Obvious Mistakes in a Strategically Simple College Admissions Environment: Causes and Consequences

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Abstract

Although many centralized school assignment systems use strategically simple mechanisms, applicants often make dominated choices. Using administrative data from Hungary, we show that many college applicants forgo the free opportunity to receive a tuition waiver. Using two empirical strategies, we provide causal evidence that applicants make more such mistakes when applying to programs where tuition waivers are more selective. First, exploiting a reform that increased the selectivity of admission with a tuition waiver in some programs, we find that the rate of mistakes quadrupled. Second, we show that applicants that apply to multiple programs are more likely to make mistakes in their applications to more selective programs. A non-negligible share of these mistakes are consequential, costing applicants more than 3,000 dollars on average. Costly mistakes transfer tuition waivers from high– to low-socioeconomic status students, and increase the number of students admitted to college. Our results suggest that mistakes are more common when their expected utility cost is lower.

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1 Introduction

Millions of students around the world are assigned to schools through centralized clearinghouses. An increasing share of these clearinghouses adopt strategically simple mechanisms, where ranking alternatives in a way that is inconsistent with one's preferences is a dominated strategy.[1] But recent evidence suggests that many participants make dominated choices, even in these simple environments.[2] Despite economists' central role in the practical design and operation of school assignment systems (e.g., Abdulkadiroğlu et al., 2005, 2006), little is known about the causes and consequences of this behavior.

We study the causes and consequences of dominated choices in strategically simple environments. Detecting dominated choices is a major challenge, as it requires information on preferences that is independent of observed choices (Gabaix, 2019). We overcome this challenge by studying college admissions in Hungary. The Hungarian centralized clearinghouse uses a strategically simple version of the Deferred Acceptance (DA) mechanism to place approximately 100,000 applicants each year. A special feature of this market is that certain alternatives have an intrinsic natural ranking: admission to the same study program with and without financial aid.

Using administrative data, we find that a large fraction of applicants make an *obvious mistake*: they submit a Rank-Order List (ROL) that is inconsistent with the natural ranking. These applicants forgo the free opportunity to receive a tuition waiver worth thousands of dollars, even though this behavior has no benefit. According to our most conservative lower bound, about 9.8 percent of high-school senior applicants made such a mistake in 2013–14. Between 5.8 and 11.9 percent of these mistakes were costly: the applicant could have received a more desirable assignment had she asked for it. When mistakes were costly, the average monetary loss was approximately 3,000–3,500 dollars.[3]

[1]Strategy-proof mechanisms, where participants have a dominant strategy of reporting their true preferences, are viewed to be appealing because of their strategic simplicity. 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). Pathak and Sönmez (2013) report on dozens of school-choice systems around the world that implemented strategically simple versions of the Deferred Acceptance mechanism (DA; Gale and Shapley, 1962), only one of which (Boston Public Schools') was strategy-proof.

[2]E.g., Rees-Jones (2018) and Hassidim et al. (2018b).

[3]The fact that we label the behavior we document as "mistakes" or dominated choices is not innocuous. It relies crucially on the assumption that agents' utility depends only on the realized assignment, and more specifically only on the agents' own assignment. While this assumption is necessary for DA to be strategy-proof and it is standard in the market design literature (e.g., Pathak and Sönmez, 2013), there are other

To explore the causes of obvious mistakes we ask who makes them and when. We find that obvious mistakes are more common among applicants of lower academic achievement. Since financial aid is merit-based, these applicants are less likely to receive it. We also find that, all else equal, applicants coming from a higher socioeconomic status (SES) background are more likely to make such mistakes. These patterns indicate that applicants tend to make obvious mistakes when they (often erroneously) do not expect them to be costly.

We bolster this theory by establishing a causal relationship between the selectivity of admission to funded positions and obvious mistakes, using two complementary empirical strategies. Our first strategy is a difference-in-differences design that leverages variation stemming from a sharp change in 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. We find that obvious mistakes in applications to the affected fields more than quadrupled as a result of the rise in admission selectivity.

A concern with our first empirical strategy is that the effect we find is the result of other changes that occurred simultaneously,[4] or that it is a short-run reaction to the reform in 2012.[5] These issues motivate our second empirical strategy, which exploits variation in the selectivity of admission with funding to different programs on the same ROL. The within-ROL design compares the rate of obvious mistakes made by a specific applicant with respect to programs with different historical admission selectivity. Additionally, by focusing on pre-reform ROLs, we are analyzing behavior in a "steady state." This design corroborates that admission selectivity has a positive causal effect on obvious mistakes.

According to both designs the effect is heterogeneous, and is stronger among applicants of low academic achievement and applicants of high socioeconomic status. Still, even among the very poor, the effect is substantial and equals about one-half of the effect on applicants of high socioeconomic status.

Obvious mistakes are inconsequential when the applicant does not meet the bar for admission with funding. If an applicant is (almost) certain that this will be the

possible interpretations (e.g., social preferences, self-image concerns). We elaborate on these interpretations in Section 7.

[4]In Section 2.2, we discuss other changes that occurred in 2012, and at the end of Section 5.1, we provide evidence that these changes do not drive the results of the first empirical strategy.

[5]For example, applicants might overreact to the increase in selectivity as a result of being surprised by the unusual environment (Bordalo et al., 2017).

case, she finds obvious mistakes (nearly) costless. Our empirical findings support the explanation that applicants make dominated choices that they expect to be (approximately) costless. The large fraction of costly mistakes suggests that applicants hold erroneous beliefs, potentially due to pessimism or overprecision.

Having shown that obvious mistakes are often costly for applicants who make them, we next ask what consequences these mistakes have for others. Applicants who make costly obvious mistakes free up funded seats, to the benefit of other applicants who are marginally admitted with financial aid. These marginal applicants come from a lower SES background relative to those who make costly obvious mistakes. Thus, 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. Intuitively, the reason why obvious mistakes increase the number of admitted students is that programs are typically constrained by the availability of funding and not necessarily by capacity (Hassidim et al., 2018a).

Our findings have important implications for the study and design of centralized school assignment systems. First, they suggest that certain features of the choice architecture, which the theory of market design deems irrelevant, are consequential in practice. For example, our findings indicate that providing applicants with information about their admission chances affects their allocation, even in environments where truthful reporting is a dominant strategy. For instance, giving publicity to affirmative action programs could amplify their effectiveness by reducing the frequency of mistakes among disadvantaged applicants.

Second, reported preferences are often used to inform policymakers about the relative desirability of different alternatives (schools, hospital internships, etc.). According to the traditional approach, preferences that are reported to strategically simple mechanisms can 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 (Fack et al., 2017). Artemov et al. (2017) propose an alternative approach to estimating preferences when applicants know their admission chances when they submit their ROL.

Finally, understanding what causes mistakes in strategically simple environments could inform researchers about the mechanisms underlying this behavior. This, in turn, could lead to new and more predictive classifications of allocation mechanisms according to their "simplicity" (Cason et al., 2006; Li, 2017a,b; Zhang and Levin, 2017).

This paper is related to studies evaluating dominated-strategy play in strategically simple environments. Two recurring themes in this literature are the negative correlation of this behavior with cognitive ability and its positive correlation with the expectation of fiercer competition (e.g., Rees-Jones and Skowronek, 2018). In practice, applicants' cognitive ability and desirability are positively correlated in the field, making it difficult to disentangle the two components (Hassidim et al., 2018b; Rees-Jones, 2018; Artemov et al., 2017). In the laboratory, Basteck and Mantovani (2016) and Rees-Jones and Skowronek (2018) 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 (TTC; Abdulkadiroğlu and Sönmez, 2003). Hassidim et al. (2018b) and Rees-Jones and Skowronek (2018) document a strong causal relationship between expected admission selectivity and dominated choices in the laboratory. Our study is the first to establish the causal relationship between admission selectivity and dominated choices in the field, ruling out cognitive limitations as a sole determinant of dominated choices in high-stakes environments. We also corroborate the correlation between cognitive ability and dominated choices.

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.[6] 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.[7] In the field, Gross et al. (2015), Chen and Pereyra (2017), and Rees-Jones (2018) document dominated-strategy play in strategically simple high-stakes environments, using survey evidence. Relying exclusively on observational data, Hassidim et al. (2018b) detect obvious mistakes in the Israeli Psychology Master's Match (IPMM; Hassidim et al., 2017b), and Artemov et al. (2017) and Arslan (2018) do likewise in centralized college admissions markets in Australia and Turkey. Our paper complements these studies by documenting dominated-strategy play in a large, well-established market, using exclusively observational data. Unlike these papers, however, our lower bound on the cost of mistakes is substantial.

More broadly, dominated choices with comparable costs have been documented in

[6]Budish and Kessler (2015) investigate a related question of students' ability to express their preferences in the more complex course-scheduling environment (Budish et al., 2016).

[7]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).

other high-stakes environments, such as health insurance (Handel and Kolstad, 2015; Bhargava et al., 2017) and retirement savings (Choi et al., 2011). The environment that we study is designed to be simple: there are no complex trade-offs or menus, hassle cost is minimal, and the dynamic aspect is limited. Still, we identify dominated choices and show that they are costly.

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.[8] 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., Abdulkadiroğlu et al., 2006; De Haan et al., 2018; He, 2017; Kapor et al., 2018). We contribute to the literature by studying a long-standing centralized market that is designed to be strategically simple, where information is accessible and abundant, and by focusing on mistakes that are unlikely to be caused by informational 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.

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 lay out our two empirical strategies, 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

[8]Examples include 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), and Andrabi et al. (2017).

Section 2.2, we describe the 2012–13 reform, 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 clearinghouse since 1985. The centralized clearinghouse is managed by a nonprofit governmental organization. Over the years, several changes have been introduced into the mechanism in place. The most recent change occurred in 2008 when a variant of the student-proposing DA was adopted.[9] The mechanism that had been in use previously was based on a similar 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 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

[9] 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.

offered. If admitted to an unfunded position, the student will not receive a tuition waiver, in spite of her eligibility. On average, 64% of admitted students received a tuition waiver in the years 2009–2014.

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 annual minimum wage in 2013 was approximately $4,000. 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-Order Lists. Students are allowed to rank any number of *contracts*, i.e., program and funding level combinations, that they wish. For example, they may submit an ROL that includes four contracts with three programs as in Table 1. Submitting an ROL that includes up to 3 programs (which may correspond to up to 6 contracts) only requires paying a fixed application fee of about 30 dollars. This is the case for the ROL in Table 1. 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, together with the standard assumption that applicants' preferences depend only on their own allocation, implies that 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 four contracts with three programs. This ROL contains two obvious mistakes. First, an unfunded BA in biology at Eötvös Lóránd University is ranked higher than a funded contract in the same program (obvious flipping). Second, the applicant ranked only an unfunded BA in applied economics at Corvinus University of Budapest, even though a funded contract was offered (obvious dropping).

The fact that we label the behavior we document as "mistakes" or dominated choices is not innocuous. It relies crucially on the assumption that agents' utility de-

Table 1: A rank-order 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
4.	Eötvös Lóránd University	BA in Mathematics	Funded

Notes: The table presents a rank-order list that includes four contracts with three programs.

pends only on the realized assignment, and more specifically only on the agents' own assignment. While this assumption is standard in the market design literature (e.g., Pathak and Sönmez, 2013), there are other possible interpretations (e.g., social preferences, self-image concerns). We elaborate on these interpretations in Section 7. Our view that funded positions unambiguously dominate unfunded positions is shared by the popular media. For example, on the day the 2017 match results were made public a major media outlet published a story with the man-bites-dog title: "*The priority-score cutoff for unfunded medicine exceeds the state-funded cutoff.*"[10]

Timeline. The application process proceeds 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 media coverage. This feature simplifies 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. 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. The clearinghouse website also

[10]Source: index.hu; https://goo.gl/zfxFFw, accessed: 20/09/2017.

provides decision support in the form of an application fee calculator.

The application interface is particularly informative about the availability of financial aid (screenshots are available in Appendix A). Applicants are required to choose contracts from a dropdown menu in which the funded and the unfunded contracts in the same program appear consecutively. Similarly, in the traditional paper-based system, applicants must copy a code corresponding to each contract. These codes appear in an information brochure, which lists funded and unfunded contracts in the same program consecutively.

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.[11] 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.[12] 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, in-

[11] See https://goo.gl/bozDkK, accessed: 01/02/2017.

[12] 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.

ternational business administration, and international relations). In still other majors, the menu was not changed, but the capacities of state-sponsored positions 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*.

Table 2: Availability of funded positions

	2009	2010	2011	2012	2012 (partial funding)
A. *Fields with little or no funding cut*					
Agriculture	1,900	1,950	1,850	2,160	150
Art	700	700	570	900	0
Art mediation	300	300	390	350	0
Computer science	4,700	4,700	6,400	4,550	1,500
Engineering	9,800	9,850	9,850	10,760	2,350
Humanities	4,800	4,450	4,100	2,700	0
Medicine	3,400	3,600	4,600	5,000	100
Public administration	-	-	-	1,017	0
Natural sciences	4,200	4,200	5,200	4,000	1,500
Pedagogy	1,900	1,800	2,000	1,600	0
Sport	600	600	500	600	0
B. *Fields with severe funding cut*					
Business/economics	5,900	6,250	4,900	250	0
Legal studies	1,500	1,350	1,300	300	0
Social sciences	3,000	2,750	2,100	1,000	0

Notes: The table describes the availability of funded positions between 2009 and 2012 by field and year. Starting in 2013, the government did not release the corresponding numbers. The rightmost column provides details on partial funding, which was offered in 2012 only. Partial funding covered 50 percent of the tuition fee. Partial funding was 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 available tuition waivers in computer science and natural sciences increased in 2011, the previous capacity was not binding.

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 previously been offered. However, state-sponsored capacities remained scarce.[13] The "reversal" of the 2012 reform did not meaningfully increase the number of state-sponsored positions

[13]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.

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, starting in 2013, the funding cut was expanded to include an additional major in the field of humanities (adult education).

Since our first empirical strategy exploits the 2012–13 reform, we must also mention other changes that occurred around the same time. As part of the reform, the government legislated a decree that introduced the *study contract*, which obliges college students who benefit from state sponsorship to work in Hungary for the number of years they spent in college within 20 years of graduation, or else repay the country with interest (a 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 is highly unlikely that this contract will be enforced (in twenty years). Its legal status is unclear, as it may violate the freedom of movement of workers in the EU,[14] and political pressure caused the government to significantly alleviate the terms already in 2013. 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.

There are some circumstances under which we are even more certain that the natural ranking does not change: first, when the applicant comes from a poor family, and second, when the applicant applies for a major providing training that is highly specific to Hungary (such as legal studies). By contrast, if the natural ranking has changed in any major, it has likely changed in medical studies, as the graduates of this field are notorious for their tendency to emigrate (see e.g., Galgóczi et al., 2013, pp. 238–239).

The government also expanded the availability of its subsidized student loan program for paying students and introduced partially funded positions. Partially funded positions were offered only in 2012. Partial funding covered half of the tuition and was also subject to the study contract. It was not possible to rank partially funded positions, but they were awarded based on merit to individuals who were assigned

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

an unfunded position (thus, the government implicitly assumed that a funded option would be preferred by the applicants, which is consistent with our interpretation).

Another change that occurred in 2012 is that the formulas for priority scores were slightly changed and rescaled. For ease of comparison, in Section 5.2, we compute within-year percentile ranks of the priority-score cutoffs. Finally, the number of programs one could rank was capped at 5 (10 contracts). We do not think this 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.

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 main data source is an administrative dataset that contains information about the bachelor's degree admissions process between 2009 and 2014 in Hungary.[15] In particular, we observe each applicant's complete ROL and program-specific priority scores,[16] 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 information includes grades in various subjects in the final two years of high school (11th and 12th grades), performance in the matriculation exams, and the number of points the applicant received for claiming a disadvantaged background.[17] This dataset also includes appli-

[15]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).

[16]Our data report up to 7 contracts from each ROL: the first 6 contracts and the contract to which the applicant is assigned. The dataset also reports the number of contracts in each ROL. We observe the complete ROL for 92.9 percent of applicants and 94.3 percent of all ranked contracts.

[17]To be eligible for disadvantaged status, an applicant must have a per capita household income that is lower than 130 percent of the minimum pension (approximately $1,500 a year). Since 2014, in addition to the income criterion, the student has 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.

cants' assignment.

Our analysis uses four additional data sources that we merged based on demographic information. The first data source is the T-STAR dataset of the Hungarian Central Statistics Office. We use it to obtain settlement-level annual information on collected income taxes.[18] In particular, we calculate the per capita gross annual income for all 3,164 settlements for each year between 2009 and 2014. The second data source is the microregional-level annual unemployment rates published by the National Employment Service in 2008, one year before the start of our sample period.[19] The territorial breakdown consists of 174 units. The third data source is a dataset including the 2013 and 2014 tuition costs of each program, which we created by scraping the centralized admission system website.

The fourth 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 large sample of students, and since 2008 it has covered 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 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 resembles the economic, social, and cultural status (ESCS) indicator of the OECD PISA survey. It combines three subindices: an index of parental education, an index of home possessions (number of bedrooms, mobile phones, cars, computers, books, etc.), and an index of parents' labor-market status.

Disadvantaged status is granted for one year. Students with disadvantaged status receive regular cash transfers and are eligible for free textbooks during high school.

[18] For further information visit https://goo.gl/EqSgaU, accessed: 05/03/2018.

[19] Source: https://goo.gl/9xiVPz, accessed: 16/11/2016. For more information on the territorial units see https://goo.gl/FffwkT, accessed: 16/11/2016.

3.2 Sample Definition

An ROL is an ordered list of *contracts*, i.e., program-funding pairs. An applicant makes an obvious mistake if she is eligible for funding, yet 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 contrast, when we analyze the effect of admission selectivity on obvious mistakes (Section 5), we treat each *application* – a program in an ROL, with up to two contracts – as a single observation.

Our full dataset consists of 565,635 ROLs submitted between 2009 and 2014. 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*. Altogether, 525,275 ROLs meet the eligibility restrictions.

We often restrict the eligible 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 subset of the eligible sample, which includes 268,981 ROLs, 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).[20] 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 *relevant* ROLs. These are ROLs that include at least one unfunded contract. Relevant ROLs are the only lists in which our methodology can potentially detect mistakes.

[20]The ROL of an applicant who did not declare ineligibility even though she exhausted her 12 funded semesters and did not rank any funded 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.

The administrative datasets we use 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 a large sample of 10th-grade students from 2006, and on the entire population since 2008. Therefore, for each year, we match only high-school senior applicants to the NABC. Whenever the match is not unique, we calculate the average test scores of matched individuals. We were able to match 179,039 applicants out of 268,981 (67 percent between 2009 and 2014, and 80 percent between 2011 and 2014). The match is unique for about 149,148 observations (55 percent). Appendix B contains further details about the matching procedure.

Some readers may have concerns that the matching introduces bias due to selection. It is important to highlight that none of our results relies on the use of the NABC dataset. For each result we present, we provide versions that rely only on the main administrative dataset and the other data sources.

3.3 Summary Statistics

Table 3 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, 20 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. The average ROL length was 3.8 contracts, which corresponds to 2.9 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). High-school senior applicants' GPAs were 0.24 of a standard deviation higher than eleigible applicants' GPAs.[21]

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 numer-

[21] 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-grade GPAs are missing from 18 percent of both samples. Indeed, the correlation between missing GPA and matriculation exam scores in our data is negative and strong.

acy 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 exhibited a 0.59 (0.64) standard deviation higher 10th-grade numeracy (literacy) skill compared to 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. Appendix Table C1 shows that this pattern is stable over years.

Table 4 presents the distribution of ROLs by the type of contracts they include. In the eligible sample, 59.3 percent of ROLs include only funded contracts, 7.4 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 (65.5 percent) and only unfunded contracts rarely (2.3 percent). Thus, 40.7 percent of the eligible sample, and 34.5 percent of the high-school senior applicant sample, are relevant. Among students who listed both funded and unfunded contracts in their ROL, 53.4 percent ranked *all* funded contracts above *all* unfunded ones in the eligible sample. The corresponding figure for the high-school senior applicant sample is 46.6 percent. Taken together, these figures suggest that funding plays an important role in students' choices between alternatives.

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. In spite of a drop in the number of applicants in 2012, the distribution of the fields remained relatively stable over time.[22] Applications to fields of study that suffered a severe cut in funding in 2012 (business and economics, legal studies, and social sciences) comprised 21 percent of all applications in the eligible applicant sample and 22 percent of all applications in the high-school senior applicant sample in 2013–14.

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 private cost. In Section 4.2, we examine the correlates of obvious mistakes and find that mistakes are more prevalent among high socioeconomic status and low academic ability applicants.

[22]Changes in the pattern of application following the reform may compromise out difference-in-differences analysis. At the end of Section 5.1, we show that our results cannot be driven by such a change.

Table 3: Individual-level summary statistics

	Non-high-school senior applicants (1)	High-school senior applicants (2)	Eligible applicants (3)
Female	0.54	0.57	0.55
	(0.50)	(0.50)	(0.50)
Age at application	24.91	19.05	21.91
	(6.62)	(0.68)	(5.50)
High school			
- secondary grammar school	0.56	0.70	0.63
	(0.50)	(0.46)	(0.48)
- vocational school	0.39	0.26	0.32
	(0.49)	(0.44)	(0.47)
Residence			
- capital	0.21	0.16	0.19
	(0.41)	(0.37)	(0.39)
- county capital	0.20	0.20	0.20
	(0.40)	(0.40)	(0.40)
- town	0.31	0.33	0.32
	(0.46)	(0.47)	(0.47)
- village	0.28	0.30	0.29
	(0.45)	(0.46)	(0.45)
11th-grade GPA (standardized)	−0.26	0.24	0.00
	(0.98)	(0.96)	(1.00)
11th-grade GPA - missing	0.18	0.18	0.18
	(0.38)	(0.38)	(0.38)
Numeracy skills	–	0.59	–
	–	(0.85)	–
Numeracy skills - missing	–	0.33	–
	–	(0.47)	–
Literacy skills	–	0.64	–
	–	(0.74)	–
Literacy skills - missing	–	0.33	–
	–	(0.47)	–
Disadvantaged status	0.04	0.10	0.07
	(0.19)	(0.29)	(0.25)
NABC-based SES index	–	0.49	–
	–	(0.84)	–
NABC-based SES index - missing	–	0.39	–
	–	(0.49)	–
Unemployment rate in 2008 (%)	7.49	7.86	7.68
	(4.31)	(4.51)	(4.42)
Unemployment rate in 2008 - missing	0.02	0.02	0.02
	(0.13)	(0.16)	(0.15)
Gross annual per capita income (1000 USD)	6.52	6.37	6.44
	(1.59)	(1.56)	(1.57)
Gross annual per capita income - missing	0.02	0.02	0.02
	(0.13)	(0.15)	(0.14)
# of contracts on the ROL	3.22	4.34	3.80
	(1.65)	(2.20)	(2.03)
# of contracts on the ROL (data)	2.98	3.81	3.41
	(1.38)	(1.48)	(1.49)
# of programs on the ROL (data)	2.51	3.25	2.89
	(1.08)	(1.17)	(1.19)
Applicants	256,294	268,981	525,275

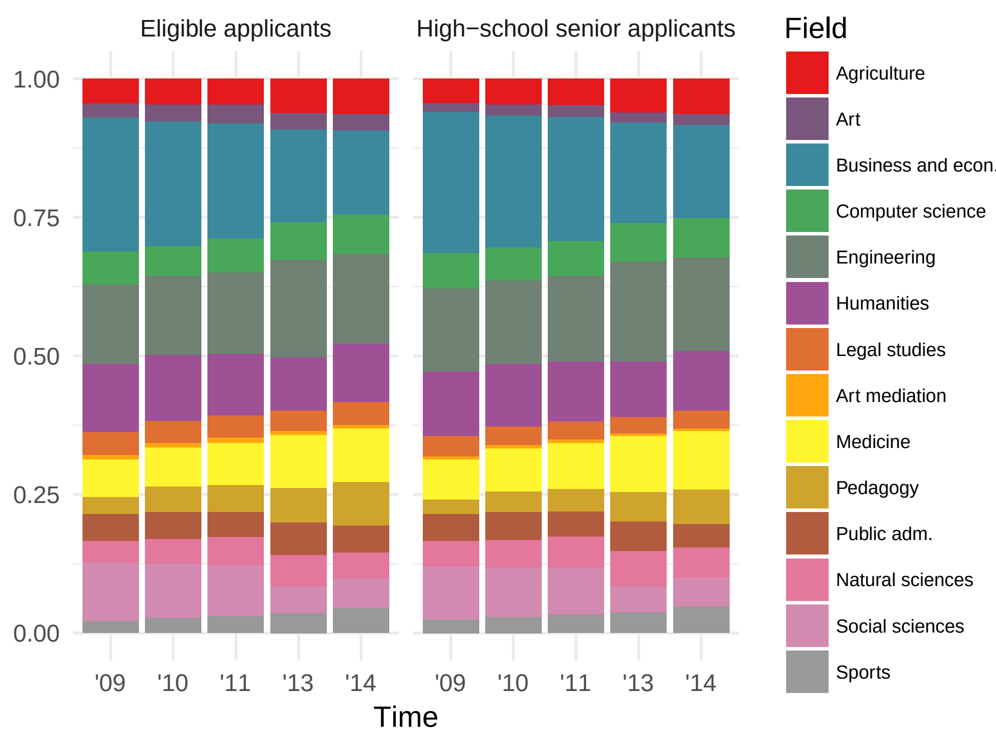
Notes: The table reports mean values of student characteristics, with standard deviations in parentheses. Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average grades in mathematics and Hungarian grammar and literature. 11th-grade GPA is standardized among eligible applicants. Some applicants have no incentive to report their GPA to the clearinghouse (see Footnote 21). Since we match the NABC to high-school senior applicants, numeracy skill, literacy skill, and NABC-based SES index are missing for non-high-school senior applicants. 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 16).

Table 4: Distribution of ROLs by funding type

ROLs	Eligible applicants		High-school senior applicants	
	(1)	(2)	(3)	(4)
Only funded contracts	311,600	59.3	176,204	65.5
Funded and unfunded contracts	175,056	33.3	86,484	32.2
Only unfunded contracts	38,619	7.4	6,293	2.3

Notes: The table presents the distribution of ROLs by funding type. Columns (1) and (3) display frequencies and columns (2) and (4) show the distribution.

Figure 1: Distribution of applications by field of study



Notes: The figure depicts the distribution of applications by field of study. 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 that year (see Section 2.2).

4.1 The Prevalence and Costs of Obvious Mistakes

Table 5 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 60,000 applicants, corresponding to 11.1 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 ranges from 3.1 percent in 2009 to 10.8 percent in 2013. Overall, 5.8 percent of high-school seniors made an obvious mistake in the same period. The rate of obvious dropping in the eligible applicant sample was 10.3 percent and the rate of obvious flipping was 1.0 percent. Among high-school seniors the rate of obvious dropping was 5.0 percent and the rate of obvious flipping was 1.1 percent.

The intensity of obvious mistakes varies between ROLs. Among eligible applicants 7.8 percent made an obvious mistake with respect to all the programs they ranked. The corresponding figure is 2.7 percent among high-school senior applicants.

Obvious mistakes can be detected only in ROLs that rank at least one unfunded contract. In the eligible applicant sample the share of such ROLs is 41 percent (see Table 4). Table 5 should be interpreted in this context. For example, 11.1 percent of ROLs with obvious mistakes in the eligible applicant sample represent 27.1 percent (= 11.1%/0.41) 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, who understand the admissions process, only use dominated strategies if they assign probability zero to the event that a dominating strategy does strictly better. Table 5 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 5 demonstrates that obvious mistakes may have hurt up to 18.5 percent of the eligible applicants and up to 9.4 percent of the high-school senior applicants who made obvious mistakes (column (4)). At least 12.3 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 4.3 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 rankings of the majority of students, suggests that the upper bound may be more indicative of the true fraction. The average monetary loss associated with ex post costly mistakes is approximately 3,000–3,500 dollars. The average cost associated with obvious mistakes is between 347 and 735 dollars for eligible applicants, and between 102 and 352 dollars for high-school senior applicants in 2013–14.

We 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 would 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 mistakes, and any subsequent effects (such as "rejection chains").

A potential explanation for the prevalence of obvious mistakes is that applicants do not understand the application fee structure. Specifically, applicants may not understand that the application fee is charged per program, not per contract. To assess this theory, we concentrate on the subsample of applicants who ranked four or more contracts, 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 that the share of eligible applicants with obvious mistakes is 7.9 percent. The corresponding number is 6.8 percent among high-school senior applicants.

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. We focus on the sample of high-school seniors, for whom we can use the richer NABC data. Appendix D.1 presents similar results in the complete high-school senior applicant sample (Table D1) and the entire eligible applicant sample (Table D2). 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 6 summarizes our findings. Applicants of a higher SES according to the NABC-based index make more obvious mistakes on average (column (1)), and this correlation is even stronger once we control for academic achievement (columns (2) and (3)). In columns (4)–(6) we corroborate the correlation of obvious mistakes with proxies for SES (microregional-level unemployment rate, settlement-level gross an-

Table 5: 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)	6.4	(6,310)	13.2	(1,131)	20.3	(1,733)
2010	9.4	(9,818)	6.8	(7,118)	10.6	(1,044)	15.8	(1,556)
2011	12.2	(12,615)	8.5	(8,787)	9.4	(1,183)	14.2	(1,797)
2012	10.4	(7,452)	7.4	(5,330)	12.6	(937)	19.9	(1,482)
2013	14.5	(10,208)	10.1	(7,111)	16.6	(1,698)	24.2	(2,467)
2014	12.3	(9,530)	8.3	(6,426)	12.4	(1,184)	18.2	(1,731)
Total	11.1	(58,178)	7.8	(41,082)	12.3	(7,177)	18.5	(10,766)
B. *High-school senior applicants*								
2009	3.1	(1,566)	1.1	(541)	4.0	(62)	7.6	(119)
2010	3.2	(1,596)	1.1	(556)	1.4	(23)	4.1	(65)
2011	4.6	(2,268)	1.6	(794)	1.4	(31)	4.9	(112)
2012	6.3	(2,494)	3.5	(1,397)	4.0	(101)	10.5	(261)
2013	10.8	(4,201)	6.1	(2,372)	8.5	(355)	15.7	(659)
2014	8.7	(3,528)	4.2	(1,686)	2.7	(97)	7.5	(263)
Total	5.8	(15,653)	2.7	(7,346)	4.3	(669)	9.4	(1,479)

Notes: Column (1) shows the share (number) of ROLs that exhibit obvious mistakes over time. Column (2) presents the share (number) of ROLs with obvious mistakes with respect to all of the programs they include. 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 of the funded version. Column (4) shows the share (number) of ROLs with obvious mistakes, where the applicant met the priority-score cutoff of a funded contract with respect to which he made a mistake.

nual per capita income, and indicator for claiming priority points for disadvantaged background).

Table 6: Demographics, socioeconomic status, academic achievement and obvious mistakes

Dependent variable	Obvious mistakes					
	(1)	(2)	(3)	(4)	(5)	(6)
NABC-based SES index	0.013***	0.018***	0.018***			
	(0.001)	(0.001)	(0.001)			
Unemployment rate in 2008 (%)				−0.002***		
				(0.000)		
Gross annual per capita income (1000 USD)					0.011***	
					(0.001)	
Disadvantaged status						−0.033***
						(0.001)
Numeracy skills		−0.023***	−0.013***	−0.011***	−0.011***	−0.010***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
11th-grade GPA (standardized)			−0.025***	−0.024***	−0.024***	−0.025***
			(0.001)	(0.001)	(0.001)	(0.001)
Female	0.014***	0.004***	0.014***	0.013***	0.013***	0.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Vocational school	0.015***	0.006***	−0.001	−0.006***	−0.005***	−0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Other school	0.022***	0.018**	0.013*	0.011	0.013*	0.013*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
County capital	−0.028***	−0.029***	−0.027***	−0.026***	−0.008***	−0.029***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Town	−0.034***	−0.038***	−0.035***	−0.031***	−0.012***	−0.039***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.022	.028	.036	.033	.034	.033

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. 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 (N = 162,978). The share of obvious mistakes is 5.7% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 15.3% of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *: p<0.1.

Applicants of low 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 for SES and obvious mistakes. In Appendix Table D3 we restrict attention to ROLs that include at least one unfunded contract, and repeat the same analysis. 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 6 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 2.4 percentage points (42.1 percent) decline in the probability of making obvious mistakes (column (2)). Once we control for 11th-grade GPA, the estimated coefficient drops to 1.1–1.4 percentage points (19.3–24.6 percent, columns (3)–(6)).

GPA is related to applicants' priority directly, since GPA enters the priority-score formula and can account for up to 20 percent of the priority score. We find that applicants with a higher GPA make fewer obvious mistakes, even controlling for numeracy skill. A one standard deviation increase in the 11th-grade GPA is associated with a 2.4–2.5 percentage points (42.1–43.9 percent) decline in the probability of making obvious mistakes (columns (3)–(6)).

We also find that female applicants were 1 percentage point (23 percent) more likely to make an obvious mistake.[23] 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 D4 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.

5 The Effect of Selectivity on 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. This section presents our main result, namely, that admission selectivity has a positive causal ef-

[23] Previous studies documented gender differences in confidence and competitiveness, and found that these differences account for educational choices (Niederle and Vesterlund, 2007; Buser et al., 2014).

fect on making obvious mistakes. We establish this result using two complementary empirical strategies. First, we use a difference-in-differences research design, which compares the rates of obvious mistakes in applications to programs that were affected by the severe reduction in funding and these rates in applications to programs that experienced little or no cut in funding (Section 5.1). Second, we use a within-ROL design, which exploits variation in the degree of selectivity of different programs in the same ROL (Section 5.2).

5.1 Evidence from the 2012–13 Reform

Our first strategy exploits the 2012–13 reform that limited the availability of funded positions in some programs, and thereby increased the selectivity of admission to funded positions in these programs.

Empirical strategy

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

$$Y_{its} = \alpha + \beta_{2013} \cdot T_{ts} \cdot (t = 2013) + \beta_{2014} \cdot T_{ts} \cdot (t = 2014) + X_{it} \cdot \Gamma + \eta_s + \nu_t + \varepsilon_{its}.$$

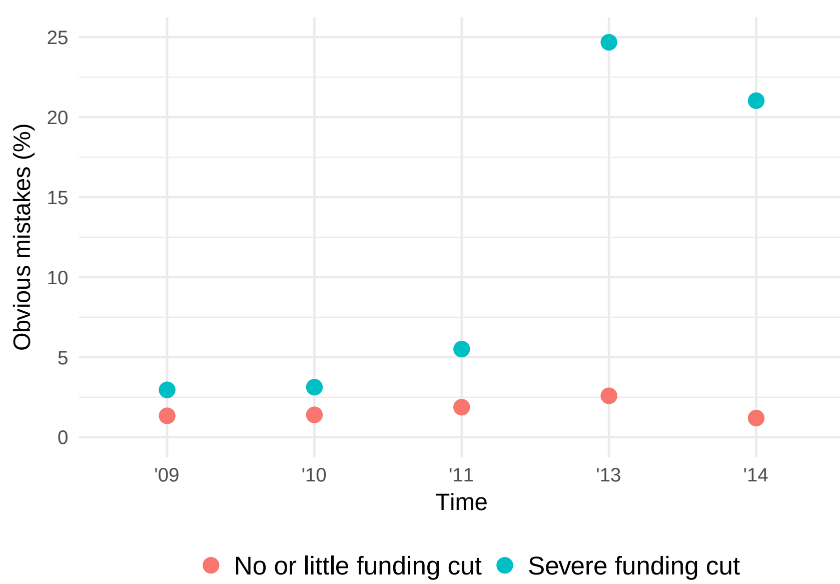
The variable Y_{its} is an indicator of obvious mistakes in applicant i's ranking of program s in year t. The variable T_{ts} is an indicator that equals one if t is equal to 2013 or 2014 and s is a program that was affected by the severe funding reduction of the 2012–13 reform, and zero otherwise. The model 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}). The year fixed effects control for changes that affected all applications in a given year. Our parameters of interest are β_{2013} and β_{2014}. These parameters measure 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_{2013} and β_{2014} relies on two key assumptions. First, in the absence of the reform, the prevalence of obvious mistakes in different programs would have evolved in tandem (parallel trends). Second, the composition of the students applying to programs with a severe funding cut and students applying to programs with little or no funding cut remained stable over time. We evaluate the plausibility of these assumptions and the robustness of our estimates to their violation at the end of this section, where we also provide evidence that supports our economic interpretation that the estimates are driven by selectivity and not by other contemporaneous changes.

Graphical illustration

Figure 2 provides a graphical illustration of the results of our difference-in-differences empirical strategy. The figure shows that the rate of obvious mistakes in the programs that experienced little or no funding cut remained at pre-reform levels. This suggests that other contemporaneous changes (e.g., the introduction of the study contract) had little effect on making obvious mistakes in these programs. By contrast, obvious mistakes increased sharply from 5.5 percent to 24.7 percent in the programs that were affected by the severe funding reduction of the 2012–13 reform. The effect of the reform persisted in 2014: the rate of obvious mistakes was 21 percent in the affected programs.

Figure 2: The effect of admission selectivity on obvious mistakes: 2012–13 reform



Notes: The figure presents the share of high-school senior applications with obvious mistakes over time, split by the severity of the funding cut in the reform.

Results

Table 7 presents our difference-in-differences estimates of the effect of admission selectivity on obvious mistakes. Our baseline specification (column (1)) indicates that the reform increased obvious mistakes by 19.3 percentage points among treated programs from a baseline of 6.3 percent in 2013.[24] The estimated effect in 2014 is similar, 17.9 percentage points. Columns (2)–(4) show that controlling for demographics, academic achievement, and high-school fixed effects barely changes the estimates and their precision. Appendix Table D5 repeats this analysis using only columns (1) and (3) of Table 7 using only the highest ranked application in each ROL, and finds similar results. Appendix Table D6 shows that the effect holds for both obvious flipping and obvious dropping, but the magnitude of the effect on 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, and the average change was almost 9 percentiles in 2013. The reduction in the number of funded positions in the directly affected programs made the system as a whole more selective through general equilibrium effects. If students who applied to programs 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.

In Table 8 we examine whether the effect of admission selectivity on obvious mistakes is heterogeneous across various subgroups. The corresponding regressions include interactions of treatment and subgroup dummies, and controls for demographics, academic achievement, and high-school fixed effects (as in column (3) of Table 7). We find that the effect of admission selectivity on obvious mistakes is 2.7 (5.6) percentage points lower for female applicants in 2013 (2014). The causal effect of admission selectivity is lower for low-SES 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 3). The estimated effect for applicants with an 11th-grade GPA of 3 is about three times as large as this estimate for applicants with a perfect GPA (5). These results suggest that applications for which mistakes cause a higher expected utility loss

[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 is 0.9 percentage points.

Table 7: The effect of admission selectivity on obvious mistakes: 2012–13 reform

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
Severe funding cut × 2013	0.193***	0.186***	0.185***	0.185***
	(0.004)	(0.004)	(0.004)	(0.004)
Severe funding cut × 2014	0.179***	0.174***	0.173***	0.173***
	(0.004)	(0.004)	(0.004)	(0.004)
Program FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Demographics & GPA	*No*	*Yes*	*Yes*	*Yes*
School FE	*No*	*No*	*Yes*	*Yes*
NABC controls	*No*	*No*	*No*	*Yes*
R-squared	0.114	0.126	0.136	0.137
# ROLs	229,009	229,009	229,009	229,009
# Obs.	729,650	729,650	729,650	729,650

Notes: The table presents the effect of admission selectivity on obvious mistakes. Robust standard errors clustered on the applicant level are in parentheses. The number of observations is 729,650, which corresponds to 229,009 ROLs among high-school senior applicants. The mean outcome in the sample is 3.6 percent. 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 and the NABC-based SES index. Missing control variables are indicated by a separate dummy variable.
***: p< 0.01, **: p<0.05, *: p<0.1.

are less responsive to increases in admission selectivity.

Recall that there are some circumstances under which we are more certain that the natural ranking is unaffected by the introduction of the study contract: namely, when the applicant is of low socioeconomic status or when she applies to a major which is highly specific to Hungary. Even among the very poor, we find that the effect is substantial and equals about one-half of the effect on applicants of high socioeconomic status. Furthermore, we find that the effect on applications to legal studies is almost identical to the main effect. By contrast, the estimated effect on medical studies is negative, but minuscule (-1.8 percentage points in 2013 and -0.6 percentage points in 2014), in spite of the medical doctors' tendency to emigrate (Galgóczi et al., 2013).

Table 8: Heterogeneity: The effect of admission selectivity on obvious mistakes: 2012–13 reform

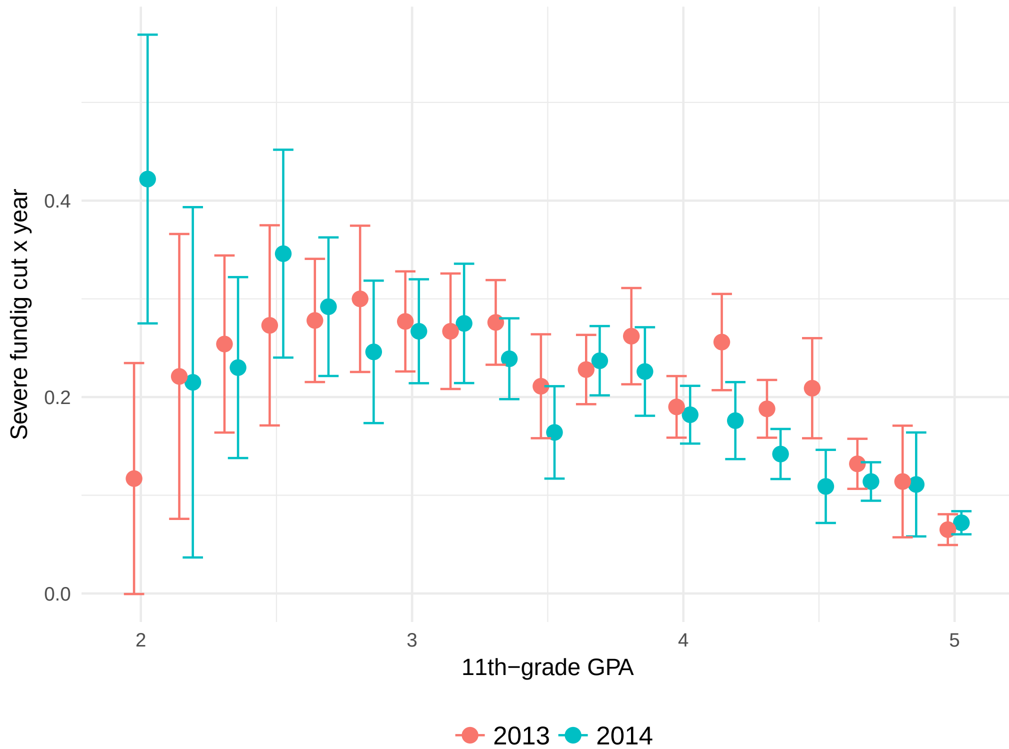
	A. *Gender*		B. *NABC numeracy skill*				
	Male	Female	Q1	Q2	Q3	Q4	Q5
Severe funding cut × 2013	0.202***	0.175***	0.227***	0.199***	0.195***	0.180***	0.114***
	(0.007)	(0.005)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Severe funding cut × 2014	0.209***	0.153***	0.212***	0.195***	0.173***	0.134***	0.120***
	(0.007)	(0.005)	(0.010)	(0.010)	(0.009)	(0.008)	(0.009)
Counterfactual mean (2013)	0.071	0.057	0.073	0.063	0.058	0.052	0.047
Counterfactual mean (2014)	0.049	0.032	0.045	0.039	0.032	0.027	0.021
R-squared	0.137				0.138		

	C. *Disadvantaged*		D. *NABC-based SES*				
	No	Yes	Q1	Q2	Q3	Q4	Q5
Severe funding cut × 2013	0.193***	0.087***	0.150***	0.156***	0.170***	0.193***	0.209***
	(0.004)	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)
Severe funding cut × 2014	0.177***	0.093***	0.141***	0.155***	0.173***	0.177***	0.174***
	(0.004)	(0.011)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
Counterfactual mean (2013)	0.063	0.041	0.049	0.056	0.058	0.061	0.064
Counterfactual mean (2014)	0.024	0.006	0.025	0.029	0.030	0.035	0.038
R-squared	0.137				0.137		

	E. *Field of study*				
	Business/ economics	Legal studies	Social sciences	Adult education	Medicine (placebo)
Severe funding cut × 2013	0.194***	0.169***	0.146***	0.118***	−0.018***
	(0.005)	(0.010)	(0.010)	(0.023)	(0.002)
Severe funding cut × 2014	0.174***	0.189***	0.151***	0.086***	−0.006***
	(0.005)	(0.010)	(0.010)	(0.020)	(0.002)
Counterfactual mean (2013)	0.060	0.090	0.081	0.044	0.015
Counterfactual mean (2014)	0.044	0.074	0.066	0.028	−0.001
R-squared		0.136			

Notes: The table presents the effect of admission selectivity on obvious mistakes 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 parentheses. The number of observations is 729,650, which corresponds to 229,009 ROLs. The mean outcome in the sample is 3.6 percent. The counterfactual mean denotes the counterfactual mean outcome of the treated group in 2013/2014. 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. All specifications include high-school fixed effects (as in column (3) of Table 7).
***: p< 0.01, **: p<0.05, *: p<0.1.

Figure 3: The effect of admission selectivity on obvious mistakes by 11th-grade GPA: 2012–13 reform



Notes: The figure presents the effect of admission selectivity on obvious mistakes by 11th-grade GPA with 95% confidence intervals. Robust standard errors are clustered on the applicant level. We estimate all the coefficients in a single regression by interacting the treatment indicators with 11th-grade GPA. We include demographic controls including gender, disadvantaged status, age, type of residence, and high-school indicator (as in column (3) of Table 7). The estimated effect on applicants with a missing 11th-grade GPA is 0.194 (s.e.: 0.008) in 2013 and 0.203 (s.e.: 0.007) in 2014. The median 11th-grade GPA is 4, and the 10th percentile is 2.8 in the distribution of high-school senior applications.

Appendix E.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. In 2013, our estimates are dominated by this mechanical effect: we find that the 2012—13 reform had a negative causal impact on ex post costly obvious mistakes. In 2014, the estimated effect is close to zero.

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 estimate the effect of the "reforms" that did not occur in 2009 and in 2010. Columns (1) and (2) of Table 9 add 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 parallel 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. Appendix Table D7 presents the results. We find that this smaller reform increased obvious mistakes by 1.2–1.3 percentage points in the affected fields. In Appendix D.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' ROLs. Applicants' responses to the reform 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 and are considering applying only to funded programs. Such applicants never make obvious mistakes. 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 (since such ROLs are free of obvious mistakes by definition).

We first note that this threat is only quantitative but not qualitative. The worst-case bias implies that our estimates are twice as high as the actual effect. To see this, note that the number of applications to treated fields decreased from 40,684 to 26,341, and that the rate of mistakes increased from 5.5 to 24.7 percent between 2011 and 2013. Assuming that students' application decisions are monotonic, i.e., that applicants substitute away from high-risk applications, the most severe bias would occur if i) all applications that disappeared from the treated group had been free of obvious mistakes, and ii) the rate of mistakes in the control had still been 2.6 percent (and not 1.8 percent as in 2011). The estimated effect would have been $(24.7 \cdot (26,341/40,684) - 5.5) - (2.6 - 1.8) = 9.7$ percentage points, more than half of the effect we estimated, and about twice the baseline rate of obvious mistakes.

Next, we address the threat to our identification strategy in several other ways. First, in columns (2)–(4) of Table 7 we add applicant-level controls. Second, we restrict the sample to those high-school senior applicants who listed programs both in the fields that experienced a severe funding cut and in the fields that were unaffected (columns (3) and (4) of Table 9). This restriction assures that the composition of applicants in the treated and untreated fields is the same (balanced subsample).[25] 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.

Third, we look at applicants who listed at least one unfunded contract in their ROL. By listing at least one unfunded contract, these applicants indicate 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 (5) and (6) of Table 9), indicating that switching behavior does not drive our results.[27]

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

[26] Another possibility is that applicants added new programs to their ROL. However, the 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, it would not yield positive estimates. If anything, it would bias the estimates downward.

Table 9: Robustness: The effect of admission selectivity on obvious mistakes: 2012–13 reform

Dependent variable	Obvious mistakes					
	(1)	(2)	(3)	(4)	(5)	(6)
Subsample/specification	Placebo		Balanced subsample		Relevant applicants	
Severe funding cut × 2013	0.181***	0.174***	0.101***	0.099***	0.162***	0.153***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
Severe funding cut × 2014	0.168***	0.162***	0.123***	0.121***	0.171***	0.163***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Placebo (2009)	−0.017***	−0.015***				
	(0.002)	(0.002)				
Placebo (2010)	−0.018***	−0.017***				
	(0.002)	(0.002)				
Program FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Demographics & GPA	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
School FE	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
R-squared	0.114	0.136	0.067	0.117	0.102	0.144
# ROLs	229,009	229,009	54,521	54,521	73,993	73,993
# Obs.	729,650	729,650	203,176	203,176	222,891	222,891

Notes: The table presents DiD estimates of the effect of admission selectivity on obvious mistakes for the high-school senior applicant sample. Columns (1) and (2) add placebo indicators for 2009 and 2010, columns (3) and (4) restrict the sample to applicants applying to both treated and untreated programs (balanced subsample), columns (5) and (6) restrict the sample to the relevant ROLs. 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.

5.2 Evidence from Within-ROL Variation in Admission Selectivity

Our second empirical strategy exploits the fact that applicants list several programs in their ROL with distinct admission selectivity. We show that applicants are less likely to make an obvious mistake with respect to a "safety school" in their ROL than they are likely to make a mistake with respect to a "reach school."

Empirical strategy

To estimate the effect of admission selectivity on obvious mistakes, we specify the following model:

$$Y_{its} = \alpha + \beta \cdot \text{priority-score cutoff}_{t-1,s} + X_{ts} \cdot \Gamma + \eta_{it} + \varepsilon_{its}.$$

The variable Y_{its} is an indicator of obvious mistakes in applicant i's ranking of program s in year t. The variable priority-score $\text{cutoff}_{t-1,s}$ is our measure of admission selectivity. It denotes the within-year percentile rank of the funded contract of program s one year prior to the application ($t-1$). 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.[28] The model includes fixed effects for program characteristics (X_{ts}), such as type of degree (BA or BA–MA), time schedule (full time or part time), field of study, and program location. The model also includes ROL-fixed effects (η_{it}) and an error term (ε_{its}). Our parameter of interest is β, which measures the effect of admission selectivity (as measured by lagged funded priority-score cutoffs) on obvious mistakes. We estimate the model on the application level. We focus on the years 2009–2011, the years prior to the introduction of the study contract.

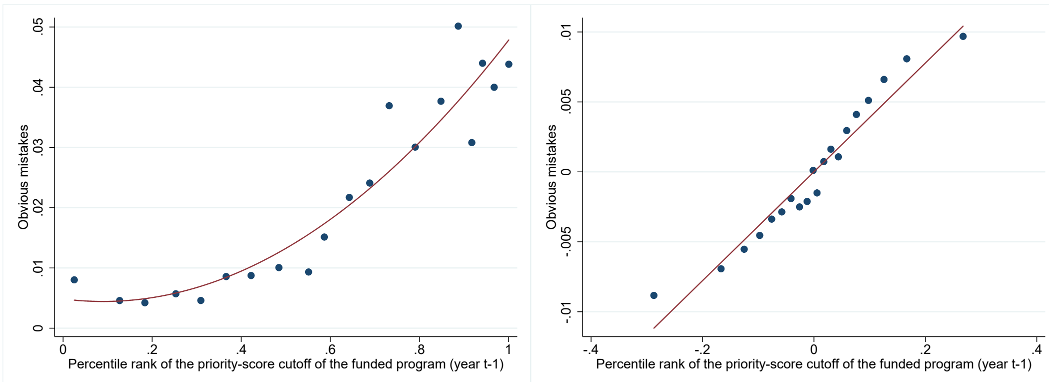
Graphical illustration

Figure 4 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,

[28] Since lagged priority-score cutoffs are not defined in the year a program is launched, we exclude ROLs that include such programs. We also exclude a handful of ROLs 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.

obvious mistakes are five times more likely to occur in applications to programs in the top quintile of the admission selectivity distribution than in applications to programs in the bottom quintile.

Figure 4: The effect of admission selectivity on obvious mistakes: A within-ROL comparison



(a) Unconditional (b) Within-ROL, full controls

Notes: The figure plots the selectivity of admission against the rate of obvious mistakes. The sample covers applications in the high-school senior applicant sample between 2009 and 2011. We exclude students who listed at least one program which does not have a lagged priority-score cutoff, leaving us with 110,398 ROLs, corresponding to 351,884 programs. 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 the bin-specific means conditional on year fixed effects. Panel (b) plots the bin-specific means conditional on ROL, field, degree, schedule, and location fixed effects (column (5) of Table 10). An increase in selectivity of 10 percentiles causes a 0.39 percentage points rise (s.e.: 0.02) in the probability of making an obvious mistake.

We cannot attribute a causal interpretation to the results depicted in Figure 4 (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 4 (a) understates the effect of admission selectivity on obvious mistakes. Second, programs differ along more dimensions than just admission selectivity (e.g., content, location, etc.), which confounds the positive relationship between admission selectivity and obvious mistakes. Our empirical strategy addresses sorting by adding ROL fixed effects and accounts for program-specific confounders by adding fixed effects for program characteristics.

Results

Table 10 presents our within-ROL estimates of the effect of admission selectivity on obvious mistakes. We identify this slope from ROLs that include programs with distinct admission selectivity. Our baseline specification (column (1)) indicates that a 10 percentile increase in (lagged) admission selectivity has a casual effect of 0.35 percentage points on obvious mistakes. Columns (2)–(5) show that controlling for program characteristics barely changes the estimates and their precision. Figure 4 (b) illustrates the results of our most preferred specification (column (5) of Table 10). Appendix Tables D9 and D10 show that the effect holds for both obvious flipping and obvious dropping, but the magnitude of the effect on obvious dropping is much larger, both in absolute and in relative terms.

Table 10: Admission selectivity and obvious mistakes: A within-ROL comparison

Dependent variable	Obvious mistakes				
	(1)	(2)	(3)	(4)	(5)
Priority-score cutoff	0.035***	0.034***	0.036***	0.035***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Field FE	*No*	*Yes*	*Yes*	*Yes*	*Yes*
Degree FE	*No*	*No*	*Yes*	*Yes*	*Yes*
Schedule FE	*No*	*No*	*No*	*Yes*	*Yes*
Location FE	*No*	*No*	*No*	*No*	*Yes*
Within R-squared	0.005	0.007	0.007	0.010	0.011
# ROLs	110,398	110,398	110,398	110,398	110,398
# Obs.	351,884	351,884	351,884	351,884	351,884

Notes: The table presents the effect of admission selectivity on obvious mistakes. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs among high-school senior applicants. The mean outcome in the sample is 2.2 percent. All specifications include ROL fixed effects.
***: p< 0.01, **: p<0.05, *: p<0.1.

In Table 11 we examine whether the effect of admission selectivity on obvious mistakes is heterogeneous across various subgroups. The corresponding regressions add interactions of the (lagged) priority-score cutoff and subgroup dummies to our most preferred specification (column (5) of Table 10). The patterns we document are similar to the ones we find using our first empirical strategy (Section 5.1). The effect of a 10-percentile increase in admission selectivity on obvious mistakes is of a 0.03 percentage point lower for female applicants. The effect of admission selectivity is lower for low-SES applicants, measured by claiming points for disadvantaged status or by the NABC-based SES index. The estimated effect is declining with numeracy skill and with academic achievement, measured by 11th-grade GPA (Figure 5). The estimated effect for applicants with an 11th-grade GPA of 3 is about three times as large as this effect for applicants with a perfect GPA (5).

Recall that applicants who ranked four or more contracts, with three or fewer programs, must have learned the pricing scheme (Section 4.1). In Appendix Table D11, we concentrate on these applicants, and find that a 10 percentile increase in (lagged) admission selectivity raises the rate of obvious mistakes by 0.76 percentage points.

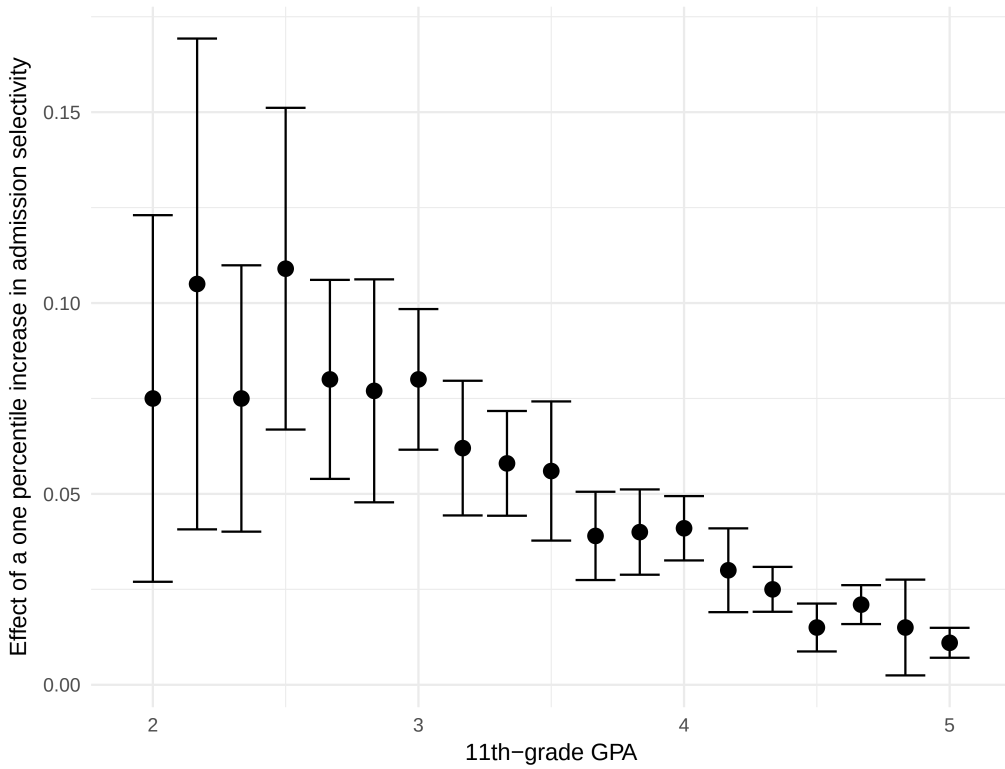
Table 11: Heterogeneity: Admission selectivity and obvious mistakes: A within-ROL comparison

	A. *Gender*		B. *NABC numeracy skill*				
	Male	Female	Q1	Q2	Q3	Q4	Q5
Priority-score cutoff	0.041***	0.038***	0.060***	0.052***	0.041***	0.029***	0.022***
	(0.002)	(0.002)	(0.005)	(0.004)	(0.004)	(0.004)	(0.002)
Within R-squared	0.011		0.011				

	C. *Disadvantaged*		D. *NABC-based SES*				
	No	Yes	Q1	Q2	Q3	Q4	Q5
Priority-score cutoff	0.041***	0.021***	0.027***	0.035***	0.040***	0.036***	0.059***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Within R-squared	0.011		0.011				

Notes: The table presents the effect of admission selectivity on obvious mistakes. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with subgroup indicators. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs among high-school senior applicants. The mean outcome in the sample is 2.2 percent. All specifications include ROL, field, degree, schedule, and program location fixed effects (as in column (5) of Table 10).
***: p< 0.01, **: p<0.05, *: p<0.1.

Figure 5: Admission selectivity and obvious mistakes by 11th-grade GPA: A within-ROL comparison



Notes: The figure presents the effect of admission selectivity on obvious mistakes by 11th-grade GPA with 95% confidence intervals. Robust standard errors are clustered on the applicant level. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with 11th-grade GPA. We include ROL, field, degree, schedule, and program location fixed effects (as in column (5) of Table 10). The estimated effect on applicants with a missing 11th-grade GPA is 0.043 (s.e.: 0.004).

A weakness of this empirical strategy is that the within-ROL variation in the selectivity of admission might be too narrow to identify the full effect. Additionally, there may be confounding factors that we do not control for. In light of the stability of the estimates in Table 10, this latter concern seems less likely.

6 Obvious Mistakes: Consequences for 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 clearinghouses 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 consequences for the applicants who 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 make costly mistakes to those of the individuals who gain from them.

Our sample consists of 2,013 ROLs with an obvious mistake that meet the criteria mentioned above. We find that 663 students, corresponding to 33 percent of the mistakes, were admitted to college as a result of others' mistakes. An additional 1,350 students received an assignment they ranked higher due to others' mistakes, of whom 596 would otherwise have been unfunded (typically in the same program). Table 12 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.

[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 an 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 with respect to the same program to avoid the double counting of affected individuals in cases where multiple costly mistakes were made with respect to the same program.

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. 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, 2017).

Table 12: The distributional consequences 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	0.28	0.450	-0.091^{***}
Age	24.14	7.069	26.57	7.912	2.433^{***}
Disadvantaged	0.03	0.178	0.02	0.136	-0.014^{***}
Unemployment rate in 2008 (%)	7.52	4.218	6.89	3.832	-0.629^{***}
Unemployment rate in 2008 (%) – missing	0.01	0.115	0.03	0.183	0.021^{***}
Gross annual per capita inc. (1000) USD	6.56	1.536	6.89	1.570	0.337^{***}
Gross annual per capita inc. (1000) USD – missing	0.01	0.109	0.03	0.182	0.022^{***}
Female	0.50	0.500	0.46	0.498	-0.044^{***}
Secondary grammar school	0.56	0.497	0.55	0.497	-0.004
Capital	0.19	0.394	0.24	0.429	0.052^{***}
# Observations	2,013		2,013		

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

7 Obvious Mistakes: Explanations

It is difficult to explain obvious mistakes, especially costly ones, using standard models of matching markets. The literature has proposed several explanations for the presence of 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 we document.

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 applicants choosing dominated strategies that they believe are (approximately) optimal. 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 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 high share of 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 priority 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 of the funded contract.

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 (i.e., they are "price-takers"), then they can

[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.

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, however, may 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.[32] 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. We think that the low rates of flipping we document, as compared to studies of markets where the algorithm outcome was not described through cutoff scores, reduce the plausibility of this explanation in our context.

Another possibility, which is specific to the Hungarian context, is that applicants are not aware of the optimal strategy because they fail to understand the application fee structure. More specifically, they may not understand that the application fee is charged per program, and not per contract. We do not think this explanation drives our results. First, information about application fees is readily available through many sources, including the official website and booklet, and the website includes an application fee calculator. Second, we assess this possibility by concentrating on the subsample of applicants who ranked four or more contracts, with three or fewer programs (Section 4.1). These applicants must have learned the pricing scheme, because they had to pay only the fixed application fee. We find that obvious mistakes are prevalent even among these applicants. Third, if agents have rational expectations, this explanation can hold only under implausibly high levels of risk aversion, loss aversion, or hyperbolic discounting.[33]

[32] 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 information on the mechanism does not.

[33] As an illustration of this claim, note that the lottery $[3000, 0.058; -7, 0.942]$, which mimics the average probability of costly mistakes and average tuition cost, is accepted by all agents with absolute risk 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 lack of trust drives our results.

Another possibility is that applicants are not aware of the optimal strategy due to the absence of information about their alternatives, and in particular information about financial aid (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, the application interface uses dropdown menus, which presents the funded and the unfunded positions in the same program consecutively (Appendix A). Third, students who make obvious mistakes come from higher socioeconomic status families and larger settlements, where informational frictions are expected to be less severe. Fourth, since the 2012–13 reform affected only the availability of funding, it would be surprising if individuals who were not informed about funding drove the effect we identify. Fifth, the majority of applicants who make obvious mistakes do so only with respect to a subset of the programs on their ROL.

An alternative explanation of the behavior we document is that applicants have

levels below $7 \cdot 10^{-3}$ — substantially higher than the range suggested by Cabrales et al. (2017) as reasonable (lower than $5 \cdot 10^{-4}$). The positive correlation between obvious mistakes and SES also requires that wealthier individuals are more risk averse, counter to the conventional view (Hart, 2011; Dohmen et al., 2011). Similarly, this lottery is accepted by all loss averse (and otherwise risk neutral) agents with a loss aversion coefficient lower than 26 – substantially lower than 5, the highest estimate reported in Abdellaoui et al. (2008). The associated payoff stream is accepted for future discounting coefficients higher than 0.05 (in the absence of further discounting or risk or loss aversion) — Gabaix (2019) suggests the substantially higher 0.7 as a portable, already estimated parameter (DellaVigna and Malmendier, 2006).

[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.

"non-classical" preferences that do not exclusively depend on their own allocation. Since there is over-demand for funding, altruistic motives are consistent with many of the patterns we document (Fehr and Fischbacher, 2002; Charness and Rabin, 2002). However, the share of obvious mistakes among low-SES applicants is substantial. 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, especially in light of the fact that many applicants drop the funded positions only in some programs. Finally, applicants who are admitted with funding have full control over the money they receive and can redistribute it to raise their utility even more.

Another possibility is that applicants have *ego utility*, and may distort their choices to avoid receiving information about their priority as this may hurt their self-image (Kőszegi, 2006). 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 possibility 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 funding provides pure option value (applicants may decline the funding 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 Discussion

Previous studies mainly focused on the properties of market clearing algorithms, giving special attention to strategic simplicity. As pointed out by Pathak (2016), "*[e]fforts to improve how participants interact with market designs ... hold great promise to comple-*

ment research on market clearing algorithms." This study makes several advances in this direction.

First, our findings suggest that letting participants know their admission priority prior to ranking alternatives mitigates the risk of costly mistakes, even when the mechanism in place is strategy-proof. To see this, note that according to our analysis applicants make dominated choices when they believe that this behavior is likely to be inconsequential. But recent studies suggest that applicants often hold erroneous beliefs about their admission chances (Kapor et al., 2018). School systems around the world have different policies regarding the timing in which preferences are reported. In China, where different provinces have different policies, an increasing share of provinces are allowing students to report their preferences after learning their score on the college entrance exam (Wu and Zhong, 2014).

More broadly, our findings indicate that other interventions affecting applicants' perceptions of the likelihood of admission (e.g., giving publicity to affirmative action policies) could have a large impact on the realized allocation, even when the mechanism is strategy-proof. This indication is supported by the findings of Bobba and Frisancho (2015), who study the Mexico City high-school assignment system. They show that providing applicants with a signal about their priority score causes those applicants who are pessimistic about their performance to apply and to be assigned to more selective schools. Such interventions may therefore have implications for equity if lower SES applicants have a less favorable or less accurate perception of their admission chances.[36]

Third, our findings provide guidance on how clearinghouses should communicate with applicants. We think that the substantially lower rates of flipping that we find relative to previous studies derive from the way the mechanism and its outcomes are communicated to participants. In Hungary, priorities are communicated to applicants as priority scores, and the outcome is expressed through priority-score cutoffs. By contrast, the clearinghouses in which high rates of flipping were documented (NRMP and IPMM) describe priorities through ROLs and provide a combinatorial description of an algorithm that determines the allocation. This observation, in turn, highlights the practical importance of research that provides tractable and transparent descriptions of mechanisms with attractive properties (e.g., Leshno and Lo, 2017; Bíró, 2007).

Finally, obvious mistakes are more common among high socioeconomic status applicants. That is, high SES applicants are more likely to forgo the free opportunity to receive a tuition waiver. As a result, obvious mistakes transfer funding from rich to

[36] In a strategically demanding environment, Kapor et al. (2018) showed that the beliefs of low-SES applicants are less precise.

poor applicants and lead to an increase in the number of students admitted to college.[37] 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. Addressing this challenge is a promising direction for future research.

[37] Intuitively, the reason why obvious mistakes increase the number of students admitted to college is that programs are typically constrained by the availability of funding and not necessarily by capacity. This intuition is formalized in Hassidim et al. (2018a).

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A The Application Interface

This Appendix presents screenshots from the online application interface.

Figure A1 presents an example of an ROL. This ROL includes three programs with six contracts. A contract is a combination of the institution (e.g., ELTE), faculty (e.g., TÁTK), major (e.g., szociológia – sociology), degree (A – Bachelors), schedule (N – full time), and funding (A – funded, K – unfunded).

Figure A1: An Example of an ROL

Notes: Source: https://goo.gl/KQGPvD, accessed: 11/12/2018.

Adding a contract to an ROL involves the following three steps:

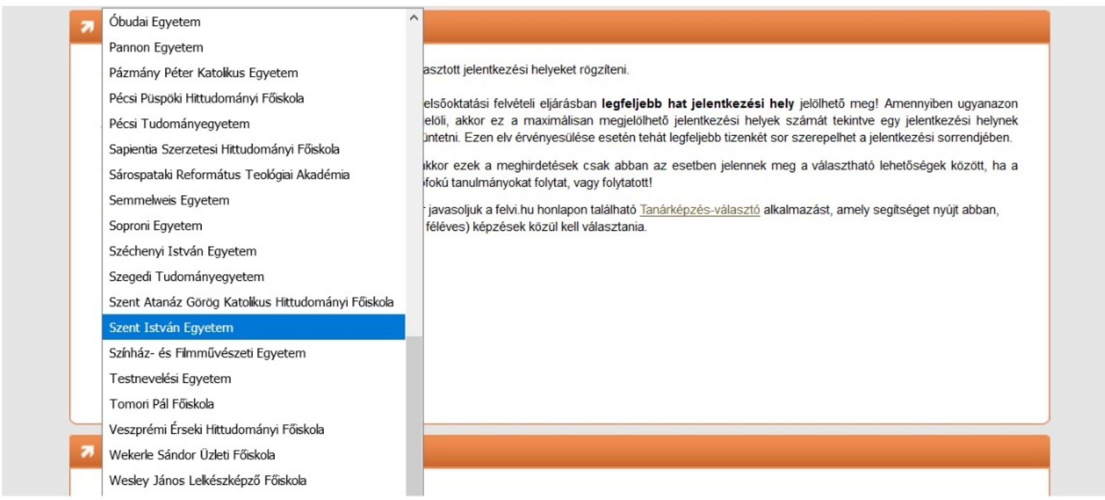
Step 1: Selecting an institution, e.g., Szent István Egyetem (Figure A2).

Step 2: Selecting a faculty, e.g., SZIE–ÉTK (Figure A3).

Step 3: Selecting a contract (i.e., a major – degree – schedule – funding combination), e.g., Építőmérnöki ANA – Civil engineering, Bachelors, full time, funded (Figure A4).

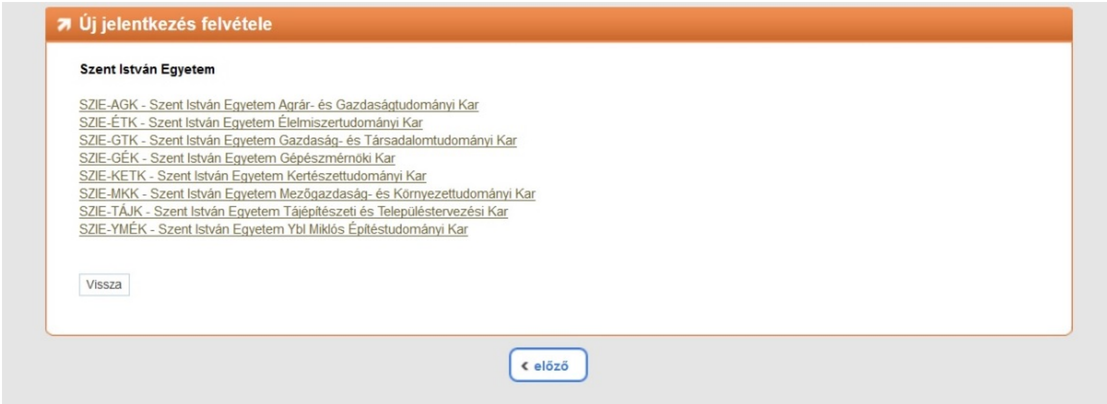
Of note, contracts that differ only in the level of funding appear consecutively in the dropdown menu in Step 3.

Figure A2: Step 1: Institution selection



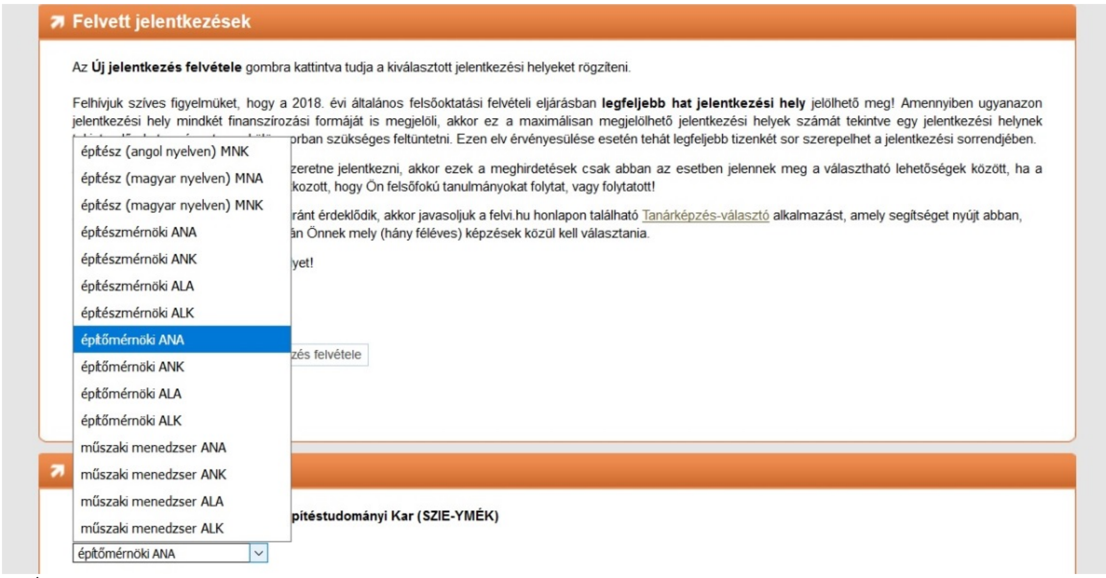
Notes: Source: https://goo.gl/PyV4mc, accessed: 11/12/2018.

Figure A3: Step 2: Faculty selection



Notes: Source: https://goo.gl/PyV4mc, accessed: 11/12/2018.

Figure A4: Step 3: Contract selection



Notes: Építőmérnöki – ANA refers to the contract "Civil engineering – Bachelors (A), Full-time (N), Funded (A)." Építőmérnöki – ANK refers to the contract "Civil engineering – Bachelors (A), Full-time (N), Unfunded (K)." Source: https://goo.gl/PyV4mc, accessed: 11/12/2018.

B Matching College Admissions Data to the NABC

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

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 school 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.

Table B1 describes the result of the matching. The more variables we use for matching, the fewer applicants we are able to match. Between 2011 and 2014, when the NABC covers the full population of tenth graders who took the exam between 2008 and 2011, the share of matched students is stable. We are able to match 91–92 percent of the high-school senior applicant sample based on 3 variables, 89–90 percent based on 4 variables, and 75–80 percent based on 5 variables. The share of unique matches is also stable in these years: 16–20 percent of the high-school senior applicant sample based on 3 variables, 41–44 percent based on 4 variables, and 63–65 percent based on 5 variables. With the exception of 2009, as the matching becomes finer, we can 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, in 2006–2007, the postal code was self-reported, leading 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 B1: Matching college admissions data to NABC

	Matched individuals		Uniquely matched individuals	
	Share (%)	Count	Share (%)	Count
	(1)	(2)	(3)	(4)
A. *Matching based on 3 variables*				
2009	89.2	45,280	28.8	14,632
2010	89.7	45,060	22.1	11,080
2011	91.8	44,941	19.5	9,544
2012	91.6	36,421	18.5	7,365
2013	91.2	35,470	15.8	6,133
2014	92.2	37,241	16.6	6,710
Total	90.9	244,413	20.6	55,464
B. *Matching based on 4 variables*				
2009	67.7	34,375	55.0	27,919
2010	83.2	41,761	51.3	25,765
2011	89.6	43,885	43.4	21,263
2012	89.7	35,687	43.9	17,444
2013	89.2	34,692	41.1	15,990
2014	89.8	36,267	43.7	17,631
Total	84.3	226,667	46.8	126,012
C. *Matching based on 5 variables*				
2009	31.7	16,111	29.3	14,858
2010	62.0	31,125	54.0	27,133
2011	78.6	38,505	64.0	31,362
2012	80.2	31,906	64.7	25,747
2013	79.6	30,940	63.5	24,689
2014	75.4	30,452	62.8	25,359
Total	66.6	179,039	55.4	149,148

Notes: The table describes the outcome of matching the NABC dataset to the high-school senior applicant sample (N = 268,981). Matching based on 3 variables: year of birth, gender, and school identifier; matching based on 4 variables: year and month of birth, gender, and school identifier; matching based on 5 variables: year and month of birth, gender, school identifier, and postal code. 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 between taking the NABC and applying to college.

C The Composition of High-school Senior Applicants over Time

In this Appendix we present summary statistics on high-school senior applicants for each year separately. Even though the number of high-school senior applicants dropped following the 2012 reform, their composition remained stable over time.

Table C1: Individual-level summary statistics over time: High-school senior applicants

	Year						
	2009	2010	2011	2012	2013	2014	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	0.57	0.57	0.57	0.55	0.56	0.56	0.57
	(0.49)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Age at application	18.97	19.03	19.07	19.06	19.09	19.09	19.05
	(0.68)	(0.68)	(0.69)	(0.68)	(0.68)	(0.69)	(0.68)
High school							
- secondary grammar school	0.69	0.70	0.70	0.69	0.70	0.72	0.70
	(0.46)	(0.46)	(0.46)	(0.46)	(0.46)	(0.45)	(0.46)
- vocational school	0.28	0.27	0.27	0.27	0.25	0.24	0.26
	(0.45)	(0.44)	(0.44)	(0.44)	(0.43)	(0.43)	(0.44)
Residence							
- capital	0.16	0.16	0.17	0.16	0.17	0.16	0.16
	(0.37)	(0.37)	(0.37)	(0.37)	(0.37)	(0.37)	(0.37)
- county capital	0.20	0.19	0.20	0.20	0.20	0.20	0.20
	(0.40)	(0.39)	(0.40)	(0.40)	(0.40)	(0.40)	(0.40)
- town	0.34	0.31	0.33	0.34	0.33	0.33	0.33
	(0.47)	(0.46)	(0.47)	(0.47)	(0.47)	(0.47)	(0.47)
- village	0.29	0.33	0.30	0.30	0.30	0.30	0.30
	(0.45)	(0.47)	(0.46)	(0.46)	(0.46)	(0.46)	(0.46)
Disadvantaged status	0.09	0.10	0.11	0.11	0.10	0.07	0.10
	(0.28)	(0.31)	(0.31)	(0.31)	(0.29)	(0.26)	(0.29)
11th-grade GPA	3.82	3.84	3.86	3.95	3.96	3.97	3.89
	(0.85)	(0.84)	(0.83)	(0.82)	(0.81)	(0.80)	(0.83)
11th-grade GPA (standardized)	0.23	0.24	0.26	0.23	0.24	0.26	0.24
	(0.96)	(0.96)	(0.96)	(0.95)	(0.94)	(0.94)	(0.96)
11th-grade GPA - missing	0.12	0.16	0.20	0.20	0.18	0.23	0.18
	(0.32)	(0.36)	(0.40)	(0.40)	(0.38)	(0.42)	(0.38)
Numeracy skills	0.64	0.57	0.56	0.60	0.62	0.60	0.59
	(0.87)	(0.86)	(0.84)	(0.89)	(0.85)	(0.81)	(0.85)
Numeracy skills - missing	0.68	0.38	0.21	0.20	0.20	0.25	0.33
	(0.47)	(0.49)	(0.41)	(0.40)	(0.40)	(0.43)	(0.47)
Literacy skills	0.69	0.61	0.59	0.65	0.67	0.64	0.64
	(0.81)	(0.76)	(0.73)	(0.72)	(0.72)	(0.73)	(0.74)
Literacy skills - missing	0.68	0.38	0.21	0.20	0.20	0.25	0.33
	(0.47)	(0.49)	(0.41)	(0.40)	(0.40)	(0.43)	(0.47)
NABC-based SES index	0.49	0.46	0.45	0.50	0.54	0.50	0.49
	(0.89)	(0.86)	(0.84)	(0.84)	(0.82)	(0.82)	(0.84)
NABC-based SES index - missing	0.68	0.43	0.31	0.30	0.29	0.27	0.39
	(0.47)	(0.49)	(0.46)	(0.46)	(0.45)	(0.45)	(0.49)
Unemployment rate in 2008 (%)	7.97	8.08	7.77	7.79	7.71	7.74	7.86
	(4.58)	(4.67)	(4.46)	(4.46)	(4.42)	(4.42)	(4.51)
Unemployment rate in 2008 - missing	0.02	0.03	0.02	0.02	0.03	0.03	0.02
	(0.14)	(0.16)	(0.15)	(0.15)	(0.16)	(0.16)	(0.16)
Gross annual per capita income (1000 USD)	6.19	6.20	6.05	6.36	6.62	6.94	6.37
	(1.49)	(1.49)	(1.53)	(1.49)	(1.57)	(1.61)	(1.56)
Gross annual per capita income - missing	0.02	0.03	0.02	0.02	0.03	0.03	0.02
	(0.14)	(0.16)	(0.15)	(0.15)	(0.16)	(0.16)	(0.15)
# of contracts on the ROL	4.22	4.29	4.25	4.72	4.48	4.19	4.34
	(2.20)	(2.20)	(2.16)	(2.57)	(2.05)	(1.90)	(2.20)
# of contracts on the ROL (data)	3.71	3.74	3.70	3.99	4.01	3.80	3.81
	(1.47)	(1.48)	(1.48)	(1.53)	(1.47)	(1.42)	(1.48)
# of programs on the ROL (data)	3.29	3.32	3.26	3.32	3.17	3.11	3.25
	(1.24)	(1.26)	(1.25)	(1.21)	(0.99)	(0.96)	(1.17)
Applicants	50,760	50,215	48,974	39,778	38,879	40,375	268,981

Notes: The table reports mean values of student characteristics, with standard deviations in parentheses over time. Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average grade in Hungarian grammar and literature and mathematics. Grades are standardized among eligible applicants. Some applicants have no incentive to report their GPA to the clearinghouse (see Footnote 21). 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 16).

D Additional Results

In Appendix D.1, we show that our findings on the correlates of obvious mistakes are robust to various sample restrictions. Then, in Appendix D.2, we provide additional results on the effect of selectivity on obvious mistakes exploiting the 2012–13 reform. Then, in Appendix D.3, we present estimates that exploit all variations in the availability of funded positions in the sample. Finally, in Appendix D.4, we provide additional results on the effect of selectivity on obvious mistakes using within-ROL comparisons.

D.1 Obvious Mistakes and Their Correlates

In Section 4.2 we have shown that obvious mistakes are more common among applicants of low academic achievement and of high socioeconomic status. In this Appendix we show that these results continue to hold if we do not rely on the NABC dataset (Table D1), when we focus on the larger eligible applicant sample (Table D2) or relevant high-school senior applicant sample (Table D3), and when we focus on the pre- and post-reform periods (Table D4).

Table D1: Demographics, socioeconomic status, academic achievement and obvious mistakes: High-school senior applicants

Dependent variable	Obvious mistakes		
	(1)	(2)	(3)
Unemployment rate in 2008 (%)	-0.002^{***}		
	(0.000)		
Gross annual per capita income (1000 USD)		0.011^{***}	
		(0.000)	
Disadvantaged status			-0.032^{***}
			(0.001)
11th-grade GPA (standardized)	-0.029^{***}	-0.029^{***}	-0.030^{***}
	(0.001)	(0.001)	(0.001)
Female	0.016^{***}	0.016^{***}	0.016^{***}
	(0.001)	(0.001)	(0.001)
Vocational school	-0.005^{***}	-0.004^{***}	-0.005^{***}
	(0.001)	(0.001)	(0.001)
Other school	0.023^{***}	0.023^{***}	-0.006^{**}
	(0.005)	(0.005)	(0.003)
County capital	-0.031^{***}	-0.014^{***}	-0.034^{***}
	(0.002)	(0.002)	(0.002)
Town	-0.033^{***}	-0.014^{***}	-0.040^{***}
	(0.002)	(0.002)	(0.002)
Age FE	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*
R-squared	.036	.037	.035

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The share of obvious mistakes is 5.7 percent. Eleventh-grade GPA is missing for 15.3 percent of the sample. We include an indicator for those missing observations in our regressions. The sample includes 268,981 ROLs.
***: p<0.01, **: p<0.05, *:p<0.1.

Table D2: Demographics, socioeconomic status, academic achievement and obvious mistakes: Eligible applicants

Dependent variable	Obvious mistakes		
	(1)	(2)	(3)
Unemployment rate in 2008 (%)	-0.001^{***}		
	(0.000)		
Gross annual per capita income (1000 USD)		0.010^{***}	
		(0.000)	
Disadvantaged status			-0.038^{***}
			(0.001)
11th-grade GPA (standardized)	-0.026^{***}	-0.026^{***}	-0.026^{***}
	(0.001)	(0.001)	(0.001)
Female	0.023^{***}	0.024^{***}	0.023^{***}
	(0.001)	(0.001)	(0.001)
Vocational school	-0.010^{***}	-0.009^{***}	-0.010^{***}
	(0.001)	(0.001)	(0.001)
Other school	-0.028^{***}	-0.027^{***}	-0.036^{***}
	(0.003)	(0.003)	(0.002)
County capital	-0.024^{***}	-0.008^{***}	-0.027^{***}
	(0.001)	(0.002)	(0.001)
Town	-0.027^{***}	-0.009^{***}	-0.032^{***}
	(0.001)	(0.002)	(0.001)
Age FE	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*
R-squared	.072	.073	.073

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The share of obvious mistakes is 11.1 percent. Eleventh-grade GPA is missing for 17.9 percent of the sample. We include an indicator for those missing observations in our regressions. The sample includes 525,275 ROLs.
***: p<0.01, **: p<0.05, *:p<0.1.

Table D3: Demographics, socioeconomic status, academic achievement and obvious mistakes: Relevant high-school senior applicants

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
NABC-based SES index	0.012***			
	(0.002)			
Unemployment rate in 2008 (%)		−0.003***		
		(0.000)		
Gross annual per capita income (1000 USD)			0.011***	
			(0.001)	
Disadvantaged status				−0.031***
				(0.006)
Numeracy skills	−0.023***	−0.023***	−0.023***	−0.022***
	(0.002)	(0.002)	(0.002)	(0.002)
11th-grade GPA (standardized)	−0.048***	−0.048***	−0.048***	−0.048***
	(0.002)	(0.002)	(0.002)	(0.002)
Female	0.020***	0.019***	0.019***	0.020***
	(0.003)	(0.003)	(0.003)	(0.003)
Vocational school	0.010***	0.007*	0.008**	0.007*
	(0.004)	(0.004)	(0.004)	(0.004)
Other school	0.016	0.014	0.015	0.016
	(0.015)	(0.015)	(0.015)	(0.015)
County capital	−0.025***	−0.021***	−0.006	−0.026***
	(0.005)	(0.005)	(0.006)	(0.005)
Town	−0.030***	−0.023***	−0.007	−0.032***
	(0.005)	(0.005)	(0.006)	(0.005)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.037	.037	.037	.036

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. 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 (N = 56,533). 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 15.0 percent of the sample. We include an indicator for those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Table D4: Demographics, socioeconomic status, academic achievement and obvious mistakes: Pre- and post-reform

Dependent variable	Obvious mistakes		
	(1)	(2)	(3)
Subsample	Full	2009–2011	2012–2014
NABC-based SES index	0.018***	0.013***	0.023***
	(0.001)	(0.001)	(0.001)
Numeracy skills	−0.013***	−0.009***	−0.017***
	(0.001)	(0.001)	(0.001)
11th-grade GPA (standardized)	−0.025***	−0.019***	−0.031***
	(0.001)	(0.001)	(0.001)
Female	0.014***	0.007***	0.021***
	(0.001)	(0.001)	(0.002)
Vocational school	−0.001	0.002	−0.003
	(0.001)	(0.002)	(0.002)
Other school	0.013*	0.013	0.013
	(0.007)	(0.010)	(0.009)
County capital	−0.027***	−0.024***	−0.030***
	(0.002)	(0.003)	(0.004)
Town	−0.035***	−0.028***	−0.042***
	(0.002)	(0.003)	(0.003)
Age FE	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*
R-squared	.036	.021	.029

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on gender, measures of academic achievement, and measures of socioeconomic status. 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 (N = 162,972). The sample includes 78,615 (84,357) pre-reform (post-reform) ROLs. The share of obvious mistakes is 5.7% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 15.3% of the sample. We include an indicator for those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

D.2 The Effect of Admission Selectivity on Obvious Mistakes: 2012–13 reform: Robustness

In this Appendix, we present three additional model specifications for the effect of admission selectivity on obvious mistakes, using our difference-in-differences identification strategy. First, in Table D5, we estimate the effect of the 2012–13 reform on obvious mistakes using only the highest ranked application in each ROL. Second, in Table D6, we estimate the effect of the 2012–13 reform on obvious dropping and on obvious flipping separately. Third, in Table D7, we analyze the effect of a small-scale reform occurred in 2011.

Table D5: The effect of admission selectivity on obvious mistakes: 2012–13 reform: highest ranked application

Dependent variable	Obvious mistakes	
	(1)	(2)
Severe funding cut × 2013	0.177***	0.171***
	(0.005)	(0.005)
Severe funding cut × 2014	0.148***	0.143***
	(0.004)	(0.004)
Program FE	*Yes*	*Yes*
Year FE	*Yes*	*Yes*
Demographics & GPA	*No*	*Yes*
School FE	*No*	*Yes*
R-squared	0.112	0.133
# ROLs	226,362	226,362
# Obs.	226,362	226,362

Notes: The table presents the effect of admission selectivity on obvious mistakes. 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. The share of obvious mistakes is 3.2 percent.
***: p<0.01, **: p<0.05, *: p<0.1.

Table D6: The effect of admission selectivity on obvious dropping and on obvious flipping: 2012–13 reform

Dependent variable	Obvious dropping	Obvious flipping
	(1)	(2)
Severe funding cut × 2013	0.171***	0.014***
	(0.004)	(0.001)
Severe funding cut × 2014	0.154***	0.019***
	(0.004)	(0.001)
Program FE	*Yes*	*Yes*
Year FE	*Yes*	*Yes*
Demographics & GPA	*Yes*	*Yes*
School FE	*Yes*	*Yes*
R-squared	0.128	0.017
# ROLs	229,009	229,009
# Obs.	729,650	729,650

Notes: The table presents the effect of admission selectivity on obvious mistakes. 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. The share of obvious dropping (flipping) is 3.1 (0.5) percent. In the baseline, the rate of obvious dropping among treated applications was 5.5 (4.2) percent in 2013 (2014). In the baseline, the rate of obvious flipping among treated applications was 1.0 (0.7) percent in 2013 (2014).
***: p<0.01, **: p<0.05, *: p<0.1.

Table D7: The effect of admission selectivity on obvious mistakes: 2011 reform

Dependent variable	Obvious mistakes
	(1)
Funding cut in 2011 – business/economics	0.012***
	(0.002)
Funding cut in 2011 – social sciences	0.013***
	(0.003)
Severe funding cut in 2013	0.188***
	(0.004)
Severe funding cut in 2014	0.176***
	(0.004)
Program FE	*Yes*
Year FE	*Yes*
Demographics & GPA	*Yes*
School FE	*Yes*
R-squared	0.136
# ROLs	229,009
# Obs.	729,650

Notes: The table presents the effect of admission selectivity on obvious mistakes. 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.

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

Section 5.1 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 2012–13 reform, we exploit all variations 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:

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

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.[38] In line with our main result, we expect the estimate of β to be negative, as more available funded seats correspond to lower admission selectivity. On the other hand, the 2012–13 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 D8 presents our estimates. We find that a 10-percent reduction in the number of funded seats increases obvious mistakes by 0.75–0.79 of a percentage point.

[38] Since the government did not release the funded quotas for 2013 and 2014, we use the realized number of funded positions in these years.

Table D8: The effect of admission selectivity on obvious mistakes: Alternative specification

Dependent variable	Obvious mistakes			
	(1)	(2)	(3)	(4)
Capacity (realized, log)	-0.082^{***}	-0.079^{***}		
	(0.001)	(0.001)		
Capacity (admin, log)			-0.078^{***}	-0.075^{***}
			(0.001)	(0.001)
Program FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Demographics & GPA	*No*	*Yes*	*No*	*Yes*
School FE	*No*	*Yes*	*No*	*Yes*
R-squared	0.107	0.130	0.106	0.128
# ROLs	229,009	229,009	229,009	229,009
# Obs.	729,650	729,650	729,650	729,650

Notes: The table presents estimates of the effect of the number of available funded positions on obvious mistakes for the high-school senior applicant sample. Robust standard errors clustered on the applicant level are in parentheses. Columns (1) and (2) use the realized number of funded positions in 2009–2011, and columns (3) and (4) use the publicly released funded quotas in 2009–2011. 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. Missing control variables are indicated by a separate dummy variable.
***: p<0.01, **: p<0.05, *: p<0.1.

D.4 The Effect of Admission Selectivity on Obvious Mistakes: A within-ROL comparison: Robustness

Table D9: Admission selectivity and obvious mistakes: A within-ROL comparison: Obvious dropping

Dependent variable	Obvious dropping				
	(1)	(2)	(3)	(4)	(5)
Priority-score cutoff	0.032***	0.030***	0.033***	0.032***	0.035***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Field FE	*No*	*Yes*	*Yes*	*Yes*	*Yes*
Degree FE	*No*	*No*	*Yes*	*Yes*	*Yes*
Schedule FE	*No*	*No*	*No*	*Yes*	*Yes*
Location FE	*No*	*No*	*No*	*No*	*Yes*
Within R-squared	0.005	0.007	0.007	0.010	0.011
# ROLs	110,398	110,398	110,398	110,398	110,398
# Obs.	351,884	351,884	351,884	351,884	351,884

Notes: The table presents the effect of admission selectivity on obvious dropping. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs among high-school senior applicants. The mean outcome in the sample is 1.9 percent. All specifications include ROL fixed effects.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table D10: Admission selectivity and obvious mistakes: A within-ROL comparison: Obvious flipping

Dependent variable	Obvious flipping				
	(1)	(2)	(3)	(4)	(5)
Priority-score cutoff	0.003***	0.004***	0.004***	0.004***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Field FE	*No*	*Yes*	*Yes*	*Yes*	*Yes*
Degree FE	*No*	*No*	*Yes*	*Yes*	*Yes*
Schedule FE	*No*	*No*	*No*	*Yes*	*Yes*
Location FE	*No*	*No*	*No*	*No*	*Yes*
Within R-squared	0.000	0.001	0.001	0.001	0.001
# ROLs	110,398	110,398	110,398	110,398	110,398
# Obs.	351,884	351,884	351,884	351,884	351,884

Notes: The table presents the effect of admission selectivity on obvious flipping. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs among high-school senior applicants. The mean outcome in the sample is 0.3 percent. All specifications include ROL fixed effects.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table D11: Admission selectivity and obvious mistakes: A within-ROL comparison: Application Fee Structure Comprehension

Dependent variable	Obvious mistakes	
	(1)	(2)
Priority-score cutoff × MU	0.078***	0.076***
	(0.005)	(0.005)
Priority-score cutoff × (1 - MU)	0.030***	0.034***
	(0.001)	(0.002)
Field FE	*No*	*Yes*
Degree FE	*No*	*Yes*
Schedule FE	*No*	*Yes*
Location FE	*No*	*Yes*
Within R-squared	0.006	0.011
# ROLs	110,398	110,398
# Obs.	351,884	351,884

Notes: The table presents the effect of admission selectivity on obvious mistakes. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with subgroup indicators. An applicant must understand (MU) the application fee structure if she ranked four or more contracts with three or fewer programs. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs among high-school senior applicants. The mean outcome in the sample is 2.2 percent. All specifications include ROL fixed effects.

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

E Additional Results on Costly Obvious Mistakes (Online Appendix)

In this appendix we replicate the analysis from the body of the paper, but this time we 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 must 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.

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

E.1 Ex Post Costly Obvious Mistakes: Correlates

Table E1: Demographics, socioeconomic status, and academic achievement vs. ex post costly obvious mistakes: Lower bound

Dependent variable	Ex Post Costly Obvious Mistakes: Lower bound			
	(1)	(2)	(3)	(4)
NABC-based SES index	0.0004**			
	(0.0002)			
Unemployment rate in 2008 (%)		−0.0001***		
		(0.0000)		
Gross annual per capita income (1000 USD)			0.0003**	
			(0.0001)	
Disadvantaged status				−0.0016***
				(0.0003)
Numeracy skills	−0.0004**	−0.0004**	−0.0004**	−0.0004**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
11th-grade GPA (standardized)	−0.0007***	−0.0007***	−0.0007***	−0.0007***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Female	0.0001	0.0001	0.0001	0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Vocational school	−0.0005*	−0.0006**	−0.0006**	−0.0006**
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Other school	0.0023	0.0022	0.0023	0.0023
	(0.0019)	(0.0019)	(0.0019)	(0.0019)
County capital	−0.0013***	−0.0011**	−0.0007	−0.0013***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Town	−0.0008*	−0.0004	−0.0001	−0.0008*
	(0.0005)	(0.0005)	(0.0006)	(0.0005)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.0035	.0036	.0035	.0036

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. 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 (N = 162,978). 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 E2: Demographics, socioeconomic status, and academic achievement vs. ex post costly obvious mistakes: Upper bound

Dependent variable	Ex Post Costly Obvious Mistakes: Upper bound			
	(1)	(2)	(3)	(4)
NABC-based SES index	0.0013***			
	(0.0002)			
Unemployment rate in 2008 (%)		−0.0001***		
		(0.0000)		
Gross annual per capita income (1000 USD)			0.0007***	
			(0.0002)	
Disadvantaged status				−0.0026***
				(0.0005)
Numeracy skills	−0.0001	0.0001	0.0001	0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
11th-grade GPA (standardized)	−0.0006***	−0.0006**	−0.0006**	−0.0006***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Female	0.0005	0.0003	0.0004	0.0004
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Vocational school	−0.0006	−0.0010**	−0.0010**	−0.0010**
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Other school	0.0053*	0.0052*	0.0053*	0.0053*
	(0.0029)	(0.0029)	(0.0029)	(0.0029)
County capital	−0.0028***	−0.0027***	−0.0017*	−0.0029***
	(0.0008)	(0.0008)	(0.0009)	(0.0008)
Town	−0.0031***	−0.0029***	−0.0018**	−0.0034***
	(0.0007)	(0.0007)	(0.0009)	(0.0007)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.0051	.005	.005	.005

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. 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 (N = 162,978). The fraction of ex post costly obvious mistakes (upper bound) in this sample is 0.56 percent.

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

Table E3: Demographics, socioeconomic status, and academic achievement vs. 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	−0.0006			
	(0.0005)			
Unemployment rate in 2008 (%)		−0.0002*		
		(0.0001)		
Gross annual per capita income (1000 USD)			0.0001	
			(0.0003)	
Disadvantaged status				−0.0024*
				(0.0013)
Numeracy skills	−0.0007	−0.0009*	−0.0008*	−0.0008*
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
11th-grade GPA (standardized)	−0.0012***	−0.0012***	−0.0012***	−0.0012***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Female	−0.0006	−0.0006	−0.0006	−0.0005
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Vocational school	−0.0011	−0.0009	−0.0009	−0.0009
	(0.0009)	(0.0008)	(0.0008)	(0.0008)
Other school	0.0043	0.0042	0.0043	0.0043
	(0.0042)	(0.0042)	(0.0042)	(0.0042)
County capital	−0.0018*	−0.0014	−0.0015	−0.0017
	(0.0011)	(0.0011)	(0.0012)	(0.0011)
Town	0.0004	0.0011	0.0007	0.0006
	(0.0010)	(0.0011)	(0.0013)	(0.0010)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.0051	.0052	.0051	.0051

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. 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 relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing (N = 56,533). The fraction of ex post costly obvious mistakes (lower bound) in this sample is 0.68 percent.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table E4: Demographics, socioeconomic status, and academic achievement vs. 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	−0.0001			
	(0.0007)			
Unemployment rate in 2008 (%)		−0.0000		
		(0.0001)		
Gross annual per capita income (1000 USD)			−0.0000	
			(0.0005)	
Disadvantaged status				−0.0013
				(0.0022)
Numeracy skills	0.0014*	0.0014*	0.0013*	0.0013*
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
11th-grade GPA (standardized)	0.0003	0.0003	0.0003	0.0003
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Female	−0.0013	−0.0013	−0.0013	−0.0013
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
Vocational school	−0.0015	−0.0014	−0.0014	−0.0014
	(0.0013)	(0.0013)	(0.0013)	(0.0013)
Other school	0.0100	0.0100	0.0100	0.0100
	(0.0063)	(0.0063)	(0.0063)	(0.0063)
County capital	−0.0027	−0.0027	−0.0027	−0.0027
	(0.0017)	(0.0017)	(0.0019)	(0.0017)
Town	−0.0023	−0.0022	−0.0023	−0.0022
	(0.0016)	(0.0017)	(0.0020)	(0.0016)
Age FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
R-squared	.0059	.0059	.0059	.0059

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. 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 relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing (N = 56,533). The fraction of ex post costly obvious mistakes (upper bound) in this sample is 1.62 percent.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table E5: Demographics, socioeconomic status, and academic achievement vs. ex post costly obvious mistakes: Lower bound, high-school senior applicants

Dependent variable	Ex Post Costly Obvious Mistakes: Lower bound		
	(1)	(2)	(3)
Unemployment rate in 2008 (%)	-0.0001^{***}		
	(0.0000)		
Gross annual per capita income (1000 USD)		0.0003^{***}	
		(0.0001)	
Disadvantaged status			-0.0013^{***}
			(0.0002)
11th-grade GPA (standardized)	-0.0009^{***}	-0.0009^{***}	-0.0009^{***}
	(0.0001)	(0.0001)	(0.0001)
Female	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)
Vocational school	−0.0002	−0.0002	−0.0002
	(0.0002)	(0.0002)	(0.0002)
Other school	0.0039^{***}	0.0039^{***}	0.0039^{***}
	(0.0015)	(0.0015)	(0.0009)
County capital	-0.0013^{***}	-0.0009^{**}	-0.0014^{***}
	(0.0004)	(0.0004)	(0.0004)
Town	-0.0006^{*}	−0.0003	-0.0008^{**}
	(0.0004)	(0.0004)	(0.0003)
Age FE	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*
R-squared	.004	.0039	.004

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The fraction of ex post costly obvious mistakes (lower bound) is 0.25 percent. The sample includes 268,981 ROLs.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table E6: Demographics, socioeconomic status, and academic achievement vs. ex post costly obvious mistakes: Upper bound, high-school senior applicants

Dependent variable	Ex Post Costly Obvious Mistakes: Upper bound		
	(1)	(2)	(3)
Unemployment rate in 2008 (%)	-0.0001^{***}		
	(0.0000)		
Gross annual per capita income (1000 USD)		0.0006^{***}	
		(0.0001)	
Disadvantaged status			-0.0024^{***}
			(0.0004)
11th-grade GPA (standardized)	-0.0004^{**}	-0.0004^{***}	-0.0005^{***}
	(0.0002)	(0.0002)	(0.0002)
Female	-0.0002	-0.0002	-0.0001
	(0.0003)	(0.0003)	(0.0003)
Vocational school	-0.0006^{*}	-0.0006^{*}	-0.0006^{*}
	(0.0003)	(0.0003)	(0.0003)
Other school	0.0063^{***}	0.0063^{***}	0.0068^{***}
	(0.0020)	(0.0020)	(0.0012)
County capital	-0.0025^{***}	-0.0015^{**}	-0.0027^{***}
	(0.0005)	(0.0006)	(0.0005)
Town	-0.0029^{***}	-0.0018^{***}	-0.0032^{***}
	(0.0005)	(0.0006)	(0.0005)
Age FE	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*
R-squared	.0057	.0057	.0057

Notes: The table presents the results of a linear regression of an indicator for obvious mistakes on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The fraction of ex post costly obvious mistakes (upper bound) is 0.55 percent. The sample includes 268,981 ROLs.
***: p< 0.01, **: p<0.05, *: p<0.1.

E.2 The Effect of Admission Selectivity on Ex Post Costly Obvious Mistakes: 2012–13 reform

Table E7: The effect of admission selectivity on ex post costly obvious mistakes: 2012–13 reform

Dependent variable	Ex Post Costly Obvious Mistakes			
	Lower bound		Upper bound	
	(1)	(2)	(3)	(4)
Severe funding cut × 2013	-0.003^{***}	-0.003^{***}	-0.006^{***}	-0.006^{***}
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2014	-0.001^{***}	-0.001^{***}	0.001^{**}	0.001^{**}
	(0.000)	(0.000)	(0.001)	(0.001)
Program FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
Demographics & GPA	*No*	*Yes*	*No*	*Yes*
School FE	*No*	*Yes*	*No*	*Yes*
R-squared	0.010	0.011	0.015	0.019
# ROLs	229,009	229,009	229,009	229,009
# Obs.	729,650	729,650	729,650	729,650

Notes: The table presents the effect of admission selectivity on ex post costly obvious mistakes. Robust standard errors clustered on the applicant level are in parentheses. The mean lower (upper) bound in the sample is 0.08 (0.24) percent. 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. Missing control variables are indicated by a separate dummy variable.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table E8: Heterogeneity by 11th-grade GPA: The effect of admission selectivity on ex post costly obvious mistakes: 2012–13 reform

Dependent variable	Ex Post Costly Obvious Mistakes			
	Lower bound		Upper bound	
	(1)	(2)	(3)	(4)
Severe funding cut × 2013 × 11th-grade GPA – missing	−0.003***	−0.003***	−0.007***	−0.007***
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2013 × 11th-grade GPA ∈ [2, 3]	−0.004***	−0.004***	−0.010***	−0.010***
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2013 × 11th-grade GPA ∈ (3, 4]	−0.004***	−0.004***	−0.009***	−0.009***
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2013 × 11th-grade GPA ∈ (4, 5)	−0.002**	−0.002**	−0.003**	−0.003**
	(0.001)	(0.001)	(0.001)	(0.001)
Severe funding cut × 2013 × 11th-grade GPA = 5	−0.003***	−0.003***	−0.001	−0.001
	(0.000)	(0.000)	(0.002)	(0.002)
Severe funding cut × 2014 × 11th-grade GPA – missing	−0.001***	−0.001***	−0.001	−0.000
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2014 × 11th-grade GPA ∈ [2, 3]	−0.001	−0.001	−0.002***	−0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Severe funding cut × 2014 × 11th-grade GPA ∈ (3, 4]	−0.001***	−0.001***	−0.002***	−0.003***
	(0.000)	(0.000)	(0.000)	(0.000)
Severe funding cut × 2014 × 11th-grade GPA ∈ (4, 5)	−0.000	−0.000	0.001	0.001
	(0.000)	(0.000)	(0.001)	(0.001)
Severe funding cut × 2014 × 11th-grade GPA = 5	0.000	0.000	0.012***	0.012***
	(0.001)	(0.001)	(0.003)	(0.003)
Program FE	*Yes*	*Yes*	*Yes*	*Yes*
Year FE	*Yes*	*Yes*	*Yes*	*Yes*
11th-grade GPA	*Yes*	*Yes*	*Yes*	*Yes*
Demographics	*No*	*Yes*	*No*	*Yes*
School FE	*No*	*Yes*	*No*	*Yes*
R-squared	0.010	0.011	0.016	0.020
# ROLs	229, 009	229, 009	229, 009	229, 009
# Obs.	729, 650	729, 650	729, 650	729, 650

Notes: The table presents heterogeneous effects of admission selectivity on ex post costly obvious mistakes. 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 parentheses. The mean lower (upper) bound in the sample is 0.08 (0.24) percent. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, and high-school type. Missing control variables are indicated by a separate dummy variable.
***: p< 0.01, **: p<0.05, *: p<0.1.