# Accepted Manuscript


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PII: S0014-2921(19)30069-8
DOI: https://doi.org/10.1016/j.euroecorev.2019.04.007
Reference: EER 3247

To appear in: *European Economic Review*

Received date: 1 October 2018
Accepted date: 21 April 2019

Please cite this article as: Marco Francesconi, Fabián Slonimczyk, Anna Yurko, Democratizing Access to Higher Education in Russia: The Consequences of the Unified State Exam Reform, *European Economic Review* (2019), doi: https://doi.org/10.1016/j.euroecorev.2019.04.007

# Democratizing Access to Higher Education in Russia: The Consequences of the Unified State Exam Reform*

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May 9, 2019

**Abstract**

This is the first paper that evaluates the effects of a reform requiring Russian universities to make admission decisions based on the results of a national high-school exam. We show the reform led to a threefold increase in geographic mobility rates among high-school graduates from small cities and towns to start college. This is robust to different techniques, samples, and specifications. The reform was also accompanied by increases in students' expectations to attend university, parental transfers, and educational expenditures. There is no evidence the reform affected parental labor supply, divorce, and employment outcomes of graduates who did not move.

JEL classification: I23, I24, J61

Keywords: Human capital investment; Student migration; University admission; Educational inequalities; Wider participation



*We are grateful to two anonymous referees and to the Editor (Robert Sauer) for useful comments and guidance. Sonia Bhalotra, Emilia Del Bono, Ina Ganguli, Sergey Guriev, Andy Mountford, Maria Yudkevich, and participants to several seminars and conferences provided helpful comments and suggestions. We also thank staff of the Centre for Labour Market Studies at the Higher School of Economics in Moscow for useful information on data issues and help in understanding institutional details. Natasha Gimpelson and Anastasia Kaygorodova provided invaluable research assistance. We gratefully acknowledge the financial support from the Higher School of Economics' Senior Scholars Visiting Program. This paper is accompanied by Supplementary Material in the Online Appendix available at: <https://sites.google.com/site/fabianslonimczyk/home/working-papers/use>.

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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 college-specific entry tests. The reform instead required higher education institutions to determine their admissions on the basis of the scores from a nationally administered, secondary-school examination, known as the Unified State Exam (USE). Ours is the first paper that evaluates the effect of this reform on a wide range of outcomes, paying special attention to student geographic mobility to pursue studies in post-secondary education.

Many countries use decentralized college admission policies that require applicants to take institution-specific entrance exams, e.g., Japan, Finland, Israel, and until recently South Korea and Brazil (Avery, Roth, and Lee, 2014; Pekkala Kerr et al., 2015). Such exams create substantial frictions, imposing large application costs to students and preventing them from applying to more than a small number of universities.[2] In the US, university candidates often have to complete college-specific requirements, such as admission essays in addition to taking SAT tests, quite often more than once (Goodman, Gurantz and Smith, 2018). This is in contrast with countries where the application and admission processes are centralized with national exams as the main criterion for admissions (such as China, Taiwan, Italy, Belgium, Norway, and Germany).[3] Although the welfare and sorting effects of university-specific versus national university entrance exams have been investigated from a theoretical perspective (e.g., Chade, Lewis, and Smith, 2014; Che and Koh, 2016; Hafalir et al., 2018), there is little empirical evidence on their effects on student decisions. The enactment of the USE reform in Russia therefore provides a fascinating opportunity for studying the effects of college application costs on student geographic mobility and parental behavior.

Knowledge about higher education in post-Soviet Russia is limited by data availability,

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

[2] For instance, Chade, Lewis, and Smith (2014) report that high application costs limit the number of applications sent by the median student in the United States to three.

[3] National exams are used in several other countries, such as Australia, Canada, and most of Western Europe, including France and Britain where, however, Grande Écoles and Oxbridge have respectively their own admission systems.

something we also have to face in our empirical work.[4] Most of what we currently know comes from correlational studies by sociologists and educationalists.[5] 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.[6] 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 in the last forty years (e.g., Carneiro and Heckman, 2002; Bowen, Chingos, and McPherson, 2009), and it is comparable to the US black-white male college graduation rate differentials observed in the 1960s (Neal, 2006).

Another important 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 over-representation 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. This slant of our analysis ties in well with the large literature that has emphasized the importance of geographic mobility to economic welfare and the functioning of the family. For example,

[4]For instance, there are no publicly available university data on the student population and institution-specific inputs nor is there any available information on department-institution-specific declared and effective entry USE tariffs. This means we cannot look at university application and enrollment decisions based on the USE results. In addition, individual information on university application, enrollment, and attendance decisions is also not consistently collected over time in standard surveys. This poses some constraint on our analysis.

[5]See, among others, Johnson (2010) and Ampilogov, Prakhov, and Yudkevich (2013).

[6]These statistics 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 later sections.

using Norwegian registry data, Løken, Lommerud, and Lundberg (2013) document a relatively low mobility of married men without a college degree, especially in rural areas (see also Konrad et al., 2002; Rainer and Siedler, 2009).

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.[7] 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. Other potential costs faced by applicants in the pre-reform period were related to favoritism and corruption, which could have penalized students from peripheral areas more than those from larger cities, in part because the latter and their families had better knowledge of (and more direct access to) the admission system and its administrators. The USE reform would have not only abated such costly frictions 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. This background motivates the primary focus of our analysis on student geographic mobility.

The USE reform, therefore, intended to democratize access to higher education for a large group of young people. In this sense one might expect that the reform would induce changes similar to those observed after the expansion of college availability in the United States over the course of the twentieth century, although Russia has long been one of the countries with the largest share of the adult population holding university-level qualifications (OECD, various years). As several influential studies show, greater college availability does increase access to higher education and pushes up college enrollment rates in the US (e.g., Card,

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

1995; Goldin and Katz, 1999; Currie and Moretti, 2003; Black and Sokoloff, 2006).[8] But so far there is no available evidence on the experience faced by high school students in Russia. This is where we make our first contribution.

In order to investigate the effect of the reform on student geographic mobility, we look at several, different (but related) pieces of evidence. First, we analyze mobility from the perspective of the household of origin using panel data from the RLMS covering the period from 1994 to 2014. We contrast the probabilities of leaving the parents' home for students in their last year of high school, who were treated by the USE reform, with those of children in a similar age range who are not in the last year of high school. We show that the exit rates of the former group sharply increased right after the reform, while the mobility behavior of the latter group remained unaltered. This result is robust to choosing alternative comparison groups. For instance, students in the last year of vocational or technical school, who do not take USE tests at the end of their studies, did not experience any significant change in their exit rates. Additional checks, such as using a propensity score matching technique in order to re-weight the control group or including household fixed effects (comparing treated and untreated siblings) does not affect this result.

We also show that the effect of the reform on geographic mobility is only present in locations that lack large higher education institutions. Specifically, our difference-in-difference estimates indicate that the fraction of high school graduates in small Russian cities and towns who left their parental home after graduation 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. Interestingly, we do not find any significant impact on mobility among students who live in rural areas, in part because of the relatively low quality of secondary schools in rural locations.

Second, we exploit the fact that the USE reform was not expected to affect high school students in Moscow and St. Petersburg as an independent natural experiment. Difference-in-difference estimates using this alternative control group yield results that are fully consistent with our findings from the earlier experiments. Finally, we repeat our exercise using

[8] Blanden and Machin (2004) and Winter-Ebmer and Wirz (2002) find a comparable relationship between access and enrollment for Britain and Europe, respectively. This phenomenon is widespread. Schofer and Meyer (2005) document the rapid expansion of higher educational enrollments over the twentieth century across the world.

a different measure of mobility that specifically focuses on changes in residence location for the purpose of starting university studies. This alternative data source allows us to control for a full set of characteristics related to the location and quality of the higher education institution, as well as for the field of study. Depending on the specification, we find that, post-reform, college students who entered university right after high school are 8 to 12 percentage points more likely to have come from a location other than the city where their university is based.[9]

Among the possible mechanisms at work behind these results, having highly educated parents and having knowledge of the reform are likely to have played an important role. The latter channel is proxied by college attendance intentions, for which we find a substantial increase exactly among the high school graduates who eventually move after the reform, i.e., only those from small cities and towns. We also investigate the effect of geographic distance to regional centers (where large universities are located). While we find no significant interaction with distance for students in small cities and towns, there seems to have been an increase in mobility in rural locations that are very distant from regional centers.

The estimated effect of the USE reform on geographic mobility are sizeable, 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 a key role in facilitating access to higher education institutions for less advantaged young people who would have not normally enrolled into university programs in Russia.[10]

Our second contribution is to explore whether the USE reform led to unanticipated consequences in other life domains. This deepens our understanding of the potential mechanisms that lie behind the estimated mobility patterns. We find that the increase in 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

[9]Another piece of descriptive evidence comes from available aggregate data on internal migration. We briefly discuss these data below.

[10]This result is relevant also to debate about the impact that education has on the type of mobility that modern labor markets need to operate efficiently. See Machin, Salvanes, and Pelkonen (2012), although their analysis focuses on the segment of the population with lower educational attainments.

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 forward-looking parents in peripheral small cities and towns, who would have not invested in the absence of the reform. Although largely unanticipated in the policy discussions that preceded the reform, this is arguably not an undesirable change. Importantly, we find no evidence that the reform led to unintended changes in parental behavior with possibly negative spillovers. In particular, we find no effect of the 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. We also find no evidence of unfavorable labor market outcomes among young adults who did not move after completing their secondary education.

A USE-like reform is quite unique in the recent experience of advanced economies.[11] By dramatically reducing college entry costs, the policy made access to higher education more democratic and easier for all students, especially those from the less advantaged Russian periphery. This is interesting in view of the discussions based on the growing body of evidence according to which college application and enrollment decisions fall short of expectations among low-income students in a number of countries, including the United States (see, for instance, Hoxby and Avery, 2013; Hoxby and Turner, 2015; Dillon and Smith, 2017) and Britain (e.g., Crawford, Macmillan, and Vignoles, 2017; Crawford et al., 2017).

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 and methods used to analyze the leaving home decision among high school graduates, presents the difference-in-difference estimates using alternative control groups, a number of robustness checks and the role of heterogeneity, as well as evidence on the possible mechanisms behind our results (i.e., distance, socioeconomic background, and information). Section 5 presents difference-

[11]Most of the existing literature focuses on different sources of variation in educational supply (not necessarily affecting the post-secondary education sector directly), such as changes in compulsory schooling laws, the minimum school-leaving age, tuition costs, and geographic proximity of schools. See, among others, Ichino and Winter-Ebmer (2004), Kane (2006), Brunello, Fort, and Weber (2009), and Black, Devereux, and Salvanes (2011).

in-difference estimates based on the alternative measure of mobility. Section 6 explores other outcomes, and Section 7 concludes. Supplementary material on the institutional background, data, and additional results discussed throughout the paper are available in the Online Appendix.

## 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.[12] 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 frictions 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 and the cost of living arrangements in Moscow or St. Petersburg for up to one month. Such costs varied, depending on the applicants' geographic origin. For instance, only the transportation cost were between $500 and $700 on average, representing between 30% and 40% of the pre-reform mean monthly income. They would have been higher for those coming from relatively far away locations, who on average would also have had lower incomes.

In addition, the new post-Soviet system provided abundant opportunities for favoritism (e.g., some insider candidates with connections had preferential access to preparation materials) and corruption (e.g., teaching preparatory courses for admission was known to be a

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

nontrivial source of additional income for university faculty members involved in the process). The former minister of education, Vladimir Filippov, in an interview given in 2003, admitted that one of the goals of then planned USE reform was to eradicate the corruption associated with university admissions. The mean size of the bribe for admission to top Moscow universities prior to the reform was estimated to be around $10,000, more than half of the pre-reform yearly average per capita income.[13] Although universities could have waived tuition fees for gifted students, connections and bribery were allegedly widely used also to have access to such state-funded places exempt from tuition fees (Osipian, 2009). If fees were not waived, they had to be paid, and these could have been substantial depending on the program or the college, with some universities requiring around $10,000 per year for a four-year program.

An inevitable consequence of the high travel costs, the substantial costs associated with admission exam preparation (in terms of time and money), and the large bribes is that committing to one university (or even one specific program) would have made it impossible for periphery candidates to apply to other programs. Attending the most prestigious universities in Moscow and St. Petersburg was therefore considered outside the feasible opportunity set for anyone except selected few with connections, considerable financial resources, or exceptional talent.

The admission reform based on the Unified State Exam (USE) is the centerpiece of a 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.[14] Although students can choose which tests to take out of 14 different subjects, Russian language and Math exams are mandatory for graduation.[15]

The USE reform was introduced in 2009. Starting from the 2009/10 academic year, university admission decisions have to be based exclusively on USE passing scores.[16] Each

[13]The source for the monetary amount of the bribe is from *RIA Novosti*, 26 August 2011 (see <https://ria.ru/20110826/424566241.html>).

[14]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 typically by 15–18 years old students.

[15]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.

[16]There are some exceptions to this rule. For example, there are contests (known as "Olympiads") designed to identify the most talented students in the country. The winners are allowed to attend the

program within a higher education institution must publicize the subject tests (which are typically one or two besides Math and Russian) and the minimum threshold scores needed for admission. Although different university programs in the same field of study typically require the same subject tests, they set different thresholds, with better programs normally requiring higher scores. This information is made available to all high schools well before their students sit for each test. Final-year high-school students therefore know which subject tests and threshold scores are needed to apply to any program in the country. There are no reasons for students to take and pass subject tests besides Russian and Math, other than university access. For instance, there is no evidence that employers use such elective tests as a screening devise in hiring.

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, without any specific coordination among universities.[17] 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.[18]

program of their choice and offered a full scholarship. The number of university students who are Olympiads winners has been historically low. 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 alternative entry exams. Also these represent a relatively small fraction of university students. 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 25% of university students in each entry cohort since 2009. Most of these students generally enroll when they are 19 or older in part-time or evening-only programs, while virtually all students in top institutions enroll at younger ages in regular, full-time programs. This is a feature we will come back to in Section 5. Since 2016/17 (outside our sample period), admission rules have become stricter and even more tightly linked to the USE results.

[17]Unfortunately, there is no institutional access to individual USE scores across higher education institutions, nor are there official data sources recording where students apply.

[18]Data on financial aid recipiency are not available at the individual level and thus this margin cannot be analyzed. Notice however that, although the reform can be viewed as a package of new college admission rules and financial aid rules, the amount of state funds devoted to financial aid to the whole post-secondary

Unified State Exams were introduced gradually. The first were piloted in 2001 in few schools in five of the 84 regions of the Russian Federation. By 2008, USEs were offered everywhere and essentially all students in the last year of high school sat in at least one subject test, although only 16% of that cohort of 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.[19] It was only with the college entry cohort of 2009 that the USE scores became the standard yardstick for university admissions. Since then, the number of elective tests also grew as more and more programs expanded the number of their required subjects.[20]

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 far away 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, especially in science majors (e.g., Ampilogov, Prakhov, and Yudkevich, 2013). This is exactly the focus of our paper.[21]

education sector in general, and to each university in particular, did not change as a result of the reform (see also the Online Appendix). Allocations of financial aid to individual students also did not change across and within institutions. We thus do not expect any response to the USE reform driven by changes in financial aid rules.

[19] As a result of this, allowing for time as well as regional variation to pick up earlier adoptions of the USE based admission system cannot identify the impact of the reform.

[20] Consistent with a strong first-stage effect, the Online Appendix documents a sharp post-2008 increase in physics tests (required by most programs in engineering and natural sciences) and social studies tests (required by most social science and humanities programs). It also discusses other issues related to enrollment and funding in greater detail. Here, we just point out that the fraction of overall admissions increased after the reform, despite the baby bust experienced by Russia at the very beginning of the 1990s.

[21] Another anticipated role of the reform was 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 was also expected to affect the educational system preceding the entry into university, not only because student performance would become a readily available indicator on which to rank high schools, but also because Russian and Math tests would be mandatory requirements for high school graduation. Despite this, the bar set for the Russian and Math tests has been possibly low, with only 1% of all high school students failing to pass on the first attempt. Our paper does not test whether the reform had an impact on these other margins. They represent interesting areas for future research.

## 3. Conceptual Framework

Here we develop a simple conceptual framework to guide the interpretation of our empirical findings. Since the primary focus 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 work on migration and education developed by Sjaastad (1962), Becker (1964/2009), and Mountford (1997).[22]

Suppose a 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 far from the core region, the student has to face costs associated with moving to and settling in the core region while sitting for the admission exams as well as during her college studies if her application is successful. 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

[22]Other related models in the migration literature are discussed in Dustmann and Glitz (2011). See also McElroy (1985), Manacorda and Moretti (2006), Dynarski (2008), Machin, Salvanes, and Pelkonen (2012) and Kennan (2015).

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 shown in Figure 1. We focus on two possibilities that help us to interpret all the interesting cases we might have to deal with in the empirical analysis.

Figure 2a shows the first case, in which 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 therefore will unambiguously increase. The second case is depicted in Figure 2b. Here the net benefit increase is concentrated among students with ability higher than $a^*$. This is captured by a slope change in the $f$ curve beyond $a^*$, rather than by a level shift across the whole ability distribution (as in Figure 2a). In this case, then, the original (pre-reform) equilibrium is unaffected and we expect to observe no change in mobility.

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.

## 4. Leaving Home Among High School Graduates

In this section we assess the impact of the USE reform on the probability that high school graduates leave their household of origin. If the reform were effective, we expect to observe a strong positive increase in this likelihood among individuals who just gain a high school qualification, since these are exactly those who can potentially benefit from the USE tests. We present difference-in-difference estimates that compare their leaving-home probability to that of other young individuals who are not in their last-year of high school. In addition,

since we expect the mobility response to depend on the local availability of higher education institutions, we take advantage of the fact that the reform is unlikely to affect the leaving-home probability among students who live in Moscow and St. Petersburg, where elite institutions are located, as an additional exercise. We perform a number of robustness checks and test if there is response heterogeneity among specific groups. And we conclude the section with an analysis of three possible channels through which we observe the results we find, namely, the roles of geographic distance, parental socioeconomic background, and information.

### 4.1 Data and Methods

We use data from the Russian Longitudinal Monitoring Survey (RLMS). The RLMS is a nationally representative annual household survey based on the first national probability sample drawn in the Russian Federation.[23] 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.[24]

For adult individuals aged 14 years or more who leave their household of origin and cannot be followed in subsequent waves, we know the reason for leaving, as long as their 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). In a later part of the paper we will use this more detailed information, even if it only covers the post-reform period.

Our sample consists of young adults who are unpartnered and childless, and co-reside with at least one carer (parent or grandparent). We refer to our outcome as leaving the "parental" home, although 4% of the individuals in the sample co-reside with their grandparents. The outcome, denoted $\ell_{ijt}$, is a dummy variable equal to 1 if individual $i$ moves

[23]Extensive documentation on the RLMS is available at <www.cpc.unc.edu/projects/rlms-hse>.

[24]Oblasts and federal districts are the primary sampling units, which are in turn subdivided into 177 population centers.

out of household $j$ within one year after the interview at time $t$ and 0 if the individual continues to co-reside with his/her parents. We let $d_{ijt}$ be an indicator variable equal to 1 if individual $i$ in household $j$ attends the last year of high school at time $t$ and 0 otherwise, and $s$ be the time period in which the reform is fully implemented.[25]

Our first approach, in which we assess the effect of the USE reform on the probability that high school graduates leave their parental home soon after graduation, is to estimate the following difference-in-difference (DD) model:

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

where $I(z)$ is a function indicating that the event $z$ occurs,[26] $\mathbf{X}_{ijt}$ is a vector of individual and household characteristics, $\theta_j$ denotes household (sibling) fixed effects which we can credibly identify as the sample contains a sufficiently large number of households with at least two siblings in both the treatment and control groups, and $\varepsilon_{ijt}$ is a random error term.

Individuals in the treatment group are in their last year of high school with a typical age around 17 years. We contrast their behavior with the behavior of other comparable individuals who are *not* in their last high school year and are aged 15–19. This is our baseline control group. We are aware of the sensitivity of choosing a control group in our analysis given the simultaneous roll-out of the reform across the whole country. For this reason, we consider three *alternative* plausible comparison groups. The first is given by a broader group of individuals aged 15–24, many of whom are already high school graduates. Another is given by a narrower group of individuals aged 15–19 who are in full-time education. Both control groups may be problematic as they can be selected on the endogenous outcome. The last, which is less likely to suffer from this selection issue, is the subsample of students in their last year of technical or vocational secondary education (and still living at home), as these institutions do not provide qualifications based on the USEs.

[25] 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. We did look for potential effects on mobility arising from regional differences in the timing of introduction of the USE and did not find any statistically significant result.

[26] 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. Thus, $s = 2009$, and the high school students affected by the reform are in the 2008 RLMS wave.

Since our results are not sensitive to the control group definition, the results obtained with the first two alternative comparisons are not reported in the text but are available in Slonimczyk et al. (2017). The results obtained with the last alternative comparison group instead are shown in a later table. The treatment group comprises between 53 and 166 students in any given wave of interviews over the sample period for a total of 1,727 person-wave observations. The size of the baseline nontreated group varies between a minimum of 514 individuals and a maximum of 934 young adults for a total of 11,983 person-wave observations.

We use two different specifications for $\psi(t)$, which captures time trends. 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. In the other specification we have $\psi(t) = \delta_0 + (\delta_1 + \delta_2 d_{ijt})t + [\delta_3 + \delta_4(t - s)]I(t \geq s)$. This specification is very flexible, as it allows not only for different intercepts (when $\alpha \neq 0$) but also for different linear trends for individuals in the treatment and control groups (when $\delta_1 \neq \delta_2$). The parameters $\delta_3$ and $\delta_4$ identify possible shifts in the intercept and slope of the process generating $\ell$ at the time of the reform.[27] With both specifications we thus identify $\beta$ under the assumption 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. We do not know of any related program that could have induced such differential responses.[28]

Figure 3 shows the leaving-home rates for treatment and baseline control groups over time. Both groups experience similar patterns (albeit different levels) during the pre-reform period, lending support to the common trend assumption that is invoked when

[27]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 $\ell$ 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.

[28]Other 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) and Slonimczyk and Yurko (2014). In addition, although Russia experienced a sharp reduction in the total pool of potential students as a result of the reduced fertility after the collapse of the Soviet Union, this reduction has affected both treatment and control groups in similar ways. Even if universities might have liked to counter the falling number of students by expanding their recruitment outside their traditional (urban) markets, this was unlikely to happen without the 2009 reform which drastically reduced application costs to periphery students.

$\psi(t) = \tau_t$.[29] 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 are likely to 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 more detailed information collected by the RLMS after 2008 confirms that at least 95% of this outflow is attributable to graduates who move out to enroll into a university program, while only a small fraction of the modest increase in leaving-home rates among young adults in the control group can be accounted for by individuals attending college.[30]

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, and the $p$-value of a pre-reform balance test. Both treatment and control groups are fairly similar along a number of characteristics, including household demographics, geographic dispersion across districts, and 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 group, more mothers than fathers have a university degree especially after the introduction of the reform, possibly capturing the fact that fewer fathers co-reside with their children. In the reform-on period, a greater fraction of children 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

[29] It is possible that the group-specific linear time trends pick up not only pre-reform trends across groups, but also differences in the evolution of the outcome variable after the reform since the mobility response of treated individuals in Figure 3 continues to rise after 2008 (e.g., Borusyak and Jaravel, 2017). This means that the inclusion of our group-specific trends may lead to an underestimation of the true treatment effect, even in the absence of differential pre-reform trends across groups.

[30] To corroborate this evidence, we also looked at the only data available on (aggregate) official statistics on internal migration by education attainment and age group. Almost all the growth in the aggregate flows is accounted for by the group of individuals with complete secondary education, i.e. with the credentials needed to enroll in a university program. Migration flows among individuals with vocational or incomplete secondary education instead remained stable throughout the period. See Appendix Figure A.1 and further discussion in the Online Appendix.

rate, suggesting an improvement in the living standards over the 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.[31] The average position in the income distribution has instead changed little over time.

Despite the similarities, we also observe some important differences between groups. For example, students in the treatment group are younger, although the difference becomes smaller and insignificant in the post-reform period. A larger fraction of individuals in the treatment group are women. Treated students are also less likely to be of Russian ethnicity, and more likely to be in a household with both parents present, to have parents with a greater level of education, and to own a car. Because of these differences, all such controls are included in the vector $\mathbf{X}$.

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. 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, covers approximately 10–11% of the sample and has at least 200 universities and most of the elite colleges in the country. The second area, which accounts for 27–30% of the sample, refers to all the other major cities and their surrounding areas within a 20 Km radius.[32] 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 covers about one-quarter of the sample. The last

[31] 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). Most of such costs are related to commuting, which is almost entirely provided for by the state. Besides public subsidies that contain price hikes, the real price for public transports is gone down together with the reduction in energy prices. 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.

[32] 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.

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 are often referred to as the "periphery" in our discussion.

By making high school graduation contingent on passing the USE tests, the reform might have had an effect on high school graduation rates, and thus on the composition of the treatment group. For instance, the disincentive to drop out of high school might have gone up if also employers (and not just universities) resorted to the USE test results to screen candidates. Using data from the RLMS we find no evidence that the probability of staying in high school beyond grade 9 has changed as a result of the reform (see also OECD, various years).

To better assess whether the reform affected the quality of the inflow of students out of high school along a large set of observable characteristics and further account for the observed differences found in Table A.1, we perform a two-step propensity score matching (PSM) exercise. 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 in a first stage,[33] while the second stage is estimated using weighted least squares and a full set of time dummies (Blundell and Costa Dias, 2009). Anticipating our results, we find no evidence of the potential bias driven by observable differences between groups.[34]

[33]The first stage 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. We experimented with several other kernel/bandwidth combinations and obtained similar results.

[34]Another domain that could potentially affect the composition of treatment and control groups is given by the changing nature of the labor market. One might expect this to be especially important after the 2008 financial crisis and influence young people's migration decisions (Kennan and Walker, 2011). However, the labor market figures remained fairly stable in Russia since the middle of the 2000s until the end of the sample period (Rosstat, various issues), and the impact of the financial crisis on the Russian labor market appeared to be modest and short lived (Gimpelson and Kapeliushnikov, 2011). In the empirical analysis, business cycle shocks are accounted for by the inclusion of year and region fixed effects as well as local unemployment rates.

### 4.2 Results

Table 1 shows the estimated impact of the USE reform on the probability of leaving the parental home, $\beta$.[35] Columns (i)–(iii) 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, while columns (iv)–(vi) present estimates from specifications that include group-specific time trends.[36] Column (vii) reports the propensity score matching estimates obtained with the two-step Blundell-Costa Dias procedure.[37] Standard errors are robust to arbitrary forms of heteroscedasticity and are clustered at the population centre level.[38]

The first panel of the table reports the estimates found for the full RLMS sample. Focusing on the common trend estimates we find that, irrespective of adding controls and accounting for fixed effects shared by siblings living in the same household, the rate at which high school graduates leave their parental home increases 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 over the entire pre-reform period. Including group-specific linear trends leads to very similar point estimates, except in column (vi) when we add sibling FE. But in all such cases the effects are not statistically significant at conventional levels. This might pick up the fact that there are time trend differences in the home-leaving rates between treatment and control groups. Or, most likely, it may reflect the loss of statistical power due to the inclusion of differential trends. The PSM estimate in column (vii) confirms a significant 4 percentage point increase in the probability of leaving.[39]

[35] The estimates associated with the variables in $\mathbf{X}$ are in the Online Appendix.

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

[37] In the Online Appendix, we display a figure which compares the PSM-weighted and unweighted leaving-home rates over the whole period of analysis. The key finding from that comparison (robust to different kernel types and bandwidth levels) is that the large set of control variables has only a modest impact on leaving-home rates. As a result, the weighted mobility rates follow closely the unweighted rates.

[38] Clustering at the state level and two-way clustering by population center and year of interview led to similar results.

[39] To assess the potential bias arising from the exclusion of unobservables more precisely, we use the methodology developed by Altonji, Elder, and Taber (2005). 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 ($\beta_R$) and the effect estimate in column (ii) as the coefficient from the full model ($\beta_F$). The AET statistic $\frac{\beta_F}{\beta_R - \beta_F}$ will tell us how much stronger the selection on unobservables must be

As discussed in Section 2, 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 and in peripheral areas. We expect therefore to find heterogeneous responses depending on where individuals (and their parents) are located. To assess this, we re-estimate equation (1) restricting the sample to each of our four locations of interest (Moscow/St. Petersburg, other major cities, small cities and towns, and rural areas). The corresponding effect estimates are reported in the next four panels of Table 1.

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. 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 (bottom panel). We shall come back to this result at the end of the subsection.

Instead the USE reform had some positive effect on young adults living in the major metropolitan centers other than Moscow and St. Petersburg (see columns (i), (ii), and (vii)). This effect is quantitatively similar to what we found earlier for the whole sample, i.e., an increased probability of about 5 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 sibling fixed effects (column (iii)) and group-specific trends (columns (iv)–(vi)).

The largest impact is observed among students who are located in small cities and towns. They experienced a significant jump of about 12–16 percentage points in the likelihood of leaving their parental home after the reform, which represents an increase of 3 to 4 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 specification of $\psi(t)$, whether

with respect to the selection on observables to explain away the entire effect of the USE reform on student mobility. We find large ratios of about 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 9 times stronger than the selection on observables, which seems unlikely. The same evidence emerges with the estimators proposed by Oster (2017).

sibling FE are included or not, and in the case of the PSM model.[40]

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 major higher education institutions. In the pre-reform regime, 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 expensive pre-sessional modules *in-situ* and away from home, 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 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 and student mobility go up.

Of course, college admission and consequent mobility can only occur if the USE test results are sufficiently good. Using RLMS data that are available only over the post-reform period, the estimates reported in the Online Appendix show 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.[41] Conversely, students from rural areas have the lowest average scores. If this held true 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 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.[42]

[40] In addition, the AET statistics indicate that the selection on unobservables should 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.

[41] We are not aware of data on school quality before the reform. In part this is because there was no agreed-upon, unified measure. At that point, in fact, each school had its own exam, and results were not recorded in any form. Despite the lack of data, quality is likely to be persistent over time.

[42] 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

It is also 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. 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.

### 4.3 Robustness and Heterogeneity

*A. Sensitivity Checks*

In what follows we present further results from three exercises to check the robustness of the estimates shown in Table 1. The first shows the estimates obtained when the control group is restricted to students who live at home while they are in their last year of technical or vocational secondary education. Such institutions typically do not provide qualifications based on the USE. As anticipated in subsection 4.1, the results which are presented in the first two columns of Table 2 confirm our baseline estimates with the strongest impact of the USE reform being concentrated in small cities and towns (although the estimates in column (ii) are not statistically significant at conventional levels). According to the estimates in Table 2, high school graduates from such locations faced an increase in excess of 10 percentage points in the likelihood of leaving their parental home after the reform.

The second exercise 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 columns (iii) and (iv) of Table 2. Across all four location domains and irrespective of time trend specification and inclusion of sibling FE, the treatment effect estimates are not only statistically insignificant but also quantitatively small. In fact, in the case of small cities and towns, we find negative effects. Similar

already to the left of the corresponding threshold in small cities and towns. Inducing extra mobility in such areas therefore is relatively harder.

findings also emerge if we use other years for the placebo reform. Although falsification tests cannot be definitive, these results provide support to our first identification strategy.

With the third exercise 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 co-reside with their parents). It might be driven also by selection if, as the 2009 reform increases the mobility rates of high school graduates, the characteristics of non-movers change, with post-reform stayers being potentially more negatively selected than pre-reform non-movers. In part, some of this concern is addressed by checking that pre- versus post-reform non-movers are comparable on the basis of their observable characteristics. Appendix Table A.1 confirms this, which will be important also for our analysis of spillover effects among non-movers in subsection 6.5 below. Another way of addressing this issue is to look at different control groups with slightly different overlaps in age and school involvement. As mentioned already, the results found with those other control groups are identical to our main estimates. The same emerges from the estimates in the first two columns of Table 2.

Another, perhaps more comprehensive, way to account for the potentially different right censoring faced by treated and untreated individuals is to estimate discrete time duration models, which combine both time varying covariates and flexible specifications of duration dependence (Jenkins, 1995). Such models also allow us to account nonparametrically for the variation in age among individuals in their last year of secondary school, due to grade repetition among low achievers on the one hand and grade skipping among academically talented students on the other. We perform the analysis on a sample of RLMS young adults aged 14–25 who are initially observed co-residing with their parents or grandparents.

The marginal effect estimates are shown in columns (v) and (vi) of Table 2. They are obtained from logit models with the same set of explanatory variables used before. Standard errors are clustered at the population center level and are computed using the delta method. The estimates confirm the results reported in Table 1. 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 3.5 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 around 7 percentage points. Although these figures are smaller than those in Table 1, they provide the same picture described earlier. By reducing the frictions inherent in the college application process, 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 conclusion.

*B. Heterogeneous Effects*

Next, we ask whether the 2009 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 parental education. 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 3 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 3 shows that there is no response heterogeneity by gender and ethnicity, regardless of whether we account for unobserved sibling fixed effects or not. This is interesting, for the treatment group has larger fractions of women and ethnic Russians. We also do not detect differential treatment effects in the top half of the household income distribution, suggesting that the reform has equally affected students from the bottom half.

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.[43] The reform thus 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 their parental home by about 20 percentage points, while graduates whose parents are not college educated have a leaving-home rates of 10 percentage points. The 10 point difference 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 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.

### 4.4 Using High School Students in Moscow and St. Petersburg as Controls

An alternative to specification (1) is to restrict the analysis to students in the last year of high school and estimate a model in which the control group is given by students who live in Moscow and St. Petersburg, that is,

$$\ell_{it} = \psi(t) + \widetilde{\alpha} I(t \geq s) + \sum_{k=1}^{3} \widetilde{\beta}_k R_{ik} \times I(t \geq s) + \mathbf{X}'_{it}\widetilde{\gamma} + \eta_{it}, \tag{2}$$

where the three indicator variables $R_{ik}$ ($k$=1,2,3) take, respectively, value 1 if student $i$ lives either in a major city (or regional capital) other than Moscow/St. Petersburg ($N$=407), a

[43] This may be 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.

small city or town ($N$=408), or a rural area ($N$=575), and 0 otherwise, while the rest of the notation is as in (1) with the dummy variables $R$ being also included in $\mathbf{X}$. We end up having a sample of 1,547 high school students, of which 157 (10%) are in the control group. The idea of this exercise is that students from Moscow and St. Petersburg may be good comparators since their incentives to move out have not changed after the reform, and focusing on people in their last year of high school is likely to reduce heterogeneity in the decision of leaving the parental home.

The results in column (i) of Table 4 strongly support the baseline estimates of Table 1. The reform led to an increase of 16 percentage points in the geographic mobility of students from small cities and towns. We find an increase of nearly 6 percentage points also in the case of high school graduates living in major cities (but this is significant only at the 10% level), and again no effect for students in rural areas.

### 4.5 Mechanisms

Several mechanisms might be at work to set in place the effect of the USE reform on geographic migration of high school graduates from small cities and towns. We investigate the relevance of three channels, which are not mutually exclusive, namely, distance, socioeconomic background, and knowledge.

The first channel is distance from the most prestigious higher education institutions, which are located in Moscow and St. Petersburg (and, to a lesser extent, in some of the major cities). Geographic distance can be seen as a proxy of the transportation costs faced by students during the pre-reform application process (since they needed to move to those cities to train for and take the admission exams) as well as during their studies (whenever they travelled back home). From Appendix Table A.1 we know that transportation costs for the average student have dramatically declined after the reform, mainly due to technological improvements and favorable oil prices. If, in terms of the framework outlined in Section 3, such costs make up an important fraction of the total cost $k$ faced by all students, we expect the function $f$ to be affected. In other words, it could be the case that the heterogeneity of the effects across location types is due to differential distance and hence also transportation

costs. To assess the impact of distance, we then estimate the following variant of (2)

$$\begin{aligned}\ell_{it} &= \psi(t) + \widetilde{\alpha} I(t \geq s) + \sum_{k=1}^{3} \widetilde{\beta}_k R_{ik} \times I(t \geq s) \\ &\quad + \pi \text{dist}_p + \sum_{k=2}^{3} \vartheta_k R_{ik} \times I(t \geq s) \times \text{dist}_p + \mathbf{X}'_{it}\widetilde{\gamma} + \xi_{it}, \end{aligned} \tag{3}$$

in which $\text{dist}_p$ is the distance to the nearest regional capital for students living in population center $p$.[44] On average, small cities and towns ($k = 2$) in our sample are 257 km away from their correspondent regional capitals (median 180 km), while rural areas ($k = 3$) are 165 km away (median 150 km). As in the case of equation (2), we estimate this new specification on our sample of students in their last year of high school.

We begin with a simplified version, in which we set $\widetilde{\beta}_2 = \widetilde{\beta}_3 = 0$ and $\vartheta_2 = \vartheta_3 = \vartheta$, so we do not distinguish between small cities and towns and rural areas. The results are in column (ii) of Table 4. They reveal that, once we control for location type, distance has no independent impact on geographic mobility of high school graduates (the null that $\pi = 0$ cannot be rejected). But the effect of the USE reform in an area in the periphery that is 100 km away from a regional capital ($\vartheta$) is estimated at 2 percentage points and is statistically significant. Because high school graduates from the small cities and towns are on average farther away from regional capitals than their counterparts located in rural areas, this result raises the possibility that geographic distance may play a role in determining our results across locations.

To further investigate this possibility, in column (iii) of Table 4, we present the estimates from the full specification in (3). These suggest that the effect of the reform on small cities and towns was strongly positive and significant regardless of distance to regional centers (see $\widetilde{\beta}_2$). For rural areas, however, the results are more subtle. The direct effect ($\widetilde{\beta}_3$) is negative but insignificant, while the triple interaction effect ($\vartheta_3$) is positive and significant at the 10% level. Taken together, these estimates suggest a very small positive impact on the likelihood of moving for students in rural locations that are at least 110 km away from a regional capital. In sum, therefore, distance cannot account for the effect of the reform in small cities and towns, and the fact that most high school students in rural areas are close enough to regional capitals may partly explain why we do not find an effect in such

[44] To facilitate the interpretation of the results, we measure distance in units of 100 km.

locations.

The second channel is socioeconomic background. Before the reform, parents without a post-secondary education qualification may have perceived the college application system as an ominous burden, which in and of itself could have made university access for their children less likely. This problem might have been particularly acute in non-urban areas, where a larger fraction of parents do not have a university degree. The results in Table 3 confirm that socioeconomic background — proxied by both parents having university qualifications — is a key mechanism for residents in small cities and towns. But, as documented in the Online Appendix, we find no evidence of the same effect among residents of rural areas. Thus, socioeconomic background matters but only in the areas where the pre-existing environment is conducive for the reform to be effective. This ties in nicely with the evidence on the quality of secondary schools discussed in subsection 4.2.

The third mechanism is related to knowledge of the application process. There is no available data on what people know (or do not know) about the application procedures used by universities to recruit students. An imperfect proxy is given by stated, self-reported intentions to attend college among all secondary school students (see, among others, Delavande and Zafar, 2014). The argument goes as follows. If the USE reform did change opportunities in a way that students and their parents (and teachers) understand, 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 towns 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 provide qualifications based on the USE. About 65% of the 14–19 year old students in our new sample are in this control group. The outcome variable takes value 1 if individuals state they have intention to study at a university in the next three

years, and zero otherwise.[45] With this new dependent 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 5 shows the relevant point estimates, along with their heteroskedasticity-robust standard errors. The introduction of the reform did 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 considerable increase of 40–60% with respect to the average stated intentions of high-school students from small cities and towns. Including sibling FE increases the standard errors and makes the impacts insignificant (columns (iii) and (vi)), but the FE point estimates are still sizable and comparable to our previous results.

This evidence is consistent with the baseline findings based on 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 expectations that they attend university while still in high school. A substantial change in default rules, such as that implied by the USE reform, might then lead to substantial changes in beliefs even though the weight of history can shape individuals' preferences rather deeply, as found in the case of the former East Germany by Alesina and Fuchs-Schündeln (2007).

## 5. Pre-college Mobility Among College Students and Graduates

So far we have focused on the mobility experienced by high school graduates out of their family of origin. As shown in Figure 3, about 95% of the post-reform home-leaving rate among high school graduates is attributable to individuals who move to enroll into a university program. But we do not know if a similar outflow was already occurring before the reform, although the reform did lead to a substantial step up in the probability of leaving

[45]The RLMS records the information into a binary (0/1) variable. Although this expectation is elicited with a non-probabilistic method, rather than asking people to express probabilities directly, 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.

home. Thus, we now complement our earlier perspective and concentrate on the mobility that university attendees and recent graduates experienced to enroll into college.

### 5.1 Data and Methods

Since 2009, RLMS respondents who attend, or have attended, a higher education institution are asked whether they changed their residence location for the purpose of starting university studies. Since 2010, the questionnaire also includes specific questions about the institution attended and its location. To limit issues of recall bias and diversity in the reasons to attend college, we only select respondents who began a university program on or after 2001 and who were between 15 and 22 years of age when they started their post-secondary studies. We end up with a sample of 3,610 individuals.

In what follows we estimate DD models of the following form:

$$m_{it} = \varphi(t) + \zeta D_{it} + \lambda D_{it} \times I(t \geq 2009) + \mathbf{H}'_{it}\rho + \nu_{it}, \tag{4}$$

where $m_{it}$ is an indicator variable that takes value one if individual $i$ lived somewhere other than the university's location prior to starting college at time $t$. The variable $I(\cdot)$ is a dummy that switches on if college attendance starts in 2009 or after, i.e., after the full implementation of the USE reform, while $D_{it}$ is equal to 1 if $i$ started college when he/she was 15–18 years old and 0 if he/she started at 19–22. Defining the treatment group in this way allows us to compare the average mobility rate of high school graduates who enroll into a university program immediately after completion of secondary school to the mobility rate of older individuals who are highly unlikely to have been admitted to college based on their USE exam results (see Section 2). $\lambda$ is the parameter that denotes the treatment effect.

The vector $\mathbf{H}_{it}$ includes personal demographic factors, such as sex, ethnicity, place of residence, and parental education. Importantly, the data also allow us to include a rich set of characteristics of the individual's higher education experience, such as field of study, college location, and university ranking. We include such variables as they allow us to check whether the reform achieved its stated objective of facilitating access to scientific programs in elite universities. When we exclude them from (4), since they could be seen as outcomes in and of themselves, the estimates on the other controls remain unchanged. Finally, $\varphi(t)$

captures time trends which, as in (1), are modeled either with a fully flexible set of time dummies that are common to treatment and control groups or with group-specific linear processes.

We estimate (4) using difference-in-difference (DD) and two-step Blundell-Costa Dias propensity-score matching (PSM) regressions. Because of the information used to construct our dependent variable and our estimating sample, sibling FE models cannot be identified in this case, as we have only one observation per individual.

Appendix Table A.2 reports the summary statistics of the main variables in **H** by group, both before and after the reform as well as the $p$-value of the balance test in the reform-off period. Individuals in both treatment and control groups are similar along a number of personal characteristics, such as sex, ethnicity, and previous geographic mobility. But there are a few pre-reform differences. For instance, university attendees and graduates have more educated parents (especially fathers), which confirms there is a great deal of intergenerational correlation in university education. They are also more likely to come from major cities, in particular from Moscow and St. Petersburg, where their post-secondary education programs are typically located.

### 5.2 Main Results

Table 6 reports the treatment effect estimates, $\lambda$, with their robust standard errors clustered at the population center level.[46] After the introduction of the USE reform, a simple DD comparison (without including time trend controls or other variables) implies that college students are 11.5 percentage points more likely to have come from a location other than the city where their university is based (column (i)). Allowing for a flexible time trend and including covariates reduces the effect to about 8.1 percentage points (column (ii)). The estimate in column (iii) is obtained by replacing the year dummies with group-specific linear time trends. This increases the effect to 11.4 percentage points. The PSM estimate with common time dummies and a 0.01 bandwidth for matching is just below 11 percentage points (column (iv)), and this is robust across different bandwidths ranging from 0.005 to 0.05.

[46]The Online Appendix contains the estimates on the variables included in **H**.

In line with what we documented in the previous section, the USE reform has then induced heightened mobility among those who enrolled into a university program. We next explore if this is associated with specific characteristics of college attendees or the universities themselves.

### 5.3 Heterogeneity

*Individual Attributes* — We consider three individual specific domains in which the effect of the reform could differ, i.e., sex, ethnic origin, and parental education (distinguishing maternal from paternal education). Each panel of Table 7 presents the results, showing in each case the estimated value of $\lambda$ as well as the coefficient on the interaction between the attribute of interest and $D_{it} \times I(t \geq 2009)$ obtained from the DD model (4) with a common flexible time trend. The level estimates with group-specific trends and the PSM estimates are very close to those shown here, and are thus not presented.

Men and individuals of non-Russian ethnic origins display a greater propensity of moving to start college studies than women and Russians respectively (see panels A and B). But only the latter of such differences is (marginally) statistically significant. The next two panels show that mobility also does not differ by parental education.

*University Characteristics* — Given the goal of the reform to democratize access to post-graduate studies and in particular to scientific programs in top institutions located in Moscow and St. Petersburg, we look at the possibility of different responses by field of study, college location, and university ranking.

We find that the effect on mobility among individuals who attended programs in universities located in Moscow and St. Petersburg (panel E) is 5 percentage points (53%) greater than the baseline estimates in column (ii) of Table 6. The estimated differential mobility to all other major and smaller cities instead is lower but such responses are not statistically significantly different from zero. The estimates in panel F indicate that the effect is substantially higher among students who enrolled into science majors: they experienced a two- to three-fold increase in the likelihood of having moved to be in college. Individuals in business, law, economics, and engineering do not show any change in mobility rates, while the large fraction of those enrolled in education programs experience a strong reduction in

mobility. Indeed, the USE reform was not introduced with an aim to facilitate participation to such (highly popular but less academic) programs.

Finally, the results in panel G show that mobility to the top 30 institutions in the country has *not* increased more than in others as a result of the reform. This finding is robust even if we change our focus to the top 10, the top 50, or the top 10–50 institutions, thus leaving out the most prestigious programs.

In sum, we strengthen our findings from the previous section that the USE reform induced more high school graduates to leave their parental home and enroll into university programs. This heightened mobility has happened equally among men and women and among students with different socioeconomic background, levelling the playing field between the sexes and different socioeconomic groups. In line with the reformers' stated intentions, we also observe more mobility to universities located in Moscow and St. Petersburg and to attend science programs, although not necessarily in the top institutions.

## 6. 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, and into university programs in Moscow and St. Petersburg, 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 affecting high school graduates who do not leave. Most of this analysis is performed at the household level, except that referring to parental labor supply and spillover effects, and uses the following DD approach:[47]

$$y_{jt} = \tau(t) + \xi d_{jt} + \phi d_{jt} I(t \geq s) + \mathbf{Q}'_{jt}\delta + u_{jt}, \tag{5}$$

where $\tau(t)$ is the time trend measured in the same two ways as we have done previously, and $d_{jt}$ is equal to 1 if household $j$ at time $t$ has at least one child in the last year of high

[47]See the Online Appendix for details on how we link households across RLMS rounds and summary statistics on the covariates used in the analysis.

school, and is equal to 0 for households that have at least one child but not in the last year of high school. We use a wide range of characteristics of the treated/comparison child to define the variables included in $\mathbf{Q}$, and find evidence of strong comparability on observables between the two groups.

We focus on the effects observed among households 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.[48]

### 6.1 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 estimate equation (5), in which the dependent variable takes value 1 if household $j$ observed at time $t$ makes a monetary transfer to an absent child in any of the following two years, $t+1$ or $t+2$, and 0 if no transfer occurs.[49]

Panel A of Table 8 reports the regression results by specification of $\tau(t)$, i.e., with a fully flexible time trend that is common to treatment and control groups (column (i)) and with group specific linear trends (column (ii)). The estimate in columns (i) indicates that high school students from small cities and towns who leave the parental home after graduation are 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

[48] At present there is no (individual or aggregate) information on universities attended in Russia, apart from what we analyzed in the previous section. Also, information on USE test results is not collected by the RLMS or other statistical agencies, and universities do not release data on their students and applicants. Thus, we cannot assess whether the composition of students changed, nor can we test if the cohorts admitted to university after the introduction of the reform were more (or less) geographically dispersed.

[49] We also redefined the outcome, from the viewpoint of the child, 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. Such results are in the Online Appendix.

to a 50% increase at the mean transfer probability. The estimate in columns (ii) is even larger, with the probability that parents make a transfer to their nonresident children going up to 23 percentage points, a staggering 77% increase.

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 requires a costly relocation away from home, is accompanied by higher financial transfers from parents to children.

### 6.2 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 savings. When we examined this outcome, no effect of the USE reform was found (see the Online Appendix). This could be attributed to the relatively coarse information on savings made available in the RLMS and to the fact that only 27% percent of Russian households report to have positive savings in the pre-reform period.

Another important aspect of family resources is given by 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 and aggregation. We then analyzed household shares in finer 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 8 show the USE reform effect on the share of educational expenditures in the total nondurable household expenditures. From the estimate in column (i) in which we impose a fully flexible common trend, we find that the reform led to an average increase of about 2.2 percentage points in the share of educational expenditures. This is a considerable impact, representing 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 reported in column (ii).

Despite the lack of statistical significance in this last case, such estimates and those in the previous subsection 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 2009 reform. They are also more likely to make financial transfers to their children and divert some of their resources 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 mainly in Moscow and St. Petersburg, and inducing more parental investments in, and transfers to, migrant children.

### 6.3 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 a few recent studies have found nonnegligible elasticity values (see the review in Keane, 2011). Perhaps unsurprisingly, the results in panel C of Table 8 show that father's monthly hours of work did not change with the introduction of the 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 (ii)). But none of these estimates is statistically significantly different from zero. The impact on paternal labor force participation is also small and never statistically significant (panel D).

Female (and maternal) labor supply elasticities, especially on the participation margin, are typically large (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 8 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, although local labor market conditions are accounted for in estimation. But they could also be inter-

preted as offering evidence that the USE reform did not create unanticipated labor market responses among parents of treated individuals.

### 6.4 Parental Divorce

Another dimension of family life that the reform could have unintentionally affected is family stability. Some parents, especially those in poorer households or with lower educational attainment, may find it difficult to face (or hard to justify) greater expenditures to support the schooling investment of their children. From our earlier analysis, we do not find any negative impact of the USE on family spending, other than educational expenditures. However, if these allocations are not agreed upon by both parents or if they are perceived as crowding out future spending, intra-family trust may be progressively eroded, and this in turn may strain family relations and lead to greater chances of union dissolution.[50] 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 (5) 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 8. Irrespective of the way in which time trends are modeled, the estimates show that the USE reform did not significantly affect family stability.

### 6.5 Spillover Effects Among Stayers

Besides the families of high school graduates who left their parental home, the 2009 reform might have also affected high school graduates who decided not to move, at least not immediately after graduation. (We label such graduates as 'stayers'.)[51] To ascertain this potential impact, we estimate multinomial logit regressions using the sample of stayers

[50] In many respects this can be seen as a 'surprise' in the sense used by Weiss and Willis (1993), although in their application, surprises consist of changes in the predicted earnings capacity of either spouse. Mencarini, Moroni, and Pronzato (2012) provide evidence of effects that go in the opposite direction, that is, from parental separation to the decision of children to leave home. In particular, they find that children who have experienced a parental separation — and co-residing with their mothers — tend to leave home earlier, but that the last child in the household, who would leave the mother alone if leaving, tends to delay his/her departure. It should be noted that, differently from such a result, the estimates in the Online Appendix reveal that coming from an intact family leads to a greater likelihood of leaving the parental home. In our analysis, however, we do not isolate the effect for the last child in the household from that of other siblings.

[51] As discussed in subsection 4.3 we find no evidence based on observables that, as the USE reform increases the mobility rates of high school graduates, the characteristics of non-movers in small cities and towns change.

in small cities and towns who keep co-residing with their parents after completing their secondary education. 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 8 reports the results that show marginal probability changes for work and study with respect to the base category. Regardless of how time trends are specified, we find a small increase in the probability of working (around 3.5 percentage points) and a decrease in the probability of continuing study in further education (around 6.5 percentage points). Both sets of impacts are statistically insignificant. The reduction in the likelihood of studying (albeit insignificant) should be interpreted in the context 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) equilibrium effects on the subpopulation of stayers that we cannot 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 often 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 baseline estimates indicate that the reform induced a substantially greater leaving home rate of 12–16 percentage points (a threefold increase, at least) 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 major universities and their secondary schools are generally of high quality. Two important channels seem to have played a role. One is parents' socioeconomic background, according to which having parents with university qualifications is a key predictor of mobility. The other is information, whereby high school students from small cities and towns were 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 definitions of the control group and different specifications of temporal trends (i.e., imposing highly flexible common trends or allowing for group specific linear trends), as well as the inclusion of a wide set of observable confounders and unobserved fixed effects that are shared among siblings. The same findings also emerge when we combine difference-in-differences with a two-step propensity score matching procedure, which pairs treated individuals with a subset of nontreated individuals who are closest to them in terms of observable characteristics, and when we estimate discrete time duration models and Cox proportional hazard models of leaving the parental home. Finally, the same results are confirmed with a subsample of young adults who are currently enrolled in university programs or have recently received a college degree.

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 2009 reform. These results are likely to reflect greater child investments among families whose children are in completion of their secondary 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. Similarly, we find no evidence of an impact on the labor market outcomes of stayers. 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 abatement in college application costs that the USE reform implied. Pre-reform application procedures imposed frictions that constrained high school graduates located in peripheral areas from optimally investing in their human capital. Their reduction induced a greater fraction of students from the periphery to apply to, and attend, university programs. As 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 college among low-income students, and democratizing access and widening participation to higher education.

Future work can build on our results and assess whether the USE reform affects later child outcomes (such as university graduation and scores, labor market participation, occupational choice, and wages among students who moved), or if it leads to undesirable spillover effects, other than those we tested. The analysis of possible changes in the secondary school sector, such as those documented by Cullen, Long, and Reback (2013) and Estevan et al. (2018) for Texas, seems another important avenue of research. Whether the USE-induced, more egalitarian access to higher education will have a lasting legacy affecting social mobility, family life, political democratization, and human rights across all Russian society remains to be seen in the longer run.

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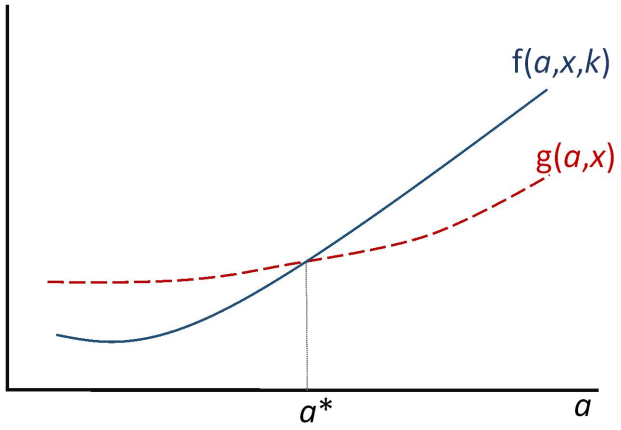
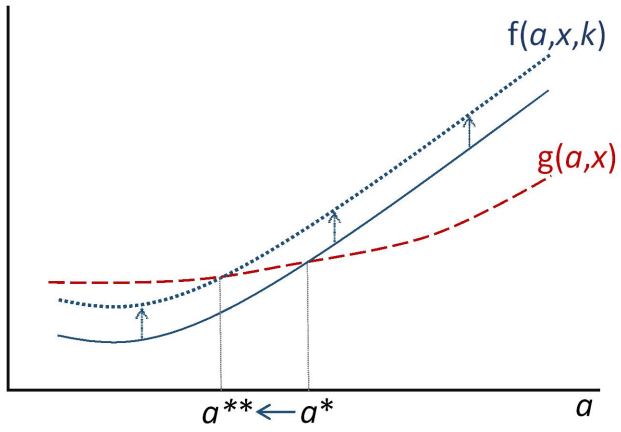
**Figure 1** – The Decision to Apply to an Elite College

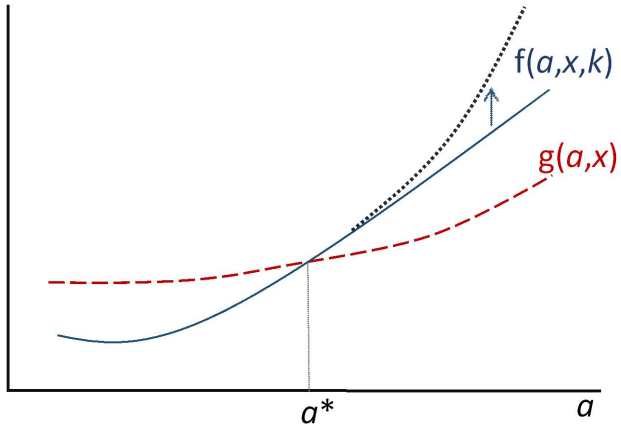



*Note*: The term $a$ on the horizontal axis denotes ability. The functions $f(\cdot)$ and $g(\cdot)$ are described in the text.

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

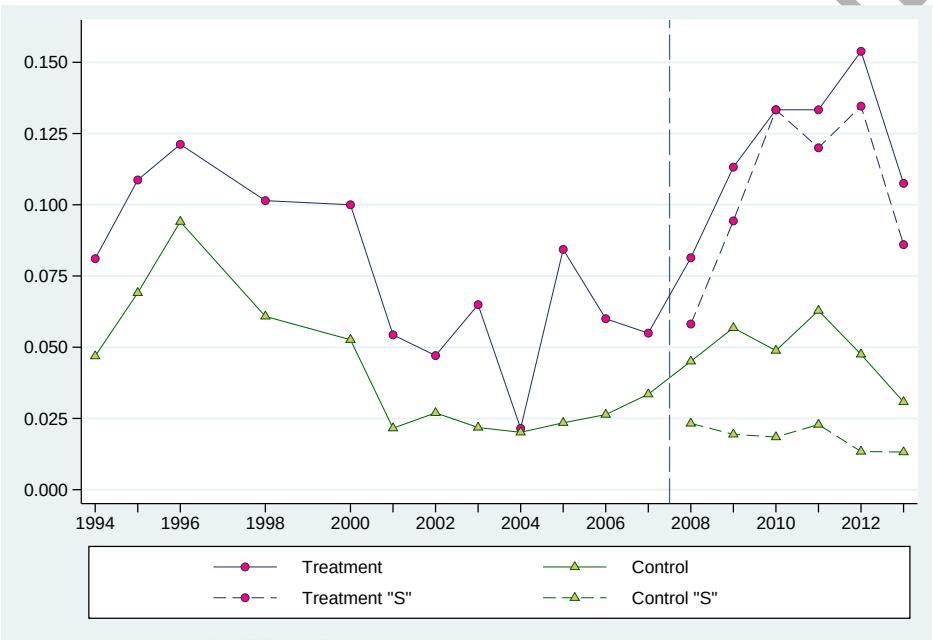



**(a)** Case with Mobility




**(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. 'Treatment' refers to students in the last year of high school, while 'Control' refers to young people aged 15–19 who are not in the last year of high school. For each of the treatment and control groups, 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.

**Table 1** – Effect of the USE Reform on Student Mobility

| | Flexible Common Trend | | | Group-specific Linear Trend | | | PSM |
|---|---|---|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) | (v) | (vi) | (vii) |
| Full Sample | | | | | | | |
| $\beta$ | 0.041** | 0.037** | 0.042** | 0.038 | 0.035 | 0.022 | 0.040** |
| | (0.018) | (0.017) | (0.018) | (0.028) | (0.027) | (0.033) | (0.018) |
| $N$ | 13,710 | 13,710 | 13,710 | 13,710 | 13,710 | 13,710 | 13,709 |
| Moscow & St. Petersburg | | | | | | | |
| $\beta$ | -0.014 | -0.020 | -0.009 | 0.071 | 0.069 | 0.059 | -0.005 |
| | (0.023) | (0.022) | (0.030) | (0.068) | (0.065) | (0.065) | (0.022) |
| $N$ | 1,373 | 1,373 | 1,373 | 1,373 | 1,373 | 1,373 | 1,369 |
| Other Major Cities | | | | | | | |
| $\beta$ | 0.057** | 0.049** | 0.039 | 0.018 | 0.013 | 0.015 | 0.049** |
| | (0.022) | (0.022) | (0.027) | (0.031) | (0.031) | (0.045) | (0.023) |
| $N$ | 4,015 | 4,015 | 4,015 | 4,015 | 4,015 | 4,015 | 4,013 |
| Small Cities and Towns | | | | | | | |
| $\beta$ | 0.119*** | 0.115*** | 0.128*** | 0.163*** | 0.157*** | 0.159** | 0.132*** |
| | (0.041) | (0.040) | (0.041) | (0.053) | (0.056) | (0.065) | (0.041) |
| $N$ | 3,801 | 3,801 | 3,801 | 3,801 | 3,801 | 3,801 | 3,776 |
| Rural Areas | | | | | | | |
| $\beta$ | -0.002 | -0.008 | 0.012 | -0.042 | -0.040 | -0.070 | -0.032 |
| | (0.027) | (0.026) | (0.027) | (0.052) | (0.051) | (0.054) | (0.037) |
| $N$ | 4,521 | 4,521 | 4,521 | 4,521 | 4,521 | 4,521 | 4,504 |
| Controls | No | Yes | Yes | No | Yes | Yes | |
| Sibling FE | No | No | Yes | No | No | Yes | |

*Note:* Columns (i) through (vi) report the estimate of $\beta$ obtained from linear probability models (equation (1)). The dependent variable is an indicator equal to 1 if the child leaves their parental home to another dwelling within one year of interview and 0 if the child stays in the household. The treatment group consists of students in the last year of high school. The control group consists of individuals aged 15–19 who are not in the last year of high school. Robust standard errors clustered at population center level are in parenthesis. Models differ on the type of time trend (wave dummies in columns (i)–(iii) and group-specific linear trends in columns (iv)–(vi)), the presence of control variables, and whether sibling fixed effects are included. $N$ is the number of person-wave observations. For sibling FE models, there are 3,439 households in the whole sample. The estimates on other explanatory variables included in each regression are reported in the Online Appendix. Column (vii) presents PSM-based estimates that are obtained from a two-step procedure.

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

**Table 2** – Sensitivity Checks

| | Alternative Control Group | | Falsification Test | | Duration Model | |
|---|---|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Moscow and | -0.008 | 0.037 | -0.012 | -0.032 | -0.002 | -0.017 |
| St. Petersburg | (0.038) | (0.117) | (0.032) | (0.032) | (0.017) | (0.025) |
| *N* | 517 | 517 | 930 | 930 | 1,143 | 1,124 |
| Other Major | 0.058** | 0.063 | 0.004 | -0.008 | 0.038** | 0.036** |
| Cities | (0.024) | (0.053) | (0.018) | (0.026) | (0.017) | (0.015) |
| *N* | 1,504 | 1,504 | 2,616 | 2,616 | 4,767 | 4,727 |
| Small Cities | 0.102** | 0.104 | -0.058 | -0.043 | 0.072** | 0.070** |
| and Towns | (0.049) | (0.095) | (0.038) | (0.034) | (0.036) | (0.035) |
| *N* | 1,690 | 1,690 | 2,474 | 2,474 | 4,361 | 4,343 |
| Rural Areas | -0.027 | -0.068 | 0.019 | 0.059 | -0.013 | -0.021 |
| | (0.030) | (0.043) | (0.042) | (0.042) | (0.026) | (0.026) |
| *N* | 1,672 | 1,672 | 2,781 | 2,781 | 5,050 | 5,037 |
| Wave Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | No | Yes |
| Sibling FE | No | Yes | No | Yes | No | No |

*Note*: The dependent variable is an indicator equal to 1 if the child leaves their parental home to another dwelling within one year of interview and 0 if the child stays in the household. Columns (i) and (ii) report estimates when the control group is restricted to vocational and technical school students. Columns (iii) and (iv) report estimates of the effect of a "placebo reform" imposed to occur in 2002 (sample restricted to the period 1994–2007). Columns (v) and (vi) report 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 parentheses. *N* is the number of person-wave observations. The control variables are listed in Table A.1. *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

**Table 3** – Heterogeneous Effects among Individuals from Small Cities and Towns

| | Level | FE |
|---|---|---|
| Female | -0.032 | -0.035 |
| | (0.072) | (0.082) |
| Russian ethnicity | 0.050 | 0.049 |
| | (0.096) | (0.103) |
| Born Elsewhere[a] | -0.188*** | -0.115 |
| | (0.060) | (0.083) |
| In the top half of the income distribution | -0.040 | -0.070 |
| | (0.083) | (0.093) |
| Both parents have university degrees | 0.203** | 0.225 |
| | (0.094) | (0.151) |
| *N* | 3,801 | 3,801 |

*Note*: The dependent variable is an indicator equal to 1 if the child leaves their parental home to another dwelling within one year of interview and 0 if the child stays in the household. 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 4** – Mobility of Students in Their Last Year of High-school

| | Equation (2) | Equation (3) Restricted | Equation (3) Full |
|---|---|---|---|
| | (i) | (ii) | (iii) |
| Regional Centers ($\widetilde{\beta}_1$) | 0.056* | 0.034 | 0.057* |
| | (0.030) | (0.031) | (0.030) |
| Small Cities & Towns ($\widetilde{\beta}_2$) | 0.158*** | | 0.158*** |
| | (0.049) | | (0.058) |
| Rural Areas ($\widetilde{\beta}_3$) | 0.006 | | -0.038 |
| | (0.036) | | (0.041) |
| Effect of Distance: | | | |
| Baseline ($\pi$) | | 0.003 | 0.006 |
| | | (0.006) | (0.006) |
| Common Effect ($\vartheta$) | | 0.020** | |
| | | (0.008) | |
| Small Cities and Towns ($\vartheta_2$) | | | -0.000 |
| | | | (0.011) |
| Rural Areas ($\vartheta_3$) | | | 0.036* |
| | | | (0.021) |
| $N$ | 1,547 | 1,547 | 1,547 |

*Note*: The dependent variable is an indicator equal to 1 if the child leaves their parental home to another dwelling within one year of interview and 0 if the child stays in the household. The sample is restricted to students in their last year of high-school. $N$ is the number of observations. Students residing in Moscow and St. Petersburg are the control group. Other locations are considered treated. The estimates are obtained from linear probability models. Robust standard errors clustered at population center level are in parenthesis. All specifications include wave dummies and the control variables listed in Table A.1.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

**Table 5** – College Attendance Intentions

| | Flexible Common Trend | | | Group-Specific Time Trend | | |
|---|---|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
| Moscow and St. Petersburg | -0.080 | -0.136 | 0.148 | 0.071 | 0.017 | 0.280 |
| | (0.182) | (0.187) | (0.612) | (0.305) | (0.308) | (0.720) |
| *N* | 276 | 276 | 276 | 276 | 276 | 276 |
| Other Major Cities | -0.023 | -0.056 | -0.029 | -0.048 | -0.065 | -0.217 |
| | (0.083) | (0.076) | (0.178) | (0.129) | (0.121) | (0.198) |
| *N* | 903 | 903 | 903 | 903 | 903 | 903 |
| Small Cities and Towns | 0.236** | 0.214* | 0.225 | 0.308** | 0.314** | 0.190 |
| | (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 |
| Controls | No | Yes | Yes | No | Yes | Yes |
| Sibling FE | No | No | Yes | No | No | Yes |

*Note*: The dependent variable is an indicator equal to 1 if the student expects to attend university (available for RLMS rounds 2006–2014). All high-school students are considered treated. Full-time students 14-19 not in high-school or university are used as control group. Estimates are obtained from linear probability models. Robust standard errors clustered at population center level are in parentheses. *N* is the number of person-wave observations. All specifications include wave dummies. The control variables are listed in Table A.1.
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level.

**Table 6** – Mobility Prior to College

| | Simple DD | Flexible Common Trend | Group-Specific Time Trend | PSM |
|---|---|---|---|---|
| | (i) | (ii) | (iii) | (iv) |
| $\lambda$ | 0.115*** | 0.081*** | 0.114** | 0.109** |
| | (0.037) | (0.029) | (0.051) | (0.033) |
| *N* | 3,610 | 3,610 | 3,610 | 3,595 |

*Note*: Estimates of $\lambda$ (see equation (4)). The dependent variable is an indicator equal to 1 if the individual changed their residence location for the purpose of starting university studies. Robust standard errors are in parentheses. *N* is the number of individuals. Column (i) shows the estimate without time trends and without control variables. Columns (ii) and (iii) include, respectively, a flexible common time trend and group-specific linear time trends, as well as the controls for individual and higher education program characteristics listed in Table A.2. The estimate in column (iv) is obtained applying PSM on these same characteristics. The estimates on other explanatory variables included in each regression are reported in the Online Appendix.
*** indicates statistical significance at the 1% level, and ** at the 5% level.

**Table 7** – Heterogeneous Effects on Mobility Prior to College

| | |
|---|---|
| A. Gender | |
| $\lambda$ (base = Male) | 0.133*** |
| | (0.045) |
| Female | -0.085 |
| | (0.058) |
| B. Ethnicity | |
| $\lambda$ (base = Non-Russian) | 0.203*** |
| | (0.0683) |
| Russian | -0.143* |
| | (0.075) |
| C. Mother's Education | |
| $\lambda$ (base = No higher education) | 0.077** |
| | (0.030) |
| Mother has university degree or more | 0.094 |
| | (0.138) |
| D. Father's Education | |
| $\lambda$ (base = No higher education) | 0.082*** |
| | (0.030) |
| Father has university degree or more | 0.012 |
| | (0.166) |
| E. University Location | |
| $\lambda$ (base = Moscow & St Petersburg) | 0.131** |
| | (0.055) |
| Other major cities | -0.062 |
| | (0.071) |
| Small cities and towns | -0.076 |
| | (0.076) |
| F. Field of Study | |
| $\lambda$ (base = Natural and social sciences) | 0.165*** |
| | (0.062) |
| Business, economics, and law | -0.102 |
| | (0.075) |
| Education | -0.323*** |
| | (0.125) |
| Engineering | -0.040 |
| | (0.087) |
| G. University Ranking | |
| $\lambda$ (base = 31+) | 0.079*** |
| | (0.030) |
| In top 30 | 0.049 |
| | (0.091) |

*Note*: Estimates of heterogeneous effects are based on regressions similar to equation (4), in which a relevant characteristic is interacted with $D_{it}$, $I(t \geq 2009)$, and $D_{it} \times I(t \geq 2009)$. The dependent variable is an indicator equal to 1 if the individual changed their residence location for the purpose of starting university studies. Robust standard errors are in parentheses. All regressions include a fully flexible common time trend, as well as the control variables listed in Table A.2. Robust standard errors are in parentheses. The number of observations (individuals) is 3,610 (except for panel F, where we restrict to 3,430 observations with non-missing field of study).
*** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 1% level.

**Table 8** – Other Outcomes (Small Cities and Towns)

| | Flexible Common Trend (i) | Group-Specific Time Trends (ii) |
|---|---|---|
| A. Monetary transfers to children [mean of dep. var. = 0.302][a] | | |
| $\phi$ | 0.150** | 0.232*** |
| | (0.063) | (0.074) |
| $N$ | 3,465 | 3,465 |
| B. Household expenditure share on education [mean of dep. var. = 0.032][b] | | |
| $\phi$ | 0.022*** | 0.0062 |
| | (0.006) | (0.0125) |
| $N$ | 3,914 | 3,914 |
| C. Father's monthly hours of work [mean of dep. var. = 140.6][c] | | |
| $\phi$ | -3.1 | 18.3 |
| | (9.2) | (12.4) |
| $N$ | 2,771 | 2,771 |
| D. Father's labor force participation [mean of dep. var. = 0.915][d] | | |
| $\phi$ | -0.021 | 0.000 |
| | (0.032) | (0.052) |
| $N$ | 2,771 | 2,771 |
| E. Mother's monthly hours of work [mean of dep. var. = 123.][c] | | |
| $\phi$ | 1.8 | -0.3 |
| | (7.2) | (14.6) |
| $N$ | 3,708 | 3,708 |
| F. Mother's labor force participation [mean of dep. var. = 0.873][d] | | |
| $\phi$ | 0.003 | -0.005 |
| | (0.028) | (0.052) |
| $N$ | 3,708 | 3,708 |
| G. Parental divorce [mean of dep. var. = 0.028][e] | | |
| $\phi$ | -0.021 | 0.005 |
| | (0.017) | (0.031) |
| $N$ | 2,795 | 2,795 |

– continues on next page

**Table 8 (continued)**

| | Flexible Common Trend (i) | Group-Specific Time Trends (ii) |
|---|---|---|
| H. Spillover effects [mean of "work" = 0.371; mean of "study" = 0.512][f] | | |
| $\phi$ (Work) | 0.036 | 0.035 |
| | (0.047) | (0.071) |
| $\phi$ (Study) | -0.067 | -0.065 |
| | (0.057) | (0.083) |
| $N$ | 3,364 | 3,364 |

*Note*: In panels A through G, $\beta$ 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. $N$ is the number of household-wave observations in panels A, B, and G, and the number of person-wave observations in panels C–F and H, respectively.

[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.

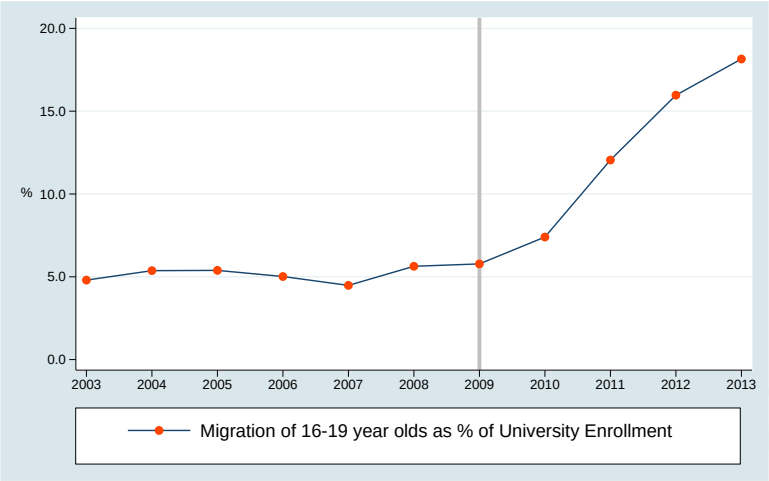
[d] Equals 1 if in work, and 0 otherwise.

[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 co-reside 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.

**Figure A.1** – Student Migration




*Note*: The figure presents the ratio of internal student migration relative to university enrolment. Migration figures come from administrative records for fifteen large Russian cities compiled by the Federal Migration Service. Enrolment figures correspond to the first year cohort in all universities in the administrative area (including Master's level programs). They come from the Ministry of Education of the Russian Federation.

**Table A.1** – Descriptive Statistics for Home Leaving Mobility

| | Control Group | | Treatment Group | | Balancing Test |
|---|---|---|---|---|---|
| | Reform off | Reform on | Reform off | Reform on | p-value |
| Individual Characteristics | | | | | |
| Female | 0.469 | 0.479 | 0.558 | 0.562 | 0.000 |
| Age (years) | 17.26 | 17.36 | 16.7 | 17.2 | 0.000 |
| Russian ethnicity | 0.612 | 0.487 | 0.570 | 0.470 | 0.004 |
| Born elsewhere[a] | 0.233 | 0.207 | 0.256 | 0.201 | 0.167 |
| Household Demographics | | | | | |
| Household Size | 4.00 | 3.96 | 4.02 | 3.99 | 0.619 |
| No. siblings, 0–6 | 0.04 | 0.11 | 0.05 | 0.12 | 0.979 |
| No. siblings, 7–14 | 0.35 | 0.31 | 0.38 | 0.35 | 0.062 |
| No. siblings, 15–19 | 0.27 | 0.17 | 0.23 | 0.17 | 0.019 |
| No. siblings, 20–24 | 0.19 | 0.16 | 0.18 | 0.14 | 0.445 |
| No. siblings, 25+ | 0.06 | 0.06 | 0.05 | 0.05 | 0.415 |
| Other children, 15–19 | 0.01 | 0.01 | 0.01 | 0.01 | 0.469 |
| Mother in household (=1, if yes) | 0.949 | 0.940 | 0.968 | 0.949 | 0.008 |
| Father in household (=1, if yes) | 0.746 | 0.718 | 0.799 | 0.754 | 0.000 |
| Both parents in household (=1, if yes) | 0.734 | 0.705 | 0.793 | 0.742 | 0.000 |
| No. grandparents in household | 0.24 | 0.30 | 0.23 | 0.29 | 0.509 |
| Socioeconomic Status | | | | | |
| Mother has a university degree (=1, if yes) | 0.175 | 0.239 | 0.216 | 0.310 | 0.004 |
| Father has a university degree (=1, if yes) | 0.127 | 0.137 | 0.173 | 0.176 | 0.001 |
| Both parents have university degrees (=1, if yes) | 0.066 | 0.081 | 0.089 | 0.113 | 0.012 |
| Home owners (=1, if yes) | 0.911 | 0.909 | 0.899 | 0.920 | 0.299 |
| Own a dacha (=1, if yes) | 0.218 | 0.194 | 0.208 | 0.216 | 0.749 |
| Own an extra apartment (=1, if yes) | 0.064 | 0.101 | 0.061 | 0.109 | 0.439 |
| Own a car (=1, if yes) | 0.357 | 0.501 | 0.429 | 0.536 | 0.000 |
| Median Income Rank[b] | 0.490 | 0.471 | 0.503 | 0.492 | 0.168 |
| Location | | | | | |
| Moscow[c] | 0.083 | 0.076 | 0.076 | 0.088 | 0.434 |
| St. Petersburg[c] | 0.038 | 0.027 | 0.030 | 0.024 | 0.380 |
| Other major cities[d] | 0.304 | 0.296 | 0.280 | 0.272 | 0.025 |
| Small cities and towns[d] | 0.279 | 0.267 | 0.267 | 0.254 | 0.454 |
| Rural areas[d] | 0.296 | 0.334 | 0.347 | 0.363 | 0.000 |
| Federation Districts | | | | | |
| North and North Western | 0.107 | 0.091 | 0.091 | 0.098 | 0.372 |
| Central and Chyornyzyomla | 0.228 | 0.237 | 0.200 | 0.265 | 0.054 |
| Volga | 0.165 | 0.168 | 0.200 | 0.174 | 0.007 |
| North Caucasus | 0.140 | 0.184 | 0.154 | 0.180 | 0.263 |
| Ural | 0.170 | 0.143 | 0.135 | 0.118 | 0.002 |
| Western Siberia | 0.093 | 0.083 | 0.102 | 0.093 | 0.354 |
| East Siberia | 0.098 | 0.093 | 0.119 | 0.072 | 0.354 |
| Local Conditions | | | | | |
| Travel cost to Moscow[e] | 16.7 | 3.6 | 21.7 | 3.4 | 0.116 |
| Travel cost to state capital[e] | 1.8 | 0.4 | 1.7 | 0.4 | 0.793 |
| Youth unemployment rate[f] | 0.169 | 0.133 | 0.176 | 0.135 | 0.012 |
| *N* | 8,571 | 5,574 | 1,054 | 788 | |

*Note*: Figures are means by group (treatment and control) and period (reform-off = 1994–2007, reform-on = 2008–2014). The treatment group consists of students in the last year of high school. The control group consists of individuals aged 15–19 who are not in the last year of high school. The balancing test is a two-tail test of the equality of means across groups in the reform-off period.

[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** – Descriptive Statistics for College Attendants and Recent Graduates

| | Control Group | | Treatment Group | | Balancing Test |
|---|---|---|---|---|---|
| | Reform off | Reform on | Reform off | Reform on | p-value |
| Place of Residence | | | | | |
| Moscow & St. Petersburg | 0.193 | 0.155 | 0.152 | 0.129 | 0.013 |
| Other Major Cities | 0.369 | 0.425 | 0.467 | 0.450 | 0.000 |
| Small Cities and Towns | 0.277 | 0.240 | 0.235 | 0.199 | 0.034 |
| Rural Areas | 0.161 | 0.180 | 0.146 | 0.222 | 0.342 |
| North & North Western | 0.070 | 0.053 | 0.042 | 0.049 | 0.010 |
| Central & Chyornyzyomla | 0.154 | 0.183 | 0.210 | 0.219 | 0.001 |
| Volga | 0.120 | 0.148 | 0.157 | 0.157 | 0.013 |
| North Caucasus | 0.115 | 0.121 | 0.144 | 0.117 | 0.053 |
| Ural | 0.172 | 0.132 | 0.114 | 0.108 | 0.000 |
| Western Siberia | 0.096 | 0.091 | 0.090 | 0.079 | 0.662 |
| East Siberia | 0.076 | 0.114 | 0.090 | 0.141 | 0.282 |
| Individual Characteristics | | | | | |
| Female | 0.592 | 0.580 | 0.611 | 0.632 | 0.393 |
| Russian ethnicity | 0.872 | 0.824 | 0.854 | 0.819 | 0.225 |
| Born elsewhere | 0.367 | 0.352 | 0.400 | 0.368 | 0.132 |
| Mother has univ. degree | 0.056 | 0.053 | 0.075 | 0.047 | 0.067 |
| Father has univ. degree | 0.035 | 0.027 | 0.065 | 0.038 | 0.001 |
| Field of Study | | | | | |
| Natural Sciences | 0.078 | 0.089 | 0.121 | 0.177 | 0.004 |
| Business, Law & Economics | 0.477 | 0.482 | 0.400 | 0.393 | 0.001 |
| Social Sciences & Humanities | 0.090 | 0.126 | 0.128 | 0.138 | 0.006 |
| Education | 0.089 | 0.073 | 0.080 | 0.068 | 0.461 |
| Engineering | 0.193 | 0.219 | 0.205 | 0.205 | 0.528 |
| Missing Field | 0.072 | 0.011 | 0.066 | 0.019 | 0.121 |
| University Location | | | | | |
| Moscow & St. Petersburg | 0.243 | 0.228 | 0.203 | 0.211 | 0.025 |
| Other Major Cities | 0.403 | 0.452 | 0.416 | 0.432 | 0.567 |
| Small Cities | 0.353 | 0.320 | 0.381 | 0.357 | 0.193 |
| Rental Index (Moscow=100) | 38.816 | 41.196 | 37.522 | 37.866 | 0.397 |
| Missing Rental Index | 0.305 | 0.247 | 0.302 | 0.301 | 0.885 |
| University Ranked Top 50 | 0.088 | 0.116 | 0.102 | 0.140 | 0.268 |
| Observations | 719 | 438 | 1618 | 835 | |

*Note*: Figures are means by group (treatment and control) and period (based on starting year in college, i.e., reform-off = 2001–2008, reform-on = 2009–2015). The balancing test is a two-tail test of the equality of means across groups in the reform-off period.