Collaborative Dishonesty: A Meta-Analysis

Margarita Leib[1], Nils C. Köbis[2], Ivan Soraperra[1], Ori Weisel[3], & Shaul Shalvi[1]

[1]University of Amsterdam

[2]Max Planck Institute for Human Development

[3]Tel-Aviv University

Address correspondence to: m.leib@uva.nl, University of Amsterdam, Roetersstraat 11, 1018WB Amsterdam, The Netherlands. Acknowledgment: The study was supported by the European Research Council (ERC-StG-637915) under the European Union's Horizon 2020 research. We are grateful to the authors who shared their data and have answered our queries during the preparation of this manuscript. We thank Mikael Kataja and Franek Madler for their assistance with screening the manuscripts.

Abstract

Although dishonesty is often a social phenomenon, it is primarily studied in individual settings. However, people frequently collaborate and engage in mutual dishonest acts. We report the first meta-analysis on collaborative dishonesty, analyzing 87,771 decisions (21 behavioral tasks; k = 123; $n_{\text{participants}}$ = 10,923). We provide an overview of all tasks used to measure collaborative dishonesty, and inform theory by conducting moderation analyses. Results reveal collaborative dishonesty is higher (i) when financial incentives are high; (ii) in lab than field studies; (iii) when third parties experience no negative consequences; (iv) in the absence of experimental deception; and (v) when groups consist of more males and (vi) younger individuals. Further, in repeated interactions, group members' behavior is correlated —participants lie more when their partners lie—and lying increases as the task progresses. These findings are in line with the justified ethicality theoretical perspective, suggesting prosocial concerns increase collaborative dishonesty, whereas honest-image concerns attenuate it. We discuss how findings inform theory, setting an agenda for future research on the collaborative roots of dishonesty.

Public Significance Statements

We present the first meta-analysis on collaborative dishonesty—when groups can lie to increase the group's profits—covering 87,771 decisions made by 10,923 people engaging in 21 different experimental tasks. Results reveal various situational and personal factors shape collaborative dishonesty, including, for example, that group members affect each other's behavior over time. We propose that prosocial and honest-image concerns drive collaborative dishonesty.

Keywords: Collaboration; Honesty; Group Decision Making; Behavioral Ethics; Morality.

Collaborative Dishonesty: A Meta-Analysis

Whereas dishonesty is often a social phenomenon, it is mostly studied in individual settings. The last decade has seen extensive multidisciplinary work in psychology, economics, management, and related fields studying the extent to which people are dishonest in settings where dishonesty is independent of others (Abeler et al., 2019; Gerlach et al., 2019; Jacobsen et al., 2018; Gino & Ariely, 2016; Suchotzki et al., 2017). However, dishonesty often depends on others and requires collaboration. For instance, in Volkswagen's emission scandal, engineers collaborated with their peers to manipulate emissions test results to meet US standards (Goodman, 2015). The damage from releasing the highly polluting vehicles to US roads alone was estimated at 59 early deaths and $450 million (Barrett et al., 2015).

Although collaboration has many benefits, it also has potential pitfalls. Collaborating with others fosters trust (Kramer, 1999), builds strong relationships (Baumeister & Leary, 1995), and helps individuals achieve complex goals they cannot achieve alone. At the same time, collaboration can be a fertile ground for dishonesty (Karg et al., 2020; Lohse & Simon, 2019; Pulfrey et al., 2018). Indeed, people lie more when collaborating than when working alone (Conrads et al., 2013; Korbel, 2017; Weisel & Shalvi, 2015), and in collaborative settings, dishonesty is contagious, spreading among group members (Gross et al., 2018). As such, examining collaborative dishonesty meta-analytically is important both theoretically and practically.

We define collaborative dishonesty as lies conducted in (i) a group setting, (ii) where more than one group member can misreport the true state of the world, (iii) group members' outcomes are interdependent, and (iv) at least one group member benefits from the group's dishonesty. Collaborative (dis)honest decisions can be made in several ways: First, groups can communicate and *jointly* reach one mutual decision. For example, a committee may collectively decide which employee deserves an annual bonus. Second, groups can make *simultaneous* decisions, in which each group member makes a separate decision affecting the entire group, and all decisions are made at the same time. For example, declaring their travel expenses for a group business trip, employees' declarations can be submitted separately. However, for the expenses to be approved and reimbursed, declarations should be similar (assuming the employees stayed at the same hotel). Third, groups can make *sequential* decisions, in which each group member makes a separate decision, and decisions are made one after the other. For example, one group member writes a report, and other group members need to approve it. In all three decision structures, groups can be honest or collaboratively lie to benefit some, or all, of the group members.

The current meta-analysis

The current meta-analysis is the first to study collaborative dishonesty and has two goals: to provide an overview of the tasks used to study collaborative dishonesty and inform theory about the factors shaping it. First, we provide a comprehensive overview of the paradigms used to measure collaborative dishonesty. Our review identified 21 different experimental tasks that measure collaborative dishonesty in financially incentivized settings, where dishonesty is not sanctioned. These tasks are employed across 34 manuscripts, 123 treatments, with 10,923 participants making 87,771 decisions. We categorized all identified tasks into one of the three decision structures: (i) joint, (ii) simultaneous, or (iii) sequential.

Second, we inform and advance leading theories on collaborative dishonesty, namely, the neoclassical economic and the justified-ethicality perspectives. Testing the neoclassical economic perspective, which suggests that people pursue their narrow self-interest, we examine the overall level of collaborative dishonesty. Testing the justified-ethicality perspective, which suggests that people have both prosocial and honest-image concerns, we examine how various situational and personal factors moderate collaborative dishonesty, as well as the social influence and development of collaborative dishonesty over time.

Overview of the paradigms measuring collaborative dishonesty

Tasks measuring collaborative dishonesty involve a group of people holding information about the state of the world, which they can misreport to increase the group's payoff. These tasks often build on commonly used individual-level tasks, expanding them to group settings. Group members decide in one of three decision structures: joint, simultaneous, or sequential. Table 1 provides a summary of the three decision structures' characteristics, and Table 2 provides an overview of the tasks used to measure collaborative dishonesty.

Table 1. Decision-structure characteristics

Category	Joint	Simultaneous	Sequential
Group membership	Each participant is part of a group of at least two participants.		
Decision structure	The group makes one joint decision.	Each group member makes a decision, and decisions are reported simultaneously.	Each group member makes a decision, and decisions are reported sequentially. That is, one group member makes a decision, then other group member(s) learn about the decision(s) and make a decision themselves.
Dishonesty	The group has the opportunity to misreport the true state of the world.	Each group member has the opportunity to misreport the true state of the world.	
Outcome interdependence	N/A	Outcomes are interdependent: at least one group member's outcome depends on the reports of other group members.	
Incentive	Misreporting is beneficial (financially or otherwise) for at least one group member.		

Joint decision structure: Groups of participants interact with one another, either face to face or via an online platform, and then make one collective decision. For example, Muehlheusser et al. (2015) employ a group version of a die-rolling task (Fischbacher & Föllmi-Heusi, 2013). In this task, groups roll one die in private and jointly report the outcome. The group members' payment depends on the outcome the group jointly reports. Reporting "1" corresponds to a payoff of €1 for each group member, reporting "2" to a payoff of €2, and so on, whereas reporting "6" leads to a payoff of €0. Because only the group members know the actual die-roll outcome, they can honestly report the outcome or decide together to lie and boost the group's pay.

Simultaneous decision structure: Each group member makes a decision individually and simultaneously. Decisions are interdependent and determine the group's outcome. For example, Kocher et al. (2018) study groups of three participants who observe the same die-roll outcome on their individual computer screens, and are asked to report the outcome. In this task, as in some other (but not all) simultaneous tasks, group members are able to communicate via an online chat before making their decision. If all group members report the same outcome, they receive payment based on the reported outcome (reporting "1" yields €2 for each group member, reporting "2" yields €4, and so on, whereas reporting "6" leads to a payoff of €0). If group members report different outcomes, they earn nothing. As in joint decisions, participants can either report the die-roll outcome honestly or misreport it to increase the group's pay.

Sequential decision structure: Each group member makes a decision individually and sequentially. Decisions are interdependent and determine the group's outcome. For example, Weisel and Shalvi (2015) study a dyadic die-rolling task. In this task, pairs of participants, randomly assigned to the role of a first mover and a second mover, interact with one another. The first mover privately rolls a die and reports the outcome to the second mover. The second mover learns about the first mover's report, privately rolls a die, and reports the outcome as well. If both dyad members report the same outcome (a double), they get paid according to the double's worth (a double "1" yields €1 for each dyad member, a double "2" yields €2, and so on, with a double "6" yielding €6 for each dyad member). If dyad members report different outcomes, they earn nothing. This task is commonly employed over multiple rounds, with dyads receiving feedback about their payoff after each round. Similar to joint and simultaneous decisions, participants can misreport their outcomes to increase the group's pay.

Table 2. Description of the tasks measuring collaborative dishonesty

Task number	Description	Employed in	# Treatments	# Participants	# Groups	# Decisions
Joint decision structure						
1	Group die-rolling task: a group of two or three participants rolls a die together and reports one outcome per group. All (or some) group members get paid based on the reported outcome.	***Muehlheusser et al., (2015)** *Korbel (2017) *Beck et al., (2020) *Dannenberg & Khachatryan (2020)	15	1,303	495	495
2	Group version of the sender-receiver game (Gneezy, 2005): a group of two or three participants decides together between sending an honest yet less profitable or deceptive yet more profitable message to their opponent.	***Sutter (2009)** *Cohen et al., (2009)	8++	351	129	129
3	Field study: Tables with two diners at a restaurant. When paying, diners receive too much change. Collaborative dishonesty is measured by the proportion of diners who do not return the extra change.	***Azar et al., (2013)**	2	194	97	97
4	Dyads chat via an online platform. They are informed that due to some technical issues, the experimenter decides to give each pair a bonus pay. Dyads can opt-out of the bonus if they did not encounter the supposed technical issue. Dyads can chat about their experience and decide together whether to opt-out or keep the bonus. Because no actual technical issues occurred, keeping the bonus is an act of collaborative dishonesty.	***Nikolova et al., (2018)** Exp. 3	2	562	281	281
5	Dyads engage in a matrix task together in which they need to solve as many puzzles correctly as possible in a given time (the more correct puzzles they solve, the more the dyad earns). After completing the task, the dyad reports together, on one sheet of paper, how many matrices they	***Dunaiev & Khadjavi (2021)**	2	84	42	42

	solved. They can report honestly or inflate their reports to increase their pay.					
6	Dyads complete a quiz together on a computer, with more points gathered in the quiz corresponding to a higher chance of earning money. The quiz is designed such that all dyads are informed that they have obtained 45 points. Then, the computer provides false feedback indicating they obtained 54 points. Dyads are asked whether they saw any inconsistency in the number of recorded points. Not correcting the computer's inconsistency or correcting the feedback to any number other than 45 is classified as collaborative dishonesty.	***Nikolova et al., (2018)** Exp. 1	1	148	74	74
Simultaneous decision structure						
7	Groups of three interact. Then, they separately engage in a matrix task in which they need to solve as many matrix puzzles as possible in a given time. The more correct solutions they report, the higher the group's earnings. Participants can either report honestly or lie and inflate the number of correct puzzles they solved.	****Hildreth et al., (2016)** Exp. 1a, 1b, 3a, 3b, 5a, 5b	17	697	232	697
8	Groups of three observe the same die-roll on a computer screen, and each group member is asked to report the outcome. Before making their reports, the group can chat for several minutes (via a computer chat). If all group members report the same outcome, they get paid based on the reported outcome. If group members report different outcomes, they get nothing.	***Kocher et al., (2018)** *Bonfim (2018) *Bonfim & Silva (2018) *Bonfim & Silva (2019) *Castillo et al., (2020)	9	663	266	798
9	Each participant completes a matrix task individually in which they need to solve as many puzzles correctly as possible. Individuals then count how many puzzles they solved, shred the test sheet, and write down the number of solved puzzles on a collection slip. Then, participants are assigned to groups of two or three and show each other their collection slips. Next, groups sum up the total number of puzzles they solved and write it down on their	**Gino et al., (2013)** Exp. 1	2	65	27	65

	own collection slips. Each group approaches the experimenter together and submits their collection slips. Each group member is paid half/a third of the joint reported performance.					
10	Participants individually complete a quiz with 50 multiple-choice questions, with more correct answers corresponding to higher pay. Then, they are paired in dyads, sit next to each other, and grade each other's quiz. Participants were either encouraged to discuss during the grading stage (or not). Grading more correct answers than actually given in the quiz is an act of collaborative dishonesty.	***Dong et al., (2018)**	4	160	80	160
11	Dyads roll a die and report the outcome simultaneously. If they report the same outcome, they get paid based on the outcome they report. If they report different outcomes, the participant who reported the higher outcome gets the pay corresponding to the lower outcome and vice versa.	***Barr & Michailidou (2017)**	1	90	45[+]	90
12	Groups of six participants individually roll a die and report the outcome. If the sum of all six members is ≥ 18, they get paid. Otherwise, they do not get paid.	***Conrads et al., (2017)**	1	67	11[+]	67
13	In dyads, each participant rolls a die and reports the outcome. Each dyad member gets half of the sum of the dyad's reports.	***Conrads et al., (2013)** Team treatment	1	132	66[+]	132
14	In dyads, each participant rolls a die and reports the outcome. One participant gets paid half of the sum of her own report plus the outcome of a random die-roll. The other participant gets paid half of the sum of her own report plus the number reported by the other participant.	***Conrads et al., (2013)** Team-mix treatment	1	266	130[+]	266
15	Participants are assigned to groups of three. Each person independently rolls a die and reports the outcome simultaneously. If all three report the same outcome, they get paid according to the outcome they report. If they report different outcomes, they get nothing. The task is	***Rilke et al., (2021)** Simultaneous treatment	1	123	41	3,690

	repeated for 30 rounds, with feedback about earnings after each round.					
Sequential decision structure						
16	Dyadic die-rolling task: Participants are assigned to dyads, with one of them being the first mover (FM) and the other the second mover (SM). FM rolls a die and reports the outcome to SM. SM rolls an independent die and reports the outcome as well. If both report the same outcome (a double), they get paid based on the double's value. If they report different outcomes, they get nothing. The task is typically employed over multiple rounds (10-30), with feedback about earnings after each round. Some treatments vary the payoff scheme such that one dyad member gets paid (i) a fixed amount if the dyad reports a double, regardless of the double's value, or (ii) a fixed amount regardless of whether a double was reported or not.	***Weisel & Shalvi (2015)** *Della Valle et al., (2017) *Wouda et al., (2017) *Soraperra et al, (2017) *Gross et al., (2018) *Spadaro et al. (2021) *Burghoorn et al., (2021) *Verwijmeren et al., (2021) *Gross & De Dreu (2021) *Nieper et al., (2021)	47	3,922	1,584	68,500
17	Die-in-the-box: Participants are assigned to dyads with a first mover (FM) and a second mover (SM). FM rolls a die and reports the outcome. SM observes the die-roll outcome FM *rolled* and *reported* and is asked to report *the outcome FM rolled*. If both report the same outcome, they get paid based on the reported outcome. If they report different outcomes, they get paid based on the lowest outcome reported.	***Irlenbusch et al., (2021)**	4	860	430	860
18	Participants are assigned to groups of three, with first, second, and third movers (FM, SM, TM). FM rolls a die and reports the outcome to SM. SM rolls an independent die and reports the outcome to TM. TM rolls an independent die and reports the outcome as well. If all three report the same outcome, they get paid according to the outcome they report. If they report different outcomes, they get nothing. The task is employed over 30 rounds, with feedback about earnings after each round.	***Rilke et al., (2021)** Fully Sequential treatment	2	225	75	6,750

19	Participants are assigned to groups of three, with one first mover (FM) and two second movers (SM1, SM2). FM rolls a die and reports the outcome to both SMs. SM1 and SM2 each roll an independent die and report the outcome at the same time. If all three report the same outcome, they get paid based on the outcome they report. If they report different outcomes, they get nothing. The task is employed over 30 rounds, with feedback about earnings after each round.	***Rilke et al., (2021)** Partially sequential treatment	1	123	41	3,690
20	Unethical loyalty game: Participants are assigned to dyads with a first mover (FM) and second mover (SM). FM gets a target outcome (1-6), rolls a die, and reports whether they got the target outcome. If FM reports "yes," SM checks if FM was honest. FM earns nothing for saying "no," and between 10-15 monetary units for saying "yes." Before SM checks, FM can transfer an amount of money to SM. SM earns the amount FM transferred if they approve FM's report. Otherwise, they both earn nothing.	***Thielmann et al., (2020)**	1	288	144	288
21	Participants are assigned to dyads, with one of them being the first mover (FM) and the other the second mover (SM). FM tosses a coin twice and reports the outcome to SM. SM tosses an independent coin twice and reports the outcome. A "win" is defined as reporting two "heads." If both FM and SM report a "win," each earns 0.5 monetary units (MU). If both report "lose," each earns 0.05 MU. If one reports "win" and the other "lose," they have a 50-50 chance to earn 0.5 or 0.05 MU.	**Kerzenmacher (2017)**	1	600	300+	600

Note. Description of the tasks measuring collaborative dishonesty, the manuscripts that employed them, and the number of treatments, participants, groups, and decisions per task. The manuscript appearing in bold is the first to introduce each task. *Received raw data, including group ID. **Received raw data, not including group ID. +Matching groups are simulated. ++Sutter (2009) employs three treatments (T1, T2, T3) with varying group sizes (2 or 3 participants). In our analyses, we treat every treatment and group-size combination as a separate treatment. In two tasks (task #8 and #16) several treatments (4 in each task) were implemented as a within-subject design. Accordingly, 45 groups in task #8 and 28 groups in task #16 took part in two treatments. In Table 2 we treat every combination of group and treatment as a separate group.

Theoretical perspectives on collaborative dishonesty

Neoclassical economics suggests that when deciding between being honest or lying, people pursue their narrow financial self-interest and try to maximize their profits to the full extent possible. Namely, people weigh the financial profits from lying against the likelihood of getting caught and the expected punishment if caught (Becker, 1968). When the expected benefits outweigh the costs, people lie. Conversely, when the costs exceed the benefits, they are honest. Accordingly, when dishonesty is financially incentivized and detection and punishment are absent, people are expected to maximize their profits by lying to the full extent possible. Furthermore, the narrow self-interest perspective predicts that the level of collaborative dishonesty would not be influenced by having groups make decisions in different decision structures, or by a variety of situational and personal factors, which we review below.

Psychological theory suggests that even when people can profit from dishonesty, they like to view themselves as honest individuals (Mazar et al., 2008; Fischbacher & Föllmi-Heusi, 2013), and thus rely on justifications when deciding to lie (Shalvi et al., 2011; 2015). Accordingly, the justified-ethicality perspective (Leib & Shalvi, 2020) suggests two main motivations that shape people's likelihood of engaging in collaborative dishonesty: (i) prosocial concern towards one's group member should amplify it, whereas (ii) honest-image concerns should attenuate it. First, people are motivated to be prosocial and to benefit others (Van Lange, 1999). The motivation to act in a prosocial way allows building long-lasting meaningful (Baumeister & Leary, 1995; Kameda et al., 2005) and healthy (Schneider et al., 2011) relationships, increases trust (Kramer, 1999; Rempel et al., 1985), and leads to successful negotiation outcomes (Bazerman et al., 2000; Halevy et al., 2020). The motivation to benefit others, however, also increases the extent to which people feel lying is justified, and the extent to which people are willing to lie to profit others (Wiltermuth, 2011; Gino et al., 2013). Thus, settings with higher levels of prosocial motivation towards one's group members are predicted to amplify collaborative dishonesty. Second, people are motivated to maintain an honest self (Mazar et al., 2008) and public image (Abeler et al., 2019). The idea is that people feel comfortable lying, as long as doing so does not pose a threat to their self or public image as honest individuals. When their honest self or public image is threatened, people tend to lie less (Abeler et al., 2019). Accordingly, settings with higher levels of honest-image concerns are predicted to attenuate collaborative dishonesty.

Table 3. Predicted level of collaborative dishonesty: Theoretical perspectives and key meta-analytical findings

Perspective / Factor	Theory: Narrow self-interest	Theory: Justified-ethicality – Prosocial concerns	Theory: Justified-ethicality – Honest-image concerns	Findings: Support for
Decision structure	No impact; lying to the full extent	Joint > simultaneous Joint > sequential	Joint < simultaneous Joint < sequential	Prosocial concern
Negative consequences		Negative consequences → less lying		Prosocial and honest-image concern
Group size		Larger groups → more lying	Inconclusive: Larger groups → more or less lying	Neither
Financial incentives		Higher incentives → more lying	Higher incentives → less lying	Prosocial concern
Payoff alignment		Aligned > non-aligned	Aligned = non-aligned	Honest-image concern
Study type (lab vs. online vs. field)		Lab = online = field	Lab > field Online > field	Honest-image concern
Experimental deception		Deception < No deception		Prosocial and honest-image concern
Gender		All men group = all women group	All men group > all women group	Honest-image concern
Age		Older → more lying	N/A	Neither
Social influence and development over time		(1) More [less] lying when interacting with a lying [honest] partner (2) More lying over time		Prosocial and honest-image concern

Note. The predictions made by the theoretical perspectives are presented under 'Theory' in the table. The theoretical perspective supported by the analyses reported in the current meta-analysis are presented under 'Findings' (see 'Group-level analyses' in the 'Results' section for details).

The current meta-analysis allows assessing the extent to which the accumulated data support the narrow self-interest and/or the justified-ethicality perspectives (see Table 3). The narrow self-interest perspective would be supported by a maximal level of collaborative dishonesty that is not moderated by various situational or personal factors. The justified-ethicality perspective would be supported if we find that collaborative dishonesty is moderated by situational and personal factors that should vary the level of prosocial and honest-image concerns. Importantly, the two types of concerns, prosocial and honest-image, are not mutually exclusive and can both influence the level of collaborative dishonesty. For some factors, prosocial and honest-image concerns yield similar predictions; for other factors, the two concerns yield different predictions. Next, we elaborate on the various factors we examined and their predicted effects in shaping collaborative dishonesty based on narrow self-interest, prosocial, and honest-image concerns perspectives. To test the extent to which collaborative dishonesty is driven by the three theoretical accounts, we focus on settings in which lie detection and punishment are absent (for a similar approach, see Gerlach et al., 2019). Doing so allows isolating the psychological mechanisms of prosocial and honest-image concerns, ruling out the fear of punishment as an alternative motivation for participants' decisions.

(i) Decision structure. Considering the three decision structures we elaborated on, the narrow self-interest perspective predicts that the decision structure should have no impact on the level of collaborative dishonesty. The reason is thatit predicts people will lie to the maximum extent possible in all decision structures.

The prosocial concerns perspective suggests that decisions made in joint settings (vs. simultaneous or sequential) will lead to higher levels of collaborative dishonesty. The reason is that acting as a group is more salient in the joint setting, where group members interact directly and need to reach one mutual decision. Indeed, people bond when working together (Spoor & Kelly, 2004) and find value in reaching consensus (Herrera-Viedma et al., 2017). Further, empathy and similarity with others increase people's likelihood of engaging in group-serving dishonesty (Shalvi & De Dreu, 2014; Irlenbusch et al., 2021).

The honest-image concern perspective suggests that decisions made in joint settings (vs. simultaneous or sequential) will lead to lower levels of collaborative dishonesty. The reason is that in joint settings group members have to make one decision together, which means that all group members know when the group lies. For the group to consider lying, at least one group member must suggest lying to the rest of the group, leaving little room for maintaining an honest public image.

(ii) Negative consequences for third parties. When groups engage in collaborative dishonesty, they can harm different parties. In some cases, groups' lies harm the organization they work for, for instance when groups over-report travel expenses. In an experimental context, such cases are equivalent to settings in which groups' earnings are paid from the experimental budget and thus harm only the experimenter. In other cases, groups' lies can harm third parties such as other members of society or the environment, as in the Volkswagen emission scandal. The experimental equivalent to such cases is when groups' earnings are paid from a budget that would otherwise go to other participants or to a charity donation.

According to the narrow self-interest perspective, the negative consequences for third parties generated by the group's collaborative dishonesty should have no impact on the level of collaborative dishonesty, because people are only concerned with the impact of their decisions on their own outcomes, not others'.

The prosocial concerns perspective suggests that negative consequences for third parties will reduce levels of collaborative dishonesty due to the harm inflicted on others. Indeed, people are averse to harming others (the do-no-harm principle; Baron, 1995) and avoid doing so in both individual (Van Beest et al., 2005) and group settings (Halevy et al., 2012). Thus, compared to situations in which collaborative dishonesty is being paid from the experimental budget, when it harms third parties, people's prosocial concerns will be directed not only towards their group members, but also towards the third parties that are being harmed. In turn, situations in which third parties are harmed will reduce levels of collaborative dishonesty. Initial work reveals that indeed, when collaborative dishonesty harms a charity, groups lie less than when it does not (Castillo et al., 2020).

Similar to the prosocial concerns perspective, the honest-image concern perspective also suggests negative consequences for third parties will reduce collaborative dishonesty, due to the difficulty of maintaining a positive self and public image when one's actions harm others. Specifically, people lie to the extent that they and others can consider them as moral individuals. Harming others reduces people's ability to engage in collaborative dishonesty while feeling moral, thus reducing the level of collaborative dishonesty (Shalvi et al., 2015; Hilbig & Hessler, 2013; Shalvi et al., 2011).

(iii) Group size. Groups can be composed of various numbers of individuals, making group size a natural factor to examine. Considering group size, the narrow self-interest perspective predicts that group size should have no impact on the level of collaborative dishonesty, because it predicts people will lie to the maximum extent possible.

The prosocial concerns perspective suggests larger groups will lead to higher levels of collaborative dishonesty due to the ability to benefit more people with one's lies. Indeed, compared with benefiting only the self, people lie more when others can benefit from their lies (Weisel & Shalvi, 2015; Wiltermuth, 2011; Conrads et al., 2013), and the more individuals they can benefit, the more people lie (Gino et al., 2013).

The honest-image concerns perspective provides an inconclusive prediction regarding how group size affects collaborative dishonesty. On the one hand, larger groups may increase image concerns (Wu et al., 2016) and thus decrease collaborative dishonesty, as more individuals are exposed to one's dishonesty in larger compared with smaller groups. On the other hand, larger groups may increase collaborative dishonesty due to peoples' ability to diffuse responsibility for their actions (Bandura, 1999; Bartling & Fischbacher, 2012). Indeed, making decisions in groups, rather than alone, reduces the psychological burden of individuals' responsibility (El Zein et al., 2019). Thus, compared to smaller groups, larger groups allow people to feel that their share of responsibility for engaging in wrongdoing is smaller (Feldman & Rosen, 1978; Darley & Latané, 1968; see also Feess et al., 2020).

(iv) Financial incentives to lie. When groups engage in collaborative dishonesty, the extent to which they can profit from doing so varies. For instance, groups can earn either small or large amounts of money, depending on the amount their company offers as a bonus for their performance. In an experimental context, groups can earn varying amounts depending on the incentives experimenters set across different experiments. The narrow self-interest perspective predicts the financial incentive should have no impact on the level of collaborative dishonesty, because it predicts that people will lie to the maximum extent possible as long as there is an incentive to lie and detection and punishment are absent.

The prosocial concerns perspective predicts that higher incentives will lead to higher levels of collaborative dishonesty, due to the increased ability to financially benefit oneself

(Hilbig & Thielmann, 2017) and others (Wiltermuth, 2011) when incentives are high, compared to low.

In contrast to the prosocial concerns perspective, the honest-image concern perspective suggests that higher incentives to lie will lead to lower levels of collaborative dishonesty, due to people's reduced ability to maintain an honest self and public image the more beneficial lying is. Specifically, higher profits from lying may threaten people's ability to view themselves as honest individuals (Mazar et al., 2008) as well as increase their fear that others view them as liars (Kajackaite & Gneezy, 2017).

(v) Payoff alignment among group members. Group members' payoffs can be aligned or misaligned. When payoffs are aligned, all group members earn the same amount from lying, and thus they all have the same financial incentive to lie. This is the case when group bonuses are split equally among all members, which may occur for instance if all group members have the same job or rank. In an experimental context, such cases are equivalent to settings where all group members earn the same amount based on the group's decisions. When payoffs are misaligned, group members can earn different amounts from one another, and thus some group members have a higher financial incentive to lie than others. This is the case when group bonuses are not split equally among its members, for instance, if bonuses are handed out based on one's position in the organizational hierarchy, and the group is composed of people from different hierarchal positions. In an experimental context, such cases are equivalent to settings in which different members earn different amounts based on the group's decision, for example, when one group member earns twice as much as the other.

The narrow self-interest perspective predicts that the payoff alignment among group members should have no impact on the level of collaborative dishonesty, because it predicts people will lie to the maximum extent possible as long as dishonesty is financially beneficial for the group.

The prosocial concerns perspective suggests that compared with settings in which the payoffs among group members are not aligned, payoff alignment will lead to higher levels of collaborative dishonesty due to the increased sense of fairness and equality that group members feel when generating and obtaining an equal share of the profit. Work on inequality aversion indeed suggests that in general, people are averse to inequality (Bellemare et al., 2008) and prefer equal outcomes. As such, people might be less willing to lie when doing so creates inequality among group members. Accordingly, groups should lie more when all group members equally share the profits generated by collaborative dishonesty[1].

The honest-image concern perspective suggests no effect for payoff alignment, because people's concerns about viewing themselves, and others viewing them, as honest individuals, should not vary according to whether incentives are aligned or not. Initial results are mixed, finding that payoff alignment increases (Weisel & Shalvi, 2015), does not change (Beck et al., 2020), and decreases (Bonfim & Silva, 2018) collaborative dishonesty, rendering it especially worthy for meta-analytical examination.

(vi) Study type (lab vs. online vs. field). Across different experiments, studies are conducted either in a behavioral lab, on online platforms (Amazon Mechanical Turk or Prolific), or in the field. The narrow self-interest perspective predicts that the setting should have no impact on the level of collaborative dishonesty.

The prosocial concern perspective suggests that levels of collaborative dishonesty should be similar across all types of studies, because the benefits generated by collaborative dishonesty are similar across settings, provided the financial incentives remain constant.

The honest-image concern perspective suggests that field settings will lead to less collaborative dishonesty (compared with lab and online settings). The reason is that whereas participation in experiments is always voluntary, different settings may attract different people to participate. In lab and online studies, participants must sign up to participate in experiments. Thus, these settings may attract people who are motivated to increase their pay and are less concerned about their honest image. By contrast, in field studies, participants do not sign up but rather approached by the experimenters, reducing the ability to self-select into the experiment. Meta-analytical evidence on individual dishonesty indeed shows participants lie more in lab and online than in field experiments (Gerlach et al., 2019).

(vii) Experimental deception. In some experiments, participants receive full and accurate information about the situation they are in. In other experiments, participants are intentionally provided with, or led to believe, false information—known as experimental deception. Common forms of experimental deception include providing participants with false feedback during a task, or making participants believe they are interacting with other individuals or that their responses are fully anonymous, when in fact they are not. Gerlach and colleagues (2019) were the first to systematically examine whether experimental deception has a notable effect on a target behavior, specifically on dishonesty in individual settings, and facilitated a discussion on the ways in which methodology can shape behavior (see Gerlach et al., 2019 for a theoretical discussion; as well as Hilbig et al., 2021).

The narrow self-interest perspective predicts that as long as people do not fear detection and punishment, and believe the financial incentive to lie exists, experimental deception should have no impact on the level of collaborative dishonesty.

The prosocial concerns perspective suggests that compared with settings involving no deception, studies employing experimental deception should lead to lower levels of collaborative dishonesty, if participants sense they are deceived and suspect that they are not *actually* part of a group or that their lies do not *actually* benefit others.

The honest-image concern perspective suggests that studies employing experimental deception should lead to lower levels of collaborative dishonesty. If participants sense they are deceived, they may fear their decisions are not anonymous, and thus lying could threaten their honest public image toward the experimenter (Gerlach et al., 2019). Indeed, meta-analytical results from individual settings reveal that studies using experimental deception have lower levels of dishonesty than studies that do not use deception (Gerlach et al., 2019).

(viii) Gender. Groups can be composed of a varying proportion of men to women. The narrow self-interest perspective predicts that gender composition should not affect the level of collaborative dishonesty, because it predicts people will lie to the maximum extent possible, regardless of their gender.

The prosocial concerns perspective predicts the groups' gender composition should not affect collaborative dishonesty, because women and men exhibit similar levels of cooperation (Balliet et al., 2011).

The honest-image concerns perspective suggests all-women groups will exhibit lower levels of collaborative dishonesty than all-men groups. Women are penalized more than men for exhibiting assertive, profit-maximizing behavior (Bowles et al., 2007), and opt-out of financially incentivized competitive settings in patriarchal societies (Gneezy et al., 2009). Thus, relative to men, the societal expectation for women is to maintain their image as an honest person who avoids profit-maximizing behaviors. Indeed, women have been repeatedly found to be more honest than men in individual (Cappelen et al., 2013; Houser et al., 2012; Capraro, 2018; Gerlach et al., 2019, but see Kouchaki & Kray, 2018) as well as collaborative settings (Muehlheusser et al., 2015; Conrads et al., 2013).

(ix) Age. Groups can be composed of individuals of varying ages. The narrow self-interest perspective predicts no effect of group members' age on collaborative dishonesty, because it predicts people will lie to the maximum extent possible regardless of their age.

The prosocial concerns perspective suggests higher levels of collaborative dishonesty in older groups, because people tend to exhibit greater emotional empathy and prosocial behavior in late life (Sze et al. 2005). Indeed, the older participants are, the more money they give to their counterparts in the dictator game (Engel, 2010).

The honest-image concern perspective has no predictions on how image concerns vary with age. Interestingly, however, in individual settings, the older participants are, the more honest they are (Gerlach et al., 2019; Abeler et al., 2019).

(x) Social influence and development over time. Whereas in individual settings people make decisions alone, in group settings people may be affected by their group members. This can be especially the case when groups interact over time. Thus, we examine the social influence and the development of collaborative dishonesty over time.

The narrow self-interest perspective predicts that group members will not affect each other, and that collaborative dishonesty will not vary over time. The reason is that this perspective suggests collaborative dishonesty will be at the maximum level from the beginning of the interaction, leaving no room for any additional dishonesty to develop.

The prosocial concerns perspective suggests that a group member's dishonesty will increase with the dishonesty of other group members, due to a desire to reciprocate the (dishonest) effort of others and to increase the group's profits. Accordingly, if at least one group member lies, collaborative dishonesty is expected to increase over time. Indeed, reciprocity is one of the main drivers of cooperative behavior (Axelrod & Hamilton, 1981; Dawes, 1980; Nowak, 2006; Rand et al., 2009) even when reciprocating another's behavior requires engaging in acts of dishonesty (Leib et al., 2019). Dishonest acts of reciprocity can lead to a slippery slope in which people are increasingly willing to disregard ethical standards and consequently lie more (Garret et al., 2016; Welsh et al., 2015).

The honest-image concerns perspective similarly suggests that one group member's dishonesty will increase the dishonesty of other group members, due to the reduced image concern created when a corrupt norm is established. Specifically, in collaborative settings, people are exposed to others' behavior, which becomes the socially normative behavior (Bicchieri, 2016: Cialdini et al., 1990; Soraperra et al., 2017). Several studies show that social information about other people's ethical rule violations increases people's own propensity to break ethical rules (O'Fallon & Butterfield, 2012; Kocher et al., 2018; Gino et al., 2009, Köbis et al., 2015, 2019; Keizer et al., 2008; Gächter & Schulz, 2016; Ferrali, 2020; but see Dimant et al., 2020). Recent work on collaborative dishonesty indeed finds that one group member's lies increase the likelihood that the partner will also lie (Gross et al., 2018), and that having rule followers in a group mitigates the spread of collaborative dishonesty (Gross & De Dreu, 2021).

All in all, the current meta-analysis examines how various moderators shape the overall level of collaborative dishonesty across and within each of the three decision structures – joint, simultaneous, and sequential decisions. We further examine the social influence and development of collaborative dishonesty over time in repeated interactions. Our analyses focus on all behavioral tasks in which groups can financially profit from dishonesty, and where lie detection and punishment are absent (for a similar approach, see Gerlach et al., 2019).

Methods

Literature search

In January-February 2021, we conducted a literature search to identify eligible studies. We searched in Web of Science, APA PsycInfo, Econlit, Google Scholar, and ProQuest Dissertations and Theses database for manuscripts that include the following combination of search terms and Boolean operators in the text of the manuscript: [cooperat* OR collaborat* OR coordinat*] AND [dishonesty OR cheating OR lying OR deception OR unethical behavior OR corruption OR honesty OR ethical behavior]. We further searched in all databases for manuscripts that contain the terms "corrupt collaboration," "joint unethical acts," "joint dishonest acts," "dyadic die-rolling task," and "dyadic die-rolling game."

At the same time, we sent a call inviting colleagues to send us published or unpublished work that captures collaborative dishonesty using various tasks. We posted the call on the mailing lists of the *Society of Judgement and Decision-Making*, *European Association for Decision Making, Academy of Management* (Organizational Behavior), *Society for Personality and Social Psychology*, *European Association for Social Psychology*, *Economic Science*

Association, and the *International Association for Research in Economic Psychology*. This literature search and call for papers followed earlier literature searches and call for papers conducted in 2018 and 2020; see supplementary online materials (SOM) for details. The search covered various types of manuscripts, including journal articles, book chapters, PhD dissertations, master's theses, and working papers.

Our pre-registration and call for papers indicated that we are interested in collaborative dishonesty as captured by three decision structures: (i) joint, (ii) sequential, and (iii) simultaneous. The inclusion criteria for the joint decision structure were as follows: (a) Each participant is part of a group of at least two participants; (b) the group (containing two or more participants) makes one joint decision; (c) the group has the opportunity to misreport the true state of the world; and (d) misreporting is beneficial (financially or otherwise) for at least one group member. The inclusion criteria for the simultaneous decision structure were as follows: (a) Each participant is part of a group of at least two participants; (b) each group member makes a decision; (c) each group member has the opportunity to misreport the true state of the world; (d) group members report simultaneously; (e) outcomes are interdependent: at least one group member's outcome depends on the reports of one (or more) other group members; and (f) misreporting is beneficial (financially or otherwise) for at least one group member. Finally, the inclusion criteria for the sequential decision structure were as follows: (a) Each participant is part of a group of at least two participants; (b) each group member makes a decision; (c) each group member has the opportunity to misreport the true state of the world; (d) group members report sequentially: for instance, one group member makes a decision first, then other group member(s) learn about the decision(s) and make a decision themselves; (e) outcomes are interdependent: at least one group member's outcome depends on the reports of one (or more) other group members; and (f) misreporting is beneficial (financially or otherwise) for at least one group member.

Based on these inclusion criteria, we excluded manuscripts in which participants interacted with a simulated, pre-programmed "partner" instead of an actual participant (Ścigała et al., 2019; Jamaluddin et al., 2020; Schabram et al., 2018). Further, similar to a previous meta-analysis on dishonesty (Gerlach et al., 2019), we focus on settings where groups had a financial incentive (or equivalent, e.g., gifts, sweets) to lie, and lying was not sanctioned. Finally, we excluded experiments in which calculating the means and standard deviation for the group-level reports was not possible (e.g., due to all participants in a treatment being in one matching group; Hildreth et al., 2016, experiments 2a and 4), because conducting statistical analyses for these settings is not possible. Figure 1 provides a PRISMA chart (Moher et al., 2009) with an overview of the identification, screening, and selection process.

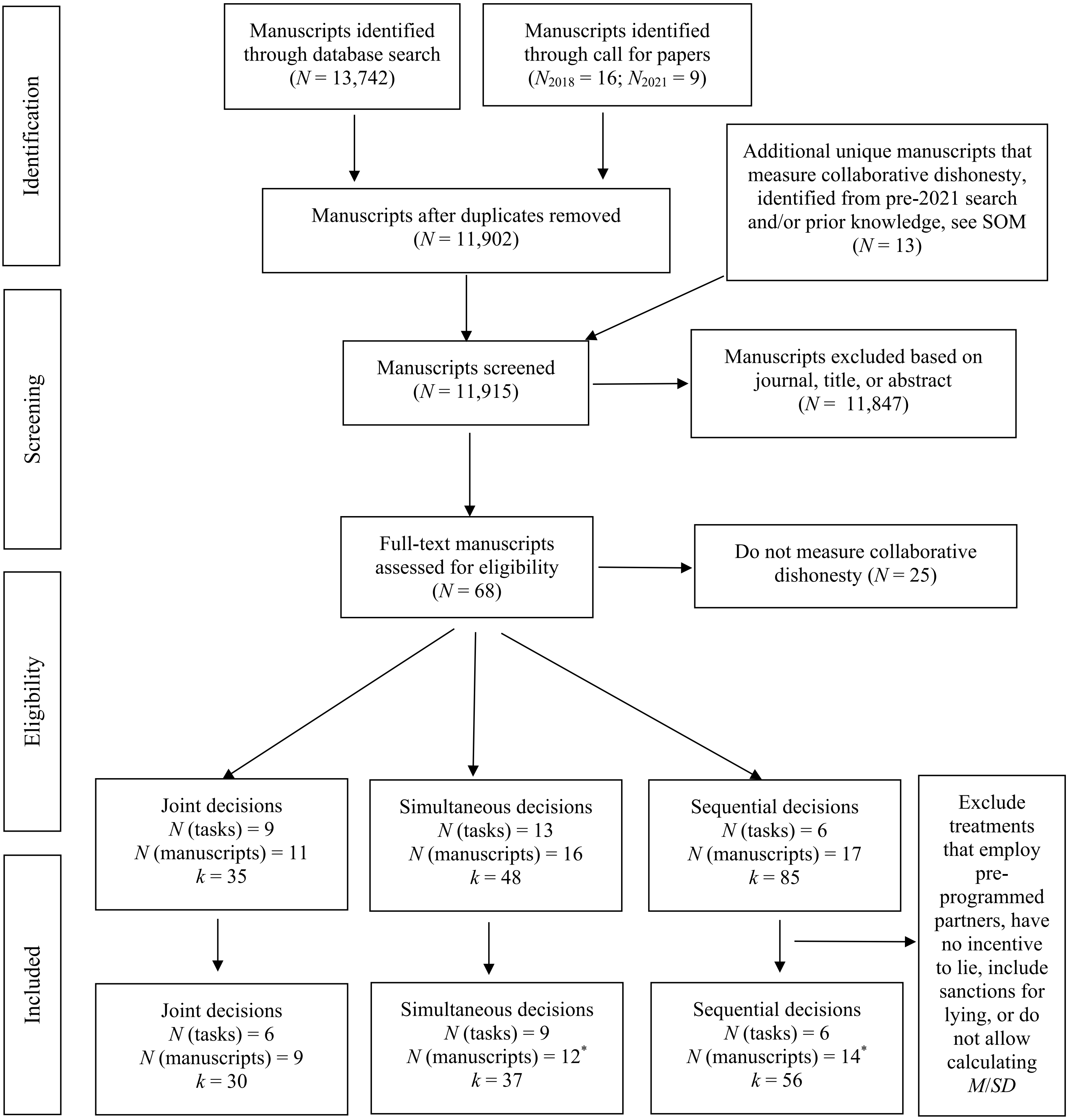

Identification
Screening
Eligibility
Included
Manuscripts identified through database search (N = 13,742)
Manuscripts identified through call for papers (N_{2018} = 16; N_{2021} = 9)
Manuscripts after duplicates removed (N = 11,902)
Additional unique manuscripts that measure collaborative dishonesty, identified from pre-2021 search and/or prior knowledge, see SOM (N = 13)
Manuscripts screened (N = 11,915)
Manuscripts excluded based on journal, title, or abstract (N = 11,847)
Full-text manuscripts assessed for eligibility (N = 68)
Do not measure collaborative dishonesty (N = 25)
Joint decisions N (tasks) = 9 N (manuscripts) = 11 k = 35
Simultaneous decisions N (tasks) = 13 N (manuscripts) = 16 k = 48
Sequential decisions N (tasks) = 6 N (manuscripts) = 17 k = 85
Exclude treatments that employ pre-programmed partners, have no incentive to lie, include sanctions for lying, or do not allow calculating M/SD
Joint decisions N (tasks) = 6 N (manuscripts) = 9 k = 30
Simultaneous decisions N (tasks) = 9 N (manuscripts) = 12* k = 37
Sequential decisions N (tasks) = 6 N (manuscripts) = 14* k = 56

Figure 1. PRISMA chart: literature search and screening process. *N* indicates the number of tasks/manuscripts; *k* indicates the number of treatments. *One manuscript (Rilke et al., 2021) included treatments with both simultaneous and sequential decisions and thus appears in the PRISMA chart in both structures.

Overall, we identified 34 manuscripts (12 unpublished, 35.29%), with 123 treatments, in which 10,923 participants made 87,771 decisions. Across all manuscripts, 21 different experimental tasks were employed to measure collaborative dishonesty; see Table 2. After identifying the relevant manuscripts, we obtained the raw data from 94.11% of the manuscripts (97.56% of the treatments) and used them to calculate the outcome variable (see "analytical approach" below). For the remaining manuscripts, we inferred the relevant information from the text, figures, and tables in those manuscripts.

Coding

Standardized group report. To compare collaborative dishonesty across different tasks, we computed a group version of the standardized report used in prior meta-analyses examining dishonesty (Gerlach et al., 2019; Abeler et al., 2019). Specifically, we calculated the standardized group report by employing the following calculation:

If group's report < report assuming honesty,

$$\text{Standardized group report} = \frac{group's\ report - report\ assuming\ honesty}{report\ assuming\ honesty - minimum\ possible\ report}$$

If group's report ≥ report assuming honesty,

$$\text{Standardized group report} = \frac{group's\ report - report\ assuming\ honesty}{maximum\ possible\ report - report\ assuming\ honesty}$$

Benchmarks and examples. For all tasks, the highest standardized group report is 100%, indicating that all groups in the treatment (for a treatment-level outcome), or a particular group (for a group-level outcome), earned the highest possible amount by engaging in collaborative dishonesty. The value 0% indicates that, on average, groups in a treatment (for a treatment-level outcome), or a particular group (for a group-level outcome), reported honestly. For the vast majority of the tasks (18 out of 21), the minimum standardized group report is -100%, indicating that all groups in the treatment, or a specific group, earned the lowest amount possible. For a minority of the tasks (3 out of 21, tasks #2, #3, and #4 in Table 2), the amount the group earns by being honest is equal to the minimum amount the group can earn. Accordingly, the range of standardized group reports for these tasks varies between 0% (indicating honesty) and 100% (indicating reporting the highest possible amount).

Consider the following examples for how the formula is implemented in three commonly used tasks. First, in the group version of a die-rolling task (Muehlheusser et al. 2015; see elaboration in 'Joint decision structure' section and task #1 in Table 2), if participants are honest, we would expect an average earning of 2.5 monetary units (MU; the expected payoff of a random die-roll when each dot on the die earns one MU, but reporting '6' leads to a payoff of 0); The minimum possible outcome is 0 MU (the payoff associated with reporting the least profitable outcome, "6"); and the maximum possible outcome is 5 MU (the payoff associated with reporting the most profitable outcome, "5"). For example, if a group reported a "4", the standardized group report was 60% [(4 - 2.5) / (5 - 2.5) = 0.6].

For the task introduced by Kocher et al. (2018; see elaboration in 'Simultaneous decision structure' section and task #8 in Table 2), if participants are honest, we would expect them to report the die-roll outcome which was displayed on the computer screen[2]; The minimum possible outcome is 0 MU (the payoff associated with group members reporting different values or all members reporting a "6"); and the maximum possible outcome is 5 MU (the payoff associated with all group members reporting the most profitable outcome, “5“). For example, if a group saw a “3“ on a computer screen, and all group members reported a “4“, the standardized group report was 50% [(4 - 3) / (5 - 3) = 0.5].

For the dyadic die-rolling task (Weisel and Shalvi, 2015; see elaboration in ‘Sequential decision structure’ section and task #16 in Table 2), if participants are honest, we would expect an average earning of 0.58 MU (3.5 [expected value of a random die-roll for a first mover] × 1/6 [the chance of a second mover to honestly match the first mover’s report]); The minimum possible outcome is 0 MU (the payoff associated with the dyad reporting different values); and the maximum possible outcome is 6 MU (the payoff associated with the dyad reporting the most profitable outcome, “6“). For example, if a group reported a double “4“, the standardized group report was 63% [(4 – 0.58) / (6 – 0.58) = 0.63].

When the experimental paradigm required participants to report a random die-roll or coin-flip outcome, we calculated the expected report if groups were honest and used that outcome as the “report assuming honesty” in the equation. When the paradigm required participants to report their performance (e.g., in a matrix task or a variation of it), we calculated the variable “report assuming honesty” based on the actual performance in the task, either by participants themselves (when their actual performance was recorded) or by participants in a control treatment in which lying was not possible (see similar approach by Gerlach et al., 2019).

We calculated the standardized group report on both the aggregated treatment level and for each independent unit of observation, and conducted separate analyses on each (see “analytical approach” below).[3] For one-shot tasks, an independent unit of observation was the group of participants interacting with one another. For tasks with multiple rounds, the independent unit of observation was all the reports made by the same group members across all rounds (if partners are fixed) or in a session (if partners are not fixed); see SOM for details. The standardized group report for each independent unit of observation was calculated for all the manuscripts in which (i) we obtained the raw data, (ii) the raw data contained a unique group ID, and (iii) the groups were not simulated (see the section on "simulated groups" below). For brevity, we hereafter refer to the independent unit of observation as "group."

Pre-determined groups. For the vast majority of the tasks (16 out of 21), individuals were matched into groups before engaging in the collaborative dishonesty task. We were able to calculate the standardized group report for all the tasks for which we obtained the raw data, and the raw data contained a unique group ID variable (14 of the 16 tasks). For the two remaining tasks (Gino et al., 2013; Hildreth et al., 2016), we used the data reported in Gerlach et al. (2019); see SOM.

Simulated groups. For a minority of tasks (5 out of 21), individuals were matched into groups merely to calculate participants' payoffs. For example, in one simultaneous task, group members were matched into dyads, reported a die-roll outcome, and earned half of the sum of both group members' reports (task #13 in Table 2; Conrads et al., 2013). In such settings, the timing of matching individuals into groups does not affect participants' behavior, because they do not interact or communicate with one another. To calculate the standardized group report and standard deviation for these tasks, we repeatedly rematched participants and calculated the mean standardized group report and standard deviation using the simulation. We ran 10,000 iterations of the simulation and used the average of the mean and *SD* of all iterations as our treatment-level variables. These tasks were not included in the group-level analyses. In Table 2, we indicate the tasks to which the standardized group report was simulated.

Analytical approach

Treatment-level analyses. For the treatment-level analyses, we assessed the (i) overall standardized group report and (ii) the heterogeneity for each decision structure (*joint*, *simultaneous*, and *sequential*). We conducted the analyses with meta (Schwarzer, 2007), metafor (Viechtbauer, 2010), and dmetar (Harrer et al., 2019) packages in R, with the default DerSimonian-Laird estimator for the random-effects model. We removed one treatment (Sutter, 2009; T2, groups of 3; with 21 participants, making 7 decisions) from the treatment-level analyses because all groups made the same decision, resulting in an *SD* equal to 0. We included this treatment in the group-level analyses. Overall, we conducted the treatment-level analyses on 34 manuscripts (k = 122) with 10,902 participants making 87,764 decisions. The treatment-level dataset and code are available on figshare: dataset and code.

Group-level analyses. To increase statistical power and examine nuanced effects, we conducted the moderation analyses on the group-level data. To account for the nested structure of the data, we specify random intercepts for each manuscript, treatment, and group (see regression tables for details). We conducted these analyses with lme4 (Bates et al., 2015) and lmerTest (Kuznetsova et al., 2017) packages in R. We removed 13 groups (from Castillo et al., 2020; task #8 in Table 2; with 39 participants, making 39 decisions) from the group-level analyses because those groups observed the highest value possible on the computer screen, and could thus not lie to further boost their pay. Overall, we conducted the group-level analyses on 28 manuscripts (k = 99) with 8,967 participants (3,766 groups) making 85,815 decisions.

Social influence and development over time. Lastly, we analyzed how group members affect each other over time and how collaborative dishonesty develops in repeated interactions. We focus on the most commonly used task that is employed in a repeated fashion, namely, the dyadic die-rolling task (task #16 in Table 2). We conducted the analyses on the raw data of all the treatments in which participants' payoffs were aligned. To account for the nested structure of the data, we specify random intercepts for each treatment, matching group, and dyad. We conducted these analyses with lme4 (Bates et al., 2015) and lmerTest (Kuznetsova et al., 2017) packages in R, and cover 32 treatments, 2,716 participants, making 51,824 decisions.

Results

Treatment-level analyses

Descriptive information. Table 4 presents the descriptive statistics (Means and *SD*s/proportions, medians, Modes, Ranges, and the number of treatments) for the standardized group report, all moderators, and additional study characteristics. The table presents these variables for all decision structures combined, as well as for each decision structure separately.

The data was collected mostly in WEIRD (Western, educated, industrialized, rich, and democratic; Henrich et al., 2010) countries. Specifically, data was collected in Germany (36.43% of the sample; 3,979 out of 10,923 participants), USA (18.52%), Netherlands (15.36%), Czech Republic (2.99%), UK (2.98%), Brazil (2.53%), Israel (2.51%), China (1.46%), and Denmark (1.32%). The remaining sample (15.89%) was either from mixed or unspecified countries. This was the case when the data was collected using online platforms (Amazon Mechanical Turk and Prolific) and the experimenter did not restrict the country of origin or did not specify doing so in the manuscript.

Table 4. Descriptive statistics for the standardized group report, moderators, and study characteristics

	Outcome	Mean (*SD*) / proportions	Median	Mode	Range	Number of treatments
Total	Standardized group report	35.59% (24.18)	33.33%	26.92 (n = 2)	-0.02% - 95.90%	123
	Negative consequences for third parties	73.17% no 26.82% yes	N/A	no (n = 90)	N/A	123
	Group size	2.38 (0.58)	2	2 (n = 79)	2 - 6	123
	Financial incentive to lie (max-min, 2015 USD, PPP)	$15.74 (13.93)	$11.55	$46.43 (n = 9)	$0.25 - $50.00	123
	Payoff alignments	76.42% aligned 23.57% misaligned	N/A	aligned (n = 94)	N/A	123
	Study type	72.35% lab 14.63% online 13.00% field	N/A	lab (n = 89)	N/A	123
	Experimental deception	73.17% no 26.82% yes	N/A	no (n = 90)	N/A	123
	Gender (average proportion of females in a group)	50.37% (36.14)	50.00%	50.00% (n = 925)	0.00% - 100.00%	72 (2,916 groups)
	Age (average age in a group)	26.31 (8.43)	24.33	23.00 (n = 120)	11.00 - 66.50	58 (2,547 groups)
	Repeated task	58.53% one shot 41.46% repeated	N/A	one shot (n= 72)	N/A	123
	Among repeated tasks, number of rounds in the task	19.33 (7.02)	20	20 (n = 25)	10 - 30	51
	Year the study was run in	2015 (3.44)	2015	2015 (n = 28)	2005 - 2021	123
	Published	29.26% no 70.73% yes	N/A	yes (n = 87)	N/A	123
Joint decisions	Standardized group report	43.40% (23.73)	46.52%	N/A	0.00% - 88.70%	30
	Negative consequences for third parties	53.33% no 46.67% yes	N/A	no (n = 16)	N/A	30
	Group size	2.40 (0.49)	2	2 (n = 18)	2 – 3	30
	Financial incentive to lie (max-min, 2015 USD, PPP)	$12.72 (12.26)	$8.55	$6.42 & $19.47 (n = 4)	$0.58 - $50.00	30
	Payoff alignments	80.00% aligned 20.00% misaligned	N/A	aligned (n = 24)	N/A	30

	Study type	80.00% lab 6.67% online 13.33% field	N/A	lab (n = 24)	N/A	30
	Experimental deception	76.67% no 23.33% yes	N/A	no (n = 23)	N/A	30
	Gender (average proportion of females in a group)	50.11% (37.10)	50.00%	50.00% (n = 298)	0.00% - 100.00%	30 (1,101 groups)
	Age (average age in a group)	27.12 (10.57)	24.67	11.33 (n = 30)	11.00 - 65.50	17 (767 groups)
	Repeated task	100% one shot	N/A	one shot (n = 30)	N/A	30
	Year the study was run in	2013 (4.67)	2014	2005 & 2015 (n = 6)	2005 - 2019	30
	Published	6.67% no 93.33% yes	N/A	yes (n = 28)	N/A	30
Simultaneous decisions	Standardized group report	25.43% (27.78)	12.72%	26.92% (n = 2)	-0.02% - 95.89%	37
	Negative consequences for third parties	72.97% no 27.02% yes	N/A	no (n = 27)	N/A	37
	Group size	2.86 (0.67)	3	3 (n = 28)	2 - 6	37
	Financial incentive to lie (max-min, 2015 USD, PPP)	$23.41 (17.57)	$13.35	$46.43 (n = 9)	$4.00 - $46.44	37
	Payoff alignments	81.08% aligned 18.91% misaligned	N/A	aligned (n = 30)	N/A	37
	Study type	67.56% lab 21.62% online 10.81% field	N/A	lab (n = 25)	N/A	37
	Experimental deception	29.72% no 70.27% yes	N/A	yes (n = 26)	N/A	37
	Gender (average proportion of females in a group)	50.31% (32.52)	58.33%	66.67% (n = 123)	0.00% - 100.00%	14 (374 groups)
	Age (average age in a group)	21.27 (5.19)	22.33	11.50 (n = 30)	11.00 - 35.33	13 (351 groups)
	Repeated task	97.29% one shot 2.70% repeated	N/A	one shot (n = 36)	N/A	37
	Among repeated tasks, number of rounds in the task	30 (0)	N/A	N/A	N/A	1
	Year the study was run in	2015 (2.41)	2015	2015 & 2017 (n = 10)	2011 - 2018	37
	Published	27.02% no 72.97% yes	N/A	yes (n = 27)	N/A	37

Sequential decisions	Standardized group report	38.12% (19.62)	36.78%	N/A	10.42% - 73.31%	56
	Negative consequences for third parties	83.92% no 16.07% yes	N/A	no (n = 47)	N/A	56
	Group size	2.05 (0.22)	2	2 (n = 53)	2 - 3	56
	Financial incentive to lie (max-min, 2015 USD, PPP)	$12.30 (9.61)	$9.18	$7.16 (n = 6)	$0.25 - $35.04	56
	Payoff alignments	71.43% aligned 28.57% misaligned	N/A	aligned (n = 40)	N/A	56
	Study type	71.43% lab 14.29% online 14.28% field	N/A	lab (n = 40)	N/A	56
	Experimental deception	100% no	N/A	no (n = 56)	N/A	56
	Gender (average proportion of females in a group)	50.59% (36.31)	50.00%	50.00% (n = 610)	0.00% - 100.00%	28 (1,441 groups)
	Age (average age in a group)	27.11 (7.28)	24.67	21.50 (n = 80)	18.50 - 66.50	28 (1,429 groups)
	Repeated task	10.71% one shot 89.29% repeated	N/A	repeated (n = 50)	N/A	56
	Among repeated tasks, number of rounds in the task	19.12 (6.93)	20	20 (n = 25)	10 - 30	50
	Year the study was run in	2017 (2.21)	2017	2015 (n = 12)	2013 - 2021	56
	Published	42.86% no 57.14% yes	N/A	yes (n = 32)	N/A	56

Note. The table summarizes all 123 treatments in the meta-analysis. The descriptive statistics of gender and age are restricted to the subsample of the data in which the experimenters recorded these variables and are calculated on the group level (see 'Gender' and 'Age' in the results section).

Standardized group report. On a treatment level, the standardized group report ranged between -2.00% and 95.90% and varied to a large extent between treatments. With the exception of one treatment, in all treatments, the standardized group report (on the treatment level) was above 0%, indicating that, on average, groups did not lie to reduce their payoffs. The average treatment-level standardized group report was 45.48% for the joint decision tasks (k = 29), 25.34% for the simultaneous decision tasks (k = 37), and 37.85% (k = 56) for sequential decision tasks. These results provide no support for the narrow self-interest perspective that predicts groups will lie to the maximum extent possible.

Heterogeneity. Similar to the results of other meta-analyses on group decision-making (Balliet & Van Lange, 2013) and dishonesty (Gerlach et al., 2019; Köbis et al., 2019), the heterogeneity of the standardized group reports was high. Combining the three decision structures, the percentage of effect-size variance that is not caused by sampling error (I^2) was 96.7%, and the variance between treatments (τ^2) was .069. Heterogeneity was also large within each decision structure: joint decision structure (I^2 = 92.2%, τ^2 = .061), simultaneous decision structure (I^2 = 98.3%, τ^2 = .151), and sequential decision structure (I^2 = 95.1%, τ^2 = .034).

A key reason for the high heterogeneity is that different tasks and experimental variations (treatments within a task) affect groups' reports. For instance, the standardized group report of all treatments employing the most commonly used task, the dyadic die-rolling task (task #16 in Table 2, k = 47), was 33.22%, whereas the standardized group report of all treatments employing the second most commonly used task, a simultaneous group version of a matrix task (task #7 in Table 2, k = 17) was 8.55%. Similarly, manipulations implemented to reduce/increase collaborative dishonesty influence groups' reports. For example, Gross and De Dreu (2021) pre-classified participants as rule followers or rule violators and then assigned them to dyads in which two rule followers (violators) interacted with one another. The standardized group report was 15.68% when two rule followers interacted with one another and 40.65% when two rule violators interacted with one another.

Outlier and influence diagnostics analyses (Viechtbauer & Cheung, 2010) did not flag any of the treatments as an outlier. Further, no particular treatment drove the high heterogeneity. Leave-one-out analysis—removing each treatment and recalculating the heterogeneity and effect size—reveals no single treatment affected the effect size much (I^2 ranged between 95.43% and 96.75%; effect size ranged between 35.44% and 36.28%). Conducting the analysis for each decision structure led to the same results (joint tasks: I^2 range: 89.69% - 92.49%; effect size range: 43.79% - 47.36%; simultaneous tasks: I^2 range: 95.78% - 98.31%; effect size range: 23.30% - 26.13%; sequential tasks: I^2 range: 94.58% - 95.16%; effect size range: 37.17% - 38.36%). These analyses suggest the high heterogeneity is driven by the overall variance in tasks and manipulations across treatments. Our analyses throughout the paper are conducted on the raw data and control for such heterogeneity by specifying random intercepts for each group, treatment, and manuscript.

Publication bias. Given the large proportion of unpublished manuscripts in the sample (12 out of 34; 35.29%), we tested for publication bias by evaluating whether the standardized group report significantly differed between published and unpublished studies. Meta-regression analysis reveals no difference between published (k = 86, 37.16%) and unpublished treatments (k = 36, 32.92%), p = .417. We find the same result within each decision structure (joint: p = .286, simultaneous: p = .811, and sequential: p = .800). These results do not indicate a publication bias that would emerge if authors publish only manuscripts with high levels of collaborative dishonesty.[4]

Group-level analyses

Table 5 presents regression models examining how various factors are associated with collaborative dishonesty overall (Model 1) and within each decision structure (Models 2-5). The theoretical perspectives supported by the analyses are summarized in 'Findings' in Table 3.

(i) Decision structure. Regression analysis reveals the standardized group report is somewhat higher in tasks measuring joint decisions (M = 49.75%, SD = 54.17) than tasks measuring sequential decisions (M = 33.39%, SD = 52.53), b = -.214, p = .058. We find no difference between joint and simultaneous decisions (M = 34.20%, SD = 54.46), p = .513, and no difference between simultaneous and sequential decisions, p = .302, see Table 5, Model 1 and Figure 2. These results are in the direction suggested by the prosocial concern perspective and are significant only at the 10% level.

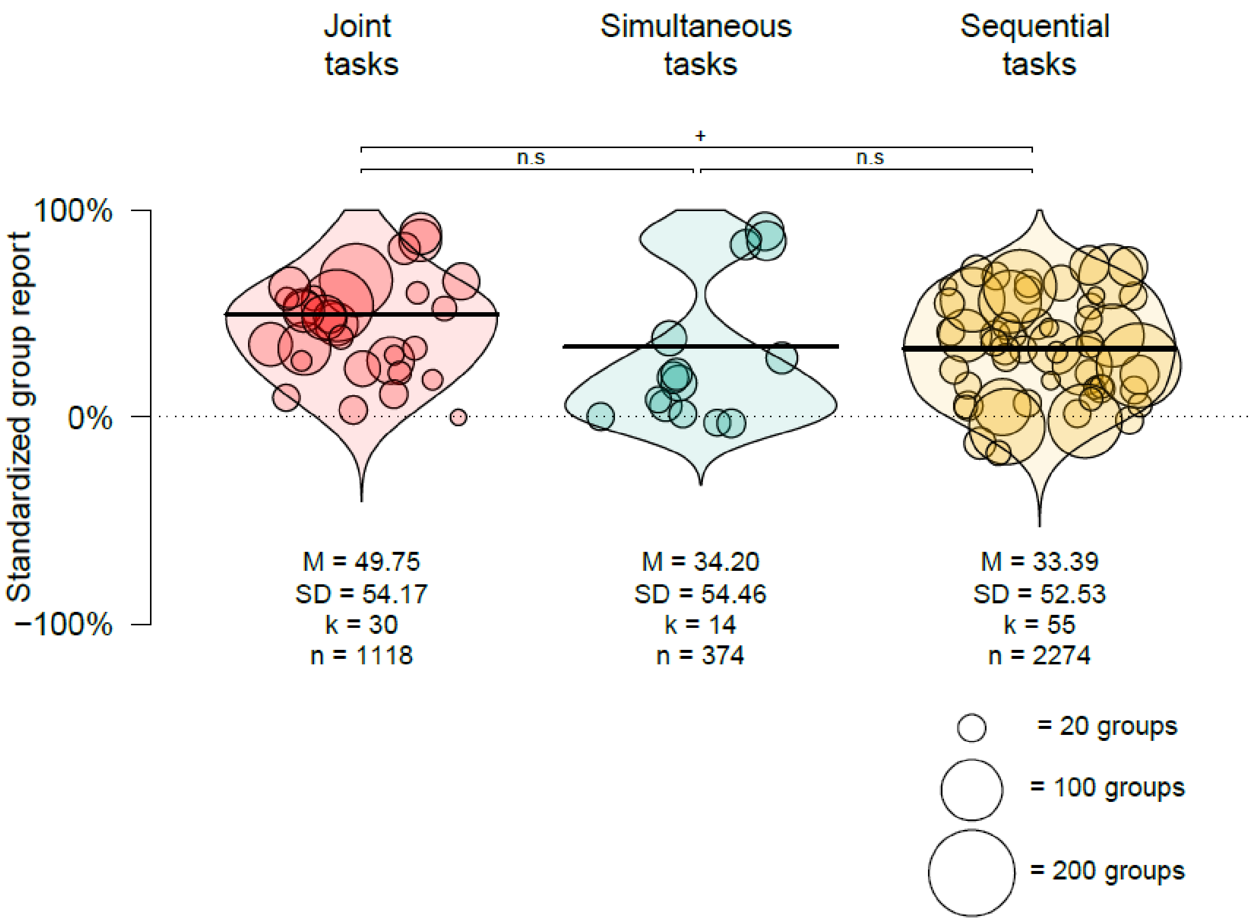


Figure 2. The standardized group report (calculated on the group level) in each of the decision structures.[5] Dashed lines represent honesty (standardized group report of 0%). Each bubble represents a treatment, and the size of the bubble is proportional to the number of groups in that treatment. *k* represents the number of treatments, and *n* is the number of groups. $^{+}p < .10$.

(ii) Negative consequences lying has for third parties. In some treatments, collaborative dishonesty affected the experimenters' budget (k = 74), whereas in other treatments, it inflicted harm on third parties, either other participants (k = 19) or charity donations (k = 6). We thus examined whether collaborative dishonesty varies as a function of whether or not lying harms third parties. Providing support for both the prosocial and honest-image concerns perspectives, results reveal that across all decision structures, when lying harmed third parties, groups' standardized reports were lower, b = -.175, p = .036 (Model 1, Table 5). Examining the effect in the different decision structures reveals that negative consequences for third parties did not affect collaborative dishonesty in the joint decision structure (p = .497; Model 2, Table 5), nor in the sequential decision structure (p = .073; Model 4, Table 5). In the simultaneous decision structure, negative consequences to third parties were associated with lower levels of collaborative dishonesty (b = -.698, p < .001, Model 3, Table 5).

(iii) Group size. Across all tasks and treatments, the group size was either two (k = 74) or three (k = 25). We thus examined whether the level of collaborative dishonesty differed among groups of two and three. Results reveal no significant effect for group size among all decision structures combined, nor for each decision structure separately, ps > .255 (Table 5, Models 1-4). Thus, the results are not in-line with the prosocial nor the honest-image concerns perspectives.

(iv) Financial incentive to lie. To examine how financial incentives shape collaborative dishonesty, we coded for each group the highest amount the group as a whole could earn from lying. In experiments in which groups could earn non-monetary rewards (e.g., gifts, sweets), we converted the non-monetary rewards to monetary amounts based on the estimations the authors of these experiments provided us. We then converted all amounts to 2015 USD purchasing power parity and subtracted the minimum amount the group could earn to calculate the financial incentive to lie. Lastly, when groups' earnings were determined based on their reports as well as on winning a lottery or a competition, we assumed the group had won.

Providing partial support to the prosocial concern perspective suggesting more dishonesty as incentives increase, results reveal that in the sequential decision structure, the amount groups could earn was positively associated with the standardized group report, b = .013, p = .007, Model 4, Table 5. That is, the standardized group report was 1.3 percentage points higher with every additional dollar the group could earn. The amount groups could earn was not associated with collaborative dishonesty for joint and simultaneous decision structures (Models 2 and 3, Table 5)[6].

(v) Payoff alignment among group members. In the majority of the treatments (*k* = 72), all group members' payoffs were aligned. That is, all group members earned the same payoff based on the groups' report, and group members could not earn different amounts from one another by behaving differently. In the remaining treatments (*k* = 27), group members' payoffs were misaligned. Results support the honest-image concern perspective, revealing no significant effect of payoff alignment for all decision structures combined, nor for each decision structure separately, *p*s > .123 (Table 5, Models 1-4).

(vi) Study type: Lab vs. online vs. field. To examine the effect of study type on collaborative dishonesty, we labeled each experiment either (i) lab, (ii) online (Mturk or Prolific), (iii) lab-in-the-field (traditional lab tasks implemented in the field), or (iv) field (experiments conducted outside of the lab, measuring contextualized, everyday behaviors). Only two treatments (from Azar et al., 2013) were labeled as field experiments; we thus collapsed the lab-in-the-field and field categories together.

Providing support for the honest-image concerns perspective, regression analysis reveals that across all decision structures, the standardized group report was higher in lab experiments (*M* = 46.52%, *SD* = 52.75) than in lab-in-the-field/field experiments (*M* = 12.69%, *SD* = 52.79, *b* = -.271, *p* = .006, Model 1, Table 5). Further, the standardized group report was higher in online (*M* = 37.50%, *SD* = 52. 79) than in lab-in-the-field/field experiments, *b* = -.374, *p* = .015. These results were driven by the sequential decision structure, in which the difference between lab and lab-in-the-field/field experiments was significant (*p* = .015, Model 4, Table 5). The difference between online and lab-in-the-field/field in sequential tasks did not reach significance (*p* = .121).

The difference between the standardized group report in lab compared with online experiments was not significant for all decision structures ($p = .444$, Model 1, Table 5), nor for the joint and sequential decision structures separately (*p*s > .279, Models 2 and 4, Table 5). The effect was not identifiable in simultaneous decisions (Model 3 in Table 5), because the study type was collinear with the financial incentive to lie and group size.

(vii) Experimental deception. We coded treatments as employing experimental deception when participants were intentionally misinformed. For example, in some treatments, participants were informed that their group was interacting with another group that did not actually exist (in a sender-receiver game; $k = 2$), observed a non-random die-roll outcome on a computer screen ($k = 5$), or received false feedback ($k = 3$). Treatments were also classified as involving experimental deception when participants were asked to report their performance in a task/quiz and were led to believe their actual performance was private and not known to the experimenter, when in fact, the experimenter had access to their actual performance ($k = 6$).

Providing support for both the prosocial and honest-image concerns perspectives, regression analysis reveals that across all decision structures, when experimental deception was implemented, groups lied less ($b = -.279$, $p = .028$; Model 1, Table 5). The effect was not significant when we examined the joint and simultaneous decision structures separately (*p*s > .504; Models 2 and 3, Table 5). The effect was not identifiable for the sequential decision structure because none of the treatments employing sequential decisions entailed experimental deception.

Table 5. The effect of situational factors on collaborative dishonesty

	Model 1 Standardized group report	Model 2 Standardized group report: **joint** decisions	Model 3 Standardized group report: **simultaneous** decisions	Model 4 Standardized group report: **sequential** decisions
Intercept	.620*** (.111)	.408+ (.168)	.487 (1.581)	.216* (.058)
Decision structure (baseline = Joint)				
Simultaneous	-.082 (.124)	--	--	--
Sequential	-.214+ (.107)	--	--	--
Negative consequences for third parties	-.175* (.082)	.151 (.207)	-.698*** (.121)	.176+ (.063)
Group size = 3	.012 (.095)	-.061 (.145)	.178 (.146)	.038 (.121)
Financial incentive to lie (Maximum minus minimum earning, 2015 USD, PPP)	.002 (.002)	-.001 (.004)	.005 (.041)	.013** (.003)
Misaligned payoff	-.099 (.063)	.019 (.164)	-.015 (.176)	-.043 (.053)
Study type (baseline = lab)				
Lab-in-the-field/field	-.271** (.096)	.099 (.238)	--	-.264* (.078)
Online platform (Mturk/Prolific)	.102 (.132)	.345 (.293)	--	-.066 (.067)
Experimental deception	-.279* (.120)	-.141 (.193)	-.514 (1.210)	--
Random intercepts for	Groups Treatments Manuscripts	Treatments Manuscripts	Groups Treatments	Group Treatments Manuscripts
# Decisions	85,815	1,118	4,609	80,088
# Participants	8,967	2,642	907	5,418
# Groups	3,766	1,118	374	2,274
# Treatments	99	30	14	55
# Manuscripts	28	9	7	13
Data	All tasks with (i) raw data (ii) group ID (iii) no simulated groups	All **joint** tasks with (i) raw data (ii) group ID (iii) no simulated groups	All **simultaneous** tasks with (i) raw data (ii) group ID (iii) no simulated groups	All **sequential** tasks with (i) raw data (ii) group ID (iii) no simulated groups

Note. Linear regression models testing the effect of situational factors on collaborative dishonesty for all decisions together (Model 1), as well as joint (Model 2), simultaneous (Model 3), and sequential (Model 4) decisions separately. Several treatments (4 in simultaneous, and 4 in sequential tasks) were implemented as a within-subject design. In these cases, groups took part in two treatments and thus had two values for the standardized group report (45 groups in simultaneous tasks and 28 groups in sequential tasks). To account for the nested structure of the data, we set random intercepts at the group, treatment, and manuscript level. In model 3, we did not set random intercepts for the manuscripts, because the model did not converge. ***$p < .001$, **$p < .01$, *$p < .05$, $^{+}p < .10$.

(viii) Gender. Gender analyses are restricted to the subsample of the data, in which experimenters recorded participants' gender (for a similar approach, see Gerlach et al., 2019). For the following analyses, we computed the proportion of females in each group and tested whether it is associated with the standardized group report, setting random intercepts for each group, treatment, and manuscript to control for the variations of the situational factors. In tasks employing repeated interactions, we focused on treatments in which groups were fixed so that the gender composition within a group was also fixed. Overall, the analysis covers 25 manuscripts ($k = 72$). Out of 2,934 groups in these treatments, in 18 (0.6%), the gender of one or more group members was missing. We thus removed these groups from our analyses and conducted the analyses on the remaining 2,916 groups. Replacing the 18 missing values with the mean proportion of females for each treatment did not change the obtained results. Overall, the proportion of females in a group varied between 0% (no females in a group) to 100% (only females in a group), with a mean of 50.37% ($SD = 36.14$).

Combining all decision structures together, a linear regression analysis provided support for the honest-image concerns perspective, revealing a significant effect of proportion of females on standardized group report ($b = -.081$, $t = -2.93$, $p = .003$). To quantify the effect, all-male groups had standardized group reports that were 8.1 percentage points higher than all-female groups. Examining the effect of gender in each of the decision structures reveals a significant effect in the joint decision structure ($b = -.102$, $t = -2.27$, $p = .023$), and in the sequential decision structure ($b = -.085$, $t = -2.17$, $p = .029$). The effect was not significant for the simultaneous decision structure ($b = .030$, $t = .40$, $p = .689$).

(ix) Age. Age analyses are restricted to the subsample of the data, in which the experimenters recorded participants' ages (for a similar approach, see Gerlach et al., 2019). For the following analyses, we computed the mean age within each group and then tested whether it was associated with the standardized group report, setting random intercepts for each group, treatment, and manuscripts to control for the variations in the situational factors. In tasks employing repeated interactions, we focused on treatments in which groups were fixed, so that the mean age of the group was also fixed. Overall, the analysis covers 21 manuscripts ($k = 58$). Out of 2,570 groups in these treatments, in 23 (0.9%), the age of one or more group members was missing. We thus removed these groups from our analyses and conducted the analyses on the remaining 2,547 groups. Replacing the 23 missing values with the mean age in each treatment did not change the obtained results. Overall, the mean age in a group varied between 11 and 66.5 years old (M = 26.30; SD = 8.43).

Combining all decision structures together, a linear regression analysis reveals a significant negative effect of mean group age on standardized group report ($b = -.005$, $t = -2.43$, $p = .015$). An increase of one year in the average age of a group corresponded to a decrease of 0.5 percentage points of the standardized group report. Examining the effect of age in each of the decision structures reveals a significant effect in the sequential decision structure ($b = -.006$, $t = -2.35$, $p = .018$), but not in the joint ($b = -.003$, $t = -1.05$, $p = .293$) and simultaneous ($b = .002$, $t = .205$, $p = .838$) decision structures. Thus, the results are not in-line with the prosocial nor the honest-image concerns perspectives.

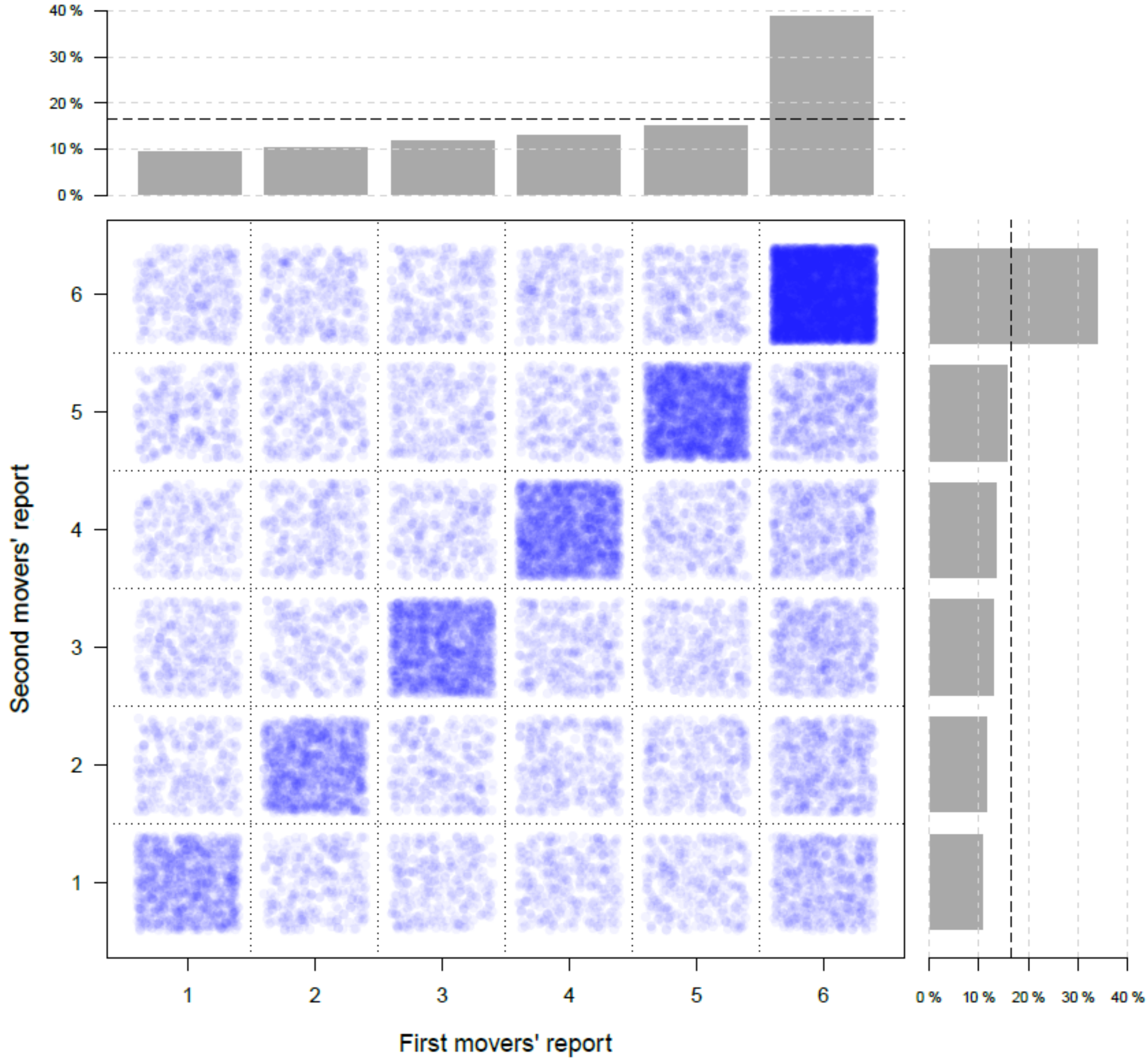
(x) Social influence and development over time

We next examine whether and the extent to which group members affect each other and how collaborative dishonesty develops over time. Repeated interactions were used in studies implementing versions of a die-rolling task (sequential dyadic die-rolling task, task #16 in Table 2, k = 47 treatments; and three-person version with different decision structures, tasks #15, #18, and #19 in Table 2, k = 4). To conduct analyses with interpretable results, we focused on the sequential dyadic die-rolling task with an aligned incentive scheme (see SOM for information about all payoff schemes and for the analyses including all payoff schemes, that led to the same results). Overall, our analyses cover 32 treatments, 2,716 participants, making 51,824 decisions.

In the dyadic die-rolling task, one participant is assigned to the role of a first mover and the other to the role of a second mover. The first mover rolls a die and reports the outcome. The second mover learns about the first mover's report, rolls an independent die, and reports the outcome as well. In the aligned-incentive scheme, if both dyad members report the same outcome (a double), they get paid according to the double's worth. If dyad members report different outcomes, they earn nothing. In the task, the first and second movers have different roles. First movers determine the potential amount the dyad can earn, by reporting the first die-roll outcome, whereas second movers determine whether the dyad earns money, by matching the first mover's report or not. Given these different roles, we focus on different dependent variables for each role. For first movers, we focus on the die-roll outcome they report, and for second movers, on whether they matched the first mover's report, that is, reported a double.

(x.i) Prevalence of dishonesty. We calculated the average die-roll report of each first mover. The mean of first movers' average die-roll reports was 4.19 (*Mdn* = 3.90, *SD* = .95; range 1.50 - 6.00), significantly higher than 3.5, the expected outcome if first movers are honest ($t = 28.39$, $p < .001$).[7,8] We calculated the proportion of doubles for each second mover. The mean proportion of doubles was 52.14% (*Mdn* = 45.00%; *SD* = 33.75; range 0% - 100%), significantly higher than 16.67%, the expected proportion of doubles if second movers are honest ($t = 41.25$, $p < .001$).[7,9]

Figure 3 presents the distribution of reports made by first and second movers, with each dot representing a combination of first and second movers' reports in one round of the task. As can be seen in the figure, dyads report doubles frequently, especially double 6's—the most profitable outcome. If participants are honest, they should report double 6's in 2.77% of rounds [1/6 × 1/6]. Overall, however, participants reported double 6's over 10 times more often (27.64%; 7,164 out of 25,912 rounds), significantly higher than chance, binomial test[7], $p < .001$. The proportion of all other double values (double 1's: 3.90%; 2's: 4.65%; 3's: 6.04%; 4's: 6.73%; and 5's: 8.74%) was also significantly higher than what is expected if dyads are honest ($p < .001$ in all cases).



(x.ii) Social influence. We tested the association between the partners' dishonesty and their influence on one another both at the aggregate (see below) and the round-by-round levels (see SOM). On the aggregate level, we (a) classified participants as either brazen liars or not, and tested whether an association exists between the brazenness of the first and second movers, and (b) tested whether the mean die-roll outcome reported by first movers is associated with the proportion of doubles reported by second movers.

We classified participants as brazen liars if they consistently reported the most profitable outcome (see a similar approach in Weisel & Shalvi, 2015). First movers were classified as brazen liars if they reported the highest possible die-roll outcome (i.e., “6”) in all rounds and as non-brazen otherwise. Second movers were classified as brazen liars if they matched the first mover’s report (i.e., reported a double) in all rounds, and as non-brazen otherwise. In a single round, a first mover’s probability of actually obtaining an outcome of “6” is 1/6, as is the probability of a second mover actually obtaining a double. Over r rounds, the probability of rolling a “6”, or matching the first mover’s report, in each round is $(1/6)^r$. For three rounds, this probability equals 0.005; for 10 rounds, 1.65×10^{-8}; and for 20 rounds—the most common number of rounds in our data set—2.7×10^{-16}. Thus, the likelihood of erroneously classifying an honest participant as a brazen liar is negligible.

Overall, 17.89% of first movers and 33.09% of second movers were classified as brazen liars. If first and second movers do not affect each other, the likelihood of one person being a brazen liar should not depend on whether their partner was a brazen liar. If, on the other hand, partners do affect each other, participants should be more likely to be brazen liars when their partners are brazen liars as well. Providing support for both the prosocial and honest-image concerns perspectives, results reveal dyad members’ behavior is positively correlated. Namely, participants were more likely to be brazen liars when their partners were brazen as well. When first movers were brazen liars, 67.70% of second movers were brazen as well; when first movers were not brazen, only 25.54% of second movers were brazen ($b = 1.175$, $p < .001$; Table S2, Model 1 in SOM). Similarly, when second movers were brazen, 36.61% of first movers were brazen, whereas when second movers were not brazen, only 8.63% of first movers were brazen.[10]

Figure 4 presents a heat map of the joint distribution of the mean die-roll outcome reported by first movers (on the X-axis) and the proportion of doubles reported by second movers (on the Y-axis). The larger the bubble, the more dyads appear on that specific X-Y coordinate. The dashed vertical and horizontal lines show the benchmarks for honest reports for first movers (average die-roll outcome of 3.5) and second movers (16.67% of doubles). The regression line shows the significant association between first and second movers' reports ($b = .047$, $p < .001$, Table S2, Model 2, in SOM), confirming that when one participant lied, the partner was more likely to lie as well.

Figure 4 further shows that most of the dyads fall into one of three main areas. In the first area, the average first mover report is around 3.5, indicating rather honest first movers, and the proportion of doubles is around 50% or less, indicating second movers who lie, but not to the full extent. In the second area, the average first mover report is around 3.5 or higher, but lower than 6, indicating first movers who lie, but not to the full extent, and the proportion of doubles is around 100%, indicating second movers who lie (almost) to the full extent (brazen, or nearly brazen, liars). In the third area, both first and second movers are brazen—or nearly brazen—liars. This area, in which the average first-mover report is nearly 6, or is 6, and the proportion of doubles is nearly 100%, or is 100%, is by far the densest. Specifically, 12.11% of dyads are all-brazen dyads, in which first movers' average report is 6 and the proportion of doubles is 100%.

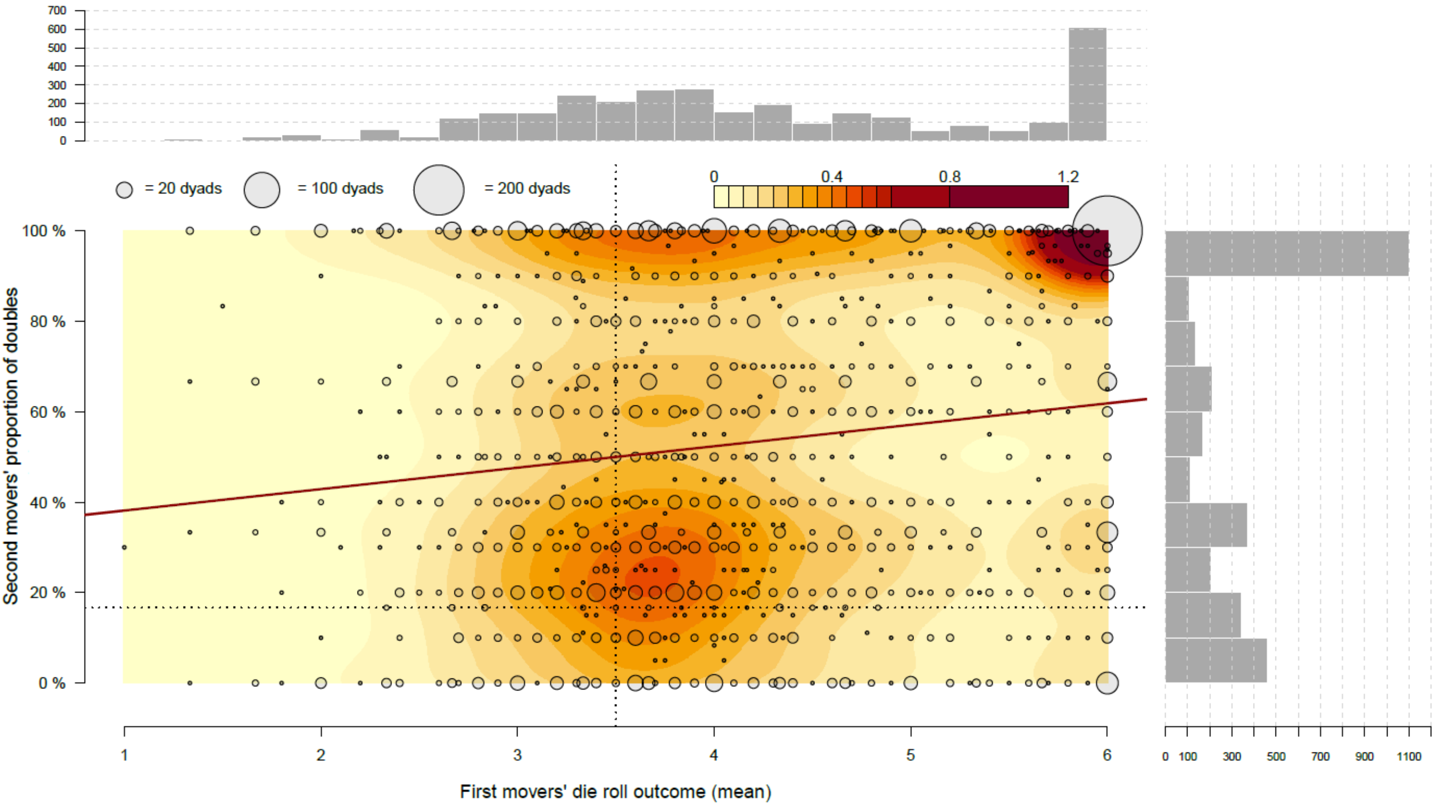
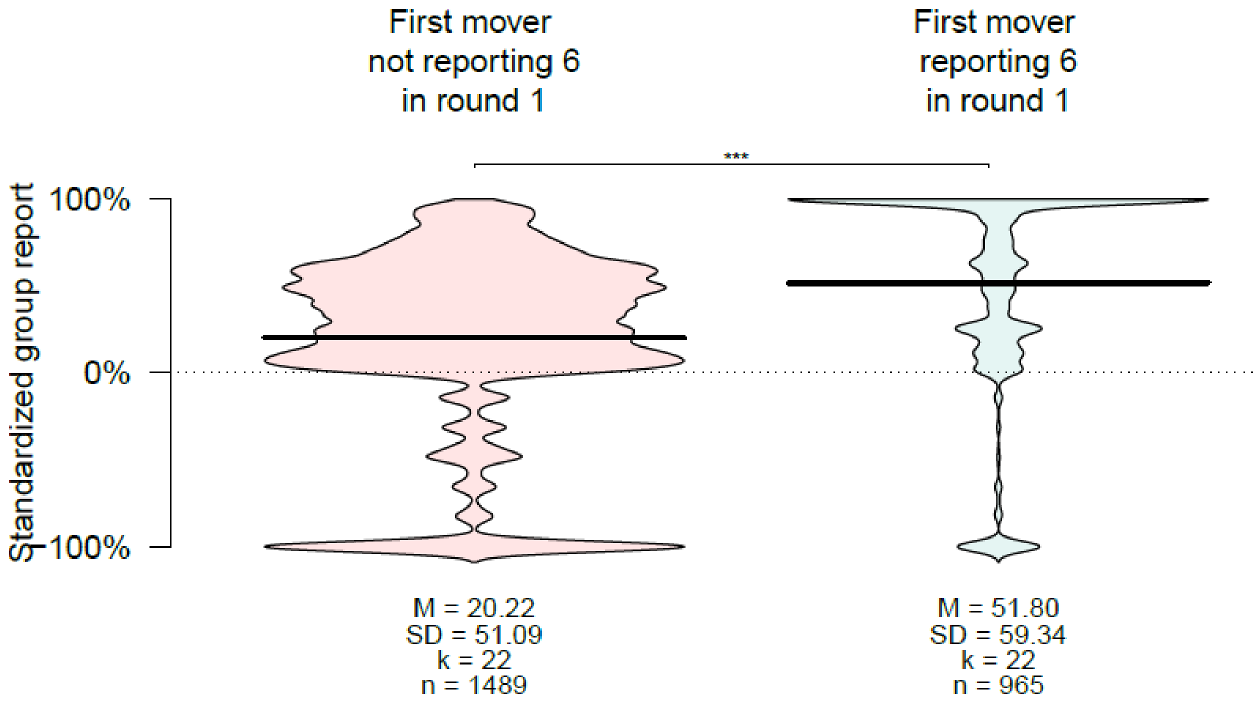


Figure 4. Heat map presenting the kernel density function of the joint distribution of the mean die-roll outcome reported by first movers (X-axis) and the proportion of doubles reported by second movers (Y-axis) in treatments with aligned payoff scheme. The larger the bubble, the more dyads appear on that specific X-Y coordinate. The dashed vertical and horizontal lines show the benchmarks for honest reports for first movers (average die-roll outcome of 3.5) and second movers (16.67% of doubles). The values on the legend present the level of the density corresponding to the color on the heat map. The values calculated such that the integral of the density across the entire domain of the range of X and Y values is equal to 1. The top panel presents the distribution of average die-roll outcomes reported by first movers. The right panel presents the distribution of the proportion of doubles reported by second movers.

(x.iii) Development over time. To assess behavior over time, we conducted separate analyses for first and second movers, examining how the round number in the task affected participants' reports. Providing further support for the prosocial and honest-image concerns perspectives, as the task progressed, first movers' reports increased (linear regression with a random intercept for each dyad, group, and treatment, $b = .009$, $p < .001$), as did the likelihood of second movers to report a double (logistic regression with a random intercept for each dyad, group, and treatment, $b = .013$, $p < .001$). The effect of round number on second movers' reports remained significant when controlling for the amount second movers could earn from reporting a double ($b = .011$, $p < .001$), with the effect of the amount being significant as well ($b = 3.91$, $p < .001$). Thus, both first and second movers lied more as the task progressed, above and beyond the financial temptation to lie.

The decision first movers made in the first round was strongly associated with the overall level of collaborative dishonesty; see Figure 5. Dyads in which the first mover reported a "6" in the first round had a much higher standardized group report ($M = 51.79\%$, $SD = 59.33$) than dyads in which the first mover reported any other number ($M = 20.22\%$, $SD = 51.08$; linear regression with a random intercept for each group, and treatment, $b = .235$, $p < .001$). Results remain significant when calculating the standardized group report, excluding reports in the first round of the interaction.



General Discussion

The current meta-analysis is the first to focus on collaborative dishonesty. The meta-analysis provides an overview of the 21 experimental paradigms used to measure collaborative dishonesty, where groups make joint, simultaneous, or sequential decisions. Further, assessing the overall level of collaborative dishonesty and the factors shaping it helps to inform and advance the theoretical understanding of collaborative dishonesty. We discuss the theoretical, practical, and methodological implications of our findings as well as their limitations and the avenues for future work.

Theoretical, practical, and methodological implications

Overall, our results provide support for the justified-ethicality theoretical perspective, not for the narrow self-interest perspective. The narrow self-interest perspective predicts groups will lie to the full extent to maximize their profits and that moderators will have no impact on their level of dishonesty. The justified-ethicality perspective predicts groups will vary the amount of their dishonesty, increasing it due to prosocial concerns towards their group, and restricting it due to a desire to maintain an honest self and public image. Supporting the justified-ethicality perspective, moderators that vary prosocial and honest-image concerns help explaining the high level of heterogeneity in collaborative dishonesty across studies and settings.

Our meta-analysis reveals that factors that increase prosocial concerns towards one's group members are associated with higher levels of collaborative dishonesty. First, when people are able to benefit their group members more, due to higher financial incentives to lie, collaborative dishonesty increases. Second, when experimental deception is not employed, suggesting that participants were relatively certain their lies would indeed benefit others, groups lied more. Third, joint decisions, which arguably make the group more salient and amplify prosocial concerns (relative to simultaneous or sequential decisions), result in higher (though significant only at the 10% level) levels of collaborative dishonesty. Finally, whereas individuals are motivated to benefit their group members, they also exhibit prosocial concerns toward third parties and are averse to harming them (Baron, 1995; Van Beest et al., 2005; Halevy et al., 2012). Indeed, we find that when collaborative dishonesty harms a third party (e.g., another person/group or a charity donation), groups lie less than when their lies are paid from the experimenter's budget.

Results further reveal that factors that increase honest-image concerns (Mazar et al., 2008; Abeler et al., 2019) are associated with lower levels of collaborative dishonesty. First, in line with the idea that lab and online studies could attract participants with lower honest-image concerns than field studies (Gerlach et al., 2019), we find higher levels of collaborative dishonesty in the lab and on online platforms than in studies conducted in the field. Second, consistent with results on dishonesty in individual settings (Gerlach et al., 2019), we also find lower levels of collaborative dishonesty when experimental deception was implemented than when it was not. In line with the reasoning first presented by Gerlach et al. (2019), experimental deception seems to lead participants to suspect their behavior is not fully anonymous, increasing their image concerns and, as a result, decreasing their willingness to lie. Third, in line with the idea that women have higher honest-image concerns (Bowles et al., 2007; Gneezy et al., 2009), we find that the more women are in a group, the lower collaborative dishonesty is. Fourth, and noting the null-effect prediction, in line with the idea that people's concern about being perceived as honest should not vary when groups' incentives are aligned or not, we find no effect of payoff alignment on collaborative dishonesty.

Our meta-analytical results suggest that in repeated settings, dishonesty is contagious and evolves over time, suggesting people may habituate to their partner's (dis)honesty. The prosocial and honest-image concerns perspectives predict that when one group member lies, other group members lie as well. This finding is arguably due to (i) a higher prosocial motivation to reciprocate the dishonest effort to increase the group's profits, and (ii) a lower honest-image concern, given that the other lying group member clearly does not value honesty (Bicchieri, 2016: Cialdini et al., 1990). Accordingly, as people habituate to the act of lying, collaborative dishonesty increases over time, leading to a slippery-slope effect (Welsh et al., 2015; Garrett et al., 2016, see also Effron et al., 2015). Such habituation means people take their own and their partner's past (dis)honest behavior as their new reference point, leading them to increasingly disregard ethical standards and lie (Welsh et al., 2015; Tenbrunsel & Messick, 2004, but see Touré-Tillery & Fishbach, 2012; Fishbach & Woolley, 2015).

Results reported here supported the justified ethicality theoretical perspective, suggesting that prosocial and honest-image concerns shape collaborative dishonesty. For some moderating factors, namely, negative consequences for third parties, experimental deception, and social influence/development over time, the prosocial and honest-image concerns predict the same pattern of results (see Table 3). For those factors, the predictions were supported by the data. For other factors, the predictions made by the prosocial and honest-image concerns diverge, and our findings supported the pattern suggested by one concern but not the other. Specifically, the effects of decision structure and financial incentives on collaborative dishonesty are in line with the predictions made by the prosocial concerns, whereas the effects of payoff alignment, study type (lab vs. online vs. field), and gender are in line with the predictions made by the honest-image concerns. A plausible interpretation of this pattern is that different moderators activate one concern more than the other. An interesting avenue to further explore is the settings most likely to activate one concern more than the other and the reasons underlying such gaps in activation. For example, work is needed to clarify which processes underlie the relatively high increase in pro-social concerns when increasing financial incentives and the relatively high increase in honest-image concerns when studies are conducted in the lab or online compared with the field. Examining such refined processes would help advance our theoretical understanding of the settings most likely to increase collaborative dishonesty and the ways such behavior can be discouraged.

Findings obtained here carry practical implications concerning the importance of curbing ethical violations as early as possible. Specifically, in sequential settings, we find that the first outcome reported by the first mover is strongly associated with how the rest of the interaction evolves. When first movers begin the interaction by reporting the most profitable outcome, the group ends up earning 2.5 times more than groups in which the first mover reports any other outcome on the first trial. Accordingly, detecting and acting upon early signs of ethical violations seems like a promising pathway to prevent collaborative dishonesty from spreading. For example, when implementing whistleblowing systems (Near & Miceli, 1995), people should be encouraged to act immediately and report wrongdoings as soon as they suspect an ethical violation (Köbis et al., 2017). Clearly, further research outside the lab is needed to validate such interventions.

Finally, our results carry methodological implications for researchers studying collaborative dishonesty. Specifically, converging with results on dishonesty in individual settings (Gerlach et al., 2019), we find higher levels of collaborative dishonesty when experimental deception is not present, and thus, participants believe their actions are anonymous and will *actually* benefit their group. Accordingly, when designing future studies on collaborative dishonesty, considering incentivization and the type of information provided to participants is worthwhile (Hilbig et al., 2021). Given that participants are sensitive to the information they receive, reducing their suspicion by providing them with accurate information about the situation they are in seems advisable.

Limitations and future directions

Most tasks used to study collaborative dishonesty are decontextualized, posing a challenge to the generalizability of the results. Studying any psychological phenomenon, employing decontextualized (economic) games provides a high level of experimental control and internal validity at the expense of a limited ability to generalize the results to contextualized settings (van Dijk & De Dreu, 2021). By contrast, using contextualized settings increases external validity at the expense of experimental control. Lying in decontextualized tasks often used to study dishonesty, such as the die-rolling task, is positively correlated with everyday dishonest and rule-violating behaviors outside the lab (Dai et al., 2018; Hanna & Wang, 2017; Potters & Stoop, 2016; Kröll & Rustagi, 2016), establishing their external validity.

Notwithstanding, to best understand the phenomenon, triangulating research methods when studying collaborative dishonesty is important. Doing so can be inspired by work on individual (dis)honesty and moral behavior examined in contextualized settings employing experience sampling techniques (Hofmann et al., 2014) and self-reported behavior (DePaulo et al., 1996; Serota et al., 2010; Halevy et al., 2014), examining diverse samples (Cohn et al., 2014; Rahwan et al., 2019), analyzing archival data (Ferrali, 2020; Gould & Kaplan, 2011), and conducting field experiments with measures that closely resemble everyday rule-violating and (dis)honest behavior (Vranka et al., 2019; Cohn et al., 2019; see a review by Pierce & Balasubramanian, 2015; Larkin et al. 2021). Indeed, Azar et al. (2013) take the first steps toward studying collaborative dishonesty in contextualized settings, examining whether groups of restaurant diners return extra change they have wrongly received. We call for a continuation of diversifying the methods used to examine collaborative dishonesty in both contextualized and decontextualized settings.

Most studies examining collaborative dishonesty were conducted in WEIRD (Western, educated, industrialized, rich, and democratic; Henrich et al., 2010) countries. Generalizing the findings reported here to different cultures and non-WEIRD countries is a promising avenue for future work to explore. Indeed, prior work found that levels of honesty and prosocial concerns vary across the globe. For example, a country-level prevalence of rule violations is positively correlated to, and predictive of the extent to which people from this country lie to serve their self-interest (Gächter & Schulz, 2016); and people's baseline civic honesty, measured by their likelihood to return a lost wallet to its rightful owner, varies greatly in countries around the globe (Cohn et al., 2019). Similarly, whereas in some countries, people are inclined to trust others and cooperate with them, in other countries, trust and cooperation are relatively low (Dorrough & Glöckner, 2016; Gächter et al., 2010). To better understand how people around the globe trade off honesty and collaboration, we call for future work to examine collaborative dishonesty in diverse and non-WEIRD countries across the globe.

Results revealed that group size did not moderate collaborative dishonesty, even though theoretically, both the prosocial and honest-image concerns perspectives suggest that it should. If the effect is indeed present, there are two possible reasons why it did not emerge. One possibility is a lack of sufficient statistical power to identify the effect. It might be that the effect of group size on collaborative dishonesty is small and requires a larger sample and more treatments with varying group sizes to identify. Reduced power and a rather small number of treatments may also explain why, for instance, we identified a significant effect of experimental deception when collapsing across all decision structures, but non-significant effects among joint and simultaneous decision structures when examined separately. Table 4 provides an overview of the frequencies with which different settings have been implemented in the literature, highlighting cells with a limited number of treatments. For instance, as Table 4 shows, 80% or more of the treatments implemented aligned payoffs in joint and simultaneous decision structures, meaning that misaligned payoffs are understudied in these decision structures. We call for future work to pay special attention to considering those understudied settings to broaden our theoretical understanding of the factors that shape collaborative dishonesty.

Another possibility for the lack of a significant effect of group size on collaborative dishonesty lies in the limited heterogeneity of group sizes across studies. With the exception of Conrads et al. (2017), who studied six-person groups, all other manuscripts included in the meta-analysis studied groups of two or three people. Accordingly, if group-size effects emerge only when comparing smaller with relatively larger groups, our ability to detect such effects here is restricted. For example, outside the context of collaborative dishonesty, research finds groups of four are better able than groups of two to use joint knowledge to tackle problems (Yetton & Bottger, 1983), and groups of seven are less committed than groups of three to the group's goal (Seijts & Latham, 2000). Our meta-analysis, therefore, suggests that studying collaborative dishonesty in groups of varying sizes would prove theoretically fruitful because such studies may more easily capture variation in both prosocial and honest-image concerns across groups.

Examining social influence and the development of collaborative dishonesty over time required analyzing repeated interactions. The meta-analysis provided support for the theoretical predictions of the prosocial and honest-image concerns perspectives. Note, however, that these analyses were limited to a single task (with a sequential decision structure), the dyadic die-rolling task (Weisel & Shalvi, 2015), since only this task was implemented with a repeated-measures design with a sufficient number of observations (see Table 4). These analyses were conducted on a substantial number of treatments (k = 32) and observations (2,716 participants, making 51,824 decisions), but generalizing the results to other settings remains an avenue for future work. Given the interesting results that emerge when people interact over time, we encourage more research to employ different experimental tasks and implement repeated-measures designs. Doing so will allow researchers to assess the robustness of the obtained results and shed new theoretical light on how one group member influences the other group members' (dis)honesty.

Whereas a meta-analytical approach provides a systematic and quantitative summary of the literature on collaborative dishonesty, the method also has limitations (Johnson, 2021). First, the results of our meta-analysis do not allow for causal inferences. For example, whereas we find a negative effect of groups' age on collaborative dishonesty, we are unable to disentangle whether the effect stems from the unique process of aging or from different shared experiences across generations (i.e., cohort effects). As highlighted also by Gerlach et al. (2019), identifying such causal effects would require longitudinal studies. Second, recent work suggests that pre-registered multiple-laboratory replication projects result in more accurate effect size estimates than meta-analyses due to methodological homogeneity and lack of publication bias concern

(Kvarven et al., 2020). As such, our meta-analysis may provide somewhat inflated effect size estimates. However, given that our meta-analysis contains a substantial proportion of unpublished studies (35%), the concern is relatively alleviated. Our analyses also reveal no evidence for publication bias as the effect size does not differ between published and unpublished treatments. Nonetheless, we encourage large, pre-registered replication studies to substantiate the effects we observe here.

Our meta-analysis considered situations in which people trade off their honesty for collaboration or vice versa. In practice, however, interacting groups, and the organizations they operate in, often want to maintain both honesty and collaboration. Indeed, maintaining and increasing both carries considerable value because collaboration allows bringing together unique skills and leads to high-quality decisions and innovation (Fraidin, 2004; Laughlin et al., 2006; West & Anderson, 1996). The meta-analytical evidence presented here marks the first step towards understanding the processes that lead people to forego their honesty to advance their own and their group's interests. A key open avenue for future research is further theorizing and identifying the mechanisms that contribute to the establishment of collaborative honesty.

Conclusion

Many—if not most—important decisions occur in collaboration with others, and such collaborations may result in mutual acts of dishonesty. By understanding the factors that shape collaborative dishonesty and the delicate dynamics leading to its emergence, we can design environments in which honesty is encouraged, and dishonesty is tamed.

References

(*denotes inclusion in the meta-analysis)

Abeler, J., Nosenzo, D., & Raymond, C. (2019). Preferences for truth-telling. *Econometrica*, *87*(4), 1115-1153.

Axelrod, R., & Hamilton, W. D. (1981). The evolution of cooperation. *Science, 211*(4489), 1390-1396.

*Azar, O. H., Yosef, S., & Bar-Eli, M. (2013). Do customers return excessive change in a restaurant?: A field experiment on dishonesty. *Journal of Economic Behavior & Organization, 93,* 219-226.

Balliet, D., Mulder, L. B., & Van Lange, P. A. M. (2011). Reward, punishment, and cooperation: A meta-analysis. *Psychological Bulletin, 137*, 594–615.

Balliet, D., & Van Lange, P. A. M. (2013). Trust, conflict, and cooperation: a meta-analysis. *Psychological Bulletin*, *139*(5), 1090–112.

Bandura, A. (1999). Moral disengagement in the perpetration of inhumanities. *Personality and Social Psychology Review, 3*(3), 193–209.

Baron, J. (1995). Blind justice: Fairness to groups and the do‐no‐harm principle. *Journal of behavioral decision making, 8*(2), 71-83.

*Barr, A., & Michailidou, G. (2017). Complicity without connection or communication. *Journal of Economic Behavior & Organization*, *142*, 1-10.

Barrett, S. R., Speth, R. L., Eastham, S. D., Dedoussi, I. C., Ashok, A., Malina, R., & Keith, D. W. (2015). Impact of the Volkswagen emissions control defeat device on US public health. *Environmental Research Letters, 10(11),* 114005.

Bartling, B., & Fischbacher, U. (2012). Shifting the blame: On delegation and responsibility. *The Review of Economic Studies, 79*(1), 67-87.

Bates D, Mächler M, Bolker B, Walker S (2015). "Fitting Linear Mixed-Effects Models Using lme4." *Journal of Statistical Software, 67*(1), 1–48

Baumeister, R.F, & Leary, M.R. (1995). The need to belong: Desire for interpersonal attachments as a fundamental human motivation. *Psychological Bulletin, 117*(3), 497–529.

Bazerman, M. H., Curhan, J. R., Moore, D. A., & Valley, K. L. (2000). Negotiation. *Annual review of psychology, 51*(1), 279-314.

*Beck, T., Bühren, C., Frank, B., & Khachatryan, E. (2020). Can Honesty Oaths, Peer Interaction, or Monitoring Mitigate Lying? *Journal of Business Ethics, 163*(3), 467-484.

Becker, G. S. (1968). Crime and punishment: An economic approach. In *The economic dimensions of crime* (pp. 13-68). Palgrave Macmillan, London.

Bellemare, C., Kröger, S., & Van Soest, A. (2008). Measuring inequity aversion in a heterogeneous population using experimental decisions and subjective probabilities. *Econometrica, 76*(4), 815-839.

Bicchieri, C. (2016). *Norms in the wild: How to diagnose, measure, and change social norms.* Oxford University Press.

*Bonfim, M. P. (2018). Honest individuals, (dis) honest groups?: experiments about the influence of the collective on individual behavior. PhD thesis.

*Bonfim, M. P., & Silva, C. A. T. (2018). The influence of the leader in the behavior of the group. *European Journal of Scientific Research, 151*(2), 241-258

*Bonfim, M. P., & Silva, C. A. T. (2019). Inhibitory Mechanisms on Dishonesty of Groups and Individuals. *Journal of Accounting, Management and Governance, 22* (2), 205-226.

Bowles, H. R., Babcock, L., & Lai, L. (2007). Social incentives for gender differences in the propensity to initiate negotiations: Sometimes it does hurt to ask. *Organizational Behavior and human decision Processes, 103*(1), 84-103.

*Burghoorn, F.J., De Irueta Florentina, A., Bijlstra, G. &, Verwijmeren, T. (2021). The influence of collaborating with an in-group or out-group member on dishonest behaviour: a field study between teams. Working paper.

Cappelen, A. W., Sørensen, E. Ø., & Tungodden, B. (2013). When do we lie? *Journal of Economic Behavior & Organization, 93*, 258 –265.

Capraro, V. (2018). Gender differences in lying in sender-receiver games: A meta-analysis. *Judgment and Decision Making, 13*, 345-355.

Carter, E. C., Schönbrodt, F. D., Gervais, W. M., & Hilgard, J. (2019). Correcting for bias in psychology: A comparison of meta-analytic methods. *Advances in Methods and Practices in Psychological Science, 2*(2), 115-144.

*Castillo, G., Choo, L., & Grimm, V. (2020). Are groups really more dishonest than individuals? (No. 01/2020). FAU Discussion Papers in Economics.

Cialdini, R. B., Reno, R. R., & Kallgren, C. A. (1990). A focus theory of normative conduct: Recycling the concept of norms to reduce littering in public places. *Journal of personality and social psychology, 58*(6), 1015-1026.

*Cohen, T. R., Gunia, B. C., Kim-Jun, S. Y., & Murnighan, J. K. (2009). Do groups lie more than individuals? Honesty and deception as a function of strategic self-interest. *Journal of Experimental Social Psychology*, *45*(6), 1321-1324.

Cohn, A., Fehr, E., & Maréchal, M. A. (2014). Business culture and dishonesty in the banking industry. *Nature, 516*(7529), 86-89.

Cohn, A., Maréchal, M. A., Tannenbaum, D., & Zünd, C. L. (2019). Civic honesty around the globe. *Science, 365*(6448), 70-73.

*Conrads, J., Ellenberger, M., Irlenbusch, B., Ohms, E. N., Rilke, R. M., & Walkowitz, G. (2017). Team goal incentives and individual lying behavior.

*Conrads, J., Irlenbusch, B., Rilke, R. M., & Walkowitz, G. (2013). Lying and team incentives. *Journal of Economic Psychology, 34,* 1–7.

Dai, Z., Galeotti, F., & Villeval, M. C. (2018). Cheating in the Lab Predicts Fraud in the Field: An Experiment in Public Transportation. *Management Science, 64*(3), 1081-1100.

*Dannenberg, A., & Khachatryan, E. (2020). A comparison of individual and group behavior in a competition with cheating opportunities. *Journal of Economic Behavior & Organization, 177*, 533-547.

Darley, J. M., & Latané, B. (1968). Bystander intervention in emergencies: Diffusion of responsibility. *Journal of Personality and Social Psychology, 8*(4), 377-383.

Dawes, R. M. (1980). Social dilemmas. *Annual review of psychology, 31*(1), 169-193.

*Della Valle, N., Gino, F., & Piovesan, M. (2017). Honesty under threat. Available at: http://eprints-phd.biblio.unitn.it/2024/

DePaulo, B. M., Kashy, D. A., Kirkendol, S. E., Wyer, M. M., & Epstein, J. A. (1996). Lying in everyday life. *Journal of personality and social psychology, 70*(5), 979-995.

Dimant, E., Van Kleef, G. A., & Shalvi, S. (2020). Requiem for a nudge: Framing effects in nudging honesty. *Journal of Economic Behavior & Organization, 172*, 247-266.

*Dong, L., Huang, L., DeScioli, P., & Du, N. (2018). Social Cheating in the Classroom: A Field Experiment. Working paper.

Dorrough, A. R., & Glöckner, A. (2016). Multinational investigation of cross-societal cooperation. *Proceedings of the National Academy of Sciences, 113*(39), 10836-10841.

*Dunaiev, Y., & Khadjavi, M. (2021). Collective Honesty? Experimental Evidence on the Effectiveness of Honesty Nudging for Teams. *Frontiers in Psychology, 12*, 2788.

Effron, D. A., Bryan, C. J., & Murnighan, J. K. (2015). Cheating at the end to avoid regret. *Journal of Personality and Social Psychology, 109*(3), 395-414.

El Zein, M., Bahrami, B., & Hertwig, R. (2019). Shared responsibility in collective decisions. *Nature human behaviour, 3*(6), 554-559.

Engel, C. (2011). Dictator games: A meta study. *Experimental Economics*, *14*(4), 583-610.

Feess, E., Kerzenmacher, F., & Muehlheusser, G. (2020). Moral Transgressions by Groups: What Drives Individual Voting Behavior?.

Feldman, R. S., & Rosen, F. P. (1978). Diffusion of responsibility in crime, punishment, and other adversity. *Law and Human Behavior, 2*(4), 313-322.

Ferrali, R. (2020). Is honesty or dishonesty more contagious? Evidence from the field.

Fishbach, A., & Woolley, K. (2015). Avoiding ethical temptations. *Current Opinion in Psychology, 6*, 36-40.

Fischbacher, U., & Föllmi-Heusi, F. (2013). Lies in disguise—an experimental study on Cheating. *Journal of the European Economic Association*, *11*(3), 525-547.

Fraidin, S. N. (2004). When is one head better than two? Interdependent information in group decision making. *Organizational Behavior and Human Decision Processes*, *93*(2), 102-113.

Gächter, S., Herrmann, B., & Thöni, C. (2010). Culture and cooperation. *Philosophical Transactions of the Royal Society B: Biological Sciences, 365*(1553), 2651-2661.

Gächter, S., & Schulz, J. F. (2016). Intrinsic honesty and the prevalence of rule violations across societies. *Nature, 531*(7595), 496-499.

Garrett, N., Lazzaro, S. C., Ariely, D., & Sharot, T. (2016). The brain adapts to dishonesty. *Nature neuroscience, 19*(12), 1727-1732.

Gerlach, P., Teodorescu, K., & Hertwig, R. (2019). The truth about lies: A meta-analysis on dishonest behavior. *Psychological Bulletin*, *145*(1), 1-44.

Gino, F., & Ariely, D. (2016). Dishonesty explained: what leads moral people to act immorally. *The social psychology of good and evil*, 322-344.

Gino, F., Ayal, S., & Ariely, D. (2009). Contagion and differentiation in unethical behavior: The effect of one bad apple on the barrel. *Psychological Science, 20*(3), 393-398.

*Gino, F., Ayal, S., & Ariely, D. (2013). Self-serving altruism? The lure of unethical actions that benefit others. *Journal of Economic Behavior & Organization, 93*, 285-292.

Gneezy, U., Leonard, K. L., & List, J. A. (2009). Gender differences in competition: Evidence from a matrilineal and a patriarchal society. *Econometrica, 77*(5), 1637-1664.

Goodman, L. M. (2015, December 15). Why Volkswagen cheated. Newsweek. Retrieved from http://www.newsweek.com/2015/12/25/why-volkswagen-cheated-404891.html

Gould, E. D., & Kaplan, T. R. (2011). Learning unethical practices from a co-worker: the peer effect of Jose Canseco. *Labour Economics, 18*(3), 338-348.

*Gross, J., & De Dreu, C. K. (2021). Rule Following Mitigates Collaborative Cheating and Facilitates the Spreading of Honesty Within Groups. *Personality and Social Psychology Bulletin, 47*(3), 395-409.

*Gross, J., Leib, M., Offerman, T., & Shalvi, S. (2018). Ethical free riding: When honest people find dishonest partners. *Psychological Science, 29*(12), 1956-1968.

Halevy, N., Halali, E., & Cohen, T. R. (2020). Brokering orientations and social capital: Influencing others' relationships shapes status and trust. *Journal of personality and social psychology, 119*(2), 293-316.

Halevy, R., Shalvi, S., & Verschuere, B. (2014). Being honest about dishonesty: Correlating self-reports and actual lying. *Human Communication Research, 40*(1), 54-72.

Halevy, N., Weisel, O., & Bornstein, G. (2012). "In-group love" and "out-group hate" in repeated interaction between groups. *Journal of Behavioral Decision Making, 25*(2), 188-195.

Hanna, R., & Wang, S. Y. (2017). Dishonesty and selection into public service: Evidence from India. *American Economic Journal: Economic Policy, 9*(3), 262-290.

Harrer, M., Cuijpers, P., Furukawa, T. & Ebert, D. D. (2019). dmetar: Companion R Package For The Guide 'Doing Meta-Analysis in R'.

Henrich, J., Heine, S. J., & Norenzayan, A. (2010). The weirdest people in the world? *Behavioral and Brain Sciences, 33*, 61– 83.

Herrera-Viedma, E., Cabrerizo, F. J., Chiclana, F., Wu, J., Cobo, M. J., & Konstantin, S. (2017). Consensus in group decision making and social networks. *Studies in Informatics and Control, 26(*3), 259-268.

Hilbig, B. E., & Hessler, C. M. (2013). What lies beneath: How the distance between truth and lie drives dishonesty. *Journal of Experimental Social Psychology, 49*(2), 263-266.

Hilbig, B. E., & Thielmann, I. (2017). Does everyone have a price? On the role of payoff magnitude for ethical decision making. *Cognition, 163*, 15-25.

Hilbig, B. H., Thielmann, I., & Böhm, R., (2021). Bending Our Ethics Code: Avoidable Deception and Its Justification in Psychological Research. *European Psychologist*. In press.

*Hildreth, J. A. D., Gino, F., & Bazerman, M. (2016). Blind loyalty? When group loyalty makes us see evil or engage in it. *Organizational Behavior and Human Decision Processes, 132*, 16-36.

Hofmann, W., Wisneski, D. C., Brandt, M. J., & Skitka, L. J. (2014). Morality in everyday life. *Science, 345*(6202), 1340-1343.

Houser, D., Vetter, S., & Winter, J. (2012). Fairness and cheating. *European Economic Review, 56*(8), 1645-1655.

*Irlenbusch, B., Mussweiler, T., Saxler, D. J., Shalvi, S., & Weiss, A. (2020). Similarity increases collaborative cheating. *Journal of Economic Behavior & Organization, 178*, 148-173.

Jacobsen, C., Fosgaard, T. R., & Pascual‐Ezama, D. (2018). Why do we lie? A practical guide to the dishonesty literature. *Journal of Economic Surveys, 32*(2), 357-387.

Jamaluddin, S. F., Adi, S. P., & Lufityanto, G. (2020). Social influences on cheating in collectivistic culture: Collaboration but not competition. *Group Dynamics: Theory, Research, and Practice.* Advance online publication. http://dx.doi.org/10.1037/gdn0000122

Johnson, B. T. (2021). Toward a more transparent, rigorous, and generative psychology. *Psychological Bulletin, 147*(1), 1-15.

Kajackaite, A., & Gneezy, U. (2017). Incentives and cheating. *Games and Economic Behavior*, *102*, 433-444.

Kameda, T., Takezawa, M., & Hastie, R. (2005) Where do social norms come from? The example of communal sharing. *Current Direction in Psychological Science*, 14(6), 331–334.

Karg, S. T., Kim, M., Mitkidis, P., Young, L., & Karg, S. T. (2020). Collaborative cheating brings people closer: How people evaluate and interact with partners in crime. Available at: https://moralitylab.bc.edu/wp-content/uploads/2020/05/2020-Karg-_-Collaborative-cheating-brings-people-closer-first_submission_preprint-1.pdf

Keizer, K., Lindenberg, S., & Steg, L. (2008). The spreading of disorder. *Science, 322*(5908), 1681-1685.

*Kerzenmacher, F. (2017). Lying to a liar: The effect of within-group reputation on moral behavior. In Moral hazard, lying, and reputation. Theory and experimental evidence (Dissertation).

Köbis, N. C., Troost, M., Brandt, C. O., & Soraperra, I. (2019). Social norms of corruption in the field: social nudges on posters can help to reduce bribery. *Behavioural Public Policy*, 1-28.

Köbis, N. C., van Prooijen, J. W., Righetti, F., & Van Lange, P. A. (2017). The road to bribery and corruption: Slippery slope or steep cliff?. *Psychological science, 28*(3), 297-306.

Köbis, N. C., Van Prooijen, J. W., Righetti, F., & Van Lange, P. A. (2015). "Who doesn't?"—The impact of descriptive norms on corruption. *PloS One*, *10*(6), e0131830.

Köbis, N. C., Verschuere, B., Bereby-Meyer, Y., Rand, D., & Shalvi, S. (2019). Intuitive honesty versus dishonesty: Meta-analytic evidence. *Perspectives on Psychological Science, 14*(5), 778-796.

*Kocher, M. G., Schudy, S., & Spantig, L. (2018). I lie? We lie! Why? Experimental evidence on a dishonesty shift in groups. *Management Science*, *64*(9), 3995-4008.

*Korbel, V. (2017). Do we lie in groups? An experimental evidence. *Applied Economics Letters, 24*(15), 1107-1111.

Kouchaki, M., & Kray, L. J. (2018). "I won't let you down:" Personal ethical lapses arising from women's advocating for others. *Organizational Behavior and Human Decision Processes, 147*, 147-157.

Kramer, R. M. (1999). Trust and distrust in organizations: Emerging perspectives, enduring questions. *Annual review of psychology, 50*(1), 569-598.

Kröll, M., & Rustagi, D. (2016). *Got milk? Motivation for honesty and cheating in informal markets: Evidence from India* (No. 134). Research Center SAFE-Sustainable Architecture for Finance in Europe, Goethe University Frankfurt.

Kuznetsova, A., Brockhoff, P.B., Christensen. R.H.B, (2017). “lmerTest Package: Tests in Linear Mixed Effects Models.” *Journal of Statistical Software, 82*(13), 1–26.

Kvarven, A., Strømland, E., & Johannesson, M. (2020). Comparing meta-analyses and preregistered multiple-laboratory replication projects. *Nature Human Behaviour, 4*(4), 423-434.

Larkin, I., Pierce, L., Shalvi, S., & Tenbrunsel, A. (2021). The opportunities and challenges of behavioral field research on misconduct. *Organizational Behavior and Human Decision Processes, 166,* 1-8.

Laughlin, P. R., Hatch, E. C., Silver, J. S., & Boh, L. (2006). Groups perform better than the best individuals on letters-to-numbers problems: effects of group size. *Journal of Personality and Social Psychology*, *90*(4), 644-651.

Leib, M., Moran, S., & Shalvi, S. (2019). Dishonest helping and harming after (un) fair treatment. *Judgment and Decision Making, 14*(4), 423-439.

Leib, M. & Shalvi, S. (2020). Justifications as a threat for honesty: a behavioral ethics approach In C. B. Miller and R. West (Eds.), *Integrity, Honesty, and Truth-Seeking.* Oxford University Press.

Lohse, T., & Simon, S. A. (2019). Compliance in Teams-Implications of Joint Decisions and Shared Consequences. CESifo Working Paper No. 7807. Available at SSRN. https://ssrn.com/abstract=3467940

Mazar, N., Amir, O., & Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance. *Journal of Marketing Research*, *45*, 633–644.

Moher, D., Liberati, A., Tetzlaff, J., & Altman, D. G. (2009). Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement. *Annals of internal medicine, 151*(4), 264-269.

*Muehlheusser, G., Roider, A., & Wallmeier, N. (2015). Gender differences in honesty: Groups versus individuals. *Economics Letters, 128*, 25-29.

Near, J. P., & Miceli, M. P. (1995). Effective whistleblowing. *Academy of Management Review, 20*, 679–708.

*Nieper, A.S., Beersma, B., Dijkstra, M.T. M., & Van Kleef, G. A..(2021). Gossip and Collaborative Cheating. Working paper.

*Nikolova, H., Lamberton, C., & Coleman, N. V. (2018). Stranger danger: When and why consumer dyads behave less ethically than individuals. *Journal of Consumer Research, 45*(1), 90-108.

Nowak, M. A. (2006). Five rules for the evolution of cooperation. *Science, 314*(5805), 1560-1563.

O'Fallon, M. J., & Butterfield, K. D. (2012). The influence of unethical peer behavior on observers' unethical behavior: A social cognitive perspective. *Journal of Business Ethics, 109*(2), 117-131.

Pierce, L., & Balasubramanian, P. (2015). Behavioral field evidence on psychological and social factors in dishonesty and misconduct. *Current Opinion in Psychology, 6,* 70-76.

Potters, J., & Stoop, J. (2016). Do cheaters in the lab also cheat in the field? *European Economic Review*, *87*, 26-33.

Pulfrey, C., Durussel, K., & Butera, F. (2018). The good cheat: Benevolence and the justification of collective cheating. *Journal of Educational Psychology, 110*(6), 764-784.

Rahwan, Z., Yoeli, E., & Fasolo, B. (2019). Heterogeneity in banker culture and its influence on dishonesty. *Nature, 575*(7782), 345-349.

Rand, D. G., Dreber, A., Ellingsen, T., Fudenberg, D., & Nowak, M. A. (2009). Positive interactions promote public cooperation. *Science, 325*(5945), 1272-1275.

Rempel, J. K., Holmes, J. G., & Zanna, M. P. (1985). *Trust in close relationships. Journal of personality and social psychology, 49*(1), 95-112.

*Rilke, R. M., Danilov, A., Weisel, O., Shalvi, S., & Irlenbusch, B. (2021). When leading by example leads to less corrupt collaboration. *Journal of Economic Behavior & Organization*, *188*, 288-306.

Schabram, K., Robinson, S. L., & Cruz, K. S. (2018). Honor among thieves: The interaction of team and member deviance on trust in the team. *Journal of Applied Psychology, 103*(9), 1057-1066.

Schneider, B., Ehrhart, M. G., & Macey, W. H. (2011). Organizational climate research. *The handbook of organizational culture and climate, 29,* 12169-012.

Schwarzer, G. (2007). meta: An R package for meta-analysis. *R News, 7*, 40-45.

Ścigała, K. A., Schild, C., Heck, D. W., & Zettler, I. (2019). Who deals with the devil? Interdependence, personality, and corrupted collaboration. *Social Psychological and Personality Science*, *10*(8), 1019-1027.

Seijts, G. H., & Latham, G. P. (2000). The effects of goal setting and group size on performance in a social dilemma. *Canadian Journal of Behavioural Science/Revue canadienne des sciences du comportement, 32*(2), 104-116.

Serota, K. B., Levine, T. R., & Boster, F. J. (2010). The prevalence of lying in America: Three studies of self-reported lies. *Human Communication Research, 36*(1), 2-25.

Shalvi, S., Dana, J., Handgraaf, M. J., & De Dreu, C. K. (2011). Justified ethicality: Observing desired counterfactuals modifies ethical perceptions and behavior. *Organizational Behavior and Human Decision Processes, 115*(2), 181-190.

Shalvi, S., & De Dreu, C. K. (2014). Oxytocin promotes group-serving dishonesty. *Proceedings of the National Academy of Sciences, 111*(15), 5503-5507.

Shalvi, S., Handgraaf, M. J., & De Dreu, C. K. (2011). Ethical manouvering: Why people avoid both major and minor lies. *British Journal of Management, 22*, S16-S27.

Shalvi, S., Gino, F., Barkan, R., & Ayal, S. (2015). Self-serving justifications: Doing wrong and feeling moral. *Current Directions in Psychological Science, 24*(2), 125-130.

*Soraperra, I., Weisel, O., Zultan, R., Kochavi, S., Leib, M., Shalev, H., & Shalvi, S. (2017). The bad consequences of teamwork. *Economics Letters, 160*, 12-15.

*Spadaro, G., Van Prooijen, J., Romano, A., Mosso, C. O., Van Lange, P. (2021). Corruption Undermines Trust and Cooperation Between People. Working paper.

Spoor, J. R., & Kelly, J. R. (2004). The evolutionary significance of affect in groups: Communication and group bonding. Group processes & intergroup relations, 7(4), 398-412.

Sterne, J. A., Sutton, A. J., Ioannidis, J. P., Terrin, N., Jones, D. R., Lau, J., ... & Higgins, J. P. (2011). Recommendations for examining and interpreting funnel plot asymmetry in meta-analyses of randomised controlled trials. *Bmj, 343.*

Suchotzki, K., Verschuere, B., Van Bockstaele, B., Ben-Shakhar, G., & Crombez, G. (2017). Lying takes time: A meta-analysis on reaction time measures of deception. *Psychological Bulletin*, *143*(4), 428-453.

*Sutter, M. (2009). Deception through telling the truth?! Experimental evidence from individuals and teams. *The Economic Journal*, *119*(534), 47-60.

Sze, J. A., Gyurak, A., Goodkind, M. S., & Levenson, R. W. (2012). Greater emotional empathy and prosocial behavior in late life. *Emotion, 12*(5), 1129-1140.

Tenbrunsel, A. E., & Messick, D. M. (2004). Ethical fading: The role of self-deception in unethical behavior. *Social justice research, 17*(2), 223-236.

*Thielmann, I., Böhm, R., & Hilbig, B. E. (2021). Buying Unethical Loyalty: A Behavioral Paradigm and Empirical Test. *Social Psychological and Personality Science, 12*(3), 363-370.

Touré-Tillery, M., & Fishbach, A. (2012). The end justifies the means, but only in the middle. *Journal of Experimental Psychology: General, 141*(3), 570-583.

Van Beest, I., Van Dijk, E., De Dreu, C. K., & Wilke, H. A. (2005). Do-no-harm in coalition formation: Why losses inhibit exclusion and promote fairness cognitions. *Journal of Experimental Social Psychology, 41*(6), 609-617.

van Dijk, E., & De Dreu, C. K. (2021). Experimental Games and Social Decision Making. *Annual Review of Psychology, 72*, 415-438.

Van Lange, P. A. (1999). The pursuit of joint outcomes and equality in outcomes: an integrative model of social value orientation. *Journal of Personality and Social Psychology, 77*(2), 337-349.

*Verwijmeren, T., van Lent, T., & Bijlstra, G. (2021). The influence of collaborating with an in-group or out-group member on dishonest behavior. Working paper , partly available at: https://theses.ubn.ru.nl/handle/123456789/6559.

Viechtbauer, W. (2010). Conducting meta-analyses in R with the metafor package. *Journal of Statistical Software, 36*, 1- 48

Viechtbauer, W., & Cheung, M. W. L. (2010). Outlier and influence diagnostics for meta‐analysis. *Research Synthesis Methods, 1*(2), 112-125.

Vranka, M., Frollová, N., Pour, M., Novakova, J., & Houdek, P. (2019). Cheating customers in grocery stores: A field study on dishonesty. *Journal of Behavioral and Experimental Economics, 83*, 101484.

*Weisel, O., & Shalvi, S. (2015). The collaborative roots of corruption. *Proceedings of the National Academy of Sciences, 112*, 10651-10656.

Welsh, D. T., Ordóñez, L. D., Snyder, D. G., & Christian, M. S. (2015). The slippery slope: How small ethical transgressions pave the way for larger future transgressions. *Journal of Applied Psychology*, *100*(1), 114-127.

West, M. A., & Anderson, N. R. (1996). Innovation in top management teams. *Journal of Applied Psychology*, *81*(6), 680-693.

Wiltermuth, S.S. (2011). Cheating more when the spoils are split. *Organizational Behavior and Human Decision Processes, 115*, 157-168.

Woodley, H. J., Bourdage, J. S., Ogunfowora, B., & Nguyen, B. (2016). Examining equity sensitivity: An investigation using the Big Five and HEXACO models of personality. *Frontiers in psychology, 6*, 2000.

*Wouda, J., Bijlstra, G., Frankenhuis, W. E., & Wigboldus, D. H. (2017). The collaborative roots of corruption? A replication of Weisel & Shalvi (2015). *Collabra: Psychology, 3*(27), 1-3.

Wu, J., Balliet, D., & Van Lange, P. A. (2016). Reputation, gossip, and human cooperation. *Social and Personality Psychology Compass, 10*(6), 350-364.

Yetton, P., & Bottger, P. (1983). The relationships among group size, member ability, social decision schemes, and performance. *Organizational Behavior and Human Performance, 32*(2), 145-159.

Footnotes

1. Note that whereas people are generally inequality averse, there are notable individual differences in inequality aversion (e.g., Woodley et al., 2016), and that some prosocial individuals may not mind inequality if they are in the disadvantageous position.
2. On the treatment level, the 'report assuming honesty' is 2.5 MU, which is the expected payoff of a random die-roll when each dot on the die earns one MU but reporting a ”6” leads to earning 0.
3. To calculate the treatment-level standardized group report, we first averaged the reports of all groups and used this measure as the "group's report" in the equation. For the group-level, we calculated the standardized group report for each group separately. We used this approach because for some manuscripts, we only had information on a treatment level.
4. Additional procedures testing for publication bias cannot be meaningfully applied for the current meta-analysis. For example, the funnel-plot asymmetry, Egger's regression, and the trim and fill approaches consider a link between the effect size and sample size as an indication for publication bias (Carter et al., 2019; Sterne et al., 2011). This assumption is built on the idea that small-sample studies with non-significant results will be less likely to be published and thus be missing from a meta-analysis sample. Given that our meta-analysis does not examine a treatment versus control effect size with an associated *p*-value, these approaches are not applicable in this case.
5. Note the average of the standardized group report calculated on the group-level is not equal to the treatment-level standardized group report when the distribution of payoffs is

not symmetric compared with honesty. The reason is that the equation for calculating the standardized group report is a non-linear transformation of groups' report. For example, consider three groups in the dyadic die-rolling task. One group reports a double 6, earning $12 together ($6 each), and two groups report non-doubles, earning $0. In this task, honest dyads should earn, on average, $(3.5 \times 1/6) \times 2 = \$1.16$, the minimum groups can earn is $0, and the maximum is $12. When calculating the standardized group report for all groups together (like we do for the treatment-level variable), the average earnings for all groups is $12/3 = $4, making the standardized group report (4 - 1.16) / (12 - 0) = 0.24. When calculating the standardized group report for each group separately, the dyad reporting a double 6 gets the value 1, and the dyads reporting non-doubles get the value -1 each, resulting in an average of -0.33.

6. See SOM for additional analyses on the effect of financial incentives between and within treatments in sequential decisions.
7. Under the null hypothesis of honesty, every die-roll is an independent draw from a uniform distribution. Testing this hypothesis does not require controlling for the nested nature of the data.
8. First movers' reports did not differ from reports of participants in equivalent tasks employed in individual settings; see SOM.
9. Second movers' reports were significantly higher than the reports of participants in equivalent tasks employed in individual settings; see SOM.

10. Results were robust to employing alternative classifications of brazenness, that is, when brazenness was defined as reporting 6's (for first movers) or doubles (for second movers) in at least 90%, 80%, and 70% of the rounds (see SOM).