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Obvious Mistakes in a Strategically Simple College Admissions Environment

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Abstract

In a centralized marketplace that was designed to be simple, we identify participants whose choices are dominated. Using administrative data from Hungary, we show that college applicants make *obvious mistakes*: they forgo the free opportunity to receive a tuition waiver worth thousands of dollars. At least 10 percent of the applicants made such mistakes in 2013. Costly mistakes have externalities: they transfer tuition waivers from high- to low-socioeconomic status students, and increase the number of students attending college. To shed light on the mechanisms underlying mistakes, we exploit a reform that substantially increased the selectivity of admission with financial aid in some fields of study. Increased admission selectivity raises the likelihood of making obvious mistakes, especially among high-socioeconomic status and low-achieving applicants. Our results suggest that mistakes are more common when their expected cost is lower. Still, the average cost of a mistake in 2013 was 114-365 dollars.

Keywords: College admissions, dominated strategies, market design, obvious misrepresentation, school choice, strategy-proof

JEL Codes: C70, D47, D61, D63

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1 Introduction

Around the world, a growing number of students are assigned to schools through centralized clearinghouses. An increasing share of these clearinghouses adopt strategically simple mechanisms (Pathak, 2016). Strategy-proof mechanisms, where participants have a dominant strategy of reporting their true preferences, are viewed to be appealing because of their strategic simplicity.[1] In practice, many clearinghouses do not employ a strategy-proof mechanism, but still choose a strategically simple mechanism, where ranking alternatives in a way that is inconsistent with one's preferences is a dominated strategy (even though the choice of which alternatives to rank may require strategic thinking).[2]

We ask a basic question in a high-stakes environment: *Do participants actually rank alternatives according to their true preferences when a strategically simple mechanism is in place?* We provide a negative answer. We then investigate the causes and consequences of the mistakes we detect. We find that increased admission selectivity has a large causal effect on dominated-strategy play. Additionally, mistakes result in the transfer of tuition waivers from rich to poor applicants, and increase the number of students attending college.

We use administrative data to study the Hungarian college admissions process, which has been using a strategically simple version of the student-proposing Deferred Acceptance Mechanism (DA; Gale and Shapley, 1962) since 2008. Each year, about 100,000 students participate in this process (Bíró, 2011). A special feature of this market is that applicants rank alternatives that have an intrinsic natural ranking: admission to the same study program with and without funding. Avery and Hoxby (2004) call such environments a "*no-trade-off situation*." We find that a large fraction of applicants makes an *obvious mistake*: they submit a Rank Ordered List (ROL) that is inconsistent with the natural ranking, and thus forgo the free opportunity to receive a tuition waiver worth thousands of dollars,[3] even though this behavior has no benefit. Such ROLs are not optimal for a rational human-capital investor. Our most conservative lower bound suggests that in 2013, about 11 percent of high-school senior applicants made such a mistake, and between 8 and 16 percent of these mistakes were costly (meaning that the applicant could have received a more desirable assignment had she asked for it). The average cost of these mistakes is between

[1] Strategy-proof mechanisms are considered desirable for a variety of other reasons. First, they are robust in the sense that equilibrium prediction does not depend on agents' beliefs, as they all have a dominant strategy (Wilson, 1987; Bergemann and Morris, 2005). Second, they are thought to *level the playing field* in the sense that, thanks to their simplicity, they do not give an advantage to more strategically sophisticated participants (Pathak and Sönmez, 2008; Abdulkadiroglu et al., 2006). Finally, they generate information that may be useful for policymakers (Machado et al., 2012; Roth, 2008). Azevedo and Budish (2013) show that in large markets strategy-proofness often holds approximately.

[2] Pathak and Sönmez (2013) report on dozens of school-choice systems around the world that implemented strategically simple versions of DA, only one of which (Boston Public Schools') was strategy-proof. Like them, we assume that applicants only care about their own assignment.

[3] Tuition varies between programs. In 2013, it ranged from 2,000 to 23,000 dollars for three years, with a median of \$3,800 and a mean of \$4,500. The per capita GDP in 2013 was \$10,300.

114 and 365 dollars.

Next, we ask, *who makes obvious mistakes and how do these mistakes affect others?* We find that obvious mistakes are more common among applicants with lower academic ability. Additionally, all else equal, students coming from higher socioeconomic status (SES) families are more likely to make mistakes. Costly obvious mistakes result in the transfer of funding from high- to low-SES applicants. Moreover, as a large fraction of applicants, especially low-SES applicants, exclusively rank funded positions, costly mistakes increase the number of students admitted to college.

Having found that dominated-strategy play in strategically simple environments is prevalent, costly, and could have significant externalities, we address a long-standing question: *what causes this behavior?* We establish a causal relationship between the selectivity of admission to funded positions and obvious mistakes. Our difference-in-differences design leverages variation stemming from a sharp change in the Hungarian government policy. Motivated by fiscal concerns, in 2012, the government severely reduced the number of tuition waivers in several fields of study (business and economics, legal studies, and social sciences), significantly increasing the selectivity of admission to funded positions in these fields. Other fields remained largely unaffected. Our estimates suggest that as a result of the rise in admission selectivity, obvious mistakes by high-school senior applicants increased by 19 percentage points (relative to a baseline of 6 percent). The effect is heterogeneous, and is stronger among students of low academic ability and students of high socioeconomic status. We also find that, within ROL, obvious mistakes are more likely with respect programs in which admission with funding is more selective. These findings are consistent with our preferred explanation: applicants with a lower expected loss from mistakes are making more of them.

The fact that we label the behavior we document as "mistakes" or dominated-strategy play is not innocuous. It relies crucially on the assumption that agents' utility depends only on the realized assignment, and more specifically only on the agent's own assignment. While this assumption is standard in the matching markets design literature (e.g., Pathak and Sönmez, 2013), there are other possible explanations (e.g, social preferences, self-image concerns, and mistrust). The patterns we detect in the data rule out many of these explanations, and others are less plausible in light of institutional details.

Two recurring themes in studies evaluating dominated-strategy play in strategically simple environments are the negative correlation of this behavior with cognitive ability and its positive correlation with the expectation of fiercer competition (Hassidim et al., 2017a). In practice, applicants' cognitive ability and desirability are positively correlated in the field, making it difficult to disentangle the two components (Hassidim et al., 2016; Rees-Jones, 2017b; Artemov et al., 2017). In the laboratory, Basteck and Mantovani (2016) find that mistakes under the DA mechanism are more common among applicants with low cognitive ability, and Guillen and Hakimov (2016) find that the same holds under the strategy-proof Top Trading Cycle (Abdulkadiroglu and Sönmez, 2003). Hassidim et al. (2016) document a strong causal relationship between expected competition and preference misrepresentation in the laboratory. This study is the first to establish the causal relationship between admission selectivity and dominated-strategy play in the field, ruling out cognitive limitations as a sole determinant of dominated-strategy play in high-stakes environments. We also corroborate the correlation between cognitive ability and dominated-strategy play.

Our findings on the prevalence of obvious mistakes are consistent with several recent studies suggesting that large fractions of participants in strategically simple environments use dominated strategies. In the laboratory, Chen and Sönmez (2006) find that approximately 30 percent of the participants misrepresented their preferences under DA. Subsequent laboratory experiments that employ numerous variants of the matching environment corroborate this finding.[4] In the field, Gross et al. (2015), Chen and Pereyra (2017), and Rees-Jones (2017b) document dominated-strategy play in strategically simple high-stakes environments using survey evidence. Relying exclusively on observational data, Hassidim et al. (2016) detect obvious mistakes in the Israeli Psychology Master's Match (IPMM) and Artemov et al. (2017) do so in a centralized college admissions market in Australia. Our paper complements these studies by documenting dominated-strategy play in a large, well-established market, using exclusively observational data. Unlike these papers, our lower bound on the cost of mistakes is substantial.

More broadly, dominated choices have been documented in other high-stakes environments, such as health insurance (Handel and Kolstad, 2015; Bhargava et al., 2017) and retirement savings (Choi et al., 2011). We identify dominated choices in an environment that was designed to simplify choice: there are no complex trade-offs or menus, hassle cost is minimal, and the dynamic aspect is limited.

Our work is also related to the large literature on suboptimal behavior in education markets (e.g., Hoxby and Avery, 2012). This literature finds that informational frictions about the cost of application, financial aid, and the returns to college attendance, as well as the complexity of the application for financial aid, play an important role, and that low-SES families are particularly affected (Avery and Kane, 2004; Hastings and Weinstein, 2008; Jensen, 2010; Ajayi, 2011; Bettinger et al., 2012; Hoxby and Turner, 2013; Hastings et al., 2015; Pallais, 2015; Andrabi et al., 2017). In the context of centralized school choice, numerous studies document evidence of suboptimal play when strategically demanding mechanisms, such as the immediate acceptance (i.e., Boston) mechanism, are in place (e.g., Abdulkadiroglu et al., 2006; De Haan et al., 2016; He, 2017). We contribute to the literature by studying a long-standing centralized market that was designed to be strategically simple, where information is accessible and abundant, and focusing on mistakes

[4]Examples include Braun et al. (2014), Calsamiglia et al. (2010), Chen and Kesten (2011), Ding and Schotter (2015), Ding and Schotter (2016), Echenique et al. (2016), Featherstone and Niederle (2016), Guillen and Hing (2014), Guillen and Hakimov (2014), Klijn et al. (2013), Pais and Pintér (2008), Pais et al. (2011), and Zhu (2014).

that are unlikely to be caused by information frictions. Yet, we find that a substantial fraction of applicants make such mistakes, and this behavior is more common among urban and high-SES applicants. We conclude that other frictions, such as lack of comprehension of the way the market clears, are also important.

Understanding the causes and correlates of dominated-strategy play in strategically simple environments is important for several reasons. First, in recent years economists often take on the role of engineers (Roth, 2002) and, increasingly, the role of "*plumbers*" (Duflo, 2017), in the sense that they make practical design decisions in the field, and adjust them as needed. For example, according to traditional theoretical analysis giving publicity to affirmative action policies should have no effect on the allocation when a strategy-proof mechanism is in place. However, if the expectation of highly selective admissions causes mistakes in strategy-proof environments (consistently with our findings), then giving publicity to affirmative action programs could amplify their effectiveness by reducing the frequency of mistakes among disadvantaged applicants.

Second, the causes and correlates of mistakes in strategically simple environments could inform researchers about the mechanisms underlying this behavior. This, in turn, could found new and more predictive classifications of allocation mechanisms according to their "simplicity" (Cason et al., 2006; Li, 2017b,a; Zhang and Levin, 2017). More immediately, it could provide "*plumber-economists*" guidance on how to communicate with participants and which populations should be particularly targeted.[5] As pointed out by Pathak (2016), "*Efforts to improve how participants interact with market designs ... hold great promise to complement research on market clearing algorithms.*"

Third, reported preferences are often used to inform policymakers about the relative desirability of different allocations (schools, hospital internships, etc.). This information is particularly important due to the absence of market clearing prices.[6] According to the traditional approach, preferences that are reported to strategically simple mechanisms could be interpreted at face value. But if, for example, agents tend to lower the ranking of desirable options where they expect fiercer competition (as we indeed find), a straightforward interpretation of school-choice data would exaggerate the importance applicants attach to proximity in the common case where individuals have priority in their neighborhood schools. Similarly, if certain groups in the population have a higher tendency to (erroneously) misrepresent their preferences, then the choice data may reflect

[5]The Center on Reinventing Public Education states in a report on the Denver and New Orleans school-choice systems that "[*n*]*one of the parents we spoke with could explain to us how the matching algorithm worked. Both Denver and New Orleans leaders aggressively conveyed the optimal choosing strategy to parents, and many of the parents we spoke with had received the message. Parents reported to us that they were told to provide the full number of choices in their true order of preference. The problem was that few parents actually trusted this message. Instead, they commonly pursued strategies that matched their own inaccurate explanations of how the match worked*" (Gross et al., 2015).

[6]Indeed, the Center on Reinventing Public Education states in a report on the Denver and New Orleans school choice systems in its abstract that "[*e*]*ducation leaders in Denver and New Orleans are making efforts to help parents become more informed and confident choosers, and to use the data provided by the enrollment system to manage the supply of schools*." (Source: http://eric.ed.gov/?id=ED556474. Accessed: 10/4/2016.)

their preferences less accurately. Artemov et al. (2017) and Fack et al. (2017) propose an alternative approach.

The remainder of the paper is organized as follows. Section 2 describes the Hungarian higher-education system, and the admissions process in particular. Section 3 describes our data. Section 4 presents results on the prevalence and costs of obvious mistakes, as well as their correlation with applicants' characteristics. In Section 5 we begin by documenting the correlation between admission selectivity and obvious mistakes. We then lay out our empirical strategy, and establish a causal relationship between admission selectivity and obvious mistakes. Section 6 analyzes the impact of obvious mistakes on other applicants. Section 7 discusses possible explanations of our findings, and Section 8 concludes.

2 College Admissions in Hungary

In this section, we describe college admissions in Hungary. We begin, in Section 2.1, by explaining the centralized admissions process and defining obvious mistakes. In Section 2.2, we describe the 2012-2013 reforms, which we exploit to study the causal effect of admission selectivity on obvious mistakes.

2.1 The Centralized Admissions Process

Higher education in Hungary is a three-cycle system (bachelor's, master's, doctorate), where bachelor's degrees typically require three years to complete (four years in a few instances), and master's degrees typically require two years. Admissions to all higher education programs is controlled centrally by the government. Each year, about 100,000 prospective students apply to bachelor's degree programs through a centralized clearinghouse, and approximately 60 percent are assigned.

College admissions have been organized through a centralized scheme since 1985. The centralized clearinghouse is managed by a nonprofit governmental organization. Over the years, several changes have been introduced to the mechanism in place. The most recent change occurred in 2008 when a variant of the student-proposing DA was adopted.[7] The mechanism that was in use previously had been based on a variant of the program-proposing version of DA. Both mechanisms endow programs with priorities based on a weighted average of several variables (mainly academic performance in the 11th and 12th grades and matriculation exam scores, but also credits

[7] To be precise, the matching system has three rounds. The main round, in which the majority of BA and MA positions are allocated, ends in July; an additional, significantly smaller round at the end of the summer for unfilled unfunded positions; and a winter round for master's programs that start in the spring term (Bíró, 2011). We use data only from the main round of the BA match.

for disadvantaged and disabled applicants, as well as for a small number of gifted applicants). Across institutions, programs in the same field of study use the same priorities. But programs in different fields use different weighting schemes (e.g., the priority score for computer science assigns greater weight to physics grades relative to the priority score for economics). Prospective students apply to particular study programs, i.e., a particular major at a particular institution (e.g., a BA in applied economics at Corvinus University of Budapest). They may apply to multiple institutions and to multiple programs in the same institution.

Tuition waivers. Hungarian nationals and citizens of the European Economic Area are eligible to receive up to six years (12 semesters) of free education in the form of a tuition waiver. Nevertheless, the government caps the number of funded positions in some majors and in each field of study (business and economics, humanities, etc.). Eligible students may apply for a funded position, but unfunded positions are also offered. If admitted to an unfunded position, the student will not receive a tuition waiver, in spite of her eligibility.

Besides the monetary benefits, funded positions have other advantages over unfunded ones. Many institutions grant funded students priority in access to subsidized housing and other amenities. In some cases, these benefits have substantial monetary value. Moreover, paying students bear the stigma of being thought "not good enough" to be admitted to the traditional funded track (cf. Aygun and Turhan, 2016).

Rank Ordered Lists. Students are allowed to rank any number of *contracts*, i.e., program and funding level combinations, they wish. For example, they may submit an ROL that includes three contracts with two programs: 1) funded BA in biology at Eötvös Lóránd University; 2) funded BA in applied economics at Corvinus University of Budapest; 3) unfunded BA in biology at Eötvös Lóránd University. Submitting an ROL that includes up to 3 programs (which may correspond to up to 6 contracts) only requires paying a fixed application fee (about 30 dollars). However, applicants are charged (about 7 dollars) for each additional program in their ROL.

Obvious mistakes. The fact that application fees are determined according to the number of *programs* in the ROL, as opposed to the number of *contracts*, implies that if a student ranks an unfunded contract with a certain program, then the marginal cost of ranking a funded contract with the same program is zero. This, in turn, implies that unless applicants' preferences depend on things other than their realized assignment, an applicant is using a dominated strategy if she ranks an unfunded contract in some program higher than a funded contract in the same program (*obvious flipping*), or if she ranks only an unfunded contract in a program that offers a funded contract (*obvious dropping*). We collectively refer to such strategies as *obvious mistakes*.

Table 1 presents an ROL that includes three contracts with two programs. This ROL contains two obvious mistakes. First, unfunded BA in biology at Eötvös Lóránd University is ranked higher than the funded contract in the same program (obvious flipping). Second, the applicant ranked only unfunded BA in applied economics at Corvinus University of Budapest, even though the funded contract was offered (obvious dropping).

Table 1: A Rank Ordered List with obvious mistakes

Rank	Program		Funding
	Institution	Major	
1.	Eötvös Lóránd University	BA in Biology	Unfunded
2.	Corvinus University of Budapest	BA in Applied Economics	Unfunded
3.	Eötvös Lóránd University	BA in Biology	Funded

Timeline. The timeline of the application process is as follows: first, applicants submit their ROLs in mid-February. Students in their final year of high school learn their 12th-grade GPA in April, and complete their matriculation exams in May and June. In early July, applicants report all their grades and exam scores, and they may change the order of their ROL or drop contracts from the list, but they may not add any contracts to the list. Finally, in mid-July, the clearinghouse releases the *priority-score cutoffs* for each contract, i.e., the minimum priority score needed to gain admission, and notifies applicants about their placement.

Information. The formulas for priority scores are public. The priority-score cutoffs are made public shortly after the match, and receive extensive coverage by the local media. This feature simplifies the applicants' comprehension of the mechanism and increases their trust, as applicants may verify that they were assigned to the highest-ranked program whose cutoff they surpassed. The clearinghouse website (`http://www.felvi.hu`) contains detailed statistics about the match in recent years, including quotas, the number of applicants and acceptances, and priority-score cutoffs. It provides decision support also in the form of an application fee calculator. Much of this information, in addition to information about all participating programs, is also available in a booklet published each year by the Ministry of Education.

2.2 The 2012–2013 Reforms

Historically, higher education in Hungary was free. Since the fall of the Iron Curtain in the early 1990s, there have been several attempts to introduce college tuition, but these attempts met with widespread public resistance. For example, in 1995, the government introduced college tuition, which was canceled in 1998.[8] In 2008, the government legislated an "improvement fee," but this legislation was overturned by a public referendum in the same year.

In 2010, a new government was elected and public debt reduction was a mainstay of its platform. As part of a wide effort to reduce public spending, in December 2011 the government passed legislation substantially reducing the number of available tuition waivers beginning in 2012.[9] Although media outlets had been speculating about such reform since September 2011, its details and the fact that it materialized came as a surprise given the history of tuition fee reforms in Hungary. The reform affected students who were supposed to submit their college application two months later, in mid-February 2012, leading to a two-week extension of the ROL submission deadline.

The severe reduction in state-sponsored (funded) positions was concentrated in three fields of study: business and economics, legal studies, and social sciences. The number of state-sponsored positions declined from 4,900 to 250 in business and economics, from 1,300 to 300 in legal studies, and from 2,100 to 1,000 in social sciences (Table 2). Altogether, the reform reduced the number of funded positions by 81 percent in these fields. Funded positions in some majors were eliminated completely (examples include business administration and management, commerce and marketing, and human resources). In other majors, funding was only offered in a subset of the institutions where it had been offered previously (for example, legal studies, international business administration, and international relations). In still other majors, the menu was not changed, but the capacities of state-sponsored options were reduced. The number of state-sponsored positions in other fields of study declined by 7 percent, from 36,000 to 33,637. We refer to these fields of study as *fields with little or no funding cut*.

The backlash following the 2012 experience led to some changes in the way the reform was implemented in subsequent years, starting in 2013. Importantly, state-sponsored positions were restored in all programs where they had been previously offered. However, state-sponsored capacities remained scarce.[10] The "reversal" of the 2012 reform did not meaningfully increase the number of state-sponsored positions in the affected fields: the number of funded positions was about 800 in business and economics, 170 in legal studies, and 1,100 in social sciences. Additionally, in 2013, the funding cut was expanded to include an additional major in the field of humanities (adult education).

Since we will be exploiting the 2012–2013 reform in our empirical analysis, we must also mention other changes that occurred around the same time (Table 3). As part of the 2012–2013 reform,

[8] https://www.felvi.hu/felsooktatasimuhely/archivum/jogi_hatter/torleszto_reszletek

[9] The legislation had mainly a fiscal motivation: the government faced pressure to consolidate the budget and initiated talks with the IMF on November 21, 2011.

[10] Starting in 2013, the reform was framed differently. Instead of publicly announcing funded capacities for each field of study, the government announced indicative priority-score cutoffs, noting that they might change depending on capacity constraints.

Table 2: The availability of funded positions

	2009	2010	2011	2012
A. *Fields with little or no funding cut*				
Agriculture	1,900	1,950	1,850	2,160
Art	700	700	570	900
Art mediation	300	300	390	350
Computer science	4,700	4,700	6,400	4,550
Engineering	9,800	9,850	9,850	10,760
Humanities	4,800	4,450	4,100	2,700
Medicine	3,400	3,600	4,600	5,000
Public administration	-	-	-	1,017
Natural sciences	4,200	4,200	5,200	4,000
Pedagogy	1,900	1,800	2,000	1,600
Sport	600	600	500	600
B. *Fields with severe funding cut*				
Business/economics	5,900	6,250	4,900	250
Legal studies	1,500	1,350	1,300	300
Social sciences	3,000	2,750	2,100	1,000

Notes: The government did not publish the number of available funded positions in 2013. The numbers do not include partial scholarships, which were offered in 2012 only. The capacity of partially funded positions was: 150 in agriculture, 1,500 in computer science, 2,350 in engineering, 100 in medicine, and 1,500 in natural sciences. A partial scholarship covered 50 percent of the tuition fee. Partial scholarships were awarded to students who were assigned an unfunded position based on merit. There was no possibility of ranking partially funded positions separately. While the number of funded capacities in computer science and natural sciences increased in 2011, the previous capacity was not binding.

the government legislated a decree that introduced the *study contract*, which obliges college students who benefit from state sponsorship to work in Hungary for twice the number of years they spent in college within 20 years from graduation, or else repay the country with interest (of base rate + three percentage points). Even though the decree makes state-sponsored positions less desirable, we do not think that it changes the natural ranking of funded and unfunded contracts or that it has a substantial effect on the composition of applicants, for several reasons. First, the decree specifies numerous exemptions, including having two or more children, military service, and disability. Second, it was highly unlikely that this contract will be enforced (in twenty years). Its legal status was unclear, as it may violate the freedom of movement of workers in the EU,[11] and political pressure caused the government to significantly alleviate the terms already in 2013, such that the number of years of obligatory work in Hungary was cut in half, and students who drop out within one semester are exempted from repayment. Third, a student who leaves Hungary and does not return for more than a decade is very likely to move to a country where she will have a much easier time earning a few thousand dollars, lowering the marginal value of money in this contingency. Fourth, if an applicant is admitted with funding, she can decide to decline the funding and still be admitted; thus, applying to a funded position provides a pure option value.

The government also introduced partially funded positions, which were offered only in 2012. Partial funding covered half of the tuition fee and required applicants to sign the study contract. It was not possible to rank partially funded positions, but they were awarded based on merit to individuals who were assigned an unfunded position (thus, the government implicitly assumed that a funded option would be preferred by the applicants, which is consistent with our interpretation).[12]

Additionally, in 2012, the formulas for priority scores were slightly changed and rescaled. For ease of comparison, we compute within-year percentile ranks of the priority-score cutoffs. Finally, in 2013 the fixed application fee was eliminated, and the number of programs one could rank was capped at 5 (10 contracts). We do not think the change had a substantial effect on the composition of ROLs as in 2011 only 4.5 percent of the ROLs included more than 5 programs and only 0.7 percent of the ROLs contained more than 10 contracts. Additionally, we do not observe an increase in the number of applicants between 2012 and 2013 (on the contrary, the number decreases).

[11] See The New York Times; https://goo.gl/VL3Rt6, accessed: 19/10/2017.

[12] Our view is also shared by the popular media. For example, on the day the 2017 match results were made public – five years since the introduction of the study contract – a major media outlet published a story titled: "*The priority-score cutoff to unfunded medicine exceeds the state funded cutoff.*" (Source: index.hu; https://goo.gl/zfxFFw, accessed: 20/09/2017).

Table 3: Summary of the 2012-2013 reforms

	2009 – 2011	2012	2013
Availability of funding		Severe cut in the number of funded positions relative to 2011 in the fields of business/economics, legal studies, social sciences, and humanities	
Available choices	Funded program Unfunded program	Funded program with study contract Unfunded program	
Maximum priority score	480 points	500 points	
Application fee structure			
- Fixed fee (three programs)	30 USD	30 USD	0 USD
- Marginal fee (per program)	7 USD	7 USD	7 USD
- Maximum ROL length	∞	∞	10

3 Data

In this section, we describe the data that we use in our empirical analysis. We begin, in Section 3.1, by presenting our data sources. In Section 3.2, we discuss the definition of our samples. Finally, in Section 3.3, we present summary statistics.

3.1 Data Sources

Our analysis uses four data sources that we merged based on demographic information. The main source is an administrative dataset that contains information about the bachelor's degree admissions process between 2009 and 2013 in Hungary.[13] This dataset includes the final allocation in each year. In particular, we observe each applicant's complete ROL and program-specific priority scores,[14] as well as the list of existing programs with their realized priority-score cutoff. For each applicant we also observe gender, age, postal code and, a high-school identifier. Additionally, the data include all information required to (re)calculate the applicant's priority score in each program she applied to. This includes grades in various subjects in the final two years of high school (11th grade and 12th grade), performance in the matriculation exams, and the number of points the applicant received for claiming a disadvantaged background.[15]

[13] The Hungarian Higher Education Application Database (FELVI) is owned by the Hungarian Education Bureau (Oktatasi Hivatal). The data were processed by the Hungarian Academy of Sciences Centre for Economic and Regional Studies (HAS-CERS).

[14] Our data report up to 7 contracts from each ROL: the first 6 contracts and the contract where the applicant is assigned. The dataset also reports the number of contracts in each ROL. We observe the complete ROL for 92.8 percent of applicants and 89.3 percent of all ranked contracts.

[15] To be eligible for disadvantaged status, an applicant must have per capita household income that is lower than 130 percent of the minimum pension. Since 2013, in addition to the income criterion, the student had to meet one of the following three conditions: (i) parents with lower than primary education, (ii) long-term unemployed parents, or (iii) poor living conditions. To receive disadvantaged status, an applicant must certify that she meets these conditions at the local municipality. Disadvantaged status is granted for one year. Students with disadvantaged status receive regular

Our second administrative data source is the National Assessment of Basic Competencies (NABC). The objectives of the NABC are similar to those of the Programme for International Student Assessment (PISA). It measures literacy and numeracy skills in a standardized way, making the scores comparable across years and cohorts. Between 2006 and 2007, the NABC covered a representative sample of students, and since 2008, it covers all students in the 6th, 8th, and 10th grades, except for those who were absent from school on the day that the exam was administered. The NABC is a low-stakes exam from the students' perspective: it is graded blindly by the Ministry of Education and only summary statistics of scores are reported to schools. The NABC numeracy and literacy skills are normalized to have zero-mean and a standard deviation of one in the general population, which includes both applicants and non-applicants to undergraduate education.

The NABC data also include administrative information on demographics, such as age, gender, and school identifier, as well as self-reported survey measures of socioeconomic status (e.g., parental education, home possessions, etc.). Following Horn (2013), we create an NABC-based SES index, which is a standardized measure that utilizes survey information of the NABC. The NABC-based SES index combines three subindices: the first is a subindex of parental education, the second is a subindex of home possessions (number of bedrooms, mobile phones, cars, computers, books, etc.), and the third is a subindex of the labor market status of the parents. The NABC-based SES index resembles the economic, social and cultural status (ESCS) indicator of the OECD PISA survey.

Third, we use information on microregional-level annual unemployment rates published by the National Employment Service in 2008, one year before the start of our sample period.[16] The territorial breakdown consists of 174 units. Fourth, we also use the T-star dataset of the Hungarian Central Statistics Office to obtain settlement-level annual information on collected income taxes.[17] In particular, we calculate the per capita gross annual income for all 3,164 settlements for each year between 2009 and 2013.

3.2 Sample Definition

An ROL is an ordered list of *contracts*, program-funding pairs. An applicant makes an obvious mistake if she ranks an unfunded contract in some program higher than a funded contract in the same program (obvious flipping), or, if she ranks only an unfunded contract in a program that offers a funded contract (obvious dropping). When we examine correlations between applicants' characteristics and obvious mistakes, we treat each ROL as a single observation (Section 4). By

cash transfers and are eligible for free textbooks during high school.

[16]Source: http://kisterseg.munka.hu/index.php?static=kister, accessed: 16/11/2016. For more information on the territorial units see https://www.ksh.hu/regional_atlas_microregions?lang=en.

[17]For further information visit http://adatbank.krtk.mta.hu/adatbazisok___tstar

contrast, when we analyze the effect of admission selectivity on obvious mistakes (Section 5), we treat each *application* – a program in an ROL, up to two contracts – as a single observation.

We restrict our sample to ROLs that can potentially exhibit obvious mistakes. These ROLs must meet two criteria. First, the applicant must be eligible for a tuition waiver. As our data do not contain direct information on tuition-waiver eligibility, we rely on indirect information: we restrict the sample to ROLs submitted by citizens of the European Economic Area who did not report being ineligible. Second, we focus on ROLs that include at least one contract with a program that offers both funded and unfunded contracts. We call this sample the *eligible sample*. Our full dataset consists of 483,891 ROLs submitted between 2009 and 2013. Altogether, 447,989 ROLs meet the eligibility restrictions.

We often restrict our full sample to ROLs submitted by applicants who, at the time, were younger than 22 and had completed their matriculation exam in the same year. We refer to this sample as the *high-school senior applicant sample*. The reason for the restriction is twofold. First, this is the subsample that we are able to match to the NABC database. And second, in this subsample we are certain that applicants did not exhaust their 12 funded semesters, but just chose not to declare their ineligibility (without ranking any funded contract).[18] The high-school senior applicant sample comprises 228,606 ROLs. These restrictions ensure with a high degree of certainty that the obvious mistakes we identify are not the result of misclassification. However, the inclusion criteria of this sample likely exclude many eligible students, especially weaker applicants, who may be more prone to mistakes according to previous studies. Finally, we sometimes refer to the subsample of *relevant ROLs*. These are ROLs that include at least one unfunded contract in a program that also offers a funded contract. Relevant ROLs are the only lists in which our methodology can potentially detect mistakes.

As the administrative datasets do not contain unique individual identifiers, we match them based on demographic information, year and month of birth, gender, postal code, and high-school identifier. The NABC dataset contains information on 10th-grade students from 2006 onward. Therefore, for each year, we only match high-school senior applicants to the NABC. Whenever a unique match is not found, we calculate the average test scores of matched individuals. We were able to match 148,604 applicants out of 228,606 (65 percent between 2009 and 2013, and 80 percent between 2011 and 2013). The match is unique for about 123,000 observations (54 percent). Appendix A contains further details about the matching procedure.

[18] The ROL of an applicant who did not declare ineligibility even though she exhausted her 12 funded semesters and did not rank any funding contracts would be incorrectly classified as a mistake. These applicants only appear to make obvious mistakes, whereas they are in fact ineligible for funding. Focusing on high-school senior applicants eliminates the risk of such misclassification.

3.3 Summary Statistics

Table 4 summarizes the means and standard deviations of the background characteristics of applicants in the eligible and high-school senior applicant samples. Applicants in the eligible sample were 21.9 years old on average, with 55 percent being female. The majority (63 percent) of the applicants attended secondary grammar schools, whose declared purpose is to prepare students for higher education. Approximately 19 percent of the applicants lived in Budapest, 10 percent lived in one of the 18 county capitals, 32 percent resided in towns, and the remainder lived in villages. About 7 percent of the applicants claimed points for disadvantaged status. Applicants' GPAs were 3.75 in the 11th grade and 3.71 in the 12th grade on average, on a scale of 1-5.[19] The average ROL length was 3.81 contracts, which corresponds to 2.91 programs.

Applicants' characteristics in the high-school senior applicant sample are largely similar to those in the eligible sample. The main differences are that high-school senior applicants are younger (by construction), and academically stronger (as one would expect). As we discussed in the previous subsection, we are able to match the NABC only for the high-school senior applicant sample. The NABC variables, such as the numeracy skill, literacy skill, and the NABC-based SES index are standardized within cohort in the general population, which includes both applicants and non-applicants. On average, high-school senior applicants had 0.59 (0.63) standard deviation higher 10th grade numeracy (literacy) skill than the general population. Similarly, high-school senior applicants' average NABC-based SES index is 0.49, indicating that they come from a higher-than-average socioeconomic background.

Table 5 presents the distribution of the ROLs by the type of contracts they include. In the eligible sample, almost 60 percent of ROLs include only funded contracts, 7 percent include only unfunded contracts, and the rest include both funded and unfunded contracts. High-school senior applicants' ROLs include only funded contracts more frequently (66.7 percent) and only unfunded contracts rarely (2.1 percent). Thus, 40 percent of the eligible sample, and 33 percent of the high-school senior applicant sample, are relevant. Among students who listed both funded and unfunded contracts in their ROL, 53.7 percent ranked *all* funded contracts above *all* unfunded ones in the eligible sample. The corresponding figure for the high-school senior applicant sample is 46.9 percent. Taken together, these figures suggest that funding plays an important role in students' choices between programs.

Figure 1 presents the distribution of the applications by field of study over time. The most popular fields of study were business and economics, engineering, and humanities for both the eligible and high-school senior applicant samples. The distribution of the fields of study was

[19] Applicants with a low high-school GPA, relative to their matriculation exam scores, have no incentive to report their GPA, as it has no effect on their priority score. As a result, 11th- and 12th-grade GPAs are missing from 30 percent of both samples. Indeed, the correlation between missing GPA and matriculation exam scores in our data is negative and strong.

Table 4: Individual-level summary statistics

	Eligible applicants		High-school senior applicants	
	Mean	Std. dev.	Mean	Std. dev.
Female	0.55	0.497	0.57	0.496
Age	21.86	5.436	19.04	0.683
High school				
- secondary grammar school	0.63	0.484	0.70	0.460
- vocational school	0.33	0.469	0.27	0.443
Residence				
- capital	0.19	0.390	0.16	0.371
- county capital	0.20	0.399	0.20	0.400
- town	0.32	0.467	0.33	0.471
- village	0.29	0.455	0.30	0.460
11th-grade GPA	3.75	0.840	3.97	0.790
11th-grade GPA - missing	0.30	0.457	0.30	0.457
12th-grade GPA	3.71	0.840	3.87	0.814
12th-grade GPA - missing	0.27	0.442	0.24	0.425
NABC numeracy skill	-	-	0.59	0.862
NABC numeracy skill - missing	-	-	0.35	0.477
NABC literacy skill	-	-	0.63	0.742
NABC literacy skill - missing	-	-	0.35	0.477
NABC-based SES index	-	-	0.49	0.848
NABC-based SES index - missing	-	-	0.41	0.492
Disadvantaged status	0.07	0.256	0.10	0.301
Unemployment rate in 2008 (%)	7.70	4.434	7.88	4.526
Unemployment rate in 2008 - missing	0.02	0.145	0.02	0.155
Gross annual per capita income (1000 USD)	6.34	1.541	6.27	1.525
Gross annual per capita income - missing	0.02	0.144	0.02	0.154
# of contracts on the ROL	3.81	2.062	4.37	2.243
# of programs on the ROL	2.91	1.208	3.28	1.203
# Observations	447,989		228,606	

Notes: Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average of Hungarian grammar and literature, mathematics, and history. Grades are on a scale of 1–5. The unemployment rate in 2008 is measured on the microregional-level. Gross annual per capita income is measured on the settlement-level, i.e., where the student lives. The number of contracts on the ROL is reported administratively, whereas we calculate the number of programs based on the contracts observed in the dataset (see Footnote 14).

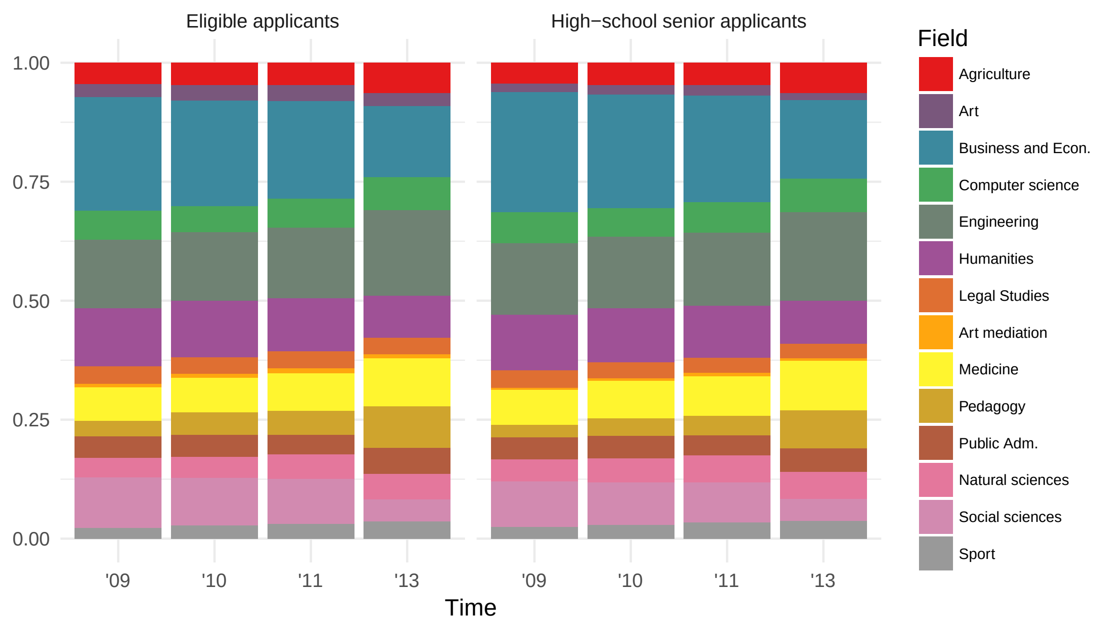
Table 5: The distribution of ROLs by the funding type they include

ROLs	Eligible applicants		High-school senior applicants	
	(1)	(2)	(3)	(4)
Only funded contracts	268,611	60.0%	152,460	66.7%
Funded and unfunded contracts	146,661	32.7%	71,309	31.2%
Only unfunded contracts	32,717	7.3%	4,837	2.1%

Notes: Columns (1) and (3) display frequencies and columns (2) and (4) show the distribution.

relatively stable over time.[20] Applications to fields of study that suffered a severe cut in funding in 2012 and 2013 (business and economics, legal studies, and social sciences) comprised 23 percent of all applications in the eligible applicant sample and 24 percent of all applications in the high-school senior applicant sample in 2013.

Figure 1: Distribution of applications by field of study



Notes: Each observation corresponds to a program in a given ROL. The figure does not display the year 2012, since the reform eliminated the availability of funding in some programs in this year (see Section 2.2) and we exclude this year when we analyze the causal effect of admission selectivity on obvious mistakes.

4 Obvious Mistakes: Prevalence and Correlates

We next study the prevalence and correlates of obvious mistakes. We start, in Section 4.1, by quantifying the share of ROLs with obvious mistakes and the associated cost. In Section 4.2, we examine the correlates of obvious mistakes and find that mistakes are more common among high socioeconomic status and low academic ability applicants.

[20] We discuss the robustness of our results to instability in the composition in Section 5.4.

4.1 The Prevalence and Costs of Obvious Mistakes

Table 6 quantifies the share of ROLs that exhibit obvious mistakes. In the eligible sample, the fraction of obvious mistakes ranges from 8.7 percent in 2009 to 14.5 percent in 2013. During the sample period almost 50,000 applicants, corresponding to 10.9 percent of the ROLs, made an obvious mistake, mostly obvious dropping. Obvious mistakes are less prevalent among high-school seniors, but, still, the share of mistakes increased from 3.1 percent in 2009 to 10.8 percent in 2013. Overall, 5.3 percent of the high-school seniors made an obvious mistake in the same period.[21] It is important to note that obvious mistakes can only be detected in ROLs that rank at least one unfunded contract. In the eligible applicant sample the share of such ROLs is 40 percent (see Table 5). Table 6 should be interpreted in this context. For example, 10.9 percent of ROLs with obvious mistakes in the eligible applicant sample represent 27.1 percent (= 10.9%/0.4) of ROLs in the sample in which a mistake could be detected.

According to our interpretation, obvious mistakes correspond to weakly dominated strategies. Rational players only use dominated strategies if they assign probability 0 to the event that a dominating strategy does strictly better. Table 6 assesses the share of obvious mistakes that are costly ex post. We provide a lower bound and an upper bound for these shares. The upper bound corresponds to the fraction of applicants who met the priority-score cutoff for receiving funding in any program whose funded contract they dropped or ranked below its unfunded version. The lower bound accounts for such ROLs only if the applicant was not assigned a higher-ranked contract. These estimates correspond to ROLs that rank the funded contract either first or directly above the unfunded contract.

Table 6 demonstrates that obvious mistakes may have hurt up to 18.6 percent of the eligible applicants and up to 10.0 percent of the high-school senior applicants who made obvious mistakes (column 4). At least 12.2 percent of the eligible applicants who made obvious mistakes could have received a tuition waiver (column 3). Similarly, among the high-school senior applicants at least 4.5 percent of those who obviously dropped or flipped could have gotten a tuition waiver in the program they were eventually assigned to. The relative importance of funding, reflected in the ranking of the majority of students, suggests that the upper bound may be more indicative of the true fraction. The expected cost associated with mistakes is between 312 and 667 dollars for eligible applicants, and between 114 and 365 dollars for high-school senior applicants in 2013.

Our estimates take a partial equilibrium approach: we do not analyze the aggregate effect of obvious mistakes. Instead, we assume that all priority-score cutoffs remain fixed and ask what would the effect of correcting one list be. By doing so, we ignore the effect that correcting one list might have on other applicants who would be displaced as a result of eliminating obvious

[21] The rate of obvious dropping in the eligible applicant sample was 10.2 percent and the rate of obvious flipping was 0.9 percent. Among high-school seniors the rate of obvious dropping was 4.6 percent and the rate of obvious flipping was 1 percent. An ROL can include both obvious dropping and obvious flipping at the same time.

mistakes, and any subsequent effects ("rejection chains").

Table 6: Obvious mistakes over time

Year	Obvious mistakes		Only mistakes		Ex post costly mistakes			
					Lower bound		Upper bound	
	(1)		(2)		(3)		(4)	
A. *Eligible applicants*								
2009	8.7%	(8,555)	3.3%	(3,268)	12.4%	(1,062)	20.3%	(1,733)
2010	9.4%	(9,818)	3.9%	(4,031)	10.6%	(1,044)	15.8%	(1,556)
2011	12.2%	(12,615)	5.4%	(5,570)	9.4%	(1,183)	14.2%	(1,797)
2012	10.4%	(7,452)	3.1%	(2,219)	12.6%	(937)	19.9%	(1,482)
2013	14.5%	(10,209)	5.9%	(4,129)	16.6%	(1,698)	24.2%	(2,468)
Total	10.9%	(48,649)	4.3%	(19,217)	12.2%	(5,924)	18.6%	(9,036)
B. *High-school senior applicants*								
2009	3.1%	(1,566)	0.8%	(393)	2.2%	(35)	7.6%	(119)
2010	3.2%	(1,596)	0.8%	(422)	1.4%	(23)	4.1%	(65)
2011	4.6%	(2,268)	1.3%	(656)	1.4%	(31)	4.9%	(112)
2012	6.3%	(2,494)	0.9%	(365)	4.0%	(101)	10.5%	(261)
2013	10.8%	(4,202)	3.5%	(1,350)	8.4%	(355)	15.7%	(660)
Total	5.3%	(12,126)	1.4%	(3,168)	4.5%	(545)	10.0%	(1,217)

Notes: Column (1) shows the share (number) of ROLs that exhibit obvious mistakes over time. Column (2) presents the share (number) of ROLs in which all listed programs exhibit an obvious mistake. Column (3) presents the share (number) of ROLs with obvious mistakes, where the applicant was assigned to the unfunded version of a program in which he met the priority-score cutoff for the funded version. Column (4) shows the share (number) of ROLs with obvious mistakes, where the applicant met the priority-score cutoff of the funded version.

4.2 The Correlates of Obvious Mistakes

This section examines the characteristics of applicants who made obvious mistakes. We regress an indicator for obvious mistakes on individual-level demographic variables, proxies of socioeconomic status, academic achievement, and year fixed effects. In the body of the paper we focus on the sample of high-school seniors, for whom we can use the richer NABC data. We repeat the analysis on the eligible sample in Appendix B.1, and obtain similar results. It is important to note that these regressions provide descriptive evidence on the characteristics of applicants who submitted ROLs with obvious mistakes, but we cannot attribute a causal interpretation to the estimated coefficients.

Table 7 summarizes our findings. Applicants with higher NABC-based SES index make more obvious mistakes on average (column 1), and this correlation is even stronger once we control for academic achievement (columns 2-3). In columns 4-6 we corroborate the correlation of obvious mistakes with proxies of SES (microregional-level unemployment rate, settlement-level gross annual per capita income, and indicator for claiming admissions points for disadvantaged background).

Table 7: Demographics, socioeconomic status, academic achievement and obvious mistakes

Dependent variable	Obvious mistakes					
	(1)	(2)	(3)	(4)	(5)	(6)
NABC-based SES index	0.011***	0.016***	0.017***			
	(0.0007)	(0.0008)	(0.0008)			
Unemployment rate in 2008 (%)				-0.002***		
				(0.0001)		
Gross annual per capita income (1000 USD)					0.010***	
					(0.0006)	
Disadvantaged status (dummy)						-0.030***
						(0.0015)
Numeracy skill		-0.022***	-0.015***	-0.013***	-0.013***	-0.012***
		(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
11th-grade GPA			-0.027***	-0.026***	-0.026***	-0.027***
			(0.0010)	(0.0010)	(0.0010)	(0.0010)
Female	0.013***	0.003***	0.011***	0.009***	0.010***	0.010***
	(0.0012)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Vocational school	0.014***	0.005***	-0.002	-0.007***	-0.006***	-0.006***
	(0.0014)	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0015)
Other high schools	0.022***	0.017**	0.012*	0.010	0.011	0.011
	(0.0068)	(0.0068)	(0.0068)	(0.0068)	(0.0068)	(0.0068)
County capital	-0.028***	-0.029***	-0.028***	-0.026***	-0.010***	-0.029***
	(0.0025)	(0.0025)	(0.0025)	(0.0025)	(0.0028)	(0.0025)
Town	-0.033***	-0.036***	-0.034***	-0.031***	-0.013***	-0.037***
	(0.0023)	(0.0023)	(0.0023)	(0.0024)	(0.0029)	(0.0023)
Village	-0.040***	-0.042***	-0.041***	-0.038***	-0.015***	-0.044***
	(0.0024)	(0.0024)	(0.0024)	(0.0025)	(0.0032)	(0.0024)
# Obs.	133,714	133,714	133,714	133,714	133,714	133,714
R-squared	0.024	0.030	0.036	0.033	0.034	0.034

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code).We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 5.2% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 28.5% of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Applicants with lower socioeconomic status were less likely to apply for unfunded positions, a necessary condition for detecting an obvious mistake. We argue that this channel does not drive the positive relationship between proxies of SES and obvious mistakes. In Appendix Table B1 we restrict attention to ROLs that include at least one unfunded contract, and repeat the same analyses. The results continue to hold.

We next investigate whether academic achievement is correlated with obvious mistakes. First, we examine the 10th-grade NABC numeracy skill. Table 7 establishes a strong negative correlation between numeracy skill and obvious mistakes. This score is normalized to have zero-mean and a standard deviation of one in the general population. A one standard deviation increase in the numeracy skill is associated with a 2.1 percentage points decline in the probability of making obvious mistakes (column 2). Once we control for 11th-grade GPA, the estimated coefficient drops to 1.2-1.5 percentage points (columns 3-6).

GPA is related to applicants' priority directly, since GPA enters the priority-score formula and could account for up to 20 percent of the priority score. We find that applicants with higher GPA make fewer obvious mistakes, even controlling for numeracy skill. A one mark (corresponding to 0.79 standard deviation) increase in the 11th-grade GPA is associated with a 2.5-2.6 percentage points decline in the probability of making obvious mistakes (columns 3-6).

We also find that female applicants were 1 percentage points (24 percent) more likely to make an obvious mistake. Additionally, the fraction of obvious mistakes was increasing in the size of the settlement in which the applicants resided. Finally, we do not find robust differences between students who attended secondary vocational schools and their peers in secondary grammar schools. Appendix Table B2 demonstrates that the positive correlation between socioeconomic status and obvious mistakes, and the negative correlation between academic achievement and obvious mistakes hold both in the pre- and post-reform periods (2009–2011, and 2012–2013, respectively).

5 The Effect of Admission Selectivity on Obvious Mistakes

This section presents our main result, namely, that admission selectivity has a positive causal effect on making obvious mistakes. We start, in Section 5.1, by providing descriptive evidence of the positive relationship between admission selectivity and obvious mistakes within an ROL. In Section 5.2, we review our difference-in-differences research design, which compares the rate of obvious mistakes involving programs in majors that were affected by the severe reduction in funding to those that experienced little or no cut in funding. We present the results in Section 5.3, and in Section 5.4 we discuss threats to the identification strategy and demonstrate the robustness of the results.

5.1 Admission Selectivity and Obvious Mistakes

In the previous section, we examined the characteristics of the individuals who submitted ROLs containing obvious mistakes. We now consider the characteristics of programs with respect to which obvious mistakes are more common. In light of previous research, we are particularly interested to know whether the selectivity of a program causes higher rates of mistakes.

We measure admission selectivity by the realized priority-score cutoff to the funded program one year prior to the application. For ease of comparison, we abstract from the fact that different fields of study use different weighting schemes, and we normalize the priority-score cutoffs to within-year percentile ranks.[22] Figure 2 presents the relationship between admission selectivity and obvious mistakes. Panel (a) demonstrates that, conditional on appearing in an ROL, obvious mistakes are more likely to occur in applications to more selective programs. Specifically, obvious mistakes are more likely to occur in applications to programs in the top quintile of the admission selectivity distribution are five times more likely than in applications with respect to programs in the bottom quintile.

We cannot attribute a causal interpretation to the results depicted in Figure 2 (a) for several reasons. First, students sort into programs based on ability. Since academic ability and obvious mistakes are negatively correlated, it is reasonable to assume that due to sorting, Figure 2 (a) understates the effect of admission selectivity on obvious mistakes. Second, programs differ along more dimensions than just admission selectivity (e.g., content, location, and prestige), which confounds the positive relationship between admission selectivity and obvious mistakes.

We address sorting by adding ROL-level fixed effects, thus exploiting only within-ROL variation in admission selectivity.[23] We find that a 10-percentile ranks increase in admission selectivity is associated with a 0.3 percentage points rise in obvious mistakes (Figure 2 (b)). We identify this slope from ROLs that include programs with distinct historical admission selectivity. However, within-applicant variation in admission selectivity might be too narrow to identify the full effect of admission selectivity. In the rest of this section, we address the causal effect of increased admission selectivity on obvious mistakes, both on the extensive and on the intensive margin.

5.2 Empirical Strategy

To estimate the causal effect of admission selectivity on obvious mistakes, we specify the following difference-in-differences (DiD) model:

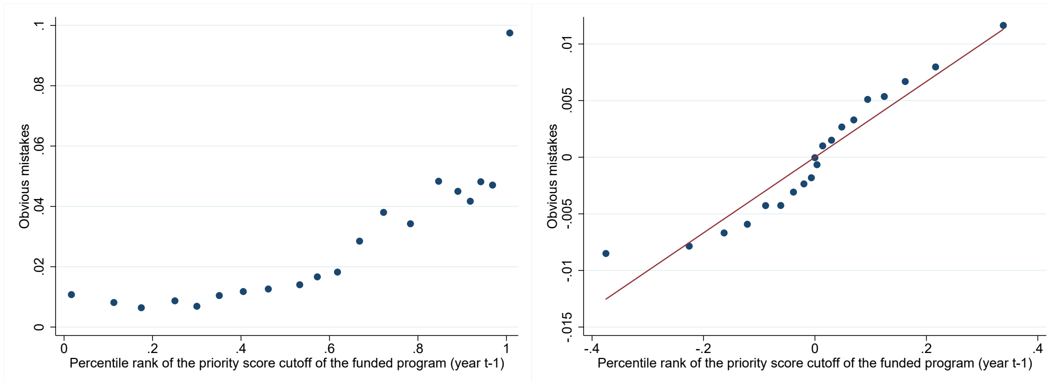
$$Y_{its} = \alpha + \beta \cdot T_{ts} + X_{it} \cdot \Gamma + \eta_s + \nu_t + \varepsilon_{its}.$$

The variable Y_{its} is an indicator for obvious mistakes in applicant i's ranking of program s in year t. The variable T_{ts} is an indicator that equals one if t is equal to 2013 and s is a program that was affected by the severe funding reduction of the 2013 reform, and zero otherwise. The model

[22] Since lagged priority-score cutoffs are not defined in the year a program is launched, we exclude such observations. We also exclude a handful of observations involving programs where a funded contract is not available. Finally, we disregard programs in the fields of art and art mediation, since these programs have eligibility exams and practical exams, and their priority scores are not calculated in the standard way.

[23] Our data do not contain a unique individual identifier; therefore, we cannot identify individuals who applied multiple times over the years.

Figure 2: Admission selectivity and obvious mistakes



(a) Unconditional

(b) Conditional on ROL-fixed effects

Notes: Admission selectivity is measured as the within-year percentile rank of the funded contract's priority-score cutoff one year prior to the application. Panel (a) plots bin-specific means that are conditional on year fixed effects. Panel (b) plots the bin-specific means that are conditional on ROL-level fixed effects. The sample covers applications in the high-school senior applicant sample between 2009 and 2013. Conditional on ROL fixed effects, a 10-percentile rank increase in admission selectivity is associated with a 0.33 percentage points rise (s.e.: 0.0097) in the probability of an obvious mistake.

includes program fixed effects (η_s), year fixed effects (ν_t), a vector of individual-specific controls (X_{it}), and an error term (ε_{its}). Our parameter of interest is β, which measures the effect of the funding cuts, which we interpret as a rise in the selectivity of admission to the funded contract, on obvious mistakes. We estimate the model on the application level. We exclude observations from 2012 since the elimination of many funded programs in that year complicates the analysis and obscures the interpretation of the results.

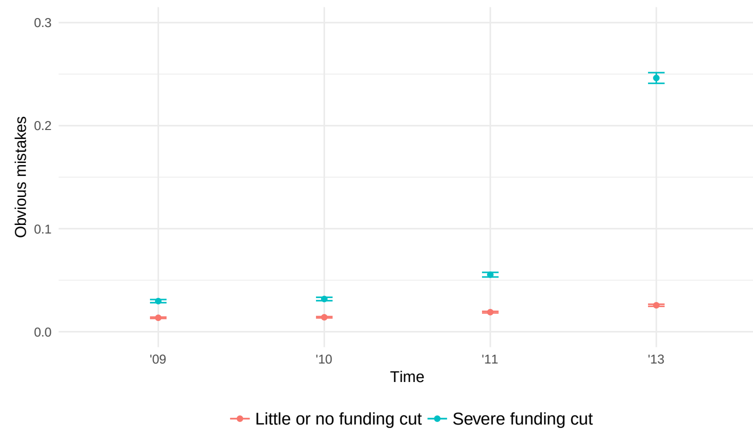
The causal interpretation of β relies on two key assumptions. First, in the absence of the reform, the prevalence of obvious mistakes in different programs would have evolved in parallel (parallel trends). Second, the composition of the students applying to majors with a severe funding cut and students applying to majors with little or no funding cut remained stable over time. In Section 5.4, we evaluate the plausibility of these assumptions and the robustness of our estimates to the violation of these assumptions.

5.3 Results

Figure 3 provides a graphical illustration of the results of our difference-in-differences research design. The figure shows that the rate of obvious mistakes in the programs that suffered little or no funding cut in 2013 remained flat. By contrast, obvious mistakes increased sharply from 5.5 to

24.6 percent in the programs that were affected by the severe funding reduction of the 2013 reform.

Figure 3: The effect of admission selectivity on obvious mistakes: graphical illustration



Notes: The figure presents the share of obvious mistakes over time, split by the severity of the funding cut in 2012/2013. The error bars represent the 95% confidence intervals around the estimates of the group means.

Table 8 presents our difference-in-differences estimates for the effect of admission selectivity on obvious mistakes. Our baseline specification (Column 1) indicates that the 2012–2013 reform increased obvious mistakes by 19.3 percentage points among treated programs from a baseline of 6.3 percent.[24]. Columns 2–5 show that controlling for demographics and academic achievement barely changes the estimates and their precision.[25] Appendix Table B4 shows that the effect holds for both obvious flipping and obvious dropping, but the magnitude for obvious dropping is much larger, both in absolute and in relative terms.

To put our estimates in context, it is instructive to examine the impact of the reform on the priority-score cutoffs of the funded programs. The percentile ranks increased for 88 percent of the treated programs, with an average change of almost 9 percentile ranks. The reduction in the number of funded positions in the directly affected fields made the system as a whole more selective through general equilibrium effects. If students who applied to fields that were not affected directly took these general equilibrium effects into account when submitting their application, then our estimates should provide lower bounds on the causal effect of admission selectivity on obvious mistakes.

[24]The baseline figure corresponds to the counterfactual mean outcome in the treated group in 2013, calculated by adding the mean treated outcome in 2011 and the estimated year effect ($\hat{v}_{2013} - \hat{v}_{2011}$). The estimated year effect for the eligible sample is 0.8 percentage points for the high-school senior applicant sample.

[25]In columns 4 and 5 of Table 8 we control for numeracy skill and the NABC-based SES index. In these specifications we account for missing NABC by including dummy variables. Appendix Table B5 demonstrates that focusing on subsamples where the NABC is non-missing (as in Table 7) does not change our results.

Table 8: The effect of admission selectivity on obvious mistakes

Dependent variable	Obvious mistakes				
	(1)	(2)	(3)	(4)	(5)
Severe funding cut	0.193***	0.187***	0.186***	0.187***	0.187***
	(0.0043)	(0.0043)	(0.0042)	(0.0043)	(0.0042)
R-squared	0.096	0.109	0.121	0.111	0.111
Demographic controls & GPA	-	✓	✓	✓	✓
High school FE	-	-	✓	-	-
NABC controls	-	-	-	✓	✓
NABC-based SES index	-	-	-	-	✓

Notes: Robust standard errors clustered on the applicant-level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs (high-school senior applicants). The mean outcome is 3.2 percent in the sample. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. NABC controls refer to dummies for 20 quantiles of the numeracy and literacy scores. NABC-based SES refers to dummies for 20 quantiles of the NABC-based SES index. Missing control variables are always indicated by a separate dummy variable.
***: p< 0.01, **: p<0.05, *: p<0.1.

In Table 9 we examine whether the effect of admission selectivity on obvious mistakes is homogeneous across various subgroups. The corresponding regressions include interactions of treatment and subgroup dummies, and controls for demographics and academic achievement (as in column 3 of Table 8). We find that the effect of admission selectivity on obvious mistakes is 3 percentage points lower for female applicants. The causal effect of admission selectivity is lower for disadvantaged applicants, measured by claiming points for disadvantaged status or by the NABC-based SES index. The effect of the reform is declining with numeracy skill and with academic achievement, measured by 11th-grade GPA (Figure 4). These results suggest that applicants for whom mistakes caused a higher expected utility loss were less responsive to increases in admission selectivity. We find that applicants to the fields of social sciences, humanities, and legal studies responded in a similar way, whereas the effect of admission selectivity on obvious mistakes was the strongest in the field of business and economics, in which the availability of funded positions changed the most.

Appendix C.2 replicates the main analysis for ex post costly obvious mistakes. Absent any behavioral response, increased selectivity to funded positions mechanically reduces the number of ex post costly obvious mistakes. Our estimates are dominated by this mechanical effect: we find that the 2012-2013 reform had a negative causal impact on ex post costly obvious mistakes.

Table 9: Heterogeneous effects

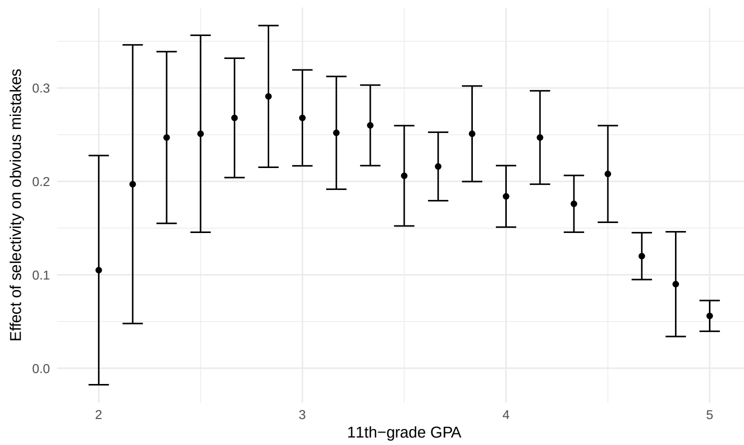
	A. *Gender*		B. *NABC-based SES*				
	Male	Female	Q1	Q2	Q3	Q4	Q5
Severe funding cut	0.204***	0.178***	0.159***	0.160***	0.165***	0.201***	0.211***
	(0.0073)	(0.0051)	(0.0115)	(0.0104)	(0.0100)	(0.0107)	(0.0108)
Counterfactual mean	0.068	0.054	0.045	0.052	0.056	0.058	0.063
R-squared	0.110				0.110		

	C. *Disadvantaged*		D. *NABC numeracy skill*				
	No	Yes	Q1	Q2	Q3	Q4	Q5
Severe funding cut	0.195***	0.091***	0.236***	0.202***	0.195***	0.178***	0.110***
	(0.0045)	(0.0113)	(0.0108)	(0.0103)	(0.0102)	(0.0101)	(0.0096)
Counterfactual mean	0.061	0.037	0.067	0.060	0.055	0.051	0.050
R-squared	0.110				0.112		

	E. *Field of study*			
	Business/ economics	Legal studies	Social sciences	Humanities
Severe funding cut	0.195***	0.178***	0.151***	0.124***
	(0.0050)	(0.0109)	(0.0106)	(0.0228)
Counterfactual mean	0.057	0.061	0.080	0.058
R-squared		0.110		

Notes: The table presents the DiD estimates by various subgroups of the high-school senior applicant sample. Each panel estimates the coefficients in a single regression by interacting the treatment variable with subgroup indicators. Robust standard errors clustered on the applicant level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs. The mean outcome in the sample is 0.032. The counterfactual mean denotes the counterfactual mean outcome of the treated group in 2013. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. We reject the null that the male and female coefficients are equal (p-value: 0.0027). The heterogeneous effect for the missing NABC-based SES category is 0.199*** (s.e.: 0.0115). We cannot reject the null that NABC-based SES Q1 = NABC-based SES Q2 = NABC-based SES Q3 (p-value: 0.8870), and that NABC-based SES Q4 = NABC-based SES Q5 (p-value: 0.4839). The estimated heterogeneous effects for disadvantaged status are significantly different from each other (p-value: 0.0000). The heterogeneous effect for the missing NABC numeracy skill is 0.197*** (s.e.: 0.0094). We cannot reject the null that NABC numeracy skill Q2 = NABC numeracy skill Q3 = NABC numeracy skill Q4 (p-value: 0.2203). However, we reject NABC numeracy skill Q1 = NABC numeracy skill Q2 (p-value: 0.0197) and NABC numeracy skill Q4 = NABC numeracy skill Q5 (p-value: 0.0000). The effect for business/economics and legal studies are not significantly different from each other (p-value: 0.1357). Similarly, the coefficients of humanities and social sciences are also not significantly different (p-value: 0.2810).
***: p< 0.01, **: p<0.05, *: p<0.1.

Figure 4: The effect of admission selectivity on obvious mistakes by 11th-grade GPA



Notes: The figure presents the DiD estimates with 95% confidence intervals by 11th-grade GPA. We estimate all the coefficients in a single regression by interacting the treatment indicator with 11th-grade GPA. We include demographic controls including gender, disadvantaged status, age, type of residence, and high-school type. The effect for applicants with missing 11th-grade GPA is 0.178*** (s.e.: 0.0076).

5.4 Threats to Identification and Robustness

We assess the plausibility of our identifying assumptions in various ways. To test the parallel trends assumption we include placebo variables of the treated programs in the pre-reform period; i.e., we compare 2009 (Placebo 2009) and 2010 (Placebo 2010) to 2011. Column 1 of Table 10 adds these placebo treatment variables to the baseline model. Although the placebo coefficients for 2009 and 2010 are statistically significant, they are an order of magnitude lower than our main estimates and precisely estimated. Thus, the potential for bias due to the violation of the common trends assumption is small.

We also study a smaller scale reform that took place in 2011, prior to the introduction of the study contract. This reform, which received much less attention from the media and the public, decreased the number of tuition waivers in business/economics and social sciences by about 20 percent (Table 2). We investigate whether this reform had a similar impact on obvious mistakes. We add indicator variables to our main specification that take the value of one in 2011 for social sciences and business/economics, respectively. Appendix Table B6 presents the results. We find that this smaller reform increased obvious mistakes by 1.1–1.3 percentage points in the affected fields. In Appendix B.3 we show that our results hold in an alternative specification that leverages all variation in the number of funded positions during our sample period.

A potential threat to our identification strategy is that treatment status is defined by applicants' ROL. Applicants' response to changes in admission selectivity may affect the composition of their ROL as well as their decision to apply. This concern is particularly pronounced for students who are not willing (or able) to pay the tuition fee and consider applying only to funded programs. As a response to the reduction in funded positions, these applicants might drop their most preferred (treated) program from their ROL and rank untreated programs instead, biasing our estimates upward. We address this concern in several ways. First, in columns 3–6 of Table 8 we add individual-level controls. Second, we look at applicants who listed at least one unfunded contract in their ROL. By listing at least one unfunded contract, these applicants signal that they are willing to pay tuition; hence we find it less plausible that the reform affected the set of programs in their ROL.[26] Reassuringly, our estimates for this subsample are very similar to the main estimates (columns 2 and 3 of Table 10), indicating that switching behavior does not drive our results.[27] Third, in columns 4 and 5 of Table 10, we restrict the sample to those high-school senior applicants who listed programs both in the fields with severe funding cut and in the fields that were unaffected. This restriction assures that the composition of applicants in the treated and untreated fields are the same.[28] We find that the coefficient estimates remain positive, large, and statistically significant, confirming that changes in the composition of applicants do not drive our results.

Table 10: Robustness analysis

Dependent variable	Obvious mistakes				
	(1)	(2)	(3)	(4)	(5)
Severe funding cut	0.181***	0.161***	0.153***	0.111***	0.107***
	(0.0046)	(0.0056)	(0.0056)	(0.0052)	(0.0051)
Placebo (2009)	-0.018***				
	(0.0021)				
Placebo (2010)	-0.019***				
	(0.0021)				
R-squared	0.098	0.091	0.112	0.068	0.086
# Obs.	607,764	174,182	174,182	161,917	161,917
# ROLs	188,696	57,362	57,362	42,973	42,973
Demographic controls & GPA	-	-	✓	-	✓

Notes: The table presents DiD estimates for the high-school senior applicant sample. Column (1) adds placebo indicators for 2009 and 2010, columns (2) and (3) restrict the sample to the relevant ROLs, columns (4) and (5) restrict the sample to applicants applying to both treated and untreated programs. Robust standard errors clustered on the applicant level are in parenthesis. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: p<0.01 **: p<0.05, *: p<0.1.

[26] Another possibility is that applicants added new programs to their ROL. However, our data show that the number of listed programs declined between 2011 and 2013.

[27] A weakness of this approach is that applicants who would have listed only funded contracts in their ROL in the absence of the reform, might have added the unfunded version of these programs to their ROL. Such behavior would change the composition of the treated group, but in the absence of any treatment effect would not yield positive estimates. If anything, it would bias the estimates downward.

[28] We thank Dániel Horn for proposing this specification.

To further investigate whether our results are not driven by changes in the composition of the treated and control groups, we run placebo regressions that estimate the effect of the reform on pre-determined characteristics of the applicants. In particular, we look at numeracy skill, literacy skill, the NABC-based SES index, and an indicator of non-missing values of the NABC. Appendix Table B7 presents the results. The only statistically significant difference is in the NABC literacy skill (0.04 standard deviations). Given the evidence presented in Section 4.2, we find it implausible that changes in the applicants' composition drive our results.

6 Obvious Mistakes: The Impact on Other Applicants

Obvious mistakes are detrimental to the utility of the applicants who make them. But, applicants' ROLs also influence the allocation of other students. Generally, as funding is over-demanded, each costly mistake translates to a utility gain by another applicant who gets the unclaimed tuition waiver. Moreover, there may be several affected individuals (e.g., one student may take the place of another student whose allocation changed as a result of the freed-up funded position). In this section we evaluate the effect of obvious mistakes on others. We find that obvious mistakes increase the number of students admitted to college. Moreover, mistakes transfer funds from the rich to the poor, thus promoting equity.

Since we do not have access to the exact algorithm that is used to allocate applicants to schools, and since some parameters are impossible to deduce from the data (e.g., how counterfactual ties are dealt with, or how funding is reallocated between programs), we make a few simplifying assumptions in our analysis. Essentially, we assume that each program has a fixed number of funded positions, and we break ties at random. These assumptions reflect the way more standard matching markets function, and presumably have a limited effect on our results. We concentrate on mistakes that are certainly costly, i.e., cases where the applicant could have been admitted to the same program, but with funding. This approach is conservative and keeps the analysis simple as at most one applicant is directly affected. We further restrict the population to those applicants who reported having never attended college before. This restriction minimizes the risk of misclassification of strategic decisions as costly mistakes.[29]

We proceed by correcting all obvious mistakes in each program.[30] We then track the implications for the applicants that are directly displaced by this change. We do not track any further (positive or adverse) effect on others. We then compare the characteristics of individuals who

[29] An applicant who has previously studied in a funded program has, perforce, exhausted some of the 12 funded semesters for which she is eligible. Such applicant may decide, strategically, not to apply for a funded position, because she intends to apply to a more expensive master's program.

[30] We correct all obvious mistakes in the same program to avoid double counting of affected individuals in case that multiple costly mistakes were made with respect to the same program.

make costly mistakes to those of the individuals who gain from them.

Our sample consists of 1,623 ROLs with an obvious mistake that meet the criteria mentioned above. We find that 597 students, corresponding to 37 percent of the mistakes, were admitted to college as a result of others' mistakes. An additional 1,026 students received an assignment they ranked higher due to others' mistakes, of whom 512 would otherwise have been unfunded (typically in the same program). Table 11 compares students with costly mistakes to those who gained from them directly. The immediate effect of a costly mistake is to reallocate funding from high to low socioeconomic status applicants.

It is often assumed that promoting truthful reporting is desirable from the perspective of the social planner. Our findings show that in the context of obvious mistakes in Hungary this assumption may not hold. One reason, which is specific to our setting in which mistakes are related to funding, is that high-SES applicants make more mistakes. Another reason, which we think applies more generally, is that any individual can make a mistake, but directly affected applicants are always marginal. In our setting these are the applicants with the lowest priority score who are still admitted to a funded position. If these applicants are typically weaker (as is the case in our setting), the mistakes we study promote diversity within the program.

It is important to reiterate, however, that our findings on welfare are context-specific, and are particularly related to the fact that money is involved. Generally, mistakes may lead to inefficiencies in allocation and may exacerbate inequity (Rees-Jones, 2017a).

7 Discussion

It is difficult to explain obvious mistakes, especially costly ones, using standard models of matching markets. The literature has proposed several explanations for mistakes in college admissions processes and for mistakes in strategically simple environments. We evaluate these explanations in light of our findings. While it is likely that no single explanation fully accounts for the behavior we document, we review them starting with the one we think drives most of the mistakes.

Submitting an ROL that is inconsistent with the applicant's true preferences is only weakly dominated. In particular, if an applicant assigns zero probability to the event that she will be admitted to a more-preferred alternative, she is indifferent between truthful reporting and making a mistake with respect to this alternative,[31] and if the probability of admission is very low she is nearly indifferent. Our findings are consistent with such behavior. First, we showed that increased admission selectivity (i.e., lower probability of admission all else equal) causes more obvious mistakes. Second, we found that students with low academic ability, who can expect to receive lower

[31] Chen and Pereyra (2017) refer to such behavior as *self-selection*. Artemov et al. (2017) relax this notion allowing behavior that is suboptimal with low probability.

Table 11: The distributional effect of costly obvious mistakes

	Directly affected applicants		Students with costly mistakes		
	Mean	Std. dev.	Mean	Std. dev.	Diff. ((3) - (1))
	(1)	(2)	(3)	(4)	(5)
High-school senior	0.37	0.484	0.30	0.457	-0.076***
Age	24.23	7.207	26.32	7.809	2.097***
Disadvantaged	0.04	0.189	0.02	0.147	-0.015***
Unemployment rate in 2008 (%)	7.46	4.373	6.67	3.942	-0.660***
Unemployment rate in 2008 - missing	0.01	0.121	0.03	0.183	0.020***
Gross annual per capita inc. (1000 USD)	6.46	1.540	6.79	1.538	0.327***
Gross annual per capita inc. - missing	0.01	0.116	0.03	0.183	0.021***
11th-grade GPA	3.49	0.787	3.61	0.811	0.129***
11th-grade GPA - missing	0.30	0.460	0.35	0.476	0.044***
12th-grade GPA	3.46	0.785	3.59	0.776	0.128***
12th-grade GPA - missing	0.27	0.445	0.33	0.472	0.062***
Female	0.50	0.500	0.46	0.498	-0.043**
High school = sec. grammar school	0.57	0.495	0.54	0.498	-0.028
Residence = capital	0.20	0.400	0.24	0.429	0.043***
# Observations.	1,623		1,623		

Notes: The table compares the characteristics of applicants who made costly mistakes that were certainly binding to the characteristics of applicants who directly benefited from these mistakes. Column (5) shows the difference in background characteristics between applicants with costly mistakes and directly affected students, conditional on year fixed effects. For a discussion on the missing GPA values see Footnote 19.
***: p<0.1, **:p<0.05, *:p<0.1.

admission priority, are more likely to make an obvious mistake. Third, high-SES applicants, who presumably are less sensitive to the availability of funding and hence, all else equal, are more likely to be nearly indifferent, make more obvious mistakes. The large average cost of ex-post costly mistakes indicates that the presence of overly pessimistic beliefs about the likelihood of admission to the funded contract is necessary for this explanation to drive our results. The fact that high-school seniors do not know their test scores when they submit their lists increases the plausibility of this theory. Overprecision may lead applicants to underestimate the uncertainty about their own priority score (Grubb, 2015), which may cause them to underestimate the likelihood of passing the priority-score cutoff to the funded contract.[32]

Another potential explanation is that applicants are not aware of the optimal strategy. Here, we do not think that information about the mechanism is an important factor, as such information is readily available through a variety of channels, especially to high-school seniors. Moreover, mistakes were more common in the capital, Budapest, and in other cities where applicants likely enjoyed improved access to information. Additionally, the mechanism generates priority-score cutoffs that become public shortly after the match. If applicants realize that they cannot affect (or are unlikely to affect) the priority-score cutoffs (that is, they are "price-takers"), then they can conclude that ranking contracts in a way that is inconsistent with their preferences is suboptimal, even without detailed knowledge of the mechanism. This feature may explain the low rates of flipping relative to dropping as compared to previous studies of markets where DA was not explained through cutoffs.

Cognitive limitations may, however, hinder applicants' ability to behave optimally (Benjamin et al., 2013), which is consistent with our findings on the correlation between academic ability and obvious mistakes.[33] Hassidim et al. (2017a) suggest that a natural behavior for applicants who do not understand the mechanism is to optimize with respect to a naive theory of the matching mechanism. They suggest that a natural idea in such theories is that the mechanism rewards higher ranking with increased probabilities of allocation (when the applicant is not allocated a higher-ranked alternative). Behavior according to such a naive theory of the market is consistent with the existence of flipping, which is difficult to explain by pessimistic beliefs and (near) indifference. However, it does not explain obvious dropping, which accounts for the overwhelming majority of obvious mistakes in our setting.

Another possibility, which is specific to the Hungarian context, is that individuals may fail to understand the application fee structure. More specifically, they may not understand that the

[32]Overprecision is a leading explanation for why consumers systematically choose suboptimal cellular plans (Grubb, 2009; Grubb and Osborne, 2015).

[33]In this context, it is worth mentioning that the clearinghouse does not provide explicit information about the optimality of honest ranking (although such information about the suboptimality of obvious mistakes is available in popular commercial websites). In a field experiment, Guillen and Hakimov (2016) find that information on the truthfulness of TTC has a positive effect on truth-telling rates, but that describing the mechanism does not.

application fee is charged per program, and not per contract. We do not think this explanation drives our results. Information about application fees is readily available through many sources, including the official website and booklet, and the website includes an application fee calculator. Additionally, in Appendix B.4 we assess this possibility by concentrating on the subsample of applicants who ranked four contracts or more, with three or fewer programs. These applicants must have learned the pricing scheme, because they had to pay only the fixed application fee. We find no evidence indicating that the misunderstanding of the application fee structure would drive obvious mistakes. Finally, if agents have rational expectations, this explanation can only hold under implausibly high levels of risk or loss aversion.

Mistrust may also cause applicants to rank programs in a way that is inconsistent with their preferences. Applicants may doubt the accuracy of information they receive about the mechanism,[34] or the policymaker's commitment to use the stated mechanism.[35] In the Hungarian context, the match has a long history, is governed by legislation, and is operated by the central government. Moreover, since priority-score cutoffs become public shortly after the match, applicants can verify that their assignment is indeed the option they ranked highest among those whose cutoff they surpassed. Hence, we do not think that the lack of trust drives our results.

Another explanation, which is independent of the strategic environment, is that lack of information, and in particular information about financial aid, may cause students to behave suboptimally in college admissions markets (Hoxby and Avery, 2012; Hoxby and Turner, 2015). We do not think that lack of information about funding explains our findings for several reasons. First, funded positions are the historical norm, whereas unfunded positions are the innovation. Thus, while it is reasonable to expect that uninformed agents will generally make more mistakes, the opposite is true for obvious mistakes (which can only occur if the agent ranks some unfunded position). Second, students who make obvious mistakes come from larger settlements and higher socioeconomic status families, where informational frictions are expected to be less severe. Third, since the 2012–2013 reform affected only the availability of funding, it would be surprising if individuals who were not informed about funding drove the effect we identify. Fourth, most mistakes are on ROLs, which also include programs without any mistakes.

An alternative explanation of the behavior we document is that applicants' preferences are "non-classical", and do not exclusively depend on their own allocation. Since there is over-demand for funding, social preferences and altruistic motives are consistent with the patterns we

[34] Applicants may falsely believe that they influence the likelihood of certain probability events that are, in fact, independent of their actions ("magical thinking"). Arad (2014) finds evidence of individuals avoiding "greedy" decisions under uncertainty out of fear that they will be "magically" punished by the universe.

[35] By restricting attention to strategically simple mechanisms, the market designer may limit her ability to achieve certain desiderata (e.g., Bogomolnaia and Moulin, 2001; Bronfman et al., 2015; Roth and Shorrer, 2015). Hence, in the absence of concerns for reputation, legality, or procedural fairness, a benevolent market maker may have an incentive to change the allocation rule after preferences have been collected.

document (Fehr and Fischbacher, 2002; Charness and Rabin, 2002). However, applicants who are admitted with funding have full control over the money they receive and can redistribute it to raise their utility even more. Moreover, the fact that 7 percent of the applicants are deemed disadvantaged by the government, which raises their priority score substantially, reduces the plausibility of this explanation.

Another potential explanation is that applicants have *ego utility* (Kőszegi, 2006), and may distort their choices to avoid receiving information about their priority as this may hurt their self-image. In the context of self-image concerns, it is worth mentioning that applicants learn their priority score, and that the priority-score cutoffs are public information. Thus applicants have access to the same information about their priority no matter what ranking they submit. On the other hand, the strategies we classify as mistakes make this information less salient and easier to ignore. A related explanation is that applicants like to be able to honestly say that they got their first choice. While we find this story plausible in general, in the context of obvious mistakes, we do not believe that many individuals can convince themselves or others that they do not like money.

Lastly, financial aid could – through *sunk cost effects* (Thaler, 1980; Arkes and Blumer, 1985) – reduce students' effort by decreasing the psychological cost of failure. Sophisticated applicants who expect to exert inefficiently low levels of effort during their time in college, may decline financial aid as a sort of commitment device. We find this explanation less plausible. First, since admission with financial aid provides pure option value (applicants may decline receiving without forfeiting their seat), this explanation would still suggest that applicants are making a mistake. And second, empirical studies largely reject the existence of sunk-cost effects in education (e.g., Ketel et al., 2016).

8 Concluding Remarks

Obvious mistakes, i.e., dominated strategies that forgo the free opportunity to receive financial aid, but have no benefit, are a "smoking gun" indicating that dominated-strategy play is prevalent in real-life, high-stakes, strategically simple environments. Applicants likely make other mistakes that we cannot detect using our approach. If the prevalence of costly obvious mistakes is indicative of the prevalence of other costly mistakes, then our findings indicate that mistakes potentially have large welfare implications.

We have established that obvious mistakes are more common among high socioeconomic status applicants. These mistakes lead to an increase in the number of students attending college and to a transfer of funding from rich to poor applicants. While this self-selection pattern emerged in the absence of incentives, it suggests a non-negligible scope for gains from adding (incentivized) screening to college admissions mechanisms. Hassidim et al. (2017b) have recently shown that there is a substantial scope for screening without compromising stability. Addressing this challenge is a promising direction for future research.

Previous studies mainly focused on the incentive properties of matching mechanisms, giving special attention to strategic simplicity. This study documents behavior that cannot be rationalized using standard models of matching markets, and thus suggests that human psychology plays an important role even in strategically simple matching environments. An upshot is that the description of a mechanism may affect user behavior. For example, while Rees-Jones (2017b) finds many instances of flipping in ROLs submitted to the NRMP and Hassidim et al. (2016) find almost equal rates of obvious flipping and obvious dropping in ROLs submitted to the IPMM, we find substantially lower rates of obvious flipping relative to obvious dropping. We think that the difference derives from the fact that in the Hungarian mechanism priorities are communicated to applicants as priority scores, and the outcome is expressed through priority-score cutoffs. By contrast, the other mechanisms describe priorities through ROLs and provide a combinatorial description of an algorithm that determines the allocation. This insight, in turn, highlights the practical importance of research that provides tractable and transparent descriptions of mechanisms with attractive properties.[36] More broadly, our findings suggest that a better understanding of human behavior in centralized matching environments holds great promise for capitalizing on the advances made in the recent decades to the study of matching markets.

[36]A prominent example is Leshno and Lo's description of the Top Trading Cycle (TTC) mechanism through cutoffs (Leshno and Lo, 2017).

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A Matching College Admissions Data to the NABC

The National Assessment of Basic Competencies (NABC) has been conducted since 2003. Our data cover the period between 2006 and 2011. Prior to 2008, the NABC did not cover the full population: only 30 students from each track in each high school completed the the assessment. For this reason, the dataset only covers approximately a half of the population. Since 2008, NABC has been mandatory. Thus our data cover each student who were not absent from school on the day of the test.

As discussed in Section 3.2, we match high-school senior applicants to the NABC dataset based on observable demographic characteristics: year and month of birth, high-school identifier, gender, and postal code. Traditionally, students attend high schools for four years. However, since 2004, certain schools have been offering five-year programs, in which the first year is dedicated to foreign languages. Students complete the NABC exam in the second year of high school, irrespective of the type of program; therefore the time lag between the competency test and the matriculation exam can be two or three years. The NABC is conducted two or three years before applicants' senior year. We are, thus, unable to match seniors who moved to a new postal code or to a new high school in that period of time.

Table A1 describes the result of the matching. The more variables we use for matching, the fewer applicants we are able to match. Between 2011 and 2013, when the NABC covers the full population (held between 2008 and 2011), the share of matched students is stable. We are able to match 91–92 percent of seniors based on 3 variables, 89–90 percent based on 4 variables, and 79–80 percent based on 5 variables. The share of unique matches is also stable in these years: 16–20 percent of seniors based on 3 variables, 41–43 percent based on 4 variables, and 63–65 percent based on 5 variables. With the exception of 2009, as the matching becomes finer, we match more individuals uniquely. The reason for the irregularity in 2009 is twofold. First, since we do not observe the full population, the match cannot be refined by including more matching variables (due to empty cells). Second, the postal code was self-reported in the first two years of our NABC data, which leads to stronger attrition as we include the postal code among the matching variables. In our main analysis we use the matching that is based on 5 variables (Panel C).

Table A1: Matching college admissions data to the NABC

	Matched individuals		Uniquely matched individuals	
	Share (%)	Count	Share (%)	Count
	(1)	(2)	(3)	(4)
A. *Matching based on 3 variables*				
2009	89	45,306	29	14,636
2010	90	45,050	22	11,069
2011	92	45,024	20	9,621
2012	92	36,438	19	7,364
2013	91	35,460	16	6,114
Total	91	207,278	21	48,804
B. *Matching based on 4 variables*				
2009	68	34,371	55	27,916
2010	83	41,742	51	25,730
2011	90	43,910	43	21,257
2012	90	35,688	44	17,442
2013	89	34,699	41	15,979
Total	83	190,410	47	108,324
C. *Matching based on 5 variables*				
2009	32	16,111	29	14,857
2010	62	31,136	54	27,133
2011	79	38,500	64	31,348
2012	80	31,910	65	25,728
2013	80	30,947	63	24,686
Total	65	148,604	54	123,752

Notes: The table describes the outcome of matching the NABC dataset to the high-school senior applicant sample (N = 228,606). Matching based on 3 variables: year of birth, gender, school identifier; matching based on 4 variables: year and month of birth, gender, school identifier; matching based on 5 variables: year and month of birth, gender, school identifier, postal code.

B Additional Results

B.1 Obvious Mistakes and Their Correlates

Table B1: The correlates of obvious mistakes (relevant applicants)

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
NABC-based SES index	0.010***			
	(0.0022)			
Unemployment rate in 2008 (%)		-0.003***		
		(0.0004)		
Gross annual per capita income (1000 USD)			0.010***	
			(0.0016)	
Disadvantaged status (dummy)				-0.030***
				(0.0069)
Numeracy skill	-0.028***	-0.028***	-0.028***	-0.027***
	(0.0023)	(0.0023)	(0.0023)	(0.0022)
11th-grade GPA	-0.055***	-0.054***	-0.054***	-0.055***
	(0.0027)	(0.0027)	(0.0027)	(0.0027)
Female	0.017***	0.015***	0.016***	0.016***
	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Vocational school	0.007	0.004	0.005	0.004
	(0.0043)	(0.0042)	(0.0042)	(0.0042)
Other high schools	0.015	0.013	0.015	0.015
	(0.0148)	(0.0148)	(0.0148)	(0.0148)
County capital	-0.027***	-0.023***	-0.009	-0.028***
	(0.0054)	(0.0054)	(0.0062)	(0.0053)
Town	-0.031***	-0.024***	-0.010	-0.033***
	(0.0050)	(0.0053)	(0.0063)	(0.0050)
Village	-0.038***	-0.030***	-0.012*	-0.038***
	(0.0054)	(0.0057)	(0.0071)	(0.0054)
# Obs.	44,786	44,786	44,786	44,786
R-squared	0.035	0.035	0.035	0.034

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those relevant high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 15.5 percent in the relevant subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 29.3 percent of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Table B2: The correlates of obvious mistakes pre- and post-reform

Dependent variable	Obvious mistakes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NABC-based SES index	0.013***	0.022***						
	(0.0008)	(0.0015)						
Unempl. rate (%)			-0.001***	-0.003***				
			(0.0001)	(0.0002)				
Gross income (1000 USD)					0.008***	0.013***		
					(0.0007)	(0.0011)		
Disadvantaged							-0.018***	-0.047***
							(0.0016)	(0.0028)
Numeracy skill	-0.011***	-0.021***	-0.009***	-0.018***	-0.009***	-0.018***	-0.008***	-0.018***
	(0.0009)	(0.0015)	(0.0008)	(0.0015)	(0.0008)	(0.0015)	(0.0008)	(0.0015)
11th-grade GPA	-0.022***	-0.035***	-0.021***	-0.034***	-0.021***	-0.034***	-0.022***	-0.035***
	(0.0011)	(0.0020)	(0.0011)	(0.0020)	(0.0011)	(0.0020)	(0.0011)	(0.0020)
Female	0.005***	0.019***	0.004***	0.018***	0.004***	0.018***	0.004***	0.019***
	(0.0014)	(0.0024)	(0.0014)	(0.0024)	(0.0014)	(0.0024)	(0.0014)	(0.0024)
Sample period	2009/11	2012/13	2009/11	2012/13	2009/11	2012/13	2009/11	2012/13
# Obs.	78,610	55,104	78,610	55,104	78,610	55,104	78,610	55,104
R-squared	0.019	0.031	0.017	0.029	0.018	0.030	0.017	0.030

Notes: The regression coefficients are conditional on year fixed effects and demographics, such as age, high-school type, and type of residence. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 5.2% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 28.5% of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Table B3: The correlates of obvious mistakes (eligible applicants)

Dependent variable	Obvious mistakes		
	(1)	(2)	(3)
Unemployment rate in 2008 (%)	-0.001***		
	(0.0001)		
Gross annual per capita income (1000 USD)		0.009***	
		(0.0005)	
Disadvantaged status (dummy)			-0.036***
			(0.0012)
11th-grade GPA	-0.027***	-0.028***	-0.028***
	(0.0007)	(0.0007)	(0.0007)
Female	0.022***	0.022***	0.023***
	(0.0009)	(0.0009)	(0.0009)
Vocational school	-0.009***	-0.008***	-0.008***
	(0.0011)	(0.0011)	(0.0011)
Other high schools	-0.026***	-0.025***	-0.029***
	(0.0031)	(0.0031)	(0.0024)
County capital	-0.025***	-0.010***	-0.027***
	(0.0016)	(0.0018)	(0.0016)
Town	-0.026***	-0.009***	-0.031***
	(0.0015)	(0.0019)	(0.0014)
Village	-0.031***	-0.009***	-0.035***
	(0.0016)	(0.0021)	(0.0015)
# Obs.	447,989	447,989	447,989
R-squared	0.078	0.078	0.078

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The share of obvious mistakes is 10.9 percent. Eleventh-grade GPA is missing for 29.8 percent of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

B.2 The Effect of Admission Selectivity on Obvious Mistakes: Robustness

Table B4: The effect of admission selectivity on obvious dropping and on obvious flipping

Dependent variable	Obvious dropping		Obvious flipping	
	(1)	(2)	(3)	(4)
Severe funding cut	0.178***	0.173***	0.015***	0.015***
	(0.0041)	(0.0041)	(0.0013)	(0.0013)
Mean outcome	0.028	0.028	0.004	0.004
R-squared	0.093	0.104	0.009	0.010
# Obs.	607,764	607,764	607,764	607,764
# ROLs	188,696	188,696	188,696	188,696
Demographic controls & GPA	-	✓	-	✓

Notes: The table presents the DiD estimates on the high-school senior applicant sample. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

Table B5: The effect of admission selectivity on obvious mistakes: missing NABC variables

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
Severe funding cut	0.187***	0.187***	0.187***	0.186***
	(0.0043)	(0.0047)	(0.0042)	(0.0050)
Mean outcome	0.032	0.033	0.032	0.031
R-sq	0.111	0.122	0.111	0.121
# Obs.	607,764	376,914	607,764	342,701
# ROLs	188,696	116,698	188,696	105,992
Demographic controls & GPA	✓	✓	✓	✓
NABC controls	✓	✓	✓	✓
NABC-based SES index	-	-	✓	✓

Notes: The table presents the DiD estimates on the high-school senior applicant sample. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program-level fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. NABC controls refer to dummies for 20 quantiles of the numeracy and literacy scores. NABC-based SES refers to dummies for 20 quantiles of the NABC-based SES index. In columns (1) and (3) we add dummy variables for the missing values of the NABC variables (numeracy skill, literacy skill, NABC-based SES index). In columns (2) and (4) we drop observations with missing NABC values.
***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

Table B6: The effect of admission selectivity on obvious mistakes: the 2011 reform

Dependent variable	Obvious mistakes	
	(1)	(2)
Severe funding cut in 2013	0.196***	0.191***
	(0.0043)	(0.0043)
Funding cut in 2011 - business/economics	0.014***	0.013***
	(0.0022)	(0.0022)
Funding cut in 2011 - social sciences	0.012***	0.011***
	(0.0032)	(0.0032)
Mean outcome	0.032	0.032
R-squared	0.098	0.110
# Obs.	607,764	607,764
# ROLs	188,696	188,696
Demographic controls & GPA	-	✓

Notes: The table presents the DiD estimates for the small-scale reform in 2011 in social sciences and business/economics. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table B7: Test for the stable composition of applicants

Dependent variable	NABC missing	Numeracy skill	Literacy skill	NABC-based SES
	(1)	(2)	(3)	(4)
Severe funding cut	0.011	-0.017	-0.040***	0.021
	(0.0066)	(0.0124)	(0.0110)	(0.0131)
R-squared	0.087	0.194	0.306	0.231
# Obs.	276,912	194,437	194,437	194,437
# ROLs	87,721	61,149	61,149	61,149

Notes: The table presents placebo DiD estimates for various background characteristics in the high-school senior applicant sample for the years 2011 and 2013. Robust standard errors clustered on the applicant level are in parentheses. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: p<0.01, **: p<0.05, *: p<0.1.

B.3 The Effect of Admission Selectivity on Obvious Mistakes: Alternative Specification

Section 5.2 established that admission selectivity has a large, positive causal effect on obvious mistakes. We test the robustness of this result by considering an alternative specification. Instead of focusing solely on the 2012–2013 reform, we exploit all variation in the availability of funded positions in the sample (Table 2). This alternative approach allows us to estimate the elasticity with respect to the available funded positions and obvious mistakes.

Analogously to our main model, we estimate the following specification:

$$Y_{itfs} = \alpha + \beta \cdot \log(capacity_{tf}) + \gamma \cdot X_{it} + \eta_s + \nu_t + \varepsilon_{itfs},$$

where capacity$_{tf}$ denotes the number of available funded positions in year t and field of study f (to which s belongs). We index capacity by f to highlight that there is no within-field of study variation in the number of available funded positions.[37] In line with our main result, we expect the estimate for β to be negative, as more available funded seats correspond to lower admission selectivity. On the other hand, the 2012–2013 reform was salient and stark relative to other changes that were small and sometimes inconsequential, which limits the comparability of this specification to our main findings.

Table B8 presents our estimates. We find that a 10 percent reduction in the number of funded seats increases obvious mistakes by 0.82–0.85 percentage points.

[37] Since the government did not release the funded quotas for 2013, we use the number of realized funded positions.

Table B8: The effect of the number of funded positions on obvious mistakes

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
Capacity (log)	-0.085***	-0.082***	-0.082***	-0.082***
	(0.0019)	(0.0019)	(0.0019)	(0.0019)
Mean outcome	0.032	0.032	0.032	0.032
R-squared	0.093	0.106	0.107	0.108
# Obs.	604,971	604,971	604,971	604,971
# ROLs	188,550	188,550	188,550	188,550
Demographic controls & GPA	-	✓	✓	✓
NABC controls	-	-	✓	✓
NABC-based SES index	-	-	-	✓

Notes: The table presents DiD estimates for the high-school senior applicant sample. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. NABC controls refer to dummies for 20 quantiles of the numeracy and literacy skills. NABC-based SES refers to dummies for 20 quantiles of the NABC-based SES index. Missing control variables are always indicated by a separate dummy variable.
***: p<0.01 **: p<0.05, *: p<0.1.

B.4 Understanding the Application Fee Structure

A potential explanation for the prevalence of obvious mistakes is that applicants do not understand the application fee structure. In particular, applicants might believe that they pay a fee for each contract beyond the third contract on their ROL, instead of per program. Under this (erroneous) belief, it might be rational to drop contracts from the ROL. We argue that this sort of misunderstanding of the application fee structure does not drive our results.

For each ROL, we create an indicator variable whether for students who ranked four contracts or more, with three or fewer programs. These applicants must have learned the pricing scheme, because they had to pay only the fixed application fee. Table B9 present the correlations between socioeconomic status, academic achievement and obvious mistakes for these two group of applicants separately. The positive correlation between socioeconomic status and obvious mistakes holds in both groups. Similarly, Table B9 shows the correlation between academic achievement and obvious mistakes is negative for both subgroups.

Table B9: The correlates of obvious mistakes: understanding the application fee structure

Dependent variable	Obvious mistakes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NABC-based SES index	0.020***	0.002						
	(0.0008)	(0.0021)						
Unempl. rate (%)			-0.002***	-0.001*				
			(0.0001)	(0.0004)				
Gross income (1000 USD)					0.012***	0.004***		
					(0.0007)	(0.0016)		
Disadvantaged							-0.033***	-0.004
							(0.0015)	(0.0069)
Numeracy skill	-0.016***	-0.008***	-0.014***	-0.008***	-0.014***	-0.009***	-0.013***	-0.008***
	(0.0009)	(0.0022)	(0.0009)	(0.0021)	(0.0009)	(0.0021)	(0.0008)	(0.0021)
11th-grade GPA	-0.029***	-0.017***	-0.028***	-0.017***	-0.028***	-0.017***	-0.028***	-0.017***
	(0.0011)	(0.0026)	(0.0011)	(0.0026)	(0.0011)	(0.0026)	(0.0011)	(0.0026)
Female	0.011***	0.012***	0.009***	0.012***	0.009***	0.012***	0.010***	0.012***
	(0.0014)	(0.0035)	(0.0014)	(0.0035)	(0.0014)	(0.0035)	(0.0014)	(0.0035)
Fee structure	-	✓	-	✓	-	✓	-	✓
# Obs.	111,612	22,102	111,612	22,102	111,612	22,102	111,612	22,102
R-squared	0.045	0.009	0.042	0.009	0.043	0.009	0.042	0.009

Notes: The regression coefficients are conditional on year fixed effects and demographics, such as age, high-school type, and type of residence. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 5.2% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 28.5% of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

C Additional Results on Costly Obvious Mistakes

In this appendix we replicate the analysis from the body of the paper, but this time focus only on the costly mistakes. We use both the permissive and the conservative definitions of costly mistakes (the lower and the upper bound). For the interpretation of the results, it is important to keep in mind that in order for a costly mistake to occur, three things must happen. First, the applicant should make a mistake. Second, she must pass the priority-score cutoff. And third, for the restrictive definition, she must be rejected from all the contracts she ranked higher.

Subsection C.1 presents the correlates of costly obvious mistakes, Subsection C.2 shows the difference-in-differences estimates for the effect of admission selectivity on ex post costly obvious mistakes.

C.1 Ex Post Costly Obvious Mistakes: Correlates

Table C1: The correlates of ex post costly obvious mistakes (lower bound)

Dependent variable	Ex post costly obvious mistakes - lower bound					
	(1)	(2)	(3)	(4)	(5)	(6)
NABC-based SES index	0.0001	0.0002	0.0003			
	(0.00016)	(0.00017)	(0.00017)			
Unemployment rate in 2008 (%)				-0.0001**		
				(0.00003)		
Gross annual per capita income (1000 USD)					0.0002	
					(0.00015)	
Disadvantaged status (dummy)						-0.0012***
						(0.00032)
Numeracy skill		-0.0006***	-0.0004**	-0.0004**	-0.0004**	-0.0004**
		(0.00017)	(0.00018)	(0.00017)	(0.00017)	(0.00017)
11th-grade GPA			-0.0008***	-0.0008***	-0.0008***	-0.0008***
			(0.00021)	(0.00021)	(0.00021)	(0.00021)
Female	-0.0000	-0.0003	-0.0001	-0.0001	-0.0001	-0.0000
	(0.00027)	(0.00029)	(0.00029)	(0.00029)	(0.00029)	(0.00029)
Vocational school	-0.0001	-0.0003	-0.0005	-0.0006*	-0.0006*	-0.0006*
	(0.00030)	(0.00031)	(0.00033)	(0.00032)	(0.00032)	(0.00032)
Other high schools	0.0004	0.0002	0.0001	0.0000	0.0001	0.0001
	(0.00155)	(0.00155)	(0.00155)	(0.00155)	(0.00155)	(0.00155)
County capital	-0.0012**	-0.0012**	-0.0011**	-0.0010**	-0.0008	-0.0011**
	(0.00053)	(0.00053)	(0.00053)	(0.00053)	(0.00062)	(0.00053)
Town	-0.0008	-0.0008*	-0.0008	-0.0006	-0.0003	-0.0008
	(0.00051)	(0.00051)	(0.00051)	(0.00053)	(0.00067)	(0.00051)
Village	-0.0017***	-0.0018***	-0.0017***	-0.0015***	-0.0011	-0.0017***
	(0.00050)	(0.00051)	(0.00051)	(0.00054)	(0.00073)	(0.00051)
# Obs.	133,714	133,714	133,714	133,714	133,714	133,714
R-squared	0.004	0.004	0.004	0.004	0.004	0.004

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parenthesis. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the high-school senior applicant sample to individuals whose numeracy skill, literacy skill, and NABC-based SES index are not missing. The fraction of ex post costly obvious mistakes (lower bound) in this sample is 0.24 percent.
***: p<0.01, **: p<0.05, *:p<0.1.

Table C2: The correlates of ex post costly obvious mistakes (upper bound)

Dependent variable	Ex post costly obvious mistakes - upper bound					
	(1)	(2)	(3)	(4)	(5)	(6)
NABC-based SES index	0.0010***	0.0011***	0.0011***			
	(0.00025)	(0.00026)	(0.00026)			
Unemployment rate in 2008 (%)				-0.0001***		
				(0.00004)		
Gross annual per capita income (1000 USD)					0.0006***	
					(0.00021)	
Disadvantaged status (dummy)						-0.0022***
						(0.00050)
Numeracy skill		-0.0003	-0.0001	0.0001	0.0001	0.0001
		(0.00026)	(0.00027)	(0.00027)	(0.00027)	(0.00026)
11th-grade GPA			-0.0008**	-0.0007**	-0.0007**	-0.0007**
			(0.00031)	(0.00031)	(0.00031)	(0.00031)
Female	0.0002	0.0001	0.0003	0.0002	0.0002	0.0002
	(0.00042)	(0.00044)	(0.00044)	(0.00044)	(0.00044)	(0.00044)
Vocational school	-0.0006	-0.0007	-0.0009*	-0.0012**	-0.0011**	-0.0011**
	(0.00045)	(0.00046)	(0.00049)	(0.00048)	(0.00048)	(0.00048)
Other high schools	0.0032	0.0031	0.0030	0.0029	0.0029	0.0029
	(0.00259)	(0.00259)	(0.00259)	(0.00259)	(0.00259)	(0.00259)
County capital	-0.0026***	-0.0026***	-0.0026***	-0.0025***	-0.0017*	-0.0027***
	(0.00084)	(0.00084)	(0.00084)	(0.00085)	(0.00095)	(0.00084)
Town	-0.0033***	-0.0034***	-0.0033***	-0.0031***	-0.0022**	-0.0035***
	(0.00078)	(0.00078)	(0.00078)	(0.00082)	(0.00100)	(0.00079)
Village	-0.0038***	-0.0038***	-0.0038***	-0.0037***	-0.0025**	-0.0040***
	(0.00080)	(0.00080)	(0.00080)	(0.00084)	(0.00108)	(0.00080)
#. Obs.	133,714	133,714	133,714	133,714	133,714	133,714
R-squared	0.006	0.006	0.006	0.006	0.006	0.006

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parenthesis. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the high-school senior applicant sample to those individuals whose numeracy skill, literacy skill, and NABC-based SES index are not missing. The fraction of ex post costly obvious mistakes (upper bound) in this sample is 0.55 percent.
***: p<0.01, **: p<0.05, *:p<0.1.

Table C3: The correlates of ex post costly obvious mistakes (lower bound) - relevant applicants

Dependent variable	Ex post costly obvious mistakes - lower bound			
	(1)	(2)	(3)	(4)
NABC-based SES index	-0.0010*			
	(0.00054)			
Unemployment rate in 2008 (%)		-0.0000		
		(0.00010)		
Gross annual per capita income (1000 USD)			-0.0002	
			(0.00041)	
Disadvantaged status (dummy)				-0.0011
				(0.00158)
Numeracy skill	-0.0006	-0.0008	-0.0007	-0.0008
	(0.00053)	(0.00052)	(0.00052)	(0.00052)
11th-grade GPA	-0.0016***	-0.0016***	-0.0016***	-0.0016***
	(0.00061)	(0.00061)	(0.00061)	(0.00061)
Female	-0.0011	-0.0011	-0.0011	-0.0010
	(0.00086)	(0.00086)	(0.00086)	(0.00086)
Vocational school	-0.0012	-0.0010	-0.0010	-0.0009
	(0.00097)	(0.00095)	(0.00096)	(0.00096)
Other high schools	-0.0007	-0.0006	-0.0006	-0.0006
	(0.00360)	(0.00360)	(0.00360)	(0.00360)
County capital	-0.0015	-0.0013	-0.0018	-0.0014
	(0.00121)	(0.00123)	(0.00146)	(0.00121)
Town	0.0004	0.0008	0.0001	0.0007
	(0.00117)	(0.00124)	(0.00154)	(0.00119)
Village	-0.0020	-0.0016	-0.0024	-0.0016
	(0.00122)	(0.00131)	(0.00170)	(0.00123)
# Obs.	44,786	44,786	44,786	44,786
R-squared	0.006	0.006	0.006	0.006

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parenthesis. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing. The fraction of ex post costly obvious mistakes (lower bound) in this sample is 0.71 percent.
***: p<0.01, **: p<0.05, *:p<0.1.

Table C4: The correlates of ex post costly obvious mistakes (upper bound) - relevant applicants

Dependent variable	Ex post costly obvious mistakes - upper bound			
	(1)	(2)	(3)	(4)
NABC-based SES index	-0.0006			
	(0.00080)			
Unemployment rate in 2008 (%)		0.0000		
		(0.00015)		
Gross annual per capita income (1000 USD)			-0.0003	
			(0.00056)	
Disadvantaged status (dummy)				0.0001
				(0.00251)
Numeracy skill	0.0015*	0.0014*	0.0015*	0.0014*
	(0.00082)	(0.00082)	(0.00082)	(0.00081)
11th-grade GPA	-0.0000	-0.0000	-0.0000	-0.0000
	(0.00090)	(0.00091)	(0.00090)	(0.00090)
Female	-0.0015	-0.0014	-0.0014	-0.0014
	(0.00130)	(0.00130)	(0.00130)	(0.00130)
Vocational school	-0.0021	-0.0019	-0.0019	-0.0019
	(0.00145)	(0.00142)	(0.00142)	(0.00142)
Other high schools	0.0052	0.0053	0.0053	0.0053
	(0.00598)	(0.00598)	(0.00598)	(0.00598)
County capital	-0.0024	-0.0023	-0.0028	-0.0023
	(0.00193)	(0.00196)	(0.00222)	(0.00193)
Town	-0.0029	-0.0029	-0.0033	-0.0027
	(0.00177)	(0.00188)	(0.00227)	(0.00178)
Village	-0.0027	-0.0027	-0.0033	-0.0026
	(0.00193)	(0.00205)	(0.00248)	(0.00194)
# Obs.	44,786	44,786	44,786	44,786
R-squared	0.007	0.007	0.007	0.007

Notes: The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parenthesis. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing. The fraction of ex post costly obvious mistakes (lower bound) in this sample is 1.64 percent.

***: p<0.01, **: p<0.05, *:p<0.1.

C.2 The Effect of Admission Selectivity on Ex Post Costly Obvious Mistakes

Table C5: The effect of admission selectivity on the lower bound of ex post costly obvious mistakes

Dependent variable	Ex post costly obvious mistakes - lower bound				
	(1)	(2)	(3)	(4)	(5)
Severe funding cut	-0.0032***	-0.0031***	-0.0032***	-0.0032***	-0.0032***
	(0.00031)	(0.00031)	(0.00032)	(0.00031)	(0.00031)
R-squared	0.010	0.011	0.012	0.010	0.010
Demographic controls & GPA	-	✓	✓	✓	✓
High school FE	-	-	✓	-	-
NABC controls	-	-	-	✓	✓
NABC-based SES index	-	-	-	-	✓

Notes: Robust standard errors clustered on the applicant-level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs (high-school senior applicants). The mean outcome is 0.08 percent in the sample. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. NABC controls refer to dummies for 20 quantiles of the numeracy and literacy scores. NABC-based SES refers to dummies for 20 quantiles of the NABC-based SES index. Missing control variables are always indicated by a separate dummy variable.
***: $p < 0.01$, **: $p<0.05$, *: $p<0.1$.

Table C6: The effect of admission selectivity on the upper bound of ex post costly obvious mistakes

Dependent variable	Ex post costly obvious mistakes - upper bound				
	(1)	(2)	(3)	(4)	(5)
Severe funding cut	-0.0058***	-0.0057***	-0.0057***	-0.0058***	-0.0058***
	(0.00068)	(0.00068)	(0.00069)	(0.00068)	(0.00068)
R-squared	0.018	0.019	0.022	0.018	0.018
Demographic controls & GPA	-	✓	✓	✓	✓
High school FE	-	-	✓	-	-
NABC controls	-	-	-	✓	✓
NABC-based SES index	-	-	-	-	✓

Notes: Robust standard errors clustered on the applicant-level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs (high-school senior applicant sample). The mean outcome is 0.22 percent in the sample. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. NABC controls refer to dummies for 20 quantiles of the numeracy and literacy scores. NABC-based SES refers to dummies for 20 quantiles of the NABC-based SES index. Missing control variables are always indicated by a separate dummy variable.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table C7: Heterogeneous effects: 11th-grade GPA

Dependent variable	Ex post costly obvious mistakes			
	Lower bound		Upper bound	
	(1)	(2)	(3)	(4)
Severe funding cut × 11th-grade GPA missing	-0.0034***	-0.0034***	-0.0070***	-0.0069***
	(0.00039)	(0.00039)	(0.00097)	(0.00097)
Severe funding cut × 11th-grade GPA $\in [2, 3]$	-0.0042***	-0.0043***	-0.0098***	-0.0099***
	(0.00030)	(0.00030)	(0.00056)	(0.00057)
Severe funding cut × 11th-grade GPA $\in (3, 4]$	-0.0040***	-0.0040***	-0.0085***	-0.0086***
	(0.00026)	(0.00026)	(0.00070)	(0.00071)
Severe funding cut × 11th-grade GPA $\in (4, 5)$	-0.0017**	-0.0017**	-0.0029**	-0.0030**
	(0.00077)	(0.00077)	(0.00147)	(0.00147)
Severe funding cut × 11th-grade GPA = 5	-0.0030***	-0.0030***	-0.0010	-0.0010
	(0.00045)	(0.00045)	(0.00198)	(0.00198)
R-squared	0.010	0.010	0.018	0.018

Notes: Each column estimates the coefficients in a single regression by interacting the treatment variable with a subgroup indicator of 11th-grade GPA. Robust standard errors clustered on the applicant level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs. The mean lower (upper) bound on the costly obvious mistakes is 0.08 percent (0.22 percent) in the sample. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: p<0.01 **: p<0.05, *: p<0.1.

Table C8: Heterogeneous effects: NABC numeracy skill

Dependent variable	Ex post costly obvious mistakes			
	Lower bound		Upper bound	
	(1)	(2)	(3)	(4)
Severe funding cut × NABC numeracy Q1	-0.0034***	-0.0035***	-0.0078***	-0.0080***
	(0.00039)	(0.00039)	(0.00083)	(0.00083)
Severe funding cut × NABC numeracy Q2	-0.0027***	-0.0028***	-0.0069***	-0.0070***
	(0.00063)	(0.00063)	(0.00105)	(0.00105)
Severe funding cut × NABC numeracy Q3	-0.0030***	-0.0030***	-0.0049***	-0.0050***
	(0.00042)	(0.00042)	(0.00140)	(0.00141)
Severe funding cut × NABC numeracy Q4	-0.0030***	-0.0031***	-0.0041***	-0.0043***
	(0.00049)	(0.00049)	(0.00159)	(0.00160)
Severe funding cut × NABC numeracy Q5	-0.0019**	-0.0019**	-0.0020	-0.0021
	(0.00087)	(0.00087)	(0.00192)	(0.00193)
R-squared	0.014	0.014	0.022	0.022
Demographic controls & GPA	-	✓	-	✓

Notes: Each column estimates the coefficients in a single regression by interacting the treatment variable (severe funding cut) with a quintile indicator of the NABC numeracy skill. We drop applicants with missing NABC numeracy or literacy skill from the sample. Robust standard errors clustered on the applicant level are in parenthesis. The number of observations is 376,914, which correspond to 116,698 ROLs. The mean lower (upper) bound on the costly obvious mistakes is 0.09 percent (0.24 percent) in the sample. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.
***: p<0.01 **: p<0.05, *: p<0.1.