

Dominated Choices in a Strategically Simple College Admissions Environment: The Effect of Admission Selectivity

Ran I. Shorrer*

Sándor Sóvágó[†]

Abstract

Although many centralized school assignment systems use the strategically simple Deferred Acceptance mechanism, applicants often make dominated choices. Using administrative data from Hungary, we show that many college applicants forgo the free opportunity to receive a tuition waiver. Using two empirical strategies, we provide causal evidence that applicants make more such dominated choices when applying to programs where tuition waivers are more selective. Our results suggest that dominated choices are more common when their expected utility cost is lower. But, a non-negligible share of these dominated choices are consequential and when they are, the cost is significant, averaging more than 3,000 dollars.

*Pennsylvania State University. E-mail: rshorrer@gmail.com.

[†]University of Groningen. E-mail: s.sovago@rug.nl.

This version: April 2022. The authors thank Nageeb Ali, Georgy Artemov, Christopher Avery, Estelle Cantillon, Kathie Baldiga Coffman, Thomas Buser, Oren Danieli, Sabien Dobbelaere, Michael Gechter, Yinghua He, Zoltán Hermann, Keisuke Hirano, Dániel Horn, Scott Kominers, Kala Krishna, David Laibson, Shengwu Li, Hessel Oosterbeek, Jenő Pál, Noémi Péter, Erik Plug, Alex Rees-Jones, Martin Rotemberg, Al Roth, Ben Roth, László Sándor, Melinda Tír, Bas van der Klaauw, Luyao Zhang, and seminar participants in Amsterdam, Budapest, St. Gallen, Groningen, Rotterdam, Philadelphia, Tsinghua BEAT in Beijing, the ASSA Annual Meeting in Atlanta and Philadelphia, the 13th Workshop of Matching in Practice at ULB, the 70th European Meeting of the Econometric Society in Lisbon, the 15th Summer Institute of HAS in Budapest, the 3rd Workshop on Marketplace Innovation at Stanford, the 3rd Workshop on Algorithmic Game Theory and Data Science, the 28th International Conference on Game Theory at Stony Brook, and the 19th ZEW Summer Workshop for Young Economists for their helpful comments. We are particularly grateful to Péter Bíró for his support, encouragement, and insightful remarks. This research was supported by the United States – Israel Binational Science Foundation (BSF grant 2016015).

1 Introduction

Millions of students around the world are assigned to schools through centralized assignment systems. An increasing share of these school assignment systems adopt strategically simple mechanisms like deferred acceptance (DA), where ranking alternatives in a way that is inconsistent with one’s preferences is a dominated choice.¹ But recent evidence on applicant behavior in a wide range of settings—ranging from medical residencies in the U.S. to public school choice in Mexico—suggests that many applicants make dominated choices when DA is in place.² These applicants give up the opportunity to be assigned to alternatives that they prefer, even though this behavior does not benefit them. Across settings, more dominated choices were detected among weaker applicants, and with respect to more selective alternatives. Yet, the mechanisms underlying this pattern are not well understood.

While admission selectivity is correlated with dominated choices, it is challenging to establish a causal relationship for several reasons. First, to identify dominated choices, one needs information on preferences that is independent of observed choices. Second, even if information on the preferences is available, search costs, which are hard to measure, may prevent students from applying to certain alternatives. Third, admission chances are often correlated with cognitive ability, making it difficult to disentangle the effect of admission selectivity.

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 inform the design of decision support tools and choice architecture to aid applicants choose according to their preferences. Second, it can help to make more accurate counterfactual predictions in centralized school-assignment systems.

In this paper, we estimate the causal effect of admission selectivity on dominated choices using administrative data from the Hungarian college admissions system. The Hungarian setting is particularly suited for overcoming the above-mentioned challenges for several reasons. First, the Hungarian centralized clearinghouse uses a strategically simple version of the Deferred Acceptance mechanism to assign approximately 100,000 applicants each year. A special feature of this market is that certain alternatives have an intrinsic natural ranking: admission to the same study program with and without financial aid. This natural ranking provides information on preferences that is independent of observed choices. Second, as these naturally ranked alternatives are related to the same study programs, search costs cannot rationalize unexpected preferences reports (and, as we discuss below, the user interface makes this explanation particularly unlikely). Third, in our sample period, the Hungarian government made funding more scarce in certain fields of study, which provides us with exogenous variation in the selectivity of admission to funded programs.

We follow [Hassidim et al. \(2021\)](#) and say that an applicant makes a *revealed dominated choice* when she submits a Rank-Order List (ROL) that is inconsistent with the natural ranking. Such an applicant forgoes the free opportunity to receive a tuition waiver worth thousands of dollars. The “dominated choice” label is appropriate as under the standard assumptions in the matching market design literature—that agents know the rules of the game and that their utility depends only on their realized assignment—this behavior has no benefit.³ Of note, revealed dominated choices are a specific subset of all dominated choices—those that involve a specific way of foregoing financial aid. We cannot detect other dominated choices, such as ones involving the choice of major. Recognizing this limitation, for conciseness, since there is no risk of confusion, when we discuss dominated choices in the Hungarian context, we refer only to revealed dominated choices.⁴

We 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 to the affected fields more than quadrupled as a result of the rise in admission selectivity.

A potential concern is that the effect we find using the first strategy is the result of contemporaneous changes,⁵ or that it is a short-run reaction to the reform in 2012. 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.

Dominated choices are inconsequential when the applicant does not meet the bar for admission with funding. According to our most conservative lower bound, about 9.8 percent of high-school senior applicants made a dominated choice in 2013–14. Between 5.8 and 11.9 percent of these dominated choices were costly: the applicant could have received a more desirable assignment had she asked for it. When dominated choices were costly, the average monetary loss was approximately 3,000–3,500 dollars. Therefore, the average loss per dominated choice is between 102 and 352 dollars, corresponding to an average loss of 10–35 dollars across the entire population of high-school senior applicants.

Our findings suggest that economic incentives affect the prevalence of dominated choices: applicants make more dominated choices when they are less costly. A potential explanation is that applicants expect this behavior to be (approximately) costless and—to explain the large fraction of costly dominated choices—the applicants must hold erroneous beliefs (potentially due to pessimism or overprecision). An alternative explanation is that the standard model of matching market design does not capture important aspects of applicants’ preferences, and, as a result, it is not necessarily optimal to rank alternatives truthfully. For example, [Dreyfuss et al. \(Forthcoming\)](#) and [Meisner and von Wangenheim \(2019\)](#) show that expectation-based loss aversion can explain the empirical patterns we document.

Our findings have important implications for the study and design of centralized school assignment systems. First, they suggest that certain features of the choice architecture, which the theory of market design deems irrelevant, are consequential in practice. For example, our findings indicate that 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, reported preferences are often used to inform policymakers about the relative desirability of different alternatives (schools, hospital internships, etc.). According to the traditional approach, preferences that are reported to strategically simple mechanisms can be interpreted at face value. But if, for example, agents tend to lower the ranking of desirable options where they expect fiercer competition (as we indeed find), a straightforward interpretation of school-choice data would exaggerate the importance applicants attach to proximity in the common case where individuals have priority in their neighborhood schools ([Fack et al., 2019](#)). [Artemov et al. \(2017\)](#) propose an alternative approach to estimating preferences when applicants know their priorities in schools when they submit their ROL (see also [Larroucau and Rios, 2020](#)).

Finally, understanding what causes dominated choices under DA, and more broadly in strategically simple environments, could inform researchers about the mechanisms underlying this behavior. This, in turn, could lead to new and more predictive classifications of allocation mechanisms according to their “simplicity” ([Cason et al., 2006](#); [Li, 2017a,b](#); [Zhang and Levin, 2017](#); [Bó and Hakimov, 2019](#)).

Relation to the literature. This paper is related to studies evaluating dominated-strategy play in strategically simple environments. Two recurring themes in this literature are the negative correlation of this behavior with cognitive ability and its positive correlation with the expectation of fiercer competition (e.g.,

Rees-Jones and Skowronek, 2018). In practice, applicants’ cognitive ability and desirability are positively correlated in the field, making it difficult to disentangle the two components (Hassidim et al., 2021; Rees-Jones, 2018; Artemov et al., 2017). In the laboratory, Basteck and Mantovani (2016) and Rees-Jones and Skowronek (2018) find that mistakes under the DA mechanism are more common among applicants with low cognitive ability, and Guillen and Hakimov (2016) find that the same holds under the strategy-proof Top Trading Cycle (TTC; Abdulkadiroğlu and Sönmez, 2003). Hassidim et al. (2021) and Rees-Jones and Skowronek (2018) document a strong causal relationship between expected admission selectivity and dominated choices in the laboratory.⁶ Our study is the first to establish the causal relationship between admission selectivity and dominated choices in the field, ruling out cognitive limitations as a sole determinant of dominated choices in high-stakes environments. We also corroborate the correlation between cognitive ability and dominated choices.

Our findings on the prevalence of dominated choices are consistent with several recent studies suggesting that large fractions of participants in strategically simple environments use dominated strategies.⁷ In the laboratory, Chen and Sönmez (2006) find that approximately 30 percent of the participants misrepresented their preferences under DA. Subsequent laboratory experiments that employ numerous variants of the matching environment corroborate this finding.⁸ In the field, Gross et al. (2015), Chen and Pereyra (2019), and Rees-Jones (2018) document dominated-strategy play in strategically simple high-stakes environments, using survey evidence. Relying exclusively on observational data, Hassidim et al. (2021) detect preference misrepresentation in the Israeli Psychology Master’s Match (IPMM; Hassidim et al., 2017), and Artemov et al. (2017) and Arslan (2018) do likewise in centralized college admissions markets in Australia and Turkey.⁹ Our paper complements these studies by documenting dominated-strategy play in a large, well-established market, using exclusively observational data. Unlike these papers, however, our lower bound on the cost of dominated choices is substantial.

Our work is also related to the large literature on suboptimal behavior in education markets (e.g., Hoxby and Avery, 2012). This literature finds that search frictions with respect to 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-socioeconomic status (SES) families are particularly affected.¹⁰ In the context of centralized school choice, numerous studies document evidence of suboptimal play when strategically demanding mechanisms, such as the Immediate Acceptance (i.e., Boston) mechanism, are in place (e.g., Abdulkadiroğlu et al., 2006; De Haan et al., 2021; He, 2017; Kapor et al., 2020). We contribute to the literature by studying a long-standing centralized market that is designed to be strategically simple, where information is accessible and abundant, and by focusing on dominated choices that are unlikely to be caused by informational frictions. Yet, we find that a substantial fraction of applicants make such dominated choices, 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.

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 assesses the costs of dominated choices. 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

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

College admissions have been organized through a centralized clearinghouse since 1985. The centralized clearinghouse is managed by a nonprofit governmental organization. Over the years, several changes have been introduced into the mechanism in place. The most recent change occurred in 2008 when a variant of the student-proposing DA was adopted.¹¹ The mechanism that had been in use previously was based on a similar variant of the program-proposing version of DA. Both mechanisms endow programs with priorities based on a weighted average of several variables (mainly academic performance in the 11th and 12th grades and matriculation exam scores, but also credits for disadvantaged and disabled applicants, as well as for a small number of gifted applicants). Across institutions, programs in the same field of study use the same priorities. But programs in different fields use different weighting schemes (e.g., the priority score for computer science assigns greater weight to physics grades relative to the priority score for economics). Prospective students apply to particular study programs, i.e., a particular major at a particular institution (e.g., a BA in applied economics at Corvinus University of Budapest). They may apply to multiple institutions and to multiple programs in the same institution.

Tuition waivers. Hungarian nationals and citizens of the European Economic Area are eligible to receive up to six years (12 semesters) of free education in the form of a tuition waiver. Nevertheless, the government caps the number of funded positions in some majors and in each field of study (business and economics, humanities, etc.). Eligible students may apply for a funded position, but unfunded positions are also offered. If admitted to an unfunded position, the student will not receive a tuition waiver, in spite of her eligibility. On average, 64% of admitted students received a tuition waiver in the years 2009–2014.

Tuition varies between programs. In 2013, it ranged from 2,000 to 23,000 dollars for three years, with a median of \$3,800 and a mean of \$4,500. In comparison, the annual minimum wage in 2013 was approximately \$4,000. Besides the monetary benefits, funded positions have other advantages over unfunded ones. Many institutions grant funded students priority in access to subsidized housing and other amenities. In some cases, these benefits have substantial monetary value. Moreover, paying students bear the stigma of being thought “not good enough” to be admitted to the traditional funded track (cf. [Aygün and Turhan, 2016](#)).

Rank-Order Lists. Students are allowed to rank any number of *contracts*, i.e., program and funding level combinations, that they wish. For example, they may submit an ROL that includes four contracts with three programs as in Table A1. Submitting an ROL that includes up to 3 programs (which may correspond to up to 6 contracts) only requires paying a fixed application fee of about 30 dollars. This is the case for the ROL in Table A1. However, applicants are charged about 7 dollars for each additional program in their ROL.

Revealed dominated choices. The fact that application fees are determined according to the number of *programs* in the ROL, as opposed to the number of *contracts*, implies that if a student ranks an unfunded contract with a certain program, then the marginal (monetary) cost of ranking a funded contract with the same program is zero. Search costs are also negligible as the contracts relate to the same program, students are well-aware of the funded option (which is the norm, and exists in all programs), and the interface makes it nearly impossible to ignore its existence (See Appendix C). These features, together with the standard assumption that applicants’ preferences depend only on their own allocation, implies that an applicant is using a dominated strategy if she ranks an unfunded contract in some program higher than a funded contract in the same program (*revealed flipping*), or if she ranks only an unfunded contract in a program that offers

a funded contract (*revealed dropping*). We collectively refer to such strategies as *revealed dominated choices* (for short, *dominated choices*).

Table A1 presents an ROL that includes four contracts with three programs. This ROL contains two dominated choices. First, an unfunded BA in biology at Eötvös Lóránd University is ranked higher than a funded contract in the same program (revealed flipping). Second, the applicant ranked only an unfunded BA in applied economics at Corvinus University of Budapest, even though a funded contract was offered (revealed dropping).¹²

The fact that we label the behavior we document as dominated is not innocuous. This label relies crucially on the assumption that applicants’ utility depends only on the realized assignment, and more specifically only on the applicants’ own assignment. While this assumption is standard in the market design literature (e.g., Pathak and Sönmez, 2013), there are other possible interpretations (e.g., social preferences, self-image concerns). Our view that funded positions unambiguously dominate unfunded positions is shared by the popular media. For example, on the day the 2017 match results were made public a major media outlet published a story with the man-bites-dog title: “*The priority-score cutoff for unfunded medicine exceeds the state-funded cutoff.*”¹³

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

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

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

2.2 The 2012–2013 Reforms

Historically, higher education in Hungary was free. Since the fall of the Iron Curtain in the early 1990s, there have been several attempts to introduce college tuition, but these attempts met with widespread public resistance. For example, in 1995, the government introduced college tuition, which was canceled in 1998.¹⁴ 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 2012.¹⁵ 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 A4). Altogether, the reform reduced the number of funded positions by 81 percent in these fields. Funded positions in some majors were eliminated completely (examples include business administration and management, commerce and marketing, and human resources). In other majors, funding was only offered in a subset of the institutions where it had been offered previously (for example, legal studies, international business administration, and international relations). In still other majors, the menu was not changed, but the capacities of state-sponsored positions were reduced. The number of state-sponsored positions in other fields of study declined by 7 percent, from 36,000 to 33,637. We refer to these fields of study as *fields with little or no funding cut*.

The backlash following the 2012 experience led to some changes in the way the reform was implemented in subsequent years, starting in 2013. Importantly, state-sponsored positions were restored in all programs where they had previously been offered. However, state-sponsored capacities remained scarce.¹⁶ The “reversal” of the 2012 reform did not meaningfully increase the number of state-sponsored positions in the affected fields: the number of funded positions was about 800 in business and economics, 170 in legal studies, and 1,100 in social sciences. Additionally, starting in 2013, the funding cut was expanded to include an additional major in the field of humanities (adult education).

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

Even though the decree makes state-sponsored positions less desirable, we do not think that it changes the natural ranking of funded and unfunded contracts or that it has a substantial effect on the composition of applicants, for several reasons. First, the decree specifies numerous exemptions, including having two or more children, military service, and disability. Second, it is highly unlikely that this contract will be enforced (in twenty years). Its legal status is unclear, as it may violate the freedom of movement of workers in the EU,¹⁷ and political pressure caused the government to significantly alleviate the terms already in 2013. Third, a student who leaves Hungary and does not return for more than a decade is very likely to move to a country where she will have a much easier time earning a few thousand dollars, lowering the marginal value of money in this contingency. Fourth, if an applicant is admitted with funding, she can decide to decline the funding and still be admitted; thus, applying to a funded position provides a pure option value.

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

The government also expanded the availability of its subsidized student loan program for paying students and introduced partially funded positions. Partially funded positions were offered only in 2012. Partial funding covered half of the tuition and was also subject to the study contract. It was not possible to rank partially funded positions, but they were awarded based on merit to 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 presenting our data sources and defining our sample. In Section 3.2, we present summary statistics.

3.1 Data Sources

Our main data source is an administrative dataset that contains information about the bachelor’s degree admissions process between 2009 and 2014 in Hungary.¹⁸ In particular, we observe each applicant’s complete ROL and program-specific priority scores,¹⁹ as well as the list of existing programs with their realized priority-score cutoff. For each applicant we also observe gender, age, postal code, and a high-school identifier. Additionally, the data include all information required to (re)calculate the applicant’s priority score in each program she applied to. This information includes grades in various subjects in the final two years of high school (11th and 12th grades), performance in the matriculation exams, and the number of points the applicant received for claiming a disadvantaged background.²⁰ This dataset also includes applicants’ assignment.

An ROL is an ordered list of *contracts*, i.e., program-funding pairs. An applicant makes a dominated choice if she is eligible for funding, yet she ranks an unfunded contract in some program higher than a funded contract in the same program (revealed flipping), or if she ranks only an unfunded contract in a program that offers a funded contract (revealed dropping). When we analyze the effect of admission selectivity on dominated choices (Section 4), we treat each *application* – a program in an ROL, with up to two contracts – as a single observation.

Our full dataset consists of 565,635 ROLs submitted between 2009 and 2014. We restrict our sample to ROLs that can potentially exhibit dominated choices. These ROLs must meet two criteria. First, the applicant must be eligible for a tuition waiver. As our data do not contain direct information on tuition-waiver eligibility, we rely on indirect information: we restrict the sample to ROLs submitted by citizens of the European Economic Area who did not report being ineligible. Second, we focus on ROLs that include at least one contract with a program that offers both funded and unfunded contracts. In addition to these two criteria, we restrict the 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, which includes 268,981 ROLs, as the *high-school senior applicant sample*. In this subsample we are certain that applicants did not exhaust their 12 funded semesters but just chose not to declare their ineligibility (without ranking any funded contract).²¹ These restrictions ensure with a high degree of certainty that the dominated choices we identify are not the result of misclassification. However, the inclusion criteria of this sample likely exclude many eligible students, especially weaker applicants, who may be more prone to make dominated choices according to previous studies.

Finally, we sometimes refer to *relevant* ROLs. These are ROLs that include at least one unfunded contract. Relevant ROLs are the only lists in which our methodology can potentially detect dominated choices.

3.2 Summary Statistics

Table A3 summarizes the means and standard deviations of the background characteristics of high-school senior applicants. Applicants were 19.05 years old on average, with 57 percent being female. The majority (70 percent) of the applicants attended secondary grammar schools, whose declared purpose is to prepare students for higher education. Approximately 16 percent of the applicants lived in Budapest, 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 eligible applicants' GPAs.²² The composition of the applicants and the distribution of the fields these students applied to remained stable over time (see Appendix Table E1 and Appendix Figure E1).²³

Table A5 presents the distribution of ROLs by the type of contracts they include. Applicants' ROLs include only funded contracts frequently (65.5 percent) and only unfunded contracts rarely (2.3 percent). Thus 34.5 percent of the applicants are relevant. Among students who listed both funded and unfunded contracts in their ROL, 46.6 percent ranked *all* funded contracts above *all* unfunded ones. Taken together, these figures suggest that funding plays an important role in students' choices between alternatives.

Table A6 quantifies the share of ROLs that exhibit dominated choices. The fraction of 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 the ROLs, made a dominated choice, mostly revealed dropping. The rate of revealed dropping was 5.0 percent and the rate of revealed flipping was 1.1 percent. The intensity of dominated choices varies between ROLs: 2.7 percent of the applicants made a dominated choice with respect to all the programs they ranked.

Dominated choices can be detected only in ROLs that rank at least one unfunded contract. The share of such ROLs is 35 percent (see Table A5). Table A6 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.

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

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

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. 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 A1 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 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 A7 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 2013.²⁴ 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 and their precision. Appendix Table F5 repeats this analysis using only columns (1) and (3) of Table A7 using only the highest ranked application in each ROL, and finds similar results. Appendix Table F6 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 the funded programs. The percentile ranks increased for 88 percent of the treated programs, and the average change was almost 9 percentiles in 2013. The reduction in the number of funded positions in the directly affected programs made the system as a whole more selective through general equilibrium effects. If students who applied to programs that were not affected directly took these general equilibrium effects into account when submitting their application, then our estimates should provide lower bounds on the causal effect of admission selectivity on dominated choices.

In Table A8 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 A7). 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 low-SES applicants, measured by claiming points for disadvantaged status. The effect of the reform is declining with academic achievement, measured by 11th-grade GPA (Figure A2). The estimated effect for applicants with an 11th-grade GPA of 3 is about three times as large as this estimate for applicants with a perfect GPA (5). These results suggest that applications for which dominated choices cause a higher expected utility loss are less responsive to increases in admission selectivity.

Recall that there are some circumstances under which we are more certain that the natural ranking is unaffected by the introduction of the study contract: namely, when the applicant is of low socioeconomic status or when she applies to a major which is highly specific to Hungary. Even among the very poor, we find that the effect is substantial and equals about one-half of the effect on applicants of high socioeconomic status. Furthermore, we find that the effect on applications to legal studies is almost identical to the main effect. By contrast, the estimated effect 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).

Appendix G.2 replicates the main analysis for ex post costly dominated choices. Absent any behavioral response, increased selectivity to funded positions mechanically reduces the number of ex post costly dominated choices. In 2013, our estimates are dominated by this mechanical effect: We find that the 2012–13 reform had a negative causal impact on ex post costly dominated choices. In 2014, the estimated effect is close to zero.

Threats to identification and robustness We assess the plausibility of our identifying assumptions in various ways. To 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 A9 add these placebo treatment variables to the baseline model. Although the placebo coefficients for 2009 and 2010 are statistically significant, they are an order of magnitude lower than our main estimates and precisely estimated. Thus, the potential for bias due to the violation of the parallel trends assumption is small.

We also study a smaller-scale reform that took place in 2011, prior to the introduction of the study contract. This reform, which received much less attention from the media and the public, decreased the number of tuition waivers in business/economics and social sciences by about 20 percent (Table A4). 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 F7 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 F.3 we show that our results hold in an alternative specification that leverages all variation in the number of funded positions during our sample period.

A potential threat to our identification strategy is that treatment status is defined by applicants’ ROLs. Applicants’ responses to the reform may affect the composition of their ROL as well as their decision to apply. This concern is particularly pronounced for students who are not willing (or able) to pay the tuition and are considering applying only to funded programs. Such applicants never make 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 such ROLs are free of dominated choices by definition).

We first note that this threat is only quantitative but not qualitative. The worst-case bias implies that our estimates are twice as high as the actual effect. To see this, note that the number of applications to treated fields decreased from 40,684 to 26,341, and that the rate of 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 had been free of dominated choices, and ii) the rate of dominated choices in the control had still been 2.6 percent (and not 1.8 percent as in 2011). The estimated effect would have been $(24.7 \cdot (26,341/40,684) - 5.5) - (2.6 - 1.8) = 9.7$ percentage points, more than half of the effect we estimated, and about twice the baseline rate of dominated choices.

Next, we address the threat to our identification strategy in several other ways. First, in columns (2)–(3) of Table A7 we add applicant-level controls. Second, we restrict the sample to those high-school senior applicants who listed programs both in the fields that experienced a severe funding cut and in the fields that were unaffected (columns (3) and (4) of Table A9). This restriction assures that the composition of applicants in the treated and untreated fields is the same (balanced subsample).²⁵ We find that the coefficient estimates remain positive, large, and statistically significant, confirming that changes in the composition of applicants do not drive our results.

Third, we look at applicants who listed at least one unfunded contract in their ROL. By listing at least one unfunded contract, these applicants indicate that they are willing to pay tuition; hence we find it less

plausible that the reform affected the set of programs in their ROL.²⁶ Reassuringly, our estimates for this subsample are very similar to the main estimates (columns (5) and (6) of Table A9), indicating that switching behavior does not drive our results.²⁷

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 less likely to make dominated choices with respect to a “safety school” in their ROL than they are likely to make a dominated choice with respect to a “reach school.”

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 $\text{priority-score cutoff}_{t-1,s}$ 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 schemes, and we normalize the priority-score cutoffs to within-year percentile ranks.²⁸ The model includes fixed effects for program characteristics (X_{ts}), such as type of degree (BA or BA–MA), time schedule (full time or part time), field of study, and program location. The model also includes ROL-fixed effects (η_{it}) and an error term (ε_{its}). Our parameter of interest is β , which measures the effect of admission selectivity (as measured by lagged funded priority-score cutoffs) on 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 A3 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.

We cannot attribute a causal interpretation to the results depicted in Figure A3 (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 A3 (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 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 A10 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 (lagged) admission selectivity 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 A3 (b) illustrates the results of our most preferred specification (column (5) of Table A10). Appendix Tables F10 and F11 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.

In Table A11 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 A10). 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 effect of admission selectivity is lower for low-SES applicants, measured by claiming points for disadvantaged status. The estimated effect is declining with academic achievement, measured by 11th-grade GPA (Figure A4). The estimated effect for applicants with an 11th-grade GPA of 3 is about three times as large as this effect for applicants with a perfect GPA (5).

5 The Private Costs of Dominated Choices

According to our interpretation, dominated choices correspond to weakly dominated strategies. Rational players, who understand the admissions process, only use dominated strategies if they assign probability zero to the event that a dominating strategy does strictly better. Table A12 assesses the share of dominated choices that are costly ex post. We provide a lower bound and an upper bound for these shares. The upper bound corresponds to the fraction of applicants who met the priority-score cutoff for receiving funding in any program whose funded contract they dropped or ranked below its unfunded version. The lower bound accounts for such ROLs only if the applicant was not assigned a higher-ranked contract. These estimates correspond to the outcome of the counterfactual ROL that ranks the funded contract either first or directly above the unfunded contract.

Table A12 demonstrates that dominated choices may have hurt up to 9.4 percent of the high-school senior applicants who made dominated choices (column (2)). At least 4.3 percent of the high-school senior applicants who made dominated choices could have received a tuition waiver (column (1)). The relative importance of funding, reflected in the rankings of the majority of students, suggests that the upper bound may be more indicative of the true fraction. The average loss associated with dominated choices is between 102 and 352 dollars for high-school senior applicants in 2013–14.²⁹ The average monetary loss associated with ex post costly dominated choices is approximately 3,000–3,500 dollars.³⁰

6 Conclusions

Previous studies mainly focused on the properties of market clearing algorithms, giving special attention to strategic simplicity. As pointed out by Pathak (2016), “[e]fforts to improve how participants interact with market designs ... hold great promise to 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 Frisncho (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.³¹

School systems around the world have different policies regarding the timing in which preferences are reported. When applicants know their priority score prior to submitting their ROL to the clearinghouse, their beliefs can be more precise (Chen and Pereyra, 2019; Artemov et al., 2017). 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).

We have focused on a specific kind of dominated choices and showed that admission selectivity has a causal effect on these dominated choices. Future research should provide more evidence on the causes of dominated strategy play, allowing to further narrow down on the specific underlying behavioral biases. This will improve our ability to design effective marketplaces and to interpret choice data.

References

- Abdellaoui, Mohammed, Han Bleichrodt, and Olivier l’Haridon.** 2008. “A tractable method to measure utility and loss aversion under prospect theory.” *Journal of Risk and uncertainty*, 36, p. 245.
- Abdulkadiroğlu, Atila, Parag A. Pathak, Alvin E. Roth, and Tayfun Sönmez.** 2006. “Changing the Boston school choice mechanism.” *NBER Working paper*, No. 11965.
- Abdulkadiroğlu, Atila, and Tayfun Sönmez.** 2003. “School choice: A mechanism design approach.” *American Economic Review*, 93(3): 729–747.
- Ajayi, Kehinde F.** 2011. “School choice and educational mobility: Lessons from secondary school applications in Ghana.” *University of California-Berkeley Working Paper*.
- Andrabi, Tahir, Jishnu Das, and Asim Ijaz Khwaja.** 2017. “Report cards: The impact of providing school and child test scores on educational markets.” *American Economic Review*, 107(6): 1535–1563.
- Antler, Yair.** 2015. “Two-sided matching with endogenous preferences.” *American Economic Journal: Microeconomics*, 7(3): 241–58.
- Antler, Yair.** 2019. “No one likes to be second choice.” *The Economic Journal*, 129(619): 1119–1138.
- Arslan, Hayri Alper.** 2018. “Preference estimation in centralized college admissions from reported lists.” Available at SSRN: <https://ssrn.com/abstract=3126391>.
- Artemov, Georgy, Yeon-Koo Che, and Yinghua He.** 2017. “Strategic “mistakes”: Implications for market design.” Mimeo.
- Avery, Christopher, and Thomas J. Kane.** 2004. “Student perceptions of college opportunities. The Boston COACH program.” In *College Choices: The Economics of Where to Go, When to Go, and How to Pay for it.*: University of Chicago Press: Chicago, 355–394.
- Aygun, Orhan, and Bertan Turhan.** 2016. “Dynamic reserves in matching markets: Theory and applications.” Available at SSRN: <https://ssrn.com/abstract=2743000>.
- Basteck, Christian, and Marco Mantovani.** 2016. “Cognitive ability and games of school choice.” Technical report, University of Milan Bicocca Department of Economics, Management and Statistics Working Paper, No. 343. Available at SSRN: <https://ssrn.com/abstract=2798709>.
- Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu.** 2012. “The role of application assistance and information in college decisions: Results from the H&R Block FAFSA experiment.” *The Quarterly Journal of Economics*, 127(3): 1205–1242.
- Bó, Inácio, and Rustamdjan Hakimov.** 2019. “Iterative versus standard deferred acceptance: Experimental evidence.” *The Economic Journal*, 130(626): , p. 356–392.
- Bobba, Matteo, and Veronica Frisanchio.** 2015. “Learning about oneself: The effects of signaling academic ability on school choice.”
- Braun, Sebastian, Nadja Dwenger, Dorothea Kübler, and Alexander Westkamp.** 2014. “Implementing quotas in university admissions: An experimental analysis.” *Games and Economic Behavior*, 85 232–251.

- Budish, Eric, Gérard P. Cachon, Judd B. Kessler, and Abraham Othman.** 2016. “Course match: A large-scale implementation of approximate competitive equilibrium from equal incomes for combinatorial allocation.” *Operations Research*, 65(2): 314–336.
- Budish, Eric, and Judd B. Kessler.** 2015. “Experiments as a bridge from market design theory to market design practice: Changing the course allocation mechanism at Wharton.” Available at <http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.720.855>.
- Bíró, Péter.** 2011. “University admission practices – Hungary.” URL: www.matching-in-practice.eu.
- Cabrales, Antonio, Olivier Gossner, and Roberto Serrano.** 2017. “A normalized value for information purchases.” *Journal of Economic Theory*, 170 266–288.
- Calsamiglia, Caterina, Guillaume Haeringer, and Flip Klijn.** 2010. “Constrained school choice: An experimental study.” *American Economic Review*, 100(4): 1860–1874.
- Cason, Timothy N., Tatsuyoshi Saijo, Tomas Sjöström, and Takehiko Yamato.** 2006. “Secure implementation experiments: Do strategy-proof mechanisms really work?” *Games and Economic Behavior*, 57(2): 206–235.
- Chen, Li, and Juan Sebastian Pereyra.** 2019. “Self-selection in school choice.” *Games and Economic Behavior*, 107 59–81.
- Chen, Yan, and Onur Kesten.** 2011. “From Boston to Shanghai to deferred acceptance: Theory and experiments on a family of school choice mechanisms.” In *International Conference on Auctions, Market Mechanisms and Their Applications*. 58–59, Springer: New York.
- Chen, Yan, and Tayfun Sönmez.** 2006. “School choice: An experimental study.” *Journal of Economic Theory*, 127(1): 202–231.
- De Haan, Monique, Pieter A. Gautier, Hessel Oosterbeek, and Bas van der Klaauw.** 2021. “The performance of school assignment mechanisms in practice.” Mimeo.
- DellaVigna, Stefano, and Ulrike Malmendier.** 2006. “Paying not to go to the gym.” *American Economic Review*, 96(3): 694–719.
- Ding, Tingting, and Andrew Schotter.** 2015. “Learning and mechanism design: An experimental test of school matching mechanisms with intergenerational advice.” Technical report, Working Paper.
- Ding, Tingting, and Andrew Schotter.** 2016. “Matching and chatting: An experimental study of the impact of network communication on school-matching mechanisms.” *Games and Economic Behavior*.
- Dohmen, Thomas, Armin Falk, David Huffman, Uwe Sunde, Jürgen Schupp, and Gert G Wagner.** 2011. “Individual risk attitudes: Measurement, determinants, and behavioral consequences.” *Journal of the European Economic Association*, 9(3): 522–550.
- Dreyfuss, Bnaya, Ori Heffetz, and Matthew Rabin.** Forthcoming. “Expectations-based loss aversion may help explain seemingly dominated choices in strategy-proof mechanisms.” *American Economic Journal: Microeconomics*.
- Echenique, Federico, Alistair J. Wilson, and Leeat Yariv.** 2016. “Clearinghouses for two-sided matching: An experimental study.” *Quantitative Economics*, 7(2): 449–482.
- Fack, Gabrielle, Julien Grenet, and Yinghua He.** 2019. “Beyond truth-telling: Preference estimation with centralized school choice and college admissions.” *American Economic Review*, 109(4): 1486–1529.

- Featherstone, Clayton R., and Muriel Niederle.** 2016. "Boston versus deferred acceptance in an interim setting: An experimental investigation." *Games and Economic Behavior*, 100 353–375.
- Gabaix, Xavier.** 2019. *Behavioral Inattention*. Chap. Handbook of Behavioral Economics, Volume 2: Elsevier.
- Gale, David, and Lloyd S. Shapley.** 1962. "College admissions and the stability of marriage." *The American Mathematical Monthly*, 69(1): 9–15.
- Galgóczi, Béla, Janine Leschke, and Andrew Watt.** 2013. *EU labour migration since enlargement: Trends, impacts and policies.*: Ashgate Publishing, Ltd..
- Gross, Betheny, Michael DeArmond, and Patrick Denice.** 2015. "Common enrollment, parents, and school choice: Early evidence from Denver and New Orleans. Making School Choice Work Series." *Center on Reinventing Public Education*.
- Guillen, Pablo, and Rustamdjan Hakimov.** 2014. "Monkey see, monkey do: truth-telling in matching algorithms and the manipulation of others." SP II 2014-202, p. 24.
- Guillen, Pablo, and Rustamdjan Hakimov.** 2016. "Not quite the best response: Truth-telling, strategy-proof matching, and the manipulation of others." *Experimental Economics* 1–17.
- Guillen, Pablo, and Alexander Hing.** 2014. "Lying through their teeth: Third party advice and truth telling in a strategy proof mechanism." *European Economic Review*, 70 178–185.
- Hart, Sergiu.** 2011. "Comparing risks by acceptance and rejection." *Journal of Political Economy*, 119(4): 617–638.
- Hassidim, Avinatan, Assaf Romm, and Ran I. Shorrer.** 2017. "Redesigning the Israeli psychology masters match." *American Economic Review: Papers & Proceedings*, 107(5): 205–209.
- Hassidim, Avinatan, Assaf Romm, and Ran I. Shorrer.** 2021. "The limits of incentives in economic matching procedures." *Management Science*, 67(2): 951–963.
- Hastings, Justine S., Christopher A. Neilson, and Seth D. Zimmerman.** 2015. "The effects of earnings disclosure on college enrollment decisions." *NBER Working Paper*, No. 21300.
- Hastings, Justine S., and Jeffrey M. Weinstein.** 2008. "Information, school choice, and academic achievement: Evidence from two experiments." *The Quarterly Journal of Economics*, 123(4): 1373–1414.
- He, Yinghua.** 2017. "Gaming the Boston school choice mechanism in Beijing." Mimeo.
- Horn, Dániel.** 2013. "Diverging performances: the detrimental effects of early educational selection on equality of opportunity in Hungary." *Research in Social Stratification and Mobility*, 32 25–43.
- Hoxby, Caroline M., and Christopher Avery.** 2012. "The missing "one-offs": The hidden supply of high-achieving, low income students." *NBER Working Paper*, No. 18586.
- Hoxby, Caroline M., and Sarah Turner.** 2013. "Expanding college opportunities for high-achieving, low income students." *Stanford Institute for Economic Policy Research Discussion Paper*(12-014): .
- Jensen, Robert.** 2010. "The (perceived) returns to education and the demand for schooling." *The Quarterly Journal of Economics*, 125(2): 515–548.
- Kapor, Adam J., Christopher A. Neilson, and Seth D. Zimmerman.** 2020. "Heterogeneous beliefs and school choice mechanisms." *American Economic Review*, 110(5): 1274–1315.

- Klijn, Flip, Joana Pais, and Marc Vorsatz.** 2013. "Preference intensities and risk aversion in school choice: A laboratory experiment." *Experimental Economics*, 16(1): 1–22.
- Larroucau, Tomas, and Ignacio Rios.** 2020. "Do "short-list" students report truthfully? Strategic Behavior in the Chilean College Admissions Problem." Unpublished.
- Li, Shengwu.** 2017a. "Obvious ex post equilibrium." *American Economic Review: Papers & Proceedings*, 107(5): 230–234.
- Li, Shengwu.** 2017b. "Obviously strategy-proof mechanisms." *American Economic Review*, 107(11): 3257–3287.
- Meisner, Vincent, and Jonas von Wangenheim.** 2019. "School choice and loss aversion." Available at SSRN: <https://ssrn.com/abstract=3496769>.
- Pais, Joana, and Ágnes Pintér.** 2008. "School choice and information: An experimental study on matching mechanisms." *Games and Economic Behavior*, 64(1): 303–328.
- Pais, Joana, Ágnes Pintér, and Róbert F. Veszteg.** 2011. "College admissions and the role of information: An experimental study." *International Economic Review*, 52(3): 713–737.
- Pallais, Amanda.** 2015. "Small differences that matter: mistakes in applying to college." *Journal of Labor Economics*, 33(2): 493–520.
- Pathak, Parag A.** 2016. "What really matters in designing school choice mechanisms." *Advances in Economics and Econometrics, 11th World Congress of the Econometric Society*.
- Pathak, Parag A., and Tayfun Sönmez.** 2013. "School admissions reform in Chicago and England: Comparing mechanisms by their vulnerability to manipulation." *American Economic Review*, 103(1): 80–106.
- Peranson, Jonah.** 2019. "Design and implementation of the genetic counseling admissions match." In *Proceedings of MATCH-UP 2019, 5th International Workshop on Matching Under Preferences.*, p. 21, Match-UP.
- Rees-Jones, Alex.** 2018. "Suboptimal behavior in strategy-proof mechanisms: Evidence from the residency match." *Games and Economic Behavior*, 108 317–330.
- Rees-Jones, Alex, Ran I. Shorrer, and Cloe Tergiman.** 2019. "Correlation neglect in student-to-school matching." Available at SSRN: <https://ssrn.com/abstract=3434662>.
- Rees-Jones, Alex, and Samuel Skowronek.** 2018. "An experimental investigation of preference misrepresentation in the residency match." *Proceedings of the National Academy of Sciences*, 115(45): 11471–11476.
- Wu, Binzhen, and Xiaohan Zhong.** 2014. "Matching mechanisms and matching quality: Evidence from a top university in China." *Games and Economic Behavior*, 84 196–215.
- Zhang, Luyao, and Dan Levin.** 2017. "Bounded rationality and robust mechanism design: An axiomatic approach." *American Economic Review: Papers & Proceedings*, 107(5): 235–239.
- Zhu, Min.** 2014. "College admissions in China: A mechanism design perspective." *China Economic Review*, 30(C): 618–631.

Notes

¹Strategy-proof mechanisms, where participants have a dominant strategy of reporting their true preferences, are viewed to be appealing because of their strategic simplicity. In practice, many clearinghouses do not employ a strategy-proof mechanism, but still choose a strategically simple mechanism, where ranking alternatives in a way that is inconsistent with one's preferences is a dominated strategy (even though the choice of which alternatives to rank may require strategic thinking). Pathak and Sönmez (2013) report on dozens of school-choice systems around the world that implemented strategically simple versions of the deferred acceptance mechanism (DA; Gale and Shapley, 1962), only one of which (Boston Public Schools') was strategy-proof.

²E.g., Chen and Pereyra (2019), Rees-Jones (2018), and Hassidim et al. (2021). Ajayi (2011) and Rees-Jones et al. (2019) also document suboptimal behavior under Deferred Acceptance.

³As we discuss below, there are other possible interpretations, but these fall outside the standard model of matching market design (e.g., Antler, 2015, 2019; Dreyfuss et al., Forthcoming; Meisner and von Wangenheim, 2019). Such interpretation implies that DA is not strategyproof.

⁴A potential limitation of our approach is that the dominated choices we focus on are special, as they are related to financial aid. The advantage of focusing on these dominated choices is that their identification requires mild assumptions on applicants' preferences. Other dominated choices, e.g., ones related to the choice of study programs, are likely present as well. However, detecting them would require strong assumptions or additional information.

⁵In 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.

⁶See also Bó and Hakimov (2019).

⁷Budish and Kessler (2015) investigate a related question of students' ability to express their preferences in the more complex course-scheduling environment (Budish et al., 2016).

⁸Examples include Braun et al. (2014), Calsamiglia et al. (2010), Chen and Kesten (2011), Ding and Schotter (2015), Ding and Schotter (2016), Echenique et al. (2016), Featherstone and Niederle (2016), Guillen and Hing (2014), Guillen and Hakimov (2014), Klijn et al. (2013), Pais and Pintér (2008), Pais et al. (2011), and Zhu (2014).

⁹In the US, dominated choices have been documented in the Genetic Counseling Admissions Match (Peranson, 2019).

¹⁰Examples include Avery and Kane (2004), Hastings and Weinstein (2008), Jensen (2010), Ajayi (2011), Bettinger et al. (2012), Hoxby and Turner (2013), Hastings et al. (2015), Pallais (2015), and Andrabi et al. (2017).

¹¹To be precise, the matching system has three rounds. The main round, in which the majority of BA and MA positions are allocated, ends in July; an additional, significantly smaller round at the end of the summer for unfilled unfunded positions; and a winter round for master's programs that start in the spring term (Bíró, 2011). We use data only from the main round of the BA match.

¹²A potential explanation for the prevalence of dominated choices is that applicants do not understand the application fee structure. Specifically, applicants may not understand that the application fee is charged per program, not per contract. To assess this theory, we concentrate on the subsample of applicants who ranked four or more contracts, with three or fewer programs. These applicants must have learned the pricing scheme, because they had to pay only the fixed application fee. We find that the share of applicants with dominated choices is 6.8 percent in this subsample. Moreover, if applicants have rational expectations, this explanation can hold only under implausibly high levels of risk aversion, loss aversion, or hyperbolic discounting. As an illustration of this claim, note that the lottery $[3000, 0.058; -7, 0.942]$, which mimics the average probability of costly dominated choices and average tuition cost, is accepted by all agents with absolute risk aversion levels below $7 \cdot 10^{-3}$ —substantially higher than the range suggested by Cabrales et al. (2017) as reasonable (lower than $5 \cdot 10^{-4}$). The positive correlation between dominated choices and SES also requires that wealthier individuals are more risk averse, counter to the conventional view (Hart, 2011; Dohmen et al., 2011). Similarly, this lottery is accepted by all loss averse (and otherwise risk neutral) agents with a loss aversion coefficient lower than 26—substantially lower than 5, the highest estimate reported in Abdellaoui et al. (2008). The associated payoff stream is accepted for future discounting coefficients higher than 0.05 (in the absence of further discounting or risk or loss aversion)—Gabaix (2019) suggests the substantially higher 0.7 as a portable, already estimated parameter (DellaVigna and Malmendier, 2006).

¹³Source: index.hu; <https://goo.gl/zfxFFw>, accessed: 20/09/2017.

¹⁴See <https://goo.gl/bozDkK>, accessed: 01/02/2017.

¹⁵The legislation had mainly a fiscal motivation: the government faced pressure to consolidate the budget and initiated talks with the IMF on November 21, 2011.

¹⁶Starting 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.

¹⁷See The New York Times; <https://goo.gl/VL3Rt6>, accessed: 19/10/2017.

¹⁸The Hungarian Higher Education Application Database (FELVI) is owned by the Hungarian Education Bureau (Oktatási Hivatal). The data were processed by the Hungarian Academy of Sciences Centre for Economic and Regional Studies (HAS-CERS).

¹⁹Our data report up to 7 contracts from each ROL: the first 6 contracts and the contract to which the applicant is assigned. The dataset also reports the number of contracts in each ROL. We observe the complete ROL for 92.9 percent of applicants and 94.3 percent of all ranked contracts.

²⁰To be eligible for disadvantaged status, an applicant must have a per capita household income that is lower than 130 percent of the minimum pension (approximately \$1,500 a year). Since 2014, in addition to the income criterion, the student has to meet one of the following three conditions: (i) parents with lower than primary education, (ii) long-term unemployed parents, or (iii) poor living conditions. To receive disadvantaged status, an applicant must certify that she meets these conditions at the local municipality. Disadvantaged status is granted for one year. Students with disadvantaged status receive regular cash transfers and are eligible for free textbooks during high school.

²¹The ROL of an applicant who did not declare ineligibility even though she exhausted her 12 funded semesters and did not rank any funded contracts would be incorrectly classified as a dominated choice. These applicants only appear to make dominated choices, whereas they are in fact ineligible for funding. Focusing on high-school senior applicants eliminates the risk of such misclassification.

²²Applicants with a low high-school GPA, relative to their matriculation exam scores, have no incentive to report their GPA, as it has no effect on their priority score. As a result, 11th-grade GPAs are missing from 18 percent of both samples. Indeed, the correlation between missing GPA and matriculation exam scores in our data is negative and strong.

²³Changes in the pattern of application following the reform may compromise our difference-in-differences analysis. By “relatively stable,” we mean that our results cannot be driven by such changes, as we show at the end of Section 4.1.

²⁴The 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.

²⁵We thank Dániel Horn for proposing this specification.

²⁶Another 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.

²⁷A 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.

²⁸Since lagged priority-score cutoffs are not defined in the year a program is launched, we exclude ROLs that include such programs. We also exclude a handful of ROLs involving programs where a funded contract is not available. Finally, we disregard programs in the fields of art and art mediation, since these programs have eligibility exams and practical exams, and their priority scores are not calculated in the standard way.

²⁹To calculate the cost of ex post costly dominated choices, we use a dataset including the 2013 and 2014 tuition costs of each program, which we created by scraping the centralized admission system website.

³⁰For comparability, we follow Hassidim et al. (2021) in taking a partial equilibrium approach: we do not analyze the aggregate effect of dominated choices. Instead, we assume that all priority-score cutoffs remain fixed and ask what would the effect of correcting one list would be. By doing so, we ignore the effect that correcting one list might have on other applicants who would be displaced as a result of eliminating dominated choices, and any subsequent effects (such as “rejection chains”).

³¹In 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.

A Tables

Table A1: A rank-order list with dominated choices

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

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

Table A2: Availability of funded positions

	2009	2010	2011	2012	2012 (partial funding)
<i>A. Fields with little or no funding cut</i>					
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
<i>B. Fields with severe funding cut</i>					
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.

Table A3: Individual-level summary statistics

	Mean	St. dev.
	(1)	(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 clearing-house (see Footnote 22). 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 19).

Table A4: Availability of funded positions

	2009	2010	2011	2012	2012 (partial funding)
<i>A. Fields with little or no funding cut</i>					
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
<i>B. Fields with severe funding cut</i>					
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.

Table A5: Distribution of ROLs by funding type

ROLs	Count (1)	Share (%) (2)
Only funded contracts	176,204	65.5
Funded and unfunded contracts	86,484	32.2
Only unfunded contracts	6,293	2.3

Notes: The table presents the distribution of ROLs by funding type. Column (1) displays frequencies and column (2) shows the distribution.

Table A6: Dominated choices over time

Year	Dominated choices		Only dominated choices	
		(1)		(2)
2009	3.1	(1, 566)	1.1	(541)
2010	3.2	(1, 596)	1.1	(556)
2011	4.6	(2, 268)	1.6	(794)
2012	6.3	(2, 494)	3.5	(1, 397)
2013	10.8	(4, 201)	6.1	(2, 372)
2014	8.7	(3, 528)	4.2	(1, 686)
Total	5.8	(15, 653)	2.7	(7, 346)

Notes: Column (1) shows the share (number) of ROLs that exhibit dominated choices over time. Column (2) presents the share (number) of ROLs with dominated choices with respect to all of the programs they include.

Table A7: The effect of admission selectivity on dominated choices: 2012–13 reform

Dependent variable	Dominated choices		
	(1)	(2)	(3)
Severe funding cut $\times$ 2013	0.193*** (0.004)	0.186*** (0.004)	0.185*** (0.004)
Severe funding cut $\times$ 2014	0.179*** (0.004)	0.174*** (0.004)	0.173*** (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 A8: Heterogeneity: The effect of admission selectivity on dominated choices: 2012–13 reform

	A. Gender		B. Disadvantaged	
	Male	Female	No	Yes
Severe funding cut $\times$ 2013	0.202*** (0.007)	0.175*** (0.005)	0.193*** (0.004)	0.087*** (0.011)
Severe funding cut $\times$ 2014	0.209*** (0.007)	0.153*** (0.005)	0.177*** (0.004)	0.093*** (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
Severe funding cut $\times$ 2013	0.194*** (0.005)	0.169*** (0.010)	0.146*** (0.010)	0.118*** (0.023)
Severe funding cut $\times$ 2014	0.174*** (0.005)	0.189*** (0.010)	0.151*** (0.010)	0.086*** (0.020)
Counterfactual mean (2013)	0.060	0.090	0.081	0.044
Counterfactual mean (2014)	0.044	0.074	0.066	0.028
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, 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 A7).

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

Table A9: Robustness: The effect of admission selectivity on dominated choices: 2012–13 reform

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Subsample/specification	Placebo		Dominated choices		Relevant applicants	
	Balanced subsample					
Severe funding cut $\times$ 2013	0.181*** (0.004)	0.174*** (0.004)	0.101*** (0.005)	0.099*** (0.005)	0.162*** (0.006)	0.153*** (0.005)
Severe funding cut $\times$ 2014	0.168*** (0.004)	0.162*** (0.004)	0.123*** (0.005)	0.121*** (0.005)	0.171*** (0.005)	0.163*** (0.005)
Placebo (2009)	-0.017*** (0.002)	-0.015*** (0.002)				
Placebo (2010)	-0.018*** (0.002)	-0.017*** (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 the relevant ROLs. Robust standard errors clustered on the applicant level are in parentheses. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA.

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

Table A10: Admission selectivity and dominated choices: A within-ROL comparison

Dependent variable	Dominated choices				
	(1)	(2)	(3)	(4)	(5)
Priority-score cutoff	0.035*** (0.001)	0.034*** (0.001)	0.036*** (0.001)	0.035*** (0.001)	0.039*** (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.

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

Table A11: Heterogeneity: Admission selectivity and dominated choices: A within-ROL comparison

	A. <i>Gender</i>		B. <i>Disadvantaged</i>	
	Male	Female	No	Yes
Priority-score cutoff	0.041*** (0.002)	0.038*** (0.002)	0.041*** (0.002)	0.021*** (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 [A10](#)).

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

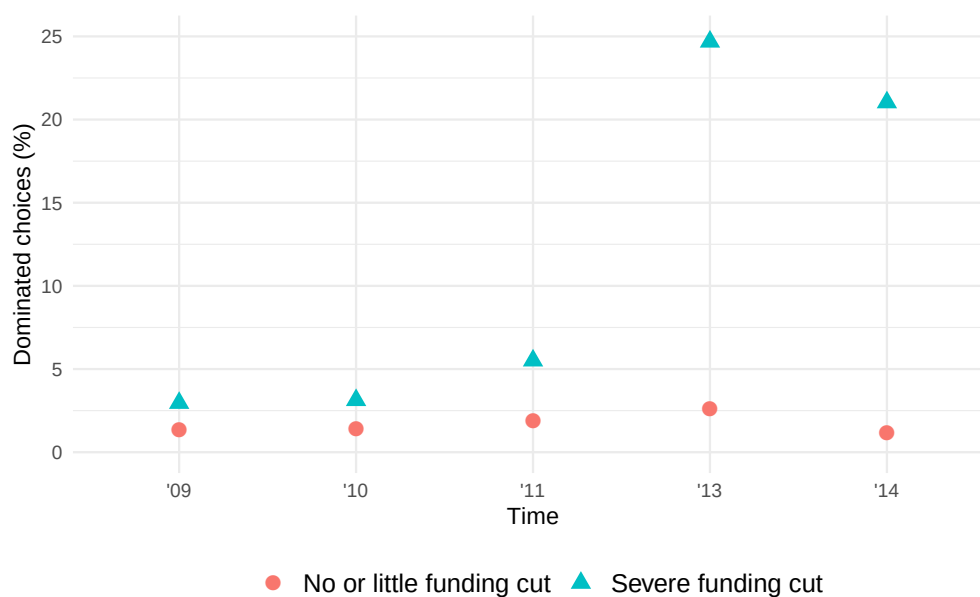
Table A12: Costly dominated choices over time

Year	Ex post costly dominated choices			
	Lower bound		Upper bound	
		(1)		(2)
2009	4.0	(62)	7.6	(119)
2010	1.4	(23)	4.1	(65)
2011	1.4	(31)	4.9	(112)
2012	4.0	(101)	10.5	(261)
2013	8.5	(355)	15.7	(659)
2014	2.7	(97)	7.5	(263)
Total	4.3	(669)	9.4	(1,479)

Notes: Column (1) presents the share (number) of ROLs with dominated choices, where the applicant was assigned to the unfunded version of a program in which he met the priority-score cutoff of the funded version. Column (2) shows the share (number) of ROLs with dominated choices, where the applicant met the priority-score cutoff of a funded contract with respect to which he made a dominated choice.

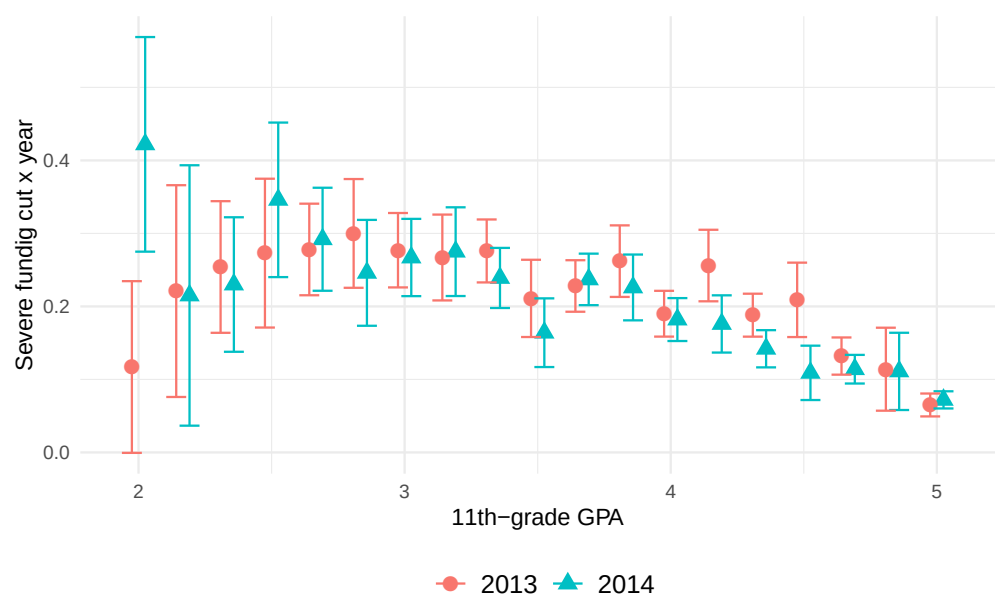
B Figures

Figure A1: The effect of admission selectivity on dominated choices: 2012–13 reform



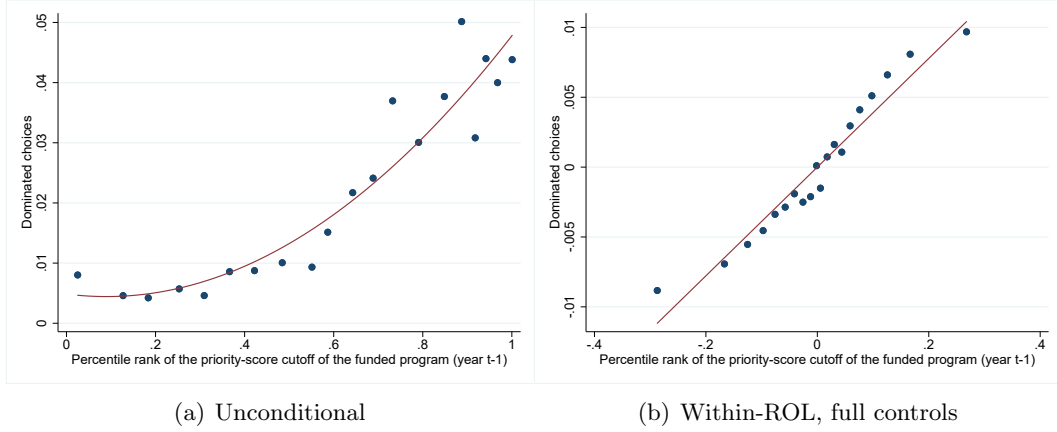
Notes: The figure presents the share of applications with dominated choices over time, split by the severity of the funding cut in the reform.

Figure A2: The effect of admission selectivity on dominated choices by 11th-grade GPA: 2012–13 reform



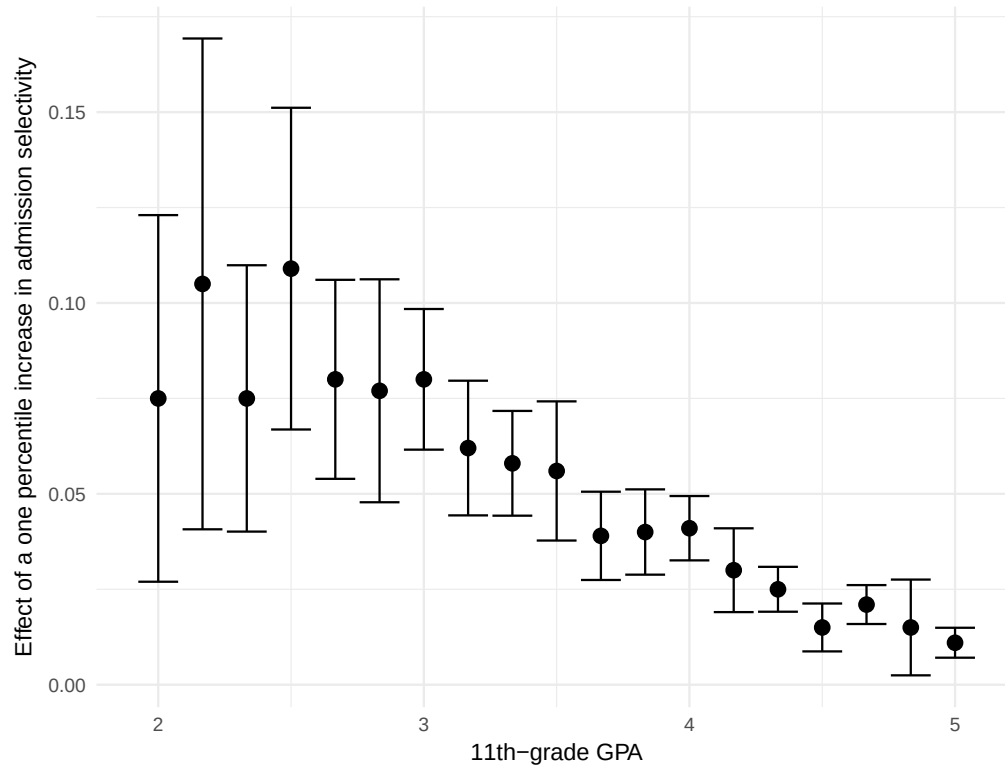
Notes: The figure presents the effect of admission selectivity on dominated choices by 11th-grade GPA with 95% confidence intervals. Robust standard errors are clustered on the applicant level. We estimate all the coefficients in a single regression by interacting the treatment indicators with 11th-grade GPA. We include demographic controls including gender, disadvantaged status, age, type of residence, and high-school indicator (as in column (3) of Table A10). The estimated effect on applicants with a missing 11th-grade GPA is 0.194 (s.e.: 0.008) in 2013 and 0.203 (s.e.: 0.007) in 2014. The median 11th-grade GPA is 4, and the 10th percentile is 2.8 in the distribution of high-school senior applications.

Figure A3: The effect of admission selectivity on dominated choices: A within-ROL comparison



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 A10). 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.

Figure A4: 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 A10). The estimated effect on applicants with a missing 11th-grade GPA is 0.043 (s.e.: 0.004).

C The Application Interface

This Appendix presents screenshots from the online application interface.

Figure A5 presents an example of an ROL. This ROL includes three programs with six contracts. A contract is a combination of the institution (e.g., ELTE), faculty (e.g., TÁTK), major (e.g., szociológia – sociology), degree (A – Bachelors), schedule (N – full time), and funding (A – funded, K – unfunded).

Figure A5: An Example of an ROL



Sorszám	Intézmény - Szak - Szint - Munkarend - Finanszírozási forma	Műveletek
1	<u>ELTE-TÁTK, szociológia ANA</u>	↓ ×
2	<u>KRE-BTK, szociológia ANA</u>	↑ ↓ ×
3	<u>BCE-TK, szociológia (magyar nyelven) ANA</u>	↑ ↓ ×
4	<u>ELTE-TÁTK, szociológia ANK</u>	↑ ↓ ×
5	<u>KRE-BTK, szociológia ANK</u>	↑ ↓ ×
6	<u>BCE-TK, szociológia (magyar nyelven) ANK</u>	↑ ×

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

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

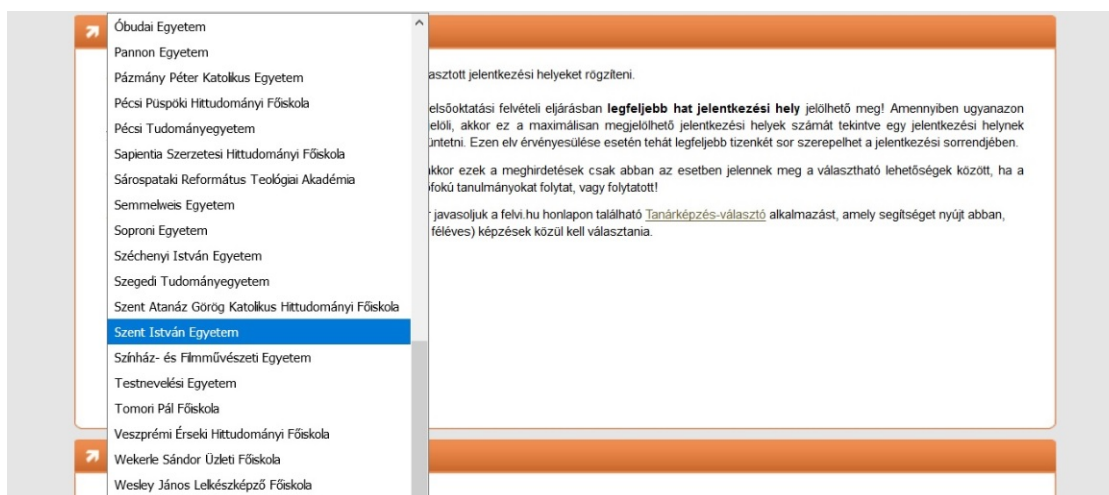
Step 1: Selecting an institution, e.g., Szent István Egyetem (Figure A6).

Step 2: Selecting a faculty, e.g., SZIE-ÉTK (Figure A7).

Step 3: Selecting a contract (i.e., a major – degree – schedule – funding combination), e.g., Építőmérnöki ANA – Civil engineering, Bachelors, full time, funded (Figure A8).

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

Figure A6: Step 1: Institution selection



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

Figure A7: Step 2: Faculty selection

Új jelentkezés felvétele

Szent István Egyetem

[SZIE-AGK - Szent István Egyetem Agrár- és Gazdaságtudományi Kar](#)
[SZIE-ÉTK - Szent István Egyetem Élelmiszertudományi Kar](#)
[SZIE-GTK - Szent István Egyetem Gazdaság- és Társadalomtudományi Kar](#)
[SZIE-GÉK - Szent István Egyetem Gépészmérnöki Kar](#)
[SZIE-KÉTK - Szent István Egyetem Kertészettudományi Kar](#)
[SZIE-MKK - Szent István Egyetem Mezőgazdaság- és Környezettudományi Kar](#)
[SZIE-TÁJK - Szent István Egyetem Tájépítészeti és Településtervezési Kar](#)
[SZIE-YMÉK - Szent István Egyetem Ybl Miklós Építéstudományi Kar](#)

Vissza

< előző

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

Figure A8: Step 3: Contract selection

Felvett jelentkezések

Az **Új jelentkezés felvétele** gombra kattintva tudja a kiválasztott jelentkezési helyeket rögzíteni.

Felhívjuk szíves figyelmüket, hogy a 2018. évi általános felsőoktatási felvételi eljárásban **legfeljebb hat jelentkezési hely** jelölhető meg! Amennyiben ugyanazon jelentkezési hely mindkét finanszírozási formáját is megjelöli, akkor ez a maximálisan megjelölhető jelentkezési helyek számát tekintve egy jelentkezési helynek számít, és a felvételi eljárásban szükséges feltüntetni. Ezen elv érvényesülése esetén tehát legfeljebb tizenkét sor szerepelhet a jelentkezési sorrendjében.

Építész (angol nyelven) MNK
 Építész (magyar nyelven) MNA
 Építész (magyar nyelven) MNK
 Építőmérnöki ANA
 Építőmérnöki ANK
 Építőmérnöki ALA
 Építőmérnöki ALK
 Építőmérnöki ANA
 Építőmérnöki ANK
 Építőmérnöki ALA
 Építőmérnöki ALK
 Műszaki menedzser ANA
 Műszaki menedzser ANK
 Műszaki menedzser ALA
 Műszaki menedzser ALK
 Építőmérnöki ANA

Új jelentkezés felvétele

Építőmérnöki Kar (SZIE-YMÉK)

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

D Data Appendix

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

D.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.³² 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.³³ 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.

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

D.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).

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 [D1](#) 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 D1: Matching college admissions data to NABC

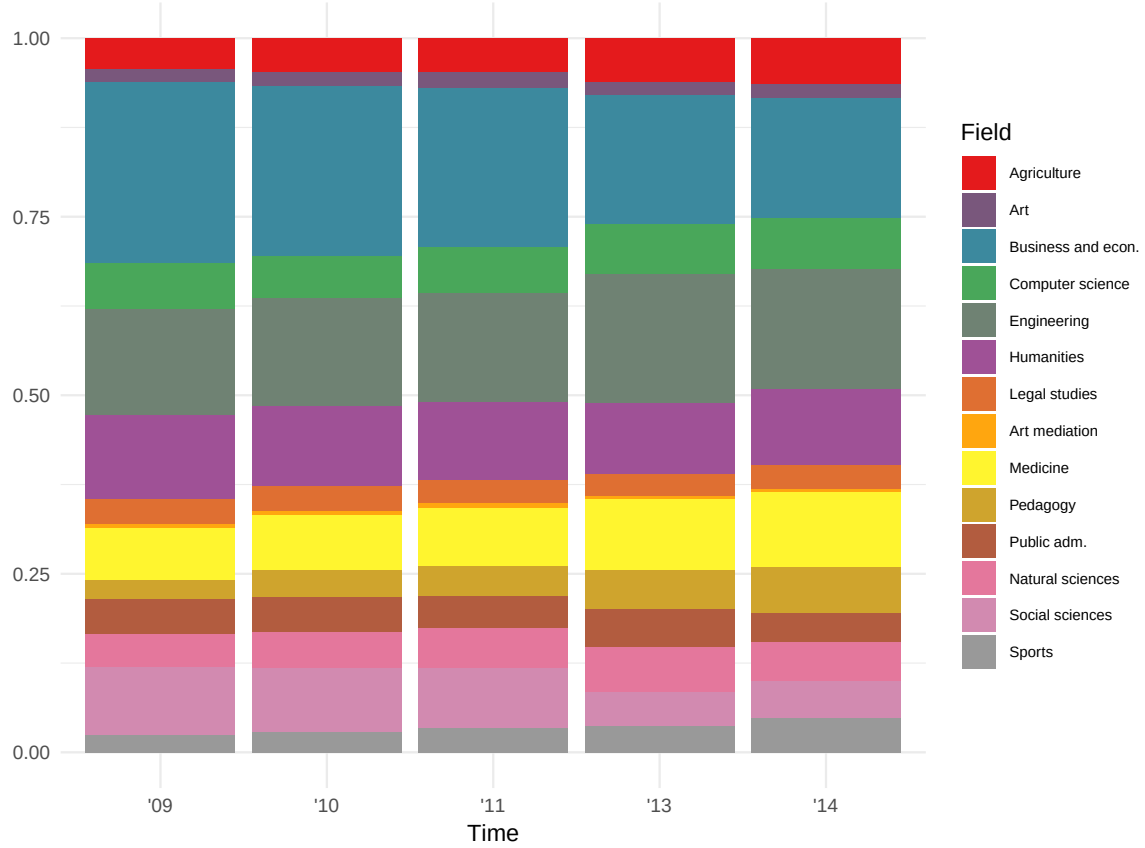
	Matched individuals		Uniquely matched individuals	
	Share (%)	Count	Share (%)	Count
	(1)	(2)	(3)	(4)
<i>A. Matching based on 3 variables</i>				
2009	89.2	45,280	28.8	14,632
2010	89.7	45,060	22.1	11,080
2011	91.8	44,941	19.5	9,544
2012	91.6	36,421	18.5	7,365
2013	91.2	35,470	15.8	6,133
2014	92.2	37,241	16.6	6,710
Total	90.9	244,413	20.6	55,464
<i>B. Matching based on 4 variables</i>				
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
<i>C. Matching based on 5 variables</i>				
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.

E 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 E1: 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 E1: Individual-level summary statistics over time

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

Notes: The table reports mean values of student characteristics, with standard deviations in parentheses over time. Disadvantaged status is an indicator for claiming priority points for disadvantaged status. GPA is the average grade in Hungarian grammar and literature and mathematics. Grades are standardized among eligible applicants. Some applicants have no incentive to report their GPA to the clearinghouse (see Footnote 22). 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 19).

F Additional Results

In Appendix F.1, we examine the correlates of dominated choices and find that dominated choices are more prevalent among high socioeconomic status and low academic ability applicants. These findings are robust to various sample restrictions. Then, in Appendix F.2, we provide additional results on the effect of selectivity on dominated choices exploiting the 2012–13 reform. Then, in Appendix F.3, we present estimates that exploit all variations in the availability of funded positions in the sample. Finally, in Appendix F.4, we provide additional results on the effect of selectivity on dominated choices using within-ROL comparisons.

F.1 The Correlates of Dominated Choices

This section examines the characteristics of applicants who made dominated choices. We regress an indicator for dominated choices on individual-level demographic variables, proxies of socioeconomic status, academic achievement, and year fixed effects. We focus on the sample of high-school seniors, for whom we can use the richer NABC data. It is important to note that these regressions provide descriptive evidence on the characteristics of applicants who submitted ROLs with dominated choices, but we cannot attribute a causal interpretation to the estimated coefficients.

Appendix Table F1 summarizes our findings. Applicants of a higher SES according to the NABC-based index make more dominated choices on average (column (1)), and this correlation is even stronger once we control for academic achievement (columns (2) and (3)). In columns (4)–(6) we corroborate the correlation of dominated choices with proxies for SES (microregional-level unemployment rate, settlement-level gross annual per capita income, and indicator for claiming priority points for disadvantaged background).

Table F1: Demographics, socioeconomic status, academic achievement and dominated choices

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

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing ($N = 162,978$). The share of dominated choices is 5.7% in this subsample. Eleventh-grade GPA is missing for 15.3% of the sample. We include an indicator of those missing observations in our regressions.

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

Applicants of low socioeconomic status were less likely to apply for unfunded positions, a necessary condition for detecting a dominated choice. We argue that this channel does not drive the positive relationship between proxies for SES and dominated choices. In Appendix Table F2 we restrict attention to ROLs that include at least one unfunded contract, and repeat the same analysis. The results continue to hold.

We next investigate whether academic achievement is correlated with dominated choices. First, we examine the 10th-grade NABC numeracy skill. Appendix Table F1 establishes a strong negative correlation between numeracy skill and dominated choices. This score is normalized to have zero mean and a standard deviation of one in the general population. A one standard deviation increase in the numeracy skill is associated with a 2.4 percentage points (42.1 percent) decline in the probability of making dominated choices (column (2)). Once we control for 11th-grade GPA, the estimated coefficient drops to 1.1–1.4 percentage points (19.3–24.6 percent, columns (3)–(6)).

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

We also find that female applicants were 1 percentage point (23 percent) more likely to make a dominated choice.³⁴ Additionally, the fraction of dominated choices was increasing in the size of the settlement in which the applicants resided. Finally, we do not find robust differences between students who attended secondary vocational schools and their peers in secondary grammar schools.

Appendix Table F3 demonstrates that the positive correlation between socioeconomic status and dominated choices, and the negative correlation between academic achievement and dominated choices, hold both in the pre- and post-reform periods. Moreover, these results continue to hold if we do not rely on the NABC dataset (Appendix Table F4).

Table F2: Demographics, socioeconomic status, academic achievement and dominated choices: Relevant applicants

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

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those relevant high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing (N = 56,533). The share of dominated choices is 15.5 percent in the relevant subsample. Eleventh-grade GPA is missing for 15.0 percent of the sample. We include an indicator for those missing observations in our regressions.

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

Table F3: Demographics, socioeconomic status, academic achievement and dominated choices: Pre- and post-reform

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

Notes: The table presents the results of a linear regression of an indicator for dominated choices on gender, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on year fixed effects and demographics, such as age, high-school type, and type of residence. Robust standard errors are in parentheses. Numeracy skill, literacy skill, and the NABC-based SES index are matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to those high-school senior applicants whose numeracy skills, literacy skills, and NABC-based SES index are not missing ($N = 162,972$). The sample includes 78,615 (84,357) pre-reform (post-reform) ROLs. The share of dominated choices is 5.7% in this subsample of the high-school senior applicant sample. Eleventh-grade GPA is missing for 15.3% of the sample. We include an indicator for those missing observations in our regressions.

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

Table F4: Demographics, socioeconomic status, academic achievement and dominated choices: All high-school senior applicants

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

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

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

F.2 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 F5, we estimate the effect of the 2012–13 reform on dominated choices using only the highest ranked application in each ROL. Second, in Table F6, we estimate the effect of the 2012–13 reform on revealed dropping and on revealed flipping separately. Third, in Table F7, we analyze the effect of a small-scale reform occurred in 2011. Fourth, in Table F8, we present heterogeneous effects by the NABC-based numeracy and NABC-based SES measures.

Table F5: The effect of admission selectivity on dominated choices: 2012–13 reform: Highest ranked application

Dependent variable	Dominated choices	
	(1)	(2)
Severe funding cut $\times$ 2013	0.177*** (0.005)	0.171*** (0.005)
Severe funding cut $\times$ 2014	0.148*** (0.004)	0.143*** (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 F6: The effect of admission selectivity on revealed dropping and on revealed flipping: 2012–13 reform

Dependent variable	Revealed dropping	Revealed flipping
	(1)	(2)
Severe funding cut $\times$ 2013	0.171*** (0.004)	0.014*** (0.001)
Severe funding cut $\times$ 2014	0.154*** (0.004)	0.019*** (0.001)
Program FE	Yes	Yes
Year FE	Yes	Yes
Demographics & GPA	Yes	Yes
School FE	Yes	<i>Yes</i>
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 F7: 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 F8: Heterogeneity: The effect of admission selectivity on dominated choices: 2012–13 reform

	A. <i>NABC numeracy skill</i>				
	Q1	Q2	Q3	Q4	Q5
Severe funding cut $\times$ 2013	0.227*** (0.011)	0.199*** (0.010)	0.195*** (0.010)	0.180*** (0.010)	0.114*** (0.010)
Severe funding cut $\times$ 2014	0.212*** (0.010)	0.195*** (0.010)	0.173*** (0.009)	0.134*** (0.008)	0.120*** (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. <i>NABC-based SES</i>				
	Q1	Q2	Q3	Q4	Q5
Severe funding cut $\times$ 2013	0.150*** (0.011)	0.156*** (0.010)	0.170*** (0.010)	0.193*** (0.011)	0.209*** (0.011)
Severe funding cut $\times$ 2014	0.141*** (0.009)	0.155*** (0.009)	0.173*** (0.010)	0.177*** (0.009)	0.174*** (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 A10).

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

F.3 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 A4). 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:

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

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.³⁵ 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 F9 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 F9: The effect of admission selectivity on dominated choices: Alternative specification

Dependent variable	Dominated choices			
	(1)	(2)	(3)	(4)
Capacity (realized, log)	−0.082*** (0.001)	−0.079*** (0.001)		
Capacity (admin, log)			−0.078*** (0.001)	−0.075*** (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.

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

F.4 The Effect of Admission Selectivity on Dominated Choices: A within-ROL comparison: Robustness

Table F10: 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*** (0.001)	0.030*** (0.001)	0.033*** (0.001)	0.032*** (0.001)	0.035*** (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 F11: 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*** (0.000)	0.004*** (0.000)	0.004*** (0.000)	0.004*** (0.000)	0.004*** (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 F12: Admission selectivity and dominated choices: A within-ROL comparison: Application Fee Structure Comprehension

Dependent variable	Dominated choices	
	(1)	(2)
Priority-score cutoff $\times$ MU	0.078*** (0.005)	0.076*** (0.005)
Priority-score cutoff $\times$ (1 - MU)	0.030*** (0.001)	0.034*** (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 F13: Heterogeneity: Admission selectivity and dominated choices: A within-ROL comparison

A. <i>NABC numeracy skill</i>					
	Q1	Q2	Q3	Q4	Q5
Priority-score cutoff	0.060*** (0.005)	0.052*** (0.004)	0.041*** (0.004)	0.029*** (0.004)	0.022*** (0.002)
Within R-squared	0.011				
B. <i>NABC-based SES</i>					
	Q1	Q2	Q3	Q4	Q5
Priority-score cutoff	0.027*** (0.004)	0.035*** (0.004)	0.040*** (0.004)	0.036*** (0.004)	0.059*** (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 A10).

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

G Additional Results on Costly Dominated Choices (Online Appendix)

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

Section [G.1](#) presents the correlates of costly dominated choices, Section [G.2](#) shows the difference-in-differences estimates of the effect of admission selectivity on ex post costly dominated choices.

G.1 Ex Post Costly Dominated Choices: Correlates

Table G1: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Lower bound

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

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the high-school senior applicant sample to individuals whose numeracy skill, literacy skill, and NABC-based SES index are not missing (N = 162,978). The fraction of ex post costly dominated choices (lower bound) in this sample is 0.24 percent.

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

Table G2: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Upper bound

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

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the high-school senior applicant sample to individuals whose numeracy skill, literacy skill, and NABC-based SES index are not missing (N = 162,978). The fraction of ex post costly dominated choices (upper bound) in this sample is 0.56 percent.

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

Table G3: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Lower bound, relevant applicants

Dependent variable	(1)	(2)	(3)	(4)
NABC-based SES index	-0.0006 (0.0005)			
Unemployment rate in 2008 (%)		-0.0002* (0.0001)		
Gross annual per capita income (1000 USD)			0.0001 (0.0003)	
Disadvantaged status				-0.0024* (0.0013)
Numeracy skills	-0.0007 (0.0005)	-0.0009* (0.0005)	-0.0008* (0.0005)	-0.0008* (0.0005)
11th-grade GPA (standardized)	-0.0012*** (0.0004)	-0.0012*** (0.0004)	-0.0012*** (0.0004)	-0.0012*** (0.0004)
Female	-0.0006 (0.0008)	-0.0006 (0.0008)	-0.0006 (0.0008)	-0.0005 (0.0008)
Vocational school	-0.0011 (0.0009)	-0.0009 (0.0008)	-0.0009 (0.0008)	-0.0009 (0.0008)
Other school	0.0043 (0.0042)	0.0042 (0.0042)	0.0043 (0.0042)	0.0043 (0.0042)
County capital	-0.0018* (0.0011)	-0.0014 (0.0011)	-0.0015 (0.0012)	-0.0017 (0.0011)
Town	0.0004 (0.0010)	0.0011 (0.0011)	0.0007 (0.0013)	0.0006 (0.0010)
Age FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
R-squared	.0051	.0052	.0051	.0051

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing ($N = 56,533$). The fraction of ex post costly dominated choices (lower bound) in this sample is 0.68 percent.

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

Table G4: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Upper bound, relevant applicants

Dependent variable	Ex Post Costly Dominated Choices: Upper bound		
	(1)	(2)	(3)
NABC-based SES index	-0.0001 (0.0007)		
Unemployment rate in 2008 (%)		-0.0000 (0.0001)	
Gross annual per capita income (1000 USD)			-0.0000 (0.0005)
Disadvantaged status			-0.0013 (0.0022)
Numeracy skills	0.0014* (0.0007)	0.0014* (0.0007)	0.0013* (0.0007)
11th-grade GPA (standardized)	0.0003 (0.0007)	0.0003 (0.0007)	0.0003 (0.0007)
Female	-0.0013 (0.0012)	-0.0013 (0.0012)	-0.0013 (0.0012)
Vocational school	-0.0015 (0.0013)	-0.0014 (0.0013)	-0.0014 (0.0013)
Other school	0.0100 (0.0063)	0.0100 (0.0063)	0.0100 (0.0063)
County capital	-0.0027 (0.0017)	-0.0027 (0.0017)	-0.0027 (0.0017)
Town	-0.0023 (0.0016)	-0.0022 (0.0017)	-0.0022 (0.0016)
Age FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
R-squared	.0059	.0059	.0059

Notes: The table presents the results of a linear regression of an indicator for dominated choices on demographics, measures of academic achievement, and measures of socioeconomic status. The regression coefficients are conditional on age and year fixed effects. Robust standard errors are in parentheses. The NABC-based SES index is matched to the main dataset based on 5 variables (year and month of birth, gender, school identifier, and 4-digit postal code). We restrict the sample to relevant high-school senior applicants whose numeracy skill, literacy skill, and NABC-based SES index are not missing (N = 56,533). The fraction of ex post costly dominated choices (upper bound) in this sample is 1.62 percent.

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

Table G5: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Lower bound

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

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

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

Table G6: Demographics, socioeconomic status, and academic achievement vs. ex post costly dominated choices: Upper bound

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

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

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

G.2 The Effect of Admission Selectivity on Ex Post Costly Dominated Choices: 2012–13 reform

Table G7: The effect of admission selectivity on ex post costly dominated choices: 2012–13 reform

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

Notes: The table presents the effect of admission selectivity on ex post costly dominated choices. Robust standard errors clustered on the applicant level are in parentheses. The mean lower (upper) bound in the sample is 0.08 (0.24) percent. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, high-school type, and dummies for 11th-grade GPA. Missing control variables are indicated by a separate dummy variable.

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

Table G8: Heterogeneity by 11th-grade GPA: The effect of admission selectivity on ex post costly dominated choices: 2012–13 reform

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

Notes: The table presents heterogeneous effects of admission selectivity on ex post costly dominated choices. Each column estimates the coefficients in a single regression by interacting the treatment variable with a subgroup indicator of 11th-grade GPA. Robust standard errors clustered on the applicant level are in parentheses. The mean lower (upper) bound in the sample is 0.08 (0.24) percent. All specifications include year and program fixed effects. Demographic controls include gender, disadvantaged status, age, type of residence, and high-school type. Missing control variables are indicated by a separate dummy variable.

***; $p < 0.01$, **; $p < 0.05$, *; $p < 0.1$.