The Effectiveness of Top-Down Advice in Strategy-Proof Mechanisms: 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 property 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 negative effect on truth-telling.

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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, Abdulkadiroğlu 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 mechanisms, in which participants have the right incentives to reveal their true preferences, 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 preferences of participants in matching markets are hard to elicit. That is why researchers turned to run matching experiments with induced valuations.[1]

Chen and Sönmez (2006) pitched two theoretically strategy-proof mechanisms, Top Trading Cycles (TTC) and Deferred Acceptance (DA) against the most popular mechanism in use at that time, 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 high er truth-telling rates for strategy-proof mechanisms than for BOS, 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, attentive reading of the experimental matching literature raises some doubts about the capacity of theoretical strategy-proof mechanisms to generate high truth-telling rates. While strategy-proof mechanisms lead to higher truth-telling than BOS, absolute rates of truthful reporting vary a lot, and drop when more information is made available to subjects, as shown by Pais and Pintér (2008) for information about preferences and priorities of other students, by Guillen and Hakimov (2017) for information about others' strategies. Hakimov and Kesten (2014) document truthful reporting for TTC as low as 30% in particular environment under complete information on preferences and priorities of all students in the market. 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. That is, there is a growing stream of the experimental matching literature eroding the idea of truth-telling driven by

[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%.

subjects' understanding strategy-proofness from reading the instructions and working on the examples provided in the laboratory.[3]

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, 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 mechanism mechanics. In New Orleans, for instance, only a sketch of TTC's principles was made available to the public through a poster published by the local newspaper. Conversely, experimental subjects 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 mechanism descriptions (the last time we checked), but they both do inform participants that the best they can do is to report their true preferences. Note that explaining the properties of the mechanisms in the lab might help to overcome the gap with the field, and 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, while the right advice does not significantly increase the truth-telling rate observed in their baseline. 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.[4] Thus, the findings about the effect of the advice in the lab are controversial. Moreover there is a concern that the advice from the experimenter in the lab could easily lead to methodological problems like demand effects and/or confusion.

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: only "mechanism description" (MD), only "properties description" (PD), and both

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

[4] Recent literature on advice includes testing the advices given not by the experimenter but by the peers. According to Ding and Schotter (2017) chatting between to subsequent DA matching markets has only a limited effect on the increase of truth-telling rates. Ding and Schotter (2015), find that after 20 rounds of intergenerational advice truth-telling decreases dramatically from above 70% to about just 45%. In contrast to Ding and Schotter (2015), Zhu (2015) shows that intergenerational advice might increase truth-telling rates in the simplest market of three agents and three objects in lab.

"mechanisms and properties descriptions" (MPD). The aim of the experiment is to assess which informational structure generates the highest truth-telling rate.

We test TTC and not DA for a number of reasons. First of all, the setup is ideal for the use of an efficient mechanism with coarse priorities. Moreover, around the time we run our experiments, New Orleans adopted TTC and San Francisco approved to adopt it. So it looked like TTC was in the way to finally become a plausible alternative to DA in the field. Market designers only advised DA for Boston as a second best, but the local authorities thought otherwise. See Abdulkadiroglu et al. (2017) for a summary of the TTC vs DA debate in the US. Finally, there is a renewed interest in TTC coming from Europe. Namely, recent empirical evidence from Barcelona by Calsamiglia et al. (2017) suggests that TTC is a better alternative to BOS than DA. TTC also outperforms DA in terms of providing access to better schools than the one in the catchment area according to Calsamiglia and Miralles (2017).

Our experiment took place at the University of Sydney, with first-year students of an introductory microeconomics course as participants.[5] The students had to write an essay about the structure of one of three markets: smartphones, TV sets or scanners. We simply elicited students' 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 TTC[6] leads to a significantly higher rate of truthful reporting. However, a description of the mechanism itself decreases truthful reporting for the most relevant subsample of participants, those who have a priority for what is most likely their second favorite topic.

2. 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.

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

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

[6] We use "properties description" and "advice" as interchangeable terms.

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.

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.[7] Our method did 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, as 383 out of 480 students chose smartphone as a topic, 84 chose the TV set, while only 13 chose the scanner. Note that this design choice provides a straightforward interpretation of our results in terms of truthful reporting of the top choice, which is the focus of this paper. In what follows, we refer to these choices as to true top choices of subjects.[8] 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 truthful reporting and information. This lesson could be applied to markets with high potential for welfare increases from truthful reporting.

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) modified for the topic allocation task. Each student had to be assigned to one of the three topics. Additionally, there was a maximum number of students who could 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."[9] 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.

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

[7] In previous years there was only one assigned topic and therefore no topic choice.

[8] Some subjects may be indifferent between topics. Nevertheless the distribution of top choices suggests that indifference is not likely to be relevant. If all students were indifferent we would expect to observe similar number of submitted choices across the three topics.

[9] We told the students about their priority class, as we wanted 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. As part of the experiment participants in all treatments were informed by the instructor that their tentative topic was randomly allocated and that the procedure was meant to potentially improve the allocation.

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.[10] We distributed the instruction and decision sheets. Students were asked to write their student ID at the top of the sheets.

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.

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 mechanism's properties 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.[11] 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.

All 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

[10] 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). Note that the instructions did not include a solved example in any case. 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.

[11] 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.

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 sections.[12]

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 25 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. 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.[13] The decisions of students who received the TV set as a tentative topic are the most interesting from a behavioral perspective, as TV set was the most likely second most preferred topic for the majority of participants.[14] These students may well be exposed to the kind of trade-off that 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

[12] 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.

[13] 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 (2017) where the local district school was the least preferred school by design and therefore only 2% of subjects submitted in line with the district school bias.

[14] This is in line with the distribution of choices in the survey and also with the fact that out of 343 submitted list which did not have TV as a top choice, 322 list TV as the second choice.

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.

4. Results

Result 1 (Treatment differences): *Across the three treatments, the rate of misrepresented top choice in MD is significantly higher than in PD and MPD. The proportion of students who misreport their top choice 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, with all differences being statistically significant (the difference between MPD and PD is significant only at 10% level).*

Table 1 shows the frequency and the corresponding percentage of the misrepresentations of the top choices by treatment and tentative topics. Overall 13.5% of the experimental subjects misreported their top choice: 18.77% in the MD treatment, 5.66% in PD treatment, and 8.85% in MPD treatment. First, we run one-way ANOVA to test a null hypothesis of equality of mean representation rates in all three treatments. The test rejects null hypothesis with $p < 0.01$. As for the pairwise comparisons, the rate of top choice misreporting is significantly higher in MD than in PD and MPD.[15]

Despite being the highest among our treatments, the rate of top choice misreporting in MD is still lower than in most laboratory experiments. Note that in our experiment 176 out of 480 students face trivial decisions, and only three of them misreport top choice. Trivial decisions are ruled out by design in laboratory experiments, and excluding them, misreporting in MD reaches 28%, which is very much in line with results from the laboratory.[16]

Next we look at the rates of top choice misreports by tentative topics. 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. The proportions top choice misreports are different for different tentative topics (one-way ANOVA $p < 0.01$), and all pairwise differences are significant.[17] Thus, we find clear

[15] 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: $p < 0.01$ for MD versus PD; $p < 0.05$ for MD versus MPD treatment; $p > 0.1$ for PD versus MPD treatments.

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

[17] The exact Fisher test for the equality of proportions of the misreported top choices provides the following *p*-values for a one-sided test for the pairwise comparisons: $p < 0.01$ for smartphones versus TV set; $p < 0.01$ for smartphone versus scanner treatment; $p < 0.01$ for TV set versus scanner.

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: out of 65 top choice misreports in all treatments, 51 are in form of TTB. This observation motivates the next focus of the analysis on the subsample of the subjects.

Table 1. Summary of submitted choices

MD	***Tentative topic***	N	Number of misrepr. of the top choice	Number of students affected by TTB	% of misrep
	Smartphone	85	4	4	4.71%
	TV set	93	31	30	33.33%
	Scanner	83	14	5	16.87%
	Total in MD	*261*	*49*	*39*	18.77%
PD	***Tentative topic***	N	Number of misrepr. of the top choice	Number of students affected by TTB	% of misrep
	Smartphone	37	1	1	2.70%
	TV set	40	3	3	7.50%
	Scanner	29	2	0	6.90%
	Total in PD	*106*	*6*	*4*	5.66%
MPD	***Tentative topic***	N	Number of misrepr. of the top choice	Number of students affected by TTB	% of misrep
	Smartphone	35	0	0	0.00%
	TV set	40	9	8	22.50%
	Scanner	38	1	0	2.63%
	Total in MPD	*113*	*10*	*8*	8.85%
All Treatments	***Tentative topic***	N	Number of misrepr. of the top choice	Number of students affected by TTB	% of misrep
	Smartphone	157	5	5	3.18%
	TV set	173	43	41	24.86%
	Scanner	150	17	5	11.33%
	Grand Total	*480*	*65*	*51*	13.54%

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

As previously discussed, the most interesting group of students is the one for which the TV set is 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.

Column 4 of Table 2 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 proportions top choice misreports are differ between the three treatments (one-way ANOVA $p < 0.01$). The differences in the

proportions of the misreported top choices between MD and PD; and between MD and MPD are significant. The difference between PD and MPD is significant at the 10% level, see columns 5–7 of Table 2 for the *p*-values of one-sided proportion tests. A conservative Fisher exact test is still significant for the first two differences, but the *p*-value increases to 0.12 when comparing PD with MPD. In summary, the description of the properties has a clear and positive effect on truth telling rates. Describing the mechanism itself does not, at the very least, increase truthful reporting and it may increase misreporting.[18]

Table 2. 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 test (Fisher exact test) *p*-value versus MD	Proportion test (Fisher exact test) *p*-value versus PD	Proportion test (Fisher exact test) *p*-value versus MPD
MD	73	30	41%		$p < 0.01$ ($p < 0.01$)	$p < 0.05$ ($p < 0.1$)
PD	30	3	10%	$p < 0.01$ ($p < 0.01$)		$p < 0.1$ ($p > 0.1$)
MPD	33	8	24%	$p < 0.05$ ($p < 0.1$)	$p < 0.1$ ($p > 0.1$)	

Note: Columns 4-6 present the test for equality of proportion of truth-telling rates by treatments. One-way ANOVA to test a null hypothesis of equality of mean proportion in all three treatments rejects null hypothesis with p=0.00. 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.

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

Result 2: *According to the Probit regression models, describing the properties of the mechanism significantly increases the truthful reporting of the top choice. Describing the mechanism has negative and significant effect on the truthful reporting of the top choice for students with TV set as a tentative topic, controlling for trivial decisions and the description of the mechanism's properties.*

Table 3 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 2 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

[18] Despite of the fact that the harm from the misreporting for efficiency is limited in our study by design, we can calculate consequences of the misreporting in each treatment from individual perspective. The number of misreporting students who would have received their true top choice had they submitted truthfully, is 27 in MD, three in PD, and six in MPD.

public, the explanation of the properties does matter for the successful practical implementation of the mechanisms. On the other hand, the explanation of the procedures of matching mechanism, the instructions, has a negative effect on truth-telling. We conjecture that this effect, driven by TTB, could be the result of participants being confused, and thus believing they understand more than they actually do. Such individuals could possibly try to outsmart the mechanism even in the presence of advice.

Table 3. Marginal effects of Probit regressions for misreported top choices

Dummy for misreported top choice	Entire sample			Only students with TV set as tentative assignment		
	(1)	(2)	(3)	(4)	(5)	(6)
Properties description	-0.09***		-0.08**	-0.18***		-0.10
	(0.03)		(0.03)	(0.06)		(0.07)
Mechanism description		0.08***	0.03		0.21***	0.17**
		(0.02)	(0.04)		(0.05)	(0.07)
Trivial situation	-0.18***	-0.18***	-0.18***	-0.24***	-0.24***	-0.24***
	(0.02)	(0.02)	(0.02)	(0.05)	(0.05)	(0.05)
N	480	480	480	173	173	173
log L	-162.0	-165.0	-161.6	-87.0	-86.1	-85.2
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. Variable "Properties description" equals 0 for the MD and 1 otherwise. "Mechanism description" equals 0 in PD and 1 otherwise.

5. Conclusion

We obtained overall high rates of truthful preference revelation in our field experiment. This result is nevertheless 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 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.

That is, in the context of our field experiment, providing a description of the mechanism identical to the standard TTC experimental instructions has negative effect on truth-telling although this effect is only significant for the most relevant subsample, participants who got the most likely second-best topic, the TV set, as their tentative allocation and therefore fall prey to what in our context we call tentative topic bias (TTB). This result supports the idea that the standard experimental instructions are too difficult to understand and thus strategy-proofness is hard to infer from them. Therefore confused participants may try to manipulate the mechanism. It follows that school districts may be right in not to mention

complicated details in their websites, at least for parents who do not request them.[19] The good news is that providing advice about strategy-proofness (properties description) appears to work very well. School districts seem to be getting that bit right. This result stands in apparent contrast with previous research by Guillen and Hing (2014), 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. In our field experiment students obtain advice from their lecturer, the most trustworthy source regarding classroom procedures. In the case of this experiment, the so-called "demand effect" is also not a concern, as students did not know that their decisions were collected also for research purposes.

Real-life markets based on strategy-proof mechanisms rely on given advice about strategy-proofness and often avoid describing the mechanism in details. Our results support this practice. If anything, the key to success rests on the reputation of the source of advice.

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[19] Note that most school districts, with the possible exception of San Francisco, will provide the details of the mechanism to parents who request them. In light of our results we believe this is a sensible way to proceed.

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