INTERACTIONS BETWEEN FAMILY AND SCHOOL ENVIRONMENTS: ACCESS TO ABORTION AND SELECTIVE SCHOOLS

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ABSTRACT. We use Romanian data to ask whether the benefit of access to better schools is larger for children who experienced better family environments because their parents had access to abortion. We combine regression discontinuity and differences-in-differences designs to estimate impacts on a high-stakes school-leaving exam. We find that access to abortion and access to better schools each have positive impacts, but find no consistent evidence of interactions between these impacts. To the extent a pattern emerges, it is suggestive of substitutability rather than complementarity of family and school environments.

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1. Introduction

There is clear evidence that family and school environments can each have causal impacts on individuals' outcomes. In the family dimension, for instance, Dahl and Lochner (2012) show that children's academic achievement is affected by family income, and Doyle (2007) that earnings vary with foster care placement. In the school dimension, Chetty et al. (2011) find analogous impacts of kindergarten quality, and Hoekstra (2009) of college quality.

Much less is known, however, about how family and school environments *interact* in determining outcomes.

This paper uses Romanian data to provide causal estimates of the interaction between family and school environments. We identify such interactions by combining variation from the lifting of an abortion ban which affects children's family environments, with variation from the allocation of high school slots which affects their school environments. We use administrative data to show that access to a higher quality environment in each of these dimensions improves outcomes, but find no consistent evidence of interactions between these impacts.

The first source of variation comes from the repeal of Romania's decades-long ban on abortions. This occurred on December 26, 1989, immediately after the collapse of Communism. We evaluate the impact of this change by comparing children born before and after July 1, 1990. July is the first month during which a decline in the number of births is observed, consistent with expectant mothers in their first trimester first having been able to access abortion six months earlier.[1] The parents of children born after July 1 were thus potentially better able to plan for their arrival, and hence to provide them with better family environments and early investments.

We take advantage of individual-level administrative data to observe the performance of pre- and post-reform cohorts on a *transition score* used to determine admission to high school, and on a *Baccalaureate exam* taken at the end of school. These measures of achievement are unlikely to be the only components of skill affected, but they are high-stakes outcomes, and the latter significantly influences admissions to college. We use a difference-in-differences (DD) estimation strategy to show that children born when there was greater access to abortion score higher in both dimensions.

The second source of variation comes from the way children are allocated to high schools in Romania. Their ability to choose a school depends solely on the transition score, which includes performance on a standardized admissions exam. After obtaining their transition score, students request schools and are allocated via a centralized process that grants priority

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[1] As in other countries, medical practice in Romania restricted abortions to the first trimester of pregnancy.

to students with higher scores. This gives rise to cutoff scores that determine access into schools, producing clear discontinuities in school quality, as measured by peer ability. We use these cutoffs in a regression discontinuity (RD) design to show that students who have access to better high schools perform better on the Baccalaureate exam.[2]

We then combine the two sources of variation using a regression discontinuity difference-in-differences (RD-DD) design. We provide a formal empirical framework that establishes conditions for causal identification of the interactions between family and school environments. This allows us to ask whether children born when there was greater access to abortion have larger Baccalaureate score improvements when they have access to a better school. In other words, we ask if the later intervention—better high school quality—produces larger gains among students who experienced greater parental investments because they were born when there was easier access to abortion. We do not find consistently significant evidence of this; if anything, there is indication of a negative interaction between the impact of better family and school environments.

We also consider behavioral responses using a survey administered to approximately 6,800 students and parents drawn from the administrative data. We focus this exercise on parental and student effort around homework. Using survey rather than administrative data reduces statistical power, but produces suggestive evidence that children who attend better schools receive less parental help with homework. This effect is more pronounced for those who, due to the increased access to abortion, attained higher levels of skill early on. There is a similar pattern in terms of children's own effort. These responses suggest that, at least in the Romanian setting, parental and student behavior could undo any complementarities between family and school environments.

We also address several issues that emerge in our setup. First, we note that the effects we find are unlikely to be driven by changes in crowding. Though children born in the years after the increase in access to abortion would have encountered less competition for primary school slots, we compare children who were born just before and just after the decline in fertility but entered school in the same academic year.

Second, greater access to abortion might have affected not just family investments, but the composition of children in these cohorts. For example, it could have increased the prevalence of children of lower socioeconomic status. We present evidence that composition, at least in

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[2] We will henceforth refer to higher-ranked schools as better schools; we acknowledge that this is only as measured by average transition scores. Pop-Eleches and Urquiola (2013) show that this measure is correlated, in the expected directions, with factors like parental involvement, teacher seniority, and perceptions of quality on the part of parents and school principals. Our results on access to better schools replicate those in Pop-Eleches and Urquiola (2013), although for a different set of cohorts.

terms of observables, is not the key factor driving our findings; e.g., there is limited evidence that access to abortion affected mothers' characteristics. In addition, our main results hold when we control for measures of poverty in the administrative data, or for characteristics such as mother's education in the survey data.

Third, we explain how our empirical framework avoids bias from controlling for the transition score, which could itself be affected by access to abortion. This enables us to provide causal estimates even if access to abortion *only* affects outcomes through the transition scores. When exploiting RDD variation, we control for the transition score within (rather than across) each of the four groups in our DD design, which identifies next-best school effects (within each such group) among those with transition scores at a given threshold. Having over one thousand such discontinuities allows us to then average over students across the support of transition scores, leveraging parallel trends assumptions both for Baccalaureate scores and for the transition scores that would have occurred absent the abortion ban.

A number of recent papers have examined the interaction between family and school environments, broadly construed. Rossin-Slater and Wust (2016) explore the interaction between a nurse home visiting program and high-quality preschool childcare in Denmark. They exploit variation in timing of program implementation and find evidence indicating that these interventions are substitutes rather than complements. Along the same lines, Adhvaryu et al. (2018) examine the interaction between parental resources and later educational investments using variation in local rainfall shocks and the Progresa program. They find that educational investments mitigate the negative effects of adverse rainfall shocks.

Our findings are also related to research specifically focused on interventions that affect early and later skills, often framed in the context of dynamic complementarities (Cunha and Heckman 2007). Aizer and Cunha (2012) exploit exogenous variation in preschool investments from the launch of Head Start to show that the effect of preschool enrollment on a subsequent measure of cognitive skill (at 4 years of age) is larger for those children with higher Bailey test scores at 8 months of age. Jackson and Johnson 2019 use changes in Head Start spending and school-finance reforms to show that the benefits of Head Start spending were larger when followed by access to better-funded public schools in the United States.[3] Gilraine (2016) uses No Child Left Behind and regression discontinuities to show that the effects of accountability in adjacent grades amplify in a manner consistent with dynamic complementarity. Our results showing that parental and student behavior could undo complementarities in human capital formation offer a cautionary note on research examining dynamic complementarities: well-identified evidence is necessary to assess dynamic

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[3] See also Duque et al. 2020 on Colombia.

complementarities, but as often, reduced form results do not necessarily reveal the possibly countervailing mechanisms that underlie them.

Finally, our paper is also related to previous work discussing the possibility of parental behavior in response to early-life shocks. For example, Royer (2009) notes the challenge of separating the biological effects of low birthweight from parental behaviors due to low birthweight. Black et al. (2007) similarly point out that parents may respond to differences in birthweight across siblings, but find little evidence of such behavior.[4] Bau et al. (2020) find that early positive rainfall shocks can lead households to invest less in education in places with a high prevalence of child labor. In addition, in reviewing work on the impact of early childhood environments, Almond and Currie (2011) note that most papers produce reduced form effects which could include either biological effects or responsive parental investments.

The remainder of the paper proceeds as follows. Section 2 describes our data. Section 3 provides background on our sources of variation, and Section 4 presents the empirical strategy. Section 5 presents results, and Section 6 concludes.

2. Data

We rely on three types of data: (i) administrative information covering the universe of children who transition from middle to high school, (ii) census data, and (iii) a survey we administered in most towns containing two or three high schools.

2.1. **Administrative data.** Our administrative data cover all the children who were allocated to a high school in the years 2005 and 2006. These data include their name, date of birth, and allocated school/track.[5] In addition, they contain each student's transition score; this number determines their priority in admissions to high school (as explained below), and is an unweighted average of their performance in a national 8th grade exam and their middle school grade point average.

We linked these data with information on whether students took the Baccalaureate exam once they were in 12th grade, and on how they performed on it (the 2005 and 2006 admissions cohorts took the exam in 2009 and 2010 respectively).[6] A satisfactory Baccalaureate grade is a prerequisite for applying to university, and a high grade raises the probability of admission to prestigious institutions.[7]

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[4] See also Bharadwaj et al. 2018 and Dizon-Ross 2019.

[5] As explained below, students within each school are allocated to tracks such as Mathematics or Literature.

[6] We merged the admissions and Baccalaureate data by student name/county using a fuzzy matching technique to allow for some misspelling of names. Our conclusions are robust to changing the precision of the matching algorithm, including using only exact matches.

[7] The Baccalaureate exam is administered nationally. Students usually take six component tests, with a combination of common subjects (written language, oral language, written foreign language) as well as two

TABLE 1. Descriptive statistics

	Mean	Standard deviation	N
Panel A: Administrative data			
Transition score	6.60	2.23	424,929
Average transition score of peers	6.60	1.54	424,929
Attend academic high school	0.36	0.48	424,929
Baccalaureate taken	0.53	0.50	424,929
Baccalaureate grade	8.33	1.00	201,912
Romanian Bacc. grade	7.56	1.49	225,264
Panel B: Census data (1992)			
Mother's characteristics			
Primary education	0.09	0.29	86,408
Secondary education	0.87	0.34	86,408
Tertiary education	0.04	0.19	86,408
Urban region of birth	0.28	0.45	86,755
Married	0.95	0.22	86,758
Divorced	0.01	0.11	86,758
Number of children	2.29	1.93	86,774
Age at birth	25.83	6.26	86,774
Panel C: Survey data			
Parent helps with homework	0.17	0.37	6,694
Child reports doing homework	0.62	0.49	6,758
Parent pays for tutor	0.30	0.46	6,723

Notes: Panel A uses administrative data to describe schooling outcomes and characteristics for children allocated to a secondary school in the years 2005 and 2006 (Source: Romanian Ministry of Education, www.edu.ro). Panel B uses a 15 percent sample of the 1992 census to describe mothers' characteristics; it refers to all women who gave birth in 1991 and 1992. Panel C uses data from a survey we implemented (in most towns with two or three high schools) to describe parental and child behaviors.

Table 1 (page 6) presents summary statistics. Panel A refers to the administrative data and shows that the average transition score among school applicants is 6.6 on a scale of 1 to 10. About 36 percent of these individuals attend academic high schools, which are more prestigious, with the remaining 64 percent attending other schools. The fraction of students taking the Baccalaureate exam is 53 percent and the average overall grade is 8.3.[8] Table 1 also features the Romanian Baccalaureate grade, which is the only subject that all students must take.

2.2. **Census data.** The administrative data contain little information on children's background, limiting their usefulness in analyzing whether the increased access to abortion

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track-specific and one elective test. The overall grade is the unweighted average of these scores. The main exam is administered in July. Students are generally not allowed to take the exam early.

[8] There are only slight differences in these numbers across the cohorts we consider. In addition, we note that the matched data do not allow us to differentiate between high school dropouts and students who complete high school but do not take the Baccalaureate exam.

changed the composition of births.[9] We therefore also use the 1992 Census to describe the background characteristics among different birth cohorts.[10] We focus on: (i) markers of mothers' socio-economic status that are likely to affect children's outcomes, such as education and urban region of birth, and (ii) markers of "unwantedness" that may indicate children were not planned, such as mothers' marital status, fertility, and age at birth. For instance, an effect of the abortion policy on the age of mothers at birth would be consistent with some children not having been optimally timed under the pre-reform restrictive regime.

While we can only recover maternal characteristics for children living with their mothers, the fact that the census took place in 1992, when children born in 1990 were only about two years old, allows for a match rate exceeding 95 percent.[11] Table 1 (Panel B) shows that women who gave birth in 1990 and 1991 were on average 26 years old, and had given birth to 2.3 children by 1992. About 28 percent of them were born in an urban region. Only 9 percent had primary education (6 years of schooling) or less, 87 percent had secondary education, and the remaining 4 percent held a university degree. Finally, 95 percent of women were married and a negligible fraction were divorced; the remainder were single.

2.3. **Survey data.** Neither the administrative nor the census data provide much information on parent and child behaviors. We therefore implemented a survey featuring parent and student questionnaires. The administrative data provided students' names, but no way of contacting them or their parents. We therefore approached schools and asked their administrators to provide us with the addresses of the students in the 2005 and 2006 cohorts (who were still in school at the time).

We used these addresses to directly approach households and administered three survey components. First, we interviewed the family head to obtain demographic information on each member of the household. Second, we surveyed the primary caregiver to elicit information on each child. Third, we interviewed the child from the selected schools.

Two factors led us to restrict our target sample to towns containing two or three schools. First, since we needed information from students on either side of admissions cutoffs, it was necessary that all schools in each town agree to participate, and therefore the effort was more likely to encounter problems in larger towns. Second, as we show below the administrative data reveal that the magnitude of the first stages is three to four times larger in smaller towns.

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[9] An exception is an indicator for participating in a scholarship program aimed at students from poor families, which we use below.

[10] Specifically, we use the publicly available 15 percent sample of the 1992 Romanian population census.

[11] We do not find evidence that the abortion policy changed the probability of living with a parent in 1992.

We started with a sample of 38,466 children and 167 schools in the 71 towns with two or three schools. If any school in a given town declined to participate, we abandoned the town. In the end, we obtained complete school surveys and student data from 148 schools in 64 towns; the administrators in these schools provided us with 21,530 addresses. We restricted the target sample further to 138 schools in 59 towns.[12] Due to financial constraints we randomly sampled 13,408 children out of this population, and obtained 8,400 parent and child surveys from this target sample.[13] After restricting the sample to children born in 1990 and 1991 who appear in 2005 and 2006 respectively, our final working sample contains 6,771 children. We found no evidence that response rates differed between households with children just above and just below cutoffs.

Table 1 (Panel C) lists the three parental and child behaviors we focus on: whether parents report helping their children with homework, whether children report doing homework, and whether parents report paying for a tutor for their children.[14] The levels for these variables are 17, 62, and 30 percent, respectively.

To compare the survey towns to the broader sample, Appendix Table 9 presents descriptive statistics from the administrative data for the full sample and the survey subsample (panel A and B, respectively). Panels A.4 and B.4 show that, as expected, the survey towns contain fewer schools and students on average. However, their academic performance is generally comparable to that of children in the full sample (panels A.1 and B.1).

3. Background on sources of variation

This section describes the two sources of variation we rely on: the 1989 repeal of the abortion ban, and the system that allocates students to high schools in Romania.[15]

3.1. **The 1989 liberalization of access to abortion.** In the 1950s Romania followed the Soviet sphere in providing liberal access to abortion, and this procedure became the main method of birth control used in the country.[16] As elsewhere in the world, medical practice restricted the procedure to the first trimester of pregnancy. In 1966, the government abruptly outlawed abortion for most women of reproductive age and severely restricted access to

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[12] The elimination of five towns reflected that at least one school in each of them, though willing to fill out the school questionnaire, was unable to provide student addresses.

[13] Our response rate of 63 percent is in line with Gallup Romania's (the firm we contracted with) interview rate for this population.

[14] Private tutoring is common in Romania, as in other countries with high-stakes tests.

[15] Section 4.1 draws on Pop-Eleches (2006); for further discussion see Kligman (1998). Section 4.2 draws on Pop-Eleches and Urquiola (2013).

[16] For instance, in 1965 about four abortions were performed for every live birth in Romania (Berelson, 1979). This approximately inverts the pattern observed at present in the United States, where there are about five live births per abortion (CDC, 2015).
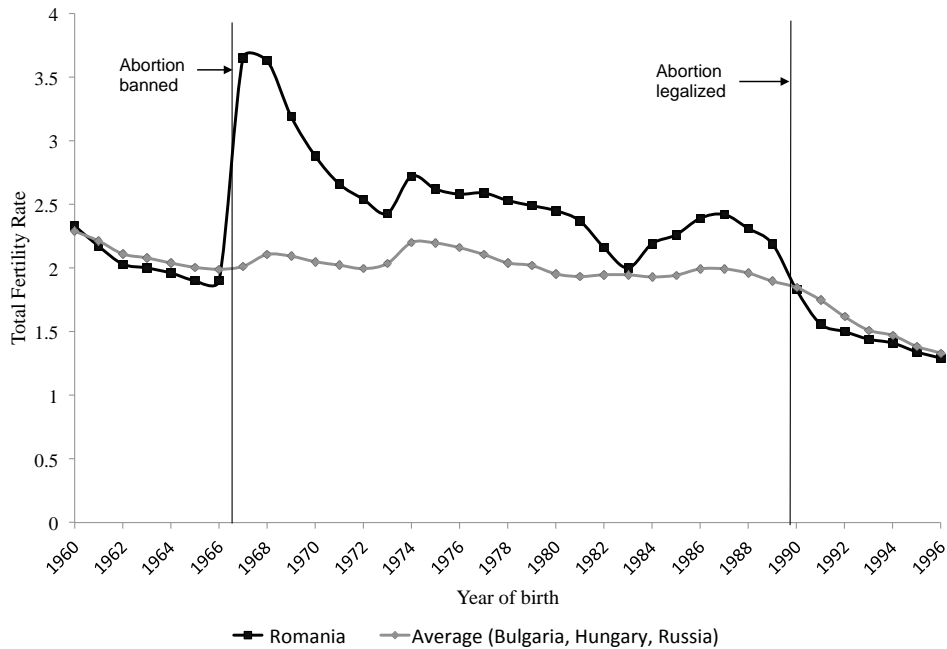
FIGURE 1. Total fertility rate by year

Notes: The figure plots the total fertility rate calculated for each year. The total fertility rate is the number of children each woman would have if she were to live through her childbearing years and have children in accordance with contemporaneous age-specific fertility rates. These data come from various years of the Population and Vital Statistics Report of the United Nations Statistical Division (http://unstats.un.org/unsd/demographic/products/vitstats/default.htm).

other modern methods of contraception.[17] This policy stance was maintained with minor modifications until the collapse of communism in 1989.

The renewal of access to abortion that took place at that point was equally abrupt, and is the focus of our paper. Specifically, mirroring events in Eastern Europe at the time, civil unrest began in Timisoara in mid-December 1989, and quickly spread to other parts of the country; on December 25th, Romania's dictator was executed. On December 26th the interim leadership abolished the ban on access to abortion, and in January of 1990 it also lifted the ban on the import of modern contraceptives.

Figure 1 (page 9) illustrates the impact these measures had by plotting the 1960-1996 total fertility rate for Romania as well as for the average of three other Eastern European countries (Hungary, Bulgaria, and Russia) that did not have similar restrictions on the supply of birth control. The vertical lines indicate the years in which abortion was banned and legalized again. Between 1960 and 1966—to the left of the first vertical line—Romania's total fertility rate tracks the average of the three other countries relatively well. The abrupt jump at the

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[17] Exceptions were made in that women could still access abortion if their age exceeded 42, they had more than four children, or they faced special circumstances involving health problems, rape, or incest.

first line shows that the effect of the 1966 ban was dramatic, with the total fertility rate roughly doubling (from about 1.9 to 3.7 children) by 1967. Fertility did decline and stabilize in the years following, although at a higher level.[18]

Figure 1 also shows an analogous and immediate, if less pronounced, decline in fertility at the second vertical line, following the 1989 liberalization. In addition, one observes a gradual decline between 1992 and 1996, in Romania and in the other three countries. This decline, which also took place in other transition economies, is likely the result of the social and economic transformations following the end of Communism.

The introduction and repeal of the abortion ban was also associated with changes in children's health at birth. Pop-Eleches (2006) shows that following the introduction of the abortion ban, the infant mortality and low birth weight rates increased by 27 and 38 percent, respectively. Analogous changes happened after the repeal of the abortion ban in 1989. Appendix Figure 7 shows that, between 1989 and 1991, the infant mortality rate decreased from 26.9 to 22.7, and that the fetal death rate from 7.6 to 6.9.[19] Similarly, Appendix Figure 8 shows post-1990 declines in the percentage of births with extremely (under 1000g) and very low (under 1500g) weight.[20]

Pop-Eleches (2010) argues that the sharp drop in fertility that followed 1989 was driven by the change 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 process. Figure 2 (page 11) provides further evidence consistent with this. It uses census data to plot the number of children born each month during 1989-1991, with June of 1990 normalized to zero, and a vertical line indicating the demarcation between June and July. It shows that an abrupt drop in fertility began in July of 1990—precisely six months after the point at which expectant mothers in their first trimester could have first accessed abortion. This sudden decline is consistent with anecdotal evidence; for instance, a Romanian gynecologist reported to us that his hospital was "stormed" by 300 women desiring an abortion on December 27 of 1989. There is no other apparent trend in the number of births over the months featured in

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[18] The increase in fertility after 1984 reflects the introduction of further restrictions: (i) stricter monitoring of pregnant women, (ii) an increase in the minimum age at which women qualified for abortions from 42 to 45 years, and (iii) an increase in the analogous threshold in number of children from 4 to 5.

[19] Not surprisingly, given the abortion ban was repealed in December 1989, the decrease occurs earlier for the fetal death rate (in 1990) than the infant mortality rate (in 1991). Micro data on these outcomes is not available, so we cannot perform a more rigorous analysis of the health impacts of the lifting of the ban. In addition, the published statistics are not available by month of birth but only by year of birth.

[20] The decrease in the percentage of children with low birth weight (under 2500g) is smaller in 1990, and this statistic begins to rise in 1992, likely due to the changing composition of births later in the transition period.
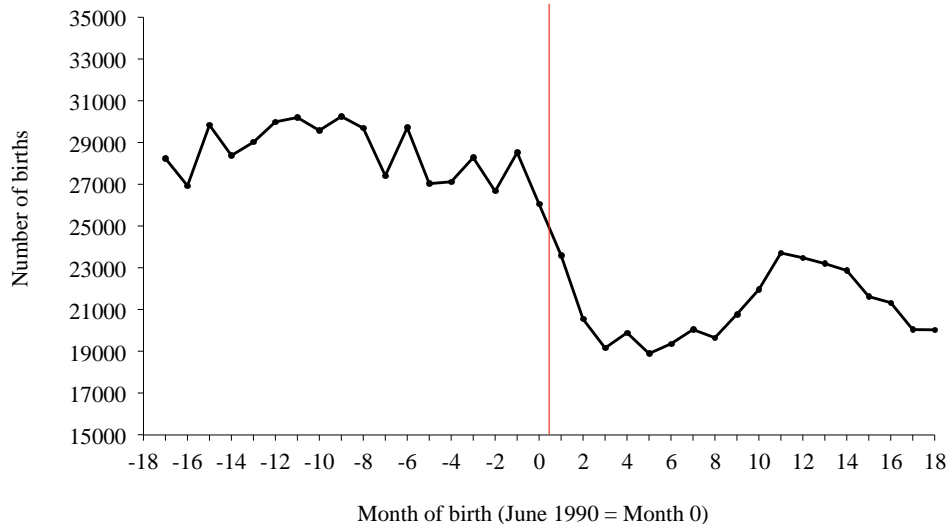
Figure 2. Cohort size by month of birth

Notes: The figure uses 1992 census data to plot the number of children born each month. June of 1990 is normalized to zero, and the vertical line indicates the demarcation between June and July of 1990.

Figure 2.[21] Finally, the decline in the size of the monthly birth cohorts is substantial: about 10 thousand births—a one-third reduction.

To summarize, Figure 2 shows that a discrete decline in fertility occurred six months after the repeal of the abortion ban, and is thus consistent with the increased access to abortion driving the lower number of births. Nevertheless, it does not rule out that the decline could be due to other factors provided that they also changed discretely at the end of 1989.

Pop-Eleches (2010) discusses two candidates. First, the government also introduced pronatalist incentives in 1966. These were small; for instance, they included a one time paid medical leave during pregnancy, a one time maternity grant of about 85 dollars, and a 3 dollar increase in the monthly child allowance. Nonetheless, these incentives might have increased the demand for children and, had they also been repealed in December of 1989, might at least partially account for the pattern observed in Figure 2. The World Bank (1992), reports, however, that there were no major changes in the provision of maternity and child benefits during the months surrounding December 1989.[22]

Second, the fall of the communist regime might have caused people to immediately lower their desired fertility if it led them to drastically update their expectations about the future. While this is possible, there is little evidence of sudden changes in fertility-related choices in

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[21] Also consistent with this, Pop-Eleches (2010) points out that the 1990-1992 period saw no increase in the use of modern contraceptives in Romania.

[22] Of course, the level of social benefits did change later during the transition period in Romania, as it did in most other formerly communist states. This reflected budgetary cuts caused by the severe drop in GDP and might have been a contributing factor to the gradual decrease in fertility during the 1990s (Figure 1).

most Eastern European countries. Instead, the consensus among demographers (e.g., David, 1999) is that the decline in fertility during transition was gradual and reflected the continuous worsening of economic conditions. One exception emerges in East Germany, where Chevalier and Marie (2015) document a rapid and temporary decrease in fertility starting in August 1990. They argue that this decline happened about nine months after the fall of the Berlin Wall. Thus, in contrast to the situation in Romania, this decline was due to a reduction in conceptions rather than to post-conception selection. Chevalier and Marie (2015) further point out that in the months immediately following the fall of the Berlin Wall, East Germany abandoned full employment policies and drastically reduced generous child benefits. This is also in contrast to the situation in Romania, where changes in such policies were much more gradual.

Thus, the fact that Romania displays a large decrease in fertility six rather than nine months after the lifting of the abortion ban suggests that the change in access to abortion, rather than a change in the demand for children, accounts for the decline in births. We will therefore study the children born immediately before and after July of 1990, attributing differences in their outcomes to factors correlated with the increased access to abortion.

Our main birth cohort of interest will be 1990. In addition, we will make use of the 1991 cohort as a control, to account for factors such as seasonality in births. To illustrate, under the rules that govern education in Romania, these cohorts should have enrolled in high school in 2005 and 2006, respectively. Figure 3 (page 13) uses the administrative data to plot the number of children born in each month for each of the admissions cohorts, with June normalized to 0 (i.e., month 0 is June 1990 for the 2005 admission cohort, and June 1991 for the 2006 cohort). Grey vertical lines indicate January and December (months -5 and 6), and a red, darker vertical line indicates the demarcation between June and July.

Figure 3 raises two observations. First, while the vast majority of children in each application cohort were born in the year expected—that is, they were born between months -5 and 6—this is not the case for all. For instance, while one expects children enrolling in 2005 to have been born in 1990, some were born in 1989, and fewer in 1991. Graphically, there are large drops in the observed densities at the grey vertical lines, but the densities do not fall to zero. The positive density for months -6 and below reflects that some children repeat grades in elementary school; in addition, some parents delay school entry for their children.[23] The positive and lower density for months greater than 6 reflects that some children begin school early.

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[23] This happens especially among children born close to the enrollment cut-offs. It is analogous to the "red-shirting" behavior observed in the U.S.
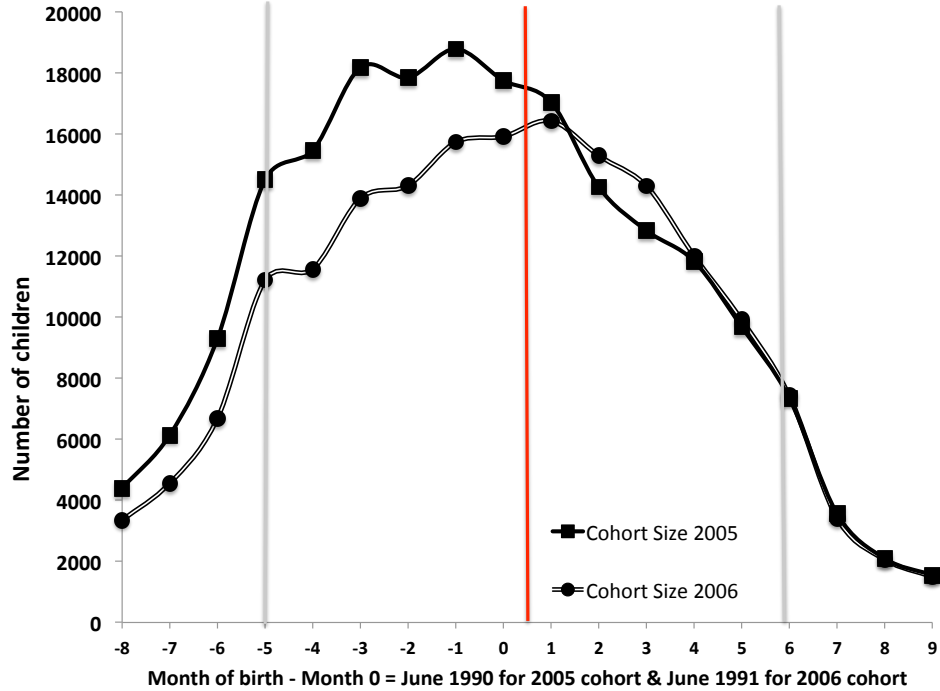
FIGURE 3. Cohort sizes by month of birth; 2005 and 2006 admission cohorts

Notes: This figure uses the administrative data to plot cohort sizes by month of birth. The months covered are October 1989 to March 1991 for the 2005 high school admission cohort, and October 1990 to March 1992 for the 2006 cohort. In each case June is normalized to 0, and -5 and 6 indicate January and December, respectively. The red, darker vertical line indicates the demarcation between June and July.

The second and more important observation is that to the left of the dark vertical line indicating the start of July, the 2005 cohort density is everywhere above that of 2006. This reflects that, as implied by Figure 2, there were more births in 1990 before access to abortion was liberalized. To the right of the line—a period with ease of access to abortion for both cohorts—the two densities essentially overlap.[24]

Finally, Figure 4 (page 14) presents densities of transition scores rather than births; its structure is otherwise similar to that of Figure 3. Two points of note emerge. First, in both application cohorts the children who are above normative age (those to the left of the first gray line) have lower transition scores, consistent with the fact that many of them likely repeated a grade.[25] Second, and more importantly, is a useful preview of one of our main results: the 2005 application cohort displays lower transition scores among children born immediately before July of 1990 (month 1); no similar difference is evident for the 2006 cohort.

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[24] To be precise, there is a slightly sharper decrease in the cohort size after Month 1 for the 2005 cohort.

[25] There is also a dip in average grades for children born around December (month 6). This is likely a result of month of birth effects, and that children who were sent to school earlier than required by the law might be different from the average population.
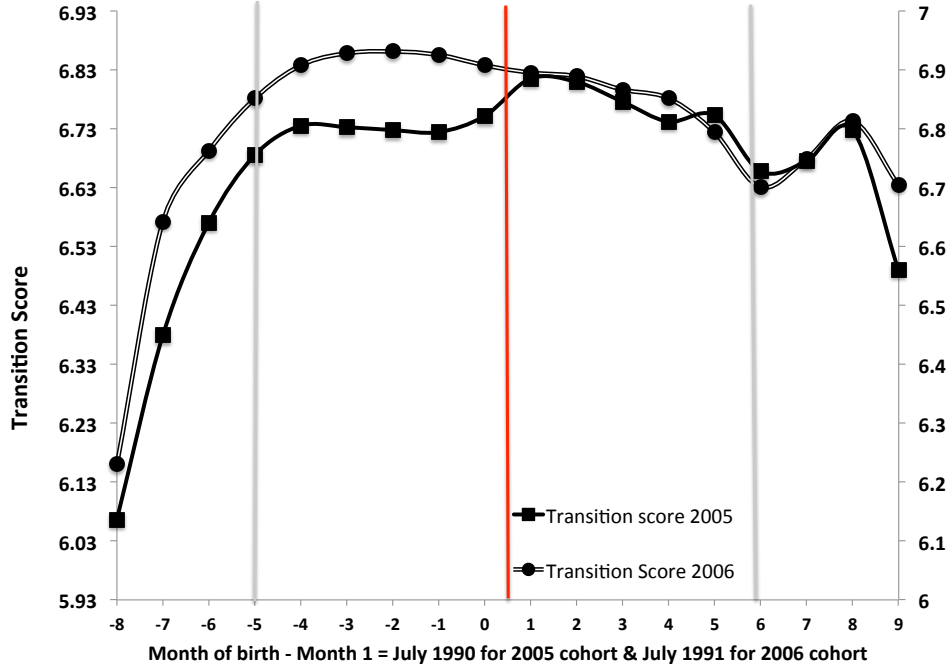
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FIGURE 4. Average transition score by month of birth; 2005 and 2006 cohorts

Notes: This figure uses the administrative data to plot average transition scores by month of birth. The months covered are October 1989 to March 1991 for the 2005 high school admission cohort, and October 1990 to March 1992 for the 2006 cohort. In each case June is normalized to 0, and -5 and 6 indicate January and December, respectively. The red, darker vertical line indicates the demarcation between June and July.

Taken together, Figures 3 and 4 provide *prima facie* evidence that the increased access to abortion resulted in smaller cohorts with better educational outcomes. In addition, despite the mandated allocation to application cohorts based on date of birth, there may be some selection, and we can address this by using the 2006 admission cohort as a control sample.[26] Section 5 below formalizes how we use this variation in an empirical strategy.

3.2. **The student allocation mechanism.** Our second source of variation originates in Romania's high school admission process. The transition between middle school, labeled *gymnasium*, and high school results in an unusually systematic allocation of students to schools.[27] Every child who completes middle school receives a transition score which equally weights: (i) her performance in a national 8th grade exam covering Language, Math, and History/Geography,[28] and (ii) her middle school grade point average.

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[26] We did not find any references indicating institutional changes at the high school level in Romania (both in terms of the structure of the school system or the rules of the admission process) between 2005 and 2006.
[27] During the period we study, schooling in Romania was compulsory until the 10th grade. As a result the entire cohort of students who complete middle school is required to participate in this allocation process.
[28] 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.

After receiving their transition scores, students must 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. These tracks operate as "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, 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' choices are expressed through an application form submitted via their middle schools to the Ministry of Education. Using a computerized system, the Ministry then ranks students by their transition score—no other criteria (e.g., sibling preferences or geographic proximity) are considered. The mechanism considers the highest ranked student and assigns her to her most preferred school/track. It then treats the second-ranked 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 does the mechanism move onto the next person.[29] Students thus have incentives to truthfully reveal their preference rankings.[30]

Schools must enroll the children in the admission list returned from the computerized allocation.[31] In most markets the result of this process is a clear hierarchy of schools by average peer quality. As stated above, for simplicity we will label higher ranked schools as "better" schools.[32]

Finally, we note that when we analyze the effects of having access to a better school, we impose three sample restrictions on the administrative data. The first two reflect that, as

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[29] 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.

[30] 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.

[31] One concern with the administrative data arises if the participation of children in the high 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 ban have higher dropout or grade repetition rates in primary school and therefore do not take the high school admissions exam. However, this source of selection is unlikely to play a major role. Appendix Figure 9 uses the census data to show that the proportion of children born in each month of 1990 who are present in the 2005 high school cohort is quite similar to the proportion of children born in each month of 1991 who are present in the 2006 high school cohort.

[32] Pop-Eleches and Urquiola (2013) show that school level peer quality is correlated, in the expected direction, with factors like parental involvement and perceptions of quality on the part of parents and school principals. Nevertheless, rankings by characteristics like peer quality need not correlate with value added, as suggested by Abdulkadiroglu et al. (2014), MacLeod and Urquiola (2015, 2019), and Ainsworth et al. (2020).

explained below, we rank schools and set cutoff scores under the assumption that towns are self-contained markets.[33] 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.[34] 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.[35]

4. EMPIRICAL STRATEGY

This section presents our empirical strategy in three steps. First, it describes the difference-in-difference (DD) framework we use to estimate the impact of access to abortion. Second, it describes the regression discontinuity (RD) approach we use to estimate the effects of access to a better school. Third, it merges these into a combined RD-DD framework that allows us to estimate reduced-form interactions between the increased access to abortion and access to a better school.

4.1. **The impact of access to abortion.** The first step is to estimate how the increase in access to abortion affected educational outcomes. Consider the following regression:

$$(1) \qquad y_i = \delta_0 + \delta_1 \cdot AccessA_i + \delta_2 \cdot before_i + \delta_3 \cdot cohort_i + \epsilon_i$$

where y_i is an outcome measured either upon applying to or upon finishing high school—the transition and Baccalaureate scores, respectively. $AccessA_i$ stands for access to abortion and is equal to 1 if individual i was born after July 1, 1990, which is six months after access to abortion increased and the point at which the decrease in fertility is first observed (Section 3). $before_i$ is a dummy for birth between January and June inclusive, and $cohort_i$ takes on a value of one for children in the 2006 admission cohort, and of zero for children in the 2005

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[33] We use the term town to denote a high school market. The term that appears in the administrative data is locality (*Localitate*, in Romanian). In most cases these units actually correspond to cities/towns. In a few, they denote the largest of a number of small towns or villages—the town which actually contains the high school that might draw from a corresponding catchment area composed of smaller towns or villages.

[34] 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.

[35] 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, but it is worth noting that less than one 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).

cohort. The overall impact of the change in abortion legislation is captured by δ_1, where standard errors are clustered by age in months (Bertrand, Duflo, and Mullainathan, 2004).

We also consider an alternative specification, replacing the indicator variable for being born in the first six months of a calendar year with a linear trend of the month of birth:

$$(2) \qquad y_i = \beta_0 + \beta_1 \cdot AccessA_i + \beta_2 \cdot trend_i + \beta_3 \cdot cohort_i + \epsilon_i,$$

where $trend_i$ is a function of the month of birth, which we model as a linear trend.[36]

To summarize, our approach essentially compares the outcomes of children born in the six months before and after the drop in fertility that occurred after July 1, 1990. The trend controls account for effects that are associated with age and vary continuously, and the 2006 cohort accounts for possible month of birth effects (e.g., associated with seasonality) as well as selection of a birth cohort into a corresponding high school admission cohort (Figure 3).[37] We note that equation (1) corresponds to the "classic" difference in differences specification (Meyer, 2005).

Finally, in our baseline implementation these specifications do not include controls. In robustness checks, we include an indicator of poverty status present in the administrative data in order to control for possible compositional changes.

4.2. **The impact of access to a better school.** The second step of our analysis estimates the impact of access to a better school. Although students can request any high school in the country, we suppose that they restrict their choices to the towns they live in, a reasonable assumption since they are 13-14 year olds typically 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.[38] 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.[39]

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[36] Using a quadratic trend instead of a linear one leads to similar estimates.

[37] All specifications are restricted to children who are in their normative admission cohort based on their date of birth; that is, children born in 1990 (1991) present in the 2005 (2006) high school cohort.

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

[39] Using the minimum admission score is in line with our "intent-to-treat" approach in 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.

This yields more than one thousand potential discontinuities. 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.

4.2.1. *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 their average transition score. 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. Let t_i denote student i's transition score, and $\tilde{t}_z$ be the minimum grade required for admission into the better of the two schools indexed by z.

Consider the specification:

(3) $$y_i = \alpha \cdot 1\{t_i - \tilde{t}_z \geq 0\} + a(t_i) + \varepsilon_i,$$

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.

If access to a better 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 (Hahn, Todd, and van der Klauuw, 2011). Intuitively, suppose the transition score is smoothly related to characteristics that affect achievement. Under this assumption, students with scores just below $\tilde{t}_z$ 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. If the effect of going to the better school is homogenous across students, then this treatment effect is identified as α in (3). Provided that $a(t)$ is specified correctly, it will capture the dependence of outcomes on the transition score away from the cut-off, and one can use all the data to estimate (3).

We use specifications that expand on (3) to allow for treatment effect heterogeneity, in order to produce "intent-to-treat" estimates of the effect of having access to a higher-ranked school.[40] Our "first stage" results show that a significant proportion of children who have access to a better school take it up. This allows us to measure the net effect of such access on

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[40] We view α in equation (3) as capturing a "reduced form" effect of access to a better school. This effect could reflect multiple channels. For example, as we will show, peer quality certainly changes at the discontinuity. But so do teacher quality and some parental investments (Pop-Eleches and Urquiola 2013).

children's outcomes. It is, however, impossible to attribute the effect to a single channel, since multiple aspects of school quality change at the cutoffs, in addition to possible behavioral responses on the part of students, parents, teachers, etc.

4.2.2. *Summarizing information for many cutoffs*. The above specifications illustrate how one might exploit one cutoff. As stated, our data contain over one thousand. 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. In fact, regressions restricted to students in bands close to the cutoffs rarely use student-level observations more than once.

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

$$(4) \qquad y_i = \eta \cdot 1\{t_i - \tilde{t}_z \geq 0\} + \alpha \cdot (t_i - \tilde{t}_z) + \psi \cdot (t_i - \tilde{t}_z) \cdot 1\{t_i - \tilde{t}_z \geq 0\} + w_z + v_i,$$

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.[41]

Finally, to simplify notation, equation (4) can be written as:

$$(5) \qquad y_i = \eta \cdot AccessB_i + \alpha \cdot score_i + \psi \cdot score_i \cdot AccessB_i + u_i,$$

where $AccessB_i$ stands for access to a better school and is a dummy equal to one when a student's transition score is greater than or equal to the cutoff; $score_i$ is the running variable and $score_i \cdot AccessB_i$ is the interaction to allow for the linear spline; the cutoff fixed effects are now implicit.

4.3. **Estimating interactions between family and school environments.** The third and final step is to combine the above two approaches to estimate potential interactions between family and school environments. We merge the difference-in-differences (DD) and

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[41] For simplicity, equation (4) does not have a time dimension; in reality our standard specification includes a full set of cutoff*year dummies.

the regression discontinuity (RD) design into an RD-DD framework. We begin by discussing the intuition behind this approach, and then present the full interacted specification.

4.3.1. *Intuition.* The increase in access to abortion defines four groups as captured in Figure 3: (i) those born July-December, 1990, (ii) those born January-June, 1990, (iii) those born July-December, 1991, and (iv) those born January-June, 1991. Figure 3 shows that group (ii) contains distinctly more children and likely the highest share of unwanted children.

One can estimate the impact of access to a better school within each of these groups—i.e., one can estimate η (equation 5) within each group, learning about the average effect of school quality from hundreds of cutoffs in each case. These estimates provide information on the interaction between our environments of interest. For instance, a positive difference

$$(6) \qquad\qquad \eta(July\text{-}Dec, 1990) - \eta(Jan\text{-}Jun, 1990)$$

would provide *prima facie* evidence of complementarity, as it suggests that the effect of access to a better school is higher among children who had higher skill upon entering high school because their parents had easier access to abortion.

A difference like (6) could be driven by factors like seasonality, and one can therefore use groups (iii) and (iv) to control for such factors, calculating a difference in differences effect:

$$(7) \quad \{\eta(July\text{-}Dec, 1990) - \eta(Jan\text{-}Jun, 1990)\} - \{\eta(July\text{-}Dec, 1991) - \eta(Jan\text{-}Jun, 1991)\}$$

4.3.2. *Non-parametric identification of interaction effects.* We now show how the intuition of (7) can be extended to non-parametrically identify a summary measure of reduced-form interaction effects, when stacking the data across cutoffs. To define our parameter of interest, let $Y_i(d, z)$ denote potential outcomes as a function of whether student i is born in a period in which their parents had access to abortion d, and the high school to which they are assigned z. By saying a student is "assigned" to school z , we mean that z is the most selective school to which they are admitted.[42] Recall that $AccessA_i$ is equal to 1 if individual i was born after July 1, 1990, and 0 otherwise. We focus on mean interaction effects among students in the pre-abortion period, evaluated at the school to which they are actually assigned, denoted as $school_i$:

$$\Delta_0 := E[\{Y_i(1, school_i) - Y_i(1, school_i - 1)\} - \{Y_i(0, school_i) - Y_i(0, school_i - 1)\} \,|\, AccessA_i = 0]$$

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[42] We focus on intent-to-treat effects. $Y_i(d, z)$ measures outcomes given attendance at school z if students choose the most selective school they are admitted to.

Here we number schools as integers within each town in order of their cutoff, so that $school_i - 1$ denotes the school with the next-highest cutoff to $school_i$. The parameter Δ_0 measures the average extent to which these "next-best"-school treatment effects $Y_i(d, school_i) - Y_i(d, school_i - 1)$ differ between $d = 0$ and $d = 1$: a mean interaction effect. We condition on $AccessA_i = 0$ because this is the population for which the difference-in-differences variation is informative with minimal assumptions.[43]

The potential outcomes notation allows us to see why a simple comparison of RDD estimands like (6) is insufficient to isolate interaction effects. Suppose for simplicity there were a single town having two schools: z and $z - 1$, separated by transition score threshold t. Then performing separate RDDs for students born before and after July 1990 would allow us to estimate the following quantity:

$$E[Y_i(1, z) - Y_i(1, z-1)|AccessA_i = 1, score_i = t] - E[Y_i(0, z) - Y_i(0, z-1)|AccessA_i = 0, score_i = t]$$

If the $AccessA_i = 1$ and $AccessA_i = 0$ groups are not comparable, the above estimand will confound interaction effects with heterogeneity in next-best school effects.

We thus instead build up identification of Δ_0 by considering a double-difference of RDD estimands at each school threshold, as in (7). Let t_{pcz} denote the transition score threshold that applies to school z for cohort c in town p, and let $after_i$ be a random variable indicating whether a student was born after June in their birth year (i.e. in July-Dec). Consider p, a, c specific discontinuities at the threshold for a given school z:

$$\eta_z(a, c, p) := \lim_{x \downarrow t_{pcz}} E[Y_i|town_i = p, after_i = a, cohort_i = c, score_i = x]$$

$$- \lim_{x \uparrow t_{pcz}} E[Y_i|town_i = p, after_i = a, cohort_i = c, score_i = x]$$

Under appropriate versions of the familiar continuity and parallel trends assumptions,[44] the double-difference of such RDD estimands $\eta_z(July\text{-}Dec, 1990, p) - \eta(Jan\text{-}Jun, 1990, p) - \eta_z(July\text{-}Dec, 1991, p) - \eta_z(Jan\text{-}Jun, 1991, p)$ is equal to

$$E[Y_i(1, z) - Y_i(1, z - 1)|town_i = p, AccessA_i = 0, score_i(1) = t_{p0z}]$$

$$- E[Y_i(0, z) - Y_i(0, z - 1)|town_i = p, AccessA_i = 0, score_i(0) = t_{p0z}]$$

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[43] In the canonical difference-in-differences (DD) setup with two periods, one group is treated in the second period only and the DD identifies an average treatment effect among the treated. In our context, three of the four cells defined by birth-half-of-year and cohort are "treated", while one is not, so our DD identifies an average treatment effect among the *untreated*. See appendix for details.

[44] We assume continuity of the functions $E[Y_i(d, z)|P_i = p, after_i = a, cohort_i = c, score_i(d) = x]$ with respect to transition scores x, and we assume that expectations like $E[Y_i(1, z'))|P_i = p, after_i = a, cohort_i = c, score_i(1) = x]$ decomposes as a separable function of a and c. See Appendix B for details.

where we introduce potential outcomes $score_i(d)$ for the transition score, depending on value d of the abortion-access treatment.

Note that both terms above condition on $AccessA_i = 0$, and the difference then *nearly* gives a local average measure of the interaction between family and school environments, $Y_i(1, z) - Y_i(1, z - 1) - Y_i(0, z) + Y_i(0, z - 1)$, for the school z. However, the first term conditions on $score_i(1)$ while the second conditions on $score_i(0)$. The raw double-difference of RDD estimands thus suffers from a version of the "bad control" problem, since students for whom $score_i(0) = t_{p0z}$ may be different from the students for whom $score_i(1) = t_{p0z}$. This arises because our regression discontinuities control for $score_i$, which may be affected by $AccessA_i$. In the appendix, we show that it is nevertheless possible to recover Δ_0 by combining estimates across many cutoffs in a way that averages the two terms of the above over the distributions of $score_i(1)$ and $score_i(0)$ respectively.

Here we describe the approach in broad strokes, and refer to Appendix B for details.[45] First, we approximate the school cutoffs t_{p0z} as being dense in the support of transition scores, on the basis of there being many schools in each town. This assumption is also made by Bertanha (2020), and in our context ensures that for any student having $score_i(1) = t_{p0z}$, there exist valid comparison students for them with $score_i(0) = t_{p0z'}$ for various schools z'. Second, we require a parallel trends assumption to hold for the CDF of transition scores (Roth and Rambachan, 2022), allowing us to impute the distribution of $score_i(1)$ conditional on $AccessA_i = 0$ and $town_i = p$.[46] This allows us to aggregate over values of $score_i(1)$ and $score_i(0)$ respectively to average out the conditioning on counterfactual transition scores. Finally, we assume that there are (on average) no indirect effects of abortion-access on untreated next-best school effects $Y_i(0, school_i) - Y_i(0, school_i - 1)$, occurring through abortion-access changing the value of $school_i$. We give evidence for this last condition by observing that average effect of abortion access on transition scores is very small relative to the typical gap between successive schools' thresholds. This suggests such effects are rarely sufficient to move a student over a threshold into a new school. If they are, Section 5.5.3 provides evidence that this is likely to bias us towards finding positive interaction effects, if anything.

4.3.3. *Implementation.* Here we first describe a simple implementation of the above identification logic in the "stacked" dataset, and then discuss a re-weighting scheme that ensures we aggregate over the $\eta_z(a, b, p)$ in an appropriate way to consistently estimate Δ_0. Consider

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[45] An alternative approach to the one described here would be to assume away the "bad control" issue directly: that $E[Y_i(0, z) - Y_i(0, z - 1)|town_i = p, AccessA_i = 0, score_i(d) = t_{p0z}]$ does not depend on d. We have found that this approach gives similar results.

[46] A similar need arises in Caetano et al. (2022), who consider difference-in-differences models with time-varying covariates that can be affected by treatment.

the following fully-interacted regression specification:

$$Y_i = \lambda_0 + \lambda_1 \cdot AccessA_i + \lambda_2 \cdot trend_i + \lambda_3 \cdot cohort_i + \lambda_4 \cdot AccessB_i + \lambda_5 \cdot score_i$$

$$+ \lambda_6 \cdot (score_i \cdot AccessB_i) + \lambda_7 \cdot (AccessA_i \cdot AccessB_i) + \lambda_8 \cdot (AccessA_i \cdot score_i)$$

$$(8) \quad + \lambda_9 \cdot (AccessA_i \cdot score_i \cdot AccessB_i) + \lambda_{10} \cdot (trend_i \cdot AccessB_i)$$

$$+ \lambda_{11} \cdot (trend_i \cdot score_i) + \lambda_{12} \cdot (trend_i \cdot score_i \cdot AccessB_i) + \lambda_{13} \cdot (cohort_i \cdot AccessB_i)$$

$$+ \lambda_{14} \cdot (cohort_i \cdot score_i) + \lambda_{15} \cdot (cohort_i \cdot score_i \cdot AccessB_i) + w_i + \mu_i$$

The coefficient of interest is λ_7, the interaction between $AccessA_i$ and $AccessB_i$. This essentially estimates whether the impact of having access to a better school is larger for children who experienced better family environments because they were born after access to abortion was liberalized. Our preferred implementation of (8) will use data from both the 2005 and 2006 school entry cohorts. We also present results with alternative specifications that: replace the trend controls with an indicator variable for being born in the first six months of a calendar year. For our main outcomes we will also show results from more restrictive specifications that drop the triple interactions $(\lambda_9, \lambda_{12}, \lambda_{15})$. These restrictive specifications assume that the change in slopes (relating the outcomes to the transition score) above and below the school cutoffs are the same across the different cohorts that determine access to abortion.

We view (8) as a local-linear approximation to the non-parametric identification result mentioned in the last section. The stacking approach allows us to improve efficiency by avoiding the need to estimate $\eta_z(a, b, p)$ separately for each school. However, it introduces a non-trivial reweighting towards towns with more schools. We thus re-estimate (8) after re-weighting the Y_i to correct for this, as described in the appendix. This allows us to run the final regression (8) in a way that pools data across all cutoffs. This approach does however require the first step of estimating the weights, for which we use local-polynomial regressions. For the re-weighted estimates, we thus calculate standard errors by non-parametric bootstrap, as a simple way to account for the various sources of estimation error. For both weighted and unweighted estimates, we include cutoff fixed effects which has been shown to alleviate issues arising from there being students located exactly at each school cutoff (Fort et al., 2022).

4.3.4. *Discussion the interacted specification.* There are two potential concerns related to the fact that the transition score serves as the running variable in our RD design. The first has been alluded to already: our empirical strategy for identifying next-best school treatment effects involves controlling transition scores, which is a "post-treatment" variable

with respect to the abortion reform. However, our identification result builds up from the RDD estimands $\eta_z(a, c, p)$, which compare across transition scores *within* rather *across* the four groups in our DD specification. Each of the $\eta_z(a, c, p)$ cleanly identifies an average next-best school effect, though care is required in aggregating across them to identify Δ_0 (as described in the appendix).

A second concern is that because (7) compares children just above and below each cutoff, there is no remaining variation in transition scores for the second comparison (the double-difference) across cohorts. In other words, the double-differenced RDD estimand of (7) will miss true interaction effects that occur through the transition score. In the extreme, it could be zero even in the presence of interaction effects if $Y_i(d, z)$ *only* depend on abortion access d through $score_i(d)$. The re-weighting estimator described in the appendix avoids this problem by aggregating the information across all thresholds before comparing students across cohorts: students with $score_i(1) = x$ are not compared only to students with $score_i(0) = x$. But even the un-weighted estimates may not suffer much from this issue: as shown in Section 4, children born under a more restrictive abortion policy appear to be disadvantaged in a range of developmental outcomes beyond those captured by the transition score. Therefore, even among children with the same transition score, we expect there to be differences in the vector of skills between individuals born under different abortion regimes.

5. Results

This section first presents results that examine the impact of access to abortion on educational outcomes. It then presents results on the impact of access to a better school. Finally, it uses our combined RD-DD framework to explore interactions between the two.

5.1. **The impact of access to abortion.** Table 2 (page 25) summarizes the impact of access to abortion. The columns feature six indicators of educational achievement and school quality, and panels A and B show the coefficient on $AccessA_i$ in two different specifications. Panel A restricts the sample to children born in 1990 and 1991 who applied to high school in 2005 and 2006 respectively, and includes a cohort dummy. Panel B is our preferred specification; it includes children in both admissions cohorts, a cohort dummy, and a linear trend in month of birth.

All the coefficients in Table 2 are positive, and 10 out of 12 are statistically significant. The robustness of this result provides strong evidence that increased parental access to abortion improved children's educational outcomes. Our preferred specifications in Panel B show that children who were born after access to abortion was liberalized on average had transition scores that were 0.1 points higher. This enabled them to gain admission to schools

TABLE 2. The effect of access to abortion on educational outcomes

	Transition score	Average transition score of peers	Attend academic high school	Baccalaureate taken	Baccalaureate grade	Romanian bacc. Grade
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2005 and 2006 cohorts						
Abortion access (*AccessA*)	0.104***	0.0362*	0.0213***	0.0461***	0.0226*	0.00935
	[0.0304]	[0.0196]	[0.00486]	[0.0141]	[0.0120]	[0.0107]
Monthly trend	N	N	N	N	N	N
Cohort dummy	Y	Y	Y	Y	Y	Y
N	336,004	336,004	336,004	336,004	171,902	191,160
Panel B: 2005 and 2006 cohorts						
Abortion access (*AccessA*)	0.0972***	0.0398**	0.0190***	0.0494***	0.0282***	0.0116
	[0.0274]	[0.0163]	[0.00448]	[0.0131]	[0.00797]	[0.00747]
Monthly trend	Y	Y	Y	Y	Y	Y
Cohort dummy	Y	Y	Y	Y	Y	Y
N	336,004	336,004	336,004	336,004	171,902	191,160
Mean of dependent variable	6.60	6.60	0.36	0.53	8.33	7.56

Notes: Standard errors are provided in brackets and are clustered by age in months. The dependent variables are defined in Table 1. The Abortion access 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 and B are based on children born in 1990 and present in the 2005 secondary school cohort as well as children born in 1991 and present in the 2006 cohort as controls. The monthly trend is a linear function of the month of birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

with peers whose transition scores were on average 0.04 points higher.[47] In addition, they were two percentage points more likely to attend an academic high school (from a baseline mean of 36 percent). Analogous positive impacts are observed four years later when these children took the Baccalaureate exam. Children born after access to abortion increased are 5 percentage points more likely to take the exam and, conditional on doing so, score 0.03 and 0.01 standard deviations higher overall and in the Romanian language component, respectively. The overall baccalaureate grade is the main measure of achievement we will use below.[48]

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[47] The average test score of the 2006 cohort is slightly higher than that of the 2005 cohort. One might therefore worry that an equal point increase in performance might have a different impact across the two cohorts in terms of how far a student moves along the ability distribution. In such a situation one might use the log of the admission score. In regressions not reported we find this does not affect our key results.

[48] We performed additional robustness checks not reported here. One potential concern with Table 2 is that the estimates may depend on how we control for trends and seasonal factors. The results are robust to a specification that is similar to Panel B but restricts the sample to the cohort born in 1990 who applied to high school in 2005, as well as to replacing the linear trends in month of birth with calendar month dummies. Another alternative is to restrict the analysis to narrower time windows. The simplest comparison is the difference in outcomes for children born in July and June of 1990. The difference in the average score between these two month is slightly smaller but still sizable and statistically significant for most of our outcomes. We find similar results with a sample that is restricted to children born in June and July but additionally includes children born in the same months of 1991. Another alternative arises given that: (i) gestation length varies across pregnancies, and (ii) the December 1989 legal abortions are probably more likely to

For further illustration Figure 5 (page 27) plots residuals from regressions that account for month of birth effects, for the months of January to December of 2005. The graphs for all six outcome variables, while displaying some noise, show a visible break in the pattern of educational achievement after July of 1990. They provide complementary evidence that children born after the repeal of the abortion ban had better educational outcomes.

5.2. **The impact of going to a better school.** We now turn to the impact of our second source of variation: access to a better school. As is common in RD-based analyses, we begin with a graphical illustration of our results. Panel A in Figure 6 (page 28) illustrates the basic first stage result, pooling all between-school cutoffs. The x-axis plots students' transition scores relative to the cutoffs that allow access a better school; the y-axis describes the peer quality that students experience, as measured by the mean transition score at their respective school. The mean transition score is collapsed into cells containing individuals who are within 0.01 points of each other. Panel B is structured similarly, but the y-axis 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 rises significantly and discontinuously when their transition score crosses a score cutoff. In other words, on average students do use the opportunity to attend a better school. The vertical distance between the points close to the discontinuity corresponds to the estimate of η_1 in expression (5).

Table 3, Panel A presents the regression analog to these two panels. Column (1) restricts the sample to children within bandwidths suggested by the procedure in Imbens and Kalyanaraman (2012) (henceforth IK).[49] Column (2) uses samples that result from the Calonico, Cattaneo, and Titiunik (2014) (henceforth CCT) optimal bandwidth, which is a refinement of the Imbens and Kalyanaraman (2012) optimal bandwidth. For both samples we regress the average transition score of children's school peers on an indicator for whether their scores are above the cutoffs. The specification includes: (i) a linear spline in students' score distance to the cutoff, allowing the slope to vary on either side of the cutoff, and (ii) cutoff/year

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have happened for mothers in the third rather than the second month of pregnancy. These factors imply that the decline in fertility after July of 1990 should not be completely instantaneous. Consistent with this, Figure 2 shows that while July of 1990 was the first month with a rapid decline in fertility, August also saw a significant further reduction. We therefore also restricted the sample to those born in June and August only. The results are very similar to those using the comparison of June and July.

[49] 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 twenty 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.
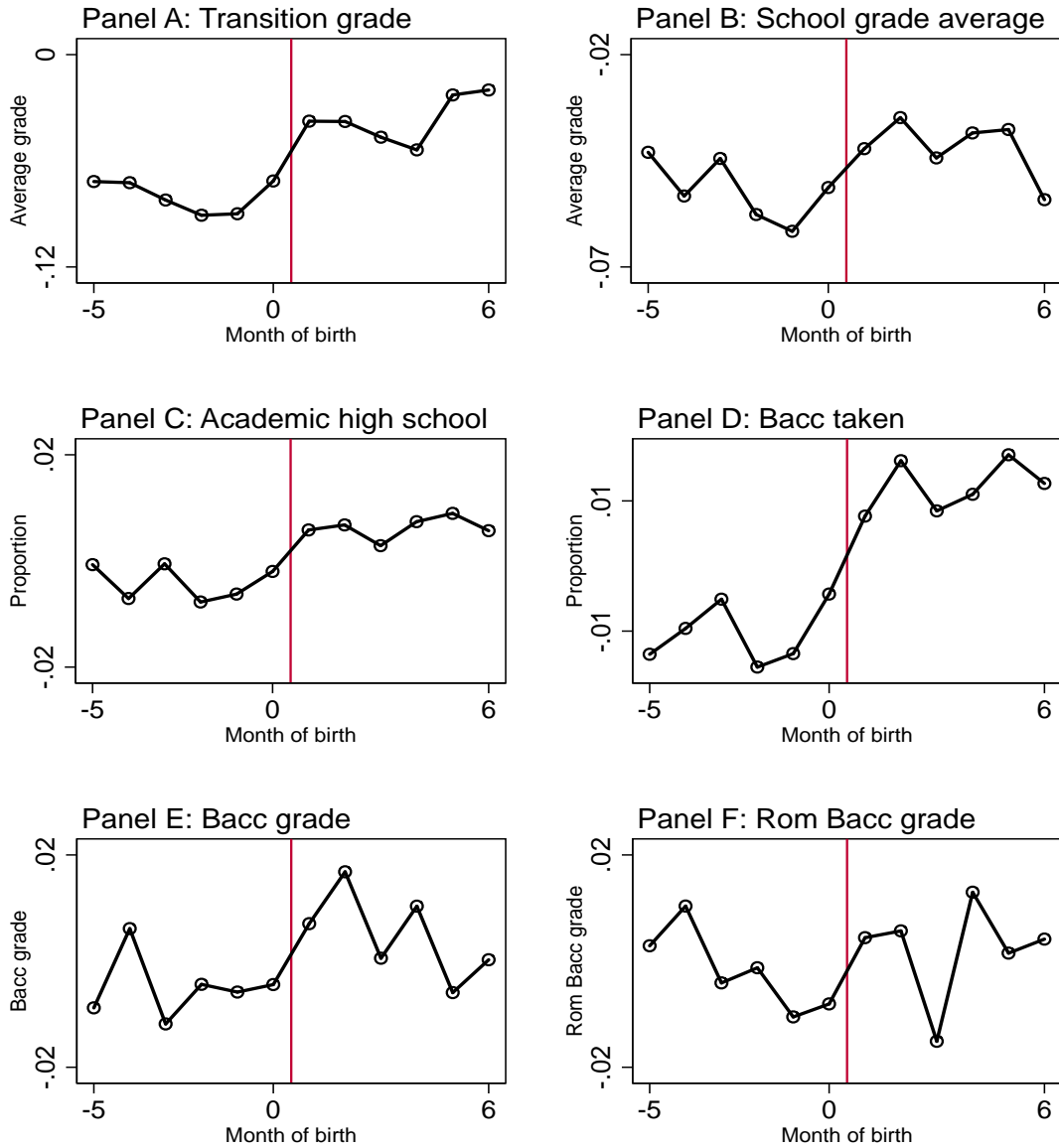
FIGURE 5. Residuals from regressions of outcomes on abortion access

Notes: All panels plot residuals from regressions similar to those in Panel D of Table 2. Specifically, they plot residuals by month of birth for children born in 1990 and present in the 2005 secondary school cohort, where the children born in 1991 and present in the 2006 cohort are included to account for month of birth effects. The fertility decrease following the introduction of greater access to abortion started with cohorts born after July of 1990 (month 0 in the above figures).

dummies—i.e., equation (5) with cutoff/year fixed effects.[50] The results suggest that scoring above a cutoff results in a highly statistically significant increase in peer quality—0.1 points, equivalent to about 0.1 standard deviations in the transition score distribution.

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[50] 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.
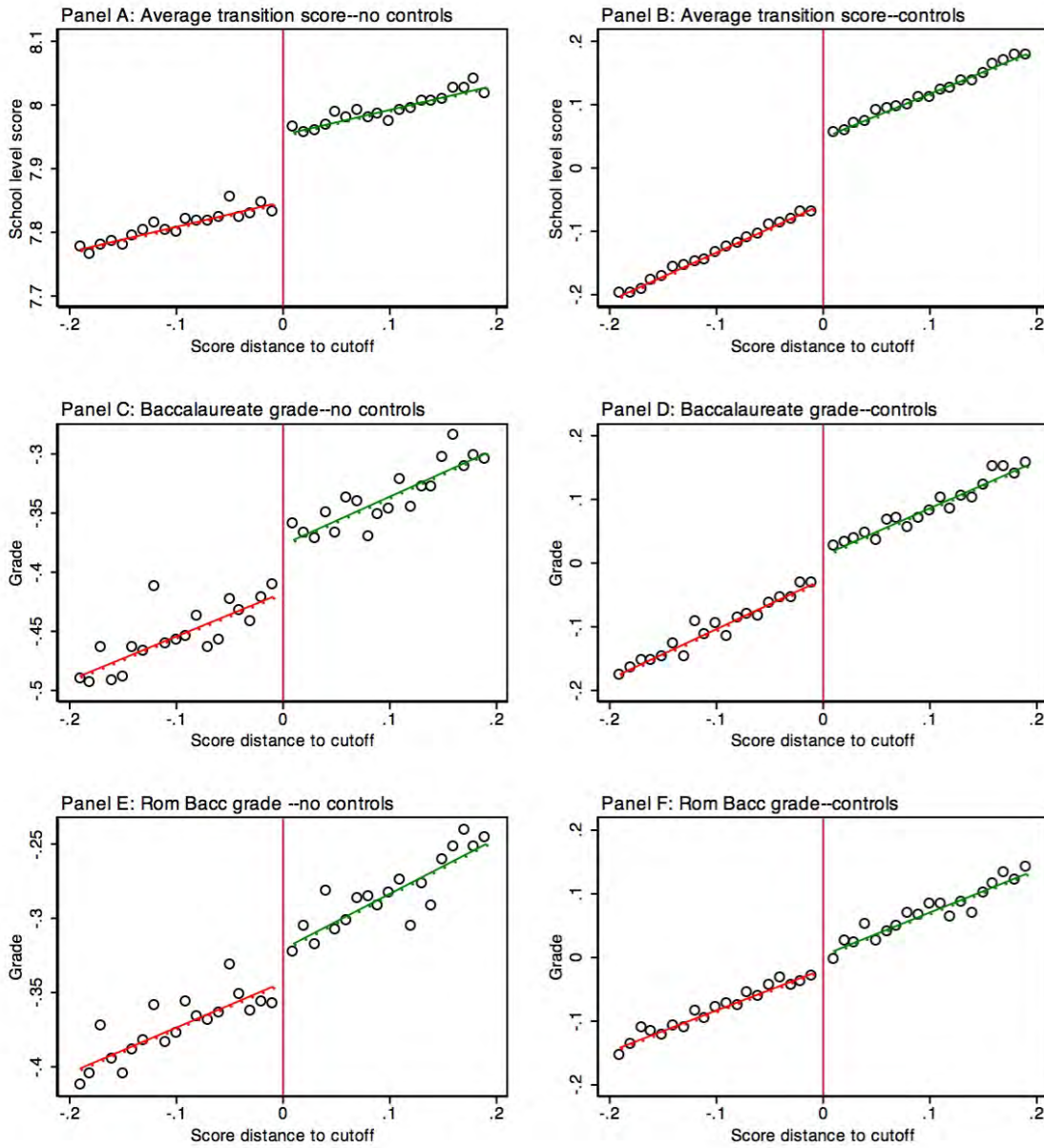
FIGURE 6. The effect of access to a better school on educational outcomes

Notes: All panels are based on administrative data for the 2005-2006 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 the Baccalaureate exam grade; the dependent variable in panels E and F is the Romanian Baccalaureate exam grade.

TABLE 3. The effect of access to a better school on educational outcomes

Dependent variable:	School-level cutoffs		Track-level cutoffs	
	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Average transition score of peers				
Access to a better school (*AccessB*)	0.106***	0.107***	0.0502***	0.0498***
	[0.00173]	[0.00181]	[0.00139]	[0.00137]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	562,609	508,899	894,685	919,759
Panel B: Bacc. taken				
Access to a better school (*AccessB*)	0.006	0.006	-0.0107***	-0.00529*
	[0.00446]	[0.00491]	[0.00250]	[0.00296]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	778,491	658,882	2,508,244	1,641,200
Panel C: Bacc. grade				
Access to a better school (*AccessB*)	0.0290***	0.0306***	0.0165***	0.0165***
	[0.00507]	[0.00543]	[0.00337]	[0.00333]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	315,590	271,071	689,545	708,166
Panel D: Romanian Bacc. grade				
Access to a better school (*AccessB*)	0.006	0.00855*	0.00775**	0.00641**
	[0.00457]	[0.00472]	[0.00339]	[0.00309]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	453,822	432,076	810,102	995,119

Notes: The regressions implement specification (5). They allow for a linear spline in the running variable with a slope that can vary on each side of the cutoff. They are also clustered at the student level and include cutoff/year fixed effects. Standard errors are in brackets and 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, giving him or her access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All these samples result in similar and highly significant estimates. The "first stages" in Panel A of Table 3 are those that will be relevant for the Baccalaureate outcomes. They show that the Romanian high school admissions process makes feasible an RD-based analysis of the impact of access to a better school.[51]

We also consider how access to a better school affects whether students take the Baccalaureate exam. Panel B of Table 3 implies that having access to a better school essentially

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[51] 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 twenty 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.

does not affect the probability of taking the exam; we can rule out differences in test-taking rates of less than one-third of a percentage point. The absence of selection into taking the Baccalaureate test makes it easier to interpret the effects on Baccalaureate performance.

Turning to this, panels C and D in Figure 6 present the impact of access to a better high school on average scores, showing a discontinuous change in achievement at the cutoff. The corresponding regression results in Panel C of Table 3 indicate statistically significant gains equivalent to about 0.03 standard deviations in the overall score. Panel D shows that the impact on Romanian scores is also positive and significant when estimated using the CCT bandwidths, although insignificant in the IK specification (the corresponding graphs are in panels E and F of Figure 6).

The bottom line is that students who score above cutoffs giving them access to better schools perform better in the high-stakes Baccalaureate exam. Columns (3) and (4) in Table 3 further confirm that these conclusions hold when one considers between-track rather than between-school cutoffs. This significantly increases the number of RD-based quasi-experiments and sample sizes, and in this case all estimates of the coefficient of interest are statistically significant.

5.3. **Interactions between family and school environments.** We now turn to the combined RD-DD specification to examine whether access to a better school is associated with a larger or smaller effect among children who grew up in different family environments as induced by changes in access to abortion. As previewed in Section 5, the intuition surrounding our strategy, as well a useful preview of the results, can be conveyed by estimating our main effects of access to a better school (Table 3) for four groups of children: (1) those born during the restrictive abortion regime between January 1 and June 30 of 1990, (2) those born immediately after access to abortion increased: July 1 to December 1990, (3) those born between January 1 and June 30 of 1991, and (4) those born between July 1 and December of 1991. The children born in the first and second half of 1991 allow us to control for potential seasonality that could arise when comparing the outcomes of children born in the first and second half of 1990.

The first two columns of Table 4 show the impact of access to a better school on the overall Baccalaureate grade for each of these four distinct groups (panels A, B, C, and D). The results in column (2) are striking. First, they show consistent positive and in three specifications statistically significant effects of having access to a better school. Second, the largest effects are observed for the group born before July of 1990; in other words the later intervention—access to a better school—seems to have had the largest effect upon the children who did not benefit from parental access to abortion and therefore had lower

TABLE 4. The effect of access to a better school for different birth cohorts

Dependent variable:	School-level cutoffs		Track-level cutoffs	
	Within IK bound	Within CCT bound	Within IK bound	Within CCT bound
	(1)	(2)	(3)	(4)
Panel A: 1990 cohort, months 1-6				
Access to a better school (*AccessB*)	0.0300***	0.0324***	0.0240***	0.0315***
	[0.00825]	[0.00844]	[0.00537]	[0.00614]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	135,987	124,617	283,600	208,314
Panel B: 1991 cohort, months 1-6				
Access to a better school (*AccessB*)	0.0272***	0.0293***	0.008	0.0110*
	[0.00785]	[0.0103]	[0.00636]	[0.00626]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	110,604	73,194	200,280	209,214
Panel C: 1990 cohort, months 7-12				
Access to a better school (*AccessB*)	0.014	0.011	0.0123**	0.0102*
	[0.00955]	[0.0101]	[0.00615]	[0.00560]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	99,585	84,304	214,135	242,768
Panel D: 1991 cohort, months 7-12				
Access to a better school (*AccessB*)	0.0273***	0.0311***	0.005	0.007
	[0.00801]	[0.0109]	[0.00641]	[0.00656]
Linear spline in score; cutoff/year dummies	Yes	Yes	Yes	Yes
N	106,086	67,228	187,037	175,277

Notes: The regressions implement the specification in Table 3. Panels A-D break the sample into four subgroups covering four six-month periods in the two birth years that make up the full sample. Specifically, Panel A refers to children born between January and June (inclusive) of 1990, and Panel B covers the same months of 1991. Panel C refers to the children born between July and December (inclusive) of 1990, and panel D covers the same months of 1991. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

achievement upon entering high school. Third, these four coefficients can be used to calculate a difference-in-differences estimate of the effect of having access to a better school among children who were born under increased access to abortion. This is very much in the spirit of equation (7) and of our interacted specification (8). A back of the envelope calculation using the sample with CCT bandwidths implies a difference-in-differences estimate of -0.023.

Columns 3 and 4 present analogous impacts on the overall Baccalaureate grade using the track level rather than the school level analysis. These are even more striking; the effects from having access to a better school are about three times larger for the cohort born during the restrictive abortion regime. Again, if anything, the interaction between access to abortion and access to a better school is negative for this outcome.

TABLE 5. The interaction of access to abortion and to a better school (school level cutoffs)

Bac grade	With dummy for birth in first six months		With linear trend in month of birth	
	Within IK bound	Within CCT bound	Within IK bound	Within CCT bound
	(1)	(2)	(3)	(4)
Panel A: Restricted				
Access to a better school (*AccessB*)	0.0451***	0.0450**	0.0296**	0.0330***
	[0.0171]	[0.0179]	[0.0117]	[0.0121]
Abortion access (*AccessA*)	0.0334**	0.0295*	0.0416***	0.0389***
	[0.0164]	[0.0167]	[0.0146]	[0.0149]
*AccessB*AccessA*	-0.0357*	-0.033	-0.0424**	-0.0440**
	[0.0202]	[0.0210]	[0.0180]	[0.0187]
Triple interactions	N	N	N	N
N	315,590	290,091	315,590	290,091
Panel B: Unrestricted				
Access to a better school (*AccessB*)	0.0434**	0.0441**	0.0310***	0.0346***
	[0.0173]	[0.0180]	[0.0118]	[0.0122]
Abortion access (*AccessA*)	0.012	0.014	0.020	0.025
	[0.0192]	[0.0197]	[0.0172]	[0.0176]
*AccessB*AccessA*	-0.0309	-0.03	-0.0375**	-0.0411**
	[0.0204]	[0.0212]	[0.0182]	[0.0188]
Triple interactions	Y	Y	Y	Y
N	315,590	290,091	315,590	290,091

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We now turn to the fully interacted RD-DD framework (equation 8). In Table 5 our preferred specification (columns 3 and 4) uses linear trends in month of birth. As before, we also consider alternative specifications in columns (1) and (2) where we use an indicator for being born in the first six months of the year. In all columns the outcome of interest is the performance on the Baccalaureate exam. As before, we use two samples: one restricted to individuals within the IK bandwidth (columns 1 and 3), and the preferred CCT bandwidths (columns 2 and 4).

Table 5 thus describes the differential impact associated with access to a better school for children born before and after the access to abortion increased. To begin with, we start with the more restrictive specifications that do not include the triple interactions. They

show a pattern of results that replicates the main effects shown above for each source of variation. Specifically, the coefficients for being above a school cutoff ($AccessB_i$) and being born in a period of access to abortion ($AccessA_i$) are positive and significant in our preferred specification using the CCT bandwidth (column 4) for the Baccalaureate grade. The key result refers to the interaction of access to abortion ($AccessA_i$) and access to a better school ($AccessB_i$). The interaction coefficients in columns 1-4 of Panel A in Table 5 are generally negative and three of the four are statistically significant. We next turn to Panel B, which uses our preferred fully interacted unrestricted specification (equation 8). The results are generally similar to those in more restrictive Panel A, but we note that now only 2 of the 4 interaction coefficients are statistically significant. The bottom line is that, at least in our setting, there is little evidence for a positive interaction between shocks to family and school environments—to the extent a pattern emerges, it is suggestive of substitutability rather than complementarity.

5.4. **Interpretation.** The "reduced-form" interactions between family and school environments estimated in the previous section are interesting in their own right. However, they may reflect the presence or absence of dynamic complementarities in the technology of human capital formation as well as behavioral responses on the part of parents and children (and other agents such as teachers). In Appendix C we present a conceptual framework that incorporates dynamic complementarities and behavioral responses in our setting

Dynamic complementarities imply that human capital investments are more productive when an individual's baseline stock of skills is higher (Cunha and Heckman 2007). Our estimates of null or negative interactions between early shocks to family environments and later shocks to school environments do not appear to support the presence of dynamic complementarities in our setting. However, even if there are dynamic complementarities in the technology of human capital formation, these may be reinforced or undone if parents or teachers respond endogenously to children's prior achievement.

We attempt to explore the possibility of behavioral responses using survey data, focusing on parental and student effort around homework, as well as parental decisions on whether to hire a tutor. Table 6 presents the results, using our preferred "unrestricted" specification from Table 5 with linear trends in month of birth. The interactions between the impact of increased access to abortion ($AccessA_i$) and access to a better school ($AccessB_i$) on whether parents help children with homework are consistently negative, albeit insignificant. This would suggest that children born after there was greater access to abortion receive less parental help with homework when they have access to a better school. Similarly,

we observe negative and occasionally significant interactions on children's reports of doing homework.The effects on whether parents hire a tutor are less conclusive.

Interpreted within the framework of dynamic complementarities, these results suggest that parental and student behavior may partially undo dynamic complementarities between family and school environments, at least in the Romanian setting. We cannot make this statement conclusively due to the lack of statistical power and because we only have a limited set of outcomes. Nevertheless, the pattern does suggest that our reduced-form estimates are not necessarily inconsistent with the existence of dynamic complementarities in the technology of human capital formation.

5.5. **Robustness checks and interpretation issues.** In this final section we address three sets of issues that arise given the sources of variation we use: crowding, composition effects, and possible biases from the distribution of children across cutoffs.

5.5.1. *Crowding.* The first issue is simply that our results might be driven by changes in crowding, since smaller cohorts were born in the months after access to abortion increased. The children born in July, 1990 and later therefore on average encountered less crowding in many settings. For example, in some cases they might have experienced smaller class sizes in elementary school. Any differential performance on their part might therefore reflect that they enjoyed more inputs on average. As stated this issue is mitigated in our setting due to the timing of the collapse of communism. Specifically, the children born just before and just after the decline in fertility—those our DD empirical strategy focuses on—entered school in the same academic year. They thus likely encountered similar crowding conditions.

Nevertheless, it is possible that the larger cohorts born during the period of the abortion ban faced larger crowding in the medical system during pre and postnatal care, and this might have affected their outcomes later in life. While this particular mechanism is strictly speaking not a shock to the family environment, we interpret it broadly to also be an exogenous shock during early childhood. We thus use it in the spirit of understanding possible interactions between early childhood environments and later educational shocks.

5.5.2. *Composition effects.* Our results might be driven by composition effects if the abortion policy led to changes in the socioeconomic characteristics of women who carry pregnancies to term. If this were the only change induced by the abortion policy, our analysis would reduce to exploring the heterogeneity of the effects of school quality by parental characteristics rather than the interaction between family and school environments.

We now consider the evidence for such composition effects. In Appendix Table 12, Panel A presents markers of mothers' socio-economic status that are likely to affect children's

TABLE 6. Behavioral responses

	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Parent helps with homework				
Access to a better school (*AccessB*)	0.0087	0.1491*	0.0353	0.1550*
	[0.045]	[0.080]	[0.047]	[0.080]
Abortion access (*AccessA*)	0.0593	0.0474	0.1072*	0.0621
	[0.044]	[0.071]	[0.065]	[0.097]
AccessB*AccessA	-0.0531	-0.0504	-0.0850	-0.0549
	[0.072]	[0.126]	[0.079]	[0.128]
Monthly trend	Y	Y	Y	Y
Cohort dummy	N	N	N	N
Triple interactions	Y	Y	Y	Y
N	4,702	1,748	4,702	1,748
Panel B: Child does homework				
Access to a better school (*AccessB*)	-0.0088	-0.0621	0.0188	-0.0745
	[0.067]	[0.096]	[0.069]	[0.096]
Abortion access (*AccessA*)	0.0990	0.1370	0.2054**	0.1902
	[0.061]	[0.087]	[0.085]	[0.118]
AccessB*AccessA	-0.1656	-0.1746	-0.2196**	-0.2029
	[0.105]	[0.156]	[0.109]	[0.158]
Monthly trend	Y	Y	Y	Y
Cohort dummy	N	N	N	N
Triple interactions	Y	Y	Y	Y
N	3,873	1,966	3,873	1,966
Panel C: Parent hires tutor				
Access to a better school (*AccessB*)	0.0322	0.1084	0.0211	0.1191
	[0.061]	[0.086]	[0.061]	[0.085]
Abortion access (*AccessA*)	0.0809	0.0299	-0.0694	0.0191
	[0.051]	[0.071]	[0.069]	[0.090]
AccessB*AccessA	-0.0435	-0.0597	0.0336	-0.0561
	[0.093]	[0.132]	[0.094]	[0.131]
Monthly trend	Y	Y	Y	Y
Cohort dummy	N	N	N	N
Triple interactions	Y	Y	Y	Y
N	3,724	1,971	3,724	1,971

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

academic performance; these include mothers' educational attainment and whether they were born in an urban area. The signs of the coefficients on educational attainment (columns 1-3) suggest that the mothers of children born after access to abortion increased were more educated, and they were less likely to have been born in urban areas (column 4). However, none of these coefficients are statistically significant.

Panel B of Appendix Table 12 complements this analysis by considering the effect of abortion access on markers often related to the prevalence of "unwanted" children. For instance, all else equal, women who are divorced as opposed to married, and older as opposed to younger, may wish to have fewer children. Column (1) shows that increased access to abortion led to a 0.7 percent increase in the probability that mothers were married (column 1), although there is little evidence of an impact on the likelihood that they were divorced (column 2). Not surprisingly, column (3) indicates a reduction in the number of children after liberalization. This, along with the large decrease in fertility after July of 1990 (Figure 2), provides the most direct evidence that many children born under the abortion ban were not wanted by their parents. However, the changes in the patterns of age at birth and life-cycle fertility provide additional support for this claim. Column (4) shows that the mothers of children born after access to abortion increased were also younger by approximately 0.25 years. This suggests that on average older women responded more to the increased availability of abortion, presumably because they were more likely to have reached or exceeded their ideal family size under the restrictive regime. This is also consistent with evidence that greater access to abortion led to a decrease in each mother's total fertility. Finally, the results in Panel B of Table 12 are consistent with research by Mitrut and Wolff (2011) who show that, following the lifting of the abortion ban, the number of abandoned children decreased.

Thus, Table 12 suggests that changes in the composition of births—at least along observable characteristics—are unlikely to fully account for the results we found above. We explore this more directly by adding controls to our previous specifications. Appendix Table 13 uses the administrative data and adds a current poverty status indicator.[52] For conciseness, we focus on our preferred specification—the one that includes linear trends in month of birth to control for seasonality. In each case, Panel A simply replicates our main results showing the combined RD-DD specifications that include interactions between access to abortion and access to a better school.

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[52] This measure of poverty is used by schools to determine eligibility for a scholarship program, and has the advantage of allowing us to maintain high sample sizes. However, there may be concern that this variable is endogenous if it is itself affected by access to abortion.

Panel B of Table 13 estimates the main effects and the interaction between poverty status and the effect of having access to a better school, including the controls. In Table 13, the interactions in Panel B show that children who are not poor are significantly more likely to take the Baccalaureate exam and to score higher, but there is no indication that the effect of having access to a better school is different for poor and non-poor students. Finally, Panel C of the same table includes both interactions between access to abortion and to a better school, as well as a further interaction between poverty status and the indicator for having access to a better school. The key interactions between access to abortion and to a better school are not much affected by the inclusion of these poverty controls. This leads us to conclude that composition is not driving our main results and that differential investments in the family environment are likely to be playing a central role.

5.5.3. ***Distribution of children across cutoffs***. If access to abortion affects children's transition scores, as our results suggest, it may also affect their ability to gain admission to better schools. This raises the possibility that children born before and after access to abortion increased may be differentially distributed across the cutoffs that determine access to better schools, and this could be a source of differential improvements in Baccalaureate scores. For example, suppose that children born after the abortion ban was repealed were systematically more likely to end up at cutoffs at which the benefit of going to a better school was smaller; this could explain why we do not find evidence of interactions between family and school environments.

Appendix Figure 10 presents evidence suggesting this is not the case. First, we estimate the interaction between access to abortion ($AccessA_i$) and access to a better school ($AccessB_i$) separately by tercile of the school quality distribution (parametrized by the transition scores of the cutoff for entry to each school). We plot these estimates as vertical bars for each of the four specifications associated with our abortion models.[53] The interaction effects are negative in each tercile and for almost every specification. Thus, it does not appear that our main findings are driven by schools in certain parts of the quality distribution. Second, we estimate the main effects of access to a better school ($AccessB_i$) separately by tercile of the school quality distribution for children who were born in 1990 either before or after access to abortion increased. These are plotted as the dotted and solid lines respectively. The fact that both of these lines slope upwards suggests that the effect of access to a better school is, if anything, increasing in school quality. Thus, it does not appear that our findings can be explained by the fact that children born after access to abortion increased were systematically more likely to end up at cutoffs at which the benefit of going to a better school was smaller.

[53] We use the IK bandwidth to generate these graphs but patterns are similar when using 1 point bandwidth.

TABLE 7. The interaction of access to abortion and to a better school (track level cutoffs)

Bac grade	With dummy for birth in first six months		With linear trend in month of birth	
	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Restricted				
Access to a better school (*AccessB*)	0.018	0.0253**	0.0285***	0.0255***
	[0.0116]	[0.0113]	[0.00773]	[0.00763]
Abortion access (*AccessA*)	0.007	0.011	0.0215*	0.0211*
	[0.0128]	[0.0127]	[0.0112]	[0.0112]
*AccessB*AccessA*	-0.008	-0.014	-0.0242**	-0.0222*
	[0.0136]	[0.0133]	[0.0120]	[0.0118]
Triple interactions	N	N	N	N
N	689,545	726,819	689,545	708,166
Panel B: Unrestricted				
Access to a better school (*AccessB*)	0.018	0.0252**	0.0290***	0.0260***
	[0.0116]	[0.0113]	[0.00775]	[0.00765]
Abortion access (*AccessA*)	0.009	0.009	0.020	0.017
	[0.0145]	[0.0143]	[0.0127]	[0.0126]
*AccessB*AccessA*	-0.00821	-0.0135	-0.0240**	-0.0217*
	[0.0137]	[0.0133]	[0.0120]	[0.0119]
Triple interactions	Y	Y	Y	Y
N	689,545	726,819	689,545	708,166

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.5.4. ***Robustness and results from the reweighting estimator***. We include three further tables with robustness checks using the fully interacted RD-DD framework. In Table 7, we first repeat the analysis of Table 5 using track rather than school cutoffs. Secondly, in appendix tables 10 and 11 we repeat the analysis in Tables 5 and 7 using the score on the Romanian language component of the Baccalaureate exam.

Finally, in Table 8 we implement the re-weighing approach described in Section 4.3.2. The table shows that our main finding from estimating equation (8) is unchanged when we use the re-weighting procedure described in Appendix Proposition 3 to estimate a mean interaction parameter $\Delta_0 = E[Y_i(1, Z_i) - Y_i(1, Z_i - 1) - Y_i(0, Z_i) + Y_i(0, Z_i - 1)|D_i = 0]$. Estimates are insignificant whether using IK or CCT computation of the RDD bandwidth.

TABLE 8. Results of the reweighting estimator

Bac grade	Within IK bound (1)		Within CCT bound (2)	
	Unweighted	Reweighted	Unweighted	Reweighted
Mean interaction effect (Δ_0)	-0.031 [0.021]	-0.017 [0.097]	-0.030 [0.023]	-0.010 [0.113]
Number of clusters	83,425	85,093	81,653	78,828
Numbers of observations	315,590	348,729	296,549	336,697

Notes: Columns labeled "Reweighted" report the results of the stacking-reweighting approach described in Appendix Proposition 3, which establishes identification of the mean interaction effect parameter $\Delta_0 = E[Y_i(1, Z_i) - Y_i(1, Z_i - 1) - Y_i(0, Z_i) + Y_i(0, Z_i - 1)|D_i = 0]$. Columns labeled "Unweighted" report the coefficient on $AccessA \cdot AccessB$ from (8) without reweighting. All standard errors in this table use nonparametric bootstrap (with 200 bootstrap draws) clustered at the student level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Conclusion

Interactions between family and school environments are of substantial interest, not least because they have major policy implications. If there is complementarity between these environments, then any efficiency-equity tradeoffs raised by some interventions might be significantly mitigated. For example, early childhood home-based interventions aimed at under-privileged children might raise their achievement even as they enhance the effectiveness of subsequent school-related investments.

This paper estimates the interaction between family and school environments by using exogenous variation in the access to abortion and to selective schools in Romania. We thereby address Almond and Mazumder's (2013) observation that obtaining credible inferences on such interactions requires identifying two arguably exogenous shocks to investments affecting the same cohort. We also provide the necessary identification assumptions required for our empirical approach.

Our administrative data suggest that both access to abortion and access to selective schools have significant positive impacts on individuals' educational outcomes, but provide little indication of significant positive *interactions* between them—to the extent a pattern emerges, it is suggestive of substitutability rather than complementarity. This leaves open the possibility that, at least in this particular context, later schooling interventions may deliver significant benefits even when they are targeted at more disadvantaged children.

We also note that such reduced form results may be necessary but not sufficient to isolate complementarities in the human capital production function. In particular, behavioral

responses on the part of students, parents, and other actors may reinforce or undo such interactions. Our survey data, despite much smaller sample sizes, provide suggestive evidence of such responses in terms of parent and student effort. In short, we cannot rule out that *ceteris paribus* dynamic complementarities in the sense of Cunha and Heckman (2007) exist in our setting, but are undone by individuals' behavior.

There are a number of directions for future work. We have obviously presented evidence in a single setting; needless to say, Romania has a distinct set of characteristics and our findings may not necessarily generalize to other countries. In addition, we have focused on only one type of interaction, that between family and school environments. One possibility is that results may differ when one considers repeated shocks within a single environment. In addition, in terms of behavioral responses in the school setting, we have data on only children and parents; information on teachers would be of interest given their much larger role in schools. Further, our evidence pertains to shocks that are chronologically far apart—for instance, our sources of variation may have affected investments in early childhood and in high school, thus separated by several years. A question is whether the results might be different when potential interactions are more immediate.

Our results also have implications for future work that may try to address issues related to dynamic complementarities. The challenge of finding multiple sources of variation and sufficient data argues for the use of experimental settings where researchers can manipulate interventions and collect data relatively quickly. Our results suggest the need to measure and understand the behavioral responses that result from these interventions. At the same time, to the extent that experiments hold factors including behavioral responses constant, they may misrepresent even the direction of the net impacts that would emerge if interventions were taken to scale (Todd and Wolpin, 2003, and Pop-Eleches and Urquiola, 2013).

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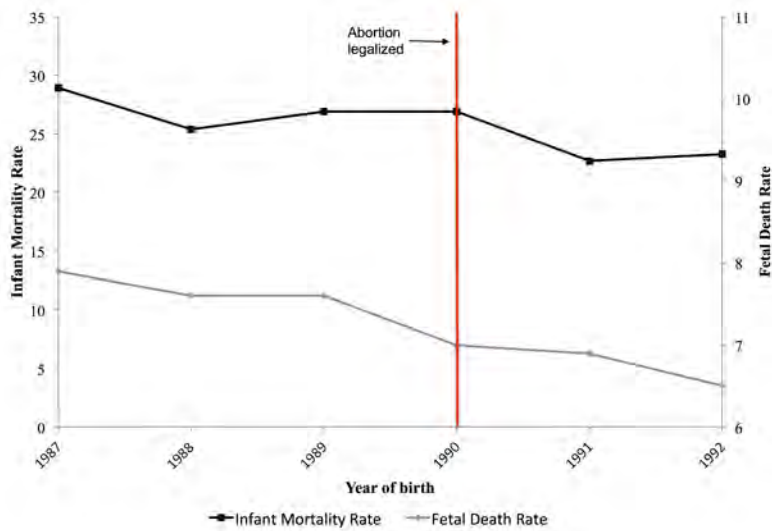(The following appendix materials are not for publication)

FIGURE 7. Proportion of births by month in admissions cohorts

Notes: The infant mortality rate and the fetal death rate are from the Romanian Demographic Yearbook (1996).
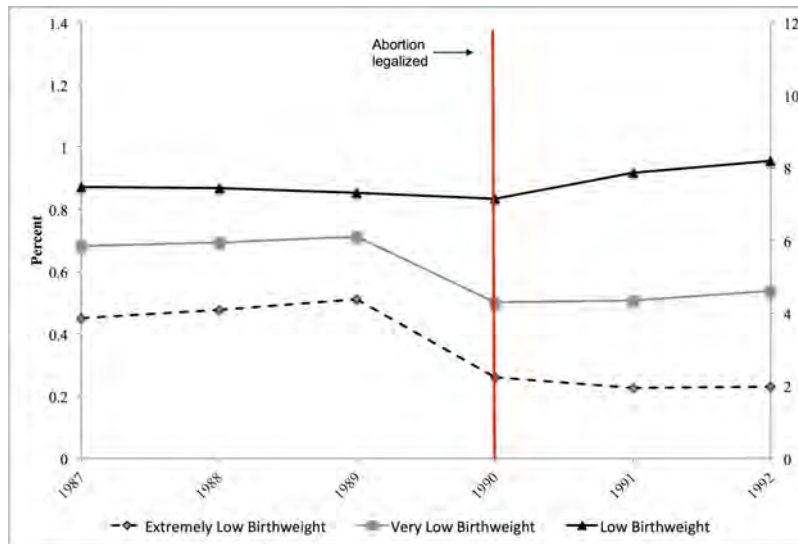

FIGURE 8. Proportion of births by month in admissions cohorts

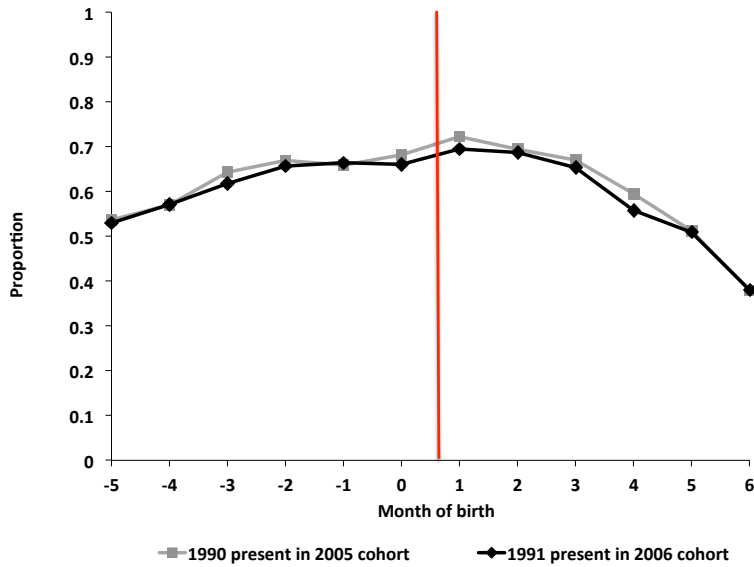*Notes*: All data are from the Romanian Demographic Yearbook (1996).

FIGURE 9. Proportion of births by month in admissions cohorts

Notes: This figure uses 1992 census data to plot the proportion of children born in each month. The first group is children born in 1990 present in the 2005 high school admission cohort. The second group is children born in 1991 present in the 2006 admission cohort.
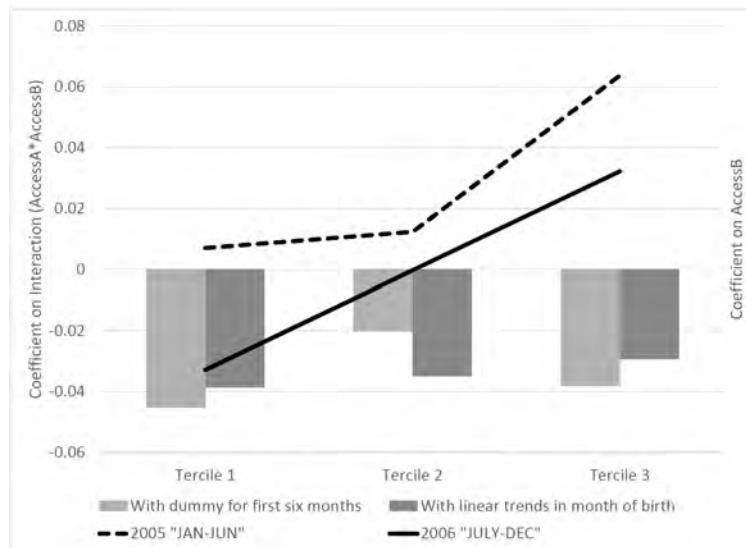

FIGURE 10. Distribution across cutoffs

Notes: This figure plots the interaction effects between access to abortion ($AccessA_i$) and access to a better school ($AccessB_i$) by tercile of the school quality distribution (parametrized by the transition scores of the cutoff for entry to each school) as vertical bars for each of the four specifications associated with our abortion models. It also plots the main effects of access to a better school ($AccessB_i$) by tercile of the school quality distribution for children who were born in 1990 before and after access to abortion as the dotted and solid lines respectively.

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TABLE 9. Descriptive statistics: All towns and survey towns

| | High school admission cohort | | | | | |
| | 2005 | | | 2006 | | |
	Mean	S.D.	N	Mean	S.D.	N
Panel A: All towns:						
Panel A.1: Individual level						
Transition score	8.14	0.87	105,737	8.26	0.87	92,772
Baccalaureate taken	0.83	0.38	105,737	0.85	0.36	92,772
Baccalaureate grade	8.73	0.78	79,873	8.02	1.08	69,945
Romanian Bacc. grade	7.48	1.59	87,383	7.80	1.33	78,243
Panel A.2: Track level						
Number of 9th grade students	53.9	40.4	1,963	52.4	37.0	1,771
Panel A.3: School level						
Number of 9th grade students	129.4	70.6	817	118.5	63.4	783
Number of tracks	2.4	1.1	817	2.3	1.1	783
Panel A.4: Town level						
Number of 9th grade students	766.2	839.8	138	708.2	757.3	131
Number of schools	5.9	6.0	138	6.0	6.2	131
Number of tracks	14.2	12.5	138	13.5	12.0	131
Panel B: Survey towns:						
Panel B.1: Individual level						
Transition score	8.03	0.82	15,177	8.22	0.81	13,685
Baccalaureate taken	0.83	0.37	15,177	0.85	0.36	13,685
Baccalaureate grade	8.80	0.72	11,914	8.06	0.98	10,860
Romanian Bacc. grade	7.61	1.52	12,623	7.79	1.25	11,539
Panel B.2: Track level						
Number of 9th grade students	40.2	26.1	378	40.1	23.7	341
Panel B.3: School level						
Number of 9th grade students	115.0	67.1	132	109.5	62.3	125
Number of tracks	2.9	1.1	132	2.7	1.1	125
Panel B.4: Town level						
Number of 9th grade students	257.2	133.6	59	244.4	128.7	56
Number of schools	2.2	0.4	59	2.2	0.4	56
Number of tracks	6.4	2.1	59	6.1	2.1	56

Notes: This table uses the administrative data to describe two samples. Panel A describes the universe of Romanian towns with two exceptions: i) towns that make up Bucharest, and ii) towns that contain a single school. Panels A.1, A.2, A.3, and A.4 refer to characteristics at the student, track, school, and town level, respectively. Panel B presents analogous information for the towns we targeted for surveying.

TABLE 10. Interaction effects using between school cutoffs

Romanian Bac grade	With dummy for birth in first six months		With linear trend in month of birth	
	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Restricted				
Access to a better school (*AccessB*)	0.019	0.014	0.016	0.0210*
	[0.0153]	[0.0160]	[0.0106]	[0.0121]
Abortion access (*AccessA*)	-0.001	-0.003	0.005	0.013
	[0.0158]	[0.0160]	[0.0139]	[0.0148]
*AccessB*AccessA*	-0.030	-0.022	-0.0339**	-0.0403**
	[0.0182]	[0.0190]	[0.0164]	[0.0187]
Triple interactions	N	N	N	N
N	453,822	424,644	453,822	337,161
Panel B: Unrestricted				
Access to a better school (*AccessB*)	0.021	0.016	0.018	0.0228*
	[0.0154]	[0.0162]	[0.0107]	[0.0121]
Abortion access (*AccessA*)	0.013	0.005	0.021	0.017
	[0.0181]	[0.0184]	[0.0161]	[0.0175]
*AccessB*AccessA*	-0.0333*	-0.0237	-0.0376**	-0.0410**
	[0.0184]	[0.0192]	[0.0166]	[0.0188]
Triple interactions	Y	Y	Y	Y
N	453,822	424,644	453,822	337,161

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 11. Interaction effects using between track cutoffs

Romanian Bac grade	With dummy for birth in first six months		With linear trend in month of birth	
	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Restricted				
Access to a better school ($AccessB$)	0.013	0.010	0.0190**	0.0192**
	[0.0116]	[0.0105]	[0.00786]	[0.00795]
Abortion access ($AccessA$)	-0.006	-0.009	0.005	0.004
	[0.0127]	[0.0123]	[0.0112]	[0.0113]
$AccessB*AccessA$	-0.008	-0.005	-0.0215*	-0.019
	[0.0137]	[0.0124]	[0.0121]	[0.0122]
Triple interactions	N	N	N	N
N	810,102	995,119	810,102	789,350
Panel B: Unrestricted				
Access to a better school ($AccessB$)	0.013	0.009	0.0185**	0.0189**
	[0.0116]	[0.0105]	[0.00788]	[0.00797]
Abortion access ($AccessA$)	-0.009	-0.008	0.004	0.002
	[0.0144]	[0.0138]	[0.0128]	[0.0129]
$AccessB*AccessA$	-0.008	-0.005	-0.0214*	-0.019
	[0.0137]	[0.0125]	[0.0122]	[0.0123]
Triple interactions	Y	Y	Y	Y
N	810,102	995,119	810,102	789,350

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 12. The effect of access to abortion on mothers' characteristics

Panel A: Markers of mothers' socioeconomic status

Dependent variable:	Primary education	Secondary education	Higher education	Urban region of birth
	(1)	(2)	(3)	(4)
Access to abortion (*AccessA*)	-0.006	0.002	0.004	-0.007
	[0.004]	[0.004]	[0.002]	[0.005]
Monthly trend	Y	Y	Y	Y
N	86,408	86,408	86,408	86,755

Panel B: Markers of unwantedness

Dependent variable:	Married	Divorced	No. of children	Age
	(1)	(2)	(3)	(4)
Access to abortion (*AccessA*)	0.007***	-0.001	-0.154***	-0.279***
	[0.003]	[0.001]	[0.024]	[0.076]
Monthly trend	Y	Y	Y	Y
N	86,758	86,758	86,774	86,774

Notes: These regressions estimate specification (5) with maternal characteristics as outcome variables. Standard errors are in brackets and are clustered by age in months. The abortion access dummy (*AccessA*) equals 1 for mothers who gave birth on or after July 1, 1990, and equals 0 for mothers who gave birth on or before June 30, 1990. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 13. Interactions controlling for non-poor status

Bac grade	With dummy for birth in first six months		With linear trend in month of birth	
	Within IK bound (1)	Within CCT bound (2)	Within IK bound (3)	Within CCT bound (4)
Panel A: Unrestricted				
Access to a better school (*AccessB*)	0.0434**	0.0441**	0.0310***	0.0346***
	[0.0173]	[0.0180]	[0.0118]	[0.0122]
Abortion access (*AccessA*)	0.012	0.014	0.020	0.025
	[0.0192]	[0.0197]	[0.0172]	[0.0176]
*AccessB*AccessA*	-0.0309	-0.03	-0.0375**	-0.0411**
	[0.0204]	[0.0212]	[0.0182]	[0.0188]
Triple interactions	Y	Y	Y	Y
N	315,590	290,091	315,590	290,091
Panel B: Unrestricted				
Access to a better school (*AccessB*)	0.029	0.0323*	0.0391*	0.0391*
	[0.0189]	[0.0189]	[0.0205]	[0.0205]
Nonpoor dummy	0.139***	0.142***	0.139***	0.139***
	[0.0165]	[0.0166]	[0.0165]	[0.0165]
*AccessB*Nonpoor	-0.00939	-0.0125	-0.00923	-0.00923
	[0.0183]	[0.0184]	[0.0183]	[0.0183]
Monthly trend	N	N	Y	Y
Cohort dummy	Y	Y	N	N
Triple interactions	Y	Y	Y	Y
N	315,590	309,300	315,590	315,590
Panel C: Unrestricted				
Access to a better school (*AccessB*)	0.0507**	0.0507**	0.0390*	0.0390*
	[0.0240]	[0.0240]	[0.0205]	[0.0205]
Abortion access (*AccessA*)	0.012	0.012	0.019	0.019
	[0.0192]	[0.0192]	[0.0172]	[0.0172]
*AccessB*AccessA*	-0.030	-0.030	-0.0367**	-0.0367**
	[0.0203]	[0.0203]	[0.0181]	[0.0181]
Nonpoor dummy	0.139***	0.139***	0.139***	0.139***
	[0.0165]	[0.0165]	[0.0165]	[0.0165]
*AccessB*Nonpoor	-0.00924	-0.00924	-0.0091	-0.0091
	[0.0183]	[0.0183]	[0.0183]	[0.0183]
Triple interactions	Y	Y	Y	Y
N	315,590	315,590	315,590	315,590

Notes: These regressions implement specification (8). They are clustered at the student level and include cutoff fixed effects, where the cutoffs are those between schools. Standard errors are in brackets. All panels present reduced form specifications where the key independent variable is a dummy for the interaction of access to abortion and access to a better school. Columns (1) and (3) restrict the sample to observations within the Imbens and Kalyanaraman (2012) bounds, and columns (2) and (4) to those within the Calonico et al. (2014) bounds. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.1. **Notation.** Let $D_i \in \{0,1\}$ indicate whether student i was exposed to the abortion reform, i.e. born on or after July 1st, 1990. Write $D_i = 1(cohort_i = 1 \text{ or } after_i = 1)$, where $after_i$ indicates that i was born in the second half of their birth year (January-June), and $cohort_i$ indicates that i was born in 2006 cohort (birth year 1991) rather than 2005 (birth year 1990). We will often abbreviate these random variables as a_i and c_i, respectively. The variable D_i is referred to as $AccessA_i$ in the main text, but we use D_i here for brevity. Let P_i be i's town, and we'll refer to a student's town/year pair (P_i, c_i) as their "market". A single market allocates students from a given cohort across the schools in a given town.

Each town p contains a set of $Z_p + 1$ schools $z \in \{0, 1, \ldots Z_p\}$, which we assume is stable over the years 2005 and 2006, and we index transition score thresholds between schools as $t_{pc1} \ldots t_{pcZ_p}$, where t_{pcz} is the threshold between schools $z - 1$ and z in market (p, c). In our notation, we will take the ordinal ranking of schools by transition score threshold to be the same across the two years, so that within a given town p e.g. $z = 4$ has the same meaning in 2005 as it does 2006. Let $Z_i(x) = \max\{z : x \geq t_{P_i, cohort_i, z}\}$ be the "best" school to which i is admitted as a function of transition score x, where the schools are ordered in increasing order of t_{pcz} within a market (p, c). Let X_i denote student i's transition score. Then $i's$ actual "assigned" school (denoted $school_i$ in the main text) is $Z_i = Z_i(X_i)$.

For a generic outcome variable Y, let $Y_i(d, z)$ indicate potential outcomes as a function of access to abortion d and high school assignment z. Under an assumption that students attend the most selective school to which they are admitted, the z appearing in $Y_i(d, z)$ denotes the school that i actually attends. However, we focus on identifying intent-to-treat effects, without such an assumption. Note that since transition scores are affected by abortion access D_i, realized transition scores can be written $X_i = X_i(D_i)$, where $X_i(0)$ and $X_i(1)$ denote counterfactual transition scores depending on abortion access.

6.2. **Identification.** To combine the DD and RDD sources of identification, we need to exploit variation in transition scores jointly with variation in groups that determine eligibility for the abortion reform. This requires making assumptions about the distribution of potential outcomes conditional on *both* types of variables.

Let Y_i indicate an outcome observed after high-school. The following continuity assumption on potential outcomes of Y lead to RDD identification:

Assumption 1 (continuity). $E[Y_i(d, z)|P_i = p, after_i = a, cohort_i = c, X_i(d) = x]$ *is a continuous function of counterfactual transition score x, for any school z and for either counterfactual value of abortion access $d \in \{0, 1\}$, as well as $b, c \in \{0, 1\}$ and market p.*

Let $\eta_z(a, c, p)$ denote the observable discontinuity

$$\lim_{x \downarrow t_{pcz}} E[Y_i|P_i = p, after_i = a, cohort_i = c, X_i = x] - \lim_{x \uparrow t_{pcz}} E[Y_i|P_i = p, after_i = a, cohort_i = c, X_i = x]$$

in the conditional expectation $E[Y_i|P_i = p, a_i = a, c_i = c, X_i = x]$ at $x = t_{pcz}$. Let $d_{ac} := 1 - (1 - b)(1 - c)$ be the abortion treatment value for group a, c. Under Assumption 1:

$$(9) \quad \eta_z(a, c, p) = E[Y_i(d_{ac}, z) - Y_i(d_{ac}, z - 1)|P_i = p, after_i = a, cohort_i = c, X_i(d_{ac}) = t_{pcz}]$$

identifying a local average treatment effect of moving from school $z - 1$ to school z, among students with scores around t_{pcz} in market p and in abortion-reform group a, c.

Now let us turn to the DD source of identification. We make the following parallel trends assumption:

Assumption 2 (parallel trends). *For any schools z and z' in town p:*

$$E[Y_i(1, z')|p, 1, 1, t_{p0z}] - E[Y_i(1, z')|p, 0, 1, t_{p0z}] = E[Y_i(1, z')|p, 1, 0, t_{p0z}] - E[Y_i(1, z')|p, 0, 0, t_{p0z}]$$

with the notation $E[Y_i(1, z')|p, a, c, x]$ for $E[Y_i(0, z')|P_i = p, a_i = a, c_i = c, X_i(1) = x]$.

Assumption 2 says that among students who would be just admitted to school z in their cohort given treatment $(X_i(1) = t_{pC_iz})$, the difference in mean abortion-treated outcomes $Y_i(1, z')$ at school z' between students born in the second half of their birth year and students born in the first half of their birth year is stable between the two cohorts. Given that we will combine with the RDD variation, we only actually need Assumption 2 to hold for $z' = z$ and $z' = z - 1$, but we state the assumption generally here for ease of notation.

While the canonical two-group, two-period difference-in-differences setup considers a treatment that "turns on" in a later period for one group (while remaining off for the other), ours is a setup in which treatment turns on for one group in a "later" period, and is always on for the second group (in our setting the group for whom treatment changes are students in the 2005 cohort, and "later" refers to students born in the months July-December). Thus whereas parallel trends assumptions are typically phrased as an assumption about differences in *untreated* outcomes, ours concerns differences in *treated* outcomes between cohorts.[54]

Accordingly, Assumption 2 also conditions on a student's treated transition score $X_i(1)$, rather than their untreated transition score $X_i(0)$ or their realized transition score X_i. The potential outcome $X_i(1)$ is a baseline characteristic of students that is not itself affected by the abortion treatment (see Caetano et al. 2022 for a similar parallel-trends assumption in DD models with time-varying covariates). We evaluate $X_i(1)$ at the cohort-specific thresholds t_{pcz} to allow for changes in transition scores across years, which could change the composition of students with an $X_i(1)$ equal to any particular value x in a given cohort.

The important implication of Assumption 2 is that it allows us to impute the mean abortion-treated outcome among the students that are *not* exposed to the abortion-reform, which is a counterfactual quantity. Combining this observation with Assumption 1 leads to our central identification result that combines RDD and DD variation:

Proposition 1. *Under Assumption 1 and 2, for each school z in town p:*

$$E[Y_i(1, z) - Y_i(1, z - 1)|P_i = p, D_i = 0, X_i(1) = t_{p0z}] = \eta_z(1, 0, p) + \eta_z(0, 1, p) - \eta_z(1, 1, p)$$

and

$$E[Y_i(0, z) - Y_i(0, z - 1)|P_i = p, D_i = 0, X_i(0) = t_{p0z}] = \eta_z(0, 0, p)$$

Proof. See Appendix B. □

Both of the LHS quantities are thus identified, since $\eta_z(a, c, p)$ is identified. Notice that the average school-effect we can identify from $\eta_z(0, 0, p)$ conditions on the event $X_i(0) = t_{p0z}$ while the effect we can identify from $\eta_z(1, 0, p) + \eta_z(0, 1, p) - \eta_z(1, 1, p)$ conditions on $X_i(1) = t_{p0z}$. This is a form of the "bad-control" problem that arises because our RDDs condition on a variable affected by the abortion reform.

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[54] There is no fundamental conceptual difference: our setup is equivalent to the canonical one if one defines "treatment" to be a lack of access to abortion, and one swaps the labels of the before and after periods.

However, an apples-to-apples comparison can be constructed by averaging the two quantities identified in Proposition 1 over the distributions of $X_i(1)$ and $X_i(0)$, respectively. This is only possible under homogeneity assumptions or if there is sufficient variation in school cutoffs t_{p0z}, since we can only identify $E[Y_i(d, z) - Y_i(d, z-1)|P_i = p, D_i = 0, X_i(d) = x]$ for $d \in \{0, 1\}$ at the school cutoffs $x = t_{p0z}$. Similar to Bertanha (2020), we go the latter route and approximate these cutoffs as "dense" in the support $\mathcal{X}$ of X_i, i.e.

Assumption 3 (density of schools). *Fix any p and $x \in \mathcal{X}$. Then in any neighborhood of x there exists a school cutoff t_{p0z}.*

Assumption 3 is best seen as an approximation: in reality, there exists a z_{xcp} with a transition score cutoff that is very close to any given x in cohort/town cp. Identification arguments will integrate over $\eta_{z_{xcp}}(a, c, p)$, as if there were school with a cutoff exactly at x. This is justified under asymptotics in which we imagine the number of schools growing to infinity along with our sample size, and assuming Riemann integrability of the function $\eta_{z_{xcp}}(a, c, p)$ (see Bertanha 2020 for details).

Secondly, our approach requires imputing the distribution of $X_i(1)|D_i = 0, P_i = p$, which is a counterfactual quantity. To identify it from the data, we impose a parallel trends assumption for treated transition scores:

Assumption 4 (distributional parallel trends for the transition score). *For all x and p:*
$$P(X_i(1) \leq x|D_i = 0, P_i = p) = F_{10p}(x) + F_{01p}(x) - F_{11p}(x)$$
where we let $F_{acp}(x) := P(X_i \leq s|a_i = a, c_i = c, P_i = p)$ denote the group-specific CDF of

observed transition score X_i. While Assumption 4 may appear stronger than conventional mean parallel trends (since it must hold for each value of x and not just on average), Roth and Rambachan (2022) show that distributional parallel trends holds if and only if mean parallel trends is robust to monotonic transformations of the outcome variable (in this case, the transition score).

We require one final assumption:

Assumption 5 (no indirect effects). *For each x and p, $E[Y_i(1, Z_i(X_i(d))) - Y_i(1, Z_i(X_i(d)) - 1)|P_i = p, D_i = 0, X_i(1) = x]$ does not depend on d.*

where recall that $Z_i(x) = \max\{z : x \geq t_{P_i, cohort_i, z}\}$ is the school that i would be assigned to with a transition score of x.

Assumption 5 says that the abortion reform d does not have indirect effects (on average) on the size of next-best-school treatment effects via school assignment $Z_i(X_i(d))$. There are two simple sufficient conditions under which this will hold:

(1) if $Z_i(X_i(d))$ does not depend on d, either because $X_i(1) = X_i(0)$ or because changes to transition score caused by the abortion reform do not push any students across a school threshold,

(2) "linearity" in average school-assignment outcomes: that is $E[Y_i(1, z) - Y_i(1, z-1)|P_i = p, D_i = 0, X_i(1) = x]$ does not depend on z (on average).

The first item above is quite plausible as an approximation, because the average effect $X_i(1) - X_i(0)$ of abortion access on transition scores is quite small in comparison with the typical distance between subsequent school thresholds. The second item would hold in a

model in which $Y_i(1, z)$ is linear in a "dose" of school quality for school z (as in Bertanha 2020) and differences in school quality for adjacent schools is roughly constant across the transition score distribution (within a town/cohort). In Section 5.5.3, we have described evidence that if anything, $Y_i(1, z)$ appears to be convex in school index z, meaning that departures from item 2 above would bias our estimates in the direction of finding positive interaction effects.

Now we can state our identification result for mean interaction effects in each town. Define

$$\Delta_p := E\left\{Y_i(1, Z_i) - Y_i(1, Z_i - 1)\right\} - \left\{Y_i(0, Z_i) - Y_i(0, Z_i - 1)\right\} | P_i = p, D_i = 0]$$

For any x, c and p, let z_{xcp} denote the largest t_{cpz} cutoff smaller than x. Note that in this notation, realized treatment assignment is $Z_i = z_{X_i, C_i, P_i}$.

Proposition 2. *Given the assumptions of Proposition 1, as well as Assumptions 3 and 5: Δ_p is identified as*

$$\int dF_{X(1)|D=0, P=p}(x) \cdot \left\{\eta_{z_{x0p}}(1, 0, p) + \eta_{z_{x0p}}(0, 1, p) - \eta_{z_{x0p}}(1, 1, p)\right\} - \int dF_{X|D=0, P=p}(x) \cdot \eta_{z_{x0p}}(0, 0, p)$$

where $F_{X(1)|D=0, P=p}(x)$ is in turn identified by Assumption 4.

Proof. See Appendix B. $\qquad\square$

6.3. **What if abortion only matters via transition scores?** Looking at the fully-interacted regression (8), it might appear that by conditioning on transition score, we have blocked the main channel by which abortion reform affects Baccalaureate scores. Thus, we might expect the coefficient on $AccessA \cdot AccessB$ in (8) to be zero, missing any interaction effects, if transition scores mediate the impacts of abortion access.

To make this critique precise, let us index potential outcomes by three arguments: $\mathcal{Y}(d, x, z)$, where x indicates a transition score and d now indicates additional impacts of abortion access d on outcomes, with transition score x held fixed. The function $\mathcal{Y}$ is related to our main potential outcomes notation by $Y(d, z) = \mathcal{Y}(d, X_i(d), z)$. To simplify notation, suppose in what follows that there is just one town p. Our parameter of interest generally combines both direct and indirect effects of the abortion reform, and can be written:

$$\Delta_p =: E[\mathcal{Y}_i(1, X_i(1), Z_i) - \mathcal{Y}_i(1, X_i(1), Z_i - 1) - \mathcal{Y}_i(0, X_i(0), Z_i) + \mathcal{Y}_i(0, X_i(0), Z_i - 1)|D_i = 0]$$

$$= \underbrace{\int dF_{X(1)|D=0}(x) \cdot E[\mathcal{Y}_i(1, x, Z_i) - \mathcal{Y}_i(1, x, Z_i - 1) - \mathcal{Y}_i(0, x, Z_i) + \mathcal{Y}_i(0, x, Z_i - 1)|D_i = 0, X_i(1) = x]}_{\text{non-score interaction effects}}$$

$$+ \underbrace{\int \left\{dF_{X(1)|D=0}(x) - dF_{X(0)|D=0}(x)\right\} \cdot \left\{E[\mathcal{Y}_i(0, x, Z_i) - \mathcal{Y}_i(0, x, Z_i - 1)]|D_i = 0, X_i(1) = x\right\}}_{\text{score-mediated interaction effects}}$$

$$+ \int dF_{X(0)|D=0}(x) \cdot \left\{E[\mathcal{Y}_i(0, x, Z_i) - \mathcal{Y}_i(0, x, Z_i - 1)|D_i = 0, X_i(1) = x]\right.$$

$$\left.\underbrace{- E[\mathcal{Y}_i(0, x, Z_i) - \mathcal{Y}_i(0, x, Z_i - 1)|D_i = 0, X_i(0) = x]\right\}}_{\text{reallocation effect}}$$

With the above notation, we can formalize the possibility that abortion access *only* affects Baccalaureate scores Y_i through transition scores. We will call abortion-access "excludable" from the outcome equation when $\mathcal{Y}_i(d, x, z) = y_i(x, z)$ for some function y_i, i.e. potential outcomes do not depend upon d, given x. If excludability holds, then the first term above is zero, and all interaction effects are mediated by changes to students' transition scores.

If the abortion reform affects the distribution of transition scores, this leads to a difference between $F_{X(1)|D=0}$ and $F_{X(0)|D=0}$, making the second term non-zero (except in knife-edge cases). If transition scores are furthermore correlated with individual heterogeneity in next-best-school effects, the third term will also contribute. The third term shows that average interaction effects can arise simply from changing which transition scores are assigned to which students, leaving the overall distribution of transition scores unaffected.

Our basic approach described in the main text (equation 8) focuses on the first and third terms above. However, below we describe a way to reweight the data before estimating equation (8) that captures all three terms, as the estimand of Proposition 2 does. This requires accounting for the effect of the abortion reform on the transition score distribution to account for the second term, which is enabled by Assumption 4. While $F_{X(0)|D=0}$ is directly observable as $F_{X|D=0}$, the CDF $F_{X(1)|D=0}$ is imputed using the distributional parallel trends assumption.

To appreciate the role that reweighting will play in estimation, let us consider the basic approach of Equation (8) and suppose for the moment that excludability holds and that there are just two schools separated by a single threshold t (continuing with a single town p). The coefficient on $AccessA \cdot AccessB$ in (8) then captures the difference-in-differences of RDD estimates: $\eta_z(1,0) + \eta_z(0,1) - \eta_z(1,1) - \eta_z(0,0)$. By Proposition 1, both $\eta_z(0,0)$ and $\{\eta_z(1,0) + \eta_z(0,1) - \eta_z(1,1)\}$ yield different conditional averages of the *same* quantity: $y_i(t_z, z) - y_i(t_z, z-1)$. The former averages over students with $X_i(0) = t$ while the latter averages over students with $X_i(1) = t$. Thus our coefficient of interest differs from zero only via the reallocation effect.

However, in actuality, regression (8) is not confined to such apples-to-oranges comparisons because it aggregates over the many thresholds, which are spread throughout the transition score distribution. Suppose for concreteness that the abortion reform has a homogeneous effect on transition scores for all students, so that $X_i(1) = X_i(0) + \delta$ for some δ. Then when comparing outcomes $Y_i(1,z)$ to $Y_i(0,z)$ among students having $X_i(0) = t_{p0z}$, the proper proper comparison group for investigating outcomes would be students for whom $X_i(1) = t_{p0z} + \delta$, rather than those for whom $X_i(1) = t_{p0z}$. When we stack the data across all thresholds as described in the next section, such $X_i(1) = t_{p0z} + \delta$ students contribute to the coefficient of interest, along with the $X_i(0) = t_{p0z}$ students. The key requirement of regression (8) is that the weights that it applies to the various $\eta_{z_{x0p}}(0,0,p)$ and $\eta_{z_{x0p}}(1,0,p) + \eta_{z_{x0p}}(0,1,p) - \eta_{z_{x0p}}(1,1,p)$ coincide with $dF_{X|D=0,P=p}(x)$ and $dF_{X(1)|D=0,P=p}(x)$ respectively, recovering Proposition 2. The reweighting scheme described in the next section ensures that the coefficient on $AccessA \cdot AccessB$ identifies a meaningful average interaction effect parameter.

6.4. **Stacked regression.** We have seen in Proposition 2 that Δ_p can be estimated by a two-step procedure in which regression discontinuity estimates are computed for each school z and town p, and then averaged over the empirical distribution of schools among abortion-treated students, or alternately by imputing a counterfactual such distribution. This procedure might not be particularly efficient, since it involves running hundreds of separate RDD's around each separate cutoff t_{pcz}.

The "stacked" approach presents an alternative to running such separate RDDs. Specifically, we make Z_{P_i} copies of each observation i, where $Z_p + 1$ is the number of schools in town p. In this expanded dataset, let index ij denote the jth copy of the observation for student

i, where $j = 1 \ldots Z_{P_i}$. Then we define X_{ij} to be $X_i - t_{P_i, j}$, the distance of i's transition score to the cutoff for school j in their town.[55] Using this stacked dataset, we can now estimate common regressions that condition on values of X_{ij} (across the entire stacked dataset) rather than $X_i - t_{pcz}$ for fixed p and z in the original dataset. For all other variables V, the value $V_{ij} = V_i$ appears in "copy" j of row i.

The stacked approach may improve efficiency, but it imposes a particular weighting over the population that with generally not coincide with the weighting imposed by the mean interaction effect parameter Δ_p for town p. To see this, let us first consider a simplified case in which there is only one town p, and we have Z copies of each observation i, where $Z + 1$ is the number of schools. An observation of our stacked dataset is a draw from the probability distribution $\tilde{P}(A_{ij}) := \frac{1}{Z} \sum_{j=1}^{Z} P(A_i(t_j))$, where $A_{ij} = A_i(t_j)$ is any event that depends on which threhold t_j is being used in that "copy" of the data, and P is the population distribution over students i.

For example, the analog of our discontinuity parameter $\eta_z(a, c)$ in the stacked approach would become:

$$\tilde{\eta}(a, c) := \lim_{\epsilon \downarrow 0} \tilde{E}[Y_{ij} | a_{ij} = a, c_{ij} = c, X_{ij} = \epsilon] - \lim_{\epsilon \uparrow 0} \tilde{E}[Y_{ij} | a_{ij} = a, c_{ij} = c, X_{ij} = \epsilon]$$

$$= \frac{1}{f_{ac}(0)} \sum_{z=1}^{Z} \left\{ f_X(t_z | a, c) \cdot \lim_{\epsilon \downarrow 0} E[Y_i | a, c, X_i = t_z + \epsilon] - f_X(t_z | a, c) \cdot \lim_{\epsilon \uparrow 0} E[Y_i | a, c, X_i = t_z + \epsilon] \right\}$$

$$= \frac{1}{f_{ac}(0)} \sum_{z=1}^{Z} f_X(t_z | a, c) \cdot \eta_z(a, c)$$

where $f_{ac}(\epsilon) := \sum_z f_X(t_z + \epsilon | a, c)$ and we have used continuity of $f_X(x | a, c)$. Echoing Lemma 1 of Cattaneo et al. (2016), the above reveals a weighted average of the parameter $\eta_z(a, c)$ across all the school thresholds indexed by z (see proof of Proposition 3 for a derivation).

Suppose for simplicity that $f_X(x | a, c)$ were the same over all values of a and c for the post-reform era (i.e. $d_{ac} = 1$). Then, given our continuity and parallel trends assumptions, the difference-in-differences $\tilde{\eta}(1, 0) + \tilde{\eta}(0, 1) - \tilde{\eta}(1, 1) - \tilde{\eta}(0, 0)$ of $\tilde{\eta}(a, c)$, captured by the coefficient on $AccessA \cdot AccessB$ in (8) estimates:

$$\sum_{z=1}^{Z} \left\{ \frac{f_X(t_z | 1)}{\sum_{z'} f_X(t_{z'} | 1)} \cdot E[Y_i(1, z) - Y_i(1, z - 1) | D_i = 0, X_i(1) = t_{0z}] \right.$$

$$\left. \frac{f_X(t_z | 0)}{\sum_{z'} f_X(t_{z'} | 0)} \cdot E[Y_i(0, z) - Y_i(0, z - 1) | D_i = 0, X_i(0) = t_{oz}] \right\}$$

with the notation that $f_X(t_z | 0) = f_X(t_z | a = 0, c = 0)$ and $f_X(t_z | 1) = f_X(t_z | a, c)$ for the other three values of (a, c). In the dense-schools limit (Assumption 3), the above sum becomes an integral over the conditional distribution of transition scores X in each abortion-reform state. What we seek, by contrast, is to average the first term in brackets above over the distribution of $X_i(1)$ conditional on $D_i = 0$, while averaging the second term over the distribution of $X_i(0)$ *again* conditional on $D_i = 0$. This can be accomplished by reweighing the post-reform observations appropriately before equation (8) is estimated, so that $f_{X(1)|D=0}(t_z)$ appears where $f_X(t_z | 1)$ does in the expression above, mirroring Proposition 2.

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[55] Note that this strategy of normalizing of the running variable to a common scale is similar to the normalizing-and-pooling strategy discussed by Cattaneo, Keele, Titiunik and Vasquez-Bare (2016) for settings in which different subgroups of the population face different cutoffs of the running variable. In our setting, all students within the same town face a common set of multiple cutoffs.

When there are multiple towns, the weights required to obtain the correct averaging in the stacked regression become somewhat more complicated. Proposition 3 shows that we can nevertheless reweight the observations so that the coefficient on $AccessA \cdot AccessB$ in Eq. (8) when applied to the stacked dataset corresponds to a mean interaction effect: $\Delta_0 = E[Y_i(1, Z_i) - Y_i(1, Z_i - 1) - Y_i(0, Z_i) + Y_i(0, Z_i - 1)|D_i = 0]$, which averages over all towns p.

Proposition 3. *Let $\tilde{Y}_{ij} := \omega_{ij} \cdot Y_i$, where $\omega_{ij} = \omega_{a_i,c_i}^{P_i,j}$ and $\omega_{ac}^{pz} := f_{ac} \cdot \frac{P(P_i=p|D_i=0)}{P(P_i=p|a_i=a,c_i=c,X_i=t_{pcz})} \cdot \frac{\Delta F_{ac}^{pz}}{f_X(t_{pcz}|a_i=a,c_i=c)}$ where $f_{ac} := \sum_{p',z'} P(P_i = p'|a_i = a, c_i = c, X_i = t_{p'cz'}) f_X(t_{p'cz'}|a_i = a, c_i = c)$,*

$$\Delta F_{ac}^{pz} = \begin{cases} F(t_{p0z}|00p) - F(t_{p,1,z-1}|00p) & \text{if } a = c = 0 \\ \{F(t_{p0z}|10p) - F(t_{p,1,z-1}|10p)\} + \{F(t_{p0z}|01p) - F(t_{p,1,z-1}|01p)\} \\ \qquad\qquad\qquad\qquad\qquad - \{F(t_{p0z}|11p) - F(t_{p,1,z-1}|11p)\} & \text{otherwise} \end{cases}$$

and $F(x|11p) := P(X_i \leq x|a_i = a, c_i = c, P_i = p)$. Then:

$$\Delta_0 = \tilde{\eta}(1,0) + \tilde{\eta}(0,1) - \tilde{\eta}(1,1) - \tilde{\eta}(0,0)$$

Proof. See Appendix B. $\square$

The components of the weights appearing in Proposition 3 play intuitive roles. The ratio of probabilities "undoes" the up-weighting of observations from large school districts in the stacked sample. The ratio $\Delta F_{ac}^{pz}/f_X(t_{pcz}|a,c)$ meanwhile "corrects" for the heterogeneous weights which which a given school z appears in $\tilde{\eta}(a,c)$ across values of (a,c) (z needs to appear with equal such weights for the parallel trends Assumption 2 to operate). Finally f_{ac} simply reflects a normalization within each (a,c) cell.

In practice, implementing the weighting $\omega_{ij} = \omega_{a_i,c_i}^{P_i,j}$ requires two non-parametric first-stage estimation problems. We implement these using standard local polynomial regression and kernel density estimators. The results of the reweighting estimator are presented in Table 8.

APPENDIX C: PROOFS

6.5. **Proof of Proposition 1.** First we prove Eq (14).

$$\eta(a,c,p) = \lim_{x\downarrow t_{pcz}} E[Y_i|P_i=p, after_i=a, cohort_i=c, X_i=x] - \lim_{x\uparrow t_{pcz}} E[Y_i|P_i=p, after_i=a, cohort_i=c, X_i=x]$$

$$= \lim_{x\downarrow t_{pcz}} E[Y_i(d_{ac}, Z_i(x))|p,a,c,X_i(d_{ac})=x] - \lim_{x\uparrow t_{pcz}} E[Y_i(d_{ac}, Z_i(x))|p,a,c,X_i(d_{ac})=x]$$

$$= \lim_{x\downarrow t_{pcz}} E[Y_i(d_{ac}, z)|p,a,c,X_i(d_{ac})=x] - \lim_{x\uparrow t_{pcz}} E[Y_i(d_{ac}, z-1)|p,a,c,X_i(d_{ac})=x]$$

$$= E[Y_i(d_{ac}, z) - Y_i(d_{ac}, z-1)|P_i=p, after_i=a, cohort_i=c, X_i(d_{ac})=t_{pcz}]$$

The second claim of Proposition 1 now follows immediately:

$$\eta_z(0,0,p) = E[Y_i(0,z) - Y_i(0,z-1)|P_i=p, after_i=1, cohort_i=1, X_i(0)=t_{p0z}]$$

For the first claim, we can rearrange terms and apply the parallel trends Assumption 2:

$$\eta_z(1,0,p) + \eta_z(0,1,p) - \eta_z(1,1,p) = E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, after_i=1, cohort_i=0, X_i(1)=t_{p0z}]$$
$$+ E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, after_i=0, cohort_i=1, X_i(1)=t_{p0z}]$$
$$- E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, after_i=1, cohort_i=1, X_i(1)=t_{p0z}]$$

$$= E[Y_i(1,z)|P_i=p, a_i=1, c_i=0, X_i(1)=t_{p0z}] + E[Y_i(1,z)|P_i=p, a_i=0, c_i=1, X_i(1)=t_{p0z}]$$
$$- E[Y_i(1,z)|P_i=p, a_i=1, c_i=1, X_i(1)=t_{p0z}]$$
$$- E[Y_i(1,z-1)|P_i=p, a_i=1, c_i=0, X_i(1)=t_{p0z}] + E[Y_i(1,z-1)|P_i=p, a_i=0, c_i=1, X_i(1)=t_{p0z}]$$
$$- E[Y_i(1,z-1)|P_i=p, a_i=1, c_i=1, X_i(1)=t_{p0z}]$$

$$= E[Y_i(1,z)|P_i=p, a_i=1, c_i=1, X_i(1)=t_{p0z}] - E[Y_i(1,z-1)|P_i=p, a_i=1, c_i=1, X_i(1)=t_{p0z}]$$

$$= E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, a_i=1, c_i=1, X_i(1)=t_{p0z}]$$

6.6. **Proof of Proposition 2.** With $P(X_i(1) \le s|D_i=0)$ in hand, we can weight the abortion-treated and untreated groups from Proposition 1 according to their respective measures, i.e. estimate:

$$\int dF_{X(1)|D=0,P=p}(x) \cdot \{\eta_{z_{x0p}}(1,0,p) + \eta_{z_{x0p}}(0,1,p) - \eta_{z_{x0p}}(1,1,p)\} - \int dF_{X|D=0,P=p}(x) \cdot \eta_{z_{x0p}}(0,0,p)$$

$$\int dF_{X(1)|D=0,P=p}(x) \cdot E[Y_i(1,z_{x0p}) - Y_i(1,z_{x0p}-1)|P_i=p, D_i=0, X_i(1)=x]$$

$$- \int dF_{X(0)|D=0,P=p}(x) \cdot E[Y_i(0,z_{x0p}) - Y_i(0,z_{x0p}-1)|P_i=p, D_i=0, X_i(0)=x]$$

$$\int dF_{X(1)|D=0,P=p}(x) \cdot E[Y_i(1,z_{x0p}) - Y_i(1,z_{x0p}-1)|P_i=p, D_i=0, X_i(1)=x]$$

$$- E[Y_i(0,Z_i) - Y_i(0,Z_i-1)|P_i=p, D_i=0]$$

$$\int dF_{X(1)|D=0,P=p}(x) \cdot E[Y_i(1,Z_i(X_i(1))) - Y_i(1,Z_i(X_i(1))-1)|P_i=p, D_i=0, X_i(1)=x]$$

$$- E[Y_i(0,Z_i) - Y_i(0,Z_i-1)|P_i=p, D_i=0]$$

Now the problem is that since when $D_i=0$ knowing that $X_i(1)=x$ does not imply that $X_i(0)=x$, we cannot replace z_{x0p} in the first term above by Z_i. This is where Assumption

5 helps. With it, we have:

$$\int dF_{X(1)|D=0,P=p}(x) \cdot \{\eta_{z x 0 p}(1,0,p) + \eta_{z x 0 p}(0,1,p) - \eta_{z x 0 p}(1,1,p)\} - \int dF_{X|D=0,P=p}(x) \cdot \eta_{z x 0 p}(0,0,p)$$

$$\int dF_{X(1)|D=0,P=p}(x) \cdot E[Y_i(1, Z_i(X_i(0))) - Y_i(1, Z_i(X_i(0)) - 1)|P_i = p, D_i = 0, X_i(1) = x]$$
$$- E[Y_i(0, Z_i) - Y_i(0, Z_i - 1)|P_i = p, D_i = 0]$$

$$\int dF_{X(1)|D=0,P=p}(x) \cdot E[Y_i(1, Z_i - Y_i(1, Z_i - 1)|P_i = p, D_i = 0, X_i(1) = x]$$
$$- E[Y_i(0, Z_i) - Y_i(0, Z_i - 1)|P_i = p, D_i = 0]$$

$$E[Y_i(1, Z_i) - Y_i(1, Z_i - 1) - Y_i(0, Z_i + Y_i(0, Z_i - 1)|P_i = p, D_i = 0] = \Delta_p$$

6.7. **Proof of Proposition 3.**

Consider a generic event A_{ij} referring to student i in stacked observation j. Given that we have Z_{P_i} copies of each observation i, our population probability distribution $\tilde{P}$ over stacked observations can be characterized by $\tilde{P}(A_{ij}|P_{ij} = p) := \frac{1}{Z_p}\sum_{j=1}^{Z_p} P(A_i(t_{pC_ij}))$ along with $\tilde{P}(P_{ij} = p) = \frac{Z_p \cdot P(P_i = p)}{\sum_{p'} Z_{p'} \cdot P(P_i = p')}$ (towns with more schools will get automatically get up-weighted in the stacked approach). Thus: $\tilde{P}(A_{ij}) = \sum_p \tilde{P}(A_{ij}, P_{ij} = p) = \frac{1}{\bar{Z}}\sum_p \sum_{j=1}^{Z_p} P(A_i(t_{pC_ij}), P_i = p)$ where $\bar{Z} := \sum_{p'} Z_{p'} \cdot P(P_i = p')$, for any event $A_i(t_{pC_ij})$ that depends on j only through the j-specific threshold t_{pC_ij}. Let $\sum_{pz}$ be a shorthand for the double sum $\sum_p \sum_{z=1}^{Z_p}$ over towns and then schools z within each town, and let $f(\epsilon|a, c)$ denote the quantity $\sum_{pz} P(P_i = p|a_i = a, c_i = c) \cdot f_X(t_{pcz} + \epsilon|a_i = a, c_i = c, P_i = p)$. We have that:

$$\tilde{\eta}(a, c) := \lim_{\epsilon \downarrow 0} \int y \cdot d\tilde{F}_{\tilde{Y}}(y|\tilde{X}_{ij} = \epsilon, a_{ij} = a, c_{ij} = c) - \lim_{\epsilon \uparrow 0} \int y \cdot d\tilde{F}_{\tilde{Y}}(y|\tilde{X}_{ij} = \epsilon, a_{ij} = a, c_{ij} = c)$$

where e.g. the first term is:

$$\lim_{\epsilon \downarrow 0} \int y \cdot d\tilde{F}_{\tilde{Y}}(y|\tilde{X}_{ij} = \epsilon, a_{ij} = a, c_{ij} = c) = \lim_{\epsilon \downarrow 0} \int y \cdot \frac{\frac{d}{d\epsilon} d\tilde{P}(\tilde{Y}_{ij} \cdot \omega_{ij} \le y, X_{ij} \le \epsilon, a_{ij} = a, c_{ij} = c)}{\frac{d}{d\epsilon} \tilde{P}(X_{ij} \le \epsilon, a_{ij} = a, c_{ij} = c)}$$

$$= \lim_{\epsilon \downarrow 0} \int y \cdot \frac{\sum_{pz} \frac{d}{d\epsilon} dP(Y_i \cdot \omega_{a_i, c_i}^{P_i, z} \le y, X_i \le t_{pcz} + \epsilon, a_i = a, c_i = c, P_i = p)}{\sum_{pz} \frac{d}{d\epsilon} P(X_i \le t_{pcz} + \epsilon, a_i = a, c_i = c, P_i = p)}$$

$$= \lim_{\epsilon \downarrow 0} \int y \cdot \frac{\sum_{pz} \frac{d}{d\epsilon} dP(Y_i \cdot \omega_{ac}^{pz} \le y, X_i \le t_{pcz} + \epsilon, P_i = p|a_i = a, c_i = c)}{\sum_{pz} \frac{d}{d\epsilon} P(X_i \le t_{pcz} + \epsilon, P_i = p|a_i = a, c_i = c)}$$

$$= \lim_{\epsilon \downarrow 0} \int y \cdot \frac{\sum_{pz} P(P_i = p|a, c) \frac{d}{d\epsilon} dP(Y_i \cdot \omega_{ac}^{pz} \le y, X_i \le t_{pcz} + \epsilon|a, c, p)}{\sum_{pz} \frac{d}{d\epsilon} P(X_i \le t_{pcz} + \epsilon, P_i = p|a_i = a, c_i = c)}$$

$$= \lim_{\epsilon \downarrow 0} \frac{1}{f(\epsilon|a, c)} \cdot \sum_{pz} P(P_i = p|a, c) \cdot \omega_{ac}^{pz} \cdot \int y \cdot \frac{d}{d\epsilon} dP(Y_i \le y, X_i \le t_{pcz} + \epsilon|a, c, P_i = p)$$

$$= \lim_{\epsilon \downarrow 0} \frac{1}{f(\epsilon|a, c)} \cdot \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|a, c) \cdot f_X(t_{pcz} + \epsilon|a, c, P_i = p) \cdot \int y \cdot dP(Y_i \le y|a, c, P_i = p, X_i = t_{pcz} + \epsilon)$$

$$= \frac{1}{f(0|a, c)} \cdot \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|a, c) \cdot \lim_{\epsilon \downarrow 0} f_X(t_{pcz} + \epsilon|a, c, P_i = p) \cdot \lim_{x \downarrow t_{pcz}} \int y \cdot dP(Y_i \le y|a, c, P_i = p, X_i = x)$$

$$= \frac{1}{f(0|a, c)} \cdot \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|a, c) \cdot f_X(t_{pcz}|a, c, P_i = p) \cdot \lim_{x \downarrow t_{pcz}} E[Y_i|a, c, P_i = p, X_i = x]$$

$$= \frac{1}{f(0|a, c)} \cdot \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|a, c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|a, c) \cdot \lim_{x \downarrow t_{pcz}} E[Y_i|a, c, P_i = p, X_i = x]$$

and similarly for the second term, where we have used continuity of $f_X(x|a_i = a, c_i = c, P_i = p)$ at t_{pcz}. Thus:

$$\tilde{\eta}(a,c) = \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|a_i = a, c_i = c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|a_i = a, c_i = c)$$

$$\cdot \left\{ \lim_{x \downarrow t_{pcz}} \cdot E[Y_i|a_i = a, c_i = c, P_i = p, X_i = x] - \lim_{x \uparrow t_{pcz}} E[Y_i|a_i = a, c_i = c, P_i = p, X_i = x] \right\}$$

$$= \sum_{pz} \omega_{ac}^{pz} \cdot \frac{P(P_i = p|a_i = a, c_i = c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|a_i = a, c_i = c)}{\sum_{p'z'} P(P_i = p'|a_i = a, c_i = c, X_i = t_{p'cz'}) \cdot f_X(t_{p'cz'}|a_i = a, c_i = c)} \cdot \eta_z(a,c,p)$$

$$= \sum_{pz} w_{ac}^{pz} \cdot E[Y_i(ac, z) - Y_i(ac, z-1)|a_i = a, c_i = c, P_i = p, X_i = t_{pcz}]$$

where $w_{ac}^{pz} := \omega_{ac}^{pz} \cdot \frac{P(P_i=p|a_i=a,c_i=c,X_i=t_{pcz}) \cdot f_X(t_{pcz}|a_i=a,c_i=c)}{\sum_{p'z'} P(P_i=p'|a_i=a,c_i=c,X_i=t_{p'cz'}) \cdot f_X(t_{p'cz'}|a_i=a,c_i=c)}$ and we have used that $f(0|a,c) = \sum_{p'z'} P(P_i = p'|a_i = a, c_i = c, X_i = t_{p'cz'}) \cdot f_X(t_{p'cz'}|a_i = a, c_i = c)$. Without re-weighting, i.e. $\omega_{ac}^{pz} = 1$, we would have $\sum_{pz} w_{ac}^{pz} = 1$, but the weights w_{ac}^{pz} would be heterogeneous across p and z. Now suppose that we instead choose $\omega_{ac}^{pz} = \frac{f(0|ac)}{P(P_i=p|a_i=a,c_i=c,X_i=t_{pcz}) \cdot f_X(t_{pcz}|a_i=a,c_i=c)} \cdot P(P_i = p|D_i = 0) \cdot \Delta F_{ac}^{pz}$ where note that

$$\Delta F_{ac}^{pz} = \begin{cases} F_{X(0)|00p}(t_{p0z}) - F_{X(0)|00p}(t_{p0,z-1}) & \text{if } b = c = 0 \\ F_{X(1)|00p}(t_{p0z}) - F_{X(1)|00p}(t_{p0,z-1}) & \text{otherwise} \end{cases} = F_{X(d_{ac})|00p}(t_{p0z}) - F_{X(d_{ac})|00p}(t_{p0,z-1})$$

where we have used the distributional parallel trends assumption for the transition score in the second case. We then have

$$\tilde{\eta}(a,c) = \sum_p P(P_i = p|D_i = 0) \sum_{z=1}^{Z_p} \Delta F_{ac}^{pz} \cdot E[Y_i(d_{ac}, z) - Y_i(d_{ac}, z-1)|a_i = a, c_i = c, P_i = p, X_i(d_{ac}) = t_{pcz}]$$

$$= \sum_p P(P_i = p|D_i = 0) \sum_{z=1}^{Z_p} \left\{ F_{X(d_{ac})|00p}(t_{p0z}) - F_{X(d_{ac})|00p}(t_{p0,z-1}) \right\}$$

$$\cdot E[Y_i(d_{ac}, z) - Y_i(d_{ac}, z-1)|a, c, P_i = p, X_i(d_{ac}) = t_{pcz}]$$

Thus, in the dense-schools limit:

$$\tilde{\eta}(a,c) \approx \sum_p P(P_i = p|D_i = 0) \int dF_{X(d_{ac})|D=0, P=p}(x) \cdot E[Y_i(d_{ac}, z_{x0p}) - Y_i(d_{ac}, z_{x0p}-1)|a, c, P_i = p, X_i(d_{ac}) = x]$$

Now invoking the parallel trends and then no-indirect-effects for the first integral below:

$$\tilde{\Delta}_{DD/RD} = \tilde{\eta}(0,1) + \tilde{\eta}(1,0) - \tilde{\eta}(1,1) - \tilde{\eta}(0,0)$$

$$\approx \sum_p P(P_i = p|D_i = 0) \int dF_{X(1)|D=0, P=p}(x) \cdot E[Y_i(1, z_{x0p}) - Y_i(1, z_{x0p}-1)|D_i = 0, P_i = p, X_i(1) = x]$$

$$- \int dF_{X(0)|D=0, P=p}(x) \cdot E[Y_i(1, z_{x0p}) - Y_i(1, z_{x0p}-1)|D_i = 0, P_i = p, X_i(1) = x$$

$$= \sum_p P(P_i = p|D_i = 0) \int dF_{X(1)|D=0, P=p}(x) \cdot E[Y_i(1, Z_i) - Y_i(1, Z_i-1)|D_i = 0, P_i = p, X_i(0) = x]$$

$$- \int dF_{X(0)|D=0, P=p}(x) \cdot E[Y_i(1, Z_i) - Y_i(1, Z_i-1)|D_i = 0, P_i = p, X_i(0) = x]$$

$$= E[Y_i(1, Z_i) - Y_i(1, Z_i-1)|D_i = 0] - E[Y_i(0, Z_i) - Y_i(0, Z_i-1)|D_i = 0] = \Delta_0$$

The assumption of no indirect effects above enters in the replacement of z_{x0p} by Z_i, which (given $D_i = 0$) is equal to $Z_i(X_i(0))$ which may differ from $Z_i(x)$ conditional on $X_i(1) = x$.

This appendix presents a conceptual framework for the interaction of family and school environments based on the notion of dynamic complementarities.

Analyses of dynamic complementarities must explicitly account for the production of skills at different stages of development. Cunha and Heckman (2007) formalize this by suggesting the following technology for skill formation:

$$\theta_{t+1} = f_t\left(h, \theta_t, I_t\right) \tag{10}$$

where θ_t is a vector of skills measured at time t, h stands for parental characteristics, and I_t denotes parental investments in child skill made during period t. Expression (10) illustrates that skill itself can be an input into the production of skill. Dynamic complementarity arises when this takes the form of higher skill making investments more productive:

$$\frac{\partial^2 f_t\left(h, \theta_t, I_t\right)}{\partial \theta_t \partial I_t} > 0. \tag{11}$$

6.8. **School investments.** Our focus is on the interaction between family and school environments; children can be the object of investments in both settings, with the relative importance of the latter increasing with age. Since I_t refers to family investments, we augment (10) to include school investment, denoted S:

$$\theta_{t+1} = f_t\left(h, \theta_t, I_t, S_t\right). \tag{12}$$

Our setting provides arguably exogenous shocks to: (a) the stock of skills, θ_t, due to the sudden increase in the ease of access to abortion, and (b) school investments, S_t, due to the rules that govern access to better schools. Thus, if there is complementarity between these, we should find:

$$\frac{\partial^2 f_t\left(h, \theta_t, I_t, S_t\right)}{\partial \theta_t \partial S_t} > 0. \tag{13}$$

To be specific, we examine the effect of increased access to abortion on later skills. In addition, we assess the effect of access to better schools. Finally, we estimate the reduced-form interaction of these effects. We next consider how behavioral responses and changes in composition affect the interpretation of these reduced-form interactions.

6.9. **Behavioral responses.** Parents may deliberately choose the human capital investments they direct towards their children (Becker, 1964). For instance, their investments may respond to their children's skill levels, and they may be crowded out or crowded in by school investments:

$$I_t = g_t(\theta_t, S_t).$$

For example, if parents engage in compensatory behavior, investments may depend on the skills children attain relative to their siblings. There is also evidence that parents can react to the level of school inputs (e.g., Das et al., 2013, Del Boca, Flinn, and Wiswall, 2013), and in our setting, Pop-Eleches and Urquiola (2013) show that children who just gained access to better schools receive less homework-related parental help than children who just missed doing so.

We explore if such effects take place in a manner that would reinforce or weaken dynamic complementarities. For example, suppose that parents who had easier access to abortion (and whose children on average therefore have higher levels of skill as they transition into high school) lower their effort by more in response to their child's admission to a better school:

$$(14) \qquad \frac{\partial^2 g_t\left(h, \theta_t, I_t, S_t\right)}{\partial \theta_t \partial S_t} < 0$$

Such an effect would lower the likelihood of finding reduced form evidence of dynamic complementarity even if mechanisms such as those in (11) and (13) are operative. Note that our estimates of behavioral responses may also be influenced by the elasticity of substitution between parental investments across different periods. For example, if the repeal of the abortion ban led to differences in parental investments that persist past early childhood and continue after children enter high school, our behavioral responses capture any interaction between these investments and those induced by the shock to school environments.

6.10. **Composition effects.** Testing for dynamic complementarities, as in (11), requires exogenous variation in θ_t, which we claim the change in abortion policy provides. That said, the manner in which this variation originates is relevant for the interpretation of our results. To see this, it is useful to write the expression for θ_{t+1} in recursive form by substituting in for the stock of skills θ_t with all prior investments:

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

where θ_1 is a child's initial level of skill. This illustrates three potential mechanisms by which increasing access to abortion can affect skills: (i) prior parental investments $I_1...I_{t-1}$, (ii) parental characteristics, h, and (iii) initial skill endowments, θ_1.

All three mechanisms are potentially relevant in our context. First, the repeal of the abortion ban is likely to have led to fewer unwanted children and spurred parental investment. This could arise if childbearing that does not occur at an optimal time affects women's educational, marriage, or labor market decisions in ways that lower parental ability to invest in children (Angrist and Evans, 1999, Goldin and Katz, 2002). Alternately, an undesired birth, by raising lifetime fertility, could adversely impact child outcomes through quantity/quality trade-offs (Becker and Lewis, 1973; Becker, 1981). Second, educational outcomes could be affected by changes in the socioeconomic composition of women who carry pregnancies to term, with the direction of the effect depending on which type of women are more likely to use abortion as opposed to other methods of birth control. Specifically, if women of lower socioeconomic status experienced the largest reductions in fertility when access to abortion increased, children born after the liberalization would tend to have more advantaged parents—a composition effect.[56] Third, it is conceivable that increased access to selective abortions resulted in children with better initial skill endowments (θ_1) by giving parents greater latitude in deciding which pregnancies to take to term based on factors like fetal health (Grossman and Jacobowitz 1981; Joyce 1987; Grossman and Joyce 1990).

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

The relevance of these mechanisms affects the interpretation of the impact of access to abortion and its interaction with access to better schools. While we do not have data on whether the repeal of the abortion ban led to more selective abortions, the screening technology required for this was all but inaccessible for most expectant parents in 1980s Romania. We expect that any differences in initial skill endowments are more likely to reflect parental investments in-utero. In addition, we present evidence that composition, at least in terms of observables, does not drive our findings. As a result, we argue that the main channel through which increased access to abortion affected outcomes is parental investment.