# Obvious Mistakes in a Strategically Simple College Admissions Environment

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**Abstract**

We provide direct field evidence that, even though the Hungarian college admissions process uses a strategically simple assignment mechanism, a large fraction of the applicants employ a dominated strategy. These applicants make *obvious mistakes*: they forgo the option for a tuition waiver worth thousands of dollars, even though this behavior has no benefit. In many cases applicants would have received the tuition waiver had they asked for it. Obvious mistakes are more common among low-achieving students and high socioeconomic status students. Costly mistakes result in a transfer of tuition waivers from high- to low socioeconomic status applicants and an increase in the number of students attending college. We exploit exogenous variation in the availability of tuition waivers and find that a rise in program selectivity substantially increases the likelihood of obvious mistakes, especially among high socioeconomic status applicants and low-achieving applicants.

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

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# 1 Introduction

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

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

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

[1]Strategy-proof mechanisms are considered desirable for a variety of other reasons. First, they are robust in the sense that equilibrium prediction does not depend on agents' beliefs, as they all have a dominant strategy (Wilson, 1987; Bergemann and Morris, 2005). Second, they are thought to *level the playing field* in the sense that, thanks to their simplicity, they do not give an advantage to more strategically sophisticated participants (Friedman, 1960; Pathak and Sönmez, 2008; Abdulkadiroglu et al., 2006). Finally they generate information that may be useful for policy makers (Machado et al., 2012; Roth, 2008). Azevedo and Budish (2013) show that in large markets strategy-proofness often holds approximately.

[2]Pathak and Sönmez (2013) report on dozens of school-choice systems around the world that implemented strategically simple versions of DA, only one of which (the one employed in the city of Boston) was strategy-proof.

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

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

Having found that dominated-strategy play in strategically simple environments is prevalent, costly, and could have significant externalities, a natural next question is, *what causes this behavior?* We establish a causal relationship between program selectivity and obvious mistakes. We use a difference-in-differences design based on variation stemming from a sharp change in the Hungarian government policy. In 2012, a severe cut in the number of tuition waivers affected several fields of study (business and economics, legal studies, and social sciences), significantly increasing the selectivity of admission to funded positions. Other fields remained largely unaffected.[4] Our estimates suggest that as a result of the rise in selectivity, obvious mistakes by high-school senior applicants increased by 19 percentage points (relative to a baseline of 6 percent). The effect is heterogeneous, and is stronger among students of low academic ability and students of high socioeconomic status. We also find that, within ROL, obvious mistakes are more likely with respect to more selective programs.

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

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

[4]The change was motivated by political and fiscal concerns, and was not related to changes in demand. For more details see Section 2.

[5]The Center on Reinventing Public Education states in a report on the Denver and New Orleans school-choice systems

*with market designs ... hold great promise to complement research on market clearing algorithms.*"

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

## Related Literature

Our findings are in line with several recent studies suggesting that large fractions of participants in strategically simple, high-stakes environments use dominated strategies. These include the National Resident Matching Program (NRMP; Rees-Jones, 2017b), the Israeli Psychology Master's Match (IPMM; Hassidim et al., 2016, 2017b), college admissions in Australia (Artemov et al., 2017), high-school choice in Mexico City (Chen and Pereyra, 2017), and elementary-school choice in Denver and New Orleans (Gross et al., 2015). This paper presents direct field evidence of dominated-strategy play in a large, well-established market. Additionally, it provides a clean lower bound on the costs of such mistakes, and finds that they are indeed costly.

Similar findings emerge from laboratory studies evaluating truthful reporting under DA. The first ones to use this approach were Chen and Sönmez (2006). They find that approximately 30% of the participants misrepresented their preferences under DA (Gale and Shapley, 1962) and the number was even higher under Top Trading Cycle (TTC; Shapley and Scarf, 1974). Subsequent laboratory experiments that employ numerous variants of the matching environment corroborate this finding.[7] Ding and Schotter (2015) show that while repeated interaction with DA leads to

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

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

[7] Examples include Braun et al. (2014), Calsamiglia et al. (2010), Chen and Kesten (2011), 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).

learning of the dominant strategy, intergenerational advice fails.

Our approach to detecting dominated-strategy play follows Hassidim et al. (2016). Similar to their paper, we provide a lower bound on the prevalence of dominated-strategy play, as we can only detect a very specific type of strategic mistakes: obvious mistakes. Applicants likely make other mistakes, with respect to their choice of school and major, we cannot detect using our methodology. We note that only 33% of the ROLs rank unfunded positions, a necessary condition for us to be able to detect a mistake.

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

Our work is also related to the large literature on behavior in school choice and college admissions (e.g. Hoxby and Avery, 2012). This literature finds that informational frictions about the cost of application, financial aid, and the returns to college attendance, as well as the complexity of the application for financial aid, play an important role, and that low-SES families are particularly affected (Avery and Kane, 2004; Hastings and Weinstein, 2008; Jensen, 2010; Ajayi, 2011; Bettinger et al., 2012; Hoxby and Turner, 2013; Hastings et al., 2015; Pallais, 2015; Andrabi et al., 2017). We contribute to this literature by studying a centralized market with a long history, where the application procedure is simple and the information is accessible and abundant, and by focusing on mistakes that are unlikely to be caused by information frictions. Yet, we find that a substantial fraction of applicants make such mistakes, and this behavior is more common among urban and high-SES applicants. We conclude that other frictions, such as lack of comprehension of the way the market clears, are also important.

The remainder of the paper is organized as follows. Section 2 describes the Hungarian higher-education system, and the admissions process in particular. Section 3 describes our data. Section 4 presents results on the prevalence and costs of obvious mistakes, as well as their correlation with applicants' characteristics. In Section 5 we begin by documenting the correlation between program selectivity and obvious mistakes. We then lay out our empirical strategy, and establish a causal relationship between program 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 Background: Higher Education in Hungary

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% are assigned. As is standard in Europe, 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.

## 2.1 Tuition and Fee 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 and fee waiver. Nevertheless, the government caps the number of funded positions in some majors and in each field of study (business and economics, computer science, etc.). Eligible students may apply for a funded position, but unfunded positions are also offered. If admitted to an unfunded position, the student will not receive a tuition and fee waiver, in spite of her eligibility.

Besides the monetary benefits, funded positions have other advantages over unfunded ones. First, many institutions grant funded students priority in access to subsidized housing and other amenities. In some cases, these benefit 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).

## 2.2 The Centralized Admissions Process

College admissions has been organized through a centralized scheme since 1985. The centralized clearinghouse is managed by a nonprofit governmental organization. Over the years, several changes have been introduced to the mechanism in place. The most recent change occurred in 2008 when a variant of the student-proposing DA was adopted.[8] The mechanism that was in use

[8]To be precise, the matching system has three rounds. The main round, where the majority of BA and MA positions are allocated, ends in July; an additional, a 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. We use data only from the main round

previously had been based on a variant of the program-proposing version of DA. Both mechanisms endow programs with priorities based on a weighted average of several variables (mainly academic performance in the 11th and 12th grades and matriculation exam scores, but also credits 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).

Students are allowed to rank any number of *contracts*, program and funding level combinations, they wish. For example, they may submit an ROL that includes three contracts with two programs: 1) funded BA in biology at Eötvös Lóránd University; 2) funded BA in applied economics at Corvinus University; 3) non-funded BA in biology at Eötvös Lóránd University. Submitting an ROL that includes up to 3 programs (which may correspond to up to 6 contracts) requires paying a fixed application fee only. However, applicants are charged for each additional program in their ROL.[9]

### 2.2.1 Obvious Mistakes

The fact that fees are determined according to the number of *programs* in the ROL, as opposed to the number of *contracts*, implies that if a student ranks an unfunded contract with a certain program, then the marginal cost of ranking a funded contract with the same program is zero. This, in turn, implies that 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 refer to such dominated strategies as *obvious mistakes*.

### 2.2.2 Timeline

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

of the BA match. This section draws heavily on Biro (2011).

[9] In 2011, the fixed fee was about 30 dollars (9,000 HUF) and the fee for each additional program was about 7 dollars (2,000 HUF).

#### 2.2.3 Information

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

### 2.3 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.[10] 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.[11] 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 1). Altogether, the reform reduced the number of funded positions by 81% in these fields. Funded positions in some majors were eliminated completely (examples include business administration and management, commerce and marketing, and human resources). In other majors, funding was only offered in a subset of the institutions where it had been offered previously (for example, legal studies, international business admin-

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

[11] 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.

istration, and international relations). In still other majors, the menu was not changed, but the capacities of state-sponsored options were reduced. The number of state-sponsored positions in other fields of study declined by 7%, from 36,000 to 33,637. We refer to these fields of study as "*fields with little or no funding cut*."

Table 1: The availability of funded positions over time by field of study

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

*Notes*: In 2013 the government did not publish the number of available funded positions. The numbers do not include partial scholarships, which were offered in 2012 only. The capacity of partially funded positions was: 150 in agriculture, 1,500 in computer science, 2,350 in engineering, 100 in medicine, and 1,500 in natural sciences. A partial scholarship covered 50% of the tuition fee. Partial scholarships were awarded to students who were assigned an unfunded position based on merit. There was no possibility of ranking partially funded positions separately. While the number of funded positions 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 been previously offered. However, state-sponsored capacities remained scarce.[12] The "reversal" of the 2012 reform did not meaningfully increase the

[12]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.

number of state-sponsored positions in the affected fields: the number of funded positions was about 800 in business and economics, 170 in legal studies, and 1,100 in social sciences. Additionally, in 2013, the funding cut was expanded to include an additional major in the field of humanities (adult education), which we include as a treated major in our empirical analysis.

Since we will be using the 2012–2013 reform as a source of variation in our empirical analysis, we are obliged to mention other changes that occurred around the same time. First, as part of the 2012–2013 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 twice the number of years they spent in college within 20 years from graduation, or else repay the country with interest (of base rate + three percentage points). Even though the decree makes state-sponsored positions less desirable, we do not think that it changes the natural ranking of funded and unfunded contracts or that it has a substantial effect on the composition of applicants, for several reasons. First, the decree specifies numerous exemptions, including having two or more children, military service, and disability. Second, it was highly unlikely that this contract will be enforced (in twenty years). Its legal status was unclear, as it may violate the freedom of movement of workers in the EU,[13] and political pressure caused the government to significantly alleviate the terms already in 2013, such that the number of years of obligatory work in Hungary that was cut in half, and students who drop out within one semester are exempted from repayment. Third, a student who leaves Hungary and does not return for more than a decade is very likely to move to a country where she will have a much easier time earning a few thousand dollars, lowering the marginal value of money in this contingency. Fourth, if an applicant is admitted with funding, she can decide to decline the funding and still be admitted; thus, applying to a funded position provides a pure option value.

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

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

[13]See http://www.nytimes.com/2012/03/05/world/europe/hungarian-students-decry-state-sponsorship-cuts.html?ref=internationaleducation.

# 3 Data

## 3.1 Data Sources

Our analysis uses four data sources that we merged based on demographic information. The main source is an administrative dataset that contains information about the bachelor's degree admissions process between 2009 and 2013 in Hungary.[14] This dataset includes the final allocation in each year. In particular, we observe each applicant's complete ROL and program-specific priority scores,[15] 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 includes all information required to (re)calculate the applicant's priority score in each program she applied to. This includes grades in various subjects in the final two years of high school (11th grade and 12th grades), performance in the matriculation exams, and the number of points the applicant received for claiming a disadvantaged background.[16]

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

The NABC data also includes 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

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

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

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

NABC-based SES index combines three subindices: the first is a subindex of parental education, the second is a subindex of home possessions (such as number of bedrooms, mobile phones, cars, computers, books, etc.), and the third is a subindex of the labor market status of the parents. The NABC-based SES index resembles the economic, social and cultural status (ESCS) indicator of the OECD PISA survey.

We use information on microregional-level annual unemployment rates published by the National Employment Service in 2008, one year before the start of our sample period.[17] The territorial breakdown consists of 174 units. We also use the T-star dataset of the Hungarian Central Statistics Office 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 2013.

### 3.2 Sample Definition

An ROL is an ordered list of *contracts*, program-funding pairs. An applicant makes an obvious mistake if she ranks an unfunded contract in some program higher than a funded contract in the same program (obvious flipping), or, if she ranks only an unfunded contract in a program that offers a funded contract (obvious dropping). When we examine correlations between applicants' characteristics and obvious mistakes, we treat each ROL as a single observation (Section 4). By contrast, when we analyze the effect of program selectivity on obvious mistakes (Section 5), we treat each *application* – a program in an ROL, up to two contracts – as a single observation.

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

We often restrict our full sample to ROLs submitted by applicants who, at the time, were younger than 22 and had completed their matriculation exam in the same year. We refer to this sample as the *high-school senior applicant sample*. The reason for the restriction is twofold. First, this is the subsample that we are able to match to the NABC database. And second, in this subsample we are certain that applicants did not exhaust their 12 funded semesters, but just chose not to de-

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

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

clare their ineligibility (without ranking any funded contract).[19] The high-school senior applicant sample comprises 228,606 ROLs. These restrictions ensure with a high degree of certainty that the obvious mistakes we identify are indeed mistakes, and are not the result of misclassification. However, the inclusion criteria of this sample are conservative, and likely exclude many eligible students, especially weaker applicants, who may be more prone to mistakes according to previous studies. Finally, we sometimes refer to the subsample of *relevant ROLs*. These are ROLs that include at least one unfunded contract in a program that also offers a funded contract. Relevant ROLs are the only lists in which our methodology can potentially detect mistakes.

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

## 3.3 Summary Statistics

Table 2 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% being female. A significant majority (63%) of the applicants attended secondary grammar schools, whose declared purpose is to prepare them for higher education. Approximately 19% of the applicants lived in Budapest, 10% lived in one of the 18 county capitals, 32% resided in towns, and the remainder lived in villages. About 7% of the applicants claimed points for disadvantaged status. Applicants' GPAs were 3.75 in the 11th grade and 3.71 in the 12th grade on average.[20] The average ROL length was 3.81 contracts, which corresponds to 2.91 programs.

Applicants' characteristics in the high-school senior applicant sample are largely similar to those in the eligible sample. The main differences are that high-school senior applicants are younger (by construction), and academically stronger (as one would expect). As we discussed

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

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

in the previous subsection, we are able to match the NABC only for the high-school senior applicant sample. The NABC variables, such as the numeracy skill, literacy skill, and the NABC-based SES index are standardized within cohort in the general population, which includes both applicants and non-applicants. On average, high-school senior applicants had 0.59 (0.63) standard deviation higher 10th grade numeracy (literacy) skill than the general population. Similarly, high-school senior applicants' average NABC-based SES index is 0.49, indicating that they come from a higher-than-average socioeconomic background.

Table 2: Individual-level summary statistics

| | Eligible applicants | | | High-school senior applicants | | |
|---|---|---|---|---|---|---|
| | Mean | Std. dev. | N | Mean | Std. dev. | N |
| Female | 0.55 | 0.497 | 447,989 | 0.57 | 0.496 | 228,606 |
| Age | 21.86 | 5.436 | 447,989 | 19.04 | 0.683 | 228,606 |
| High school | | | | | | |
| - secondary grammar school | 0.63 | 0.484 | 447,989 | 0.70 | 0.460 | 228,606 |
| - vocational school | 0.33 | 0.469 | 447,989 | 0.27 | 0.443 | 228,606 |
| Residence | | | | | | |
| - capital | 0.19 | 0.390 | 447,989 | 0.16 | 0.371 | 228,606 |
| - county capital | 0.20 | 0.399 | 447,989 | 0.20 | 0.400 | 228,606 |
| - town | 0.32 | 0.467 | 447,989 | 0.33 | 0.471 | 228,606 |
| - village | 0.29 | 0.455 | 447,989 | 0.30 | 0.460 | 228,606 |
| Grade 11 GPA | 3.75 | 0.840 | 314,531 | 3.97 | 0.790 | 160,527 |
| Grade 11 GPA - missing | 0.30 | 0.457 | 447,989 | 0.30 | 0.457 | 228,606 |
| Grade 12 GPA | 3.71 | 0.840 | 328,832 | 3.87 | 0.814 | 174,699 |
| Grade 12 GPA - missing | 0.27 | 0.442 | 447,989 | 0.24 | 0.425 | 228,606 |
| NABC numeracy skill | - | - | - | 0.59 | 0.862 | 148,627 |
| NABC numeracy skill - missing | - | - | - | 0.35 | 0.477 | 228,606 |
| NABC literacy skill | - | - | - | 0.63 | 0.742 | 148,638 |
| NABC literacy skill - missing | - | - | - | 0.35 | 0.477 | 228,606 |
| NABC-based SES index | - | - | - | 0.49 | 0.848 | 134,065 |
| NABC-based SES index - missing | - | - | - | 0.41 | 0.492 | 228,606 |
| Disadvantaged status | 0.07 | 0.256 | 447,989 | 0.10 | 0.301 | 228,606 |
| Unemployment rate in 2008 (%) | 7.70 | 4.434 | 438,297 | 7.88 | 4.526 | 222,954 |
| Unemployment rate in 2008 - missing | 0.02 | 0.145 | 447,989 | 0.02 | 0.155 | 228,606 |
| Gross annual per capita income (1000 EUR) | 6.34 | 1.541 | 438,449 | 6.27 | 1.525 | 223,045 |
| Gross annual per capita income - missing | 0.02 | 0.144 | 447,989 | 0.02 | 0.154 | 228,606 |
| # of contracts on the ROL | 3.81 | 2.062 | 447,989 | 4.37 | 2.243 | 228,606 |
| # of programs on the ROL | 2.91 | 1.208 | 447,989 | 3.28 | 1.203 | 228,606 |

*Notes*: Disadvantaged status is an indicator for claiming points for disadvantaged status. GPA is the average of Hungarian grammar and literature, mathematics, and history. Grades are on a scale of 1–5. The unemployment rate in 2008 is measured on the microregional-level. Gross annual per capita income is measured on the settlement-level, i.e., where the student lives. The number of contracts on the ROL is reported administratively, whereas we calculate the number of programs based on the contracts observed in the dataset (see footnote 16).

Table 3 presents the distribution of the ROLs by the type of contracts they include. In the eligible sample, almost 60% of ROLs include only funded contracts, 7% include only unfunded contracts, and the rest include both funded and unfunded contracts. High-school senior applicants'

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

Table 3: The distribution of ROLs by the funding type they include

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

*Notes*: This table presents the distribution of the ROLs according to funding structure. Columns (1) and (3) display frequencies and columns (2) and (4) show the distribution.

Figure 1 presents the distribution of the applications by field of study over time. The most popular fields of study were business and economics, engineering, and humanities for both the eligible and high-school senior applicant samples. The distribution of the fields of study was relatively stable over time.[21] In 2013, applications to fields of study that suffered a severe cut in funding in 2012 and 2013 (business and economics, legal studies, and social sciences) comprised 23% of all applications in the eligible applicant sample and 24% of all applications in the high-school senior applicant sample in 2013.

# 4 Obvious Mistakes: Prevalence and Correlates

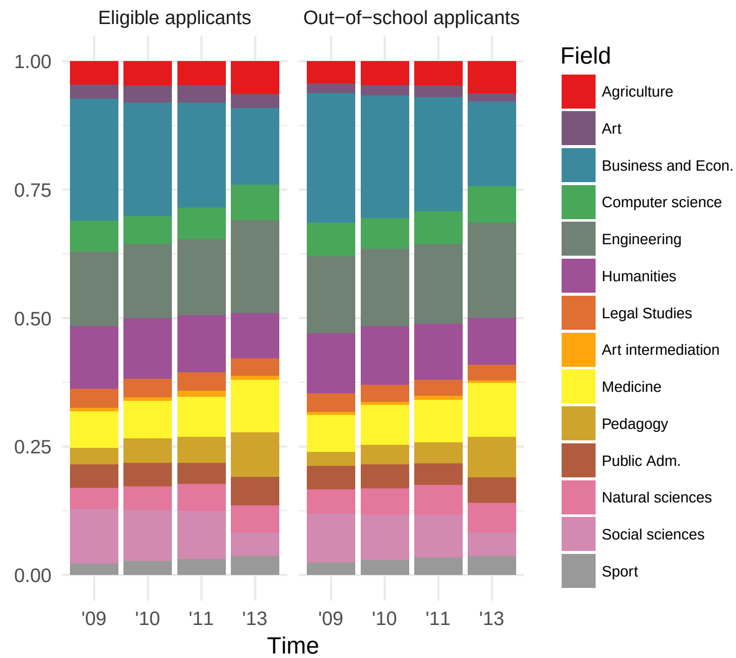
In this section, we study the prevalence and correlates of obvious mistakes. We start, in Section 4.1, by quantifying the share of ROLs with obvious mistakes and showing that a large fraction of these mistakes were costly ex post. In Section 4.2, we examine the socioeconomic and demographic characteristics of applicants who made obvious mistakes. Finally, in Section 4.3, we document a negative correlation between various measures of academic ability and obvious mistakes.

## 4.1 The Prevalence and Costs of Obvious Mistakes

Table 4 quantifies the share of ROLs that exhibit obvious mistakes. In the eligible sample, the fraction of obvious mistakes ranges from 8.7% in 2009 to 14.5% in 2013. During the sample period almost 50,000 applicants, corresponding to 10.9% of the ROLs, made an obvious mistake, mostly

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

Figure 1: Distribution of applications by field of study




*Notes*: The figure presents the distribution of the applications categorized by field of study over time. Each observation corresponds to a program in a given ROL. The figure does not display the year 2012, since the reform eliminated the availability of funding in some programs in this year (see Section 2.3) and we exclude this year when we analyze the causal effect of program selectivity on obvious mistakes.

obvious dropping. Obvious mistakes are less prevalent among high-school seniors, but, still, the share of mistakes increased from 3.1% in 2009 to 10.8% in 2013. Overall, 5.3% of the high-school seniors made an obvious mistake in the same period.[22] It is important to note that obvious mistakes can only be detected in ROLs that rank at least one unfunded contract. In the eligible applicant sample the share of such ROLs is 40% (see Table 3). Table 4 should be interpreted in this context. For example, 10.9% of ROLs with obvious mistakes in the eligible applicant sample represent 27.1% (= 10.9%/0.4) of ROLs in the sample in which a mistake could be detected.

Obvious mistakes correspond to weakly dominated strategies. Rational players only use dominated strategies if they assign probability 0 to the event that a dominating strategy does strictly better. Table 4 assesses the cost of obvious mistakes by calculating the share of obvious mistakes that are binding 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 4 demonstrates that obvious mistakes may have hurt up to 18.6% of the eligible applicants and up to 10.0% of the high-school senior applicants (columns 3 and 6). At least 12.2% of the applicants who made obvious mistakes could have received a tuition and fee waiver (column 2). Similarly, among the high-school-senior applicants at least 4.5% of those who obviously dropped or flipped could have gotten a tuition and fee waiver in the program they were eventually assigned to (column 5). The relative importance of funding, reflected in the ranking of the majority of students, suggests that the upper bound may be more indicative of the true cost.

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

## 4.2 Demographics and Obvious Mistakes

This subsection examines the characteristics of applicants who made obvious mistakes. We regress an indicator for obvious mistakes on individual-level demographic variables and year fixed effects using the high-school senior applicant sample. It is important to note that these regressions pro-

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

Table 4: Obvious mistakes over time

| Year | Eligible applicants | | | High-school senior applicants | | |
|---|---|---|---|---|---|---|
| | Obvious mistakes | Ex-post costly mistakes | | Obvious mistakes | Ex-post costly mistakes | |
| | | Lower bound | Upper bound | | Lower bound | Upper bound |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 2009 | 8.7% (8,555) | 12.4 (1,062) | 20.3% (1,733) | 3.1% (1,566) | 2.2% (35) | 7.6% (119) |
| 2010 | 9.4% (9,818) | 10.6% (1,044) | 15.8% (1,556) | 3.2% (1,596) | 1.4% (23) | 4.1% (65) |
| 2011 | 12.2% (12,615) | 9.4% (1,183) | 14.2% (1,797) | 4.6% (2,268) | 1.4% (31) | 4.9% (112) |
| 2012 | 10.4% (7,452) | 12.6% (937) | 19.9% (1,482) | 6.3% (2,494) | 4.0% (101) | 10.5% (261) |
| 2013 | 14.5% (10,209) | 16.6% (1,698) | 24.2% (2,468) | 10.8% (4,202) | 8.4% (355) | 15.7% (660) |
| Total | 10.9% (48,649) | 12.2% (5,924) | 18.6% (9,036) | 5.3% (12,126) | 4.5% (545) | 10.0% (1,217) |

*Notes*: Columns (1) and (4) show the share (number) of ROLs that exhibit obvious mistakes over time. Columns (2) and (5) present the share (number) of ROLs with obvious mistakes, where the applicant was assigned to the unfunded version of a program in which he met the priority-score cutoff for the funded version. Columns (3) and (6) show the share (number) of ROLs with obvious mistakes, where the applicant met the priority-score cutoff of the funded version.

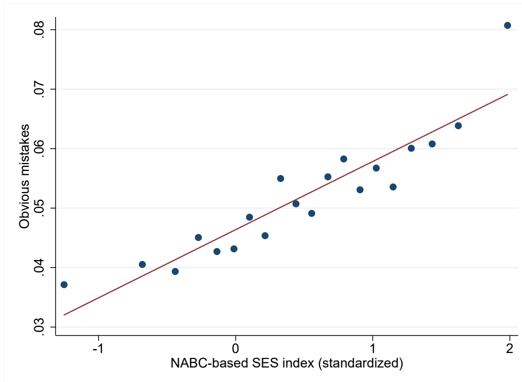
vide descriptive evidence on the characteristics of applicants who submitted ROLs with obvious mistakes, but we cannot attribute a causal interpretation to the estimated coefficients.

We investigate the role of socioeconomic status (SES) by correlating its various proxies with obvious mistakes. Figure 2 presents the results. Panel (a) establishes a positive relationship between the NABC-based SES index and obvious mistakes. Panel (c) shows that applicants from settlements with higher gross annual per capita income were more likely to err. Panel (e) demonstrates a negative correlation between microregional-level unemployment rate and obvious mistakes. Appendix Table B1 shows that obvious mistakes were less frequent among applicants who claimed admissions points for coming from a disadvantaged background.

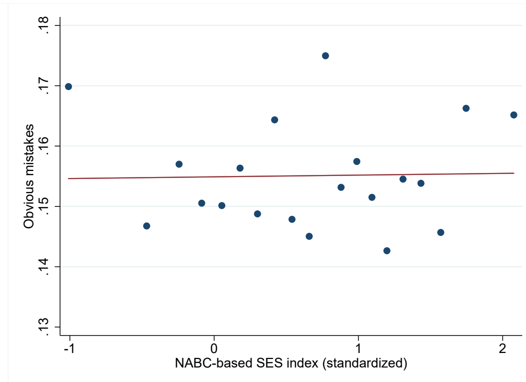
Applicants with lower socioeconomic status were less likely to apply for unfunded positions, a necessary condition for detecting an obvious mistake. We argue that this channel does not drive the positive relationship between proxies of SES and obvious mistakes. Panels (d) and (f) show that even among students who ranked at least one unfunded contract, those coming from high-income or low-unemployment areas were more likely to make mistakes. Similar findings apply to those who claimed admission points for coming from a disadvantaged background (Appendix Table B1). The exception is Panel (b), where we find no significant correlation between the NABC-based SES index and obvious mistakes in the relevant sample. In Section 4.3, we demonstrate that the positive relationship reemerges once we control for academic achievement.

We also find that older applicants were more likely to make mistakes, and that female applicants were 1.1–1.2 percentage points (21–23%) more likely to make an obvious mistake (Appendix Table B1). Additionally, the fraction of obvious mistakes was increasing in the size of the settlement in which the applicants resided. Finally, students who attended secondary vocational schools were 0.8–1.3 percentage points more likely to make a mistake relative to their peers in

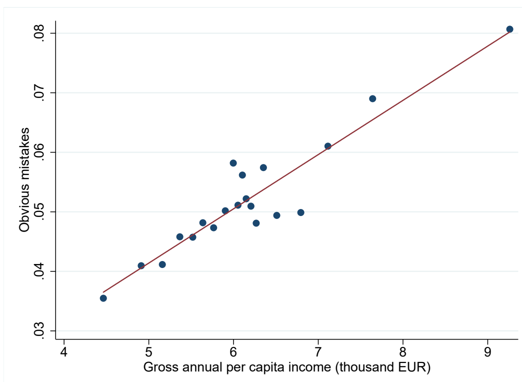
Figure 2: Socioeconomic status and obvious mistakes



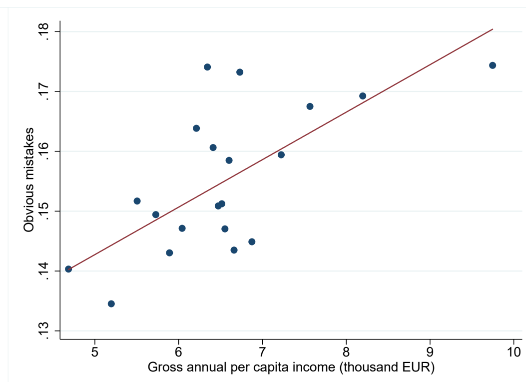

(a) NABC-based SES index



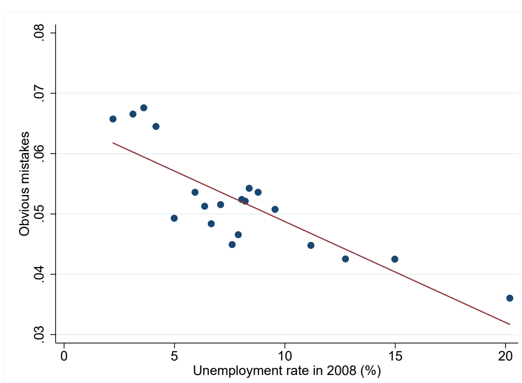

(b) NABC-based SES index



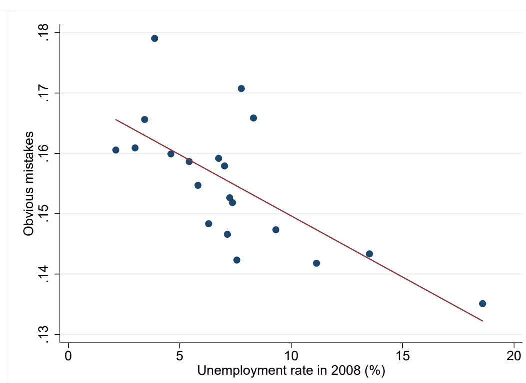

(c) Gross annual per capita income




(d) Gross annual per capita income




(e) Unemployment rate in 2008




(f) Unemployment rate in 2008

*Notes*: The figure displays the relationship between obvious mistakes and various measures of socioeconomic status, such as the NABC-based SES index (Panels (a) and (b)), gross annual per capita income measured on the settlement level (Panels (c) and (d)), and microregional-level unemployment rate in 2008 (Panels (e) and (f)). Panels in the left column focus on the high-school senior applicant sample, whereas panels in the right column are restricted to the relevant high-school senior applicants sample. Each dot (bin-specific mean) and the fitted regression line are conditional on year fixed effects and demographics, including gender, age, high-school type, and type of residence. Appendix Table B1 displays the corresponding regression coefficients.

secondary grammar schools.[23]

### 4.3 Academic Achievement and Obvious Mistakes

We next investigate whether academic achievement is correlated with obvious mistakes. We address this question by regressing an indicator for obvious mistakes on academic achievement, conditional on the NABC-based SES index, demographic controls, and year fixed effects.

First, we examine the 10th grade NABC numeracy and literacy skills. These scores are normalized to have zero-mean and a standard deviation of one in the general population, which includes both applicants and non-applicants to undergraduate education. Figure 3 and Appendix Table B2 present our results. Panel (a) establishes a strong negative correlation between numeracy skill and obvious mistakes. A one standard deviation increase in the numeracy skill is associated with a 2.2 percentage points decline in the probability of making obvious mistakes. Similarly, Panel (b) shows that the literacy skill and obvious mistakes are negatively related. A one standard deviation increase in the literacy skill is associated with a drop of 1.9 percentage points in the likelihood of making obvious mistakes.

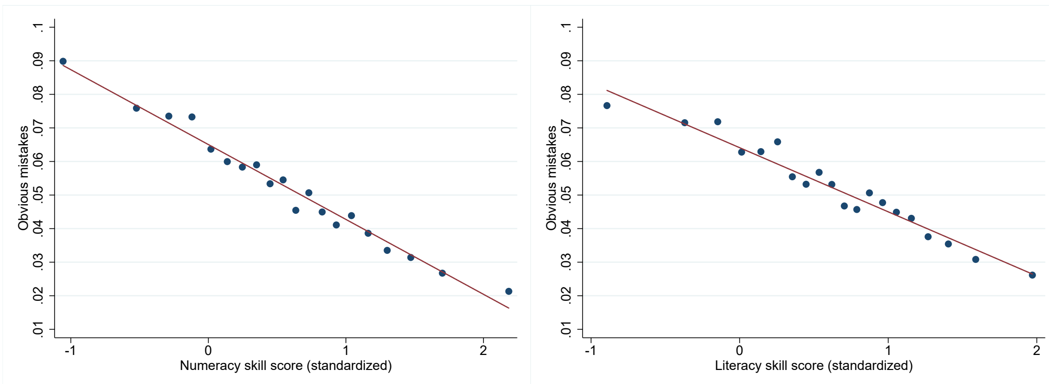
Next, we investigate the relationship between obvious mistakes and other measures of academic ability, such as 11th- and 12th-grade GPAs. GPA is related to applicants' priority directly, since it enters the priority score formula and account for 20% of the priority score.

Panel (c) illustrates the strong negative relationship between obvious mistakes and applicants' 11th-grade GPA, even conditional on numeracy and literacy skills. A one mark (corresponding to 0.79 standard deviation) increase in the 11th-grade GPA is associated with a 2.7 percentage points decline in the probability of obvious mistakes. Panel (d) demonstrates a similar result for 12th-grade GPA. Appendix Table B2 shows that, even conditional on academic achievement, female applicants and students with higher socioeconomic status are more likely to make an obvious mistake. Appendix Table B3 demonstrates that all our results hold if we focus on high-school senior applicants who ranked at least one unfunded contract in their ROL. In particular, we find that the NABC-based SES index is positively correlated with obvious mistakes conditional on academic achievement.

We investigate whether the descriptive evidence on demographics and academic achievement hold before and after the 2012–2013 reform separately. Figures A1 and A2 in the Appendix demonstrate that the positive correlation between socioeconomic status and obvious mistakes, and the negative correlation between academic achievement and obvious mistakes hold both in the pre- and post-reform periods, (2009–2011, and 2012–2013, respectively).

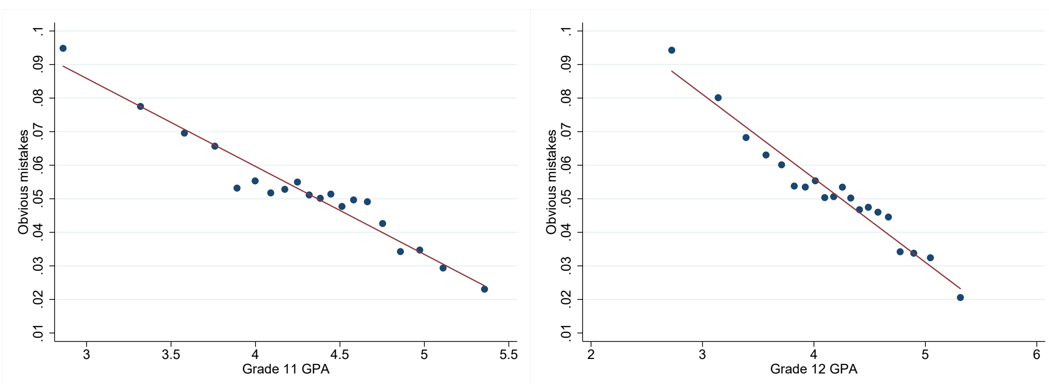
[23]These findings hold qualitatively in the relevant subsample of high-school senior applicants.

Figure 3: Academic achievement and obvious mistakes




(a) Numeracy skill in grade 10

(b) Literacy skill in grade 10




(c) 11th-grade GPA

(d) 12th-grade GPA

*Notes*: The figure displays the relationship between obvious mistakes and various measures of academic achievement: the numeracy skill in grade 10 (Panel (a)), literacy skill in grade 10 (Panel (b)), 11th-grade GPA (Panel (c)), and 12th-grade GPA (Panel (d)) in the high-school senior applicants sample. Each dot (bin-specific mean) and the fitted regression line is conditional on year fixed effects, the NABC-based SES index, and demographics (including gender, age, high-school type, and type of residence). Numeracy skill, literacy skill, and socioeconomic status are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and postal code). Table B2 displays the corresponding regression coefficients. Panel (a) corresponds to column 1, slope coefficient: -0.022*** (s.e.: 0.0008). Panel (b) refers to column 2, slope coefficient: -0.019*** (s.e.: 0.0009). Panels (c) and (d) correspond to columns 4 and 5; thus, the regression line is conditional on the 10th-grade numeracy and literacy skills as well. The slope of 11th-grade GPA is -0.027*** (s.e.: 0.0010), the slope of 12th-grade GPA is -0.026*** (s.e.: 0.0010). The 11th-grade (12) GPA is missing for 28.5% (22.6 %) of the sample.

# 5 The Effect of Selectivity on Obvious Mistakes

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

## 5.1 Selectivity and Obvious Mistakes

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

We measure selectivity by the realized priority-score cutoff to the funded program one year prior the application. For ease of comparison, we abstract from the fact that different fields of study use different weighting schemes, and we normalize the priority-score cutoffs to within-year percentile ranks.[24] Figure 4 presents the relationship between program selectivity and obvious mistakes. Panel (a) demonstrates that, conditional on appearing in an ROL, obvious mistakes are more likely to occur in applications to more selective programs. Specifically, obvious mistakes are more likely to occur in applications to programs in the top quintile of competitiveness are five times more likely than in applications with respect to programs in the bottom quintile.

We cannot attribute a causal interpretation to the results depicted in Figure 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 selectivity on obvious mistakes. Second, programs differ along more dimensions than just selectivity (e.g., content, location, and prestige), which confounds the positive relationship between selectivity and obvious mistakes.

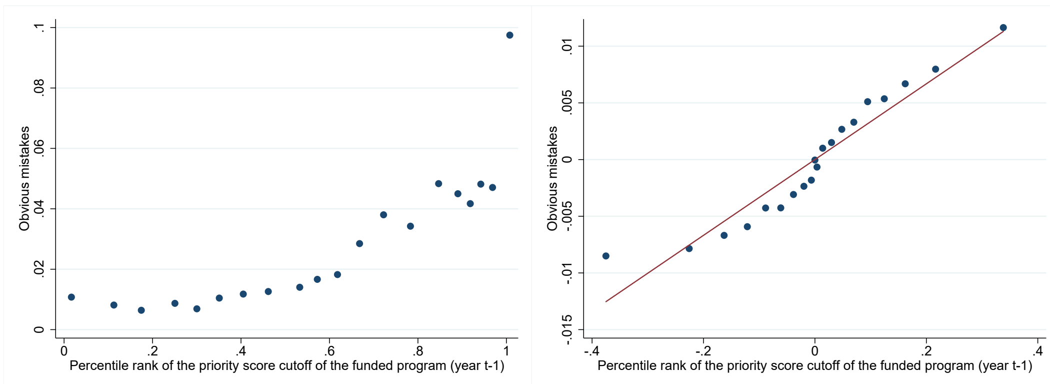
We address sorting by adding ROL-level fixed effects, thus exploiting only within-ROL variation in program selectivity.[25] We find that a 10-percentile ranks increase in program selectivity

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

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

is associated with a 0.3 percentage points rise in obvious mistakes (Figure 4 (b)). We identify this slope from ROLs that include programs with distinct historical program selectivity. However, within-applicant variation in program selectivity might be too narrow to identify the full effect of program selectivity. In the rest of this section, we address the causal effect of increased selectivity on obvious mistakes, both on the extensive and on the intensive margin.

Figure 4: Selectivity and obvious mistakes




(a) Unconditional (b) Conditional on ROL-fixed effects

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

## 5.2 Empirical Strategy

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

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

The variable $Y_{its}$ is an indicator for obvious mistakes in applicant $i$'s ranking of program $s$ in year[26] $t$. The variable $T_{ts}$ is an indicator that equals one if $t$ is equal to 2013 and $s$ is a program that was affected by the severe funding reduction of the 2013 reform, and zero otherwise. The model

[26] Program $s$ is a tuple that consists of the major, institution and the schedule (part time vs. full time).

includes program fixed effects ($\eta_s$), year fixed effects ($\nu_t$), a vector of individual-specific controls ($X_{it}$), and an error term ($\varepsilon_{its}$). Our parameter of interest is $\beta$, which measures the effect of the funding cuts, which we interpret as a rise in the selectivity of the affected programs, on obvious mistakes. Based on the laboratory findings of Hassidim et al. (2016) and the evidence presented in the previous subsection, we expect the estimate of $\beta$ to be positive. 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.

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

## 5.3 Main Results

Table 5 presents our difference-in-differences estimates for the effect of selectivity on obvious mistakes. Our baseline specification (Column 1) indicates that the 2012–2013 reform increased obvious mistakes by 18.5 percentage points among treated programs from a baseline of 12.4 percent.[27] The estimated coefficient for the high-school senior applicant sample (column 2) is similar in size (19.3 percentage points); however, the relative magnitude is substantially larger, as high-school senior applicants made obvious mistakes less frequently (6.3 percent). Columns 3–6 show that controlling for demographics and academic achievement barely changes the estimates and their precision.[28] Appendix Table B4 shows that the effect holds for both obvious flipping and obvious dropping, but the magnitude for obvious dropping is much larger, both in absolute and in relative terms.

To put our estimates in context, it is instructive to examine the impact of the reform on the priority-score cutoffs of the funded programs. The percentile ranks increased for 88% of the treated programs, with an average change of almost 9 percentile ranks. The reduction in the number of funded positions in the directly affected fields made the system as a whole more selective through general equilibrium effects. If students who applied to fields that were not affected directly took

[27]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 ($\hat{\nu}_{2013} - \hat{\nu}_{2011}$). The estimated year effect for the eligible sample is 2.8 percentage points, and it is 0.8 percentage points for the high-school senior applicant sample.

[28]In columns 5 and 6 of Table 5 we control for numeracy skill and the NABC-based SES index. In these specifications we account for missing NABC by including dummy variables. Appendix Table B5 demonstrates that focusing on subsamples where the NABC is non-missing (as in Appendix Table B2 and B3) does not change our results.

these general equilibrium effects into account when submitting their application, then our estimates should provide lower bounds on the causal effect of selectivity on obvious mistakes.

Table 5: The effect of selectivity on obvious mistakes

| Dependent variable | Obvious mistakes | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Severe funding cut | 0.185*** | 0.193*** | 0.187*** | 0.186*** | 0.187*** | 0.187*** |
| | (0.0036) | (0.0043) | (0.0043) | (0.0042) | (0.0043) | (0.0042) |
| Mean outcome | 0.069 | 0.032 | 0.032 | 0.032 | 0.032 | 0.032 |
| R-squared | 0.125 | 0.096 | 0.109 | 0.121 | 0.111 | 0.111 |
| # Obs. | 1,040,686 | 607,764 | 607,764 | 607,764 | 607,764 | 607,764 |
| # ROLs | 376,106 | 188,696 | 188,696 | 188,696 | 188,696 | 188,696 |
| Demographic controls & GPA | - | - | ✓ | ✓ | ✓ | ✓ |
| High school FE | - | - | - | ✓ | - | - |
| NABC controls | - | - | - | - | ✓ | ✓ |
| NABC-based SES index | - | - | - | - | - | ✓ |
| Eligible applicants | ✓ | - | - | - | - | - |
| High-school senior applicants | - | ✓ | ✓ | ✓ | ✓ | ✓ |

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

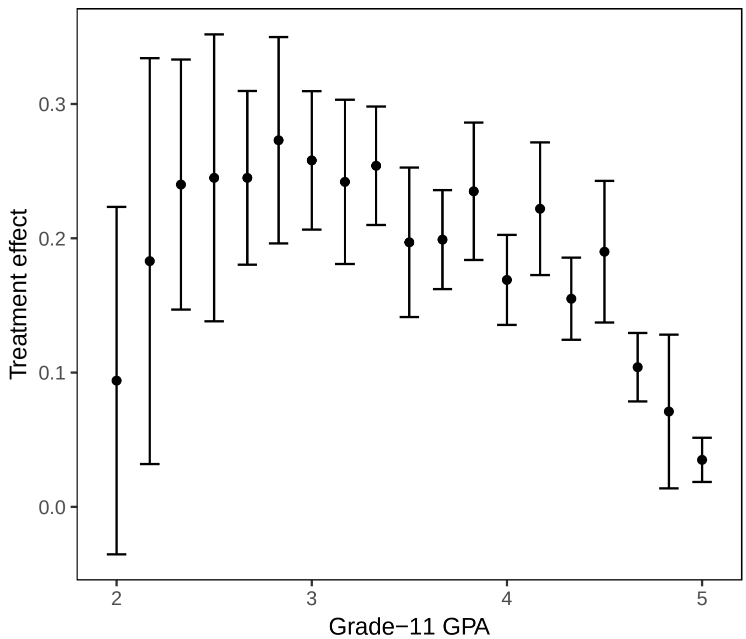
**Heterogeneity.** In Table 6 we examine whether the effect of selectivity on obvious mistakes is homogeneous across various subgroups. The corresponding regressions include interactions of treatment and subgroup dummies, and controls for demographics and academic achievement (as in column 3 of Table 5). We find that the effect of selectivity on obvious mistakes is 3 percentage points lower for female applicants. The causal effect of selectivity is lower for disadvantaged applicants, measured by claiming points for disadvantaged status or by the NABC-based SES index. The effect of the reform is declining with numeracy skill and with academic achievement, measured by 11th-grade GPA (Figure 5). This suggests that applicants for whom mistakes caused a higher expected utility loss were less responsive to increases in selectivity. We find that applicants to the fields of social sciences, humanities, and legal studies responded in a similar way, whereas the effect of selectivity on obvious mistakes was the strongest in the field of business and economics, where the availability of funded positions changed the most.

Table 6: Heterogeneous effects

| Dependent variable | Obvious mistakes | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Severe funding cut | 0.187*** | 0.204*** | 0.195*** | 0.199*** | 0.197*** | 0.151*** |
| | (0.0043) | (0.0073) | (0.0045) | (0.0079) | (0.0094) | (0.0106) |
| Severe funding cut × Female | | -0.026*** | | | | |
| | | (0.0088) | | | | |
| Severe funding cut × Disadvantaged | | | -0.104*** | | | |
| | | | (0.0121) | | | |
| Severe funding cut × NABC-based SES Q1 | | | | -0.040*** | | |
| | | | | (0.0138) | | |
| Severe funding cut × NABC-based SES Q2 | | | | -0.039*** | | |
| | | | | (0.0130) | | |
| Severe funding cut × NABC-based SES Q3 | | | | -0.034*** | | |
| | | | | (0.0127) | | |
| Severe funding cut × NABC-based SES Q4 | | | | 0.002 | | |
| | | | | (0.0132) | | |
| Severe funding cut × NABC-based SES Q5 | | | | 0.012 | | |
| | | | | (0.0133) | | |
| Severe funding cut × NABC numeracy Q1 | | | | | 0.039** | |
| | | | | | (0.0142) | |
| Severe funding cut × NABC numeracy Q2 | | | | | 0.005 | |
| | | | | | (0.0138) | |
| Severe funding cut × NABC numeracy Q3 | | | | | -0.002 | |
| | | | | | (0.0138) | |
| Severe funding cut × NABC numeracy Q4 | | | | | -0.019 | |
| | | | | | (0.0137) | |
| Severe funding cut × NABC numeracy Q5 | | | | | -0.087*** | |
| | | | | | (0.0134) | |
| Severe funding cut × Business/economics | | | | | | 0.044*** |
| | | | | | | (0.0115) |
| Severe funding cut × Humanities | | | | | | -0.027 |
| | | | | | | (0.0250) |
| Severe funding cut × Legal studies | | | | | | 0.027* |
| | | | | | | (0.0147) |
| R-squared | 0.109 | 0.109 | 0.110 | 0.110 | 0.112 | 0.109 |

*Notes*: The table presents the DiD estimates by various subgroups of the high-school senior applicant sample. Each column estimates the coefficients in a single regression by interacting the treatment variable with subgroup indicators. Robust standard errors clustered on the applicant level are in parenthesis. The number of observations is 607,764, which correspond to 188,696 ROLs. The mean outcome in the sample is 0.032. 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. In column (4), the baseline category corresponds to the missing values of the NABC-based SES index. In column (5), the baseline category corresponds to the missing values of the numeracy test of the NABC. In column (6), the baseline category corresponds to social sciences.
***: p<0.01 **: p<0.05, *: p<0.1.

Figure 5: The effect of selectivity on obvious mistakes by academic achievement




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

## 5.4 Threats to Identification and Robustness

We assess the plausibility of our identifying assumptions in various ways. To test the parallel trends assumption we include placebo variables of the treated programs in the pre-reform period; i.e., we compare 2009 (Placebo 2009) and 2010 (Placebo 2010) to 2011. Column 1 of Table 7 adds these placebo treatment variables to the baseline model. Although the placebo coefficients for 2009 and 2010 are statistically significant, they are an order of magnitude lower than our main estimates and precisely estimated. Thus, the potential for bias due to the violation of the common trends assumption is small.

We also study a smaller scale reform that took place in 2011, prior to the introduction of the study contract. This reform, which received much less attention from the media and the public, decreased the number of tuition waivers in business/economics and social sciences by about 20% (Table 1). We investigate whether this reform had a similar impact on obvious mistakes. We add indicator variables to our main specification that take the value of one in 2011 for social sciences and business/economics, respectively. Appendix Table B6 presents the results. We find that this smaller reform increased obvious mistakes by 1.1–1.3 percentage points in the affected fields.

We demonstrate the robustness of our results by considering an alternative specification that leverages all variation in the number of funded positions during our sample period (2009–2011, 2013). Appendix B.3 describes the model specification and presents the corresponding estimates. We find that a 10 percent reduction in the number of funded positions increases obvious mistakes by 8.2–8.8 percentage points in the high-school senior applicant sample.

A potential threat to our identification strategy is that treatment status is defined by applicants' ROL. Applicants' response to changes in selectivity may affect the composition of their ROL as well as their decision to apply. This concern is particularly pronounced for students who are not willing (or able) to pay the tuition fee and consider applying only to funded programs. As a response to the reduction in funded positions, these applicants might drop their most preferred (treated) program from their ROL and rank untreated programs instead, biasing our estimates upward. We address this concern in several ways. First, in columns 3–6 of Table 5 we add individual-level controls. Second, we look at applicants who listed at least one unfunded contract in their ROL. By listing at least one unfunded contract, these applicants signal that they are willing to pay tuition; hence we find it less plausible that the reform affected the set of programs in their ROL.[29] Reassuringly, our estimates for this subsample are very similar to the main estimates (columns 2 and 3 of Table 7), indicating that switching behavior does not drive our results.[30]

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

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

Table 7: Robustness analysis

| Dependent variable | Obvious mistakes | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Severe funding cut | 0.181*** | 0.161*** | 0.153*** |
| | (0.0046) | (0.0056) | (0.0056) |
| Placebo (2009) | -0.018*** | | |
| | (0.0021) | | |
| Placebo (2010) | -0.019*** | | |
| | (0.0021) | | |
| R-squared | 0.098 | 0.091 | 0.112 |
| # Obs. | 607,764 | 174,182 | 174,182 |
| # ROLs | 188,696 | 57,362 | 57,362 |
| Demographic controls & GPA | - | - | ✓ |

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

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

# 6 Obvious Mistakes: The Impact on Other Applicants

Obvious mistakes are detrimental to the utility of the applicants who make them. But, applicants' ROLs also influence 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 thanks to 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.

If anything, it would bias the estimates downward.

Since we do not have access to the exact algorithm that is used to allocate applicants to schools, and since some parameters are impossible to deduce from the data (e.g., how counterfactual ties are dealt with, or how funding is reallocated between programs), we make a few simplifying assumptions in our analysis. Essentially, we assume that each program has a fixed number of funded positions, and we break ties at random. These assumptions reflect the way more standard matching markets function, and presumably have a limited effect on our results. We concentrate on mistakes that are certainly costly, i.e., cases where the applicant could have been admitted to the same program, but with funding. This approach is conservative and keeps the analysis simple as at most one applicant is directly affected. We further restrict the population to those applicants who reported having never attended college before. This restriction minimizes the risk of misclassification of strategic decisions as costly mistakes.[31]

We proceed by correcting all obvious mistakes in each program. We then track the implications for the applicants that are 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 1,623 ROLs with an obvious mistake that meet the criteria mentioned above. We find that 597 students, corresponding to 37% of the mistakes, were admitted to college thanks only to others' mistakes. An additional 1,026 students received an assignment they ranked higher due to others' mistakes, of whom 512 would otherwise have been unfunded (typically in the same program). Table 8 compares students with costly mistakes to those who gained from them directly. The immediate effect of a costly mistake is to allocate funding from high to low socioeconomic status applicants.

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

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

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

Table 8: The distributional effect of costly obvious mistakes

| | Directly affected applicants | | | Students with costly mistakes | | | |
|---|---|---|---|---|---|---|---|
| | Mean | Std. dev. | N | Mean | Std. dev. | N | Diff. ((4) - (1)) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| High-school senior | 0.37 | 0.484 | 1,623 | 0.30 | 0.457 | 1,623 | -0.076*** |
| Age | 24.23 | 7.207 | 1,623 | 26.32 | 7.809 | 1,623 | 2.097*** |
| Disadvantaged | 0.04 | 0.189 | 1,623 | 0.02 | 0.147 | 1,623 | -0.015*** |
| Unemployment rate in 2008 (%) | 7.46 | 4.373 | 1,623 | 6.67 | 3.942 | 1,623 | -0.660*** |
| Unemployment rate in 2008 - missing | 0.01 | 0.121 | 1,623 | 0.03 | 0.183 | 1,623 | 0.020*** |
| Gross annual per capita inc. (1000 USD) | 6.46 | 1.540 | 1,601 | 6.79 | 1.538 | 1,567 | 0.327*** |
| Gross annual per capita inc. - missing | 0.01 | 0.116 | 1,623 | 0.03 | 0.183 | 1,623 | 0.021*** |
| 11th-grade GPA | 3.49 | 0.787 | 1,131 | 3.61 | 0.811 | 1,059 | 0.129*** |
| 11th-grade GPA - missing | 0.30 | 0.460 | 1,623 | 0.35 | 0.476 | 1,623 | 0.044*** |
| 12th-grade GPA | 3.46 | 0.785 | 1,182 | 3.59 | 0.776 | 1,082 | 0.128*** |
| 12th-grade GPA - missing | 0.27 | 0.445 | 1,623 | 0.33 | 0.472 | 1,623 | 0.062*** |
| Female | 0.50 | 0.500 | 1,623 | 0.46 | 0.498 | 1,623 | -0.043** |
| High school = sec. grammar school | 0.57 | 0.495 | 1,623 | 0.54 | 0.498 | 1,623 | -0.028 |
| Residence = capital | 0.20 | 0.400 | 1,623 | 0.24 | 0.429 | 1,623 | 0.043*** |

*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 (7) shows the difference in background characteristics between applicants with costly mistakes and directly affected students, conditional on year fixed effects. For a discussion on the missing GPA values see footnote 20.
***: p<0.1, **:p<0.05, *:p<0.1.

# 7 Discussion

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

Submitting an ROL that is inconsistent with the applicant's true preferences is only weakly dominated. In particular, if an applicant assigns zero probability to the event that she will be admitted to a more-preferred alternative, she is indifferent between truthful reporting and making an obvious mistake,[32] and if the probability of admission is very low she is nearly indifferent. Our findings are consistent with such behavior. First, we showed that increased selectivity 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, were more likely to make an obvious mistake. The large fraction of ex-post costly mistakes indicates that the presence of overly pessimistic beliefs is

[32] Chen and Pereyra (2017) refer to such behavior as "*self-selection*."

necessary for this explanation to drive our results. The fact that high-school seniors do not know their test scores when they submit their lists increases the plausibility of this theory.

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

Cognitive limitations may, however, hinder applicants' ability to behave optimally (Benjamin et al., 2013; Esponda and Vespa, 2016), which is consistent with our findings on the correlation between academic ability and obvious mistakes. In this context, it is worth mentioning that the clearinghouse does not provide explicit information about the optimality of honest ranking (although such information about the suboptimality of obvious mistakes is available in popular commercial websites). In a field experiment, Guillen and Hakimov (2016) find that information on the truthfulness of TTC has a positive effect on truth-telling rates, but that describing the mechanism does not.

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 all of our findings, including the existence of flipping, which is difficult to explain by pessimistic beliefs and (near) indifference.[33]

Another possibility, which is specific to the Hungarian context, is that individuals may fail to understand the application fee structure. More specifically, they may not understand that the fee is charged per program, and not per contract. We do not think this drives our results. Information about fees is readily available through many sources, including the official website and booklet,

[33] For example, if applicants use a rationale that suits the Boston mechanism, since low-SES applicants presumably value funding more, they should be less likely to rank the funded and the unfunded contracts in a way that is inconsistent with their preferences (see Abdulkadiroğlu et al., 2011). Applicants with low academic achievement may think that they are unlikely to be admitted to a funded contract even if they rank it first, and thus (mistakenly) prefer to rank a more realistic option at the top. Pathak and Sönmez (2008) report that sophisticated families used this strategy under the Boston mechanism.

and the website includes a fee calculator. Additionally, we assessed this possibility in many ways, including concentrating on the subsample of applicants who ranked four or more contracts with three or fewer programs. These applicants must have learned the pricing scheme. We find no evidence that this subsample behaves differently than the rest of the population.

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, or the policy maker's commitment to use the stated mechanism.[34] In the Hungarian context, the match has a long history, is governed by legislation, and is operated by the central government. Moreover, since priority-score cutoffs become public shortly after the match, applicants can verify that their assignment is indeed the option they ranked highest among those whose cutoff they surpassed. Hence, we do not think that the lack of trust drives our results.

Another explanation, which is independent of the strategic environment, is that lack of information, and in particular information about financial aid, may cause students to behave suboptimally in college admissions markets (Hoxby and Avery, 2012; Hoxby and Turner, 2015). We do not think that lack of information about funding explains our findings for several reasons. First, funded positions are the historical norm, whereas unfunded positions are the innovation. Thus, while it is reasonable to expect that uninformed agents will generally make more mistakes, the opposite is true for obvious mistakes (which can only occur if the agent ranks some unfunded position). Second, students who make obvious mistakes come from larger settlements and higher socioeconomic status families, where informational frictions are expected to be less severe. Third, since the 2012–2013 reform affected only the availability of funding, it would be surprising if individuals who were not informed about funding drove the effect we identify. Similarly, this explanation cannot account for the within-ROL variation we report.

Another class of explanations of dominated-strategy play in strategically simple environments involves preferences that are "non-classical." Since there is over-demand for funding, social preferences and altruistic motives are consistent with the patterns we detect in the data (Fehr and Fischbacher, 2002; Charness and Rabin, 2002). For example, if the benefit from giving up the opportunity to receive funding is independent of the likelihood one assigns to meeting the priority-score cutoff, one would expect lower-ability individuals (who can expect to receive low priority, and thus low admission chances to a funded position) to purposefully make more obvious "mistakes." This explanation is plausible in our setting; however, we do not believe that social preferences are driving non-obvious mistakes, like the ones in Rees-Jones (2017b).

Another potential explanation is that applicants have "ego utility" (Kőszegi, 2006), and may

[34] 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.

distort their choices to avoid receiving information about their priority as this may hurt their self-image. In the context of self-image concerns, it is worth mentioning that applicants learn their priority score, and that the priority-score cutoffs are public information. Thus applicants have access to the same information about their priority no matter what ranking they submit. On the other hand, the strategies we classify as mistakes make this information less salient and easier to ignore.

Another possible explanation is that applicants like to be able to honestly say that they got their first choice. While we find this story plausible in general, in the context of obvious mistakes, we do not believe that many individuals can convince themselves or others that they do not like money.

# 8 Concluding Remarks

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

We have established that obvious mistakes are more common among high socioeconomic status applicants. These mistakes lead to an increase in the number of students attending college and to a transfer of funding from rich to poor applicants. While this self-selection pattern emerged in the absence of incentives, it suggests a non-negligible scope for gains from adding (incentivized) screening to college admissions mechanisms.

Previous studies mainly focused on the incentive properties of matching mechanisms, giving special attention to strategic simplicity. This study documents behavior that cannot be rationalized using standard models of matching markets, and thus suggests that human psychology plays an important role even in strategically simple matching environments. An upshot is that the description of a mechanism may affect user behavior. For example, while Rees-Jones (2017b) finds many instances of flipping in ROLs submitted to the NRMP and Hassidim et al. (2016) find almost equal rates of obvious flipping and obvious dropping in ROLs submitted to the IPMM, we find substantially lower rates of obvious flipping relative to obvious dropping. We think that the difference derives from the fact that in the Hungarian mechanism priorities are communicated to applicants as priority scores, and the outcome is expressed as priority-score cutoffs. By contrast, the other mechanisms describe priorities through ROLs and provide a combinatorial description of an algorithm that determines the allocation. This, in turn, highlights the practical importance of research that provides tractable and transparent descriptions of mechanisms with attractive properties.[35] More broadly, our findings suggest that a better understanding of human behavior in centralized matching environments holds great promise for capitalizing on the advances made in the recent decades to the study of matching markets.

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

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# A Matching College Admissions Data to the NABC

The National Assessment of Basic Competencies (NABC) has been conducted since 2003. Our data covers the period between 2006 and 2011. In 2006 and 2007, the NABC did not cover the full population: only 30 students completed the test in each program of each high school. This means that we have competency skill for about 50% of the population in these years. From 2008 onward, NABC has been mandatory; hence our dataset covers each student who was not absent from school on the day of the test.

As explained in Section 3.2, we match high-school senior applicants to the NABC dataset based on observable demographic characteristics, such as year and month of birth, high-school identifier, gender, and postal code. Traditionally, students attend high schools for four years. However, since 2004, certain schools have offered five-year programs, which include an additional year of foreign language courses at the beginning of the program. 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 A1 describes the result of the matching. The more variables we use for matching, the fewer applicants we are able to match. The NABC is conducted two or three years before applicants' senior year; thus we are unable to match seniors who moved to a new postal code or to a new high school in that period of time. Between 2011 and 2013, when the NABC covers the full population, the share of matched students is very stable. We are able to match 91–92% of seniors based on 3 variables, 89–90% based on 4 variables, and 79–80% based on 5 variables. The share of unique matches is also stable in these years: 16–20% of seniors based on 3 variables, 41–43% based on 4 variables, and 63–65% based on 5 variables. As the matching becomes finer, we match more individuals uniquely. This property does not hold in 2009 for two reasons. First, since we do not observe the full population, the match cannot be refined by including more matching variables (due to empty cells). Second, the postal code was self-reported in the first two years of our NABC data, which leads to stronger attrition as we include the postal code among the matching variables. In our main analysis we use the matching that is based on 5 variables (Panel C).

Table A1: Matching college admissions data to the NABC

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

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

# B Additional Results

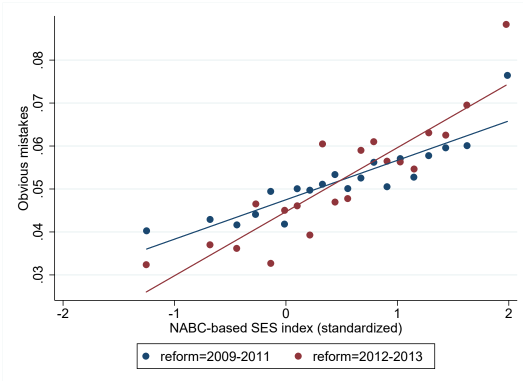
## B.1 Obvious Mistakes and Their Correlates

Table B1: Socioeconomic status and obvious mistakes

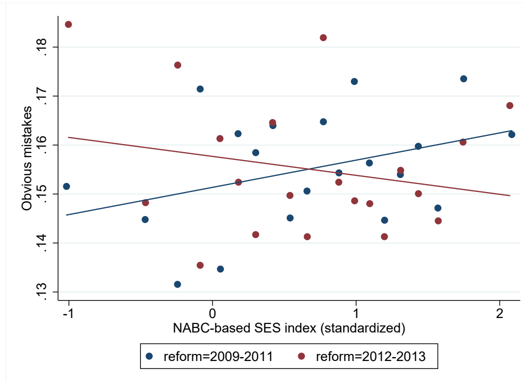
| Dependent variable | Obvious mistakes | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| NABC-based SES index | 0.011*** | 0.000 | | | | | | |
| | (0.0007) | (0.0022) | | | | | | |
| Gross annual per capita income | | | 0.009*** | 0.008*** | | | | |
| | | | (0.0006) | (0.0016) | | | | |
| Gross annual per capita income (missing) | | | 0.076* | 0.137 | | | | |
| | | | (0.0446) | (0.1748) | | | | |
| Unemployment rate in 2008 (%) | | | | | -0.002*** | -0.002*** | | |
| | | | | | (0.0001) | (0.0004) | | |
| Unemployment rate (missing) | | | | | -0.012 | 0.068 | | |
| | | | | | (0.0209) | (0.0938) | | |
| Disadvantaged status (dummy) | | | | | | | -0.027*** | -0.022*** |
| | | | | | | | (0.0015) | (0.0069) |
| Female | 0.013*** | 0.016*** | 0.012*** | 0.016*** | 0.011*** | 0.016*** | 0.012*** | 0.016*** |
| | (0.0012) | (0.0035) | (0.0012) | (0.0035) | (0.0012) | (0.0035) | (0.0012) | (0.0035) |
| Age = 18 | 0.024** | 0.113*** | 0.013 | 0.109*** | 0.012 | 0.100*** | 0.016* | 0.113*** |
| | (0.0102) | (0.0224) | (0.0093) | (0.0246) | (0.0103) | (0.0331) | (0.0093) | (0.0223) |
| Age = 19 | 0.030*** | 0.124*** | 0.018* | 0.120*** | 0.017* | 0.111*** | 0.021** | 0.125*** |
| | (0.0102) | (0.0221) | (0.0092) | (0.0244) | (0.0103) | (0.0330) | (0.0092) | (0.0221) |
| Age = 20 | 0.042*** | 0.134*** | 0.028*** | 0.129*** | 0.028*** | 0.121*** | 0.033*** | 0.135*** |
| | (0.0103) | (0.0224) | (0.0093) | (0.0246) | (0.0104) | (0.0332) | (0.0094) | (0.0224) |
| Age = 21 | 0.071*** | 0.189*** | 0.056*** | 0.182*** | 0.056*** | 0.175*** | 0.061*** | 0.190*** |
| | (0.0148) | (0.0326) | (0.0142) | (0.0341) | (0.0149) | (0.0408) | (0.0142) | (0.0326) |
| Vocational school | 0.014*** | 0.033*** | 0.010*** | 0.035*** | 0.009*** | 0.034*** | 0.010*** | 0.034*** |
| | (0.0014) | (0.0042) | (0.0014) | (0.0041) | (0.0014) | (0.0041) | (0.0014) | (0.0041) |
| Other high schools | 0.022*** | 0.032** | 0.020*** | 0.032** | 0.020*** | 0.031** | 0.020*** | 0.032** |
| | (0.0068) | (0.0150) | (0.0068) | (0.0150) | (0.0068) | (0.0150) | (0.0068) | (0.0150) |
| County capital | -0.028*** | -0.026*** | -0.012*** | -0.010 | -0.026*** | -0.021*** | -0.029*** | -0.025*** |
| | (0.0025) | (0.0054) | (0.0028) | (0.0062) | (0.0025) | (0.0055) | (0.0025) | (0.0054) |
| Town | -0.033*** | -0.029*** | -0.013*** | -0.010 | -0.029*** | -0.020*** | -0.035*** | -0.028*** |
| | (0.0023) | (0.0051) | (0.0030) | (0.0064) | (0.0024) | (0.0053) | (0.0023) | (0.0050) |
| Village | -0.040*** | -0.033*** | -0.015*** | -0.010 | -0.036*** | -0.025*** | -0.041*** | -0.032*** |
| | (0.0024) | (0.0055) | (0.0032) | (0.0072) | (0.0025) | (0.0058) | (0.0024) | (0.0054) |
| Year = 2010 | -0.000 | -0.004 | -0.001 | -0.004 | -0.000 | -0.003 | -0.000 | -0.004 |
| | (0.0016) | (0.0067) | (0.0016) | (0.0067) | (0.0016) | (0.0067) | (0.0016) | (0.0067) |
| Year = 2011 | 0.012*** | 0.039*** | 0.013*** | 0.041*** | 0.012*** | 0.039*** | 0.012*** | 0.039*** |
| | (0.0017) | (0.0067) | (0.0017) | (0.0067) | (0.0017) | (0.0067) | (0.0017) | (0.0067) |
| Year = 2012 | 0.028*** | 0.002 | 0.026*** | 0.001 | 0.028*** | 0.002 | 0.028*** | 0.002 |
| | (0.0019) | (0.0061) | (0.0019) | (0.0061) | (0.0019) | (0.0061) | (0.0019) | (0.0061) |
| Year = 2013 | 0.074*** | 0.089*** | 0.071*** | 0.087*** | 0.074*** | 0.089*** | 0.075*** | 0.090*** |
| | (0.0022) | (0.0064) | (0.0023) | (0.0064) | (0.0022) | (0.0064) | (0.0023) | (0.0064) |
| Constant | 0.009 | 0.001 | -0.045*** | -0.061** | 0.040*** | 0.023 | 0.029*** | 0.001 |
| | (0.0104) | (0.0235) | (0.0108) | (0.0284) | (0.0106) | (0.0339) | (0.0095) | (0.0232) |
| $N$ | 133,714 | 44,786 | 133,714 | 44,786 | 133,714 | 44,786 | 133,714 | 44786 |
| $R^2$ | 0.024 | 0.016 | 0.024 | 0.016 | 0.023 | 0.016 | 0.023 | 0.016 |
| Hig-school senior applicant sample | ✓ | - | ✓ | - | ✓ | - | ✓ | - |
| Relevant subsample | - | ✓ | - | ✓ | - | ✓ | - | ✓ |

*Notes*: The table presents the estimates of a linear regression of an indicator of obvious mistakes on various measures of socioeconomic status, demographic controls, 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 individuals whose numeracy skill, literacy skill, and NABC-based SES index are not missing. The fraction of obvious mistakes in this sample is 5.2%, and the fraction is 15.5% in the relevant subsample.
***: p<0.01, **: p<0.05, *:p<0.1.

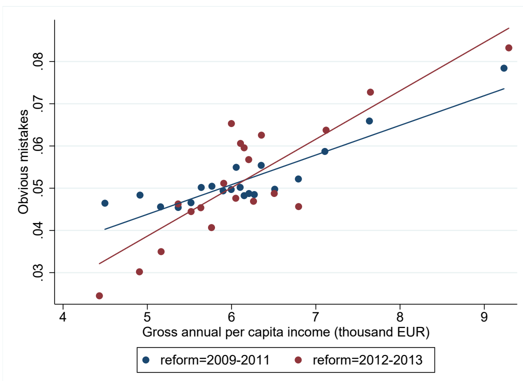
Figure A1: Socioeconomic status and obvious mistakes pre- and post-reform



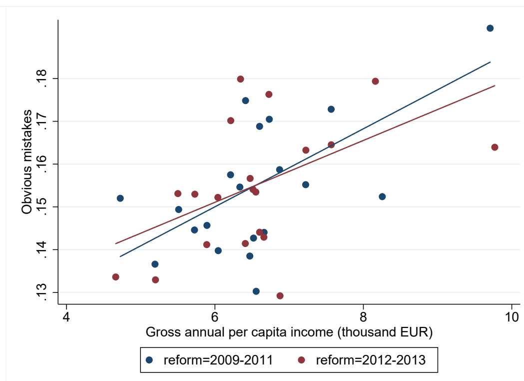

(a) NABC-based SES index



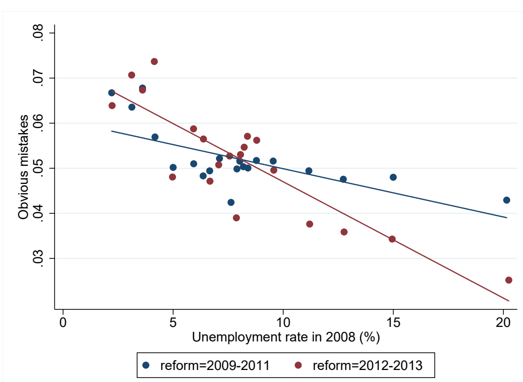

(b) NABC-based SES index



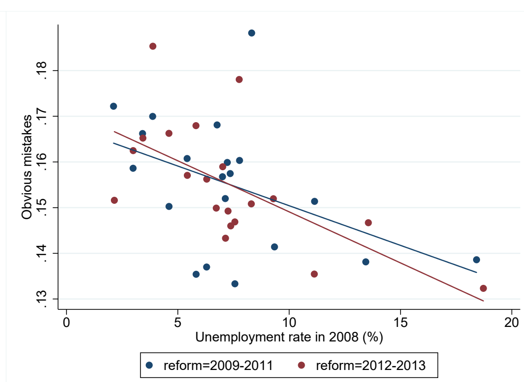

(c) Gross annual per capita income




(d) Gross annual per capita income




(e) Unemployment rate in 2008




(f) Unemployment rate in 2008

*Notes*: The figure displays the relationship between obvious mistakes and various measures of socioeconomic status, such as the NABC-based SES index (Panels (a) and (b)), gross annual per capita income measured on the settlement level (Panels (c) and (d)), and the microregional-level unemployment rate in 2008 (Panels (e) and (f)). The sample is split to pre- and post-reform observations. Panels in the left column focus on the high-school senior applicant sample, whereas panels in the right column are restricted to the relevant ROLs in the high-school senior applicants sample. Each dot (bin-specific mean) and the fitted regression line are conditional on year fixed effects and demographics, such as gender, age, high-school type, and type of residence.

Table B2: Academic achievement and obvious mistakes

| Dependent variable | Obvious mistakes | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Numeracy skill | -0.022*** | | -0.019*** | -0.014*** | -0.015*** | -0.014*** |
| | (0.0008) | | (0.0010) | (0.0010) | (0.0010) | (0.0010) |
| Literacy skill | | -0.019*** | -0.006*** | -0.002* | -0.002 | -0.002 |
| | | (0.0009) | (0.0011) | (0.0011) | (0.0011) | (0.0011) |
| 11th-grade GPA | | | | -0.027*** | | -0.015*** |
| | | | | (0.0010) | | (0.0019) |
| 11th-grade GPA (missing) | | | | 0.041*** | | 0.022*** |
| | | | | (0.0017) | | (0.0038) |
| 12th-grade GPA GPA | | | | | -0.026*** | -0.014*** |
| | | | | | (0.0010) | (0.0018) |
| 12th-grade GPA (missing) | | | | | 0.040*** | 0.020*** |
| | | | | | (0.0019) | (0.0041) |
| NABC-based SES index | 0.016*** | 0.015*** | 0.017*** | 0.017*** | 0.017*** | 0.017*** |
| | (0.0008) | (0.0008) | (0.0008) | (0.0008) | (0.0008) | (0.0008) |
| Female | 0.003*** | 0.016*** | 0.006*** | 0.012*** | 0.013*** | 0.013*** |
| | (0.0013) | (0.0012) | (0.0013) | (0.0013) | (0.0014) | (0.0014) |
| $N$ | 133,714 | 133,714 | 133,714 | 133,714 | 133,714 | 133,714 |
| $R^2$ | 0.030 | 0.027 | 0.030 | 0.036 | 0.036 | 0.036 |
| Demographic controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

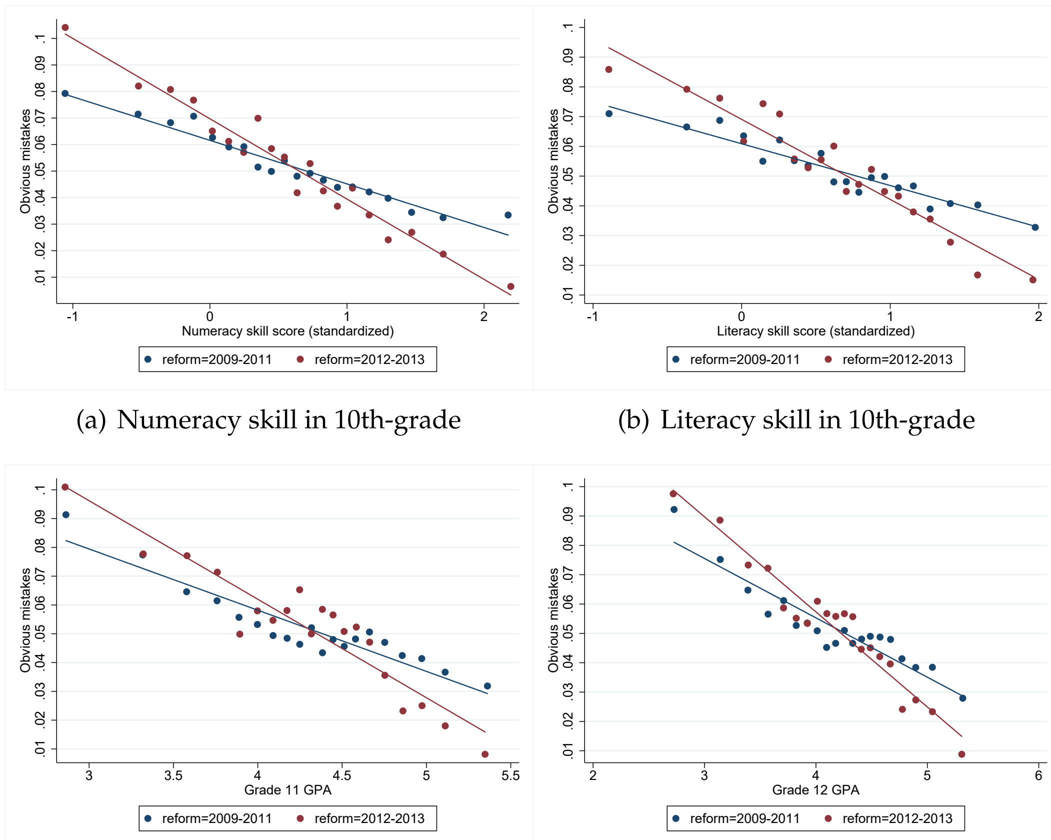
*Notes*: The table presents the estimates of a linear regression of an indicator of obvious mistakes on various measures of academic achievement, conditional on demographic controls and year fixed effects. Demographic controls include age, high-school type, and residence type. Robust standard errors are in parentheses. We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 5.2% in this subsample of the high-school senior applicant sample. Eleventh-grade (12th-grade) GPA is missing for 28.5% (22.6 %) of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Table B3: Academic achievement and obvious mistakes (relevant applicants)

| Dependent variable | Obvious mistakes | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Numeracy skill | -0.043*** | | -0.034*** | -0.024*** | -0.025*** | -0.023*** |
| | (0.0022) | | (0.0027) | (0.0028) | (0.0028) | (0.0028) |
| Literacy skill | | -0.040*** | -0.017*** | -0.008*** | -0.007** | -0.007** |
| | | (0.0025) | (0.0031) | (0.0031) | (0.0031) | (0.0031) |
| 11th-grade GPA | | | | -0.054*** | | -0.035*** |
| | | | | (0.0028) | | (0.0051) |
| 11th-grade GPA (missing) | | | | 0.098*** | | 0.089*** |
| | | | | (0.0048) | | (0.0113) |
| 12th-grade GPA | | | | | -0.053*** | -0.023*** |
| | | | | | (0.0027) | (0.0050) |
| 12th-grade GPA (missing) | | | | | 0.089*** | 0.007 |
| | | | | | (0.0051) | (0.0120) |
| NABC-based SES index | 0.008*** | 0.007*** | 0.010*** | 0.010*** | 0.011*** | 0.011*** |
| | (0.0022) | (0.0022) | (0.0022) | (0.0022) | (0.0022) | (0.0022) |
| Female | -0.001 | 0.023*** | 0.006 | 0.020*** | 0.021*** | 0.022*** |
| | (0.0036) | (0.0035) | (0.0037) | (0.0038) | (0.0038) | (0.0038) |
| *N* | 44,786 | 44,786 | 44,786 | 44,786 | 44,786 | 44,786 |
| $R^2$ | 0.024 | 0.021 | 0.024 | 0.035 | 0.034 | 0.036 |
| Demographic controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |

*Notes*: The table presents the estimates of a linear regression of an indicator of obvious mistakes on various measures of academic achievement, conditional on demographic controls and year fixed effects. Demographic controls include age, high-school type, and residence type. Robust standard errors are in parentheses. We restrict the sample to those high-school senior applicants who ranked at least one funded contract and whose numeracy skills, literacy skills, and NABC-based SES index are not missing. The share of obvious mistakes is 5.2% in this subsample of the high-school senior applicant sample. Eleventh-grade (12th-grade) GPA is missing for 28.5% (22.6 %) of the sample. We include an indicator of those missing observations in our regressions.
***: p<0.01, **: p<0.05, *:p<0.1.

Figure A2: Academic achievement and obvious mistakes pre- and post-reform




(a) Numeracy skill in 10th-grade

(b) Literacy skill in 10th-grade

(c) 11th-grade GPA

(d) 12th-grade GPA

*Notes*: The figure displays the relationship between obvious mistakes and various measures of academic achievement, such as numeracy skill in 10th-grade (Panel (a)), literacy skill in 10th-grade (Panel (b)), 11th-grade GPA (Panel (c)), and 12th-grade GPA (Panel (d)) in the high-school senior applicants sample. The sample is split by the 2012–2013 reform. Each dot (bin-specific mean) and the fitted regression line are conditional on year fixed effects, the NABC-based SES index, and demographics (such as gender, age, high-school type, and type of residence). Numeracy skill, literacy skill, and indicator of socioeconomic status variables are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and postal code). The estimated slope coefficients: Panel (a): 2009–2011: -0.016*** (0.0008), 2012–2013: -0.030*** (0.0015), Panel (b): 2009–2011: -0.014*** (0.0009), 2012–2013: -0.027*** (0.0017); Panel (c): 2009–2011: -0.021*** (0.0010), 2012–2013: -0.034*** (0.0020); Panel (d): 2009–2011: -0.020*** (0.0010), 2012–2013: -0.032*** (0.0018). Eleventh grade (12th-) GPA is missing for 28.5% (22.6 %) of the sample.

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

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

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

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

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

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

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

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

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

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

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

Table B7: Test for the stable composition of applicants

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

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

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

## B.3 The effect of Selectivity on Obvious Mistakes: Alternative Specification

Section 5.2 established that program selectivity has a large, positive causal effect on obvious mistakes. We test the robustness of this result by considering an alternative specification. Instead of focusing solely on the 2012–2013 reform, we exploit all variations in the availability of funded positions in the sample (Table 1). This alternative approach allows us to estimate the elasticity between the available funded positions and obvious mistakes.

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

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

where capacity$_{ts}$ denotes the number of available funded positions in year $t$ and field of study $f$. By the index $f$ we highlight that there is no within-field of study variation in the number of available funded positions.[36] In line with our main result, we expect the estimate for $\beta$ to be negative, as more available funded seats correspond to lower program selectivity. On the other hand, the 2012–2013 reform was salient and stark relative to other changes that were small and sometimes inconsequential, which limits the comparability of this specification to our main findings.

Table B8 presents our estimates. We find that a 10 percent reduction in the number of funded seats increases obvious mistakes by 8.2–8.5 percentage points.

[36] Since the government did not release the funded quotas for 2013, we use the number of funded positions, ex post.

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

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

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