THE LIMITS OF INCENTIVES IN ECONOMIC MATCHING PROCEDURES*

Avinatan Hassidim[a], Assaf Romm[b] and Ran I. Shorrer[c]

Organizations often require agents' private information to achieve critical goals such as efficiency or revenue maximization, but frequently it is not in the agents' best interest to reveal this information. Strategy-proof mechanisms give agents incentives to truthfully report their private information. In the context of matching markets, they eliminate agents' incentives to misrepresent their preferences. We present direct field evidence of preference misrepresentation under the strategy-proof deferred acceptance in a high-stakes matching environment. We show that applicants to graduate degrees in psychology in Israel often report that they prefer to avoid receiving funding, even though the mechanism preserves privacy and funding comes with no strings attached and constitutes a positive signal of ability. Surveys indicate that other kinds of preference misrepresentation are also prevalent. Preference misrepresentation in the field is associated with weaker applicants. Our findings have important implications for practitioners designing matching procedures and for researchers who study them.

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[a]Bar-Ilan University, Ramat Gan, Israel. avinatanh@gmail.com

[b]Hebrew University, Jerusalem, Israel. assaf.romm@mail.huji.ac.il

[c]Penn State University, State College, PA, United States. rshorrer@gmail.com

1. INTRODUCTION

In many settings, organizations require agents' private information to achieve critical goals such as efficiency or revenue maximization, but it is often not in agents' best interest to reveal this information. For example, an auctioneer may want to charge the highest price some agent is willing to pay, but this will give bidders incentive to misrepresent their true valuation. Similarly, firms may want to prioritize customers with more urgent needs, and this gives customers an incentive to overstate the urgency of their case. In light of this tension, market designers have devoted much attention to the problem of designing strategy-proof allocation mechanisms, where it is in agents' best interest to reveal their private information.

In recent years, a growing number of students are being assigned to schools through centralized clearinghouses. Inspired by market design theory, both existing and new clearinghouses are adopting strategy-proof allocation mechanisms (i.e., where agents have no incentive to misrepresent their true preferences), especially ones based on the deferred acceptance algorithm (DA, Gale & Shapley, 1962).[1] Thaler & Sunstein (2008) summarize the main benefits of strategy-proof allocation mechanisms for school choice in saying that they do *"not penalize parents who are unsophisticated about the choice process,"* and that *"in return administrators do not have to guess about parents' true preferences."* But do agents always truthfully report their preferences to these mechanisms?

We identify a substantial fraction of applicants in a high-stakes environment who misrepresent their preferences even though the mechanism uses the strategy-proof (applicant-proposing) DA (Dubins & Freedman, 1981; Roth, 1982).[2] Misrepresentation is systematic: weaker applicants misrepresent their preferences more often.

[1] Examples include school choice in Boston and New York City (Abdulkadiroğlu *et al.*, 2005, 2009), and college admissions in Australia and Hungary (Artemov *et al.*, 2017; Biró, 2007). Many popular mechanisms are approximately strategy-proof in large markets (Azevedo & Budish, 2018).

[2] We intentionally do not label this behavior as "mistake" throughout the paper, as it can be rationalized (e.g., if students' preferences violate the standard assumptions made in the matching literature). See the discussion in Section 6 for more details.

Detecting deviations from the truthful reporting assumption in the field is a difficult challenge. We present a novel approach to detecting such deviations in college-admissions environments, where applicants are required to rank admission to the same academic program under different financial terms (e.g., with or without a scholarship). We say that an applicant *obviously misrepresents her preferences* if she submits to the mechanism a rank-order list (ROL) that is inconsistent with the natural ordering (i.e., she reports that she prefers not to receive funding).[3]

We apply our approach to administrative data from the Israeli Psychology Master's Match (IPMM), a clearinghouse that matches students to graduate programs in psychology (including MA programs in clinical psychology, a requirement for becoming a therapist). Approximately one out of five ROLs that rank programs that offer admission under multiple levels of financial aid is an obvious misrepresentation. Between 2% and 8% of the cases of obvious misrepresentation were costly ex post, with an average monetary loss of more than $5,000.[4]

Notably, the fraction of untruthful agents may be substantially higher as applicants may have misrepresented their preferences in ways that cannot be detected using our approach, such as ranking as first a program other than their favorite. We provide survey evidence supporting this hypothesis. We also provide evidence suggesting that survey-based estimates understate the rate of misrepresentation; comparing reports of obvious misrepresentation with observed behavior, we find that survey-based estimates of obvious misrepresentation are biased downward.

Obvious misrepresentation is more common among academically weaker applicants, as measured by their academic achievements, the prestige of their undergraduate program, and their position in

[3]Following the circulation of an earlier draft of this paper, our method was used by others to detect preference misrepresentations in college-admissions markets in Turkey, Australia, and Hungary (Arslan, 2018; Artemov *et al.*, 2017; Shorrer & Sóvágó, 2017). Obvious misrepresentations have also been detected in the American Genetic Counseling Admissions Match (see Peranson, 2019). Our approach for detecting preference misrepresentation can be used in a variety of other markets where ranked alternatives are offered.

[4]While under the standard assumptions preference misrepresentations are individually suboptimal, their implications for social welfare can be both positive and negative (Rees-Jones, 2017a).

programs' rankings. A potential explanation is that academically weaker applicants are more likely to misunderstand the instructions provided by the clearinghouse.[5] However, the applicants, all of whom are college graduates with a competitive major (psychology), receive very simple instructions about the optimal strategy on multiple occasions. An alternative explanation is that applicants tend to misrepresent their preferences more when they expect that the likelihood of admission to their preferred choices is low.

We propose three explanations for the body of evidence that we document. The first is that applicants, who typically have no experience with DA, misperceive the rules of the mechanism (Cason & Plott, 2014), leading them to use the heuristic that a higher ranking is rewarded with a higher probability of assignment. This heuristic is consistent with the optimal behavior in many common economic environments, including many popular assignment mechanisms. The second explanation is that applicants who assign zero (or negligible) probability to admission with funding use a weakly dominated strategy that is still (approximately) optimal under their beliefs (Artemov *et al.*, 2017; Chen & Pereyra, 2019). The third explanation is that the standard model of matching market design does not capture an important aspect of applicants' preference (e.g., applicants may be trying to avoid disappointment Dreyfuss *et al.*, 2019), and as a result ranking alternatives truthfully is sometimes suboptimal. In Section 6, we elaborate on these and other potential explanations. Importantly, all explanations imply that administrators cannot regard preference reports as true preferences.

Our findings have important implications for the study and design of matching marketplaces. First, they highlight the critical importance of the way that advice is communicated to applicants. In particular, the way the mechanism is described and the availability of decision support are crucial elements of inducing truthful reports. For example, comparing our findings to those of Shorrer & Sóvágó (2017), who use our approach, suggests that misrepresentations due to lack of understanding of the rules can be minimized by describing the mechanism in terms of admission cut-offs.

Industry has not overlooked the importance of these issues, as is epitomized by the business prac-

[5] See Benjamin *et al.* (2013); Basteck & Mantovani (2018); Guillen & Hakimov (2017).

tices of National Matching Services (NMS). NMS is a private firm operates in North America. This company organizes entry-level placement matches in a number of industries and professions, including healthcare, education, law, and financial services. With years of accumulated experience, NMS is well aware of applicants' failures in reporting their true preferences over positions. Consequently, they have developed tutorials and decision support tools to assist applicants in making their choices, as well as post-match surveys to assess their success. In 2018, NMS created a matching marketplace for North American Genetic Counseling programs – a medical profession with approximately 1,000 new graduates each year in recent years. The design adopted by NMS closely resembles the design of the IPMM (Peranson, 2019). Importantly, a large share of the participating programs offer admission under multiple levels of funding. In light of the special features of the Genetic Counseling Admissions Match, and the potential for obvious misrepresentation, NMS created a decision support system called CONFIRM (CONtact For Identified Ranking Mistakes) that aims to identify applicants who reported unexpected preferences and verifying that their report is not the result of a mistake or misunderstanding. The system uses several algorithms to identify potential mistakes while they can still be corrected (using information from both students and programs preference reports).

A second issue is whether preference reports to DA can be assumed truthful. This assumption is used by many studies to evaluate the consequences of alternative strategy-proof mechanisms. For example, it is used to evaluate different randomization methods (Abdulkadiroğlu *et al.*, 2009), different seat reservation schemes (Dur *et al.*, 2018), and different matching algorithms (Abdulkadiroğlu *et al.*, 2017; Che & Tercieux, 2019). The underlying rationale is that when one strategy-proof mechanism is replaced with another, only the matching algorithm is changed, and there should be no behavioral response.[6] Some readers of the working paper version of this article have suggested that in light of evidence on preference misrepresentations, this methodology may be problematic. Fack *et al.* (2019)

[6]The assumption of truthful reporting is also used to evaluate the consequence of changing a manipulable mechanism to a strategy-proof alternative and vice versa (Budish & Cantillon, 2012; He & Magnac, 2017), and to estimate applicants' preferences (e.g., Hällsten, 2010).

find that *"incorrectly imposing truth-telling leads to a serious under-estimation of preferences for popular or small schools,"* and propose replacing the assumption of truthful reporting with the weaker assumption of "stability," when applicants are not facing substantial uncertainty about the set of feasible alternatives (see Artemov *et al.*, 2017). Arslan's (2018) findings in Turkish college admissions are similar.

1.1. *Related Literature*

There is a large literature documenting suboptimal behavior in education markets. Informational frictions and the complexity of the application process are often blamed (e.g., Bettinger *et al.*, 2012; Hastings & Weinstein, 2008; Pallais, 2015). Suboptimal behavior has also been documented in centralized school-choice environments where a strategically demanding mechanism, such as the Boston mechanism, is in place (e.g., He, 2015; Kapor *et al.*, 2016). By contrast, the environment we study was designed to eliminate strategic considerations, and informational frictions and the complexity of the application process are not likely explanations.[7]

Numerous recent studies suggest that a substantial fraction of agents may misrepresent their preferences under DA. In the lab, Chen & Sönmez (2006) find that about 30% of the "proposers" failed to report their true preferences under DA, and the number was even higher for the strategy-proof top trading cycles mechanism (Shapley & Scarf, 1974). This finding is robust: similar results were found under a variety of treatments and variations of these environments.[8]

In the field, where the stakes are high and individuals are informed of the optimal strategy and are free to seek advice, Rees-Jones (2017b) provides survey-based evidence of preference misrepresentation, and Chen & Pereyra (2019) provide suggestive evidence. Rees-Jones & Skowronek (2018) detect

[7] Budish & Kessler (2017) address the related question of students' ability to express their preferences using a reporting language in a more complex course-scheduling environment.

[8] Examples include Braun *et al.* (2014), Calsamiglia *et al.* (2010), Chen & Kesten (2019), Ding & Schotter (2016, 2019), Echenique *et al.* (2016), Featherstone & Niederle (2016), Guillen & Hing (2014), Pais & Pintér (2008), Pais *et al.* (2011), and Zhu (2015).

misrepresentation in an online experiment whose participants were medical doctors who submitted their preferences to the National Resident Matching Program (NRMP) days before. Our approach provides direct evidence of extensive misrepresentation in the field, relying exclusively on observational data.

Several recent studies find that weaker applicants misrepresent their preferences more under DA (see Hassidim *et al.*, 2017a). Artemov *et al.* (2017) and Shorrer & Sóvágó (2017) use the approach we present here for detecting misrepresentation and find a negative correlation between obvious misrepresentation and academic ability in Australia and Hungary, respectively. Rees-Jones (2017b) finds a similar pattern in a survey of participants in the NRMP. In all of the above-mentioned settings, academic ability and admission priority (i.e., the strength of the applicant) are positively correlated, making it difficult to separate the effects of the two. In the working paper version of this article (Hassidim *et al.*, 2016) we use data from one of the experimental treatments of Li (2017), and establish a strong negative causal relationship between the strength of an applicant and the rates of misrepresentation.[9] This result is corroborated by Rees-Jones & Skowronek (2018) and Shorrer & Sóvágó (2017).

Preference misrepresentation in strategy-proof environments is not a phenomenon limited to matching markets. Laboratory experiments have found a similar phenomenon in a variety of strategy-proof environments (e.g., Attiyeh *et al.*, 2000; Kagel *et al.*, 1987). In light of such findings, there is increasing interest in mechanisms that are robust to behavioral faults (McFadden, 2009) and in criteria stronger than strategy-proofness such as secure implementation (Cason *et al.*, 2006; Saijo *et al.*, 2007) and obvious strategy-proofness (Li, 2017). These notions have already influenced the design of the radio spectrum allocation auctions (Leyton-Brown *et al.*, 2017). Our findings further underscore the practical importance of such notions.[10]

[9]For additional experimental evidences, see Bó & Hakimov (2019) and Echenique *et al.* (2016).

[10]Ashlagi & Gonczarowski (2018) show that stable outcomes cannot be implemented in a manner that is obviously strategy-proof for applicants.

2. BACKGROUND: THE ISRAELI PSYCHOLOGY MASTER'S MATCH

In this section we provide a brief review of the IPMM (see Hassidim *et al.*, 2017b). We begin with a brief review of the pre-existing institutions and then describe the centralized market, focusing on the unique features that allow us to detect applicants' deviations from truthful reporting.

2.1. *Admissions to Psychology Graduate Programs prior to 2014*

Prior to 2014, admission to Master's and PhD programs in psychology was a mostly decentralized process, with some coordination between departments with regard to the dates on which notifications of admission, rejection, or wait-list status were sent to applicants. Applicants applied to different programs by sending materials such as undergraduate transcripts, MITAM scores,[11] and recommendation letters.[12] Next, the programs selectively invited applicants to interviews, after which each program ranked its applicants. At this point the actual matching process began.

There were three agreed-upon dates on which programs were supposed to contact applicants:

- On the first date (henceforth round), programs called applicants and notified them about their admission, wait-list status, or rejection. Applicants then had about a week to choose between the offers they had received. By the end of the week, they had to inform programs about the rejection of offers or the tentative acceptance of a single offer.
- On the second round, programs called wait-listed applicants and notified them about admission, wait-list status, or rejection. The applicants again had a week to respond. At the end of this week, they were allowed to withdraw their previous acceptance and to accept (deterministically) at most one offer.

[11] The MITAM is an exam that was designed to facilitate screening of applicants for advanced degrees in psychology. It is administered once a year by the Israeli National Institute for Testing and Evaluation. The exam is comprised of two sections: (i) proficiency in psychological research methods and (ii) comprehension of scientific texts in psychology. For more information see https://www.nite.org.il/index.php/en/tests/mitam.html (accessed 7/27/2015).

[12] Each institution charges a flat application fee of 460NIS (about $120).

- On the third and final round, programs called applicants on their wait-list and offered admission. Applicants could no longer withdraw previous acceptances, but could only deterministically accept incoming offers. Offers at this stage were often "exploding" (had to be accepted or rejected by the end of the phone call).

This process raised several concerns, which mirror concerns about the decentralized matching process for American clinical psychologists in the 1990s (Roth & Xing, 1997). Specifically, programs had an incentive to recruit in early rounds, and thus they acted strategically by offering admission to more applicants than their intended cohort size and by approaching applicants who were likely to accept their offers early on (e.g., applicants whose family lives in the vicinity of the institution). Applicants also faced strategic dilemmas. For example, applicants who were on the wait-list of their most preferred program by the end of the second round and received an offer from a program they liked less faced the strategic choice between the "riskier" option of waiting and the "safer" acceptance of the offer from the less preferred program.

2.2. *The Israeli Psychology Master's Match*

In response to concerns about pre-existing market institutions, Hassidim *et al.* (2017b) proposed to replace the existing strategically demanding decentralized protocol with a centralized clearinghouse that uses DA. The new mechanism is largely based on the DA algorithm, with the required adaptations to accommodate the unique preference structure of departments as well as of couples on the applicant side.[13] The admission process begins in the exact same way it used to prior to the redesign,[14] but after

[13]Departments could use affirmative action, submit different rankings of applicants for different programs and terms, and use quotas as in Kamada & Kojima (2018). The possibility to apply as a couple was introduced only in 2015 to accommodate a very small number of couples (one couple used this option in the 2015 match). The adapted DA used in 2014 was extended in a similar fashion to the Sorted Deferred Acceptance algorithm suggested by Ashlagi *et al.* (2014), which is approximately strategy-proof in large markets with a small number of couples.

[14]In particular, institutions still charge a flat application fee of 460NIS (about $120), independently of the number of programs or tracks the student applies to.

the interview stage is completed, applicants are prompted to submit an ROL ranking the positions (program-terms pairs) they may wish to enroll in. Programs are also asked to report their preferences to the centralized clearinghouse at this time. After the preferences reporting stage, the adapted version of the applicant-proposing DA is applied and students and programs are informed about their assignment.

Participants. There are nine universities (PhD-granting institutions) and about twenty colleges in Israel. Universities are publicly funded and have identical tuition costs. College tuition varies, but it is always greater than or equal to university tuition. In general, graduating from a university is more prestigious than graduating from a college.

Thirteen departments offered admissions to their PhD and Master's programs in psychology exclusively through the centralized clearinghouse. More than 90% of the applicants completed their Bachelor's studies in one of the participating departments. Only one college that offers graduate degrees in psychology did not participate. This college was not part of the pre-existing decentralized protocol, and was not considered a competitor by the participating institutions.

Funding and dual listing. Some departments offer positions in the same program, but under different terms. In particular, several programs endow a small subset of admitted students with prestigious no-strings-attached scholarships (e.g., "Presidential Scholarships"). Such scholarships may be a key determinant of applicants' preferences. For example, an applicant's most preferred options could be: 1) program A with funding, 2) program B, and 3) program A without funding. The mechanism is expressive enough to accommodate such preferences. Applicants are asked to rank each alternative (e.g., A with funding) separately, as in Sönmez (2013).

The ability to attract high-quality applicants using a small number of exclusive scholarships was demanded by some departments, which felt that historically scholarships allowed them to improve the quality of their incoming class. In 2014, a total of 10 programs in 3 universities allowed applicants to rank their programs with and without funding. In 2015, one more university allowed applicants to rank its five programs with and without funding. Three universities offered two-year MA scholarships that ranged from 8,000NIS ($2,070) a year up to 90,000NIS ($23,323) a year. Another university offered PhD scholarships that ranged from 16,182NIS ($4,218) for three years up to 213,879NIS ($55,760) for a five-year program. The lowest level of funding covers roughly a year's tuition, whereas the highest pays slightly more than the median salary in Israel.

Releasing information. Departments and applicants were informed that their reported preferences and placement would not be revealed to anyone (other than in the form of aggregate statistics), including other applicants and programs. The only exception was that contact information of unmatched applicants would be transferred to programs that either failed to fill their capacity or had open positions due to "no-shows." As a result of this policy, programs could only learn that an applicant had expressed a preference for receiving funding if she was assigned to that program with funding. Specifically, if an applicant was assigned without funding, the program could not tell if she ranked the funded position above the non-funded one.

Educating participants. Faculty and staff in participating departments attended presentations in which both DA and the fact that it was strategy-proof for applicants were covered in great detail. It was also explained that untruthful reporting could, in theory, be beneficial for the programs, but that gaining something from such misrepresentation usually requires extensive knowledge of both applicants' and other programs' behavior.

Applicants participating in the match were advised on multiple occasions to submit their true preferences, and were told that misrepresenting their preferences could only hurt them as compared to telling the truth. This advice was communicated in all emails and letters received from the automated matching system and from the departments themselves. Furthermore, this issue was addressed in multiple forms on the Frequently Asked Questions (FAQ) section of the matching system's website (see Appendix B).

The system's support team replied to hundreds of inquiries, and strategy-proofness was the subject of dozens. The details of DA and its strategy-proofness were carefully explained to all applicants who inquired about the mechanism. These applicants also received a link to a video of a general-audience lecture on DA in Hebrew.

User interface. Applicants were asked to submit their ROLs online. There was no limit on ROLs' length. The drag-and-drop interface was simple and friendly (see Appendix C), as reflected in responses to user surveys. If an applicant submitted an ROL that included only a subset of the alternatives offered by a particular program (e.g., only the funded position), a pop-up alert appeared. This feature was meant to mitigate the risk of applicants accidentally ranking only some of the positions offered by a program.[15]

Obvious misrepresentation. Under the assumption that, holding their placement fixed, applicants prefer to receive a prestigious no-strings-attached scholarship, an ROL ranking a non-funded position in some program higher than a funded position in the same program (henceforth *obvious flipping*), or ranking only a non-funded position in a program that offers funded positions (henceforth *obvious dropping*), is a misrepresentation of the applicant's true preferences. When an ROL is an obvious flipping or an obvious dropping, we say that the ROL is an *obvious misrepresentation (of true preferences)*. Under the standard assumptions made in the matching literature—that agents know the rules of the game and that their utility depends only on their realized assignment—obvious misrepresentation is a weakly dominated strategy.

3. DATA

3.1. *Administrative Match Data*

Our sample consists of all preference reports submitted to the 2014 and 2015 matches and personal information reported to the matching system (including Bachelor's degree institutions and gender).[16] In 2014, there were 13 departments that offered a total of 52 different programs. Of the 970 applicants who participated in the match, 75.6% were female, 69.6% received their Bachelor's degree from a

[15]This feature was also incorporated into NMS's decision support system CONFIRM (Peranson, 2019).

[16]An anonymized version of the data is available on the journal's website, along with information on how to apply for receiving the full dataset.

university, and 89.4% received their Bachelor's degree from a department with a Master's program. A total of 540 positions were filled through the system, including 25 funded positions.

We call an ROL *relevant* if it includes a non-funded position in a program that also offers a funded position (the ROL need not include the funded position to be considered relevant). Obvious misrepresentation can be detected only in relevant ROLs. In 2014, 260 applicants submitted a relevant ROL. Of these, 73% were female, 68.5% received their Bachelor's degree from a university, and 87.7% received their Bachelor's degree from a department with a Master's program. Only 11 ROLs included a funded position but not the non-funded position in the same program.

In 2015, there were 13 departments that offered 50 different programs. Of the 964 applicants,[17] 74.7% were female, 73.4% received their Bachelor's degree from a university, and 91.6% received their Bachelor's degree from a department with a Master's program. A total of 197 of the applicants had already applied in 2014. Due to the increase in the number of dually listed programs, the number of relevant ROLs grew to 444 (72.3% of which were female, 80.6% with a Bachelor's degree from a university, and 92.8% with a Bachelor's degree from a department with a Master's program). A total of 588 positions were filled through the match, including 35 funded positions. Only 14 ROLs included a funded position but not the non-funded position in the same program.

The level of observation we chose for the main analyses was a particular applicant ROL. This choice left us with 704 ($= 260 + 444$) relevant observations. We further eliminated from our sample 32 observations that corresponded to the first ROL submitted by individuals who applied in both years. We did this to allow for learning when possible (none of our results change if we consider either the complete sample or only the first ROLs).[18] Of the 672 remaining relevant ROLs, 72.7% were submitted by females, 76% by university graduates, and 90.8% by graduates of institutions with a Master's program.

[17] In 2015 couples were allowed to submit a joint preference list. Only one couple used this option, and was excluded from the analysis.

[18] Our results also continue to hold if we include in the sample all ROLs that contain any position in a dually listed program (that is, if we include the 25 ROLs that included only funded positions in dually listed programs).

TABLE I

DESCRIPTIVE STATISTICS: IPMM 2014–2015[a]

	2014	2015
Departments	13	13
Programs	52	50
Dually listed	10	15
Applicants (with repetitions)		
Female	733	720
Male	237	244
BA from university	675	708
BA from MA-granting department	867	883
Placed	540	588
Placed with funding	25	35
Average ROL length	4.09	4.56
Total	970	964
Applicants who submitted relevant ROLs (with repetitions)		
Female	190	321
Male	70	123
BA from university	178	358
BA from MA-granting department	228	412
Placed	193	341
Placed with funding	23	35
Average ROL length	7.15	7.03
Total	260	444
Applicants who submitted relevant ROLs with OMRs (with repetitions)		
Female	33	63
Male	14	27
BA from university	26	63
BA from MA-granting department	39	83
Placed	30	56
Placed with funding	1	1
Average ROL length	6.77	6.88
Total	47	90

[a] Repetitions refer to the 197 applicants who applied in both years. Source: IPMM 2014–2015 administrative data.

3.2. *Survey Data*

In addition to the administrative match data, we also use data from two post-match surveys. The first survey was commissioned by the participating departments and was administered online following the 2014 match in order to assess the reaction to the new system. It was voluntary and anonymous. A total of 367 applicants responded. Since this survey was completely anonymous, results cannot be linked to the administrative match data.

Following the 2015 match, we conducted a telephone survey that was designed to assess user satisfaction with the system, and to assess user perception of the system's incentive properties. We focused on the population of applicants who submitted relevant ROLs. Shortly after the match results were published, we contacted these applicants by phone, and asked them if they would be willing to participate in a voluntary survey about the admission process. They were told that the survey was being conducted on behalf of the administrators of the matching system and that their answers would be kept private and secure, would be used only for research purposes and for improving the system, and that in any case their responses would not be transferred to any department of psychology (except as aggregate results). Applicants who agreed were asked several types of questions. First, their identity was ascertained and they were asked if this was the first year they were applying. Second, they were asked about their experience using the automated system. Third, they were asked about the degree to which they were informed about the mechanism. Fourth, they were asked about the degree to which they misrepresented their preferences. Fifth, they were asked about their degree of satisfaction with the admission process in general and the automated matching system in particular. Sixth, they were asked for some demographic information, including their MITAM score and their assessment of their family's socioeconomic status. Finally, they were asked to provide any additional feedback they had, and were offered the opportunity to receive the results of the survey. Appendix D lists all survey questions in the order they were asked, and Table V describes the variables that we used and provides summary statistics.

The response rate was high, 292/444, over 65%. Many non-respondents were abroad or otherwise unavailable to take the call. This high response rate is consistent both with the high level of satisfaction with the matching system among respondents (an average score of 8.1/10 relative to 4.7/10 for satisfaction with the admission process in general) and with the fact that many of the respondents expressed interest in receiving the survey results or volunteered advice on how to improve the system. Respondents and non-respondents were not statistically different in terms of any of the following characteristics: gender, Bachelor's degree institution, whether the applicant was ranked by some program, whether the applicant submitted an obvious misrepresentation, and type of obvious misrepresentation (obvious flipping or obvious dropping).

4. THE PREVALENCE OF PREFERENCE MISREPRESENTATION

4.1. *Direct Evidence*

TABLE II

THE PREVALENCE OF OBVIOUS MISREPRESENTATIONS: IPMM 2014–2015[a]

	2014	2015	Full sample
Relevant ROLs	260	444	672
Obvious misrepresentation	47	90	130
Obvious dropping	25	43	64
Obvious flipping	25	48	70
Costly misrepresentation (lower bound)	3	0	3
Costly misrepresentation (upper bound)	6	4	10

[a] The full sample does not include the first ROL of applicants who applied in both years. The lower (upper) bound corresponds to the result from ranking the funded contract directly above the non-funded contract (at the top of the ROL). Source: IPMM 2014–2015 administrative data.

Of the 672 ROLs in our sample, 130 (19.3%) obviously misrepresented the applicant's true preferences, with almost equal shares of obvious flipping and obvious dropping. The fractions are stable across years. Preferences over all dually listed programs were obviously misrepresented by some ROLs, with the percentage of ROLs misrepresenting preferences for funding in a certain program ranging from 9% to 29% (mean=16.7%, std. dev.=5.35%) across dually listed programs. In 2015, the fraction of obvious misrepresentations with respect to programs that were already dually listed in 2014 was slightly lower than the fraction of obvious misrepresentations with respect to programs that were dually listed for the first time, but the difference is not statistically significant (14.5% versus 16.7%, $p = .43$).

Of the 289 ROLs that include multiple dually listed programs, 55 ROLs (19.0%) were an obvious misrepresentation. Of these, 24 ROLs (8.3%) ranked the funded position higher for one program but not for another program, 13 (4.5%) of which reversed the order of one pair but not that of another.

These findings refute the assumption of truthfulness under the weaker assumption that the direction of each applicant's preference for (or aversion to) funding is not program-specific.

4.2. *Survey-based Evidence*

Of the 292 participants in the 2015 survey, 38 (13%) reported submitting an ROL that ranked some program higher relative to their true preferences, and 49 (16.8%) reported submitting an ROL that ranked some program lower relative to their true preferences. A total of 59 participants (20.2%) reported at least one of these forms of misrepresentation. When respondents gave a verbal justification for their behavior, it often involved (strategically irrelevant) considerations of chances of admission. Three applicants reported lack of trust in the system as the reason. Only 18 of the 59 participants who reported increasing or decreasing the rank of some program submitted an obvious misrepresentation.

Of the 54 respondents who actually submitted an obvious misrepresentation, only 29 (53.7%) reported this behavior (17 denied and 8 refused to answer this question).[19] By contrast, only 12 of the other 238 respondents (5.0%) falsely reported that they submitted an obvious misrepresentation, and only 8 (3.4%) refused to answer (the differences are statistically significant at $p < 0.01$ using Fisher's exact test). The most common justifications given by respondents for obvious misrepresentation were thinking that chances were slim and improving admission probability. Only three respondents attributed obvious misrepresentation to misunderstanding or mistrusting the system.

The above figures, combined with the lack of evidence of selection in responding to the survey, suggest a downward bias in survey-based estimates of preference misrepresentation, potentially due to individuals' reluctance to report socially undesirable behavior. Of the 367 participants in the 2014 survey, 18% reported submitting an ROL that was only "*partially truthful*," with 1% reporting not submitting their true preferences. Of the 13% of the respondents who reported giving a higher ranking to a study track "*that ranked you high (even though you may have preferred other study tracks)*," more

[19] Of the 28 respondents who submitted an obvious flipping, 16 (57%) reported such behavior. Similarly, only 14 of the 27 (52%) respondents who submitted an obvious dropping reported such behavior.

than a third (5%) also reported that they were truthful.

The 2014 survey was quite direct in its attempt to understand agents' behavior. For example, 18% responded positively to the question: "*In your opinion, was there a strategic advantage in ranking programs to which you think you have a better chance of being admitted (even though it was made clear that there was no such advantage)?*" Most of these respondents could not explain why. An additional 21% reported that they thought that the answer was negative, but they could not explain why.

5. THE COST AND CORRELATES OF PREFERENCE MISREPRESENTATION

Is obvious misrepresentation in the IPMM costly? We address this question in two ways to get a lower and an upper bound for the cost of obvious misrepresentation. First, we change each ROL that obviously misrepresented true preferences, by ranking funded positions just above the corresponding non-funded positions while leaving the rest of the ROLs unchanged (including other ROLs with obvious misrepresentations) and recalculate the outcome of the matching mechanism. This gives us a lower bound of 3 affected individuals. That is, 3 out of 130 individuals would have received a scholarship (of more than $6,000 on average) in the program they were assigned to had they asked for one. For an upper bound, we repeat the same exercise, this time ranking the funded positions as first choices. We get an upper bound of 10 affected individuals. The additional 7 individuals were placed in a program they ranked higher than the non-funded position in the program where they could have received a scholarship (of more than $5,000, on average). Since, in ROLs that were not an obvious misrepresentation, the funded and non-funded contracts in the same program were typically ranked consecutively, it is natural to assume that the true value is closer to the lower bound (with the caveat that this paper establishes that ROLs often do not reflect true preferences, especially with respect to funding).

It is important to stress, however, that the above bounds account for the cost of *obvious* misrepresentation only. Our approach cannot detect other kinds of preference misrepresentation, and thus we are unable to measure their costs. Furthermore, the potential cost of obvious misrepresentation is bounded by the limited availability of scholarships in the IPMM. By contrast, Shorrer & Sóvágó (2017) study an environment with many funded positions and establish a substantial lower bound for the cost of obvious misrepresentation. In Australia, Artemov *et al.* (2017) provide a wider range for the cost of obvious misrepresentation, with a lower bound of 1.39% and an upper bound of 19.92%. In Appendix E, we show that in the absence of a behavioral response (i.e., holding students' ROLs fixed) an increase in the number of scholarships offered by programs would translate to a proportional increase in the share of cases where obvious misrepresentation is costly.

To put the numbers above into perspective, note that 58 out of 567 (10.2%) relevant ROLs with no obvious misrepresentation resulted in placement in a funded position. That is, if we were to perform the symmetric analysis and separately change each ROL with no obvious misrepresentation to an obvious misrepresentation (in all programs, where applicable) holding the rest of the ROLs fixed, 10.2% of the students will be affected.[20] Moreover, the value of lost scholarships would have been over $12,000. The smaller proportion among misrepresenters of applicants who had the potential to be placed in a funded position suggests that it is not the strongest applicants who submit obvious misrepresentations.

Another indication that obvious misrepresentation is more common among weaker applicants is that the number of misrepresenters who hold a (more prestigious) university Bachelor's degree is 85 (65.4%), significantly lower than their share in the population of applicants who submitted relevant ROLs (76.1%, $p = 0.016$). By contrast, the gender ratio of the misrepresenters is similar to that of the general population (92 women and 38 men).

Next, we perform linear regressions with the dependent variable being an indicator that equals one if the ROL is an obvious misrepresentation. The right-hand side variables include year, gender,

[20] In Appendix E, we perform a similar analysis that eliminates the aggregate correlation between obvious misrepresentation and strength of application and find similar results. We thank an anonymous referee for suggesting the analyses in this appendix.

TABLE III

CORRELATES OF OBVIOUS MISREPRESENTATION: ADMINISTRATIVE DATA[a]

	(1) OMR	(2) Flipped	(3) Dropped
Female	−0.0290	−0.0300	−0.00656
	(0.0356)	(0.0280)	(0.0266)
NotRanked	0.202***	0.0905*	0.101**
	(0.0635)	(0.0499)	(0.0510)
DesirabilityQuintile(1)	0.0352	0.0273	0.0152
	(0.0533)	(0.0416)	(0.0413)
DesirabilityQuintile(2)	−0.0437	−0.0278	−0.0165
	(0.0506)	(0.0375)	(0.0392)
DesirabilityQuintile(3)	0.0315	0.0240	−0.00268
	(0.0531)	(0.0412)	(0.0400)
DesirabilityQuintile(4)	−0.0706	−0.0152	−0.0662**
	(0.0479)	(0.0387)	(0.0325)
Year and BA institution fixed effects	YES	YES	YES
Observations	672	672	672
R-squared	0.071	0.038	0.039

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of a dummy variable for obvious misrepresentation (OMR) on variables that are available from the administrative match data. The analysis is repeated breaking down obvious misrepresentation by type. In all specifications, the year dummy and all institution dummies have coefficients that are not statistically distinguishable from 0. Robust standard errors are in parentheses.

and Bachelor's degree institution fixed effects, a dummy for being the ROL of one of the 15% of the applicants who were not ranked by any program in the year the ROL was submitted, and dummies for the quintile of the eigenvector centrality measure of the desirability of an applicant.[21] We repeat this analysis further refining the dependent variable by the type of misrepresentation: flipping or dropping. Table III and Table VI summarize our findings.

[21]The eigenvector centrality measure of the desirability of applicants is based on the eigenvector associated with the largest eigenvalue of the matrix A that summarizes pairwise comparisons of applicants' rankings by the programs. Specifically, $A_{ij} = (n_{ij} + 1)/(n_{ji} + 1)$, where n_{ij} denotes the number of programs that ranked both i and j and ranked i above j. Both the eigenvector centrality measure of the desirability of applicants and the quintiles are calculated separately for each year. Table VIII provides evidence of a positive correlation between this ad-hoc measure of desirability and our measure of ability, applicants' (self-reported) MITAM scores.

The estimated coefficients indicate that being "unpopular" with departments correlates with submitting an obvious misrepresentation. For example, column (1) illustrates that unranked applicants were more than twice as likely to submit an obvious misrepresentation relative to applicants who were ranked by some program. While the regressions in this section are used only as a convenient means to summarize the data and should not be given a causal interpretation, this could suggest that more desirable applicants are more likely to be truthful, or at least less likely to submit an obvious misrepresentation.

In Appendix F we change the level of observation to a specific *application* – a program on an ROL. This allows us to add program-specific controls, as well as applicant-program-specific controls. The correlation with being unranked by programs persists, and in addition we find a lower tendency for obvious misrepresentation in applications where funding was feasible for the applicant (i.e., ones where the applicant would have been funded had she placed the funded position at the top of her ROL).[22]

The 2015 post-match survey allows us a more refined look into the correlates of misrepresentation. In particular, we have a better measure of academic ability in the form of the (self-reported) MITAM score. We regress a dummy for obvious misrepresentation on administrative and survey-based controls. The results are summarized in Table IV. All specifications suggest a negative relation between MITAM and obvious misrepresentation above the median MITAM score in the sample.

In Table VII we further break down the obvious misrepresentation variable to obvious dropping and obvious flipping. In both cases the relationship with the MITAM score persists. Additionally, we find that obvious dropping is correlated with high socioeconomic status, and that obvious flipping is positively correlated with not reading the FAQ. Such correlations can be explained by wealthier indi-

[22]A referee pointed out that applicants may be embarrassed to ask for aid in their own undergraduate institution if they come from a wealthy background. Following the referee's suggestion, we added to these regressions a dummy variable for applications from graduates of the same institutions. The results are presented in Table X – they do not support this theory.

TABLE IV

CORRELATES OF OBVIOUS MISREPRESENTATION: SURVEY DATA[a]

	(1) OMR	(2) OMR	(3) OMR	(4) OMR
Female	−0.0481	−0.0530	−0.0649	−0.0717
	(0.0573)	(0.0569)	(0.0576)	(0.0566)
FaqHelpful	−0.0117	−0.00645	−0.0212	−0.0166
	(0.0896)	(0.0897)	(0.0837)	(0.0859)
FaqNotRead	0.0709	0.0758	0.0655	0.0705
	(0.103)	(0.103)	(0.0965)	(0.0985)
ExplanationConfidence	$6.73e{-}05$	−0.00391	$8.70e{-}05$	−0.00377
	(0.0267)	(0.0257)	(0.0272)	(0.0261)
Age	0.0216	0.0180	0.00586	0.000448
	(0.0264)	(0.0259)	(0.0258)	(0.0254)
SocioeconomicStatus	0.0290	0.0327	0.0307	0.0349
	(0.0209)	(0.0206)	(0.0205)	(0.0202)
MITAM	−0.0702***	−0.0922***	−0.0824***	−0.106***
	(0.0242)	(0.0240)	(0.0259)	(0.0260)
MITAM^2		−0.0608***		−0.0609***
		(0.0145)		(0.0146)
NotRanked	0.138	0.118	0.0534	0.0194
	(0.0846)	(0.0853)	(0.107)	(0.108)
DesirabilityQuintile(1)			−0.00500	−0.0348
			(0.0955)	(0.0945)
DesirabilityQuintile(2)			−0.246***	−0.261***
			(0.0898)	(0.0878)
DesirabilityQuintile(3)			−0.0416	−0.0569
			(0.0938)	(0.0921)
DesirabilityQuintile(4)			−0.151*	−0.163**
			(0.0787)	(0.0763)
BA institution fixed effects	YES	YES	YES	YES
Observations	240	240	240	240
R-squared	0.104	0.141	0.149	0.187

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of a dummy variable for obvious misrepresentation (OMR) on variables that are available from the 2015 post-match survey in addition to administrative match data. Columns (2) and (4) include a quadratic term in the MITAM score. Columns (3) and (4) include controls for desirability quintiles. Explanation confidence, age, socioeconomic status, and MITAM were normalized to have a mean of zero and standard deviation of one. Robust standard errors are in parentheses.

viduals putting less weight on funding in their (mistaken) trade-off, and by less attentive individuals being less aware of the way the mechanism works.

Finally, we repeat the analysis of Table IV with reported misrepresentation as the left-hand side variable. The results are summarized in columns (1) and (2) of Table IX. Unranked applicants are significantly more likely to report misrepresentation. Additionally, a higher level of self-confidence about the ability to explain the way the system works is associated with lower rates of (reported) misrepresentation.[23]

6. DISCUSSION

This paper is dedicated to exploring evidence of preference misrepresentation under a strategy-proof mechanism. To make the strongest case possible, our approach focuses on detecting ROLs that are extremely unlikely to represent applicants' straightforward and honest ranking of alternatives. In the IPMM, almost a fifth of the students ranked programs without funding above their funded counterparts (or did not rank the funded contracts at all). These obvious misrepresentations in the IPMM demonstrate that, even in near-optimal conditions for truth-telling—a high-stakes decision by highly educated participants who have received plenty of information, advice, and time—one cannot treat reports to a strategy-proof mechanism as necessarily representing an applicant's true ranking of programs.

We stress that obvious misrepresentations are but the tip of the iceberg. Since applicants flip naturally-ranked alternatives or drop dominating alternatives, it only stands to reason that they act similarly on alternatives that are more difficult for them to compare directly. This kind of behavior is also frequently reported by applicants in the surveys we conducted. This observation reflects on many other high-stakes matching systems, where misrepresentation cannot be directly detected.

An important aspect of misrepresentation is the associated cost to applicants who miss out on feasible alternatives. In our setting, the cost of obvious misrepresentation had to be low, due to the scarcity of the scholarships offered in the IPMM. In a follow-up work, Shorrer & Sóvágó (2017) study

[23] In this context, it is important to reiterate the fact that the survey was conducted after the match results were published; thus, the reports may have been affected by the match outcomes.

an environment where funding is more abundant, and show that the costs associated with obvious misrepresentation are indeed substantial.[24] In Australia, Artemov *et al.* (2017) provide a high upper bound and a low lower bound for the cost of obvious misrepresentation.

The phenomenon of preference misrepresentation under a strategy-proof mechanism, and especially the case of obvious misrepresentation, raises the question of "why." Here we list three explanations that we believe are relevant for the specific case of the IPMM. These explanations and others are more thoroughly discussed and evaluated by Hassidim *et al.* (2017a).

First, applicants may misunderstand or mistrust the advice communicated to them (in multiple forms and on various occasions) regarding the optimal strategy.[25] If applicants do not fully understand the mechanism or do not trust the information they are given, a natural intuition is to believe that the mechanism rewards a higher ranking with an increased probability of matching. Survey responses that indicated misrepresentation with an intention to increase the likelihood of admission provide direct support for this theory, which is also consistent with the negative correlation between the strength of an applicant and obvious misrepresentation.

A second potential explanation of misrepresentation is that applicants use a *weakly* dominated strategy because they believe that, in practice, the chances that the dominant strategy will do better are zero or close to zero. In the case of obvious misrepresentation, applicants may not believe that they can actually get the scholarship (e.g., because they suspect they are ranked low compared to other applicants).[26] This explanation is consistent with the negative correlation between the strength of an applicant and obvious misrepresentation, and with survey responses indicating misrepresentation due to low chances of admission.

Finally, it is possible that applicants' preferences do not solely depend on their final assignment.

[24]Similarly, Dwenger *et al.* (2018) show that preference reversals in German college admissions, which are suboptimal under the standard assumptions of the matching literature, have substantial allocative consequences.

[25]After all, individuals are sometimes told that "honesty is the best policy," even when being honest is not optimal. And verifying the incentive properties of DA is not entirely straightforward.

[26]We find this explanation less plausible for obvious flipping.

Specifically, misrepresentation may be motivated by trying to avoid disappointment (Dreyfuss *et al.*, 2019) or by self-image concerns (Bénabou & Tirole, 2011; Kőszegi, 2006). That is, participants may distort their choices to restrain themselves from developing unrealistic expectations regarding their outcome, or to avoid receiving information about their desirability or priority, as this may hurt their self-image.[27] These two motives are also consistent with the correlation between applicants' strength and truthful behavior. However, our surveys did not directly investigate these concerns.[28]

To conclude, our novel method for detecting (certain) preference misrepresentations allowed us to affirm the prediction of previous evidence from experimental studies. We have shown that preference misrepresentation is prevalent even in real-life high-stakes strategy-proof matching systems. Our market design recommendation is, therefore, to focus attention on these populations that are most likely to misrepresent their preferences, and suffer the consequences. One way to do this is to develop tools, like the ones used in the Genetic Counseling match, to identify applicants who likely belong to this population. Other meaningful interventions may include direct explanation efforts, adding transparency to promote trust, and facilitating data access to create reliable and accurate expectations among participants.

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[27]This last motivation is less plausible in environments where applicants learn whether they would have been allocated other options had they asked for them (Artemov *et al.*, 2017; Chen & Pereyra, 2019; Shorrer & Sóvágó, 2017). For other possible motivations see Antler (2015, 2018); Hassidim *et al.* (2017a).

[28]Future research can use field experiments to assess the relative importance of these explanations. For example, different explanations lead to different predictions regarding agents' responses to decision support systems that alert students about obvious misrepresentations. Similarly, different theories lead to different predictions about the importance of using a simplified description of the mechanism.

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APPENDIX A: ADDITIONAL TABLES AND FIGURES – FOR ONLINE PUBLICATION

TABLE V

VARIABLE LIST

Variable	Mean (SD)	Number of observations	Definition
A. Administrative data			
OMR	.194 (.395)	672	1 if ROL is an obvious misrepresentation
Flipped	.104 (.306)	672	1 if ROL is obvious flipping
Dropped	.095 (.294)	672	1 if ROL is obvious dropping
Female	.728 (.446)	672	1 if applicant is female, 0 if male
NotRanked	.149 (.356)	672	1 if applicant was not ranked by any program in the year the ROL was submitted
DesirabilityRank	344.46 (187.0)	672	Eigenvector centrality desirability rank of the applicant in the year the ROL was submitted
DesirabilityQuintile(i)		672	1 if applicant was in quintile (i) of DesirabilityRank among applicants who were ranked in the year the ROL was submitted
Year	.661 (.474)	672	0 if ROL was submitted in 2014, 1 if ROL was submitted in 2015
BA*		672	Dummy variables for Bachelor's degree from each of the participating institutions
B. 2015 post-match survey data			
DecreasedPosition	.17 (.376)	289	1 if reported ranking some position lower than actual preferences
IncreasedPosition	.132 (.339)	288	1 if reported ranking some position higher than actual preferences
ReportedMisrepresentation	.204 (.404)	288	1 if IncreasedPosition=1 or DecreasedPosition=1
AwareOfScholarship	.965 (.185)	283	1 if reported being aware of the option to rank some programs with and without a scholarship
ReportedOMR	.149 (.356)	276	1 if reported submitting an obvious misrepresentation
Age	27.63 (4.126)	289	Self-reported age
SocioeconomicStatus	2.793 (1.008)	285	Answer to socioeconomic status question (see Appendix D), 0 (lowest) to 5 (highest)
MitamScore	118.82 (14.93)	248	Self-reported MITAM score
MatchingSatisfaction	8.08 (2.07)	291	Reported satisfaction from matching process, 1 (lowest) to 10 (highest)
ApplicationSatisfaction	4.68 (2.58)	290	Reported satisfaction from application process, 1 (lowest) to 10 (highest)
FaqNotRead	.762 (.426)	290	1 if reported not reading the FAQ
FaqHelpful	.682 (.467)	290	1 if reported reading the FAQ and that it was helpful
Explanation-ThiConfidence	8.34 (1.63)	291	Self-confidence in ability to explain how the matching process works, 1 (lowest) to 10 (highest)

TABLE VI

Correlates of obvious misrepresentation: Administrative data[a]

	(1) OMR	(2) OMR	(3) OMR	(4) Flipped	(5) Flipped	(6) Flipped	(7) Dropped	(8) Dropped	(9) Dropped
Female		−0.0269	−0.0290		−0.0298	−0.0300		−0.00415	−0.00656
		(0.0354)	(0.0356)		(0.0276)	(0.0280)		(0.0266)	(0.0266)
NotRanked	0.207***	0.212***	0.202***	0.0891**	0.0891**	0.0905*	0.111***	0.116***	0.101**
	(0.0508)	(0.0521)	(0.0635)	(0.0403)	(0.0418)	(0.0499)	(0.0409)	(0.0418)	(0.0510)
Desirability Quintile(1)			0.0352 (0.0533)			0.0273 (0.0416)			0.0152 (0.0413)
Desirability Quintile(2)			−0.0437 (0.0506)			−0.0278 (0.0375)			−0.0165 (0.0392)
Desirability Quintile(3)			0.0315 (0.0531)			0.0240 (0.0412)			−0.00268 (0.0400)
Desirability Quintile(4)			−0.0706 (0.0479)			−0.0152 (0.0387)			−0.0662** (0.0325)
Year and BA institution fixed effects	NO	YES	YES	NO	YES	YES	NO	YES	YES
Observations	672	672	672	672	672	672	672	672	672
R-squared	0.035	0.062	0.071	0.011	0.034	0.038	0.018	0.031	0.039

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of a dummy variable for obvious misrepresentation (OMR) on variables that are available from the administrative match data. The analysis is repeated breaking down obvious misrepresentation by type. Robust standard errors are in parentheses.

TABLE VII

Correlates of dropping and flipping[a]

	Flipped	Flipped	Flipped	Flipped	Dropped	Dropped	Dropped	Dropped
Female	−0.0199	−0.0230	−0.0272	−0.0316	−0.0242	−0.0262	−0.0327	−0.0355
	(0.0427)	(0.0428)	(0.0445)	(0.0443)	(0.0434)	(0.0435)	(0.0426)	(0.0427)
FaqHelpful	0.0554	0.0588	0.0523	0.0553	−0.0682	−0.0660	−0.0741	−0.0723
	(0.0459)	(0.0462)	(0.0453)	(0.0462)	(0.0794)	(0.0796)	(0.0758)	(0.0768)
FaqNotRead	0.150**	0.154**	0.147**	0.150**	−0.0619	−0.0599	−0.0636	−0.0616
	(0.0667)	(0.0669)	(0.0638)	(0.0647)	(0.0863)	(0.0864)	(0.0836)	(0.0844)
ExplanationConfidence	−0.0169	−0.0195	−0.0163	−0.0188	0.0212	0.0196	0.0204	0.0188
	(0.0197)	(0.0192)	(0.0196)	(0.0191)	(0.0239)	(0.0238)	(0.0243)	(0.0241)
Age	0.00311	0.000763	−0.00450	−0.00799	0.0171	0.0156	0.00951	0.00733
	(0.0171)	(0.0169)	(0.0171)	(0.0170)	(0.0199)	(0.0198)	(0.0195)	(0.0194)
Socioeconomic Status	0.00497	0.00737	0.00847	0.0112	0.0332**	0.0348**	0.0310**	0.0326**
	(0.0176)	(0.0174)	(0.0171)	(0.0169)	(0.0156)	(0.0157)	(0.0154)	(0.0154)
MITAM	−0.0279	−0.0422**	−0.0352*	−0.0506**	−0.0447**	−0.0537***	−0.0490**	−0.0586***
	(0.0174)	(0.0177)	(0.0191)	(0.0198)	(0.0180)	(0.0195)	(0.0199)	(0.0220)
MITAM^2		−0.0394***		−0.0392***		−0.0248**		−0.0245**
		(0.0124)		(0.0124)		(0.00987)		(0.0101)
NotRanked	−0.00271	−0.0153	−0.0272	−0.0491	0.138*	0.130*	0.0841	0.0705
	(0.0647)	(0.0637)	(0.0791)	(0.0785)	(0.0711)	(0.0724)	(0.0892)	(0.0918)
Desirability Quintile(1)			0.0152	−0.00394			0.00735	−0.00461
			(0.0742)	(0.0740)			(0.0752)	(0.0759)
Desirability Quintile(2)			−0.143**	−0.153**			−0.102	−0.109
			(0.0627)	(0.0629)			(0.0726)	(0.0727)
Desirability Quintile(3)			0.0200	0.0101			−0.0596	−0.0657
			(0.0745)	(0.0734)			(0.0676)	(0.0686)
Desirability Quintile(4)			−0.0399	−0.0474			−0.109**	−0.114**
			(0.0641)	(0.0627)			(0.0513)	(0.0520)
BA institution fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	240	240	240	240	240	240	240	240
R-squared	0.076	0.102	0.104	0.130	0.122	0.134	0.150	0.162

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

[a] The table presents the results of a linear regression of a dummy variable for obvious flipping (dropping) on variables that are available from the 2015 post-match survey in addition to administrative match data. The specifications follow those in Table IV. Explanation confidence, age, socioeconomic status, and MITAM were normalized to have a mean of zero and a standard deviation of one. Robust standard errors are in parentheses.

TABLE VIII

MITAM VS. DESIRABILITY[a]

	(1) MITAM	(2) MITAM
DesirabilityQuintile(1)		0.0498
		(0.209)
DesirabilityQuintile(2)		−0.207
		(0.236)
DesirabilityQuintile(3)		−0.0725
		(0.221)
DesirabilityQuintile(4)		0.510***
		(0.195)
DesirabilityQuintile(5)		0.788***
		(0.197)
DesirabilityRank	−0.00164***	
	(0.000311)	
Constant	0.507***	−0.223
	(0.106)	(0.150)
Observations	216	248
R-squared	0.108	0.125

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of the self-reported MITAM score from the 2015 post-match survey on our measures of desirability. Column 1 uses the desirability rank, which can only be calculated for individuals who were ranked by some program. Column 2 uses desirability-quintile dummies and includes unranked applicants (omitted dummy). Robust standard errors are in parentheses.

TABLE IX

CORRELATES OF REPORTED MISREPRESENTATION[a]

	(1) Reported Misrepresentation	(2) Reported Misrepresentation	(3) Reported OMR	(4) Reported OMR
OMR		0.00266		0.502***
		(0.0735)		(0.0812)
Female	−0.0394	−0.0392	−0.0271	0.00179
	(0.0573)	(0.0580)	(0.0506)	(0.0428)
FaqHelpful	−0.169	−0.169	−0.0626	−0.0554
	(0.107)	(0.108)	(0.0793)	(0.0728)
FaqNotRead	0.0185	0.0183	0.0271	0.00394
	(0.122)	(0.122)	(0.0932)	(0.0788)
Explanation Confidence	−0.0538*	−0.0538*	−0.0346	−0.0332
	(0.0273)	(0.0274)	(0.0273)	(0.0204)
Age	−0.0257	−0.0257	0.0610*	0.0607*
	(0.0271)	(0.0272)	(0.0318)	(0.0328)
SocioeconomicStatus	0.0463	0.0462	0.0271	0.00995
	(0.0288)	(0.0288)	(0.0206)	(0.0170)
MITAM	−0.00650	−0.00621	−0.0336	0.0165
	(0.0290)	(0.0306)	(0.0234)	(0.0207)
MITAM^2	−0.00359	−0.00343	−0.0187	0.0123
	(0.0211)	(0.0222)	(0.0138)	(0.0116)
NotRanked	0.230**	0.230**	0.269**	0.235***
	(0.113)	(0.113)	(0.105)	(0.0825)
DesirabilityQuintile(1)	0.0126	0.0127	0.182**	0.188***
	(0.0877)	(0.0884)	(0.0705)	(0.0685)
DesirabilityQuintile(2)	−0.160*	−0.159*	−0.0292	0.0882*
	(0.0867)	(0.0904)	(0.0579)	(0.0531)
DesirabilityQuintile(3)	0.0458	0.0459	0.0616	0.0893
	(0.0973)	(0.0976)	(0.0620)	(0.0589)
DesirabilityQuintile(4)	−0.0711	−0.0707	0.00437	0.0767
	(0.0767)	(0.0783)	(0.0474)	(0.0501)
BA institution fixed effects	YES	YES	YES	YES
Observations	239	239	230	230
R-squared	0.211	0.212	0.195	0.449

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of a dummy variable for reported (obvious) misrepresentation on variables that are available from the 2015 post-match survey in addition to administrative match data. OMR is a dummy variable for submitting an ROL that obviously misrepresented the applicant's preferences. Explanation confidence, age, socioeconomic status, and MITAM were normalized to have 0 mean and standard deviation of 1. The difference in the number of observations stems from survey responders who chose not to respond to the OMR question. Robust standard errors in parentheses.

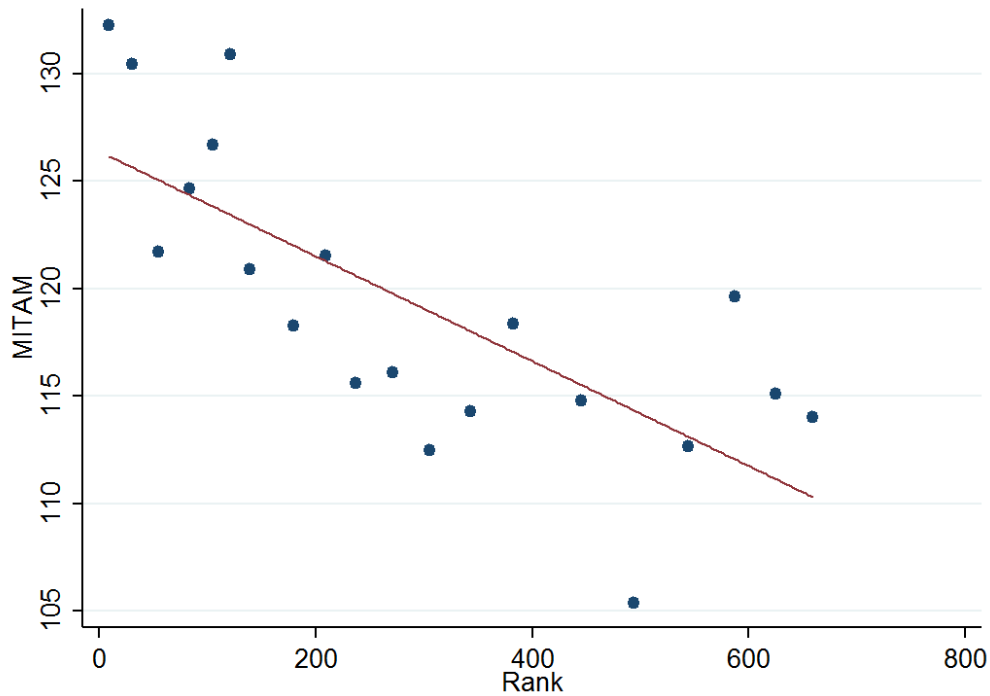

FIGURE 1.— MITAM vs. desirability. Observations (216, corresponding to respondents to the 2015 post-match survey who were ranked by some program and reported their MITAM score) are partitioned into 20 equal bins by their eigenvector centrality rank. Lower rank corresponds to higher desirability.

APPENDIX B: SELECTED FREQUENTLY ASKED QUESTIONS – FOR ONLINE PUBLICATION

This appendix includes a translation of the relevant part of the FAQ page of the matching system website. Any non-person-specific question that was received was paraphrased and posted publicly on this page.[29]

[29] The complete list of questions and answers (in Hebrew) is available at http://psychologymatch.org/info/FAQ.aspx (accessed 7/29/2015).

Q: Will anyone else see my ROL?

A: Your ROL is secret and no track will ever have access to it. You are the only person permitted to access your ROL, unless you give out your user name and password.

Q: How does the computerized placement process work?

A: The algorithm tries to place each candidate in her most preferred track. If the track prefers other candidates, the algorithm tries to place the candidate in her second favorite track, and so on, until the candidate is temporarily placed, or until she has been rejected by all tracks. After all candidates go through this process the temporary assignment becomes permanent.

Q: Is there room for strategizing? Should I rank a track that I am interested in but feel like I have no chance of being admitted to?

A: The system was designed so that there is no need for being strategic while ranking the tracks. The only thing that should influence the ranking is the degree of desirability of the track for you. Strategic thinking can only hurt your probability of admission, and cannot improve it. To be specific, it is advisable to rank all of the tracks you interviewed with, even if you think your chances of admission are slim. This will not hurt your chances of being admitted to another track.

Q: I want to study clinical psychology, and I am willing to study [anywhere], even on the moon. I had a good interview with program A and a bad one with program B. On the other hand, I prefer B [to A]. How should I rank them?

A: When you determine your ranking, think only of where you want to study, assuming that you will be admitted. Do not worry about odds! In this case, rank B first and A second. If you rank A first you will not increase your chances of being accepted to a psychology program, and you are only hurting yourself.

Q: I had an interview with program A and they told me that if I ranked them first I would be admitted. I prefer B, but they made no promises. What should I do?

A: Great! You are surely going to be admitted to a psychology program. Rank B first and A second. If B wants you (even though you were not promised admission) you will go there; otherwise you will go to A. It is important to underscore that no one will ever see your ranking!

Q: Does the algorithm take into account the fit between my ranking of the track and the track's ranking of me? That is, if another candidate and I are ranked by one of the tracks so that I am ranked 12th and she is 13th, but she gave the track a higher priority than I did, is it possible that she will be admitted and I will not (assuming that I am not admitted to another track)?
A: This is impossible. The matching algorithm (intentionally) does not take into account your ranking of the track, but only the track's ranking of you. The reason why the algorithm works this way is to circumvent contrivances.

Q: Will I know after the fact which tracks admitted me (even if I was not placed there)?
A: Not exactly. Tracks do not submit acceptance/rejection lists to the system, but submit a ranking over candidates and the planned size of the track. Applicants are placed in the best track they can get. That is, if you do not get into a track that you ranked higher, you can deduce that this program has filled its capacity. As for programs you ranked lower than the one you were placed in, you can only tell by contacting the track after the fact. Even if you had been admitted to this track, it would have been impossible to move there after the placement was set.

APPENDIX C: USER INTERFACE SCREENSHOTS – FOR ONLINE PUBLICATION

FIGURE 2.— Ranking screen. Programs (with terms, when applicable) appear on the right-hand side of the screen, and are classified by institution. Applicants can drag and drop any number of alternatives (programs with terms) from the right-hand side of the screen to their ROL on the left-hand side of the screen. They can also drag ranked programs to change their order, or remove them from the ROL.

APPENDIX D: 2015 POST-MATCH SURVEY QUESTIONS – FOR ONLINE PUBLICATION

- Was 2015 the first year you applied for a graduate degree?
- If not, in what year did you first apply? Did you use the automated matching system last year?
- Did you encounter any technical difficulties in registering or ranking?
- If so, did you reach out to technical support? Was the response helpful?
- On the matching system website there is a FAQ page. Did you see this page and read the answers that appear there?
- Were the answers helpful?
- On a scale of 1 to 10, if you had to explain to next year's applicants how the matching process works, how well could you explain it?

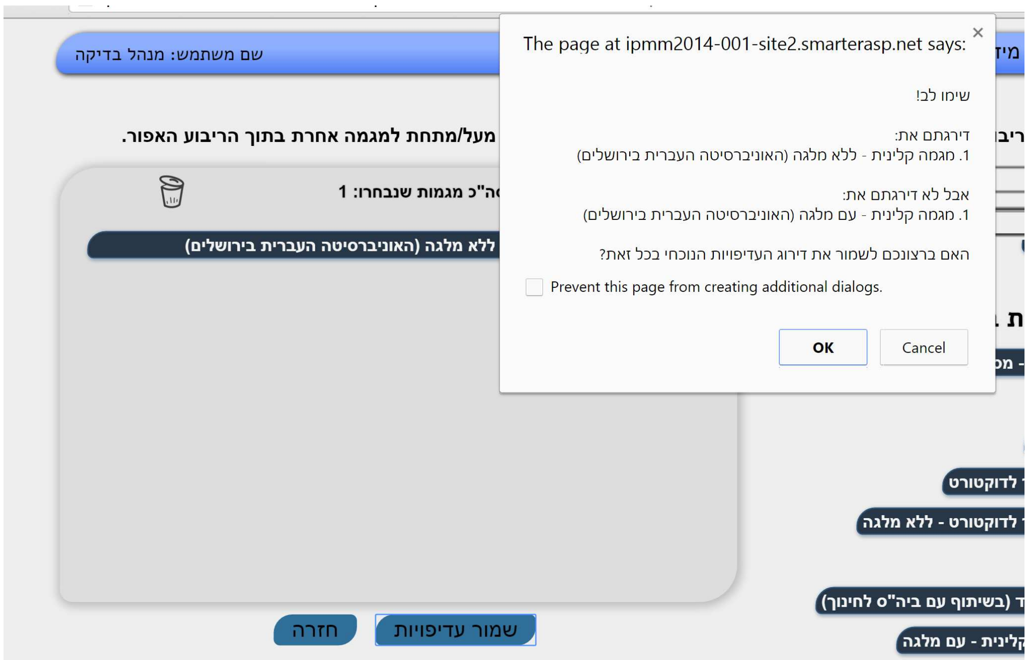

FIGURE 3.— Missing terms warning. This pop-up alert appears since the applicant chose to rank a program without funding, but did not rank it with a scholarship even though such an option existed. The message reads: "Attention! You ranked: (1) Clinical psychology – without scholarship (The Hebrew University of Jerusalem), but you did not rank (1) Clinical psychology – with scholarship (The Hebrew University of Jerusalem). Do you want to save the current ranking anyway?"

- What were the factors that were important in ranking programs?
- Is there a program you ranked lower than what you really wanted because you thought your chance of being admitted was relatively low?
- If so, please elaborate.
- Is there a program you ranked higher than what you really wanted because you thought your chance of being admitted was relatively high?
- If so, please elaborate

- Did you apply to A, B, C, or D (names of institutions offering dually listed programs)?
- If so, you could have ranked some of the programs in those institutions with and without a scholarship. Were you aware of that? Which did you rank higher? Why?
- There was an option to register as a couple. Were you aware of this option? Was it relevant to you?
- If so, did you register as a couple? If you didn't, why not?
- On a scale of 1 to 10, how satisfied are you with the automated matching system?
- On a scale of 1 to 10, how satisfied are you with the admission process generally?
- Would you agree to share some demographic information?
- How old are you?
- Where did you go to high school?
- Where are you from (prior to undergraduate studies)?
- How would you describe the economic status of your family (very high, high, medium-high, medium, medium-low, low)?
- What was your MITAM score?
- Would you like to add any more comments?
- Would you like to receive the results of this survey?

APPENDIX E: ADDITIONAL METHODS FOR ASSESSING THE COSTS OF MISREPRESENTATION – FOR ONLINE PUBLICATION

The cost of OMRs is determined by a combination of factors, including the availability of funding and the correlation between OMRs and their potential consequences. In the body of the paper, we provided evidence indicating that OMRs are more likely to appear when they are less likely to be consequential. In this appendix, we provide additional benchmarks. First, we show that the costs of misrepresentation were to increase had misrepresentation not been correlated with potential consequences altogether (in the main text, we eliminated the correlation only at the assessed ROL level, keeping the aggregate behavior fixed). Second, we show that if the availability of scholarships were to suddenly increase, the fraction of consequential OMRs would increase proportionately.

E.1. *Randomizing the identity of misrepresenters*

To assess the effect of the correlation between OMRs and their potential consequences, we provide benchmarks in which this correlation is absent. We hold all ROLs fixed, except that OMRs are reassigned uniformly at random across applications, holding the total number of applications with OMRs fixed. When an application that is not an OMR in practice is drawn to be an OMR, we rank the non-funded position immediately after the funded position. When an OMR application is drawn to be non-OMR, we place the funded position immediately above the non-funded position. In all other cases, we do not change the ROL. We repeat this process 100 times, each time calculating the lower and the upper bounds of the cost of OMR as in the main text. On average, we get a lower bound of 9.6 (compared to 3 in the real data), and an upper bound of 22.7 (compared to 10 in the real data).

Next, we repeat this exercise, this time randomizing the OMR status at the ROL level (i.e., if an ROL is an OMR, it misrepresents preferences over all pairs of positions of dually listed programs that appear on the original ROL). In this case we get a lower bound of 6.2 (compared to 3 in the real data), and an upper bound of 15.3 (compared to 10 in the real data). These analyses provide further evidence that OMRs are more common among weaker applicants.

E.2. *Expanding the availability of scholarships*

The scarcity of financial aid in the context of the IPMM practically limits the number of consequential OMRs. In this section, we assess the importance of this channel by increasing the availability of funding. To do so we increasingly replace non-funded positions with funded positions. Over the two years, the number of scholarships that could potentially be added to the market without exceeding the total quotas is almost 160. We iterate over the number of scholarships that can be added (from 0 to 150, in steps of 10), and repeatedly randomize to which positions these scholarships relate 100 times. In each iteration we calculate the average lower and the upper bounds as above. When we expand the number of available scholarships, we also proportionately expand the fraction of students who are ranked by the relevant funded seats. The results of this analysis are summarized in Figure 4.

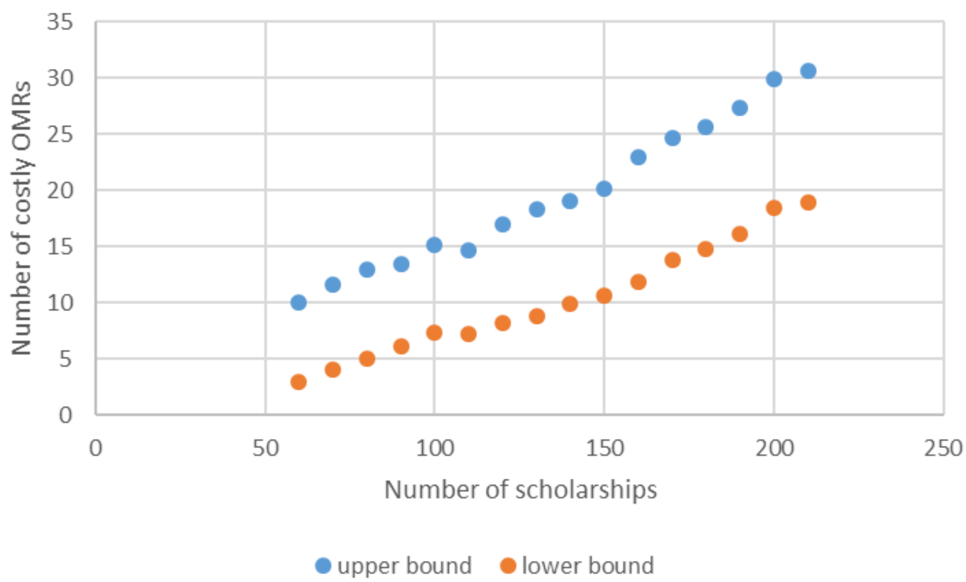

FIGURE 4.— The hypothetical costs of misrepresentation when the number of scholarships increases

The effect of adding funding opportunities to the market is that more misrepresentations become costly. Not all misrepresentations become costly since, as we show in the paper, many of the misrepresenters are not eligible for admission in the relevant programs (or any other program), and because in some cases, the programs are assigned higher ranked students who did not submit OMRs.

Linearly fitting the above two curves, we get slopes of 0.1 for the lower bound, and 0.14 for the upper bound. In other words, adding a scholarship to the market increases, on average, the probability that OMR is costly for a random applicant with an OMR by .07–.1%. The corresponding hypothetical cost for relevant non-OMR ROLs is .12%.

APPENDIX F: APPLICATION LEVEL ANALYSES – FOR ONLINE PUBLICATION

In this appendix, we change the level of observation from a particular ROL to a particular *application* (a program on an ROL). We limit our sample to relevant applications (i.e, ones that include a non-funded contract with a dually listed program). This allows us to consider program-specific characteristics as well as agent-program-specific characteristics.

Table X presents the results from regressions that add program-specific and agent-program-specific controls to the specifications studied in Table III and Table IV. The variable *competitive ratio* is our measure of the selectivity of funding in the particular program. Its value is the ratio between the number of available scholarships and the number of applicants to the program (the lower the number the more selective funding is). The variables *feasible funded* and *feasible non-funded* are dummy variables that take the value one if the applicant could have been accepted (with funding) to the program, had she ranked the corresponding contract first. The variable *BA=MA* is a dummy variable that takes the value one if the applicant graduated from the institution where the program is located.

As in the main analysis, we find a statistically and economically significant correlation between not being ranked by any program and obvious misrepresentation. In addition, we find statistically and economically significant negative correlation between OMRs and the variable feasible funded. This correlation is in line with applicants making fewer OMRs when they are likely to be costly.

A key limitation of the above analysis (beyond the limitations discussed in the paper) is that the estimated correlation may result from sorting. For example, stronger applicants rank more selective programs, and if they behave differently from weak applicants, this can be absorbed by the selectivity coefficient. To address this concern, in Table XI we augment our analyses by adding an ROL fixed effect. This allows us to use variation in program characteristics and agent-program characteristics in the same ROL (which corresponds to the same individual).

Our finding are in line with the findings above, but they are no longer statistically significant. This is not surprising as variation comes from a very limited number of individuals who applied to multiple dually listed programs and obviously misrepresented their preferences only with respect to some of

TABLE X

Application-level regressions

VARIABLES	(1) OMR	(2) flipped	(3) dropped	(4) OMR	(5) flipped	(6) dropped
BA=MA	-0.0302	-0.00983	-0.0204	-0.0336	-0.00594	-0.0276
	(0.0346)	(0.0245)	(0.0256)	(0.0356)	(0.0250)	(0.0263)
feasible unfunded	0.0412	0.0478*	-0.00664	0.0414	0.0515*	-0.0101
	(0.0338)	(0.0263)	(0.0243)	(0.0350)	(0.0264)	(0.0257)
feasible funded	-0.115***	-0.0833***	-0.0320	-0.103***	-0.0855***	-0.0176
	(0.0387)	(0.0313)	(0.0260)	(0.0398)	(0.0322)	(0.0273)
competitive ratio	0.128	0.123*	0.00517	0.121	0.132*	-0.0112
	(0.0938)	(0.0725)	(0.0697)	(0.0951)	(0.0734)	(0.0723)
female	-0.0163	-0.0166	0.000340	-0.0155	-0.0159	0.000380
	(0.0322)	(0.0218)	(0.0239)	(0.0320)	(0.0214)	(0.0240)
not_ranked_by_programs	0.184***	0.0819**	0.102**	0.187***	0.0781*	0.108**
	(0.0554)	(0.0377)	(0.0452)	(0.0618)	(0.0426)	(0.0493)
q1				0.0506	0.0300	0.0206
				(0.0470)	(0.0335)	(0.0338)
q2				-0.00501	-0.0391	0.0341
				(0.0477)	(0.0276)	(0.0390)
q3				0.00978	0.0167	-0.00696
				(0.0414)	(0.0317)	(0.0273)
q4				-0.0554	-0.0265	-0.0289
				(0.0379)	(0.0273)	(0.0268)
BA institution fixed effect	YES	YES	YES	YES	YES	YES
Observations	1,187	1,187	1,187	1,187	1,187	1,187
R-squared	0.064	0.037	0.038	0.071	0.045	0.043

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

[a] The table presents the results of a linear regression of a dummy variable for obvious misrepresentation (OMR) in a particular application, on variables that are available from the administrative match data. The analysis is repeated breaking down obvious misrepresentation by type. In all specifications, the year dummy and all institution dummies have coefficients that are not statistically distinguishable from 0. Robust standard errors are in parentheses.

these programs.

TABLE XI

WITHIN-ROL ANALYSIS

VARIABLES	(1) OMR	(2) flipped	(3) dropped
competitive ratio	0.0499	0.0540	-0.00412
	(0.0776)	(0.0621)	(0.0585)
BA=MA	0.0196	-0.0102	0.0298
	(0.0418)	(0.0335)	(0.0315)
feasible non-funded	0.0237	0.0422	-0.0185
	(0.0373)	(0.0299)	(0.0281)
feasible funded	-0.0855*	-0.0336	-0.0519
	(0.0491)	(0.0393)	(0.0370)
Observations	1,187	1,187	1,187
R-squared	0.009	0.006	0.023
Number of sid	672	672	672

*** p<0.01, ** p<0.05, * p<0.1

[a] The table presents the results of a linear regression of a dummy variable for obvious misrepresentation (OMR) in a particular application, on program-specific and applicant-program-specific variables that are available from the administrative match data and ROL fixed effects. The analysis is repeated breaking down obvious misrepresentation by type. Robust standard errors are in parentheses.