Online Supplemental Materials

Contents

The Effect of Dishonesty on Each Subjective Value Dimension

Although our main analyses focus on a 13-item measure of subjective value used in prior research (Curhan et al., 2009), the full 16-item version of the scale has a four-factor structure: feeling about the instrumental outcome, self, process, and relationship (Curhan et al., 2006). To explore the robustness of our effects for each dimension of subjective value, we conducted a meta-analysis of Studies 1-3B by assessing the overall effect of dishonesty on each subjective value dimension across the studies.

First, we assigned each item on the 13-item subjective value inventory to the subscale it most strongly loaded onto in the original scale validation study conducted by Curhan et al. (2006). After assigning each item to its appropriate subscale, we averaged all items within each

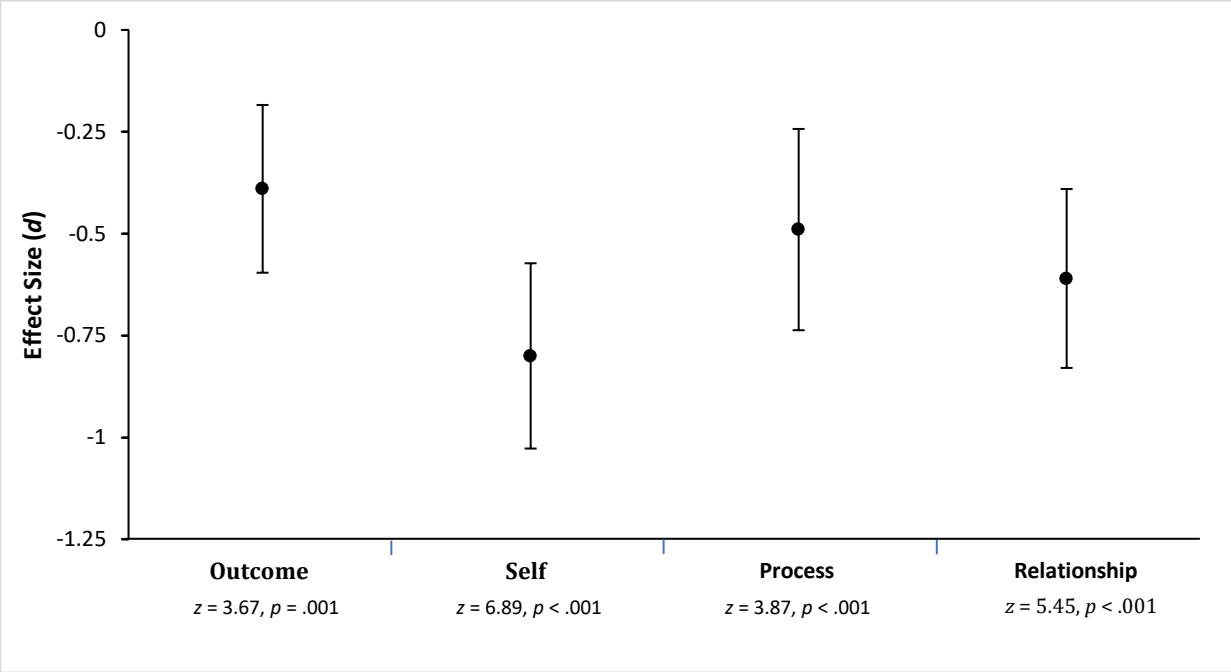
subscale into a single index. We followed this same approach with Study 3A, which only used a selection of four items from the original scale. To correct for differences in the items used in Study 3A compared to the other studies, we standardized each subjective value subscale within each study.

To estimate the meta-analytic effect of dishonesty on each subjective value dimension, we regressed the standardized score corresponding to each dimension on participants' dishonesty in a series of multilevel regression models nesting observations within studies using a study-specific random intercept (estimated using maximum likelihood estimation). To relax assumptions that each study was testing a homogenous effect of dishonesty on subjective value, we incorporated a random coefficient allowing the effect of dishonesty to vary across studies (estimated using an unstructured covariance matrix). In the interest of obtaining coefficient estimates that approximate effect size *d*, we contrast coded the dishonesty variable for the experimental studies (-½ for honest participants, ½ for dishonest participants). For Study 3A, we standardized the continuous dishonesty measure and multiplied the resulting value by 0.5 to place it on the same scale as the contrast-coded measure used for the other studies. All models controlled for dyad-level agreement.

We document the effect of dishonesty on each subjective value dimension in Figure S1. The effect of dishonesty was significant for each of the four dimensions of subjective value.

Figure S1

Meta-Analytic Estimates of the Effect of Dishonesty on Each Subjective Value Dimension



Note. This figure illustrates the mean estimated effect (dots) and 95% confidence intervals for the effect of dishonesty on each of the four subjective value dimensions: feelings about the instrumental "outcome," self, process, and relationship.

Analyses of 10-Item SVI Inventory

As discussed in Footnote 2 of the main manuscript, we conducted alternative analyses to assess whether our results might have been driven by some items in the SVI inventory assessing direct reactions to the choice to be dishonest. As documented in this section, the pattern of our results does not change when using a 10-item SVI measure that omits these items: "I think the terms of my agreement were fair," "I behaved according to my own principles and values," and "the negotiation process was fair."

Study 1 Results

A 2 (dishonesty, control) X 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 = 4.8, SD = 1.3), $F(1, 420) = 15.06$, $p < .001$, $\eta_p^2 = .03$. We did not find evidence of a main effect of incentive, $F(1, 420) = 0.35$, $p = .55$, $\eta_p^2 < .01$, nor a dishonesty X incentive interaction, $F(1, 420) = 0.72$, $p = .40$, $\eta_p^2 < .01$. The effect of dishonesty on subjective value held for both the large incentive, $F(1, 420) = 11.08$, $p < .001$, $\eta_p^2 = .03$, and the small incentive, $F(1, 420) = 4.65$, $p = .032$, $\eta_p^2 = .01$. We continued to find evidence of an independent indirect effect of dishonesty on subjective value through guilt, 95% CI [-0.19, -0.003]; this indirect effect held in both the large (95% CI [-0.15, -0.002]) and small (95% CI [-0.24, -0.004]) incentive conditions.

Study 2 Results

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, $F(1, 197) = 4.70$, $p = .031$, $\eta_p^2 = .02$. Relative to participants in the control condition (M = 4.1, SD = 1.0), participants who chose to be dishonest were less satisfied with the negotiation experience (M = 4.2, SD = 0.9).[1]

A separate ANCOVA revealed that dishonesty in the first negotiation also undermined focal participants' subjective value in the second negotiation, $F(1, 197) = 9.51$, $p = .002$, $\eta_p^2 = .05$.

[1] Although the unadjusted raw mean of the dishonesty opportunity condition was similar (and slightly exceeded) the raw mean of the control condition, the adjusted means accounting for the influence of preregistered model covariates confirmed that the subjective value of dishonest participants was lower than that of control condition participants after accounting for agreement and perceived counterpart suspicion ($M_{Dishonest}$ = 4.0 vs. $M_{Control}$ = 4.4).

Relative to participants in the control condition (M = 5.5, SD = 1.0), those who chose to be dishonest in the first negotiation were less satisfied with their experience in the second negotiation (M = 5.0, SD = 0.9).

We continued to find evidence of a trend where the effect of dishonesty on Negotiation 2 subjective value was mediated by an indirect effect through Negotiation 1 guilt and Negotiation 1 subjective value, in serial 90% CI [-0.05, -0.001], although the effect was not significant by conventional standards, 95% CI [-0.06, 0.005]. Similarly, we continued to find evidence of a trend where the effect of dishonesty on counterpart choice in the final negotiation was mediated by a serial indirect effect through Negotiation 1 guilt and Negotiation 1 subjective value, 90% CI [-0.20, -0.002], although the effect was also not significant by conventional standards, 95% CI [-0.22, 0.02].

Study 3A Results

Because Study 3A used an abridged four-item measure of subjective value, it only contained a single item to be omitted for our robustness check ("Would you characterize the process as fair?"). Therefore, we conducted alternative analyses of a three-item measure that omitted this item. As documented in Table S1, the effect of dishonesty on subjective value continued to hold when omitting this item, $t(101) = 2.43$, $p = .017$.

Table S1

Study 3A: Regression Predicting Focal Participants' Subjective Value

Variable	*B*	*SE*	β
Dishonesty	-0.23	0.10	-0.23*
Agreement	0.86	0.21	0.38***
Instructor Fixed Effect		Included	

Note. * $p < .05$. *** $p < .001$

Study 3B Results

Replicating the prior studies, and supporting the deceiver's guilt account (Hypothesis 1b) an ANCOVA testing the effect of the dishonesty instruction manipulation on focal participants' subjective value identified an effect of the dishonesty instruction, $F(1, 524) = 12.91$, $p < .001$, $\eta_p^2 = .02$. Relative to participants who were instructed by their client to be honest ($M = 5.3$, $SD = 1.2$), those who were instructed to behave dishonestly ($M = 5.2$, $SD = 1.1$) derived lower subjective value from the negotiation. We continued to find evidence of an indirect effect of dishonesty on counterpart choice through guilt and subjective value, in serial, 95% CI [-0.18, -0.06].

Validating that Lies Went Undetected and Enhanced Negotiators' Economic Outcomes

A core assumption of our investigation is that dishonesty frequently goes undetected by counterparts and tends to enhance negotiators' economic outcomes. To check whether these assumptions held true in our studies, we collected additional variables for supplemental analyses. As documented below, we failed to find evidence in any of our studies that counterparts could detect negotiators' dishonesty. Although we found some evidence that counterparts could detect honesty (Study 1), we did not find evidence of dishonest participants appearing more untrustworthy than participants who did not have the opportunity to lie (Studies 1 and 2). Further, dishonesty enhanced negotiators' economic outcomes in each study.

Study 1

Did Counterparts Detect Dishonesty?

Main comparison: Dishonest vs. control participants. Counterparts completed the same subjective value items as focal participants ($\alpha = .93$) and indicated their perception of how dishonest focal participants were during the negotiation (two items: honest and truthful; 1 = "not

at all" to 7 = "extremely;" α = .97; reverse-scored to reflect dishonesty). We analyzed both measures using 2 (dishonesty) X 2 (incentive) ANOVAs.

Despite being deceived, the counterparts of dishonest focal participants perceived them to be *less* dishonest than the counterparts of control condition participants ($M_{\text{Dishonest}}$ = 2.9, *SD* = 1.5 vs. M_{Control} = 3.3, *SD* = 1.5), $F(1, 422) = 7.61$, $p = .006$, $\eta_p^2 = .02$. This indicates that dishonest focal participants not only got away with their lies, but that their counterparts found them to be more credible than the counterparts of participants in the control condition. We did not find evidence of either an incentive main effect, $F(1, 422) = 0.21$, $p = .65$, $\eta_p^2 < .01$, or a dishonesty X incentive interaction, $F(1, 422) = 0.91$, $p = .34$, $\eta_p^2 < .01$.

An analysis of counterparts' subjective value also largely mirrored their perceptions of focal participants' dishonesty. Despite being lied to, the counterparts of dishonest focal participants derived greater subjective value from the negotiation than the counterparts of control condition participants ($M_{\text{Dishonest}}$ = 5.3, *SD* = 1.2 vs. M_{Control} = 4.8, *SD* = 1.2), $F(1, 422) = 15.39$, $p < .001$, $\eta_p^2 = .04$. Neither the incentive main effect, $F(1, 422) = 0.09$, $p = .76$, $\eta_p^2 < .01$, nor the dishonesty X incentive interaction, $F(1, 422) = 1.54$, $p = .21$, $\eta_p^2 < .01$, were significant.

Ancillary comparisons: Honest vs. dishonest and control participants. Participants who chose to be honest in the dishonesty opportunity condition were perceived to be less dishonest than all other participants. A 3 (participant choice: dishonest, honest, control) X 2 (incentive) ANOVA found a main effect of participants' choice, $F(2, 485) = 12.86$, $p < .001$, $\eta_p^2 = .05$. Honest participants (*M* = 2.3, *SD* = 1.4) were perceived to be less dishonest than participants who were (a) dishonest, $t(485) = 2.82$, $p = .005$, $d = 0.26$, 95% CI [0.08, 0.43], and (b) in the control condition, $t(485) = 0.44$, $d = 0.44$, 95% CI [0.26, 0.62]. We did not find

evidence of either an incentive main effect, $F(1, 485) = 0.09$, $p = .76$, $\eta_p^2 < .01$, or an interaction, $F(2, 485) = 0.46$, $p = .63$, $\eta_p^2 < .01$. These findings suggest that counterparts might have detected participants' honesty: those who passed on the opportunity to lie were perceived as particularly trustworthy. However, given that counterparts also perceived dishonest participants to be more credible than those in the control condition, participants generally got away with their lies despite not appearing quite as credible as those who passed on the opportunity to lie.

A 3 X 2 ANOVA on counterparts' subjective value also revealed a main effect of focal participants' choice on counterparts' subjective value, $F(2, 485) = 9.34$, $p < .001$, $\eta_p^2 = .04$. The counterparts of participants who chose to be honest ($M = 5.3$, $SD = 1.2$) derived greater subjective value than those of control condition participants, $t(485) = 2.96$, $p = .003$, $d = 0.27$, 95% CI [0.09, 0.45]. However, despite perceiving honest focal participants to be less dishonest than participants who chose to be dishonest, the counterparts of honest participants derived similar subjective value as those of dishonest participants, $t(485) = 0.22$, $p = .82$, $d = 0.02$, 95% CI [-0.16, 0.20]. We did not find evidence of an incentive main effect, $F(1, 485) = 0.03$, $p = .87$, $\eta_p^2 < .01$, or an interaction, $F(2, 485) = 0.86$, $p = .42$, $\eta_p^2 < .01$.

Did Dishonesty Impact Negotiators' Economic Outcomes?

We also tested whether focal participants' dishonesty impacted their economic outcomes. Dishonest focal participants more likely to reach an agreement with counterparts (86%) than participants in the control condition (78%), $\chi^2(1, N = 426) = 4.67$, $p = .031$, $OR = 1.76$, 95% CI [1.05, 2.96]. Further, when they did strike a deal, dishonest focal participants also achieved a better economic outcome (i.e., sold the computer for a higher price) than those in the control condition ($M_{\text{Dishonest}} = \$3{,}442.47$, $SD = 530.86$ vs. $M_{\text{Control}} = \$3{,}119.46$, $SD = 540.05$), $t(345) = 5.59$, $p < .001$, $d = 0.60$, 95% CI [0.39, 0.82]. Thus, dishonesty reduced focal participants'

subjective value even while it made it easier to strike a profitable deal and enhanced their economic outcome.

Study 2

Did Counterparts Detect Dishonesty?

Main comparison: Dishonest vs. control participants. Counterparts completed the same subjective value items as focal participants after the first (α = .89) and second negotiation (α = .92). They also indicated their perception of focal participants' dishonesty after the first negotiation using the same measure as Study 1 (α = .95).

We found evidence of a trend where the counterparts of dishonest focal participants perceived them to be directionally less dishonest than those in the control condition ($M_{\text{Dishonest}}$ = 3.3, *SD* = 1.3 vs. M_{Control} = 3.6, *SD* = 1.5), $t(199) = 1.85$, $p = .065$, $d = 0.26$, 95% CI [-0.02, 0.54]. As with Study 1, this indicates that dishonest focal participants got away with their lies.

An analysis of counterparts' Negotiation 1 subjective value also mirrored Study 1. Despite being lied to, the counterparts of dishonest focal participants derived greater subjective value from the first negotiation than the counterparts of control condition participants ($M_{\text{Dishonest}}$ = 4.3, *SD* = 0.9 vs. M_{Control} = 4.8, *SD* = 1.2), $t(199) = 3.44$, $p < .001$, $d = 0.49$, 95% CI [0.21, 0.77]. However, this effect disappeared in the second negotiation ($M_{\text{Dishonest}}$ = 5.4, *SD* = 0.9 vs. M_{Control} = 5.41, *SD* = 0.9), $t(199) = 0.12$, $p = .90$, $d = 0.02$, 95% CI [-0.26, 0.29].

Ancillary comparisons: Honest vs. dishonest and control participants. Unlike Study 1, we failed to find evidence that focal participants' choice to be honest impacted counterparts' perceptions of their dishonesty. An ANOVA failed to find evidence of a main effect of participants' choice on counterpart perceptions of their dishonesty, $F(2, 225) = 1.88$, $p = .15$, η_p^2

= .02. Counterparts perceived the dishonesty of honest participants (M = 3.3, SD = 1.3) to be similar to that of dishonest and control condition participants (both ps > .30).

Further, while an ANOVA revealed a main effect of participants' choice on counterparts' Negotiation 1 subjective value, $F(2, 225) = 6.17$, $p = .002$, $\eta_p^2 = .05$, the effect appeared to be primarily driven by the counterparts of dishonest focal participants deriving greater subjective value from Negotiation 1 than the counterparts of control condition participants. Relative to the counterparts of dishonest participants, $t(225) = 1.60$, $p = .11$, $d = 0.21$, 95% CI [-0.05, 0.47], and those in the control condition, $t(225) = 0.67$, $p = .50$, $d = 0.09$, 95% CI [-0.17, 0.35], the counterparts of participants who chose to be honest derived similar subjective value (M = 4.5, SD = 0.9). The main effect of participants' honesty disappeared in the second negotiation, $F(2, 225) = 0.02$, $p = .98$, $\eta_p^2 < .01$.

Did Dishonesty Impact Negotiators' Economic Outcomes?

As with Study 1, dishonest focal participants were more likely to reach an agreement with counterparts in Negotiation 1 (64%) than those in the control condition (31%), $\chi^2(1, N = 201) = 22.89$, $p < .001$, $OR = 4.10$, 95% CI [2.27, 7.41]. Further, when they did strike a deal, dishonest participants also achieved a better economic outcome in Negotiation 1 than those in the control condition ($M_{\text{Dishonest}}$ = \$3,615.33, SD = 535.61 vs. M_{Control} = \$3,127.44, SD = 489.22), $t(90) = 4.35$, $p < .001$, $d = 0.94$, 95% CI [0.49, 1.38]. Once again, dishonesty reduced participants' subjective value in Negotiation 1 even while it made it easier to strike a profitable deal and increased their payoff.

However, the superior economic outcomes of dishonest focal participants in Negotiation 1 did not persist in Negotiation 2 ($M_{\text{Dishonest}}$ = \$2,408.72, SD = 534.66 vs. M_{Control} = \$2,408.73, SD = 578.81), $t(194) < 0.01$, $p > .99$, $d < 0.01$, 95% CI [-0.28, 0.28]. Thus, although it did not

impact their economic outcomes in Negotiation 2, dishonesty still undermined focal participants' subjective value. However, despite all control condition participants reaching agreement in Negotiation 2, not all focal participants who were dishonest in Negotiation 1 reached agreement in Negotiation 2 (94%), $\chi^2(1, N = 201) = 6.32$, $p = .012$. Follow-up analyses revealed that the higher agreement rates in Negotiation 2 of participants who were dishonest in Negotiation 1 cannot account for the impact of dishonesty-induced guilt in Negotiation 1 on focal participants' Negotiation 2 subjective value, or their choice of counterpart in the final negotiation.[2]

Study 3A

Did Counterparts Detect Dishonesty?

Counterparts completed the same subjective value items as focal participants after the negotiation ($\alpha = .88$; two counterparts did not complete SVI items). As with the prior studies, we did not find evidence that counterparts could detect focal participants' dishonesty. Controlling for instructor, the coded measure of dishonesty did not predict counterparts' subjective value, $B = -0.12$ ($SE = 0.10$), $t(100) = 1.15$, $p = .25$, $\beta = -0.12$. This provides some indirect evidence that focal participants' lies—which should presumably be negatively correlated with counterparts' subjective value if they were detected—went undetected by counterparts. Further, although our dataset only contained the variable for a subset of participants,[3] we did not find evidence of a correlation between focal participants' dishonesty and counterparts' perceptions of their dishonesty after controlling for instructor, $B = 0.04$ ($SE = 0.27$), $t(29) = 0.14$, $p = .89$, $\beta = 0.02$.

[2] When adding Negotiation 2 agreement as a covariate in our Study 2 mediation analyses, the serial indirect effects of dishonesty through Negotiation 1 guilt and Negotiation 1 subjective value held for Negotiation 2 subjective value, 95% CI [-0.09, -0.01], and counterpart choice in the final negotiation, 95% CI [-0.39, -0.06].

[3] One instructor did not include this item on the post-negotiation survey, and a programming error resulted in this item not being incorporated into the post-negotiation survey for one of the other instructor's sections. One additional counterpart did not respond to the item.

This contains more direct, albeit limited, evidence that counterparts did not detect focal participants' dishonesty.

Did Dishonesty Impact Negotiators' Economic Outcomes?

We also explored whether focal participants' dishonesty impacted their economic outcomes, controlling for instructor idiosyncrasies. First, we regressed dyad-level agreement on focal participant dishonesty in a logistic regression model. Although dishonesty directionally increased a dyad's likelihood of reaching agreement, the effect was not significant, $B = 0.39$ ($SE = 0.25$), $z = 1.58$, $p = .11$, $OR = 1.48$. However, the more dishonest focal participants were, the better the economic terms of their deal tended to be (i.e., a lower purchase price, in millions of dollars), $B = -0.75$ ($SE = 0.43$), $t(73) = 1.74$, $p = .086$, $\beta = -0.21$. Thus, focal participants' dishonesty reduced their subjective value even as it marginally improved their economic outcomes.

Study 3B

Did Counterparts Detect Dishonesty?

Counterparts completed the same subjective value items as focal participants ($\alpha = .93$) and indicated their perception of how honest and truthful focal participants were during the negotiation ($\alpha = .96$). Consistent with Study 3A, we did not find evidence of the dishonesty instruction manipulation impacting counterparts' perception of focal participants' dishonesty ($M_{\text{Dishonesty}} = 2.3$, $SD = 1.3$ vs. $M_{\text{Honesty}} = 2.5$, $SD = 1.5$), $t(526) = 0.97$, $p = .33$, $d = 0.08$, 95% CI [-0.09, 0.26]. We also failed to find evidence of counterparts' subjective value being impacted by the dishonesty instruction manipulation ($M_{\text{Dishonesty}} = 5.6$, $SD = 1.0$ vs. $M_{\text{Honesty}} = 5.4$, $SD = 1.1$), $t(526) = 1.63$, $p = .103$, $d = 0.14$, 95% CI [-0.03, 0.31]. These findings indicate that participants'

dishonesty went undetected by their counterparts and, like Study 3A, had little impact on counterparts' perception of the negotiation.

Did Dishonesty Impact Negotiators' Economic Outcomes?

Here, we found that negotiators' dishonesty facilitated agreements and enhanced negotiators' economic outcomes.[4] Focal participants instructed to behave dishonestly were more likely to reach an agreement with counterparts (92%) than those instructed to be honest (85%), $\chi^2(1, N = 528) = 5.42$, $p < .02$, $OR = 1.91$, 95% CI [1.10, 3.31]. Further, when they did strike a deal, focal negotiators instructed to behave dishonestly also achieved a better economic outcome (i.e., purchased the property at a lower price) than those instructed to be honest ($M_{\text{Dishonesty}}$ = \$17.3 million, $SD = 3.2$ vs. M_{Honesty} = \$17.9 million, $SD = 3.1$), $t(463) = 2.08$, $p = .038$, $d = 0.19$, 95% CI [0.01, 0.38]. Thus, dishonesty reduced participants' subjective value even while it made it easier to strike a profitable deal that enhanced their economic outcome.

Study 1: Incentive Pretesting

Method

To identify an incentive that would be perceived by our target sample (Amazon Mechanical Turk workers, or MTurk workers) as being sufficiently large to potentially alleviate any cognitive dissonance dishonest Study 1 participants may experience, we conducted a pretest on 102 MTurk workers (M_{Age} = 37.3 years, $SD = 10.3$, 47 % female).

Participants were informed that they would read about a hypothetical negotiation and answer questions about it at the outset of the study. We then presented them with the same seller role instructions as focal participants in the dishonesty opportunity condition of Study 1.

[4] We did not preregister these as exploratory variables and made a post-hoc decision to validate that dishonesty impacted negotiators' agreement rates and economic outcome.

However, rather than presenting a payoff chart listing a specific bonus payment associated with different sale prices, we instead framed each sale price as a percentage of a "maximum bonus" payment sellers could earn. We then prompted participants to indicate an amount (in dollars) they would consider sufficiently large to make them feel comfortable with behaving dishonestly. Participants read the following prompt before indicating their response on a sliding scale ranging from $0 to $20.

> Based on this payment structure, what is the minimum amount of money (in dollars) that you would consider to be a "large" maximum bonus payment? Remember: By "large," we mean an amount that would be just enough to make you feel comfortable with using any tactics necessary to perform as well as possible in the negotiation (including lying).

To incentivize participants to report honestly, we subjected them to the Bayesian Truth Serum algorithm (Prelec, 2004). The formula compares participants' actual responses to their predictions about how prevalent the same response would be among sampled participants, and rewards responses that are more common than predicted. Replicating a prompt demonstrated to increase the prevalence of truth-telling, we told participants that they would be assigned a "Truth Score," which was "invented by an MIT professor and published in the academic journal *Science*," and that the score "rewards you for answering truthfully" (Weaver & Prelec, 2013). We then informed participants that we would randomly select ten participants to distribute a pool of $100 in bonus payments between. If selected to receive a bonus payment, participants' cut of the $100 bonus payment pool stood to increase the higher their "Truth Score" relative to the score of other bonus-eligible participants. To reinforce this incentive, we told participants "What this means for you is simple: the more honest you are about what you consider to be a 'large' bonus payment, the larger your expected bonus payment."

Results

Participants' mean response to what would constitute a "large" maximum bonus payment was $12.31 (*SD* = $6.09). Based on this response, we adopted a maximum bonus payment of $12.50 for the large incentive condition of Study 1.

Studies 1 and 2: Dishonesty Opportunity Manipulation Checks

Because our preregistered data stopping rule required us to actively monitor how many participants met our criteria for inclusion in final analyses as we decided whether it would be necessary to post additional assignments to MTurk, we included two dishonesty manipulation checks to assist us in quickly estimating the number of participants in the dishonesty incentive condition who chose to be dishonest.

First, we asked focal participants how "honest" and "truthful" they were during the negotiation (1 = "not at all" to 7 = "extremely"). Both items were reliable (α = .98), so we reverse-scored and averaged them into a measure of self-reported dishonesty. Second, we asked participants what they told their counterpart (the buyer) about the computer's condition. They chose the best description among three options: "I told the buyer that the graphics card is defective" (truth; coded 1), "I did not tell the buyer anything about the condition of the graphics card" (lie by omission; coded 2), and "I told the buyer that the graphics card is in perfect working condition" (lie by commission; coded 3).

Study 1

2 (dishonesty) X 2 (incentive) ANOVAs conducted across all focal participants in Study 1 (i.e., including those who did not choose to lie in the dishonesty opportunity condition) revealed that, relative to participants in the control condition, those in the dishonesty opportunity condition (a) considered themselves to have been more dishonest in the negotiation ($M_{\text{Dishonesty}}$

$_{\text{Opportunity}}$ = 3.2, *SD* = 2.0 vs. M_{Control} = 2.1, *SD* = 1.3), $F(1, 487) = 52.22$, $p < .001$, $\eta_p^2 = .10$, and (b) reported that they spoke about the graphics card in a more misleading fashion ($M_{\text{Dishonesty Opportunity}}$ = 1.7, *SD* = 0.6 vs. M_{Control} = 1.4, *SD* = 0.5), $F(1, 487) = 33.03$, $p < .001$, $\eta_p^2 = .06$. We did not find effects of incentive (both *p*s < .33), or a dishonesty opportunity X incentive interaction (both *p*s < .44) for either of these measures. These findings indicate that the dishonesty opportunity manipulation was effective at enticing participants to lie on their own volition.

Study 2

As with Study 1, an analysis across all focal participants (i.e., including those who did not choose to lie in the dishonesty opportunity condition) revealed that the dishonesty opportunity manipulation (a) increased participants' self-reported dishonesty ($M_{\text{Dishonesty Opportunity}}$ = 3.6, *SD* = 1.9 vs. M_{Control} = 2.6, *SD* = 1.6), $t(226) = 4.16$, $p < .001$, $d = 0.55$, 95% CI [0.29, 0.81], and (b) increased self-reports of speaking about the graphics card in a misleading fashion ($M_{\text{Dishonesty Opportunity}}$ = 1.8, *SD* = 0.7 vs. M_{Control} = 1.3, *SD* = 0.6), $t(226) = 4.83$, $p < .001$, $d = 0.64$, 95% CI [0.73, 0.91].

Studies 1 and 2: Details of Ancillary Analyses

Study 1

A 3 (participant choice: dishonest, honest, control) X 2 (incentive) ANOVA revealed a main effect of participants' choice to be honest, $F(2, 483) = 29.30$, $p < .001$, $\eta_p^2 = .11$. Follow-up contrasts revealed that honest participants (*M* = 5.7, *SD* = 0.9) derived greater subjective value than dishonest participants, $t(483) = 6.71$, $p < .001$, $d = 0.61$, 95% CI [0.43, 0.79], and control condition participants who did not have an opportunity to lie, $t(483) = 2.79$, $p = .005$, $d = 0.25$, 95% CI [0.07, 0.43].

A 3 X 2 ANCOVA on participants' guilt also revealed a main effect of participants' choice, $F(2, 483) = 65.58$, $p < .001$, $\eta_p^2 = .21$. Follow-up contrasts revealed that, relative to dishonest participants, honest participants ($M = 1.4$, $SD = 1.0$) felt less guilty, $t(483) = 8.16$, $p < .001$, $d = 0.74$, 95% CI [0.56, 0.93]. However, honest participants felt similar levels of guilt as control condition participants, $t(483) = 1.10$, $p = .27$, $d = 0.10$, 95% CI [-0.08, 0.28].

Study 2

An ANCOVA revealed a main effect where participants' Negotiation 1 subjective value varied with their choice of whether to be honest, $F(2, 223) = 6.98$, $p = .001$, $\eta_p^2 = .06$. Follow-up contrasts revealed that, like in Study 1, honest participants ($M = 4.6$, $SD = 1.1$) derived greater subjective value than dishonest participants, $t(223) = 2.82$, $p < .001$, $d = 0.38$, 95% CI [0.11, 0.64]. However, while their subjective value was directionally larger than that of control condition participants who did not have an opportunity to lie, the difference was not significant, $t(223) = 0.63$, $p = .53$, $d = 0.08$, 95% CI [-0.18, 0.35].

An analysis of participants' Negotiation 1 guilt revealed an identical pattern to their subjective value. An ANCOVA revealed a main effect of participants' choice, $F(2, 223) = 16.12$, $p < .001$, $\eta_p^2 = .13$. Replicating Study 1, follow-up contrasts revealed that honest participants ($M = 2.0$, $SD = 1.2$) felt less guilty than dishonest participants, $t(223) = 3.89$, $p < .001$, $d = 0.52$, 95% CI [0.25, 0.78]. However, honest participants did not feel any less guilt than control condition participants, $t(223) = 0.38$, $p = .71$, $d = 0.05$, 95% CI [-0.21, 0.31].

We also analyzed downstream consequences of participants' choice to be honest. To analyze the impact of participants' choice to be honest on counterpart choice, we created dummy variables corresponding to (a) whether participants chose to be dishonest in the dishonesty opportunity condition (1 = yes, 0 = no) and (b) whether they were in the control condition (1 =

yes, 0 = no); this enabled us to compare (a) honest to dishonest participants and (b) honest to control condition participants. When adding these dummy variables to a logistic regression that contained our preregistered controls (agreement and perceived counterpart suspicion), they improved the model's fit, $\chi^2(2, N = 228) = 8.92$, $p = .011$. Honest participants (93%) were more likely to choose to negotiate again with the same counterpart than dishonest participants, $B = -1.85$ ($SE = 0.78$), $\chi^2(1) = 5.57$, $p = .018$, $OR = 0.16$, 95% CI [0.03, 0.73], but they were not more likely than control condition participants to choose to negotiate again with the same counterpart, $B = -1.20$ ($SE = 0.77$), $\chi^2(1) = 2.40$, $p = .12$, $OR = 0.30$, 95% CI [0.07, 1.37].

Surprisingly, an analysis of Negotiation 2 subjective value revealed that honest participants experienced lower subjective value than control condition participants. An ANCOVA revealed a main effect of participants' choice of whether to be honest, $F(2, 223) = 5.62$, $p = .004$, $\eta_p^2 = .05$. Follow-up contrasts revealed that honest participants ($M = 5.2$, $SD = 1.3$) derived lower subjective value in the second negotiation than control condition participants, $t(223) = 2.20$, $p = .029$, $d = 0.29$, 95% CI [0.03, 0.56]. However, honest participants' Negotiation 2 subjective value did not differ from that of dishonest participants, $t(223) = 0.09$, $p = .93$, $d = 0.01$, 95% CI [-0.25, 0.27]. Considering that dishonest *and* honest participants' Negotiation 2 subjective value was lower than that of control condition participants, this pattern may suggest that merely being presented with the opportunity to deceive undermined negotiators' Negotiation 2 subjective value. On the one hand, the relatively small sample of honest participants in our analyses could mean the finding that honesty reduced negotiators' subjective value in Negotiation 2 is a false positive. On the other hand, it could potentially indicate that something about passing on the opportunity to lie could have triggered psychological processes between the first and second negotiation that undermined negotiators' experience in the second negotiation.

For example, upon realizing that they no longer benefitted from an information asymmetry like in the first negotiation, focal participants in the dishonesty opportunity condition may have been dissatisfied with their prospect of attaining a deal as favorable as they could have earned in the first negotiation.

Study 2: Details of Moral Character Analyses

To assess whether the distribution of participants' moral character might have varied as a function of participants' dishonesty, we ran t-tests on each moral character measure comparing dishonest to control condition participants. We did not find evidence of differences between dishonest participants and participants randomly assigned to the control condition in (a) moral identity internalization ($M_{\text{Dishonest}}$ = 4.5, SD = 0.5 vs. M_{Control} = 4.6, SD = 0.6), $t(199) = 0.46$, p = .65, d = 0.06, 95% CI [-0.21, 0.34], (b) empathic concern ($M_{\text{Dishonest}}$ = 3.8, SD = 0.7 vs. M_{Control} = 3.9, SD = 0.7), $t(199) = 0.79$, p = .43, d = 0.11, 95% CI [-0.17, 0.39], or (c) self-control ($M_{\text{Dishonest}}$ = 3.2, SD = 0.6 vs. M_{Control} = 3.3, SD = 0.7), $t(199) = 0.70$, p = .49, d = 0.10, 95% CI [-0.18, 0.38]. Separate logistic regressions also failed to find evidence of participants' choice of whether to behave dishonestly in the dishonesty opportunity condition being predicted by either (a) moral identity internalization (B = -0.37, SE = 0.46, z = 0.79, p = .43, OR = 0.69, 95% CI [0.28, 1.72], (b) empathic concern (B = 0.19, SE = 0.34, z = 0.56, p = .58, OR = 1.21, 95% CI [0.62, 2.33], or (c) self-control (B = -0.22, SE = 0.35, z = 0.63, p = .53, OR = 0.81, 95% CI [0.41, 1.58]).

Tests of Moral Character Moderating the Effect of Dishonesty on Affect

Table S2

Descriptive Statistics and Correlations Between Variables in Moral Character Analyses

Variable	*M*	*SD*	1	2	3	4	5	6	7	8
1. $\text{Guilt}_{\text{Neg 1}}$	2.8	1.8	-							
2. $\text{Guilt}_{\text{Neg 2}}$	2.2	1.4	.35***	-						
3. $\text{Pos Affect}_{\text{Neg 1}}$	3.4	1.5	<.01	.07	-					

4. Pos Affect$_{Neg\ 2}$	3.7	1.6	.07	.04	.67***	-				
5. Moral Identity	4.5	0.5	<.00	-.02	.04	.01	-			
6. Empathic Concern	3.9	0.7	-.12	-.10	.14*	.07	.52***	-		
7. Self-Control	3.2	0.7	.05	-.08	.09	.05	.15*	.08	-	
8. Agreement$_{Neg\ 1}$	0.5	0.5	.24***	.23***	.08	.04	.11	.02	-.06	-
9. Suspicion	3.6	1.1	-.04	.02	-.03	-.03	-.09	-.01	.03	-.39***

Note. Agreement$_{Neg\ 1}$ = 1 if dyad reached agreement, 0 otherwise. *Neg 1* = Negotiation 1; *Neg 2* = Negotiation 2.

* $p < .05$. *** $p < .001$.

Table S3

Models Testing for Moderation of Dishonesty's Impact on Affect by Moral Character

Variable	Guilt		Positive Affect	
	B	*SE*	*B*	*SE*
Moral Identity Internalization				
Dishonesty	0.38***	0.09	-0.05	0.11
Negotiation	-0.33***	0.06	0.14**	0.04
Dishonesty X Negotiation	-0.23***	0.06	-0.09*	0.04
Moral Identity Internalization	<0.01	0.08	<0.01	0.10
Dishonesty X Moral Identity Internalization	0.12	0.08	-0.11	0.10
Negotiation X Moral Identity Internalization	-0.02	0.06	-0.02	0.04
Dishonesty X Negotiation X Moral Identity Internalization	-0.01	0.06	0.04	0.04
Empathic Concern				
Dishonesty	0.37***	0.09	-0.04	0.11
Negotiation	-0.32***	0.06	0.14**	0.04
Dishonesty X Negotiation	-0.23***	0.06	-0.09*	0.04
Empathic Concern	-0.16	0.08	0.15	0.10
Dishonesty X Empathic Concern	-0.01	0.08	-0.01	0.10
Negotiation X Empathic Concern	0.03	0.06	-0.04	0.04
Dishonesty X Negotiation X Empathic Concern	0.04	0.06	0.05	0.04
Self-Control				
Dishonesty	0.39***	0.09	-0.04	0.11
Negotiation	-0.33***	0.06	0.14**	0.04
Dishonesty X Negotiation	-0.23***	0.06	-0.09*	0.04
Self-Control	0.03	0.09	0.12	0.10
Dishonesty X Self-Control	0.06	0.09	0.07	0.10
Negotiation X Self-Control	-0.11	0.06	-0.03	0.04
Dishonesty X Negotiation X Self-Control	-0.01	0.06	0.02	0.04

Note. Models are multilevel linear regression models nesting negotiation round within focal negotiators with a random intercept (estimated with maximum likelihood estimation). Agreement and perceived counterpart suspicion are controlled for in all models. *Dishonesty* = -1 for control condition, 1 for dishonest focal participants; *Negotiation* = -1 for Negotiation 1, 1 for Negotiation 2. All other independent variables are standardized.

* $p < .05$. ** $p < .01$. *** $p < .001$.

Study 3A: Correlations Between Variables in Main Analyses

Table S4

Study 3A: Means, Standard Deviations, and Correlations Between Variables

Variable	*N*	*M*	*SD*	1	2
1. Dishonesty	106	2.2	1.0	-	-
2. SVI	106	0.0	1.0	-.21*	-
3. Agreement	106	0.74	0.44	.11	.27**

Note. *Agreement* = 1 if dyad reached agreement, 0 if not.
* $p < .05$. ** $p < .01$.

Study S1: Deceiver's Guilt in an Ultimatum Bargaining Context

Study S1 compares the subjective value and affect of participants who chose to be dishonest (after being instructed and incentivized to behave dishonestly) to those who chose to be honest (after being instructed and incentivized to behave honestly). Unlike the studies reported in the main manuscript, this study involved an ultimatum bargaining context, which enabled us to send focal participants an unambiguous signal that their dishonesty went undetected, while holding agreement and counterparts' behavior constant.

Method

Participants

Anticipating a moderate effect size, we aimed to collect a final sample of 225 participants for our main analyses. To account for the possibility that up to 40% of participants might not follow their incentive and therefore would be excluded from the analyses (i.e., choosing to be dishonest in the honesty incentive condition or honest in the dishonesty incentive condition), we posted 375 assignments to MTurk, which resulted in a sample of 376 participants who completed the study in exchange for a $1.25 base payment ($M_{Age}$ = 37.2 years, *SD* = 11.0, 42% female).

Procedure

Participants played a modified ultimatum game where they negotiated over how to split a specified number of "tokens" that would determine their bonus payment for the study (Kagel et al., 1996), on top of the guaranteed $1.25 base payment. At the beginning of the study, participants learned that they would be assigned to a "proposer" or "responder" role in a "brief negotiation." Whereas proposers were responsible for making an offer for how to split a predetermined number of "tokens," responders could choose to "accept" or "reject" the proposer's offer. If responders chose to accept the offer, the tokens would be divided as specified in the proposer's offer. However, if responders chose to reject the offer, each party would only receive ¼ of the tokens available to split. We presented participants with an opportunity to briefly chat with a specific "counterpart" (ostensibly someone they would later negotiate with) by pairing them in a chatroom with another MTurk worker. Participants were instructed to "talk about whatever you like, whether it is to strategize about the negotiation that will follow or to simply talk about how your day is going."

After leaving the chatroom, all participants were assigned to play the responder role. We informed them that, unlike their counterpart in the bargaining task (for whom tokens would always be worth $0.01), participants' tokens would be worth either $0.01 or $0.02. However, they were also told that their partner "will not know how valuable each token is to you before making an offer." Participants were therefore instructed to start the negotiation by sending "a message to your counterpart indicating how valuable each token is to you" by "choosing between one of two messages" to send their counterpart. We also informed participants that, "in order to keep things moving as quickly as possible and minimize time delays resulting from one person waiting for the other to make a decision," the study was programmed to automatically

accept any offers "greater than or equal to ¼ of the total number of available tokens," as "this ensures that your bonus payment is maximized." To help acclimate them with the study procedures, participants engaged in a practice round where tokens were worth $0.01 to both themselves and their counterpart.

Participants then proceeded to the main round, where they were told that tokens were worth $0.02 to themselves but only $0.01 to their counterpart. Participants could then choose to send a message to their counterpart indicating that tokens were either worth (a) $0.01 to themselves (deceptive message) or (b) $0.02 to themselves (honest message). If participants presumed that their counterpart would make an offer that results in a relatively even economic outcome for both parties, then sending the deceptive message should lead their counterpart to propose an event split of tokens under the assumption that both parties valued the tokens equally. Thus, misleading their counterpart stood to enhance negotiators' economic outcome at their counterpart's expense (i.e., a 50/50 split of tokens would result in participants earning twice as much as their counterpart).

Incentive Manipulation. Participants were randomly assigned to either the honesty or dishonesty incentive condition. We structured the number of tokens participants bargained over to ensure they earned the same economic outcome in both conditions but arrived at the outcome through either honesty or dishonesty.

Participants in the honesty incentive condition bargained over how to split 30 tokens. We told participants that we would like them "to be as honest as possible about the value of each token to you," and offered them a $0.50 incentive to send their counterpart a message indicating that tokens were worth $0.02 to themselves. To signal that participants' counterpart believed their honest message, they received the following message: "I would like to offer you 10 out of

30 tokens. This split ensures that each of us earns the same bonus payment." As a result of this offer, participants earned $0.20.

In contrast, participants in the dishonesty incentive condition bargained over how to split 20 tokens. We told participants that we would like them to "understate the value of each token to you," and offered a $0.50 incentive to send their counterpart a deceptive message indicating that tokens were worth $0.01 to themselves. They then received a message indicating that their counterpart believed their message: "I would like to offer you 10 out of 20 tokens. This split ensures that each of us earns the same bonus payment." Like the honesty incentive condition, this offer resulted in participants earning $0.20 if they made choices consistent with their incentive.

Dependent Measures. After learning about their outcome, we assessed participants' subjective value in the bargaining task using the 13-item SVI (α = .89). To measure positive and negative affect ($\alpha_{Positive}$ = .86, $\alpha_{Negative}$ = .93), we used the short form PANAS (Mackinnon et al., 1999). To capture participants' guilt, we adapted two items from Grant & Wrzesniewski (2010): "I feel guilty" and "I feel that I have not lived up to my ethical standards" (α = .89).[5] Because the affect items (i.e., positive and negative affect) and guilt items were on different scales with different endpoints (affect: 1 = "very slightly or not at all" to 5 = "extremely"; guilt: 1 = "strongly disagree" to 7 = "strongly agree"), we standardized each affect measure by averaging all items in the scale and converting to a *z*-score.

Results

[5] As in Study 3, we also included "I feel that I have let my partner down" as an item in this study, and in Studies S2 and S3. We 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 $p < .05$ in this and other studies when including this item. We thank an anonymous reviewer and the Associate Editor for raising this concern.

We excluded six participants from our main analyses who expressed suspicion about whether their partner was an actual participant; all effects reported at $p < .05$ hold at $p < .05$ when including them in analyses. Among the 370 remaining participants, 318 made a choice consistent with the incentive they were offered (86%) and were included in our main analyses. We analyzed conditional differences in participants' subjective value and the affect measures using *t*-tests with 95% power to detect a simple effect as small as $d = 0.41$. In analyzing the affect measures, we used a 3 (affect) X 2 (dishonesty) mixed ANOVA, with affect varying within-subjects and dishonesty varying between subjects. All reported effects hold in alternative analyses that compare honest participants to dishonest participants, irrespective of original assignment to incentive condition.

Subjective Value

Consistent with the deceiver's guilt account, dishonesty undermined participants' subjective value, $t(316) = 7.36$, $p < .001$, $d = 0.83$, 95% CI = [0.60, 1.06]. Relative to honest participants ($M = 5.6$, $SD = 1.0$), those who were dishonest were less satisfied with the bargaining experience ($M = 4.7$, $SD = 1.0$).

Affect

A 3 X 2 mixed ANOVA revealed a main effect where dishonesty heightened participants' overall affect, $F(1, 316) = 17.91$, $p < .001$, $\eta_p^2 = .05$. We did not identify a main effect of affect measure, $F(2, 632) = 1.85$, $p = .16$, $\eta_p^2 = .01$.

More importantly, the model revealed an affect measure X dishonesty interaction, $F(2, 632) = 70.66$ $p < .001$, $\eta_p^2 = .18$. To better understand the interaction, we examined the effect of dishonesty on each affect measure. As documented in Table S4 (which contains conditional means and standard deviations), and replicating our other studies, we found that dishonesty

induced guilt, $t(316) = 11.19$, $p < .001$, $d = 1.26$, 95% CI [1.02, 1.50]. Relative to honest participants, those who were dishonest experienced more guilt. Dishonest participants also experienced more negative affect than honest participants, $t(316) = 2.86$, $p = .004$, $d = 0.32$, 95% CI [0.10, 0.55]. We also found that dishonesty reduced participants' positive affect, $t(316) = 4.86$, $p < .001$, $d = -0.55$, 95% CI [-0.77, -0.32]. Relative to honest participants, those who were dishonest experienced less positive affect. Thus, although dishonesty induced guilt and negative affect, it reduced participants' positive affect.

Consistent with the deceiver's delight account, a pair of contrasts with 95% power to detect effect size differences as small as $\eta^2 = .04$ revealed that the effect of the dishonesty incentive on participants' guilt exceeded its effect on their positive affect, $F(1, 316) = 105.68$, $p < .001$, $\eta_p^2 = .25$, and generalized negative affect, $F(1, 316) = 68.10$, $p < .001$, $\eta_p^2 = .18$. Although dishonesty elicited generalized negative affect, it more strongly elicited guilt.

Mediation Model

To examine whether participants' subjective value was more strongly influenced by dishonesty-induced guilt than the impact of dishonesty on the other affect measures, 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 participants' dishonesty and the other affect measures, guilt negatively predicted subjective value, $B = -0.45$ ($SE = 0.07$), $t(313) = 6.82$, $p < .001$, $\beta = -0.41$ and positive affect positively predicted subjective value, $B = 0.34$ ($SE = 0.05$), $t(313) = 6.79$, $p < .001$, $\beta = 0.31$; negative affect did not impact participants' subjective value independently of the other affect measures, $B = -0.07$ ($SE = 0.07$), $t(313) = 1.02$, $p = .31$, $\beta = -0.06$. A bootstrap with 5,000 replications revealed negative indirect effects of the dishonesty incentive on subjective value through guilt, indirect effect = -0.47, 95% CI [-0.67, -

0.29], and positive affect, indirect effect = -0.18, 95% CI [-0.27, -0.09]. However, there was not an independent indirect effect through generalized negative affect, indirect effect = -0.02, 95% CI [-0.07, 0.02].

Study S2: Subjective Value in Future Interactions with the Same Counterpart

Study S2 attempted to replicate the finding in Study 2 that dishonesty reduces subjective value in a follow-up negotiation with the same counterpart. Like Study S1, Study S2 induces dishonesty in an ultimatum bargaining context.

Method

Participants

We collected data from all undergraduate business students who showed up to a study session in exchange for course credit during a data collection wave that occurred at two different universities. In total, 360 participants comprising 180 dyads participated in the study (M_{Age} = 20.24 years, *SD* = 1.71, 50% female).

Procedure

Participants' procedure followed two stages. First, they completed an ultimatum bargaining task with a partner. This task served to manipulate the dishonesty of focal participants playing the responder role in the bargaining task, assess their affect, and present them with the subjective value inventory. Participants then completed a separate face-to-face negotiation with the same counterpart and indicated their subjective value.

Stage 1: Ultimatum Bargaining Task. Participants entered the lab and were seated in front of a computer by an experimenter. The experimenter then paired them with another participant in an ultimatum bargaining task and randomly assigned them to proposer or responder roles. Focal participants were assigned to the responder role while their counterparts

were assigned to the proposer role. Like Study S1, participants bargained over how to split a fixed sum of tokens. However, tokens corresponded to "tickets" towards a $50 Amazon gift card raffle, rather than a bonus payment. Participants in the honesty incentive condition were offered 50 raffle tickets if they chose to send an honest message, and participants in the dishonesty incentive condition were offered 50 tickets if they chose to send a dishonest message.

We made three modifications to the ultimatum bargaining task used for Study 1. Two modifications involved adapting the honesty incentive condition to compare participants telling a self-serving lie to those telling a self-serving truth. First, participants assigned to the honesty incentive condition learned that tokens were worth one raffle ticket to both parties. Thus, they chose between a message indicating that tokens were worth one raffle ticket (honest message) or two raffle tickets to themselves (dishonest message). Second, participants in the honesty incentive condition bargained over how to split 40 tokens. They then received the following message from their partner: "I would like to offer you 20 out of 40 tokens. This split ensures that each of us earns the same bonus payment." The dishonesty incentive condition remained identical to Study 1, which meant participants who were dishonest in the dishonesty incentive condition stood to claim the same amount of value as those who were honest in the honesty incentive condition (20 raffle tickets towards the $50 gift card).[6]

As a third modification, we informed focal participants there was a "small chance that the computer may override your message (this will occur for 10% of participants)." Participants only

[6] To rule out the possibility that conditional differences in the absolute number of tokens counterparts earned might influence their subjective value and impact their behavior in the subsequent negotiation, we informed all counterparts in the dishonesty incentive condition that they were "negotiating for fewer tokens than some other participants" and that they would be awarded "an additional 10 tokens to make up this difference." A *t*-test confirmed that the manipulation did not impact counterparts' subjective value ($M_{\text{Dishonesty}}$ = 5.0, SD = 1.0 vs. M_{Honesty} = 5.2, SD = 1.0), $t(158) = 1.21$, $p = .23$, $d = 0.19$, 95% CI [-0.12, 0.50]. This indicates that the counterparts of honest and dishonest focal participants entered the follow-up negotiation after deriving a similar level of subjective value from the ultimatum bargaining task.

had their message overridden if they did not select the message consistent with their incentive.[7] This change allowed us to ensure that counterparts always received the same message and could therefore not be aware of whether focal participants deceived them in the bargaining task as they entered a follow-up face-to-face negotiation with focal participants.

To ensure that the fairness of the offer they received from counterparts did not impact focal participants' behavior in the subsequent negotiation, we also incentivized counterparts to propose an even split of tokens and informed them that there was a chance the computer could override their offer. After receiving a message from focal participants, counterparts proceeded to choose between making a fair offer (50% of available tokens) and an unfair offer (25% of available tokens). Ultimately, we only overrode the offers of those counterparts who did not make a fair offer. Thus, focal participants always received the following offer (dishonesty incentive condition presented first in brackets): "I would like to offer you [10 / 20] out of [20 / 40] tokens. This split ensures that each of us receives the same number of raffle tickets."

After the ultimatum bargaining task, focal participants proceeded to complete the same dependent measures as in Study S1 (α_{SVI} = .87, $\alpha_{Pos\ Affect}$ = .83, $\alpha_{Neg\ Affect}$ = .88, α_{Guilt} = .93); we once again converted each affect measure into a *z*-score. Participants then learned that they would "complete another negotiation with the same counterpart you just negotiated with" and were instructed to get the attention of an experimenter.

Stage 2: Face-to-Face Negotiation Exercise. Once they got the experimenter's attention, focal participants and their counterparts received role instructions for an upcoming negotiation. They were presented with an adapted version of the New Recruit negotiation

[7] The "10% of participants" figure was a predicted rate estimated from a pretest. Ultimately, 11% of participants had their message overridden. Therefore, this estimate was a relatively accurate representation of how many participants had their message overridden.

exercise (Neale, 1997), which is a scorable negotiation exercise involving a hypothetical job offer negotiation between a job candidate and a recruiter. As detailed in Appendix S-C, we condensed the negotiation to involve one distributive issue (salary), two integrative issues (bonus and vacation time), and one compatible issue (location). In the exercise, participants' outcome determined how many "tickets" they earned for a separate $50 Amazon gift card raffle; participants received a scoring matrix in their role preparation instructions that specified the number of raffle tickets associated with each possible outcome.

Once focal participants and their counterpart both had at least five minutes to prepare for the negotiation, they were seated together in a private room and given ten minutes to complete the upcoming negotiation. In the interest of increasing statistical power by reducing variance in participants' subjective value attributable to their role in the second negotiation exercise, focal participants always played the recruiter role while their counterpart always played the job candidate role. They were allowed ten minutes to complete the negotiation. After the negotiation, participants filled out a form indicating the terms of the negotiated agreement (which we used to compute participants' economic outcome in terms of points earned). The experimenter then separated and reseated participants in front of the computer they used to perform the ultimatum bargaining task. At this point, focal participants and their counterparts confirmed the terms of their agreement and completed the same 13-item SVI as they did before the ultimatum bargaining task, except they were now instructed to reflect on their experience in the face-to-face negotiation exercise they just completed ($\alpha_{\text{Focal Participants}} = .87$, $\alpha_{\text{Counterparts}} = .91$).

Results

We focus our main analyses on the 180 focal participants assigned to the responder role during the ultimatum bargaining task and recruiter role in the subsequent face-to-face negotiation

exercise. Among these 180 participants, 160 made a choice consistent with the incentive they were offered (89%) and are included in our main analyses. All reported effects held in alternative analyses that compare honest participants to dishonest participants, irrespective of original assignment to incentive condition.

Because we triangulated three different surveys and forms into a single dataset, our degrees of freedom vary slightly across analyses. Whether due to an experimenter error or participants running out of time in the study, we could not link four participants' data from the ultimatum bargaining task to their responses in the subjective value questionnaire that followed the face-to-face negotiation exercise. Therefore, the degrees of freedom in our analyses of variables collected from the face-to-face negotiation exercise are lower than they are in our analyses of variables collected from the ultimatum bargaining task.

Stage 1 Subjective Value

As with Study 1, we analyzed focal participants' subjective value following the ultimatum bargaining task using a *t*-test comparing dishonest participants to honest participants; this analysis had 95% power to detect effects as small as $d = 0.57$. Replicating Study S1, and consistent with the deceiver's guilt account, dishonesty undermined participants' subjective value, $t(158) = 10.70$, $p < .001$, $d = 1.69$, 95% CI [1.33, 2.05]. Relative to honest participants telling a self-serving truth ($M = 5.5$, $SD = 0.9$), those who were dishonest were less satisfied with their experience in the ultimatum bargaining task ($M = 4.2$, $SD = 0.7$).

Affect

We again analyzed focal participants' affect after the ultimatum bargaining task using a 3 (affect measure: positive affect, negative affect, guilt) X 2 (dishonesty: honest, dishonest) mixed ANOVA with dishonesty varying between subjects and affect measure varying within-subjects.

First, the model revealed a main effect where dishonesty heightened participants' overall affect, $F(1, 158) = 34.53$ $p < .001$, $\eta_p^2 = .18$. We did not identify a main effect of affect measure, $F(2, 316) = 0.04$, $p = .96$, $\eta_p^2 < .01$.

Replicating the prior studies, we found evidence of an affect measure X dishonesty interaction, $F(2, 316) = 30.84$, $p < .001$, $\eta_p^2 = .16$. To better understand the interaction, we examined the effect of dishonesty on each affect measure. As documented in Table 2 (which contains conditional means and standard deviations), and replicating the prior studies, dishonesty induced guilt, $t(158) = 11.06$, $p < .001$, $d = 1.75$, 95% CI [1.38, 2.11]. Relative to honest participants, those who were dishonest experienced more guilt. However, in contrast to Study S1, dishonesty did not impact participants' positive affect, $t(158) = 0.65$, $p = .52$, $d = 0.10$, 95% CI [-0.21, 0.41]. We again found evidence that dishonesty increased participants' generalized negative affect, $t(158) = 3.48$, $p < .001$, $d = 0.55$, 95% CI [0.23, 0.87].

A pair of contrasts with 95% power to detect effect size differences as small as $\eta^2 = .08$ confirmed that the effect of dishonesty on participants' guilt exceeded its effect on their positive affect, $F(1, 158) = 48.70$, $p < .001$, $\eta_p^2 = .24$, and generalized negative affect, $F(1, 158) = 25.50$, $p < .001$, $\eta_p^2 = .14$. This pattern is consistent with our other studies.

Table S4

Studies 1 and 2: Affect by Condition

	Study 1				Study 2			
	Dishonest		Honest		Dishonest		Honest	
Variable	*M*	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Guilt	0.5^{a}	1.0	-0.5^{b}	0.7	0.7^{a}	0.9	-0.6^{b}	0.5
Positive Affect	-0.3^{c}	0.9	0.2^{d}	1.0	-0.1^{c}	1.1	0.1^{c}	1.0
Negative Affect	0.0^{d}	0.9	-0.3^{c}	0.8	0.3^{cd}	1.2	-0.2^{ce}	0.6

Note. Means with different superscripts for a given study differ at $p < .05$.

Stage 2 Subjective Value

As expected, dishonesty also affected participants' subjective value in the subsequent face-to-face negotiation with the same counterpart, $t(154) = 2.35$, $p < .021$, $d = 0.38$, 95% CI [0.06, 0.69]. Relative to participants who were honest during the ultimatum bargaining task (M = 5.4, SD = 0.9), those who were dishonest experienced less satisfaction with the subsequent negotiation (M = 5.0, SD = 1.0).[8]

Serial Mediation Model

We then tested a serial mediation model examining the indirect effect of dishonesty on subjective value in the face-to-face negotiation exercise (Stage 2 subjective value) through guilt and subjective value in the ultimatum bargaining task (Stage 1 subjective value), controlling for generalized negative affect. First, consistent with Study S1, a regression revealed that focal participants' guilt negatively predicted their Stage 1 subjective value after controlling for their dishonesty, B = -0.44 (SE = 0.08), $t(152) = 5.55$, $p < .001$, β = -0.43.[9] When controlling for participants' dishonesty in the ultimatum bargaining task and guilt, their Stage 1 subjective value positively predicted their Stage 2 subjective value, B = 0.26 (SE = 0.11), $t(151) = 2.49$, $p = .014$, β = 0.28. Consistent with serial mediation, we identified a negative indirect effect of dishonesty on Stage 2 subjective value through guilt and Stage 1 subjective value in serial, indirect effect = -0.14, 95% CI [-0.29, -0.04]. These analyses suggest that, above and beyond the influence of generalized negative affect, participants' dishonesty-induced guilt stemming from their behavior

[8] Participants' Stage 2 subjective value was weakly correlated with Stage 1 subjective value, $r(154) = .27$, $p < .001$. This weak correlation suggests that, despite using the same scale about the same counterpart, Stage 2 subjective value is relatively distinct from Stage 1 subjective value, as only approximately 7% of the variance in participants' subjective value in Stage 2 was attributable to their subjective value in the previous negotiation.

[9] To replicate Study 1, we also conducted a mediation analysis testing the indirect effect of dishonesty on Stage 1 subjective value through guilt, negative affect, and positive affect. As in Study 1, we identified an independent indirect effect of dishonesty on Stage 1 subjective value through guilt, indirect effect = -0.52, 95% CI [-0.76, -0.31].

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 prior negotiation.

Ancillary Analyses

Did Dishonesty Impact Counterparts' Subsequent Subjective Value? In an exploratory vein, we also analyzed the subjective value of counterparts in the follow-up negotiation. While we structured the Stage 1 ultimatum bargaining task so that counterparts all earned the same economic outcome and could not have been aware of focal participants' dishonesty, it could be possible that dishonest focal participants behaved in a manner that impacted counterparts' subjective value in the Stage 2 negotiation. We did not find evidence of focal participants' dishonesty impacting counterparts' subjective value ($M_{\text{Dishonest}}$ = 5.2, SD = 0.9 vs. M_{Honest} = 5.4, SD = 0.9), $t(154) = 1.15$, $p = .25$, $d = 0.18$, 95% CI [-0.13, 0.50]. This finding suggests that relative to honest focal participants, dishonest participants did not necessarily behave in a manner that adversely impacted counterparts' experience in a subsequent face-to-face bargaining encounter.

Did Dishonesty Impact Negotiators' Economic Outcomes in the Next Negotiation? Although we structured the ultimatum bargaining task so that honest and dishonest focal participants' economic outcomes were held constant, it could be possible that negotiators' dishonesty in the ultimatum bargaining task influenced their economic outcomes in the follow-up face-to-face negotiation. Participants who were dishonest in the Stage 1 ultimatum bargaining task (91%) reached agreements in the Stage 2 negotiation at similar rates to those who were honest (95%), $\chi^2(1, N = 160) = 1.14$, $p = .29$, $OR = 0.51$, 95% CI [0.14, 1.80]. Among participants who reached agreement, we failed to find evidence of focal participants' dishonesty in Stage 1 impacting the number of raffle tickets they earned while playing the recruiter role in

the Stage 2 exercise ($M_{Dishonest}$ = 81.8, SD = 16.2 vs. M_{Honest} = 78.4, SD = 16.2), $t(147) = 1.25$, $p = .21$, $d = 0.20$, 95% CI [-0.12, 0.52]. Thus, the reduced subjective value of dishonest participants in the subsequent negotiation could not be accounted for by any impact that previously getting away with a lie might have had on their negotiation performance.

Supplemental Analysis: Was it Better to be a Sucker Than a Liar?

Unlike focal negotiators in our studies, whose subjective value was consistently undermined by their dishonesty, counterparts failed to detect dishonesty. Being lied to either had no effect, or even enhanced, their subjective value in our studies. This pattern raises the intriguing possibility that, despite getting away with their lies, dishonest participants were worse off than their unsuspecting counterparts from the standpoint of subjective value. In Table S5, we document that this tended to be the case across our studies.

Table S5

Effect of Participant Role on SVI by Study and Dishonesty

Study	Dishonest Participants		Honest or Control Participants	
	d	95% CI	*d*	95% CI
1	-0.65[a]	[-0.87, -0.44]	0.22[b]	[0.04, 0.40]
2	-0.72[a]	[-1.02, -0.41]	-0.04[b]	[-0.31, 0.23]
3A[1]	-0.10	[-0.36, 0.16]	0.09	[-0.17, 0.35]
3B	-0.45[a]	[-0.62, -0.28]	-0.02[b]	[-0.16, 0.20]

Note. d values represent the simple effect of role (focal participants = 1, counterparts = 0), controlling for agreement (all studies), counterpart suspicion (Studies 1, 2, and 3B), and instructor (Study 3B only). *d* values in the same row with different superscripts differ at $p < .05$. All effects for Studies 1, 2, and 3B were estimated using the same ANCOVA models reported in the main manuscript, with role added as a within-subjects factor. The Study 3A effects were estimated using a multilevel linear regression model nesting participants within dyads with a dyad-specific random intercept (estimated with maximum likelihood).
[1]Effects estimated at +1 SD above the mean dishonesty code (dishonest participants column) and -1 SD below the mean dishonesty code (honest or control participants column).

Study S3: Deceiver's Guilt When Dishonesty Elicits Positive Affect

In our main studies, we fail to find any evidence of dishonesty eliciting positive affect. This contrasts with previous studies showing dishonesty causes positive affect (Ruedy et al., 2013; Peer et al., 2014). Study S3 aimed to reconcile our findings with previous work by first replicating the effect of dishonesty on positive affect and then comparing the effect to dishonesty's impact on guilt. If dishonesty elicits more guilt than positive affect, then this would indicate that the deceiver's guilt account can continue to hold even when dishonesty elicits positive affect.

We used a paradigm from previous research where dishonesty elicited positive affect within the deceiver (Ruedy et al., 2013). However, we adapted the paradigm to emulate a fundamental feature of negotiation contexts: dishonesty can impact a counterpart's outcome. Notably, whereas prior research has only tested for the effect of dishonesty on generalized positive and negative affect, we also incorporated a direct measure of guilt that is distinct from generalized negative affect.

As documented below, we replicated Ruedy et al. (2013) in finding that dishonesty elicits positive affect in this context. However, we continued to find evidence that dishonesty elicited more guilt than positive affect. This suggests that, even in contexts where dishonesty elicits positive affect, the deceiver's guilt account can still hold. Because prior studies examining the affective consequences of dishonesty have relied on measures of generalized negative affect that either obscure the effect of guilt with a host of other negative emotions (Lee et al., 2015; Peer et al., 2014; Ruedy et al., 2013) or do not directly account for guilt (Ruedy et al., 2013), they may have underestimated the adverse affective consequences of dishonesty.

Method

Participants

Aiming to replicate the effect of dishonesty on positive affect in Study 4 of Ruedy et al. (2013), our target final sample size for conducting main analyses was approximately 2.5 times the sample reported in the Ruedy et al. (2013) study, or 400 participants (see Simonsohn, 2015). Assuming that up to 40% of participants in the dishonesty incentive condition would not choose to cheat (and thus not be included in our main analyses),[10] we posted 500 assignments to Amazon Mechanical Turk (MTurk), resulting in a final sample of 502 participants who completed the study in exchange for a $2.25 base payment ($M_{Age}$ = 36.2 years, SD = 11.1, 41% female).

Procedure

The study closely followed the procedures of Study 4 in Ruedy et al. (2013),[11] but we adapted the paradigm so that participants' choices impacted the outcome of a specific assigned "partner." Consistent with Ruedy et al. (2013), participants read that they would perform "a variety of tasks in a sequential order" to help us understand how their personality impacts their emotions and use of different problem-solving techniques.

However, participants were first paired in a chatroom with another research participant rather than proceeding directly to a problem-solving task. In the interest of providing a task that appeared to assess participants' personality, we prompted them to perform the "fast friends" task by taking turns answering a series of personal questions (Aron et al., 1997).

After exiting the chatroom, participants then answered a series of 20 math and logic questions. They learned that their "partner" (ostensibly the same person they just chatted with)

[10] This assumption was a conservative estimate derived from Study 4 of Ruedy et al. (2013), in which 32% of participants randomly assigned to a dishonesty opportunity condition were honest.

[11] We selected this paradigm because participants were unlikely to be familiar with the experimental task. Unlike the other paradigms reported by Ruedy et al. (2013), which adapted widely used matrix and anagram-solving tasks from prior studies (Mazar et al., 2008; Wiltermuth, 2011), we are not aware of any other studies that use this paradigm.

was tasked with completing the same questions.[12] Following the same procedure as Ruedy et al. (2013), we informed participants that they would have 25 seconds to answer each problem and that incorrect answers would not harm their score. As an additional instruction, we told participants that, because we wanted to see how they respond to feedback, the correct answer to the problem they just completed would flash on their screen before displaying the next question.

Participants then learned that they would compete with their partner on the task after completing a sample problem. They were awarded $0.05 for each correct answer, on top of their guaranteed $2.25 base payment. To clarify that participants' performance would impact their counterpart, they read that their partner's bonus payment would "depend on how well you perform." Partners earned a bonus of $0.10 for each correct answer if they beat the participant's score, $0.05 per correct answer for tying the participant, and nothing if they lost to the participant. To reinforce their partner's interdependence, participants read that "for every question you answer correctly on the test, your bonus payment will increase, but your partner's chances of earning a bonus payment will DECREASE."

Dishonesty Opportunity Manipulation. Participants were randomly assigned to either a dishonesty opportunity or control condition. They read that the correct answers were not displaying properly on some web browsers and, after completing the sample problem, were relayed embedded data verifying their web browser and operating system. In the control condition, participants received a confirmation that the answers were displaying properly on their browser and their partner's browser.

Like Ruedy et al. (2013), participants in the dishonesty opportunity condition could surreptitiously access the correct answers to each problem before submitting their response

[12] Participants' "partner" was a previous participant who correctly answered 4 out of 20 questions in a pretest.

(including the sample problem). After seeing embedded data on their web browser and operating system, participants in the dishonesty opportunity condition read the following:

> Based on this data, the answers to each question were not displaying properly on your computer. It is likely that, instead of hiding the correct answer to a given question until after you completed it, your browser instead showed a button that said "CORRECT ANSWER."

To clarify that clicking the CORRECT ANSWER button would constitute an act of dishonesty, we told participants that their partner would "not be able to see the correct answer to each question until AFTER completing a given question, but before proceeding to the next one." Participants then read that "because choosing to access the answers to a question before submitting your response would put your partner at a competitive disadvantage, please disregard the 'CORRECT ANSWER' button if you see it and solve the problems on your own." Irrespective of whether they chose to access the answer to a given question before submitting their response, participants in the dishonesty opportunity condition saw the correct answer flash on their screen for two seconds between problems.

Dependent Measures. Because the main task was quite challenging, there was a risk of participants rationalizing that their partner could have outperformed them on the task (cf. Moore & Healy, 2008). To limit this possibility, we informed participants after completing the math and logic questions that their partner scored below average, and they most likely "earned a higher score" than their partner. We then reminded participants of their partner's potential payoffs, which would likely be $0 based on the probability participants earned a higher score than their partner.

After telling them their partner's score, we measured participants' positive affect, negative affect, and guilt using the same items as in Studies S1 and S2 (counterbalanced order; $\alpha_{Positive}$ = .86, $\alpha_{Negative}$ = .93; α_{Guilt} = .89). Participants then learned their score on the task and answered two demographic questions. Consistent with Ruedy et al. (2013), we then gauged participants' suspicion with open-ended questions asking them to indicate the study's purpose and any other comments they were interested in sharing.

Results

Our suspicion checks revealed that three participants were skeptical about whether their partner was an actual participant. Eight additional participants indicated that they believed the study was testing dishonesty in some manner. We excluded these eleven suspicious participants from analyses, resulting in a final sample of 491 participants for our main analyses. All effects reported at $p < .05$ hold at $p < .05$ when including these participants in analyses.

Among the 248 participants assigned to the dishonesty opportunity condition, 176 (71%) cheated by choosing to access the correct answer to at least one problem before submitting their answer. Replicating the approach of Ruedy et al. (2013), we compared the affect of only the 176 participants who chose to be dishonest to those in the control condition. Therefore, we analyzed participants' affect using a 3 (affect measure: positive affect, negative affect, guilt) X 2 (dishonesty: dishonest, control) mixed analysis of variance (ANOVA) with affect varying within-subjects and dishonesty varying between subjects.

Our results are summarized in Figure S1 (which contains a box-and-whisker plot of affect by condition). The model revealed a main effect where dishonesty heightened participants' overall affect. Most importantly, we found evidence of an affect measure X dishonesty

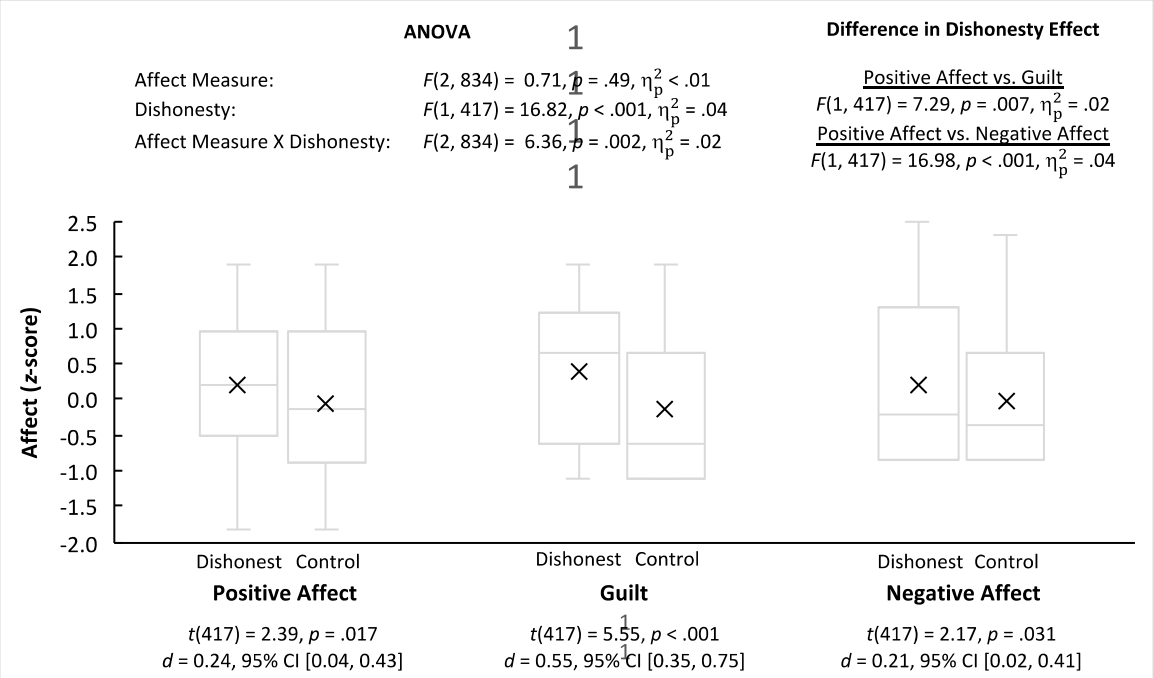
interaction.[13] As documented in Figure 1, and replicating Ruedy et al. (2013), dishonesty induced positive affect. Relative to the control condition, participants who chose to be dishonest experienced more positive affect. However, dishonesty also induced guilt. Dishonest participants experienced more guilt than those in the control condition. In addition to these effects, we also found evidence that dishonesty induced generalized negative affect. Relative to participants in the control condition, those who were dishonest experienced more negative emotions that were not guilt-specific.

A pair of contrasts revealed that the effect of dishonesty on participants' guilt exceeded its effect on their positive affect and negative affect. These analyses suggest that, although dishonesty elicited positive affect, it more strongly elicited guilt. This finding is consistent with the deceiver's guilt account (Hypothesis 1b) and inconsistent with the deceiver's delight account (Hypothesis 1a). Further, because the effect of dishonesty on guilt was stronger than the effect of dishonesty on negative affect, this indicates that the impact of dishonesty on guilt was distinct from the impact of dishonesty on generalized negative affect.

[13] Like Ruedy et al. (2013), we conducted alternative analyses comparing participants in the dishonesty incentive condition who cheated to participants who did not cheat and those in the control condition. The same interaction pattern held in these analyses, $F(2, 978) = 6.83$, $p = .001$, $\eta_p^2 = .01$.

Figure S2

Study S3: ANOVA Results and Box-and-Whisker Plot of Affect by Condition



Note. Conditional means are denoted by an "X."

Ancillary Analysis: Did Honesty Reduce Guilt?

In an analysis that also included participants who chose to be honest in the dishonesty opportunity condition, an ANCOVA on participants' guilt revealed a main effect of participants' choice to be honest, $F(2, 499) = 24.46$, $p < .001$, $\eta_p^2 = .09$. Follow-up contrasts revealed that participants who chose to be honest ($M = 0.4$, $SD = 1.0$) experienced less guilt than (a) dishonest participants, $t(499) = 6.15$, $p < .001$, $d = 0.55$, 95% CI [0.37, 0.73], and (b) control condition participants, $t(499) = 2.22$, $p = .028$, $d = 0.20$, 95% CI [0.03, 0.37].

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Appendix S-A

Payoff Charts for Study 1 Participants

Focal Participants (Sellers)

	Bonus Payment	
Sale Price	Large Incentive	Small Incentive
No Agreement	$0.00	$0.00
$2,500 or less	$0.00	$0.00

$2,501-$2,749	$1.25	$0.10
$2,750-$2,999	$2.50	$0.20
$3,000-$3,249	$3.75	$0.30
$3,250-$3,499	$5.00	$0.40
$3,500-$3,749	$6.25	$0.50
$3,750-$3,999	$7.50	$0.60
$4,000-$4,249	$8.75	$0.70
$4,250-$4,499	$10.00	$0.80
$4,500-$4,749	$11.25	$0.90
$4,750 or higher	$12.50	$1.00

Counterparts (Buyers)

Dishonesty Opportunity Condition			Control Condition	
Sale Price	Bonus Payment		Sale Price	Bonus Payment
	Computer in Good Condition	Computer Needs Repair		
$2,500 or less	$1.00	$0.60	$2,500 or less	$0.60
$2,501-$2,749	$0.90	$0.50	$2,501-$2,749	$0.50
$2,750-$2,999	$0.80	$0.40	$2,750-$2,999	$0.40
$3,000-$3,249	$0.70	$0.30	$3,000-$3,249	$0.30
$3,250-$3,499	$0.60	$0.20	$3,250-$3,499	$0.20
$3,500-$3,749	$0.50	$0.10	$3,500-$3,749	$0.10
$3,750-$3,999	$0.40	$0.00	$3,750 or higher	$0.00
$4,000-$4,249	$0.30	$0.00	No Agreement	$0.00
$4,250-$4,499	$0.20	$0.00		
$4,500-$4,749	$0.10	$0.00		
$4,750 or higher	$0.00	$0.00		
No Agreement	$0.00	$0.00		

Appendix S-B

Payoff Charts for Study 2 Participants

Focal Participants (Sellers)

Sale Price	Raffle Tickets
No Agreement	0
$2,500 or less	0
$2,501-$2,749	1
$2,750-$2,999	2
$3,000-$3,249	3
$3,250-$3,499	4
$3,500-$3,749	5
$3,750-$3,999	6
$4,000-$4,249	7
$4,250-$4,499	8
$4,500-$4,749	9
$4,750 or higher	10

Counterparts (Buyers)

Dishonesty Opportunity Condition			Control Condition	
Sale Price	Raffle Tickets		Sale Price	Raffle Tickets
	Computer in Good Condition	Computer Needs Repair		
$2,500 or less	10	6	$2,500 or less	6
$2,501-$2,749	9	5	$2,501-$2,749	5
$2,750-$2,999	8	4	$2,750-$2,999	4
$3,000-$3,249	7	3	$3,000-$3,249	3
$3,250-$3,499	6	2	$3,250-$3,499	2
$3,500-$3,749	5	1	$3,500-$3,749	1
$3,750-$3,999	4	0	$3,750 or higher	0
$4,000-$4,249	3	0	No Agreement	0
$4,250-$4,499	2	0		
$4,500-$4,749	1	0		
$4,750 or higher	0	0		
No Agreement	0	0		

Appendix S-C

Study S2 Role Instructions and Payoff Matrices for Stage 2 Negotiation

Role-specific instructions appear in the following order: [recruiter role / candidate role].

RECRUITER

This is a negotiation between a job recruiter and a job candidate. You will play the role of the Job [Recruiter / Candidate]. There are four issues of concern in this negotiation:

- Bonus
- Vacation Time
- Salary
- Location

Your goal, as the [recruiter / candidate], is to reach an agreement with the [candidate / recruiter] on all four issues that is best for you. You will negotiate with the same counterpart you completed the prior negotiation with over raffle tickets for a separate $50 Amazon gift card raffle. THE MORE RAFFLE TICKETS YOU EARN, THE BETTER YOUR CHANCES OF WINNING A $50 GIFT CARD. You may determine what agreement is best for you by referring to the "[Recruiter / Candidate] Point Sheet" on the next page.

The 4 issues are listed separately. There are five different alternatives for each of the issues. For example, the salary can range from $82,000 to $90,000. Please note the number of raffle tickets you will receive for each type of agreement. As a negotiator, you may settle upon any of the five alternatives for each of the issues. Thus, there are a very large number of feasible settlements.

You should note that each issue has a different degree of importance to you, as indicated by the magnitude of the number of raffle tickets you could gain. You will have 10 minutes to reach agreement on all 4 issues. In order for any agreement to be binding, you need to reach an agreement with the candidate on all 4 issues. If you do not reach an agreement with your counterpart, both of you will receive 0 raffle tickets.

IMPORTANT INSTRUCTIONS: *Do not, at any time, tell the other person how many raffle tickets you are getting. Also, do not let the other negotiator see your "[Recruiter / Candidate] Scoring Sheet." This information is strictly for you.*

Please become very familiar with the "[Recruiter / Candidate] Scoring Sheet" on the next page. Feel free to make notes or write on it. The highest number of total raffle tickets you can obtain from this negotiation is 128 and the lowest number is 0 (*see next page*).

RECRUITER POINT SHEET		
ISSUE	OPTIONS	RAFFLE TICKETS
Bonus	10%	[0 / 40]
	8%	[4 / 30]
	6%	[8 /20]
	4%	[12 / 10]
	2%	[16 / 0]
Vacation Time	25 days	[0 / 16]
	20 days	[10 / 12]
	15 days	[20 / 8]
	10 days	[30 / 4]
	5 days	[40 / 0]
Salary	$90,000	[0 / 60]
	$88,000	[15 / 45]
	$86,000	[30 / 30]
	$84,000	[45 / 15]
	$82,000	[60 / 0]
Location	New York	[0 / 0]
	Boston	[3 / 3]
	Chicago	[6 / 6]
	Atlanta	[9 / 9]
	San Francisco	[12 / 12]

Note: Do not let the other person see your Scoring Sheet.