Moving On Up for High School Graduates in Russia: The Consequences of the Unified State Exam Reform*

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Abstract

In 2009, Russia introduced a reform that changed the admissions process in all universities. Before 2009, admission decisions were based on institution-specific entry exams; the reform required universities to determine their decisions on the results of a national high-school test known as Unified State Exam (USE). One of the main goals of the reform was to make education in top colleges accessible to students from peripheral areas who typically did not enroll in university programs. Using panel data from 1994 to 2014, we evaluate the effect of the USE reform on student mobility. We find the reform led to a substantial increase in mobility rates among high school graduates from peripheral areas to start college by about 12 percentage points, a three-fold increase with respect to the pre-reform mobility rate. This was accompanied by a 40—50% increase in the likelihood of financial transfers from parents to children around the time of the move and a 70% increase in the share of educational expenditures in the last year of the child's high school. We find no effect on parental labor supply and divorce.

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1. Introduction

In 2009, Russia introduced a reform that changed the admission process for all universities in the country.[1] Before 2009, admission decisions were based on idiosyncratic, institution-specific entry tests. The reform instead required colleges to determine their admissions on the basis of the scores from a nationally administered, secondary-school examination, known as the Unified State Exam (USE). For the first time, we evaluate the effect of this reform on a wide range of outcomes, paying special attention to students' geographic mobility to pursue studies in higher education.

Most of what we currently know about higher education in post-Soviet Russia comes from correlational studies by sociologists and educationalists (e.g., Johnson, 2010, Ampilogov, Prakhov, and Yudkevich, 2013). A consistent message stemming from that strand of research is that, before the 2009 reform, university access was highly unequal. Students from low socioeconomic status (SES) were considerably less likely to apply to college and gain a degree than their high SES peers. For instance, data from the 2006 Russian Longitudinal Monitoring Survey (RLMS) reveal that about 65% of 25–29 year old individuals reported to have a university qualification if their father also had a university degree, as opposed to only 20% among those whose fathers had no qualification.[2] The substantial and statistically significant differential of 45 percentage points is twice as large as the college participation gap observed in the United States between children from high- and low-income families (Carneiro and Heckman, 2002), and it is comparable to the US black-white male college graduation rate differentials observed in the 1960s (Neal, 2006).

Another dimension of the large college achievement gap in pre-reform Russia was associated with the geographic origin of university graduates. Before 2009, less than 20% of young Russians were born in the ten largest cities (including Moscow and St. Petersburg) and yet they represented more than 60% of all university graduates, whereas only one in ten graduates were born in small cities, towns, and rural areas, which instead accounted for about half of the population. This overrepresentation of high school graduates from large cities among university students was associated with a steep socioeconomic gradient. Households from small cities and other peripheral areas had a total income that was on average 40–50% lower than the total income for households living in major cities, even after adjusting for differences in family size and the cost of living across locations.

[1] Throughout the paper, we use the terms "university", "college", "higher education institution", and "post-secondary education institution" interchangeably.

[2] These statistics from our own calculations are in line with those recently found by Borisov and Pissarides (2016). The RLMS data, which we use in our main analysis, will be described in detail in Section 4.

Proponents of the USE reform argued that college application costs would have been considerably reduced, especially for students living in peripheral areas far from the main urban centers, where elite universities were (and still are) located.[3] This was because a USE-based admission would have spared applicants from preparing for college- or department-specific entry exams and from taking such exams in each of the different programs chosen. The pre-reform costs materialized in terms of time and money, as they included preparation time (in addition to the time devoted to standard school tests), expenses on preparatory materials, books and tutors, as well as transportation times and fares, and temporary relocation rents while sitting for the exams. The USE reform would have not only reduced such costs considerably, but also allowed simultaneous applications to multiple colleges/programs, something that was highly impractical in the pre-reform regime. The expectation then was that a large share of the benefits would have gone to high school graduates from areas of Russia which did not have physical proximity to post-secondary education institutions.

In this sense, therefore, the USE reform was expected to operate in the same way as the growth in college availability in the United States over the course of the twentieth century. As several influential studies document, greater college availability does increase access to higher education and pushes up college enrollment (e.g., Card, 1995, Goldin and Katz, 1999, Currie and Moretti, 2003).

Our main estimates emphatically confirm this expectation about the USE reform. They indicate that the fraction of high school graduates in peripheral Russia who leave their parental home after graduation and started post-secondary education went up by at least 12 percentage points as a direct response to the USE reform, a three-fold increase with respect to the mobility rate before 2009. Such estimates are robust to different specifications of temporal trends, the inclusion of unobserved fixed effects shared among siblings, the use of propensity score matching procedures combined with difference-in-differences, and the use of time duration models.

These effect estimates are large, corresponding to at least 60% of the out-of-state migration rates observed in recent years among college students in the United States (Kennan, 2015) and accounting for about one-fifth of the overall interregional migration rate in Russia in the years since the reform (Guriev and Vakulenko, 2015). We take this result as a strong indication that the USE reform has played an important role in easing the access to higher education institutions for young people who would have not normally enrolled into university programs in Russia.

[3]For instance, more than 200 higher education institutions were/are based in Moscow and St. Petersburg alone.

The empirical analysis also suggests that the greater student mobility associated with college enrollment was accompanied by a 40–50% increase in the likelihood of financial transfers from parents to children around the time of the migration decision and a 70% increase in the share of household educational expenditures in the last year of the child's high school. This reveals significant child investments made by a nonneglible fraction of parents in small cities and towns, who would have not invested in the absence of the reform. Importantly, we find no evidence that the reform led to unintended changes in parental behavior or to undesirable spillovers. In particular, we find no effect of the USE reform on mother's and father's labor supply (both at the extensive and intensive margins), on major categories of household consumption, and on parental divorce.

Our results are consistent with the evidence found in the growing empirical literature that documents that college application decisions in the United States are likely to be suboptimal, especially among low-income students (Ellwood and Kane, 2000, Bowen, Chingos, and McPherson, 2009, Hoxby and Avery, 2013, Dillon and Smith, 2017). Recent related studies provide a number of explanations about why this might be the case. There is general agreement that providing prospective students with salient information about universities (e.g., the range of colleges available and the actual costs and benefits of attending specific university programs) and giving them assistance (financial or otherwise) with the application process can lead to substantial changes in college application choices and matriculation decisions (e.g., Bettinger et al., 2012, Hoxby and Turner, 2013, Pallais, 2015, Carrell and Sacerdote, forthcoming). Of course, unlike such "small" interventions, which are meant to affect the simple heuristics used by students in making their college applications, the USE reform is a major institutional change in the default rules governing the entire college admission process.

Our results also speak to the literature that looks at the implications of different college admission rules, such as rules based on the Scholastic Assessment Test or the American College Testing on the one hand and high school grades on the other. Advocates of a rule based on high school grades, such as the marks in the Unified State Exam, often claim that a curriculum-based type test is likely to produce more socioeconomic diversity on campus, since this rule is expected to expand access and equity in university admissions (e.g., Geiser and Santelices, 2007, Espenshade and Chung, 2010). The finding that the USE reform led to a higher migration rate of high school graduates from (lower SES) peripheral places to enroll into university programs confirms that expectation.

The next section describes the institutional background against which the USE reform took place. In Section 3 we develop a simple conceptual framework which guides the interpretation of our empirical findings. Section 4 describes the data, while Section 5 discusses the statistical methods. Section 6 presents the results,

and Section 7 concludes.

2. Institutional Background and the USE Reform

Since the fall of the Soviet Union in 1991, the newly formed Russian Federation witnessed a rapid dismantling of the preexisting higher education system. The new environment gave universities an increasing degree of autonomy with respect to the central government.[4] Taking advantage of this opportunity, some universities — including many new private institutions — committed resources to high quality educational provision. Others remained bureaucratic and hierarchical. Reflecting this marked diversity in provision and quality, admission procedures were equally highly heterogeneous. Most institutions developed their own entry tests, which were administered and graded in-house, and tests often required a face-to-face oral examination assessed by an internal committee.

Keeping in mind the high concentration of post-secondary education institutions in a few large cities, one clear disadvantage of this fragmented admission system is that potential applicants had to face hefty costs in pursuing an application. Because entry tests had to be taken in person, students living far from (high-quality) universities had first to incur the cost of travelling to examination venues. In addition, the new post-Soviet system provided abundant opportunities for favoritism (e.g., some candidates had preferential access to preparation materials) and corruption (e.g., teaching preparatory courses for admission was known to be a nontrivial source of additional income for university faculty members involved in the process).[5] Attending the most prestigious programs in Moscow and St. Petersburg was therefore considered outside the feasible opportunity set for anyone except a select few with connections, substantial financial resources, or exceptional talent.

The admission reform based on the Unified State Exam (USE) is the centerpiece of the most recent strategy to upend the pre-existing higher education system. The USE consists of a series of tests taken by all students in Russian secondary schools at the end of their final year.[6] Although students can choose which tests to take out of 14 different subjects, Russian language and Math exams are mandatory for

[4]For an overview of the changes to, and evolution of, the educational landscape in Russia over the last twenty years, see Johnson (2010), Lukyanova (2012), Belskaya and Sabirianova Peter (2014), and Denisova-Schmidt and Leontyeva (2014).

[5]Connections and bribery were allegedly widely used also to have access to state-funded places exempt from tuition fees (Osipian, 2009).

[6]Since 2007, mandatory education in Russia covers grade 1 (ages 6 and 7) through grade 11. Schooling is split into primary (grades 1–4), middle (grades 5–9) and senior (grades 10–11) classes. The USE is taken by the end of the eleventh grade.

graduation.[7]

The USE reform was introduced in 2009. Starting from the 2009/10 academic year, university admission decisions have to be based exclusively on USE scores.[8] Each program within a higher education institution must publicize the subject tests and the minimum threshold scores needed for admission. Only students scoring above the threshold in each of the required subjects can apply to the program. Applicants are then ranked according to their total score (the simple sum of scores in the required tests), and the final admission decision is made according to this ranking, until either all vacancies are filled or the pool of eligible applicants is exhausted.[9] After learning their USE scores, students are allowed to apply simultaneously to up to 15 programs. Specifically, they may apply to up to five different universities and three programs within each institution. The USE-based ranking is also used to determine the financial aid students receive from the state, whereas before the reform state funds were allocated to universities which in turn assigned scholarships based on their own admission criteria.

Unified State Exams were introduced gradually. The first were piloted in 2001 in few schools in five of the 84 regions of Russia. By 2008, the USE was offered everywhere and essentially all students in the last year of high school sat in at least one subject test, although only 16% of high school graduates took a test other than Math and Russian. While some (very few) universities used USE scores as part of their admission process before 2009, the majority — and notably the largest and most prestigious state universities in Moscow and Saint Petersburg — kept relying on their own practices.[10] It was only with the entry cohort of 2009 that the USE

[7]USEs are not required for students pursuing a vocational/technical track, which usually begins after completion of the ninth grade and extends over 2–4 additional years of study.

[8]There are some exceptions to this rule. For instance, there are organized contests (known as "Olympiads") designed to identify the most talented students in the country. The winners of the Olympiads are allowed to attend the program of their choice and offered a full scholarship. Another exception applies to programs requiring specific skills (e.g. fine arts and journalism) for which USE scores are complemented with additional tests. At the discretion of universities, students who obtained a secondary school diploma through a technical or vocational school, or pursuing part-time, long-distance and evening-only programs, and students with disabilities can be given an alternative entry exams. Finally, a "grandfathering" clause allows individuals who graduated from high school before 2009 or who already possess a Bachelors diploma to be exempted and be admitted to university based on alternative criteria. All such exceptions cover about 25% of university students in each entry cohort since 2009. Most of these are students in part-time or evening-only programs, while virtually all students in top institutions are enrolled in regular, full-time programs. Since 2016/17 (outside our sample period), admission rules have become stricter and more tightly linked to the USE results.

[9]Unfortunately, there is no institutional access to individual USE scores across universities, nor are there data sources recording where students apply.

[10]As a result of this, allowing for time as well as regional variation to pick up earlier adoptions of the USE based admission system does not identify any significant impact. Such estimates are

scores became the standard yardstick for university admissions.[11]

One of the main objectives of the 2009 reform was that university admissions should rely on students' performance in the USE. Across the entire country, the tests are taken at the same time in all secondary schools, drastically reducing the cost of applying to college for students located in regions faraway from major cities. One recurrent theme surrounding the introduction of the reform in fact has been the exigency of attracting young talented students from the countryside and peripheral areas to Russia's top metropolitan universities (e.g., Ampilogov, Prakhov, and Yudkevich, 2013). This is exactly the focus of our paper.[12]

3. Conceptual Framework

Here we develop a simple conceptual framework to guide the interpretation of our empirical findings. Since the focal point of the analysis is to understand whether and how the USE reform affects the likelihood that high school graduates leave home to enroll into a post-secondary education program, our setup is based on the important work on migration and education developed by Sjaastad (1962), Becker (1964/2009), and Mountford (1997).[13]

Suppose a large country is composed of two regions. The "core" region is where high-quality elite universities are located. The other region, which we refer to as the "periphery", has no college (or only lower quality institutions). Consider a student living in the periphery who is about to complete the last year of her high school education. She has to decide whether to apply to an elite university in the core region. Her objective is to maximize lifetime earnings, which depend on her ability, a, and her past human capital investments, x. Attending an elite college is costly. There are preparation costs involving personal time and effort, tutors, and the necessary materials for taking admission exams. In addition, since she lives faraway from the core region, there are costs associated with moving and settling

therefore not reported.

[11]The supplementary material in the Online Appendix (`https://sites.google.com/site/fabianslonimczyk/home/working-papers/use/Appendix_2017_02_27.pdf`) discusses issues related to enrollment and funding in greater detail.

[12]Another anticipated role of the reform is that, by moving the admission tests away from higher education institutions, the USE could eliminate (or greatly reduce) illegitimate practices associated with the old system. Moreover, the USE is also expected to affect the educational system preceding the entry into university, not only because student performance becomes a readily available indicator on which to rank high schools, but also because Russian and Math tests are mandatory requirements for high school graduation. This paper however cannot test whether the reform had an impact on these other margins. They represent interesting areas for future research.

[13]Other related models are elegantly discussed in Dustmann and Glitz (2011). See also Kennan (2015).

there while sitting for the exams. The total cost of applying is denoted by k.

In order to decide whether to apply, she compares the net benefits of both alternatives. The net benefit of pursuing an application and moving to the core region is given by $f(a, x, k)$, which is increasing in a and x and decreasing in k. We assume that the net benefit of not applying to an elite college and staying in the periphery is not affected by k and is thus given by $g(a, x)$. We also assume that the cost of the effort associated with applying to an elite college is decreasing in ability, hence the slope of f with respect to a is steeper than that of g.

This setup is sufficiently general to encompass a variety of cases, including corner solutions (either all students or none will apply), and multiple equilibria. The case with a unique interior equilibrium is shown in Figure 1, where the student will find it optimal to apply and eventually move if her ability is above a^*, whereas if the student has an ability level lower than a^* she will stay in the periphery.

Consider now the introduction of a reform that increases the net benefit of applying to elite colleges, as intended by the USE reform. This can happen either through a reduction in the costs k or through an increase in the benefits that accrue to those who move. There are many ways in which such a reform may change the equilibrium situation shown in Figure 1. We focus on two possibilities. In Figure 2a, the net benefit increase is enjoyed by all students irrespective of ability. As a result, the f curve shifts upward and the new critical level of ability, a^{**}, is to the left of pre-reform threshold, a^*. Applications to elite universities and student mobility will unambiguously increase. A second case is depicted in Figure 2b. Here the net benefit increase is concentrated among students with ability higher than a^*. In this case, then, the original (pre-reform) equilibrium is unaffected.

In summary, it is an empirical question whether a reform such as the one described in Section 2 will lead to an increase in student mobility. If ability is not uniformly distributed across the periphery, it is possible that the reform would have heterogenous effects. In particular, if a sub-region or a sub-set of the population in the periphery have ability levels below a^{**}, then no effect is expected to be found.

This analysis also clarifies that the resulting group of 'compliers' in our study is likely to be different from the compliers in other studies such as Bettinger et al. (2012), Pallais (2015), Carrell and Sacerdote (forthcoming). In our case, the compliers are those high school graduates for whom the perceived net return to schooling is relatively high (whose ability level is around a^*), whereas information nudging elicits changes in behavior among students who do not have (or find it costly to access) the salient information needed to enroll in a university program. We thus look at a potentially different margin of the population, which is closer to the one investigated by Goldin and Katz (1999) and Currie and Moretti (2003).

4. Data

The data we use in our empirical analysis are from the Russian Longitudinal Monitoring Survey (RLMS). The RLMS is a household survey based on the first national probability sample drawn in the Russian Federation.[14] We use all the 19 annual rounds available from 1994 to 2014 (except 1997 and 1999 when the survey was not conducted). The RLMS covers 32 "oblasts" (or states) and 7 federal districts.[15] As standard in most household panels, face-to-face questions about the household are answered by one household member, while each individual in the household aged 14 or more fills up the adult questionnaire.

The RLMS follows dwellings and not households or individuals. All adults in the same dwelling are reinterviewed each successive year. If they split from their original households to form new households in a different dwelling or if original households change address, they are followed up and interviewed as long as they remain in the same proximate geographic area. If a new household moves into an original sampled dwelling, this is incorporated into the panel and all adult members of this households are also interviewed. For individuals who leave their household of origin and cannot be followed in subsequent waves, we know the reason for leaving, as long as his/her relatives are interviewed. Up to the 2008 wave, there are only three possible reasons: (i) move to a different address; (ii) stay at the same address in a new household; and (iii) death. For our purposes, leaving home is identified with reason (i). From 2009 onwards, more detailed reasons are recorded, including one in which an adult household member has moved to study in another location. The exact place however is not specified.

Our sample consists of young adults who are unpartnered and childless, and coreside with at least one carer (parent or grandparent). We refer to our main outcome as leaving the "parental" home, although 4% of the individuals in the sample coreside with their grandparents. Individuals in the last year of high school are in the treatment group as they are at risk of leaving their household of origin. Their age is typically around 17 years, but there are cases of slightly younger or older individuals. The idea is to contrast their behavior with the behavior of other comparable individuals who are not in their last high school year. For robustness we consider three alternative control groups, i.e.: (a) a broad group of individuals

[14]The RLMS-HSE is conducted by the National Research University Higher School of Economics and the "Demoscope" team in Russia, together with the Carolina Population Center, University of North Carolina at Chapel Hill and the Institute of Sociology RAS. The RLMS website (`www.cpc.unc.edu/projects/rlms-hse`) contains extensive documentation and details on the sampling design.

[15]Oblasts and federal districts are the primary sampling units, which are in turn subdivided into 177 population centers. The RLMS gathers geographic and local information at the population center level.

aged 15–24; (b) a more select group of individuals aged 15–19; and (c) an even narrower group of individuals aged 15–19 who are full-time students. We shall refer to them as control groups (a), (b), and (c), respectively.[16]

The treatment group comprises between 53 and 166 students in any given year over the sample period for a total of 1,727 person-wave observations. The size of the nontreated group varies depending on the definition used. The smallest sample is when we use definition (c), with a minimum of 371 students and a maximum of 813 per year for a total of 10,849 person-wave observations. The largest control sample is produced by definition (a) with a number of individuals varying from 780 and almost 1,700 per year for a total of 22,289 person-wave observations.[17]

The USE reform was introduced in the 2009/10 academic year. Because most of the RLMS interviews are between October and December (and never before October), the first students at risk of leaving their parental home to enroll at university as a result of the USE reform are those attending the last year of high school in 2008/09. The reform-on period therefore refers to the years from the 2008 RLMS wave onwards, while the reform-off period covers all the preceding years.

Figure 3 shows the leaving-home rates for treatment and control groups over the whole sample period. High school graduates experience the greatest rates of leaving both before and after the reform. The high rates recorded in the middle of the 1990s up until 2000 might reflect the economic turbulence faced by Russia at that time, although the destination of high school graduates before then was not just college (see, for instance, Guriev and Vakulenko, 2015). The reform is followed by a sizeable jump in the probability of leaving home among high school graduates, increasing from an average of about 5% in the seven years preceding the reform to more than 12% over the post-reform period. The information collected by the RLMS after 2008 confirms that about 90–95% of this outflow is attributable to graduates who move out to enroll into a university program.

All individuals in any of the control groups display lower leaving rates than the students in the treatment group. But the time trend patterns, especially in the pre-reform period, are very similar to those observed for the treatment group. Moreover, the disaggregated post-reform information reveals that only a small fraction of the more modest increase in leaving-home rates among young adults in either of the control groups can be accounted for by individuals attending college.

[16]Besides comparability in terms of age (and accumulated life experiences) between individuals in the treatment and control groups, the selection of the three sets of comparators is driven by differential exposure to the risk of leaving home with the purpose of starting a new program of study. The differences in such an exposure before and after the reform are expected to be minimal for each of the three control groups.

[17]More details on the sample are available in the Online Appendix.

Table A.1 shows the summary statistics of the main explanatory variables by group, both before (reform-off) and after (reform-on) the USE reform. Both treatment and control groups are fairly similar along a number of characteristics, including household demographics, geographic dispersion across districts, and a few indicators of socioeconomic status. About 20–25% of the children in the sample do not live in an intact household where both parents are present. Irrespective of treatment or control groups, more mothers than fathers have a university degree especially after the introduction of the reform, possibly capturing the fact that slightly fewer fathers coreside with their children. In the period after the reform, a greater fraction of all children, regardless of the group, live with university educated mothers, reflecting a likely cohort effect on educational attainment. Approximately one in five young adults live in a family that owns a vacation house (dacha), and another 6–10% have an extra apartment. This latter fraction has increased between the period before and the period after the introduction of the reform, as has the car ownership rate, suggesting an improvement in the living standards over this time period. This was also accompanied by a substantial fall in transportation costs (both to Moscow and to each state capital) and a moderate reduction in local youth unemployment rate.[18] The average position in the income distribution has instead changed little over time.

Despite the similarities, we also observe some important differences between treatment and control groups. For example, students in the treatment group are younger, although unsurprisingly the difference becomes smaller and insignificant when we move from control group (a) to (c). A larger fraction of individuals in the treatment group are women. Treated students are also less likely to be of Russian ethnicity, more likely to be in a household with the father present, and more likely to have parents with a greater level of education than any of their control group counterparts. Because of these differences, we include all such controls in the analysis.[19]

As emphasized in the Introduction and Section 2, one objective of the USE reform was to encourage high school graduates from peripheral areas to move to prestigious universities, which by and large are located in Moscow, St. Petersburg, and other major cities. Location therefore is expected to play an important role, and this in turn is underlined by the way in which we set up our framework in

[18]Using the community survey of the RLMS, transportation costs are measured at the population center level (177 centers) and deflated with the Consumer Price Index (base=2013). The unemployment rate is computed using the Russian Labor Force Survey on individuals aged 16–25 years for each participating Oblast, and separately for rural and urban areas. As a result, we have 62 distinct unemployment rates per period.

[19]To assess whether the baseline estimates are sensitive to differences in all observables between groups and over time, we also check them against alternative estimates obtained from a matching procedure. We anticipate that the baseline results are robust. See subsection 6.D.

Section 3. In the empirical analysis we distinguish four areas, which are strongly correlated not only with population size but also with the availability of university programs. The first includes Moscow, St. Petersburg, and their metropolitan areas within a 50 Km radius. Across all groups, this category, which has an average population of about 8.5 million people over the sample period, covers approximately 10–11% of the sample and counts at least 200 universities and most of the elite colleges in the country. The second area, which covers 27–30% of the sample, refers to all the other major cities and their surrounding areas within a 20 Km radius.[20] The mean population per city within this area is 700,000 and each city has at least one higher education institution. The third area gathers individuals who live in small cities and towns, has an average population of approximately 100,000 individuals per location, and identifies about one-quarter of the sample. The last location refers to rural areas, which include smaller towns and villages, with a population of about 5,000 people per location. A slightly larger fraction of individuals in the treatment group live in rural areas than their control counterparts. None of the latter two categories (small cities and towns and rural areas) have universities and thus, in the context of the framework outlined in Section 3, can be thought of as our "periphery".

5. Methods

To assess the impact of the USE reform on the likelihood that high school graduates leave their household of origin and enroll into a university program, let d_{ijt} denote a dummy variable that is equal to 1 if individual i in household j attends the last year of high school at time t and 0 otherwise, and let s be the time period in which the USE reform occurs, i.e., $s = 2009$ (which, following the discussion in the previous section, means that high school students are affected by the reform in the 2008 RLMS wave).[21] We model the outcome y_{ijt}, which is a dummy variable equal to 1 if individual i moves out of household j within one year after the interview at time t and 0 if the individual continues to coreside with his/her parents, as being determined by the following specification

$$y_{ijt} = \psi(t) + \alpha d_{ijt} + \beta d_{ijt} I(t \geq s) + \mathbf{X}'_{ijt}\gamma + \theta_j + \varepsilon_{ijt} \tag{1}$$

where $I(z)$ is a function indicating that the event z occurs, $\mathbf{X}_{ijt}$ is a vector of individual and household characteristics, θ_j denotes household (siblings) fixed effects,

[20] The different radius around such centers as opposed to Moscow and St. Petersburg is due to the geographic nature of the respective commuting zones and the availability of local public transports.

[21] As mentioned in Section 2, we cannot take advantage of the variation across time and states in the adoption of the Unified State Exams by high schools, because universities did not determine their admission criteria based on the USE until 2009.

and ε_{ijt} is an error term that is contemporaneously uncorrelated with the variables in the right-hand side of (1).

The term $\psi(t)$ is an expression that captures time trends. In the analysis, we shall use two alternative specifications. In one we assume $\psi(t) = \tau_t$, i.e., we include a fully flexible set of time dummies that are common to treatment and control groups. Indeed, the leaving-home patterns shown in Figure 3 lend support to this common trend assumption.

In another specification we impose $\psi(t) = \delta_0 + (\delta_1 + \delta_2 d_{ijt})t + [\delta_3 + \delta_4(t-s)]I(t \geq s)$. This specification not only allows for different intercepts (when $\alpha \neq 0$) but also for different linear trends (when $\delta_2 \neq 0$) for individuals in the treatment and control groups. The parameters δ_3 and δ_4 identify possible shifts in the intercept and slope of the process generating y at the time of the reform. For instance, they measure the effects of other non-USE policy changes that occur at s and might influence the likelihood that high school graduates have to leave their parental home. While we are not aware of other federal educational programs introduced at that time, there might have been local changes in the education system or the labor market that affected y for individuals in the treatment and/or the control group. To the extent that these shocks are common to treatment and control groups, they will be picked up by our time and geographic controls, including changes in the local youth unemployment rate and transportation costs. By assuming that high school graduates would have responded to other (unobserved) potential changes in the same way as individuals in the control group, we are able to net out the impact of the USE reform, which is captured in equation (1) by β.

It is important to point out the main identification condition underlying our approach. We explicitly assume that, other than the introduction of the USE reform, there are no contemporaneous shocks that affect the *relative* outcome of the treatment and control groups. As mentioned, we do not know of any related program that could have induced such differential responses.[22]

The group-specific summary statistics presented in the previous section suggest there are differences in some observables between treatment and control groups (e.g., gender, ethnicity, and parental education). Besides controlling for a large set of explanatory variables, we also attempt to deal with this issue using a two-step procedure. This combines the difference-in-difference estimator with a matching method that pairs each treated individual with a subset of individuals in the nontreated group who are closest to him/her in observable characteristics. The weights needed in this procedure are computed using propensity score matching

[22]Other major reforms in post-transition Russia include the 2001 flat tax reform and the 2007 maternity capital policy. Albeit important, neither of such reforms would have affected our treatment and control groups differently (see Slonimczyk, 2012, Slonimczyk and Yurko, 2014).

in a first stage,[23] while the second stage is estimated using weighted least squares and a full set of time dummies (Blundell and Costa Dias, 2009).

The exposure to the risk of leaving the parental home is likely to be different for high school students in their last high school year as opposed to the other (untreated) individuals. As emphasized in Section 4, we gain some understanding of the importance of this differential risk by using three different control groups, with slightly diverse overlaps in age and school involvement. But to better account for the potentially different right censoring faced by treated and untreated individuals, we estimate discrete time duration models as well as Cox proportional hazard models on a sample of RLMS adults aged 14–25 who are initially observed coresiding with their parents or grandparents. This additional analysis will provide us with further evidence about the robustness of our benchmark results.

6. Results

A. Benchmark Estimates

Table 1 shows the estimated impact of the USE reform on the probability of leaving the parental home separately for each of our three control groups. We present the results from a linear probability specification of model (1) in which we impose a fully flexible set of time dummy variables common to treatment and control groups.[24] Standard errors are robust to arbitrary forms of heteroscedasticity and are clustered at the population centre level.[25] Column (i) reports our baseline results with no fixed effects ($\theta_j = 0$) and no other control variable besides the time trend; the results in column (ii) are obtained after including the full set of controls, while the estimates in column (iii) are found from the model that also accounts for fixed effects shared by siblings living in the same household.[26]

Focusing on the estimates in column (i) we find that, irrespective of the control group, the rate at which high school graduates leave their parental home increased significantly by about 4 percentage points after the introduction of the USE reform. This is a large effect, representing a 55% increase of the leaving-home rate averaged

[23]The propensity scores are the predicted probabilities obtained from a logit model where the outcome variable is $d \times I(t \geq s)$ and the right-hand side variables are all the covariates listed in Table A.1. We use a kernel matching technique with an Epanechnikov kernel and a 0.05 bandwidth.

[24]The marginal effects from a probit specification are virtually identical to those shown in Table 1 and are thus not presented.

[25]Clustering at the individual level leads to the same results.

[26]Standard errors are clustered at the population centre level. The results in Table 1 and the subsequent tables are essentially unchanged if we cluster either at the state level or at the individual level. Such estimates are not presented.

over the entire pre-reform period. Adding the set of observable determinants in column (ii) reduces the USE effect just slightly to 3.7 and 3.5% when we use control groups (b) and (c) respectively. This evidence suggests that the potentially different risk of leaving home between individuals in the treatment group and those in each of the three control groups is likely to play only a small role in affecting our estimates. We shall come back to this point in subsection 6.D.

Including siblings fixed effects in column (iii) leads to effect estimates that are identical to those reported in column (i), the only exception being the 3.8 percentage point impact obtained when using control group (c) which is statistically significant only at the 10 percent level. The strong similarity between level and FE estimates indicates that the impact of unobserved determinants of mobility shared among siblings in the same household is possibly quite modest.

To assess the potential bias arising from the exclusion of unobservables more precisely, we use the methodology developed by Altonji, Elder, and Taber (2005) in the implementation of Nunn and Wantchekon (2011). To compute the Altonji-Elder-Taber (AET) statistic we use the treatment effect estimate without controls given in column (i) as the estimated coefficient from the restricted model (β_R) and the effect estimate in column (ii) as the coefficient from the full model (β_F). The AET statistic $\frac{\beta_F}{\beta_R - \beta_F}$ will tell us how much stronger the selection on unobservables must be with respect to the selection on observables to explain away the entire effect of the USE reform on student mobility. When using control groups (b) and (c) we find large ratios (8.8 and 13.9): this suggests that, to attribute the entire effect of the USE reform on student mobility to selection, the selection on unobservables would have to be at least 8.8 times stronger than the selection on observables, which seems unlikely.[27]

B. Estimates by Location of the Family of Origin

As discussed earlier, one of the intended consequences of the USE reform is the substantial cost reduction in applying to prestigious institutions (located in Moscow, St. Petersburg, and some of the other major metropolitan centres) for all high school graduates but especially for those who live far from the main cities or in peripheral areas. We expect therefore to find heterogeneous responses depending on where individuals (and their parents) are located. To assess this, we estimate a variant of equation (1) in which the treatment variable, d, the time trend terms, $\psi(t)$, and the interaction between d and the post-reform time period are interacted with our four household location indicators (Moscow/St. Petersburg, other major

[27]The AET ratio is -44.9 in the case of control group (a). A negative value of the AET statistic means that our estimated treatment effect is a downward biased estimate of the true USE reform impact, as long as the selections on unobservables and observables are positively correlated.

cities, small cities and towns, and rural areas). The corresponding effect estimates are reported in Table 2.

The estimates indicate that the introduction of the reform did not affect the probability of moving for high school graduates living in Moscow and St. Petersburg (first row of the table). This is not surprising given most of the top universities are located in those two cities and students can choose among more than 200 institutions there. The reform also did not have any impact on students living in the least populated rural areas (fourth row). We shall come back to this result at the end of this subsection.

Instead the USE reform had a positive effect on young adults living in the major metropolitan centers other than Moscow and St. Petersburg. This effect, which is significant only at the 10 percent level, is quantitatively similar to what we found earlier from the benchmark analysis for the entire sample, i.e., an increased probability of about 4 percentage points, a four-fold increase with respect to the pre-reform leaving-home rate of high-school students in major cities. This effect however loses statistical significance when we account for fixed unobserved heterogeneity shared between siblings in the same household (see column (ii) irrespective of the control group).

The largest impact is observed among students who are located in small cities and towns. They experienced a significant jump of about 12 percentage points in the likelihood of leaving their parental home after the USE reform, an increase of more than three times over the average leaving-home rate among high school students from small cities and towns in the pre-reform period. This effect emerges regardless of the control group and whether siblings fixed effects are included or not.[28]

The framework developed in Section 3 provides a straightforward interpretation of why high school graduates from the periphery (small cities and towns) have the greatest demand for college enrollment. Russian towns and small cities do not have higher education institutions. In the pre-reform regime, therefore, most students in such locations could hope to gain a college education only if they were willing to bear the high costs associated with the admission process, i.e., collecting the appropriate preparatory material, attending pre-sessional modules *in-situ*, and taking and passing the admission test in one specific program. Repeating the whole process for another program would have been prohibitive in terms of both time and financial resources. Imposing the USE test results as the default criterion for college admission essentially eliminated most of such costs. Consistent with the

[28]In addition, we find AET statistics ranging between 30 and 100, depending on the control group used. This indicates that the selection on unobservables would have to be at least 30 times larger than the selection on observables in order to attribute the whole effect of the reform to selection itself. Arguably, this is implausible.

story behind Figure 2a, if the USE reform generates a net benefit increase for all town-based students, the $f(\cdot)$ curve shifts upward implying that applications to (top) university programs as well as student mobility go up.

Of course, mobility can only occur if the USE test results are sufficiently good. Using RLMS data that are available only over the post-reform period, we find that high school graduates from small cities and towns have among the best USE scores in the country, second only to those achieved by students in Moscow and St. Petersburg. Conversely, students from rural areas have the lowest average USE scores. If this held also before the reform, it could explain why we do not find an effect on the leaving-home probability among high school graduates from rural areas, although their incentive to attend top college programs should be relatively similar to that of town-based students. The quality of secondary schools in rural areas is likely to be deficient to equip their pupils with the adequate skills to enter elite (core) programs.[29]

It is worthwhile noting that families in small cities and towns are on average poorer than families living in major cities and particularly in Moscow and St. Petersburg.[30] By inducing a greater fraction of (poorer) students from small cities and towns to enroll into university programs than what would have happened otherwise, the USE reform was successful in promoting greater mobility from the bottom of the parental income distribution. It is however too early to see whether this greater geographic mobility translates into greater social mobility; this is left for future research. But this sort of geographic mobility is an important stepping stone to the achievement of greater social equality.

C. Estimates with Group-Specific Trends and College Attendance Intentions

The findings presented so far are obtained under the assumption of a fully flexible time trend that is common to treatment and control groups. We check whether the results by location are robust to the inclusion of group-specific time trends,

[29]Evidence on USE scores by location is presented in the Online Appendix, together with pre-reform mobility rates. Interestingly, the leaving-home rates from rural areas were already relatively high even before the reform. In terms of Figure 1, this means that the critical level of ability, a^*, in rural areas was already to the left of the corresponding threshold in small cities and towns. Inducing extra mobility in rural areas therefore is relatively harder.

[30]For instance, the median income rank is 0.783 for households in Moscow and St. Petersburg, 0.582 for those in other major cities, 0.447 for families in small cities and towns, and 0.330 for those in rural areas. These figures are similar if we focus only on the pre-reform period or on mean (rather than median) income ranks.

requiring us to estimate (1) with $\psi(t) = \delta_0 + (\delta_1 + \delta_2 d_{ijt})t + [\delta_3 + \delta_4(t-s)]I(t \geq s)$.[31]

The estimates in Table 3 confirm those shown in Table 2. The only exception is that the level effect on students from the major metropolitan centers other than Moscow and St. Petersburg is no longer statistically significant (second row). This adds to the lack of significance of the FE estimates which we had already found when imposing a flexible common trend in Table 2.

Allowing for group-specific trends, however, leads to strong treatment effect estimates of 16–18 percentage points for students from small cities and towns (third row), implying an increase in the leaving-home rate of about four times over the pre-reform location specific average rate. Such an impact is large, and it is found consistently across all three control groups and irrespective of whether or not we account for siblings fixed effects. These results offer a clear back up to the story we have documented so far.

An additional piece of evidence that would lend support to our interpretation may be given by stated intentions to attend college among all secondary school students. The argument is simple. If the USE reform did change opportunities, this change would be reflected also in students' expectations about the likelihood of their college education. Thus, not only would the reform lead to greater mobility among high school graduates from small cities and town but it would also heighten their intention to attend a university program while they are still in high school.

To assess this possibility we use data collected by the RLMS since 2006 on the intention that young people have to enroll into a university program during the next three years. High-school students aged 14–19 are in the treatment group, while the control group consists of all other students aged 14–19, essentially attending technical and vocational secondary institutions which do not prepare for college education and do not provide qualifications based on USEs. About 35% of the 14–19 year old students in our sample are in this treatment group. Our outcome variable takes value 1 if individuals state they have intention to study at a university in the next three years, and zero otherwise.[32] With this new dependent

[31]This specification is a straightforward extension of the difference-in-difference-in-difference (DDD) estimator. Ignoring individual characteristics and fixed effects, it is easy to see that the treatment effect, β, is identified by the DDD estimator defined as

$$\begin{aligned}
\text{DDD} &= \big\{ [E(y_{ijt} \mid d_{ijt}=1, t=s-1+k) - E(y_{ijt} \mid d_{ijt}=1, t=s-1)] \\
&- [E(y_{ijt} \mid d_{ijt}=1, t=s-1) - E(y_{ijt} \mid d_{ijt}=1, t=s-1-k)] \big\} \\
&- \big\{ [E(y_{ijt} \mid d_{ijt}=0, t=s-1+k) - E(y_{ijt} \mid d_{ijt}=0, t=s-1)] \\
&- [E(y_{ijt} \mid d_{ijt}=0, t=s-1) - E(y_{ijt} \mid d_{ijt}=0, t=s-1-k)] \big\},
\end{aligned}$$

where s is the period the reform is introduced and k indicates the length of time periods over which the differences are computed. See also Francesconi and van der Klaauw (2007).

[32]The RLMS records this information into a binary (0/1) variable. Although this expectation

variable, we then repeat the analysis performed earlier stratified by location, with a set of specifications that allow for a flexible common trend and others that allow for group specific trends.

Table 4 shows the relevant estimates. The introduction of the USE reform does not change the intention to attend a university program among students from all locations except those from small cities and towns. Among them, the reform led to an increase in college attendance intentions by 21–24 percentage points if a common flexible trends is imposed and by 31 percentage points if we allow for group-specific trends. These estimates imply a substantial increase of 40–60% with respect to the average stated intentions of high-school students from small cities and towns. This evidence is fully consistent with the earlier results from actual choices. The USE reform not only induced more students from the periphery to enroll into a college program when they completed high school, but it also increased their expectation that they attend university while still in high school.

D. Robustness Checks

In what follows we present further results from three important exercises to check the robustness of the estimates shown in Tables 2 and 3. The first consists of a falsification test, which takes advantage of the long time span covered by the RLMS data. We impose a placebo reform in 2002 and exclude the actual post-reform period, which refers to the RLMS waves from 2008 up to 2014. The results from this analysis are in Table 5. Across all four location domains and irrespective of the specification, the treatment effect estimates are not only statistically insignificant but also quantitatively small. Although placebo tests cannot be definitive, these results provide support to our identification strategy.

With the second exercise we account for the differences in observable characteristics across individuals in the treatment and control groups described in Section 4 using a two-step propensity score matching technique. For each of the three control groups separately, Table 6 reports the estimates stratified by location. These results are in line with those shown in Table 2. Students in Moscow and St. Petersburg, and those from rural locations do not experience any change in their likelihood of leaving the parental home after the introduction of the USE reform. Students from major metropolitan centers other than Moscow or St. Petersburg

is elicited with a non-probabilistic method, rather than asking people to express probabilities directly (Manski, 2004), it is likely to suffer less from the standard problem of variables constructed from Likert scales that make it difficult to perform interpersonal comparisons, as different respondents may interpret the scales differently (McFadden et al., 2005). In our case, in fact, the distinction between intention to attend and intention not to attend is very sharp.

see their odds of leaving home after high school graduation increased by about 5 percentage points (although this effect disappears when control group (c) is used). But graduates from small cities and towns are estimated to experience the highest jump in leaving-home rates of approximately 11–13 percentage points. This evidence suggests that the potential bias due to differences in observables between treatment and control groups is likely to be negligible.

Third, we check whether our benchmark estimates are sensitive to the potential bias due to the differential exposure to the risk of leaving the parental home between treated and untreated individuals. The differential exposure might be driven by sampling design issues (e.g., parents and children observed only once cannot be assigned a mobility status in our benchmark analysis) or differential attrition bias (e.g., older children might be more difficult to identify even if they coreside with their parents).[33] For this purpose, we estimate discrete time duration models that combine both time varying covariates and flexible specifications of duration dependence (Jenkins, 1995).

The marginal effect estimates are shown in Table 7. They are obtained from logit models with the same set of explanatory variables used in Section 6.B. Standard errors are clustered at the population center level and are computed using the delta method. For each of the three different control groups, the estimates confirm the results reported in Table 2. In particular, there is no impact on the hazard rate of moving among high school graduates in Moscow and St. Petersburg as well as among those from rural areas. Again, we find evidence of a 3–4 percentage point increase in the hazard of leaving the parental home among graduates in other major metropolitan centres. We know, however, that this relationship is sensitive to the inclusion of siblings fixed effects and group specific time trends.

As before, the greatest increase is found among graduates living in small cities and towns and is estimated to be between 7 and 9 percentage points. Although these figures are smaller than those in Table 2, they provide the same picture described earlier: by reducing the cost of applying to (elite) colleges, the USE reform induces more high school graduates from the periphery to increase their demand for post-secondary education, and this in turn translates in greater mobility rates among periphery-resident graduates. Repeating the same exercise using Cox proportional hazard models (reported in the Online Appendix) leads exactly to the same conclusions.

We take the three sets of results reported in this subsection as evidence that our benchmark estimates by location are robust and provide clear support to the case put forward in our study.

[33]For interesting insights about attrition issues in panel data, see, among others, Fitzgerald, Gottschalk, and Moffitt (1998).

E. Response Heterogeneity

Here we ask whether the USE reform had heterogenous effects on students' mobility along a number of observable characteristics. In particular, we investigate the possibility of differential responses by gender, ethnicity, earlier household mobility status, household income, and family structure. Because the lion's share of the USE reform effect is driven by high school graduates from small cities and towns, the estimates in Table 8 refer exclusively to them. For completeness, however, we have performed the same analysis on individuals from the other three locations in the sample and found no statistically significant effect difference. For the sake of brevity, therefore, such results are not discussed here but are available in the Online Appendix.

Table 8 shows that there is no response heterogeneity by gender and ethnicity, regardless of the control group used in the analysis and whether we account for unobserved siblings fixed effects (FE estimates) or not (level estimates). This is interesting, for the treatment group has a larger share of women and ethnic Russians than any of the control groups (see Appendix Table A.1).[34] We also do not detect differential treatment effects in the top half of the household income distribution as opposed to the bottom half.

Instead, we find a substantially *lower* impact among individuals who were born in a location other than small cities and towns (where they are observed at the time of the reform). Earlier household mobility is thus inversely correlated with subsequent mobility away from the family of origin, and essentially offsets the benchmark impact of being located in small cities and towns. This differential impact weakens and becomes statistically insignificant at conventional levels when we account for siblings fixed effects.[35] In general, we can claim that the reform had a stronger positive impact on the mobility rate of students who were born and bred in the same small city or town.

Finally, having parents with university qualifications *increases* the probability that high school graduates from small cities and towns leave the parental home by about 16–21 percentage points, while graduates whose parents are not college educated have a leaving-home rates of 10–11 percentage points. The difference of 5–10 percentage points is statistically significant, but it is not robust to the inclusion of fixed effects. We interpret this difference as indicating a non-negligible positive intergenerational correlation in university education, in line with the findings reported in Borisov and Pissarides (2016). An implication of this result is that it

[34]Reassuringly, this is consistent with the results displayed in Table 5 and discussed in the previous subsection.

[35]In part, this is because only one in four individuals in the sample had experienced earlier geographic mobility, and thus the statistical power here is likely to be low.

provides evidence that, *within* small cities and towns, the reform might have done little to level the playing field for disadvantaged children in terms of improving their access to higher education. As mentioned before, however, across the whole country the reform did induce greater mobility among students from relatively poor backgrounds (living in small cities and towns) as opposed to students from large metropolitan centers who are richer on average.

F. Other Outcomes

Having established that the USE reform has induced a substantial mobility of high school graduates out of their parental home, especially in small cities and towns where the demand for post-secondary education cannot be satisfied locally, we ask whether the reform has led to other (perhaps unintentional) responses.

For this purpose we analyze a wide range of outcomes, i.e., monetary transfers from parents to nonresident children, household expenditures and their composition, parents' labor supply, and divorce, and spillover effects.[36] We focus on the effects observed among families located in small cities and towns. For families in all other locations we do not detect any significant effect (see the supplementary material in the Online Appendix). This is not surprising since in locations other than small cities and towns we also find no USE impact on high school graduates' mobility, which is the most obvious candidate outcome to be affected by the 2009 reform.

Parental Transfers — If the leaving-home decision of high school graduates is followed by college attendance (and this implies a physical relocation in a different city center), then arguably migrant students remain financially dependent on their parents to a greater extent than if they left to start a job. We thus analyze the probability that parents make a money transfer to nonresident children and re-estimate equation (1), in which the dependent variable takes value 1 if the transfer is observed in any of the following two years, and 0 if no transfer occurs.[37]

[36]With the exception of spillover effects analyzed at the end of this subsection, all the other outcomes are measured at the household level. See the Online Appendix for details on how we link households across RLMS rounds. A household is considered treated if there is at least one child in the last year of high school. Control households have at least one child that serves as a comparison and no children in the last year of high school. We use the characteristics of the treated/comparison child to define our explanatory variables.

[37]We also redefined the outcome as receiving a transfer within one year, and found similar results to those shown below. In addition, we estimated the effect on transfer levels conditional on observing a positive transfer. These effect estimates are never statistically significant, possibly reflecting problems of measurement error as well as selection issues, given that only 15% of households report positive transfers on average. The full set of results is shown in the Online Appendix.

Table 9 reports the regression results by specification with a full set of controls, and with a fully flexible time trend that is common to treatment and control groups (first three columns) or treatment specific trends (last three columns). The results in columns (i)–(iii) show that high school students from small cities and towns who leave the parental home after graduation are between 13 and 15 percentage points more likely to receive financial transfers from their parents over the two years after leaving as a result of the USE reform. This is a large impact, corresponding to a 40–50% increase at the mean transfer probability. The estimates in columns (iv)–(vi) are even larger, with the probability that parents make a transfer to their nonresident children going up by 19–23 percentage points.

These results confirm that, after the implementation of the USE reform, high school graduates in small cities and towns do leave their family of origin to continue their studies in post-secondary education institutions. This decision, which is likely to require a costly relocation away from the parents' home, is accompanied by higher financial transfers from parents to children.

Household Expenditures — Changes in the pattern of parental transfers to children may affect other aspects of family behavior related to the allocation of financial resources within households. One of such aspects are household expenditures. The RLMS collects detailed data on expenditures on food and other nondurables, clothes, and durables. We first aggregated all items and analyzed the impact of the reform on total household expenditure levels. We detected no effect (see the estimates in the Online Appendix). This may not be surprising given there might be issues of measurement error. We then analyzed household shares in major expenditure aggregates, such as durables, food, and other nondurables. Again, we found no effect induced by the USE reform.

The only exception to this result emerges in the case of household expenditures on education. The estimates in panel B of Table 9 show the USE reform effect on the share of educational expenditures in the total nondurable household expenditures. From the first three columns of the table (in which we impose a fully flexible common trend), we find that the reform led to an average increase of about 2.2–2.3 percentage points in the share of educational expenditures. This is a considerable impact, as it represents a 70% increase with respect to the sample mean of approximately 0.032. This result, however, is not robust to the inclusion of group specific trends (columns (iv)–(vi)).

Despite the lack of statistical significance in this last set of regressions, such estimates help us clarify the picture. Not only are families located in small cities and towns more likely to see their children leave after high school graduation as a result of the USE reform. They are also more likely to make financial transfers to their children and divert some of their finances to educational expenditures after

graduation. All these pieces of evidence point to the USE reform as a catalyst of change within households, triggering greater geographic mobility among high school graduates from the periphery to post-secondary education institutions, and inducing more parental investments in, and transfers to, migrant children.

Parental Labor Supply — Larger monetary transfers to children and higher household educational expenditures could require a greater labor market involvement of parents who plan to support their children after high school graduation and into their university careers.

Father's labor supply is generally believed to be inelastic, although for males in general a sizable minority of studies have recently found nonnegligible elasticity values (see the extensive review in Keane, 2011). Perhaps unsurprisingly, the results in panel C of Table 9 show that father's monthly hours of work did not change with the introduction of the USE reform. Allowing for group-specific trends leads to larger positive effect estimates, corresponding to an increase of about 15% over the monthly mean hours (columns (iv)–(vi)). But none of these estimates is statistically significantly different from zero. The impact on paternal labor force participation is also small and statistically insignificant (panel D).

Female (and maternal) labor supply elasticities, especially on the participation margin, are typically believed to be large (e.g., Blundell and MaCurdy, 1999, Keane, 2011). One therefore might expect to observe some change in mothers' labor market behavior. Instead, the estimates in panels E and F of Table 9 reveal essentially no change in hours worked and in labor force participation among mothers in small Russian cities and towns. This zero-effect result could reflect modest or insufficient labor market opportunities in the local economy in the post-reform years. But they could also be interpreted as offering evidence that the USE reform did not create unanticipated labor market responses among parents of treated individuals.

Divorce — Another dimension of family life that the USE reform could have unintentionally affected is family stability. To assess whether this is the case or not, we examine the probability that parents separate from one interview to the next and re-estimate equation (1) in which the dependent variable is equal to 1 if either of the parents leaves the household between two successive RLMS rounds, and 0 otherwise. The effect estimates are in panel G of Table 9. Regardless of the control group used and irrespective of the way in which time trends are modeled, the estimates show that the USE reform did not significantly affect family stability.

Spillover Effects — Besides the families of high school graduates who left their parental home, the USE reform might have also affected high school graduates who decided not to move, at least immediately after graduation. To ascertain

this potential impact, we estimate multinomial logit regressions using the sample of stayers in small cities and towns who keep coresiding with their parents. The outcome is a three-level categorical variable: the first level includes any sort of paid labor market involvement, the second refers to studying (in "local" further education institutions), and the third is a residual category that comprises unpaid activities and, primarily, inactivity.

Panel H of Table 9 reports the results that show marginal probability changes for work and study with respect to the base category (i.e., inactivity). Regardless of the control group and how time trends are modelled, we find a small increase in the probability of working (between 1 and 6 percentage points) and a decrease in the probability of continuing study in further education (between 6 and 8 percentage points). Both sets of impacts are statistically insignificant. The reduction in the likelihood of studying (albeit insignificant) should be interpreted in terms of the lack of post-secondary educational provision in the periphery: if people in small cities and towns intend to continue studying after high school completion, they ought to do so by moving to larger cities.

It appears, therefore, that the reform did not generate negative spillover effects on the activities performed by young adults who stayed with their parents in small cities and towns. Of course, there might be other (longer term) general equilibrium effects on the subpopulation of stayers that we do not consider here (e.g., fertility, health, and crime). These are important, policy-relevant areas, which are left for future research.

7. Conclusion

This paper provides the first evidence of the effect of the 2009 reform that required all universities in Russia to determine their admission decisions on the basis of the results from the national high-school test, known as Unified State Exam. The USE-based admission criterion does not impose costs to college applicants other than those related to taking the USE test itself. The pre-reform rules instead required applicants to sit in expensive preparatory courses and incur the cost of travelling to examination venues, which would have been prohibitively high for most applicants located in remote areas far from major city centres, such as Moscow and St. Petersburg, where most of the elite institutions were (and still are) based. Our estimates indicate that the reform induced a substantially greater mobility among high school graduates living in small cities and towns. These are exactly the places most likely to be characterized by a pent-up demand for college enrollment, since small Russian cities and towns do not have universities and their high schools are generally of high quality. High school students from small cities and towns were also the only ones who, after the introduction of the USE reform,

showed a greater intention to attend university. The reform therefore might have changed their expectations.

These results are robust to different specifications of temporal trends (i.e., imposing highly flexible common trends or allowing for group specific trends), the inclusion of a wide set of observable confounders as well as unobserved fixed effects that are shared among siblings. The same findings also emerge consistently when we combine difference-in-differences with a propensity score matching procedure, which pairs each treated individual with a subset of nontreated individuals who are closest to him/her in terms of observable characteristics, and when we estimate discrete time duration models and Cox proportional hazard models of leaving the parental home after graduating from high school. All these pieces of evidence therefore point to the USE reform as the key force behind the greater mobility rate among high school graduates from small cities and towns who move from their families of origin to enroll in university programs in major urban centers.

Families located in small cities and towns are also more likely to make financial transfers to their children after graduation and use their resources to increase the share of educational expenditures as a result of the reform. These results are likely to reflect greater child investments among families whose children are in completion of their high school studies and start higher education. We also find no evidence of unintended or detrimental consequences of the reform on a wide range of family behaviors, such as all items of expenditures other than education, paternal and maternal labor supply (along both the extensive and intensive margins), and parental separation. The lack of a response in such domains suggests that the USE reform did not induce changes in parental behavior other than in aspects that are educationally salient to children at the end of high school and in the early years of college.

Our results can be explained by the substantial reduction in college application costs that the USE reform implied. Pre-reform application procedures imposed a costly barrier that prevented high school graduates located in peripheral areas from optimally investing in their human capital. Their removal induced a greater fraction of students from the periphery to apply to, and attend, university programs.

Since the average high school graduate from small cities and towns is substantially poorer than the average student from major urban centers, the USE reform seems to have gone some way in facilitating the transition to post-secondary education among low-income students. In essence, this result is similar to those found, among others, by Goldin and Katz (1999) and Currie and Moretti (2003) who argue that greater college availability reduces college costs and thus increases access and enrollment. In many respects, our findings are also comparable to the results illustrated in Bettinger et al. (2012) and Pallais (2015). But differently from their

nudging experiments, a reform that imposes all universities to base undergraduate enrollment decisions on the results from a national test like the Unified State Exam in Russia is not a "small difference". This is arguably a fundamental change in defaults that is likely to have large effects on welfare, as it is the case in other settings, such as employee retirement savings plans (Beshears et al., 2008), public school choice (Hastings and Weinstein, 2008), and bequest intentions (Francesconi, Pollak, and Tabasso, 2015). Future work will build on our results and assess whether the USE reform affects later outcomes (such as university graduation and scores, labor market participation, occupational choice, and wages among students who moved), or if it induces undesirable spillover effects.

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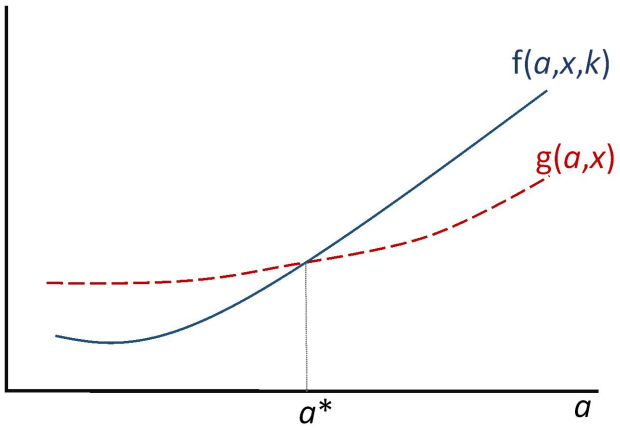
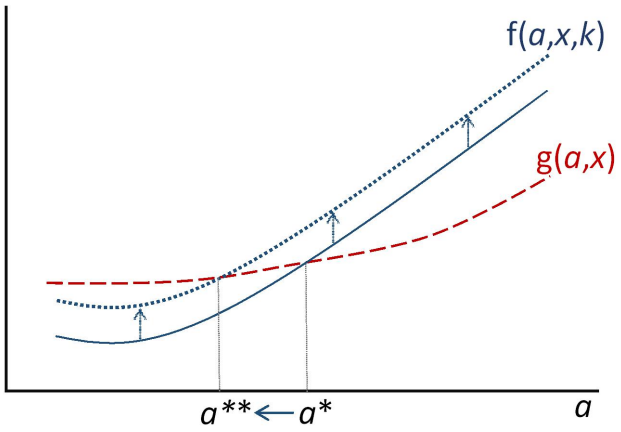
Figure 1 – The Decision to Apply to an Elite College

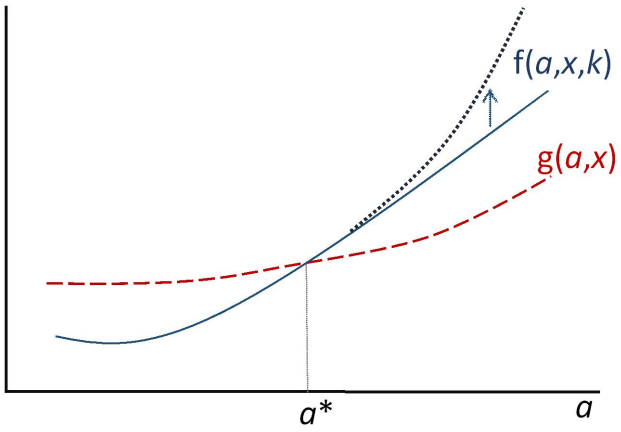


Note: The term a on the horizontal axis denotes ability. The functions $f(\cdot)$ and $g(\cdot)$ are described in the text.

Figure 2 – The Decision to Apply and Migrate to an Elite College in the Presence of the USE Reform

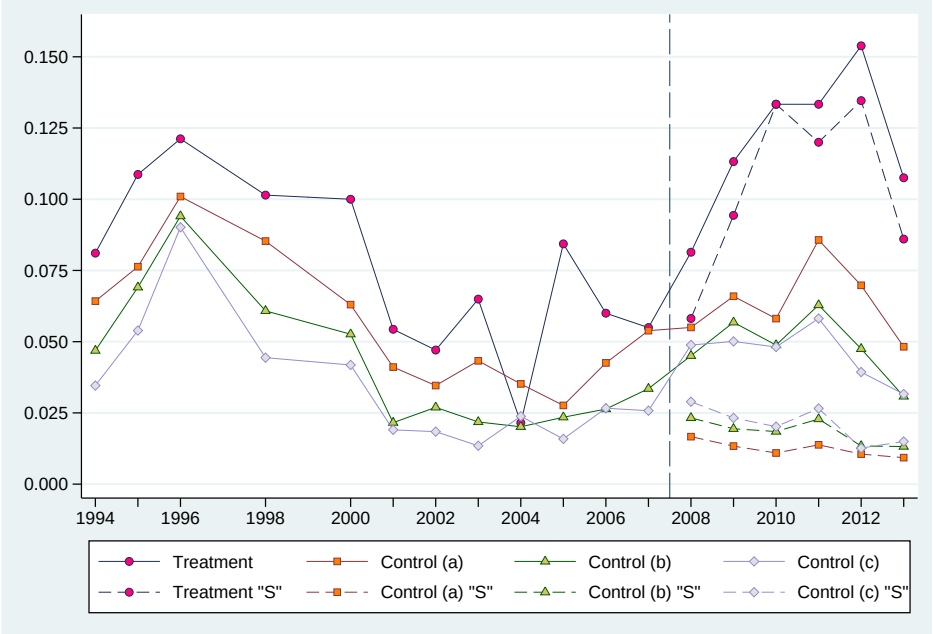


(a) Case with Mobility



(b) Case with No Mobility

Figure 3 – Leaving-Home Rates Among Young Adults



Note: The figure shows the fraction of children who leave their parental home to another dwelling within one year of interview. The "S" series (dashed lines) refer to the fraction of individuals who leave their parental home to another dwelling with the purpose of starting a new program of study (only available from the 2008 RLMS wave onwards). The vertical line indicates the introduction of the USE reform in terms of the data reported in (and used from) the RLMS.

Table 1 – Effect of the USE Reform on Student Mobility

	(i)	(ii)	(iii)
Control Group (a)			
β	0.040**	0.041**	0.047***
	(0.018)	(0.017)	(0.017)
N	21,678	21,678	21,678
Control Group (b)			
β	0.041**	0.037**	0.042**
	(0.018)	(0.017)	(0.018)
N	13,710	13,710	13,710
Control Group (c)			
β	0.037**	0.035**	0.038*
	(0.018)	(0.017)	(0.019)
N	11,370	11,370	11,370
Wave dummies	Yes	Yes	Yes
Other controls	No	Yes	Yes
Siblings FE	No	No	Yes

Note: Estimates are obtained from linear probability models. Robust standard errors clustered at population center level are in parenthesis. N is the number of person-wave observations. The number of household fixed effects is 4,094, 3,439, and 3,249 for control group (a), (b), and (c), respectively. The estimates on all the other explanatory variables included in each regression are reported in Table A.2.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 2 – Effect of the USE Reform on Student Mobility by Location

	Control Group (a)		Control Group (b)		Control Group (c)	
	Level	FE	Level	FE	Level	FE
	(i)	(ii)	(i)	(ii)	(i)	(ii)
Moscow and St. Petersburg	-0.004	-0.013	-0.011	-0.017	-0.009	-0.016
	(0.018)	(0.027)	(0.020)	(0.027)	(0.019)	(0.025)
Other Major Cities	0.042*	0.031	0.042*	0.027	0.040*	0.024
	(0.022)	(0.022)	(0.021)	(0.027)	(0.022)	(0.030)
Small Cities and Towns	0.125***	0.124***	0.115***	0.123***	0.120***	0.125***
	(0.040)	(0.038)	(0.040)	(0.041)	(0.041)	(0.045)
Rural Areas	-0.005	0.019	-0.005	0.011	-0.016	-0.003
	(0.026)	(0.025)	(0.027)	(0.028)	(0.027)	(0.029)
N	21,678	21,678	13,710	13,710	11,370	11,370

Note: Each column reports the linear probability estimate on the interaction of the treatment effect, $d \times I(t \geq s)$, with the four location indicators (see text for definitions). Robust standard errors clustered at population center level are in parenthesis. Each regression includes the same set of explanatory variables as in Table A.2. See the note to Table 1 for other details.

*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 3 – Effect of the USE Reform on Student Mobility with Group Specific Time Trends, by Location

	Control Group (a)		Control Group (b)		Control Group (c)	
	Level	FE	Level	FE	Level	FE
	(i)	(ii)	(i)	(ii)	(i)	(ii)
Moscow and St. Petersburg	0.075	0.088	0.067	0.068	0.072	0.073
	(0.069)	(0.071)	(0.066)	(0.070)	(0.065)	(0.066)
Other Major Cities	0.002	0.017	0.008	0.011	0.011	0.017
	(0.029)	(0.044)	(0.028)	(0.044)	(0.028)	(0.042)
Small Cities and Towns	0.165***	0.179**	0.158***	0.156**	0.155***	0.163**
	(0.056)	(0.074)	(0.053)	(0.068)	(0.052)	(0.065)
Rural Areas	-0.028	-0.046	-0.042	-0.072	-0.040	-0.066
	(0.050)	(0.057)	(0.051)	(0.055)	(0.051)	(0.056)
N	21,678	21,678	13,710	13,710	11,370	11,370

Note: For details see the note to Tables 1 and 2.
*** indicates statistical significance at the 1% level, and ** at the 5% level.

Table 4 – Intention to go to college in the future

	Flexible Common Trend			Group-Specific Time Trend		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Moscow &	-0.080	-0.136	0.148	0.071	0.017	0.280
St. Petersburg	(0.182)	(0.187)	(0.612)	(0.305)	(0.308)	(0.720)
N	276	276	276	276	276	276
Other Major	-0.023	-0.056	-0.029	-0.048	-0.065	-0.217
Cities	(0.083)	(0.076)	(0.178)	(0.129)	(0.121)	(0.198)
N	903	903	903	903	903	903
Small Cities	0.236**	0.214*	0.225	0.308**	0.314**	0.190
and Towns	(0.105)	(0.112)	(0.164)	(0.126)	(0.136)	(0.235)
N	863	863	863	863	863	863
Rural Areas	-0.013	0.052	-0.033	-0.008	0.024	-0.097
	(0.063)	(0.067)	(0.115)	(0.093)	(0.089)	(0.174)
N	968	968	968	968	968	968
Other controls	No	Yes	Yes	No	Yes	Yes
Siblings FE	No	No	Yes	No	No	Yes

Note: Estimates are obtained from linear probability models run on RLMS data covering the period 2006–2013. Robust standard errors clustered at population center level are in parentheses. N is the number of person-wave observations. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 5 – Robustness Checks: Placebo Test

	Control Group (a)		Control Group (b)		Control Group (c)	
	Level	FE	Level	FE	Level	FE
	(i)	(ii)	(i)	(ii)	(i)	(ii)
Moscow and St. Petersburg	-0.010	-0.053*	-0.017	-0.049	-0.019	-0.044
	(0.035)	(0.031)	(0.033)	(0.035)	(0.032)	(0.037)
Other Major Cities	0.020	0.008	0.007	-0.007	0.003	-0.010
	(0.014)	(0.020)	(0.016)	(0.023)	(0.015)	(0.021)
Small Cities and Towns	-0.043	-0.051	-0.057	-0.044	-0.051	-0.046
	(0.036)	(0.043)	(0.037)	(0.038)	(0.035)	(0.034)
Rural Areas	0.007	0.053	0.017	0.061	0.002	0.047
	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.045)
N	13,148	13,148	8,801	8,801	7,108	7,108

Note: The "placebo reform" is imposed to occur in 2002. Each regression is restricted to the period 1994–2007. See the note to Tables 1 and 2 for other details.
* indicates statistical significance at the 10% level.

Table 6 – Robustness Checks: Combining Propensity Score Matching with the Difference-in-Difference Estimator

	Control Group (a)	Control Group (b)	Control Group (c)
Moscow and	-0.001	-0.004	-0.008
St. Petersburg	(0.021)	(0.024)	(0.024)
N	2,238	1,369	1,158
Other Major	0.052*	0.050*	0.034
Cities	(0.025)	(0.027)	(0.032)
N	6,568	4,011	3,408
Small Cities	0.112**	0.118**	0.132***
and Towns	(0.046)	(0.045)	(0.044)
N	5,768	3,778	3,161
Rural Areas	-0.026	-0.038	-0.045
	(0.032)	(0.037)	(0.038)
N	7,011	4,514	3,601

Note: Estimates are obtained from a two-step procedure. The first step uses propensity score matching (estimated with an Epanechnikov kernel and a 0.05 bandwidth) to pair each treated individual in the post-reform period with a subset of individuals who are closest to him/her in all the available observable characteristics in the other three groups (i.e. treated before the reform, nontreated before, and nontreated after). Propensity scores are the predicted probabilities obtained from a logit model in which the outcome variable is $d \times I(t \geq s)$ and the right-hand side variables are all the covariates listed in Table A.1 and after imposing a standard common support restriction. The second step estimates of the effect of the USE reform on the probability of leaving the parental home, reported in the table, are obtained using weighted least squares and a full set of time (wave) dummy variables, where the weights are the result of the matching stage. Robust standard errors are clustered at the population center level are reported in parenthesis. *N* denotes the number of person-wave observations.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 7 – Robustness Checks: Treatment Effect Estimates from a Discrete Time Duration Model

	Control Group (a)		Control Group (b)		Control Group (c)	
	(i)	(ii)	(i)	(ii)	(i)	(ii)
Moscow and	0.002	-0.008	-0.002	-0.017	0.000	-0.017
St. Petersburg	(0.012)	(0.014)	(0.017)	(0.025)	(0.018)	(0.030)
N	2,801	2,754	1,143	1,124	829	815
Other Major	0.037**	0.036**	0.038**	0.036**	0.037**	0.034**
Cities	(0.017)	(0.016)	(0.017)	(0.015)	(0.017)	(0.015)
N	7,851	7,776	4,767	4,727	3,871	3,841
Small Cities	0.086***	0.091***	0.072**	0.071**	0.072**	0.073**
and Towns	(0.033)	(0.029)	(0.036)	(0.035)	(0.036)	(0.034)
N	6,710	6,671	4,361	4,343	3,667	3,654
Rural Areas	-0.016	-0.020	-0.013	-0.021	-0.045	-0.051
	(0.024)	(0.022)	(0.026)	(0.026)	(0.033)	(0.032)
N	7,875	7,835	5,050	5,037	4,040	4,028
Time dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Note: Each figure is the marginal effect obtained from a (logit) discrete time duration model. The corresponding standard errors, clustered at the population center level and obtained via delta method, are reported in paretheses.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 8 – Heterogeneous Effects among Individuals from Small Cities and Towns

	Control Group (a)		Control Group (b)		Control Group (c)	
	Level	FE	Level	FE	Level	FE
Female	-0.028	-0.035	-0.032	-0.035	-0.038	-0.044
	(0.070)	(0.086)	(0.072)	(0.082)	(0.072)	(0.080)
Russian ethnicity	0.056	0.038	0.050	0.048	0.062	0.048
	(0.092)	(0.102)	(0.097)	(0.103)	(0.096)	(0.101)
Born Elsewhere[a]	-0.167***	-0.146*	-0.188***	-0.115	-0.178**	-0.079
	(0.061)	(0.078)	(0.061)	(0.084)	(0.067)	(0.090)
In the top half of the income distribution	-0.065	-0.106	-0.057	-0.094	-0.061	-0.080
	(0.092)	(0.096)	(0.089)	(0.098)	(0.085)	(0.101)
Both parents have university degrees	0.159*	0.234	0.203**	0.226	0.211**	0.240
	(0.094)	(0.146)	(0.095)	(0.152)	(0.095)	(0.160)
N	5,816	5,816	3,801	3,801	3,179	3,179

Note: The estimates are obtained from linear probability models in which time dummies and all the controls listed in Table A.1 are included besides the interactions between the indicator variables d, $I(t \geq s)$, $d \times I(t \geq s)$, and the variable of interest. The table shows the estimate on $d \times I(t \geq s)$ interacted with the variable of interest. Robust standard errors clustered at population center level are in parenthesis. N is the number of person-wave observations.

[a] 'Elsewhere' means in a different population center.

*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table 9 – Other Outcomes (Small Cities and Towns)

	Flexible Common Trend			Group-Specific Time Trends		
	Control Group (a) (i)	Control Group (b) (ii)	Control Group (c) (iii)	Control Group (a) (iv)	Control Group (b) (v)	Control Group (c) (vi)
A. Monetary transfers to children [mean of dep. var. = 0.302][a]						
β	0.127*	0.150**	0.141**	0.186**	0.232***	0.216***
	(0.064)	(0.063)	(0.061)	(0.075)	(0.074)	(0.070)
N	4,981	3,465	2,949	4,981	3,465	2,949
B. Household expenditure share on education [mean of dep. var. = 0.032][b]						
β	0.023***	0.022***	0.022***	0.0100	0.0062	0.0051
	(0.006)	(0.006)	(0.006)	(0.0129)	(0.0125)	(0.0124)
N	5,657	3,914	3,352	5,657	3,914	3,352
C. Father's monthly hours of work [mean of dep. var. = 140.6][c]						
β	1.8	-3.1	-5.2	23.0	18.3	16.8
	(9.4)	(9.2)	(9.4)	(13.7)	(12.4)	(12.9)
N	3,908	2,771	2,400	3,908	2,771	2,400
D. Father's labor force participation [mean of dep. var. = 0.915][d]						
β	-0.012	-0.021	-0.019	0.010	0.000	-0.010
	(0.034)	(0.032)	(0.032)	(0.056)	(0.052)	(0.053)
N	3,908	2,771	2,400	3,908	2,771	2,400
E. Mother's monthly hours of work [mean of dep. var. = 123.][c]						
β	4.8	1.8	0.08	5.9	-0.3	3.4
	(8.0)	(7.2)	(7.1)	(14.8)	(14.6)	(14.3)
N	5,364	3,708	3,178	5,364	3,708	3,178
F. Mother's labor force participation [mean of dep. var. = 0.873][d]						
β	0.006	0.003	-0.004	0.018	-0.005	0.006
	(0.032)	(0.028)	(0.029)	(0.060)	(0.052)	(0.053)
N	5,364	3,708	3,178	5,364	3,708	3,178
G. Parental divorce [mean of dep. var. = 0.028][e]						
β	-0.017	-0.021	-0.022	0.012	0.005	0.015
	(0.017)	(0.017)	(0.017)	(0.030)	(0.031)	(0.033)
N	3,922	2,795	2,432	3,922	2,795	2,432

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Table 9 (continued)

	Flexible Common Trend			Group-Specific Time Trends		
	Control Group (a) (i)	Control Group (b) (ii)	Control Group (c) (iii)	Control Group (a) (iv)	Control Group (b) (v)	Control Group (c) (vi)
H. Spillover effects [mean of "work" = 0.371; mean of "study" = 0.512][f]						
Work	0.022	0.036	0.051	0.007	0.035	0.064
	(0.044)	(0.047)	(0.049)	(0.066)	(0.071)	(0.079)
Study	-0.059	-0.067	-0.077	-0.060	-0.065	-0.059
	(0.054)	(0.057)	(0.062)	(0.078)	(0.083)	(0.088)
N	5,099	3,364	2,850	5,099	3,364	2,850

Note: In panels A through G, β is the treatment effect estimate obtained from regressions that include all the the control variables listed in Table A.1. Robust standard errors clustered at population center level are in parenthesis.

[a] 'Monetary transfers to children' takes value 1 if the household makes a transfer to a child outside the household in any of the two RLMS waves following the child's move out of the household, and 0 otherwise.

[b] Share of household expenditures in education over the total household nondurable consumption. The regressions also control for the log of total nondurable expenditures.

[c] Measured as actual total hours worked in all jobs during the month before interview.

[d] Equals 1 if in work, and 0 otherwise.

[e] Equals 1 if one of the two parents leaves the household within one year of the interview at time t in households in which both parents are present at t, and 0 otherwise.

[f] Each figure is the estimated treatment effect of the USE reform obtained from multinomial logit models that include all the the control variables listed in Table A.1. The base category is inactivity. The household sample is restricted to individuals who coreside with their parents in the following survey round. Robust standard errors (in parentheses) are clustered at the population center level and derived via the delta method.

*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A.1 – Descriptive Statistics

	Control Group (a)		Control Group (b)		Control Group (c)		Treatment Group	
	Reform off	Reform on	Reform off	Reform on	Reform off	Reform on	Reform off	Reform on
Individual Characteristics								
Female	0.449	0.450	0.469	0.479	0.481	0.487	0.558	0.562
Age (years)	19.0	19.6	17.26	17.36	16.97	17.15	16.7	17.2
Russian ethnicity	0.679	0.495	0.612	0.487	0.596	0.503	0.570	0.470
Born elsewhere[a]	0.228	0.219	0.233	0.207	0.230	0.203	0.256	0.201
Household Demographics								
Household Size	3.97	3.90	4.00	3.96	.93	3.91	4.02	3.99
No. siblings, 0–6	0.03	0.07	0.04	0.11	0.04	0.11	0.05	0.12
No. siblings, 7–14	0.28	0.23	0.35	0.31	0.34	0.30	0.38	0.35
No. siblings, 15–19	0.28	0.18	0.27	0.17	0.25	0.16	0.23	0.17
No. siblings, 20–24	0.19	0.18	0.19	0.16	0.18	0.15	0.18	0.14
No. siblings, 25+	0.10	0.11	0.06	0.06	0.05	0.05	0.05	0.05
Other children, 15–19	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
Mother in household (=1, if yes)	0.948	0.945	0.949	0.940	0.948	0.942	0.968	0.949
Father in household (=1, if yes)	0.737	0.706	0.746	0.718	0.753	0.724	0.799	0.754
Both parents in household (=1, if yes)	0.725	0.692	0.734	0.705	0.741	0.713	0.793	0.742
No. grandparents in household	0.23	0.27	0.24	0.30	0.24	0.29	0.23	0.29
Socioeconomic Status								
Mother has a university degree (=1, if yes)	0.175	0.231	0.175	0.239	0.198	0.260	0.216	0.310
Father has a university degree (=1, if yes)	0.127	0.134	0.127	0.137	0.144	0.152	0.173	0.176
Both parents have university degrees (=1, if yes)	0.067	0.080	0.066	0.081	0.077	0.091	0.089	0.113
Home owners (=1, if yes)	0.923	0.924	0.911	0.909	0.914	0.913	0.899	0.920
Own a dacha (=1, if yes)	0.226	0.205	0.218	0.194	0.233	0.202	0.208	0.216
Own an extra apartment (=1, if yes)	0.067	0.095	0.064	0.101	0.069	0.107	0.061	0.109
Own a car (=1, if yes)	0.365	0.498	0.357	0.501	0.380	0.521	0.429	0.536
Median Income Rank[b]	0.503	0.487	0.490	0.471	0.500	0.470	0.503	0.492
Location								
Moscow[c]	0.092	0.079	0.083	0.076	0.086	0.077	0.076	0.088
St. Petersburg[c]	0.037	0.027	0.038	0.027	0.040	0.027	0.030	0.024
Other major cities[d]	0.316	0.304	0.304	0.296	0.316	0.304	0.280	0.272
Small cities and towns[d]	0.269	0.257	0.279	0.267	0.280	0.274	0.267	0.254
Rural areas[d]	0.285	0.333	0.296	0.334	0.277	0.319	0.347	0.363
Federation Districts								
North and North Western	0.099	0.085	0.107	0.091	0.112	0.094	0.091	0.098
Central and Chyornyzyomla	0.234	0.236	0.228	0.237	0.236	0.242	0.200	0.265
Volga	0.162	0.165	0.165	0.168	0.160	0.171	0.200	0.174
North Caucasus	0.148	0.193	0.140	0.184	0.134	0.168	0.154	0.180
Ural	0.167	0.143	0.170	0.143	0.168	0.143	0.135	0.118
Western Siberia	0.092	0.089	0.093	0.083	0.090	0.087	0.102	0.093
East Siberia	0.098	0.088	0.098	0.093	0.099	0.095	0.119	0.072
Local Conditions								
Travel cost to Moscow[e]	16.2	3.6	16.7	3.6	15.6	3.6	21.7	3.4
Travel cost to state capital[e]	1.6	0.4	1.8	0.4	1.70	0.4	1.7	0.4
Youth unemployment rate[f]	0.168	0.134	0.169	0.133	0.167	0.132	0.176	0.135
N	13,379	10,218	8,571	5,574	6,721	4,770	1,054	788

Note: Figures are means by group (treatment and control) and period (defined on RLMS waves, i.e., reform-off = 1994–2007, reform-on = 2008–2014).

[a] Elsewhere means in a different population center.

[b] Refers to the median of the cross-sectional income rank taken over all the waves in which each household is observed. Notice in all the regressions we include indicators for income rank quartiles.

[c] Includes the whole metropolitan area within a 50 Km radius.

[d] Includes all the area within a 20 Km radius from the main center.

[e] Refers to the mean cost to travel to Moscow or the State (Oblast) capital, expressed in thousands of 2013 rubles.

[f] Oblast unemployment rate (measured separately for urban and rural areas) for individuals ages 16–25.

Table A.2 – Control Variable Estimates for Mobility Regression (table 1)

	Control Group (a)		Control Group (b)		Control Group (c)	
	Level	FE	Level	FE	Level	FE
Female	0.013***	0.012*	0.010**	0.014*	0.006	0.010
	(0.003)	(0.007)	(0.004)	(0.008)	(0.004)	(0.010)
Age	0.009***	0.013***	0.013***	0.020***	0.012***	0.021***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
Russian ethnicity	0.010**	0.011	0.007	0.002	0.005	-0.000
	(0.004)	(0.009)	(0.005)	(0.012)	(0.005)	(0.012)
Born elsewhere	0.008**	0.010	0.006	0.001	0.005	0.003
	(0.004)	(0.010)	(0.005)	(0.011)	(0.005)	(0.013)
Household Size	0.007**	-0.002	0.008**	-0.005	0.008**	-0.001
	(0.003)	(0.005)	(0.004)	(0.007)	(0.004)	(0.007)
No. of Siblings, 0–6	-0.023***	-0.009	-0.017***	-0.006	-0.016**	-0.004
	(0.006)	(0.018)	(0.006)	(0.019)	(0.007)	(0.018)
No. of Siblings, 7–14	-0.003	0.026***	-0.006	0.018	-0.006	0.010
	(0.004)	(0.009)	(0.005)	(0.011)	(0.005)	(0.011)
No. of Siblings, 15–19	-0.007	0.029***	-0.015***	0.020*	-0.018***	0.012
	(0.004)	(0.008)	(0.005)	(0.010)	(0.006)	(0.011)
No. of Siblings, 20–24	-0.013**	0.028***	-0.016***	0.016	-0.021***	0.003
	(0.005)	(0.009)	(0.006)	(0.010)	(0.006)	(0.012)
No. of Siblings, 25+	-0.017**	0.024**	-0.015*	0.015	-0.015	0.010
	(0.007)	(0.011)	(0.008)	(0.015)	(0.010)	(0.019)
Other Children, 15–19	-0.034**	-0.021	-0.028	0.021	-0.030	0.024
	(0.016)	(0.021)	(0.024)	(0.048)	(0.020)	(0.041)
Mother in household	-0.084***	-0.130***	-0.086***	-0.144***	-0.081***	-0.153***
	(0.013)	(0.030)	(0.016)	(0.041)	(0.016)	(0.050)
Father in household	-0.066***	-0.124***	-0.068***	-0.102**	-0.054**	-0.095*
	(0.019)	(0.038)	(0.023)	(0.044)	(0.027)	(0.056)
Both parents	0.067***	0.112***	0.068***	0.105**	0.054**	0.094*
in household	(0.020)	(0.039)	(0.024)	(0.045)	(0.027)	(0.057)
No. of grandparents	-0.006	0.000	-0.009	-0.005	-0.005	-0.012
in household	(0.006)	(0.009)	(0.006)	(0.013)	(0.007)	(0.016)
Mother has a	0.012**	-0.032	0.006	-0.043*	0.004	-0.054*
university degree	(0.005)	(0.022)	(0.006)	(0.024)	(0.006)	(0.030)
Father has a	0.012**	-0.015	0.011	-0.026	0.008	-0.050
university degree	(0.006)	(0.027)	(0.007)	(0.026)	(0.007)	(0.033)
Both parents have	-0.021**	-0.050	-0.019*	-0.047	-0.011	-0.012
university degrees	(0.009)	(0.047)	(0.011)	(0.064)	(0.011)	(0.071)
Home owners	-0.002	0.012	-0.003	0.000	-0.002	-0.012
	(0.007)	(0.014)	(0.007)	(0.013)	(0.008)	(0.014)
Own a dacha	-0.003	0.004	0.002	0.000	0.002	0.001
	(0.005)	(0.008)	(0.005)	(0.010)	(0.005)	(0.011)
Own an extra	0.007	0.004	0.008	0.005	-0.002	-0.012
apartment	(0.006)	(0.011)	(0.007)	(0.013)	(0.007)	(0.011)
Own a car	-0.001	-0.000	-0.004	-0.010	-0.001	-0.003
	(0.004)	(0.007)	(0.004)	(0.010)	(0.005)	(0.010)
Median income	-0.011	0.017	-0.012	0.016	-0.013*	0.016
rank Q1	(0.007)	(0.019)	(0.007)	(0.023)	(0.008)	(0.029)
Median income	-0.010*	0.017	-0.011*	0.026	-0.011	0.029
rank Q2	(0.006)	(0.016)	(0.006)	(0.018)	(0.007)	(0.022)
Median income	-0.002	0.012	-0.008	0.015	-0.011*	0.018
rank Q3	(0.005)	(0.013)	(0.005)	(0.014)	(0.006)	(0.018)
North and	0.016		0.016		0.016	
North Western	(0.014)		(0.012)		(0.012)	
Central and	-0.011		-0.007		-0.008	
Chyornyzyomla	(0.010)		(0.011)		(0.012)	
Volga	0.021*		0.032**		0.028**	
	(0.013)		(0.014)		(0.014)	
North Caucasus	-0.032**		-0.012		-0.016	
	(0.013)		(0.011)		(0.011)	
Ural	0.002		0.003		0.002	
	(0.011)		(0.011)		(0.011)	
Western Siberia	-0.004		-0.000		-0.003	
	(0.009)		(0.010)		(0.011)	
Travel cost	-0.000	0.000	-0.000	0.000	0.000	0.000
to Moscow	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Travel cost	-0.000**	-0.000	-0.000**	-0.000	-0.000***	-0.000
to state Capital	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Local youth	-0.037	0.051	-0.009	0.020	-0.003	0.024
unemployment	(0.038)	(0.052)	(0.037)	(0.058)	(0.036)	(0.064)

Note: The estimates on year dummies are not reported. Standard errors (in parentheses) are clustered at the population center level.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.