2020-07-23

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Kanngiesser, P

10.1002/bdm.2203
Journal of Behavioral Decision Making
Wiley

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accepted in: Journal of Behavioral Decision Making

please cite as:
Kanngiesser, P., Sunderarajan, J., & Woike, J. K. (2020). Keeping them honest: Promises reduce cheating in adolescents. Journal of Behavioral Decision Making. https://doi.org/10.1002/bdm.2203

Abstract

People frequently engage in dishonest behavior at a cost to others, and it is therefore beneficial to study interventions promoting honest behavior. We implemented a novel intervention that gave participants a choice to promise to be truthful or not to promise. To measure cheating behavior, we developed a novel variant of the mind game—the dice-box game—as well as a child-friendly sender-receiver game. Across three studies with adolescents aged 10 to 14 years (N = 640) from schools in India, we found that promises systematically lowered cheating rates compared to no-promise control conditions. Adolescents who sent truthful messages in the sender-receiver game cheated less in the dice-box game and promises reduced cheating in both tasks (Study 1). Promises in the dice-box game remained effective when negative externalities (Study 2) or incentives for competition (Study 3) were added. A joint analysis of data from all three studies revealed demographic variables that influenced cheating. Our findings confirm that promises have a strong, binding effect on behavior and can be an effective intervention to reduce cheating.

Keywords: honesty, cheating, behavioral ethics, moral decision making

Keeping them honest: Promises reduce cheating in adolescents

People engage in dishonest behavior such as filing taxes incorrectly (Slemrod, 2007) or cheating on academic assignments (McCabe, Trevino, & Butterfield, 2001). Studying cheating in real world settings is difficult when using self-reports, as it is unclear whether cheaters can be trusted to report truthfully on their cheating behavior. A number of tasks have therefore been devised to obtain objective measures of dishonesty: for example, deviation of self-reported, incentivized coin flips or dice rolls from statistically expected outcomes (Fischbacher & Föllmi-Heusi, 2013; Houser, Vetter, & Winter, 2012; Rosenbaum, Billinger, & Stieglitz, 2014). These measures have been validated by demonstrating their correlation with real-world cheating behaviors as diverse as absenteeism from work, fare-dodging, misbehavior at school, or failure to return overpaid money (Cohn & Maréchal, 2018; Dai, Galeotti, & Villeval, 2018; Hanna & Wang, 2017; Potters & Stoop, 2016).

Developmental studies suggest that children first begin to show dishonest behavior during the preschool years (Lee, 2013; Lewis, Stanger, & Sullivan, 1989; Polak & Harris, 1999). It increases during middle and late childhood, and then decreases during adolescence (Evans & Lee, 2011; Glätzle-Rützler & Lergetporer, 2015; Maggian & Villeval, 2016). This produces an inverted U-shaped developmental curve, with adults being at least partly dishonest (Abeler, Nosenzo, & Raymond, 2016; Gerlach, Teoderescu, & Hertwig, 2019; Gneezy, 2005; Mazar, Amir, & Ariely, 2008). While self-serving dishonesty generally decreases from late childhood to adolescence, more sophisticated forms of dishonesty (such as white lies to benefit others) have been shown to increase during this period (Fu, Evans, Wang, & Lee, 2008; Talwar, Murphy, & Lee, 2007; Xu, Bao, Fu, Talwar, & Lee, 2010).

Given the costs of dishonesty, there is a societal interest in factors that promote more honest behavior. Particular attention has been paid to promises as as a means to reducing dishonest behavior in children and adults (Evans & Lee, 2010; Heyman, Fu, Lin, Qian, & Lee, 2015; Kataria & Winter, 2013; Lyon & Dorado, 2008; Lyon, Malloy, Quas, & Talwar, 2008; Quas, Stolzenberg, & Lyon, 2018; Talwar, Lee, Bala, & Lindsay,

2002, 2004). Promises are defined as voluntary commitments to perform a specific act in the future (Austin, 1975; Searle, 1969). There is an ongoing debate on why there exists an obligation to perform a promised act (Habib, 2018). Some philosophers have argued that promises are binding because of a social contract that everyone benefits from and thus everyone has an obligation to uphold (Rawls, 1955). Other theorists have suggested that promises are binding because others expect and trust that one will perform the promised action and violating others' trust causes harm (Scanlon, 1990). More recently, philosophers have suggested a hybrid account according to which trust in promises arises due to social conventions and, again, breaking one's promise violates the established trust (Kolodny & Wallace, 2003). These theoretical debates are mirrored by different stances in the behavioral sciences with some researchers suggesting that promises are binding because of a preference for keeping one's word (Ellingsen & Johannesson, 2004; Vanberg, 2008), because of an aversion to disappointing other's expectations (Charness & Dufwenberg, 2006), or because of a combination of both (conditional-expectation account: Mischkowski, Stone, & Stremitzer, 2019; Ederer & Stremitzer, 2017).

Irrespective of the different stances, research has consistently found that many people will keep their word—even at a cost to themselves (Woike & Kanngiesser, 2019)—and that promises can promote desirable behaviors in children and adults such as cooperation, helping, recycling, or visits to doctors (Bicchieri, 2002; Charness & Dufwenberg, 2006; Ellingsen & Johannesson, 2004; Kanngiesser, Köymen, & Tomasello, 2017; Kulik & Carlino, 1987; Ostrom, Walker, & Gardner, 1992; Wang & Katzev, 1990). Promises to tell the truth become effective in children from five to six years of age. For example, Heyman et al. (2015) used a peeking game with a hidden camera and found that from five years of age Chinese children cheated less when they had promised not to peek. Similarly, six- to seven-year-old North American children who had promised to tell the truth revealed more often that they had played with a forbidden toy (Lyon & Dorado, 2008). Promises to tell the truth increase their effectiveness between four to nine years of age for North American children (Quas et al., 2018) and, generally, remain

effective in adolescence (Evans, O'Connor, & Lee, 2018) and adulthood (Kataria & Winter, 2013).

Many of these previous studies have forced participants to promise without an opportunity to opt out of the commitment (Evans & Lee, 2010; Heyman et al., 2015; Kataria & Winter, 2013; Lyon & Dorado, 2008; Lyon et al., 2008; Quas et al., 2018; Talwar et al., 2002, 2004). Yet, promises are per definition *voluntary* commitments (Searle, 1969). This point is articulated clearly by Rawls (1999, p. 303) when describing the practice of promising:

> "[...] in order to make a binding promise, one must be fully conscious, in a rational frame of mind, and know the meaning of the operative words, their use in making promises, and so on. Furthermore, these words must be spoken freely or voluntarily, when one is not subject to threat or coercion, and in situations where one has reasonably fair bargaining position, so to speak. A person is not required to perform if the operative words are uttered while he is asleep, or suffering delusions, or if he was forced to promise, or if pertinent information was deceitfully withheld from him."

Forcing people to promise, therefore, relieves them of their obligation to perform the promised act. Moreover, on ethical grounds, it can be considered a violation of people's autonomy and deliberate decision making (Hertwig & Grüne-Yanoff, 2017).

Some studies have investigated the effects of promises in free-form communication, in which one could reasonably expect an absence of coercion (e.g., Charness & Dufwenberg, 2006; Vanberg, 2008; Servátka, Tucker, & Vadovič, 2011). However, these studies have regularly coded both statements of intent ("I will do X") and promises ("I promise to do X") as commitments. Yet, statements of intent and promises are different types of speech acts. It is perfectly plausible to utter a sentence such as "I will come to your party, but I cannot promise.", which only states an intention to do something and explicitly hedges against circumstances that may hinder the fulfilment of this intention (e.g., other obligations, change of mood, etc.). This illustrates that statements of intent cannot be considered unambiguous and firm expressions of commitment.

In the current study, participants had a choice to promise to be truthful or to opt out of the promise. First, to measure cheating, we implemented a novel variant of the mind game (Jiang, 2013; Kajackaite & Gneezy, 2017; Potters & Stoop, 2016; Rahwan, Hauser, Kochanowska, & Fasolo, 2018; Shalvi & Dreu, 2014): participants received a box with 16 dice in a 4x4 grid (see Fig. 1). They were instructed to privately pick one of the 16 locations, to shake the box, and to write down the number of eyes on the chosen die. This set-up gives participants the opportunity to secretly switch to locations with more favorable outcomes. As such switches are unobservable, the task does not require privacy booths (Bucciol & Piovesan, 2011; Fischbacher & Föllmi-Heusi, 2013), deception or hidden cameras (Evans & Lee, 2010; Heyman et al., 2015; Markiewicz & Gawryluk, 2019; Talwar et al., 2002). Participants played the game repeatedly across 15 rounds and received prizes based on the total number of eyes on the 15 reported dice (one die per round). Given the known probability distribution of die roll outcomes, this allowed us to estimate dishonesty on the group level and with some precision even on the individual level.

To implement the promise intervention, participants had a choice at the start of the dice-box task between (a) receiving 1 point per eye conditional on making a promise to tell the truth about the number of eyes on their chosen die or (b) receiving 0.5 points per eye without any conditions. The promise option thus always resulted in higher pay-offs compared to the no-commitment option. We chose these incentives so that even potentially dishonest participants had a reason to choose the promise option. While we incentivized participants to choose the promise option, we did not force them to promise as it was ultimately the participants' decision to opt for the higher pay-off and promise (or not). Moreover, we explicitly used the word "promise" to invoke a commitment and left no ambiguity about the nature of the commitment. We also included a control condition (between subjects), in which participants had a choice between payments of 0.5 points and 1 point per eye without requiring or mentioning a promise for either choice option. We included a choice between the same pay-offs as in the promise condition to control for possible effects this choice may have had on

participants' honesty. Comparing reported outcomes in the promise condition and the control condition allowed us to estimate the effectiveness of the promise intervention. A similar design was recently implemented in a different task with US participants (Woike & Kanngiesser, 2019). Instructions in the control condition made no reference to truth telling and, arguably, did not make it explicit that cheating would be an option (Kajackaite & Gneezy, 2017). If anything, this difference in instructions should make it harder to show an effect of promises. Nevertheless, we expected that participants in the control condition would be aware of the possibility to cheat and make use of it - similar to previous cheating studies that did not explicitly mention the option to cheat (Jiang, 2013; Potters & Stoop, 2016).

To measure the effectiveness of the promise intervention, we conducted three studies with adolescents ($N = 640$) in schools in India. The Indian context is unique regarding the severity of cheating in academic settings and the creativity of preventive countermeasures (BBC World News, 2015; The Guardian, 2016, 2018). Furthermore, in India, academic cheating has its parallels on the societal level: for example, only a small fraction of the population reportedly pays income tax (The Times of India, 2016). A recent worldwide study with university students (excluding India) showed that higher societal levels of tax evasion and corruption correlated with increased cheating in an experimental dice-rolling task (Gächter & Schulz, 2016). Previous work has also found that cheating in an experimental task predicted school misconduct in Swiss students (Cohn & Maréchal, 2018). Despite the relevance of the Indian context for studying cheating behavior and interventions, few rigorous studies have been conducted in India (Gerlach et al., 2019; for an exception see Hanna & Wang, 2017) and, to our knowledge, none with adolescents. This seems particularly striking given that as of 2018 there were about 250 million adolescents in India, which corresponds to 20.4% of the world's adolescents (UNICEF, 2019). By focusing on Indian adolescents in our study, we thus contribute to reducing the W.E.I.R.D. (Western Educated Industrialized Rich Democratic) sampling bias in the behavioral sciences and psychology (Henrich, Heine, & Norenzayan, 2010; Nielsen, Haun, Kärtner, & Legare, 2017).

In addition to the dice-box task, we included a child-friendly variant of a sender-receiver game in Study 1, in which participants could send free-form messages to other participants. This allowed us to test for correlations between two types of dishonesty: towards the experimenter (in the dice-box task) and another participant (in the sender-receiver task). In Study 2, we introduced a negative externality (Maggian, 2019; Meub, Proeger, Schneider, & Bizer, 2016): each point claimed by the participant reduced the gain of another student in the same school. In Study 3, we created inter-group competition (Charness, Masclet, & Villeval, 2014; Vriend, Jordan, & Janssen, 2016) by randomly assigning participants to virtual groups and awarding a special attractive prize to the group of participants with the highest overall point score. In each study, we also asked participants to fill in an honesty self-report and collected socio-demographic variables from their parents (e.g., parental education, faith) that we entered in a joint analysis of the dice-box data from all three studies.

To summarize, in a series of three studies we make several contributions to the literature: (1) we implemented child-friendly variants of the mind game and the sender-receiver game to measure cheating behaviors, (2) we studied the relation between different cheating measures, (3) we investigated the effectiveness of an incentivized, non-forced promise intervention, (4) we studied this intervention outside the lab in school settings, (5) we conducted our study in India, where a societal concern about cheating in academic settings is prevalent, and (6) we worked with Indian adolescents that are still largely under-represented in behavioral studies, despite comprising a fifth of the world's adolescents.

Study 1

Adolescents participated in the dice-box game, a child-friendly sender-receiver game, and they completed an honesty self-report. Half of the participants were assigned to the promise and the control condition, respectively. Participants played both games in the same condition. We implemented the promise condition in the dice-box game as described in the introduction. In the sender-receiver game, participants first acted as

senders and then as receivers (within-subject design). Therefore, we included two different game versions: a circle-game and a squares-game. Senders did not know that they would also play a variant of the game as receivers later on. In the promise condition of the sender-receiver game, participants had a choice between playing the game for a total of (a) 30 points or (b) 60 points conditional on the promise to report the rules truthfully to the receiver (control condition: the same choice without mentioning of a promise).

Methods

Participants. All participants were recruited from English-speaking medium schools in Pune, India. Pune is a city with 3.4 million inhabitants in the Indian state of Maharashtra. Participants in all studies were aged 10 to 14 years. In Study 1, $n = 200$ adolescents ($M_{age} = 12.6$ years, $SD = 0.92$, 96 [48%] females) from three schools participated in the experiments (for further details, see Supplementary Table S1). One additional adolescent was excluded due to learning disabilities. All adolescents who had received parental consent were invited to take part in the study[1].

The sample size was predetermined. Since our study implemented novel variants of previous paradigms, we aimed for a larger sample size than in many previous studies on cheating in adolescents (e.g., Bucciol & Piovesan, 2011; Evans & Lee, 2010)—but see the study by Glätzle-Rützler and Lergetporer (2015) for sample sizes similar to ours. An additional 24 adolescents took part in piloting sessions to ensure that materials and wordings were age-appropriate and understood by all participants (these results are not reported). The study was approved by the ethics committee of the Faculty of Education and Psychology at Freie Universität Berlin. Written, informed consent had been obtained from parents before adolescents took part in the study.

Procedure.

[1] It is possible that some adolescents did not take part because they were absent on the testing day or because the sample size had already been reached

Overview. Participants were tested in groups of four, seated well apart at individual tables in their school (for the set-up, see SI). All adolescents in a group participated in the same condition (balanced by gender and age for each school). Participants played two games: 1) the dice-box game and 2) the sender-receiver game (in fixed order). They also filled in a brief honesty self-report at the end of the study. All testing was conducted in English by the second author.

Figure 1. Example of a dice box used in the study - closed (left panel) and open (right panel). Participants privately picked one of 16 locations, shook the box, and wrote down the number of eyes on their chosen die.

Dice-box game. In the dice-box game, each participant played with a box containing 16 dice in a 4x4 grid. To prepare the materials, we used boxes from a letter word game and substituted the letter dice with regular dice. The boxes were taped shut and labelled with "top" and "down" (see Fig. 1). Participants privately picked one of the 16 locations, shook the box, and wrote down the number of eyes on their chosen die.

The experimenter handed each participant an instruction sheet that explained the dice-box game (see SI). After participants had read the instructions, the experimenter demonstrated the task once: she announced her chosen die location to the participants, shook the box and asked participants to state the number of eyes on the die in this location. Next, she collected the instruction sheets, and participants individually answered a set of comprehension questions to test their understanding of the dice-box

game (see SI).

Depending on the condition, participants received one of two versions of an answer sheet on which they could indicate how many points they would like to receive for each eye on their chosen dice throughout the game (see SI). Participants in the promise condition had a choice between:

> *Option A: For each dot, you receive 1/2 point. You have to do nothing more.*
> *Option B: For each dot, you receive 1 point. You will have to promise that you will tell the truth about how many dots there are on your die.*

Participants in the control condition instead had a choice between:

> *Option A: For each dot, you receive 1/2 point.*
> *Option B: For each dot, you receive 1 point.*

Participants in both conditions were also provided with an example to illustrate how many points each option would yield (see SI). We also reminded them that all eyes on the chosen die would be summed up after 15 rounds, that the points would be exchanged for real prizes, and that the more points they had the more prizes they would receive (see SI).

Once participants had chosen an option, each participant received a dice-box and played the game for 15 rounds. All adolescents in a group shook their boxes at the same time. For each round, their answer sheet listed the six possible die outcomes as pictures and participants indicated the number of eyes on their chosen die by circling the respective picture (see SI).

Sender-receiver game. In the sender-receiver game, participants played for points that—similar to the dice game—were later converted into prizes. Participants first played the part of senders and then—unannounced—the part of receivers. To test participants in both roles, we used two different versions of the sender-receiver game: a circle-game and a squares-game. Half of the senders played the circle-game and half of them played the squares-game (balanced across conditions). Senders who had played the circle-game played the squares game as receivers and vice-versa.

In the circle-game, the placement of a cross by the receiver inside, outside or on the line of a printed circle determined the outcomes for both sender and receiver (see SI). In case the cross was placed inside the circle, the sender received all the points. In case the cross was placed on the line of the circle, the points were equally split between the sender and the receiver. Finally, in case the cross was placed outside the circle, the receiver was awarded all the points.

In the squares-game, the receiver had an option to circle one, two or three printed squares to determine the outcomes for both sender and receiver (see SI). In case one square was circled, the sender received all the points. In case two squares were circled, the points were equally split between the sender and the receiver. In case three squares were circled, the receiver was awarded all the points.

In the sender role, participants first read instruction sheets explaining the general rules of the game (called the "message game"; see SI) as well as the specific rules of the circle-game or squares-game. The instruction sheets included visualisations to facilitate comprehension of the game sequence and pay-offs. In addition, the experimenter repeated the rules to the entire group of adolescents. Senders learnt that they would play the game with another student (receiver) from the same school and that both would remain anonymous to each other. Senders were further told that their message was the only information about game rules available to the receiver, and that the receiver's choice would determine the points for both players. Senders answered a set of comprehension questions to test their understanding of the sender-receiver game (see SI).

Depending on condition, participants received one of two answer sheets, on which they could choose for how many points they would like to play the game (see SI) - it should be noted that we assigned all participants to matching conditions in the dice-box and the sender-receiver game (promise/promise or control/control). Participants in the promise condition faced the following choice:

> *Option A: Play the game with a total of 30 points. You have to do nothing more.*

Option B: Play the game with a total of 60 points. You have to promise to the other student that you told the truth about how to play the game and the consequences of the decision in the game.

Participants in the control condition instead faced the following choice:

Option A: Play the game with a total of 30 points.
Option B: Play the game with a total of 60 points.

Once participants had indicated their choice, they wrote an open-form message to the receiver on a blank piece of paper and handed it to the experimenter in a sealed envelope.

In the receiver role, participants first received an instruction sheet that explained their role in the game and included visualizations to facilitate comprehension (see SI). Specifically, receivers learnt that they would play with another student (sender) from their school and that the receivers as well as the sender would remain anonymous. They were also told that they would receive a message from the sender explaining the rules of the game, and that their choices would determine the points for both the sender and the receiver. The instructions also emphasized that the rules of the game differed from the rules of the game that they had just played as senders. They then received a sealed envelope with the message of another participant from their school. They also received an answer sheet that, depending on the game variant, either showed a printed circle or three printed squares (see SI). Receivers then indicated their answers either by positioning a cross (inside, outside or on the circle in the circle-game variant) or by drawing a circle around a chosen number of boxes (one, two, or three in the squares-game variant). We did not tell receivers whether senders had made a promise or not.

Honesty self-report and socio-demographics. After completing the sender-receiver game in both roles, participants filled in an honesty self-report by indicating on a five-point-Likert scale how strongly they agreed with five different statements (see Table 1, Supplementary Figure S2). When giving informed consent,

parents provided socio-demographic information on, for example, their educational background and faith (for details, see SI). We entered these variables in a joint analysis of the dice-box game data from Studies 1–3.

Rewards. Participants played for points, but were unaware of the specific rewards and the conversion rate of points to rewards. We only handed out rewards after all eligible adolescents in a school had taken part in the study. In consultation with schools, we rewarded participants with stationary items (e.g., pencils, erasers). We used a conversion rate of 14 points per item, and participants received $M = 8.2$ items ($SD = 2.35$) in Study 1.

Data coding and analyses. Data were analyzed in IBM SPSS (Version 24.0.0.0, 64bit) and ESCI (Cumming, 2016).

We report the data from dice-box game as statistical over-reporting (hence "over-reporting"). Summed across 15 random dice rolls, the statistically expected number of eyes is 52.5—assuming honest reporting—and the maximum possible number is 90. We report results as the deviation of observed from expected outcome (with a possible maximum of $90 - 52.5 = 37.5$ eyes and negative values possible). While we cannot know for certain whether someone deliberately over-reported their results or was simply lucky; statistically, any positive deviation from 52.5 eyes is considered over-reporting.

Two independent coders, who were blind to condition, coded the 200 messages

Table 1

Items in the honesty self-report

No.	Item	Polarity
1	Today, I have told the truth in all of my answers.	positive
2	When talking to other students, I am honest.	positive
3	When talking to teachers, I am honest.	positive
4	Cheating is sometimes necessary in school.	negative
5	Cheating is sometimes necessary in life.	negative

sent by participants in the sender-receiver game. A message was scored as (a) *truthful* if the sender truthfully reported the rules and stated all three pay-off options or (b) *non-truthful* if the sender did not truthfully report the rules and did not state all three pay-off options. There were only two mismatches between the two coders (across 200 messages coded; $\kappa = 0.97$). These mismatches were moderated by the first author (blind to condition), who sided once with each coder.

Data availability. The dice-box task data for all three studies and the sender receiver game data for Study 1 can be found here [anonymous link for blind review]: https://osf.io/hzdfe/?view_only=e1ee38281a7b40b8bab5d39778b5bf7b. Note that socio-demographic variables were removed to protect participants' privacy. Data with socio-demographic information are available upon reasonable request by researchers at academic institutions.

Results

Dice game. Two participants in the promise condition provided incorrect answers to the comprehension questions for the dice-box task and were excluded from the analyses. The higher payment option (one point per eye) was chosen by nearly all participants in the control condition ($n_{1dc} = 98$) and by all participants in the promise condition ($n_{1dp} = 98$). We restricted analyses to participants who chose the higher payment rate.

Participants over-reported an average of $M = 10.40$ eyes ($SD = 7.93$) in the control condition (see Fig. 2). This corresponds to $M_r = 27.7\%$ of the maximum possible over-reported eyes (i.e. 37.5 eyes), which is close to the average of 21.6% cheating found in a recent meta-analysis (Abeler et al., 2016). Participants in the promise condition over-reported an average of $M = 7.19$ eyes ($SD = 7.99$; $M_r = 19.2\%$), a difference of 3.20 eyes (95% $CI = [0.96, 5.45]$, $t(194) = 2.82$, $p = 0.005$, two-sided). These results are confirmed by a non-parametric test (see SI).

Sender-receiver game. All participants gave correct answers to the comprehension questions for the sender-receiver game. The higher payment option was

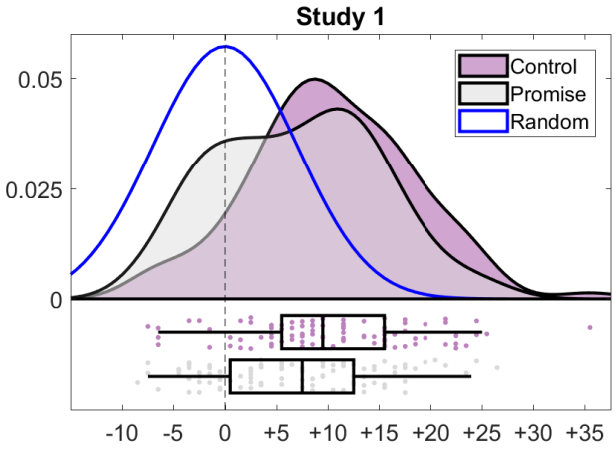

Figure 2. Distribution of over-reporting in Study 1: Dots represent individual over-reporting scores separated by condition (jittered vertically for readability only), the boxes show the three quartiles, and the horizontal lines include the inner 96% of the data. The colored curves depict a smoothed data distribution for the two conditions (y-axis corresponds to the relative frequency of scores above/below the expected average); the blue curve shows the expected distribution assuming randomly selected dice (with a mean of zero).

chosen by the majority of participants in both the control condition ($n_{1sc} = 84$ participants) and the promise condition ($n_{1sp} = 83$ participants). Messages were coded as fully truthful (i.e., communicating all rules and payment consequences correctly) or as non-truthful. Table 2 shows some examples of messages participants sent.

Participants in the promise condition who chose the higher payment option (i.e., promised to be truthful) sent fewer untruthful messages (5 out of 83, 6%, 95% $CI = [3\%, 13\%]$) than participants who chose the higher payment option in the control condition (17 out of 84, 20.2%, 95% $CI = [13\%, 30\%]$; $\chi^2(1) = 7.37$, $p = 0.007$; see Fig. 3). Conversely, participants who chose the lower rate in the promise condition (i.e., did not promise to be truthful) sent more untruthful messages (16 out of 17, 94%, 95% $CI = [73\%, 99\%]$) than participants in the control condition (4 out of 16, 25%, 95% $CI = [10\%, 50\%]$; $\chi^2(1) = 16.49$, $p < 0.001$). Across both conditions, participants who sent a truthful message had a higher score on the honesty self-report scale ($n = 158$, $M = 3.16$, $SD = 0.63$) than participants who did not send a truthful message

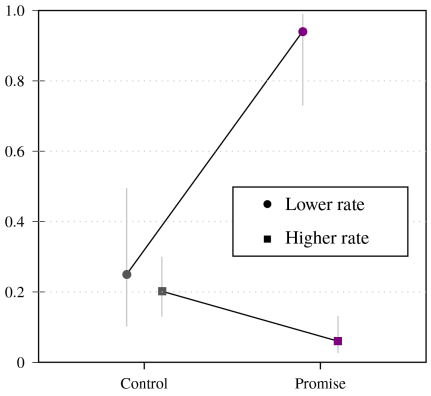

Figure 3. Proportions of messages coded as untruthful split by condition (control/promise) and by choice of payment rate. Markers indicate observed proportions with vertical lines showing their 95% CIs.

($n = 42$, $M = 2.83$, $SD = 0.70$, 95% $CI(\Delta) = [0.11, 0.55]$, $t(198) = 2.95$, $p = 0.004$).

Next, we analyzed outcomes for senders in the sender-receiver game by running an ANOVA with points received as dependent variable and main effects of payment choice (30 vs. 60 points), promise given (yes/no), truthful message (yes/no) and the interaction between promise given and truthful message in the model. We found significant main effects of truth-telling ($F(1, 195) = 9.84$, $p = 0.002$, partial $\eta^2 = 0.05$) and payment rate choice ($F(1, 195) = 3.91$, $p = 0.049$, partial $\eta^2 = 0.02$), but no significant effect of promise given ($F(1, 195) = 0.12$, $p = 0.73$, partial $\eta^2 = 0.001$) and no significant interaction ($F(1, 195) = 3.21$, $p = 0.07$, partial $\eta^2 = 0.02$). Both being untruthful and choosing the higher pay-off resulted in higher outcomes for senders.

To link the dice game and the sender-receiver game, we conducted an ANOVA with over-reported eyes as dependent variable[2] and condition and truth-telling in the sender-receiver game as factors. We found significant main effects of condition ($F(1, 192) = 6.72$, $p = 0.01$, partial $\eta^2 = 0.03$) and truth-telling ($F(1, 192) = 4.82$, $p = 0.03$, partial $\eta^2 = 0.02$), but no significant interaction ($F(1, 192) = 0.21$, $p = 0.65$, partial $\eta^2 = 0.00$). Participants in the promise condition and those who sent truthful messages over-reported fewer eyes.

[2] We excluded the four participants who chose the lower payment rate in the dice-box game or who failed the control questions in that game.

Table 2

Examples of messages sent by participants in the sender-receiver game in Study 1.

No.	Example	Code	Comment
1	If one box is circled then I will get the point. If two boxes are circled then we both will get half points. If all three boxes are circled then you will get all points.	True	—
2	If there is cross inside circle, I will get point and you nothing will get. If there is cross on the circle, I will get half point and you will get half point. If there is cross outside the circle, I will not get any point, all point will be your.	True	—
3	You have a circle in front of you. If there is a cross inside the circle you get all the points and I get nothing. If there is a cross outside the boundry of the circle I get all the points. If there is a cross on the boundry of the circle then I get all the points. Do what you want - choice is yours.	Untrue	None of the pay-off options are stated correctly; the pay-offs for making a cross inside and outside the circle, respectively, are presented in inverse.
4	If you do cross in the circle, I will get all the points. If the cross is on the circle, I will get half point and you will get half point. And if you do cross outside the circle, no one will get point, you also and me also.	Untrue	The sender fails to convey that a cross outside the circle will deliver all the points to the receiver.
5	Rules for the game: If 1 box is circled I will get all the points and you will get nothing. If 2 boxes are circled we both will get half of the points. If 3 boxes are circled you will get all the points and I will get twice of your points.	Untrue	Circling three squares would not give the sender twice as many points as the receiver.

Study 2

Study 2 investigated the link between cheating behavior and the consequences of this behavior, in particular, negative consequences for others. Past research has revealed contradictory findings regarding the impact of negative externalities on cheating behaviour, with some studies finding reductions in bribery and cheating (Barr & Serra, 2009; Meub et al., 2016; Senci, Hasrun, Moro, & Freidin, 2019) and others finding no reduction (Abbink, Irlenbusch, & Renner, 2002; Maggian, 2019). For example, Meub et al. (2016) reported lower levels of cheating when it harmed another participant (though effects were non-significant). In contrast, Maggian (2019) found no difference in cheating rates when it reduced the budget of the experimenter as compared to donations to a charity. In Study 1, cheating in the dice-box task only harmed the experimenter(s), while cheating in the sender-receiver game resulted in potential harm to another student. Our results indicated that behaviours in these two games were related, but this does not allow us to draw any direct conclusions about the role of negative externalities as the two tasks varied on a range of factors. We therefore introduced a negative externality into the dice-box task by announcing that we would subtract each point claimed by the participant from the fixed endowment of another student in the same school. Over-reporting thus resulted both in gains for the participant and losses for another student. If participants are reluctant to inflict costs on peers, we will find that cheating rates are generally reduced. In addition, participants who are sensitive to such moral cues may report honestly irrespective of the promise intervention, rendering it ineffective. Adolescents participated in the dice-box game and completed an honesty self-report. Half of the participants were assigned to the promise and half to the control condition, respectively.

Methods

Participants. All participants were again recruited from English medium schools in Pune, India. In this study, $n = 200$ adolescents aged 10 to 14 years ($M_{age} = 12.5$ years, $SD = 0.88$, 103 [52%] females) from five schools participated (for

further details see Supplementary Table S1). One additional group of four participants was excluded due to outside interference. The sample size was predetermined to be the same as in Study 1. The study was approved by the ethics committee of the Faculty of Education and Psychology at Freie Universität Berlin. Written, informed consent had been obtained from parents before adolescents took part in the study.

Procedure. The set-up and procedure were similar to those in Study 1 with the exception that participants only played the dice-box game and that we introduced a negative externality. Specifically, participants were told that they would be paired with another student from their school (identities remained mutually anonymous), that there was a limited number of points in the game, and that their gains would be subtracted from the other student's fixed initial endowment of points (see SI). We set the total endowment to 100 points and allocated the remaining points to other students in the school who had participated in the game (but were not tested together with the participant in a group of four students). Participants filled in the five-item honesty self-report (see SI). We used the same rewards and conversion rate as in Study 1. Participants received $M = 7.1$ items ($SD = 1.06$) in Study 2.

Results

One participant in the control condition provided incorrect answers to the comprehension questions and was excluded from the analyses. The higher payment option was chosen by the vast majority of participants in the control condition ($n_{2c} = 93$ participants) and in the promise condition ($n_{2p} = 99$ participants). We restricted analyses to participants who chose the higher payment rate. Participants over-reported an average of $M = 10.53$ eyes ($SD = 8.68$; $M_r = 28.1\%$) in the control condition and an average of $M = 8.00$ eyes ($SD = 8.52$; $M_r = 21.3\%$) in the promise condition (see Fig. 4), a difference of 2.54 eyes (95% $CI = [0.09, 4.99]$, $t(190) = 2.04$, $p = 0.042$). This result is confirmed by a non-parametric test (see SI).

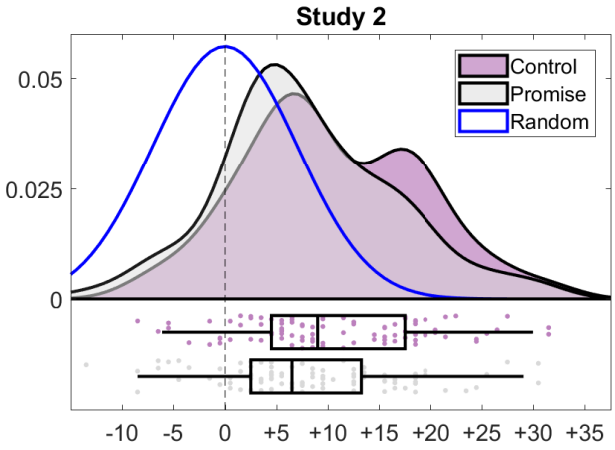

Figure 4. Distribution of over-reporting in Study 2 split by condition (see Fig. 1 for details).

Study 3

Competition describes a situation of scarcity, in which achieving one's goal excludes others from achieving their goal (Deutsch, 1949). Social comparisons of one's own achievement with that of others can lead to increased competition (Garcia, Tor, & Schiff, 2013). Past research has shown that envy for other's achievements can lead to deception in negotiations (Moran & Schweitzer, 2008) and that situational factors like ranking systems can increase unethical behaviors (Vriend et al., 2016), lead to destructive competition (Hafenbrädl & Woike, 2018), sabotage of the work of others (Charness et al., 2014) and lower rates of cooperation (Woike & Hafenbrädl, 2020). In academic contexts, a meta-analysis identified a positive relationship between perceived competition for grades and cheating in college students (Whitley, 1998). Given this, we wanted to test whether participants would cheat more in our task with competition, and, if this were the case, whether our promise intervention would remain effective. We created inter-group competition by randomly assigning participants to groups and awarding a special, attractive prize (gel pens and notebooks) to the group with the highest overall score. Adolescents participated in the dice-box game and completed an honesty self-report. We compared the special-prize group with a no-special-prize group, and assigned half of the participants per group to the promise condition and half to the

control condition, respectively (2x2 between-subject design).

Methods

Participants. All participants were recruited from English medium schools in Pune, India. In this study, $n = 240$ adolescents ($M_{age} = 11.8$ years, $SD = 0.91$, 120 [50%] females) from three schools participated (see Supplementary Table S1 for further details). The sample size was predetermined: we decided on a larger sample size than in Studies 1 and 2 due to the more complex design of this study. The study was approved by the ethics committee of the Faculty of Education and Psychology at Freie Universität Berlin.. Written, informed consent had been obtained from parents before adolescents took part in the study.

Procedure. The set-up and procedure were similar to those in Study 1 with the exception that participants only played the dice-box game and that we introduced a special prize. Specifically, half of the participants in each condition were assigned to the special-prize condition (see SI): They were randomly assigned to virtual groups of four students in their school (excluding those from the same test session) and told that the group with the highest number of points in their school would win a special prize (i.e., gel pens and notebooks—established to be highly desired items). Participants in the no-special-prize condition received the same instructions as in Study 1. Overall, this resulted in a 2x2 between-subject design with 60 participants per cell of the design. Special prizes were awarded in addition to the individually won prizes. Participants again filled in a five-item honesty self-report (see SI). We used the same rewards and conversion rate as in Study 1 for individual prizes. Participants received $M = 4.7$ items ($SD = 0.79$) in Study 3.

Results

All participants gave correct answers to the comprehension questions. The higher payment option was chosen by all participants in the control conditions (no-prize: $n_{3npc} = 60$; prize: $n_{3pc} = 60$) and by almost all participants in the promise conditions (no-prize: $n_{3npp} = 59$; prize: $n_{3pp} = 59$). A two-factorial ANOVA with over-reporting as

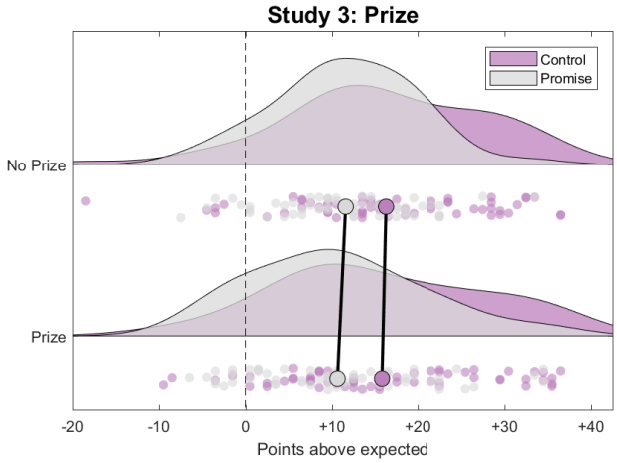

Figure 5. Comparison of over-reporting split by prize group (prize/no prize) with different colors for each condition (promise/control). Dots show individual over-reporting scores, curves show smoothed data distributions and large dots connected by lines show the means for each of the four conditions.

dependent variable and group (prize/no-prize) and condition (promise/control) as factors showed a significant main effect of condition ($F(1, 234) = 13.26$, $p < .001$, partial $\eta^2 = 0.05$), but a non-significant main effect of prize ($F(1, 234) = 0.27$, $p = .60$, partial $\eta^2 = 0.001$) and no significant two-way interaction of prize and condition ($F(1, 234) = 0.03$, $p = 0.86$, partial $\eta^2 = 0.00$). Fig. 5 demonstrates that the average over-reporting was, in fact, (non-significantly) smaller when a special prize was offered than when it was not offered. Collapsing across the prize groups, participants in the control condition over-reported an average of $M = 16.03$ eyes ($SD = 11.57$; $M_r = 42.7\%$), participants in the promise condition an average of $M = 11.09$ eyes ($SD = 9.12$; $M_r = 29.6\%$), a difference of 4.94 eyes (95% $CI = [2.28, 7.60]$, $t(236) = 3.66$, $p < .001$; see Fig. 6). This result is confirmed by a non-parametric test (see SI).

Combined analyses across Studies 1–3

We analysed the combined dice-box data from Studies 1–3 to test whether over-reporting was predicted by socio-demographic variables and by participants' honesty self-reports.

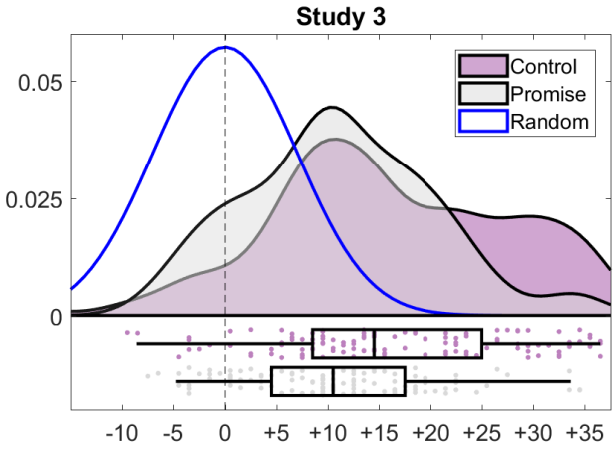

Figure 6. Distribution of over-reporting in Study 3 split by condition (see Fig. 1 for details).

Analysis

For our analysis, we simplified the socio-demographics data (see SI): (i) we created a binary variable for siblings (yes/no; 70% at least 1 sibling), (ii) we did not include school performance as we realized that many parents were unable to give a meaningful answer to this question (e.g., students are usually not ranked in their classes), (iii) we included only the three most frequently mentioned types of faith (Hinduism: 87% of the sample; Jainism: 5.6%; Buddhism: 1.7%) and collapsed all other answers (including no-answers) into a fourth category (5.6% of the sample), and (iv) we created a binary variable for parental education (university degree: yes/no; 72% of mothers and 68% of fathers had university degrees, respectively; missing values were replaced with the average across participants with degree information).

In all three studies, we asked participants to indicate on a five-point Likert scale how strongly they agreed with five statements about (dis)honest behavior (see Table 1). Each item was scored 0–4 with inverse scoring for negative items. The mean scale score (sum of items divided by the number of items) was 3.20 (SD = 0.60).

We used the dice-box data from all three studies and included only participants who had chosen the higher payment, passed the comprehension checks of the dice-box game and completed all items of the honesty scale, $n_{meta} = 622$.

We conducted an ANCOVA with (statistical) over-reporting as dependent variable and the following factors and covariates: condition (promise/control), prize (special prize/no special prize), negative externality (yes/no), age in years (rounded to the first decimal), gender, siblings (yes/no), father's degree (university degree/no university degree), mother's degree (university degree/no university degree), school, faith, and self-reported honesty. As in Studies 1–3, over-reporting was defined as the deviation of observed from expected outcomes across 15 rounds for each participant.

Results

Fig. 7 demonstrates that there was a stable effect of promises on reducing over-reporting across all three studies. The ANCOVA revealed that promises, school, age, faith and maternal education had the strongest effects on over-reporting (see Table 3). Specifically, promises, age, and maternal degree were related to decreases in over-reporting. The seven schools varied in their average over-reporting. The effect of faith is driven by a (relatively small) group of participants with Buddhist faith who over-reported less. The honesty scale was negatively related to over-reporting, but not significantly so. Additional analyses, including Promise×NegativeExternality and Promise×Prize interactions revealed the same significant main effects (see SI).

In addition, Fig. 8 shows that reported dice outcome distributions did not vary substantially across the fifteen rounds.

General Discussion

Across three studies, we found that a novel intervention, that gave participants a choice to promise to be truthful, reduced cheating behavior in Indian adolescents aged 10 to 14 years. The promise intervention was effective across two different tasks—a novel variant of a mind game (the dice-box game) and a child-friendly version of the sender-receiver game (Study 1). Moreover, the effect of promises remained stable when we introduced a negative externality (Study 2) or incentivized inter-group competition (Study 3). Overall, this confirms that promises have a strong, binding effect on behavior and extends previous findings with MTurk workers that kept their promise at

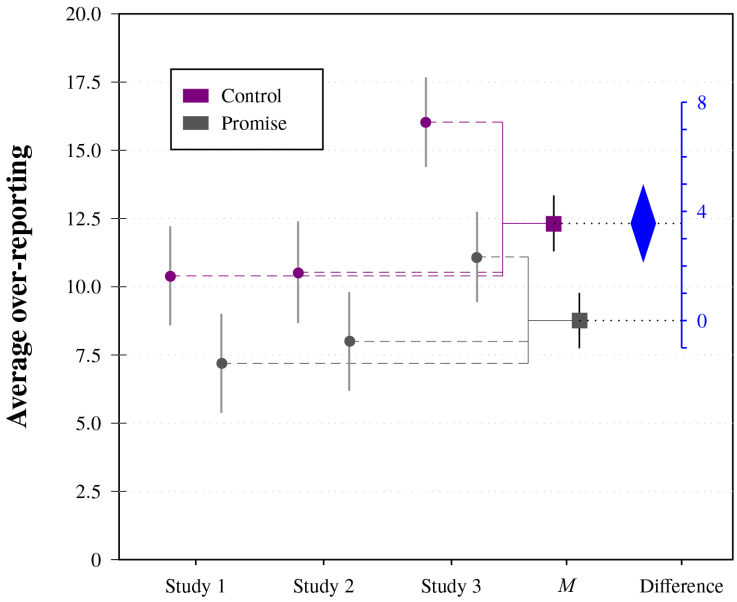

Figure 7. Differences in over-reporting between control and promise conditions across studies: Points show means for each condition with 95% CIs, squares show means across studies, the diamond reports mean and 95% CI for the difference shown on a floating axis (based on Cumming, 2016).

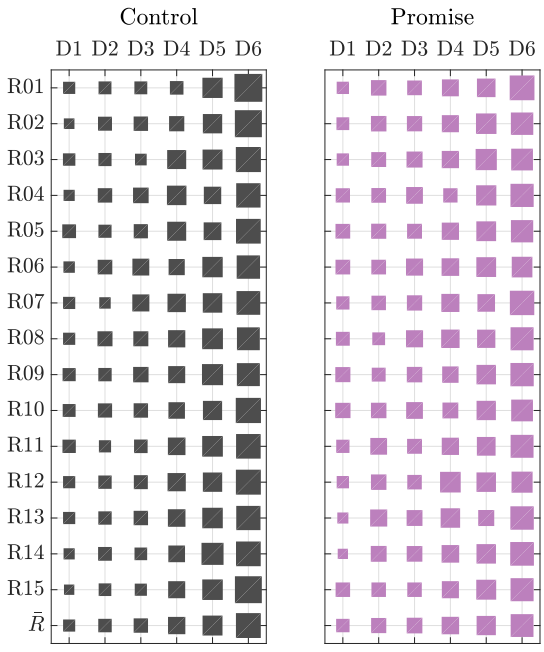

Figure 8. Distribution of individual dice results across studies split by round number and condition. The areas of squares are proportional to relative frequencies. Average scores across rounds are summarized in the bottom row.

Table 3

ANCOVA results for over-reporting scores across all three studies ($n_{meta} = 622$)

Source	B	Mean Sq.	df	F	p	part. η^2
Intercept	24.06	1166.19	1	14.29	<0.001	0.02
Promise Condition (0 = no, 1 = yes)	-3.49	1845.71	1	22.61	<0.001	0.04
Prize (0 = no, 1 = yes)	-0.41	10.23	1	0.13	0.723	0.00
Negative Externality (0 = no, 1 = yes)	0.45	15.22	1	0.19	0.666	0.00
Age in years	-0.87	331.68	1	4.06	0.044	0.01
Gender (0 = male, 1 = female)	-0.14	2.92	1	0.04	0.850	0.00
Siblings (0 = no, 1 = yes)	-0.61	43.44	1	0.53	0.466	0.00
Degree father (0 = no, 1 = yes)	0.24	4.59	1	0.06	0.813	0.00
Degree mother (0 = no, 1 = yes)	-2.91	437.81	1	5.36	0.021	0.01
School	m[a]	372.45	6	4.56	<0.001	0.04
Faith	m[a]	246.03	3	3.01	0.030	0.02
Honesty Scale	-0.49	48.40	1	0.59	0.442	0.00
Error		81.63	603			

Note.[a]Multiple coefficients, individual coefficients not reported.

a cost to themselves (Woike & Kanngiesser, 2019). Promises can thus be a powerful intervention to promote desirable behaviors (Bicchieri, 2002; Kulik & Carlino, 1987; Ostrom et al., 1992; Wang & Katzev, 1990) and to curb dishonesty.

Previous studies have investigated honesty-enhancing interventions such as reminders of university honor codes (Mazar et al., 2008) or reminders not to cheat (Bucciol & Piovesan, 2011), appeals to honesty (Talwar, Arruda, & Yachison, 2015), forced honesty oaths (Jacquemet, Joule, Luchini, & Shogren, 2013; Jacquemet, Luchini, Rosaz, & Shogren, 2018; Shu, Mazar, Gino, & Bazermann, 2012) or forced promises (Heyman et al., 2015; Kataria & Winter, 2013). Across three internal replications, we demonstrated the effectiveness of a new promise intervention that, in contrast to previously studied interventions, has the advantage of giving participants an option to

refrain from making a promise. It thereby respects individual autonomy and conscious decision making (Hertwig & Grüne-Yanoff, 2017). Moreover, our intervention clearly and unambiguously marks the commitment as a promise and is transparent about the nature of the commitment.

Our promise intervention featured a choice between a lower pay-off and a higher pay-off under the condition that adolescents promised to tell the truth. The control condition implemented a similar choice, but without the promise. We included this incentive to counter self-selection of potentially honest participants in the promise condition who may want to signal their honesty via promises (Ismayilov & Potters, 2016; van den Assem, van Dolder, & Thaler, 2012). In addition, the higher payment option created an incentive for potentially dishonest participants to choose this option and cheat. The vast majority of participants chose this higher payment option in all conditions, indicating that theses incentives worked as intended. It is possible that the promise to answer honestly and the fact that participants were paid more (to be honest) worked in conjunction to create the observed reduction in cheating in the promise condition. However, teasing apart the contribution of these two factors would prove difficult. Giving people simply a choice between a promise to be honest or no promise without any incentives would create the above mentioned self-selection of participants. Removing the choice altogether would force people to promise and undermine the deliberately voluntary nature of our intervention. One possibility for future studies could be to focus on the control condition and compare a condition with and without a choice. This could provide some insight into whether the choice situation by itself (irrespective of a promise) has an impact on cheating behavior.

Cheating rates have been found to vary across different experimental paradigms (Gerlach et al., 2019), but different paradigms are rarely presented within-subjects. We established in Study 1 that truth-telling in a sender-receiver game predicted lower rates of over-reporting in the dice-box task and that responses on the honesty scale predicted behavior in the sender-receiver game. However, our combined analyses across all three studies did not reveal an effect of responses on the honesty scale on over-reporting in

the dice-box task. It is possible that our self-report scale only predicted cheating behavior in the sender-receiver game because both measures are, in contrast to the dice-box task, language-based measures and to some extent directly observable. Additionally, the self-report scale we developed for this study may have been narrower in scope than other measures. For example, recent analyses have shown that the honesty-humility personality factor from the HEXACO scale strongly predicted dishonest behavior in experimental tasks for adults (Heck, Thielmann, Moshagen, & Hilbig, 2018). Further systematic, large-scale studies are needed to investigate the relation between behavioral and self-reported honesty measures in adolescents.

Our combined analysis of data from all three studies showed that over-reporting in the dice-box task decreased with age. These results are in line with previous findings for this age group which found a decrease in cheating rates from late childhood throughout adolescence (Evans & Lee, 2011; Glätzle-Rützler & Lergetporer, 2015; Maggian & Villeval, 2016). One previous study did not observe age effects in 5- to 15-year-olds, possibly because the study included a relatively small sample over a wide age range (Bucciol & Piovesan, 2011). Moreover, we found that adolescents in the control condition in Study 1 over-reported 27.7% of the maximum possible number of eyes, which is only somewhat higher than the average cheating rates found for adults in a recent meta-analysis (Abeler et al., 2016).

Our combined analysis further revealed that adolescent's gender had no significant effect on their over-reporting. To date, evidence for gender differences in adolescents' cheating behavior is mixed, with some studies finding no effects (Evans & Lee, 2011; Glätzle-Rützler & Lergetporer, 2015; Maggian & Villeval, 2016) and other studies showing significant gender differences (Cohn & Maréchal, 2018; Markiewicz & Gawryluk, 2019). We found that adolescents whose mothers had a university degree over-reported fewer points. There was no effect of paternal education—possibly, because mothers are often the primary caregivers. Maternal education may have positively influenced adolescents' own academic performance, and higher academic achievement has been linked to reductions in cheating behavior (Ruffle & Tobol, 2017). Faith also

influenced over-reporting, though this effect was mainly due to reduced over-reporting by a small group of participants of Buddhist faith and we are hesitant to draw strong conclusions from this small sub-sample. We also found that over-reporting varied between schools, yet, to date we cannot say which aspects of the school environment resulted in this variation. Overall, these findings open up exciting avenues for future studies on dishonest behavior in academic settings (Cohn & Maréchal, 2018) such as the role of school environments or adolescents' academic performance.

Negative externalities (Study 2) and incentives for competition (Study 3) had no significant impact on cheating rates or the effectiveness of the promise intervention. Past research has revealed a mixed picture of the effect of negative externalities on cheating or bribery (Barr & Serra, 2009; Meub et al., 2016; Senci et al., 2019; Abbink et al., 2002; Maggian, 2019)—our findings are in line with recent work showing that cheating is not affected by whether it reduces the experimenter's budget or donations to a charity (Maggian, 2019). Furthermore, while past research has identified a positive relation between competition and academic cheating (Whitley, 1998) and found that competition can increase unethical behaviors (e.g., Vriend et al., 2016; Moran & Schweitzer, 2008), we found no effect of incentives to compete on cheating rates. While participants knew that there was an incentive to perform better than everyone else, they did not receive direct feedback on their own performance or their ranking within the school (Garcia, Tor, & Gonzalez, 2006; Woike & Hafenbrädl, 2020). Future studies could investigate the effects of rank feedback on cheating in adolescents.

Participants in our study attended English middle schools and were tested in the main language of instruction (English) since we wanted to investigate dishonesty in academic settings. English is one of the two official languages in India. Generally, linguistic diversity is the norm in India and children regularly encounter more than one language in their everyday lives. Previous research with adults has found that testing participants in their second (non-native) language affected moral judgements and cheating in an experimental task (Geipel, Hadjichristidis, & Surian, 2016; Bereby-Meyer et al., 2018). For example, Bereby-Meyer et al. (2018) found that adults from Israel,

Korea, Spain and the US cheated less in their second language as compared to their native language. However, it is currently unclear whether findings from predominantly monolingual settings can be applied to the Indian context. Future work could determine whether language has an impact on levels of honesty in multilingual settings.

Our newly developed cheating paradigms offer a series of advantages over previous paradigms: The dice-box game and the child-friendly version of the sender-receiver game can be easily employed across a wide age range—from late childhood to old age—to explore, for example, dishonest behavior across the life span (Mata, Josef, & Hertwig, 2016) or its inter-generational transmission (Chowdhury, Sutter, & Zimmermann, 2018). Furthermore, the dice-box game is very portable, easy to implement and does not require a computer set-up or other technical equipment. It can thus be employed both in the lab and under challenging field settings. It also gives each participant the protection of reasonable doubt by producing no electronic or paper trails of true results—in fact, observation is impossible. This has the advantage that participants can play the dice-box game in full view of the experimenters or other participants. Even in environments, where other researchers use deceptive paradigms, the logical impossibility of observation in the dice-box task should reassure participants that their responses cannot be fact-checked.

To conclude, we implemented two new, child-friendly variants of cheating measures and demonstrated the effectiveness of a new promise intervention to promote more honest behavior. Importantly, all measures employed in the current study are deception-free. Given the ethical mandate to avoid participant deception when in any way feasible (American Psychological Association, 2017), the measures used in our study have a privileged claim of fulfilling ethical standards of psychological experimentation. Our paradigms also address concerns about deceptive practices negatively impacting participants' trust and performance (Hertwig & Ortmann, 2001, 2008), which seems particularly appropriate in studies on honesty and trustworthiness.

Author contributions

JKW, PK, and JS designed the study, JS collected the data, JKW analyzed the data and prepared the figures, PK wrote the paper and JKW provided critical feedback.

Acknowledgements

We thank all participating schools for their support. We further thank Süheyla Yilmaz and Carmen Kaiser for coding the messages in the sender-receiver game, and Katarzyna Dudzikowska for proof-reading the manuscript. P.K. was supported by a Freigeist Fellowship from VolkswagenFoundation.

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