Dominated Choices under Deferred Acceptance Mechanism: The Effect of Admission Selectivity

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Abstract

College applicants often make dominated choices even when a strategically simple mechanism such as deferred acceptance is in place. We study Hungarian college admissions, where deferred acceptance is used, and still many college applicants make revealed dominated choices: they forgo the free opportunity to receive a tuition waiver. Using two empirical strategies, we show that when admission with a tuition waiver becomes more selective, applicants make more revealed dominated choices. Our results suggest that dominated choices respond to economic incentives.

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1 Introduction

School districts around the world are using centralized assignment systems to match students to schools. One of the great success stories of matching market design is the wide-spread adoption of strategically simple mechanisms like deferred acceptance (DA). According to the standard model of matching market design, these mechanisms guarantee that ranking alternatives in a way that is inconsistent with one's preferences is a dominated choice.[1] But, dominated choices are prevalent in a wide array of markets, including medical residencies in the US, college admissions in Australia, and high-school choice in Mexico City.[2] Furthermore, across settings, more dominated choices were detected among weaker applicants, and with respect to more selective alternatives. Yet, the mechanisms that underlie these patterns are not well understood.

In this paper, we estimate the causal effect of admission selectivity on dominated choices. Understanding whether admission selectivity affects dominated choices is important for at least two reasons. First, it speaks to the mechanisms that underlie dominated choices, and it can be useful in the design of decision support tools and choice architecture to aid applicants choose according to their preferences. Second, it can inform more predictive theories of behavior in matching markets and can help to make more accurate predictions in centralized school-assignment systems.

While several studies find that admission selectivity is correlated with dominated choices, it is challenging to establish a causal relationship for several reasons. First, it is difficult to detect dominated choices in the field. Second, admission chances are often correlated with cognitive ability (e.g., through test scores), making it difficult to isolate the effect of admission selectivity.

We overcome these challenges by studying Hungarian college admission, where a strategically simple version of DA is used to assign approximately 100,000 applicants each year. This setting is particularly suited for overcoming the above-mentioned challenges. First, most college programs are offered both with and without state-funding, and each of these options can be ranked separately. This allows us to detect dominated choices using the approach of Hassidim et al. (2021). Specifically, we say that an applicant makes a *revealed dominated choice* when she submits a Rank-Order List (ROL) that indicates that she prefers to attend a particular study program without state-funding. Such an applicant forgoes the free opportunity to receive a tuition waiver worth thousands of dollars. Shorrer and Sóvágó (2022) show that 11 percent of the applicants in this market submit an ROL that contains a dominated choice, with each dominated choice costing 6,600 on average.

Second, this setting allows us to establish a causal relationship between the selectivity of admission to funded positions and (revealed) dominated choices using two complementary empirical strategies. The first strategy is a difference-in-differences design that leverages variation stemming from a sharp change in the Hungarian government policy. Motivated by fiscal concerns, in 2012 the government severely reduced the number of tuition waivers in several fields of study (business and economics, legal studies, and social sciences), significantly increasing the selectivity of admission to funded positions in these fields. Other fields remained largely unaffected. We find that dominated choices in applications

[1] Strategy-proof mechanisms guarantee that "honesty is the best policy." Formally, they have a weakly dominant strategy of truthful reporting. While academics consider this property particularly desirable, in the field, strategy-proof school assignment mechanisms are rare. For example, Pathak and Sönmez (2013) report on dozens of school systems around the world that adopted strategically simple versions of the deferred acceptance mechanism (DA; Gale and Shapley, 1962), only one of which (Boston Public Schools') was strategy-proof.

[2] E.g., Chen and Pereyra (2019), Rees-Jones (2018), Artemov et al. (2022), Hassidim et al. (2017), Hassidim et al. (2021), and Shorrer and Sóvágó (2022). For an extensive survey of laboratory findings, see Hakimov and Kübler (2021). Ajayi (2011) and Rees-Jones et al. (2019) document another type of suboptimal behavior under Deferred Acceptance.

to the affected fields more than quadrupled as a result of this reform.

A potential concern is that the effect we find using the first strategy is the result of contemporaneous changes,[3] or that it is a short-run reaction to the reform. These issues motivate our second empirical strategy, which exploits variation in the selectivity of admission with funding to different programs on the same ROL. The within-ROL design compares the rate of dominated choices made by a specific applicant with respect to programs with different historical admission selectivity at a given point in time. Additionally, by focusing on pre-reform ROLs, we are analyzing behavior in a "steady state." This design corroborates that admission selectivity has a positive causal effect on dominated choices.

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

Our findings suggest that economic incentives affect the prevalence of dominated choices: applicants make more dominated choices when they are less costly. Shorrer and Sóvágó (2022) provide further evidence suggesting that applicants make more dominated choices when their expected cost is low. First, students with low academic ability, who can expect to receive lower admission priority, are more likely to make a dominated choice. Second, high-SES applicants, who presumably are less sensitive to the availability of funding and hence, all else equal, are more likely to be nearly indifferent, make more dominated choices.

A potential explanation is that applicants (often mistakenly) expect this behavior to be (approximately) costless. An alternative explanation is that the standard model of matching market design does not capture some important aspects of applicants' preferences, and, as a result, it is not necessarily optimal to rank alternatives truthfully (in which case the label "dominated choice" is a misnomer, since DA is not strategically simple for agents with such preference). For example, Dreyfuss et al. (2022) and Meisner and von Wangenheim (2019) show that expectation-based loss aversion can explain the empirical patterns we document. We discuss how these and other theories align with our findings in Section 5.

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

Second, they suggest that preferences reports cannot be taken at face value even when the mechanism in place is strategically simple. Failing to account for applicants systematically dropping selective alternatives from their preference reports may lead to mistaken conclusions, for example, that selective schools are less desirable than they actually are.

Finally, they contribute to the ongoing effort to pin down the behavioral mechanisms that underlie dominated choices. This, in turn, informs new theoretical models of in matching markets, and new classifications of allocation mechanisms according to their simplicity (e.g., Li, 2017; Zhang and Levin, 2017; Bó and Hakimov, 2019).

[3]In Section 2.2, we discuss other changes that occurred in 2012, and at the end of Section 4.1 we provide evidence that these changes do not drive the results of the first empirical strategy.

Relation to the literature. The most closely related paper is Shorrer and Sóvágó (2022). That paper finds that 11 percent of all eligible Hungarian college applicants make a revealed dominated choice, and that between 12.3 and 18.7 percent of dominated choices are costly, costing the applicants that make them more than $6,600 on average. Shorrer and Sóvágó (2022) also find that dominated choices are more common among high-socioeconomic-status applicants, among applicants with low academic achievement, and in applications to more selective programs. The negative correlation of dominated choices with cognitive ability and the positive correlation with the expectation of fiercer competition are a recurring theme in the literature. In the laboratory, Basteck and Mantovani (2016) and Rees-Jones and Skowronek (2018) find that mistakes under the DA mechanism are more common among applicants with low cognitive ability, and Guillen and Hakimov (2016) find that the same holds under the strategy-proof top trading cycles. Rees-Jones and Skowronek (2018) document a strong causal relationship between expected admission selectivity and dominated choices in the laboratory. Our study is the first to establish the causal relationship between admission selectivity and dominated choices in the field, ruling out cognitive limitations as a sole determinant of dominated choices in high-stakes environments.

While the literature on inattention is growing, evidence on the causes of inattention is still scarce (DellaVigna, 2009; Gabaix, 2019). As suboptimal behavior is a leading measure of inattention, we contribute to this literature by documenting that, in our context, it responds to economic incentives. Other studies (in different domains) have also found that the prevalence of suboptimal behavior responds to economic incentives (e.g., Taubinsky and Rees-Jones, 2018; Taubinsky, 2018).

Organization of the paper. 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 lays out our two empirical strategies, and establishes a causal relationship between admission selectivity and dominated choices. Section 5 discusses possible explanations of our findings, and Section 6 concludes.

2 College Admissions in Hungary

In this section, we describe college admissions in Hungary. We begin, in Section 2.1, by explaining the centralized admissions process and defining dominated choices. In Section 2.2, we describe the 2012–13 reform, which we exploit to study the causal effect of admission selectivity on dominated choices.

2.1 The Centralized Admissions Process

Admissions to all higher education programs in Hungary are organized through a centralized clearinghouse that has been operated by the government since 1985.[4] Each year, about 100,000 applicants seek admission to bachelors degree programs, and about 70 percent are admitted. Starting in 2008, the centralized clearinghouse adopted an assignment mechanism based on student-proposing DA. It displaced a similar variant of college-proposing DA. Both mechanisms endow programs with priorities based on a (program-specific) weighted average of several several variables (mainly academic performance in the 11th and 12th grades and matriculation exam scores, but also credits for disadvantaged

[4]This section draws heavily on Shorrer and Sóvágó (2022)

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

Tuition waivers. Most bachelors degree programs a are offered both with and without state-funding (in the form of a tuition waiver). To be eligible for state-funding, the applicant must be a citizen of the European Economic Area. Each individual is eligible to receive a waiver for up to six years (12 semesters). But, the government caps the number of funded positions in some majors and in each field of study (business and economics, humanities, etc.). As a result, eligible students are also allowed to apply for self-funded positions (if they are admitted to a self-funded position, they will pay the full tuition in spite of their eligibility).

In 2013, tuition ranged from 850 to 10,000 dollars per year. On average, 64% of admitted students received a tuition waiver in the years 2009–2014. In addition to the monetary benefit, many universities give state-funded students priority in access to subsidized housing and other valuable amenities. Additionally, self-funded students are stigmatized as "not good enough" for the traditional state-funded track (cf. Aygun and Turhan, 2016).

Rank-Order Lists. Applicants are allowed to rank any number of contracts (program–funding level pairs) that they wish. Table 1 presents an example of an ROL that include 4 contracts with 3 different programs. The fixed application fee (approximately 40 dollars) covers applications for up to 3 programs (where a program is defined as a major–institution pair). Applicants incur a charge of about 9 dollars for each additional program that they include in their ROL. Importantly, students are charged by the number of *programs*—not the number of *contracts*—in their ROL. For example, an applicants submitting the ROL from Table 1 will incur no additional charges other than the fixed application fee.

Table 1: A rank-order list with dominated choices

Rank	Program		Funding
	Institution	Major	
1.	Semmelweis University	BA in Medicine	Self-funded
2.	Budapest University of Technology and Economics	BA in Civil Engineering	State-funded
3.	Budapest University of Technology and Economics	BA in Computer Science	Self-funded
4.	Budapest University of Technology and Economics	BA in Computer Science	State-funded

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

Revealed dominated choices. Since the application fee is based on the number of programs (not the number of contracts) in the ROL, if an applicant ranks the self-funded contract with some program, she can add the state-funded contract with that program at no additional cost. And because the underlying program is the same one, there are also no additional search costs. Therefore, according to the standard model of matching market design, an applicant is using a dominated strategy if she ranks a self-funded contract in some program higher than the state-funded contract in the same program (*revealed flipping*), or if she ranks only the self-funded contract in a program that offers a state-funded contract (*revealed dropping*). We collectively refer to such strategies as *revealed dominated choices* (for short, *dominated choices*).[5]

Table 1 presents an ROL containing four contracts with three programs. This ROL contains two dominated choices. First, the applicant ranked only a self-funded BA in medicine at Semmelweis University, even though a state-funded contract was offered (revealed dropping). Second, a self-funded BA in computer science at Budapest University of Technology and Economics is ranked higher than a state-funded contract in the same program (revealed flipping).

Timeline. The application process proceeds as follows. First, applicants submit their ROLs in mid-February. High-school seniors learn their 12th-grade GPA in April, and take their matriculation exams in May and June. In early July, applicants report all the information required to calculate their priorities (including their grades), and they may reorder their ROL or drop contracts from it, but they may not add any new contracts. Finally, in mid-July, applicants learn their assignment and the *priority-score cutoffs*, i.e., the minimum priority score needed to gain admission to a particular contract, are made public.

Information. The formulas for priority scores are public, and, as we discuss above, the priority-score cutoffs are made public shortly after the match. This feature simplifies applicants' comprehension of the mechanism and increases their trust, as they may verify that they were assigned to the highest-ranked program whose cutoff they surpassed. The clearinghouse website (`http://www.felvi.hu`) provides detailed historical statistics about the match, including quotas, the number of applicants, acceptance rate, and priority-score cutoffs. The Ministry of Education also issues a yearly booklet containing much of this information as well as information about all participating programs. The clearinghouse website also provides an automated application fee calculator.

The application interface is particularly informative about the availability of financial aid. Applicants choose contracts from a dropdown menu where the state-funded contract in each program appears immediately above the self-funded contract in the same program. The interface mimics the traditional paper-based system in which each contract is associated with a code, and applicants form their ROL by copying codes from a brochure that lists state-funded and self-funded contracts in the same program consecutively.

2.2 The 2012–2013 Reforms

Historically, higher education in Hungary was free. Since the fall of the Iron Curtain in the early 1990s, there have been several attempts to introduce college tuition, but these attempts met with widespread public resistance. For example, in 1995, the government introduced college tuition, which

[5]The popular media shares our view that state-funded positions are unambiguously preferable to self-funded ones. For example, when the 2017 match results were released, a major news outlet published a story with the man-bites-dog title: "*The priority-score cutoff for unfunded medicine exceeds the state-funded cutoff.*". Source: index.hu; `https://goo.gl/zfxFFw`, accessed: 20/09/2017. Shorrer and Sóvágó (2022) show that, even if applicants do not understand the application fee structure, rationalizing dominated choices would require extreme levels of risk aversion, loss aversion, or time discounting, that lie well outside the conventional ranges.

was canceled in 1998.[6] 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.[7] 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-funded positions was concentrated in three fields of study: business and economics, legal studies, and social sciences. The number of state-funded positions declined from 4,900 to 250 in business and economics, from 1,300 to 300 in legal studies, and from 2,100 to 1,000 in social sciences (Table 2). Altogether, the reform reduced the number of funded positions by 81 percent in these fields. Funded positions in some majors were eliminated completely (examples include business administration and management, commerce and marketing, and human resources). In other majors, funding was only offered in a subset of the institutions where it had been offered previously (for example, legal studies, international business administration, and international relations). In still other majors, the menu was not changed, but the capacities of state-funded positions were reduced. The number of state-funded positions in other fields of study declined by 7 percent, from 36,000 to 33,637. We refer to these fields of study as *fields with little or no funding cut*.

The backlash following the 2012 experience led to some changes in the way the reform was implemented in subsequent years, starting in 2013. Importantly, state-funded positions were restored in all programs where they had previously been offered. However, state-funded capacities remained scarce.[8] The "reversal" of the 2012 reform did not meaningfully increase the number of state-funded 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, starting in 2013, the funding cut was expanded to include an additional major in the field of humanities (adult education).

Since our first empirical strategy exploits the 2012–13 reform, we must also mention other changes that occurred around the same time. As part of the reform, the government legislated a decree that introduced the *study contract*, which obliges college students who benefit from state sponsorship to work in Hungary for the number of years they spent in college within 20 years of graduation, or else repay the country with interest (a base rate + three percentage points).

Even though the decree makes state-funded positions less desirable, we do not think that it changes the natural ranking of funded and unfunded contracts or that it has a substantial effect on the composition of applicants, for several reasons. First, the decree specifies numerous exemptions, including having two or more children, military service, and disability. Second, it is highly unlikely that this contract will be enforced (in twenty years). Its legal status is unclear, as it may violate the freedom of movement of workers in the EU,[9] and political pressure caused the government to significantly alleviate the terms already in 2013. Third, a student who leaves Hungary and does not

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

[7]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.

[8]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.

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

Table 2: Availability of funded positions

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

Notes: The table describes the availability of funded positions between 2009 and 2012 by field and year. Starting in 2013, the government did not release the corresponding numbers. The rightmost column provides details on partial funding, which was offered in 2012 only. Partial funding covered 50 percent of the tuition fee. Partial funding was awarded to students who were assigned an unfunded position based on merit. There was no possibility of ranking partially funded positions separately. While the number of available tuition waivers in computer science and natural sciences increased in 2011, the previous capacity was not binding.

return for more than a decade is very likely to have moved to a country where earning a few thousand dollars is substantially easier, 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 pure option value.

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

The government also expanded the availability of its subsidized student loan program for self-funded students and introduced partially funded positions. Partially funded positions were offered only in 2012. Partial funding covered half of the tuition and was also subject to the study contract. It was not possible to rank partially funded positions, but they were awarded based on merit to applicants who were assigned an unfunded position (thus, the government implicitly assumed that a funded option would be preferred by the applicants, which is consistent with our interpretation).

Another change in 2012 is that the formulas for priority scores were slightly changed and rescaled. For ease of comparison we compute within-year percentile ranks of the priority-score cutoffs. Finally, the number of programs one could rank was capped at 5 (10 contracts). We do not think this change had a substantial effect on the composition of ROLs as in 2011 only 4.5 percent of the ROLs included more than 5 programs and only 0.7 percent of the ROLs contained more than 10 contracts.

3 Data and Summary Statistics

In this section, we describe the data that we use in our empirical analysis. We begin, in Section 3.1, by describing our data and defining our sample. In Section 3.2, we present summary statistics.

3.1 Data Source

We use an administrative dataset that contains information about the bachelor's degree admissions process between 2009 and 2014 in Hungary. In particular, we observe each applicant's complete ROL, as well as the list of existing programs with their realized priority-score cutoff. For each applicant we also observe gender, age, postal code, and a high-school identifier.Additionally, the data include all components of applicants' priority scores, including grades in various subjects in the final two years of high school (11th and 12th grades) and the number of points the applicant received for claiming a disadvantaged background.[10]

Our full dataset contains all ROLs submitted between 2009 and 2014. We restrict attention to high-school senior applicants. We classify an applicant as a high-school senior if she was, at the time, younger than 22 and had completed her matriculation exams in the same year. We focus on high-school seniors for two reasons. First, we are less concerned about the possibility that the 2012–2013

[10]To be eligible for disadvantaged status, the applicant's per capita household income must be below 130 percent of the minimum pension (approximately $1,900 a year). Since 2014, in addition to the income criterion, the student has to meet one of the following three conditions: (i) parents with lower than primary education, (ii) long-term unemployed parents, or (iii) poor living conditions. To receive disadvantaged status, an applicant must certify that she meets these conditions at the local municipality. Students with disadvantaged status receive regular cash transfers and are eligible for free textbooks during high school.

reforms changed the composition of high-school senior applicants. Second, focusing on high-school seniors allows us to use an auxiliary dataset (which we describe in Appendix A) to demonstrate the robustness of our findings (see Appendix C).

We further restrict attention to those applicants who can potentially exhibit a dominated choice. These applicants are citizens of the European Economic Area and who claim to be eligible and rank at least one contract with a program that offers both self-funded and state-funded seats. The resulting dataset includes 268,981 ROLs.

3.2 Summary Statistics

Table 3 reports summary statistics on the characteristics of high-school senior applicants. Applicants were 19.05 years old on average, with 57 percent being female. The majority (70 percent) of applicants attended secondary grammar schools, whose declared purpose is to prepare students for higher education. Approximately 16 percent of the applicants lived in the capital, 20 percent lived in one of the 18 county capitals, 33 percent resided in towns, and the remainder lived in villages. About 10 percent of the applicants claimed points for disadvantaged status. The average ROL length was 4.3 contracts, which corresponds to 3.3 programs. High-school senior applicants' GPAs were 0.24 of a standard deviation higher than the GPA of all applicants who are eligible for state-funding.

Changes in the pattern of applications following the 2012–2013 reforms may compromise our difference-in-differences analysis. Appendix Table B1 and Appendix Figure B1 show that the composition of applicants and the distribution of fields they applied to remained relatively stable over time. We show that our results cannot be driven by such changes at the end of Section 4.1.

The fraction of ROLs with dominated choices ranges from 3.1 percent in 2009 to 10.8 percent in 2013. During the sample period more than 15,000 applicants, corresponding to 5.8 percent of high-school seniors' ROLs, made a dominated choice, mostly revealed dropping. The share of ROLs with revealed dropping was 5.0 percent and the share of ROLs with revealed flipping was 1.1 percent.

Dominated choices can be detected only in ROLs that rank at least one unfunded contract. Only 35 percent of the ROLs in our sample meet this requirement. The share of dominated choices should be interpreted in this context. For example, 5.8 percent of ROLs with dominated choices represent 16.6 percent (= 5.8/0.35) of ROLs in the sample in which a dominated choice could be detected.

4 The Effect of Admission Selectivity on Dominated Choices

This section presents our main result, namely, that admission selectivity has a positive causal effect on making dominated choices. We establish this result using two complementary empirical strategies. First, we use a difference-in-differences research design, which compares the rates of dominated choices in applications to programs that were affected by the severe reduction in funding and these rates in applications to programs that experienced little or no cut in funding (Section 4.1). Second, we use a within-ROL design, which exploits variation in the degree of selectivity of different programs in the same ROL (Section 4.2).

4.1 Evidence from the 2012–13 Reform

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

Table 3: Individual-level summary statistics

	Mean (1)	St. dev. (2)
Female	0.57	(0.50)
Age at application	19.05	(0.68)
High school		
- secondary grammar school	0.70	(0.46)
- vocational school	0.26	(0.44)
Residence		
- capital	0.16	(0.37)
- county capital	0.20	(0.40)
- town	0.33	(0.47)
- village	0.30	(0.46)
11th-grade GPA (standardized)	0.24	(0.96)
11th-grade GPA - missing	0.18	(0.38)
Disadvantaged status	0.10	(0.29)
# of contracts on the ROL	4.34	(2.20)
# of contracts on the ROL (data)	3.81	(1.48)
# of programs on the ROL (data)	3.25	(1.17)

Notes: The table reports mean values of student characteristics, with standard deviations in parentheses. The number of high-school senior applicants in the sample is 268,981. Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average grades in mathematics and Hungarian grammar and literature. 11th-grade GPA is standardized among applicants who are eligible for funding. Some applicants have no incentive to report their GPA to the clearinghouse. Applicants with a high matriculation exam scores relative to their high-school GPA have no incentive to report their GPA, as it has no effect on their priority score. As a result, 11th-grade GPAs are missing for 18 percent of high-school senior applicants. The number of contracts on the ROL is reported administratively. Our data includes at most 7 contracts from each ROL: six contracts that are ranked the highest and the contract to which the applicant was admitted. We use this information to compute the variables "number of contracts on the ROL (data)" and "number of programs on the ROL (data)".

Empirical strategy

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

$$Y_{its} = \alpha + \beta_{2013} \cdot T_{ts} \cdot (t = 2013) + \beta_{2014} \cdot T_{ts} \cdot (t = 2014) + X_{it} \cdot \Gamma + \eta_s + \nu_t + \varepsilon_{its}. \tag{1}$$

The variable Y_{its} is an indicator of dominated choices in applicant i's ranking of program s in year t. The variable T_{ts} is an indicator that equals one if t is equal to 2013 or 2014 and s is a program that was affected by the severe funding reduction of the 2012–13 reform, and zero otherwise. The model includes program fixed effects (η_s), year fixed effects (ν_t), a vector of individual-specific controls (X_{it}), and an error term (ε_{its}). The year fixed effects control for changes that affected all applications in a given year. Our parameters of interest are β_{2013} and β_{2014}. These parameters measure the effect of the funding cuts, which we interpret as a rise in the selectivity of admission to the funded contract, on dominated choices. We estimate the model on the application level, where an *application* is a program in an ROL (with up to two contracts). We exclude observations from 2012 since the elimination of many funded programs in that year complicates the analysis and obscures the interpretation of the results.

The causal interpretation of β_{2013} and β_{2014} relies on two key assumptions. First, in the absence of the reform, the prevalence of dominated choices in different programs would have evolved in tandem (parallel trends). Second, the composition of the students applying to programs with a severe funding cut and students applying to programs with little or no funding cut remained stable over time. We evaluate the plausibility of these assumptions and the robustness of our estimates to their violation at the end of this section, where we also provide evidence in support of our economic interpretation that the estimates are driven by the increase in selectivity and not by other contemporaneous changes.

Graphical illustration

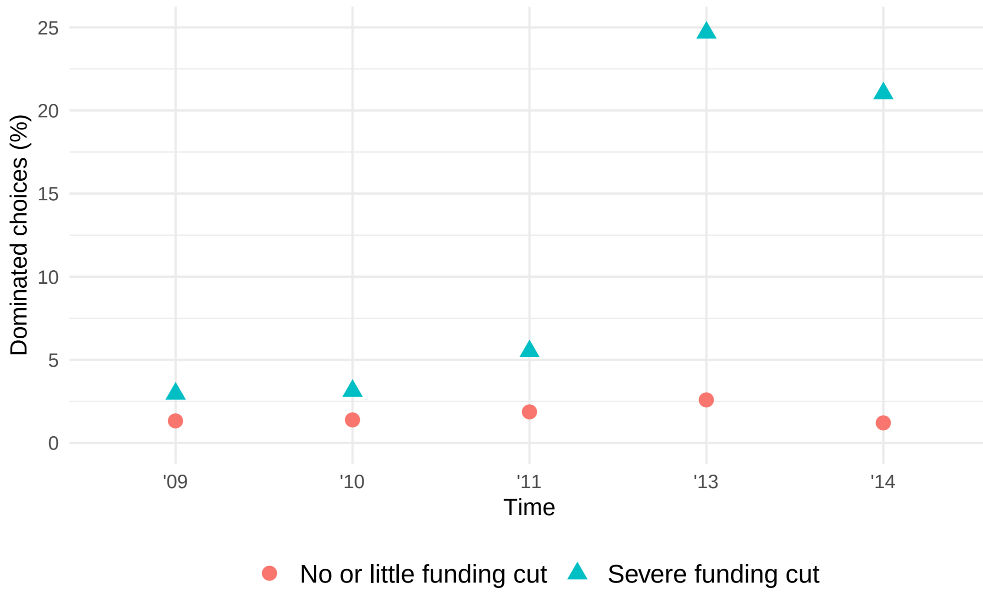
Figure 1 provides a graphical illustration of the results of our difference-in-differences empirical strategy. The figure shows that the rate of dominated choices in the programs that experienced little or no funding cut remained at pre-reform levels. This suggests that other contemporaneous changes (e.g., the introduction of the study contract) had little effect on making dominated choices in applications to these programs. By contrast, dominated choices increased sharply from 5.5 percent to 24.7 percent in the programs that were affected by the severe funding reduction of the 2012–13 reform. The effect of the reform persisted in 2014: the rate of dominated choices was 21 percent in the affected programs.

Results

Table 4 presents our difference-in-differences estimates of the effect of admission selectivity on dominated choices. Our baseline specification (column (1)) indicates that the reform increased dominated choices by 19.3 percentage points among treated programs from a baseline of 6.3 percent in 2013.[11] The estimated effect in 2014 is similar, 17.9 percentage points. Columns (2)–(3) show that controlling for demographics, academic achievement, and high-school fixed effects barely changes the estimates

[11] 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 is 0.9 percentage points.

Figure 1: The effect of admission selectivity on dominated choices: 2012–13 reform



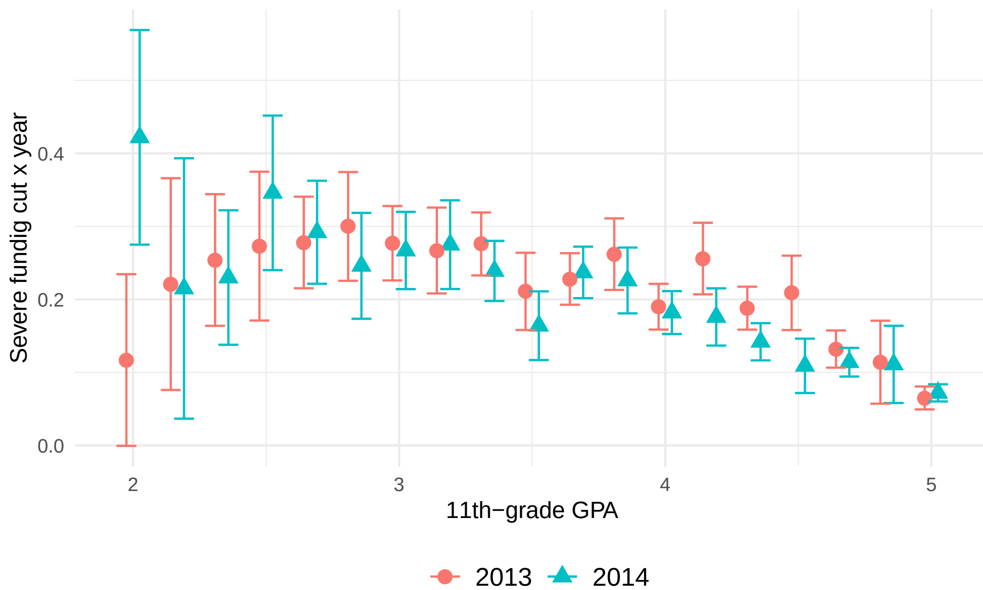
Notes: The figure presents the share of applications with dominated choices over time, split by the severity of the funding cut in the reform.

and their precision. Appendix Table C1 repeats this analysis using only the highest ranked application in each ROL, and finds similar results. Appendix Table C2 shows that the effect holds for both revealed flipping and dropping, but the magnitude of the effect on revealed 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 state-funded programs. The percentile ranks increased for 88 percent of the treated programs, and the average change was almost 9 percentiles in 2013. The reduction in the number of funded positions in the directly affected programs made the system as a whole more selective through general equilibrium effects. If students who applied to programs that were not affected directly took these general equilibrium effects into account when submitting their application, then our estimates should provide lower bounds on the causal effect of admission selectivity on dominated choices.

In Table 5 we examine whether the effect of admission selectivity on dominated choices is heterogeneous across various subgroups. The corresponding regressions include interactions of treatment and subgroup dummies, and controls for demographics, academic achievement, and high-school fixed effects (as in column (3) of Table 4). We find that the effect of admission selectivity on dominated choices is 2.7 (5.6) percentage points lower for female applicants in 2013 (2014). The causal effect of admission selectivity is lower for applicants claiming disadvantaged status and applicants with high 11th-grade GPA (Figure 2). In Appendix C we use additional measures of socioeconomic status and academic achievement and find similar results. These results suggest that applications for which dominated choices cause a higher expected utility loss are less responsive to increases in admission selectivity.

Figure 2: The effect of admission selectivity on dominated choices by 11th-grade GPA: 2012–13 reform



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

Table 4: The effect of admission selectivity on dominated choices: 2012–13 reform

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

Notes: The table presents the effect of admission selectivity on dominated choices. Robust standard errors clustered on the applicant level are in parentheses. The number of observations is 729,650, which corresponds to 229,009 ROLs among high-school senior applicants. The mean outcome in the sample is 3.6 percent. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. Missing control variables are indicated by a separate dummy variable.
***: p<0.01, **: p<0.05, *: p<0.1.

Table 5: Heterogeneity: The effect of admission selectivity on dominated choices: 2012–13 reform

	A. *Gender*		B. *Disadvantaged*		
	Male	Female	No	Yes	
Severe funding cut × 2013	0.202***	0.175***	0.193***	0.087***	
	(0.007)	(0.005)	(0.004)	(0.011)	
Severe funding cut × 2014	0.209***	0.153***	0.177***	0.093***	
	(0.007)	(0.005)	(0.004)	(0.011)	
Counterfactual mean (2013)	0.071	0.057	0.063	0.041	
Counterfactual mean (2014)	0.049	0.032	0.024	0.006	
R-squared	0.137		0.137		
			C. *Field of study*		
	Business/ economics	Legal studies	Social sciences	Adult education	Medicine (placebo)
Severe funding cut × 2013	0.194***	0.169***	0.146***	0.118***	−0.018***
	(0.005)	(0.010)	(0.010)	(0.023)	(0.002)
Severe funding cut × 2014	0.174***	0.189***	0.151***	0.086***	−0.006***
	(0.005)	(0.010)	(0.010)	(0.020)	(0.002)
Counterfactual mean (2013)	0.060	0.090	0.081	0.044	0.015
Counterfactual mean (2014)	0.044	0.074	0.066	0.028	−0.001
R-squared			0.136		

Notes: The table presents the effect of admission selectivity on dominated choices by various subgroups. Each panel estimates the coefficients in a single regression by interacting the treatment variable with subgroup indicators. Robust standard errors clustered on the applicant level are in parentheses. The number of observations is 729,650, corresponding to 229,009 ROLs. The mean outcome in the sample is 3.6 percent. The counterfactual mean denotes the counterfactual mean outcome of the treated group in 2013/2014. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. All specifications include high-school fixed effects (as in column (3) of Table 4). ***: $p<0.01$, **: $p<0.05$, *: $p<0.1$.

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

Threats to identification and robustness

We assess the plausibility of our identifying assumptions in various ways. To assess the parallel trends assumption we include placebo variables of the treated programs in the pre-reform period; i.e., we estimate the effect of the "reforms" that did not occur in 2009 and in 2010. Columns (1) and (2) of Table 6 add these placebo treatment variables to the baseline model. Although the placebo coefficients for 2009 and 2010 are statistically significant, they are an order of magnitude lower than our main estimates and precisely estimated. Thus, the potential for bias due to the violation of the parallel trends assumption is small.

We also study a smaller-scale reform that took place in 2011, prior to the introduction of the study contract. This reform, that received much less attention from the media and the public, decreased the number of tuition waivers in business/economics and social sciences by about 20 percent (see Table 2). We investigate whether this reform had a similar impact on dominated choices. We add indicator variables to our main specification that take the value of one in 2011 for social sciences and business/economics. Appendix Table C3 presents the results. We find that this smaller reform increased dominated choices by 1.2–1.3 percentage points in the affected fields. In Appendix C.2 we show that our results hold in an alternative specification that leverages all the variation in the number of funded positions during our sample period.

A potential threat to our identification strategy is that treatment status is defined by applicants' choice of ROLs. Applicants' responses to the reform may affect the composition of their ROL as well as their decision to apply to college at all. This concern is particularly pronounced for students who are not willing (or able) to pay the tuition and are considering applying only to stat-funded programs. Such applicants never make dominated choices. As a response to the reduction in funded positions, these applicants might drop their most preferred (treated) program from their ROL and rank untreated programs instead, biasing our estimates upward (since ROLs that contain no sulf-funded contracts are free of dominated choices by definition).

Table 6: Robustness: The effect of admission selectivity on dominated choices: 2012–13 reform

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

Notes: The table presents DiD estimates of the effect of admission selectivity on dominated choices. Columns (1) and (2) add placebo indicators for 2009 and 2010, columns (3) and (4) restrict the sample to applicants applying to both treated and untreated programs (balanced subsample), columns (5) and (6) restrict the sample to *relevant ROLs* – ROLs that include at least one self-funded contract. 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.

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

Next, we address this threat to our identification strategy in several other ways. First, in columns (2)–(3) of Table 4 we add applicant-level controls. Second, we restrict the sample to those high-school senior applicants that applied to both treated and untreated programs (columns (3) and (4) of Table 6). This restriction assures that the composition of applicants in the treated and untreated fields is the same (balanced subsample).[12] We find that the coefficient estimates remain positive, large, and statistically significant, confirming that changes in the composition of applicants do not drive our results.

Third, we look at applicants who listed at least one unfunded contract in their ROL. By listing at least one unfunded contract, these applicants indicate that they are willing to pay tuition; hence we find it less plausible that the reform affected the set of programs in their ROL.[13] Reassuringly, our estimates for this subsample are very similar to the main estimates (columns (5) and (6) of Table 6), indicating that switching behavior does not drive our results.[14]

4.2 Evidence from Within-ROL Variation in Admission Selectivity

Our second empirical strategy exploits the fact that applicants list several programs in their ROL with distinct admission selectivity. We show that applicants are more likely to make dominated choices with respect to more selective programs in their ROL.

Empirical strategy

To estimate the effect of admission selectivity on dominated choices, we specify the following model:

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

The variable Y_{its} is an indicator of dominated choices in applicant i's ranking of program s in year t. The variable priority-score cutoff$_{t-1,s}$ is our measure of admission selectivity. It denotes the within-year percentile rank of the funded contract of program s one year prior to the application $(t-1)$. For ease of comparison, we abstract from the fact that different fields of study use different weighting

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

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

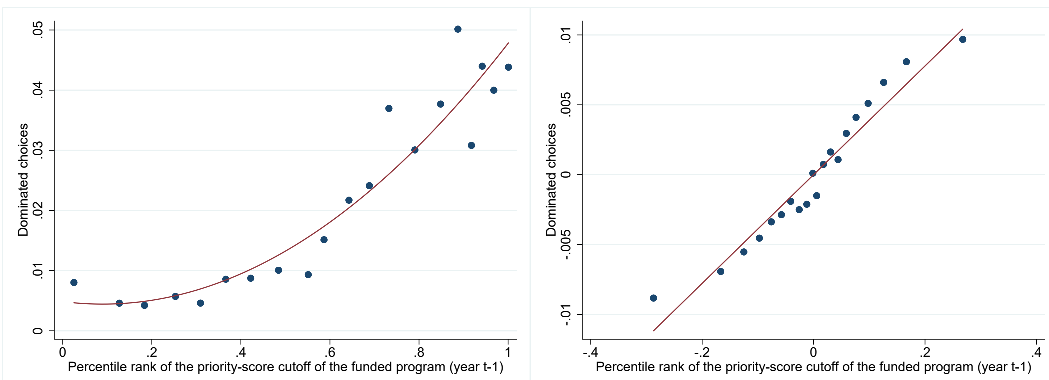
[14] A weakness of this approach is that applicants who would have listed only funded contracts in their ROL in the absence of the reform might have added the unfunded version of these programs to their ROL. Such behavior would change the composition of the treated group, but in the absence of any treatment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

schemes, and we normalize the priority-score cutoffs to within-year percentile ranks.[15] The model includes fixed effects for program characteristics (X_{ts}), such as type of degree (BA or BA–MA), time schedule (full time or evening programs), field of study, and program location. The model also includes ROL-fixed effects (η_{it}) and an error term (ε_{its}). Our parameter of interest is β, which measures the effect of admission selectivity (as measured by lagged funded priority-score cutoffs) on dominated choices. We estimate the model on the application level. We focus on the years 2009–2011, the years prior to the introduction of the study contract.

Graphical illustration

Figure 3 presents the relationship between admission selectivity and dominated choices. Panel (a) demonstrates that, conditional on appearing in an ROL, dominated choices are more likely to occur in applications to more selective programs. Specifically, dominated choices are five times more likely to occur in applications to programs in the top quintile of the admission selectivity distribution than in applications to programs in the bottom quintile.

Figure 3: The effect of admission selectivity on dominated choices: A within-ROL comparison



(a) Unconditional

(b) Within-ROL, full controls

Notes: The figure plots the selectivity of admission against the rate of dominated choices. The sample covers applications between 2009 and 2011. We exclude students who listed at least one program which does not have a lagged priority-score cutoff, leaving us with 110,398 ROLs, corresponding to 351,884 programs. Admission selectivity is measured as the within-year percentile rank of the funded contract's priority-score cutoff one year prior to the application. Panel (a) plots the bin-specific means conditional on year fixed effects. Panel (b) plots the bin-specific means conditional on ROL, field, degree, schedule, and location fixed effects (column (5) of Table 7). An increase in selectivity of 10 percentiles causes a 0.39 percentage points rise (s.e.: 0.02) in the probability of making a dominated choice.

We cannot attribute a causal interpretation to the results depicted in Figure 3 (a) for several reasons. First, students sort into programs based on ability. Since academic ability and dominated choices are negatively correlated, it is reasonable to assume that due to sorting, Figure 3 (a) understates the effect of admission selectivity on dominated choices. Second, programs differ along more dimensions than just admission selectivity (e.g., content, location, etc.), which confounds the positive

[15]Since lagged priority-score cutoffs are not defined in the year a program is launched, we exclude ROLs that include such programs. We also disregard programs in the fields of art and art mediation, since these programs have eligibility exams and practical exams, and their priority scores are not calculated in the standard way.

relationship between admission selectivity and dominated choices. Our empirical strategy addresses sorting by adding ROL fixed effects and accounts for differences between programs by adding fixed effects for program characteristics.

Results

Table 7 presents our within-ROL estimates of the effect of admission selectivity on dominated choices. We identify this slope from ROLs that include programs with distinct admission selectivity. Our baseline specification (column (1)) indicates that a 10 percentile increase in admission selectivity (as measured by the lagged state-funded priority score cutoff) has a casual effect of 0.35 percentage points on dominated choices. Columns (2)–(5) show that controlling for program characteristics barely changes the estimates and their precision. Figure 3 (b) illustrates the results of our most preferred specification (column (5) of Table 7).[16] Appendix Tables C6 and C7 show that the effect holds for both revealed flipping and dropping, but the magnitude of the effect on revealed dropping is much larger, both in absolute and in relative terms.

Table 7: Admission selectivity and dominated choices: A within-ROL comparison

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

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

In Table 8 we examine whether the effect of admission selectivity on dominated choices is heterogeneous across various subgroups. The corresponding regressions add interactions of the (lagged) priority-score cutoff and subgroup dummies to our most preferred specification (column (5) of Table 7). The patterns we document are similar to the ones we find using our first empirical strategy (Section 4.1). The effect of a 10-percentile increase in admission selectivity on dominated choices is of a 0.03 percentage point lower for female applicants. The causal effect of admission selectivity is lower for applicants claiming disadvantaged status and applicants with high 11th-grade GPA (Figure 4). In Appendix C we use additional measures of socioeconomic status and academic achievement and find similar results.

A weakness of this strategy is that the within-ROL variation in the selectivity of admission might be too narrow to identify the full effect. There may also be unobserved factors that we do not control

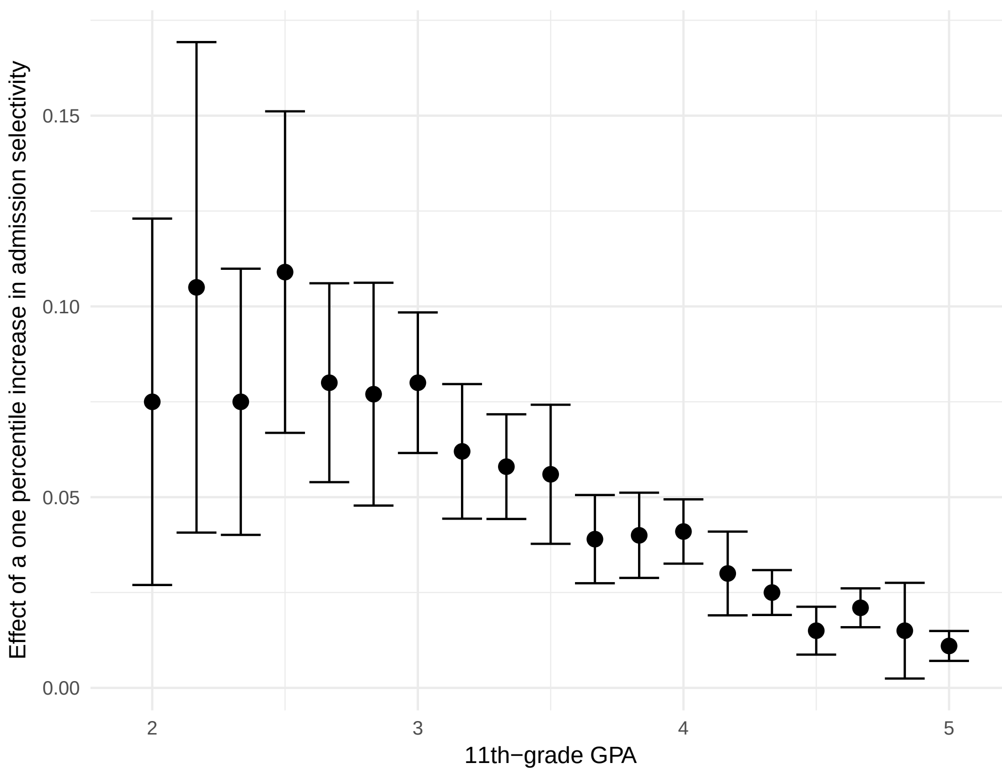
[16] Shorrer and Sóvágó (2022) perform a similar analysis, but do not restrict attention to high-school seniors, and to the pre-reform period. They also use contemporaneous, rather than lagged, priority score cutoffs. The correlation that they document is similar.

Table 8: Heterogeneity: Admission selectivity and dominated choices: A within-ROL comparison

	A. *Gender*		B. *Disadvantaged*	
	Male	Female	No	Yes
Priority-score cutoff	0.041***	0.038***	0.041***	0.021***
	(0.002)	(0.002)	(0.002)	(0.003)
Within R-squared	0.011		0.011	

Notes: The table presents the effect of admission selectivity on dominated choices. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with subgroup indicators. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs. The mean outcome in the sample is 2.2 percent. All specifications include ROL, field, degree, schedule, and program location fixed effects (as in column (5) of Table 7).
***: p<0.01, **: p<0.05, *: p<0.1.

Figure 4: Admission selectivity and dominated choices by 11th-grade GPA: A within-ROL comparison



Notes: The figure presents the effect of admission selectivity on dominated choices by 11th-grade GPA with 95% confidence intervals. Robust standard errors are clustered on the applicant level. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with 11th-grade GPA. We include ROL, field, degree, schedule, and program location fixed effects (as in column (5) of Table 7). The estimated effect on applicants with a missing 11th-grade GPA is 0.043 (s.e.: 0.004).

for. In light of the stability of the estimates in Table 7, this latter concern seems less likely.

5 Discussion

Shorrer and Sóvágó (2022) show that 11 percent of Hungarian college applicants make revealed dominated choices, and that 12–18 percent of these dominated choices are costly, costing the applicant more than $6,600 on average. In this paper, we show that increased selectivity in admissions to state-funded contracts (i.e., lower probability of admission all else equal) raises the rate of revealed dominated choices. Furthermore, the effect is stronger for high-SES applicants (who presumably have lower marginal utility from money) and for low academic achievers (who can expect lower admission priority). These findings suggest that applicants make more dominated choices when their expected cost is lower.

An applicant that makes a revealed dominated choice is forgoing the free opportunity to receive a tuition waiver worth thousands of dollars even though this behavior has no benefit – at least in terms of her final assignment. What, then, can explain this surprising behavior?

Revealed dominated choices are only *weakly* dominated according to the standard model of matching market design. Thus, one possibility is that applicants are behaving (approximately) optimally and those who make dominated choices are (nearly) certain that no dominating strategy would yield higher payoff. In our context, these applicants should be (nearly) certain that their priority score will be lower than the cutoff of the state-funded contract that they drop or flip. Since the expected cost of dominated choices is high, this explanation requires applicants to hold overly pessimistic beliefs about their probability of passing the these cutoffs. Many of our findings are consistent with this theory. In particular, increases in the selectivity of state-funded positions reduce, all else equal, the expected cost of dominated choices. Furthermore, all else equal, high-SES applicants are more likely to become nearly indifferent when the probability of being admitted with state-funding drops sharply (since their marginal utility from money is lower).

It is also possible that applicants do not understand how to play optimally under DA, or that they do not believe the description provided by the clearinghouse. Such disbelief can arise from mistrust in the clearinghouse or from believing that one can "magically" influence events that are objectively outside of her control (e.g, that asking for funding bring bad luck). Our findings are consistent with some theories in this broad family of explanations.

Another possibility is that the standard model of matching market design ignores an important aspect of applicants preferences. Other-regarding preferences are a natural candidate in our context. In particular, our findings are consistent with applicants trading off the warm-glow they get from not asking for state-funding (Andreoni, 1990) against the monetary loss they may incur. It is worth mentioning, in this context, that the rate of dominated choices is non-negligible even among disadvantaged applicants (Shorrer and Sóvágó, 2022).

There are other non-traditional preferences that can explain our findings. Dreyfuss et al. (2022) and Meisner and von Wangenheim (2019) show that expectation-based loss aversion can explain the patterns we document, since applicants may "manage their expectations" to avoid disappointment once they learn the results of the match. Meisner (2022) shows that these patterns can emerge when applicants derive utility from being assigned an option they rank higher, since this motive gives applicants incentive to lower the ranking of desirable options when chances of admission are low. Finally, ego utility (Kőszegi, 2006) may cause applicants to distort their choices with the goal of avoiding information that is detrimental to their self image. This latter explanation seems less plausible in our context, since applicants learn their priority score, and priority-score cutoffs become public after the match.

6 Conclusions

Previous studies mainly focused on the properties of market clearing algorithms, giving special attention to strategic simplicity. As pointed out by Pathak (2017), "*[e]fforts to improve how participants interact with market designs ... hold great promise to complement research on market clearing algorithms.*"

Our findings indicate that interventions affecting applicants' perceptions of the likelihood of admission (e.g., giving publicity to affirmative action policies) could have a large impact on the realized allocation, even when the mechanism is strategy-proof. This indication is supported by the findings of Bobba and Frisancho (2015), who study the Mexico City high-school assignment system. They show that providing applicants with a signal about their priority score causes those applicants who are pessimistic about their performance to apply and to be assigned to more selective schools. Such interventions may therefore have implications for equity if lower SES applicants have a less favorable or less accurate perception of their admission chances.[17]

School systems around the world have different policies regarding the timing in which preferences are reported. The standard model of matching market design does not provide guidance on the timing in which preferences should be collected, and different school systems apply very different policies. Our findings indicate that this design choice may be consequential, as the timing of preference reporting has a dramatic effect on applicants' perceptions of the likelihood of admission: when applicants know their priority score prior to submitting their ROL to the clearinghouse, their beliefs can be substantially more precise (Artemov et al., 2022). In China, where different provinces have different policies, an increasing share of provinces are allowing students to report their preferences after learning their score on the college entrance exam (Wu and Zhong, 2014).

[17] In a strategically demanding environment, Kapor et al. (2020) showed that the beliefs of low-SES applicants are less precise and that errors in beliefs have a significant impact on application patterns.

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A Data Appendix

In this Appendix, we present information on additional data sources that we use in our robustness analysis. In Appendix A.1, we describe these additional data sources, and, in Appendix A.2, we explain how match our main administrative dataset to the National Assessment of Basic Competencies dataset. None of the results presented in the main text rely on these additional data sources.

A.1 Additional Data Sources

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

The third data source is the National Assessment of Basic Competencies (NABC). The objectives of the NABC are similar to those of the Programme for International Student Assessment (PISA). It measures literacy and numeracy skills in a standardized way, making the scores comparable across years and cohorts. Between 2006 and 2007, the NABC covered a large sample of students, and since 2008 it has covered all students in the 6th, 8th, and 10th grades, except for those who were absent from school on the day that the exam was administered. The NABC is a low-stakes exam from the students' perspective: it is graded blindly by the Ministry of Education and only summary statistics of scores are reported to schools.

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

A.2 Matching College Admissions Data to the NABC

The administrative datasets we use do not contain unique individual identifiers. We match them based on demographic information: year and month of birth, gender, postal code, and high-school identifier. The NABC dataset contains information on a large sample of 10th-grade students from 2006, and on the entire population since 2008. Therefore, for each year, we match only high-school senior applicants to the NABC. Whenever the match is not unique, we calculate the average test scores of matched individuals. We were able to match 179,039 applicants out of 268,981 (67 percent between 2009 and 2014, and 80 percent between 2011 and 2014). The match is unique for about 149,148 observations (55 percent).

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

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

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

We match high-school senior applicants to the NABC dataset based on observable demographic characteristics: year and month of birth, high-school identifier, gender, and postal code. Traditionally, students attend high school for four years. However, since 2004, certain schools have been offering five-year programs in which the first year is dedicated to foreign languages. Students complete the NABC exam in the second year of high school, irrespective of the type of program; therefore, the time lag between the competency test and the matriculation exam can be two or three years.

Table A1 describes the result of the matching. The more variables we use for matching, the fewer applicants we are able to match. Between 2011 and 2014, when the NABC covers the full population of tenth graders who took the exam between 2008 and 2011, the share of matched students is stable. We are able to match 91–92 percent of the high-school senior applicant sample based on 3 variables, 89–90 percent based on 4 variables, and 75–80 percent based on 5 variables. The share of unique matches is also stable in these years: 16–20 percent of the high-school senior applicant sample based on 3 variables, 41–44 percent based on 4 variables, and 63–65 percent based on 5 variables. With the exception of 2009, as the matching becomes finer, we can match more individuals uniquely. The reason for the irregularity in 2009 is twofold. First, since we do not observe the full population, the match cannot be refined by including more matching variables (due to empty cells). Second, in 2006–2007, the postal code was self-reported, leading to stronger attrition as we include the postal code among the matching variables. In our main analysis we use the matching that is based on 5 variables (Panel C).

Table A1: Matching college admissions data to NABC

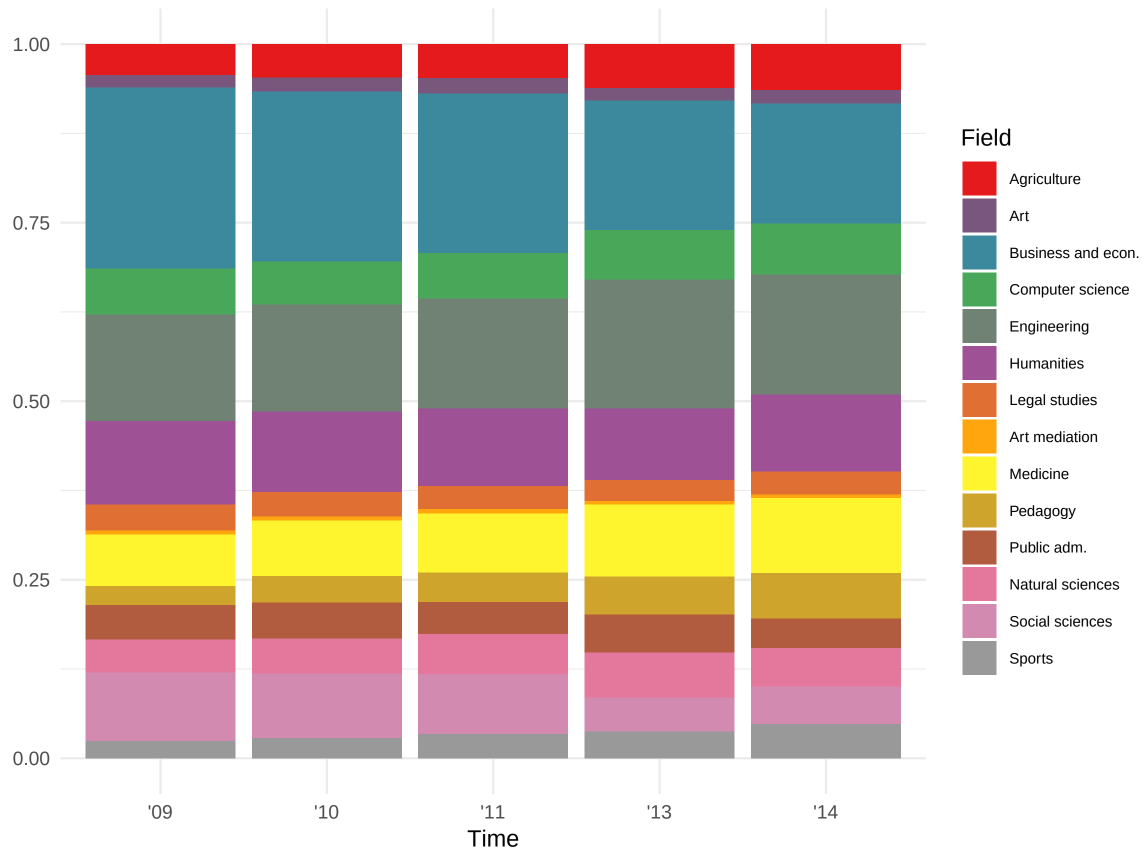
	Matched individuals		Uniquely matched individuals	
	Share (%)	Count	Share (%)	Count
	(1)	(2)	(3)	(4)
A. *Matching based on 3 variables*				
2009	89.2	45,280	28.8	14,632
2010	89.7	45,060	22.1	11,080
2011	91.8	44,941	19.5	9,544
2012	91.6	36,421	18.5	7,365
2013	91.2	35,470	15.8	6,133
2014	92.2	37,241	16.6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

Notes: The table describes the outcome of matching the NABC dataset to the high-school senior applicant sample (N = 268,981). Matching based on 3 variables: year of birth, gender, and school identifier; matching based on 4 variables: year and month of birth, gender, and school identifier; matching based on 5 variables: year and month of birth, gender, school identifier, and postal code. The NABC is conducted two or three years before applicants' senior year. We are thus unable to match seniors who moved to a new postal code or to a new high school between taking the NABC and applying to college.

B The Composition of High-school Senior Applicants over Time

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

Figure B1: Distribution of applications by field of study



Notes: The figure depicts the distribution of applications by field of study. Each observation corresponds to a program in a given ROL. The figure does not display the year 2012, since the reform eliminated the availability of funding in some programs in that year (see Section 2.2).

Table B1: Individual-level summary statistics over time

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

Notes: The table reports mean values of student characteristics, with standard deviations in parentheses over time. Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average grade in Hungarian grammar and literature and mathematics. Grades are standardized among eligible applicants. Some applicants have no incentive to report their GPA to the clearinghouse. Applicants with a high matriculation exam scores relative to their high-school GPA have no incentive to report their GPA, as it has no effect on their priority score. As a result, 11th-grade GPAs are missing for 18 percent of high-school senior applicants. The number of contracts on the ROL is reported administratively. Our data includes at most 7 contracts from each ROL: six contracts that are ranked the highest and the contract to which the applicant was admitted. We use this information to compute the variables "number of contracts on the ROL (data)" and "number of programs on the ROL (data)." 11

C Additional Results

Appendix C.1 provides additional results on the effect of selectivity on dominated choices exploiting the 2012–13 reform. Appendix C.2 presents estimates that exploit all variations in the availability of funded positions in the sample. Finally, Appendix C.3 provides additional results on the effect of selectivity on dominated choices using within-ROL comparisons.

C.1 The Effect of Admission Selectivity on Dominated Choices: 2012–13 reform: Robustness

In this Appendix, we present four additional model specifications for the effect of admission selectivity on dominated choices, using our difference-in-differences identification strategy. First, in Table C1, we estimate the effect of the 2012–13 reform on dominated choices using only the highest ranked application in each ROL. Second, in Table C2, we estimate the effect of the 2012–13 reform on revealed dropping and on revealed flipping separately. Third, in Table C3, we analyze the effect of a small-scale reform occurred in 2011. Fourth, in Table C4, we present heterogeneous effects by the NABC-based numeracy and NABC-based SES measures.

Table C1: The effect of admission selectivity on dominated choices: 2012–13 reform: Highest ranked application

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

Notes: The table presents the effect of admission selectivity on dominated choices. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. The share of dominated choices is 3.2 percent.

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

Table C2: The effect of admission selectivity on revealed dropping and on revealed flipping: 2012–13 reform

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

Notes: The table presents the effect of admission selectivity on dominated choices. Robust standard errors clustered on the applicant level are in parentheses. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. The share of revealed dropping (flipping) is 3.1 (0.5) percent. In the baseline, the rate of revealed dropping among treated applications was 5.5 (4.2) percent in 2013 (2014). In the baseline, the rate of revealed flipping among treated applications was 1.0 (0.7) percent in 2013 (2014).

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

Table C3: The effect of admission selectivity on dominated choices: 2011 reform

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

Notes: The table presents the effect of admission selectivity on dominated choices. 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 C4: Heterogeneity: The effect of admission selectivity on dominated choices: 2012–13 reform

	A. *NABC numeracy skill*				
	Q1	Q2	Q3	Q4	Q5
Severe funding cut × 2013	0.227***	0.199***	0.195***	0.180***	0.114***
	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Severe funding cut × 2014	0.212***	0.195***	0.173***	0.134***	0.120***
	(0.010)	(0.010)	(0.009)	(0.008)	(0.009)
Counterfactual mean (2013)	0.073	0.063	0.058	0.052	0.047
Counterfactual mean (2014)	0.045	0.039	0.032	0.027	0.021
R-squared			0.138		

	B. *NABC-based SES*				
	Q1	Q2	Q3	Q4	Q5
Severe funding cut × 2013	0.150***	0.156***	0.170***	0.193***	0.209***
	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)
Severe funding cut × 2014	0.141***	0.155***	0.173***	0.177***	0.174***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
Counterfactual mean (2013)	0.049	0.056	0.058	0.061	0.064
Counterfactual mean (2014)	0.025	0.029	0.030	0.035	0.038
R-squared			0.137		

Notes: The table presents the effect of admission selectivity on dominated choices by various subgroups. Each panel estimates the coefficients in a single regression by interacting the treatment variable with subgroup indicators. Robust standard errors clustered on the applicant level are in parentheses. The number of observations is 729,650, which corresponds to 229,009 ROLs. The mean outcome in the sample is 3.6 percent. The counterfactual mean denotes the counterfactual mean outcome of the treated group in 2013/2014. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. All specifications include high-school fixed effects (as in column (3) of Table 4).
***: p< 0.01, **: p<0.05, *: p<0.1.

C.2 The Effect of Admission Selectivity on Dominated Choices: Alternative Specification

Section 4.1 established that admission selectivity has a large positive causal effect on dominated choices. We test the robustness of this result by considering an alternative specification. Instead of focusing solely on the 2012–13 reform, we exploit all variations in the availability of funded positions in the sample (Table 2). This alternative approach allows us to estimate the elasticity with respect to the available funded positions and dominated choices.

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

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

where capacity_{tf} denotes the number of available funded positions in year t and field of study f (to which s belongs). We index capacity by f to highlight that there is no within-field-of-study variation in the number of available funded positions.[20] In line with our main result, we expect the estimate of β to be negative, as more available funded seats correspond to lower admission selectivity. On the other hand, the 2012–13 reform was salient and stark relative to other changes that were small and sometimes inconsequential, which limits the comparability of this specification to our main findings.

Table C5 presents our estimates. We find that a 10-percent reduction in the number of funded seats increases dominated choices by 0.75–0.79 of a percentage point.

Table C5: The effect of admission selectivity on dominated choices: Alternative specification

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

Notes: The table presents estimates of the effect of the number of available funded positions on dominated choices. Robust standard errors clustered on the applicant level are in parentheses. Columns (1) and (2) use the realized number of funded positions in 2009–2011, and columns (3) and (4) use the publicly released funded quotas in 2009–2011. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. Missing control variables are indicated by a separate dummy variable.
***: p<0.01, **: p<0.05, *: p<0.1.

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

C.3 The Effect of Admission Selectivity on Dominated Choices: A within-ROL comparison: Robustness

Table C6: Admission selectivity and dominated choices: A within-ROL comparison: Revealed dropping

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

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

Table C7: Admission selectivity and dominated choices: A within-ROL comparison: Revealed flipping

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

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

Table C8: Admission selectivity and dominated choices: A within-ROL comparison: Application Fee Structure Comprehension

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

Notes: The table presents the effect of admission selectivity on dominated choices. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with subgroup indicators. An applicant must understand (MU) the application fee structure if she ranked four or more contracts with three or fewer programs. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs. The mean outcome in the sample is 2.2 percent. All specifications include ROL fixed effects.
***: p< 0.01, **: p<0.05, *: p<0.1.

Table C9: Heterogeneity: Admission selectivity and dominated choices: A within-ROL comparison

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

	B. *NABC-based SES*				
	Q1	Q2	Q3	Q4	Q5
Priority-score cutoff	0.027***	0.035***	0.040***	0.036***	0.059***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Within R-squared			0.011		

Notes: The table presents the effect of admission selectivity on dominated choices. We estimate all the coefficients in a single regression by interacting the lagged priority-score cutoffs with subgroup indicators. Robust standard errors clustered on the applicant level are in parentheses. The sample covers the period between 2009 and 2011. The number of observations is 351,884, which corresponds to 110,398 ROLs. The mean outcome in the sample is 2.2 percent. All specifications include ROL, field, degree, schedule, and program location fixed effects (as in column (5) of Table 7).
***: p< 0.01, **: p<0.05, *: p<0.1.