DYNAMIC COMPLEMENTARITIES IN HUMAN CAPITAL FORMATION? FAMILY AND SCHOOL ENVIRONMENTS

OFER MALAMUD
UNIVERSITY OF CHICAGO AND NBER

CRISTIAN POP-ELECHES
COLUMBIA UNIVERSITY AND NBER

MIGUEL URQUIOLA
COLUMBIA UNIVERSITY AND NBER

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ABSTRACT. There is growing interest in how conditions during early childhood influence the productivity of later human capital investments. We examine whether the educational benefits of going to a better school are larger for children who experienced better family environments because they were born after the liberalization of a restrictive abortion policy in Romania. We use an empirical strategy that combines a regression discontinuity (RD) design and a differences-in-differences (DD) framework to estimate impacts on a high stakes school-leaving exam. Although we find positive and significant effects of both going to a better school and being born under a less restrictive abortion regime, we do not find evidence of significant interactions between these two human capital shocks/investments. This suggests that dynamic complementarities may not be particularly important in this context, and points to further research to determine under what circumstances dynamic complementarities are important.

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1. Introduction

Growing evidence suggests that conditions at birth and during early childhood are important determinants of later educational and economic outcomes (Almond and Currie, 2011). These relationships may be generated or reinforced by the presence of *dynamic complementarities* whereby investments in human capital are more productive when an individual's baseline stock of skills is higher (Cunha and Heckman, 2007). Such complementarities have important implications—the greater their magnitude, for instance, the stronger the argument for prioritizing early interventions.

There is relatively little evidence, however, on these complementarities. Estimating their magnitude poses a substantial empirical challenge. As Almond and Mazumder (2013) point out, "causal inference on dynamic complementarities requires (a) exogenous variation in the baseline stock and (b) exogenous variation in subsequent investment... In an observational setting, this may be asking for lightning to strike twice: two identification strategies affecting the same cohort but at adjacent developmental stages."

This paper's contribution is to identify two separate "lighting strikes" that generate exogenous shocks to children's family and school environments in Romania, and to examine if they interact in ways suggestive of dynamic complementarities. We combine variation on how the lifting of an abortion ban affects children's family environments, with variation on how the allocation of high school slots affects their school environments. We find clear evidence that higher quality environments in each of these dimensions have positive impacts on children's outcomes, but little indication that they interact in significant ways. This suggests that at least in this setting, dynamic complementarities are not substantial.

The first source of variation comes from the repeal of Romania's 23-year abortion ban, which occurred on December 26, 1989, immediately after the collapse of the Communist regime. We evaluate the impact of this policy change by comparing children who were born before July 1, 1990, and therefore conceived prior to the repeal of the abortion ban, with children who were born after July 1, 1990. The mothers of the latter group could have taken advantage of abortion, or of other newly available contraceptive options. As a result, on average, these post-reform cohorts likely benefited from better family environments and early parental investments.

The pre- and post-reform cohorts transitioned from middle to high school in 2005 and 2006. We rely on administrative data to observe their performance on a high school admissions exam, and on a Baccalaureate exam taken at the end of high school in 2009 and 2010. Both

of these are high stakes tests, and the latter significantly influences admissions to college.[1] We use a difference-in-differences (DD) estimation strategy and find that being born under the less restrictive abortion regime resulted in higher achievement both in the high school admissions and the Baccalaureate exam.

The second source of variation—one which applies to the same cohorts—comes from the way children are allocated to high schools in Romania. Their ability to choose a school depends solely on a transition score that heavily weights scores on the national high school admissions exam (Pop-Eleches and Urquiola, 2013). After obtaining their transition score, students request schools and are allocated via a centralized process that honors higher scoring students' requests subject to slot constraints. This gives rise to cutoff scores that determine access into schools, with clear discontinuities in educational quality—particularly as measured by peer ability—at these thresholds.

We use these cutoffs to estimate the effect of attending a better high school using a regression discontinuity (RD) design. We find that, as in Pop-Eleches and Urquiola (2013), students who attend a better high school perform better on the Baccalaureate exam.

Finally, we combine these two sources of variation in a regression discontinuity difference-in-differences (RD-DD) design. This allows us to test whether children who were born under a less restrictive abortion regime experience larger Baccalaureate score improvements when they attend a better school. In other words, we ask if the later intervention—high school quality—produces greater gains among student who had higher initial stocks of skill due to being born after the abortion liberalization.

We do not find significant evidence of this—i.e., there is little evidence of an interaction between the impacts associated with the repeal of the abortion ban and the impacts associated with attending a better high school. The bottom line is that at least in this setting there appears to be little evidence of dynamic complementarities between family and school environments.

Our findings inform a recent strand of studies that use other settings to test for the presence of dynamic complementarities. Aizer and Cunha (2012) exploit exogenous variation in preschool investments from the launch of Head Start in 1966 to examine dynamic complementarities between these investments and early measures of human capital. In particular, they show that the effect of preschool enrollment on a subsequent IQ measure of cognitive

[1] We also have extensive survey data on these students, their parents, and their principals; we have not yet analyzed these data for this paper.

skills (at 4 years of age) is larger for those children with higher stocks of "early human capital" as measured by the Bailey test of cognitive development (at 8 months of age). They also present evidence consistent with parental investments reinforcing this pattern.

In addition, two studies by Bhalotra and Venkataramani (2012a, 2012b) examine whether the impacts of health interventions on educational outcomes are larger for groups that may face higher returns to human capital investments. Bhalotra and Venkataramani (2012a) show that the effect of improved water quality in Mexico on cognitive skills was larger for girls than for boys, which is consistent with dynamic complementarities if there are higher returns to investing in girls because of comparative advantage. Bhalotra and Venkataramani (2012b) show that the introduction of sulfa drugs in the United States had larger impacts for blacks in the North than in the South, which is also consistent with dynamic complementarities given that the greater presence of institutional racism in the South would have dampened incentives to invest in schooling.[2] However, these suggestive findings emerge from settings in which human capital investment cannot be directly observed, and questions about the exogeneity of these investments remain. Indeed, Bhalotra and Venkataramani (2012b) call on future work to find "exploitable situations where both endowments and subsequent investments are exogenously assigned." We believe that this paper offers the first compelling approach to exploit such a situation, as occurred in Romania.

The remainder of the paper proceeds as follows. Section 2 presents a basic conceptual framework. Section 3 provides some background to the abortion liberalization and the student allocation mechanism to schools in Romanian. Sections 4 and 5 describe our data and methodology, respectively. Section 6 presents the results, and Section 7 concludes.

2. Conceptual framework

Any analysis of dynamic complementarities must explicitly account for the production of skills at different stages of development. Heckman et al. (2007) suggests the following technology for the production of skills:

$$\theta_{t+1} = f_t\left(h, \theta_t, I_t\right)$$

where t indexes time, h represents parental characteristics, θ_t is a vector of skills, and I_t stands for parental investments in child skill. In this framework, dynamic complementarity arises when higher skills make later investments more productive:

[2] In arguing for this mechanism, they also show similar evidence from the interaction of sulfa drugs with the birth state share of slaves in the population in 1860 and the ratio of educational attainment between young black and white men.

$$\frac{\partial^2 f_t(h, \theta_t, I_t)}{\partial \theta_t \partial I_t} > 0$$

We follow this setup, except that we will allow I_t to represent school investments as well as parental investments. Our setting provides exogenous shocks to (a) the stock of skills at time t, θ_t, due to the repeal of an abortion ban, and (b) school investments, I_t, associated with attending a better school at time t. Our measure of skills at time $t+1$, θ_{t+1}, stems from the Baccalaureate exam.

Note that we can write the expression for θ_{t+1} in recursive form by substituting in for the stock of skills θ_t with all prior investments:

$$\theta_{t+1} = g_t(h, \theta_1, I_1 ... I_t)$$

where θ_1 is a child's initial level of skill. Given this expression, we are able to consider several potential mechanisms by which the lifting of an abortion ban can affect skills, θ_t, at time t: (i) prior parental investments $I_1 ... I_{t-1}$, (ii) initial skill endowments, θ_1, and (iii) parental characteristics, h.

All three of these mechanisms are potentially relevant in our context. First, the repeal of the abortion ban is likely to have led to fewer unwanted children which may have spurred greater parental investment. Second, because women from disadvantaged backgrounds experienced the largest reductions in fertility due to the repeal of the abortion ban, a compositional effect would lead children in affected cohorts to have more advantaged parents. Third, it is possible that the repeal of the abortion ban led children to have better initial skill endowments because of selective abortions. While we cannot disentangle the underlying mechanisms, we can show that the lifting of the abortion ban led to higher skills, θ_t, at time t.[3]

3. Background

3.1. **The liberalization of abortion and birth control in 1989.** This section provides a brief description of Romania's unique history of access to abortion and birth control.[4] After the introduction of liberalized access to abortion in the 1950's, abortion become the main birth control method in Romania.[5] In 1966, the Romanian government abruptly outlawed abortion for most women of reproductive age and severely restricted access to other modern

[3] Note that we are assuming that parental investments at time t are not affected by the lifting of the abortion ban or attending a better school. In fact, we can examine this directly using information from the parental survey conducted while students are in high school.

[4] This section is based on Pop-Eleches (2006). For further discussion see Kligman (1998).

[5] In 1965, there were four abortions performed for every live birth (Berelson, 1979).

methods of contraception.[6] This policy was maintained with very minor modifications until the fall of communism in December of 1989.

The renewal of liberal access to abortion in 1989 was equally abrupt and is the focus of the current paper. Specifically, following the civil unrest that started in Timisoara in mid-December and spread to other parts of the country, Romania's dictator was executed on December 25th. On December 26th, the interim leadership abolished the ban on access to abortions and, in early January of 1990, the ban on import of modern contraceptives was lifted.[7]

Figure 1 plots the 1956-1997 fertility rate for Romania as well as for the average of three other Eastern European countries (Hungary, Bulgaria, Russia) that did not have similar restrictions on the supply of birth control methods during this period. The initial increase induced by the 1966 abortion ban was dramatic, with the total fertility rate roughly doubling between 1966 and 1967 (from about 1.9 to 3.7 children). Following the initial spike in fertility, the number of births did stabilize in the 1970s and 1980s, but at a higher level.[8] Following the lifting of the ban, one can observe an immediate drop in fertility in 1990 along with a downward trend between 1992 and 1997 that is similar to trends in Hungary, Bulgaria and Russia. The gradual decline in fertility for transition economies during the 1990s is likely the result of the social and economic transformations in these countries following the fall of communism.

In another paper, Pop-Eleches (2010) has argued that the sharp drop in fertility immediately after July of 1990 is driven by changes in access to abortion and not by changes in access to other methods of birth control or by changes in the demand for children caused by the transition period.[9] The most convincing evidence that the change in fertility was caused by the repeal of the abortion ban is presented in Figure 2, where data from the 1992 census are used to plot the number of children born in particular months during 1989-1991. There is an abrupt one-time drop in fertility exactly six months after the lifting of the ban—in July of 1990—without any apparent trend in birth rates during this period, precisely when children conceived up to three months prior to the repeal of the ban could have been aborted.

[6] Exceptions were made in cases in which women were over the age of 42 or had more than four children. Expections were also possible given some special circumstances, such as health problems, rape, or incest.

[7] According to an account of a Romanian gynecologist, his hospital was stormed by over 300 women in need of an abortion on December 27 of 1989 (information based on a personal interview).

[8] The increase in fertility after 1984 is due to further restrictions of the abortion regime. In addition to stricter monitoring of pregnant women, the minimum abortion age was increased from 42 to 45 years, and the minimum number of births in order to be able to receive a legal abortion from 4 to 5.

[9] The evidence presented in Pop-Eleches (2010) shows that between 1990 and 1992 there was no increase in the use of modern contraceptives in Romania.

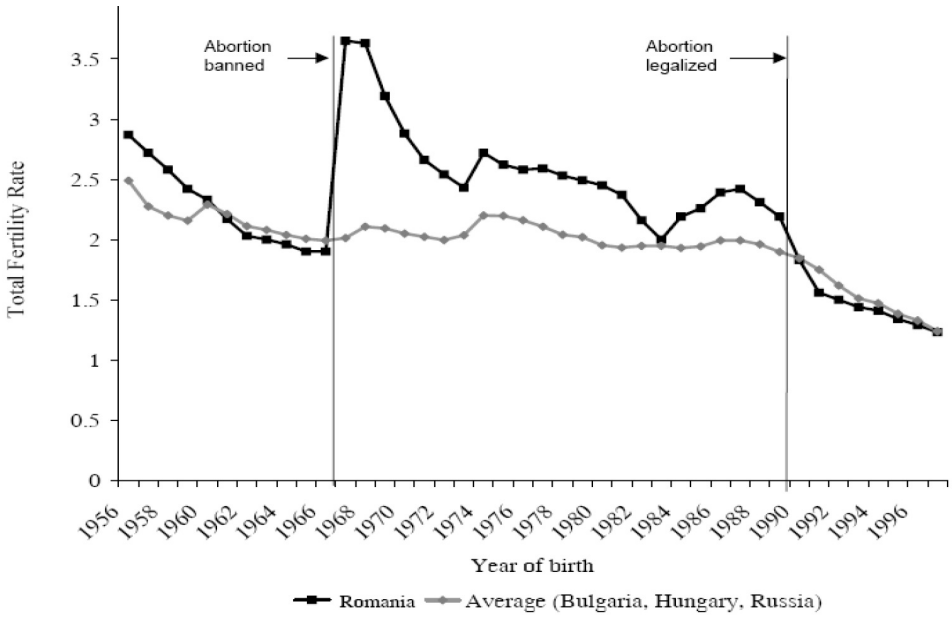


FIGURE 1. Total fertility rates

Notes: The total fertility rate is the average total number of children that would be born per woman in her lifetime, assuming no mortality in the childbearing ages, calculated from the age distribution and age-specific fertility rates of a specified group in a given reference period.

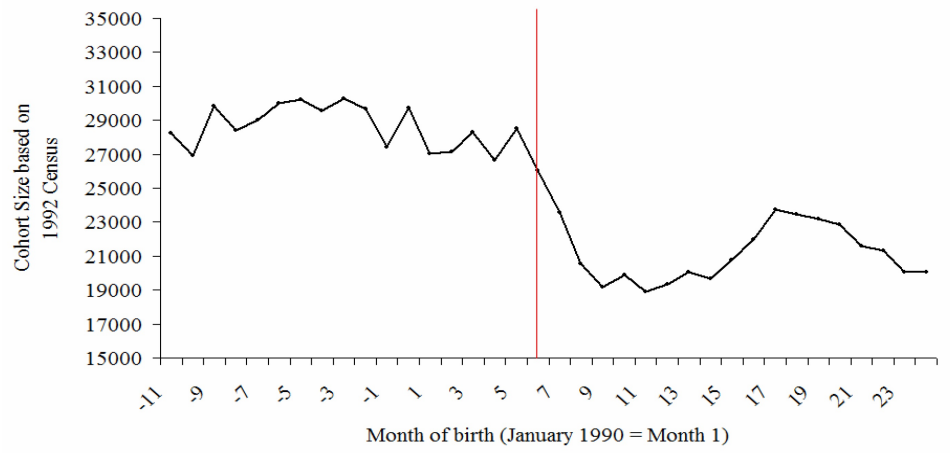


FIGURE 2. Total fertility rates

Notes: The total fertility rate is the average total number of children that would be born per woman in her lifetime, assuming no mortality in the childbearing ages, calculated from the age distribution and age-specific fertility rates of a specified group in a given reference period.

There are two alternative stories that could explain the drop in fertility after July of 1990 that are discussed in greater detail in Pop-Eleches (2010). First, the government introduced a number of pronatalist incentives in 1966 in addition to the ban of abortions and modern contraception. These incentives, although small (one time paid medical leaves during

pregnancy, one time maternity grant of about $85, and a $3 increase in the monthly child allowance) might have changed the demand for children. The change in demand for children would be of concern for the present analysis only if the government also repealed these incentives in December of 1989, at the same time as the change in abortion legislation. According to a report by the World Bank (1992), no major changes in the provision of maternity and child benefits happened in this period.[10] Second, the fall of the communist regime might have caused people to immediately update their expectations about the future. They might have made immediate adjustments to their life-cycle behavior, including fertility decisions. While this is intuitively possible, no such changes in fertility were observed in other Eastern European countries following the collaps of communist regimes (David 1999). Instead, the consensus among demographers working in this area (David, 1999) is that the decline in fertility during transition was gradual and reflected the continuous worsening of economic conditions.

Below we will focus on the December 1989 lifting of the abortion ban and analyze the outcomes of children born immediately before and after July of 1990, the time when the ban affected fertility levels. Since the size of the monthly birth cohorts after July of 1990 declined from roughly 30 to 20 thousand births, this implies that about 1/3 of children born in the months prior to 1990 were born as a result of the restrictions to abortion.

Since this paper presents evidence on the effect of changes in access to abortion on child educational outcomes, it is crucial to know the mechanisms through which changes in an abortion regime affect the selection of women who give birth. First, average educational outcomes could be affected by changes in the *socio-economic composition* of women who carry pregnancies to term. The direction of the effect depends on which type of women are more likely to use abortion as supposed to other methods of birth control.[11] In terms of our conceptual framework, this corresponds to changes in parental characteristics, h. Second, changes in access to abortion can affect the number of children who are unplanned or unwanted at birth. This effect of being *unwanted* can arise for a number of reasons: (1) through changes in initial skill endowments, θ_1 , when lack of access to abortion does not allow parents to decide whether to carry a pregnancy to term based on fetal health (Grossman and Jacobowitz 1981; Joyce 1985; Grossman and Joyce 1990); and (2) when the abortion ban leads to changes in prior parental investments $I_1...I_{t-1}$. This latter effect could arise

[10] The level of social benefits during the transition period certainly changed in Romania as in most other transition countries. This reflected budgetary cuts caused by the severe drop in GDP and might have been a contributing factor for the gradual decrease in fertility during the 1990's.

[11] Ananat et al. (2006)suggest the possibility of another source of selection given that changing the cost of abortion will also change pregnancy behavior. The present study assumes that at least in the short period studied immediately after the change in abortion regime there are no changes in marginal pregnancies.

when childbearing does not occur at an optimal time and therefore it can affect educational, marriage and labor market decisions of a woman (Angrist and Evans, 1999, Goldin and Katz, 2002); or when an undesired additional birth affects lifetime fertility, resulting in child outcomes being are negatively influenced through the standard child quality/quantity trade-off (Becker and Lewis,1973; Becker, 1981). Finally, any changes in cohort size caused by legislative changes regarding abortion can affect educational outcomes through changes in the *crowding* of educational resources.

3.2. **The student allocation mechanism.** Next we provide a brief description of the high school admission process that provides exogenous variation in school environment and related investments.[12] The transition between middle and high school (8^{th} to 9^{th} grade) in Romania results in an unusually systematic allocation of students to schools. Specifically, every child who completes middle school receives a transition score which equally weights: i) her performance in a national 8^{th} grade exam covering Language, Math, and History/Geography,[13] and ii) her gymnasium (grades 5-8) grade point average.

After receiving their transition scores, students are required to submit a list of ranked choices specifying combinations of: i) a high school, and ii) one of seven academic tracks: Mathematics, Natural Sciences, Technical Studies, Services, Social Studies, Literature, and Natural Resources and Environmental Protection.[14] These tracks constitute "schools within a school" since students in each track take all their coursework together and do not take classes with members of other tracks—although they share infrastructure and a principal, meet during breaks, and may share teachers. Not all schools offer all tracks, but all must submit their track-specific capacities in advance, and these are public information.

Students' school/track choices are expressed through an application form submitted (through their middle school) to the Ministry of Education .[15] Using a computerized system, the Ministry then ranks students by their transition score—no other criteria (e.g., sibling preferences or geographic proximity) go into the ranking. The mechanism considers the highest ranked student and assigns her to her most preferred school/track choice. It then moves on to the second and treats her similarly. Eventually, the procedure will reach a student whose first choice is full. If this happens, it tries to assign the student to her second choice; if that one is full as well, then to the third, and so on. Only once this student has been assigned to a

[12] This section draws heavily on Pop-Eleches and Urquiola (2013).

[13] All tests and grades use a scale ranging from 1 to 10, with a passing grade of 5. Students who score below 5 are not allowed to apply to high school, but can enroll in vocational school.

[14] For the 2001 sample, the administrative data on tracks is not as precise; it combines three of the tracks (Technical Studies, Services, and Natural Resources and Environmental Protection) into one technical track.

[15] During the period we study, schooling in Romania was compulsory until the 10^{th} grade. As a result the entire cohort of students who complete middle school is required to participate in this allocation process.

school does the mechanism move onto the next person.[16] Under this set up students have incentives to truthfully reveal their preference rankings.[17]

Schools must enroll the children in the admission list returned from the computerized allocation. In cases in which they offer multiple classes of the same track, the system just returns the list of students admitted into the track, without further instructions on how to divide them into classes.

As shown in the regression results below and consistent with the evidence in Pop-Eleches and Urquiola (2013), the result of this process in most markets is a clear hierarchy of schools by average peer quality, average observable teacher characteristics, and average parental effort. The lack of administrative data on the actual school choices that families submit prevents us from fully exploring what information is used to make these choices. The anecdotal evidence indicates that school quality is the most important determinant of school choice in Romania. That said, we cannot make definitive statements, for example, on how parents' rankings of schools weigh aspects like peer quality, parental participation, or teacher value added.[18]

4. Data

The current version of this paper relies on administrative information covering the universe of children in the 2005 and 2006 high school admission cohorts in Romania, These data provide the name, date of birth, gymnasium, transition score, and the allocated school/track for all students, but no information on their ranking of school/tracks or their socio-economic characteristics. For this sample we linked admissions data with information on whether students took the Baccalaureate exam and how well they performed (these cohorts took the exam in 2009 and 2010 respectively).[19] A satisfactory Baccalaureate grade is a prerequisite

[16] Some students only request school-track choices with minimum entry scores above their own transition score. These individuals are assigned, in a second round, to schools/tracks that did not fill. Students are warned against this outcome and allowed to submit a list of choices of essentially unlimited length. As a result, for example, in 2007 only 1.1 percent of applicants moved to the second round.

[17] The existing legislation does not allow children to decline their initial assignment, although in rare situations children do manage to switch schools and/or tracks over the years. Such switching does not pose a threat to our "intent-to-treat" research design, which as discussed below, is based on the assigned school/track.

[18] See MacLeod and Urquiola (2012) for a framework in which a preference for schools with better reputation might result in tradeoffs between attributes like peer quality and teacher value added.

[19] We merged the admission and Baccalaureate data by student name and county using a fuzzy matching technique to allow for some misspelling of names. Our conclusions are not sensitive to different levels of precision in the matching algorithm, and are also similar if we use only exact matches. Our matched data do not allow us to differentiate between high school drop-outs and students who complete high school but do not take the Baccalaureate exam.

for applying to university, and an excellent one raises the probability of admission to the most prestigious institutions.[20]

Our main sample covers the universe of students admitted to high school during these years, with three exceptions. The first two reflect that, as explained below, we rank schools and set cutoff scores under the assumption that towns are self-contained markets. We therefore omit the capital, Bucharest, which is composed of six towns the borders of which students can cross with relative ease. We do not find this omission to affect our key conclusions. Second, when our analysis focuses on between-school cutoffs, we omit towns that have only one high-school.[21] Third, we drop all students who enroll in the vocational sector; this precludes their access to higher education and hence we do not observe Baccalaureate outcomes for them.[22]

Our main cohort of interest is composed of children born in 1990, since the drop in fertility as a result of the lifting of the ban at the end of December of 1989 started roughly six months later, after July of 1990. The children born between January 1st and December 31st of 1990 were required by law to enroll in first grade in the fall of 1997 and scheduled to graduate from grade 8 in the spring of 2005. While non-compliance with this law could lead to the selection of children into a particular secondary school cohort, we expect a similar pattern of selection in the 2006 cohort which serves as our control group.[23]

Another source of selection could arise if the composition of children participating in the centralized secondary school allocation process is affected by the lifting of the abortion ban. The direction of the bias is likely to be downward if children born under the abortion ban have lower school performance in primary school and therefore a larger fraction do not take the exam for entry into secondary school due to higher dropout rates or grade repetition

[20] The Baccalaureate exam is administered nationally. Students usually take six component exams, with a combination of common subjects (written language, oral language, written foreign language) as well as two track-specific exams and one elective exam. The overall grade is the unweighted average of these scores. The exam is first administered in July. Students who fail are allowed to retake the exam in August (we use the August score for students who took the exam twice). Additionally, students are generally not allowed to take the Baccalaureate exam early.

[21] Despite these omissions, for simplicity we will describe the sample as covering "all towns" unless we focus only on those towns covered by our specialized survey.

[22] The omission of students who enroll in vocational tracks could be problematic if the probability of enrolling is affected by options in non-vocational schooling. We nonetheless decided to drop these students because it is very unlikely that a large proportion of students would prefer to attend a vocational track over a non-vocational track. We have explored the actual school/track choices as collected ex-post in our survey sample. These responses may suffer from ex-post rationalization as discussed below, but it is worth noting that less than 1 percent of students who attend a non-vocational track claim that they ranked a vocational track above their assigned track. For further information on vocational education in Romania, see Malamud and Pop-Eleches (2010).

[23] We did not find any indication for institutional changes at the secondary education level in Romania (both in terms of the structure of the school system or the rules of the admission process) between 2005 and 2006.

rates. However, we can show that this source of selection is unlikely to play a major role in our setting. Using the 1992 Romanian Census, we demontraste that the proportion of children born in each month of 1990 who are present in the 2005 high school cohort is very similar to the proportion of children born in each month of 1991 who are present in the 2006 high school cohort.

Since the administrative data does not contain any background variables for the children, we use data from the 2002 Romanian Census to describe the effect of the lifting of the abortion ban on the composition of children affected by the abortion ban. The education level of the mother serves as our main variable to understand how the lifting of the abortion ban has changed the composition of families that have children. In addition we use the age of the mother at birth to understand how the change in the abortion policy affected the timing of births. Any change in the age of the mothers after the repeal of the ban would be an indication that under the restrictive regime a share of the births were not optimally timed. Note that we can only recover maternal characteristics for those children who still live with their mothers at the time of the census in 2002; but since children born in 1990 were only 12 years old at the time of the census, the matching rate was very high at 95 percent.[24]

Finally we provide summary statistics for the main variables used in the study (NOTE: Summary statistics table to be added in future draft). The average admission grade for the 2005 secondary school admission cohort is 6.55 (on a scale from 1-10), about 63% of them get admitted to a high school and 36% to the most prestigious academic high schools. The means for the 2006 cohort are similar, although the average admission grades are somewhat higher (6.67). In the 2002 census, mothers who have given birth between 1988 and 1992 are on average less than 25 years old at the time of birth, they have given birth to 2.77 children by 2002 and have the following educational achievement: 44% have primary education or less (8 years of schooling or less), about 48% have secondary education and the remaining 7% hold a university degree.

While the administrative data set offers substantial sample sizes, it contains only basic information. To explore behavioral responses, we therefore implemented a survey that featured principal, parent, and student questionnaires. The analysis of this survey data will be included in a future draft.

[24]We have not found any evidence that the abortion ban has changed the probability of living with a parent in 2002.

5. Empirical strategy

Our empirical strategy proceeds in three steps. First we describe a difference-in-difference framework that can be used to estimate the impact of the abortion ban on child outcomes. Second, we propose a regression discontinuity approach to estimate the effects of attending a better school. Third, we combine the difference-in-differences (DD) and the regression discontinuity (RD) design into a combined framework that allows us to estimate dynamic complementarities between family and school environments. We describe each of these three approaches in this section.

5.1. **Estimating the impact of the abortion ban.**

The first step of our analysis is to estimate the impact of the 1989 repeal of the abortion ban on educational outcomes. We consider the following regression:

$$y_{it} = \theta_0 + \theta_1 Unwntd_{it} + \theta_2 trend_i + \theta_3 cohort_t + \epsilon_{it}, \tag{1}$$

where y_{it} is an educational outcome measured either at the start or at the end of high school. $Unwntd_{it}$ is equal to 1 if individual i was born before July 1, 1990, which is six months after the repeal of the abortion ban and also the point at which the decrease in fertility is first observed in birth data. $trend_i$ is a quadratic polynomial in the month of birth, and $cohort_t$ is a dummy taking on a value of one for children in the 2006 admission cohort, and of zero for those in the 2005 admission cohort.

The overall impact of the change in abortion legislation is captured by the coefficient θ_1. This could be due to either of the two mechanisms discussed earlier, an *unwantedness* or a *composition* effect. Note that since we restrict attention to a particular school cohort, any crowding effects due to changes in the cohort size—resulting from change in access to abortion and birth control methods—should be constant within a particular cohort.

All specifications are restricted to those children who should be in their respective admission cohort based on their date of birth; that is children born in 1990 (1991) and present in the 2005 (2006) high school cohort. Some specifications only include the 2005 cohort, while others replace the $trend_i$ controls with a set of calendar month of birth dummies. Our estimation approach essentially compares the outcomes of children born six months before and after the the repeal of the abortion ban. We estimate this change using the parametric regression described above and assume that $trend_i$ accounts for any effects that are associated with age and vary continuously. At the same time the 2006 cohort controls for possible

month of birth effects as well as possible selection effects of a birth cohort into a corresponding high school admission cohort. Standard errors are clustered by age in months (Bertrand, Duflo and Mullainathan 2004).

5.2. **Estimating the impact of going to a better school.** The second step of our analysis is to estimate the impact of attending a better school for the same cohorts of children used in step one. Although, in principle, a student can request any high school in the country, we suppose that students restrict their choices to the towns they live in, a reasonable assumption since the applicants are 13-14 year olds likely to be living with their parents. Within each town, we rank schools and school/tracks (in separate exercises) according to their average score, and set the cutoffs equal to their minimum scores.[25] In other words, we set each school's (or school/track's) cutoff equal to the score of the child with the lowest transition score.[26]

This yields more than 1000 quasi-experiments if one considers schools since each cutoff score makes for a potential RD analysis. In this section we first discuss the conceptual basis for analyzing any given one of these experiments, focusing on schools for simplicity. We then describe how we go about summarizing them.

5.2.1. ***A single between-school cutoff.*** Consider a town in which i indexes students and $s = 1, \ldots, S$ indexes schools, where the latter have been ordered from the worst to the best in terms of the average transition score observed among their students. Additionally, let $z = 1, \ldots, (S-1)$ index cutoffs, such that, for example $z = 1$ denotes the cutoff between the worst and next-to-worst school in the town, and $z = (S-1)$ indicates the cutoff between the top-ranked school and the next best institution. Let t_i denote student i's transition score, and $\tilde{t}_z$ be the minimum grade required for admission into the higher-ranked of the two schools indexed by z.[27]

In this setup, consider the "reduced form" regression:

[25] We also implemented the exercise ranking schools and tracks by their minimum score, with quite similar results.

[26] Using the minimum admission score is in line with our "intent-to-treat" approach (discussed below), in the sense that only schools that reach capacity will generate meaningful first stages. An alternative approach would have been to set each school's (or school/track's) cutoff equal to the transition score of the child that fills its last slot. We could potentially identify that child since classes are limited to 28 slots (e.g., the track-specific slot availabilities which schools submit prior to the allocation process must be multiples of 28). However, our process for collecting and matching the administrative files (from hundreds of thousands of web pages) creates some measurement error. This limits our ability to determine with certainty if a school reached capacity. Nevertheless, using some approximations, we estimate that excluding the bottom ranked school in each town, the percent of schools that reach capacity ranges (depending on the cohort) between 80 and 90 percent.

[27] We abuse notation as above we used t to denote a time period; henceforth it will stand for the transition score.

$$y_i = \alpha 1\{t_i - \tilde{t}_z \geq 0\} + a(t_i) + \epsilon_i, \tag{2}$$

where y_i is Baccalaureate performance for student i, $1\{t_i - \tilde{t}_z \geq 0\}$ is an indicator for whether a student's transition score is greater than or equal to the cutoff indexed by z, and $a(t_i)$ is a flexible control function for the transition score. .

The idea behind the RD design is that if access to a higher-ranked school changes discontinuously at $\tilde{t}_z$, then the causal impact of this access can be identified even if students' transition scores are systematically related to factors that affect outcomes like Baccalaureate grades. Intuitively, suppose the transition score is smoothly related to characteristics that affect achievement, i.e., $a(t)$ is constant in a small enough neighborhood around the cut-off. Under this assumption, students with scores just below $\tilde{t}_z$ will provide an adequate control group for individuals with scores just above, and any difference in their outcomes can be attributed to the fact that they have access to schools of different quality—i.e., α is non-parametrically identified at $\tilde{t}_z$ (Hahn, Todd, and van der Klauuw (2011)). More generally, if $a(t)$ is specified correctly, it will capture all dependence of outcomes on the transition score away from the cut-off, and one can use all the data to estimate (2). Below we present such results and also estimates that rely only on observations close to cutoff scores.

As is common in the RD literature, we use specifications like (2) to produce "intent-to-treat" estimates of the effect of having the *opportunity* to attend a higher-ranked school. Our "first stage" results show that a significant proportion of children who have a chance to go to a better school take this opportunity. This allows us to measure the effect that having this opportunity has on a child's outcomes, although not the effect of any specific change in school quality because the the children above and below the cutoff attend schools with a range of different qualities.

5.2.2. ***Summarizing information for many cutoffs.*** The above specifications illustrate how one might exploit one cutoff. As stated, our data contain over 1000 cutoffs. In order to summarize these and for the sake of statistical power, we focus on regressions which pool data across cutoffs, relying on the fact that $t_i - \tilde{t}_z$ measures the distance between each cutoff and the transition score of each student in a town. Specifically, we "stack" the data such that every student in a town serves as an observation for every cutoff, and (when observations are used more than once) run the analyses clustering at the relevant level. Including all observations for every cutoff is relevant in that, for example, the student with the best score in town could successfully request any school. We note, however, that regressions restricted

to students in bands close to the cutoffs in fact rarely use student-level observations more than once.

For concreteness, most of our reduced form regressions are specified as follows:

$$y_i = \alpha 1\{t_i - \tilde{t}_z \geq 0\} + \eta(t_i - \tilde{t}_z) + \psi(t_i - \tilde{t}_z) * 1\{t_i - \tilde{t}_z \geq 0\} + w_z + v_i, \tag{3}$$

that is, a regression of outcomes on a dummy for whether a student's transition score is greater than or equal to the cutoff, along with controls that include: i) a linear spline in students' grade distance to the cutoff, one which allows the slope to vary on each side of the cutoff, and ii) a full set of cutoff dummies, w_z.[28]

Finally, to simplify notation, the equation above can be written as:

$$y_i = \alpha_1 above_i + \alpha_2 score_i + \alpha_3 score_i * above_i + w_z + v_i, \tag{4}$$

where $above_i$ is a dummy for whether a student's transition score is greater than or equal to the cutoff , $score_i$ is the running variable and $score_i * above_i$ is the interaction to allow for the linear spline.

5.3. **Estimating the dynamic complementarities between family and school environments.** Finally, we use the two empirical approaches described above to estimate potential interactions between the family and school environments. As discussed earlier, we argue that the repeal of the abortion ban can be used to generate plausibly exogenous variation in the family environment, while the school admission system generates plausibly exogenous variation in school environments. Thus, we combine the difference-in-differences (DD) and the regression discontinuity (RD) design into a RD-DD framework that allows us to estimate dynamic complementarities between family and school environments. Our main estimating equation is:

$$\begin{aligned} y = & \beta_0 + \beta_1 above_i + \beta_2 Unwntd_i + \beta_3 Unwntd_i * above_i + \beta_4 trend_i + \beta_5 trend_i * above_i \\ & + \beta_6 cohort_i + \beta_7 cohort_i * above_i + \beta_8 score_i + \beta_9 * Unwntd_i * score_i \\ & + \beta_{10} trend * score_i + \beta_{11} cohort_i * score_i + \beta_{12} score_i * above_i \\ & + \beta_{13} Unwntd_i * score_i * above_i + \beta_{14} trend * score_i * above_i \\ & + \beta_{15} cohort * score_i * above_i + w_z + v_i \end{aligned} \tag{5}$$

[28] For simplicity, equation (3) does not have a time dimension; in reality our standard specification includes a full set of cutoff*year dummies.

where all the variables are as described above using data from both the 2005 and 2006 school entry cohorts. As above, we also explore alternative specifications that only include the 2005 cohort or use a set of calendar month of birth dummies to model time trends. Our main coefficient of interest is β_3, the interaction between $above_i$ and $Unwntd_i$. This coefficient essentially estimates whether the educational benefits of going to a better school are smaller for children who experienced worse family environments because they were born prior to the liberalization of the abortion ban in Romania.

6. Results

This section begins by presenting results that examine the impact of the abortion ban on educational outcomes. It then presents the effects of attending a better school on similar education outcomes. Finally, it uses our combined RD-DD framework to explore the dynamic complementarities between family and school environments.

6.1. **The impact of the abortion ban.** Panels A, B and C of Table 1 present the coefficient on the *Unwntd* dummy variable for five indicators of educational achievement in three different specifications. Panel A is based on regressions restricted to children born in 1990 who applied to high school in 2005, and includes quadratic functions of month of birth. Panel B is our preferred specification, in which we also add the 1991 cohort and include a cohort dummy as a control variable. Finally, Panel C uses the same sample as Panel B but includes a set of calendar month of birth controls instead of the quadratic monthly controls.

All of the coefficients in Table 1 are negative and statistically significant suggesting that the repeal of the abortion ban improved children's educational outcomes. Our preferred specifications in Panel B show that children affected by the abortion ban score an average of 0.02 lower on the admission grade. This also enables them to gain admission to a school with peers who have an average admission grade that is 0.02 lower.[29] At the same time, children born after the abortion ban are also 1.3 percentage points more likely to attend an academic high school (from a baseline mean of 36 percent). The same negative educational outcomes can be observed four years later when these same children take the Baccalaureate exam. Children born prior to the abortion ban are 1.49 percentage points less likely to take the Baccalaureate exam and, conditional on taking this exam, they show statistically significant

[29] Since the average test score of the 2006 cohort is higher than the 2005 cohort one might worry that an equal point increase in performance might have a different impact across the two cohorts in terms of how far a student is moving in the ability distribution. In such a situation a more appropriate specification might be one that uses the log of the admission score. In regressions not reported in the paper we have checked that the results are robust when using a log specification.

TABLE 1. The effect of the 1989 abortion legislation on educational outcomes

	Transition Grade	Average Transition Grade of Peers	Attend Academic Highschool	Baccalaureate Taken	Baccalaureate Grade
	(1)	(2)	(4)	(5)	(6)
Panel A					
	-0.0228**	-0.0178***	-0.0127***	-0.0211***	-0.0363***
Unwanted dummy	[0.00843]	[0.00393]	[0.00306]	[0.00450]	[0.00755]
Monthly trend	Y	Y	Y	Y	Y
Cal. month dummies	N	N	N	N	N
Cohort dummy	N	N	N	N	N
Samples	2005	2005	2005	2005	2005
Sample Size	88550	88550	88550	88550	66716
Panel B					
	-0.0189***	-0.0158***	-0.0130***	-0.0149***	-0.0249***
Unwanted dummy	[0.00647]	[0.00378]	[0.00331]	[0.00372]	[0.00678]
Monthly trend	Y	Y	Y	Y	Y
Cal. month dummies	N	N	N	N	N
Cohort dummy	Y	Y	Y	Y	Y
Samples	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006
Sample Size	170847	170847	170847	170847	128324
Panel C					
	-0.0147***	-0.0119**	-0.0120***	-0.00967***	-0.0181***
Unwanted dummy	[0.00397]	[0.00441]	[0.00313]	[0.00314]	[0.00424]
Monthly trend	N	N	N	N	N
Cal. month dummies	Y	Y	Y	Y	Y
Cohort dummy	Y	Y	Y	Y	Y
Samples	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006
Sample Size	170,847	170,847	170,847	170,847	128,324
Mean of dependent variable (2005 cohort only)	6.55	6.55	0.36		

Notes: Standard errors are provided in brackets and are clustered by age in months. The dependent variables are defined in Table 1. The Treatment dummy is defined as 1 for individuals born on or after July 1, 1990 and 0 for individuals born on or before June 30, 1990. Panel A is based on children born in 1990 and present in the 2005 secondary school cohort. Panels B and C also include children born in 1991 and present in the 2006 cohort as controls.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

reductions of 0.03 standard deviations in the exam score. These final two variables are the main measures of educational achievement for our analysis.[30,31]

[30] We performed a number of additional robustness checks that are not reported here. One potential criticism of the results in Table 1 is that our estimates rely to a large extent on how we control for trends and seasonal factors. One approach that we take is to restrict our analysis to much narrower time windows. The simplest comparison is the difference in educational outcomes for children born in July and June of 1990 who are present in the 2005 administrative dataset. The difference in the average score between these two month is slightly smaller but still sizable and statistically significant for most of our outcome variables. We find similar results if we use a sample that is also restricted just to children born in June and July but additionally includes children born in the same months of 1991 who are in the 2006 administrative dataset.

[31] Given that the gestation length varies across pregnancies and that legal abortions are probably more likely to happen for mothers in their third rather than their second month of pregnancy in December 1989, the decline in fertility after July of 1990 is not completely instantaneous. As can be seen in Figure 2, July

In Figure 3 we plot the residuals for the months January to December of 2005 based on regressions similar to those in Panel C of Table 3. The graphs for all five outcome variables, while sometimes noisy, show a break in the pattern of educational achievement after July of 1990 and they confirm that children born prior to the repeal of the abortion ban in Romania in 1989 have worse educational outcomes, based on high school admission and graduation scores.

In order to explore the possible mechanisms driving these results, we show regression results of the effect of the liberalization of abortion in Romania after 1989 on the composition of women who give birth (in Appendix Table 1). Column (1) of Appendix Table 1 shows that children born after July of 1990 were 1.6 percent less likely to have a mother that only received a primary level of education. Columns (2) and (3) show an equally large increase in the proportion of mothers with secondary education and no change in the proportion mothers with tertiary education. These results are similar to those in Pop-Eleches (2006) where the largest decrease in the level of fertility is found for women with less education as a result of the lift of the ban in 1989. These results imply that children born after the lifting of the ban in 1989 were more likely to live in more privileged households. As a result we expect the direction of the composition effect to go in the same direction as the unwantedness effect, implying that θ_1 should be, if anything, positive.

Therefore, the direction of the liberalization of abortion in Romania on child outcomes should be similar to the case of the U.S. after the liberalization of abortion following Roe vs. Wade, and different from the effects in Romania at the time of the introduction of the ban in 1966 (Pop-Eleches, 2006). Column 4 of Appendix Table 1 shows that the average age at birth decreased by almost half a year after July of 1990, suggesting that older women responded more to the lift of the ban, presumably because they were more likely to have reached or exceeded their ideal family size under the old abortion regime. The last column of Appendix Table 1 show that a birth under the ban had a lasting effect on fertility levels even more than 10 years later, implying that children born after the ban grew up in families with fewer siblings.[32] The large decrease in fertility immediately after July of 1990 provides the most direct evidence that many of the children born under the ban were not wanted by

of 1990 was the first month with a rapid decline in fertility but the month of August also saw very large further reductions in births. Therefore we also performed some robustness checks (not reported in the paper) restricting the sample to those born in June and August only. The results are very similar to those using the comparison of June and July.

[32] These results are robust to controlling for mothers education. They suggest that the observed effects are not just due to changes in composition.

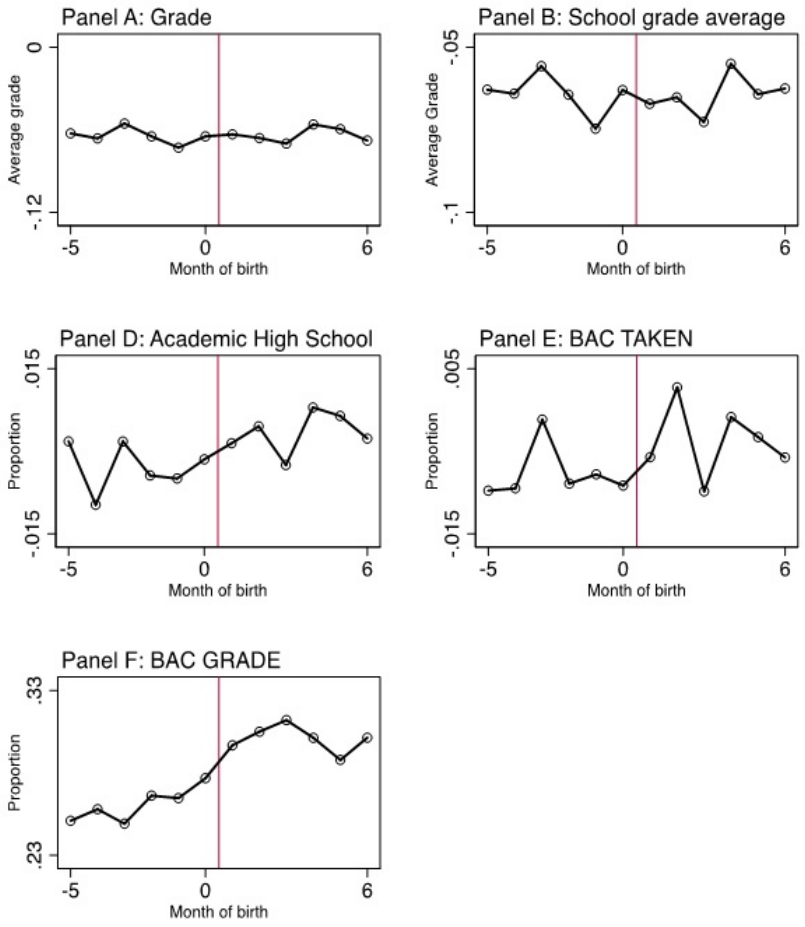


FIGURE 3. Abortion ban and educational outcomes

Notes: All panels plot residuals from regressions similar to those of Panel C in Table 3. They represent residuals of educational attainment by month of birth for children born in 1990 and present in the 2005 secondary school cohort. Children born in 1991 and present in the 2006 cohort are used in these regressions to account for month of birth effects. The fertility decrease as a result of the abortion legalization started with cohorts born after July (Month 0 in graph) of 1990. Source: Romanian Ministry of Education.

their parents. However, the changes in the patterns of age at birth and life-cycle fertility provide additional support for this claim.[33]

Finally, we compare the evidence presented in this section on the effect of the lift of the abortion ban on educational outcomes with those from the introduction of the ban in 1967 presented in Pop-Eleches (2006). The lifting of the ban led to a reduction in fertility by one

[33] While in the case of Romania a decrease in the average age at birth after the lifting of the ban is consistent with the view that the restrictive ban lead to unwanted birth, this results might not be general to other settings. For example the legalization of abortion could decrease the percentage of teen pregnancies, therefore increasing the average age when women give birth.

third in the first six months and the resulting increase in high-school attendance compared to vocational school attendance of 2.1 percentage points is due to the combined composition and unwantedness effects. In 1967, the abortion ban resulted in a doubling of fertility in the short run and the decrease in high school graduation due to the unwantedness effect was 1.7 percentage points. While the direct comparison of these effects is difficult given the differences in data sources and institutional settings, the magnitudes of the effects are remarkably similar. Finally it should be noted that although the Romanian evidence shows significant schooling effects any comparison with the US experience associated with *Roe v. Wade* should take into consideration that the fertility impact in Romania was much larger.

6.2. **The impact of going to a better school.** We next describe estimates for the educational benefits of attending a better school in Romania. We note that these results use essentially the same specifications as those described in Pop-Eleches and Urquiola (2013), although the data comes from the 2005 and 2006 cohorts rather than the 2001, 2002, and 2003 high school entry cohorts. Figure 4, Panel A illustrates the basic first stage result in our data, pooling all between-school cutoffs. The x-axis describes students' transition scores relative to the cutoffs that allow the opportunity to access a higher-ranked school; the y-axis describes the peer quality students experience, as measured by the mean transition score at their respective school. Panel A plots this mean transition score collapsed into cells containing individuals who are within 0.01 of a transition grade from each other. Panel B plots the analogous information, but with a y-axis that is based on residuals from a regression of the mean transition score on cutoff fixed effects. Both panels suggest that the average peer quality experienced by students increases significantly and discontinuously if their transition score crosses the threshold that gives them the option of going to a better-ranked school. The vertical distance between the points close to the discontinuity corresponds to the estimate of α_1 in expression (3) and (4).

Table 2, Panel A presents the regression analog to these figures. Panel A, Column 1 uses data all the data on students in the 2005 and 2006 cohorts, while Column 2 restricts the sample to children within one point of the cutoff—about 1.1 million observations. It regresses the average transition grade that students experience at school on an indicator for whether their scores are above the cutoffs. The specification includes: i) a linear spline in students' grade distance to the cutoffs, one which allows the slope to vary on either side of the cutoff, and ii) cutoff/year dummies—i.e., equation (3) with cutoff/year fixed effects.[34] The results suggest that scoring above a cutoff results in a highly statistically significant

[34] Our regression results are not qualitatively affected by instead using a linear, quadratic, or cubic specification for $a(t_i)$ in equation (3), or by excluding the cutoff fixed effects.

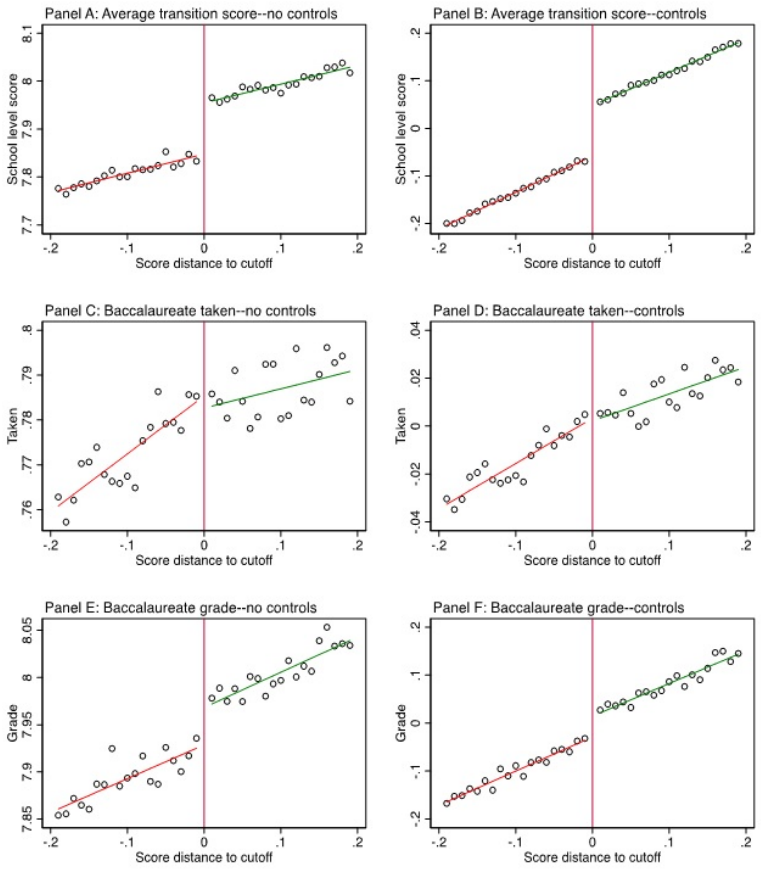


FIGURE 4. The effect of school quality

Notes: All panels are based on administrative data for the 2001-2003 admission cohorts, and restrict observations to individuals with transition scores within 0.2 points of a cutoff. The left hand side panels plot (0.01 point) transition score cell means of the dependent variable. The right hand side panels plot analogous means of residuals from a regression of the dependent variable on cutoff fixed effects. In each panel, the solid lines are fitted values of regressions of the dependent variable on a linear trend in the transition score, estimated separately on each side of the cutoff. The dependent variable in panels A and B is the average transition score of the peers students encounter at school; the dependent variable in panels C and D is an indicator for having taken the Baccalaureate test; the dependent variable in panels E and F is the Baccalaureate exam grade.

jump in peer quality—0.11 points, equivalent to about 0.1 standard deviations in transition test performance.

As noted above, Column 2 restricts the sample to include only students whose transition scores are within 1 point of a cutoff (about half of the full sample). This is our preferred specification; it attempts to balance the goal of focusing on observations close to the cutoffs while providing enough data to yield fairly precise estimates. We also experimented with several more stringent windows, with similar conclusions. We opt to feature, in Column 3, a regression using the bandwidths suggested by the procedure in Imbens and Kalyanaraman

TABLE 2. The effect of going to a better school on educational outcomes

Dependent variable:	**Administrative data**		
	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)
Panel A: Average transition grade of peers			
1{Trans. grade≥Cutoff}	0.109***	0.104***	0.104***
	[0.000957]	[0.00124]	[0.00119]
Linear spline	Yes	Yes	Yes
R^2	0.823	0.797	0.799
N	2,204,676	1,066,252	1,151,817
Panel B: Baccalaureate taken			
1{Trans. grade≥Cutoff}	-0.00287**	0.000	0.000
	[0.00114]	[0.00147]	[0.00142]
Linear spline	Yes	Yes	Yes
R^2	0.083	0.072	0.072
N	2,204,676	1,066,252	1,133,435
Panel B: Baccalaureate grade			
1{Trans. grade≥Cutoff}	0.0490***	0.0256***	0.0256***
	[0.00241]	[0.00309]	[0.00309]
Linear spline	Yes	Yes	Yes
R^2	0.546	0.508	0.508
N	1,618,115	724,347	725,320

Notes: Standard errors are provided in brackets and are clustered by age in months. The dependent variables are defined in Table 1. The Treatment dummy is defined as 1 for individuals born on or after July 1, 1990 and 0 for individuals born on or before June 30, 1990. Panel A is based on children born in 1990 and present in the 2005 secondary school cohort. Panels B and C also include children born in 1991 and present in the 2006 cohort as controls.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(2009) (henceforth IK), which in our data is generally more restrictive than the 1 point band used in Column 2.[35] All of these samples result in similar and highly significant estimates. The "first stages" in Table 2, Panel A are those that will be relevant for the Baccalaureate outcomes. They show that the Romanian high school admissions process provides a potentially fruitful setting for an RD analysis of the impact of having access to a better school, at least if school quality is judged by average transition scores.[36]

[35] Specifically, we follow Lee and Lemieux (2010) and use a simple rectangular kernel. Further, we implemented the bandwidth selection procedure using the Stata ado file labeled rdob.ado, available at http://www.economics.harvard.edu/faculty/imbens/software_imbens.

[36] The RD approach additionally requires that there be no discrete changes in student characteristics that affect outcomes like Baccalaureate performance. While our administrative data do not contain such variables, our survey data suggest this condition is fulfilled. Specifically, results presented in Pop-Eleches and Urquiola (2013) shows that a number of mother, child, and household characteristics do not vary discontinuously around the cutoffs (all but one of the 20 estimates are insignificant in the sample within 1 point of the cutoffs). As an additional test, Figure A.7 of the same paper shows that there is no visible jump in the density around the discontinuity; as expected, the McCrary (2008) test shows no statistically significant break (log difference in height is 0.074 with a standard error of 0.058).

We first consider how admission to a better school affects whether student take the Baccalaureate exam. Panels C and D in Figure 4 present graphical evidence suggesting few if any changes in test-taking rates at the cutoffs. This is confirmed in the regressions shown in Panel B of Table 2. These results imply that having access to a higher-ranked school does not affect the probability of taking the Baccalaureate exam; we can rule out differences in test-taking rates of less than one-third of a percentage point. Moreover, the absence of selection into taking the Baccalaureate exam makes it easier to interpret the effects on Baccalaureate performance. Turning to this question, panels E and F in Figure 4 present the impact of access to a higher-ranked high school on test scores, suggesting a discontinuous increase in average achievement at the relevant cutoffs. The corresponding regression results are shown in Panel of Table 2, which presents statistically significant gains equivalent to about 0.2 standard deviations.

The bottom line is that students who score above cutoffs giving them access to higher ranked schools perform better in the high stakes Baccalaureate exam, and under the assumptions underlying RD designs, this impact can be viewed as causal.

6.3. **Dynamic complementarities between family and school environment.** Using our combined RD-DD framework, we now investigate whether access to a higher-ranked school is associated with a differential effect for children who grow up in different family environments induced by changes in access to abortion. As in a previous section, we present our preferred specification (Panel B), but we also examine alternative specifications that only include the 2005 cohort (Panel A) or where we use a set of calendar month of birth dummies to model time trends (Panel C). In columns (1-3) the outcome of interest is an indicator variable for taking the Baccalaureate exam, while in columns (4-6) we consider performance in the Baccalaureate exam. And similar to our previous analysis, for each of the two outcome variables we present three specifications in Table 3: using the whole sample, a sample that is restricted to individuals within one transition score point from the cutoff, and a sample using the IK optimal bandwidth. Our preferred specification is based on specification (5) in Panel B.

Table 3 describes the differential impact associated with scoring above a school cutoff for children born before and after the lifting of the abortion ban. To begin with, one can observe similar patterns for the main effects based on the two sources of exogenous variation underlying our analysis. The coefficients on our indicator for being above a school cutoff (the Education Treatment dummy) is small and statistically insignificant for taking the Baccalaureate exam and positive and significant in two of the three specifications for the Baccalaureate grade. Similarly, controlling for season of birth, the coefficient on Unwanted

(the Abortion Treatment dummy) is negative and significant for 5 out of the 6 specifications for both Baccalaureate outcome measures. The main result of the paper is based on the coefficient capturing the interaction of the Education and Treatment dummies in Panel B of Table 3. All of the coefficients are small and statistically insignificant, suggesting that, at least in our setting, there appears to be little evidence for dynamic complementarities between shocks to family and school environments. We have also examined a number of additional robustness checks that confirm our main results. In Panels A and C, which present alternative specifications for modeling the time of birth effects, one can observe a similar pattern for the main effects and no significant interaction effects between the education and abortion treatments.

An alternative specification testing for dynamic complementarities in our setting involves estimating our main education effects for four groups of children: (1) those still born during the restrictive abortion regime between January and June of 1990, (2) those born immediately after the lifting of the ban in the period July to December 1990, (3) those born between January and June of 1991, and (4) those born between July and December of 1991. Children born in 1991 arrived after the lifting of the abortion ban and are used to control for potential season of birth effects, while the children born in the first and second half of 1990 experienced potentially very different family environments due to the lifting of the abortion ban.

In Table 4, we present the impact of scoring above the school cutoff on taking the baccalaureate exam for these four distinct groups. The results across most specifications are small and insignificant. This result is not surprising given that our previous analysis found few effects of scoring above the cutoff for this educational outcome.

In Table 5, we present a similar analysis but using the Baccalaureate grade as our outcome variable. The results are striking, and they show a remarkably constant pattern of positive and statistically significant effects of going to a better school across all four groups (although we note that the effects in 1991 are somewhat smaller than those in 1990). These four coefficients could essentially be used to calculate a difference-in-differences effect that is in same spirit of our combined specification in Table 5. A back of the envelope calculation using the sample within 1 point of the cutoff, implies that the difference-in-differences between these 4 sample estimates to be -0.002.

7. Conclusion

This paper tests for dynamic complementarities between family and school environments by exploiting exogenous variation in early skills resulting from the lifting of an abortion ban in Romania together with exogenous variation in later educational investments due to

TABLE 3. The effect of going to a better school and the abortion legislation on educational outcomes

	Bacc. taken	Bacc. taken	Bacc. taken	Bacc. grade	Bacc. grade	Bacc. grade
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
above (Education Treatment dummy)	0.0412	0.0131	-0.00112	0.0773**	0.00925	0.0131
	[0.0285]	[0.0375]	[0.0361]	[0.0328]	[0.0415]	[0.0396]
Unwanted (Abortion Treatment dummy)	-0.0246	-0.0278	-0.0312	-0.0652**	-0.0618**	-0.0677**
	[0.0257]	[0.0255]	[0.0251]	[0.0269]	[0.0279]	[0.0275]
above*unwanted (Interaction of Education and Unwanted	-0.0181	-0.00958	-0.0057	0.0145	0.0226	0.0315
	[0.0183]	[0.0237]	[0.0228]	[0.0208]	[0.0267]	[0.0255]
Monthly trend	Y	Y	Y	Y	Y	Y
Cal. month dummies	N	N	N	N	N	N
Cohort dummy	N	N	N	N	N	N
Bandwidth	All	Within 1 point	IK Bandwidth	All	Within 1 point	IK Bandwidth
Samples	2005	2005		2005		2005
Sample Size	982675	470717	500565	732963	326850	327304
Panel B						
above (Education Treatment dummy)	-0.00623	-0.00449	-0.0019	0.0946***	0.0439***	0.0480***
	[0.00730]	[0.00941]	[0.00900]	[0.00827]	[0.0105]	[0.0101]
Unwanted (Abortion Treatment dummy)	-0.0278	-0.0231	-0.0224	-0.00605	-0.00638	-0.00818
	[0.0191]	[0.0190]	[0.0188]	[0.0199]	[0.0204]	[0.0202]
above*unwanted (Interaction of Education and Unwanted	0.0119	0.015	0.014	-0.00653	-0.00316	-0.00541
	[0.0132]	[0.0171]	[0.0166]	[0.0151]	[0.0186]	[0.0180]
Monthly trend	Y	Y	Y	Y	Y	Y
Cal. month dummies	N	N	N	N	N	N
Cohort dummy	Y	Y	Y	Y	Y	Y
Bandwidth	All	Within 1 point	IK Bandwidth	All	Within 1 point	IK Bandwidth
Samples	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006
Sample Size	1860776	892226	948675	1384633	615673	616493
Panel C						
above (Education Treatment dummy)	0.0161	0.0018	-0.00391	0.0957***	0.0311	0.0420*
	[0.0167]	[0.0217]	[0.0210]	[0.0191]	[0.0234]	[0.0226]
Unwanted (Abortion Treatment dummy)	-0.0313*	-0.0300*	-0.0306*	-0.0314*	-0.0284	-0.0309*
	[0.0169]	[0.0169]	[0.0167]	[0.0177]	[0.0183]	[0.0181]
above*unwanted (Interaction of Education and Unwanted	0.00758	0.0131	0.0134	0.00162	-0.000259	0.000454
	[0.0119]	[0.0154]	[0.0149]	[0.0135]	[0.0171]	[0.0164]
Monthly trend	Y	Y	Y	Y	Y	Y
Cal. month dummies	N	N	N	N	N	N
Cohort dummy	Y	Y	Y	Y	Y	Y
Bandwidth	All	Within 1 point	IK Bandwidth	All	Within 1 point	IK Bandwidth
Samples	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006	2005/2006
Sample Size	1860776	892226	948675	1384633	615673	616493

Notes: Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of the unwanted dummy and the dummy for whether a student's transition score is greater than or equal to the cutoff. The dependent variables are defined in Table 1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

attending a better high school. We use an empirical strategy that combines (i) a regression discontinuity (RD) design comparing students who just barely got admitted to a better high school with those students who just barely missed the threshold of acceptance and (ii) a difference-in-differences (DD) strategy comparing children who were conceived prior

TABLE 4. The effect of going to a better school on Bacc. taking for different cohorts

Dependent variable: Baccalaureate Taken	Administrative data		
	All towns		
	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)
Panel A: Whole sample			
1{Trans. grade≥Cutoff}	-0.00728**	0.000	-0.001
	[0.00296]	[0.00380]	[0.00365]
R^2	0.082	0.071	0.072
N	2,203,864	1,065,830	1,151,384
Panel B: 1990 cohort, Months 1-6			
1{Trans. grade≥Cutoff}	-0.002	0.009	0.010
	[0.00596]	[0.00781]	[0.00742]
R^2	0.088	0.076	0.077
N	577,905	276,370	298,726
Panel C: 1991 cohort, Months 1-6			
1{Trans. grade≥Cutoff}	-0.004	0.002	-0.001
	[0.00614]	[0.00776]	[0.00753]
R^2	0.075	0.068	0.068
N	471,154	224,494	242,805
Panel D: 1990 cohort, Months 7-12			
1{Trans. grade≥Cutoff}	-0.0168**	-0.004	-0.005
	[0.00715]	[0.00910]	[0.00862]
R^2	0.076	0.067	0.066
N	404,770	194,347	210,018
Panel E: 1991 cohort, Months 7-12			
1{Trans. grade≥Cutoff}	-0.005	-0.001	-0.004
	[0.00652]	[0.00812]	[0.00795]
R^2	0.064	0.059	0.059
N	406,947	197,015	212,690

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

to the 1989 repeal of an abortion ban with those children whose mothers had access to abortion. We find that being born under a less restrictive abortion regime following the repeal of abortion results in higher academic achievement, both in an entrance examination for high school and the Baccalaureate exam following the completion of high school. We also confirm that students who attend a better high school perform better on the Baccalaureate exam. However, using a combined RD-DD strategy, we do not find any significant interaction effects between the impacts associated with the repeal of the abortion ban and the impacts

TABLE 5. The effect of going to a better school on Bacc. grades for different cohorts

Dependent variable: Baccalaureate Grade	Administrative data		
	All towns		
	Full sample	Within 1 point of cutoff	Within IK bound
	(1)	(2)	(3)
Panel A: Whole sample			
1{Trans. grade≥Cutoff}	0.0497***	0.0274***	0.0305***
	[0.00248]	[0.00326]	[0.00313]
R^2	0.526	0.454	0.460
N	1,617,482	724,079	784,586
Panel B: 1990 cohort, Months 1-6			
1{Trans. grade≥Cutoff}	0.0446***	0.0342***	0.0395***
	[0.00503]	[0.00676]	[0.00634]
R^2	0.544	0.477	0.483
N	427,984	189,926	205,921
Panel C: 1991 cohort, Months 1-6			
1{Trans. grade≥Cutoff}	0.0527***	0.0222***	0.0256***
	[0.00494]	[0.00641]	[0.00636]
R^2	0.514	0.441	0.447
N	348,087	152,427	165,403
Panel D: 1990 cohort, Months 7-12			
1{Trans. grade≥Cutoff}	0.0430***	0.0323***	0.0352***
	[0.00601]	[0.00805]	[0.00758]
R^2	0.529	0.462	0.468
N	304,979	136,924	148,349
Panel E: 1991 cohort, Months 7-12			
1{Trans. grade≥Cutoff}	0.0583***	0.0213***	0.0235***
	[0.00545]	[0.00695]	[0.00691]
R^2	0.508	0.438	0.443
N	303,583	136,396	147,721

Notes: All regressions are clustered at the student level and include cutoff fixed effects. Standard errors are in parentheses. All panels present reduced form specifications where the key independent variable is a dummy for whether a student's transition score is greater than or equal to the cutoff.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

associated with attending a better high school. In other words, there appears to be little evidence for dynamic complementarities between our exogenous shocks to family and school environments in our setting.

How do we interpret the lack of evidence for dynamic complementarities between family and school environments? One possibility is that dynamic complementarities are more important across shocks to common environments. For example, there may be larger dynamic complementarities across repeated shocks to family environment that occur over time, and separately across repeated shocks to the school environment, but not for interactions of these shocks across different types of environments. More generally, it may be the case that

dynamic complementarities are simply not as ubiquitous as is commonly believed. Our findings are unlikely to be explained by that fact that the shocks to the schooling environment occur relatively late in adolescence; we do observe significant impacts of these educational shocks on subsequent outcomes, just not for the interaction with the abortion policy. It is important to note that, despite finding magnitudes very close to zero, our estimates of these interactions are not extremely precise so we cannot rule out some meaningful effect sizes. Nevertheless, these result suggest that there may be substantial benefits associated with later school interventions even when they are targeted at more disadvantaged children.

There are a number of directions for further work. As mentioned earlier, we have extensive survey data on these students, their parents, and their principals during high school which could shed further light about why dynamic complementarities may or may not arise. We can also examine the interaction of the abortion policy with the effect of enrolling in a "better" track within a school (as opposed to simply a "better" school). Finally, we can broaden the analysis sample by incorporating those students who enrolled in vocational schools in Romania, although most of them will not have taken the Baccalaureate exam by default.

Appendix table 1

	Primary Education	Secondary Education	Tertiary Education	Mother's Age at Birth	Live Births in 2002
	(1)	(2)	(3)	(4)	(5)
Treatment dummy	-0.016**	0.017***	-0.001	-0.469***	-0.034**
	[0.006]	[0.005]	[0.003]	[0.071]	[0.015]
Linear monthly trend	Y	Y	Y	Y	Y
Cal. month dummies	Y	Y	Y	Y	Y
Sample Size	139,126	139,126	139,126	139,511	135,790
Mean of dep. variable	0.44	0.48	0.07	24.72	2.77

Notes: Standard errors are provided in brackets and are clustered by age of child in months. Samples include cohorts born between January 1, 1988 and December 31, 1992. The dependent variables are defined in Table 1. AFTER is defined as 1 for individuals born on or after July 1, 1990 and 0 for individuals born on or before June 30, 1990.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

8. Appendix

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