, this is not an easy question to answer empirically.⁹ The answer depends largely on the extent and direction of the compositional effects: we examine this in the current context directly in Section 4.3. We provide multiple pieces of evidence that (i) the 1966 abortion ban resulted in positive compositional effects among the mothers giving birth - consistent with the conclusions of Pop-Eleches (2006) - which could theoretically offset unwantedness effects, but that (ii) the 1989 legalization had minimal to no observable compositional effects. The implications of these findings are clearer for the 1989 reform than for 1966. In particular, a null effect of the 1989 abortion legalization reform can be interpreted as a lack of an unwantedness (and compositional changes) effect on crime rates. As further support of this conclusion, we demonstrate that the rate of child institutionalization, i.e. parents placing their children in state institutions, which was an extreme measure of unwantedness, did not change around the 1989 reform.

2. Institutional background and the abortion reforms

Romania experienced two large and unexpected shifts in abortion availability: an abortion ban in 1966, followed in 1989 by the legalization of abortion because of the fall of Communism. In this section, we describe these reforms and the institutional background surrounding them.

Before 1966, Romania had one of the most liberal abortion policies in the world, with abortions being provided at no cost at state medical institutions. In October 1966, the communist government

issued Decree 770, which abruptly shifted to a restrictive and conservative policy regime that made abortion and family planning, e.g., contraceptives, illegal as of December 1, 1966. The 1966 decree stipulated that abortion was allowed only for: women who already had four children, women over age 45, women whose lives were jeopardized by the pregnancy, and pregnancies that resulted from rape or incest. Fertility rates in Romania (see Appendix Fig. 1) instantly increased from 1.9 to 3.9 children per woman in one year. This sharp increase was followed by a steady decrease until 1985, largely because of massive increases in illegal abortions (Kligman, 1998). In 1985, Ceausescu reinforced the 1966 law by increasing the number of children needed to be eligible for a legal abortion and compelled women to monthly gynecological exams at their workplaces (Greenwell, 2003). Moreover, throughout the eighties, the conditions of everyday life deteriorated dramatically in Romania (little food, less heat and electricity), which, together with the abortion ban and lack of any family planning lead to a new phenomenon: child abandonment. Children (especially newborns) were left in state-run institutions (orphanages) by parents with social or economic problems, either as a temporary measure or in the form of abandonment (Kligman, 1998). Even though the backgrounds of these children are poorly documented, we will be able to check whether there are significant changes in both levels and rates of institutionalized children around the 1989 reform cut-off point.¹⁰

Abortion remained illegal until 1989. A day after the execution of Ceausescu and his wife on the 25th of December, which marked the end of the Communist regime, the new provisional government abolished the abortion ban. The consequences of this immediate and unexpected lifting of the ban shows the gravity of the lack of

⁹ Separating unwantedness and compositional effects is particularly difficult both empirically and theoretically given that (i) unwantedness itself is not easy to empirically or theoretically quantify and define, and (ii) unwantedness and composition are likely to be related in observable and unobservable dimensions.

¹⁰ This analysis will complement the findings in Mitrut and Wolff (2011) who show some marginally significant drops in the number and the share (in total births) of the abandoned children born around the December 1989 cut-off point. We do not have data to look for changes around the 1966 cut-off point. Moreover, it is worth clarifying that, because prior to 1997 the existing laws did not address child abandonment, these children were not considered to be in difficulty, but rather that “their needs were being guaranteed by the state”. Healthy children, but also children with deficiencies (such as mental problems, dystrophies, or deafness), were placed by their families in state institutions. Since child institutionalization became such a widespread phenomenon in the 1980s in Romania, it continued to be common practice after December 1989.

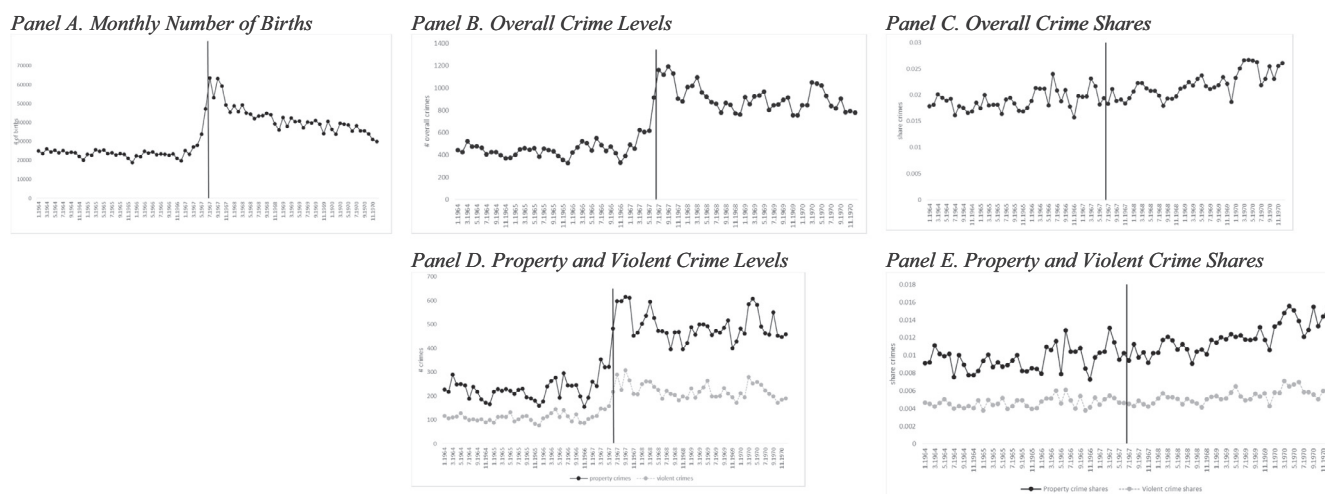


Fig. 2. The 1966 Abolition of Abortion: Number of Births, Crime Levels and Crime Shares by Crime Type. Note: This figure shows the effect of the 1966 abortion ban on the monthly # of births (Panel A), # of convictions - overall (Panel B), share of convicted individuals overall (Panel C), and # and share of convictions by crime type (Panels D and E). The Y-axis indicates the # or the share convicted, while the X-axis indicates the month and year of birth for the individuals born between January 1964 and December 1970 and observed in the crime registers from 1997 to September 2017. Property and violent crimes are defined in the text. Data sources: The Romanian Demographic Year Book (2005) (birth data) and the National Penitentiary Administration (crime registers).

reproduction policies for the Romanian people. In 1990, Romania reached the highest rate of induced abortion – 200 per 1,000 women aged 15–44 – in the world (Serbanescu et al., 1995).¹¹ At the same time, maternal mortality dropped to 83 deaths per 100,000 live births, which was almost half the ratio in 1980's when Romania experienced the highest recorded maternal mortality of any country in Europe (Hord et al., 1991). These statistics indicate that, even though the legalization coincided with the transition from a communist regime, the sudden fertility decrease in mid-1990 in Romania did not just represent changes in behavior and/or preferences for children, but free abortion access. Moreover, for other countries from the former Communist Bloc, which also experienced a dissolution of the communist regime in 1989 (but no changes in abortion availability), an abrupt change in fertility in 1990 is not seen. Appendix Fig. 1 shows that there is a gradual decrease of annual total fertility rates in the other former Communist Bloc countries (Bulgaria, Hungary, Russia and Poland), while there is an abrupt change for Romania. In fact, the change in Romania was so abrupt that its total fertility rate was higher than each of these four other countries before the 1989 reform but lower immediately after. Moreover, as this figure presents annual data, the change in Romania was even sharper than it appears because, as we show below, 1990 includes both treated and untreated birth month cohorts.

Using data from the highly reliable Romanian natality files, we document more clearly the variation in the number of births in the months around both reforms. Panel A of Fig. 1 shows a large decrease in the number of children born in Romania starting in July 1990, i.e. six months after December 1989. Similarly, a large increase in the monthly cohort size can be seen with a 6-month lag after the abortion ban on December 1, 1966, i.e. in June 1967 (Panel A of Fig. 2). Such a six-month lag makes sense since abortion was legal only during the first trimester. This contrasts the nine-month lag in fertility in East Germany after the fall of the Berlin Wall (Chevalier and Marie, 2017), which was driven by a change in the selection into conceptions rather than post-conception selection through abortion (Malamud et al., 2016). Moreover, this

drop in fertility six months after abortion became legal cannot be explained by the repeal of different pronatalist policies introduced during the communist era: no major changes in the monthly child allowances or maternity leave policies took place immediately around the abortion ban or after the fall of communism (see Kligman, 1998 and Pop-Eleches, 2006, for a more exhaustive discussion).¹²

3. Data

Our main data source is the official crime registers from the Romanian National Penitentiary Administration, which include all criminal convictions that result in time served in prison from January 1997 to September 2017 and the stock of prisoners in January 1997 who were released after this date.¹³ Our analysis samples for the 1966 and 1989 reforms, respectively, consist of all individuals born between 1964–1970 and 1987–1993. For the cohorts born around the 1989 reform, we also observe individuals who are held in preventative detention but not convicted.¹⁴

This data structure implies that crimes are observed at different ages for each cohort. For individuals born around the 1989 reform,

¹² A number of further incentives meant to encourage women to have children were introduced in the 1970s and 1980s: financial child allowances, maternity grant and leaves and work protection, access to medical attention during the pregnancy and mother and child medical care (Kligman, 1992). The maternity grant was a one-time payment of approximately \$85, or about one average monthly salary. The monthly child allowance was increased by about \$3 per month and child, which is very small compared to the cost of providing for a child. Since these incentives were not introduced around the 1966 ban, they should not affect the cohorts born around the 1966 cut-off. Moreover, while these incentives might have had an independent effect on the demand for children born starting with the 1970s, this concern should also not apply for the lifting of the ban in 1989 because these incentives were not repealed during the regime change. However, some rules introduced when fertility started declining, around 1985 (e.g. fertility controls for fertile age women at the working place, imprisonment of the medical practitioners for any infraction of the abortion law), were not effective after December 1989 (see Tudor, 2020). Even though these rules faded away towards the end of the 1980s, we cannot clearly disentangle the effect of abortion legalization per se from the strict monitoring (and the extra stress around this).

¹³ Those already in prison in January 1997 account for 8.4% of the prisoners born 1964–1970. Dropping these individuals does not affect the results.

¹⁴ This accounts for about 9,512 (or 24%) individuals. We could not get the same information for the cohorts born around the 1966 reform.

¹¹ We do not have accurate data about (illegal) abortions before 1990. Moreover, because in the early 1990s other methods of contraception were not widely used or available on the market, abortion remained the main contraceptive method in Romania (Kligman, 1992).

we observe crimes committed before age 24 for those born in 1993 but before age 30 for those born in 1987. Around the 1966 reform, we observe crimes between ages 27 and 47 for those born in 1970 and 33 and 53 for the 1964 cohort. Thus, for the 1989 reform, we observe criminal behavior when individuals are closer to the peak of the age-crime profile, while for the 1966 reform sample we observe more years of crime data per individual but when they are older and less crime prone. Appendix Fig. 2 demonstrates the unbalanced nature of the data as well as the comparability of the Romanian age-crime profile to other countries (in the age ranges that we can observe), peaking in the early 20s and falling afterwards. We demonstrate that the results are robust to such censoring by also defining the outcomes conditional on ages observed for all cohorts in the analysis sample. Appendix Fig. 2 also draws one's attention to the fact that the highest crime rate observed for the cohorts surrounding the 1989 reform is 10% at age 23: these cohorts are born in 1987–1993 and reach age 23 between 2012 and 2016. Given that crime typically peaks in the early 20s and that the crime data for the cohorts born around the 1966 reform is measured about 10 years after this typical peak, it is perhaps surprising that their maximum crime rate (at age 33, which occurs for these cohorts between 1997 and 2003) is close to 9%. The comparability of these peaks across the two reform samples is driven by the general downward trend in Romanian crime rates (for which we control in our estimation) over this time period: national crime rates decreased by more than 60% between 1997 and 2012.¹⁵ Similar decreases in crime were seen during this period in many countries around the world.¹⁶

We look at two types of dependent variables – levels and rates. The first is simply the aggregate number of individuals in a given birth month cohort with a conviction resulting in a prison sentence between 1997 and 2017. We scale this count by the cohort size at birth from the Romanian Demographic Yearbook (NIS, 2005) (where the cohort is defined by birth month and year) to compute the share of convicted individuals.¹⁷ Both men and women are included (despite prisoners being predominantly male) because (i) the denominator, or number of births, cannot be separated by gender and (ii) we only have the offender gender for the 1989 reform sample, but not for that in 1966.¹⁸ Finally, each prison sentence can have multiple charges associated with it. Descriptive statistics (for the analysis sample in Table 1 and the full sample in Appendix Table 1) indicate that there are on average two convictions per sentence. Using the observed crime categories, we also create indicators (not mutually exclusive) for violent, property and other (not violent or property) crime convictions, respectively.

For each individual, these prison registers also include information on educational attainment, number of children, and marital

status; we use these variables to study whether the composition of prisoners changed for cohorts differentially exposed to the reforms. In other words, we will also use these data to indirectly study compositional effects around the reforms.

There are some limitations, especially for the 1966 reform, to measuring crime using these data. First, for the 1966 reform, the crime records are observed at ages past the peak of the age-crime profile. Second, one may question the appropriateness of using incarceration numbers to measure changes in criminal behavior – e.g. if judges aim for specific capacity levels or prison authorities adjust capacity to reflect population size. This limitation is again more relevant for the 1966 reform as well, given that the data for the 1989 reform sample also include information about preventative detention (i.e. cases that held in prison pre-trial and did not result in a conviction). A related concern common to all papers measuring crime using administrative crime data is that we only observe those offenders who got caught and, in this context, held in prison. Third, for both the 1966 and 1989 reforms, as the majority of prisoners are male, these data do not tell us much about females.

To address these limitations we supplement our crime data with the 2007–2017 National Inpatient Registers, which contain individual-level data (including birth month) for more than 7,000,000 stays in Romanian hospitals. Hospitalization is more equally distributed across genders, as about 29% and 47% of hospitalizations for cohorts born around the 1989 and 1966 reforms, respectively, were male. Moreover, hospitalization does not have the same age profile as crime. Thus, we can look at behaviors that are related to crime but are more common at the ages for which we observe the 1966 cohorts. Based on over 7 million hospital entries for our cohorts of interest, we create indicators of whether an individual spent at least one night in hospital care because of (i) mental and behavioral disorders and (ii) risky behavior, respectively.¹⁹ We scale these hospitalization counts by the size of the birth month cohort to find the relevant hospitalization rate.

4. Methodology and preliminary analyses

4.1. Graphical evidence – The impact of legalizing and abolishing abortion on crime

We begin our analyses of the impact of abortion on crime with simple plots of the raw data. Figs. 1 and 2 present the results for the 1989 legalization and 1966 abolition, respectively. The vertical line in each figure corresponds to July 1990 and June 1967: as highlighted in the background section, individuals born in these months or later were exposed to the new abortion regimes. Panel B of each figure presents the number of individuals in each birth month cohort convicted of any charge (regardless of crime type) that resulted in a prison sentence. Panel C normalizes this by the

¹⁵ See (last accessed March 11, 2021): <https://www.macrotrends.net/countries/ROU/romania/crime-rate-statistics#:~:text=Romania%20crime%20rate%20%26%20statistics%20for,a%204.89%25%20increase%20from%202014>.

¹⁶ For instance, in the US, arrests rates from 1995 to 2014 decreased by almost 40%. See <https://www.bjs.gov/index.cfm?ty=datool&url=/arrests/index.cfm#>.

¹⁷ The number of births by month and year are measured with high accuracy and they are retrieved from the Romanian Natality Files. However, we have also assessed the sensitivity of our results to normalizing the crime count by the number of live births. Specifically, we have also taken into account selective mortality and (selective) external migration. The results are robust and available upon request. One potential bias we cannot address are deaths in orphanages. To the extent that the 'unwanted' children suffered early death in these institutions, we cannot observe these deaths in our mortality records (starting in the 1994) for the 1966 reform.

¹⁸ Note that our denominator – the cohort size at birth, by month and year of birth –, which was retrieved from the Romanian Demographic Yearbook (NIS, 2005), cannot be observed separately by gender. Therefore, in both the numerator and denominator we keep both genders. Parent' preferences over sons or daughters and the corresponding sex-selective abortion was not a common practice in Romania. Moreover, the use of ultrasound technology was rarely available around the two reforms. Finally, there were no significant changes in the shares of males in the total number of births when comparing aggregate numbers around the reforms.

¹⁹ The *Mental and behavior disorder* indicator takes the value 1 if the episode of hospitalization was any an ICD-10 code F "Mental and Behavioral Disorders". The largest shares for those born around the 1966 reform were: F30-F39 (*Mood [affective] disorders*) and F20-F29 (*Schizophrenia, schizotypal and delusional disorders*) each about 30% of all individuals with mental disorders born 1964–1970. Other important categories (over 10% each) were F10-F19 (*Mental and behavioural disorders due to psychoactive substance use (alcohol, drugs, substances)*) and F00-F09 (*Organic, including symptomatic, mental disorders*). For the 1987–1993 cohorts the largest shares were F20-F29 (*Schizophrenia, schizotypal and delusional disorders*) and F70-F79 (*Mental retardation*) with 30% and 19%, respectively. Codes F30-F39 covered 14%, while those related to *Mental and behavioural disorders due to psychoactive substance use (alcohol, drugs, substances)* covered 11%. The *Risky behavior* indicator takes the value 1 if the episode of hospitalization was an ICD-10 code U50-Y98 "External causes of morbidity and mortality" – including for both reforms exclusively (99.8%) transport related accidents – or "Injury, poisoning and certain other consequences of external causes" (ICD-10 codes S00-T98) including *Injuries* (Codes S) about 85% and *Poisoning and certain other consequences of external causes* (T15-T98) (15%).

Table 1
Summary Statistics for the 1989 and 1966 Reform Analysis Samples.

	All individuals born 1989–1991		All individuals born 1966–1968	
	Mean	S.D.	Mean	S.D.
I. Imprisonment registers (1997–2017)				
# of charges per case	1.835	1.636	1.476	1.167
Sentence length (in days)**	2488.091	3200.297	2187.882	3444.293
Age when convicted	21.878	3.143	35.094	5.923
Age when arrested	21.82	3.154	35.021	5.847
Juvenile arrested (<18)	0.136	0.343		
Recidivist	0.453	0.497	0.684	0.464
Type of crime: violent	0.42	0.493	0.242	0.428
Type of crime: property	0.482	0.499	0.523	0.499
Type of crime: other	0.201	0.4	0.292	0.454
Male	0.964	0.184		
Total number of 'cases'***	12,456		26,313	
II. In-patient registers (2007–2017)				
Hospitalization length (in days)	5.64	10.387	8.449	33.589
Age when hospitalized	21.662	3.354	44.67	3.345
<i>Cause of hospitalization:</i>				
Mental and Behavioural Disorders	0.041	0.197	0.111	0.313
Risky behaviour	0.104	0.305	0.069	0.255
Male	0.285	0.451	0.472	0.499
Total # of hospitalizations***	1,170,338		1,170,338	
III. 2011 Census				
'At Risk' for Crime	0.02241	0.05653	0.00757	0.08668
Risk measure 1	0.00274	0.05228	0.00681	0.08225
Risk measure 2	0.00047	0.02158	0.00076	0.02755
Total # of individuals	999,862		840,180	

Note – Panel I: The crime registers include all criminal convictions that result in time served in prison from January 1997 to September 2017. ** Each individual can have more than one case, and each case can include multiple charges. Violent crimes cover categories: 174–192 from the penal code (CP) and the new penal code (NCP/L286), 193–200 (NCP) and codes 233, 234, 236 (NCP), 267 (CP), 287 (NCP), 335, 336 (CP), 401, 402 (NCP), and 438–445 (NCP). The most common violent crimes were: robbery, murder, rape, and premeditated murder. Property crimes include premeditated theft (CP209, 209CP) – with over 85% of all property crimes, followed by trespassing (192CP), theft (208CP), and stealing (229 NCP). *Other* refers to crimes that are not violent or property. Gender information is not available. Panel II: The inpatient registers include all hospitalizations in Romania from January 2007 to December 2017. Mental and Behavioural Disorder hospitalizations have the following ICD-10 codes: F30-F39 (*Mood [affective] disorders including depressive episodes and bipolar affective disorder*), F20-F29 (*Schizophrenia, schizotypal and delusional disorders*), F10-F19 (*Mental and behavioural disorders due to psychoactive substance use*), F00-F09 (*Organic, including symptomatic, mental disorders*), and F70-F79 (*Mental retardation*). The *Risky behavior* indicator captures: ICD-10 code U50-Y98 “*External causes of morbidity and mortality*” – almost exclusively transport accidents, “*Injury, poisoning and certain other consequences of external causes*” (ICD-10 codes S00-T98) which is primarily (85%) *Injuries* (Codes S), and *Poisoning and certain other consequences of external causes* (T15-T98).***Same individual can have multiple hospitalizations, but cannot be matched over time. Panel III: Our *risk measure* = 1 if the individual is: (i) institutionalized at the time of the census in an institution: prison, jail, juvenile detention centers, orphanages, asylum, sanatorium, hospital homes and hospitals, and emergency centers and night shelters (the risk 1 measure) or (ii) reported as not living in the household for at least the last 12-months for a reason other than education, working or family reasons, e.g. being detained (the risk 2 measure). *Data sources*: The Romanian National Penitentiary Administration (Panel I), the Romanian National Inpatient Registers (Panel II) and the 2011 Romanian Census (Panel III).

number of live births in that month; specifically, the y-axis presents the *share* of individuals convicted (and incarcerated) per 1,000 live births. Panels D and E, respectively, present the number and share of individuals convicted for both violent (grey, dashed line) and property (solid, black line) crime offenses. Our main findings are, in fact, readily visible in these simple graphs: both overall and across crime categories, there is a large decrease (increase) in crime levels but not an apparent decrease (increase) around the cut-offs in crime rates when abortion is legalized (abolished) in 1989 (1966). These figures also highlight that crime appears to be trending down (linearly) for the cohorts born around the 1989 reform while crime trends are fairly flat for cohorts around the 1966 reform.

4.2. Empirical framework

These figures visually present the reduced form relationship between exposure to the 1989 and 1966 abortion reforms and both crime levels and rates. As already described in the introduction, these reduced form effects can potentially encompass multiple mechanisms: changes in cohort size (i.e. a mechanical effect), compositional and unwantedness effects, as well as general GE effects. Though the cohort size channel is specific to the effect of abortion reform on the level of crime, the remaining channels could also

feasibly affect an individual's propensity to engage in criminal behavior or the crime rate. This section presents the empirical framework we use to (i) estimate the effects of the reforms over and above any crime trends across cohorts, which can be unrelated to abortion related fertility changes, (ii) deal with possible general equilibrium (GE) effects (not taken into account in i), and (iii) disentangle the mechanisms underlying the reduced form patterns seen in Figs. 1 and 2. We discuss each channel in turn, and highlight both the extent that we can test/control for this mechanism as well as any potentially related identifying assumptions.

To the extent that each birth month or birth year cohort are exposed to societal shocks (such as criminal justice reforms, increased drug access, or income/employment related shocks) for differential amounts of time, these shocks can result in crime trends that are completely unrelated to the abortion reforms. Our empirical analysis estimates the effects of the reforms over and above such trends by including a linear trend (as motivated by the figures) in our baseline specification presented in equation (1).

Moreover, we aim to identify the effect of the abortion reform on the treated individuals, over and above any GE effects of the reform. That is, we aim to separate the immediate selection effects (i.e. the combined composition and unwantedness effects) from the GE effects. Such GE effects can occur both across households in the society (e.g. crowding effects in a classroom or changing

capacity of the criminal justice and health care systems) and also within the household.²⁰ With regards to the latter, all children in the household can potentially be affected by the birth of an unwanted child either (i) directly (i.e. via birth order or siblings effects),²¹ or (ii) indirectly, when the unwanted birth affects the psychological or economic situation of the mother and thus affects the entire household. To estimate the effects of the reforms over and above any such GE effects (which are not captured by the trends), we trade-off sample size and restrict the baseline sample to individuals born within a relatively short period (18 months of the reform) on either side of the cut-off.

$$y_{mt} = \alpha + \beta \text{treat}_{mt} + \gamma_m + \text{month}_{mt} + \varepsilon_{mt} \quad (1)$$

Specifically, the baseline specification regresses outcome y (e.g. number of births, number/share convicted) for those born in birth month m and birth year t on a dummy indicating whether the birth month cohort is treated. This treatment variable turns on for individuals born in July 1990 and June 1967 for the respective reforms.²² Parallel to the graphical analysis, we emphasize again that our data are aggregated to the birth month/year cohort level, and the crime outcomes are aggregated over all available years of crime data. In other words, our estimates are identified off of birth month cohorts born immediately after the reforms, but we study the long-run crime outcomes of these cohorts. We include month of birth fixed effects (γ_m) to control for seasonality in month of birth, which can be related to outcomes later in life, and a linear trend in birth month (month_{mt}). As highlighted above, this trend captures societal trends unrelated to the abortion reforms but also potentially GE effects to the extent that each birth month cohort is differentially exposed to them over their childhood. Moreover, controlling for such trends may also be important given the unbalanced nature of the crime data (i.e. the fact that fewer peak crime years/months are observed for younger cohorts). Robustness tests allow for separate pre and post-reform trends or year of birth fixed effects. Moreover, we will provide multiple pieces of evidence that our results (or the lack thereof with respect to crime rates) are not driven by small sample sizes.²³ Finally, Section 5.3 provides extensive evidence that our baseline specification is robust to alternative sample windows (12 to 60 months) and counties with varying treatment intensity.²⁴

Under the assumption that GE effects (and any other possible trends) are controlled for in equation (1), β now captures the combination of the composition and unwantedness channels (i.e. the

so-called selection channel). Can these be disentangled?²⁵ We first turn to the composition effect, which refers to the fact that abortion availability can affect the socio-economic composition of birth cohorts if mothers of different backgrounds differentially utilize abortion. In other words, the composition effects refer to the effects on crime of changing characteristics of mothers, keeping the proportion of wanted to unwanted children constant. In contrast, we think of the unwantedness effect as the differences in crime rates between unwanted and wanted children when holding parent characteristics (i.e. composition) constant. There is no theoretical consensus on the direction of these compositional changes, as it depends on who the marginal users of abortion are. Thus, before presenting the empirical results, it is important to get a handle on both the extent to which such compositional effects are relevant for each reform and the direction of these effects. It is only with this information that one can (potentially) disentangle unwantedness from composition changes. Specifically, if there are large compositional changes that work in the *opposite* direction of unwantedness effects, then the lack of an effect of abortion reforms on crime rates (as seen in Figs. 1 and 2) may be due to the two channels completely offsetting each other, rather than a true null effect. But, if there is little (or no) evidence of compositional changes, and if these changes work in the same direction as the unwantedness effect, then the effect of abortion reforms on crime rates becomes easier to interpret. In this case, the lack of an effect on crime rates would suggest the conclusion that there is not an unwantedness effect on crime rates as a result of the reform.

We take two steps to control for and understand the potential importance of compositional effects. First, we demonstrate the robustness of our results to controlling for observable changes in composition, by including a vector X of controls in the specification. However, given that there is a limited set of variables that we can measure at the birth month cohort level (ethnicity, parent education, parent year of birth) and potential measurement error in these variables, we do not just rely on these results. Rather, we extensively discuss what is already known and present new evidence about the compositional effects of each reform in Section 4.3. We use this knowledge to draw conclusions about the potential role of unwantedness (when possible).

4.3. Compositional changes

We take three steps to assess the extent and direction of compositional changes with respect to each abortion reform. First, we discuss the findings of the existing literature. Second, we provide direct empirical evidence of the changes in the composition of women giving birth around the abortion reforms using the 1977 and 1992 Censuses. Third, we examine whether the characteristics of offenders changed around the reforms; while this is not a direct test of whether the composition of mothers changed, to the extent that there are intergenerational relationships, one would expect that a change in parental education would be reflected in the education of their children (see e.g. Björklund and Salvanes, 2011).

With regards to the existing literature, Pop-Eleches (2006) shows large compositional changes for the 1966 reform: children born after the unexpected abortion ban were more likely to be born to higher educated mothers, because these women were more

²⁰ Classroom crowding is a specific type of GE effect that we directly address later (e.g. children born in the first five months of 1967 just prior to the abortion ban had to go to school as a result of compulsory schooling laws with a much larger cohort born as a result of the abortion restrictions).

²¹ Examples of sibling peer effects in crime include Bhuller et al. (2018), who find that sibling spill-over effects in crime pass only from the older to the younger sibling. Similar findings are reached by Averett, Argys and Rees (2011) and Breining et al., (2020). The sibling disability literature raises the possibility that an unwanted child affects other children in the family: for instance, Black et al. (2021) find that the second child in the family is affected when the third child has a disability, and that some of these spillovers are at least in part driven by parental time and financial resources.

²² The results are also robust to allowing the treatment to occur in June for the 1989 reform. Similarly, we have chosen the cut-off to be July for the 1966 reform, but the results do not change if we use June as a cut-off point. We also show that our results are not driven by the spikes (or drop) in births in the months near the cut-off points.

²³ An alternative empirical specification would be to collapse the data by birth month/year and year of crime. However, this leads to an unbalanced panel and concerns about modelling the age crime profile. Estimating such a specification yields the same conclusion as our baseline. Our results are also robust if we cluster by the birth month cohort with the standard errors very similar (up to the third digit). All of these results are available upon request.

²⁴ The crime data available prevents the expansion of the window further; as described in the data section, we can observe all prison records before age 25 for the 1989–1991 birth cohorts. But for the 1987–1993 cohorts, we only observe those before age 23. Concerns about censoring increasing the more the window is expanded.

²⁵ This is theoretically (and empirically) a very difficult task because unwantedness and compositional effects are connected on various (un)observable dimensions. Moreover, the relation and the relative importance of these two channels on the probability of being convicted depends not only on the magnitude and direction of the compositional effects, but also on the social norms available in the society regarding both abortion and unwantedness. Because the direction of the composition effects resulting from a change in the abortion law is theoretically unclear (i.e. for the two reforms it varies in magnitude and size or importance), in this paper we do not make any assumptions about the relative importance of these two channels on the probability of being convicted.

likely to use abortion before the ban.²⁶ In contrast, for the 1989 reform, Malamud et al. (2016) use a 15% random sample of the 1992 Census and show that abortion access had little impact on the characteristics of women giving birth before and after July 1990: if anything, abortion legalization had an influence on households from disadvantaged backgrounds (low educated mothers from rural areas were less likely to give birth), but effects were small in magnitude and not significant.

We replicate this pattern of results using the full sample from the 1992 Census in Table 2. We run our baseline specification (1) for the sample of mothers who gave birth around the two reforms and where our outcomes are mothers' characteristics. While for the 1989 legalization our measures are less likely to be problematic (children born around the July 1990 cut-off were maximum two years old in January 1992), the sample around the 1966 ban is restricted to individuals still living with their parents at the time of the census (children born around the June 1967 cut-off were about 25 years old in 1992, accounting for about 50% of all births) (see also Pop-Eleches, 2006).²⁷ In columns (1) to (5), we show indicators that are more likely to proxy mothers' compositional changes (ethnicity, whether the mother was born in an urban area or education), while columns (6) to (10) are markers more likely linked to unwantedness (whether the mother was married, single, or divorced, the mothers' age of birth and the total number of children born to the woman).²⁸ Panel A of Table 2 shows no impact of the 1989 reform on the mothers' composition; the effects are very small in magnitude and not significant. The coefficients on the education outcomes suggest that, if anything, there is positive selection after the ban on abortion is lifted in 1989. The only coefficient that is statistically significant is the urban place of birth of the mother indicator. Because, on average, there are fewer crimes committed in rural than in urban areas, this result means that the composition effect (via urban birth) and the unwantedness effect would go in the same direction and not offset each other. In contrast, the same table presents evidence that is consistent with the existence of some unwantedness effect. For example, children after the lifting of the ban grow up in households with fewer children. Similarly, women who give birth after the 1989 reform, where about 0.38 years younger, which suggests that older women who had exceeded their targeted family size, responded most to the reform. There is also some evidence that women who gave birth after the reform are statistically significantly less likely to be divorced while other indicators such as married or single mom are not significant. Overall we argue that the composition effects around the 1989 reform are very small in magnitude and not economically significant; however, it is not clear whether the unwantedness effects (as proxied by these observables) are large enough to translate into children's crimes later in life. In contrast, large and significant changes in mother characteristics and unwantedness markers are observed for the 1966 ban, which is consistent with the earlier Pop-Eleches (2006) analyses.²⁹

edness markers are observed for the 1966 ban, which is consistent with the earlier Pop-Eleches (2006) analyses.²⁹

Additionally, we can provide some further (indirect) evidence that the 1989 reform did not result in differential changes in the families giving birth around the legalization cut-off by looking at the education and family outcomes observed for our samples of prisoners. If the selection on mother characteristics were large then we would expect to see some difference in similar observables for their children. Comparable to our baseline specification, but using the disaggregated individual data for the convicted sample, we regress prisoner characteristics on whether they were exposed to the reform as well as county, month of birth and year fixed effects or trends. These results are presented in Appendix Table 2. For the 1966 reform, we find that prisoners born to treated mothers are more educated (0.4 more years of schooling) and less likely to be married and have fewer children. Similar effects are not seen around the 1989 reform. In fact, coefficients are much smaller in magnitude and far from significant. At face value, these descriptive results suggest that the 1989 reform did not significantly affect the education outcomes of those born after the reform.³⁰ This would also suggest that any compositional changes in parent characteristics (as described above, these are small) are not large enough to translate into an observable effect on child education outcomes.

Finally, we can also check directly whether the abortion legalization led to changes in the rate of extreme unwantedness, proxied here as the number of children left in state-run institutions by their parents. In this exercise, similar to our baseline specification, we consider all children born around the 1989 cut-off point and found in institutional care in 1994.³¹ The results shown in Appendix Fig. 3, show that there is no change in the rate of such childhood institutionalization around the 1989 reform.

Taken together, these pieces of evidence suggest that there are compositional changes with respect to the 1966 reform but little to no evidence of such changes for the 1989 reform. These findings are important to keep in mind when interpreting our results, as changes in crime rates around the reforms could only be theoretically driven by compositional changes or unwantedness effects. For the 1966 ban, one would expect an increase in unwantedness, which would imply an increase in crime rates, while the positive compositional effects would be expected to decrease crime rates. Thus, for the 1966 reform, the lack of an effect on crime rates observed in the figures can either be a zero effect or the result of both unwantedness and compositional changes completely can-

²⁶ Because better off families give birth after June 1967, not controlling for the composition effects may result in this case in estimating the opposite effects. Pop-Eleches (2006) finds that, before controlling for the compositional effects, the unwanted children born after June 1967 were better-off in terms of education and labor market outcomes. Controlling for the composition effects the pattern of these effects is reversed. In our regressions, we use aggregate controls from a sample of the 1977 Census, but results are similar when using the 1992 Census.

²⁷ Pop-Eleches (2006) shows that children still living with their parents (and for whom we can recover parent background variables in 1992) are, on average, not affected very differently by the policy compared to the whole population of children. Using aggregate data from the 1977 Census shows a similar pattern for the 1966 abortion ban.

²⁸ Note that variables such as education, number of children and marital status could be endogenous. This is less of a concern for the 1989 sample because these variables are measured soon after the cut-off point.

²⁹ While the evidence we show in this table comes from the 1992 census, in our regressions for the 1966 reform we include (aggregate) controls using the 1977 Census. However, our results are unchanged when the vector X is measured using the 1992 or the 1977 Censuses.

³⁰ For the 1989 reform, education could be endogenous. Our results (available upon request) are robust to: i) only keeping individuals of age 24 or below; ii) controlling for the age at arrest in the regressions; and iii) excluding all individuals that were arrested before turning 18 (the age when the secondary education is finished and individuals may get admitted into universities, so the primary and secondary education variables are not biased).

³¹ We use the specially designed 1994 census on institutionalized children that provides information on over 100,000 children (accounting for about 2 percent of all 0–18 year old children) institutionalized in 562 institutions spread across the country. This number includes abandoned children with no connection to their parents, children institutionalized but in contact with their families (in general from poor families and single mothers), and disabled children part of the special programs. We do not attempt to disentangle some special categories of children because of various measurement and endogeneity concerns (e.g. the numbers measured in 1994 could be affected by (selective) mortality because of the awful conditions in these institutions). Finally, note that we cannot do a similar exercise for the 1966 reform because these children would have been over 18 years old (maximum age when a child can be institutionalized) at the time of the special 1994 census.

Table 2
Changes in Family Composition Around the Reforms Based on 1992 Romanian Census.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Markers of mothers' composition changes					Markers of unwantedness:				
	Romanian	Urban birth	Primary or less education	Secondary education	Higher education	Married	Divorced	Single	Age at birth	No. of children
<i>Panel A. 1989 reform</i> (N = 848,682)	−0.000773 (0.00171)	−0.0119* (0.00590)	−0.00554 (0.00561)	0.00443 (0.00465)	0.000798 (0.00129)	0.00406 (0.00295)	−0.00262** (0.00106)	−0.0009 (0.00155)	−0.378*** (0.0573)	−0.144*** (0.0166)
<i>Panel B. 1966 reform</i> (N = 500,541)	0.0182*** (0.00305)	0.0649*** (0.00906)	−0.0429*** (0.00908)	0.0386*** (0.00796)	0.00469*** (0.00146)	−0.0218*** (0.00354)	0.0107*** (0.00211)	0.0001 (0.0007)	0.902*** (0.0774)	0.227*** (0.0386)
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note – This table estimates the changes in the composition of the mothers who have birth around (January 1989–December 1991) and the 1966 ban (January 1966–December 1968) of abortion using the 1992 Census. The drawback of the data is that we only capture children (still) living with one or both parents. In 1992, children born around the 1989 reform were about 2 years old and living with their parents. However, children born around the 1966 reform were about 25 years old in 1992 that may induce some selection because only about 50% of them were still living with their parents. The baseline results with birth month fixed effects and a month of birth linear trend. Note that some dependent variables (education, marriage) are endogenous. The categories for the marital status are married, divorced, single, widowed, and others. The analysis sample are the mothers giving birth 1.5 years on either side of the reform. Standard errors in parentheses. Significance noted by *** 1%, ** 5%, and * 10%. Data sources: the 1992 Romanian Census.

celling each other out. However, because the evidence shown above is more comprehensive regarding the 1989 reform, a much cleaner interpretation can be assigned to the abortion legalization results. The 1989 legalization should, if anything, lead to a decrease in unwantedness and correspondingly crime rates. To the extent that compositional effects exist, they are also in an off-setting direction; but the evidence points towards these effects being negligible to non-existent, such that a zero-effect on crime rates can be unambiguously interpreted.

Of course, these results also highlight the importance of controlling for family background data, especially for the 1966 reform. Since our crime registers cannot be linked with such data, our ability to control for these potentially important effects is limited to adding controls from the 1977 (for the 1966 reform) and 1992 (for the 1989 reform) Censuses. We show that adding these controls does not affect the crime and crime related results.

5. Results

5.1. Baseline results and specification checks

5.1.1. 1989 Legalization

Panel A of [Table 3](#) presents the main results for the 1989 abortion legalization. Columns (1)–(3) present the estimated treatment effect from the baseline specification for: number of births, number of individuals with a conviction (of any crime) resulting in incarceration, and share of convicted individuals. Standard errors are in parentheses and pre-treatment means of the dependent variable in italics. The legalization of abortion resulted in an immediate decrease in the number of births by approximately 7,700 (25% relative to the pre-treatment mean). Similarly, the number of convicted individuals decreased by 22% for any offense. [Table 4](#) shows a 20% (Panel A) and 25% (Panel B) reduction in the number of violent and property crimes, respectively, for the 1989 reform. Given the proportionate change in the number of births and number of convictions, the lack of a significant effect on the crime rate in column (3) of [Tables 3 and 4](#) is unsurprising. The point estimate of the effect of the 1989 legalization on the overall crime rate in

[Table 3](#) is itself quite small: 1.3% relative to the mean.³² However, standard errors are such that the 95% confidence interval cannot rule out an effect of abortion legalization ranging from a 10% reduction to a 12% increase in the conviction rate.

The lack of a significant effect of abortion legalization on crime rates is robust to replacing the linear trend in birth month with birth year dummies (column (4) of [Table 3](#)) as well as a split linear trend that is allowed to differ for pre and post-reform birth cohorts (column (5) of [Table 3](#)). The results are also robust to dealing with data censoring in column (6) by restricting the outcome to be age-specific (convictions before age 26).

One limitation of the analysis so far is that it measures crime using convictions resulting in incarceration, i.e. arguably the most serious crimes. How can we rule out that there is no effect on more minor crimes? For cohorts around the 1989 reform, we can actually expand the definition of conviction to include those observed in the data because they were held in preventative detention (and not sentenced to incarceration). However, the same pattern of results – a level effect but no rate effect – is seen in Panel C of [Table 4](#). Moreover, in results presented in [Appendix Table 3](#), we do not find any effect on other crime measures potentially related to offense severity, including number of days sentenced, average time served, average age at arrest, and the share of individuals who are repeat offenders.

One potential concern with our baseline specification is whether the insignificant effect on crime rates can be driven by

³² The pre-treatment mean for the 1989 reform sample is 0.014: 1.4% of sample is observed with a conviction resulting in incarceration between the ages of 16 and 25. While this statistic may seem lower than one would expect, we highlight three factors that may affect this perception: (i) Because of data restrictions, the denominator includes males and females; yet most crimes in society are committed by males; (ii) These are only convictions that result in prison, i.e., the most serious of offenses, and does not capture the more common minor offenses; (iii) These are not lifetime incarceration statistics but rather measured only at certain ages. Though it is difficult to compare criminal justice statistics across countries – given varying definitions – the World Prison allows for a ranking of prison population rates. The US is ranked highest, with 639 prisoners per 100,000 in the population. Romania is ranked 136 with 115 prisoners per 100,000 in the population. See https://www.prisonstudies.org/highest-to-lowest/prison-population-rate?field_region_taxonomy_tid=All. Thus, just 0.1% of the population in Romania is incarcerated at a specific point in time.

Table 3
The Impact of the 1989 Legalization and 1966 Abolition of Abortion Laws on Any Crime.

	(1)	(2)	(3)	Dependent Variable:			(7)
	# births	# Convicted		Share Convicted Individuals			
<i>Panel A. 1989 Abortion Legalization</i>							
	-7,699*** (770)	-91.33*** (19.13)	0.000179 (0.000746)	0.000179 (0.000762)	-0.000987 (0.00153)	0.000103 (0.000675)	0.000299 (0.00133)
	<i>30535.56</i>	<i>414.0556</i>	<i>0.0135721</i>	<i>0.0135721</i>	<i>0.0135721</i>	<i>0.011402</i>	<i>0.0135721</i>
Outcome restriction	No	No	No	No	No	conv. before age 26	No
<i>Panel B. 1966 Abortion Abolition</i>							
	32,355*** (4625)	628*** (83.54)	-0.000292 (0.000989)	-0.000161 (0.000936)	-0.00151 (0.00189)	-0.000496 (0.000852)	-0.0056 (0.00323)
	<i>24161.59</i>	<i>484.58</i>	<i>0.0200384</i>	<i>0.0200384</i>	<i>0.0200384</i>	<i>0.0179155</i>	<i>0.0200384</i>
Outcome restriction	No	No	No	No	No	conv. from 31 to 49	No
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month Linear Trend	Yes	Yes	Yes	No	No	Yes	Yes
Birth Year Dummies	No	No	No	Yes	No	No	No
Birth Month Split Trend	No	No	No	No	Yes	No	No
Composition Controls	No	No	No	No	No	No	Yes

Note – This table estimates the effect of the 1989 legalization of abortion (Panel A) and 1966 abolition (Panel B) on the number of births, number of convictions (for any crime type) and share convicted. The baseline results are in columns (1) - (3), while robustness and sensitivity checks are presented for the main variable of interest (the share convicted) in columns (4) - (7). The analysis sample is individuals born from January 1989 to December 1991 and January 1966 to December 1968 for the 1989 and 1966 reforms (36 observations for all regressions). The dependent variable is the number of convictions observed in the crime register from 1997 to September 2017. Column (6) assesses the sensitivity of the results to censoring in this data. Column (7) includes controls for averages of ethnicity and the parents' education (gymnasium, secondary, university) and years of birth observed at the 1992 Census for the 1989 reform and 1977 Census for the 1966 reform. Standard errors in parentheses. Data sources: The Romanian Demographic Year Book (2005) (birth data) and the National Penitentiary Administration (crime registers). Pre-treatment means in italics. Significance noted by *** 1%, ** 5%, and * 10%.

Table 4
The Impact of the 1989 Legalization of Abortion on Property Crime, Violent Crime, and Preventative Detention.

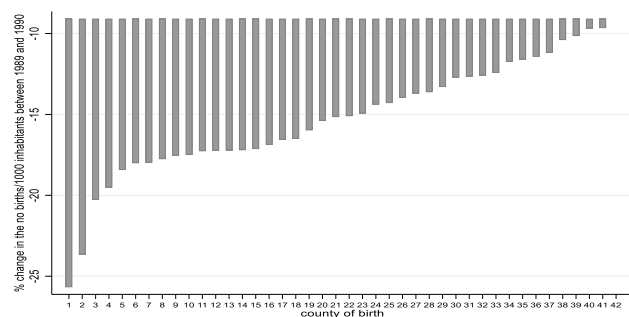
	(1)	(2)	(3)	Dependent Variable:			(7)
	# births	# Convicted		Share Convicted Individuals			
<i>Panel A. Violent Crime</i>							
	-7,699*** (770)	-34.83*** (11.19)	0.000296 (0.000493)	0.000296 (0.000480)	0.00143 (0.00222)	0.000425 (0.000476)	0.000878 (0.000517)
	<i>30535.56</i>	<i>171.7778</i>	<i>0.0056295</i>	<i>0.0056295</i>	<i>0.0056295</i>	<i>0.0049472</i>	<i>0.0056295</i>
<i>Panel B. Property Crime</i>							
	-7,699*** (770)	-50.42*** (12.92)	-0.000217 (0.000471)	-0.000217 (0.000482)	-0.00131 (0.000821)	-0.000355 (0.000435)	-0.000263 (0.000834)
	<i>30535.56</i>	<i>199.0556</i>	<i>0.0065288</i>	<i>0.0065288</i>	<i>0.0065288</i>	<i>0.005844</i>	<i>0.0065288</i>
<i>Panel C. Preventative Detention</i>							
	-7,699*** (770)	-41.08*** (8.273)	-0.000323 (0.000308)	-0.000323 (0.000312)	-0.000173 (0.000625)	-0.000422 (0.000247)	-0.0000725 (0.000259)
	<i>30535.56</i>	<i>131.3333</i>	<i>0.0042991</i>	<i>0.0042991</i>	<i>0.0042991</i>	<i>0.0035896</i>	<i>0.0042991</i>
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month Linear Trend	Yes	Yes	Yes	No	No	Yes	Yes
Birth Year Dummies	No	No	No	Yes	No	No	No
Birth Month Split Trend	No	No	No	No	Yes	No	No
Composition Controls	No	No	No	No	No	No	Yes
Outcome restriction	No	No	No	No	No	conv. before age 26	No

Note – This table estimates the effect of the 1989 legalization of abortion on the number of births, number of convictions by crime type and share convicted in Panels A and B and the number and share sentenced to preventative detention in Panel C. The baseline results with birth month fixed effects and a month of birth linear trend are in columns (1) - (3), while robustness and sensitivity checks are presented for the main variable of interest (the share convicted) in columns (4) - (7). The baseline analysis sample is used, such that it includes individuals born from January 1989 to December 1991, i.e. individuals 1.5 years on either side of the reform. Thus, N = 36 in all columns. The dependent variable is the number of convictions observed in the crime register from 1997 to September 2017. Column (6) assesses the sensitivity of the results to censoring in this data, as the youngest cohorts are only observed until age 25. Column (7) includes controls for potential compositional effects: averages of ethnicity and the parents' education (gymnasium, secondary, university) and years of birth observed at the 1992 Census. Standard errors in parentheses. Property and violent crimes are defined in the text. Significance noted by *** 1%, ** 5%, and * 10%. Data sources: The Romanian Demographic Year Book (2005) (birth data) and the National Penitentiary Administration (crime registers). Pre-treatment means in italics.

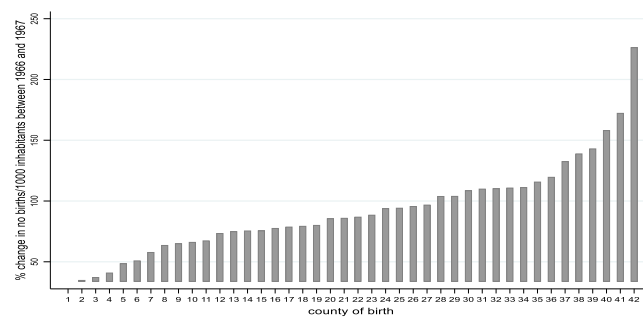
the small sample of 36 birth month cohort observations. Our first approach to assess this issue is to transform the national time series into a county by birth cohort panel (42 counties \times 36 birth cohorts = 1,512 observations). This is not our preferred baseline specification given that (i) the policy variation is national and (ii) we cannot perfectly measure the cohort specific birth rates at the county level. With respect to the second concern, we can only measure the approximate number of county births per birth cohort as observed in the full 1992 census. Using this measure, Panel A of

Fig. 3 demonstrates that the effect of 1989 legalization varied substantially across counties: birth rates decreased by more than 25% in one county and as little as 9% in other counties. With these measurement issues in mind, **Table 5** presents two sets of results. First, to demonstrate the high correlation between the approximate and actual measures of births, columns (1)-(3) present the results of estimating Eq. (1) at the national level (with 36 observations) when using this alternative measure of births. These results are very comparable to the baseline seen for the 1989 reform in Panel

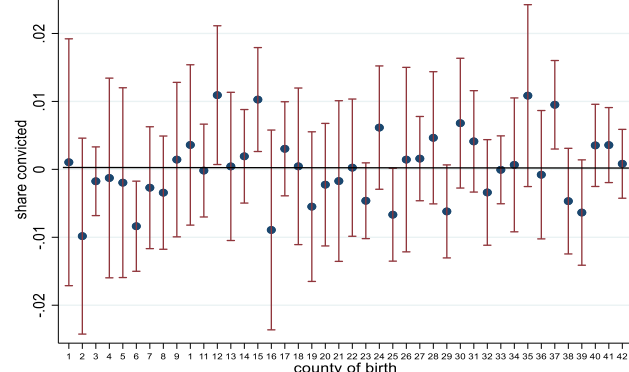
Panel A. % Change in Births After the 1989 Legalization



Panel B. % Change in Births After the 1966 Abolition



Panel C. County Estimates of 1989 Reform Impact on Share Convicted



Panel D. County Estimates of 1966 Reform Impact on Share Convicted

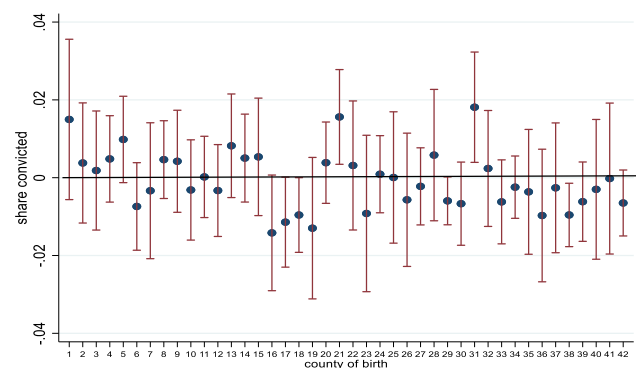


Fig. 3. County Variation in the Impact on Birth Rates and Conviction Rates. Note – Panels A and C presented the estimated percent change in birth rates after the 1989 and 1966 reforms for each of the 42 Romanian counties. We calculate the number of births for each county around the two reforms using the full 1992 census. In the main text we discuss the possible concerns regarding these numbers. Counties are sorted on the size of the change in Panels A and C. Panels C and D present the results of estimating the baseline specification (Eq. (1)) separately for each county for the 1989 and 1966 reforms respectively, where the dependent variable is the share convicted; coefficients and 95% confidence intervals are plotted. Counties labels coincides within each reform (Panels A and C for the 1989 reform; Panels B and D for the 1966 reform) but not across reforms (county \times for the 1989 reform is not the same as county \times for the 1966 reform). Data sources: The 1992 Romanian Census (birth data) and the National Penitentiary Administration (crime registers).

Table 5

Estimating the Impact of the 1989 and 1966 Reforms Using County of Birth Variation.

	(1)	(2)	(3)	(4)	(5)	(6)
	Main specification with #births approximated using the 1992 census			Using County of Birth Variation		
	Dependent Variable:					
	# births approx.	# Convicted	Share convicted	# births approx.	# Convicted	Share convicted
Panel A. 1989 Abortion Legalization						
	-6,682*** (618.5)	-91.33*** (19.13)	0.000222 (0.000847)	-159.1*** (6.409)	-2.175*** (0.422)	0.000151 (0.000784)
N	36	36	36	1,512	1,512	1,512
Panel B. 1966 Abortion Abolition[^]						
	25,672*** (3,776)	628*** (83.54)	-0.00111 (0.00113)	611.2*** (23.83)	14.95*** (0.793)	-0.000773 (0.000992)
N	36	36	36	1,512	1,512	1,512
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes
County of Birth FE	No	No	No	Yes	Yes	Yes

Note – This table estimates the effect of the 1989 legalization of abortion (Panel A) and 1966 abolition (Panel B) on the number of births, number of convictions (for any crime type) and share convicted using an approximation of the number of births (by year, month and county of birth) from the full 1992 census. The baseline results are in columns (1) - (3) we replicate the baseline specification at the county level using the approximation of the number of births. Columns (4)-(7) show the same specification but we aggregated the data at the month-year and county of birth level (42 counties). The analysis sample is individuals born from January 1989 to December 1991 and January 1966 to December 1968 for the 1989 and 1966 reforms (36 observations in columns (1)-(3) and 1,512 observations in columns (4)-(6)) who have not moved in another county or died before 1992. As before, the dependent variable is the number of convictions observed in the crime register from 1997 to September 2017. [^]For the individuals born around the 1966 reform we only know the county of residence. Standard errors in parentheses. Data sources: The Romanian Demographic Year Book (2005) (birth data) and the National Penitentiary Administration (crime registers). Significance noted by *** 1%, ** 5%, and * 10%.

A (columns 1–3) of [Table 3](#).³³ Second, taking advantage of the variation across counties in the impact of the reform, columns (4) – (6) present the parallel results when using the county of birth by birth-month cohort panel; county fixed effects are added to the baseline specification. The fact that we again see a significant impact of the 1989 reform on the level of crime but not the share convicted suggests that these results are not driven by the small sample size in the aggregated baseline data. We will provide further evidence that such aggregation does not drive the results by using an alternative data source to conduct an individual level analysis of the effect of the reform on one's propensity to engage in risky behavior in [Section 5.5](#).

5.1.2. 1966 Abolition

Panel B of [Table 3](#) presents a parallel set of baseline results for the 1966 abolition of abortion. Abolishing abortion immediately increased the number of births by 32,355 (133% relative to the pre-treatment mean) and the number convicted of any offense resulting in incarceration by 129%. Similarly sized effects are seen for violent and property crimes in [Appendix Table 4](#). Yet, as for the legalization of abortion, column (3) shows that abolishing abortion did not significantly affect the share of individuals with a conviction; the point estimate represents an increase in the conviction rate of 1.5%. The associated 95% confidence interval includes effects ranging from an 11% decrease to an 8.4% increase. This non-effect is robust to specification (birth year dummies and split trends) and age at which the outcome is measured. Again, there are no effects for the additional outcomes of prison sentence length, time served, age at arrest, and recidivism (see [Appendix Table 3](#)).

Finally, we also demonstrate that the results are not driven by the sample size by again turning to a county by birth month cohort panel. Unfortunately, compared to the 1989 reform, it is less trivial to obtain county of birth statistics at the birth month cohort level for the 1960s. The best we can do is to again use the full 1992 census, but we are losing now about 20% of the births.³⁴ Yet, columns (1) – (3) of [Table 5](#) demonstrate that there is a high degree of correlation between our approximation of the county of births and that when aggregating the data we find similar effects as the baseline (Panel B, [Table 3](#)). Finally, when using this county specification variation in columns (4) – (6), we again find significant level effects but no effect on rates.

5.2. Robustness to compositional changes

This section demonstrates the insensitivity of the results to controlling for observable compositional changes. Column (7) of [Table 3](#) presents the results of estimating the baseline specification when including controls for the 1989 legalization in Panel A and the 1966 abolition in Panel B. For the 1989 reform, we include averages for ethnicity, parents' education and parents' years of birth from the 1992 Census.³⁵ For the 1966 reform, we use data from the 1977 Census to control for potential compositional changes,

³³ The correlation between the two measures is 0.98. We lose about 11% of the sample, because of e.g. selective mortality or migration. This is perhaps not a big concern since a similar share of individuals are affected on either side of the reform cut-off point.

³⁴ Again, the correlation between the actual number of births and our approximation is as high as 0.92. Because in 1992 the individuals born around the 1966 reform cut-off point are 25, our measure suffers from more severe bias due to mortality and migration. Moreover, for the 1966 cohorts we do not have the county of birth for those convicted but rather the county of residence. However, it is important for the validity of these results that the percent does not vary before and after the cut-off point.

³⁵ We include averages at the month and year of birth of the child for: children's ethnicity: Romanian or other, parent's education: gymnasium (grade 1–8), secondary (9–12) and university, and parents' year of birth. The results are also robust to controlling for county of birth.

i.e. measured when children born around the 1966 reform were still living with their parents. Likewise, adding these controls does not impact the estimates of the reforms on violent and property crime rates nor preventative detention rates (for the 1989 reform in [Table 4](#)).

5.3. Sensitivity and robustness checks

This section shows a number of additional sensitivity and robustness tests to our main specification and discusses whether (or how) some possible GE related effects may affect our results. First, [Appendix Table 5](#) demonstrates that the results are insensitive to the sample window studied. Our baseline conclusions are insensitive to using a window of 18-months on either side (the baseline) or expanding it to 24 or 30 months per side: there is no effect of the abortion reforms on crime rates. Of course, these specifications will only adequately capture changes if they are linear in nature. Yet, when reducing the baseline window to 12 and 6 months per side (clearly asking a lot of this small sample, but where trends may be less relevant), we still see no effect. Moreover, though the nature of our data do not allow us to conduct any within household analyses, we note that the effects are the same even when using a sample period short enough to imply different households on either side of cut-off. To the extent that our baseline results are affected by spikes in abortions and we would only measure the very immediate impact of the policies (i.e. on birth month cohorts born immediately after the reform), [Appendix Table 5](#) shows that our results remain the same after excluding donuts around the reforms (one or two months on either side).

Next, we return to the county level variation in the effect of the reform on births and demonstrate that the same pattern of results is seen regardless of the size of the fertility shock. Specifically, as described before, the reform effects were large in some counties but small in others: with the 1989 legalization, county birth rates decreased by as little as 9% and as much as 25.6%, while with the 1966 abortion ban county birth rates increased from about 33.7% all the way to 226% (in Bucharest county). Thus, Panels C and D in [Fig. 3](#) present the results of estimating the baseline specification separately for each county for the 1989 and 1966 reforms respectively, where the dependent variable is the share convicted. The 42 counties are ordered on the x-axis based on the relative effect size of the reform on births, i.e. from the greatest to smallest reduction in births following the 1989 legalization (Panel C) and from the smallest to largest increased in births following the 1966 abolition (Panel D). Regardless of the first-stage impact of the reforms on births, there is little evidence of any effect on crime rates. Moreover, there is no consistent pattern of larger effects on crime rates for counties with the largest effects on births. This pattern for 1989, where we found little evidence of composition effects, is particularly suggestive that unwantedness does not significantly impact crime.

Finally, we expand the discussion on whether our empirical strategy can successfully abstract from GE related effects of the abortion reforms. Our baseline specification (and supplementary robustness tests) relies on the use of cohorts born within short time windows around the reform and controlling for trends to ensure that we capture the effect of the reforms on crime over and above any possible GE effects, as such effects are not likely to vary discontinuously around the cut-off. In particular, given the outcome of interest is crime and risky behavior, perhaps the most relevant GE effects are with respect to the criminal justice and health systems: there is no reason to think that police or prison capacity when individuals reach age 15 (i.e. ages at which criminal activity occurs) are discontinuously different for individuals just *born* on either side of the cut-off.

This contrasts the kinds of crowding effects (i.e. a cohort specific effect) that could arise in the classroom, where all students in a school year may be exposed to a class size shock even if just part of the school cohort was directly treated. Though this specific type of GE effect can occur in the Romanian context, it is also a concern that we can empirically address. In Romania, for the children affected by the 1989 reform, most kids enrolled in grade 1 in September of the year following the calendar year in which they turned 6 years of age. This means that among the children in our sample, those born in 1990 (and 1991) were assigned to e.g. smaller classes than children born the year before, in 1989. Similar crowding effects can occur as these individuals, for instance, exit high school and enter the labor market. This, in turn, could have positive long-term effects, including lower likelihood of committing crimes. Our results do not change when using the baseline sample of 36 birth month cohorts around the 1989 reform, but controlling for an extra dummy variable indicating whether the birth month cohort was potentially exposed to such a crowding effect. In this way, we allow for a separate treatment effect for individuals born in the treated school year – even if they were not directly treated themselves. Neither the main treatment dummy nor this spill-over treatment group show any evidence of an impact on crime rates.³⁶ Moreover, to the extent that such crowding effects play more of a role in counties with the largest fertility shocks, we re-estimated this specification separately for each county and found no evidence of such a GE (crowding) effect, even in the counties with the largest ‘treatment’.

Lastly, another GE related effect is the within family effect, where all children, including those born in the control period, could be directly affected because of changes to the family environment by the birth of unwanted sibling(s). We believe that, for children born in the narrow window used in our estimation (and the robustness of our results when we decrease the window even further up to 6 months on each side of the cut-off points), we can assume that family changes related to the birth of an unwanted child born outside this window should have a similar effect on children born on either side of the cut-off. As a result, this type of GE family effect, should not affect differently children born around the cut-off points.

5.4. Other crime related Outcomes: Mental health and risky behavior hospitalizations

The analyses thus far highlight that the (de)legalization of abortion had significant effects on crime levels but not rates. Does this pattern extrapolate to offenses that do not result in incarceration or criminal behavior at different points in the age-crime profile? This is perhaps particularly relevant for the 1966 cohorts, for whom crime is measured at ages beyond the peak of the age-crime profile. In the absence of arrest data for offenses that do not result in detention, we complement our analysis with hospitalization data (detailed below) for mental and behavioral disorders and risky behavior. These variables are of interest because they are highly related with criminal behavior: they are both known-risk factors of crime and potential consequences of crime and incarceration. Piquero et al (2014) provides an overview of literature relating physical and mental health to crime. One example is Sailas et al (2005), who find a seven times higher mortality rate among young male offenders sentenced to prison than an age-matched population sample, as well as an association with hospitalization for psychiatric disorders or substance abuse. Moreover, there is an increasing number of papers demonstrating a direct causal channel between both mental health and health care access and crime (see Webbink et al. (2012), Jácome (2020), Bondurant et al. (2018), Wen et al. (2017), Vogler (2017), and Aslim et al. (2019)).

As shown in the Data section, the main diagnoses included in the *mental and behavioral disorder* category include mood disorders such as depression or bipolar disorder (30%), schizophrenia related disorders (31%), and mental and behavioral disorders due the use of alcohol, drugs, or other substances (over 15%), while the main diagnoses in the *risky behavior* variable are injuries, transport related accidents (84%), and poisoning, burns, and toxic effects of substances (15%). As in the case of the prison registers, for both hospitalization categories, we look at both the number of hospitalizations and the hospitalization rate per birth cohort size. See Table 1 and Appendix Table 1 for analysis sample and population summary statistics, respectively. Given that we have hospitalization registers from 2007 to 2017, the average age when hospitalized is around 22 (45) for the 1989 (1966) reform sample. About 4% and 10% of the 1989 sample are hospitalized for mental/behavioral disorders and risky behavior respectively, while the comparable numbers for the 1966 sample are 11% and 7%.

In addition, mental health and risky behavior hospitalizations occur with much greater frequency than incarceration, implying that we are potentially capturing a different marginal individual. Of course, some of these risky behaviors (e.g. drug use) are also crimes in and of themselves. We can only observe these registers from 2007 to 2017. While this again implies, for instance, that those born in 1967 are between the ages of 40 and 50, these outcomes are not subject to the same age profile as crime.

Table 6 presents the effect of the 1989 legalization in Panel A and the 1966 abolition in Panel B on these crime-related outcomes. Columns (1) and (2) look at the level and rate of hospitalizations for mental and behavioral disorders while (5) and (6) do the same for risky behavior hospitalizations. The remaining columns present robustness checks concerning the age at which hospitalization is measured and compositional controls. In sum, the same general story is seen. There are large and significant effects of both the legalization and abolition on the number of hospitalizations, but no significant effects on the rate of hospitalization.

5.5. Other crime related Outcomes: Propensity to be ‘At-Risk’ in 2011 Census

The previous analyses cannot study how the reforms affect the propensity to be incarcerated or hospitalized at the individual level, i.e. without aggregating the data. Appendix Table 6 presents an analysis of how the reforms affected an individual’s propensity for these behaviors using the 2011 Romanian Census data. This data is also not problem-free, as one cannot cleanly measure incarceration or hospitalization (especially by cause) in the census data.³⁷ Rather, we can see: (i) whether an individual is *institutionalized at the time of the census*; the largest category in this group, labeled “common spaces of living”, includes prison, jail, pre-trial detention, penitentiaries, and correction centers; the other largest categories include asylums, sanatoriums, and hospital homes and child protection institutions;³⁸ (ii) whether someone is reported as not living in the household for at least the last 12-months, for a reason other than education, working or family reasons. We combine

³⁶ In particular, for the 1989 reform we include a crowding indicator for the cohorts born in 1990 which is not significant - the coefficient is 0.00010 (0.00038) -, while for the 1966 reform the crowding indicator for those born in 1967 is -0.000392 (0.000468). In both specifications, the main treatment indicator remains almost unchanged.

³⁷ Moreover, with the census data our outcomes are measured at just one point in time – October 2011 – when individuals are on average 21 years old (for the abortion legalization) and 45 years old (for the abortion ban).

³⁸ Other categories with few individuals are school homes, hospitals and emergency centers and night shelters.

Table 6

The Impact of the 1989 Legalization and 1966 Abolition of Abortion on Mental Health and Risky Behavior Hospitalizations.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: # and Share of hospitalizations for Mental Disorder and Risky Behavior							
	# mental disorder	Share mental disorder		# risky behavior		Share risky behavior		
<i>Panel A. 1989 Legalization (Baseline sample months January 1989- December 1991)</i>								
	-454.4***	-0.00356	-0.000932	-0.00686	-847.3*	-0.001684	0.00119	-0.00522
	(105.4)	(0.00388)	(0.00296)	(0.00647)	(427.0)	(0.01336)	(0.0113)	(0.0103)
	<i>1495.222</i>	<i>0.0490112</i>	<i>0.0333912</i>	<i>0.0490112</i>	<i>4453.056</i>	<i>0.1457351</i>	<i>0.1134897</i>	<i>0.1457351</i>
<i>Panel B. 1966 Abolition (Baseline sample months January 1966- December 1968)</i>								
	4.892***	0.00476	0.00545	-0.00864	2.978***	0.00166	0.00148	-0.00479
	(702.1)	(0.00585)	(0.00430)	(0.0141)	(413.0)	(0.00476)	(0.00367)	(0.0123)
	<i>2628.966</i>	<i>0.0962198</i>	<i>0.0992119</i>	<i>0.0962198</i>	<i>2917.154</i>	<i>0.1054467</i>	<i>0.061046</i>	<i>0.1054467</i>
Month of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Month Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Composition Controls	No	No	No	Yes	No	No	No	Yes
Age restrictions for outcome variable:	No	No	Panel A: 18–26 Panel B: 41–49	No	No	No	Panel A: 18–26 Panel B: 41–49	No

Note -This table presents the effect of the 1989 legalization of abortion (Panel A) and the 1966 abolition of abortion (Panel B) on hospitalizations for mental disorders or risky behavior. Mental disorders include all hospitalization with the ICD-10 codes F "Mental and Behavioral Health" and risky behaviors include "External causes of morbidity and mortality" (ICD-10 codes U50-Y98) and "Injury, poisoning and certain other consequences of external causes" (ICD-10 codes S00-T98). The baseline results are in columns (1) - (2) and (5) - (6). Columns (3) and (7) restrict the sample to individuals during the same age of hospitalization. Columns (4) and (8) assess robustness to controls for compositional effects: averages of ethnicity and the parents' education (gymnasium, secondary, university) and years of birth observed at the 1977 and 1992 Census (for the 1966 and 1989 reforms, respectively). Data sources: the National Inpatient Registers. Standard errors in parentheses. Pre-treatment means in italics. Significance noted by *** 1%, ** 5%, and * 10%.

categories (i) and (ii) to identify individuals 'at risk' of being part of this criminal population. However, as these data only capture those institutionalized at the time of the 2011 census, such a snapshot can be skewed towards disproportionately observing long sentences.

When using this individual level data set, we find that Romania's abortion policies had no significant impact on this individual measure of being at risk. This finding is robust to adding controls for compositional changes and restricting the sample to individuals born just one year on each side of the reform.³⁹ Just like in Table 3, the point estimates for our preferred specification (column 1) are small and, given 95% confidence intervals, we cannot rule out an effect of the lifting of the abortion ban ranging from a 14.6% reduction to a 4.5% increase in the probability of being at risk of being in the criminal population.

We also do not find an effect when aggregating these data to the birth-month level (columns (4)-(6)) and using a measure of being at risk that parallels those in the baseline prison and hospitalization analyses. That is, the lack of an effect for the individual-level analysis is not purely driven by the specific outcome studied or a specific sample of the population. Specifically, when using the aggregated census data, we again find significant effects on the number of individuals "at risk" for both the legalization and abolition, but not on the rates. These results taken together with the individual-level analysis suggest the lack of an effect on prison and hospitalization rates (once again) is not simply driven by an aggregation of the data.

6. Discussion

Our bottom-line findings show that Romanian abortion policy – both the legalization and abolition – had large and significant impacts on the number of crimes and hospitalizations for mental disorders and risky behaviors. But, these impacts were proportionate to the change in the size of the population, such that there is no significant effect on crime or hospitalization rates. Point estimates suggest changes in crime rates of about 1%, though we acknowledge that we cannot conclusively rule out changes of up to 10%.

³⁹ The same (non)-results (available on request) are found when considering the two risk measures separately.

What do these findings imply about the main channels through which abortion reform can affect crime? We discuss our conclusions separately with respect to each reform, keeping in mind that, while we try to open the black box of compositional changes and unwantedness, these channels remain extremely difficult to separate both theoretically and even more so empirically.

The findings for the 1966 reform are less conclusive with respect to the main channels in place. There is evidence of selection in general: there are both compositional changes and unwantedness around this reform, which go in opposite directions. How large could the effect from the compositional changes channel be given that previous work (Pop-Eleches, 2006) has shown that abortion restrictions led to worse health at birth, education and labor market outcomes? Pop-Eleches (2006) finds that the 'parental selection' effects are associated with changes in childrens' years of schooling of about 0.27. Scaling these results by estimates from the literature on the causal effect of education on crime from Sweden (Hjalmarsson et al., 2015) or the US (Lochner and Moretti, 2004), this should be associated with roughly a 2% (4%) change in convictions (incarceration) and 2–3% in arrests, respectively.⁴⁰ We interpret these back of the envelope calculations as providing suggestive evidence that the implied impact on crime coming from the education channel is (at the very least) small.⁴¹ If these selection effects of the 1966 reform are indeed small, then our overall null effect on crimes suggests that any unwantedness effects are either small or non-existent.

Since cohort size effects can impact crime levels but crime rates can only be affected by the compositional changes and unwantedness channels, the lack of compositional effects for the 1989 legalization strongly suggests that, despite some effects of the lifting of the ban on markers of unwantedness – arguably best captured by the large changes in fertility – this did not get reflected in changing crime rates. This result may appear surprising in light of findings from the literature in other settings. For example, Donohue and Levitt's (2001) back of the envelope calculations, which are based

⁴⁰ Hjalmarsson et al (2015) find that an additional year of schooling reduces the chance of conviction by 8% and incarceration by 16% while Lochner and Moretti (2004) estimate that, on average, a one-year increase in high school education reduces the incarceration rate by 10–15%.

⁴¹ These results also explain why controlling for observable compositional effects do not affect our crime results.

in part on assumptions from Rasanen et al. (1999), imply that, in the US, 6% of the reduction in crimes could be due to this unwantedness channel. If we use the same assumptions as Donohue and Levitt (2001), including that 75% of unwanted births are aborted, the number of convicted individuals for violent crimes should have decreased in Romania after 1990 by 86 and the crime rate by 0.00125, rather than our estimates of 34.83 and 0.000296 (in Table 5). For both of these outcomes and given the size of our standard errors, we can reject that the expected and estimated effects are the same.

Why do not we find similar effects in Romania with respect to unwantedness than Donohue and Levitt (2001) conclude in the U.S.? One may especially expect such an effect to be found given changes in some markers of unwantedness: the sharp drop in the maternal mortality rate immediately after the 1989 legalization suggests that, under the ban, many women exposed themselves to risky and costly illegal abortions. At the same time, we find no changes in the share of child institutionalization (by their parents) around the 1989 legalization, which may indicate no changes in the shares of 'truly' unwanted or unplanned children. One explanation could be that Romania during the communist period was a very traditional society with the majority of children being born into intact families. Thus, the potential negative consequences of unwantedness (as defined in the US studies) might be mitigated or offset by these dominant societal norms. Another explanation could be that abortion availability may have a different impact on unwantedness and crime if we were to measure these effects for cohorts born further away from the cut-off points, as social norms (related to unwantedness and the correlation between changes in family composition and having an unwanted kids) may not shift immediately after these reforms.

With respect to this latter point, our paper indeed demonstrates that for those birth month cohorts immediately treated by the reforms (up to 3 years around the cut-off points), there is no effect of abortion legalization or abolition on crime rates, over and above any GE related effects of the reforms. This does not mean, however, that there are no effects of the reforms on cohorts born further away or that there are no GE-related effects of the reforms on crime rates for our analysis sample cohorts. We simply cannot measure them – in fact, we would argue that this is an advantage of our research design: we can disentangle unwantedness and composition effects from such general equilibrium effects (which have similar effects for birth month cohorts immediately on either side of the cut-off). One possible channel through which such GE effects (affecting those born near and far from the cut-off points) of abortion reforms can arise is the shock to the level of crimes. The 1966 abolition sharply increased the number of offenders by 129% while the 1989 legalization decreased the number of offenders by 22%. Depending on the criminal justice system's foresight in planning for the size of the police force or prison capacity, these shocks to the number of criminals can result in a changing number of officers or prison spaces per criminal. To the extent that the ability of officers to detect and clear crimes depends on this ratio or to the extent that prison conditions change (e.g. via overcrowding), these level shocks could in theory affect the criminal propensity of the general population (Becker, 1968). Whether or not such long-run GE effects exist, and contribute to the long-run trend in Romanian crime rates, however, is beyond the scope of the current paper.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jpubeco.2021.104468>.

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