"STRATEGIC" BEHAVIOR IN A STRATEGY-PROOF ENVIRONMENT[1]

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We present direct field evidence of preference misrepresentation under deferred acceptance. A large fraction of highly educated participants, who had been informed about the strategy-proof nature of the mechanism in numerous ways, failed to play truthfully: they ranked a non-funded position above a funded position in the same program. This is despite being informed that rank-ordered lists are never made public, that funding is a positive signal of ability, and that funding comes with no strings attached. Preference misrepresentation is associated with weaker applicants. A laboratory experiment documents a strong negative causal relationship between applicants' expected desirability and preference misrepresentation.

A mechanism is said to be strategy-proof if no agent has an incentive to misrepresent her true preferences. This property is considered highly desirable for mechanisms that are used in real-life markets. And indeed, many of the great success stories of market design employ strategy-proof mechanisms, such as the second-price sealed-bid auction (Vickrey, 1961), or Deferred Acceptance (DA, Gale & Shapley, 1962; Dubins & Freedman, 1981; Roth, 1982). Specifically, in school-choice settings (Abdulkadiroğlu & Sönmez, 2003), the appeal of strategy-proof mechanisms is one of the main reasons many school districts choose the applicant-proposing version of DA over pre-existing mechanisms (Pathak, 2011). At the core of the attractiveness of these mechanisms is the assumption that agents report their preferences truthfully in strategy-proof environments.

This paper presents direct field evidence of *preference misrepresentation* by applicants under the applicant-proposing DA. We show that preference misrepresentation is costly and is associated with "weaker" applicants, as measured by academic achievement or by desirability for programs. We augment our finding from the field with experimental evidence demonstrating that the *same* applicant is much more likely to misrepresent her preferences when she expects to have low priority (i.e., be less desirable) relative to others, and that this behavior translates to significant welfare losses.

Our study is based on the recently redesigned admission process in Israel for graduate studies in psychology (MA or direct PhD track), where several partic-

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ipating institutions offer positions in the same study track, but under different *terms*. In particular, in some cases only some of the positions are funded. Until recently, this market employed a matching process that left much room for strategic behavior and was quite complex and demanding. In response to concerns with the performance of this process, in 2014 a centralized matching mechanism, based on a variant of the applicant-proposing version of DA, was introduced. With the goal of achieving strategy-proofness and stability in a matching-with-contracts environment (Hatfield & Milgrom, 2005), the mechanism was designed to be expressive enough to accommodate the potentially crucial role of funding in some applicants' preferences. Similar to Sönmez (2013) and Sönmez & Switzer (2013), applicants were asked to rank program-terms pairs. Thus, applicants could rank a funded position in program A over a position in program B over a non-funded position in program A. The fact that the mechanism was strategy-proof for applicants was emphasized and communicated in numerous ways, and applicants had a long period of time to familiarize themselves with the rules and with the simple user interface.

The unique features of the Israeli Psychology Master's Match (IPMM) imply that for the common case where terms correspond to funding, applicants are asked to report their preferences between naturally ranked alternatives.[1] For programs with both funded and non-funded positions, it is unlikely that applicants truly prefer the latter, as, beyond the monetary benefit, funding is also prestigious and comes with no strings attached.[2] This feature allows us a rare opportunity to directly assess the degree of truth-telling in the field. We find that a significant fraction of the applicants submitted Rank-Ordered Lists (ROLs) in which a non-funded position was ranked higher than its funded counterpart. We term this behavior *obvious misrepresentation*. Obvious misrepresentation can be detected only in applicant ROLs that include a non-funded position in a program that offers both funded and non-funded positions. We call these *relevant ROLs*. Out of 704 relevant ROLs that were submitted during the 2014 and 2015 matches, 137 (19.5%) contained an obvious misrepresentation.[3]

Our results clearly show that a large fraction of the applicants failed to employ their dominant strategy and did not report their true preferences.[4] In the subsample of relevant ROLs, 19.5% of the applicants obviously misrepresented their true preferences.[5] The large proportion of untruthful agents is particularly

[1] The Turkish college admission process has the same feature, but it uses a mechanism that is not strategy-proof (Balinski & Sönmez, 1999).

[2] Subsection 4.1 provides more detail on why these positions are naturally ranked.

[3] Of the 137 ROLs that obviously misrepresented applicants' preferences, 68 (49.6%) did not include a funded position even though they included the non-funded position in the same program, and 73 (53.3%) included it below the non-funded position (4 ROLs included both types of misrepresentation).

[4] We believe that our finding are driven by game-form misconceptions (Cason & Plott, 2014). We discuss alternative explanations in the body of the paper. While we find these alternative explanations less plausible, it is important to note that they too complicate the interpretation of choice data as true preferences.

[5] Only 25 ROLs include a funded position but not the non-funded position in the same

striking given that our pool of participants was composed of well-educated individuals who faced a high-stakes decision and who were provided with all the necessary information about the dominant strategy.

The number of untruthful agents may, in fact, be much higher as applicants may have misrepresented their preferences in ways that we cannot detect, such as ranking a program other than their favorite first. We provide survey evidence supporting this hypothesis. In addition, comparing reports of obvious misrepresentation with observed behavior, we find that survey-based estimates of misrepresentation are biased downwards.

Apart from potentially affecting the untruthful agent, misrepresentation under DA also imposes externalities (both positive and negative). Here, the intuition suggests that the effect will be amplified if misrepresentation is positively correlated with agent desirability, and mitigated if the two are negatively correlated (Rees-Jones, 2016). The correlation between misrepresentation and ability may also be of interest to social planners with egalitarian motives aiming to "level the playing field," as well as to market designers aiming to mitigate misrepresentation through informed, targeted interventions. The correlation between misrepresentation and ability may also affect the informativeness of choice data on the true preferences.

We evaluate the correlation between misrepresentation and ability (desirability) in several ways. First, we inspect the preferences departments reported to the mechanism and note that applicants who were not acceptable to any program under any circumstances are twice as likely to submit an obvious misrepresentation. This correlation persists once we add various controls that are available from the administrative match data. Second, we provide a lower and an upper bound for the number of applicants who lost a funded position as a result of their obvious misrepresentation. The magnitudes are non-negligible, but the fact that the proportion of students who could potentially get funding is smaller among misrepresenters indicates that obvious misrepresentation is not more frequent among the most desirable applicants (who are the ones most likely to be eligible for funding). Third, we augment the administrative data with data from a survey we conducted following the 2015 match and find a negative correlation between obvious misrepresentation and ability as measured by the self-reported standardized admission test score.[6] These approaches all suggest that obvious misrepresentation is more prevalent in the weaker segment of the market.

As the abovementioned approaches cannot disentangle the effects of ability and desirability, we complement our findings with laboratory evidence. We re-

program. If we include these ROLs, the proportion of obvious misrepresentations becomes 18.8%. Moreover, even without restricting attention to ROLs that include a program that offers funding, slightly more than 7% of all ROLs are obvious misrepresentations.

[6] In fact, we find a hump-shaped relationship, but most of the responders are in the decreasing part. This finding persists when we break down obvious misrepresentation by type. In addition, we find a positive correlation between reporting higher socioeconomic status and dropping a funded position from the ROL and a negative correlation between reading the frequently asked questions and ranking the funded position below the non-funded position.

visit data from one of the treatments in Li (2016), where Serial Dictatorship is used to allocate common-value prizes. Prior to submitting their ROL, each subject receives a strong signal of their priority rank. Li reports that nearly 30% of ROLs misrepresent true preferences. We establish a strong negative *causal* relationship between desirability (proxied by the priority signal) and preference misrepresentation. According to our estimates, the same applicant is about three times more likely to misrepresent her preferences with the lowest signal, relative to when she receives the highest one. Misrepresentation has significant implications: it is associated with an average loss that equals about a third of the average earnings.

Our findings complicate the interpretation of a variety of empirical studies. In Subsection 6.2 we discuss a field experiment from Mexico City, where high-school seats are allocated using a variant of Serial Dictatorship. Bobba & Frisancho (2016) show that providing students with information about their expected performance in the standardized test that determines their priority in the mechanism (which is not known at the time ROLs are to be submitted) changes the realized allocation. In particular, under-confident students are likely to increase the number of academic-track schools on their lists and are more likely to be assigned to such schools, and applications become more responsive to measures of ability (more assortative). A plausible explanation to their empirical findings, that is consistent with our findings, is that applicants learn about their desirability (priority), and that pessimistic applicants misrepresent their preferences more often. This, in turn, suggests that in some circumstances the effect we identified has sizable welfare implications.

1. RELATED LITERATURE

The early literature on strategy-proof mechanisms focused mainly on their existence and properties (Vickrey, 1961; Clarke, 1971; Groves, 1973; Dubins & Freedman, 1981; Roth, 1982). In the context of matching markets, strategy-proofness is thought to be particularly desirable, and this has led many real-life markets to adopt mechanisms with this property. Prominent examples include the National Resident Matching Program (NRMP, Roth & Peranson, 1999) and Boston Public Schools (Abdulkadiroğlu *et al.*, 2005).

The validity of the theoretical prediction of truthful behavior has been evaluated using three different approaches. First, a sequence of laboratory experiments tested the behavior of individuals under strategy-proof matching mechanisms. Chen & Sönmez (2006) found that about 30% of the "proposers" failed to report their true preferences under DA, and the number was even larger for Top Trading Cycles (see Shapley & Scarf, 1974). This finding is robust: similar results were found under a variety of treatments and variations of these environments.[7]

[7]See, for example, Braun *et al.* (2014); Calsamiglia *et al.* (2010); Chen & Kesten (2013); Ding & Schotter (2015); Echenique *et al.* (2015); Featherstone & Niederle (2016); Guillen & Hing (2014); Pais & Pintér (2008); Zhu (2015).

Up until recently, the ecological validity of these findings was unclear as laboratory settings differ in several ways from the real-life environments in which these mechanisms are employed. First, in laboratory experiments the stakes are typically low, while in real life participation in centralized matching markets usually involves a once-in-a-lifetime, life-changing decision. Second, agents in the lab may not be able to process information about the mechanism the way they can in real life. Indeed, time constraints in the lab force agents to make decisions shortly after they are instructed about the mechanism, whereas in real life they can investigate the properties of the mechanism more thoroughly. Third, while agents in the lab generally have to make decisions on their own, in real life agents can seek advice from trusted sources. Fourth, due to concerns related to experimenter demand, lab experiments are not typically explicit about the strategy-proof nature of the mechanism, whereas in real-life environments designers make sure to communicate to all participants that being truthful is a dominant strategy.

As pointed out by Pathak (2011), "*measuring true preferences in the field is considerably more difficult than in the lab.*" The second approach for evaluating the validity of theoretical predictions uses structural models to estimate these preferences. Fack *et al.* (2015) use this approach to reject truth-telling in Parisian school-choice data.[8]

The third approach was taken by Rees-Jones (2016), who conducted a survey among a sample of U.S. medical seniors participating in the NRMP. He finds that 17% of the 579 responders reported that the list they submitted to the NRMP did not represent their *"true preference order,"* with 5% attributing the gap between their true and reported preferences to strategic considerations.[9] Rees-Jones validates these self-reports by showing that proxies for welfare are less predictive of the reported submitted ROLs of applicants reporting non-truthful behavior.

This innovative approach has several limitations. First, participation in the survey is voluntary, and hence the fraction of misrepresenters found in the survey population might be biased (either upward or downward) due to self-selection (i.e., the group of people who chose to answer the survey may not be representative). Moreover, participants might not be willing to reveal their ROL and the reasons that motivated them to choose it or whether they were being truthful, especially as they are not incentivized to do so. Thus, the results of Rees-Jones refute the hypothesis of truthful reporting, yet their ability to quantify the true scale of misrepresentation is limited.

Not much is known about the correlates of misrepresentation under DA and its causes. In the lab, Pais & Pintér (2008) and Pais *et al.* (2011) show that

[8]Importantly, though, the data they use comes from a mechanism that deviates from the applicant-proposing version of DA in several important ways, and is not strategy-proof (Hiller & Tercieux, 2014).

[9]The other 12% either cited another reason, such as family or location preferences, or reported that they had made a mistake.

information may play a role. Basteck & Mantovani (2016) show that low cognitive ability is positively correlated with preference misrepresentation. In the field, Rees-Jones (2016) finds that preference misrepresentation is correlated with lower academic achievement and with applying to more competitive specialties, but these results are not statistically significant.[10]

More broadly, preference misrepresentation in strategy-proof environments is not a phenomenon limited to matching markets. Lab experiments have found similar phenomena in a variety of strategy-proof environments such as private-value sealed-bid second-price auctions (e.g. Kagel *et al.*, 1987) and pivot mechanisms (Attiyeh *et al.*, 2000). 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, more recently, obvious strategy-proofness (Li, 2016). Our findings underscore the practical importance of such notions.[11]

Finally, our paper has important implications for the growing literature on estimating preferences based on behavior in centralized matching markets. Casalmiglia *et al.* (2014), He (2014), and Agarwal & Somaini (2014) estimate preferences based on behavior under the Boston mechanism, and calculate the counterfactual welfare implications of the introduction of DA. Following the reasoning of Pathak & Sönmez (2008), these papers assume that some agents are "sophisticated" (behave optimally under each mechanism) and others are "naïve" or "truthful." To borrow terminology from program evaluation literature (Angrist *et al.*, 1996), this assumption requires that the data has "always-takers" (who are always truthful) and "responders" (who behave truthfully only under DA), but no "never-takers" (i.e. agents who misrepresent their preferences under both mechanisms), as both naïve and sophisticated agents should behave truthfully under DA. Our results suggest that other types of agents, potentially "never-takers," are an empirical reality, at least in some real-life markets.

Other papers, such as Agarwal (2015) and Bobba & Frisancho (2016), rely on the assumption of truthful reporting under DA for the identification of true preferences, and use the estimated preferences to evaluate alternative policies. Our results provide caution to the validity of the truthful reporting assumption, thus raising the question of the robustness of estimates to deviations from truth-

[10] According to the 2015 NRMP Applicant Survey (NRMP, 2015), that had close to 50% response rate, 5% of U.S. medical seniors and 17% of independent applicants "*ranked the programs based on the likelihood of matching (most likely first, etc.).*" Similarly, while 92% of U.S. medical graduates provide an affirmative response to "*I ranked the programs in order of my preferences,*" only 75% of independent applicants answer affirmatively. The independent applicants category comprise of the weaker segments of the market. It includes prior allopathic medical school graduates, U.S. citizen and non-U.S. citizen students and graduates of international medical schools, students and graduates of schools of osteopathy, students and graduates of Canadian medical schools, and graduates of the Fifth Pathway program.

[11] The first draft of this paper motivated the work of Ashlagi & Gonczarowski (2016), who showed that the outcome of DA cannot, in general, be implemented in a manner that is obviously strategy-proof for applicants.

ful reporting. Similarly, while choice data can inform policy makers on demand and unobserved quality (as in Machado *et al.*, 2012), our results complicate its interpretation.

2. BACKGROUND: THE ISRAELI PSYCHOLOGY MASTER'S MATCH

In this section we provide a brief review of the redesign of the IPMM (see Hassidim *et al.*, 2016). We begin with a review of the pre-existing market structure, and then turn to the new admission process, focusing on the unique features that allow us to detect applicants' deviations from truthful reporting.

2.1. *The IPMM 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 (including MA in clinical psychology, which is a requirement for becoming a therapist) by sending materials such as undergraduate transcripts, MITAM scores,[12] and recommendation letters.[13] 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.
- 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).

[12] 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).

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

This process was problematic in several respects. A main concern was that there was much room for strategic behavior by applicants. 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 their less preferred program.

2.2. *The IPMM since 2014*

In response to concerns about pre-existing market institutions, which mirror concerns about the decentralized matching process for American clinical psychologists in the 1990s (Roth & Xing, 1997),[14] two of us proposed an alternative centralized mechanism for this market, which was accepted by a unanimous vote of all institutions (Hassidim *et al.*, 2016). 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.[15] The admission process begins in the exact same way it used to prior to the redesign.[16] Based on applicants' grades and extra material, each program interviews applicants in the format of its choosing. Upon completion of this stage, programs are asked to report their preferences. Applicants are also prompted to submit an ROL ranking the positions (program-terms pairs) they may wish to enroll in. At this point the adapted version of the applicant-proposing DA is applied.

Participants. There are nine universities (PhD-granting institutions) and about twenty colleges in Israel. In general, graduating from a university is more prestigious than graduating from a college, which can be inferred from the minimal admission criteria to different Bachelor's degree programs, the percentage of graduates who earn advanced degrees, and the average income of graduates (Romanov, 2012). Universities are publicly funded and have identical tuition costs. College tuition varies, but it is always greater than or equal to university tuition.

Eight universities (all but the Weizmann Institute of Science) and three colleges offered admission to their PhD and Master's programs in psychology (clinical psychology and other areas) through the automated matching system. There were 13 different departments that offered such a degree (some universities have

[14] Apart from the applicants, programs also 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).

[15] Departments could use affirmative action, submit different rankings of applicants for different programs and terms, and use quotas as in Kamada & Kojima (2015). 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.

[16] 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.

multiple departments that offer these degrees).[17] Only one institution that offers a graduate program did not participate in the match. This college was not part of the coordinated decentralized process that preceded the 2014 redesign, and was not considered a competitor by the institutions participating in the match.

Funding. Some departments offer positions in the same program, but under different terms. In particular, several programs offer positions that are identical, except that only some come with funding. In 2014, a total of 10 programs in 3 universities allowed applicants to rank the programs with and without funding. In 2015, one more university, with 5 different programs, allowed applicants to rank its programs with and without funding. Three universities offered two-year MA scholarships that ranged from 8000NIS ($2070) a year up to 90,000NIS ($23,323) a year. Another university offered PhD scholarships that ranged from 16,182NIS ($4218) for three years up to 213,879NIS ($55,760) for a five-year program. The lowest level of scholarships covers roughly a year's tuition, whereas the highest pays slightly more than the median salary in Israel.

Dual listing. For some applicants the availability of funding may be a key determinant in deciding which program they prefer.[18] For example, an applicant's most preferred option could be program A with a scholarship, then program B, and then program A without a scholarship. The mechanism was designed to be expressive enough to accommodate such preferences.[19] Positions in a program like program A were represented by two "programs" $A1$ and $A2$ (representing program A with and without a scholarship), and applicants were asked to rank $A1$ and $A2$ separately. The ability to "court" particular applicants using a small number of exclusive scholarships was one of the most important features of the mechanism for some departments that felt that this enabled them to attract some "stars," thus improving the quality of their incoming class.

Releasing information. Departments were informed that their preferences would not be revealed to other departments or to applicants. Additionally, programs and applicants were both informed that applicants' ROLs and placement would not be revealed to anyone, including other applicants and programs, regardless of whether they applied to the program or not. The only exception was that the names and some personal information (but not ROLs) of unmatched applicants would be transferred to programs that either failed to fill their capacity using the match or had open positions due to "no-shows."[20] An upshot of this policy is that a program could only learn that an applicant had expressed

[17] More than 90% of the applicants completed their Bachelor's studies in one of these departments.

[18] Responses to the 2014 post-match survey (details below) suggest that the availability of a scholarship was a factor that was taken into account by nearly half of the applicants in choosing between programs.

[19] See Sönmez (2013) for an example illustrating the problems that may arise when a mechanism does not have this property.

[20] Clearly, another exception was that preferences were to become available to us for the purpose of academic research.

preference for receiving funding if she is admitted to a funded position.[21]

Educating participants. Faculty in participating departments attended presentations in which both DA and the fact that it was strategy-proof for the applicants were covered in great detail. It was also made clear 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 reporting false 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 or from the departments themselves. Furthermore, the matching system's website included a Frequently Asked Questions section that addressed this issue in multiple forms (see Appendix B). The details of DA and its strategy-proofness were carefully explained to all applicants who inquired about the mechanism (those applicants also received a link to a YouTube video of a lecture on DA in Hebrew).[22] Finally, administrative staff in all participating departments attended informational sessions about DA and its strategy-proofness, in the hope that they would be better able to provide good advice to applicants during (or after) interviews.

User interface. Applicants were asked to submit their ROLs online. There was no limit on ROLs length. The drag-and-drop interface was rather simple and friendly (see Appendix C), a fact that was reflected in responses to user surveys. If an applicant submitted an ROL that included a position in some program in which she did not rank all positions (program-terms combinations), a pop-up alert appeared. This design feature was meant to mitigate the risk of applicants accidentally ranking only some of the positions offered by a program.

3. DATA

This section provides a brief review of the data we use.

3.1. *Match Data*

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

[21] Programs were allowed to make inquiries about the applicant's preferences with regard to funding during the interviewing period. We are not aware of any program that had different application processes for different funding levels. In particular, there was no need for informing reference letter writers about the possibility of application with funding.

[22] The automated matching system administrators replied to hundreds of email inquiries about the system, and strategy-proofness was the subject of dozens of emails.

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

Recall that 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). The rationale is that if a truthful applicant included the non-funded position in her ROL, then the funded position should also have been acceptable to her and hence should be in her ROL and should be ranked higher. 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. In addition, only 11 ROLs included a funded position, but not the non-funded position in the same program.

In the 2015 match, there were 13 departments that offered 50 different programs. Of the 964 applicants who participated in the match,[23] 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 were repeat applicants, who 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 university Bachelor's, 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. 14 ROLs included a funded position, but not the non-funded position in the same program.

TABLE I

DESCRIPTIVE STATISTICS – IPMM SINCE 2014[a]

	2014	2015
Departments	13	13
Programs	52	50
Dually-listed	10	15
Applicants		
Female	733	720
Male	237	244
Placed	540	588
Placed with funding	25	35
Total	970	964
Relevant ROLs (with repetitions)		
Female	190	321
Male	70	123
BA from university	178	358
BA from MA-granting institution	228	412
Placed	193	341
Placed with funding	23	35
Total	260	444

[a] Sources: IPMM 2014-2015 administrative data.

[23] In 2015 couples were allowed to submit a joint preference list. Only one couple chose to use this option, and we excluded this observation from the analysis.

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, we cannot link the results to the administrative match data.

Following the 2015 match, we conducted a telephone survey. The survey had two purposes: first, to assess user satisfaction with the system and, second, to assess the degree to which users comprehend the "safety," or strategy-proofness, of the system. 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 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 various questions about their degree of satisfaction with the automated system. Third, they were asked several questions aimed at quantifying the degree to which they were informed about the mechanism. Fourth, they were asked questions aiming to quantify the extent to which they misrepresented their preferences. Fifth, they were asked about their degree of satisfaction with the admission process in general, and with 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 use and provides summary statistics.

The response rate was high, 292/444, over 65%. Many of the non-responders 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 that was expressed by 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. Responders and non-responders were not different in terms of any of the following observable characteristics:[24] gender, Bachelor's degree institution, being ranked by some program, whether

[24] See, however, Section 5 for evidence suggesting selection in the choice to opt not to answer particular questions.

the applicant submitted an obvious misrepresentation, and the type of obvious misrepresentation (misrepresentation types are described below).

4. EVIDENCE

In this section, we report on direct evidence of preference misrepresentation by applicants, and then turn to supporting survey-based evidence.

4.1. *Direct Evidence*

In this subsection, we focus on relevant ROLs, namely, ROLs ranking the non-funded position of a dually-listed program. Our key assumption is that placement in a program with funding is preferred by (almost all) applicants to placement in the same program without funding. We claim that this assumption is quite weak for several reasons. First and foremost, most applicants prefer having more money. Moreover, getting funding is prestigious (the funding is awarded as an excellence scholarship, e.g., a "Dean's Scholarship" or a "Presidential Scholarship"). In addition, ROLs are kept secret, a fact that was underscored and reiterated on numerous occasions in order to eliminate negative signaling concerns. Finally, the funding is in the form of a no-strings-attached scholarship.

We also assume that (almost all) applicants did not make a technical mistake in submitting their ROL. We think applicants understood the user interface while ranking, and did not make careless mistakes. If applicants would have made such mistakes, we would expect them to complain about the user interface or to notify us after the fact that they had made mistakes in the ranking (no such complaints were made). Moreover, applicants care a lot about their ranking and bother to verify what they enter (the department secretaries, on the other hand, did in a few cases make mistakes when inputting their departments' preferences). Finally, according to the applicants' own assessment (in both surveys), the user interface was accessible and easy to understand. For example, in the 2015 survey only 8% mentioned that they experienced any technical problem with registration or with ranking.[25]

It follows that an ROL ranking a non-funded position in some program higher than a funded position in the same program (henceforth *obvious flipping*), or ranking just a non-funded position in a program that offers funded positions (henceforth *obvious dropping*), is a misrepresentation of the applicant's true preferences. It is this fact that allows us to detect deviations from truthful behavior.[26] When an ROL is an obvious flipping or an obvious dropping, we say that the ROL is an *obvious misrepresentation of true preferences* or simply an *obvious misrepresentation*.

[25] Of the applicants who submitted an obvious misrepresentation, less than 4% mentioned experiencing any technical problem. The difference is not statistically significant.

[26] Such deviations constitute a weakly dominated strategy in the standard model (where lengthening the ROL is not costly).

The level of observation we choose is a particular applicant ROL. This choice leaves us with 704(= 260 + 444) relevant observations. We further eliminate from the 704 observations 32 observations that correspond to the first ROL submitted by individuals who applied in both years.[27] We report separately the results for the complete sample. Out of 672 remaining relevant ROLs, 72.7% were submitted by females, 76% by university graduates, and 90.8% by graduates of a Master's-granting institution.

Out of the 672 ROLs in our sample, 130 (19.3%) obviously misrepresented the applicant's true preferences. In the 2014 match, out of 260 relevant ROLs, 47 (18.1%) were obvious misrepresentations. In the 2015 match, out of 444 relevant ROLs, 90 (20.3%) were obvious misrepresentations. Altogether, out of 704 relevant ROLs, 137 (19.5%) obviously misrepresented the applicant's true preferences.[28] Preferences over all dually-listed programs were obviously misrepresented by some ROLs, with the percentage of ROLs misrepresenting preferences over funding terms in a certain program ranging from 9% to 29% (mean=16.7%, std. dev.=5.35%) across the various dually-listed programs.

Obvious misrepresentations are indeed misrepresentations of true preferences under the assumption that placement in any program with funding is preferred to placement in the same program with no funding. We next turn to a test of truthfulness under a weaker assumption, namely, that the direction of preferences for funding does not depend on the program the applicant attends. To this end, we focus on the 289 ROLs that include multiple dually-listed programs. We find that 24 ROLs (8.3%) rank the funded position higher for one program, but not for another program.[29]

Finally, we consider a model that slightly deviates from the "standard" model of matching markets in the market design literature, by assuming that agents face a cost that is increasing in their ROL length (as in Fack *et al.*, 2015). Such a model can rationalize certain kinds of misrepresentations known as *dropping strategies*, namely, ranking only some of the acceptable positions, but preserving their order under the true preferences. In our context, the model can rationalize obvious dropping, but cannot rationalize obvious flipping.[30] Concentrating on this refinement of the obvious misrepresentation criterion, we find that out of 652 ROLs that include both a funded and a non-funded position in the same

[27] This choice is conservative and was made to allow for learning when possible. None of our results change if we consider either the complete sample or only the first ROLs.

[28] The ROLs of 23 individuals were relevant in both years. Out of those, 4 (17.4%) submitted an obvious misrepresentation in 2015. Out of the 4 returning applicants who obviously misrepresented their preferences in 2015, 2 also submitted an obviously misrepresentative ROL in 2014.

[29] The proportion of obvious misrepresentations in this subsample is 19.0% (55/289).

[30] We do not think that costs explain much of the obvious dropping we observe in the data for several reasons. First, the user interface is very simple. Second, there were no constraints on the length of submitted lists. Third, if the source of the cost has mental foundations, then, given that the applicant has already considered the more "complex" part (the program itself), considering the funding terms presumably comes at little cost.

program, 73 ROLs (11.2%) reversed the order of a funded and a non-funded position (obviously flipped). Furthermore, out of 265 ROLs that included both a funded and a non-funded position in multiple dually-listed programs, 13 ROLs (4.9%) reversed the order of one pair, but not of that of another.

4.2. *Survey-based Evidence*

We start by reviewing some of the findings from the 2014 survey. Out of the 367 participants, 18% reported submitting an ROL that was only "*partially truthful*," with 1% reporting not submitting their true preferences. In response to another question, 13% reported intentionally giving a higher ranking to a study track "*that ranked you high (even though you may have preferred other study tracks)*." These figures illustrate the unreliable nature of survey-based estimates, even if we ignore concerns about selection bias. It is likely that responders are particularity sensitive to the phrasing of questions where the answer can reflect one's level of honesty.

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)?*" However, most of these responders could not explain why. An additional 21% reported that they thought that the answer was negative, but they could not explain why.

Out of 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 responders gave a verbal justification for their behavior, it often involved the (strategically irrelevant) consideration of increasing chances of admission.[31] Only 18 out of the 59 participants who reported increasing or decreasing the rank of some program submitted an obvious misrepresentation.

Out of the 53 responders who actually submitted an obvious misrepresentation, only 29 (54.7%) reported such behavior (17 denied and 8 refused to answer this question).[32] By contrast, only 12 out of the other 230 responders (5.2%) falsely reported that they submitted an obvious misrepresentation, and only 8 (3.5%) refused to answer (the differences are statistically significant with $p < 0.01$ using Fisher's exact test). The most common justifications given by responders for obvious misrepresentation were thinking that chances were slim and improving

[31] Three applicants reported lack of trust in the system as the reason.

[32] Out of 28 responders who submitted an obvious flipping, 16 (57%) reported such behavior. Similarly, only 14 out of the 27 (52%) responders who submitted an obvious dropping reported such behavior.

admission probability. Only three responders 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, may suggest a downward bias in survey-based estimates of preference misrepresentation.[33]

5. CHARACTERISTICS OF MISREPRESENTERS

We now attempt to get a better understanding of the characteristics of the agents we identified as misrepresenters. With this goal in mind, we first use the 672 relevant ROLs matched with the administrative information we hold. We note that the gender composition of the misrepresenters was 92 women and 38 men (similar to the proportions in the general population). The number of applicants who hold a (more prestigious) university Bachelor's degree was 85 (65.4%), significantly lower than their share in the population of applicants who submitted relevant ROLs (76.1%, $p = 0.016$).

Next, we perform linear regressions with the dependent variable being an indicator that equals one if the ROL is an obvious misrepresentation.[34] The right-hand-side variables include year, gender, and Bachelor's degree institution fixed effects, a dummy for being the ROL of one of the 15% of applicants who were not ranked by any program in the year the ROL was submitted, and in one specification dummies for the quintile of the eigenvector centrality measure of the desirability of an applicant.[35] We repeat this analysis further refining the dependent variable by the type of misrepresentation: flipping or dropping. Table II summarizes our findings.[36]

First, a word of caution is in order. The regressions above are used only as a convenient means to summarize the data, and should not be given a causal interpretation. That said, the regression results 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. With the above caveats, this could suggest that more desirable applicants are more likely to be truthful, or at least less likely to submit an obvious misrepresentation. In all specifications, the year dummy and all

[33] Such a bias may derive from individuals' unwillingness to admit to a "lie." In the 2014 survey, we find that 5% of the participants reported being truthful and also reported giving a higher ranking to a program that ranked them high.

[34] We use heteroscedasticity-consistent standard errors (White, 1980).

[35] 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. If n_{ij} denotes the number of programs that ranked both i and j and ranked i above j, we let $A_{ij} = (n_{ij}+1)/(n_{ji}+1)$. Both the eigenvector centrality measure of the desirability of applicants and the quintiles are calculated separately for each year. Though imperfect and somewhat arbitrary, Table IX provides evidence of a positive correlation between the eigenvector centrality measure of desirability and our measure of ability, namely, applicants' (self-reported) MITAM scores.

[36] Table VI contains additional specifications.

TABLE II

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. Robust standard errors in parentheses.

institution dummies had coefficients that were not statistically distinguishable from 0.

Next, we ask what would have happened had obvious misrepresenters stopped behaving in this manner. We address this question in two ways to get a lower and an upper bound. First, we changed ROLs that obviously misrepresented true preferences, by ranking funded positions just above the corresponding non-funded positions, and leaving the rest of the ROLs untouched. This gives us a lower bound of 3 individuals who would have been affected. More specifically, 3 out of 130 individuals who submitted an obvious misrepresentation of their preferences lost a scholarship due to their sub-optimal behavior. For an upper bound, we repeat the same exercise, this time placing the funded position as the first choice. We get an upper bound of 10 individuals who would have been affected.[37] To put these numbers into perspective, note that out of 567 applicants who submitted an ROL with no obvious misrepresentation, 60 were placed in a funded position. The smaller proportion of applicants that had the potential to be placed in a funded position is further evidence that it is not the most desirable applicants who submit obvious misrepresentations.[38] It is important

[37] Since in ROLs that were not an obvious misrepresentation, the mean difference in ranks between a funded position and a non-funded position in the same program was 1.34 (in the common case that both positions were ranked), it is natural to assume that the true value is closer to the lower bound. However, in light of our findings, it is likely that many of the ROLs that did not obviously misrepresent true preferences did not reflect true preferences, particularly with respect to funding.

[38] The adaptations that were required from our variant of DA in order to accommodate the

to stress that the above bounds only cover the implications of *obvious* misrepresentation. It is reasonable to assume that there were other kinds of preference misrepresentations that we cannot detect, and thus we are unable to measure their welfare implications or correlates.

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. In Figure 1, we start by binning observations by MITAM score, using 20 bins, and plotting them against the fraction of misrepresenters. A negative, potentially hump-shaped relationship is apparent.

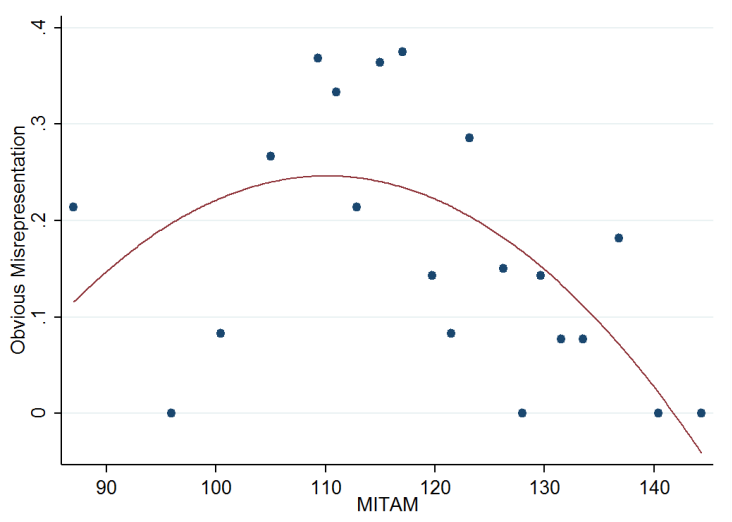

FIGURE 1.— Plot of obvious misrepresentation against MITAM score. Observations are collected to 20 equal bins. The red line represents the best quadratic fit. Sources: 2015 match data and 2015 post-match survey.

Next, we regress a dummy for obvious misrepresentation on administrative and survey-based controls. Table III presents the results from two such regressions.[39] All specifications suggest a negative relation between MITAM and obvious misrepresentation above the median MITAM score in the sample (119).

In Table VIII we further break down the obvious misrepresentation variable to obvious dropping and obvious flipping. In both cases the results are similar to the results in Table III. Additionally, we find that obvious dropping is negatively

departments' preferences with respect to funding make it difficult to deduce much from the aggregate effect of misrepresentation. For example, the 3 applicants in the lower bound did not displace any other candidate (they were accepted by the same program, but without funding).

[39] Table VII contains additional specifications.

TABLE III

CORRELATES OF OBVIOUS MISREPRESENTATION – SURVEY DATA[a]

	(1) OMR	(2) OMR
Female	−0.0649	−0.0717
	(0.0576)	(0.0566)
FaqHelpful	−0.0212	−0.0166
	(0.0837)	(0.0859)
FaqNotRead	0.0655	0.0705
	(0.0965)	(0.0985)
ExplanationConfidence	8.70e−05	−0.00377
	(0.0272)	(0.0261)
Age	0.00586	0.000448
	(0.0258)	(0.0254)
SocioeconomicStatus	0.0307	0.0349*
	(0.0205)	(0.0202)
MITAM	−0.0824***	−0.106***
	(0.0259)	(0.0260)
$MITAM^2$		−0.0609***
		(0.0146)
NotRanked	0.0534	0.0194
	(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
Observations	240	240
R-squared	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. Column 2 includes a quadratic term in MITAM score. Explanation confidence, age, socioeconomic status, and MITAM were normalized to have 0 mean and standard deviation of 1. Robust standard errors in parentheses.

correlated with socioeconomic status, and that obvious flipping is positively correlated with not reading the frequently asked questions. Such correlations could be explained by wealthier individuals putting less weight on funding in their (mistaken) trade-off, and by less attentive individuals being less aware of the way the mechanism works.

We then try to evaluate whether obvious misrepresenters are making a costly mistake. We already know that this was clearly the case for 3 candidates, and that 7 others may have been affected, or at least would have been affected had they not been admitted to a more preferred program (each affected individual potentially lost at least $2050 per year). In addition, in both years we find that applicants who submitted an obvious misrepresentation were no less likely to appear in departments' choice-rules as acceptable with funding. In the absence of

a probabilistic model we cannot assign an (ex-ante) cost to applicants' behavior, but we conclude that it was indeed costly.

Finally, we repeat the analysis of Table III with reported misrepresentation as the left-hand-side variable. The results are summarized in columns 1 and 2 of Table X. 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.[40]

6. CHARACTERISTICS OF MISREPRESENTERS: EVIDENCE FROM OTHER ENVIRONMENTS

In the previous section we documented a negative correlation between applicants' desirability and obvious misrepresentation. This correlation could be driven by at least two forces. First, it could be the case that *lower ability* individuals are more likely to err (see Basteck & Mantovani, 2016, and Benjamin *et al.*, 2013). But a second possibility is that lower ability individuals are typically less desirable and applicants who perceive themselves as less desirable are more likely to submit an obvious misrepresentation (e.g., because they are more stressed or because they think they stand no chance).

In the absence of exogenous variation, we cannot attribute causal interpretation to this relationship. Furthermore, our capacity to disentangle ability and desirability was limited, as the measures we used were noisy and correlated with each other. Additionally, applicants do not know how programs rank them, and thus their MITAM score could be thought of as a signal on their desirability.[41]

A further limitation comes from our reliance on (self-selected) responders' self-reports, which may be misleading. Subsection 4.2 provides two examples of such problems in our data. First, we find that reports of misrepresentation were sensitive to the way questions were phrased. Second, we find that almost 50% of the responders who submitted an obvious misrepresentation failed to report this behavior, while more than 90% of the responders who did not submit an obvious misrepresentation provided an accurate account.[42]

In addition, responders who refused to provide an answer to the question about their MITAM score were more likely to have a lower score. To see this, note that responders who chose not to answer the question about their MITAM score were more likely to be unacceptable to all departments, and were less

[40] In this context, it is important to re-iterate the fact that the survey was conducted after the match results were published, thus reports regarding self-confidence about ability may have been affected by match outcomes.

[41] The correlation between MITAM and desirability is documented in Figure 3 and in Table IX.

[42] These findings are in line with the large literature on the magnitude and correlates of misreporting in surveys (especially with regard to reports of socially desirable behavior). For example, Ansolabehere & Hersh (2012) show that many non-voters report in a survey that they voted, but virtually all voters report truthfully.

desirable according to the eigenvector centrality measure. Table XI illustrates this point. Column 1 concentrates on the subsample of responders who were acceptable to at least one program, and shows a negative correlation between (eigenvector centrality) desirability and refusing to answer the MITAM question in the survey. Column 2 considers the complete sample and reports a large and marginally significant correlation between being unacceptable to all programs and refusing to answer.

To address these limitations, in this section we present evidence from a laboratory experiment providing a strong validation to the causal relationship between expected desirability and misrepresentation. We then discuss further suggestive evidence from the field experiment of Bobba & Frisancho (2016).

6.1. *Characteristics of Misrepresenters in the Lab*

We revisit data from one of the treatments in Li's (2016) experiment.[43] Four subjects could receive one of four common value monetary prizes, drawn uniformly without replacement from the set $\{\$0.00, \$0.25, \$0.50, \$0.75, \$1.00, \$1.25\}$.

At the start of each of 10 rounds, subjects observe the values of all four prizes. They are then privately informed of their priority score, which is an integer between 1 and 10, drawn independently and uniformly at random. Next, they simultaneously submit ROLs ranking all four prizes (shorter lists are not allowed). Prizes are allocated using the Serial Dictatorship mechanism, with players being processed from the highest to the lowest priority score and with ties broken at random. At the end of each round, subjects observe the prize they have obtained and learn the rank of their priority score (highest, second highest, etc.).

Serial Dictatorship could be interpreted as a particular instance of DA, where subjects are on the proposing side, prizes are on the receiving side, and all prizes are endowed with the same ROL (which corresponds to the order of processing). The existence of a dominant strategy in this setting was not mentioned to avoid experimenter demand effects.[44]

A total of 18 groups of four subjects participated in this treatment. Li reports that 29.0% of the ROLs misrepresented true preferences (did not list the four prizes in order), and that 36.1% of the rounds did not yield the outcome dictated by dominant strategy play. That is, in line with the experimental literature we reviewed earlier, mistakes were frequent and costly. On average, on each round subjects lost \$.06 due to their sub-optimal behavior,[45] while the average profit

[43] We are enormously grateful to Shengwu Li for sharing this data. A detailed description of the experimental design could be found in Li (2016).

[44] This treatment followed earlier parts of the experiment, where variants of the second-price auction were played by the same groups of four subjects. They held printed copies of the instructions and the experimenter read aloud the part relevant to these 10 rounds. In addition, it was explained that play in earlier rounds will not affect later ones.

[45] Since the game played in the laboratory was constant-sum, mistakes had no aggregate effect.

per round was about \$.64. Conditional on submitting a misrepresentation the average loss is \$.21.

The source of variation that is of interest to us is the priority score. We posit that an agent's priority score is a good proxy for expected desirability in the field. As priority scores were assigned at random, a correlation between misrepresentation and priority scores can be attributed a causal interpretation.

Figure 2 presents the raw data. For each priority score, we plot the fraction of lists containing a misrepresentation out of the lists that were submitted by an agent with this particular priority score in the round when the list was submitted. The observed pattern seems consistent with our findings in the field: High priority appears to be negatively correlated with misrepresentation. Table IV summarizes findings from running a linear regression with standard errors clustered at the subject level.[46] Our estimates suggest a difference of about 37 percentage points (more than 200%) in the probability of misrepresentation between otherwise identical subjects with priority scores of 1 and 10.

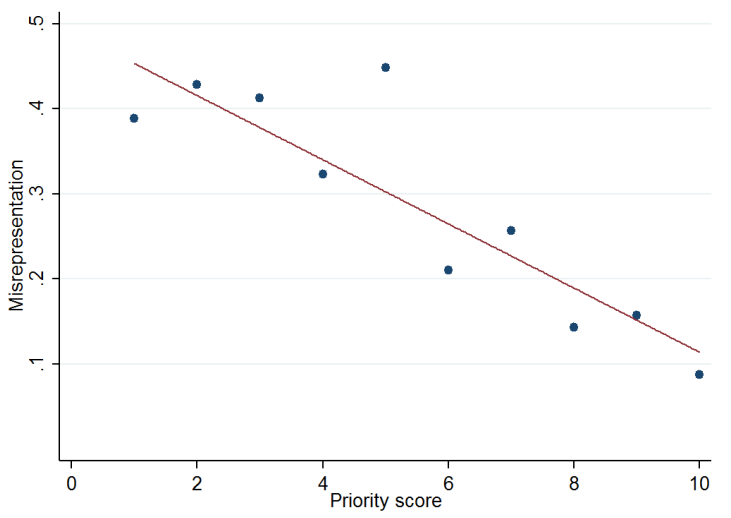

FIGURE 2.— Plot of misrepresentation against priority score. The figure plots the rate of preferences misrepresentation for each priority score. Subjects were informed that ROLs were processed from high to low priority score with ties broken at random. The red line represents the best linear fit.

Finally, we inspect the correlation of misrepresentation with a measure of ability, self-reported GPA.[47] Table XIII documents a positive correlation between

[46] Table XII contains additional specifications.
[47] All subjects are students in the Ohio State University.

TABLE IV

MISREPRESENTATION VS. PRIORITY SCORE – EXPERIMENTAL DATA[a]

	(1) Misrepresentation	(2) Misrepresentation
Score	−0.0377***	−0.0401***
	(0.00686)	(0.00668)
Constant	0.491***	0.358***
	(0.0629)	(0.0500)
Period FE	NO	YES
Subject FE	NO	YES
Observations	720	720
R-squared	0.062	0.529

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

[a] Notes: The table presents the results of a linear regression of a dummy variable for misrepresentation on priority score, period in the experiment (or period dummy for rounds 1 to 10), and subject fixed effects. Standard errors clustered at the subject level in parentheses.

GPA and misrepresentation for female subjects but not for male subjects. Additionally, women seem to be more affected by a low score, but the effect is not statistically significant.[48] These exploratory results may be of interest for future studies.

6.2. *Characteristics of Misrepresenters in Mexico City's High-School Match*

The previous subsection documents a causal relationship between low expected priority under SD and preference misrepresentation in the lab. In this subsection, we cite further evidence from the field in support of this theory.

We revisit the findings of Bobba & Frisancho (2016) who conducted a field experiment in the Mexico City metro area, where each year over 238,000 public high-school seats are offered using a variant of SD, with priorities based on performance in a standardized achievement test. Interestingly, students are required to submit their ROL early in the process, well before the standardized test is held. Given that less than 2% of the applicants submit the maximal number of options that the forms allow (20), and under standard assumptions on preferences and utility maximization, it follows that individuals' lists represent their true preferences.[49]

In their study, Bobba & Frisancho provided a sample of students from schools in worse-off neighborhoods in Mexico City with a mock version of the achievement test prior to the submission of ROLs. They elicited beliefs about perfor-

[48] This difference may be interpreted as related to gender differences in attitudes toward competition (Niederle & Vesterlund, 2007).

[49] Note, however, that the fact that an overwhelming majority of the applicants did not rank the maximum number of programs allowed is in itself a hint that the standard assumptions do not hold in light of applicants' uncertainty about their performance in the standardized achievement test.

mance in the test both before and after taking the mock exam, and then informed a subset of students about their performance in the exam. Matching their field data with administrative match data allows them to identify the effect of the intervention on treated students' beliefs, performance, ROLs, and assignment.

While the majority of students overestimate their score (in both the mock exam and the actual exam), about 20% of the students under-estimate their performance. Bobba & Frisancho find that students' expectations about their performance in the exam are a good predictor of school-track choices and outcomes.[50] Conditional on performance on the test, a one standard deviation increase in expected performance is associated with an increase of 4.5% in the share of academic programs in students' ROLs and an increase of 7.5% in the probability of being assigned to such a track.

The authors find that treated students update their beliefs in the right direction and that, on average, the fraction of academically oriented tracks on their ROL becomes more correlated with their performance on the (actual) standardized test and with their middle school GPA. This effect is also translated into differences in actual assignment, which becomes more assortative, suggesting a potential for long-term implications.[51] The effect is heterogeneous and is concentrated in under-confident students. The fraction of academically oriented tracks on ROLs of treated under-confident students is 18% greater than that of controls.

Bobba & Frisancho attribute the effect they identify to changes in preferences due to increased information about one's own academic ability.[52] They support this conclusion by showing that students are uninformed about the way they would perform on the standardized exam, and update their beliefs about their expected performance following the treatment. But as the measure of ability that they use is the score on the (mock) standardized admission exam, one cannot disentangle the effect of increased information about ability from the effect of information on expected priority in the mechanism. Moreover, we find it likely that students are much better informed about their academic ability than about how their skills are aligned with the particular features of the exam.[53]

This observation suggests an alternative interpretation of the findings of Bobba & Frisancho: that the effect is a result of preferences misrepresentation, and particularly by individuals who under-estimate their priority (desirability). This

[50] The three possible school tracks are general (academically oriented), technical, and vocational.

[51] De Janvry *et al.* (2016) analyze this market using a regression discontinuity design and find an effect of particular programs on university admission test scores and drop-out rates.

[52] Under this interpretation, the paper is related to previous work that found that informational barriers play a role in educational choices and that informational interventions affect these choices (Dinkelman & Martínez A, 2014; Hastings & Weinstein, 2008; Hoxby & Avery, 2013; Hoxby & Turner, 2015; Jensen, 2010).

[53] Bobba & Frisancho state that *"most applicants apply to high schools without a clear idea of their own academic ability. Students are required to submit their final preference sheets early during the application process, not only before the admission test, but even before the majority of applicants start preparing for the exam."*

interpretation is in line with Chen & Pereyra (2015) who document suggestive evidence of preference misrepresentation in the Mexico City high-school match, especially by academically weaker students. They attribute their findings to "self-selection," namely applicants assigning zero probability to being admitted to a competitive program, often mistakenly.

Conservatively, we can say that it is not possible to separate the effects of the two channels on the results. A more controversial statement would be that while self-perceptions of ability are not likely to change by much following a single mock exam, perceptions about performance in the actual exam are clearly affected (as Bobba & Frisancho establish), and thus our alternative explanation is more likely.

7. DISCUSSION

Strategy-proofness is one of the core properties of the deferred-acceptance algorithm, and is a key reason for its adoption in real-life markets such as the NRMP and school choice in Boston and NYC. But evidence suggesting that a significant percentage of participants misrepresent their preferences under DA is accumulating. In the lab, a robust finding is that a large portion of the participants (typically more than 20%) misrepresent their preferences. In the field, evidence comes from surveys as well as structural econometric models. Until now, however, there was no direct evidence of misrepresentation in the field.

We use data from the redesigned admission process for graduate degrees in psychology in Israel to provide direct evidence of such misrepresentation in the field. Our estimates are consistent with findings from the lab, even though the environment we study has none of the features of lab experiments discussed above: applicants are well aware of the rules and invest time and money before making their choices,[54] the ranking system is open for two weeks (and most applicants think about their ROL well before the ranking system opens), and plenty of advice is available. Furthermore, many applicants are very involved in the mechanics of the ranking process, as is evident from the hundreds of email inquiries sent to the matching system administrators. The results are also robust to standard critiques of surveys or structural models: we require no assumptions other than the weak assumption of monotonicity of preference for money, and since we use *all* (not a self-selected subsample) *actual* (not self-reported) ROLs, our findings are not susceptible to selection bias or reporting issues. Finally, instead of providing a rejection of truthfulness in the field, we provide a lower bound on the number of misrepresenters.

The misrepresentation we find is clear cut. Applicants report privately that, if assigned to some program, they prefer not to get a professional excellence scholarship, which is a positive signal of their ability, provides generous funding, and

[54] Many of them spend two months studying for the MITAM exam, and most pay hundreds of dollars to universities in application fees and undergo numerous interviews. Several companies offer a MITAM preparation course.

comes with no strings attached. To support this finding we provide survey evidence of further misrepresentation. We also provide evidence of under-reporting of misrepresentation by comparing survey self-reports to observed behavior.

In school-choice systems, one of the arguments for using a truthful mechanism is that strategizing requires information and resources that are more readily available to affluent families, and that using a strategy-proof mechanism "levels the playing field." Our results raise the question of who is misreporting in such truthful environments. The misrepresentation we detect in the IPMM seems to be associated with the weaker segment of the market.[55] This finding is in line with the growing literature that documents a negative correlation between "mistakes" and cognitive ability. But another explanation is that applicants make more mistakes when they perceive themselves as "less desirable" or when perceived competition is fiercer.[56] We establish this second channel experimentally, and provide further suggestive evidence from the Mexico City high-school match. Additionally, we provide evidence suggesting that the first channel may also play a role.

As for the motives for preference misrepresentation, we can only speculate based on survey responses and on our interaction with market participants. We believe that some applicants misrepresent their preferences in a futile attempt to increase their chances of being admitted to *any* program.[57] For many applicants the difference between getting into a clinical psychology program (which offers a good high-income career path) and not getting into one (which means that the applicant graduated from a Bachelor's program in psychology and is now left with no "profession") is enormous,[58] and if an applicant believes that misrepresenting her ROL gives her a slightly better chance of being admitted to *any* program then it makes sense for her to misrepresent her preferences. Other applicants misrepresent their preferences because they think that they "have no chance" of getting a scholarship.[59]

[55] Recall, however, that all IPMM applicants hold an undergraduate degree in psychology and apply for a graduate degree.

[56] Unlike in many school-choice settings in the U.S., in the IPMM it is difficult to disentangle ability and expected desirability (or priority). In this context, it should be noted that in many other contexts outside the U.S., such as school choice in Mexico City and Romania (Pop-Eleches & Urquiola, 2013; Chen & Pereyra, 2015) and college admission in Turkey and Hungary (Balinski & Sönmez, 1999; Biró *et al.*, 2010), priorities are a function of academic success. This is also true to some extent in the NRMP.

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

[58] This is evident from the fact that over the course of two years, only 25 ROLs included a funded position in a program without including the non-funded position of the same program.

[59] In an attempt to rationalize suggestive evidence of preference misrepresentation in Mex-

Our findings have implications for several streams of literature. First, from the practical market design perspective, our results highlight the need for decision support systems, even in strategy-proof environments.[60] Second, they underscore the need for mechanisms that are robust to behavioral faults and for stronger and potentially more predictive notions of implementation. Finally, our findings raise concerns about the validity of econometric estimates based on match data, which typically assume truthful reporting by the proposing side under DA.

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ico City's high-school match (which uses a variant of serial dictatorship), Chen & Pereyra (2015) attribute misrepresentation to assigning zero probability to certain alternatives. However, this explanation cannot account for obvious flipping, assuming there is even a small cost for increasing the length of an ROL. More importantly, even though in the IPMM obvious misrepresentation was associated with the weaker segment of the market, it was still the case that many misrepresenters' chances of a funded position were non-zero.

Additional explanations that we cannot completely rule out but find implausible are that applicants misrepresent their preferences in a way that takes into account the information that the match outcomes reveal (Köszegi, 2006) or changes to their reference point (Kőszegi & Rabin, 2006). For example, applicants may obviously drop to avoid learning that they would not have been admitted, or to avoid disappointment. Survey responses do not seem to support these theories, and they cannot explain the experimental findings.

[60] This need was also highlighted by a report by the Center on Reinventing Public Education on the Denver and New Orleans school choice systems that states: *"[i]t is not enough to tell parents how to choose. Simple interactive tools that allow parents to engage in mock lotteries may go a long way in helping to unseat parents' misconceptions and may reduce the sense that a distant (and unsympathetic) computer is making the decision about their child's school"* (Gross *et al.*, 2015).

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APPENDIX A: ADDITIONAL TABLES AND FIGURES – FOR ONLINE PUBLICATION

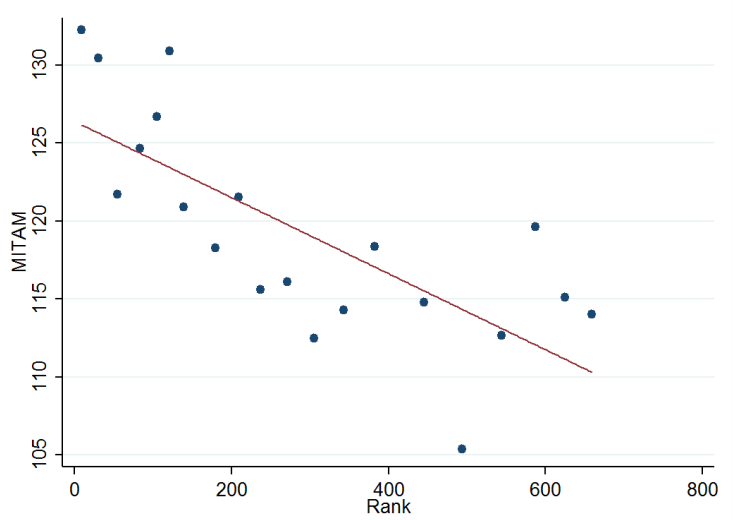

FIGURE 3.— MITAM vs. desirability. Observations (216 responders who were ranked by some program and reported their MITAM score) are partitioned to 20 equal bins by their eigenvector centrality rank. Lower rank corresponds to higher desirability.

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
Admitted	.784 (.412)	672	1 if applicant was assigned a position through the match
NotRanked	.149 (.356)	672	1 if the 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 frequently asked questions and that it was helpful
ExplanationConfidence	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			0.0273			0.0152
			(0.0533)			(0.0416)			(0.0473)
Desirability Quintile(2)			−0.0437			−0.0278			−0.0465
			(0.0506)			(0.0375)			(0.0392)
Desirability Quintile(3)			0.0315			0.0240			−0.00268
			(0.0531)			(0.0412)			(0.0400)
Desirability Quintile(4)			−0.0706			−0.0152			−0.0662**
			(0.0479)			(0.0387)			(0.0335)
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	0.049

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

[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 in parentheses.

TABLE VII

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 MITAM score. Columns 3 and 4 include controls for desirability quintiles. Explanation confidence, age, socioeconomic status, and MITAM were normalized to have 0 mean and standard deviation of 1. Robust standard errors in parentheses.

TABLE VIII.— 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)
Explanation	−0.0169	−0.0195	−0.0163	−0.0188	0.0212	0.0196	0.0204	0.0188
Confidence	(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	0.00497	0.00737	0.00847	0.0112	0.0332**	0.0348**	0.0310**	0.0326**
Status	(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 the specifications presented in Table VII (and Table III). Explanation confidence, age, socioeconomic status, and MITAM were normalized to have 0 mean and standard deviation of 1. Robust standard errors in parentheses.

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.[61]

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 his most preferred track. If the track prefers other candidates, the candidate tries to be placed in his second favorite track and so on, until the candidate is temporarily placed, or until he 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 the 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 the 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 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 he is 13th, but he gave the track a higher priority than I did, is it possible that he will be admitted and I will not (assuming 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

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

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. Still, even if you would have been admitted to this track, it is impossible to move there after the placement is set.

APPENDIX C: USER INTERFACE SCREENSHOTS – FOR ONLINE PUBLICATION

FIGURE 4.— 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 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.

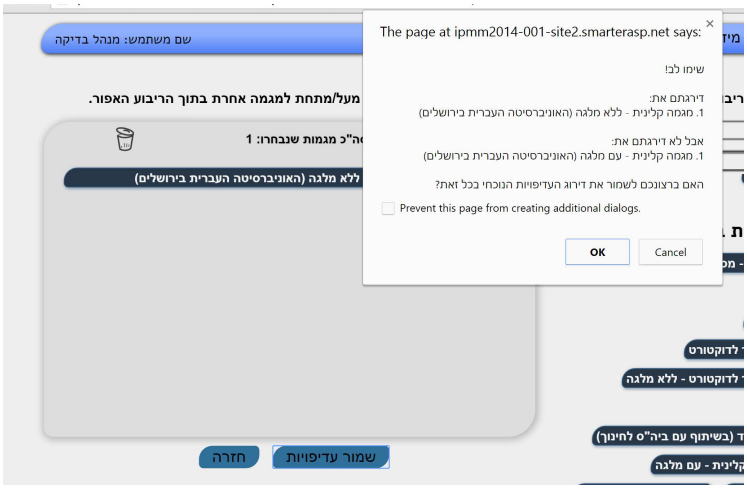

FIGURE 5.— Missing terms warning. This pop-up alert appears since the applicant chose to rank a program without funding, but did not rank it with a scholarship even though such an option existed. The message reads: "Attention! You ranked: (1) Clinical psychology without scholarship (The Hebrew University 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?"

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

- Was 2015 the first year you applied for a graduate degree?
- If not, on 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?
- 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?

TABLE IX

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, allowing to add unranked applicants to the regression (omitted dummy). Robust standard errors in parentheses.

TABLE X

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 (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.

TABLE XI

CORRELATES OF RESPONDERS REFUSING TO REPORT MITAM[a]

	(1) Refused	(2) Refused
DesirabilityRank	0.000261***	
	(9.52e−05)	
NotRanked		0.123*
		(0.0702)
Constant	0.0537*	0.133***
	(0.0275)	(0.0216)
Observations	249	292
R-squared	0.024	0.015

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

[a] The table presents the results of a linear regression of a dummy for refusing to respond to the MITAM question on our desirability measures. Column 1 uses the desirability rank, whereas column 2 uses a dummy for not being ranked by programs. The difference in the number of observations stems from the fact that desirability rank can only be calculated for individuals who were ranked by some program. Robust standard errors in parentheses.

TABLE XII

MISREPRESENTATION VS. SCORE – EXPERIMENTAL DATA[a]

	(1) Misrepresentation	(2) Misrepresentation	(3) Misrepresentation	(4) Misrepresentation	(5) Misrepresentation
Score	−0.0377***	−0.0410***	−0.0408***	−0.0401***	
	(0.00686)	(0.00665)	(0.00667)	(0.00668)	
score=2					−0.0287
					(0.0691)
score=3					−0.0582
					(0.0643)
score=4					−0.0635
					(0.0705)
score=5					−0.0293
					(0.0562)
score=6					−0.190***
					(0.0665)
Score=7					−0.190***
					(0.0694)
Score=8					−0.306***
					(0.0760)
Score=9					−0.270***
					(0.0650)
Score=10					−0.369***
					(0.0654)
Period			−0.00736		
			(0.00650)		
Constant	0.491***	0.311***	0.351***	0.358***	0.988***
	(0.0629)	(0.0505)	(0.0651)	(0.0500)	(0.0653)
Period FE	NO	NO	NO	YES	YES
Subject FE	NO	YES	YES	YES	YES
Observations	720	720	720	720	720
R-squared	0.062	0.521	0.523	0.529	0.536

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

[a] The table presents the results of a linear regression of a dummy variable for misrepresentation on priority score (or score dummies), period in the experiment (or period dummy for rounds 1 to 10), and subject fixed effects. Column 5 presents the most flexible specification. Standard errors clustered at the subject level in parentheses.

TABLE XIII

MISREPRESENTATION VS. ABILITY – EXPERIMENTAL DATA[a]

	(1) Misrepresentation	(2) Misrepresentation	(3) Misrepresentation
Male	−0.132*	−0.113*	−0.0943
	(0.0697)	(0.0653)	(0.100)
STEM			−0.0802
			(0.101)
Score	−0.0451***	−0.0460***	−0.0452***
	(0.00902)	(0.00908)	(0.00899)
GPA		0.122**	0.118**
		(0.0476)	(0.0483)
Male × STEM			−0.0268
			(0.135)
Male × Score	0.0133	0.0132	0.0126
	(0.0135)	(0.0133)	(0.0131)
Male × GPA		−0.170***	−0.170***
		(0.0596)	(0.0599)
Period			−0.00749
			(0.00624)
Constant	0.348***	0.318***	0.395***
	(0.0537)	(0.0489)	(0.0764)
Observations	720	720	720
R-squared	0.086	0.123	0.136

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

[a] The table presents the results of a linear regression of a dummy variable for misrepresentation on period in the experiment (rounds 1 to 10), in addition to priority score, GPA and a dummy for STEM major, as well as their interaction with Gender. GPA and priority score were normalized to have 0 mean and standard deviation of 1. All subjects are students in the Ohio State University. Standard errors clustered at the subject level in parentheses.