

How to Get Truthful Reporting in Matching Markets: a Field Experiment*

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Abstract

We run a field experiment to test the truth-telling rates of the theoretically strategy-proof Top Trading Cycles mechanism (TTC) under different information conditions. First, we asked first-year economics students enrolled in an introductory microeconomics unit about which topic, among three, they would most like to write an essay on. Most students chose the same favorite topic. Then we used TTC to distribute students equally across the three options. We ran three treatments varying the information the students received about the mechanism. In the first treatment students were given a description of the matching mechanism. In the second they received a description of the strategy-proofness of the mechanism without details of the mechanism. Finally, in the third they were given both pieces of information. We find a significant and positive effect of describing the strategy-proofness on truth-telling rates. On the other hand, describing the matching mechanism has a significant and negative effect on truth-telling rates.

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1. Introduction

The use of matching mechanisms for school choice programs is, without a doubt, one of the most relevant and successful real-world applications of game theory, see for instance Abdulkadiroglu et al (2005). In such scenarios parents are asked to rank the available public schools in the area. On the other side of the market students are ordered by a priority score. Submitted ranks and priorities are fed into a mathematical algorithm to produce a match of students to school seats. The use of strategy-proof matching algorithms, in which participants have the right incentives to reveal their true ranking, is considered desirable. Indeed, if parents devote their energy to devise manipulation strategies they will have less time to discover the true quality of the schools available. Matching mechanisms are, however, complex and most likely difficult to understand for lay people. Whether theoretically strategy-proof mechanisms induce high truth-telling rates among participants is a critical empirical question for which answers are not readily available in the field. The true rankings of participants in matching markets are hard to elicit. That's why researchers turned to run matching experiments with induced valuations.¹

Chen and Sönmez (2006) pitched two theoretically strategy-proof mechanism, Top Trading Cycles (TTC) and Deferred Acceptance (DA) against the then most popular mechanism in use, the non-strategy-proof Boston (BOS). In Chen and Sönmez (2006) both TTC and DA do induce higher truth-telling rates (and efficiency) than BOS. More recent experiments also find remarkably high truth-telling rates for strategy-proof mechanisms, between 62% and 96% for TTC in particular.² All in all, there is little doubt experimental findings played a role convincing school districts to adopt strategy-proof mechanisms, either DA or TTC.

However, the laboratory and real-life implementations of matching mechanisms often differ, critically, in terms of the information available to participants. That is, experimental subjects are generally given a very accurate, if not cumbersome, description of the mechanism together with a solved example. On the other hand, and although the details vary from one school choice program to another, it is generally quite difficult for participants in real life markets to obtain a description of the algorithm mechanics. Conversely, experimental subjects

¹ That is, experimental subjects are given monetary values for the variety of objects they can be assigned to. For instance, \$20 for school A, \$35 for school B and \$10 for school C.

² Calsamiglia et al (2010): DA 57%-58%, TTC 62%-74% ; Pais and Pintér (2008): DA 67-82%, TTC 87%-96%; Pais et al (2011): DA 58%-76%, TTC 62%-84%.

are typically not directly informed of the properties of the mechanism (strategy-proofness, stability, etc.), while participants in real-life markets are often told about strategy-proofness in one way or another. For instance, both the Boston Public Schools (BPS) system (Boston Public Schools, 2014) and the New Orleans Recovery District (Vanacore, 2012) websites do not contain algorithm descriptions (the last time we checked), but they both do inform participants that the best they can do is report their true preferences.

This paper focuses on the informational differences between matching markets implemented in the laboratory and the field. For that purpose we designed a controlled field experiment. We run a TTC-based, in-class topic allocation task to compare three treatments that differ in the information given to participants: an only “mechanism description” (MD), only “properties description” (PD), and both “mechanisms and properties descriptions” (MPD). The aim of the experiment is to assess which informational structure generates the highest truth-telling rate.

Our experiment took place at the University of Sydney, with first-year students of an introductory microeconomics course as participants.³ The students had to write an essay about the structure of one of three markets: smartphones, TV sets or scanners. We simply elicited student’s actual first preference by asking them to nominate their favorite topic (smartphone, TV set, scanner). The vast majority of students chose the smartphone. Then students were told in class that the topics had to be evenly allocated: one-third of the students to each topic. They were also told that to achieve this goal a matching mechanism would be used. Each one of the three sections of the course received the instructions for one of the three treatments. We find that describing the strategy-proofness property of TTC⁴ leads to a significantly higher rate of truthful reporting. However, a description of the mechanism itself leads to a lower rate of truthful reporting.

³ It needs to be clear that this is a field experiment regardless of participants being university students. Indeed: 1) the experiment is done in its “natural” environment for the decision of interest, by real strategic agents, 2) we do not impose preferences of the participants, 3) participants are not volunteers but students going over a classroom procedure.

⁴ We use “properties description” and “advice” as interchangeable terms.

2. Literature review and motivation

Since Abdulkadiroğlu and Sönmez (2003) proposed the use of the TTC mechanism to solve the school choice problem several experimental papers have examined the truth-telling rates in TTC, mostly by pitching it against competing mechanisms. In a path-finding experimental paper, Chen and Sönmez (2006) compared TTC (Pareto efficient and strategy-proof) to DA (non-Pareto efficient but strategy-proof) and the Boston mechanism (BOS) (non-strategy-proof mechanism⁵) to show how both TTC and DA outperform BOS terms of truth-telling and efficiency. In light of their result, Chen and Sönmez (2006) advocated for the use of strategy-proof mechanisms and recommended educating the parents in order to increase truth-telling rates. Since then, many other experimental matching papers have reported fairly high truth-telling rates for TTC (62-96%).⁶ Given the desirable properties of TTC, the efficiency obtained and the relatively high truth-telling rates, there is a generally positive perception about the adequacy of the mechanism.

Nevertheless, attentive reading of the experimental matching literature raises some doubts about the capacity of theoretical strategy-proof mechanisms to generate high truth-telling rates. For instance, Pais and Pintér (2008) found that TTC outperforms DA and BOS with respect to the criterion of truthful preference revelation in all the informational settings tested. However, they also demonstrated that additional information leads to higher rates of preference misrepresentation in all three mechanisms. Klijn et al. (2013) compared BOS with DA, devoting special attention to individual behavior. In particular, they include a simple lottery to elicit risk aversion. Klijn et al. (2013) showed a positive correlation between risk aversion and the probability to play *protective* (out-of-equilibrium in any case) strategies under DA, thus showing that more risk-averse subjects are less likely to reveal their true preferences. Guillen and Hakimov (2014) found how truth-telling decreases dramatically when experimental subjects are informed about the strategies played by others. That is, there is a growing stream of the experimental matching literature eroding the idea of truth-telling driven by subjects understanding strategy-proofness from reading the instructions and working on the examples provided in the laboratory.⁷

⁵ BOS is Pareto efficient if participants reported their true rankings.

⁶ Not only the already mentioned Calsamiglia et al (2010), Pais and Pintér (2008) and Pais et al (2011), but also Guillen and Hing (2014) and Guillen and Hakimov (2014) in their baselines.

⁷ There is a critical difference between the mere procedural understanding of the mechanics of an algorithm shown in an example and being actually able to infer strategy-proofness. The idea has been recently captured by Li (2015): “Suppose an agent is unable to distinguish games that generate the same experiences: He retains substantial knowledge about the structure of the game. He knows the information sets at which he may be called to play, and the actions available at each information set. He knows, for any experience, what outcomes may result. However, he is unable to reason case-by-case about hypothetical scenarios.” The same paper introduces

Interestingly, the school districts rarely provide any explanation of the matching mechanism equivalent to the experimental instructions used in the laboratory experiments and therefore they consciously refrain from trying to “educate” parents. For instance, the New Orleans Recovery District (NORD) adopted TTC for student assignment a few years ago (Hakimov and Kesten, 2014). In New Orleans, a sketch of TTC’s mechanics was only made available to the public once through a poster published by the local newspaper. NORD does inform participants that the best they can do is report their true preferences (Vanacore, 2014). Following Chen and Sönmez (2006) recommendation to educate the initial DA implementation of Boston’s BPS match offered a detailed explanation of DA together with seminars for interested parents. BPS also explained that truth-telling is the best strategy for parents in informational packages. Nowadays, the BPS website only includes a fade mention to strategy-proofness, but no procedural explanations. In summary, given the available information, it’s unlikely that parents fully take the time and energy to seek out and understand the details involved in the procedure that assigns their children to schools.

The main interest of the current paper is, for practical purposes, to test the effect of strategy-proofness advice on truth-telling behavior. Note that explaining the properties of the mechanisms in the lab might help to overcome the gap with the field, but it could easily lead to methodological problems like demand effects and/or confusion. Nevertheless, a new stream of the literature adds advice to matching experiments. For instance, Guillen and Hing (2014) provide experimental evidence that wrong third-party advice can easily mislead participants and result on very low truth-telling rates in the lab. Ding and Schotter (2014a) find that chatting in between two DA matching markets does not increase truth-telling rates. Ding and Schotter (2014b) find that after 20 rounds of intergenerational advice truth-telling decreases dramatically from above 70% to about just 45%⁸. Those three papers provide further indication of truth-telling not being driven by the transparency of the experimental instructions.⁹ However, can correct advice induce participants to make the right choices? Braun et al. (2014) report some success in this direction: it includes correct advice in the experimental instructions which helps subjects to behave optimally. In contrast to Ding and Schotter (2014b), Zhu (2015) shows that intergenerational advice might increase truth-telling rates in the simplest market of three agents and three objects in lab, but only

the Obviously Strategy-Proof (OSP) concept and proves that With 3 or more agents, there does not exist a mechanism that OSP-implements TTC.

⁸ Note that these truth-telling rates are observed for the only proposing side of DA with the possibility of submitting a full list. Truth-telling is a dominant strategy in this unilateral version of DA.

⁹ An alternative explanation for the high truth telling rates found in many matching experiments could be a demand effect stemming from induced preferences.

when preferences are uncorrelated. Further than that, there exists some limited evidence suggesting that simple advice or simplified information might lead to improvements in the field. For instance, Hastings and Weinstein (2007) show in their field experiments that provision of simple information about school test scores to lower-income families increases the chance of higher-performing schools being listed in the choice lists for school choice program in the Charlotte-Mecklenburg Public School District, North Carolina. In a similar vein, Bhargava and Manoli (2015) show that simplifying information and increasing the saliency of the benefit from information increases the take-up rate of earned income tax credits. Our study goes beyond that by providing advice about strategic behavior.

3. Experimental design and procedures

We design a field experiment to compare the behavior of students in a matching market under different information conditions. That is, we vary the explanation of the allocation procedure and the presence of advice across treatments.

3.1 Preliminaries

Students of an undergraduate introduction to economics class had to write a market structure essay in which they had to answer a series of questions to argue whether the market for a particular product approaches perfect competition, a monopoly or an oligopolistic structure. There were three possible products to write about (smartphone, TV set, and scanner) but other than for the product (or topic) the assignments were identical. More than 700 students were enrolled in the course which was taught across three sections. The essay mark was worth 15% of the final mark for the course.

The main challenge for the design of a field matching experiment is the elicitation of the true preferences of participants. We worked around this limitation in the following way: in Week 5 (starting on April 8) the lecturers announced that the students had to write an essay for which there were three available topics. All students were expected to submit their choice of topic into the online course management system. Thus, students were under the impression that this was their final choice. We have little reason to believe that their submitted choices

were not truthful. Students simply submitted their favorite topic to the system, most likely choosing their real top choice.¹⁰

Our method does not elicit the full preference list of students, but knowing the true top choice allows for a sufficiently rich analysis.

We tried to come up with topics for which student preferences are highly correlated. Our selection achieved the desired correlation in preferences (see Figure 1). As expected, a sizeable majority of students reported the smartphone as their true top choice. Note that this design choice provides a straightforward interpretation of our results in terms of truth-telling, which is the focus of this paper. On the other hand, because the loss of one subject is most likely the gain of another, there would be only modest potential welfare increases achieved by universal truth-telling in our set-up. The lesson to be learnt from our paper is, in any case, one about truth-telling and information. This lesson could be applied to markets with high potential for welfare increases from truth-telling.

3.2 Procedures

The allocation of the topics to students was done through a direct reformulation of TTC for the school choice problem by Abdulkadiroğlu and Sönmez (2003).

Given preferences of students and priorities of schools, TTC works as follows:

“Step 1: Assign a counter for each school which keeps track of how many seats are still available at the school. Initially set the counters equal to the capacities of the schools. Each student points to her favorite school under her announced preferences. Each school points to the student who has the highest priority for the school. Since the number of students and schools are finite, there is at least one cycle. (A cycle is an ordered list of distinct schools and distinct students $(s_1, i_1, s_2, \dots, s_k, i_k)$ where s_1 points to i_1 , i_1 points to s_2 s_k points to i_k , i_k points to s_1 .) Moreover, each school can be part of at most one cycle. Similarly, each student can be part of at most one cycle. Every student in a cycle is assigned a seat at the school she points to and is subsequently removed. The counter of each school in a cycle is reduced by

¹⁰ In previous years there was only one assigned topic and no topic choices. Thus a possibility of learning from previous cohorts, or inferring any experiment-related knowledge, is excluded.

one and if it is reduced to zero, the school is also removed. The counters of all the other schools stay put.

In general, at **Step k**:

Each remaining student points to her favorite school among the remaining schools and each remaining school points to the student with the highest priority among the remaining students. There is at least one cycle. Every student in a cycle is assigned a seat at the school that she points to and is subsequently removed. The counter of each school in a cycle is reduced by one and if it is reduced to zero the school is also removed. The counters of all the other schools remain in place. The algorithm terminates when all students are assigned a seat. Note that there can be no more steps than the cardinality of the set of students.”

In the case of the topic allocation task the modifications are straightforward. Each student has to be assigned to one of the three topics. Additionally, there is a maximum number of students who can be assigned to each of the topics, corresponding to the number of slots in schools in the original formulation.

The priorities of students for topics were generated as an analogue of the district school priority. Every student received a priority for one of the topics. The priority topic was written at the top of the instruction page and was called “Tentative topic.”¹¹ The allocation of tentative topics was random. The ties inside the same priority group as well as ties for non-priority students were broken randomly in the process of the topic allocation and the students were informed about it.

Let us consider an example of the topic allocation problem in the context of our experiment. Imagine the preferences of six students are the following:

	Student 1	Student 2	Student 3	Student 4	Student 5	Student 6
1st choice	Scanner	TV set	Smartphone	Smartphone	Smartphone	TV set
2 nd choice	TV set	Scanner	TV set	TV set	TV set	Smartphone
3 rd choice	Smartphone	Smartphone	Scanner	Scanner	Scanner	Scanner

¹¹ We told the students about their priority class, as we want to replicate the structure of the real school choice problem in our experiment, and the “home school priority” is one of the most important features of it.

The tentative topics are the following:

Student 1	Student 2	Student 3	Student 4	Student 5	Student 6
Smartphone	Smartphone	TV set	TV set	Scanner	Scanner

Then TTC works as follows:

First the ties in priority classes are broken randomly. Imagine the draw when Student 1 has higher priority than Student 2, Student 3 has higher priority than Student 4, and Student 5 has higher priority than Student 6.

1. First round:

- Student 1 points to Student 5, Student 5 points to Student 1.
- Student 3 points to Student 1.

There is one cycle, so the beneficial trades are implemented between Student 1 and Student 5.

2. Second round:

- Student 3 points to Student 2, Student 2 points to Student 3.
- Student 6 points to Student 3.

There is one cycle, so the beneficial trades are implemented between Student 2 and Student 3.

3. Third round: No more quotas are left for the topic Smartphone.

- Student 6 points to Student 4.
- Student 4 points to herself.

Thus Student 4 gets the TV set as a topic.

4. Last round: There is only Student 6 left in the market and one quota for Scanner. She receives this topic.

Thus, the final assignments are:

Smartphone	TV set	Scanner
Student 3	Student 2	Student 1
Student 5	Student 4	Student 6

The three experimental treatments took place at the beginning of the corresponding Week 6 lecture for each of the three sections, exactly one week after the topics had been announced and just a couple of days after the deadline for reporting the choice through the class administration system. At the beginning of the class the lecturer announced that the distribution of submitted choices was skewed too much in favor of one topic (without mentioning which topic) and that there should be an approximately equal division of the topics among students. For that reason he announced that an allocation procedure would be implemented. Then the students had 15 minutes to read the instructions for the allocation mechanism and write down their preference order of the three topics.¹² We distributed the instruction and decision sheets. Students were asked to write their student ID at the top of the sheets.

3.3 Treatments

In all three treatments students received the instruction and decision sheets including their tentative topic.

The mechanism description treatment (MD)

In this treatment the instructions included an explanation of the TTC mechanism framed in the language of the topic allocation problem. We used a formulation similar to Chen and Sönmez (2006). The instructions for all treatments can be found in the online Appendix. The MD treatment is therefore very close to the typical laboratory setup.

¹² The instructions of all treatments, as well as the ranking list, had to be fitted to one A4 sheet (double sided for MD and MPD). Each participant had to read only one or two pages and submit her choice on the same paper sheet. For details check instructions in Appendix.

The properties description treatment (PD)

In this treatment, there is no explanation of the TTC mechanism, but the instruction sheet does include a description of the properties of the mechanism as follows:

“Each participant is first randomly assigned a tentative topic. Your tentative topic is _____ (This assignment is random). You will be asked to submit Decision Sheet rankings, which are used to determine the final allocation. For these purposes we will use the Top Trading Cycles Mechanism.¹³ This mechanism takes into account your preferences and the preferences of others in order to provide as many top choices as possible and it is strategy proof. Thus, every participant has no incentive to misrepresent her preferences, as no matter what other subjects do, she is always better off by submitting true ranking lists.”

The mechanism and properties description treatment (MPD)

This treatment is the aggregation of the two previous treatments. Students received the instructions from MD with a typical TTC explanation and then, just like in PD, received the description of its properties at the end of the instructions.

3.4 Sessions

All the three sessions were run on April 18 and 19, 2013. We ran just one session per treatment, corresponding to one of the three sections. The MD treatment was run at the beginning of the 2pm to 4pm class on April 18. PD was run at the beginning of the 4pm to 6pm class on the same day. The MPD treatment was run the next day at the beginning of the 9am to 11am class. The order of the sessions and the relative short time frame allowed us to assume the minimum possibility of information transfer between students from different sections¹⁴.

¹³ We use the name of the mechanism to sound more scientific for the students, and also to be verifiable. We assume that none of the first-year students are familiar with the mechanism.

¹⁴ The classes of MPD and PD treatments were in the same classroom one after another. There is short break between the end of the first class and the beginning of the second in which students rush to get to their next class. We did not observe any interaction between students of two sections.

Topics were allocated by inputting the submitted rank order lists to our custom-made TTC software and students were notified of their topic assignment on the Monday after the classes, April 22. Those students who did not show up to the class and thus did not submit their rankings were automatically allocated to the under-demanded topic.

A total of 505 students submitted their decision sheets with a rank list. We are able to use only 480 of them as 35 students who submitted a rank list in the classroom had failed to previously submit their favorite topic choice through the online system. As student attendance across sections was not uniform we ended up with 261 observations in MD, 106 in PD, and 113 in the MPD treatment.

3.5 Behavioral predictions

Strategy-proofness predicts that all students should report truthfully and should thus state their online favorite choice according to the online survey as the top choice in the rank list submitted in the classroom.

We believe that the complexity of the class submission task varies remarkably across tentative topics. The students whose tentative topic is their elicited favorite topic face a *trivial* decision which does not require much understanding of the mechanism properties. According to the data submitted online, the smartphone is clearly the most popular topic, thus, getting the smartphone as a tentative topic makes the decision trivial with a high probability.

Students whose tentative topic is the least preferred topic are in a nothing-to-lose situation. It is hard to find a behavioral justification to rank the scanner, the seemingly overall least favorite topic, first in this situation.¹⁵

The decisions of students who received the TV set as a tentative topic are the most interesting from a behavioral perspective. According to the online survey the TV set was the most likely second choice. These students may well be exposed to the kind of trade-off that

¹⁵ That could happen if the student actually likes the scanner best, which is quite unlikely given the survey. Note that students who got the scanner as their tentative choice might still lie about the way they rank the TV set vs the scanner. Our design does not allow for detecting these manipulation attempts. The situation is similar to the design in Guillen and Hakimov (2014) where the local district school was the least preferred school by design and therefore only 2% of subjects did not play the district school bias.

often results in the so-called District School Bias (DSB), see Chen and Sönmez (2006). That is, in the school choice context, ranking the pre-assigned school for which the applicant has a priority higher in the submitted preference list than it is in reality. DSB has been identified as being extremely relevant in most subsequent matching experiments. In our context we will call this behavior *tentative topic bias (TTB)*: if a student did not understand or trust the advice on strategy-proofness, she is likely to think that stating the true ranking list can lead to the loss of the priority for the second best topic and thus risk ending up with the least preferred topic.

Therefore we hypothesize that students with the TV set as their tentative topic are more likely to misreport their top choice when submitting their rank list.

We also hypothesize that the description of properties given to students in MP and MPD should increase the number of truthfully stated top choices by students. Note that in a field experiment such as ours advice comes from a reputable source, the lecturer, and therefore it has a better chance of succeeding than in previous laboratory experiments.

4. Results

Result 1: Across the three treatments, 13.5% of the experimental subjects misreported their top choice. Misrepresentation reached 18.8% in the MD treatment. When taking only into account non-trivial decisions, 20.3% of the subjects misreport. In that case misrepresentation reached 28.1% in the MD treatment.

Support: Table 1 shows the frequency and the corresponding percentage of the misrepresentations of the top choices by treatment. We include both the results for the whole sample and for the non-trivial decisions (see columns in *italics*). The exact Fisher test for the equality of proportions of the students who misrepresent their preferences provides the following p -values for one-sided tests for the full sample: $p = 0.00$ for MD versus PD; $p = 0.01$ for MD versus MPD treatment; $p = 0.26$ for PD versus MPD treatments. If only non-trivial decisions are considered, the exact Fisher test p -values for one-sided tests are as follows: $p = 0.00$ for MD versus PD; $p = 0.00$ for MD versus MPD treatment; $p = 0.41$ for PD versus MPD treatments.

When considering the full sample, the truth-telling rate for the MD treatment is the lowest among our treatments, but it is still higher than in most laboratory experiments. Note that the high truth-telling rate in our experiment is driven by the students facing trivial decisions, ruled out by design in laboratory experiments. Excluding them, misreporting in MD reaches 28%, which is very much in line with results from the laboratory.¹⁶

Result 2: *The vast majority of subjects report a truthful top choice when they face a trivial decision.*¹⁷

Support: Only 3 out of 176¹⁸ students facing a trivial decision misreported their top choice in the experiment. The binomial probability test rejects the null hypothesis that the proportion of representation is higher than 5% ($p=0.02$) and thus we conclude that students reporting under a trivial decision situation is in line with our hypothesis. Trivial decisions are indeed trivial.

Next we look at the truth-telling rates by tentative topics.

Result 3: *The proportion of misreported top choices is the highest among students with a TV set as a tentative topic, the second highest among students who have the scanner as a tentative topic and the lowest among students with the smartphone as a tentative topic. All those differences are significant at the 1% level.*

Support: The last section of Table 2 (rows 13 to 16) presents the number of misreported choices and the proportion of truthful reporting for tentative topics. The exact Fisher test for the equality of proportions of the misreported top choices provides the following p -values for a one-sided test: $p = 0.00$ for smartphones versus TV set; $p = 0.00$ for smartphone versus scanner treatment; $p = 0.00$ for TV set versus scanner.

Thus, we find clear support for our hypothesis: students with the TV set as a tentative assignment are significantly more likely to misrepresent their top choices. Additionally, we are able to differentiate between misrepresentations of students in the form of TTB and other misrepresentations.

¹⁶ Take into account that our design only allows for detecting top choice manipulation.

¹⁷ Note that this result can be seen as a manipulation check for our top choice elicitation method.

¹⁸ Two of these subjects were in the MD treatment and one in the MPD treatment.

Result 4: *TTB explains 78% of all the misrepresentations of top choices. TTB explains five out of five (100%) misrepresentations for the smartphone. TTB explains 41 out of 45 (91%) misrepresentations for the TV set. TTB explains only five out of 17 (29%) misrepresentations for the scanner.*

Support: Column 4 of Table 2 presents the number of misrepresentations when the reported top choice is the tentative topic. In line with our hypothesis TTB most often occurs in the case of the TV set as the tentative topic, as students understand that they can guarantee themselves their most probable second choice by reporting the tentative topic as a top choice, thus escaping the worst option (scanner). The misrepresentations of preferences among students with the scanner as the tentative topic are harder to explain, but most likely they just skip the top choice, hoping that their chances of receiving the second choice are then higher. These students would not like to be assigned to their tentative topic and thus aim for the middle option. However, we cannot claim the latter with certainty as we know only the true top choice of the students.

As previously discussed, the most interesting group of students are those with the TV set as the tentative topic, as they are more likely exposed to TTB. To make a fair comparison, we consider only students for whom the decision is non-trivial, as otherwise the difference among the truth-telling rates could be driven by the unequal distribution of students with trivial situations across the treatments.

Result 5: *The proportion of students who misreport when the TV set is their tentative topic and they face a non-trivial decision, is the highest in MD, the second highest in MPD, and the lowest in PD. All the differences are statistically significant.*

Support: Column 4 of Table 3 reports the percentage of misreported top choices for students with TV set as a tentative topic among students with a non-trivial decision by treatments. The difference in the proportions of the misreported top choices between MD and PD treatments is significant at the 1% level; between MD and MPD treatments at the 5% level; between PD

and MPD treatments at the 10% level (see columns 5–7 of Table 3 for the p-values of one-sided proportion tests).¹⁹

Next we use Probit regressions to test jointly the effects of describing both the properties and the mechanism.

Result 6: *The properties description increases the truthful reporting of the top choice. Conversely, describing the mechanism decreases the truthful reporting of the top choice. When including both variables at the same time, the properties description variable remains significant for the whole sample, while the mechanism description variable remains significant only when the sample is restricted to students with the tentative topic “TV set.”*

Support: Table 4 presents Probit regressions predicting the misrepresentation of the top choice by students under different specifications. We generate two dummy variables. “Properties description” equals 0 for the MD and 1 otherwise. “Mechanism description” equals 0 in PD and 1 otherwise.

Result 6 is the main result of the paper. We show that in our field experiment with student participants, who on average should be much better at understanding the mechanism than the general public, the explanation of the properties does matter for the successful practical implementation of a market. On the other hand, the explanation of the procedures of matching mechanism, the instructions, has a clear negative effect. We conjecture that this effect could be the result of participants being confused, and thus believing they understand more than they actually do. Such individuals could try to outsmart the mechanism even in the presence of advice.

5. Conclusion

We obtained overall high rates of truthful preference revelation in our field experiment. Nevertheless, this result is driven to a large extent by a substantial proportion of participants making a trivial decision. When the decision is non-trivial, that is, when the student is tentatively allocated her second best choice, truthful preference revelation is significantly

¹⁹ *p*-values differ for the Fisher exact test. The comparison of PD and MPD gives a *p*-value of 0.12. We still report the 10% significance of the result due to the high conservatism of the Fisher test.

lower and in line with previous laboratory experimentation. Furthermore, truth-telling in non-trivial decisions does not differ from truth-telling in trivial decisions when only advice about the properties of the mechanism is given. Conversely, truthful preference revelation is much lower in non-trivial decisions when only the mechanism description is provided. We obtain an intermediate result when both mechanism description and properties are provided.

We can therefore conclude that, in the context of our field experiment, providing a description of the mechanism identical to the standard TTC experimental instructions has a detrimental effect on truth-telling. That is, the standard experimental instructions are not transparent, strategy-proofness is hard to infer from them, and confused participants often try to manipulate the mechanism. This is an important finding and thus school districts are right not to mention complicated details, at least for parents who do not request them. The good news is that providing advice about strategy-proofness (properties description) appears to work well, and school districts seem to be getting that bit right too. This result stands in apparent contrast with previous research by Guillen and Hing (2014) and Ding and Schotter (2014a, 2014b), in which correct advice does not have a significant effect on truth telling. We believe that the difference can be explained by the reputation of the source of advice. Indeed, Guillen and Hing (2014) use stylized advice from Internet sources and Ding and Schotter (2014a, 2014b) rely on advice from other participants. In our field experiment students obtain advice from their lecturer, a trustworthy source regarding classroom procedures.

Real-life markets based on strategy-proof mechanisms rely on given advice about strategy-proofness and often avoid describing the mechanism in details. Our result gives strong support to this practice. Most likely, the key to success rests on the reputation of the source of advice. Distrust on the School Board, or more generally on the institution organizing the market and providing advice, may well cause substantial efficiency losses.

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Table 1. Misrepresentation (misrep.) rates by treatment

Treatment	Sample size	Top choice misrep.	% of misrep.	<i>Non-trivial decisions</i>	<i>Top choice misrep.</i>	<i>% of misrep.</i>
MD	261	49	18.8%	167	47	28.1%
PD	106	6	5.7%	63	6	9.5%
MPD	113	10	8.8%	74	9	12.1%
Total	480	65	13.5%	304	62	20.3%

Table 2. Summary of submitted choices

	<i>MD</i>	N	Number of misrepresentations of the top choice	Number of students affected by TTB	Proportion of truth
1	Smartphone	85	4	4	95.29%
2	TV set	93	31	30	66.67%
3	Scanner	83	14	5	83.13%
4	Total	<i>261</i>	<i>49</i>	<i>39</i>	<i>81.23%</i>
	<i>PD</i>	N	Number of misrepresentations of the top choice	Number of students affected by TTB	Proportion of truth
5	Smartphone	37	1	1	97.30%
6	TV set	40	3	3	92.50%
7	Scanner	29	2	0	93.10%
8	Total	<i>106</i>	<i>6</i>	<i>4</i>	<i>94.34%</i>
	<i>MPD</i>	N	Number of misrepresentations of the top choice	Number of students affected by TTB	Proportion of truth
9	Smartphone	35	0	0	100.00%
10	TV set	40	9	8	77.50%
11	Scanner	38	1	0	97.37%
12	Total	<i>113</i>	<i>10</i>	<i>8</i>	<i>91.15%</i>
	<i>All treatments</i>	N	Number of misrepresentations of the top choice	Number of students affected by TTB	Proportion of truth
13	Smartphone	157	5	5	96.82%
14	TV set	173	43	41	75.14%
15	Scanner	150	17	5	88.67%
16	Grand Total	<i>480</i>	<i>65</i>	<i>51</i>	<i>86.46%</i>

Note: *N* in the second column represents the number of students with a given tentative topic.

Table 3. Preference reporting for students with TV set as the tentative topic, non-trivial decisions

Treatment	Students with non-trivial decisions	Number of misreported top choices	Percent of misreported top choices	Proportion (Fisher exact) test p-value versus MD	Proportion (Fisher exact) test p-value versus PD	Proportion (Fisher exact) test p-value versus MPD
MD	73	30	41%		0.00 (0.00)	0.04 (0.07)
PD	30	3	10%	0.00 (0.00)		0.07 (0.12)
MPD	33	8	24%	0.04 (0.07)	0.07 (0.12)	

Note: Column 4-6 present the test for equality of proportion of truth-telling rates by treatments. One-sided p-values of the proportion test are presented, followed by one-sided p-values for the Fisher exact test for the equality of proportions in parenthesis.

Table 4. Probit regression for misreported top choices

Dummy for misreported top choice	Aggregated data			Tentative assignment is TV-set		
	(1)	(2)	(3)	(4)	(5)	(6)
Properties description	-0.605 ^{***} (0.167)		-0.506 ^{**} (0.201)	-0.629 ^{***} (0.225)		-0.355 (0.264)
Mechanism description		0.608 ^{***} (0.230)	0.234 (0.277)		0.944 ^{***} (0.33)	0.693 [*] (0.380)
Trivial situation	-1.318 ^{***} (0.25)	-1.308 ^{***} (0.250)	-1.322 ^{***} (0.251)	-1.104 ^{***} (0.360)	-1.115 ^{***} (0.371)	-1.123 ^{***} (0.369)
Constant	-0.594 ^{***} (0.102)	-1.334 ^{***} (0.214)	-0.828 ^{***} (0.294)	-0.257 [*] (0.146)	-1.303 ^{***} (0.307)	-0.948 ^{**} (0.405)
N	480	480	480	173	173	173
log L	-161.98	-165.00	-161.62	-86.98	-86.14	-85.22
Pseudo R ²	0.15	0.13	0.15	0.10	0.11	0.12

Note: Values in parentheses represent standard errors. * p<0.1, ** p<0.05, *** p<0.01

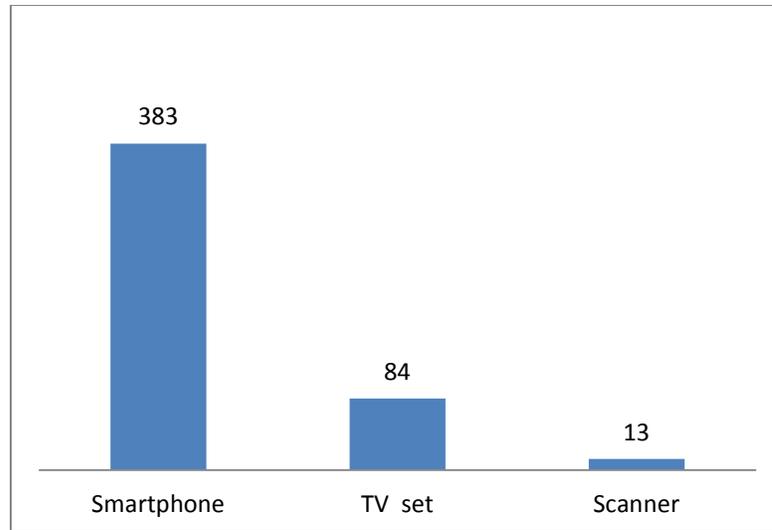


Figure 1: Distribution of favorite topics