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Does Hoodwinking Others Pay? The Psychological and Relational Consequences of Undetected Negotiator Deception

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Lies often go undetected, and we know little about the psychological and relational consequences of successfully deceiving others. While the evidence to date indicates that undetected dishonesty induces positive affect in independent decision contexts, we propose that it may elicit guilt and undermine satisfaction in negotiations despite facilitating better deals for deceivers. Across four studies, we find support for a *deceiver's guilt account*, whereby dishonesty triggers guilt and lessens negotiators' satisfaction with the bargaining experience. This pattern is robust to several factors, including the size of negotiators' incentives and individual differences in negotiators' moral character. It holds for both lies issued of negotiators' own volition and in compliance with others' orders. Large incentives also exacerbated dishonesty-induced guilt. Further, dissatisfaction stemming from dishonesty-induced guilt had downstream relational consequences. Despite going undetected, dishonesty in a focal negotiation reduced deceivers' likelihood of choosing to interact again with the same counterpart and adversely impacted their satisfaction in future negotiations with that counterpart.

Keywords: unethical behavior, deception, negotiation, subjective value, guilt

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Dishonesty is prevalent in social interactions. Most people tell at least one lie per day and deceive at least 30% of people they interact with during any given week (DePaulo et al., 1996). Dishonest behavior is particularly prevalent in mixed-motive social interactions like negotiations where individuals face a tension between pursuing their self-interest and cooperating with others whose outcomes are dependent on their actions (Lewicki, 1983; Murnighan et al., 1999). Deceptive tactics can entice targets into deals they would otherwise reject (Jap et al., 2011; Kray et al., 2014). Hence, negotiators frequently resort to dishonest tactics like misrepresenting their interests (O'Connor & Carnevale, 1997), omitting information relevant to their counterparts' bottom line (Schweitzer & Croson, 1999), emphasizing misleading truthful statements (Rogers et al., 2017), and even telling outright lies (Kern & Chugh, 2009).

Given the potency and questionable ethicality of deceiving others, the topic has drawn the interest of negotiation scholars (e.g., Boles et al., 2000; Robinson et al., 2000; Schweitzer & Croson, 1999; Tenbrunsel, 1998). While many have tested the antecedents of dishonesty (e.g., Gneezy, 2005; Kern & Chugh, 2009; Kray et al., 2014; Malhotra & Gino, 2011; Van Zant & Kray, 2014), along with the economic and relational consequences of *detected* deception (e.g., Boles et al., 2000; Schweitzer et al., 2006; Shapiro, 1991; Tyler et al., 2006), relatively little work has considered the psychological and relational consequences of undetected negotiator dishonesty (Gaspar & Schweitzer, 2013)—particularly from the perspective of deceivers.

The lies people tell often go undetected (Bond & DePaulo, 2006). While exposed dishonesty elicits negative feelings from negotiation counterparts (Smith et al., 2002) and damages relationships with those targeted by one's dishonesty (Boles et al., 2000), it is unclear how getting away with a lie impacts focal negotiators' psychological experience and their relationship with counterparts. There are two predominant perspectives about the consequences of getting away with deception in a negotiation. On the one hand, getting away with

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Data, materials, and analysis code for all our studies, along with preregistrations for Studies 1, 2, and 3B are available at: https://researchbox.org/644.

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a lie can be lucrative, thrilling, and elicit positive affect (Ekman, 1992; Ruedy et al., 2013). On the other hand, it is also widely assumed that dishonesty can elicit feelings of guilt (Triandis et al., 2001), leaving deceivers feeling dissatisfied and undermining their relationship with counterparts.

In the current research, we test the validity of these two perspectives by examining how undetected dishonesty impacts focal negotiators' affect, subjective satisfaction with the negotiation experience, and relationship with the counterparts they misled. By doing this, we make several contributions to the literature on interpersonal deception and negotiation. First, we attempt to reconcile two seemingly discrepant theoretical perspectives about dishonesty's affective consequences in interpersonal interactions. Second, we identify negotiators' dishonesty as a critical determinant of their satisfaction with the negotiation experience. By attempting to understand how dishonesty impacts deceivers' subjective perception of negotiation, we move beyond prior research that has primarily focused on the economic consequences of negotiator dishonesty toward understanding how dishonesty impacts the psychological and relational value deceivers derive from the act of negotiating (cf. Curhan et al., 2006; Van Zant & Kray, 2015). Finally, we expand on prior research examining the relational consequences of dishonesty in interpersonal interactions. In contrast to prior work that has tested the adverse relational consequences of *detected* deception in interpersonal interactions (e.g., Boles et al., 2000; Tyler et al., 2006), we examine the downstream impact of undetected deception on the relationship between focal negotiators and their counterparts. Our inquiry is the first to our knowledge to explore the relative validity of two plausible theoretical perspectives regarding the consequences of dishonesty in negotiation settings.

The Affective Consequences of Undetected Dishonesty for Negotiators

Dishonesty and Positive Affect

Ekman (1992) suggested that people feel a sense of accomplishment for misleading others, which elicits positive emotions. He labeled this hypothesized phenomenon "duping delight." In the first empirical demonstration of duping delight, Ruedy et al. (2013) found that, compared to those who put forth an honest effort on a task, people who resorted to lying and cheating to enhance their performance reported a boost in positive affect after learning that their dishonesty went undetected by experimenters. Peer et al. (2014) later conceptually replicated this finding. In their studies, participants who overstated their success at predicting the outcomes of coin flips experienced more positive affect than those who were honest.

Although scholars have found evidence of duping delight in independent decision tasks, they have yet to test the hypothesis that dishonesty elicits positive affect in the context of social interaction (DePaulo et al., 2003). The evidence of duping delight comes from contexts where dishonesty does not adversely impact a tangible target, such as breaking a rule on an individual task (Ruedy et al., 2013) or privately misreporting one's performance on a task to an unspecified target with no stake in the task (Peer et al., 2014; Ruedy et al., 2013). These private decision contexts are insightful in understanding phenomena like why people cheat on their tax returns, overclaim expenses at work, and even concoct elaborate schemes to manipulate financial markets. While significant, these contexts do not necessarily speak to the consequences of lying to tangible targets adversely impacted by dishonesty. Relative to individual tasks and interactions with an unspecified target, people are more likely to consider the ethical ramifications of their actions when they will directly impact a concrete person (Small & Loewenstein, 2003).

We focus on interdependent decision contexts in this research. Negotiating is necessarily interdependent because one party's outcome depends on the other party's behavior (De Dreu, 2010; Thompson, 1990). While misleading other negotiators might feel like a triumph and produce positive affect (Ruedy et al., 2013), we also consider the possibility that dishonest negotiators may instead construe their behavior as an ethical violation, triggering guilt that overrides any positive affect.

Dishonesty and Guilt

While the empirical evidence supports the notion that dishonesty elicits positive affect, there is also reason to suspect that it elicits guilt. Guilt is a moral emotion that arises when people consider the impact of their ethical transgressions on others (Smith et al., 2002; Tangney, 1991; Tangney et al., 2007). Therefore, dishonesty in interdependent social interactions like negotiations may be particularly likely to trigger guilt.

Direct empirical evidence for the link between dishonesty and guilt is surprisingly lacking. Existing studies focus on anticipated, not experienced, emotion (e.g., Mazar et al., 2008; Ruedy et al., 2013), or they rely on recall tasks where people describe past events that triggered a manipulated target emotion (e.g., Baumeister et al., 1995; Tangney, 1992; Tracy & Robins, 2006). People struggle to accurately anticipate the affective consequences of their ethical infractions (Mazar et al., 2008; Ruedy et al., 2013) and are prone to errors in recalling their affective experiences while retrieving memories of distant events (Thomas & Diener, 1990). Often, people rationalize unethical behavior through moral disengagement, a process that helps them to reduce negative emotions such as shame and guilt after learning the consequences of their actions (Tillman et al., 2018). As a result, studies measuring prospective or retrospective guilt may not generalize to individuals' immediate affective response to a dishonest act. Further, while severe ethical infractions like the administration of an electric shock can elicit guilt (Buss & Brock, 1963), studies directly examining the affective consequences of more mundane forms of unethical behavior (i.e., lying and cheating) have found evidence that appears inconsistent with unethical behavior eliciting guilt. Instead, these studies report null effects on generalized negative affect (Ruedy et al., 2013) and even evidence that dishonesty *reduces* negative affect (Lee et al., 2015).

One possible reason for the lack of evidence for dishonesty inducing guilt in previous studies is that any potential effect was suppressed by a selection bias: People who choose to be dishonest are also the least prone to experiencing guilt in the first place. Anticipatory guilt regulates individuals' ethical judgment in interpersonal interactions (Baumeister et al., 1994, 2007; Kouchaki & Kray, 2018; Tangney et al., 2007). Due to their predisposition to anticipate that they will feel guilty after committing an ethical transgression, highly guilt-prone individuals are less likely to engage in dishonest acts than people who are not particularly guilt-prone (Cohen et al., 2011). Because some studies allowed participants to self-select their degree of dishonesty without comparison to a control condition where they did not have the

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opportunity to be dishonest (e.g., Lee et al., 2015), any apparent effects of dishonesty on negative affect could be an artifact of self-selection such that people with a low propensity for guilt are overrepresented in researchers' samples of participants who choose to behave dishonestly.

Another issue is that prior studies examining the affective consequences of dishonesty do not directly measure participants' guilt. Some studies report generalized negative affect measures that do not include a guilt item (e.g., Ruedy et al., 2013). Others report a 10-item measure of negative affect that only includes a single item relevant to guilt (e.g., Lee et al., 2015; Peer et al., 2014; Ruedy et al., 2013). These choices raise the possibility that dishonesty elicits guilt, but that null or opposing effects on other negative emotions obscure the effect. Scholars have argued that guilt is the single emotion most directly associated with ethical transgressions (Cohen et al., 2011; Eisenberg, 2000; Tangney et al., 2007). Thus, prior research might have underestimated the affective drawbacks of dishonesty. The current research aims to provide more conclusive evidence about the link between dishonesty and guilt by incorporating guilt-specific measures to explore the effects of both self-selected and experimentally manipulated dishonesty.

Subjective Value in Negotiation and Its Relational Consequences

We suggest that the degree to which dishonest negotiators feel positive affect or guilt could shape their subjective appraisal of a negotiation. Beyond a deal's economic terms, negotiations produce psychological and relational outcomes grounded in both parties' perception of the bargaining experience (Olekalns & Kennedy, 2020; Thompson, 1990). These psychological outcomes, termed *subjective value*, capture negotiators' overall satisfaction with the negotiation process, the agreement terms, their relationship with counterparts, and feelings about their self-efficacy (Curhan et al., 2006).

Independently of their objective deal terms in a focal negotiation, negotiators' subjective value predicts their economic outcomes in future negotiations with the same counterpart (Curhan et al., 2010). Some evidence even suggests that subjective value impacts long-term consequences of negotiated agreements (e.g., job satisfaction and turnover intentions) better than negotiators' economic outcomes do (Curhan et al., 2009). The greater negotiators' subjective value in a focal negotiation, the more favorably they appraise their negotiation counterpart and the greater their desire to bargain again with that person (Curhan et al., 2006, 2010; Oliver et al., 1994). Therefore, understanding the impact of dishonesty on negotiators' subjective value could reveal important relational consequences. Any impact of dishonesty on subjective value could have implications for negotiators' appraisal of future interactions with counterparts they successfully deceived and impact decisions about whether to maintain relationships with those counterparts.

Affect and Subjective Value

To predict how negotiator dishonesty impacts subjective value and relationships with counterparts, we make two assumptions. The first is that an action can simultaneously elicit positive affect and guilt (Goldsmith et al., 2012; Macht & Dettmer, 2006; Ramanathan & Williams, 2007). Although they are often negatively correlated, positive and negative emotions are distinct constructs (Diener et al., 1985; Watson & Tellegen, 1985), and people frequently experience them in tandem (Fong, 2006; Larsen et al., 2001; Rothman et al., 2017). We therefore assume that dishonesty may simultaneously trigger positive affect and guilt. Although they do not report effects on guilt specifically, Peer et al. (2014) provide evidence consistent with this assumption by reporting that dishonesty simultaneously elicited positive and negative affect.

Our second assumption is that the affective state most strongly triggered by dishonesty will exert the most influence on negotiators' subjective value. Individuals' actions can lead to changes in their affective state that provide a signal about how they should evaluate their environment (Allen et al., 1992; Westbrook, 1987) and shape their satisfaction (Oliver, 1993). Whereas negotiators should perceive that they are satisfied with a negotiation if it elicits a boost in positive affect, they should perceive that they are relatively *dis*satisfied with the negotiation if it more strongly elicits guilt.

We propose two accounts for how the relative degree to which dishonesty triggers positive affect versus guilt should shape negotiators' subjective value and, in turn, their relationship with counterparts. Whereas one account holds that dishonesty increases negotiators' subjective value because it elicits more positive affect than guilt, the other holds that dishonesty reduces negotiators' subjective value because it elicits more guilt than positive affect.[1] We do not view these accounts as mutually exclusive. Because the degree to which dishonesty induces guilt may depend on negotiators' incentives and moral character, dishonesty may simultaneously trigger more positive affect than guilt in some situations and for some individuals, while triggering more guilt than positive affect in other situations and for other individuals.

Deceiver's Delight Account

The *deceiver's delight account* assumes that dishonest negotiators experience more positive affect than guilt. Dishonest behavior tends to reflect a mindset where negotiators prioritize their self-interest above social and ethical outcomes (Wang et al., 2014; Zhong, 2011). By enabling negotiators to claim more value for themselves (O'Connor & Carnevale, 1997) and distorting or concealing information that would otherwise make agreement difficult (Jap et al., 2011; Kray et al., 2014), dishonesty often enhances negotiators' economic outcomes. Thus, lying enables dishonest negotiators to achieve what is typically their primary objective. This should leave them relatively satisfied with the negotiation experience.

Further, negotiations may represent a context where people more easily rationalize deception and alleviate guilt. The immediate social context shapes people's ethical standards (for a review, see Moore & Gino, 2013), and negotiations are one context where many forms of dishonesty are perceived as acceptable (Mason et al., 2018; Robinson et al., 2000). Because negotiators often perceive dishonesty as normative competitive behavior, they might be relatively unlikely to feel guilty and more likely to experience positive affect after successfully using deception to gain an edge over their

[1] We recognize it could also be possible that dishonesty elicits similar levels of guilt and positive affect. However, we did not formally hypothesize an effect of dishonesty on subjective value in this case because we assume any satisfaction triggered by positive affect should be canceled out by a similar reduction in satisfaction triggered by guilt.

counterpart. Thus, dishonesty could elicit levels of positive affect that dwarf any guilt it may trigger. This pattern would be consistent with Ruedy et al. (2013), who consistently show that dishonesty increases positive affect more than negative affect. However, as described earlier, the authors found this pattern in independent decision contexts where dishonesty does not impact a tangible target. These findings also do not directly address the impact of dishonesty on guilt. Thus, it is unclear whether prior studies generalize to negotiations, where one's dishonesty adversely impacts a specific victim.

Positive affect tends to increase the accessibility of positive cognitive associations in one's mind (Isen et al., 1978), which could presumably increase negotiators' subjective appraisal of a focal negotiation experience. Negotiators' subjective value is shaped by perceptions of their own self-efficacy, the negotiation process, their relationship with counterparts, and outcome satisfaction (Curhan et al., 2006). Thus, if positive affect increases the degree to which negotiators view a negotiation as easy, fair, socially engaging, and economically lucrative, then it should increase the subjective value they derive from the negotiation.

Several findings in the negotiation literature support the assertion that positive affect increases negotiators' subjective value. First, incidentally induced positive affect can increase negotiators' self-efficacy (Baron, 1990; Kramer et al., 1993). Second, positive emotions tend to increase negotiators' cooperativeness with counterparts (Allred et al., 1997; Anderson & Thompson, 2004; Moore et al., 1999; Shirako et al., 2015) and reduce their use of aggressive tactics (Carnevale & Isen, 1986; Forgas, 1998). Finally, relative to negotiators in a neutral mood, those in a positive mood tend to evaluate their performance more favorably (Kramer et al., 1993).

Thus, to the extent that any positive affect induced by dishonesty overrides its impact on guilt, these findings suggest that dishonesty increases negotiators' subjective value. The increased subjective value of dishonest negotiators should, in turn, lead them to derive more subjective value in future interactions with the same counterpart and increase their likelihood of choosing to maintain a relationship with the counterpart.

Hypothesis 1a (Deceiver's Delight Hypothesis): Dishonesty causes negotiators to experience more positive affect than guilt and *increases* the subjective value they derive from a negotiation.

Deceiver's Guilt Account

In contrast, the *deceiver's guilt account* holds that dishonesty induces more guilt than positive affect. Guilt tends to trigger upward counterfactual thoughts in which people envision a more desirable alternative world where they engaged in actions more consistent with their ethical values (Niedenthal et al., 1994). Upward counterfactual thinking undermines individuals' satisfaction with several outcomes, including their performance in a competition (Markman et al., 1993; Medvec et al., 1995), grade in a class (Medvec & Savitsky, 1997), and even the economic outcome of a negotiation (Galinsky et al., 2002). Thus, even in situations where dishonesty objectively improves one's economic outcome and elicits some positive affect, it could trigger more guilt than positive affect, leaving negotiators dissatisfied. This dissatisfaction should negatively color their perceptions of future interactions with the same counterpart and increase their likelihood of avoiding future negotiations with the counterpart.

Hypothesis 1b (Deceiver's Guilt Hypothesis): Dishonesty causes negotiators to experience more guilt than positive affect and *reduces* the subjective value they derive from a negotiation.

Person and Situation Factors Impacting Dishonesty-Induced Guilt

Relative to other emotions, guilt is particularly sensitive to the degree to which actors perceive their actions as diverging from their own internal standards for acceptable behavior (Baumeister et al., 1994). Therefore, any factors that shape how much guilt is elicited by dishonesty have the potential to influence whether dishonesty-induced guilt exceeds or is dwarfed by increases in positive affect that may be associated with lying.

People are motivated to see themselves as having high ethical standards (Mazar et al., 2008), and guilt is an aversive state that emerges when they perceive their behavior as violating internal ethical standards. Here, we consider how two potential moderating factors shape the degree to which negotiators perceive their dishonesty as falling short of their own ethical standards, thereby shaping the potential for increased guilt. Although these factors may also impact positive affect, we assume that guilt should be particularly sensitive to these factors due to its intricate link with perceiving an inconsistency between one's behavior and one's morals.

Incentives to Lie

The size of incentives to lie are one factor that could impact negotiators' perception that a dishonest act falls short of their ethical standards. Evidence for the directional effect of incentive size on dishonesty-induced guilt is mixed. Therefore, we consider two possibilities.

Why Large Incentives May Reduce Dishonesty-Induced Guilt

One possibility is that large incentives alleviate any guilt experienced by dishonest negotiators because they provide a stronger external justification for dishonesty than small incentives. Engaging in an act of dishonesty can elicit discomfort that people attempt to alleviate by denying that their behavior was dishonest (Kouchaki & Gino, 2016). However, when people can attribute their dishonesty to external causes, they perceive themselves as less culpable for their actions (Dana et al., 2007; Murnighan et al., 2001).

In a classic demonstration of psychological reactions to incentives, Festinger and Carlsmith (1959) found that participants provided with a small incentive to convince someone that a mundane task was exciting came to perceive the task as more enjoyable. However, those provided with a much larger incentive did not change their attitudes about the task. The authors attributed this pattern to cognitive dissonance. When the incentive was small, participants did not have a strong justification for their dishonesty. Facing the realization that they misled another person, participants convinced themselves that they genuinely enjoyed the task to minimize their self-perceived dishonesty. In contrast, when the incentive was large, participants had a strong external justification

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for their dishonesty that enabled them to rationalize it. The large incentive generated perceptions of having little choice but to lie about the task. Because the large incentive made participants feel less culpable for their behavior, it minimized their self-perceived unethicality and removed the need to delude themselves into believing the task was enjoyable.

Readers could extrapolate from Festinger and Carlsmith's (1959) findings to conclude that participants presented with the small incentive felt guiltier about their behavior than participants in the large incentive condition. If so, then incentives moderate the impact of dishonesty on negotiator guilt such that small incentives exacerbate dishonesty-induced guilt and large incentives reduce it. Thus, dishonest negotiators might experience stronger guilt in the presence of a small incentive but weaker guilt as the incentives to be gained from lying increase.

> *Hypothesis 2a (Incentives Alleviate Guilt Hypothesis):* Compared to small financial incentives, larger financial incentives alleviate negotiators' dishonesty-induced guilt.

Why Large Incentives May Increase Dishonesty-Induced Guilt

Yet another possibility could be that larger incentives exacerbate the guilt experienced by dishonest negotiators. Several scholars have suggested that emotional reactions triggered by dishonesty amplify in high-stakes settings (DePaulo et al., 2003; Ekman, 1992). Taking this hypothesis into account with guilt's intricate link to committing ethical transgressions, guilt may be an emotion that is particularly likely to magnify as the stakes to be gained from lying increase.

Two sets of findings are consistent with this account. First, many studies across disciplines find that choices to lie are surprisingly insensitive to the payoff to be gained from lying (Abeler et al., 2019). Although increased financial stakes should presumably make lying more appealing to one's self-interest, the empirical findings suggest that would-be deceivers weigh deception's increased appeal against an offsetting psychological cost that also increases with the financial stakes—perhaps in the form of anticipatory guilt.

A second finding consistent with this account comes from Rahwan et al.'s (2018) finding that participants who cheated in the presence of large financial stakes perceived their actions to be less ethical than those who told lies for smaller financial rewards. Although the authors do not directly measure guilt, this finding could indicate that, relative to low-stakes dishonesty, high-stakes dishonesty induces more guilt. Extending this logic, dishonest negotiators might be less prone to experiencing guilt in the presence of a small incentive to lie, and more likely to feel guilty as the financial stakes increase.

> *Hypothesis 2b (Incentives Exacerbate Guilt Hypothesis):* Compared to small financial incentives, larger financial incentives exacerbate negotiators' dishonesty-induced guilt.

Moral Character

Whereas incentives vary by situation, a relevant individual difference variable that influences negotiator ethics across situations is moral character. Moral character plays a central role in shaping individuals' ethical standards for what constitutes acceptable behavior. A broad construct encompassing many personality traits (Cohen et al., 2014), moral character at its core can be distilled into three components: empathy for others, resisting the temptation to behave impulsively, and defining oneself in terms of moral traits (Cohen & Morse, 2014).

Moral character is strongly linked with guilt. Because anticipatory guilt is a powerful driver of ethical choice, people high in moral character anticipate feeling strong guilt for committing an ethical transgression (Cohen et al., 2012). Individual differences central to moral character—empathy, self-control, and identifying morality as core to one's self-concept—all positively correlate with guilt proneness and the perception that it is inappropriate to use unethical negotiation tactics (Cohen et al., 2014). These findings suggest that relative to negotiators with low moral character, those with high moral character perceive dishonesty to be a greater ethical infraction and anticipate that deceiving a counterpart will make them feel guiltier. If negotiators' anticipatory guilt accurately captures their experienced guilt, then dishonesty may trigger more guilt in negotiators high in moral character than those low in moral character.

> *Hypothesis 3 (Moral Character Exacerbates Guilt Hypothesis):* Compared to negotiators with relatively low moral character, those with higher moral character experience more dishonesty-induced guilt.

Notably, this prediction depends on moral character's link with anticipatory guilt, which may not necessarily translate to patterns in experienced guilt. People are notoriously prone to overestimating the intensity and duration of their emotions (Wilson & Gilbert, 2005). Affective forecasting errors are particularly prevalent for negative emotions because people are motivated to engage in rationalizations that lessen their intensity. Rationalizations for unethical behavior are often triggered by cognitive dissonance (Bandura et al., 1996; Detert et al., 2008), and lying might be particularly likely to elicit cognitive dissonance among negotiators relatively high (vs. low) in moral character because it is more discrepant with their self-concept. Thus, negotiators with relatively high moral character may be particularly prone to engaging in rationalizations that minimize their guilt, such as forgetting that their counterpart was unaware of information they concealed (Shu et al., 2011) or justifying their dishonesty as a defensive tactic designed to protect them from a counterpart's potential use of unethical tactics (Tenbrunsel, 1998). This raises the possibility that possessing a high level of moral character may not exacerbate dishonesty-induced guilt.

Overview of Studies

Across a series of four studies, we test both the *deceiver's delight* and *deceiver's guilt* accounts (Hypotheses 1a and 1b). In addition to examining the impact of important person (i.e., moral character) and situation (i.e., incentive size) factors that might influence the degree to which these accounts hold, we also explore the robustness of our findings to several different contexts, such as self-serving lies told of one's own volition (Studies 1 and 2), lies told as an agent on others' orders (Studies 3A and 3B), and lies told both in anonymous interactions with strangers (Studies 1 and 3B) and face-to-face interactions with acquaintances (Study 3A).

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Our studies also take three additional steps to provide insight into the underlying affective processes that shape negotiators' reaction to getting away with a lie. First, in addition to comparing the impact of dishonesty on positive affect and guilt, Studies 1 and 2 also test for indirect effects on subjective value through positive affect and guilt. Second, these studies collect measures of generalized negative affect that exclude guilt. In so doing, we aim to differentiate the consequences of dishonesty attributable to guilt from other negative emotions. Third, in a study spanning multiple negotiations (Study 2), we explore whether the affective consequences of a focal negotiation carry over to a second negotiation with the same counterpart where negotiators did not have an opportunity to lie.

Unless reported otherwise, we measure subjective value using the 13-item Subjective Value Inventory (SVI) developed by Curhan et al. (2009). Although subjective value captures negotiators' satisfaction on four dimensions, they are highly intercorrelated (Curhan et al., 2006). As a result, we follow other researchers' practice of analyzing subjective value as a single global construct (e.g., Becker & Curhan, 2018; Curhan et al., 2009, 2010). That said, our results are robust to each of the four dimensions of subjective value (see online Supplemental Materials, Figure S1).[2] In our studies, negotiators' dishonesty went undetected and enhanced their economic outcomes (see online Supplemental Materials for supplemental analyses).

We report how we determined our sample sizes, all data exclusions (if any), all manipulations, and all measures. Following the recommendations of Giner-Sorolla et al. (2019), we report sensitivity analyses that describe the minimum effect we could detect with 80% power, given the observed sample size of each study and the statistical model used for a given test. We describe these sensitivity analyses in the results section of each study. Unless noted otherwise, we conducted these analyses using the G*Power default settings (Faul et al., 2007). This research received institutional review board approval. Data, materials, and analysis code for all our studies, along with preregistrations for Studies 1, 2, and 3B are available at https://researchbox.org/644; Study 3A was not preregistered.

Study 1: Large and Small Incentives to Be Dishonest in a Negotiation

Study 1 tests the deceiver's delight and guilt accounts within a dyadic negotiation involving an information asymmetry typical of many buyer-seller interactions (Akerlof, 1970). Participants assigned to a dishonesty opportunity condition could mislead counterparts about the condition of a computer they were selling; those in a control condition negotiated with a counterpart who was aware of a defect in the computer, thus eliminating the opportunity to deceive. Importantly, whereas prior research finding evidence of duping delight has only compared generalized negative affect to generalized positive affect (Peer et al., 2014; Ruedy et al., 2013), we incorporated a guilt-specific measure to assess the degree to which dishonesty induces guilt.

Study 1 compares the affect and subjective value of participants who lied on their own volition to those without the opportunity to lie. If dishonesty elicits more positive affect than guilt and boosts negotiators' subjective value (relative to a control condition where participants do not have the opportunity to behave dishonestly), then this would indicate the deceiver's delight hypothesis (Hypothesis 1a) holds in negotiation contexts. However, if dishonesty elicits more guilt than positive affect and reduces negotiators' subjective value, this would lend support for the deceiver's guilt account (Hypothesis 1b).

Additionally, Study 1 tests whether the stakes to be gained from a successful negotiation moderate any impact of dishonesty on guilt. We compare a relatively small incentive to a large one that could potentially enable dishonest negotiators to justify their dishonesty. Should the large incentive alleviate dishonesty-induced guilt (Hypothesis 2a), then this would suggest that the deceiver's guilt account is more likely to emerge in the presence of large incentives than small incentives. However, if the large incentive exacerbates dishonesty-induced guilt (Hypothesis 2b), then this would suggest the deceiver's guilt account is less likely to emerge in the presence of large incentives than small incentives.

Method

Participants

Because we did not have an a priori expectation of what effect sizes to expect, we aimed to recruit a sample large enough to detect any simple effects equivalent to the average published effect size in social psychology ($\eta^2 = .04$; Richard et al., 2003) with at least 80% power. Thus, we preregistered a target sample of at least 400 negotiation dyads for our analyses and posted 1,200 assignments to Amazon Mechanical Turk to achieve a sample of this size. Based on preregistered criteria like those used by Rogers et al. (2017), we excluded participants in dyads where (a) both participants did not complete the study,[3] (b) at least one member of the dyad did not send a message during the negotiation, and (c) both parties disagreed about whether they reached agreement in the negotiation. After applying these exclusion criteria, we arrived at a sample of 982 participants paired into 491 negotiation dyads who participated in exchange for a $3.00 base payment ($M_{Age} = 40.4$ years, $SD = 12.6$, 47% female).

Procedure

The study follows a 2 (dishonesty opportunity, control) × 2 (incentive: large, small) between-subjects design; affect was a repeated measure varying within subjects (positive affect, negative affect, guilt). We created a negotiation task that enabled us to manipulate the presence or absence of an opportunity to deceive a counterpart. Participants learned that they were randomly assigned to buyer or seller roles in a negotiation with another MTurk worker over the hypothetical sale of a computer. To help the process of pairing participants in a chatroom go as smoothly as possible, we eliminated inattentive participants at the start of the study (i.e., before random assignment to experimental condition) by redirecting participants out of the study who either (a) did not indicate that they agreed to devote up to 20 min of uninterrupted time on the study, or

[2] This SVI scale also contains three items one could expect to be direct reactions to dishonesty: "I think the terms of my agreement were fair," "I behaved according to my own principles and values," and "the negotiation process was fair." Additional analyses revealed that the pattern of results holds when omitting these items (see online Supplemental Materials). We report analyses of the full 13-item scale because it is consistent with our preregistrations.

[3] This criterion also excludes participants who were never paired with a counterpart due to technical issues.

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(b) ignored an instruction on one page to wait for the screen to auto-advance to the next page without clicking the "next" button. All participants who passed those attention screeners continued through the remainder of the study but were required to pass a series of comprehension checks verifying their understanding of study procedures before being allowed to advance in the study.

After reading their role instructions, participants were paired with another MTurk worker in a chatroom using the SMARTRIQS platform (Molnar, 2019). We allotted participants a maximum of 10 min to negotiate, which was the platform's maximum allowable duration. Focal participants played the seller role. They imagined operating a small business where they "purchase used computers that are difficult to find, refurbish them, and sell them for a profit." Participants then read about a lightly used, limited-edition desktop computer they recently purchased for $2,500 through a connection. The computer is known for excelling at video-editing and costs $5,000 new. However, after purchasing the computer, participants discovered that its graphics card contains a manufacturing defect that causes the computer to crash. It would cost $1,000 to replace the defective graphics card.

Counterparts played the role of "an aspiring filmmaker looking to get serious about making short films and entering them into local film festivals." In search of the best computer on the market at a more affordable price than $5,000, counterparts searched for a used version of the computer on the advice of a friend who works in IT. They eventually found one sitting on a counter at the seller's store. After testing the computer in the store, they later sent the seller a message hoping to work out a deal.

Dishonesty Opportunity Manipulation. We randomly assigned dyads to either the dishonesty opportunity or control condition. In the dishonesty opportunity condition, counterparts did not notice the computer's defect and focal participants were aware of this information asymmetry. In the control condition, counterparts discovered the defective graphics card and acknowledged their awareness of the defect in a message to focal participants ahead of the negotiation. Focal participants received the following messages [dishonesty opportunity condition instructions/control condition instructions]:

> I'm currently quarantining but I tried the computer out last time I was over there. [Everything seemed to work just fine/I noticed that the machine crashed] while I was using the video editing software. I have heard nothing but great things about this computer, [and/but] its graphics card seems [to perform quite well/like it might be defective]. [So/Even though I might have to replace the graphics card], I'm interested in seeing if we can work out a deal for this computer so that I can possibly come in and pick it up in a few days.

To assure focal participants that counterparts would never learn anything else about the computer's condition unless they chose to reveal this information, we provided them with the following instructions just before they entered a chatroom to negotiate:

> Your negotiation role instructions are completely private. Other than the information the buyer has already told you about his or her experience using the computer in your store, the buyer does not know anything about the contents of your role instructions. What you choose to reveal to the buyer is completely up to you. We will never tell the buyer anything about the negotiation role instructions you have been provided. Feel free to tell the buyer whatever you would like in order to get the best deal for yourself.

We reinforced these instructions in a comprehension check question focal participants were required to answer correctly before they could proceed to the negotiation chatroom.

We structured counterpart buyers' payoffs to encourage them to raise the issue of the computer's condition. Prior to the negotiation, all counterparts were advised by their IT friend that failing to double-check that the computer's graphics card, hard drive, and monitor were in good condition could lead to them needing to pay for repairs that should be expected to cost an average of $1,000. All counterparts stood to earn a $0.10 bonus for every $250 they "managed to save on the sale price below $4,750" (up to a minimum purchase price of $2,500). Importantly, we factored in the cost of repairs into negotiators' payoffs. However, to maximize the external validity of counterparts' bargaining considerations, we also attempted to capture the fundamental difference between real-world buyers who know they are likely purchasing a defective used product (i.e., control condition counterparts) versus those who are unsure whether the product is defective or not. Therefore, whereas counterparts in the control condition were informed that we would add $1,000 the final purchase price to factor in the cost of repairing the defective graphics card when determining their bonus payment, those in the dishonesty opportunity condition were informed that we would add $1,000 to the final purchase price *if* the computer turned out to be defective (see online Supplemental Materials, Appendix S-A, for the payoff charts used for this study).

Incentive Manipulation. We conducted a pretest (N = 102 MTurk workers) to identify an incentive that participants should perceive as a "large bonus payment" sufficient to make them "comfortable with using any tactics necessary in the negotiation (including lying)" (see online Supplemental Materials). Based on the results of this pretest, we randomly assigned participants to either a large performance incentive where they stood to earn $1.25 for every $250 they sold the computer for above a price of $2,500 (up to a maximum of $12.50), or a small incentive where they stood to earn $0.10 for every $250 above this sale price (up to a maximum of $1.00).[4]

Postnegotiation Measures. Following the negotiation exercise, focal participants were asked a series of questions about their negotiation experience in a counterbalanced order.

Incentive Manipulation Check. To verify that participants perceived the large incentive to be stronger than the small incentive, we asked them to indicate their agreement with two statements [large incentive condition prompt/small incentive condition prompt]: "The [$1.00/$12.50] I stood to earn if I sold the computer for $4,750 or higher is a large bonus payment," and "The [$0.10/$1.25] I stood to earn for every $250 I managed to sell the computer for is a strong incentive" (1 = *strongly disagree* to 7 = *strongly agree*). The two items were reliable (α = .82) and averaged into a single index of perceived incentive size.[5]

[4] The ratio of difference between the high and low incentives in this study (12.5:1) exceeds the ratio used in previous manipulations of incentive size on MTurk populations described by researchers as "high" and "low" (10:1 or lower; Mason & Watts, 2009; Rogstadius et al., 2011; Yin et al., 2013).

[5] To assist us with quickly estimating how many participants met our preregistered criteria for inclusion in our main analyses as we monitored our progress towards achieving a preregistered data stopping rule, we also asked participants in this study, as well as Study 2 participants, to self-report how honest they were in the negotiation. We report analyses of these items in the online Supplemental Materials.

Dependent Measures. After the negotiation exercise, we assessed participants' subjective value on a scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*) using an adapted version of the 13-item SVI ($\alpha = .91$; Curhan et al., 2009). Sample items include: "I am satisfied with my outcome in this negotiation," "this negotiation made me feel more competent as a negotiator," "I am satisfied with the ease (or difficulty) of reaching agreement").

To measure positive and negative affect, we used the short form Positive and Negative Affect Schedule (PANAS; Mackinnon et al., 1999). Importantly, the short form PANAS does not include a guilt item, so the negative affect measure does not directly assess participants' guilt. We captured participants' guilt by adapting two items from Grant and Wrzesniewski (2010): "I feel guilty" and "I feel that I have not lived up to my ethical standards." All affect items were assessed on the same 7-point scale (1 = *very slightly or not at all* to 7 = *extremely*). As with the subjective value measure, we averaged all items for each affect measure into separate indices ($\alpha_{\text{Pos Affect}} = .88$, $\alpha_{\text{Neg Affect}} = .87$, $\alpha_{\text{Guilt}} = .86$).

Control Variables. To help rule out alternative explanations for any effects, we preregistered two covariates that we controlled for in our analyses. First, because dishonesty in negotiation exercises involving information asymmetries can increase agreement rates (Jap et al., 2011; Kray et al., 2014; Sheldon & Fishbach, 2015), we controlled for dyad-level agreement (1 = agreement, 0 = no agreement). Failing to reach an agreement tends to undermine negotiators' self-efficacy and elicit negative attitudes about counterparts (O'Connor & Arnold, 2001). Thus, controlling for agreement allowed us to disentangle effects on subjective value driven by agreement rates from those driven by participants' dishonesty and affect.

Second, we controlled for focal participants' concerns that their dishonesty may have been detected by their counterpart, as this could potentially explain any adverse effect of dishonesty on subjective value. We adapted five items from a scale used in prior research on deception in negotiation (Kray et al., 2014): "my counterpart doubted what I had to say about the condition of the computer and graphics card," "my counterpart was skeptical of what I had to say," "my counterpart was persistent in questioning me," "my counterpart believed me," and "my counterpart was satisfied with what I had to say about the condition of the graphics card" (last two items reverse-scored; 1 = *strongly disagree* to 7 = *strongly agree*). We averaged the five items into a single index of perceived counterpart suspicion ($\alpha = .81$).

Results

Following our preregistration, analyses compare focal participants in the dishonesty opportunity condition who chose to lie to control condition participants, unless otherwise noted. Three independent coders rated participants' honesty in the dishonesty opportunity condition ($\alpha = .90$; 0 = no lie, 1 = lie of omission, 2 = lie of commission). Among 246 focal participants in the dishonesty opportunity condition, 181 lied (74%) and were included in our main analyses. We followed our preregistered data analysis plan by using analyses of covariance (ANCOVA) to test for effects on subjective value and guilt; these analyses had 80% power to detect effects as small as $\eta^2 = .02$.[6] All models control for dyad-level agreement and perceived counterpart suspicion.[7]

Subjective Value

A 2 (dishonesty, control) × 2 (incentive: high, low) ANCOVA compared focal participants in the dishonesty opportunity condition who chose to be dishonest to control condition participants. Supporting the deceiver's guilt account (Hypothesis 1b), participants who were dishonest were less satisfied with the bargaining experience ($M = 4.7$, $SD = 1.1$) than those in the control condition ($M = 5.0$, $SD = 1.2$), $F(1, 420) = 32.60$, $p < .001$, $\eta_p^2 = .07$. We did not find evidence of a main effect of incentive, $F(1, 420) = 0.01$, $p = .92$, $\eta_p^2 < .01$, nor a Dishonesty × Incentive interaction, $F(1, 420) = 0.54$, $p = .46$, $\eta_p^2 < .01$. The effect of dishonesty on subjective value held for both the large incentive, $F(1, 420) = 12.49$, $p < .001$, $\eta_p^2 = .03$, and the small incentive, $F(1, 420) = 20.58$, $p < .001$, $\eta_p^2 = .05$.

Affect

We analyzed affect using a 3 (affect measure: positive affect, negative affect, guilt) × 2 (dishonesty) × 2 (incentive) mixed ANCOVA. The results of these analyses are documented in Figure 1. We focus here on all effects relevant to our accounts about the relative effect of dishonesty on positive affect versus guilt.

First, we found evidence of an Affect Measure × Dishonesty interaction. Supporting the deceiver's guilt hypothesis (Hypothesis 1b), dishonesty induced more guilt than positive affect, $F(1, 420) = 57.68$, $p < .001$, $\eta_p^2 = .12$. The effect of dishonesty on guilt also exceeded its impact on generalized negative affect, $F(1, 420) = 86.57$, $p < .001$, $\eta_p^2 = .17$, suggesting that the impact of dishonesty on guilt is distinct from its impact on other negative emotions. Separate 2 × 2 ANCOVAs confirmed that these patterns held within both incentive conditions (see Figure 1).

Relative to the control condition, dishonest focal participants experienced more guilt, $F(1, 420) = 99.83$, $p < .001$, $\eta_p^2 = .19$. Dishonest participants also experienced more generalized negative affect than those in the control condition, $F(1, 420) = 5.25$, $p = .022$, $\eta_p^2 = .01$. We failed to find evidence of dishonesty eliciting positive affect, $F(1, 420) = 1.96$, $p = .16$, $\eta_p^2 < .01$.

As documented in Figure 1, we also found evidence that the magnitude of the Affect Measure × Dishonesty interaction was moderated by incentive size. A series of 3 (affect measure) × 2 (dishonesty) mixed ANCOVAs revealed that the Affect Measure × Dishonesty interaction was larger in the large incentive condition, $F(2, 422) = 34.46$, $p < .001$, $\eta_p^2 = .14$, than in the small incentive condition, $F(2, 414) = 25.94$, $p < .001$, $\eta_p^2 = .07$. Contrasts revealed that the effect of dishonesty on guilt held in both the large incentive and small incentive conditions but was stronger in the large incentive condition, supporting Hypothesis 2b. In contrast, the degree to

[6] In this and all subsequently reported mixed ANOVA models, we used the "as in SPSS" effect-size specification option (Giner-Sorolla et al., 2019).

[7] Anticipating the possibility that counterparts might detect dishonesty, we also preregistered that we would conduct analyses controlling for counterpart perceptions of participants' dishonesty if it varied by condition at $p < .05$. Although participants perceived control condition participants to be *more* dishonest than dishonest participants, they did perceive participants in the dishonesty opportunity condition who chose to be dishonest as more dishonest than those who chose to be honest (see online Supplemental Materials). All effects reported at $p < .05$ in Study 1 hold at $p < .05$ when controlling for counterpart perceptions of focal participants' dishonesty, suggesting that our findings hold when accounting for counterparts' ability to detect lies.

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Figure 1

Study 1: ANCOVA Results and Box-and-Whisker Plot of Affect by Condition

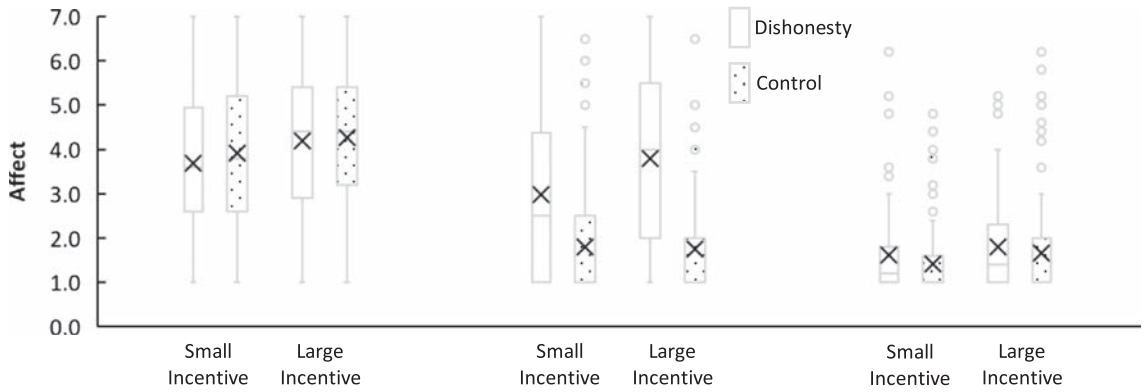
Overall ANCOVA

Affect Measure:	$F(2, 840) = 32.94$, $p < .001$, $\eta_p^2 = .07$
Dishonesty:	$F(1, 420) = 44.43$, $p < .001$, $\eta_p^2 = .10$
Incentive Size:	$F(1, 420) = 15.76$, $p < .001$, $\eta_p^2 = .04$
Affect Measure X Dishonesty:	$F(2, 840) = 48.17$, $p < .001$, $\eta_p^2 = .10$
Affect Measure X Incentive:	$F(2, 840) = 0.69$, $p = .50$, $\eta_p^2 < .01$
Dishonesty X Incentive:	$F(1, 420) = 3.57$, $p = .06$, $\eta_p^2 = .01$
Affect Measure X Dishonesty X Incentive:	$F(2, 840) = 3.14$, $p = .044$, $\eta_p^2 = .01$

Difference in Dishonesty Effect

Positive Affect vs. Guilt
SMALL INCENTIVE
$F(1, 207) = 18.45$, $p < .001$, $\eta_p^2 = .08$
LARGE INCENTIVE
$F(1, 211) = 39.21$, $p < .001$, $\eta_p^2 = .16$

Negative Affect vs. Guilt
SMALL INCENTIVE
$F(1, 207) = 22.46$, $p < .001$, $\eta_p^2 = .10$
LARGE INCENTIVE
$F(1, 211) = 63.14$, $p < .001$, $\eta_p^2 = .23$



Positive Affect

Dishonesty X Incentive
$F(1, 420) = 0.01$, $p = .92$, $\eta_p^2 < .01$
Dishonesty Effect by Incentive
SMALL INCENTIVE
$F(1, 420) = 1.12$, $p = .29$, $\eta_p^2 < .01$
LARGEINCENTIVE
$F(1, 420) = 0.85$, $p = .36$, $\eta_p^2 < .01$

Guilt

Dishonesty X Incentive
$F(1, 420) = 7.28$, $p = .007$, $\eta_p^2 = .02$
Dishonesty Effect by Incentive
SMALL INCENTIVE
$F(1, 420) = 26.45$, $p < .001$, $\eta_p^2 = .06$
LARGE INCENTIVE
$F(1, 420) = 81.11$, $p < .001$, $\eta_p^2 = .16$

Negative Affect

Dishonesty X Incentive
$F(1, 420) = 0.01$, $p = .91$, $\eta_p^2 < .01$
Dishonesty Effect by Incentive
SMALL INCENTIVE
$F(1, 420) = 2.35$, $p = .13$, $\eta_p^2 = .01$
LARGE INCENTIVE
$F(1, 420) = 2.92$, $p = .088$, $\eta_p^2 = .01$

Note. Conditional means are denoted by an "×." ANCOVA = analyses of covariance.

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which dishonesty induced positive affect and generalized negative affect did not vary with the size of the incentive.

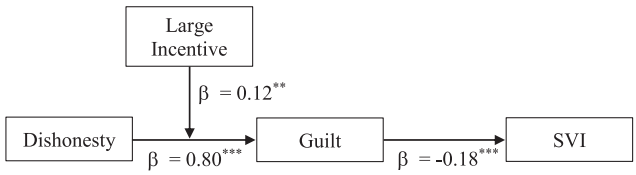
Mediation Analysis

We tested a mediation model to assess the extent to which dishonesty impacted participants' subjective value through guilt, positive affect, and negative affect. Controlling for our preregistered covariates (perceived counterpart suspicion and agreement), participants' dishonesty, and the other affect measures, guilt negatively predicted subjective value, $B = -0.11$ ($SE = 0.02$), $t(419) = 4.36$, $p < .001$, $\beta = -0.18$, positive affect positively predicted subjective value, $B = 0.28$ ($SE = 0.03$), $t(419) = 11.32$, $p < .001$, $\beta = 0.38$, and generalized negative affect negatively impacted participants' subjective value, $B = -0.16$ ($SE = 0.04$), $t(419) = 3.64$, $p < .001$, $\beta = -0.13$. A bootstrap with 5,000 replications revealed negative indirect effects of dishonesty on subjective value through guilt, indirect effect = −0.18, 95% CI [−0.29, −0.09], and negative affect, indirect effect = −0.04, 95% CI [−0.08, −0.005]. However, there was not an independent indirect effect through positive affect, indirect effect = −0.05, 95% CI [−0.14, 0.03].

Because the effect of dishonesty on guilt was moderated by incentive size, we followed a preregistered plan to rerun the above analyses testing for whether the mediation results were robust to the incentive size manipulation. As documented in Figure 2, a bootstrap with 5,000 replications revealed that, while the indirect effect of dishonesty on subjective value through guilt held in the small incentive condition (95% CI [−0.23, −0.05]), it was more strongly negative in the large incentive condition (95% CI [−0.35, −0.11]), difference = −0.10, 95% CI [−0.19, −0.02]. However, the size of the indirect effects of dishonesty on subjective value through positive affect (difference = 0.01, 95% CI [−0.15, 0.17]) and

Figure 2

Study 1: Incentive Size Moderates the Indirect Effect of Dishonesty on Subjective Value Through Guilt



Note. All standardized coefficient estimates control for agreement and perceptions of counterpart suspicion. The effect of guilt on SVI also controls for positive affect and generalized negative affect. *Dishonesty* = 1 if dishonest, 0 if control condition; *large incentive* = 1 if large incentive, 0 if small incentive. SVI = Subjective Value Inventory.
** $p < .01$. *** $p < .001$.

generalized negative affect (difference = −0.003, 95% CI [−0.07, 0.06]) did not vary with incentive size.

Incentive Manipulation Checks

In analyses across all focal participants (i.e., including participants randomly assigned to the dishonesty opportunity condition who chose to be honest), we confirmed that the incentive manipulation produced different perceptions of the incentive's size. A 2 (dishonesty opportunity) × 2 (incentive) analysis of variance revealed a main effect of incentive size, such that participants in the large incentive condition perceived their performance incentive to be stronger than those in the small incentive condition ($M_{High} = 6.2$, $SD = 0.8$ vs. $M_{Low} = 4.2$, $SD = 1.6$), $F(1, 487) = 298.48$, $p < .001$, $\eta_p^2 = .38$. We did not identify evidence of a dishonesty opportunity main effect, $F(1, 487) = 0.10$, $p = .75$, $\eta_p^2 < .01$, or a Dishonesty Opportunity × Incentive interaction, $F(1, 487) = 0.11$, $p = .74$, $\eta_p^2 < .01$.

In addition to validating that the incentive size manipulation was successful, we tested the possibility that the large incentive may have caused focal participants to lie to a larger magnitude in the large incentive condition than in the small incentive condition (i.e., by telling a lie of commission instead of omission). We compared coders' ratings of focal participants' dishonesty among those participants who chose to be dishonest in the dishonesty opportunity condition. This analysis failed to find a difference between the large and small incentive conditions ($M_{Large} = 1.4$, $SD = 0.5$ vs. $M_{Small} = 1.3$, $SD = 0.5$), $t(179) = 0.88$, $p = .38$, $d = 0.13$, 95% CI [−0.16, 0.42]. This pattern indicates that dishonest participants' tendency to feel guiltier in the large incentive condition is unlikely to be accounted for by differences in the magnitude of their dishonesty.

Ancillary Analysis: Did Honesty Boost Negotiators' Subjective Value?

Dishonesty clearly undermined negotiators' subjective value, but might honesty have boosted subjective value? To test this, we conducted analyses of the 65 participants who were honest despite having the opportunity to lie. As documented in the online Supplemental Materials, we found that honesty boosted negotiators' subjective value. Honest participants derived greater subjective value than dishonest participants *and* control condition participants who did not have an opportunity to lie.[8] Honesty boosted negotiators' subjective value relative to control condition participants independently of guilt: although honest participants felt less guilt than dishonest participants, they felt similar levels of guilt to control condition participants. Thus, resisting the temptation to lie not only allowed focal participants to avoid the dishonesty-induced guilt that undermines subjective value, but it also conferred a benefit beyond guilt reduction that enhanced their subjective value.

Discussion

Study 1 finds that dishonesty elicited more guilt than positive affect and reduced negotiators' subjective value. These findings provide direct evidence supporting the deceiver's guilt account (Hypothesis 1b). Further, although dishonesty increased generalized negative affect, it induced even more guilt. Guilt indirectly impacted the effect of dishonesty on subjective value independently of negative affect. These findings suggest that guilt plays a distinct role from positive affect and other negative emotions in driving dishonesty's tendency to decrease participants' subjective value. It is also noteworthy that Study 1 failed to find an effect of dishonesty on positive affect. This null effect emerged despite dishonesty facilitating negotiators' ability to strike deals that enhanced their economic outcomes (see online Supplemental Materials). Although they were economically better off, the experience of guilt left dishonest negotiators less satisfied than those who did not have the opportunity to get away with a lie.

The size of participants' performance incentive did not influence the overall pattern of results. However, larger incentives exacerbated the degree to which dishonesty elicited guilt (Hypothesis 2b). This finding runs counter to the hypothesis that a strong external incentive might provide participants with a salient justification for their dishonesty that enables them to reduce their guilt (Hypothesis 2a). However, it should be interpreted with some caution. Because we did not power the study to detect an attenuated three-way interaction between affect measure, dishonesty, and incentive size, the test showing that the large incentive exacerbated the degree to which dishonesty elicited guilt more than it exacerbated the impact of dishonesty on the other affect measures was likely underpowered. Additional research is needed to determine whether this finding is robust.

Study 2: The Relational Consequences of Dishonesty

In Study 2, we examine the relational consequences of dishonesty across multiple negotiations. If dishonesty-induced guilt has a lasting impact on negotiators' perception of their relationship with counterparts, then their dishonesty in one negotiation might also adversely impact their subjective value in a subsequent negotiation with the same counterpart and reduce their willingness to negotiate again with the counterpart. To test these possibilities, we randomly assigned focal participants to a dishonesty opportunity or control condition as in Study 1, measured their subjective value in a second negotiation with the same counterpart that did not present them with a deception opportunity, and then gave them a choice

[8] We did not preregister this analysis or a similar one in Study 2. We thank an anonymous reviewer for suggesting it.

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about whether to negotiate again with that counterpart in a third and final exercise.

Study 2 also builds our understanding of dishonesty's affective consequences in three ways. First, we test whether dishonesty-induced guilt is exacerbated by individual differences in three key components of moral character (Hypothesis 3): moral identity internalization, empathic concern, and self-control. If these traits moderate the extent to which dishonesty elicits guilt, then the deceiver's guilt account might not hold for participants with relatively low moral character. For them, dishonesty may elicit more positive affect than guilt, consistent with the deceiver's delight account. However, if these traits do not moderate the effect of dishonesty on guilt, then the deceiver's guilt account is likely robust to individual differences in negotiators' moral character.

Second, we explore temporal dynamics in negotiators' affect. People tend to be successful at alleviating their guilt, as studies exploring the temporal dynamics of guilt have found that it decays rapidly (Duke & Amir, 2019; Macht & Dettmer, 2006). This raises the possibility that guilt may decline to baseline levels after a second negotiation with the deceived counterpart. If so, then any relational consequences of dishonesty beyond a focal negotiation are unlikely to be attributable to residual guilt, but instead to the inferior subjective value triggered by deceivers' initial experience of guilt.

Finally, we test whether our findings hold in the presence of even larger stakes than the ones presented in the large incentive condition of Study 1. In Study 2, we incentivized all participants to earn the best deal possible by linking their chances of winning a $500 prize to their economic outcome. Although Study 1 participants perceived that their smaller (but guaranteed) incentive scheme was undoubtedly sizable, we reasoned that a chance at a $500 prize may provide an even stronger external justification for participants' dishonesty.

Method

Participants

We wanted to have sufficient power to achieve two goals in Study 2. First, we aimed to achieve at least 80% power to detect effects of dishonesty on relational consequences equivalent to the average published effect in social psychology ($\eta^2 = .04$; Richard et al., 2003). Second, we wanted to have at least 80% power to detect any interactions between components of moral character (moral identity internalization, empathic concern, and self-control) large enough in magnitude to completely attenuate the effect of dishonesty on guilt observed in the large incentive condition of Study 1 ($\eta_p^2 = .16$). To achieve these goals, we preregistered a data collection plan targeting a minimum of 200 negotiation dyads for our final analyses. We ended up recruiting 483 participants across paid and for-credit participant pools at two different universities (for-credit students were undergraduate business students participating in exchange for course credit and the opportunity to earn an Amazon gift card). Using the same preregistered exclusion criteria as Study 1, we ended up with a final sample of 456 participants paired into 228 negotiation dyads ($M_{Age} = 23.3$ years, $SD = 8.0$, 59% female).

Procedure

Participants were required to complete a brief personality assessment at least 24 hr before they could sign up for a live study session. On the day of their scheduled session, they were provided with a link to a Zoom meeting room, where they were greeted by an experimenter who moderated sessions of two or four participants; to maintain participants' anonymity ahead of the first negotiation, the experimenter assigned each participant an ID number for their screen name and asked participants to turn off their cameras as they entered the meeting room. The experimenter then informed participants that they would engage in a negotiation with another participant in a chatroom and provided them with a link to a survey, where they were paired with another participant in the same session.

The purpose of the initial chat-based negotiation was to enable us to manipulate the opportunity to deceive and then measure the immediate impact of dishonesty on participants' affect and subjective value. Aside from a desire to replicate Study 1's dishonesty opportunity manipulation, we used a chat-based exchange here for two reasons. First, dishonesty is more prevalent in text-based than face-to-face communication (Rockmann & Northcraft, 2008; Van Zant & Kray, 2014). A chat-based negotiation could therefore maximize our sample of dishonest participants, increasing statistical power. Second, people expect to get away with lying in text-based interactions more than in audiovisual interactions (Bond & DePaulo, 2006; Toma et al., 2018), which should reduce negotiators' concerns about counterparts' suspicion.

After completing this preliminary negotiation, participants learned that they would be completing a second video-based negotiation with the same counterpart in a Zoom breakout room with both parties' cameras on. The purpose of this second interaction was to enable focal participants and counterparts to further develop a relationship over the course of a negotiation so that we could examine the downstream relational consequences of dishonesty. Audiovisual cues promote greater trust, liking, and rapport-building in negotiations than text-based communication (Jap et al., 2011; Rockmann & Northcraft, 2008; Valley et al., 1998).

Neither party had an opportunity to deceive in the second negotiation, as it was designed to isolate the effect of dishonesty in the first negotiation on subjective value in the second one. By placing participants in a context where they could build a rapport with their counterpart without succumbing to the temptation to lie, we could test whether dishonesty impacts subjective value derived from subsequent negotiations that are conducted honestly and amicably.

Prestudy Personality Assessment. The personality assessment measured three dimensions of moral character in counterbalanced order: moral identity internalization, empathic concern, and self-control (Cohen et al., 2014). We measured moral identity internalization using the five-item measure from Aquino and Reed (2002). Participants were asked to visualize the "kind of person" who has the characteristics of being caring, compassionate, fair, friendly, generous, helpful, hardworking, honest, and kind. On a scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*), they then indicated to what degree possessing these characteristics was important to their self-concept ($\alpha = .72$; sample item: "being someone who has these characteristics is an important part of who I am").

To measure empathic concern, we used the seven-item empathic concern scale from the interpersonal reactivity index developed by Davis (1983). On a scale ranging from 1 (*does not describe me at all*) to 5 (*describes me very well*), participants indicated their agreement with statements about the degree to which they feel compassion toward others ($\alpha = .82$; sample item: "I often have tender, concerned feelings for people less fortunate than me").

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Finally, we measured self-control using the brief self-control scale of Tangney et al. (2004). On a scale ranging from 1 (*not at all*) to 5 (*very much*), participants indicated the extent to which a series of 13 statements about their self-control and ability to resist temptation described themselves ($\alpha = .85$; sample item: "people would say that I have iron self-discipline").

Negotiation 1: Manipulating Dishonesty. Participants began their live study session by completing the same negotiation exercise from Study 1 in a chatroom. After opening a survey link provided by the experimenter, participants were randomly assigned to buyer or seller roles, and dyads were randomly assigned to dishonesty opportunity or control conditions. The only difference from Study 1 is that, rather than being presented with a payoff chart where they were guaranteed to earn a particular amount for every $250 in sale price ($0.10 or $1.25), all participants stood to earn one raffle ticket that would increase their chances of winning a $500 gift card raffle for every $250 in sale price (see online Supplemental Materials Appendix S-B, for payoff charts).

After completing the negotiation, focal participants proceeded to complete the same dependent measures as in Study 1 ($\alpha_{SVI} = .85$, $\alpha_{Pos\ Affect} = .89$, $\alpha_{Neg\ Affect} = .84$, $\alpha_{Guilt} = .79$). Replicating Study 1, and following our preregistration, we also measured dyad-level agreement and focal participants' perception of their counterpart's suspicion in the first negotiation ($\alpha = .76$) to use as covariates in analyses.

Negotiation 2: Follow-Up Negotiation With the Same Counterpart. After completing the first negotiation and answering questions about it, participants learned that they would engage in a second negotiation with the same counterpart. Focal participants were once again assigned to the seller role and counterparts played the buyer role. The buyer, who had returned to the computer store a few days after negotiating over the video-editing desktop computer, was now looking for a portable laptop to travel with that could excel at handling minor video-editing tasks. While visiting to try out the high-end video-editing desktop computer, the buyer happened to notice that the seller had an unused laptop computer that is well-regarded for handling some of the less intensive aspects of video editing. Sellers' best alternative to agreement was to return the computer to the manufacturer for $1,500, and buyers' maximum willingness to pay was $3,750. There was no opportunity to deceive in this negotiation.

To incentivize participants to perform their best, they received one raffle ticket toward a separate $50 Amazon gift card raffle for every $250 in sale price (buyers: one ticket for every $250 above $1,500; sellers: one ticket for every $250 below $3,750). After reading instructions for the second negotiation exercise, participants were told to reenter the Zoom meeting room they signed into to begin the study. The experimenter then instructed participants to turn on their cameras and paired them in a breakout room with the same counterpart they previously negotiated with in the chatroom. At the conclusion of the second negotiation, the experimenter directed participants back to the survey they used to complete the first negotiation exercise so that they could complete the same measures of SVI and affect as they did after the first negotiation ($\alpha_{SVI} = .89$, $\alpha_{Pos\ Affect} = .89$, $\alpha_{Neg\ Affect} = .92$, $\alpha_{Guilt} = .60$).

Choice of Partner in a Third Negotiation. After answering questions about the second negotiation exercise, focal participants learned that they would "complete one last negotiation exercise," where they would "have the opportunity to earn up to 10 tickets for a separate $50 Amazon gift card raffle." However, unlike the prior negotiation, participants were given a choice of whether to interact with the same counterpart from the previous negotiations or a different counterpart (described as a "randomly selected participant from this study in the buyer role"). Participants' choice of whether to negotiate with the same counterpart (0 if no, 1 if yes) represented a dependent measure of interest. After choosing whom they would like to interact with, focal participants then learned that they would play the proposer role in a follow-up ultimatum bargaining task by proposing how to split 10 tickets. They then proposed a split and, after the study, learned that the acceptance of their offer would be determined by the minimum amount their assigned counterpart indicated a willingness to accept.

Results

Consistent with Study 1 and our preregistration, analyses compare focal participants in the dishonesty opportunity condition who chose to lie to control condition participants. Three independent coders rated participants' dishonesty in the dishonesty opportunity condition ($\alpha = .95$; 0 = no lie, 1 = lie of omission, 2 = lie of commission). Among 117 focal participants in the dishonesty opportunity condition, 90 lied (77%) and were included in our main analyses.

Following our preregistered data analysis plan, we used ANCOVA to test the effect of the dishonesty incentive manipulation on subjective value and guilt; whereas the models testing effects on subjective value had 80% power to detect effects as small as $\eta^2 = .04$, the model testing the effect on affect could detect interactions with affect measure as small as $\eta^2 = .02$ with 80% power. We tested for effects on counterpart choice using a logistic regression model with 80% power to detect effects as small as $OR = 0.44$.[9] All models control for dyad-level agreement and perceived counterpart suspicion.

Negotiation 1 Subjective Value

Replicating Study 1 and supporting the deceiver's guilt account (Hypothesis 1b), an ANCOVA revealed that dishonesty undermined focal participants' subjective value in the first negotiation, $F(1, 197) = 13.38$, $p < .001$, $\eta_p^2 = .06$. Relative to participants in the control condition ($M = 4.3$, $SD = 1.0$), those who chose to be dishonest were less satisfied with the negotiation experience ($M = 4.1$, $SD = 0.9$).

Negotiation 2 Subjective Value

A separate ANCOVA revealed that dishonesty in the first negotiation also undermined focal participants' subjective value in the second negotiation, $F(1, 197) = 10.33$, $p = .002$, $\eta_p^2 = .05$. Relative to participants in the control condition ($M = 5.5$, $SD = 0.9$), those who chose to be dishonest in the first negotiation were less satisfied with their experience in the second negotiation ($M = 5.1$, $SD = 0.9$).[10]

[9] This power analyses assumes a binomial distribution of dishonesty. It also accounts for dishonest participants' likelihood of choosing to negotiate again with the same counterpart and the squared multiple correlation coefficient of counterpart choice on model covariates ($R^2 = .01$).

[10] Participants' Negotiation 2 subjective value was moderately correlated with Negotiation 1 subjective value, $r(201) = .31$, $p < .001$. This moderate correlation suggests that, despite using the same scale about the same counterpart, Negotiation 2 subjective value is relatively distinct from Negotiation 1 subjective value, as only approximately 10% of the variance in participants' subjective value in Negotiation 2 was attributable to their subjective value in the previous negotiation.

Choice of Partner in a Third Negotiation

A logistic regression revealed a trend where dishonesty in the first negotiation reduced focal participants' likelihood of choosing to negotiate with the same counterpart in the final negotiation exercise, $B = -0.63$ ($SE = 0.35$), $\chi^2(1, N = 201) = 3.32$, $p = .069$, $OR = 0.53$, 95% CI [0.27, 1.05]. Relative to participants in the control condition (77%), those who chose to be dishonest in the first negotiation (68%) were directionally less likely to choose to negotiate again with the same counterpart in the final negotiation exercise.

Affect

To assess affect across the two negotiations, we ran a 3 (affect measure: positive affect, negative affect, guilt) × 2 (negotiation: first, second) × 2 (dishonesty) mixed ANCOVA. The results of these analyses are documented in Figure 3. We focus here on all effects relevant to our accounts about the relative effect of dishonesty on positive affect versus guilt.

First, we found evidence of an Affect Measure × Dishonesty interaction. Replicating the prior studies, and supporting the deceiver's guilt account (Hypothesis 1b), dishonesty induced more guilt than positive affect, $F(1, 197) = 10.17$, $p = .002$, $\eta_p^2 = .05$. Relative to the degree to which dishonesty impacted participants' mean level of positive affect across the two negotiations, it elicited greater mean levels of guilt. The effect of dishonesty on guilt also exceeded its impact on generalized negative affect, $F(1, 197) = 14.33$, $p < .001$, $\eta_p^2 = .07$, suggesting that the impact of dishonesty on guilt across the two negotiations is distinct from its impact on other negative emotions.

Relative to participants in the control condition, those who were dishonest in the first negotiation reported higher mean levels of guilt across the two negotiations, $F(1, 197) = 18.22$, $p < .001$, $\eta_p^2 = .08$. However, dishonesty did not impact negotiators' mean reported positive affect across the two negotiations, $F(1, 197) = 0.21$, $p = .65$,

Figure 3

Study 2: ANCOVA Results and Box-and-Whisker Plot of Affect by Condition and Negotiation

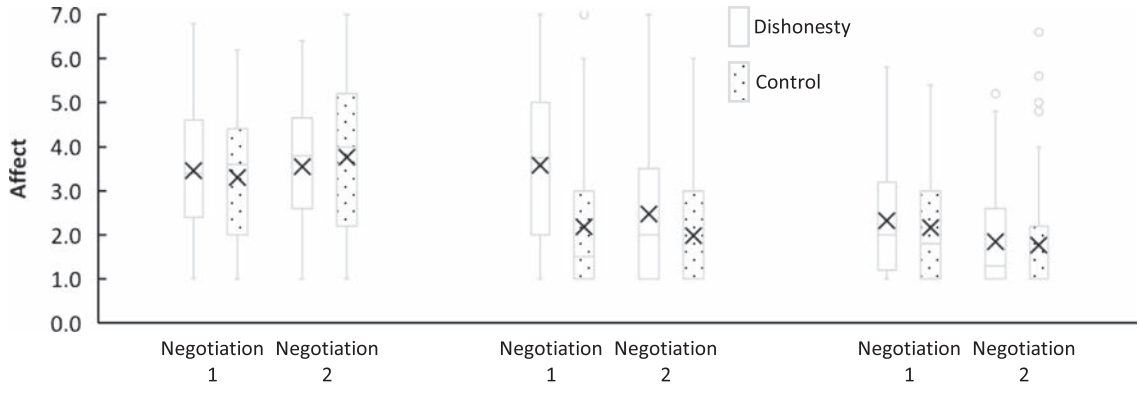
Overall ANCOVA

Affect Measure:	$F(2, 394) = 11.79$, $p < .001$, $\eta_p^2 = .06$
Dishonesty:	$F(1, 197) = 3.33$, $p = .07$, $\eta_p^2 = .02$
Negotiation:	$F(1, 197) = 2.33$, $p = .13$, $\eta_p^2 = .01$
Affect Measure X Dishonesty:	$F(2, 394) = 8.27$, $p < .001$, $\eta_p^2 = .04$
Affect Measure X Negotiation:	$F(2, 394) = 2.99$, $p = .051$, $\eta_p^2 = .01$
Dishonesty X Negotiation:	$F(1, 197) = 14.75$, $p < .001$, $\eta_p^2 = .07$
Affect Measure X Dishonesty X Negotiation:	$F(2, 394) = 4.55$, $p = .011$, $\eta_p^2 = .02$

Difference in Dishonesty Effect

Positive Affect vs. Guilt
NEGOTIATION 1
$F(1, 197) = 11.96$, $p < .001$, $\eta_p^2 = .06$
NEGOTIATION 2
$F(1, 197) = 3.42$, $p = .066$, $\eta_p^2 = .02$

Negative Affect vs. Guilt
NEGOTIATION 1
$F(1, 197) = 20.16$, $p < .001$, $\eta_p^2 = .09$
NEGOTIATION 2
$F(1, 197) = 1.83$, $p = .18$, $\eta_p^2 = .01$



Positive Affect

Dishonesty X Negotiation
$F(1, 197) = 3.96$, $p = .048$, $\eta_p^2 = .02$
Dishonesty Effect by Negotiation
NEGOTIATION 1
$F(1, 197) = 0.17$, $p = .69$, $\eta_p^2 < .01$
NEGOTIATION 2
$F(1, 197) = 1.37$, $p = .24$, $\eta_p^2 = .01$

Guilt

Dishonesty X Negotiation
$F(1, 197) = 12.62$, $p < .001$, $\eta_p^2 = .06$
Dishonesty Effect by Negotiation
NEGOTIATION 1
$F(1, 197) = 26.12$, $p < .001$, $\eta_p^2 = .12$
NEGOTIATION 2
$F(1, 197) = 2.06$, $p = .15$, $\eta_p^2 = .01$

Negative Affect

Dishonesty X Negotiation
$F(1, 197) = 0.18$, $p = .67$, $\eta_p^2 < .01$
Dishonesty Effect by Negotiation
NEGOTIATION 1
$F(1, 197) = 0.12$, $p = .73$, $\eta_p^2 < .01$
NEGOTIATION 2
$F(1, 197) < 0.01$, $p = .97$, $\eta_p^2 < .01$

Note. Conditional means are denoted by an "×." ANCOVA = analyses of covariance.

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$\eta_p^2 < .01$, or their mean generalized negative affect, $F(1, 197) = 0.03$, $p = .86$, $\eta_p^2 < .01$.

However, these effects were qualified by an Affect Measure × Negotiation × Dishonesty interaction. A series of 3 (affect measure) × 2 (dishonesty) mixed ANCOVAs revealed that the Affect Measure × Dishonesty interaction held in Negotiation 1, $F(2, 394) = 11.57$, $p < .001$, $\eta_p^2 = .06$, but to a lesser extent in Negotiation 2, $F(2, 394) = 2.33$, $p = .098$, $\eta_p^2 = .01$. Replicating Study 1, participants who were dishonest in the first negotiation experienced more guilt in Negotiation 1 than control condition participants; this effect dissipated in Negotiation 2. Further, dishonesty in the first negotiation did not impact participants' positive affect or generalized negative affect after either the first or second negotiation. Follow-up contrasts replicated Study 1 in finding that dishonesty in the first negotiation elicited more guilt in Negotiation 1 than positive affect and generalized negative affect. Conversely, in Negotiation 2, the effect of dishonesty on guilt was similar in magnitude to its effect on generalized negative affect and only marginally greater than its effect on positive affect.

Did Moral Character Moderate the Affective Impact of Dishonesty?

To explore whether the impact of dishonesty on affect might be moderated by focal participants' moral character, we preregistered an analysis exploring the impact of participants' moral identity internalization, empathic concern, and self-control on the relationship between dishonesty and guilt, as well as on the relationship between dishonesty and positive affect (see online Supplemental Materials Table S2, for descriptive statistics and correlations between variables).[11]

As documented in the online Supplemental Materials (Table S3), we did not find evidence of any moral character measure moderating the effect of dishonesty on guilt or positive affect (all ps > .15), nor did we identify evidence of any three-way interactions with dishonesty and negotiation round (all ps > .21). Further, simple slopes analyses confirmed that the effect of dishonesty on Negotiation 1 guilt held across participants both high (+1 *SD* above mean) and low (−1 *SD* below the mean) in moral identity internalization ($B_{High} = 0.74$, $SE = 0.15$, $z = 4.87$, $p < .001$ vs. $B_{Low} = 0.48$, $SE = 0.15$, $z = 3.22$, $p = .001$), empathic concern ($B_{High} = 0.54$, $SE = 0.15$, $z = 3.62$, $p < .001$ vs. $B_{Low} = 0.66$, $SE = 0.15$, $z = 4.31$, $p < .001$), and self-control ($B_{High} = 0.68$, $SE = 0.15$, $z = 4.42$, $p < .001$ vs. $B_{Low} = 0.56$, $SE = 0.15$, $z = 3.72$, $p < .001$). These results do not support Hypothesis 3a.

Mediation Analyses

Did dishonesty-induced guilt in the first negotiation impact participants' subjective value in Negotiation 2 and choice of negotiation counterpart in the final interaction? To assess these questions, we used Model 6 of the PROCESS Macro (Hayes, 2017) to test preregistered serial mediation models assessing the indirect effect of dishonesty on relational consequences (Negotiation 2 subjective value and counterpart choice) through guilt and Negotiation 1 subjective value, in serial order (see Figure 4).

Negotiation 2 Subjective Value. First, consistent with Study 1, focal participants' Negotiation 1 guilt negatively predicted Negotiation 1 subjective value after controlling for their dishonesty, $B = -0.13$ ($SE = 0.03$), $t(196) = 3.77$, $p < .001$.[12] Second, Negotiation 1 subjective value positively predicted Negotiation 2 subjective value, controlling for dishonesty and guilt, $B = 0.27$ ($SE = 0.08$), $t(195) = 3.40$, $p < .001$. Finally, consistent with serial mediation, a bootstrap with 5,000 replications revealed a negative indirect effect of dishonesty in the first negotiation on Negotiation 2 subjective value through Negotiation 1 guilt and Negotiation 1 subjective value, in serial, indirect effect = −0.04, 95% CI [−0.09, −0.01]. This analysis suggests that participants' dishonesty-induced guilt stemming from their behavior in a previous negotiation tainted their experience in a subsequent negotiation with the same counterpart through its detrimental impact on their subjective value in the negotiation where they lied.

Counterpart Choice in the Final Negotiation. A logistic regression model revealed that participants' Negotiation 1 subjective value positively predicted their likelihood of choosing to negotiate again with the same counterpart in a final exercise after controlling for their dishonesty and guilt in the first interaction, $B = 1.08$ ($SE = 0.25$), $Z = 4.34$, $p < .001$. A bootstrap with 5,000 replications found evidence of an indirect effect of dishonesty on counterpart choice through Negotiation 1 guilt and Negotiation 1 subjective value, in serial, indirect effect = −0.17, 95% CI [−0.38, −0.06]. This finding suggests that participants' tainted experience in the first negotiation, which was triggered by dishonesty-induced guilt, also reduced their likelihood of choosing to maintain a relationship with the same counterpart after the second negotiation.

Ancillary Analysis: Did Honesty Boost Negotiators' Subjective Value?

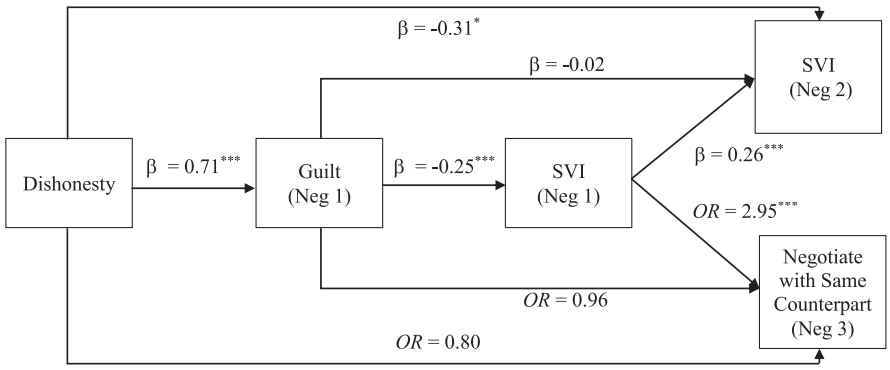
We also analyzed the 27 focal participants in the dishonesty opportunity condition who chose to be honest (see online Supplemental Materials). We again found that honest participants' subjective value exceeded that of dishonest participants in Negotiation 1, and they experienced less guilt than dishonest participants. Honest participants were also more likely to choose to negotiate again with the same counterpart than dishonest participants. However, unlike Study 1, the differences relative to the control condition were not significant. Thus, passing on the temptation to lie did not benefit Study 2 participants relative to not having the option to lie.

[11] Because participants in the dishonesty opportunity condition self-selected whether to be dishonest (and thus whether they were included in our main analyses), it is possible for moral character to systemically differ between dishonest focal participants and those in the control condition. We did not find evidence of dishonest participants' moral character differing from that of control condition participants on any of the traits we measured (all ps > .43). This suggests that our moderation analyses are unlikely to be limited by a restricted range of values where the observed moral character of dishonest and control condition participants overlapped. We did not find evidence of the moral character traits we measured being correlated with participants' choice of whether to behave dishonestly in the dishonesty opportunity condition (see online Supplemental Materials).

[12] To replicate Study 1, we also conducted a mediation analysis testing the indirect effect of dishonesty on Negotiation 1 subjective value through Negotiation 1 guilt, negative affect, and positive affect. Replicating Study 1, we found an indirect effect of dishonesty on Negotiation 1 subjective value through Negotiation 1 guilt that was independent of positive and negative affect, indirect effect = −0.11, 95% CI [−0.23, −0.04].

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Figure 4
Study 2: Serial Mediation Models



Note. All standardized coefficient estimates and odds ratios control for agreement, perceptions of counterpart suspicion, and the preceding variables in the mediation model. *Neg 1* = Negotiation 1, *Neg 2* = Negotiation 2. *Neg* 3 = choice for Negotiation 3. SVI = Subjective Value Inventory.
* $p < .05$. *** $p < .001$.

Discussion

Study 2 makes three contributions. First, it supports the deceiver's guilt account in a setting with higher financial stakes. When all focal negotiators had the opportunity to earn a sizable reward for performing well ($500 gift card), dishonesty-induced guilt once again undermined their subjective value. Second, Study 2 documents relational consequences of dishonesty extending beyond a focal negotiation. Because dishonest negotiators' guilt negatively colors their perception of a focal negotiation, they leave subsequent negotiations with the same counterpart relatively dissatisfied and may be more likely to opt out of a relationship with the counterpart. Notably, this pattern emerged despite dishonesty not having an impact on negotiators' economic performance in the subsequent negotiation (see online Supplemental Materials). Our results substantiate prior findings that subjective value impacts relationships over time (Curhan et al., 2009). Further, by capturing behavioral choice, we move beyond prior studies that have measured the hypothetical willingness to negotiate again with the same counterpart (e.g., Curhan et al., 2006, 2010; Oliver et al., 1994) and link this choice to undetected deception. Third, we studied how guilt persists over time. Dishonest participants felt guilty immediately after the first negotiation, but their guilt dissipated by the end of the second negotiation with the same counterpart, consistent with prior work documenting a decay of guilt over time (Duke & Amir, 2019; Macht & Dettmer, 2006). While liars' guilt may have been fleeting, its consequence for subjective value lingered into the second negotiation.[13]

Finally, we found the deceiver's guilt account to be surprisingly robust to individual differences in moral character. Whether participants scored high or low on moral identity internalization, empathic concern, or self-control, we continued to find evidence that dishonesty induced guilt in the first negotiation.

One limitation of this study is that the observed effect of dishonesty on guilt in Negotiation 1 ($\eta_p^2 = .12$) is smaller than the effect size we assumed when powering this study ($\eta_p^2 = .16$). This raises the possibility that this study was not sufficiently powered to detect interaction effects large enough to completely attenuate the impact of dishonesty on guilt. Future research examining the replicability of the finding that dishonesty-induced guilt erodes over the course of repeated negotiations would be well-advised, as would better-powered tests of the extent to which moral character moderates the impact of dishonesty on guilt.

Studies 3A and 3B: Deceiver's Guilt When Acting as an Agent

In the prior studies, negotiators behaved dishonestly of their own volition and for their own benefit. However, people often engage in negotiations where they represent others and act according to others' wishes. For instance, lawyers negotiate settlements that meet a minimum benchmark set by their client. Employees negotiate deals within guidelines set by their employer. Real estate agents follow strategies approved by clients when negotiating transactions. In those settings, people act as agents who might be acting on others' orders. Lying on others' orders can facilitate moral disengagement by enabling people to attribute their lies to coercion (Bandura, 1999) or industry practices (Kennedy et al., 2017), which may reduce guilt (Bandura, 2002). Further, behaving honestly while representing others can sometimes be guilt-inducing if it means deviating from the principal's wishes (Kouchaki & Kray, 2018). Therefore, the support we find for the deceiver's guilt account (Hypothesis 1b) may not hold in contexts where dishonest negotiators act on others' orders. Studies 3A and 3B test whether the previously demonstrated effect of dishonesty on subjective value holds in a context where

[13] One may wonder whether the relational consequences we document were driven by liars' concerns about being discovered as dishonest during subsequent interactions with counterparts. We believe this is unlikely. Nervousness is an emotion that emerges in liars from concerns about having their lies detected (DePaulo et al., 2003), and an analysis of the "nervous" item in the generalized negative affect scale suggests that dishonest participants did not feel more nervous than control condition participants after the second negotiation ($M_{\text{Dishonest}} = 2.1$, $SD = 1.4$ vs. $M_{\text{Control}} = 2.2$, $SD = 1.7$), $t(199) = 0.76$, $p = .45$, $d = 0.10$, 95% CI [−0.17, 0.39].

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negotiators are agents following instructions that require them to engage in some level of dishonesty.

Study 3A: Self-Selected Dishonesty in the MBA Classroom

Study 3A tests the deceiver's guilt hypothesis (Hypothesis 1b) in a face-to-face negotiation between acquaintances. By testing for naturalistic patterns in deception among acquaintances, we aimed to test the hypothesis in a context high in ecological validity.

Method

Participants

We compiled archival data from multiple negotiation courses, resulting in a data set containing responses from 212 Master of Business Administration students taught by one of three different instructors. Students were paired into dyads, so the data represent 106 negotiation dyads. Participants completed the exercise after approximately 1 month of taking the course and getting acquainted with classmates through face-to-face negotiation role-playing exercises.

Procedure

Participants were paired in dyads, randomly assigned to buyer or seller roles, and allotted up to 45 min to complete the Bullard Houses role-playing exercise (Karp et al., 2008); this exercise has been used in prior research to examine how advocating on behalf of others impacts negotiators' dishonesty (Kouchaki & Kray, 2018). The exercise involves the sale of a piece of real estate. Focal participants played the role of an agent representing a buyer interested in converting the property into a high-rise commercial hotel. Importantly, this intended use of the property is inconsistent with the wishes of the sellers, who would prefer to keep it as a residential property. Focal negotiators were aware of this entering the negotiation, as they received instructions from their client prohibiting them from revealing their client's intended use of the property. However, they were not provided with any guidance about what to say about the client's intentions instead. Focal participants' counterparts played the role of an agent representing a seller who gave them instructions to only sell to a buyer that will use the property for "tasteful" purposes, and ideally to one who would maintain the property's status as a residential complex.

This exercise provides focal participants with an opportunity to externally attribute their dishonesty to following their client's orders, which could potentially alleviate feelings of guilt (Kouchaki & Kray, 2018; Tenbrunsel et al., 2010). However, it also allows them the leeway to deceive to a degree they find tolerable. On the less deceptive end, focal participants could conceal their client's intentions by admitting that they have been instructed not to reveal the property's intended use. On the most deceptive end, they could commit a blatant lie by conveying that their client's intentions are entirely consistent with the seller's wishes.

Dishonesty. We adapted a scoring scheme from Kray et al. (2014) to measure focal participants' dishonesty based on counterparts' responses to an open-ended question asking them what they were told about the property's intended use. Two independent coders scored counterparts' understanding of the property's intended use on the following dishonesty scale: 1 (focal participant was honest by either revealing the client's intentions or admitting they could not reveal this information),[14] 2 (focal participant provided vague and subjective information about their client's intentions, e.g., "uncertain"), 3 (focal participant provided misleading information that emphasized "residential" use), and 4 (focal participant told a blatant lie, e.g., "luxury condos"). Coders' ratings were reliable ($\alpha = .88$), so we averaged them into a single index of dishonesty.[15]

Dependent Measures. After the negotiation exercise, but before the classroom exercise was debriefed, negotiators indicated whether they reached an agreement and, if so, the property's sale price. They then completed a condensed SVI using four items adapted from the 13-item measure used in the prior studies and placed on a scale ranging from 1 (*not at all*) to 7 (*extremely*): "How satisfied are you with the ease of reaching agreement?," "Did the negotiation build a good foundation for a future relationship?," "How satisfied are you with your own outcome?," and "Would you characterize the process as fair?"[16] The items were reliable ($\alpha = .85$) and averaged into a single index. To account for one instructor's accidental use of a 5-point scale for the items, we standardized participants' subjective value within instructors.

Control Variables. To help rule out alternative explanations for any effects, we controlled for two variables in our analyses. As with prior studies, we controlled for dyad-level agreement in our main analyses. Second, because the data were collected from three different instructors, we controlled for instructor-specific idiosyncrasies with instructor fixed effects.

Results

As with Studies 1 and 2, we focus our main analyses on the subjective value of 106 focal participants in our data set (i.e., the buyer role). We tested the effect of focal participants' dishonesty on their subjective value and guilt using a linear regression model with 80% power to detect effects as small as $r = .26$ (see online Supplemental Materials Table S4, for descriptive statistics and correlations).

As documented in Table 1 and supporting the deceiver's guilt account (Hypothesis 1b), participants' dishonesty negatively predicted their subjective value, $r(104) = -0.21$, $p = .033$. This effect held in a model controlling for instructor and agreement (Table 1),

[14] Kray et al. (2014) also differentiated between honesty that was most beneficial to counterparts (revealing the client's true intentions, coded 0) and honesty that still conceals the client's intentions (coded 1). In separate analyses measuring dishonesty with this coding scheme ($\alpha = .90$), we find that dishonesty reduced negotiators' subjective value, $r(104) = -0.22$, $p = .022$; this effect held in a model with controls, $t(101) = 2.97$, $p = .004$.

[15] Students in two of the instructors' classes self-reported their own honesty during the negotiation (1 = *not at all* to 7 = *extremely*). This measure (reverse-scored to reflect dishonesty) correlated with the coded dishonesty measure we used in our main analyses, $r(51) = .37$, $p = .007$, suggesting that focal participants' perception of dishonesty corresponded with our coded measure. Two of the sections contained in our data set also included a question for focal participants about the extent to which they "let down" their partner ($n = 41$), but because of its unclear overlap with the focal construct of guilt, we do not report analyses of that item.

[16] Consistent with the prior studies, we also conducted analyses that omitted this item. Our results hold when using the resulting three-item SVI measure (see online Supplemental Materials Table S1).

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Table 1
Study 3A: Regression Predicting Focal Participants' Subjective Value

Variable	*B*	*SE*	β
Dishonesty	−0.28	0.10	−0.27**
Agreement	0.73	0.21	0.32***
Instructor fixed effect		Included	

Note. *SE* = standard error.
** $p < .01$. *** $p < .001$.

$t(101) = 2.83$, $p = .006$. The more dishonest focal participants were, the less satisfied they were with the negotiation experience.

Discussion

Study 3A replicates the prior studies' findings in a face-to-face context where negotiators could attribute their dishonesty to acting on others' orders. The greater the degree of negotiators' dishonesty, the lower their subjective value in a negotiation with a classmate. As with Studies 1 and 2, dishonesty adversely impacted negotiators' subjective value despite going undetected by counterparts and enabling negotiators to achieve a superior economic outcome.

A key strength of this study is that it enabled us to test the effect of dishonesty in a naturalistic, face-to-face setting higher in ecological validity than online experiments. However, it has two noteworthy limitations. First, unlike the other studies in this article, we did not preregister this one. Second, a limitation of classroom settings like the one used for this study is that both parties know an instructor will debrief the entire class about the purpose of the negotiation exercise. Therefore, while counterparts did not appear to detect dishonesty during the exercise, focal negotiators likely knew dishonesty would eventually be revealed during the debrief. This raises the possibility that deceivers' reduced subjective value could be accounted for by concerns that their counterpart might respond unfavorably to discovering they were lied to after the negotiation exercise. We attempted to address this concern in a subsample of our data containing a measure of the extent to which focal participants were concerned that their counterpart might retaliate ($n = 41$).[17] The effect of dishonesty on subjective value held when adding this control to the model reported in Table 1, $B = -0.50$ ($SE = 0.16$), $t(36) = 3.19$, $p = .003$. This provides some evidence that focal participants' concerns about facing backlash from their counterparts are unlikely to account for our findings. We further address this issue in Study 3B by using the same exercise in a negotiation between MTurk workers who are anonymous to one another and controlling for perceived counterpart suspicion like in Studies 1 and 2.

Study 3B: Following Orders to be Honest or Dishonest

Study 3B was designed to add internal validity to Study 3A and extend it in several respects. First, it attempts to causally validate Study 3A by randomly assigning participants to follow instructions requiring them to be deceptive or honest. Second, it measures guilt and tests its role in shaping the impact of dishonesty on subjective value. Third, it uses a larger sample to attempt to replicate the trend from Study 2 where dishonesty reduced negotiators' willingness to interact again with the same counterpart.

Method

Participants

Anticipating a modest effect of dishonesty on negotiators' willingness to bargain again with the same counterpart (as evidenced by the Study 2 results), we aimed for at least 500 complete negotiation dyads for analyses, or 2.5 times the sample obtained in Study 2. To achieve this sample, we posted a total of 1,288 assignments to MTurk. After applying the same preregistered exclusion criteria as Studies 1 and 2, we arrived at a final sample of 1,056 participants paired into 528 complete negotiation dyads who participated in exchange for a $2.00 base payment ($M_{Age} = 36.4$ years, $SD = 11.1$, 45% female).

Procedure

Participants expected to engage in a negotiation with another MTurk worker and were randomly assigned to buyer or seller roles adapted from the Bullard negotiation exercise used in Study 3A. To ensure participants' understanding of their role instructions, they were required to pass a series of comprehension checks before being allowed to continue with the study.

Focal participants, who played the buyer role, were randomly assigned to honesty or dishonesty instruction conditions. Participants assigned to the honesty instruction condition read the following:

> Because failing to reveal your client's intentions of converting the property into a high-rise commercial hotel would violate disclosure regulations and put your client at risk of being exposed to costly fines and lawsuits, your client has instructed you to disclose the intended use of the site regardless of the circumstances.

To incentivize focal participants to follow their client's orders to reveal the property's intended use, we told them that we would ask their counterpart about their client's intentions at the end of the study; if a participant's counterpart could successfully answer the question, the participant would earn a bonus payment of $0.50.

In contrast, focal participants assigned to the dishonesty instruction read the following:

> Because revealing your client's intentions of converting the property into a high-rise commercial hotel would likely increase its purchase price and jeopardize the plan to convert it into a hotel, your client has instructed you *not* to reveal the intended use of the site under any circumstances.

As with the honesty instruction condition, participants in the dishonesty instruction condition were informed that their counterpart would be asked about their client's intentions at the end of the study. However, they learned that they would earn a bonus of $0.50 if their counterpart *failed* to answer the question successfully.

In addition to manipulating focal participants' instructions to be honest or dishonest, we made three key modifications to the full-length version of the Bullard exercise we used in Study 3A. First, we informed focal participants that their client's plans to convert the

[17] One instructor did not include this item in a postnegotiation survey, and in response to a reviewer's concern about the item, we asked another instructor to remove it from data collected in another class section.

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Bullard property into a high-rise hotel could be carried out without needing to destroy the original property. While counterparts were informed that their client would prefer to sell the property to a buyer who intends on keeping it as a residential complex, their clients empowered them to strike any favorable deal, so long as it did not "result in the property being destroyed."

Second, we incentivized participants to reach an agreement and perform their best. All participants stood to earn an additional bonus payment of $0.25 for reaching an agreement. Additionally, they could earn an extra $0.05 for every $2 million they saved their client on the purchase price below $24 million (focal participant buyers only) or $0.05 for every $2 million they earned their client over $10 million (counterpart sellers only), up to a maximum of $0.30. Unlike focal participants, who were incentivized to be honest or dishonest, counterparts were instead incentivized to avoid an agreement that violated their client's interest in avoiding commercial development on the property. To maximize the chances that counterparts would directly ask focal participants about the buyer's intentions, we informed them that they would receive an additional $0.50 bonus payment if they sold the property to a buyer "whose proposal does not involve destroying the property." Thus, all participants stood to earn a maximum performance-based bonus of $1.05, on top of a guaranteed $2.00 base payment.

Third, to keep the study duration manageable for participants and minimize study attrition, we shortened the amount of time participants had to negotiate. After reading their role instructions, participants were paired in a chatroom using the ChatPlat interface used by other scholars for online negotiation exercises (e.g., Brooks & Schweitzer, 2011; Rogers et al., 2017). We allotted participants a maximum of 10 min to negotiate and gave them a 2-min warning before their chatroom expired; this duration is consistent with Rogers et al. (2017), who ran a similar negotiation exercise on MTurk workers and reported that as little as 8 min were "sufficient for most negotiators to reach an agreement" (p. 468).

Post Negotiation Measures. Following the negotiation exercise, focal participants were asked a series of questions about their negotiation experience in a counterbalanced order.

Manipulation Checks. Unlike Studies 1 and 2, which compared dishonest participants to those who had no opportunity to lie, the present study compared the effect of instructions to be honest or dishonest. Thus, rather than attempting to identify which participants were honest or dishonest, we instead relied on two manipulation checks to verify that, on average, participants in the dishonesty instruction condition were more dishonest than those in the honesty instruction condition.

As a first manipulation check, we asked focal participants how "honest" and "truthful" they were during the negotiation (1 = *not at all* to 7 = *extremely*). Both items were reliable ($\alpha = .98$), so we reverse-scored and averaged them into a measure of self-reported dishonesty.

To enable us to quickly determine focal participants' bonus payment without a lengthy coding process, we also presented counterparts with a multiple choice question asking them to identify which of five possible uses of the property "most accurately describes" the buyer's intentions; one option contained the buyer's true intentions while the other four options served as foils. This served as a manipulation check of focal participants' dishonesty (0 = did not identify the buyer's intended use, 1 = identified the buyer's intended use).[18]

Dependent Measures: Subjective Value and Guilt. Focal participants completed the same 13-item SVI ($\alpha = .89$) and two-item measure of guilt[19] ($\alpha = .88$; scale endpoints: 1 = *strongly agree* to 7 = *strongly agree*) as those in Studies 1 and 2.

Control Variables. As in the previous studies, we controlled for dyad-level agreement. We also controlled for perceived counterpart suspicion using the same scale as Studies 1 and 2 but modified the five items to refer to focal participants' perception that counterparts were suspicious of what they had to say about the buyer's intentions with the property ($\alpha = .78$).

Choice of Partner in a Follow-Up Negotiation. After completing the postnegotiation measures, focal participants learned that they would "negotiate again in a much briefer interaction." They then made the same choice as Study 2 participants about whether to negotiate again with the same counterpart or a different one. After making a choice, they completed the same follow-up ultimatum bargaining task, but proposed how to split a $0.10 bonus payment.

Results

We focus our main analyses on focal participants' subjective value, guilt, and counterpart choice in the subsequent negotiation. Following our preregistered data analysis plan, we used ANCOVA to test the effect of the dishonesty instruction manipulation on subjective value and guilt; these analyses had 80% power to detect effects as small as $\eta^2 = .01$. We analyzed the impact of the dishonesty instruction on participants' counterpart choice using a logistic regression model with 80% power to detect an *OR* as small as 0.61.[20] All models control for dyad-level agreement and perceived counterpart suspicion.

Subjective Value

Replicating previous studies' findings, an ANCOVA testing the effect of the dishonesty instruction manipulation on focal participants' subjective value identified an effect of the dishonesty instruction, $F(1, 524) = 22.10$, $p < .001$, $\eta_p^2 = .04$. Relative to participants who were instructed by their client to be honest ($M = 5.4$, $SD = 1.1$), those who were instructed to behave dishonestly ($M = 5.2$, $SD = 1.1$) derived lower subjective value from the negotiation.

Guilt

An ANCOVA also revealed a main effect of the dishonesty instruction on guilt, $F(1, 524) = 44.74$, $p < .001$, $\eta_p^2 = .08$. Relative to participants instructed to be honest ($M = 1.8$, $SD = 1.1$), those

[18] We did not preregister any analyses of this item. However, we made an ex-post decision that it would be a useful measure for validating that focal participants' self-reports impacted how frequently their counterparts were misinformed about the buyer's intentions.

[19] We also included "I feel that I have let my partner down" as an item but made a post hoc decision to drop it in response to concerns that it might capture something different from the affective experience of guilt in a negotiation context. All effects reported at $p < .05$ remain at $p < .05$ when including this item. We thank an anonymous reviewer and the associate editor for raising this concern.

[20] This power analyses assumes a binomial distribution of dishonesty. It also accounts for participants' likelihood of choosing to negotiate again with the same counterpart in the dishonesty instruction condition and the squared multiple correlation coefficient of counterpart choice on model covariates ($R^2 = .01$).

instructed to behave dishonestly ($M = 2.6$, $SD = 1.6$) felt more guilty.

Chosen Partner in the Subsequent Negotiation

A logistic regression revealed that the dishonesty instruction manipulation impacted focal participants' likelihood of choosing to negotiate with the same counterpart in the subsequent negotiation exercise, $B = -0.48$ ($SE = 0.20$), $\chi^2(1, N = 528) = 5.92$, $p = .015$, $OR = 0.62$, 95% CI [0.42, 0.91]. Relative to participants instructed to be honest (72%), those instructed to behave dishonestly (64%) were less likely to choose to negotiate again with the same counterpart in the follow-up negotiation.

Serial Mediation Model

We next tested the indirect effect of the dishonesty instruction on counterpart choice through guilt and SVI, in serial order. First, a linear regression revealed that focal participants' guilt negatively predicted their subjective value after controlling for the dishonesty instruction manipulation, $B = -0.19$ ($SE = 0.02$), $t(523) = 7.58$, $p < .001$, $\beta = -0.26$.[21] Second, a logistic regression revealed that participants' subjective value positively predicted their likelihood of choosing to negotiate again with the same counterpart after controlling for the dishonesty instruction manipulation and guilt, $B = 0.87$ ($SE = 0.13$), $Z = 6.55$, $p < .001$, $OR = 2.39$. A negative indirect effect of the dishonesty instruction on counterpart choice through guilt and subjective value emerged in serial order, indirect effect = −0.14, 95% CI [−0.23, −0.08].

Manipulation Checks

To verify our manipulation, we confirmed that the dishonesty instruction impacted participants' self-reported dishonesty, $t(526) = 11.52$, $p < .001$, $d = 1.00$, 95% CI [0.82, 1.18]. As intended, participants instructed to behave dishonestly ($M = 3.1$, $SD = 2.1$) indicated that they were more dishonest than those instructed to be honest ($M = 1.4$, $SD = 1.0$).

An analysis of counterparts' response to the question about the buyer's intended use of the property also revealed that the dishonesty instruction impacted counterparts' likelihood of being misinformed, $\chi^2(1, N = 528) = 44.53$, $p < .001$, $OR = 0.30$, 95% CI [0.21, 0.43]. Counterparts paired with a focal participant in the dishonesty instruction condition (43%) had less than one-third the odds of correctly identifying the buyer's intentions compared to counterparts paired with a focal participant in the honesty instruction condition (72%). These checks confirm that instructions to be dishonest impacted focal participants' self-perceived dishonesty and increased counterparts' likelihood of being misled about the buyer's intentions.

Discussion

In a more tightly controlled context where negotiators were instructed to be honest or dishonest, Study 3B builds on Study 3A by providing causal evidence consistent with the prior studies. It replicates support for the deceiver's guilt account (Hypothesis 1b) and several findings from our previous studies; once again, guilt mediated dishonesty's negative effect on subjective value (Studies 1 and 2) and the reduced subjective value of dishonest participants led them to avoid future interactions with the same counterpart (Study 2).

General Discussion

Across four studies, undetected dishonesty undermined negotiators' subjective value by inducing guilt. This pattern emerged when lies had both high and low financial stakes, and when participants had high and low levels of moral character. While moral character failed to moderate the results, higher financial stakes intensified guilt. The pattern also held for lies told of one's own volition, lies executed as an agent acting on others' orders, and in contexts ranging from anonymous interactions with strangers to face-to-face interactions with acquaintances. The decline in subjective value triggered by their dishonesty undermined negotiators' future interactions with counterparts they deceived and led them to avoid those counterparts—despite their lies enhancing their economic outcomes and going undetected.[22]

Theoretical Implications

Dishonesty's Consequences

Our research extends knowledge of dishonesty's consequences in negotiation. Dishonesty has the potential to enhance negotiators' economic outcomes (Bazerman et al., 2000; O'Connor & Carnevale, 1997), yet undermines their reputation when others detect it (Schweitzer et al., 2006; Shapiro & Bies, 1994). By describing the psychological impact of *undetected* dishonesty on deceivers, our research shifts the portrait of deception in negotiation from "nasty but effective" (O'Connor & Carnevale, 1997, p. 504) toward recognizing that even successful deceit harms deceivers by inducing guilt, dissatisfaction, and leading them to truncate relationships with deceived parties. Despite getting away with their lies, deceivers were often less satisfied with the negotiation experience than the victims of their dishonesty (see online Supplemental Materials, Supplemental Analysis). These advances represent a shift from studying the antecedents of deception toward understanding its consequences (Gaspar et al., 2019).

We build on prior studies by documenting whether dishonesty's positive affective consequences (Ruedy et al., 2013) extend to mixed-motive interactions with others. While we did find evidence of dishonesty inducing positive affect in a supplemental study of privately misreporting one's performance (see online Supplemental Materials Study S3), dishonesty elicited more guilt than positive affect even in this context. We conclude that dishonesty can induce positive affect and guilt in independent decision contexts or situations that do not involve lying to a salient target. But dishonesty may not induce positive affect when it involves deceiving another negotiator.

Our findings highlight dishonesty-induced guilt as a crucial driver of how individuals appraise interactions with others. Prior research

[21] Replicating Studies 1 and 2, a separate mediation analysis found a negative indirect effect of dishonesty on subjective value through guilt, indirect effect = −0.16, 95% CI [−0.23, −0.09].

[22] Supplemental studies corroborate our findings in another bargaining context where counterparts' behavior was more tightly controlled (see online Supplemental Materials Studies S1 and S2).

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has studied anticipatory guilt's effect on relationships (Wiltermuth & Cohen, 2014) and noted guilt's functionality for limiting behavior that harms others (e.g., Cohen et al., 2011; Flynn & Schaumberg, 2012; Schaumberg & Flynn, 2017). We demonstrate that experienced guilt triggered by dishonesty negatively colors negotiators' future interactions with counterparts and can motivate them to exit relationships.

High-Stakes Lies' Consequences

We also contribute to knowledge of the affective consequences of high stakes lies. In contrast to previous evidence that large incentives can reduce cognitive dissonance and alleviate guilt from exaggerating one's attitudes (Festinger & Carlsmith, 1959), we find that they can exacerbate guilt from misleading others about material facts in a negotiation. This finding might suggest that, in contrast to exaggerating one's attitudes (which can be rationalized as a matter of opinion and thus not deceptive), lies about material facts are more difficult to rationalize in a manner that alleviates guilt. It also lends credence to Ekman's (1992) assumption that emotions triggered by lying increase in high-stakes settings.

Subjective Value's Consequences

Finally, our results attest to the importance of attending to subjective value in negotiation (Curhan et al., 2006). Despite enabling negotiators to achieve superior economic outcomes, dishonesty undermined their satisfaction with the bargaining experience. Like auction winners (cf. Thaler, 1988), dishonest negotiators were "cursed" by dissatisfaction with the outcomes of the negotiation process despite achieving exactly what they had hoped by getting away with a lie.

Our findings also corroborate prior evidence of subjective value's enduring impact beyond a focal negotiation (e.g., Curhan et al., 2006, 2009) by documenting a behavioral consequence of dishonesty's tendency to undermine subjective value: selecting a different negotiation partner. Without assessing subjective value, researchers might focus too keenly on dishonesty's economic impact at the expense of its relational impact (Van Zant & Kray, 2015).

Limitations and Future Directions

Future research should investigate boundary conditions for our effects. We tested the affective consequences of dishonesty in contexts where deceivers' actions adversely impacted a salient target of equal social standing. However, because successful deceit can produce a sense of accomplishment (Ruedy et al., 2013), it might elicit more positive affect when utilized against competitors or those with greater power. Also, our studies focus on a type of dishonesty common in negotiations: information-based deception for personal gain. But the intent and type of deception can vary. Some lies are prosocial (i.e., intended to benefit the counterpart; Levine & Schweitzer, 2014, 2015) or emotional (i.e., intended to misrepresent the negotiator's emotions rather than critical facts; Barry, 1999; Fulmer et al., 2009). Negotiators might find both types of deception more acceptable than the lies studied here (Gaspar etal., 2019).

There also could be value in future research attempting to identify individual difference variables that might influence the impact of dishonesty on guilt. We failed to find evidence of three different traits relevant to moral character (moral identity internalization, empathic concern, and self-control) moderating the effect of dishonesty on guilt. Our selection of these traits was motivated by their correspondence to Cohen and Morse's (2014) tripartite model of moral character. However, guilt proneness is another trait with potential to shape negotiators' affective response to deceiving others. Although the moral character traits we assessed tend to correlate with guilt proneness (Cohen et al., 2014), it is a theoretically and empirically distinct construct. We suspect that, because guilt proneness directly captures how guilty people believe they will feel after committing an ethical infraction (Cohen et al., 2011), it may be the single trait most likely to impact the extent to which dishonesty elicits guilt.

Conclusion

Scholars have long known the risks of detected dishonesty (Schweitzer et al., 2006; Shapiro, 1991). Our investigation breaks new ground by showing how even *undetected* dishonesty harms negotiators. Despite lending the upper hand in a bargaining context, it leads negotiators to feel guilty, undermines their satisfaction, and reduces their interest in continuing a relationship with counterparts. Considering dishonesty's psychological and relational costs, it could be less appealing as a value-claiming tactic than is commonly believed. Living with the costs of dishonesty might be psychologically more challenging than forgoing its benefits.

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