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

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(The following appendix materials are not for publication)

Appendix A: Additional figures and tables

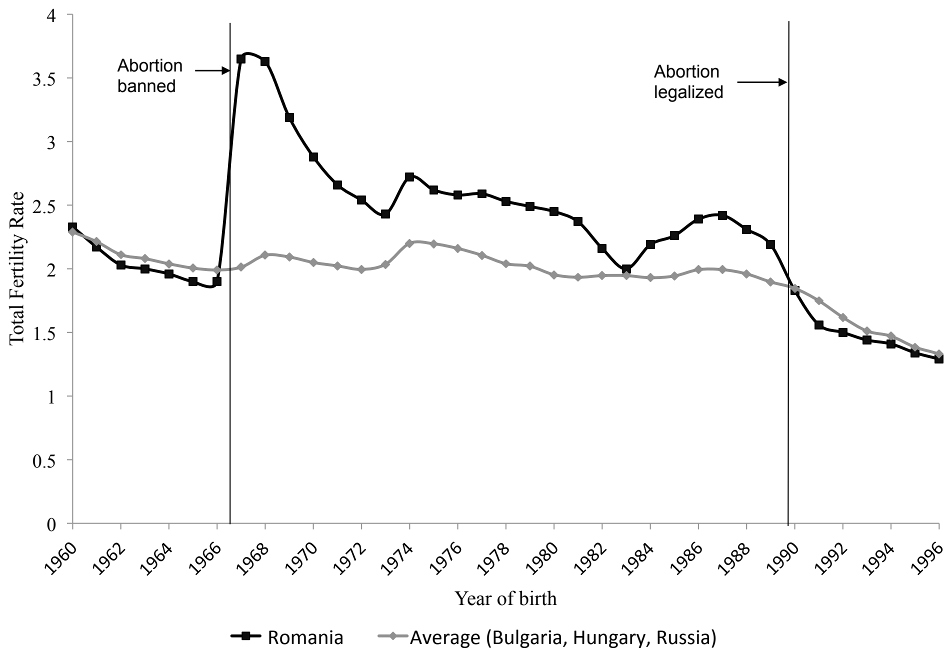

Figure 1. Total fertility rate by year

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).

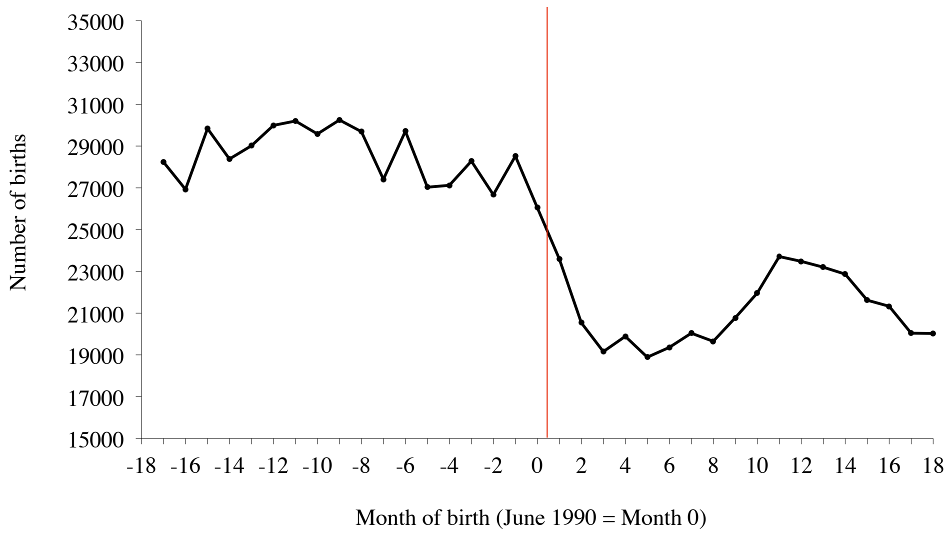

FIGURE 2. Cohort size by month of birth

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.

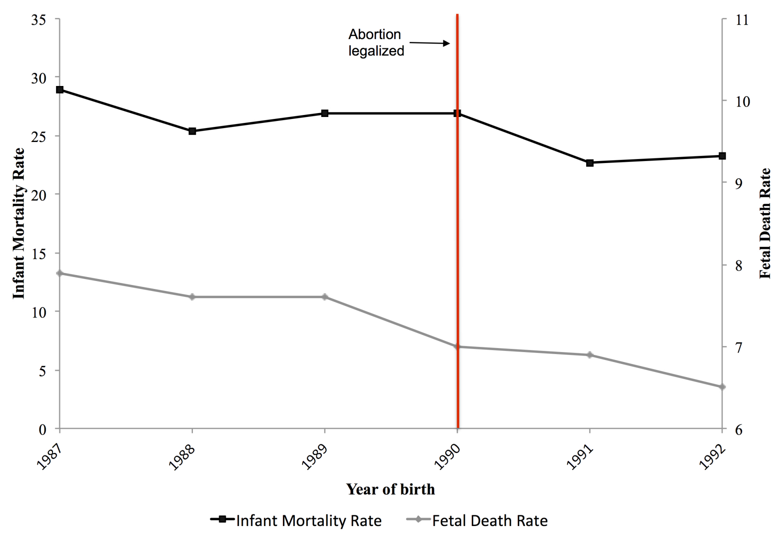

FIGURE 3. Infant mortality and fetal death rates by year of birth

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

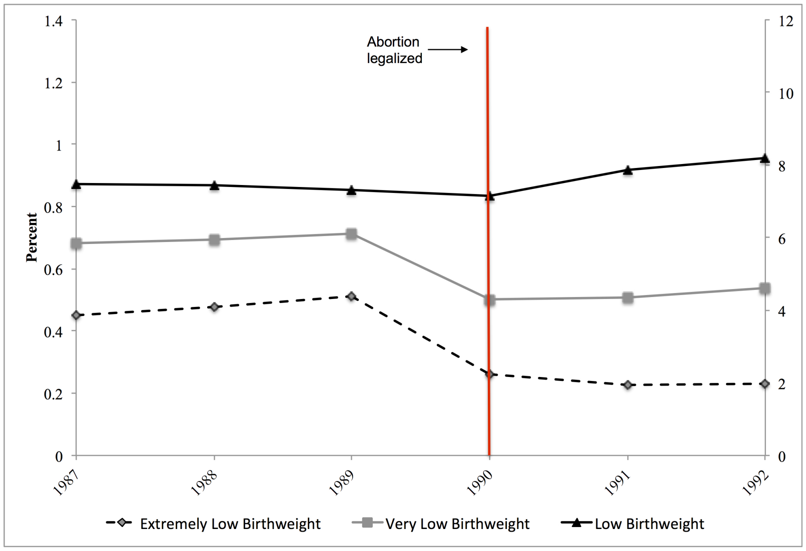

FIGURE 4. Low birthweights by year of birth

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

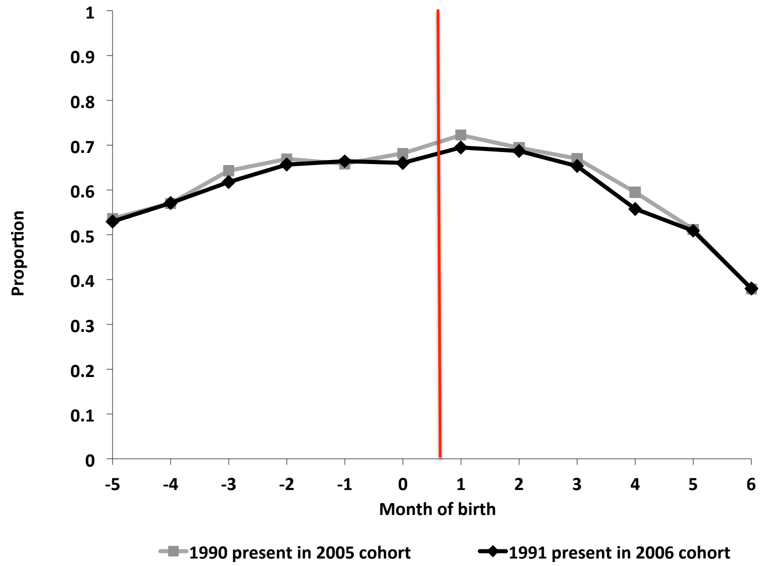

FIGURE 5. Proportion of births by month in admissions cohorts
Notes: This figure uses 1992 census data to plot the proportion of children born in each month. The first group is children born in 1990 present in the 2005 high school admission cohort. The second group is children born in 1991 present in the 2006 admission cohort.

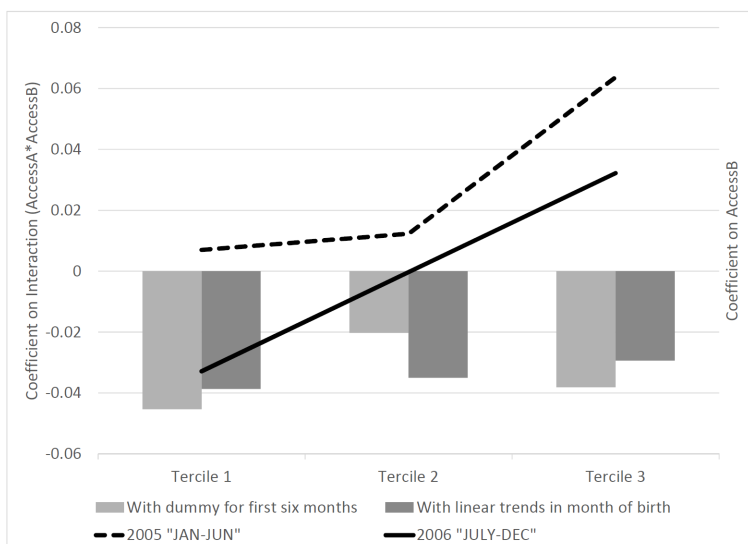

FIGURE 6. Distribution across cutoffs
Notes: This figure plots the interaction effects between access to abortion ($AccessA_i$) and access to a better school ($AccessB_i$) by tercile of the school quality distribution (parametrized by the transition scores of the cutoff for entry to each school) as vertical 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.

Table 1. 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 9^{th} grade students	53.9	40.4	1,963	52.4	37.0	1,771
Panel A.3: School level						
Number of 9^{th} 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 9^{th} 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 9^{th} grade students	40.2	26.1	378	40.1	23.7	341
Panel B.3: School level						
Number of 9^{th} 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 9^{th} 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 2. 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	Within CCT bound	Within IK bound	Within CCT bound
	(1)	(2)	(3)	(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 3. 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	Within CCT bound	Within IK bound	Within CCT bound
	(1)	(2)	(3)	(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 4. 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 5. 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	Within CCT bound	Within IK bound	Within CCT bound
	(1)	(2)	(3)	(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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B: Identification Analysis

0.1. **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 (July-December), and $Cohort_i$ indicates that i was in 2006 cohort (birth year 1991) rather than the 2005 cohort (birth year 1990).[1] We will often abbreviate the random variables $Access_i$ and $Cohort_i$ 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. We 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 $z \in \{0, 1, \dots Z_p\}$, which we assume is stable over the years 2005 and 2006. Let t_{pcz} be the transition score threshold between schools $z-1$ and z in market (p,c),where the school indices are ordered in increasing order of t_{pcz} within a market (p,c) so that $t_{pc1} \leq t_{pc2} \leq t_{pcZ_p}$. Out notation takes the ordinal ranking of schools to be the same across the two years, so that within a given town p, a given value of z 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, and let X_i denote student i's realized transition score (denoted $score_i$ in the main text). Since the transition score of a given student may be affected by abortion access D_i, let us write $X_i = X_i(D_i)$, where $X_i(0)$ and $X_i(1)$ denote counterfactual transition scores depending on abortion access. Student i's "assigned" school Z_i (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. If students attend the most selective school to which they are admitted, then 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 assuming this.[2]

0.2. **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.

We begin with the following continuity assumption on potential outcomes, which leads 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 town* p*.*

[1] Note that $After_i = 1 - before_i$, where $before_i$ was introduced in Section 4.

[2] In the main text, we describe the schools as being ordered by their average transition score, rather than by their minimum score (the threshold t_{pcz}). In that notation, a next-best school treatment effect like $Y_i(d,z) - Y_i(d,z-1)$ captures the effect of being assigned to the z^{th} worst school rather than the $z-1^{th}$ worst school, when schools are ranked according to their average transition score. When schools are instead ordered by their minimum transition score, $Y_i(d,z) - Y_i(d,z-1)$ captures the effect of having access to the z^{th} least selective school rather than the $z-1^{th}$ least selective one. This is what is picked up by RDD estimands that use discontinuities at the transition score threshold between two schools, and we thus in this appendix take the notation z to refer to the ordering by minimum transition score. In practice, the two orderings are nearly the same.

Let $\eta_z(a,c,p)$ denote the observable discontinuity in the conditional expectation $E[Y_i|P_i = p, A_i = a, C_i = c, X_i = x]$ at $x = t_{pcz}$:

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

Under Assumption 1:

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

where $d_{ac} := 1 - (1-a)(1-c)$ is the abortion treatment value for group a, c, identifying a local average treatment effect of moving from school $z-1$ to school z, among students with scores around t_{pcz} in town 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:*

$$E[Y_i(1,z')|p,1,1,t_{p1z}] - E[Y_i(1,z')|p,0,1,t_{p1z}] = E[Y_i(1,z')|p,1,0,t_{p0z}] - E[Y_i(1,z')|p,0,0,t_{p0z}]$$

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

Assumption 2 says that among students who would be just admitted to school z in their cohort given the abortion treatment ($X_i(1) = t_{pC_iz}$), the difference in mean abortion-treated outcomes $Y_i(1,z')$ at school z' between those born in the first and second halves of their birth year is stable between the two cohorts. Given that we will combine this difference-in-differences variation with the RDD variation between adjacent schools, 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.

Note that 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 group), 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. Accordingly, while parallel trends assumptions are typically phrased as an assumption about differences in *untreated* outcomes, ours concerns differences in *treated* outcomes between cohorts.[3]

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 certain means of abortion-treated outcomes among the students that are *not* exposed to the abortion-reform, which is a counterfactual quantity. For example:

[3] 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.

$$E[Y_i(1,z)|P_i=p,D_i=0,X_i(1)=t_{p0z}]=\{E[Y_i|P_i=p,A_i=1,C_i=0,X_i=t_{p0z}]$$
$$+E[Y_i|P_i=p,A_i=0,C_i=1,X_i=t_{p1z}]-E[Y_i|P_i=p,A_i=1,C_i=1,X_i=t_{p1z}]\}$$

Together with Assumption 1 this leads to our central result that combines RDD and DD variation to identify interaction effects:

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 C.

Since $\eta_z(a,c,p)$ is identified for each (a,c,p), both of the *LHS* quantities in Proposition 1 are 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 (Angrist and Pischke, 2009). □

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 allows us to identify the mean interaction effect within each town p:

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

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 $d\in\{0,1\}$ for values of x that are equal to school cutoffs t_{p0z},identifying Δ_p is only possible if there is sufficient variation in school cutoffs t_{p0z} across schools, or under treatment effect homogeneity assumptions. We go the former route and approximate these cutoffs as "dense" in the support $\mathcal{X}$ of X_i as in Bertanha (2020), 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, motivated by there being a school z_{xcp} with a transition score cutoff that is sufficiently close to any given x, for each market c,p. 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). For concreteness, define z_{xcp} to be the school having the largest t_{pcz} cutoff smaller than x (so that e.g. $Z_i(x)=z_{xC_iP_i}$, and realized treatment assignment is $Z_i=z_{X_iC_iP_i}$).

Our approach requires two further assumptions. Firstly, we must impute 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 $F_{acp}(x) := P(X_i \leq x | A_i = a, C_i = c, P_i = p)$ denote the group-specific CDF of observed transitions score X_i. While Assumption 4 may appear stronger than conventional mean parallel trends (since it must hold for each value of x), Roth and Sant'Anna (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.* 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 along the school ladder (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, which implies 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 Δ_p in each town.

Proposition 2. *Given assumptions 1-5, Δ_p is identified as*

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

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

Proof. See Appendix C.

Section 0.4 discusses how Proposition 2 can be implemented through regression (8), by "stacking" the data across schools and then reweighting observations. □

0.3. **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 any 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. The mean interaction effect parameter Δ_0 can be decomposed as follows, combining both direct and indirect effects of the abortion reform:

$$\begin{aligned}
\Delta_0 =& E[\mathcal{Y}_i(1,X_i(1),Z_i) - \mathcal{Y}_i(1,X_i(1),Z_i-1) - \mathcal{Y}_i(0,X_i(0),Z_i) + \mathcal{Y}_i(0,X_i(0),Z_i-1)|D_i=0] \\
=& \underbrace{\int dF_{X(1)|D=0}(x) \cdot E[\mathcal{Y}_i(1,x,Z_i) - \mathcal{Y}_i(1,x,Z_i-1) - \mathcal{Y}_i(0,x,Z_i) + \mathcal{Y}_i(0,x,Z_i-1)|D_i=0, X_i(1)=x]}_{\text{non-score interaction effects}} \\
&+ \underbrace{\int \{dF_{X(1)|D=0}(x) - dF_{X(0)|D=0}(x)\} \cdot \{E[\mathcal{Y}_i(0,x,Z_i) - \mathcal{Y}_i(0,x,Z_i-1)\}|D_i=0, X_i(1)=x]}_{\text{score-mediated interaction effects}} \\
&+ \underbrace{\int dF_{X(0)|D=0}(x) \cdot \{E[\mathcal{Y}_i(0,x,Z_i) - \mathcal{Y}_i(0,x,Z_i-1)|D_i=0, X_i(1)=x] - E[\mathcal{Y}_i(0,x,Z_i) - \mathcal{Y}_i(0,x,Z_i-1)|D_i=0, X_i(0)=x]\}}_{\text{reallocation effect}}
\end{aligned}$$

With the above notation, we can formalize the possibility that abortion access *only* affects Baccalaureate scores Y_i through transition scores. 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 for all students, 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 in Δ_0 generally non-zero. 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, even with the overall distribution of transition scores unchanged (a "reallocation" effect).

The basic approach described in the main text (equation 8) focuses on the first and third terms above, because it does not account for changes in the distribution of transition scores arising from the abortion reform. However, in Section 0.4 below we describe a way to reweight the data before estimating equation (8) that allows it to capture all three terms, as the estimand of Proposition 2 does.

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 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 $X_i(1) = t_{p0z} + \delta$, not those for whom $X_i(1) = t_{p0z}$. When we stack the data across all thresholds as described in the next section, these $X_i(1) = t_{p0z} + \delta$ students contribute to the coefficient of interest, along with the $X_i(0) = t_{p0z}$ students. The key requirement is that the weights that regression (8) 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 $dF_{X(0)|D=0,P=p}(x)$ and $dF_{X(1)|D=0,P=p}(x)$ respectively, recovering Proposition 2. The reweighting scheme described in the next section does so to ensure that the coefficient on $AccessA \cdot AccessB$ identifies a meaningful average interaction effect parameter.

0.4. **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-nontreated students, as well as an imputed counterfactual 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, by transforming the data such that the average interaction effect across towns

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

can be estimated through a single run of regression (8). 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 j^{th} copy of the observation for student i, where $j = 1 \ldots Z_{P_i}$. Then we define X_{ij} to be $X_i - t_{P_i C_i j}$, the distance of i's transition score to the cutoff for school j in their town.[4] 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.

Despite it's appeal as an estimator, the stacked approach imposes a particular weighting over the population that will generally not coincide with the parameter of interest 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 an event (like $X_{ij} = x$) 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.

[4] 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 instead face a common set of multiple cutoffs.

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

$$
\begin{aligned}
\tilde{\eta}(a,c) :=& \lim_{\epsilon\downarrow 0} \tilde{E}[Y_{ij}|A_{ij}=a, C_{ij}=c, X_{ij}=\epsilon] - \lim_{\epsilon\uparrow 0} \tilde{E}[Y_{ij}|A_{ij}=a, C_{ij}=c, X_{ij}=\epsilon] \\
=& \frac{1}{f_{ac}(0)} \sum_{z=1}^{Z} \left\{ f_X(t_z|a,c)\cdot \lim_{\epsilon\downarrow 0} E[Y_i|a,c,X_i=t_z+\epsilon] - f_X(t_z|a,c)\cdot \lim_{\epsilon\uparrow 0} E[Y_i|a,c,X_i=t_z+\epsilon] \right\} \\
=& \frac{1}{f_{ac}(0)} \sum_{z=1}^{Z} f_X(t_z|a,c)\cdot \eta_z(a,c)
\end{aligned}
$$

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 during 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:

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

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 second term in brackets above over the distribution of $X_i(0)$ conditional on $D_i=0$, while averaging the first term over the distribution of $X_i(1)$ *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.

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* $Y_{ij} := \omega_{ij}\cdot Y_i$, *where* $\omega_{ij} = \omega^{P_i,j}_{A_i,C_i}$ *and* $\omega^{pz}_{ac} := f_{ac}\cdot \frac{P(P_i=p|D_i=0)}{P(P_i=p|A_i=a,C_i=c,X_i=t_{pcz})}\cdot \frac{\Delta F^{pz}_{ac}}{f_X(t_{pcz}|A_i=a,C_i=c)}$ *where* $f_{ac} := \sum_{p',z'} P(P_i=p'|A_i=a,C_i=c,X_i=t_{p'cz'}) f_X(t_{p'cz'}|A_i=a,C_i=c)$,

$$
\Delta F^{pz}_{ac} = \begin{cases} F(t_{p0z}|00p) - F(t_{p,0,z-1}|00p) & \text{if } a=c=0 \\ \{F(t_{p0z}|10p) - F(t_{p,0,z-1}|10p)\} + \{F(t_{p1z}|01p) - F(t_{p,1,z-1}|01p)\} & \\ \qquad - \{F(t_{p1z}|11p) - F(t_{p,1,z-1}|11p)\} & \text{if } max(a,c)=1 \end{cases}
$$

and $F(x|acp) := P(X_i \le x|A_i=a, C_i=c, P_i=p)$. *Then:*

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

Proof. See Appendix C. □

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 $\Delta F^{pz}_{ac}/f_X(t_{pcz}|a,c)$ meanwhile "corrects" for the heterogeneous weights which which a given school z appears in $\tilde{\eta}(a,c)$ across values of (a,c) (whoch must be equal for Assumption 2 to be employed). Finally f_{ac} simply reflects a normalization within each (a,c) cell. In practice, implementing the weighting $\omega_{ij} = \omega^{P_i,j}_{A_i,C_i}$ requires two non-parametric first-stage estimation problems. We use standard local polynomial regression and kernel density estimators. Results of the reweighting estimator are presented in Table 8.

Appendix C: Proofs

0.5. **Proof of Proposition 1.** First we prove Eq (14). By Assumption 1:

$$\begin{aligned}\eta(a,c,p) &= \lim_{x\downarrow t_{pcz}} E[Y_i|P_i=p, After_i=a, Cohort_i=c, X_i=x] - \lim_{x\uparrow t_{pcz}} E[Y_i|P_i=p, After_i=a, Cohort_i=c, X_i=x] \\ &= \lim_{x\downarrow t_{pcz}} E[Y_i(d_{ac}, Z_i(x))|p,a,c,X_i(d_{ac})=x] - \lim_{x\uparrow t_{pcz}} E[Y_i(d_{ac}, Z_i(x))|p,a,c,X_i(d_{ac})=x] \\ &= \lim_{x\downarrow t_{pcz}} E[Y_i(d_{ac}, z)|p,a,c,X_i(d_{ac})=x] - \lim_{x\uparrow t_{pcz}} E[Y_i(d_{ac}, z-1)|p,a,c,X_i(d_{ac})=x] \\ &= E[Y_i(d_{ac},z) - Y_i(d_{ac},z-1)|P_i=p, After_i=a, Cohort_i=c, X_i(d_{ac})=t_{pcz}]\end{aligned}$$

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:

$$\begin{aligned}\eta_z(1,0,p) + \eta_z(0,1,p) - \eta_z(1,1,p) = &E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, After_i=1, Cohort_i=0, X_i(1)=t_{p0z}] \\ &+ E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, After_i=0, Cohort_i=1, X_i(1)=t_{p1z}] \\ &- E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, After_i=1, Cohort_i=1, X_i(1)=t_{p1z}] \\ =&E[Y_i(1,z)|P_i=p, A_i=1, C_i=0, X_i(1)=t_{p0z}] + E[Y_i(1,z)|P_i=p, A_i=0, C_i=1, X_i(1)=t_{p1z}] \\ &- E[Y_i(1,z)|P_i=p, A_i=1, C_i=1, X_i(1)=t_{p1z}] \\ &- E[Y_i(1,z-1)|P_i=p, A_i=1, C_i=0, X_i(1)=t_{p0z}] - E[Y_i(1,z-1)|P_i=p, A_i=0, C_i=1, X_i(1)=t_{p1z}] \\ &+ E[Y_i(1,z-1)|P_i=p, A_i=1, C_i=1, X_i(1)=t_{p1z}] \\ =& E[Y_i(1,z)|P_i=p, A_i=0, C_i=0, X_i(1)=t_{p0z}] - E[Y_i(1,z-1)|P_i=p, A_i=0, C_i=0, X_i(1)=t_{p0z}] \\ =& E[Y_i(1,z) - Y_i(1,z-1)|P_i=p, D_i=0, X_i(1)=t_{p0z}]\end{aligned}$$

0.6. **Proof of Proposition 2.** With $F_{X(1)|D=0,P=p}(x) = P(X_i(1) \leq x|D_i=0, P_i=p)$ in hand, we can weight the abortion-treated and untreated groups from Proposition 1 according to their respective measures, i.e. estimate:

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

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

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

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

Note that when $D_i=0$, knowing that $X_i(1)=x$ does not imply that $X_i=x$, so we cannot replace z_{x0p} in the first term above by $Z_i = Z_i(X_i)$. This is where Assumption 5 helps. With it, we have:

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

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

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

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

where in the second line we've replaced $Z_i(X_i(1))$ with $Z_i(X_i(0))=Z_i$.

0.7. **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 $\tilde{P}(P_{ij}=p)=\frac{Z_p\cdot P(P_i=p)}{\sum_{p'}Z_{p'}\cdot P(P_i=p')}$ and $\tilde{P}(A_{ij}|P_{ij}=p):=\frac{1}{Z_p}\sum_{j=1}^{Z_p}P(A_i(t_{pC_ij})|P_i=p)$. 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 (z, which indexes schools, now plays the role of j, which indexed stacked "observations" for a given student i. Given that $\tilde{\eta}(a,c)$ captures the discontinuity in the conditional expectation of Y at $X_{ij}=0$ with respect to the probability distribution $\tilde{P}$, we can write:

$$\tilde{\eta}(a,c):=\lim_{\epsilon\downarrow 0}\int y\cdot d\tilde{F}_Y(y|X_{ij}=\epsilon,A_{ij}=a,C_{ij}=c)-\lim_{\epsilon\uparrow 0}\int y\cdot d\tilde{F}_Y(y|X_{ij}=\epsilon,A_{ij}=a,C_{ij}=c)$$

The first term e.g. is:

$$\lim_{\epsilon\downarrow 0}\int y\cdot d\tilde{F}_Y(y|X_{ij}=\epsilon,A_{ij}=a,C_{ij}=c)=\lim_{\epsilon\downarrow 0}\frac{\int y\cdot\frac{d}{d\epsilon}d\tilde{P}(Y_{ij}\cdot\omega_{ij}\le y,X_{ij}\le\epsilon,A_{ij}=a,C_{ij}=c)}{\frac{d}{d\epsilon}\tilde{P}(X_{ij}\le\epsilon,A_{ij}=a,C_{ij}=c)}$$
$$=\lim_{\epsilon\downarrow 0}\frac{\int y\cdot\frac{d}{d\epsilon}\sum_{pz}dP(Y_i\cdot\omega_{A_i,C_i}^{P_i,z}\le y,X_i\le t_{pcz}+\epsilon,A_i=a,C_i=c,P_i=p)}{\frac{d}{d\epsilon}\sum_{pz}P(X_i\le t_{pcz}+\epsilon,A_i=a,C_i=c,P_i=p)}$$
$$=\lim_{\epsilon\downarrow 0}\int y\cdot\frac{\sum_{pz}\frac{d}{d\epsilon}dP(Y_i\cdot\omega_{ac}^{pz}\le y,X_i\le t_{pcz}+\epsilon,P_i=p|A_i=a,C_i=c)}{\sum_{pz}\frac{d}{d\epsilon}P(X_i\le t_{pcz}+\epsilon,P_i=p|A_i=a,C_i=c)}$$
$$=\lim_{\epsilon\downarrow 0}\int y\cdot\frac{\sum_{pz}P(P_i=p|a,c)\frac{d}{d\epsilon}dP(Y_i\cdot\omega_{ac}^{pz}\le y,X_i\le t_{pcz}+\epsilon|a,c,p)}{\sum_{pz}P(P_i=p|a,c)\cdot f_X(t_{pcz}+\epsilon|a,c,p)}$$
$$=\lim_{\epsilon\downarrow 0}\frac{1}{f(\epsilon|a,c)}\cdot\sum_{pz}P(P_i=p|a,c)\cdot\omega_{ac}^{pz}\cdot\int y\cdot\frac{d}{d\epsilon}dP(Y_i\le y,X_i\le t_{pcz}+\epsilon|a,c,p)$$
$$=\lim_{\epsilon\downarrow 0}\frac{1}{f(\epsilon|a,c)}\cdot\sum_{pz}\omega_{ac}^{pz}\cdot P(P_i=p|a,c)\cdot f_X(t_{pcz}+\epsilon|a,c,p)\cdot\int y\cdot dP(Y_i\le y|a,c,p,X_i=t_{pcz}+\epsilon)$$

where we let $f(\epsilon|a,c)$ denote the quantity $\sum_{pz}P(P_i=p|a,c)\cdot f_X(t_{pcz}+\epsilon|a,c,p)$, and we've used a change of variables in the fifth equality.[5] Now, using continuity of $f_X(x|A_i=a,C_i=c,P_i=p)$ at t_{pcz}, we can write the above as

[5]Quantities of the form $\int y\cdot dP(Y\le y,E)$ are understood as Riemann–Stieltjes integrals with respect to $P(Y\le y,E)$ viewed as a function of y, for a fixed event E.

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

Thus:

$$\tilde{\eta}(a,c) = \frac{1}{f(0|a,c)} \cdot \sum_{pz} \omega_{ac}^{pz} \cdot P(P_i = p|A_i = a, C_i = c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|A_i = a, C_i = c)$$
$$\cdot \left\{ \lim_{x \downarrow t_{pcz}} \cdot E[Y_i|A_i = a, C_i = c, P_i = p, X_i = x] - \lim_{x \uparrow t_{pcz}} E[Y_i|A_i = a, C_i = c, P_i = p, X_i = x] \right\}$$
$$= \sum_{pz} \omega_{ac}^{pz} \cdot \frac{P(P_i = p|A_i = a, C_i = c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|A_i = a, C_i = c)}{\sum_{p'z'} P(P_i = p'|A_i = a, C_i = c, X_i = t_{p'cz'}) \cdot f_X(t_{p'cz'}|A_i = a, C_i = c)} \cdot \eta_z(a,c,p)$$
$$= \sum_{pz} w_{ac}^{pz} \cdot \eta_z(a,c,p)$$

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 we can rewrite $f(0|a,c) = \sum_{pz} P(P_i = p|A_i = a, C_i = c, X_i = t_{pcz}) \cdot f_X(t_{pcz}|A_i = a, C_i = c)$. Suppose that we chose $\omega_{ac}^{pz} = 1$ for all a, c, p, z, i.e. no re-weighting. Then we would have $\sum_{pz} w_{ac}^{pz} = 1$, but the weights w_{ac}^{pz} would be heterogeneous across a and c, preventing us from leveraging the parallel-trends assumption for Y. 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 ΔF_{ac}^{pz} is as-defined in Proposition 3 . Using the distributional parallel trends assumption for the transition score, note first that

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

With the above choice of ω_{ac}^{pz} we thus have that

$$\tilde{\eta}(a,c) = \sum_p P(P_i = p|D_i = 0) \sum_{z=1}^{Z_p} \Delta F_{ac}^{pz} \cdot \eta_z(a,c,p)$$
$$= \sum_p P(P_i = p|D_i = 0) \sum_{z=1}^{Z_p} \left\{ F_{X(d_{ac})|00p}(t_{p0z}) - F_{X(d_{ac})|00p}(t_{p0,z-1}) \right\} \cdot \eta_z(a,c,p)$$

Therefore, in the dense-schools limit:

$$\tilde{\eta}(a,c) \approx \sum_p P(P_i = p|D_i = 0) \int dF_{X(d_{ac})|D=0,P=p}(x) \cdot \eta_{z_{x0p}}(a,c,p)$$

Finally, applying Proposition 2:

$$\tilde{\Delta}_{DD/RD} = \tilde{\eta}(0,1) + \tilde{\eta}(1,0) - \tilde{\eta}(1,1) - \tilde{\eta}(0,0) \approx \sum_p P(P_i = p|D_i = 0) \cdot \Delta_p = \Delta_0$$

Appendix D: Conceptual framework

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:

$$\theta_{t+1} = f_t\left(h, \theta_t, I_t\right) \tag{2}$$

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 (2) 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:

$$\frac{\partial^2 f_t\left(h, \theta_t, I_t\right)}{\partial\theta_t \partial I_t} > 0. \tag{3}$$

0.8. **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 (2) to include school investment, denoted S:

$$\theta_{t+1} = f_t\left(h, \theta_t, I_t, S_t\right). \tag{4}$$

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:

$$\frac{\partial^2 f_t\left(h, \theta_t, I_t, S_t\right)}{\partial\theta_t \partial S_t} > 0. \tag{5}$$

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.

0.9. **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:

$$\frac{\partial^2 g_t\left(h, \theta_t, I_t, S_t\right)}{\partial \theta_t \partial S_t} < 0 \tag{6}$$

Such an effect would lower the likelihood of finding reduced form evidence of dynamic complementarity even if mechanisms such as those in (3) and (5) 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.

0.10. **Composition effects.** Testing for dynamic complementarities, as in (3), 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.[6] 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).

[6] 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.

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