Liar Liar: Experimental evidence of the effect of confirmation-reports on dishonesty.

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Abstract

We identify the effect of confirmation-reports on dishonesty using data from an experiment where subjects are asked to roll a die and report its outcome using either a self-report or confirmation-report mechanism. We find that relative to self-reports, confirmation-reports have a positive effect on the share of subjects who report honestly. The effect on the magnitude of lies told depends greatly on the accuracy of the pre-filled information on the confirmation-report. We argue that these results are driven by changes in the intrinsic costs of lying induced by the confirmation report.

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1 Introduction

People often choose to misreport their private information in order to improve their own monetary payoffs. Common examples include used-car markets, auto-insurance, and tax returns. This behavior often occurs in response to one of two types of request for information. An individual may be asked to provide a self-report (e.g., what is your taxable income?) or a confirmation-report (e.g., is your taxable income $50,000?). Because both requests ask for the same information - the person's taxable income -, we would expect a person's response to be independent of the form of the request. However, to the extent that intrinsic lying costs are affected by the form of information request, we speculate that dishonesty will be different between self-reports and confirmation-reports.[1] Confirmation-reports may affect several sources of intrinsic lying costs including moral-lying costs (how I see my self), social-lying costs related to changes in perception of the requester's information set (how others see me), and lying costs related to the mental burden of devising a lie.

Despite a very extensive literature on lying, there is very little empirical evidence on the relative effect of these two types of reports on dishonesty (see discussion below). We contribute to the literature by identifying the effect of confirmation-reports on dishonesty. We answer the following specific research questions; are people more likely to respond honestly to confirmation-reports than self-reports? Also, do confirmation-reports affect the magnitude of lies told? Because it is difficult to observe lying in historical data, we answer our research questions with data collected from a field-experiment. We follow the traditional dishonesty literature in that we ask subjects to report the outcome of a six-sided die roll (Fischbacher and Föllmi-Heusi 2013; Ruffle and Tobol 2014; Hao and Houser 2013). Both the roll and reporting are done in private with no opportunity for either action to be observed by the experimenter. Subjects are paid according to their reported outcome, and their pay-offs are increasing in the reported die roll; with the exception of six, which receives a pay-off of zero. Therefore, subjects have an incentive to lie. Over $1,400$ subjects are recruited from Amazon's mechanical turk, and are randomly assigned to one of seven groups. Subjects in group one are asked to make a self-report, while subjects in the remaining groups are asked to respond to a confirmation-report. The confirmation-reports are pre-filled with a number between one and six, depending on the group, and subjects can either accept the report or reject it. Those who reject the

[1] Recent empirical evidence shows that intrinsic lying costs matter (Fischbacher and Föllmi-Heusi 2013; Ruffle and Tobol 2014; Abeler et al. 2014; Gneezy 2005), and that these lying costs can be manipulated in order to affect dishonesty (Shu et al. 2012; Erat 2013; Conrads et al. 2013). The form of information request will most certainly affect dishonesty via changes in extrinsic costs. For example, the pre-filled information on a confirmation-report might affect the respondent's subjective assessment of the probability of being caught and thus the resulting expected penalty (an extrinsic cost of lying). We emphasize intrinsic-lying costs here because our analysis takes place in a context that is devoid of any extrinsic costs.

report are given the opportunity to report any other number between 1 and 6.

We find evidence that confirmation-reports increase honesty when we pool the data from the confirmation treatments. The share of honest subjects is 44 percent in the self-report treatment compared to 63 percent in the pooled-confirmation treatment. This suggests that approximately 19 percentage points of the subjects in our sample are conditionally honest. There does not appear to be any effect on the share of maximal liars in the pooled data; 15 percent and 18 percent in self and confirmation reports, respectively. Looking at each confirmation treatment separately reveals some important findings. First, the share of honest subjects in the confirmation-report treatments is always greater than in the self-report treatment; the effect ranges from 5 percentage points to 18 percentage points. This suggests, if anything, that confirmation-reports have a positive extrinsic-margin effect. Second, we find that the effect on the magnitude of lies told (intrinsic-margin) depends on the accuracy of the pre-filled number. At one extreme, asking subjects to confirm rolling a six, which has a pay-off of zero, increases the share of zeroes by approximately eight percentage points and reduces the share of threes and fours by three percentage points each; there is no effect on the share of fives. At the other extreme, asking subjects to confirm rolling a five, which has the highest pay-off, increases the share of subjects who lie maximally by approximately seven percentage points, while reducing the shares of ones and fours by four percentage points, respectively. Lastly, we find that females are more honest than males, and that females are less likely to lie maximally under the confirmation report while there is no effect on maximal lying among males.

Given the design of our experiment, we argue that these results are driven by intrinsic costs broadly defined to include moral lying-costs and lying costs related to the mental burden of whether or not and how much to lie. First, we argue that an accurately pre-filled confirmation-report increases moral lying-costs and thus reduces dishonesty since subjects who face an accurate confirmation-report must reject the true report and then file a false report. Concurrently, a favorably inaccurate pre-filled confirmation-report reduces moral lying-costs and thus increases dishonesty since subjects can 'shift' the burden of lying onto the person who prepared the form. Second, the decision to lie is mentally burdensome since it generally involves two steps; the person must decide 1) whether or not to lie and 2) the magnitude of the lie she wants to tell. The mental burden associated with these decisions is likely to be especially high given that liars generally want to maintain the perception of honesty (Greenberg et al. 2015; Hao and Houser 2013). In cases like these, people often turn to rules-of-thumb, which makes them susceptible to decision-making biases. Therefore, a subject might more readily accept an accurate or favorably inaccurate confirmation-report because it is easier than having to make the lying decision herself.

The presence of pre-filled information on confirmation-reports might also change respondents' beliefs about the requester's information set.[2] Therefore, it is also possible that our results are partly driven by another type of intrinsic cost related to the shame of being identified as a liar. However, the experiment is conducted in the field, and while subjects know they are participating in an academic study, they do not know that the die-rolling exercise is part of an experiment. Furthermore, we emphasize the fact that we have no way of observing their die roll. This suggests that our results are not being driven by such social costs related to changes in perception about the experimenters' information set.

Our paper makes several contributions to the academic literature and policy discussions. First, to our knowledge, we are the first to identify the effect of confirmation-reports on dishonesty in an environment where extrinsic costs do not matter. Fonseca and Grimshaw (2015) and Kotakorpi and Laamanen (2015) estimate the compliance effect of administering an income tax with pre-populated tax forms rather than blank tax forms using data from a field experiment and a natural experiment, respectively.[3] Both studies find that a favorably inaccurate pre-filled tax form increases noncompliance on the non-pre-filled items. This finding is similar to our result where overstating the outcome of the die roll increases the share of subjects who confirm that die roll. Because these studies focus on tax evasion, the observed responses are driven by both extrinsic (audit and fine) and intrinsic costs. Unlike these two papers, we are able to show that confirmation-reports affect dishonesty even in the absence of extrinsic costs. This is particularly important in the context of tax evasion where existing evidence shows that compliance is generally much higher than can be explained by extrinsic costs alone (Alm 2012). Our results suggest that pre-populated tax forms are likely to influence compliance by changing the intrinsic motivations for paying taxes.

We also add to the extensive literature on lying. Existing studies have explored several important aspects of dishonesty including the role of shame (Greenberg et al. 2015), guilt (Battigalli et al. 2013; Charness and Dufwenberg 2006), gender (Dreber and Johannesson 2008; Muehlheusser et al. 2015), magnitude of pay-off (Fischbacher and Föllmi-Heusi 2013; Suri et al. 2011), time preference (Ruffle and Tobol 2014), education (Ruffle and Tobol 2016), morality (Shu et al. 2012), and lying costs more broadly (Abeler et al. 2014; Gneezy 2005; Kajackaite and Gneezy 2015). A common feature of all of these

[2]On the one hand, a correctly pre-filled report might cause people to believe that their private information is known to the requester. In this case, the social cost of lying increases because the person believes her decision to be dishonest will be observed by the requester; i.e., she will be a known liar. On the other hand, an inaccurate confirmation-report might confirm the respondents belief that the requester does not know the respondents' true information. In this case, the social cost of lying decreases because the decision to lie remains unknown to the requester.

[3]Pre-populated tax forms require taxpayers to complete their annual tax reconciliation by filing a form that is pre-filled with information available to the tax administrator, and is therefore comparable to a confirmation-report.

studies is that they depend on subjects making self-reports using blank forms. We add to this literature by showing that the form of the request matters. Requesting information via confirmation-reports influences lying costs and thus increases or decreases dishonesty depending on the accuracy of the pre-filled information on the reports. This finding is similar to studies that show that dishonesty can be influenced by changing lying costs either by having people commit to telling the truth (Shu et al. 2012), delegating the reporting responsibility (Erat 2013) or team incentives (Conrads et al. 2013).

Understanding the effect of self and confirmation reports on dishonesty is important for everyday life. It is particularly relevant in instances where people are hoping to obtain correct information but the provider faces little or no extrinsic costs. For example, used-car markets and students giving excuses for missing exams or assignments. Other applications include state-provided services for which there are no penalties for lying.[4] There is also an obvious application to tax administration.[5] However, we are careful in extrapolating our results to this context because cheating on taxes is a risky decision that includes extrinsic costs. Still, our results suggest that the type of form used in the reconciliation process matters for compliance, and that the effect of the form will depend crucially on the accuracy of the information available to the administrator. Therefore, our findings support the strategy of limiting pre-populated tax-forms to taxpayers whose income sources are easily verifiable.[6]

The remainder of the paper is organized as follows. First we describe the experimental design. This is followed by a description of our results, discussion and conclusion.

2 Design and Implementation

Our primary objective is to determine if, relative to self-reports, confirmation-reports lead to more liars and larger lies in a context where extrinsic costs do not matter. This section describes the experimental design used to answer our research question.

[4] One example would be parks for which entry fees are conditional on location of residence. Example, the Eagle Creek Park in Indianapolis, Indiana charges a lower fee for residence of Marion county. However, there is no process in place to verify patrons' county of residence.

[5] While most tax administrators implement tax reconciliations with blank-tax forms, others use pre-populated forms. Pre-populated tax forms are used in Finland, Denmark, Sweden, Norway. The UK is currently considering the adoption of pre-populated tax forms. Although there has been discussions about the merits of a pre-populated tax form in the US, the State of California is the only US government to have implemented them under their ready-return initiative. ReadyReturn has since been cancelled, but its primary feature – prepopulating tax forms with pre-existing information – is still available with CalFile.

[6] Of course, this strategy does not preclude the possibility of increased non-compliance among taxpayers who have only verifiable income in the current period, but earn income from non-verifiable sources in the future. Confirmation-reports could also increase noncompliance by inducing taxpayers to reduce their verifiable income and increase their non-verifiable income.

2.1 Experimental Design

The experiment is completed in two steps. First, we instruct subjects to roll a six-sided die and make a note of the number thrown on their first attempt. Subjects are told that they can throw the die as many times as they want, but only the first throw counts. Second, subjects are asked to report the outcome of their die roll. The subjects are told that we have no way of knowing the outcome of the die roll, and that their payment will be based solely on the number they report following the schedule in Table 1.[7] The information in Table 1 is shown to the subjects twice; when they read the instructions for the experiment and again when they make their reporting decisions.

This design conforms with the previous literature; e.g., Fischbacher and Föllmi-Heusi (2013), Ruffle and Tobol (2014). The main difference between our design and that of the existing literature is in the way subjects are asked to report the outcome of the die roll. The previous literature simply asked subjects to self-report the outcome of the die roll. We add to this by also asking some subjects to provide confirmation-reports. Subjects are randomly assigned to one of seven groups; one self-report treatment and six confirmation-report treatments. The only difference between the self-report treatment and the confirmation-report treatment is in step two where subjects are asked to report the die roll outcome.

Self-Report Treatment. Subjects in the Self Report Treatment (SRT) are shown an empty text box and are asked to report the outcome of their first die roll (see Figure 2). We again show subjects Table 1 so that the relationship between the reported number and the bonus payment remains salient. We also remind them that we have no way of knowing the number that they actually rolled on the die. The session ends when a subject submits the reported die roll.

Confirmation-Report Treatment. Subjects in the Confirmation Report Treatments (CRT) are randomly assigned to one of six sub-groups. Subjects in each group are shown a text box that is pre-filled with a fixed number; either 1, 2, 3, 4, 5 or 6 depending on their assigned group (see Figure 3). In other words, every subject assigned to CRT0 is shown a text box that is pre-filled with the number 6. Similarly, every subject assigned to CRT5 is shown a text box that is pre-filled with the number 5, and so on. Subjects are asked to confirm whether the pre-filled number is the number on the face of their first die roll. The pre-filled number is reported and the session ends if they respond 'yes'. Subjects who respond 'no' are given an opportunity to self-report a different number.

[7]Notice that the payment for rolling a six is zero as in Fischbacher and Föllmi-Heusi (2013). Because the pay-off for rolling a six is zero, we recode the outcome six to be zero. This adjustment is made to the data for all tables and figures reported in the paper.

We use a between-subject design, where each subject only participates in one of the seven treatments. Each session of the experiment is a one-shot game.

2.2 Implementation

Because the design is simple and can be completed in only a few minutes, we implement the experiment online with the aid of Amazon's Mechanical Turk (Mturk).[8] Mturk is an online labor market where job offers are posted and workers choose jobs for payment. It has numerous benefits for running experiments, including access to a large stable subject pool, diverse subject background, and low costs (Mason and Suri 2012; Horton and Chilton 2010; Paolacci et al. 2010). Furthermore, the behavior of online workers has been shown to be comparable to those of subjects in laboratory studies (Buhrmester et al. 2011; Paolacci et al. 2010; Horton et al. 2011; Suri et al. 2011).

We create a survey-based human intelligence task (HIT) on Mturk (see Figure 1) and provide a link to our own website where subjects are able to complete the survey and a bonus task.[9] Following the survey, subjects are asked to complete a bonus task, which involves rolling a six-sided die and reporting the outcome. They are told that the amount of the bonus payment is determined by the reported die roll according to Table 1. A link to the website random.org, which allows people to roll virtual die, is embedded into our website. Subjects are instructed to click on the link in order to roll the die, and to return to our website to report the outcome of the die roll. We make it explicitly clear that our website is not affiliated with random.org, so the actual outcome of the die roll is not known to us. Subjects are told that we can only observe the number that they report on our website. Subjects are shown an ID code after submitting their reported die roll, and are told to report this code on the Mturk website in order for us to process their bonus payment.

Mturk is especially advantageous for our study because subjects can be assured that we have no way of telling whether they are lying or not. Subjects complete the tasks in their own environment immune to the influence from other participants or the experimenters. Therefore, the design does not involve any social interactions. Additionally, the experiment does not include a compliance mechanism; i.e., no audit, no penalty, and no opportunity for the experimenter to observe a subject's actions. This implies that the actual and expected extrinsic costs of lying are zero. We argue that these two characteristics of the experimental design implies that the decision to be dishonest in our

[8] Subjects took an average of 9 minutes to complete both stages of the experiment, and 50% of subjects completed the experiment in less than 5 minutes and 20 seconds.

[9] We created the external website for the sole purpose of hosting the survey and bonus task. The survey includes questions about road mileage user-fees, number of miles driven, age, gender, race, and education. Subjects are paid a flat participation fee for completing the survey. The experiment was done in December 2015 and January of 2016.

experiment depends only on a tradeoff between the external monetary benefits and the internal costs of the dishonest act.

3 Results

This section begins with a description of our data. We then describe dishonesty in the self-report treatment, which serves as our baseline. Next we describe dishonesty in the confirmation-report treatments. We then describe additional analyses that account for gender effects as well as dishonesty among subjects whom we know rolled the virtual die versus those who probably did not roll the die.

3.1 Data

Summary Statistics A total of 1475 subjects participated in the experiment; 203 in the Self-Report Treatment and 1272 in the Confirmation-Report Treatment. Subjects took an average of 9 minutes to complete the survey and bonus task, and were paid an average of \$0.81; this is equivalent to an hourly wage of approximately \$5.40. Our hourly wage is comparable to that of other surveys conducted on Mturk (e.g., Kuziemko et al. (2015) paid \$6.00 per hour), and much higher than the estimated reservation wage of Mturk workers; \$1.38 (Horton and Chilton 2010). Further, the average effective hourly wage on Mturk is estimated to be \$4.80 according to Amazon.

Table 2 shows that the two treatments are balanced with respect to age, race, and education. The self-report treatment has slightly more females compared to the confirmation-report treatment, but the difference is only marginally different from zero at the 10% level (Wilcox ranksum test pvalue =0.092).

Uniformity of Random.org Since subjects rolled a virtual die through random.org, it is important to establish that the virtual die on this website is fair. Because each group has 203 to 225 observations, we provide evidence that the virtual die is fair by collecting the outcome of approximately 210 die rolls from random.org and present the resulting distribution in Figure 4. The evidence presented in Figure 4 shows that the virtual die is fair. We cannot reject the null that the distribution obtained from random.org is uniform: pvalue from Kolmogorov-Smirnov two-sample test is 0.97 and pvalues from two-sided binomial tests that the share of each reported number is equal to 16.67% are all greater than 0.2. We take this as evidence that each of the observed distributions from our experiment should be uniform if subjects report honestly.

3.2 Self-report Treatment Results (SRT)

Result 1. *We find strong evidence for an overall pattern of dishonesty among subjects. However, there is a non-trivial share of unconditionally-honest subjects.*

The distribution of reported die rolls in Panel A of Figure 5 indicates the presence of lying in SRT. Subjects report low-paying numbers at frequencies below 16.67% and report high-paying numbers at frequencies above 16.67%, which is indicative of dishonesty. A Kolmogorov-Smirnov two-sample test confirms that the observed distribution is statistically different from a uniform distribution ($pvalue < 1\%$). This is further supported by a two-sided binomial test that the share of subjects reporting each number is equal to 16.67%; we can reject the null that the share of reported 0, 2 and 5 is 16.67% at the 1% level. However, we cannot reject that the share of 1, 3 and 4 is 16.67%. This finding implies that some subjects lied about their die roll and reported higher numbers than what they actually rolled.

Since there is no incentive for people to lie for lower payoffs, it is reasonable to assume that subjects who report a zero payoff are honest. In our experiment, 7.4% of subjects in SRT report a die roll of 6, which results in a zero payoff. Following Fischbacher and Föllmi-Heusi (2013), we use this information to estimate the percentage of unconditionally honest subjects to be approximately 44% ($= 7.4\% * 6$).[10] In other words, we find that 44% of subjects are honest despite the fact that lying is both unobservable and profitable. The SRT results also provide evidence of maximal lying. We find that 32% of the subjects in SRT report a payoff of 5, which is statistically greater than the expected probability of 1/6. This implies that some subjects lied to maximize their payoffs. Since there is no incentive for subjects who rolled 5 to report a lower payoff, we estimate the percentage of maximal liars to be 18% ($= (32 - 16.7) * (6/5)$).

3.3 Confirmation-report Treatment (CRT)

Recall that the confirmation treatment is divided into six sub-groups where every subject in a given sub-group is asked to confirm the same pre-filled number. Therefore, we are able to pool the data and analyze the confirmation treatments as a single group, and we can analyze each sub-group separately. The analysis below exploits this flexibility in our data. We first present the pooled results where we compare the aggregate CRT distribution to that of SRT. We then disaggregate the CRT results into its component groups and compare each sub-group to SRT.

[10] This is based on the assumption that unconditionally honest subjects in fact rolled a uniform distribution of numbers. To the extent that some subjects reported a six despite rolling another number, then this estimate represents an upper bound on the share of unconditionally honest subjects.

3.3.1 Pooled-CRT Results

Result 2. *The pooled-CRT data indicates the presence of dishonesty.*

Panel B of Figure 5 shows the distribution of reported payoffs in the pooled CRT data. We again find strong evidence of lying. A Kolmogorov-Smirnov two-sample test confirms that the observed distribution is statistically different from a uniform distribution ($pvalue < 1\%$). Additionally, using a two-sided binomial test, we are able to reject the null that the share of subjects reporting each number (except 3) is equal to 16.67% at the 1% level. This finding implies that some subjects lied about their die roll and reported higher payoffs than what they actually rolled.

Result 3. *The pooled CRT data suggest that, relative to SRT, confirmation-reports increase the share of honest subjects, but have limited effect on the share of maximal liars.*

An ocular comparison of Panels A and B of Figure 5 shows that the distribution of reported outcomes is very similar across treatments. A Kolmogorov-Smirnov two-sample test confirms that the observed CRT distribution is not statistically different from the SRT distribution ($pvalue = 0.98$).[11]

An alternative way of defining the treatment effect is to subtract the share of subjects who report each number in SRT from the respective share in CRT; this is illustrated in Figure 6. We find that the confirmation report increases the share of reported zeroes and twos, and reduces the share of ones, threes and fives.[12] These differences are not statistically distinguishable from zero according to ranksum tests ($pvalues > 0.167$), but some of them are economically meaningful. For example, following the same procedure as in section 3.2, we estimate the share of honest subjects in the pooled-CRT data to be approximately 63% ($= 10.56 * 6$). Given that subjects are randomly assigned to groups, the share of unconditionally honest subjects should be the same as in SRT; i.e., 44%. This implies that the confirmation-report induced an additional 19 percentage points of subjects to make honest reports, which is a fairly large extensive-margin response. We refer to these subjects as conditionally honest subjects; i.e., honest conditional on our treatment. Our treatment effect on the share of maximal liars is only 3 percentage points; from 18% to 15%. We acknowledge that these calculations rely on strong assumptions, and we present a more careful discussion of the impact on the share of honest subjects

[11]We also compare the SRT distribution to the distribution produced by various combinations of the CRT sub-groups. For example, we generate two CRT distributions - one based on reports of subjects who are asked to confirm large numbers (3, 4 and 5) and another based on reports of subjects who are asked to confirm small numbers (0, 1, and 2) -, and compare them to the SRT distribution. We cannot reject the null that each of these CRT distributions is similar to the SRT distribution; Kolmogorov-Smirnov two-sample test $pvalues > 0.2$ in all cases.

[12]The impact on reported fours is negligible; less than 0.5 percentage points.

in section 3.3.2.

3.3.2 Disaggregated CRT Results

This section describes the disaggregated CRT results, which allows us to identify the treatment effect of CRT conditional on the pre-filled number.

Result 4a. *The disaggregated CRT data show that dishonesty persists regardless of the pre-filled number.*

Figure 7 presents the distribution of reported payoffs for each CRT sub-group.[13] A Kolmogorov-Smirnov two-sample test confirms that the observed distributions in Figure 7 are statistically different from a uniform distribution ($pvalues < 5\%$), which suggests the presence of lying across all groups. Visual inspection suggests that dishonesty is influenced by the pre-filled number. For example, the share of maximal liars is highest in Panels A, B and F, where subjects are asked to confirm zero, one, and five, respectively. We also observe that the share of partial liars (based solely on the share of reported fours) is highest in Panels C, D, and E where subjects are asked to confirm two, three and four, respectively. Additionally, the observed distribution that most closely resembles a uniform distribution is Panel A where subjects were asked to confirm rolling a 6. Only the reported share of twos and fives in Panel A of Figure 7 can be distinguished from 16.67% according to a two-sided binomial test, while at least four of the six bars in the remaining panels of Figure 7 can be distinguished from 16.67%.

Result 4b. *The share of subjects confirming a pre-filled number increases in the pay-off associated with that number.*

Figure 7 also shows that the share of subjects confirming a given pre-filled number is increasing in the pre-filled number, and almost always greater than the expected share of 16.67%. To see this more clearly, we present - in Figure 8 - the share of subjects in each group who confirm the number they are shown on their pre-filled report. We refer to these subjects as *confirmers*. Figure 8 shows clearly that subjects are more likely to confirm a one than a zero, and more likely to confirm a two than a one, and so on. Notice that this finding is inconsistent with honest reporting. In other words, the share of confirmers should be equal to 16.67% if subjects are reporting honestly.

Result 4c. *The share of subjects confirming a pre-filled number is greater than the share of subjects who self-report that same pre-filled number.*

Relative to SRT (see panel A in Figiure 5), Figure 8 shows that the share of subjects confirming a given pre-filled number is greater than the share of subjects who self-report

[13] Recall that the only difference between sub-groups is the prefilled number on the confirmation report.

that same number. To illustrate this treatment effect more clearly, we compare each CRT sub-group in Figure 7 to SRT in Panel A of Figure 5. For example, we identify the treatment effect of CRT0 by subtracting the share of subjects who self-report each number in Panel A of Figure 5 from the share of subjects who report the respective number in Panel A of Figure 7. We repeat this for each panel in Figure 7 in order to identify the treatment effect of the other pre-filled numbers. The results from this exercise are reported in Figure 9; each panel reports the treatment effect for a different CRT sub-group.

Figure 9 shows that relative to SRT, confirmation reports increase the share of subjects who report a given pre-filled number by 6.5 to 11.8 percentage points across all groups. These effects are both economically meaningful and statistically different from zero, and is further evidence that confirmation reports influence reporting behavior.

Result 5. *The extensive margin effect is always positive while the intensive margin response varies across sub-groups.*

An interesting question to ask at this point is whether or not confirmation-reports affect the share of honest subjects, and the share of partial and maximal liars. The results in Figure 9 shed some light on this question. First, we find evidence of a positive extensive-margin effect; the confirmation-reports caused an increase in the share of honest subjects. We estimate the impact on the share of honest subjects in two ways. The first approach uses the same procedure as in section 3.2. In other words, we use the share of reported zeroes in each treatment to determine the share of honest subjects. Using this approach, we estimate the share of honest subjects to be 50 to 62 percent in the confirmation treatments, which represents an 11% to 41% increase over the share of unconditionally-honest subjects in the self-report treatment.

While informative, the estimates above are likely biased since they ignore the fact that subjects are more likely to report the pre-filled numbers. The second approach attempts to account for this response by estimating the extensive-margin effect in only CRT0 where identification is cleaner. The results in panel A show that CRT0 increases the share of honest subjects by approximately 8 percentage points.[14] The treatment effects in Panel A of Figure 9 show that the share of zeroes increases, the share of threes and fours decreases, and there is no economically significant effect on any other number. Since no one has an incentive to report a smaller number than she actually rolled, this suggests

[14]This is based on three reasonable assumptions regarding behavior in Panel A; 1) the share of unconditionally honest subjects is 7.4 percent, just as in SRT, 2) the confirmation-report doesn't affect the propensity to report any of the other numbers honestly, and 3) subjects who report a zero payoff are honest. The first assumption follows from randomization of subjects into groups. The fact that the share of zeroes in each panel of Figure 9 is not statistically different from 7.4% is further evidence that the share of unconditionally honest subjects is at least 7.4% across all groups. The second assumption is supported by the treatment effects in Panel A of Figure 9.

that the confirmation report in CRT0 reduces the share of partial liars, and increases the share of subjects who report honestly by 8 percentage points.[15] As in section 3.3.1, we refer to these subjects as conditionally honest. Together with the unconditionally honest subjects, this implies that the total share of honest subjects in CRT0 is 52%.

The treatment effect in each of the remaining confirmation-report treatments reflect both extensive and intensive margin responses, and it is difficult to separate the two because we do not know the true outcome for each subject. For example, we observe a significant reduction in the share of reported threes and fours coupled with an increase in the share of reported ones in CRT1 (Panel B of Figure 9). This suggests that subjects who would have self-reported threes and fours report a one when presented with a CRT1. This behavior is consistent with two possible explanations related to extensive and intensive margin lying-responses. On the one hand, it is possible that subjects who rolled a one and would have lied by self-reporting a three now report honestly, thus reflecting an extensive margin response. On the other hand, CRT1 could also reflect an intensive margin response in the sense that subjects who rolled a six now lie less intensely by reporting one instead of a three. Our data do not allow us to distinguish between these two possible effects, but we suspect that some combination of the two is at play. In either case, this represents an increase in conditional honesty of approximately 7 percentage points.[16]

Similarly, the results in CRT2, CRT3 and CRT4 show that these treatments have the dual effect of increasing the magnitude of the lies told by some subjects while reducing the magnitude of the lies told by others. For example, CRT4 reduces the share of ones, threes and fives, and increases the share of fours. This implies that subjects who would have self-reported ones and threes now report four thus increasing the magnitude of lies told. Simultaneously, the share of subjects who would have lied maximally by self-reporting five, now lie partially or do not lie at all (in the case of subjects who actually rolled a four) by reporting a four instead.

Another case worth examining is CRT5 (Panel F of Figure 9), which reduced partial lying and increased maximal lying, but had negligible effects on the share of unconditionally-honest subjects. This is evident in the noticeable increase in the share of reported fives and reduction in share of reported ones and fours with no noticeable effect on the share of reported zeroes. This suggests that subjects who would have only partially lied, are now induced to become maximal liars, whereas the unconditionally honest subjects are unaffected. However, it is possible that conditionally-honest subjects were induced to become liars. For example, subjects who reported a four honestly in

[15]The reduction in partial liars is approximately 8%, which suggests that roughly one half of the subjects who rolled a six reported rolling a 3 or a 4 in SRT.

[16]Note that we are implicitly defining a reduction in the intensity of lying as an increase in conditional honesty.

the self-report treatment because of the relatively high pay-off, may have been induced to report a five in the confirmation-report treatment. Therefore, while we believe the increase in the share of reported fives reflect an intensive-margin effect, we cannot rule out the possibility that it also captures an extensive-margin response.

3.4 Rollers

Subjects who are committed to lying might find it pointless to roll the die since their reported outcome will be independent of the die roll. However, it is usually impossible to tell if subjects rolled the die because they make their die roll and reporting decisions in private. This information is important in our context since it is possible that subjects who are committed liars respond differently to the treatment than other subjects. Therefore, we designed the experiment such that we can identify subjects who clicked on the die-roll link. We refer to those subjects who clicked on the link as 'rollers', and those who did not click on the link as 'non-rollers'.[17] This section describes how our results are affected when we cut the data by roller status.

Table 4 shows that approximately 36% of subjects in both SRT and CRT are non-rollers; i.e., they did not click on the die-roll link before making their reporting decision. With the exception of CRT1 (30%) and CRT2 (40%), this share is fairly flat across CRT sub-groups. Additionally, there is no difference in the likelihood of being a non-roller across gender or race. Younger subjects are more likely to be non-rollers, but the estimated effect is not economically meaningful. The only demographic characteristic that is strongly correlated with non-roller is education level; the likelihood of being a non-roller decreases with education level.[18] These differences are not worrisome since the treatment groups are balanced with respect to education level. However, given that each group has approximately 200 observations, the number of non-rollers ranges from 62 to 84. As a result, we urge caution when interpreting the results in this section.

Result 6. *Subjects who clicked on the die-roll link are more honest than subjects who did not click on the link.*

Figure 10 presents the distribution of reported die rolls for SRT and pooled CRT conditional on roller status. Although the observed distributions are all statistically different from a uniform distribution, the results show clearly that rollers are more honest than

[17]We are only able to tell if the link was clicked; we cannot observe the outcome of the die roll. Also, we cannot rule out the possibility that non-rollers actually rolled a die. For example, it is possible that they simply bypassed our link to the virtual die, or that they rolled their own physical die. As mentioned before, our inability to observe the die outcome was made clear to subjects.

[18]The share of non-rollers among subjects with a graduate degree is 25%. Relative to graduate degree holders, subjects with a bachelors degree, some college, and high school diploma are 11%, 10% and 15%, respectively, more likely to be non-rollers. These differences are both economically and statistically significant.

non-rollers.[19] In particular, relative to non-rollers, the share of honest subjects is higher and the share of maximal liars lower among rollers. We find that the share of honest subjects in SRT is 51% and 32% among rollers and non-rollers, respectively. Corresponding numbers for CRT are 67% and 56% among rollers and non-rollers, respectively. The share of maximal liars is $13.4(= 23.87 - 10.46)$ points higher among non-rollers in SRT and $6.66(= 16.96 - 10.3)$ points higher among non-rollers in pooled CRT. The difference in propensity to lie is also apparent in Figures 11 and 12, which report results for rollers and non-rollers, respectively. Again, while the observed distributions among rollers suggests the presence of lying, we find even stronger evidence of lying among non-rollers.

Result 7. *The estimated treatment effect of CRT is much stronger among non-rollers.*

The results presented in Figure 13 show that non-rollers are not only more dishonest, they are also more responsive to the treatment. Relative to SRT rollers, CRT rollers reduce the share of reported ones and increase the share of reported zeroes; there is no effect on the other reported numbers. As with the main results, this finding suggest that CRT increases honesty among rollers. We find a similar, but much larger effect among non-rollers. In particular, there is a large reduction in the share of fives and threes, and a correspondingly large increase in the share of zeroes and twos. Therefore, it appears that the CRT also increased honesty among non-rollers. However, this interpretation depends on the motivation for not clicking the die-roll link. On the one hand, if non-rollers rolled an actual die, then the interpretation is that CRT increased honesty among non-rollers. On the other hand, if non-rollers did not roll a die, this behavioral difference across treatments cannot be directly interpreted as an increase in honesty since the subjects themselves do not know the outcome of the die roll. Nonetheless, the fact that some non-rollers report a zero in CRT suggests that they are possibly more likely to report a zero in CRT had they rolled the die and observed a zero. In other words, we suspect that CRT would increase honesty among non-rollers had they been forced to observe the outcome of an actual die roll.

As in section 3.3.2, the results presented in Figures 14 and 15 show that the treatment effect depends on the pre-filled number. Subjects are more likely to confirm the pre-filled number than to self-report that same number. However, this effect is much stronger among non-rollers.[20] Figure 14 also shows that, relative to SRT, a confirmation-report pre-filled with zero or one has very little effect on the reporting behavior of rollers; differences are indistinguishable from zero. At the same time, a pre-filled report with two, three or four reduces maximal lying while a report pre-filled with five increases maximal lying. We find somewhat similar results for non-rollers in Figure 15. Pre-filling the

[19] Kolmogorov-Smirnov two-sample test confirms that the observed distribution from both SRT snd CRT are statistically different from a uniform distribution ($pvalue < 1\%$).

[20] The effect is only distinguishable from zero for pre-filled number zero, three, four, and five.

report with a number less than five reduces maximal lying, but by larger amounts compared to rollers. Interestingly, pre-filling the report with a five increases maximal lying by a smaller amount compared to rollers; approximately 5.5 points among non-rollers compared to approximately 9 points among rollers.

Overall, the results show that while the treatment affected both groups in similar ways, it had a much larger effect on non-rollers. We discuss the implications of this finding for our main results in section 4.

3.5 Gender Effect

A common finding in the literature is that males tend to be more dishonest than females (Erat 2013; Muehlheusser et al. 2015; Dreber and Johannesson 2008; Childs 2012). We explore whether this finding holds in our setting. First we note that while each group has more males than females, gender is fairly balanced within and between groups (see Table 3).

Result 8. *Men are more dishonest than women in both SRT and CRT.*

Figure 16 shows the observed distributions by gender for SRT and CRT. Visually, the distributions suggest the presence of lying. Except for the SRT female distribution, a Kolmogorov-Smirnov two-sample test rejects the null that the distributions are uniform ($pvalues <= 1\%$); the pvalue for the SRT-female distribution is 0.16. The non-uniformity of the distributions is confirmed by a binomial two-sided test that the share of subjects reporting each number is 16.67%.[21]

These two results confirm the presence of lying among both males and females. Next we confirm that dishonesty differs across the groups by rejecting the null that CRT-female distribution is similar to the CRT-male distribution (Kolmogorov-Smirnov two-sample test pvalue=0.05). Further evidence of greater dishonesty among males is presented in Figure 17, which shows that females are more likely to report smaller numbers compared to males.

Gender differences are also obvious in each subgroup of the CRT data. Both females and males lie regardless of the pre-filled number on the confirmation report (see Figures 18 and 19). Additionally, Figure 20, which reports - for each CRT subgroup - the difference between the share of females reporting a given number and the share of males reporting that same number, confirms our earlier finding that females are more likely to report smaller numbers compared to males.

Over all, then, our results suggest that relative to males, females are either more

[21] We find that females are more likely to report the theoretically expected share of each number compared to males in the SRT group. There is no gender difference in the propensity to report the theoretically expected share of each number in the CRT group.

likely to report honestly and/or more likely to tell smaller lies.

Result 9. *The treatment effect of CRT varies across gender. In particular,*

- *CRT has a larger treatment effect on the share of conditionally honest subjects among males than among females.*
- *CRT has a larger treatment effect on share of maximal liars among females than among males.*
- *CRT reduces the share of partial liars among males, but increase the share of partial liars among females.*

Relative to males, the share of unconditionally honest subjects in SRT is approximately 18 percentage points higher among females: 54% and 36% for females and males, respectively. The share of honest subjects (conditional and unconditional) in the pooled-CRT treatment is 68% and 59%, for females and males, respectively. Therefore, while females are more unconditionally honest, they are less likely to be induced to become honest by the treatment. This is evident by the fact that the treatment effect on conditional honesty is 22.8 percentage points among males compared to 14 percentage points among females.

Additionally, CRT affects partial and maximal lying differently across gender. On the one hand, CRT increases partial lying and reduces maximal lying among females. On the other hand, CRT reduces partial lying and has no effect on maximal lying among males.

4 Discussion

The empirical results described in section 3 show that approximately 44% of subjects are unconditionally honest. We also find that only 18% of subjects lied maximally, which suggests that the majority of liars do so partially. Importantly, we estimate that the CRT has an economically meaningful effect on the share of subjects who report each number, but the magnitude depends greatly on the pre-filled number in the CRT. This section places our findings to the context of the existing literature and offers possible explanations for the observed treatment effects.

4.1 Relation to literature

Our baseline findings reported in section 3.2 are consistent with the existing literature (Fischbacher and Föllmi-Heusi 2013; Ruffle and Tobol 2014; Abeler et al. 2014; Suri et al. 2011; Hao and Houser 2013). A particularly useful point of comparison is Fischbacher and Föllmi-Heusi (2013) whose baseline-experimental design is similar to ours. They estimate that their sample includes 39% unconditionally honest subjects, 22% maximal liars, and a non-trivial amount of partial lying. Our results are largely comparable to theirs, which is reassuring on two levels. First, though use of the mTurk subject pool has grown significantly in recent years, there still remains some skepticism. Therefore, the fact that Fischbacher and Föllmi-Heusi (2013) baseline results are replicated in the mTurk sample ought to increase confidence in our results. Second, mTurkers generally complete HITs for very low wages and as such the stakes in our experiment are low relative to the existing literature. For example, ignoring exchange rate differences, the stakes in Fischbacher and Föllmi-Heusi (2013) are ten times as high as in our experiment. Still, our baseline results are very similar to theirs, which suggests that the size of the stake is not that important in matters of dishonesty (at least not in the context that we explore).[22]

The greater honesty that we observe in our experiment is expected since studies conducted in the field generally find higher levels of honesty; e.g., Abeler et al. (2014) conduct a coin-toss experiment and find no evidence of lying in the field but significant amounts of lying in the lab. We also note that our treatment effects are consistent with Fonseca and Grimshaw (2015) who find that correctly pre-populated tax returns have a small positive effect on compliance relative to a blank return. Like us, they also find that pre-populating a return with too low values (similar to overstating the die outcome in our case) increases dishonesty.

4.2 Channels

Again, the novelty of our experiment is that we test whether confirmation-reports cause subjects to become more or less honest in an environment where there are no extrinsic lying costs. Our results suggest that, overall, confirmation reports increase honesty. However, we also observe that the effect of CRT depends on the the pre-filled information and whether or not the subject clicked our roll-die link. We argue that this heterogenous effect is due to intrinsic costs broadly defined to include moral lying-costs and decision-making costs related to the mental burden of whether and how much to lie.

Moral Lying-costs Dishonesty is morally costly for individuals because they must violate their moral standards in order to engage in a dishonest act (Bandura 1990; Bandura 1996; Aquino and Reed II 2002). We argue that confirmation-reports affect the moral cost of lying by changing the salience of subjects' own moral standards. For example, a subject who faces an accurate confirmation-report has to engage in two distinct dis-

[22]Fischbacher and Föllmi-Heusi (2013) came to a similar conclusion after tripling the stakes in their experiment and finding that the higher stakes did not affect their results.

honest acts if she wishes to lie; she must reject the true statement, then report a false statement. These two decisions have a greater effect in activating her moral standards relative to a self-report, and thus increases the moral costs of lying. On the other hand, CRT reduces the moral-lying costs of subjects who are asked to confirm an advantageous false statement because they can shift the blame of lying to the person who prepared the confirmation report without necessarily activating her own moral standards. For example, Erat (2013) finds that people are more likely to lie if the burden of lying can be shifted to a delegate. We argue that these opposing effects on the moral cost of lying is a possible explanation for our results.

To illustrate, let us take to the two extreme pay-off numbers of zero (lowest pay-off) and five (highest pay-off). CRT subjects who roll a zero and are asked to confirm a zero face higher lying costs relative to subjects who rolled a zero in SRT. We would therefore expect an increase in the share of reported zeroes in CRT relative to SRT, and this is exactly what we find. Importantly, because there is no incentive to report a zero unless it is the truth, we can interpret the increase in the share of reported zeroes as an increase in honesty. Similarly, CRT subjects who rolled a number less than five, but are asked to confirm rolling a five, face lower lying costs relative to SRT subjects who rolled a number less than five. Therefore, we would expect an increase in the share of reported fives relative to SRT. Again, this is what we find. Because subjects do not have an incentive to report a number lower than their actual roll, we argue the treatment effect for reported fives reflects an increase in dishonesty.

We can use the same logic to explain the responses for the intermediate numbers one to four. Consider, for example, the number three. We can classify subjects who are asked to confirm the number three into four bins: 1) those who roll a number lower than three, and would self-report a number lower than three, 2) those who roll a number lower than three, but would self-report a number greater than or equal to three, 3) those who roll the number three, and 4) those who roll a number greater than three. Pre-filling the confirmation report with the number three reduces the lying cost for subjects in the first two bins and increases the lying cost for those in the third bin. The impact on lying costs for those who roll a four is unclear. This suggests that the share of reported three should increase, which is what we observe, and that this effect is driven by the response of subjects in the first three bins. The greater share of threes reflects an increase in lying among subjects in the first bin since they experience lower lying costs. It also possibly includes a reduction in the magnitude of the lie told by subjects in the second bin. In other words, subjects who roll a one and would have self-reported a five now report a three; they still lie, but the magnitude of the lie is smaller. Finally, the effect of pre-filling the report with the number three includes increased honesty among subjects who actually roll a three because these subjects experience higher lying costs.

All together, the combination of effects discussed above makes it less surprising that the pooled-CRT and SRT distributions look so similar. In other words, the pooled results reflect a combination of increased lying and increased honesty.

Decision-Making Costs Another possible explanation for our findings is that subjects simply accept the number that is pre-populated because it is the default setting of the confirmation report. Economists have highlighted in numerous areas, including health care plans (Samuelson and Zeckhauser 1988), automobile insurance (Johnson et al. 1993), retirement saving plans (Madrian and Shea 2001), and organ donor registration (Johnson and Goldstein 2003; Li et al. 2013), that people often choose the default option they are assigned in order to reduce decision-making costs. Because it is less mentally burdensome to accept the default number shown on the confirmation report than to decide on a number to report, we would expect subjects who roll a number $x \leq y$ to be more likely to confirm rolling y than self-reporting y. We argue that this channel is especially relevant among non-rollers. To the extent that non-rollers did not in fact roll a die, they need to decide which number to self-report in order to maximize their pay-off while maintaining a semblance of honesty.[23] The default option presented in the confirmation-report simplifies this decision-making calculus by allowing the subject to simply accept the pre-populated number. Unlike rollers, non-rollers do not have an actual die outcome (x) with which to compare the pre-populated number (y). Therefore, it is not surprising that, relative to rollers, non-rollers are more likely to accept y, thus leading to much larger treatment effects among non-rollers.

Shame Another possible explanation for our result is that it is driven by shame aversion induced by a change in subjects' beliefs of the researchers' information set. The argument goes as follows. Upon seeing a pre-filled number, subjects update their beliefs of the accuracy of the researcher's information set regarding the actual die outcome. Subjects who see a pre-filled number equal to their actual die roll now believe that the researcher knows the actual outcome, and will therefore report accurately. Subjects who see a pre-filled number that is different from their actual die roll will confirm their prior belief that the researchers do not know the actual outcome, and are therefore more likely to confirm a favorable false statement. Note that in both cases the effect of the updated beliefs on lying works through its effect on intrinsic costs only.[24] In other words, subjects wish to lie, but would like to appear honest in order to avoid the shame of being known as a liar (Hao and Houser 2013; Greenberg et al. 2015). As a result, subjects who believe the

[23]Recall that we can only tell if the subject clicked ont he die-roll link. We cannot tell if they roll their own personal die or a virtual die on some other website.

[24]Recall that there is no audit and no penalty for lying. Therefore, even if a subject believes that the researchers knows her actual die roll, it is still profitable to lie since there are no extrinsic costs associated with lying.

researchers information set is correct are more likely to confirm an accurately pre-filled confirmation report while those who believe the researchers information set is inaccurate are more likely to confirm a favorably inaccurate confirmation report.

We argue that this mechanism is an unlikely reason for our results given the design of the experiment. First, we make it explicitly clear to subjects that our website is not connected to random.org in anyway, and that we cannot observe their die rolls. Second, subjects know that we only observe their mTurk ID and therefore have no way of knowing any of them personally. Third, the actual reported outcomes appear to be inconsistent with this explanation. For example, if pre-filling the form with say three, fours and fives lead subjects who roll a zero to believe we do not know their actual outcomes, then we would expect to observe a significant drop in the share of reported zeroes in treatments CRT3, CRT4 and CRT5 relative to SRT and relative to CRT0, CRT1 and CRT2. However, the share of reported zeroes is basically constant across all of these treatments.

Finally, we also rule out this alternate explanation based on the fact that subjects who did not click on the "die roll " link embedded in our website responded similarly to those who did click on the link. Subjects' belief of the researchers' information set should be unaffected by the pre-filled number for subjects who did not click on the "die roll" link. This is because these subjects either rolled a physical die, rolled a virtual die on some other website, bypassed our weblink in order to roll the die on random.org, or did not roll a die at all. In either case, these subjects should be reassured that the researchers do not know the actual outcome of the die roll, and so, their beliefs of the researchers' information set should be independent of the pre-filled number. This suggests that the treatment effect of CRT0 on the share of subjects who confirm rolling zero should be lower among non-rollers relative to rollers. Similarly, the treatment effect of CRT5 on the share of subjects who confirm rolling five should be higher among non-rollers relative to rollers. However, this is not what we observe. In fact, the treatment effect of CRT0 on confirmed zeroes is much higher among non-rollers, while the treatment effect of CRT5 on confirmed fives is lower among non-rollers. This suggests that a change in beliefs about the probability of being observed is not the driving factor.

Of course we cannot rule out the possibility that different subjects are influenced by different factors. For example, it is possible that non-rollers are influenced by decision-making costs while rollers are mostly influenced by moral costs with some residual social (shame) costs.

4.3 Policy Implications

Our findings are important for policy areas where information requests can reasonably take the form of either the self-report or confirmation-report; e.g., tax administration.

Several countries have adopted pre-populated tax forms, and others are considering adoption. It is generally argued that a pre-populated tax system lowers compliance costs and thus increases tax compliance. However, there is very little empirical evidence on the compliance effects of pre-populated tax systems; Fonseca and Grimshaw (2015) provide experimental evidence for the UK and Kotakorpi and Laamanen (2015) evaluate the effect of Finland's switch to pre-populated returns. While our experiment is not framed as a study of tax compliance, the results do provide some insights into the possible effects of pre-population since pre-populated tax systems rely on confirmation-reports.[25] For example, we show that the effect of confirmation-reports on dishonesty (and hence tax compliance) depends greatly on the accuracy of the pre-populated information. Correctly pre-populated forms will increase compliance while forms that understate income will increase non-compliance. Of course, the importance of accuracy is not a new finding.[26] What is novel in our setting is that we show that this compliance effect exists even in the absence of extrinsic evasion costs; i.e., costs related to being audited and fined. We argue that this effect is driven by changes in the intrinsic costs of lying that are induced by the confirmation-report; while accurate information increases intrinsic lying costs, inaccurate information reduces intrinsic lying costs.

Another application closely related to our field experiment context is the reporting of annual vehicle miles traveled (VMT) for auto-insurance policies. Auto insurers often ask policy holders to self-report their annual VMT and use this information in pricing insurance policies. Those who drive more tend to have a higher accident risk and are therefore charged higher insurance premiums. Therefore, policyholders have an incentive to underreport their VMT. [27] Insurance companies can rely on several techniques to obtain accurate mileage information.[28] One suggested technique uses two steps. First, known information about customers is used to predict their annual mileage; e.g., gender, age, education, employment commute times, vehicle type, and vehicle age. Second, customers are asked to confirm that the predicted mileage is correct using confirmation reports. The effect of this approach on underreporting depends on the accuracy of the predicted mileage. Assuming that the predicted mileage is fairly accurate, our findings suggest that this approach could significantly reduce the under-reporting problem.

A similar strategy might also prove useful if road mileage user-fees become widely

[25]Recall that our experiment does not include any extrinsic costs. Therefore, our findings/policy implications are based solely on the direct effect of the form, and excludes any indirect effects that might occur through perceived (or actual) changes in extrinsic costs.

[26]Almost all countries with a pre-populated tax system either limit to their system to (or began with) taxpayers with only wage income that is easily monitored for accuracy.

[27]According to a 2008 report by Quality Planning company, the car insurance industry lost $15.9 billion of which more than $3 billion is due to underreported vehicle mileage.

[28]These include asking the customer, asking the agent, historical odometer readings, mileage models and telematics using an after-market device, smartphone OEM-based or smartphone odometer app (Cantwell 2015).

adopted. Ideally, a road mileage user-fee would be implemented with the aid of advanced technology that accurately measures vehicle mileage. However, due to costs and privacy concerns, there has been some resistance to mileage user-fees and the use of advanced technology in particular (Duncan et al. 2014). Because of these costs and privacy concerns, administration with odometer readings is generally included as an optional way of collecting mileage information. While odometer reading largely solves the privacy and cost problems, it raises concerns about evasion. Our results suggest that obtaining a fairly accurate estimate of vehicle miles traveled coupled with a confirmation report is likely to reduce non-compliance. This could serve to reduce the number of required audits and hence the cost of administration.

5 Conclusion

We implement a field experiment to study the effect of confirmation reports on dishonesty. Subjects are recruited from Amazon's Mechanical Turk labor pool. Each subject is asked to roll a virtual die and report the outcome of the die roll either by completing a self-report or confirmation report. We find strong evidence of lying in both treatments. Although differences across the two treatments are not statistically significant, results from the pooled-confirmation-report data suggest that the confirmation report induced an additional 19 percentage points of subjects to report honestly. We also find that the effect of confirmation report is heterogenous across pre-filled number, gender, and roller status. Overall, we find that people are more likely to confirm the truth than to self-report the truth, and more likely to confirm a lie than to tell a lie. We attribute our findings to intrinsic costs which are influence through the impact of confirmation reports on moral-lying costs and decision-making costs.

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6 Tables

Table 1: Bonus Pay-off Schedule

Number reported	1	2	3	4	5	6
Resulting pay-off	\$0.10	\$0.20	\$0.30	\$0.40	\$0.50	\$0.00

Table 2: Summary of Demographic Variables

	SRT		CRT		Pvalue	Total	
Variables	Mean	N. Obs	Mean	N. Obs		Mean	N. Obs
Age	34.5	203	34.6	1268	0.85	34.6	1471
Female (%)	50.0	201	44.0	1258	0.09	44.8	1459
White (%)	83.0	203	79.0	1272	0.20	80.0	1475
Wage (\$)	0.82	203	0.81	1269	0.33	0.81	1472
Education							
Graduate Degree	8.4	17	10.8	137		10.4	154
Bachelors	37.4	76	39.4	501		39.1	577
Some College	37.4	76	35.9	457		36.1	533
High School	16.8	34	13.9	177		14.3	211

Notes: SRT is self-report treatment, and CRT is confirmation-report treatment. Pvalue is from the Wilcox-ranksum test for differences in means.

Table 3: Distribution of Gender across treatments

Treatment	Male	Female	Obs
SRT	49.8	50.3	201
CRT (pooled)	56.1	43.9	1258
CRT Sub-groups			
CRT:0	52.5	47.5	223
CRT:1	59.4	40.6	207
CRT:2	63.2	36.8	201
CRT:3	50.8	49.3	199
CRT:4	53.7	46.3	216
CRT:5	57.6	42.5	212
Total	55.24	44.76	1459

Notes: SRT is self-report treatment, and CRT is confirmation-report treatment.

Table 4: Share of Die Rollers

	Non-rollers		Rollers		
Die	Percent	Obs	Percent	Obs	Total
SRT	36.5	74.0	63.6	129.0	203
CRT (Pooled)	35.22	448	64.78	824	1272
CRT Sub-groups					
CRT:0	36.89	83	63.11	142	225
CRT:1	29.81	62	70.19	146	208
CRT:2	41.18	84	58.82	120	204
CRT:3	33.66	68	66.34	134	202
CRT:4	36.53	80	63.47	139	219
CRT:5	33.18	71	66.82	143	214

Notes: SRT is self-report treatment, and CRT is confirmation-report treatment. Rollers is a binary variable indicating that the subject clicked on the die-roll link. Non-rollers did not click on the die-roll link.

7 Figures

Figure 1: mTurk HIT

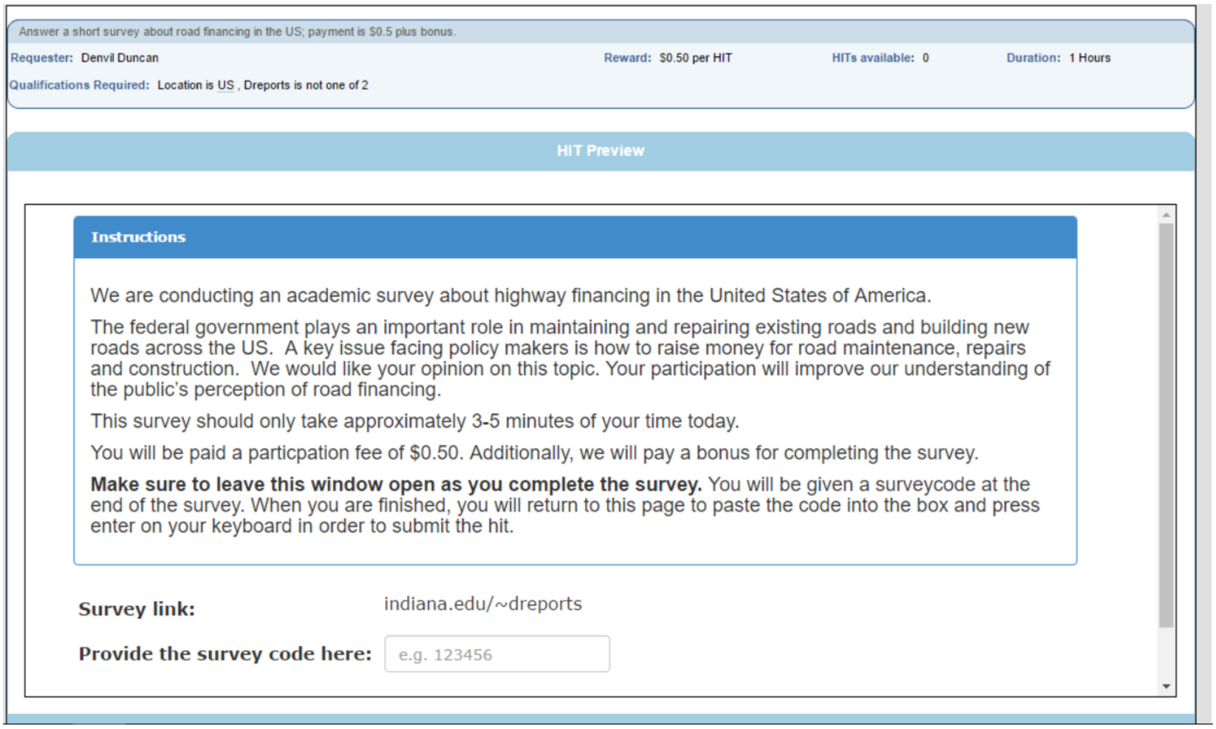


Figure 2: Die reporting screenshot for Self-report Treatment

REPORT DICE ROLL

What is the number on the face of your first dice roll? Please enter the number in the space below:

Number Reported	1	2	3	4	5	6
Resulting pay-off	$0.10	$0.20	$0.30	$0.40	$0.50	$0

Please click "Continue" if you are done with your choice

Continue

Figure 3: Die reporting screenshot for Confirmation-report Treatments

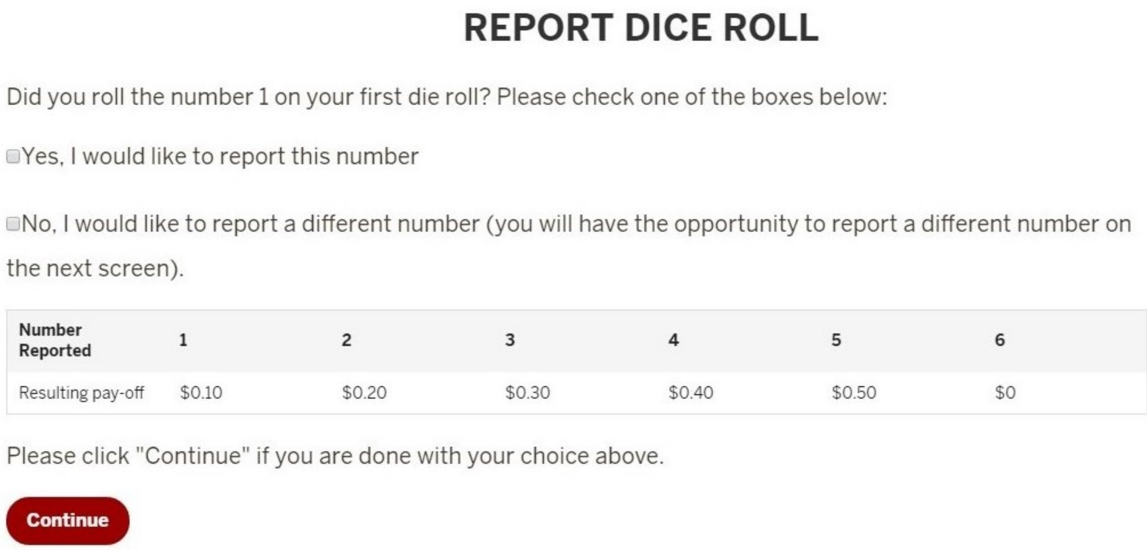
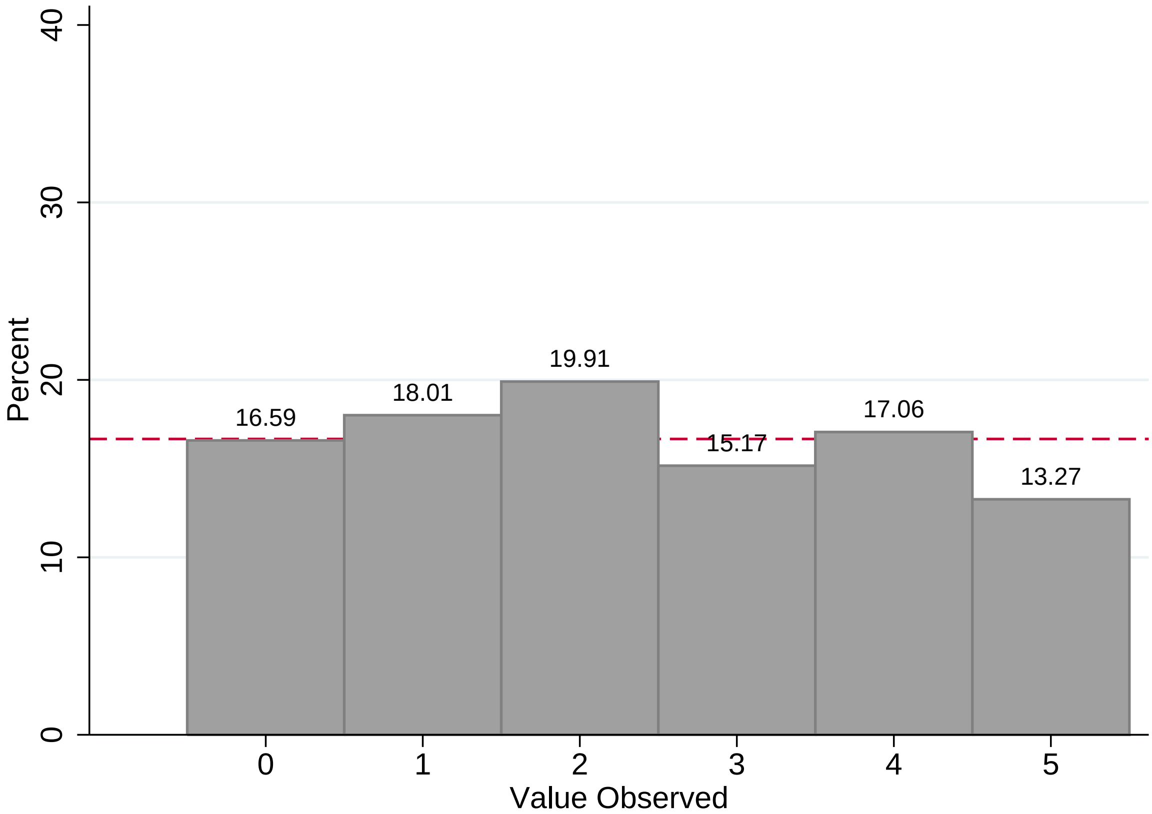
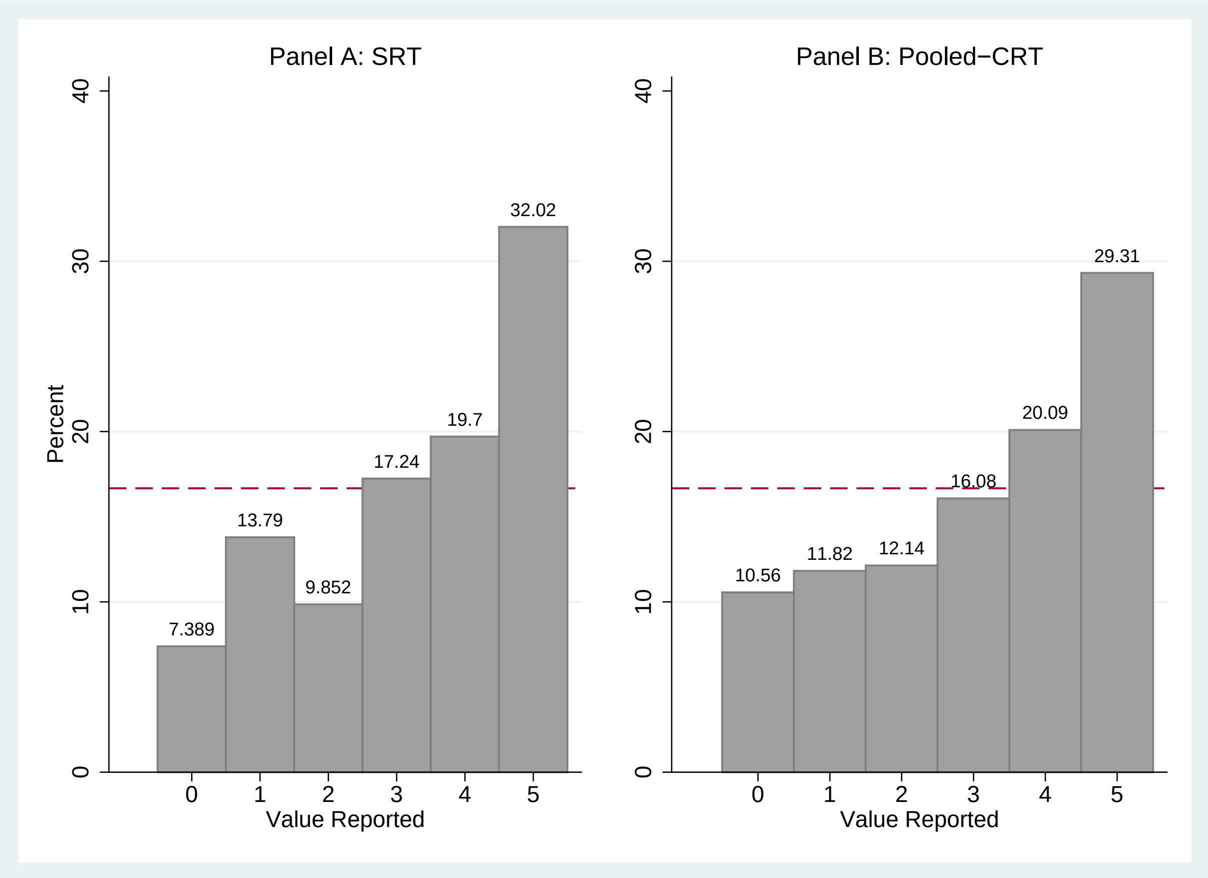


Figure 4: Distribution of Virtual Die Roll Outcomes



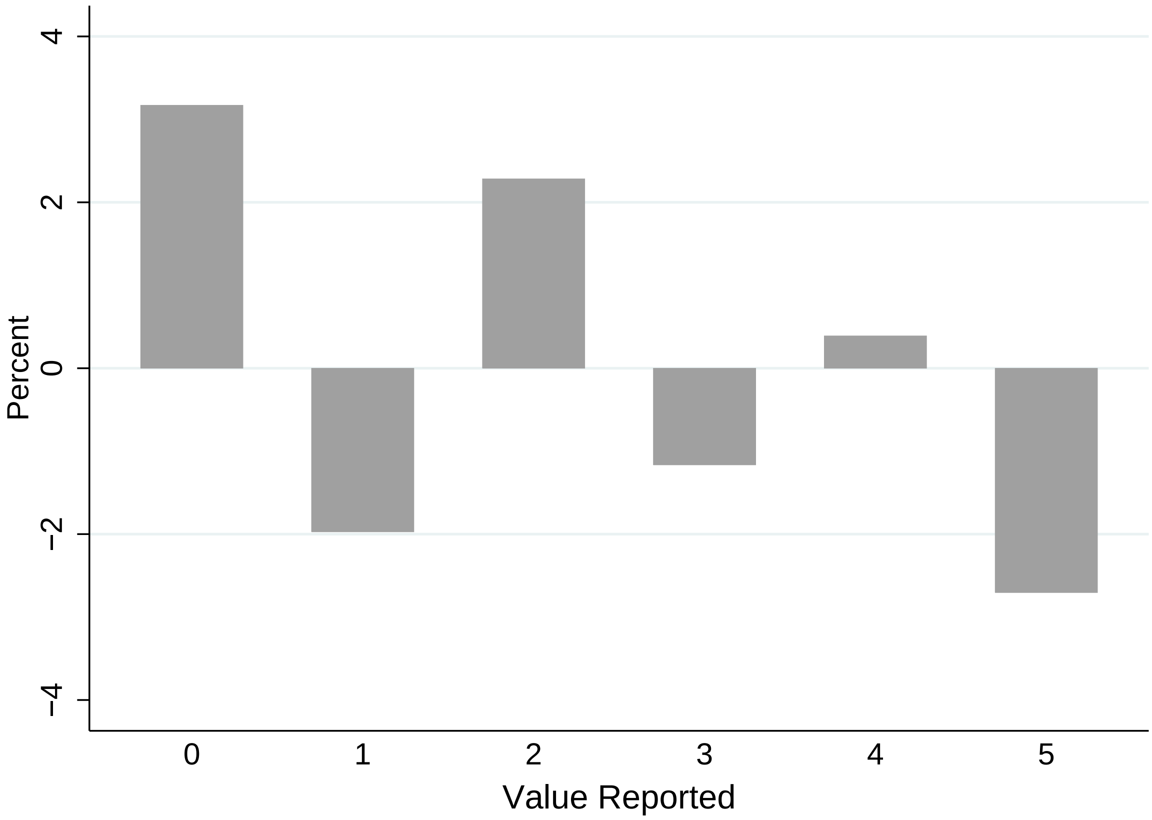
Notes: Reported is the distribution of die outcomes from the website Random.org. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 5: Distribution of reported die outcomes in SRT and CRT



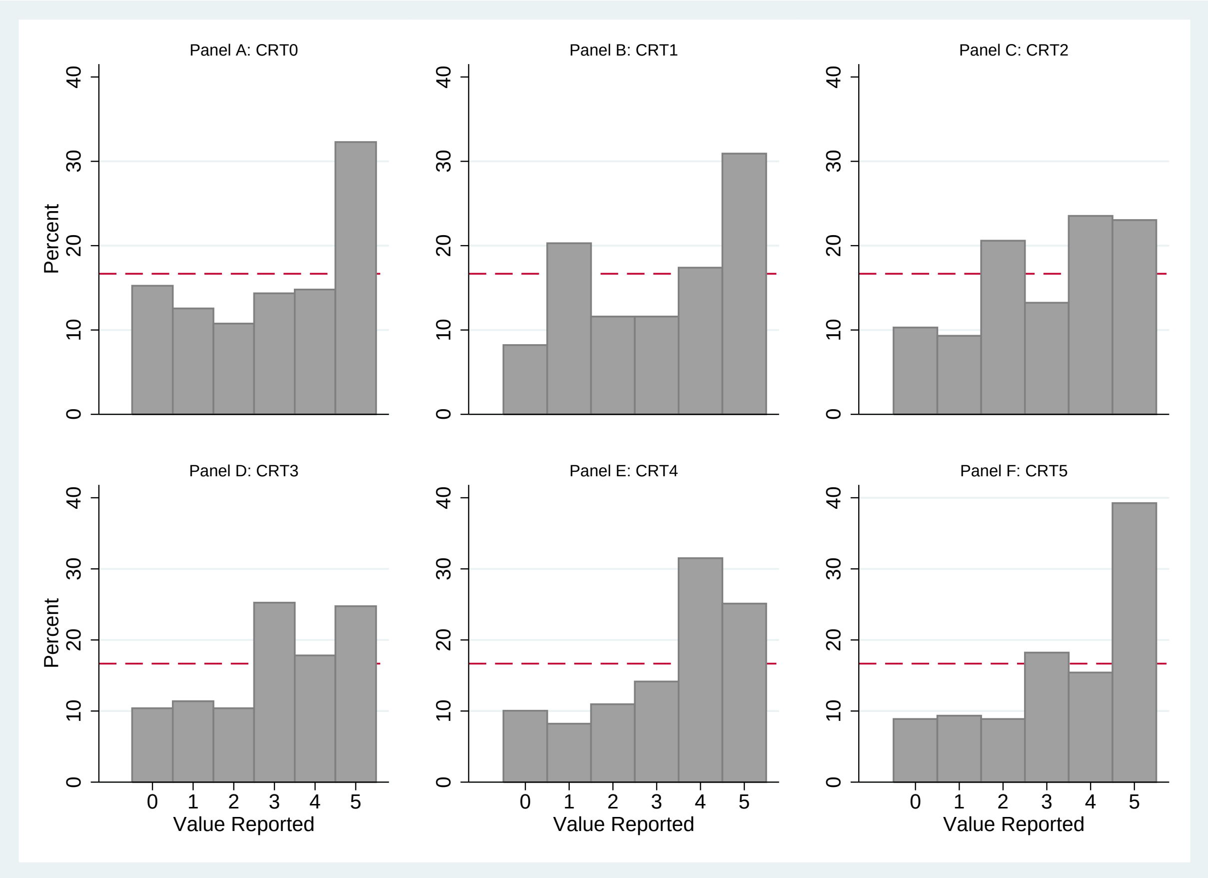
Notes: Reported is the distribution of die outcomes for the self-report (SRT) and confirmation report treatments (CRT). Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 6: Treatment effect of CRT



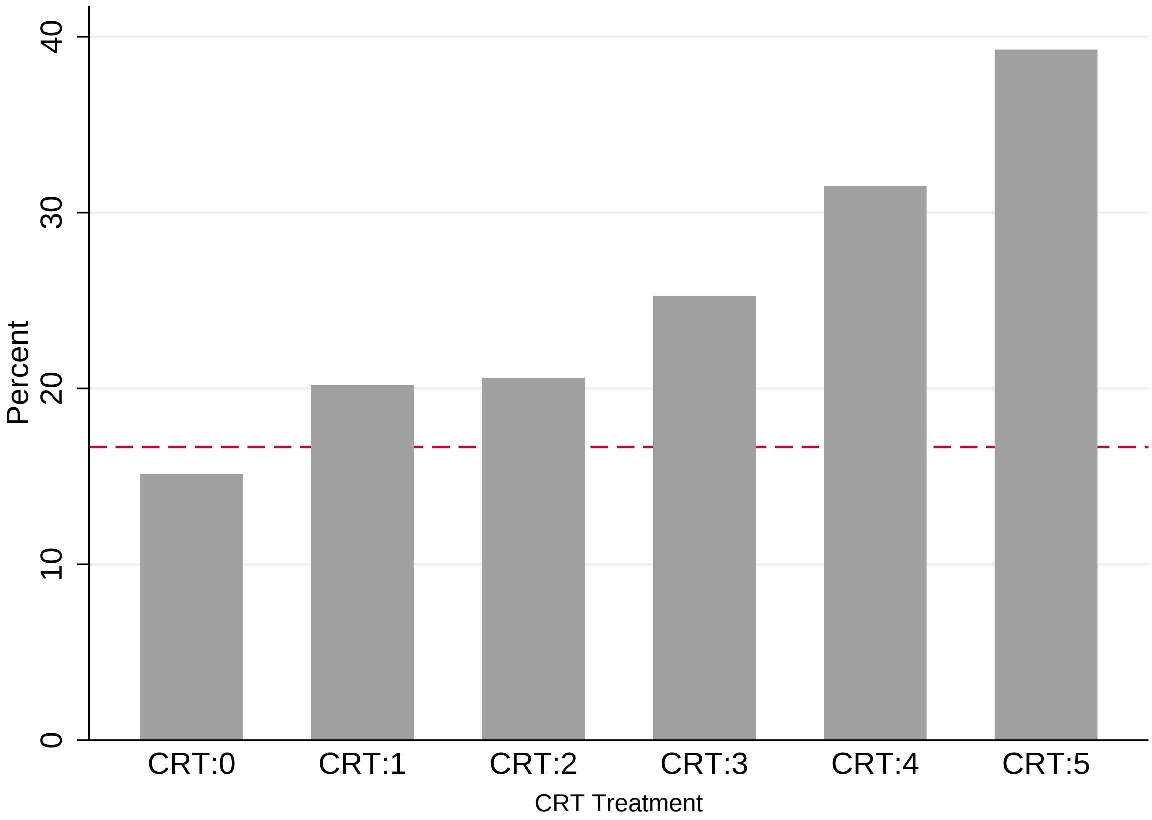
Notes: Reported is the treatment effect of the confirmation report treatments (CRT) using the pooled CRT data. Each bar represents the share of subjects reporting that number in CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 7: CRT Conditional on pre-populated number



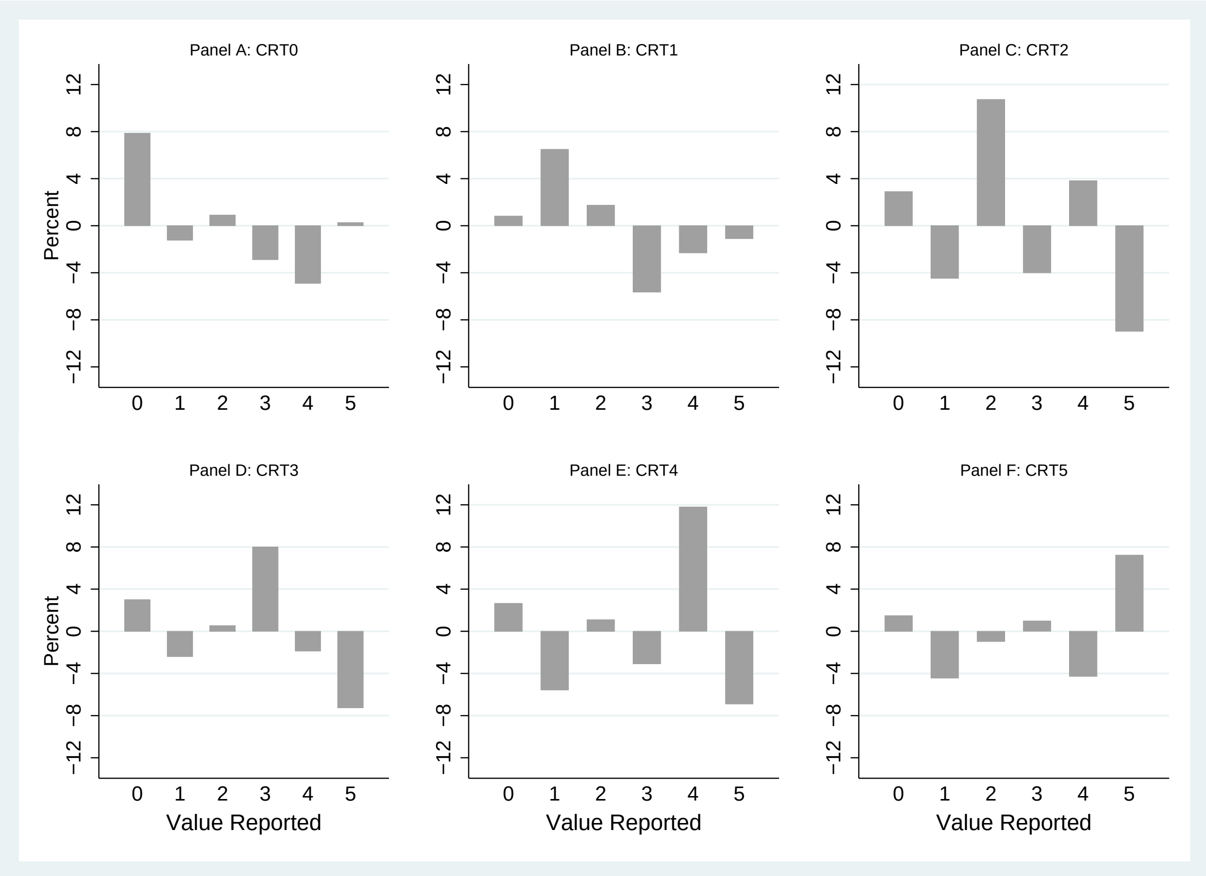
Notes: Reported is the distribution of die outcomes for the confirmation report treatments (CRT). Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 8: CRT Share of Confirmers Conditional on pre-populated number



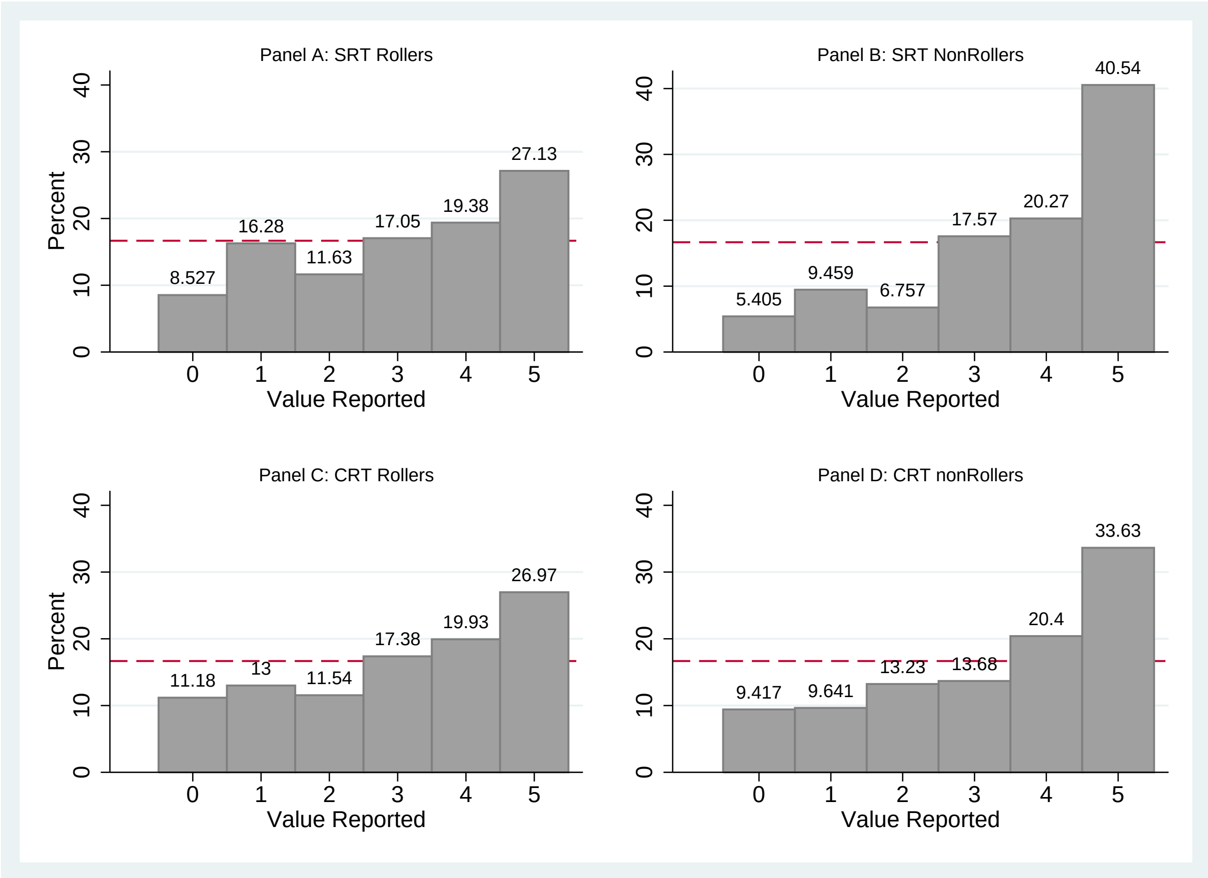
Notes: Reported is the share of subject who confirm the pre-populated number on their confirmation report for each CRT treatment. The horizontal line indicates 16.67%.

Figure 9: SRT versus CRT Treatment effect Conditional on pre-populated number



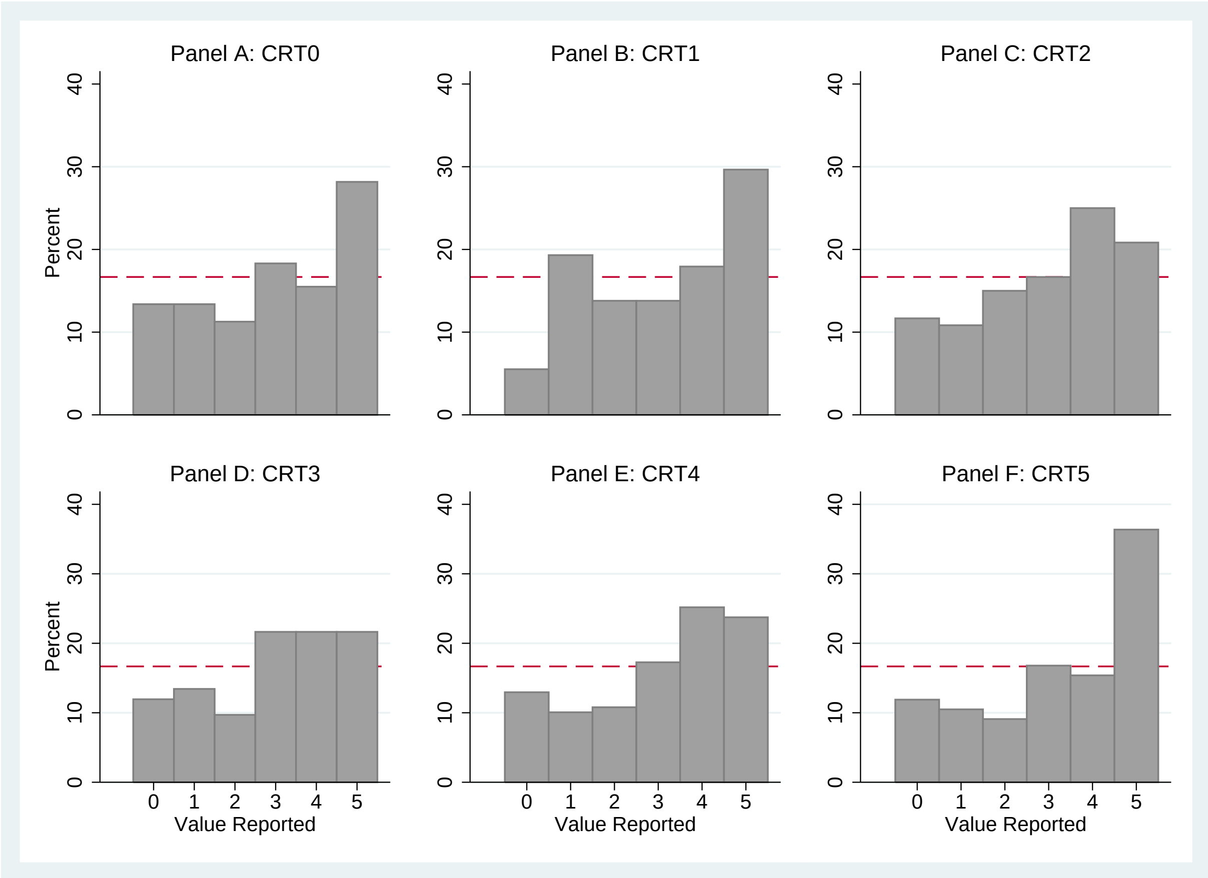
Notes: Reported is the treatment effect for each of the confirmation report treatments (CRT). For each panel, each bar represents the share of subjects reporting that number in the respective CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 10: Non-Rollers: SRT versus CRT



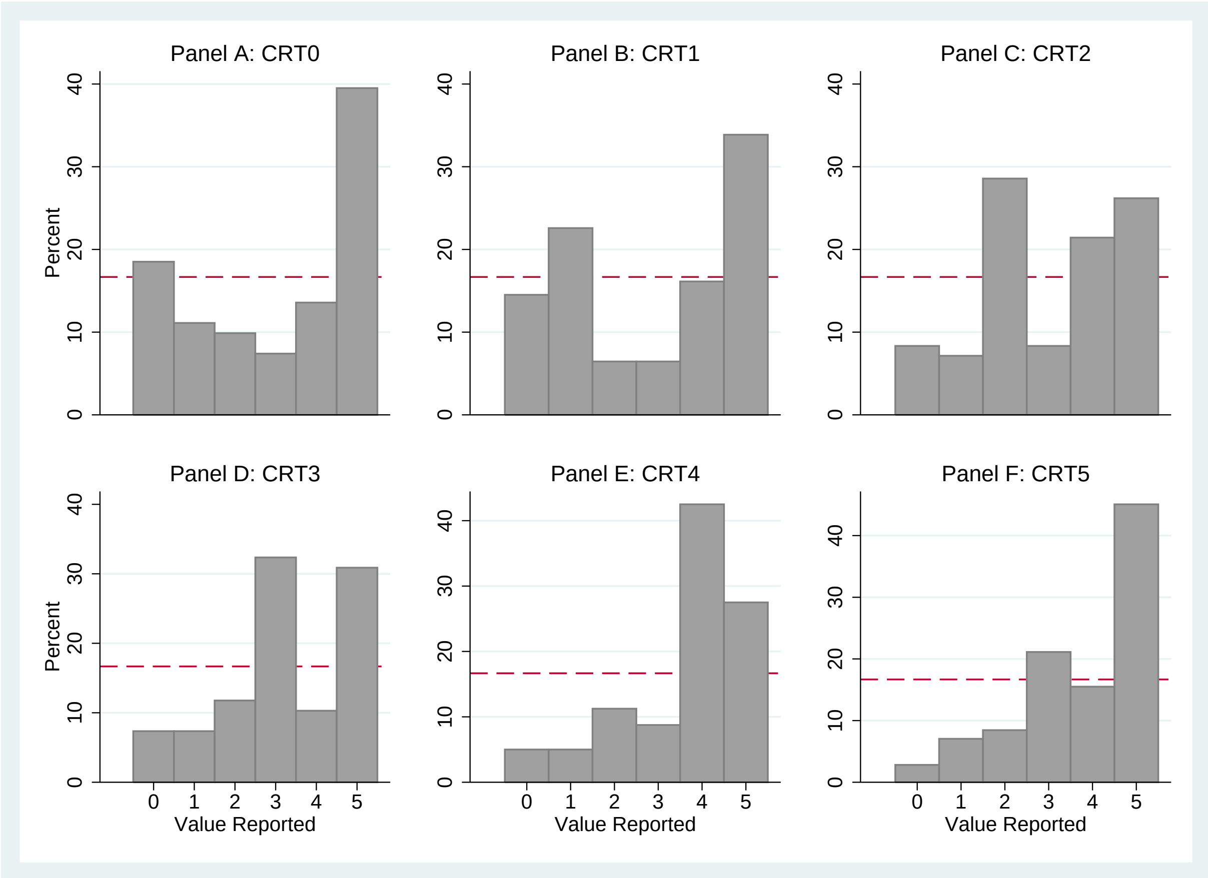
Notes: Reported is the distribution of die outcomes for the self-report (SRT) and confirmation report treatments (CRT) for rollers and non-rollers. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 11: Rollers by CRT Sub Group



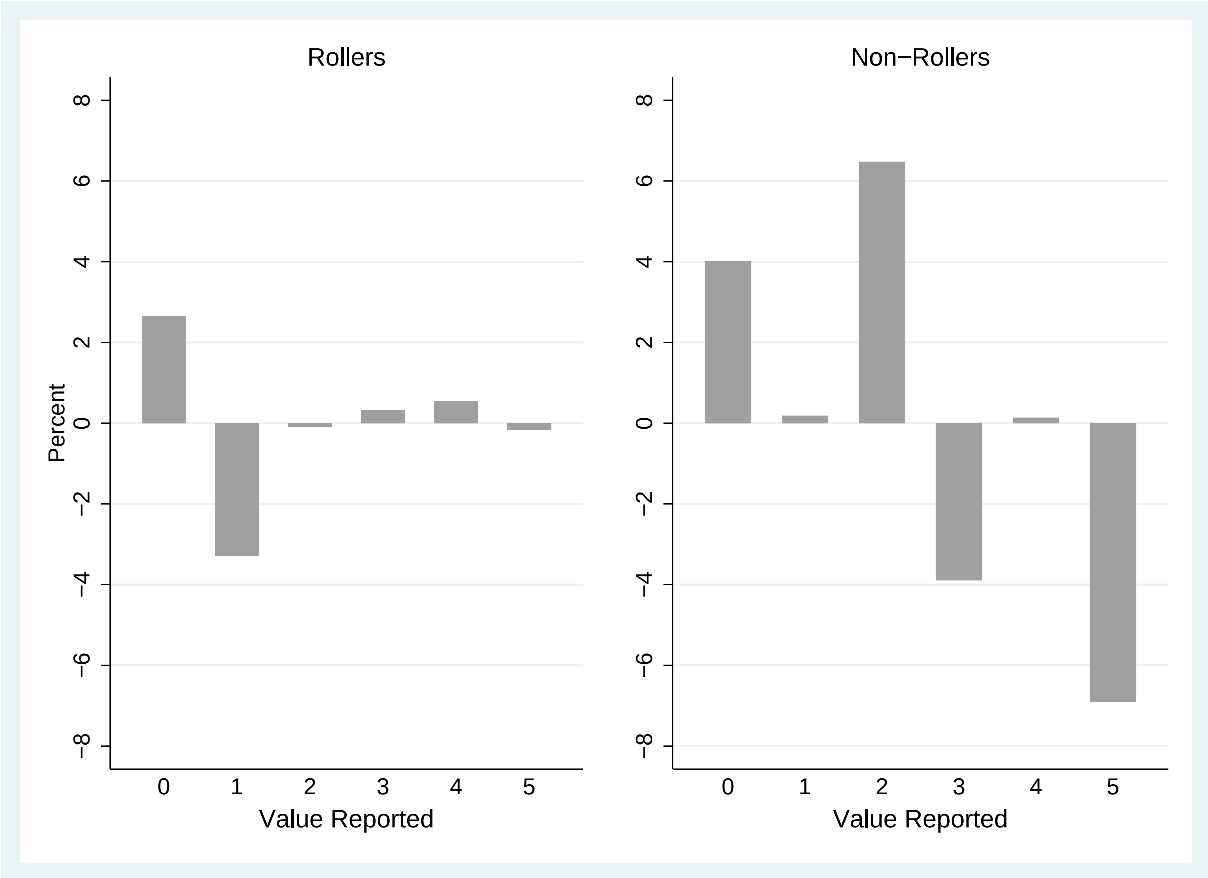
Notes: Reported is the distribution of die outcomes for the confirmation report treatments (CRT) among rollers. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 12: Non-Rollers by CRT Sub Group



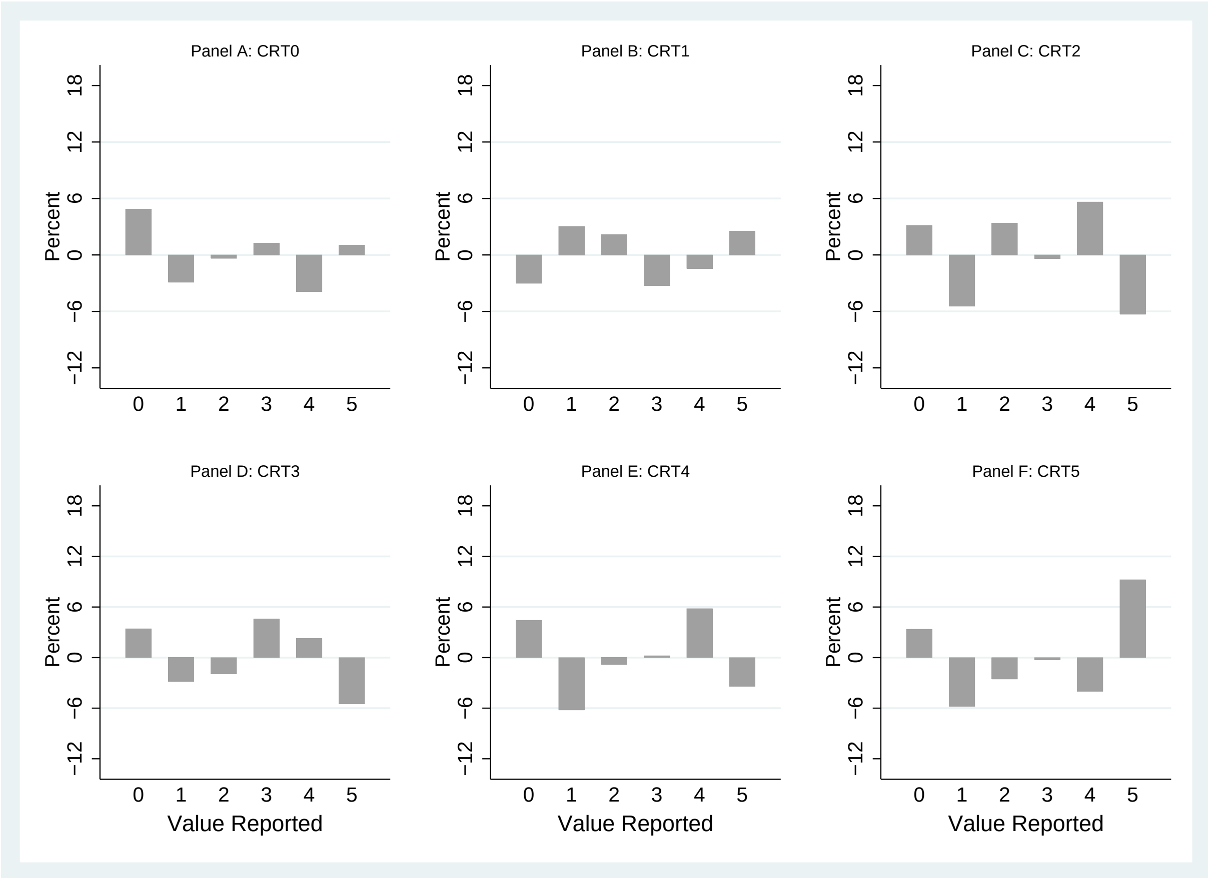
Notes: Reported is the distribution of die outcomes for the confirmation report treatments (CRT)among non-rollers. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 13: Treatment effect of CRT: Rollers versus Non-Rollers



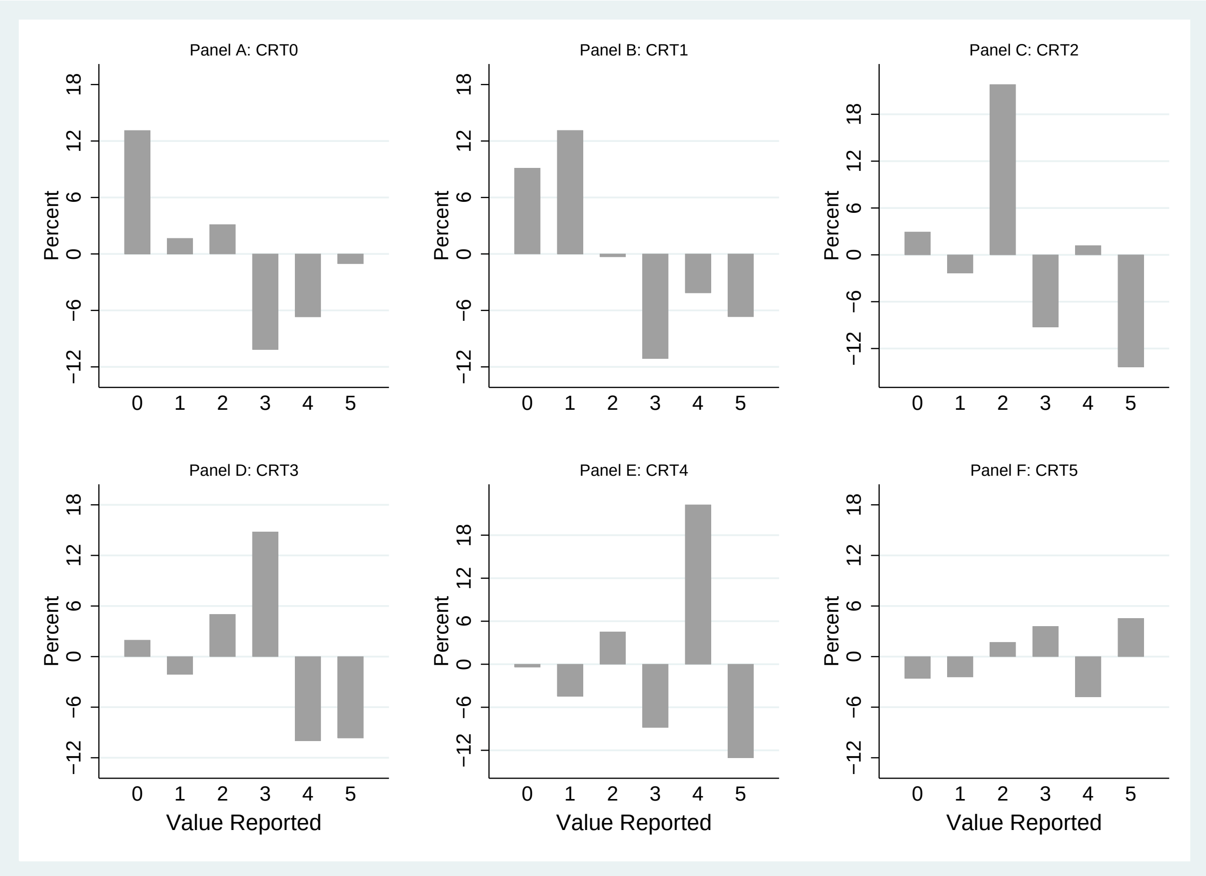
Notes: Reported is the treatment effect of the confirmation report treatment (CRT) by roller status. For each panel, each bar represents the share of subjects reporting that number in the pooled CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 14: Treatment effect of CRT: Rollers



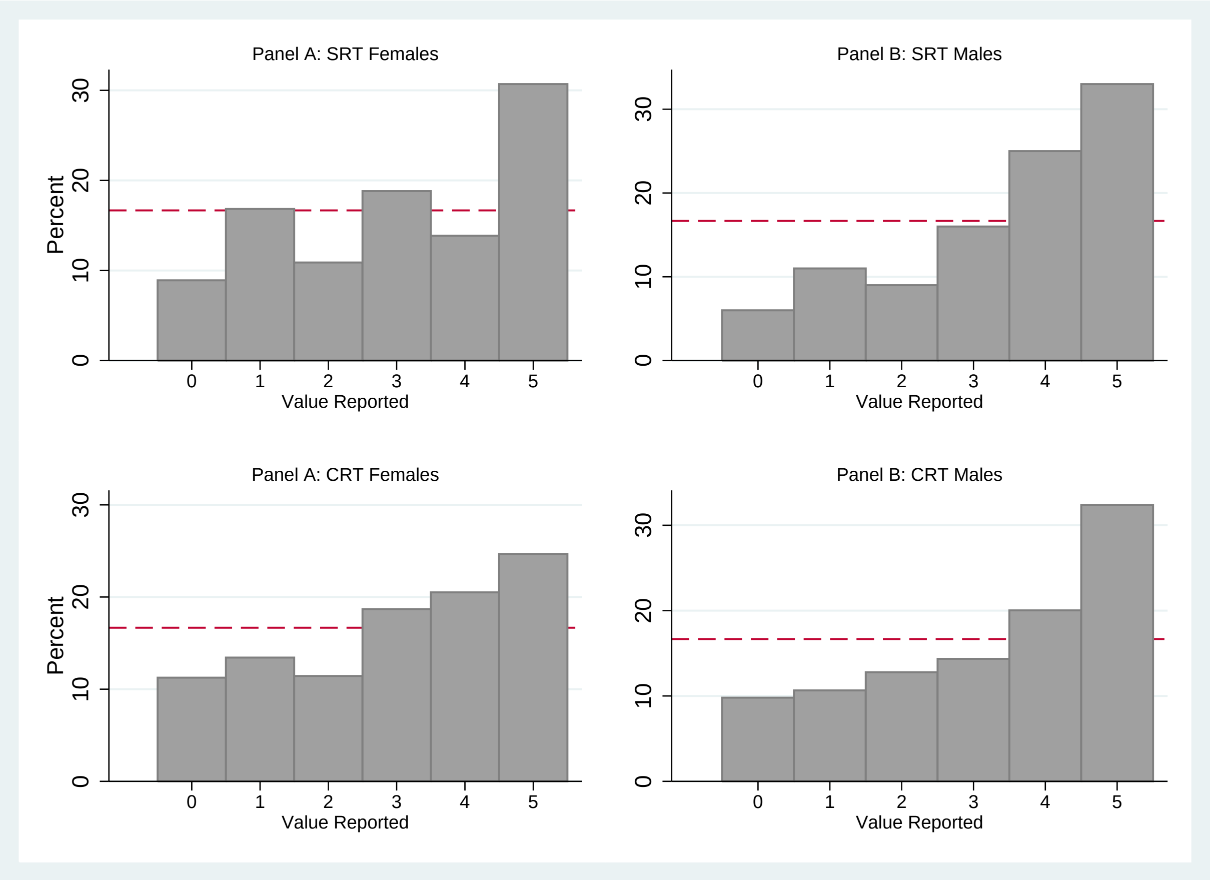
Notes: Reported is the treatment effect for each of the confirmation report treatments (CRT) among rollers. For each panel, each bar represents the share of subjects reporting that number in the respective CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 15: Treatment effect of CRT: Non-Rollers



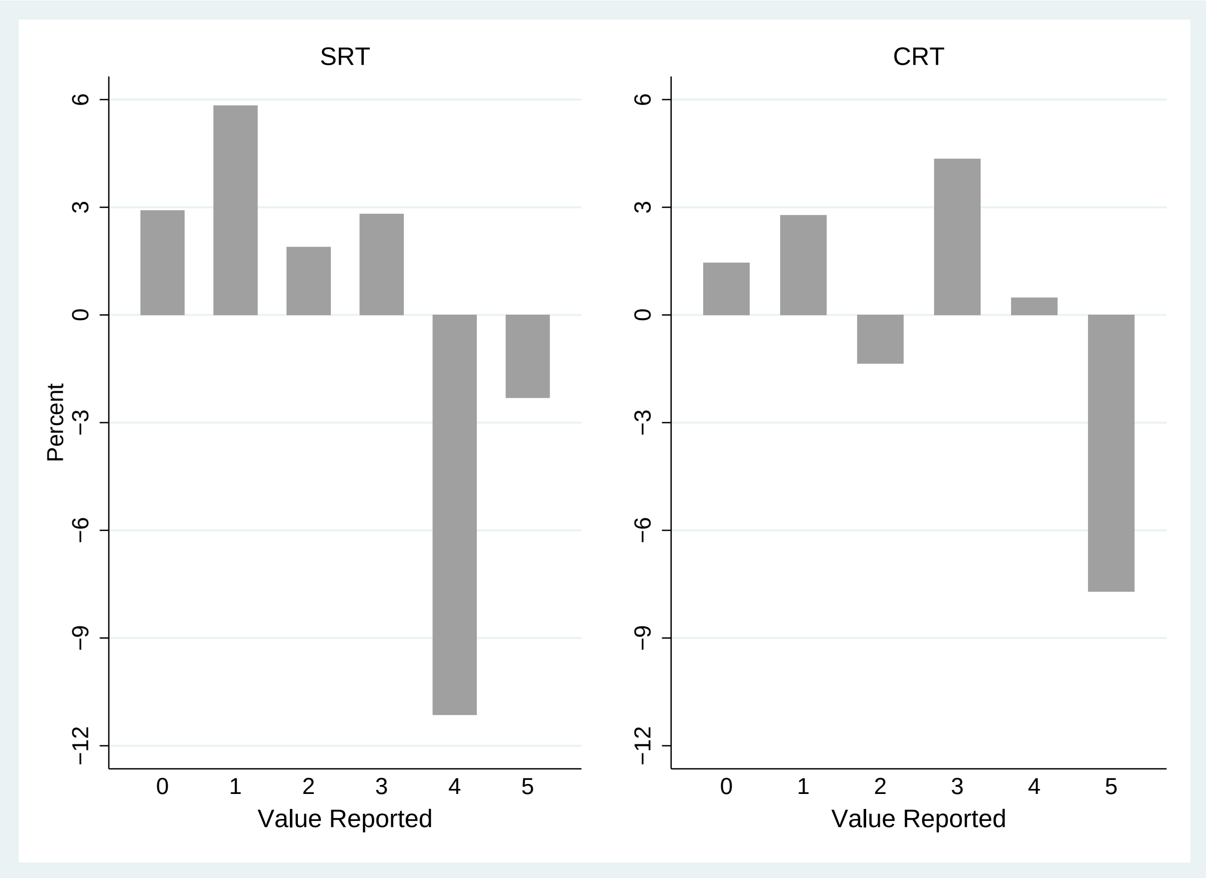
Notes: Reported is the treatment effect for each of the confirmation report treatments (CRT) among non-rollers. For each panel, each bar represents the share of subjects reporting that number in the respective CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 16: SRT Gender effect



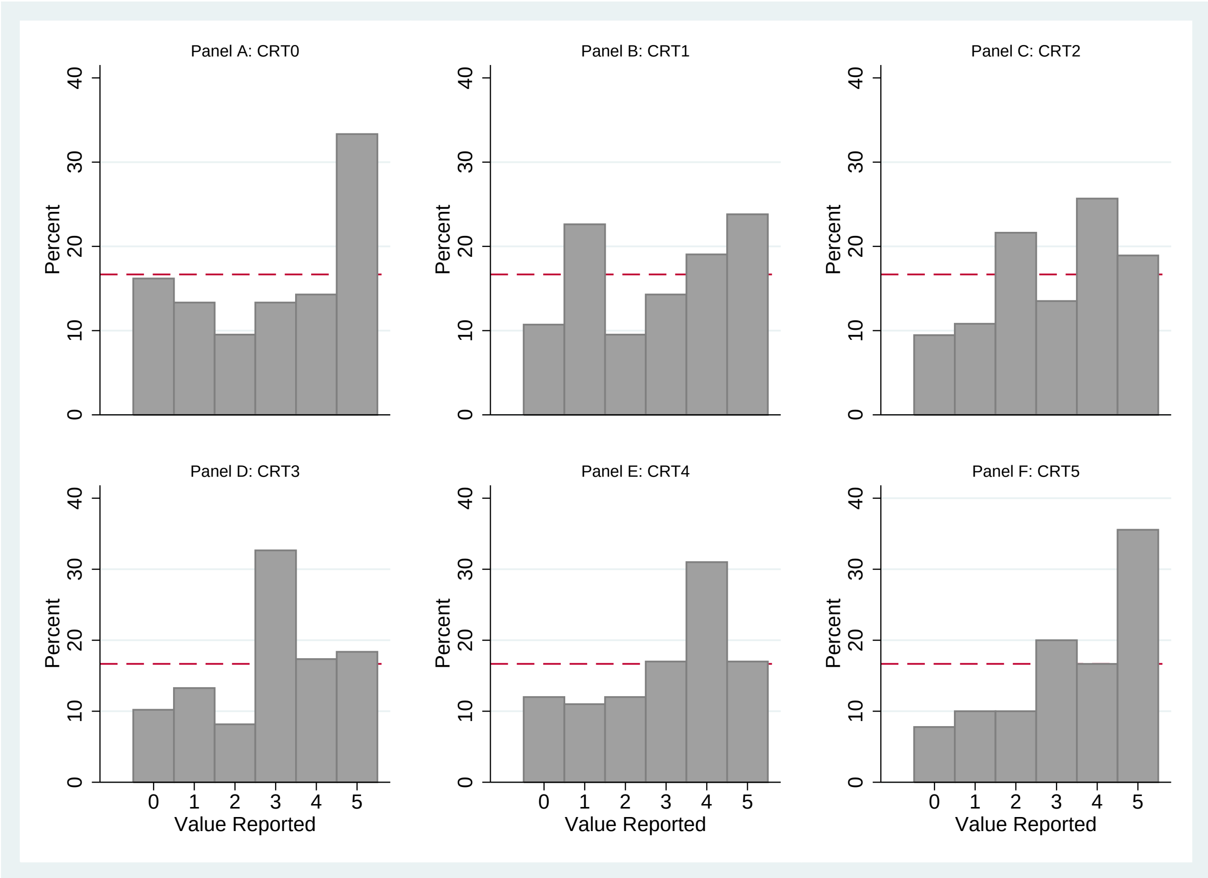
Notes: Reported is the distribution of die outcomes for the self-report (SRT) and confirmation report treatments (CRT) by gender. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 17: Females versus Males by Treatment



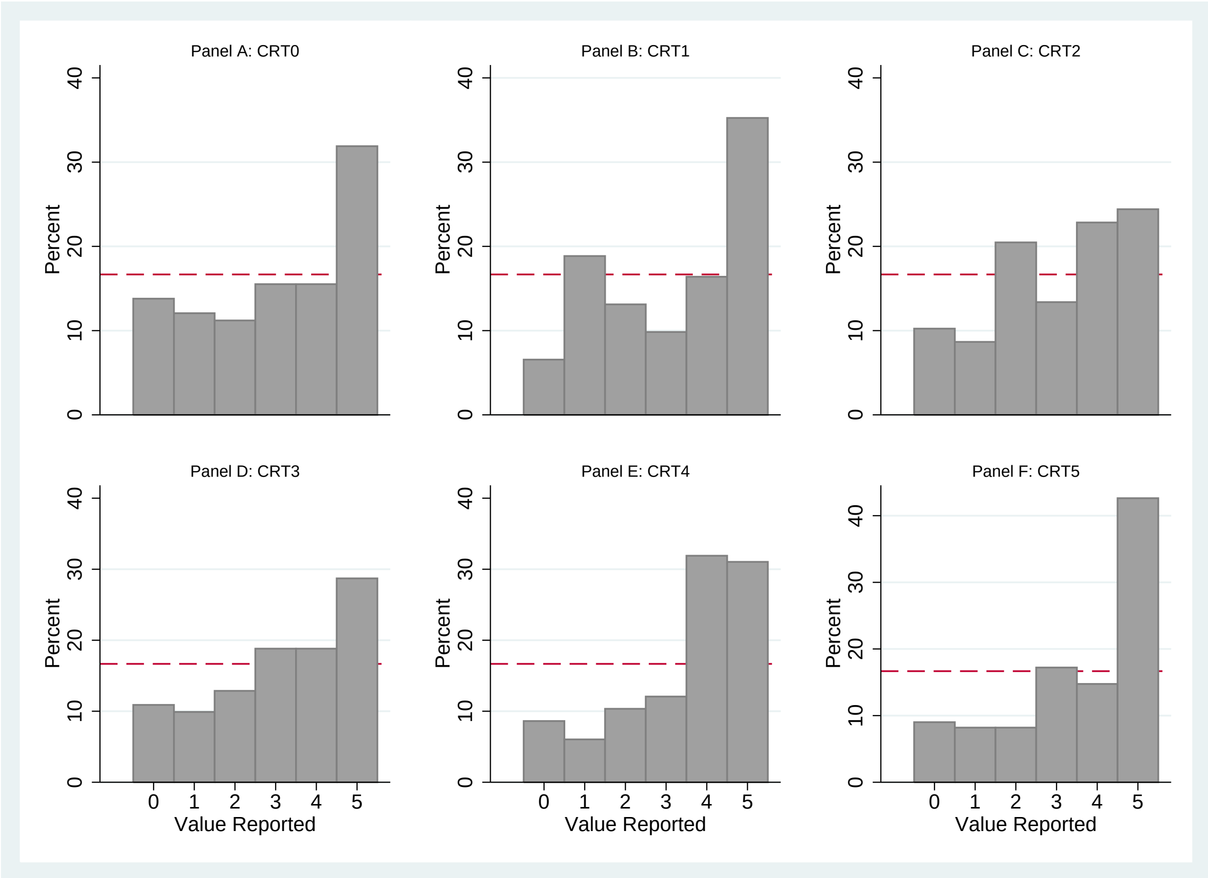
Notes: Reported is the effect of gender on reporting behavior for self-report and confirmation-report, respectively. For each panel, each bar represents the share of females reporting that number minus the share of males reporting that number. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 18: CRT Female Conditional on pre-populated number



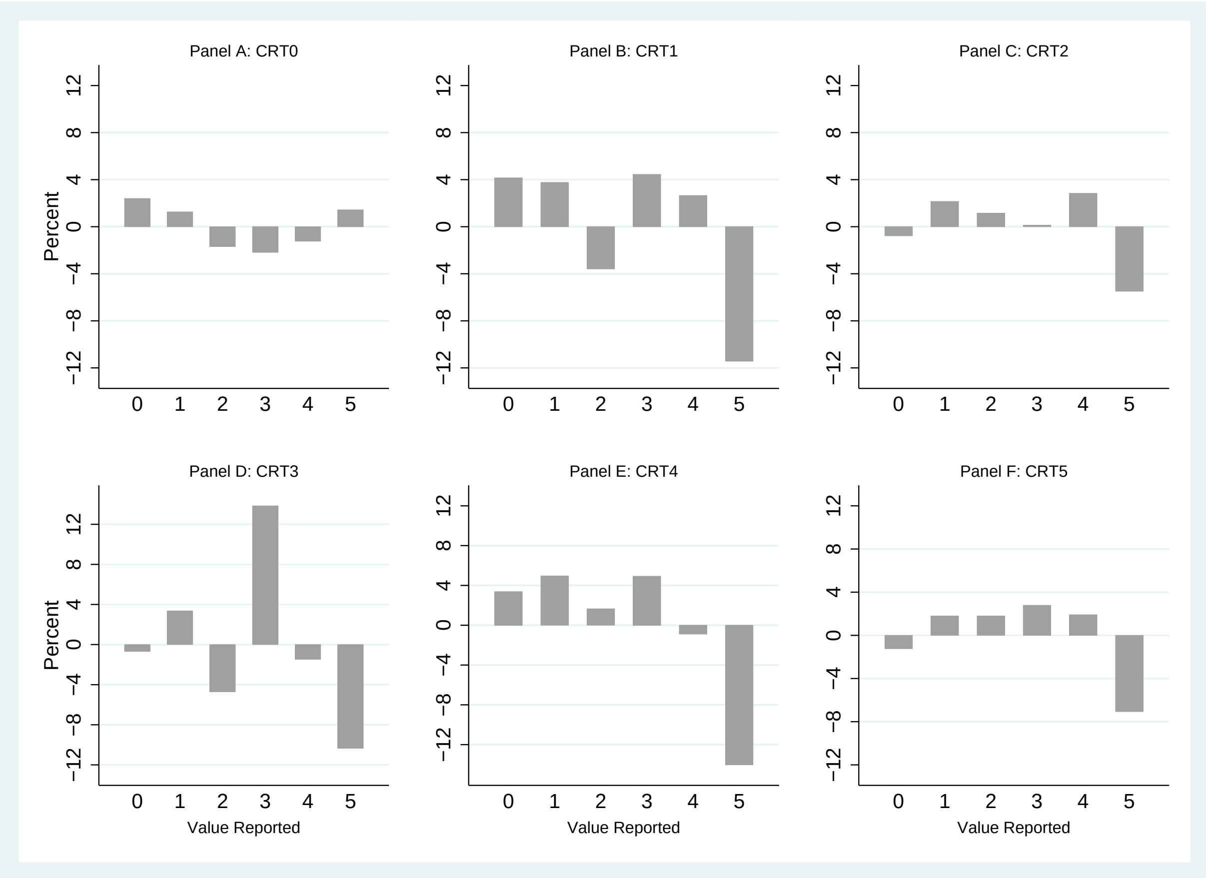
Notes: Reported is the distribution of die outcomes for the confirmation report treatments (CRT) for females. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 19: CRT Male Conditional on pre-populated number



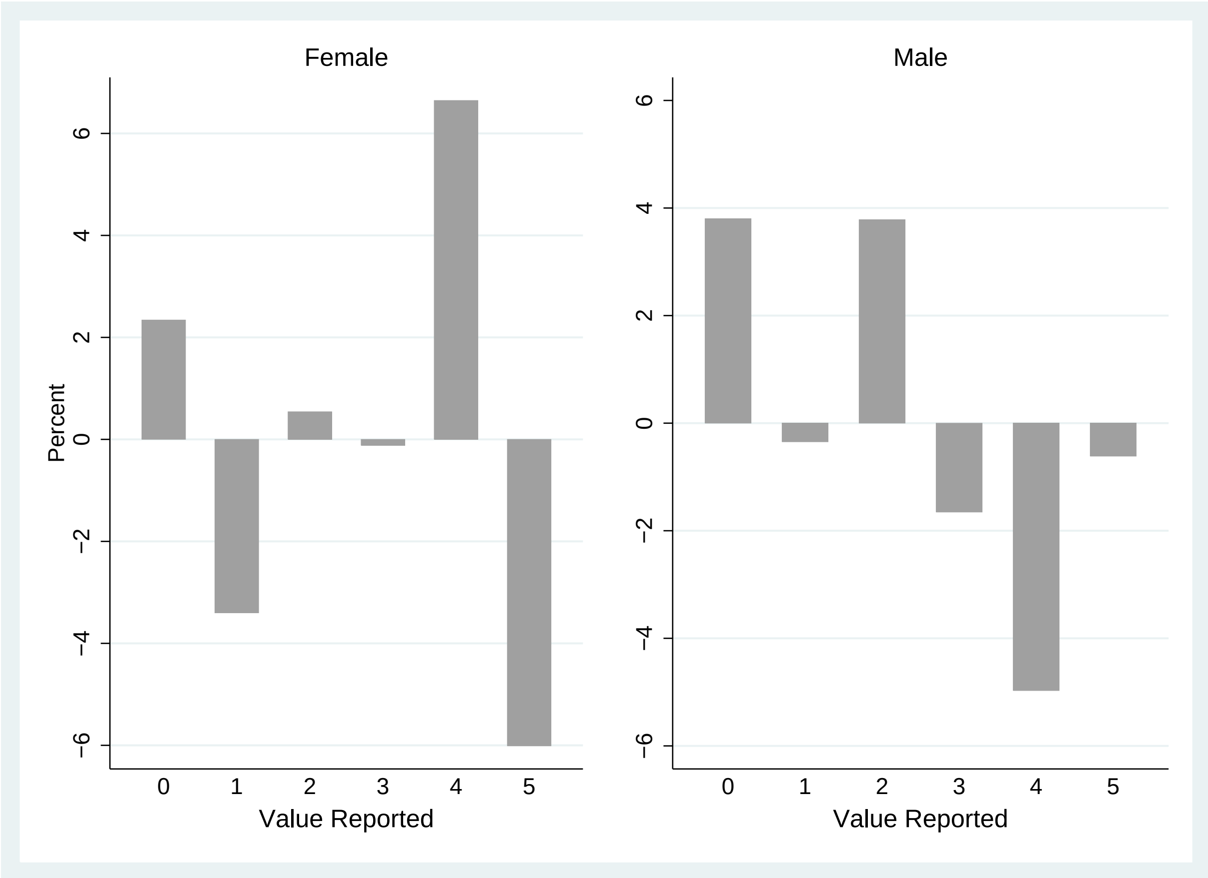
Notes: Reported is the distribution of die outcomes for the confirmation report treatments (CRT) for males. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0. The horizontal line indicates 16.67%.

Figure 20: CRT Male vs Female Conditional on pre-populated number



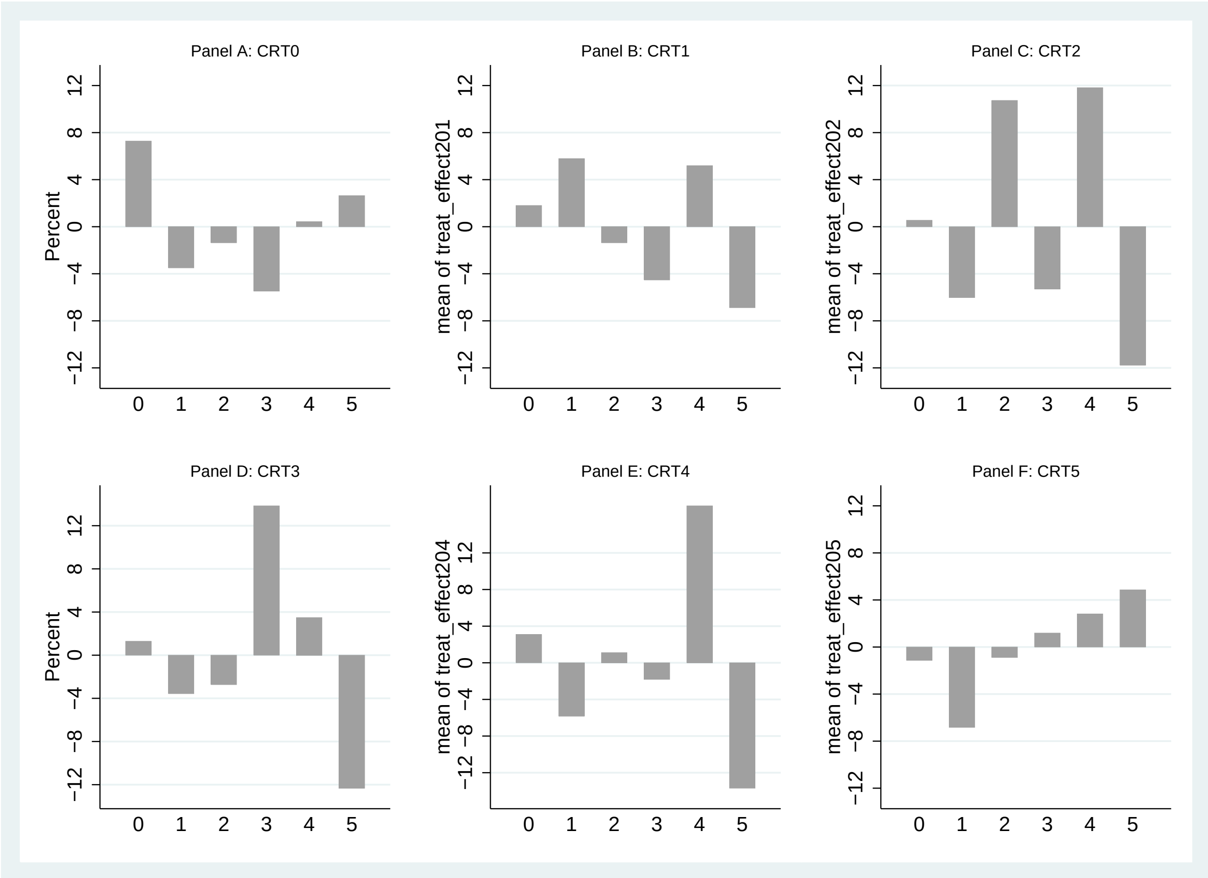
Notes: Reported is the effect gender on reporting behavior for the confirmation-report treatments. For each panel, each bar represents the share of females reporting that number minus the share of males reporting that number. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 21: CRT (Pooled) versus SRT by Gender



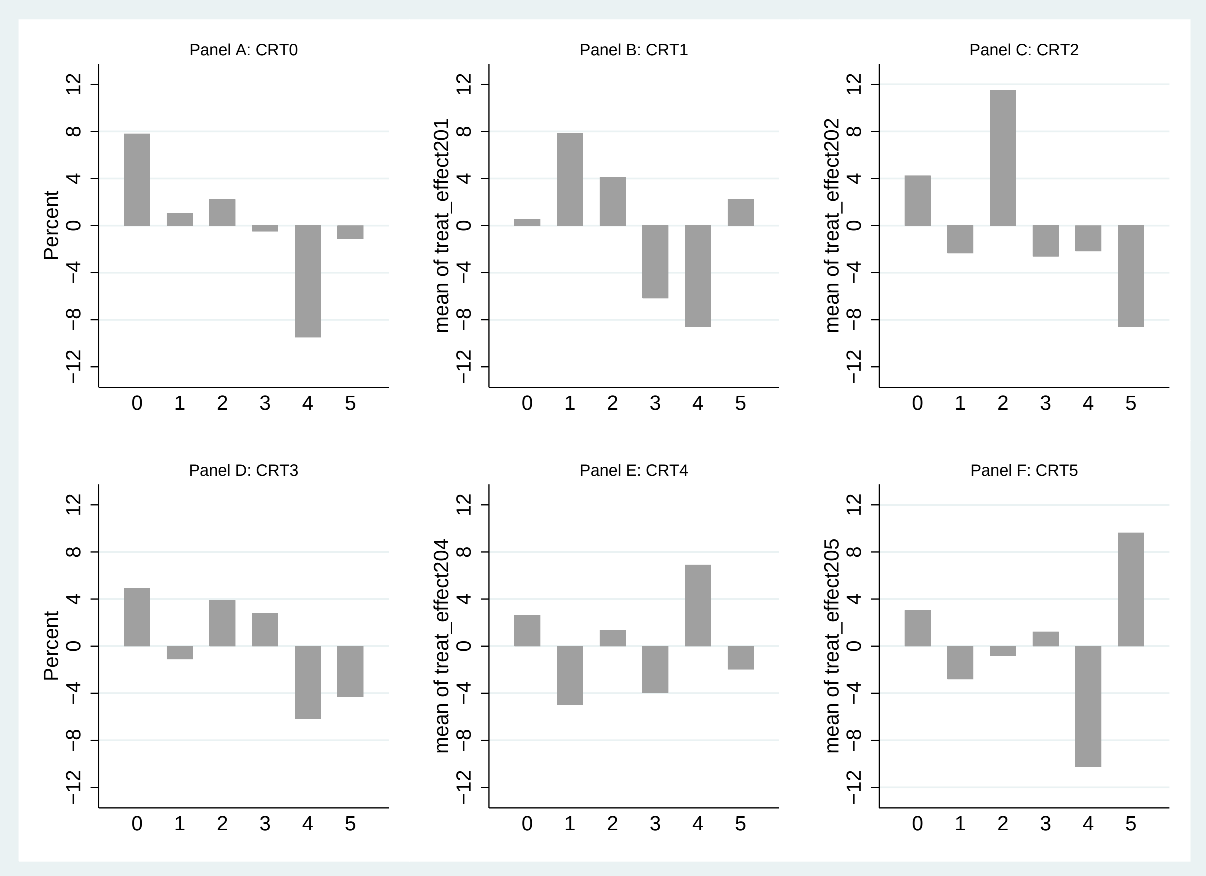
Notes: Reported is the treatment effect of the confirmation report treatment (CRT) by gender. For each panel, each bar represents the share of subjects reporting that number in the pooled CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 22: CRT subgroup versus SRT for Females



Notes: Reported is the treatment effect for each of the confirmation report treatments (CRT) among females. For each panel, each bar represents the share of subjects reporting that number in the respective CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.

Figure 23: CRT subgroup versus SRT for Males



Notes: Reported is the treatment effect for each of the confirmation report treatments (CRT) among males. For each panel, each bar represents the share of subjects reporting that number in the respective CRT minus the share of subjects reporting that number in SRT. Because the pay-off for rolling a 6 is zero, we recode the outcome 6 as 0.